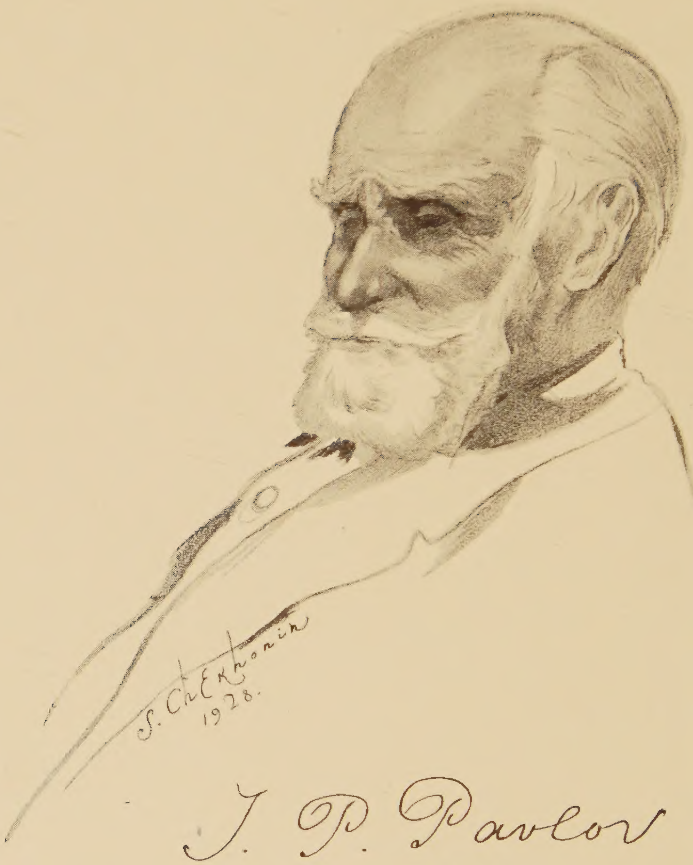
LECTURES ON CONDITIONED REFLEXES


S. Chekhonin
1928.
I. P. Pavlov

LECTURES ON CONDITIONED REFLEXES

Twenty-five Years of Objective Study of the Higher Nervous Activity (Behaviour) of Animals

By

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TRANSLATOR'S PREFACE

This book is one of three which Professor Pavlov has written during his life of nearly four score years. The first covered his researches on the functions of the digestive glands, and appeared in Russian in 1897, and later in German, French, and English. A large part of our knowledge of the physiology of the digestive system is based on the discoveries of this investigator.

Over a quarter of a century ago Pavlov began, "after persistent deliberation," to study the activity of the higher parts of the brain by physiological methods, mainly through the use of his conditioned reflexes. This book shows the historical development of the subject from the very beginning to the present, and it contains most of the important facts which he has discovered. As the subject is a new and somewhat difficult one, the many repetitions in varying form will probably not be unwelcome to the reader.

Because the facts and methods described in the following pages cover most of Pavlov's important work on conditioned reflexes, it seemed worth while to us to make the translation from the original Russian. The labour bestowed on this task has not been given grudgingly; on the other hand, we have often felt the joy and privilege of thus paying a personal debt of gratitude to the Author by helping to disseminate the truths described herein—the products of an unusual life inspired by a passionate, untiring, and uninterrupted search for Scientific Truth.

Feeling, therefore, the importance of the book, we have taken pains to preserve every slightest shade of meaning of the Author. We regret we were not able to translate the fiery ardour which glows in the original—the zeal for the hunt of scientific facts.

The present volume contains five new chapters from the Author besides those of the third Russian edition from which the translation was made. We personally have put in all the italics, the explanatory footnotes, the chapter epitomes, and the biographical sketch, with the hope that these additions would help to bring the book to a wider circle of readers. The biography is an attempt to give some idea of the life of this remarkable man, though to do so adequately is, we confess, a well-nigh hopeless goal. Furthermore, it is included on our own initiative, without the cognisance of the Author; for we fear he would consider it a useless and superfluous appendix to the work. The photographs are our own original snapshots, and they were taken, sometimes after many hours of waiting, without the knowledge of Professor Pavlov.

In such epochal researches as those described in this book, we felt the desirability of enlisting the aid of experienced physiologists, and the collaboration we have obtained has proved a highly fitting and happy choice.

Dr. G. Volborth, the translator of the German edition, and for many years one of Pavlov's chief collaborators, shares the responsibility for this book. His assistance has been indispensable from beginning to end. He read and corrected the first drafts of the translation—not a light task—and contributed many of the footnotes.

Dr. Cannon has unselfishly devoted much labour and consideration to the physiological expressions, idioms, and style of this volume, having assiduously perused every word of it. His modifications form an integral part of the book.

Also others have willingly dedicated their time in aiding with its production: Dr. Spirov has rendered invaluable help in the actual work of translation, and my secretary, Miss S. Kiryanova, in the preparation of the manuscript; Dr. Babkin has compared it with the original Russian and verified its accuracy. K. Chukovsky, the Russian literary critic, and P. S. Kupalov have given me suggestions; and Chehkonin, a prominent contemporary Russian artist, has made the portrait for the frontispiece. Last but not least, we, as well as the readers, are indebted to A. H. Easley for having advanced the funds for the completion of the translation, thereby making possible its appearance in English. Many friends have thus united in preparing this edition, and we gratefully thank each of them. Furthermore, the publishers deserve credit for the patience and careful attention which they have devoted to the numerous details; and all the more so because of the difficulties necessitated by the great distance separating the translator and them.

W. HORSLEY GANTT.

Leningrad, September 1, 1928.

IVAN P. PAVLOV: A BIOGRAPHICAL SKETCH

By W. Horsley Gantt

I

Seventy-nine years ago, September 26, 1849, there was born in Ryazan, a peasant town of central Russia, the first son of a poor priest. The name of this boy was Ivan Petrovitch Pavlov. In the family in which the child grew up an earnest though friendly spirit ruled, and the atmosphere of his home life was conducive to fostering a certain seriousness with which he looked upon the questions confronting him.

Even from early childhood we can find those tendencies which characterise the adult Pavlov. Whenever the boy Ivan, or "Vanya" as he was called, undertook to do a thing he put heart and soul into it until the goal was attained—whether in play or athletics, physical or school work. In later years he showed the same zeal in the discussions and disputes of the lecture hall and laboratory. He was not so much concerned with excelling his rivals as he was in applying to each task all his might and knowledge—his whole being—in order that it should be done in the most perfect way of which he was capable.

For the following facts concerning his ancestry and boyhood life we are indebted to Prof. V. V. Savitch, one of Pavlov's oldest and most revered collaborators and pupils, who has given us a vivid picture of the early life of his famous teacher.[1] We have by the kind permission of Prof. Savitch drawn freely on his interesting biography without making separate acknowledgment in every instance.

The father, Peter Dimitrievitch Pavlov, was a village priest, and the grandfather was only a sexton of a country church. The life of the rural clergy at that time was very hard and especially so for those who were lower in rank. They had to work for their daily bread, and their mode of living was just the same as that of any peasant. Agriculture was the chief source of income. This strenuous physical work together with a degree of intellectual development produced a generation which was strong, healthy, and energetic. Constant need tempered those robust natures, and a certain amount of intelligence made them strive to fight their way under the difficult conditions of life at that period.

Intellectually Peter Dimitrievitch was a marked contrast to the other clergy of that parish. He always had a great liking for reading, and, even in those backward times, indulged in buying books; and these purchases, owing to his restricted means, were real family festivities. Prof. Pavlov often gratefully recalls his father's instructions to read a book twice in order to understand it thoroughly. Peter Dimitrievitch gained general respect among the clergy, and besides standing high intellectually, possessed other exceptional characteristics. Very exacting of others, he was also exacting of himself. He was

[1] "Ivan Petrovitch Pavlov, A Biographical Sketch," *Pavlov's Seventy-fifth Anniversary Volume,* Moscow, 1924 (Russian).

intensely persistent, had a determined will and great physical strength. From his parents he had inherited the love of the soil; and forced to live in town he devoted his attention to his orchard and vegetable garden, where he did all the work himself. Of all the children only Ivan Petrovitch had this same liking.

When the question of daily bread had become less acute, the excess of energy sought for an outlet. Thus it happened that the paternal uncle of Prof. Pavlov used to take part in the fist-fighting which flourished at Ryazan. These combats and the conversation about them created and kept up high spirits in the family. Because of this display of energy there was an unusual liveliness in the Pavlov household. The cheerfulness, laughter and humour of this uncle were irresistible. Doubtless a great comedian was lost in him, but being of the clergy he suffered constant rebuffs from his superiors, and was even reduced to the position of a sexton. His children inherited those qualities, which made them welcome members of every social party.

Also Ivan Petrovitch's mother, Varvara Ivanovna, came from a clerical family. All the schooling she had was given her at home—in those times it was not considered necessary that daughters of the clergy should be educated. In her youth Varvara Ivanovna had excellent health, which was inherited by three of her children, Ivan, Dimitry, and Peter. All of these finished the theological seminary and the university, and received posts at the university or at the academy. One of his brothers was later an assistant to Mendeleév. After the birth of these three sons, the mother had a serious illness, and the next six children died from epidemics at an early age. The last two, Serge and Lydia, were not as gifted as the first three. Serge Petrovitch finished only the seminary and remained as a priest at Ryazan: he died of typhus during the revolution.

Thus Ivan Petrovitch grew up with his younger brothers, companions in his games and tricks while the parents were busy with their daily tasks. The mother loved and spoiled her children, but as she was usually busy, she allowed them great liberty, and they grew up free-spirited. They soon made friends with the neighbouring children, and spent much time in the streets with their playmates, taking part in the ordinary sports of the village, the chief of which was *gorodkee* (a Russian country game like ninepins), which Pavlov plays to this day.

Ivan Petrovitch played with his left hand; his father also was left-handed. The son developed his right hand only by constant training, so that now he can use both hands alike, even while operating. However, his left hand is stronger and he makes use of it when special skill or power is necessary. He writes with his right hand, but can do so equally well with his left, with which he can also write mirror fashion.

His education began at the age of seven when he learned to write and read from an old lady. Those lessons were not attractive to the restive boy, for digging the ground in the garden with his father was more to his taste, and he enjoys this work even now. He learnt a little carpentry and lathe work while a house was being built. Thus from his early youth Ivan Petrovitch had a great inclination for every sort of physical exercise. Later that changed into a passion for sport, and he was the soul of the Physicians' Gymnastic Society. Prof. Pavlov often says that the feeling of gratification after successful muscular work was much greater with him than the joy of having solved some important mental problem. He calls it "muscular gladness."

At about the age of ten he fell from a fence on a brick pavement. Though previously healthy, he was now often ill, and at one time his parents feared he had lung trouble. This accident delayed his education. He was already eleven years old when he entered, together with his brother Dimitry, the second class of the church school at Ryazan.

Fighting was a custom there, and as Ivan Petrovitch was in poor health he often had a bad time of it. This led him to try to strengthen himself by muscular exercise. His father made the necessary arrangements for gymnastics in the garden. It was noticeable here that Ivan Petrovitch showed the greatest persistency, while his brothers soon grew tired of gymnastics and sought other pleasures. And "when the boys were sent to the orchard to gather raspberries, Ivan Petrovitch tried to fill his basket quickly, while his brother Dimitry (like all boys) tried to fill his mouth." Thus from his early youth Ivan Petrovitch showed great tenacity in fulfilling the tasks which had been set, whatever they might be.

Having finished the church school, the Pavlov brothers entered the theological seminary at Ryazan. The curriculum was made up mainly of courses in the ancient languages. Logic and rhetoric were also on the programme, and not only elementary philosophy was studied, but the several philosophical systems furnished the material for frequent debates. Here Pavlov was thoroughly drilled in logical reasoning and in its application. It was now that Pavlov became seriously interested in science. He says he was first attracted to it by the Russian translation of G. H. Lewes' *Practical Physiology,* and to-day he treasures with pride the well-worn copy of that book which he began to read when a boy of fifteen.

The great wave of enlightenment which swept over Russia during the reign of Alexander II, ushered in by the abolition of serfdom and a certain amount of self-government, was also felt in the seminaries. Teachers and pupils united to form a single front in which the older generation endeavoured to give their best to the younger, and the latter strove with might and main to take advantage of this.

At the doors of the library throngs would be waiting to rush in and get at the latest literature. The Pavlov brothers used to stand in these crowds, though there was little chance of getting in first, as there were many competitors. Endless discussions followed the reading of the new books of that period. And it happened often that the peaceful town of Ryazan witnessed groups of students debating loudly all over the streets. Among those debaters Ivan Petrovitch was prominent; for his animated arguments were always accompanied by energetic gesticulations. These discussions, however, had the effect of teaching him to be careful in his criticism, because those who made evident flaws were held up to ridicule.

Pavlov recalls the liberal atmosphere of his school with gratitude, and particularly the fact that if a pupil made great progress in one subject, he might devote less attention to other studies which interested him less, thus giving him the opportunity to advance along the line of his special tastes.

In 1870 Pavlov relinquished the idea of becoming a priest, and left without finishing the seminary to enter the University of St. Petersburg.

He lived with his brother, and by degrees the common tasks of every-day life fell to Dimitry's lot. This went so far that he even used to order Ivan Petrovitch's clothes. After the latter's marriage it was his wife who used to buy her husband's boots and wearing apparel. "Sometimes the young Pavlov would unexpectedly get a suit of clothes and his choice of colours would be such as to make his friends laugh and his family angry."

The two brothers used to spend their summers with their father at Ryazan. Ivan Petrovitch generally stayed at home, and never went hunting as the others loved to do. Many friends visited them and their usual pastime was *gorodkee;* for hours one could hear the clashing of sticks, interrupted by explosions of laughter, and outbursts of rival yells of enthusiasm. Even in this Ivan Petrovitch's character stood out prominently. He was excitable by nature, but he could nevertheless keep his temper and hurl the clubs with great force and precision. He was passionate in all he undertook, but this passion was always controlled and checked. Evidently the processes of strong emotion were well regulated and limited by the necessary inhibition, as described in some of the chapters of this book.

II

While at the university he sat under such talented professors as Mendeleév (inorganic chemistry) and Buttlerov (organic chemistry). To the brilliant physiologist Elie Tsyon, however, Pavlov feels that he owes the most. We have heard him say what a deep impression he retains of the demonstration by this investigator of the experiment with the stimulation of the anterior and posterior spinal roots in the dog; and that Tsyon worked so cleanly that he often operated in a frock coat and white gloves to avoid the necessity of going home to dress for a faculty meeting.

In 1874 while in his third year at the university Pavlov became an active collaborator of Tsyon, and definitely took up physiology as his major subject.

His first scientific investigation, made with Afanasiev, concerned the pancreatic nerves, and for this he was awarded the gold medal at the Academy. "The clinical subjects did not attract him and he even failed at one of the examinations (internal medicine). But the advances of surgery at that time made the greatest impression on him. His interest for chemistry was not great, and he gave most of his attention to the neural control and the nerve connections of the organism" (Savitch).

His university life was spent quietly, his time being divided between the studies, including laboratory work and a zealous application to scientific literature, and recreation. His chief diversions were sports, belletristic literature, and association with his brothers and a narrow circle of friends.

In 1875, being ready to pass from the University to the medical academy, Pavlov accepted the offer of an assistantship to Tsyon. A characteristic incident now took place. Tsyon unexpectedly went to Paris, and though his successor invited the young Pavlov to remain with him, he defiantly refused—even though he was faced with the necessity of finding something to live on—because the new professor had recently endorsed "the transgressions of a man simply because he had an important post and influence, without any regard for the truth." Then, as in later life, Pavlov would not sacrifice his convictions for the sake of worldly goods, in spite of the fact that he was in great need.

In December, 1879, after finishing the course at the Military Medical Academy, he passed the state examination and became an approved physician. He received a fellowship, awarded on the basis of merit, enabling him to spend two more years in research at the Academy. In 1883 he completed his dissertation for the degree of Doctor of Medicine.

Most graduates, on entering their profession, begin to plan their careers, to ask themselves Whither? and Why?

Such questions never troubled Pavlov. In his scientific investigation he had discovered the joy of bringing to light some hidden truth so that it was solved beyond the shadow of a doubt, and he applied himself to this task with an unwonted enthusiasm. In this he was like Sir William Osler, who said that it was one of his cardinal principles to do the immediate work of the day without thought for the morrow. His whole life was to be dedicated to this work of revealing ever new facts; his entire energy was devoted to science. Practical questions, such as securing a position, pecuniary difficulties, etc., did not exist for him.

He and his brother continued living together in cramped quarters, poverty-stricken, though happy and cheerful, with a small circle of friends.

In 1880 he made the acquaintance of a young and attractive student of pedagogy, Serafima Karchevokaya, and was married to her a year later in-Ekaterinoslav. Here a comical incident occurred. Savitch tells us Pavlov had no money, and the bride's sister had to forward the necessary means without which the young couple could not leave the town. This again shows how little Pavlov cared about the needs of every-day life.

In Petersburg they continued living in the small apartment with Pavlov's brother. Both of them had to work to earn enough to live on. It is fortunate for science that he found a woman so loving, kind, and admirably fitted to make him a happy home. It is perhaps due to her that he has always spent his entire evenings in the quiet recesses of his home, in this fortress protected from the routine world of cares—serene, as comfortable as possible with limited means, and contented. The practical affairs of life he has always preferred to leave to others, and he has rarely even travelled alone. How few of the ordinary duties Pavlov has had to attend to is shown by a remark of his wife in 1927—that he had never so much as bought a pair of shoes for himself; only in the difficult years following the war and revolution did Pavlov undertake any of the home chores.

Having received the Wylie fellowship, Pavlov spent the years 1884-86 with two of the greatest physiologists of that time—Ludwig in Leipzig and Heidenhain in Breslau. Strict economy was necessary, but for him financial questions hardly rose to the threshold of consciousness.

On returning to St. Petersburg, he received several appointments as assistant, one of which was with the noted clinician S. P. Botkin, who made it a practice to control his therapeutics by experimental pharmacology. In this laboratory Pavlov performed his famous experiments with the cardiac nerves and his first great research upon the nerves of the digestive glands.

In 1888 he discovered the secretory nerves of the pancreas, which, because of the difficulties in repeating the experiments, were recognised universally only twenty years later. The next year he published together with Simanovsky the renowned experiments with sham feeding.

At the same time with such progress in science he was rather unsuccessful in his professional career. He bore this failure easily, though his financial affairs were in a sad plight. "His attention was so much taken up by the study of the chrysalid's transformation into a butterfly that he quite forgot his misfortunes" (Savitch).

With the growth of his family it became necessary to look for a more lucrative place, and Pavlov applied for the chair of pharmacology in Tomsk.

He never accepted this, however, as in 1890 he was elected, on a ballot of seventeen to five, Professor of Pharmacology in the Military Medical Academy of St. Petersburg.

Soon after Pavlov's election to this position, he came into open collision with the rector, Pashootin, a "despotic man to whom most of the professors were obsequious." Pavlov used to oppose him in the most decided manner, though most of the faculty were on the side of the rector. Pavlov was punished for his unsubmissiveness; for his appointment to the professorship of physiology in 1895 was not confirmed till 1897. Both at that time and afterward he has constantly upbraided his countrymen for their servility. From 1895 he held the chair of professor of physiology continuously until his resignation in 1924.

In 1891 the first (in the world) surgical department of a physiological laboratory was constructed according to the plans of Prof. Pavlov in the new Institute of Experimental Medicine founded in that year by the Prince of Oldenburg. It was here that Pavlov first had the opportunity to carry on the so-called chronic experiments, described in Part IV of this biography, as prior to this there were no provisions made for the care of the animals.

In 1904 Pavlov was awarded the Nobel Prize for his researches on the activity of the digestive glands, and in 1906 he was elected a member of the Russian Academy of Sciences. His work at that time was conducted in the three above mentioned laboratories.

His fame abroad, however, as is often the case, created a certain amount of envy at home, where he had not a few enemies. After he had received the Nobel Prize, the attacks on his researches with the digestive glands ceased. But even more vituperative were the criticisms of the work with conditioned reflexes. It was stated "this is not science; every dog trainer knew it long ago." The irritation against him grew to such a point that the conference of the Military Medical Academy even disapproved of the dissertations from his laboratory. Shortly afterwards his enemies became so active that they prevented his becoming president of the Society of Russian Physicians of which he had been vice-president. This was chiefly because his laboratory produced many more papers than any of the others.

III

Now we may consider some of the characteristics of Pavlov as an investigator.

Prominent among these is one which has guided and governed his whole life—the fervent desire to obtain knowledge, and the energy and singleness of purpose with which he has always championed scientific truth. As the noted physiologist, Robert Tigerstedt, said of him in 1904, "Pavlov's life can be summed up in this: an untiring search for truth has led him to the attainment of scientific facts of the first order." The element of personal welfare as a motive for action has always been absolutely foreign to his nature. And this was not because fortune placed him in such a position that he could spurn the ordinary practicalities of life. Hard necessity and bitter need, on the contrary, were his companions from birth. But his scientific investigation never slackened; his first thought was ever in his work.

In the beginning of his laboratory experience, he had no means of properly caring for the dogs, so he would take an animal home with him to his small lodging where he nursed it after an operation. And some of this time, due

FIGS. 1-2-3: PROFESSOR PAVLOV OUTDOORS AND AT PLAY

to lack of money, he could not afford independent quarters, but had to live with his friend, N. P. Simanovsky, removed from his devoted family.

Not only has Pavlov diligently sought truth, but the question is for him the ruling passion of his life. Not in the vague philosophical way of a Diogenes wandering the daylit streets with his lantern, but through unswerving and passionate attention to all the concrete details of laboratory research. With an unwavering conviction that truth and science are the only worthwhile goals in life, Pavlov has always promptly rejected all compromises, all considerations which seem to him dishonourable, and he has zealously thrown his whole being on the side of truth and right.

Along with his spiritual gifts, the love of the truth, and the ability to forget everything for his work, we find in Pavlov still other traits which mark him as a capable investigator.

His rare memory is one of the most extraordinary of these. The smallest details in the order of an experiment or in its results, even though it had been performed more than a decade previously, can be recalled by Pavlov at will. Former assistants or pupils, on returning to the laboratory for a visit, have often been astonished when the master in discussing some physiological problem would suddenly mention accurately and in detail all the figures of an experiment they themselves had performed years ago and long since forgotten, or would name the dog with which they had worked, and this in spite of the fact that during the interval hundreds of animals had passed before his eyes. Once having seen the protocols of a collaborator, he has never forgotten them, although before the war he personally directed the work of as many as thirty investigators at one time, and recently even more.

With the passing of the three quarter century mark, Pavlov has resorted at times to the use of a notebook, and he remarks that his memory is not so good as formerly. But even at this it would be the envy of any one of half his years. It may be stated almost as axiomatic that, "once having seen a figure, he never forgets it."

A vivid example of this came to our notice in 1925. Pavlov was curious to know the significance of a medal one of his collaborators was wearing. When told it was given for a race, he being interested in athletics of all sorts, examined it more closely. A few months later when the subject of races came up again the medal was referred to, and some one asked how long it took to run this distance. The owner of the medal had himself forgotten the exact time, though he had been wearing it for fifteen years. Pavlov, who had seen it only once, however, immediately remembered it to the fifth of a second—and answered, "10:11⅘."

We have this evidence from Dr. Thomas R. Elliott of the University of London (written May 14, 1928): "I saw Prof. Pavlov at the Croonian Lecture and also at a dinner of the Royal Society. He was in splendid form and his energy and charming personality delighted every one. He astonished me by remembering my face and the exact occasion on which he had met me in England nearly twenty years ago."

Probably Pavlov's memory is based in some way upon his power to concentrate, and also to select. But it is just as typical of him, that he does not clutter his mind with a mass of trivial, ordinary details, such as are carried in the newspapers, popular journals, etc. For Pavlov rarely ever reads or attends to these, and he has a remarkable power of apparently keeping out of his field of consciousness events and data which have no relevant bearing on his direct interests, nor will he even venture opinions on any subject with which he is not thoroughly familiar nor for which he does not possess an ample number of facts. This ability to look only at bare facts, to judge solely on the basis of facts as well as at once to divine the relations between them, are characteristics paramount in Pavlov.

This brings us to another quality which he possesses in a high degree—the ability to analyse, to penetrate immediately beyond the veil of trivialities into the essential, and to bring order to a series of seemingly chaotic events. To do this he often boldly plans new experiments, perhaps in varying form, and spares no energy to execute them. Pavlov says that the most difficult part of the work of an investigator is the assigning of problems to collaborators.

By play and sport Pavlov developed great physical agility and suppleness. One sees these same qualities in his skill at operating; for his dexterity in this direction was extraordinary (he has not operated regularly for the past five years on account of the demands of his other work and the less acute vision of his advanced age, although as late as March, 1928, he performed a new operation on the pancreas). He is ambidextrous, operating equally well with either hand. The late Tigerstedt said of him: "Pavlov does a simple operation so rapidly that it is finished while the onlooker thinks it has only begun" (*Archives des sciences biologiques*, vol. xi, 1904). To say his movements are lightning-like is apt analogy; for they are made so quickly that one has to look carefully to discern them. Severing of the spinal cord from the brain in the dog is carried out by Pavlov in about 30 seconds; and to find, tie, and divide either the vagus or the sciatic nerve takes him only three to five seconds from the time the skin incision is commenced. As a matter of comparison to those not familiar with the operation, it might be mentioned that the fastest that this can be done by any of his assistants is 90 seconds, and this is not by any means slow.

Instead of being prejudiced in favour of the results of his own experiments, the reverse is true of Pavlov—*viz.,* he has the ability not only never to overestimate his results, but to look at them impartially or even hostilely. A theory when it does not correspond to new facts is ruthlessly rejected, and he begins to plan another structure on the basis of these new findings; for facts are the fundamentals upon which Pavlov constructs every theory, the guide-posts which constantly direct him to new generalisations and conceptions. Never does he attempt to force a fact into a theory when there is a misfit. In this he is of a like mind with the late Sir William Bayliss, who said,

> There must never be the least hesitation in giving up a position the moment it is found to be untenable. It is not going too far to say that the greatness of a scientific investigator does not rest on the fact of his having never made a mistake, but rather on his readiness to admit that he has done so, whenever the contrary evidence is cogent enough.

The following incident, related by Savitch, illustrates this:

> It was I whom Ivan Petrovitch asked to verify the results obtained by Bayliss and Starling with secretine. We started the experiment in his presence and it wholly confirmed their opinion. Ivan Petrovitch stood silently for a time, then went to his study, returned in half an hour and said: "Of course, they are right. We cannot aspire to the monopoly of discovering new facts." That question was definitely settled; facts and results always decide any question for him. He considered theories as worth while only for finding new ways and accumulating facts. However, the action of the secretory nerves on the pancreatic gland was denied abroad

until these experiments were demonstrated in England by one of Ivan Petrovitch's collaborators (Anrep).

Another striking example occurred with the heredity of conditioned reflexes in mice. Pavlov stated in 1923 from his preliminary experiments that he believed he had obtained inheritance of conditioned reflexes in these animals, and that many facts of every-day life agree with this view. More refined methods undertaken in 1925, have failed to confirm the first trials, and Pavlov, instead of defending his former hypothesis and experiments which had become widely published, at once repudiated them, and has absolutely withheld any further opinions till the facts will have been proved beyond doubt.

As a result of his disagreement with Pashootin, Pavlov had no permanent collaborators till the beginning of the twentieth century, and all who worked with him were deprived of the privilege of going abroad. His early co-workers were general practitioners entirely unprepared for physiological investigation.

In reviewing Pavlov's work in the laboratory, Savitch tells us:

> He was the soul of the laboratory, and where he was the work went on with the greatest intensity. He attended to the smallest details, and often used to count the millimetres of the Mett tubes of his collaborators. He was very punctual and always kept appointments; he was not very exact in other things; he answered letters tardily if at all.
>
> As a teacher in the laboratory, his individuality was marked. Being animated and sociable, he inspired his collaborators with the keenest interest for investigation. He is romantic, as Ostwald might call him. His impulsiveness makes him explosive at times during discussions. He easily flares up at trivialities; the elements of activity and of fighting are too deeply rooted in him.

Pavlov has modified the method of laboratory control with the different periods of his life. At first he gave all his attention to one or two investigations, and spent nearly the whole time with those collaborators. Then more time was given to the others as their work would progress. We might call it the principle of concentration. Since the renewal of laboratory work in 1919 Pavlov commenced to direct the experimentation in another way: We may compare him to a chess player who moves many pieces with a concerted plan; facts obtained from one collaborator's researches go to advance that of another. Just now conditioned reflexes attract most attention in his laboratory, especially among the younger collaborators. This teaching is creating a novel conception of the psychical life of man, and it will sooner or later be the foundation of a new philosophy.

"Pavlov intuitively grasps all the relationships in an experiment. When a new fact is observed it is repeated; then begins a period of doubt, criticism and verification and numbers of theories are considered and rejected" (Savitch).

IV

And now we can pass to the work itself.

Three territories of physiology have successively demanded Pavlov's attention. He began his independent investigations with experiments on the circulatory system, and these were performed chiefly between 1878 and 1888. But when Pavlov became professor of pharmacology, his interest gradually turned toward the digestive glands, and finally his whole laboratory was devoted exclusively to this subject. Since 1902, however, Pavlov's energies have taken still another direction, and work on the digestive glands has receded into the background. Now his whole attention is devoted to the investigation of the processes of the central nervous system by the method of conditioned reflexes.

Pavlov's work concerning the circulation of the blood falls into two groups. One of these has to do with the regulation of the blood pressure. During these experiments Pavlov desired to have as nearly normal conditions as possible. In the usual procedure, a skin incision is made, the blood vessel is found, and a glass tube inserted—an undertaking requiring tying of the dog. Pavlov, however, so skilfully accustomed the animal to the operation that he would voluntarily jump upon the table, and permit the procedure without being bound. Pavlov's operative technic was so quick and accurate that there was barely any pain. Thus it was that Pavlov was able to obtain on this dog certain new and exact facts concerning the normal blood pressure and its daily oscillations. Further, by the use of drugs and by cutting various nerves, he arrived at the laws governing the regulation of the blood pressure.

Another series of studies on the circulatory system concerned the regulation of cardiac activity. These experiments required great perseverance. In order to immobilise the experimental animal, poisoning through narcotics had to be avoided; and the pain reflexes were eliminated by severing the spinal cord. The animal was then rendered immobile. The chest cavity was opened and the separate nerve filaments from the cardiac plexus to the heart dissected out. Great difficulties are encountered here on account of the fineness of the work, and these are heightened by the fact that there is considerable variation in the anatomical course of the nerves, with the result that nerves which are identical in their position may be very different in their functions. In every case the activity of each nerve had to be tested. By assiduously doing this in a large number of experiments Pavlov was able to correlate the anatomical positions with the functions.

As a result of this exhaustive work, Pavlov came upon an important discovery which was also arrived at independently by Gaskell. He found that certain nerve fibres had a special effect on the heart muscle, increasing or decreasing the strength of the beat, but not affecting the rhythm. After stimulation of these nerves in a fatigued heart, the beat is stronger and the amount of work performed greater.

On completing this, Pavlov turned his energies again to the study of the digestive glands. His investigations were conducted along two lines: the analysis of the mechanism through which the glands are innervated and regulated; and secondly, the study of the part played by these glands during the normal conditions of life.

Pavlov had previously shown that the process of operating on the animal has a marked inhibitory action on certain glandular activity. Owing to this inhibition, negative results were often obtained in acute operative experiments. From this new point of view Pavlov now performed all his operations. He attempted to exclude every central as well as peripheral inhibitory process, partly by using combinations of various experiments, and partly by speed and accuracy of the operation. Thus it came about that from nerves which his predecessors had found to be without action, he was able to elicit a secretory effect.

The experimental proof of secretory nerves to the stomach and pancreas was a great scientific feat at that time, and it placed Pavlov in the front rank of experimenters. As proof of the extreme difficulty of solving this problem of the activity of the pancreas, we quote the great Breslau experimenter, Heidenhain: "During the course of three years I have only too often experienced the numerous perplexities which the pancreas more than any other gland of the digestive system presents to the would-be investigator. How many times I have seen the matter conclude not in any definite answer, but with a series of new questions!"

But Pavlov found the answer. He had demonstrated that the older experimental methods were unsuitable for work with the circulatory system and digestive glands. Therefore he now discarded the acute experiment, and decided to observe the activity of the glands under normal conditions of life. At that time there existed not even the barest suggestion of any appropriate method of investigation, and Pavlov had to discover one.

That all physiological processes are, to a certain degree, distorted by operative interference, as has been stated, is a fundamental idea of Pavlov's. He says:

> We can not calmly consent to the rough breaking of this mechanism, the hidden secrets of which have occupied our thoughts for long years, perhaps for life. If the mechanic often refuses to alter or to interfere with some delicate machine on the ground that it would be a pity to spoil so fine a mechanism; if the artist reverently fears to touch his brush to the production of some great master—then should not the physiologist, who deals with the finest mechanism of all, the supreme creation of nature, have this same feeling?

The above mentioned experiments on blood pressure were performed under normal conditions insofar that the experimental subject had been thoroughly accustomed to the whole procedure. Even in these first experiments one can see an indication of the direction which his later work was to take.

In the post-operative care of such higher animals as the dog and the cat Pavlov employed the same precautions as were used in human surgery—*viz.*, anæsthesia, asepsis, narcosis, etc. After operation, the dogs are put into clean and well-heated rooms, and nursed with much the same attention as is used with patients. He has always emphasised the importance of having healthy and well-cared-for animals. Special operative rooms and a dog clinic were designed by him for this purpose.[2] This new type of laboratory, first constructed by Pavlov nearly a third of a century ago, is now essential to every good physiological institute.

That Pavlov has been humane with his animals is shown further by his following discussion of vivisection:

> As is known, vivisection has excited in many countries of Europe an energetic objection from the uninitiated. These protests have at times seriously interfered with the progress of biological investigation (for example in England). But what is the basis for this? The feeling of pity. . . . But the experimenter also has this feeling. I. M. Setchenov, the father of Russian physiology, never performed experiments on warm-blooded animals.
>
> In spite of our measures inspired by the feelings of mercy and pity, there are, nevertheless, painful and violent deaths of animals. Is there justification for this? The human mind has no other means of becoming acquainted with the laws of the organic world except by experiments and observations on living animals. . . . If we continue to permit the hunting of animals, *i.e.*, their suffering and death for our own recreation; if we slaughter them for our food; if we consent to the torture and killing of thousands of even our fellow-men in wars: then how can we object to

2 See I. P. Pavlov: *Work of the Digestive Glands*, chapter i, Griffin and Co.

the sacrifice of a few animals on the altar of the supreme aspiration of man for knowledge in the service of a high ideal, the ideal of attaining to truth! [3]

The general principle which Pavlov employed in his study of the digestive glands was the following: an operation is made on the organ to be investigated so that its juice, which normally flows into the gastro-intestinal canal, is conducted to the outside of the body, where it can be collected in pure form. Special care must be taken not to injure the nerves. During the operation, the isolated part of the organ or the duct of the gland is transfixed and permanently attached to the skin so that after healing of the wound no further procedure is necessary, except to prevent loss of the juice on the skin and erosion therefrom. When one desires to collect the juice for experimentation he has only to insert a cannula into the transfixed duct and connect it with a graduated receptacle. The animals become so well accustomed to the experiments that, as a rule, they will remain standing in the supporting harness for seven or eight hours without even complaining.

This method of making a permanent opening into the intestinal canal has received the name of the "Method of the chronic fistula." But as the procedure can be applied to other organs as well as to those of the digestive system for the purpose of studying physiological functions while the animal is living under normal conditions, it is generally termed the "chronic experiment." This has now to a large extent replaced the older "acute experiment" of the vivisectionist. The operative method, the post-operative care of the animals, and the principles of observation in the chronic experiment have been for the main part worked out by Pavlov. By severing certain nerves much information has been obtained with this method concerning the innervation of the glands of the digestive system. We may even say that in Pavlov's hands the method has yielded the basic results upon which most of our knowledge of the normal functions of the digestive apparatus is built up.

However, other and more far-reaching results sprang out of the work with the chronic experiments. It had been observed that the activity of the digestive glands was called out not only when the food was in the mouth or had passed further along the digestive tube, but by agents acting from a distance, such as the sight, odour, etc., of the foodstuff. During the chronic experiments it was seen that in addition to the properties of the food *per se* (sight, odour, etc.), accidentally associated stimuli, coinciding normally with the beginning of the feeding, such as the dish in which it was presented, or the footstep of the person who brought it, were also able to evoke identically the same reaction as the food itself.

The intrusion of these facts into the normal course of the experiments demanded Pavlov's special attention, and led him to the investigation of what was then known as psychical reactions. The salivary glands presented certain advantages for the study of the actions of agents at a distance ("psychical reactions"), as described in chapters i, ii, and iii.

It was very difficult in dealing with such complicated reactions, which had up until this time been considered as mental activities, to avoid the use of psychical conceptions, and in the first chapters of this book Pavlov often refers to "psychical" activity, but he usually adds the word "so-called" psychical. To circumvent the employment of psychological terms in his physiological researches, Pavlov substitutes "disappearance of the reflex," "restoration of

[3] Vivisection,' *Encyclopedia of Medical Science*, St. Petersburg, 1893 (Russian).

the reflex," etc. Further, all anthropomorphic and psychological conceptions were avoided by considering and measuring the secretory component in preference to the movement reaction of the animal.

As soon as Pavlov had persuaded himself to look at the reactions in this new light, as objective and physiological occurrences in the organism, there was no difficulty for him to study them according to a physiological plan. The reactions of the salivary glands to food in the mouth had for a long time been classed as a physiological reflex. It remained now to show that the new reactions did not arise spontaneously, but always had as their cause the stimulation of special receptor organs, as the eye, ear, nose, etc.

In the first part of chapter iv Pavlov explains why these new reactions must be considered as reflexes. To distinguish them from what had already been known in physiology as reflexes, the new reactions were called *conditioned* (acquired) reflexes and the old ones *unconditioned* (inherited), as it was seen that the chief difference consisted in the large number of circumstances to which the conditioned reflexes were subject. (Compare Pavlov's statement in chapter iv.)

Pavlov's interpretation of the new phenomena can be briefly characterised as follows. The old conception of a reflex as the answering reaction to certain stimuli, an inborn relation conditioned by the function of the structure of the given nervous system—this conception was extended by Pavlov to "psychical" reactions. He postulated the existence in the highest parts of the central nervous system of a special functional property, the property to form new connections between certain agents and the responding activities of the organism. Thus a purely physiological explanation can be given to such a fact, for example: a sound stimulus which in the given animal called out no special response, by coincidence with some other agent evoking a specific reaction, finally comes to produce the same reaction as the original effective agent.

When the chief importance of the brain process is considered as the ability to form new functional reflex paths, it is immaterial for the experiment whether these paths are formed between the receptor body surfaces and the motor activity or between the receptor body surfaces and the secretory reaction of the experimental object. The process of formation is the same in each case. The reasons why Pavlov selected the latter reaction are mentioned in many places of this book. (See chapters x, xiii, paragraph 3.)

The analysis of the whole procedure led Pavlov to the belief that the accidental properties of a food substance (sight, odour, etc.) become the stimulators of the salivary secretion because they have since birth constantly coincided in time with the activity of the salivary glands (from the stimulation of the mouth cavity by the food). (See chapters ii and iii.)

In order to prove this hypothesis, experiments had to be carried out in which various agents of the external world are made to coincide with stimulations of the mouth cavity which produce a salivary secretion. The results of these experiments are clearly explained in chapter iii toward the end, in chapter iv, and also in chapter x.

The further physiological questions as to which parts of the central nervous system participate in the formation and function of the conditioned reflexes is taken up in chapters xv and xvii. Chapters v, vi, and ix also contain some additional material on this point.

Ensuing experiments showed that there is a special spreading or wandering of the nerve processes—of inhibition as well as of excitation—in the brain, and this led to the laws of *concentration* and of *irradiation*. (See Index.)

The process of *sleep*, which in the early work often disturbed the course of the experiments, was eventually shown to have many properties in common with the process of inhibition, and in fact to be a kind of flooding of the brain by inhibition. (See chapter xxxii.)

Induction, which had already been known in the lower parts of the central nervous system, was shown to play a part also in the higher brain processes. (See Index.)

And presently the cerebral hemispheres appear as a nerve tissue in whose mass occur the delicate equilibrations between the processes of excitation and inhibition which form the functional substratum conditioning the reactivity of the highest parts of the central nervous system. (Chapter xxi; see also Index under equilibration.) Finally, in the concluding three chapters of the book, there is material showing how this normal activity can be modified by strain or functional destruction of parts of the cortex, and also concerning the various types of nervous systems, temperaments and neuroses.

Thus from the observation of such a simple fact as "the mouth waters" at the sight of food, there developed in Pavlov's mind an entirely new teaching of the functions of the highest parts of the central nervous system, a teaching which even at the present can explain physiologically, be it only schematically and partially, many riddles of human activity. (Refer to chapters xxvii, xxviii, xxix, xxxv, xl.) Does not this represent an important extension of physiology and a forward march of science?

And with the conclusion of chapter xli Pavlov's work is not yet ended. It goes forward unhaltingly and every step reveals important new facts of the higher nervous activity which are brought into strict physiological alignment.

The early years of the work with conditioned reflexes were fraught with doubt, discouragement and criticism, not only from Pavlov's enemies but from his friends. Sir Charles Sherrington, the great British physiologist, told Pavlov in 1912 that the teaching of conditioned reflexes would not be popular in England because it would be considered too materialistic, and his close friend, the late Robert Tigerstedt, advised him to "drop that fad and return to real physiology."

What of the future of conditioned reflexes? Pavlov says it has already made important contributions to the nature of sleep, neuroses, and temperament. He has the strongest conviction that this method will bring untold happiness to the human race by enabling the individual to understand himself, and that when we know the laws governing the higher nervous activity we shall have freedom of the will and control over our actions just as we have acquired power over nature with the solution of her secrets. He says, "the study of the dog has helped me not only to understand myself but also others."

But he thinks the laws of the higher nervous phenomena must first be worked out on the simpler nervous systems of animals, and that the application to the human can be made only slowly and cautiously.

One may make his own decision as to whether Pavlov is a materialist; many say that he is. Political leaders in Russia have drawn upon his teaching for their materialistic point of view. But he himself claims that he is not. He explains that the teaching of conditioned reflexes is science, that it has nothing to do with materialism. As long as the dualistic theory is accepted and mind and matter are considered separate entities, it is difficult to reconcile the new facts with this belief; but according to Pavlov, "We are now coming to think of the mind, the soul, and matter as all one, and with this view there will be no necessity for a choice between them." (Personal conversation, April, 1928.) The dualistic theory in his opinion has kept physiologists from working with the higher nervous phenomena.

V

It will doubtless now be of interest to the reader of these lectures to know something of the personal habits of so successful an investigator.

One of Pavlov's most striking traits is his systematic regularity in all the details of life. He goes to work and leaves with military punctuality, and it is extremely rare that he is a minute behind in arriving, or more than a few late in leaving.

The following incident is related by one of Pavlov's assistants:

> During the revolution it was very difficult to get to the laboratory at all, because besides other things there was often shooting and fighting on the streets. However, Pavlov was generally present, even though nobody else was. One of those days when I was about ten minutes late for an experiment, I found Pavlov already there punctually, though no one else had come. Seeing that I was not on time, he immediately lit into me with his customary vivaciousness. "Why are you late, Sir?" I asked him if he did not know there was a revolution going on outside. "What difference does a revolution make when you have work in the laboratory to do!"

Formerly he came to work at nine o'clock and left at six, taking one-half hour for lunch; but having passed the three score years and ten, he has reduced the working day by two hours from 10 A.M. to 5 P.M. Before the revolution he also spent holidays in the laboratory, even New Year and Christmas. He has said that he never missed a day from September first to June first. "On Sundays I remained only till three o'clock, and afterwards went for a walk somewhere. It was quieter then, and I could work and observe the animals undisturbed." He does not advocate work every day in the week for others though, and never required his assistants to be present on Sundays or holidays.

But he is just as regular in his habits of rest. It is almost never that he remains in the laboratory after the accustomed time for him to leave; the stroke of the clock is for him like the factory whistle to an employé. After dinner he plays one or two hands of solitaire, no more, and then lies down from seven to nine. It is an invariable rule with him not to see any one at this time, and the telephone is always cut off. From nine until ten or eleven in the evening he has tea and converses animatedly with his family or visitors. From this hour till about one or two o'clock he reads and studies or writes. Part of the evening is given to calisthenics or gymnastics.

He has always taken two or three full months of rest during the summer. Until the war he had a country house in Esthonia, but since the loss of that he has gone to Finland or elsewhere. When vacation comes every physiological journal and medical book is banned, and he religiously avoids routine mental work, although he includes in his reading poetry and his three chief poets are Shakespeare, Goethe, and Pushkin. Most of his vacation time is taken up in muscular exercise, either gardening or such sports as cycling, and *gorodkee.* He continued to ride a bicycle until only a few years ago. We last saw him playing *gorodkee* in the summer of 1926 when in his seventy-sixth year (illness prevented this in 1927), and he entered into the sport with all the glee of a youngster at baseball or cricket. His muscular energy is remarkable, for at the age of seventy-six he not only outplayed men of one-half his years, but after three days of continued exercise for eight hours daily at this strenuous game, he alone had enough reserve left to want to resume the play on the fourth day.

Pavlov considers physical work and exercise the best recreation from mental activity, and believes there is just as much benefit in ordinary agricultural labour for this purpose as in sport.

During the summers he spent much time in raising flowers, his favourites being stock-gilliflowers. He used to go to his country place in May to prepare the beds for them, and would then work so hard that he could not sleep for exhaustion. Thus Pavlov always had the greatest interest in all that surrounded him: he delighted in everything: in a good book, a flower, a butterfly, a game of *gorodkee.* He therefore has retained his mental and physical buoyancy notwithstanding his age. Darwin, on the other hand, became an invalid very early, and his life was kept up by the care of his family.

While resting every summer in the country from his laboratory work, Pavlov began to collect first butterflies, and later stamps, and at last paintings. At first he used to say that he collected the butterflies for his son, which was in reality only an excuse. He was accustomed to be constantly amassing facts in the laboratory so that he must needs be interested in always gathering and collecting, even though in the summer it took new forms. Here again he went about it passionately. While stealthily approaching a butterfly which he wanted very much, he would whisper kind words that it might not fly away.

Doing everything with a will he led, by example, all his collaborators to take to work and sport in the same way. The Physician's Gymnastic Society owed its long existence to this ability of his to lead others by his example. The Society lasted until 1914 when the war deprived it of most of the members. Being very animated and energetic, Ivan Petrovitch had the capacity of inspiring the most apathetic characters with energy and interest for their work (Savitch).

Pavlov says that he attributes his long and healthy life to three things mainly: heredity, regularity of habits (proper periods of work, rest, exercise, etc.), and temperance (abstinence from alcohol, smoking, etc.).

Pavlov's highly developed sense of time—the reflex of periodicity—is probably at the basis of his self-control, and because he has control over himself he can lead others. Rarely does he permit his interest or energy to violate the law of periodicity. He is never late for an appointment, and no matter how much a thing interests him he does not allow it to upset his routine.

Other traits which Pavlov possesses to a remarkable degree are impetuosity, uncompromising honesty, straightforwardness, and insuperable energy. Nearly all of his life has been spent between the laboratory and his home, and he would probably never have travelled further from either than to a convenient place to play *gorodkee* in the summer, had not medical meetings called him abroad (he has never even beheld his native and beloved Volga, the Mecca of Russians), so that we might expect to see a person who is at least stiff and formal if not flat and insipid; but the rare combination of the above-mentioned qualities existing in such a marked degree has produced a man irresistibly lively and interesting. It is hard to conceive of one who could be as excitable, as subject to strong emotion, and as absolutely carried away by the vigour of his feelings, and yet at the same time retain perfect control over himself so that every emotion is subjugated to his will just as savage Bengal tigers may be to their trainer. To watch Pavlov's face is a study in itself—fascinating, like peering into the rapids of some Niagara. One is both amazed and captivated by his vigour, animation, variety of expression. At times his countenance is placid and smiling like that of a benevolent Santa Claus: on other occasions, when some subject is under discussion on which he has decided and vehement opinions, we have seen it assume a fierceness and his whole being a tensity that awed into silence and nothingness all those about him. No actor could better portray the various emotions than he. With Pavlov, these emotions are nearly always those of the positive, active type—almost never negative and dejected. We have not heard of a case in which Pavlov's excessive energy and strong feeling caused him to commit any grievous error which he later recognised as such, although there have been instances where his impetuousness and hot-headedness in trivialities carried him a little beyond what would have been his limit under less exciting circumstances.

When disturbed he can swear energetically, though the strongest terms he is known to use are, "the devil take it!" and "to Hell with it!" and these are favourite and every-day expressions with him and freely interspersed in both lectures and conversation, in the laboratory and at home. On an occasion when the constant repetition of these phrases, uttered vociferously, scoldingly, and with apparent feeling, during an operation, caused an assistant to become somewhat discouraged and disgruntled, Pavlov seeing this afterwards, told him not to mind what he said any more than the dog's barking outside. Babkin related how Pavlov once in a state of excitement whipped an unruly dog with a towel, and then shortly afterwards penitently came to Babkin to admonish him never to beat the animals as it would spoil the experiment.

Another assistant he often referred to in the laboratory as "no better than a cobbler," though at other times as "his beloved collaborator." It has also been mentioned above that Pavlov often has been irreconcilably at variance with certain colleagues, but in these cases, Pavlov's action seems to have been based more on honest principle than on personal prejudice, and in many instances these antipathies were forgotten after a while.

The longer one knows Pavlov, the more one becomes impressed with his simplicity. Not only in his habits and routine but even more in his thinking. There are no curves in his mind, no compromising, no beating about the bush; all his thoughts move in straight lines. Like thoughts, like deeds; for Pavlov, though often harsh and bitter in his attacks, never hits below the belt; his blows though straight from the shoulder and at times savage, are not foul. He is free from all intrigue and backbiting; his honesty is transparent. Examples are numerous, such as the following one, which though seemingly trivial is important for this very triviality. Pavlov not long ago was asked to carry a certain letter with him abroad to avoid the censorship of the mails. However, he refused to violate the law even in so simple a matter. Besides Pavlov, we are not acquainted with any who we believe would be so scrupulous.

And with all his greatness he is unassuming and entirely lacking in ostentation or any show of arrogance, and extremely democratic in his actions. He has been known to rebuke severely a laboratory *Diener* who, when asking for a raise in salary, addressed him as "Barin" [4] (personal communication from

[4] *Barin* is an old term of respect formerly used by servants, etc., in Russia to their superiors, and somewhat equivalent to the employment of the word "Master" by the Negroes in some parts of the South. It is not the same as the title "Baron."

Babkin), though Pavlov himself always uses the term "Gentlemen," and "Sirs" (Gospoda) to his students and collaborators in preference to the modern one of "Citizen" or "Comrade."

With strangers he is warmly cordial and hospitable if they are interested in his work, but frankly brusque to those who are evidently more concerned with him than with his researches, regardless of their rank or station. In fact, we have often seen him particularly unattentive to officials who might expect or could give favours owing to their position. Anything that smacks of pure extolment or panegyric he usually condemns or neglects, though at times tolerates.

As the reader of these lectures can discern for himself, Pavlov is an ardent investigator whose prodigious energy has been dedicated to facts and objectivity. Although it is upon these that he lays the chief values, he is not by any means lacking in an appreciation of art, music, religion, etc. In a burst of enthusiasm we have heard him exclaim, facetiously perhaps, that he would gladly exchange his accomplishments for a voice like Chaliapin's, and his love of pictures is borne witness to by the rare paintings with which he has surrounded himself.

During the utter dissolution of life in Russia following the Great War, Pavlov like other of his countrymen suffered "the slings and arrows of outrageous fortune." Besides the loss of two sons, he was at one time in very difficult material circumstances, having to subsist on a ration of black bread and half rotten potatoes, and in lodgings so cold that some days he had to remain in bed to keep warm. The vegetables he raised in his own garden helped to furnish his table. More intolerable for him than these discomforts was the impossibility of carrying on successful research in the laboratory, on account of the death of animals from starvation, the absence of light and heat, etc. "He was greatly depressed by the Russian Revolution, especially because he thought that the devastation of economic life would hamper science for a long time, and of his conviction that only science can lead the human race to a bright future without war, revolution or catastrophes" (Savitch). These overwhelming events, however, did not crush Pavlov nor break his spirit, and he said to us in 1927, with much defiance and a certain fierceness, that he had survived and could view these misfortunes not from beneath, as most of his countrymen, but from above.

There is one matter which cannot be conscientiously neglected in spite of its controversial nature. And that is the stand which Pavlov has always taken in regard to politics, and particularly since the Revolution. Pavlov did not by any means approve of the old Tsarist government, although it seems that he did not give quite the same vent to his feelings then as now. But he is recorded as saying that he is not sure but that no government at all would not have been better than the Tsarist. Pavlov is a Russian patriot. As he said in one of his lectures, "Gentlemen, I do not know what you have become, but I was, am, and will remain a Russian citizen, a son of my native country!" At the present time, Pavlov is even more vehement in pointing out, decrying, and protesting against politics and political errors. Here, as in everything about which he has an opinion, he states it boldly and even fiercely. We have heard him shout out in the laboratory with all the ire and righteous indignation of his nature terms of vituperation toward the government that not only in Russia but in any country, especially under revolutionary conditions, might have been suppressed as treason. His recent public speeches abound in phrases that Theodore Roosevelt would have hesitated to employ against his worst political enemies. Yet these invectives have usually been based on those facts which Pavlov has seen, and on principle and not mere expressions of hatred. No personal elements enter into the question. He expresses himself with the same force now when he is receiving all that he needs and wants for his work as during the years when, due to impoverished conditions, he got nothing. He has always championed the cause which he considers right. When religion was attacked, he came out in a public lecture and said it was the highest of all conditioned reflexes, and the one that distinguished man from beast; when the portrait of the Prince of Oldenburg was removed from the institute founded by him, Pavlov hung it in his own office; when certain students including many sons of priests were expelled from the medical schools, Pavlov himself resigned from the chair of physiology at the Military Medical Academy as a protest, stating that he also was the son of a priest.

We desire to give Pavlov's views without entering into a controversy, but at the same time it should be stated that the Soviet Government has sincerely endeavoured to further Pavlov's work. Pavlov's life and art place him above all forms of politics. The Soviet Government has wisely recognised this, and has given him full liberty to speak and act as he pleases even when the object of his criticism is themselves, and this at a time when the granting of such licence to others would result in a state of anarchy. No higher proof of their interest in science exists.

He is not only revered by the Soviet Government and esteemed by the intelligenzia, but honoured far and wide all over Russia. Lenin exerted his influence to improve the conditions so that Pavlov might continue to work in Russia after the Revolution when he was contemplating the necessity of going abroad; and on January 24, 1921 the *Sovnarkom* (Council of People's Commissars) passed a decree requiring the Petrograd Soviet to do their best to favour Pavlov's scientific researches. But it is important to note that Pavlov expressed himself boldly and defiantly in 1917 and 1918 during the civil war when to do so was extremely dangerous, and before it was known how the new government would look upon him.

Pavlov is Russian in his intellectual powers, depth of insight, incapacity to accept foregone conclusions, freshness of outlook, originality, etc., but unlike most Russians, and more like the Anglo-Saxon, he subjugates feeling to action, fancy to fact. In some ways he is comparable to his countryman Tolstoi, albeit their philosophies and natures are rather opposite, one being active, the other passive; albeit the spiritual world is to Pavlov somewhat more than the material was to Tolstoi. But the place he occupies in Russia to-day is as singular as that taken by Tolstoi twenty-five years ago.

It is highly characteristic of Pavlov that in the spring of 1927 he decided to undergo the difficult operation for gallstones, an ordeal which many younger men would have shunned. But even at his advanced age he did not shrink from the pain and possible danger, and he furthermore declined the invitation of the government to obtain the best possible foreign surgeon to perform this (the Russian surgeons naturally preferred that some one else do it), and with his characteristic patriotism, averred that he would have only a Russian to operate upon him. One thought was uppermost in his mind—was there not some cure for his malady that would restore the ability to carry on his scientific work? After an unwilling absence of several months from the laboratory, and a persistent and faithful trial of all the prescribed measures, in view of his deepening jaundice, loss of weight, and increasing weakness, he resolutely faced the fact that operation was his only hope. During all his illness he complained more of his inability to work than of the paroxysms of pain. The operation was performed, and even though followed by pneumonia, his indomitable will and energy and his fanaticism for science saved him.

In view of the characteristics already mentioned, it is hardly necessary to add that all Pavlov's lectures are delivered with extraordinary zeal and animation. For the students the explanations are so clear and simple that every new scholar can readily understand them. His lectures are always the most popular, and on many occasions, especially when he is to discuss some timely theme, the auditorium is packed as tightly as are the grand stands of a Thanksgiving football match between two rival colleges. It is unfortunate, however, that not many outside of Russia have ever been able to witness this unusual display of dynamic energy; for there are few who understand the only language (Russian) in which he is fluent. He has lectured abroad on many occasions, went to America in 1923, lectured at the Sorbonne in Paris in 1925; and this year at the age of seventy-eight, he went to England to deliver the Croonian Lecture. He says that it always stimulates him to participate in a scientific meeting.

Not only is Pavlov an indefatigable worker himself, but his spirit and his soul pervade the atmosphere of his laboratories. What he means to his co-workers is attested to by the following comment written in 1922 by an assistant. Although it may seem like extravagant praise to those not knowing Pavlov, it is reality for all who have come in contact with him:

> As a contrast to this picture [the conditions in the laboratories after the war] we see before us the personality of Prof. Pavlov, a living and bright model of inflexible devotion to science, passionately convinced of the fact that scientific research alone, leading to true knowledge, can hold out hopes of a better future to humanity. If all the aforementioned circumstances have undermined little by little the strength and energy of the workers of the laboratory, and sometimes have tended to banish the very desire for work, personally Pavlov has always done his utmost to feed the sacred fire of science and to lend energy to the desire for work without counting the cost of either effort or deprivation, and this through a personal example of passionate seeking after the truth. No difficulties or barriers could exist for Professor Pavlov which were able to force him to leave off any investigation begun; he could therefore be seen working on cold days in the laboratory in a winter coat, fur cap, and snow boots. When the whole city was immersed in darkness on the short winter days by the absence of electric light, and no candles or petroleum could be had, Prof. Pavlov used to continue his experiments in the laboratory by the light of wood torches. Only one who has himself been able to witness this strained and passionate struggle in order not to stop for one single moment the path of scientific investigation, can evaluate this human endurance of the highest sort to attain those truths on the work of the central nervous system, which according to the conviction of the author of these lines will become the pride of contemporary physiology.

His extraordinary energy stood out in bold relief:

> The personal qualities of I. P. Pavlov are alone responsible for the fact that notwithstanding the worst conditions of life, and under nearly impossible conditions for scientific work, both this and the lectures in physiology at the Military Medical Academy have continued without a break. If there was no electric light, the demonstrations were carried out before an auditorium of 200 people by the light of one kerosene lamp, and we had to hurry during the big vivisectional experiments because the immobilized animals froze rapidly in the low temperature. But

nevertheless, all experiments necessary to the lectures of Prof. Pavlov were done in full, as well as the practical studies with the students.

Those who have been privileged to see him in the midst of his creative work do not soon forget it. H. G. Wells after meeting him in his laboratory in 1922, wrote, "Pavlov is a star which lights the world, shining down a vista hitherto unexplored." (*New York Times*, November 13, 1927.) And the following case mentioned by Savitch is characteristic:

An interesting example shows how the work of Pavlov's laboratories impressed his collaborators: During the disorderly retreat of the army after the defeat at Lau-Yan (Manchuria) I met at Mukden a typical military doctor of those times. They all forgot their medical studies and became mere officers. But when he saw me he dashed at me and started speaking enthusiastically of Pavlov's laboratory, and of his dogs, especially "Hector" that had given such excellent results during the experiments. The war and the Japanese were completely forgotten!

And whoever sees him to-day will be struck not only by the simplicity of his manner and conversation, but also by the sudden ardour which dominates the great master of science when the subject turns to something in which he is intensely interested. A grey-haired contemporary, returning to the laboratory in 1926, said exultingly, though Pavlov's face had become that of an old man, when he opened his mouth and began to speak he saw it was the same Ivan Petrovitch of thirty years ago.

Pavlov's personality has been well characterised by Yerkes:

For his remarkable personal qualities I was unprepared. To meet Prof. Pavlov, even at the age of three score and fifteen, was stimulating and invigorating—like facing a fresh breeze from the sea. He was interested in everything, alert, generous in his praise, but also in his constructive criticism, sympathetic—a citizen of the world because a disinterested and devoted searcher for the truth.

What a pleasure it was to learn from his own lips about the progress of his investigation, his plans, expectations and hopes. The years fell away as he talked and his being radiated opinions and strength. (*Pavlov's Seventy-fifth Anniversary Volume*, Moscow, 1924.)

All his working life has been devoted to the discovery and analysis of facts. But Pavlov is not only a successful laboratory investigator. He is a great scientist, and a prophet, whose voice sounds above the din and confusion of the world with a challenge to find and face the facts, to subject our pride and prejudice in conformity to them, and to follow where they lead us. This is the goal toward which his energies converge. But did Pavlov's interest lay in politics or wealth instead of in facts, what limits his fame might have reached!

His epitaph might be written in his own words, uttered with a reverberation of his whole being and with such awe-inspiring force that it compelled silence daring protests made to unwelcome statements in one of his lectures: "I am speaking only the scientific truth, and whether you will or no you must hear it!"

INTRODUCTION TO THE ENGLISH TRANSLATION

This volume is a collection of reports of progress. For about twenty-five years Professor Pavlov prepared no comprehensive account of his highly interesting and important experiments on the functions of the more complex parts of the brain; instead he presented the results of his researches in occasional lectures. Such a presentation has both defects and virtues. Chief among what will be regarded as defects is repetition. In successive lectures the same phenomena are defined again and again. Usually, however, these common phenomena become the basis for describing new developments; and as the developments become more and more complex a frequent return to the fundamental features of the researches makes a more intelligible approach to the new observations.

As a series of reports of progress the volume has importance both as a collection of historical records and as a disclosure of the gradually developing methods and interpretations of a master-workman in physiology. We learn about the first hints and the earliest attempts at what has grown to be a highly elaborate system of facts and ideas concerning the functions of the cerebral cortex, and we have the opportunity of watching stage by stage from its simple beginnings the building of the system. As a revelation of the modes of action of Professor Pavlov's own cortex the book is valuable not only to the professional scientific investigator but to all who are interested in human nature. It discloses his free and bold application of the concept of a *reflex* to all forms of behaviour. Who else would discourse simply and frankly on the "reflex of freedom" and the "reflex of slavery"? It discloses his ingenuity in devising novel and significant experiments which lay before us the movements and relations of the nerve impulses in the brain. The higher strategy of scientific research is here admirably displayed. The book discloses also the readiness of a great investigator to let his imagination play over the facts and give them meaning.

The reader who has heard of "behaviourism" will find here the words of the chief scientific exponent of that manner of regarding the responses of animals. Emphasis is laid strictly on the objective study of these responses. They result from contractions of muscles; the musclar contractions are caused by nerve impulses discharged from the brain; the brain in turn has been disturbed by nerve impulses discharged from surface receptors; these receptors or "sense organs" are stimulated by changes in the environment. The linkage of events throughout is followed on the physiological level. This emphasis on the physiological aspect of complex responses to the environment has already led to new views regarding the relations of processes in the higher nervous centers. It is certain to lead to a formulation of laws of action and interaction of these processes—laws the knowledge of which may reasonably be expected to have highly important significance for the control of conduct. Recognising the stupendous benefits which science has conferred we are led to share fully Professor Pavlov's faith in that result of the analysis of intricate behaviour by scientific methods.

Professor Pavlov is modest in his claims. He recognises that the new territory which he has discovered is vast. It is rich with interesting possibilities. It is difficult to explore. Many years of patient labour by many workers will be required for its conquest. The splendid example of industry and devotion to science which the first explorer has given during his long life will be an incentive to all who will follow after him and push further into the unknown.

WALTER B. CANNON.

Harvard Medical School, Boston, September, 1928.

AUTHOR'S PREFACE TO THE ENGLISH TRANSLATION

This book concerns the investigation of the physiology of the cerebral hemispheres by the strictly objective method of conditioned reflexes. It is a collection of articles, reports, lectures, and addresses which appeared during the twenty-five years when these investigations were being conducted, and it represents the historical advance of our researches, assuming at times a fairly popular character. The lectures are documented and are set forth in detail.

The present volume has been rendered into English owing to the initiative of Dr. Gantt. Others participating in this translation have been, on the one hand, my highly esteemed collaborators in these experiments, G. V. Volborth of the University of Kharkov, and B. P. Babkin of Dalhousie University, Halifax, both of whom are familiar with English as well as with Russian; and, on the other hand, my friend, Dr. Cannon, of Harvard University, who has kindly undertaken the task of perfecting the English rendition. To all these, beginning with Dr. Gantt, I am deeply and sincerely grateful.

Ivan P. Pavlov,
Member of the Academy of Sciences.

Leningrad, August, 1928.

PREFACE TO THE FIRST RUSSIAN EDITION

BEGINNING OF THE WORK WITH CONDITIONED REFLEXES—ADOPTION OF PHYSIOLOGICAL POINT OF VIEW AND REJECTION OF THE SUBJECTIVE METHODS OF PSYCHOLOGY—SIMULTANEOUS BEGINNINGS OF OBJECTIVE EXPERIMENTAL METHODS BY AMERICAN PYSCHOLOGISTS, THORNDIKE, WATSON AND OTHERS—DIFFERENCES BETWEEN THEIR AND PAVLOV'S WORK—BECHTEREV—GOAL AND IDEALS OF TRUE SCIENCE—THE CONDITIONED REFLEX—REASONS FOR COMPLETING THIS BOOK, ITS SHORTCOMINGS AND CONTENTS.

More than twenty years ago I began these experiments independently, passing to them from my former physiological work. I entered this field under the influence of a powerful laboratory impression. For many years previously I had been working on the digestive glands. I had studied carefully and in detail all the conditions of their activity. Naturally I could not leave them without considering the so-called psychical stimulation of the salivary glands, *i.e.*, the flow of saliva in the hungry animal or person at the sight of food or during talk about it or even at the thought of it. Furthermore, I myself had demonstrated a psychical excitation of the gastric glands.[1]

[1] In his *Work on the Digestive Glands*, 1897, Prof. Pavlov states that the excitation of the gastric glands which begins during eating (sham feeding) depends not only upon the stimuli from the mouth cavity, but that there is necessary an active motor reaction of the dog to the food (food reflex). This factor, which must be added to the stimuli from the mouth cavity in order to initiate the gastric secretion, Pavlov considered psychical, and he described it in *Work on the Digestive Glands* as follows:

Consequently, in sham feeding the excitation of the nerves of the gastric glands by the process of chewing and swallowing depends largely on a psychic factor which has here grown into a physiological one, that is to say, is just as much a matter of course and appears quite as regularly under these conditions as any other physiological result. Regarded from the purely physiological side, the process may be said to be a complicated reflex act. Its complexity lies in this, that the ultimate object is attained by the co-operation of many separate organic functions. The material to be digested—the food—is found only outside the organism in the surrounding world. It is acquired not alone by the exercise of muscular exertion, but also by the intervention of higher functions, such as judgment, will, desire. Hence the simultaneous excitation of the different sense-organs, of sight, of hearing, of smell and taste, is (much as in the case of the salivary glands) the first and strongest impulse which arouses the activity of the gastric glands. This especially applies to the two latter senses, since they are only excited when the food has come very near to or has already entered the organism. Through the medium of the response, Nature, resourceful and unerring, has linked the seeking and finding of food with the commencement of digestion. That this initiation of secretion should stand in closest connection with an every-day phenomenon of human life, namely, appetite, might easily have been surmised. Thus appetite, so important to life and so full of mystery to science, here at length assumes a tangible existence and becomes transformed from a subjective sensation into a concrete factor within reach of physiological investigation.

We are therefore justified in saying that appetite is the first and most potent exciter of the secretory nerves of the stomach, a factor which embodies in itself something able to compel the empty stomach of the dog during the fictitious meal

I began to investigate the question of psychic secretion with my collaborators, Drs. Wolfson and Snarsky. Wolfson collected new and important facts for this subject; Snarsky, on the other hand, 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—the thoughts, feelings, and desires—is analogous to ours.[2] We were now brought face to face with a situation which had no precedent in our laboratory. In our explanation of this internal world we diverged along two opposite paths. New experiments did not bring us into agreement nor produce conclusive results, and this in spite of the usual laboratory custom, according to which new experiments undertaken by mutual consent are generally decisive. Snarsky clung to his subjective explanation of the phenomena, but I, putting aside fantasy and seeing the scientific barrenness of such a solution, began to seek for another exit from this difficult position. After persistent deliberation, after a considerable mental conflict, I decided finally, in regard to the so-called psychical

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to secrete large quantities of the strongest juice. A good appetite in eating gives origin at the outset to a vigorous secretion of the most active juice; where there is no appetite this juice is absent. To restore appetite to a man means to provide him with a large stock of gastric juice wherewith to begin the digestion of a meal.

Further investigation and deliberation led him to assume that one has to deal in this case only with a special excitability of the organism by food, which can be considered as a phenomenon of the physiology of the nervous system without recourse to psychical conceptions. A fuller discussion will be found in chapter xiii. As "psychical secretion" is a widely used term, Pavlov employs it here for the sake of conciseness, in spite of his desire to divorce physiology from psychological and subjective expressions.—*Translator.*

[2] In the light of our present exact knowledge of nervous processes, and the laws which govern the salivary reaction, we are able to explain the variations in the secretion, and therefore Snarsky's account of this seems almost humorous. He wrote: "Under the influence of inedible substances one can see that the quantity of saliva does not answer to the degree of pleasantness of the substances; for example, much more saliva flows on sand or glycerine than on a solution of extract of quassia, that is, on an exceedingly bitter solution, although in the last case the grimace of disgust is stronger. . . . After sand the dog licks vigorously and smacks his lips, and it is clear that the grimace of disgust is not so prominent as the desire to cleanse the mouth." (A. T. Snarsky: *Analysis of the Work of the Salivary Glands in the Dog*, St. Petersburg, 1901.)

Snarsky described the different stages of the grimace of disgust, and wrote: "It must be mentioned that one of the experiments was not performed at the usual hour for the dog, but after six o'clock, *i.e.*, after the time when the dogs were usually fed. This change in time was evidently very disagreeable for the dog: he was agitated, he howled, secreted more saliva than usually, and the longer the experiment lasted the more. . . ."

"It was quite evident that the dog was not so much excited by the concentration of the acid solution used as by all the procedure itself, by being disturbed at an unusual hour in the day." (*Ibid.*, p. 28.) Comparing these descriptions with the present description, is it not clear that our present conception of this phenomenon is much simpler? Our explanation is that late in the day or toward the usual feeding hour there is an increased excitability of the central nervous system. (See chapter xiii.)—*Translator.*

stimulation, to remain in the rôle of a pure physiologist, *i.e.*, of an objective external observer and experimenter, having to do exclusively with external phenomena and their relations. I attacked this problem with a new co-worker, Dr. Tolochinov, and from this beginning there followed a series of investigations with my highly esteemed collaborators, which has lasted for more than twenty years.

When I began our investigations with Dr. Tolochinov, I was aware that in the extension of physiological investigation throughout the whole animal world (in the form of comparative physiology), dealing with other animals in addition to those common to the laboratory (dogs, cats, rabbits, and frogs), the physiologist is obliged to abandon the subjective point of view, to endeavour to employ objective methods, and to try to introduce an appropriate terminology (the doctrine of tropism of Jacques Loeb, and the objective terminology of Baer, Bethe, and Uxküll). Indeed, it would be difficult and unnatural to speak of the thoughts and wishes of an ameba or infusorian. But our study concerned the dog, the intimate and faithful companion of man since prehistoric times. And I take it that the most important motive for my decision, even though an unconscious one, arose out of the impression made upon me during my youth by the monograph of I. M. Setchenov, the father of Russian physiology, entitled *Cerebral Reflexes* and published in 1863. The influence of thoughts which are strong by virtue of their novelty and truth, especially when they act during youth, remains deep and permanent even though concealed. In this book, a brilliant attempt was made, altogether extraordinary for that time (of course, only theoretically, as a physiological outline), to represent our subjective world from the standpoint of pure physiology. Setchenov had made at that time an important physiological discovery (concerning central inhibition) which deeply impressed European physiologists. That was the first Russian contribution to this important branch of natural science, which just previously had been remarkably advanced through the successes of German and French physiologists.

The great effort which this discovery demanded, and the joy which it brought, mixed perhaps with personal emotion, gave rise to the ideas expressed by Setchenov, which are certainly those of a genius. Afterwards, it is interesting to note, he never referred to this theme in the same resolute manner as he did at first.[3]

Some years after the beginning of the work with our new method I learned that somewhat similar experiments on animals had been performed in America, and indeed not by physiologists but by psychologists.

[3] Compare with the idea expressed in the last paragraph of chapter xx.—*Translator.*

Thereupon I studied in more detail the American publications, and now I must acknowledge that the honour of having made the first steps along this path belongs to E. L. Thorndike.[4] By two or three years his experiments preceded ours, and his book must be considered as a classic, both for its bold outlook on an immense task and for the accuracy of its results. Since the time of Thorndike the American work (Yerkes, Parker, Watson, *et al.*) on our subject has grown. It is purely American in every sense—in collaborators, equipment, laboratories, and publications. The Americans, judged by the book of Thorndike, set out on this new path of investigation in quite a different manner from us. From a passage in Thorndike one may conjecture that the practical American mind applied to everyday life found that it is more important to be acquainted with the exact outward behaviour of man than with guesses about his internal states with all their combinations and changes. With these considerations concerning man, the American psychologists proceeded to their laboratory experiments on animals. From the character of the investigations, up to the present, one feels that both the methods and the problems are derived from human interests.

I and my co-workers hold another position. As all our work developed out of physiology, so it has continued directly in that path. The methods and the conditions of our experimentation, as well as the scheme of the separate problems, the working up of the results, and finally their systematisation—all this has remained in the realm of the facts, conceptions and terminology of the central nervous system. This approach to our subject from both the psychological and the physiological sides enlarges the sphere of the phenomena under investigation. To my deep regret, I know absolutely nothing about what has been done on this question in America during the last four or five years; up to this time it has been impossible to obtain American literature on the subject here, and my request last year to go to America to learn of the recent work was not granted.[5]

Some years after we started, the problem was also taken up by Bechterev here and by Kalischer * in Germany. In our work we used an inborn reflex upon which all nervous activity is modelled, *viz.*, the food reflex and the defensive reflex against acid, the secretory component of which we had observed. Bechterev used instead the defensive reflex against destructive (painful) irritation of the skin in the form of a

[4] E. L. Thorndike: *Animal Intelligence—An Experimental Study of the Associative Processes in Animals,* 1898.—*Translator.*

[5] Since this preface was written Prof. Pavlov received permission and money from the Soviet Government to go abroad, and he spent the summer of 1923 in France, England, and America.—*Translator.*

* The claims of these as to any priority in such investigations are, of course, for any one having even a slight knowledge of the subject, wholly ephemeral.

movement reaction. Kalischer, however, employed as we did the food reflex, concentrating his attention on the motor reaction. Bechterev designated as "associative" these new reflexes which we called "conditioned," while Kalischer called the whole method the "*Dressurmethode*" (training method). If I may judge from what I saw in the physiological literature during my five weeks' stay in Helsingfors this spring (1923), the objective examination of the behaviour of animals has already attracted the attention of many physiological laboratories in Europe—in Vienna, Amsterdam, etc.

About myself I shall add the following. At the beginning of our work and for a long time afterwards we felt the compulsion of habit in explaining our subject by psychological interpretations. Every time the objective investigation met an obstacle, or when it was halted by the complexity of the problem, there arose quite naturally misgivings as to the correctness of our new method. Gradually with the progress of our research these doubts appeared more rarely, and now I am deeply and irrevocably convinced that along this path will be found the final triumph of the human mind over its uttermost and supreme problem—the knowledge of the mechanism and laws of human nature. Only thus may come a full, true and permanent happiness. Let the mind rise from victory to victory over surrounding nature, let it conquer for human life and activity not only the surface of the earth but all that lies between the depth of the seas and the outer limits of the atmosphere, let it command for its service prodigious energy to flow from one part of the universe to the other, let it annihilate space for the transference of its thoughts—yet the same human creature, led by dark powers to wars and revolutions and their horrors, produces for itself incalculable material losses and inexpressible pain and reverts to bestial conditions. Only science, exact science about human nature itself, and the most sincere approach to it by the aid of the omnipotent scientific method, will deliver man from his present gloom, and will purge him from his contemporary shame in the sphere of interhuman relations.[6]

The freshness of the subject in addition to the hope just expressed should inspire all workers in this new field. The work advances along a wide front. Much has been accomplished during the twenty-five years since its beginning made by Thorndike.

My laboratories have contributed not a little to this progress. Our

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[6] This idealistic conception of scientific work, as a means for building the future happiness of mankind, is characteristic of Prof. Pavlov. He remarked to one of the translators in 1926 on seeing an aeroplane that this sight always stimulated him to think, allegorically, that the human mind would some time be able to rise as high above its present perplexities as the aviator does above the earth.—*Translator.*

investigations have continued unbrokenly up to the present time. A slackening of the work was caused in 1919 and 1920 owing to extraordinary difficulties (cold, darkness, starvation of the experimental animals, etc.). Since 1921 the conditions have improved, and now they approach normal except for lack of instruments and literature.[7]

Our data increases, the outline of our research becomes larger, and gradually there looms up before us a general system of the phenomena in this new field—in the physiology of the cerebral hemispheres, the organ of the highest nervous activity.[8]

These are at present the main features of our work. We are becoming better and better acquainted with the fundamental mode of conduct with which the animal is born,—with the congenital reflexes, heretofore usually called instincts. We observe and intentionally participate in building new reactions on this fundamental conduct, in the form of so-called habits and associations, which now increase, enlarge, become complicated and refined. According to our analysis, these are also reflexes, but *conditioned reflexes.* Step by step we approach the inner mechanism of these reflexes, become more accurately informed about the general properties of the nervous masses in which they move and have their being, and with the hard and fast rules by which they are governed. There pass before us several individual types of nervous systems highly characteristic, strongly expressed, showing us the different aspects and properties of nervous activity upon which is based the whole complicated behaviour of animals. And even more than this! The results of animal experimentation are of such nature that they may at times help to explain the hidden processes of our own inner world.

Such is the situation as I conceive it. The reason that I have not given a systematic exposition of our results during the last twenty years is the following. The field is an entirely new one, and the work has constantly advanced. How could I halt for any comprehensive conception, to systematise the results, when each day new experiments and observations brought us additional facts!

Five years ago, when I was confined to my bed for several months on account of a serious fracture of the leg, I prepared a general review of all our investigations. Then the Revolution began. This, of course, distracted my attention. It was, moreover, my habit to lay aside a written article in order to forget it, so that when I re-read it I could the better note its shortcomings. Thus it happened that what I had prepared was never printed. After half a year of uninterrupted work, it had become

7 The Soviet Government has greatly increased the funds for scientific work in the past few years, and in December, 1926, Prof. Pavlov told one of the translators that his laboratories were receiving all the money they needed.—*Translator.*

8 This outline is described in greater detail in chapter xxxi.—*Translator.*

out of date, and at present it is not suitable for publication, and needs a thorough revision. But to perform such a task quickly and adequately, while living under the present painful conditions which obtain in Russia, is very difficult, indeed I might say, well nigh impossible. And I do not know myself exactly when I shall be able to fulfil this important duty of making an especial and final systematisation of the scientific data accumulated over so long a period. To study this material from the published papers of all my co-workers would be a task of exceeding difficulty, possible only for a few.

On this ground, I, complying with the numerous and repeated requests and desires of my closest collaborators, have ventured to publish in the present volume everything that I have written on our subject, in articles, reports, lectures and speeches, in Russia and abroad, during these twenty years. May this book be a substitute, even though incomplete, for a systematic exposition of the matter for those who are endeavouring to acquaint themselves with the subject or to work in this field. Certainly I see quite clearly the faults of this collection. The chief of these is the number of repetitions. They occurred for a quite comprehensible reason. The subject was new—it developed in the mind of the physiologist only little by little. And in order to comprehend it, to grasp it better, to get a conception of it, there was naturally a desire to expound and interpret every variation, however small it may have been.

But to select, condense, and arrange the material would be at present a difficult and unprofitable labour for me. Perhaps these repetitions and slight variations may not be entirely useless for the reader, especially as the articles are arranged in chronological order, so that before him passes the entire history of our efforts. He will see how little by little the facts expanded and were verified, how they gradually unfolded in our conceptions of the several sides of the subject; and how finally appeared before us a perspective of the highest nervous functions. Nevertheless I should admonish those who are not physiologists nor biologists, and, indeed, every one who will honour my book with his attention, to begin by reading in chronological order my speeches delivered in Madrid, Stockholm, London, the three in Moscow, and the two reports in Gröningen and Helsingfors.[9] Only after these would I advise him to turn to the other special treatments of the subject. In this way the foundation and the general tendency of our work will become clear for the reader, and the details will then show up more definitely on this background.

For those who wish to become acquainted with the original articles of my co-workers, I append at the end of the book a bibliography.

I. P. PAVLOV.

Petrograd, 1923.

9 Chapters i, iii, iv, x, xi, xx, xxi, xxxi.—*Translator.*

LECTURES ON CONDITIONED REFLEXES

LECTURES ON CONDITIONED REFLEXES

CHAPTER I

EXPERIMENTAL PSYCHOLOGY AND PSYCHO-PATHOLOGY IN ANIMALS

(Read before the International Congress of Medicine, Madrid, April, 1903.)

EXPERIMENTAL BASIS FOR THE PHYSIOLOGICAL POINT OF VIEW—BEGINNINGS OF THE PHYSIOLOGICAL STUDY OF PSYCHOLOGICAL FACTS, FIRST WITH THE SALIVARY GLANDS—THE NORMAL FUNCTIONS OF THESE GLANDS, SHOWING APPARENT INTELLIGENCE—PHYSIOLOGICAL ANALYSIS OF THIS INTELLIGENCE—ACTION OF THE SALIVARY GLANDS TO SUBSTANCES AT A DISTANCE ("PSYCHICAL") SAME AS WHEN IN THE MOUTH ("CHEMICAL")—PROOF THAT THESE SO-CALLED "PSYCHICAL" REACTIONS ARE ONLY REFLEX IN NATURE—DISTANT, UNESSENTIAL PROPERTIES BECOME SIGNALS—REACTION DEPENDENT UPON CONDITION OF THE ANIMAL—"PSYCHICAL REACTIONS" (CONDITIONED REFLEXES) ARE ADAPTABLE AND CHANGEABLE—WISHES—TEMPORARY CONNECTIONS—DEVIATION OF EXCITATION PROCESS—ADVANTAGES OF THE METHOD OF CONDITIONED REFLEXES AND OF THE USE OF THE SALIVARY GLANDS IN THEIR STUDY—ADAPTABILITY—THE HOPE AND IDEAL OF SCIENCE—ANIMISM AND VITALISM.

Esteeming the language of facts as the most eloquent, I ask your attention to the experimental material which gives me the right to speak on to-day's subject.

First is the history of the physiologist's shifting his attention from purely physiological to so-called psychical questions. Though this transition was unexpected, it came about in a natural way, and it seems to me to be of great importance for science that this change occurred without abandoning the methodological front of physiology.

While studying, over a course of many years, the normal activity of the digestive glands, and analysing the constant conditions of this activity, I came upon facts (which had also been observed by others) of a psychical character, facts which could not be rationally neglected, as they participated constantly and prominently in the normal mechanism of the physiological processes.[1] I was obliged to consider them if I wished to make the most thorough possible study of my subject. But the question arose, How? And the following exposition will be in answer to this question.

From all our material I shall select only the experiments with the salivary glands—organs having apparently a very insignificant physiological rôle; nevertheless, I am convinced that they will be classical

[1] The word psychical is for Prof. Pavlov only a label for a set of phenomena. As they may be reproduced, he starts studying the circumstances under which they appear or disappear.—*Translator.*

objects in this investigation of a new type, the experiments of which I have the honour to describe to you to-day, in part as performed, in part as planned.

Observing the normal activity of these glands, it is impossible not to be struck with the high degree in which they are adapted to their work. Give the animal some dry, hard food, and there is a great flow of saliva, but with watery food there is much less. Now it is clear that for the chemical testing of the food and for mixing it and preparing it as a bolus capable of being swallowed, water is necessary. This water is supplied by the salivary glands. From the mucous salivary glands there flows for every kind of food, saliva rich in mucin. This facilitates the passage of the food through the œsophagus. Upon all strongly irritant chemical substances, as acids and salts, there is also a free flow of saliva, varying to a certain degree with the strength of the stimulus. The purpose of this is to dilute or neutralise the irritant, and to cleanse the mouth. This we know from every-day experience. This saliva contains much water and little mucin. For, what could be the use of mucin here?

If you put some quartz pebbles into a dog's mouth he moves them around, or may try to chew them, but finally drops them. There is no flow of saliva, or at most only two or three drops. Indeed, what purpose could saliva serve here? The stones are easily ejected and nothing remains in the mouth. But, if you throw some sand in the dog's mouth (the same stones but pulverised), there is an abundant flow of saliva. It is apparent that without fluid in the mouth, the sand could neither be ejected nor passed on to the stomach. We see here facts which are exact and constant, and which really seem to imply intelligence.[2] The entire mechanism of this intelligence is plain. On the one hand physiology has known for a long time of the centrifugal nerves to the salivary glands which may cause either water or organic material to pass into the saliva. On the other hand, in certain regions, the lining of the oral cavity acts as a receptor for mechanical, chemical and thermal stimuli. These different stimuli may be further subdivided: the chemical, for example, into salts, acids, etc. There is reason for assuming the same in regard to mechanical irritants. From these special regions of the oral cavity the specific centripetal nerves take their origin.

All these reactions of adaptation depend upon a simple reflex act which has its beginning in certain external conditions, affecting only certain kinds of centripetal nerve endings. From here the excitation runs along a certain nerve path to the centres whence it is conducted to the salivary glands, calling out their specific function.

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[2] The following statement shows the difference between the vague term intelligence and the successful analysis of the physiologist.—*Translator.*

In other words, here is a certain agent which calls forth in living matter a definite reaction. It is a typical example of adaptation and fitness. Let us consider somewhat closer these facts, which play such an important rôle in modern physiological thought. In what does the adaptation consist? It is nothing more, as we have seen, than the exact co-ordination of the elements of a complicated system, and of their complexes, with the outer world.

This, however, is exactly the same as one can see in every inanimate system. Take, for instance, a complex chemical substance. It can exist only so long as its individual atoms and groups are in equilibrium, and its whole complex is in equilibrium with the surrounding conditions.

In the same way the wonderful complexity of the higher and lower animals can exist as a whole only so long as all the delicate and exact balances of their constituents remain in equilibrium one with the other and with the outside world.[3]

The analysis of the equilibration of this system is the primary aim of physiological enquiry, as a pure, objective investigation. Upon this point there can hardly be two opinions. It is to be regretted that up to the present we have no scientific term to denote this fundamental characteristic of the organism—its ability to maintain an external and internal equilibrium. The words adaptation and fitness, used for it, continue to connote a certain subjectiveness (in spite of Darwin's scientific analysis), which leads to a misunderstanding in two opposite directions. The strict adherents of the physico-mechanical school see in these words an anti-scientific tendency—a retreat from pure objectiveness to speculation and teleology. On the other hand, the philosophically inclined biologists consider every fact which concerns adaptation or fitness as proof of the existence of a vital force, or, as we more and more often hear it called, a spiritual force (vitalism gives way, it seems, to animism), which sets its goal, chooses its means, and adapts itself.

Thus, in the foregoing physiological experiments we remain within the bounds of a strictly naturalistic problem. Now, however, we will proceed to other facts which seem to fall into another category.

All the foregoing substances, which when placed in the mouth influence specifically the salivary glands, act exactly the same upon these glands, at least in a qualitative way, when they are a certain distance from the dog. Dry food, even from a distance, produces much saliva; moist

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3 This is perhaps the most objective, naturalistic conception of living organisms, the objective study of all the phenomena of life. It led Pavlov to consider even the temporary or acquired reactions also as a mechanism of equilibration with the conditions of the outer world, altering at every moment and for every individual. It is plain that for a physiologist the mechanism of these new reactions in higher animals could be only a nervous one.—*Translator.*

food, only a little. To the stimulation by food at a distance, there flows into the mouth from the mucous glands a thick, lubricating saliva. Inedible substances also produce a secretion from all the glands, but the secretion from the mucous glands is watery and contains only a small amount of mucin. The pebbles when shown to the dog have no effect on the glands, but the sand provokes an abundant flow of saliva. The above facts were partly discovered, partly systematised by Dr. Wolfson in my laboratory. The dog sees, hears, and sniffs all these things, directs his attention to them, tries to obtain them if they are eatable or agreeable, but turns away from them and evades their introduction into the mouth if they are undesired or disagreeable. Every one would say that this is a psychical reaction of the animal, a psychical excitation of the salivary glands.

How should the physiologist treat such facts? How can he state them, how analyse them? Where do they stand in comparison with physiological facts? What are their common and what their individual characteristics?

To understand these phenomena, are we obliged to enter into the inner state of the animal, and to fancy his feelings and wishes as based on our own?

For the investigator, I believe there is only one possible answer to the last question—an absolute "No." Where does there exist so incontestable a criterion that one may judge by it, and may use it in understanding the internal state of an animal by comparison with our own, even though the animal be so highly developed as the dog? And further: does not the eternal sorrow of life consist in the fact that human beings cannot understand one another, that one person cannot enter into the internal state of another? Where is that knowledge, where is the understanding that might enable us to know correctly the state of our fellow man? In our "psychical" experiments on the salivary glands (we shall provisionally use the word "psychical"), at first we honestly endeavored to explain our results by fancying the subjective condition of the animal. But nothing came of it except unsuccessful controversies, and individual, personal, inco-ordinated opinions. We had no alternative but to place the investigation on a purely objective basis. The first and most important task before us, then, is to abandon entirely the natural inclination to transpose our own subjective condition upon the mechanism of the reaction of the experimental animal, and instead, to concentrate our whole attention upon the investigation of the correlation between the external phenomena and the reaction of the organism, which in our case is the salivary secretion. Reality must decide whether the elaboration of these new phenomena is possible in this direction. I dare to think that the following account will convince you, even as I am convinced, that in the given cases there opens before us an unlimited territory for successful research in a second immense part of the physiology of the nervous system as a system which establishes the relation, not between the individual parts of the organism with which we previously dealt, but between the organism and the surrounding world. Unfortunately, up to the present time, the influence of the environment on the nervous system has been explained for the most part subjectively, and this comprises the whole contents of the contemporary physiology of the sense organs.

In our psychical experiments we have before us definite, external objects, exciting the animal and calling forth in it a definite reaction—the secretion of the salivary glands. The effect of these objects, as has been shown, is essentially the same as in the physiological experiments in which they come in contact with the tongue and palate, as in eating. This is nothing more than a further adaptation, *i.e.*, that the object influences the salivary glands if it is brought even *near* the mouth.

What is the characteristic of these new phenomena compared with the earlier physiological ones? At first, it seems that the difference lies in the fact that in the physiological form of the experiment, the substance is brought into direct contact with the organism, whilst in the psychical, it acts from a distance. If one considers carefully, one sees that there exists no essential difference between these quasi-peculiar and the purely physiological experiments. The difference consists in the fact that in these cases the substances act upon other specific body surfaces—upon the nose, eye, ear—thanks to the surrounding medium (air, ether), in which the organism and the exciting substances are immersed. How many simple physiological reflexes start from the nose, the eye, and the ear, and therefore originate at a distance! The essential difference between these new phenomena and the purely physiological is not to be sought for here.

It is necessary to seek more deeply for this difference and, as it seems to me, in the following facts. In the physiological case the activity of the salivary glands is connected with those properties of the substance upon which the effect of the saliva is directed. The saliva serves to moisten and lubricate the material to be swallowed and to neutralise the effect of the chemically active substances. And this is exactly the function of the special stimulators of the specific mouth surfaces. Consequently, in the physiological experiments the animal is excited by the essential, unconditioned properties of the substance, *i.e.*, by those intimately connected with the physiologic rôle of the saliva.

In the psychical experiments the animal is excited by properties of the external object which for the work of the salivary glands are unessential, or by even entirely accidental and unimportant properties. Visual, auditory, and even pure olfactory properties of our objects, *per se*, applied to other objects, remain without any influence on the salivary glands; for they, on their side, possess no business relation, so to speak, to these properties. In our psychical experiments there appear before us as stimulators of the salivary glands not only such properties (appearance, sound, odour, etc.) of the various objects which are unessential for the work of these glands, but absolutely all the surroundings in which these objects are presented to the dog, or the circumstances with which they are connected in real life. For example, the dish in which it is presented, the furniture upon which it is placed, the room, the person accustomed to bring it, and the noises produced by him—his voice, and even the sound of his feet—though at the moment he cannot be seen. Thus in the psychical experiment the connection of the objects exciting the salivary glands becomes more and more distant and delicate. Undoubtedly we have before us here an extreme degree of adaptation. We may admit that in this special case such a remote and fine reaction as that of the salivary glands to the characteristic step of the person who usually feeds the animal has no other physiological importance than its subtleness. Yet one need only remind oneself of a case in which the saliva of certain species contains poisons as a protection against other animals to see what a great significance for life this expectant production of the poison gland can have when enemies only approach, *i.e.*, when they are first seen or heard (signalling action). The importance of the remote signs (signals) of objects can be easily recognised in the movement reaction of the animal. By means of distant and even accidental characteristics of objects the animal seeks his food, avoids enemies, etc.

If that is so, then the chief difficulty of our subject is expressed in the question, Can all this seeming chaos of relations be included in a certain scheme? Is it possible to make the phenomena constant, to discover the laws which govern their mechanism? The examples which I shall present now give me the right, I think, to answer these questions categorically, "Yes," and to find at the basis of our psychical experiments always the same special reflexes as the common mechanism. In its physiological form our experiment—provided, of course, all extraordinary conditions are excluded—always gives the same results; this is an *unconditioned reflex*. The main characteristic of the psychical experiment is the inconstancy of its results and its apparent capriciousness. The results of a psychical experiment, however, recur with more or less constancy, otherwise we could not speak of it as a scientific experiment. The difficulty of the psychical experiment lies in the greater number of factors which must be considered. Thus the reflex obtained is *conditioned.*[4]

Now I shall present the facts which bear testimony that our psychical material obeys certain laws. These data were obtained by Dr. F. Tolochinov in my laboratory.

It is not difficult to recognise in the first psychical experiments certain important conditions which insure constant results and guarantee the success of the experiment. You stimulate an animal (*i.e.,* his salivary glands) by food from a distance; the success of the experiment depends exactly upon whether the animal has been prepared by a previous period of fasting. In a hungry dog we get a positive result, but, on the contrary, in even the most avaricious and greedy beast we fail to get a response to food at a distance if he has just satiated himself. Thinking physiologically we can say that we have a different excitability of the salivary centre—in the one case greatly increased, in the other decreased.[5] One may rightly suppose that just as the carbonic acid of the blood determines the energy of the respiratory centre, the composition of the blood in the fasting or fed animal likewise regulates the threshold of excitability of the salivary centre, as noted in our experiment. From the subjective point of view this change in excitability could be designated as attention.[6] With an empty stomach the sight of food causes the mouth to ''water''; in a satiated animal this reaction is very weak or may be entirely lacking.

Let us go further. If you only show the dog food, or some undesired substance, and repeat this several times, at each repetition you get a weaker result, and finally no reaction whatever. But there is a sure method of restoring the lost reaction: this is by giving the dog some food or by putting any undesired substance into the mouth. This provokes, of course, the usual strong reflex, and the object is again effective from a distance. It is immaterial for our result whether food is given or the undesired substance is put into the mouth. For instance, if meat powder, having been repeatedly brought before the dog, fails to produce a flow of saliva, we may again make it active by either giving it to the dog to eat (after showing it), or by putting an undesired substance into his mouth, *e.g.,* acid. Owing to the direct reflex, the irritability of the salivary centre has been increased, and now the

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[4] This is the first time we meet with the term "conditioned reflex." From the foregoing one will see why Pavlov gave this adjective to the new phenomena. It is explained in many other places in the book.—*Translator.*

[5] This is the essence of the theory developed in chapter xiii.—*Translator.*

[6] One will notice that here Pavlov tries to find correlations in the subjective world for the nervous phenomena leading to the production of the conditioned reflexes. At this stage the motor reactions were considered as the expression of desire, while the salivary reaction might be the effect of consciousness or thought.—*Translator.*

weak stimulus—the object at a distance—becomes strong enough to produce its effect. Does it not happen the same with us when, having no desire for food, an appetite comes as we begin to eat, or also when we have experienced shortly before some unpleasant emotion (anger, etc.)?

Here is another series of constantly recurring facts. The object acts upon the salivary glands at a distance not only as a complex of all its properties, but through each of its individual properties. You can bring near the dog your hand having the odour of the meat powder, and that will be enough to produce a flow of saliva. In the same manner the sight of the food from a further distance, and consequently only its optical effect, can also provoke the reaction of the salivary glands. But the combined action of all these properties always gives at once the larger and more significant effect, *i.e.,* the sum of the stimuli acts more strongly than they do separately.

The object acts from a distance upon the salivary glands not only through its inherent properties but also through accidental qualities accompanying the object. For example, if we colour the acid black, then water to which we add a black colour will affect the salivary glands from a distance. But these accidental properties of the substance become endowed with the quality of stimulating the salivary glands from a distance only if the object with the new property has been introduced into the mouth at least once. The black coloured water acts on the salivary glands only in case the black coloured acid has been previously put into the mouth. To this group of conditioned properties belong stimuli of the olfactory nerves. The experiments of Snarsky in our laboratory showed that there exist simple physiological reflexes from the nasal cavity acting on the salivary glands, and that they are conducted only through the trigeminal nerve; for example, ammonia, oil or mustard, etc., always produce a constant action in the curarised animal. This action fails, however, if the trigeminal nerves are cut. Odours without local irritating effects have no influence on the salivary glands. If you bring before a dog with a salivary fistula oil of anise for the first time, there is no secretion of saliva. If, however, simultaneously with the odour of anise you touch the oral cavity with this oil (producing a strong local reaction), there will afterwards be a secretion of saliva from only the smell of the oil of anise.

If you combine food with an undesired object, or even with the qualities of this object—for instance, if you show the dog meat moistened with acid—notwithstanding the fact that the dog approaches the meat, you note a secretion from the parotid gland (there is no secretion from this gland with pure meat), *i.e.,* a reaction to an undesired object. And further, if the effect of the undesired object at a distance, owing to its repetition, is diminished, combining it with food which attracts the animal always strengthens the reaction.

As mentioned above, dry food causes a great flow of saliva, while moist food produces a very weak secretion or none at all. If you show the dog two such oppositely acting substances as dry bread and moist meat, the result, as judged by the salivary reaction, will depend solely upon that one which stimulates the dog more strongly. If, as usually happens, the dog is more strongly stimulated by the meat, then you will see only the reaction characteristic of the meat, *i.e.,* there will be no flow of saliva. In this way the bread, although lying before the eyes of the animal, remains without effect. You can impart the odour of meat or sausage to the bread and remove the meat and sausage so that only the odour remains; then the dry bread can act on the eye only, but the reaction is, notwithstanding, that from the sausage or meat. That is, the reaction to one of the properties of the meat, its odour, is the reaction which we saw to all the properties of the meat, *viz.,* to the presence of the actual meat.

The influence of objects at a distance can be inhibited in other ways. If another dog is fed with dry bread in the presence of a greedy, highly excitable dog, then the salivary glands, which at the sight of the bread formerly reacted strongly, fail to secrete. When a dog is brought for the first time on the stand, the sight of the dry bread, which called forth a marked flow of saliva when the dog was on the floor, now has not the slightest influence.

I have stated easily and exactly reproduceable facts. It is evident that many striking instances of animal training belong to the same category as some of our phenomena, and they have borne witness for a long time to a constant lawfulness in some of the psychical manifestations in animals. It is to be regretted that science has so long overlooked these facts.

Up to this point in my exposition I have not mentioned those manifestations which might correspond to what in the subjective realm are called wishes. In reality we have met no such phenomena. Our fundamental fact may be reiterated: to dry bread the dog will hardly turn, but nevertheless, the sight of it produces a strong flow of saliva; whereas meat, toward which the dog, breaking from his frame, rushes with avidity and which he snaps at with his teeth, is without action on the salivary glands from a distance. From this we can say that what in the subjective realm we call a wish was expressed in the animal only by a movement reaction, but that on the salivary secretion the wish did not manifest itself. Therefore, the statement that an ardent desire excites the salivary or gastric glands does not correspond to the facts. This fault of confusing evidently different things can be imputed also to me in earlier articles. Indeed, we must discriminate sharply between the secretory and motor reactions of the organism in our experiments; and in regard to the work of the glands, if we would correlate our results with the phenomena of the subjective world, we must emphasise that not the wish of the dog, but his attention, is the chief condition for the success of the experiments. The salivary reaction of the animal might be considered in the subjective world as a substratum of pure, elementary representation of thought.

All the above facts lead, on the one hand, to important and interesting conclusions about the processes in the central nervous system, and, on the other hand, to the possibility of a more detailed and successful analysis. Let us now consider some of our facts physiologically, beginning with the cardinal ones. If a given object—food or a chemical—is brought in contact with the special oral surface, and stimulates it by virtue of those of its properties upon which the work of the salivary glands is especially directed, then it happens that at the same time other properties of the object,[7] unessential for the activity of these glands, or the whole medium in which the object appears, stimulate simultaneously other sensory body surfaces. Now these latter stimuli become evidently connected with the nervous centre of the salivary glands, whither (to this centre) is conducted through a fixed centripetal nervous path also the stimulation of the essential properties of the object. It can be assumed that in such a case the salivary centre acts in the central nervous system as a point of attraction for the impulses proceeding from the other sensory body surfaces. Thus from the other excited body regions, paths are opened up to the salivary centre. But this connection of the centre with accidental pathways is very unstable and may of itself disappear. In order to preserve the strength of this connection it is necessary to repeat time and again the stimulation through the essential properties of the object simultaneously with the unessential. There is established in this way a temporary relation between the activity of a certain organ and the phenomena of the external world. This temporary relation and its law (reinforcement by repetition and weakening if not repeated) play an important rôle in the welfare and integrity of the organism; by means of it the fineness of the adaptation between the activity of the organism and the environment becomes more perfect. Both parts of this law are of equal value. If the temporary relations to some object are of great significance for the organism, it is also of the highest importance that these relations should be abandoned as soon as they cease to correspond to reality.

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[7] One will notice that at this place is given a scheme of the origin of conditioned reflexes which was confirmed by experiment (chapter iv, footnote 3) and which up to the present remains unchanged.—*Translator.*

Otherwise the relations of the animal, instead of being delicately adapted, would be chaotic.

Let us consider another thing. How can we represent physiologically the fact that the sight of meat destroys the reaction of the parotid to the sight of bread, *i.e.*, that the saliva which flowed earlier at the sight of bread, is abolished by the simultaneous stimulation from meat? One can imagine that the strong movement reaction to meat corresponds to a strong irritation of a certain motor centre, and that in consequence of this, according to the above-mentioned law, the stimulation is diverted from the other parts of the central nervous system, and in particular from the salivary centres, *i.e.*, their excitability is decreased. This explanation is favoured by the other experiment, in which the salivary secretion at the sight of bread is inhibited by the sight of another dog who is eating. Here the motor reaction to the bread is greatly strengthened. Even more convincing would be an experiment in which, if possible, one could find a dog preferring dry food to moist, and showing a stronger motor reaction to it. In such an animal, under the above conditions, if the salivary secretion should fail or be much less for the dry bread than in the usual dogs, then we should be entirely right in explaining our experiment in this way. It is well known that often a very powerful desire may inhibit certain special reflexes.

Among the above-mentioned facts there are some which at present are with difficulty explained from the physiological point of view. For example, why does a conditioned reflex, if repeated, lose its activity? One would naturally think of fatigue, but this cannot be so, as we have to deal here with a weak stimulus. The repetition of a strong stimulus of the unconditioned reflex does not result in such early fatigue. Probably we have to deal here with quite peculiar conditions for the excitation which travels along the temporary centripetal paths.

From all the foregoing it is clear that our new subject can be considered entirely objectively, and that it is essentially a physiological subject. It is hardly possible to doubt that the analysis of this group of stimuli, affecting the nervous system from the outside world, will show us such laws of nervous activity and disclose its mechanism in such aspects as have been left entirely untouched or only suggested in the previous investigation of the nervous phenomena of the internal organism.

In spite of the complexity of the new phenomena, our method has certain advantages. In the contemporary study of the mechanism of the nervous system, first, the experiments are done on an operated and injured animal, and secondly, and this is worse, the stimuli are applied directly to the nerve trunks, *i.e.*, the excitation spreads simultaneously in the same manner over a mass of widely differing nerve fibres. Such combinations never occur in reality. Thus, by our artificial stimulation, we throw the normal activity of the nervous system into a state of chaos and we are greatly hindered in the discovery of its laws. Under the normal conditions, as maintained in our new experiments, the stimuli are isolated and their intensity regulated.

This advantage relates in general to all psychical experiments, but in our psychical phenomena, observed in the work of the salivary glands, there is another special advantage. For the successful investigation of such a complicated subject it is important that it be in some way simplified. By our methods this simplification can be obtained. The rôle of the salivary glands is so evident that their relation to the organism must also be simple and easily available for investigation. One must not conclude, however, that all the services of the salivary glands are contained in their elementary function. By no means. Saliva is used by the animal, for example, to lick and promote healing of its wounds, as we constantly see. That is why we may obtain varieties of saliva from the stimulation of several afferent, centripetal nerves. But the complexity of the reaction of the salivary glands is much less than that of skeletal muscle; through the latter reactions, the organism is entangled with the outer world in an endless number of ways. Furthermore, the simultaneous comparison of the glandular (in particular the salivary) reaction with the motor will give us the possibility, on the one hand, of differentiating the special points from the general ones, and, on the other hand, of getting rid of the habitual and routine anthropomorphic conceptions and explanations which we have accumulated and which confuse our understanding of the motor reaction of animals.

After having established the possibility of analysis and systematisation of our phenomena, we come to the following phase of the work—the systematic division and destruction of the central nervous system in order to see how the previously established relations will be changed.[8] In this way will occur the anatomical analysis of the mechanism of these relations. This will constitute the future of an already approaching experimental psycho-pathology.

The salivary glands as objects of investigation will be of value even for this purpose. The nervous system having to do with movement is so intricately developed, and predominates to such an extent in the brain, that often only a slight injury of it produces an undesirable and exceedingly complicated result. The nervous mechanism of the salivary glands, owing to their minor physiological importance, constitutes, we may think, only a small portion of the brain substance, and is so slightly distributed that partial and isolated destruction of the brain

[8] See chapter v, footnote 1.—*Translator.*

will not give rise to those difficulties which ensue from the disturbance of the motor apparatus. Certainly psycho-pathological experiments had their beginning at the time when the first physiologists removed that or another part of the central nervous system, and observed the animals that survived the operations. The last twenty or thirty years have given us some capital facts. We already know the definite limitations of the accommodating capacity of animals having the cerebral hemispheres or a part of them removed. But the investigation of this theme is not yet arranged in such a way that its study might proceed without interruption according to a definite plan. The cause of this lies, I think, in the fact that the investigators do not possess at present any considerable detailed knowledge of the normal relations of the animal to its surroundings, by the help of which they might make an exact and objective comparison of the state of the animal before and after operation.

Only by proceeding along the path of objective investigation can we step by step arrive at the complete analysis of that infinite adaptability in every direction which constitutes life on this earth. The movement of plants toward the light and the seeking of truth through a mathematical analysis—are these not phenomena belonging to the same order? Are they not the last links in an almost endless chain of adaptabilities which appear everywhere in living creatures?

We can analyse adaptability in its simplest form by use of objective facts. What reason is there to change this method in studying adaptability in the higher orders!

Work in this direction began in different regions of life, and, not stopping at obstacles, has made a brilliant advance. The objective investigation of living matter, the initial study of tropisms of elementary living things, can and must remain the same when it reaches the highest manifestations of the animal organism, the so-called psychical phenomena of the higher animals.

Guided by the similarity or identity of the external manifestations, science will sooner or later bring the obtained objective results to our subjective world, and will at once illuminate our mysterious nature, will explain the mechanism and the vital meaning of that which eternally occupies the human mind—its conscience, and its tribulations. This is why I have admitted in my exposition some contradictory terms. In the title of my address and during my entire exposition, I have used the term "psychical," but at the same time I have brought forward only objective investigations, entirely neglecting everything subjective. The so-called psychical phenomena, although observed objectively in animals, are distinguished from the purely physiological, though only in degree of complexity. What can be the importance of how they are designated—"psychical" or "complicated nervous"—in distinction from the simple physiological, once it is recognised that the duty of the naturalist is to approach them only from the objective side, in nowise taking into consideration the question of the essence of the phenomena?

Is it not clear that contemporary vitalism, that is, animism, confuses the different points of view of the naturalist and of the philosopher? All the marvellous successes of the first have been founded on the investigation of objective facts and their comparisons, ignoring on principle the question of essence and final causes. The philosopher, himself personifying the highest human aspiration to synthesise, though up to the present time this synthesis has been fantastic, striving to give an answer to everything that concerns man, must now create the whole from the objective and subjective. For the naturalist everything is in the method, in the chances of attaining a steadfast, lasting truth, and solely from this point of view (obligatory for him) is the soul, as a naturalistic principle, not only unnecessary for him, but even injurious to his work, vainly limiting his courage and the depth of his analysis.

CHAPTER II

THE PSYCHICAL SECRETION OF THE SALIVARY GLANDS (COMPLEX NERVOUS PHENOMENA IN THE WORK OF THE SALIVARY GLANDS) [1]

(From the *Archives Internationales de physiologie*, 1904.)

THE RÔLE OF SALIVA; AMOUNT SECRETED WITH VARIOUS SUBSTANCES WHEN IN THE MOUTH AND AT A DISTANCE—DISAPPEARANCE AND RESTORATION OF THE CONDITIONED REFLEXES—EFFECT OF HUNGER—ESSENTIAL AND UNESSENTIAL PROPERTIES—SUBJECTIVE AND OBJECTIVE METHODS—EXPERIMENT SHOWING DISAPPEARANCE OF CONDITIONED REFLEXES—EXPERIMENT OF RESTORATION—ADVANTAGES OF THE OBJECTIVE METHOD, ILLUSTRATED BY EXAMPLES—RELATION OF CONDITIONED REFLEX TO UNCONDITIONED REFLEX—DEPENDENCE OF CONDITIONED REFLEX ON CEREBRAL HEMISPHERES—PROPERTIES OF THE CONDITIONED REFLEX—EXPLANATION OF RESTORATION—RENUNCIATION OF PSYCHOLOGICAL METHODS NECESSARY.

RECENTLY the physiology of the salivary glands has brought into the limelight special phenomena of their activity, usually called psychical.

The latest investigations of the work of the salivary glands by Glinsky, Wolfson, Henri and Malloizel, and Borisov * have demonstrated the beautiful adaptation of these glands to external stimulations, as had already been foreseen by Claude Bernard. Under the influence of hard, dry food introduced into the mouth, the salivary glands secrete a large quantity of saliva, and this makes possible the manifestation of the chemical properties of the food when in solution, and helps in its mechanical preparation, thus favouring its passage along the œsophagus into the stomach. On the other hand, the saliva is produced in much smaller quantity when the food contains much free water, and the more water, the less saliva. With milk, it is true, a great amount of saliva is secreted, but it must be taken into consideration that the addition of mucous saliva to milk prevents the formation of large curds in the stomach owing to strands of mucus; the saliva in this way aids the digestive effect of gastric juice on the milk. With water or with a physiological saline solution (*i.e.*, 0.9 per cent. of table salt), there is no trace of saliva secreted; for with them saliva would be useless. To all strong chemical excitants introduced into the mouth, saliva is secreted in an amount strictly conditioned by the stimulating strength of these

[1] The subject matter of this chapter is the same as the foregoing, but the processes are illustrated by experiments.—*Translator.*

* Glinsky: *Annals of the Society of Russian Physicians*, St. Petersburg, 1895. Wolfson: *Dissertation*, St. Petersburg, 1898. Henri et Malloizel: *Comptes rendus de la Société de biologie*, Paris, 1902. Borisov: *Russian Physician*, 1903, p. 869.

substances. In such a case saliva makes these substances more dilute, and rinses and cleanses the mouth. With food substances the mucous salivary glands secrete a saliva rich in mucus. With inedible, or chemical substances, there flows, on the contrary, a thin, watery saliva, containing little or no mucin. In the first instance the saliva serves as a lubricant for the passage of food into the stomach and to effect certain changes in it; in the second case, only as a cleansing agent. Pure sea- or river-sand introduced into the mouth calls forth a secretion of saliva; for it can be removed only by a flow of fluid. Clean quartz pebbles are simply ejected from the mouth without any salivary secretion; for their removal liquid is unnecessary and useless.

In all the foregoing cases special reflexes are involved which, thanks to the specific irritability of the peripheral endings of the centripetal nerves of the mouth (through various mechanical and chemical stimuli), condition the difference in the activity of the glands in their response to these stimuli.

The same relations are observed between the above mentioned stimuli and the activity of the salivary glands when these stimuli are not in contact with the mouth, but are at some distance from the dog. They need only attract the attention of the animal.

Now arose a question of great importance: how may these latter relations be investigated? Having tried several methods, we decided to persist in studying them objectively. This means that the experimenter, completely ignoring the imaginary and subjective state of the animal, must concentrate all his attention on those exact external conditions which might have an influence on the activity of the salivary glands.

The starting point for this investigation was the idea that the so-called psychical salivary secretion is fundamentally a specific reflex just like the secretion originating from stimuli in the oral cavity, but with this difference only, that the psychical reflex originates from stimuli acting on the other receptor surfaces, and that it is a temporary and conditioned reflex.[2] Thus the purpose of further investigations consisted in the study of the conditions under which these specific reflexes appear. The first experiments of this sort in our laboratory were performed by Dr. Tolochinov.*

His experiments showed convincingly, I think, that our subject can actually be investigated along these lines with great success. The following constant relations were established. The aforementioned reflexes

[2] Here the conditioned reflex is first spoken of as a temporary connection.—*Translator.*

* *Comptes rendus du Congres des naturalistes et médecins du Nord à Helsingfors*, 1902.

with food substances as well as with those the dog refused, which excited the salivary glands from a distance, entirely disappear if the experiment is repeated several times at short intervals. But their effect can be easily restored under the following conditions. If for example meat powder is held in front of the dog without feeding it, and if this is repeated a number of times at short intervals, then the action of this stimulus from a distance gradually diminishes and finally vanishes entirely. But it is only necessary to give some of this powder to the dog to eat in order to restore its action from a distance. The same result may be obtained if, instead of feeding the dog with the meat powder, some acid is put into his mouth.

When the acid after some repetitions has lost its ability to call forth saliva when presented from a distance, then it is possible to restore the reflex from a distance by another method analogous to the aforementioned one (putting acid into the mouth or feeding with the meat powder) ; and this is by showing the dog meat which is moistened with acid. It should be mentioned that meat alone, as it is a watery food, produces only a weak salivary secretion, and often none at all from the parotid gland.

In the case of the food substances, their effect from a distance is markedly influenced by the state of hunger of the animal. When the animal is satiated the reaction is much less than it is in hunger, and on repetition of the stimulation by the food substances at a distance the reaction disappears much more quickly. The individual properties of a substance acting separately from a distance have a much weaker effect when one of these properties acts alone than when the substance with all of its properties and attributes acts; for example, a sniff alone of the meat powder produces less salivary secretion than if the meat powder stimulates not only the nose but also the eyes of the animal. One sees the same thing when the experiment on the distant action of the substance is repeated—the effect of the isolated action of the individual properties disappears more quickly than that of the object with all its attributes.

The conditioned reflex (reflex from a distance) can by certain means be quickly destroyed. If immediately after the production of a strong salivary secretion (by using dry bread at a distance), the dog is shown raw meat, then the secretion is instantaneously arrested. If a hungry dog is shown dry bread, and at the same time a neighbouring dog is given the bread to eat, the salivary secretion which had already begun in the first dog may suddenly stop. A dog which has never been used for such an experiment will give a reaction to the bread when the dog is on the floor, but it is only necessary to remove the animal to the stand on the table and the reaction ceases. The same phenomenon can be reproduced by every substance which acts from a distance.

If acid which is coloured black with India ink is put into the dog's mouth several times, then only showing the dog water similarly coloured produces exactly the same effect. Now this connection between the coloured liquid and the secretion of saliva we can cause to disappear by putting repeatedly into the dog's mouth coloured (black) water, and then, we are able to restore it again by the introduction of coloured acid.

If some odour having no exciting local action on the nasal mucous membrane and issuing from a substance which the dog has never before met, acts on the dog, then this odour is entirely without effect on the salivary glands. But once this substance has been put into the dog's mouth and has produced a flow of saliva, its odour alone will suffice to evoke the secretion.

In the preceding chapter, I endeavoured to draw general conclusions of a scientific nature from all the investigations which had been published concerning the new type of reflexes in the work of the salivary glands, systematising our facts from a purely physiological point of view.

From this point of view, to understand thoroughly the basis of the new aspects of physiological investigation of the activity of the salivary glands, it is necessary to distinguish in the objects of the external world acting on the living organism, two series of properties: the *essential* properties, which determine absolutely a certain reaction in that or another organ; and the *unessential* properties, which act only temporarily and conditionally. Take, for instance, a solution of acid. Its action, as that of a definite chemical agent on the mouth cavity, is expressed among other things always by the flow of saliva, which, in neutralising, diluting and removing the acid, is of prime importance for the welfare of the organism. The other properties of this solution, its appearance, colour and odour, have no intrinsic relation to the saliva, or, vice versa, the saliva to them. But it is impossible not to be struck with a fact of great importance for the living organism,—that the unessential properties of a substance become stimuli of a given organ (in our case of the salivary glands) only when the action of these properties on the sensory receptor surfaces of the organism have coincided with the action of their essential properties. If, on the contrary, the unessential properties act repeatedly alone (without the interference of the essential), and if this continues for a long time or always, then they either lose their importance for the given organ, or never attain such importance. The physiological mechanism of this relation can be explained in the following way: suppose that the action in the oral cavity of those properties of the object essential for salivary secretion, *i.e.,* the stimulation of the lower lying salivary centre, coincides with the action of the unessential properties of the object on other receptor surfaces, or coincides with the influence of many phenomena of the external world (stimulation of the eye, nose, etc.) ; in this event the stimulation of the corresponding centre of the higher parts of the brain will have to choose between countless and different paths which are open to them, or, such of them as lead to the active reflex salivary centre. One is compelled to suppose that this latter centre, being in a state of high excitation, in some way attracts to itself the stimulations from other less strongly excited centres. This may be the general mechanism of all our observed phenomena of psychical stimulation of the salivary glands.

The fact that the salivary reaction to the appearance of bread at a distance decreases in intensity at the sight of another dog's being fed could be explained by the transference of the stimulation to another centre, the motor centre, which, as we may conclude in this case from the extreme increase of the energy of the animal's movements, is strongly excited.

The influence of the state of hunger or satiety on the result of the action of food at a distance may be explained by the changes in the irritability of the salivary centre, which in turn depends upon the different chemical composition of the blood in these two states.

Considering these phenomena from this point of view, the physiologist is hardly inclined to designate them as "psychical"; but in order to distinguish them from the nervous phenomena which until the present time have been analysed physiologically, he may classify them as "complex nervous phenomena."

Reviewing the above facts and results, the reader may say that everything which has been described here as "complex nervous phenomena" is comprehensible from the subjective point of view, and that in the physiological description of these facts there is nothing new. In this assertion there is a grain of truth. But by our physiological scheme, we intend to provide a basis for the collection and exposition of additional facts along this new path of investigation.

In the preceding chapter, I expressed the hope that the enumerated facts might be further studied with complete success. This hope, thanks to the further investigations in my laboratory, has been fully realised.

Dr. Babkin has added much to our knowledge of the disappearance [3] and restoration of new reflexes. Here I give a typical experiment:

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[3] Prof. Pavlov purposely uses here an expression only describing the fact, without suggesting its explanation. This phenomenon was later called *extinction* of the conditioned reflex, and in the following chapters the reader will meet with it under this name.—*Translator.*

TABLE I.

Time	*Stimulus*	*Duration of Action*	*Quantity of Saliva (cc.)*
2:04	Sight of Meat Powder	1 Min.	0.4
2:49	" " " "	1 "	0.3
2:52	" " " "	1 "	0.2
2:55	" " " "	1 "	0.1
2:58	" " " "	1 "	0.05
3:01	" " " "	1 "	0.05
3:04	" " " "	1 "	0.0

This vanishing of the reflex as a result of repetition takes place with exact regularity only when the conditions remain absolutely the same, *i.e.,* when the stimulation is produced by the same method, by the same person, and when this person makes the same movements and uses the same object (*i.e.,* the same vessel and the same contents). Consequently, this identity of the conditions relates especially to everything which is connected in one way or another with the act of eating, or with the introduction into the dog's mouth of inedible substances. Fluctuations of other conditions, if they do not call out any additional reactions from the animal, have no significance.

The speea with which the reflex disappears, occurring as a result of repetition, is undoubtedly connected with the length of the interval which separates the consecutive stimulations. The shorter the interval, the more quickly the reflex disappears, and vice versa. Here is an example. The stimulation is again produced by showing the meat powder exactly once every minute. If the stimulation is given every two minutes, the reflex disappears after 15 minutes. If an interval of four minutes between repetitions of the showing of the meat powder is used, the reflex vanishes after 20 minutes; with an 8-minute interval the reflex disappears after 54 minutes; and with a 16-minute interval the reflex does not disappear even after two hours. Again with the stimulation given every two minutes, the reflex vanishes after 18 minutes.

Once the stimulation has disappeared spontaneously, unless special measures are applied it sometimes does not return in less than two hours.

Every change in the details of the conditioned stimulus immediately augments or restores the salivary reaction. If the dog is stimulated by holding meat powder in the hand and raising and lowering the hand constantly during the stimulation, it is only necessary to stop moving the hand in order that the salivary secretion, which, owing to the repetition of the stimulation had already considerably decreased or even entirely ceased, be markedly increased. If a given stimulus ceases to act on repetition, when performed by a certain person, it immediately becomes active again if it is done by some one else.

Reasoning from this fact, it may be foreseen that if a certain conditioned reflex has temporarily ceased to act, owing to repetition, this will not hinder the manifestation of another conditioned reflex. The following example illustrates this:

TABLE II

Time	*Kind of Stimulus (acting for one minute)*	*Quantity of Saliva (cc.)*
1:10	Sight of a glass of extract of quassia	0.8
1:13	" " " " " " "	0.3
1:16	" " " " " " "	0.15
1:19	" " " " " " "	0.0
1:22	" " " " " " "	0.05
1:25	" " " " " " "	0.0
1:28	Sight of Meat Powder	0.7
1:31	" " " "	0.3
1:34	" " " "	0.1
1:37	" " " "	0.05
1:40	" " " "	0.0

As has been shown in the experiments of Dr. Tolochinov, a conditioned reflex which has disappeared due to repetition, may at any time be restored. If a conditioned reflex, for example, meat powder at a distance, has lost its effect owing to repetition, it is necessary only to use the unconditioned reflex on the same meat powder or on some other food, or indeed on any inedible substance, in order to restore the lost conditioned reflex, *i.e.,* with meat powder at a distance. And even more. Other conditioned reflex stimuli whose effects have been lost by repetition, may restore the action of the lost reflex if the newly applied stimuli have considerable strength.

The restoring effect of these interposed reflexes (unconditioned as well as conditioned reflexes) is the greater and surer, the larger the salivary secretion provoked by them (*i.e.,* by the interposed reflexes) Here is an experiment illustrative of this:

TABLE III

Time	*Kind of Stimulus (acting for one minute)*	*Quantity of Saliva (cc.)*
11:34	Sight of Meat Powder	0.7
11:37	" " " "	0.4
11:40	" " " "	0.2
11:43	" " " "	0.05
11:46	" " " "	0.0
	TOTAL	1.35

At 11:49 the stimulation of acid at a distance (conditioned reflex) acting for one minute produces 1.2 cc. of saliva. Then the experiment with meat powder is immediately continued:

TABLE IV

Time	*Kind of Stimulus (acting for one minute)*		*Quantity of Saliva (cc.)*
11:52	Sight of Meat Powder		0.1
11:55	" " " "		0.0
		TOTAL	0.1

At 11:58 the acid is introduced into the mouth of the dog (unconditioned reflex), and produces 3.5 cc. of saliva. The experiment with meat powder is resumed as follows:

TABLE V

Time	*Kind of Stimulus (acting for one minute)*		*Quantity of Saliva (cc.)*
12:02	Sight of Meat Powder		0.4
12:05	" " " "		0.3
12:08	" " " "		0.1
12:11	" " " "		0.0
		TOTAL	0.8

At 12:14 a stronger solution of acid is put into the dog's mouth. It produces 8.0 cc. of saliva. The experiment with meat powder follows:

TABLE VI

Time	*Kind of Stimulus (acting for one minute)*		*Quantity of Saliva (cc.)*
12:20	Sight of Meat Powder		0.7
12:23	" " " "		0.4
12:26	" " " "		0.2
12:29	" " " "		0.15
12:32	" " " "		0.05
12:35	" " " "		0.0
12:38	" " " "		0.0
		TOTAL	1.5

The restoring effect of the interposed reflexes was strongest immediately after their application. The greater the interval between the interposed reflex and the first trial of the conditioned reflex, the weaker was the restoring effect.

The restoring effect of one and the same unconditioned reflex becomes smaller and smaller and finally disappears if it is often repeated. In this case the replacing of one unconditioned reflex by another unconditioned reflex will result in the new unconditioned reflex again restoring the conditioned reflex. In the following example this relation is seen: the dog is given meat powder to eat, and 4.0 cc. of saliva is obtained.

TABLE VII

Time	*Kind of Stimulus (acting for one minute)*	*Quantity of Saliva (cc.)*
11:48	Sight of meat powder	0.8
11:51	" " " "	0.7
11:54	" " " "	0.5
11:57	" " " "	0.3
12:00	" " " "	0.2
12:03	" " " "	0.1
12:06	" " " "	0.0
12:09	" " " "	0.0
	TOTAL	2.6

At 12:10 the dog is given meat powder, and 3.4 cc. is obtained, after which the experiment with the conditioned reflex is continued:

TABLE VIII

Time	*Kind of Stimulus (acting for one minute)*	*Quantity of Saliva (cc.)*
12:14	Sight of meat powder	0.6
12:17	" " " "	0.4
12:20	" " " "	0.1
12:23	" " " "	0.0
12:26	" " " "	0.05
12:29	" " " "	0.0
	TOTAL	1.15

At 12:30 the meat powder is again fed to the dog, and 3.6 cc. of saliva is obtained. The experiment continues:

TABLE IX

Time	*Kind of Stimulus (acting for one minute)*	*Quantity of Saliva (cc.)*
12:34	Sight of meat powder	0.3
12:37	" " " "	0.2
12:40	" " " "	0.0
12:43	" " " "	0.0
	TOTAL	0.5

At 12:44 the dog is given meat powder, and 4.0 cc. of saliva is secreted: the experiment proceeds as follows:

TABLE X

Time	*Kind of Stimulus (acting for one minute)*	*Quantity of Saliva (cc.)*
12:48	Sight of meat powder	0.0
12:51	" " " "	0.0
	TOTAL	0.0

At 12:52, acid is put into the dog's mouth, and 4.9 cc. of saliva is secreted, and the experiment continues thus:

TABLE XI

Time	*Kind of Stimulus (acting for one minute)*	*Quantity of Saliva (cc.)*
12:56	Sight of meat powder	0.7
12:59	" " " "	0.4
1:2	" " " "	0.2
1:5	" " " "	0.1
1:8	" " " "	0.05
1:11	" " " "	0.0
	TOTAL	1.45

But the procedure of using again and again a new unconditioned reflex as a means of restoring the conditioned reflex lost in consequence of repetition, has its limits; a moment comes when further changes of the unconditioned reflex fail to bring about a restoration of the vanished conditioned reflex.

The material given so far comprises only a part of that which has been investigated by Dr. Babkin; we are indebted to him also for experiments demonstrating the rapid disappearance of conditioned reflexes. Previously the experiments of Tolochinov had brought out the fact that during some considerable motor stimulation of the dog, the conditioned reflex becomes weaker or disappears entirely. A general motor stimulation of the dog was produced in the experiments of Babkin either through strong irritation of the eye or of the ear (loud knocking on the door of the dog's experiment room, or an instantaneous flash of bright light into the previously darkened room), or by some entirely new and unusual irritation (playing of a gramophone). For example, one uses the conditioned reflex on meat powder. It displays its full strength. Now one tries on the dog the effect of the above described stimulations. Immediately after these stimulations, the conditioned reflex is without any effect. In the earlier tests as well as in the present, the conditioned reflex is purposely always accompanied by the unconditioned, *i.e.*, after the exhibition of the meat powder, it is given to the dog to eat, in order not to weaken the conditioned reflex. In the second trial after these strong stimulations, there is during the conditioned irritation a certain salivary secretion which is still small, and only with further trials does it begin to grow and gradually reach its normal size.

In this category must be mentioned the following curious fact. In especially greedy dogs having a strong motor reaction at the sight of meat powder, there is often no flow of saliva from the parotid, whereas in less greedy and quieter ones, saliva flows. In the former animals, the secretion of saliva may start from the beginning of the stimulation with the meat powder, at its presentation, but afterwards, with the setting in and increase of the motor reaction, the salivary activity may cease.

The facts of this material are not detached and disconnected, but they form an introduction to the systematic study, the investigation and explanation of the new and complex phenomena which interest us. This new subject is very complicated, and its problems pile high one upon another; but such a complexity does not hinder an exact and ever deeper research. The experiments can be easily systematised. The laboratory results obtained by one worker have been readily confirmed by others on different dogs. It was evident that the way chosen for the study of the complex nervous phenomena was a fortunate one. At every turn one is convinced of the advantages of the objective way. The rapidity with which exact facts have been collected, and the ease of understanding them, presents a striking contrast to the uncertain and contestable facts of the subjective method. In order to make this difference clearer, let us take some examples.

In repeated stimulations by meat powder acting from a distance and not followed by feeding, there is soon a disappearance of the reflex. Why? Thinking subjectively, one would answer thus: the dog becomes convinced of the uselessness of its efforts to obtain the meat powder, and therefore ceases to give it attention. But let us consider the following experiment of Dr. Babkin. When meat powder acting from a distance has lost its effect in consequence of repetitions, the dog is given water to drink. It drinks, but as mentioned above, saliva does not flow. From the subjective point of view, what could one expect regarding the vanished conditioned reflex on meat powder? It might seem that the dog, having received water from the experimenter, would have reason to think that the powder would follow, and that the dog would concentrate his attention on this hope. In reality, however, the reaction on the meat powder remains nil. But now bring the acid before the dog. The acid calls out a salivary secretion, and afterwards meat powder from a distance will again be effective. How can these facts be explained?

From the subjective standpoint it would indeed be difficult.

Showing the dog acid alone, could hardly awaken his hope of getting the meat. The objective observer is content to state the real and concrete relations existing between the phenomena he observes. Consequently, he notices without especial difficulty that everything which produces in more or less degree a salivary secretion, forms the essential condition for the restoration of the vanished reflex.

Yet another example. The conditioned reflex disappears owing to repetition, and is restored of itself only after a considerable lapse of time. Why? From the subjective point of view one can say that the dog has forgotten the deception, owing to the large number of stimulations impinging upon it during this time. One can, however, during this interim, subject the dog to many different influences and stimuli, and the time necessary for restoring the lost conditioned reflex will not be shortened. You need only to bring before the dog some stimulus which calls forth saliva, and the animal immediately forgets the deception.

In this way, the objective investigation of those biological phenomena of the animal commonly called psychical, becomes a direct continuation and widening of physiological experimentation on the living organism, and the facts thus gathered and systematised must be treated from the physiological standpoint exclusively, if they are to form the basis for our conception of the properties and relations between the different parts of the nervous system. And by varying and repeating our experiments in which one or another part of the nervous system is excluded—now the central, now the peripheral—this conception will correspond closer and closer to reality.

Concerning this last experimental method, I shall give an example. On the basis of the facts given above, it is necessary to admit that every conditioned reflex arises because of the presence of an unconditioned one. A conditioned reflex forms even though the conditioned stimulus and the unconditioned stimulus have coincided in their time of action only once, and it disappears if this coincidence does not occur for a long time. The justification of such a relation for old and firmly established conditioned reflexes is of great interest, and has been made a subject of investigation in my laboratory by Dr. Zelheim.* These experiments were performed by Dr. Snarsky * before but they were not then sufficiently analysed. In Zelheim's experiments a series of conditioned and unconditioned reflexes to both food and non-food substances were formed, first in a normal dog. The lingual and glossopharyngeal nerves were then cut on both sides. When the animal had entirely recovered from the operation, all the elaborated reflexes were repeated. On the first trials there seemed to be no difference from the normal state; the salivary reaction had

* A. P. Zelheim: *Dissertation,* 1904, St. Petersburg; Snarsky: *Dissertation,* 1902, St. Petersburg.

almost the same strength as formerly, on both the presentation of objects from a distance, and their introduction into the dog's mouth. With the repetition of these experiments, however, we noticed that the reflexes to certain substances, such as an extract of quassia and saccharine, as well as a dilute solution of hydrochloric acid and sodium chloride, became gradually weaker. As the unconditioned reflex is characterised by its constancy on repetition, we are forced to the conclusion that for certain stimuli, the unconditioned reflex disappeared, and that the effect which remained after the operation was dependent upon the conditioned reflex, the more so because now the secretory effect of both stimuli was almost equal, no matter whether they were applied from a distance or brought into the mouth of the dog. On the repetition of these experiments after two weeks, the reflex to bitter substances entirely disappeared in both forms (its action from a distance and in the mouth), but the reflex to saccharine, acid and salt remained, though they were much weaker. If is plain that these last substances excited not only the special chemical fibres which were cut through, but other centripetal nerves responsible for the conduction of the remaining unconditioned reflexes.

Of great interest is the following question: what is the essential unconditioned stimulus of the food substances? The facts hitherto collected are not sufficient for the solution of this problem. In acute experiments by Dr. Heimann in my laboratory, it was shown that in animals which had been poisoned and immediately operated on, the chemical properties of the food substances, when they are brought into the mouth cavity, are entirely without effect on the salivary secretion. In these experiments, much more than in any other acute experiments, numerous defects were found in the acute experiment as an experimental method, and therefore the work of Dr. Heimann must be repeated and controlled. Dr. Zelheim, in his above-mentioned experiments with permanent salivary fistulæ, could not note any difference in the salivary secretion during the feeding of the animal before and after operation.

Now that I have explained this new material relating to the physiology of the salivary glands, it may not be superfluous if I revert to a most important point in the physiological significance of these phenomena. Surely these phenomena are much more complicated than we have described them here. But thanks to our new scheme, we are enabled to go forward in the exploration of our subject. This then is the meaning and justification of our plan.

The designation of "reflex" which we have given to these "complex nervous phenomena" is entirely logical. The phenomena are always the result of the stimulation of the peripheral endings of various centripetal nerves, and this stimulation spreads through the centrifugal nerves to the salivary glands.

These reflexes are, as are all natural reflexes, strictly specific (and therefore unlike the artificial reactions which are often produced in the laboratory by artificial stimulation), and they are the expression of a definite reaction of the organism, or of one or another of its organs, to a certain stimulus.

These new reflexes are the function of the highest structure of the nervous system of the animal, and they must be explained on the following basis. First, they represent the most complicated phenomena among nervous functions, and consequently they must be connected with the highest parts of the nervous system. Reasoning further from animal experiments with various poisonings or with total or partial extirpation of the cerebral hemispheres, we can conclude that the conditioned reflex demands for its formation the assistance of the hemispheres.

The reflexes are temporary and conditional, and these qualities characterise and separate them from the old simple reflexes with which physiology has concerned itself in the past. Their temporary character manifests itself in two ways: they can be formed when they did not previously exist, and they may disappear again forever; besides this, when they exist, they often fluctuate in degree even to vanishing, either for a short time, or under certain circumstances, permanently. As we have seen, their formation and extinction are determined by (one or several) coincidences in time of stimulation of the lower lying reflex centres, which govern some functioning organ, with the stimulation of different points of the cerebral hemispheres through the corresponding centripetal nerves. If the stimulation of these two centres coincides many times, then the paths leading from the higher to the lower centres become more and more passable, and the conduction of the excitations along them becomes easier and easier. When these coincidences occur more rarely, or cease altogether, the paths again become less permeable, and finally impassable.

What physiological explanation can be given for the rapid, and unfailing, though temporary, disappearance of a conditioned reflex, when it is repeated alone for several times at short intervals without the support of the unconditioned stimulus with which it was formed? Certain facts indicate, I think, that this event belongs to the category of exhaustion phenomena. First, the vanished conditioned reflex, if left alone without any stimulation from the experimenter, reappears after a certain time. Secondly, the disappearance of the conditioned reflex due to repetition occurs the more quickly, the smaller the interval between repetitions, and vice versa. Such an explanation would agree with the generally accepted opinion concerning the rapid exhaustion of the higher nervous centres brought about by repetition of monotonous stimulations.

The possibility of restoring a conditioned reflex, which has disappeared owing to repetition, and which is determined by the application of the corresponding unconditioned or other conditioned reflex of sufficient strength can be thus explained: that, in spite of a certain degree of exhaustion in the higher nervous centre, its stimulation again penetrates to the lower lying salivary centre, and from that very moment the paths leading to this centre become especially permeable owing to the recent and especially strong stimulation. In favour of this explanation we may cite the above-mentioned experiments on the restoration of the vanished conditioned reflex by means of repeated feedings, though these feedings finally, however, lose their effect.

At the end of that experiment we had a fact which revealed the mechanism of this process as a very complicated one. When the repetition of feeding had lost its efficacy as a means of restoring the reflex, the introduction of acid into the mouth of the dog to assist this restoration was accompanied by a positive effect. Therefore we must bring new elements into our scheme of explanation. If, however, these experiments are continued, there finally comes a moment, whether or not the unconditioned reflexes are varied, when neither of these stimuli is effective, and when the conditioned reflex is restored *per se* only at the cost of a great interval of time.

It is evident that for a full decision of the suggested problems, further investigations are required.

In conclusion, we must count it an incontestable fact that the physiology of the highest parts of the central nervous system of higher animals can not be successfully studied, unless one utterly renounces the indefinite presentations of psychology, and stands upon a purely objective ground. What interest, for example, can there be for physiological analysis in the statements of some authors, that after extirpation of certain parts of the cerebral hemispheres, the animals become now fiercer, now gentler, less intelligent, etc., when these terms themselves represent very complicated conceptions requiring an exact scientific analysis?

CHAPTER III

THE FIRST SURE STEPS ALONG THE PATH OF A NEW INVESTIGATION [1]

(Read by the author on the occasion of his receiving the Nobel Prize, 1904.)

EFFECT OF APPETITE—PHYSIOLOGICAL EXPLANATION OF SOME "PSYCHICAL" FACTS—CONSTANCY OF THE REACTIONS ILLUSTRATED BY EXAMPLES—DIFFERENCES BETWEEN CONDITIONED AND UNCONDITIONED REFLEXES—SIGNALS—OUR PSYCHICAL CONTENT REMAINS A MYSTERY.

DURING the study of the gastric glands, I became more and more convinced, that the appetite acts not only as a general stimulus of the glands, but that it stimulates them in different degrees according to the object upon which it is directed. For the salivary glands the rule obtains that all the variations of their activity observed in physiological experiments are exactly duplicated in the experiments using a psychical stimulation, *i.e.,* in those experiments in which the stimulus is not brought into direct contact with the mucous membrane of the mouth, but attracts the attention of the animal from some distance. Here are examples of this. The sight of dry bread calls out a stronger salivary secretion than the sight of meat, although the meat, judging by the movement of the animals, excites a much livelier interest. On teasing the dog with meat or other foods, there flows from the submaxillary glands a concentrated saliva rich in mucus (lubricating saliva) ; on the contrary, the sight of a disagreeable substance produces from these same glands a secretion of very fluid saliva which contains almost no mucus (cleansing saliva). In brief, the experiments with psychical stimuli represent exact miniatures of the experiments with physiological stimulations by the same substances.

Thus, psychology, in relation to the work of the salivary glands, occupies a place close to that of physiology. And even more! On first view the psychological explanation of the activity of the salivary glands seems to be as incontrovertible as the physiological. When any object from a distance attracting the attention of the dog produces a flow of saliva, one has ground for assuming that this is a psychical and not a physiological phenomenon. When, however, after the dog has eaten something or has had something forced into his mouth, saliva flows, it is necessary to prove that in this phenomenon there is actually present a physiological cause, and not only a purely psychical one which, owing

[1] In 1904 Prof. Pavlov was awarded the Nobel Prize for his investigation on the digestive glands. In his lecture delivered on that occasion in Stockholm (the Nobel *Vortrag*), he gave a résumé of his principal new achievements.—*Translator.*

to the special conditions, is perhaps reinforced. From the following experiment this conception is seen to correspond in a remarkable way with reality. Most substances which during eating or forceful introduction into the mouth produce a flow of saliva, evoke a secretion after severance of all the sensory nerves of the tongue similar to that which they evoked before this operation. One must resort to more radical measures, such as poisoning of the animal or extirpation of the higher parts of the central nervous system, in order to convince oneself that between a substance stimulating the oral cavity and the salivary glands there exists not only a psychical but a purely physiological connection. Thus we have two series of apparently entirely different phenomena. How must the physiologist treat these psychical phenomena? It is impossible to neglect them, because they are closely bound up with purely physiological phenomena and determine the work of the whole organ. If the physiologist decides to study them, he must answer the question, How?

Following the examples of the study of the lowest representatives of the animal kingdom, and naturally not desiring to abandon physiology for psychology—especially after an entirely unsuccessful trial in this direction—we chose to maintain in our experiments with the so-called psychical phenomena a purely objective position. Above all, we endeavoured to discipline our thoughts and our speech [2] about these phenomena, and not to concern ourselves with the imaginary mental state of the animal; and we limited our task to exact observation and description of the effect on the secretion of the salivary glands of the object acting from a distance. The results corresponded to our expectations—the relations we observed between the external phenomena and the variations in the work of the salivary glands appeared quite regularly, could be reproduced at will again and again as usual physiological phenomena, and were capable of being definitely systematised. To our great joy, we are convinced that we have started along the path which leads to a successful goal. I shall give some examples of the constant relations which have been established by the aid of this new method of research.

If the dog is repeatedly excited by the sight of substances calling forth a salivary secretion from a distance, the reaction of the salivary glands after each stimulation becomes weaker and weaker, and finally falls to zero.[3] The shorter the intervals between separate stimulations, the quicker the reaction reaches zero, and vice versa. These rules are fully manifested only when the conditions of the experiment do not change. The identity

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[2] It should now be evident to every one how important it was to adopt a new terminology for all the phenomena observed in order to get rid of the former psychical suggestions and their associations, and to introduce a purely physiological conception of all the facts.—*Translator.*

[3] For an illustration of this phenomenon see the record of the first experiment in chapter ii.—*Translator.*

of the conditions, however, need be only relative; it may be limited to those phenomena of the outer world with which had been associated the acts of eating or the forceful introduction of the corresponding substances into the animal's mouth; the variation of other conditions may remain without any effect. This relative identity can be easily attained by the experimenter, so that an experiment in which a stimulus is repeatedly applied from a distance gradually loses its effect, can be readily demonstrated in the lecture hall. If a substance, owing to its repeated employment as a distant stimulus, has become ineffective, the influence of other stimulating substances is not thereby annihilated: if milk from a distance ceases to stimulate the salivary glands, the distant action of bread remains clearly effective. After this has lost its influence by repetition, showing the dog acid will produce again a full effect on the salivary glands. These relations also explain the real meaning of the above-mentioned identity of the experimental conditions; every detail of the surrounding objects appears as a new stimulus. When a certain stimulus has lost its efficacy due to repetition, then its action after a certain interval of minutes or of hours is restored without fail.

The effect when temporarily lost, can be restored at any given time, however, by special means. If bread repeatedly shown to the dog fails to stimulate the salivary glands, it is only necessary to give it to the dog to eat and thereupon the full effect of the bread at a distance is at once restored. The same result is obtained when the dog receives some other food. And even more. When some substance producing a salivary secretion, for example, acid, is forced into the dog's mouth, the original distant effect of the bread previously lost is again fully manifested. In general, everything that stimulates the salivary glands restores the lost reaction, and the more fully, the greater has been their activity.

Our reaction can be inhibited by certain influences with the same regularity; if, for example, some stimulus which evokes in the animal a definite motor reaction acts on the eye or ear of the dog.

For the sake of brevity, I shall limit myself to the above mentioned material, and now pass on to theoretical considerations of the experiments. Our given facts can readily be included in a framework of physiological description. The effects we produced on the salivary glands from a distance may properly be considered and termed reflexes. It is impossible not to see, by close attention, that the activity of the salivary glands, when present, is always excited by some external phenomenon; *i.e.,* in the same way as the usual physiological salivary reflex, it is always produced by an external stimulus. The difference consists chiefly in that the usual reflex is determined by the stimulation from the mouth cavity, whereas the new reflexes are evoked by stimulation of the eye, ear, etc. A further essential difference between the old and the new reflexes is that the former are constant and unconditional, while the latter are subject to fluctuation, and dependent upon many conditions. They, therefore, deserve the name of "conditioned."[4]

Considering the phenomena more closely, I can not fail to see the following distinction between these two kinds of reflexes: in the *unconditioned* reflex, those properties of the substance to which the saliva is physiologically adapted act as the stimulus, for example, the hardness, the dryness, the definite chemical properties, etc.; in the *conditioned* reflex, on the other hand, those properties which bear no direct relation to the physiological rôle of the saliva act as stimuli, for example, colour, form, and the like. These last properties evidently receive their physiological importance as *signals*[5] for the first ones, *i.e.,* for the essential properties. In their response one can not but notice a further and more delicate adaptation of the salivary glands to the external world. This is seen in the following case. We prepare to put acid into the dog's mouth, and the dog sees it. In the interest of the integrity of the buccal mucous membrane, it is highly desirable that before the acid comes into the mouth, there should be some saliva present; on the one hand, the saliva will hinder the direct contact of the acid with the mucous membrane, and, on the other hand, will serve to dilute the acid and thus weaken its injurious effect. But, of course, in reality the signals can have only a conditional significance, they are readily subject to change, as, for example, when the signalling objects do not come into contact with the mucous membrane. In this way the finer adaptation is based on the fact that the properties of the substances which serve as signals, now stimulate (*i.e.,* call out the reflex), now lose their exciting action. This is what occurs in reality. Any given phenomenon can be made a temporary signal of the object which stimulates the salivary glands, if the stimulation of the mucous membrane by the object has been once or several times associated simultaneously with the action of the stimulating phenomenon on another receptor surface of the body. We are now trying in our laboratory, with great success, to apply many such, and even highly paradoxical, combinations.

On the other hand, closely related and stable signals can be deprived of their stimulating action if they are often repeated without bringing the corresponding object into contact with the mucous membrane. If any food is shown to a dog for days or weeks without giving it to the

[4] Conditional (*ooslovny*) and not conditioned is Prof. Pavlov's term, but as *conditioned reflex* has become fixed in English usage instead of conditional reflex, we adhere to the term conditioned. In French and German translation, Prof. Pavlov's original term (conditional) has been preserved.—*Translator.*

[5] The conditioned stimuli provoking the conditioned reflexes are described here for the first time as *signals* of the qualities calling forth the salivary secretion.—*Translator.*

animal it finally completely loses its distant stimulating effect on the salivary glands. The mechanism of the stimulation of the salivary glands through the signalising properties of objects, *i.e.,* the mechanism of the "*conditioned stimulation,*" may be easily conceived of from the physiological point of view as a function of the nervous system. As we have just said, at the basis of each conditioned reflex, *i.e.,* a stimulation through the signallising properties of an object, there lies an unconditioned reflex, *i.e.,* a stimulation through the essential attributes of the object. Then it must be assumed that the point of the central nervous system which during the unconditioned reflex becomes strongly stimulated, attracts to itself weaker impulses arriving simultaneously from the outer or internal worlds at other points of this system, *i.e.,* thanks to the unconditioned reflex, there is opened for all these stimulations a temporary path leading to the point of this reaction. The circumstances influencing the opening or closing of this path in the brain are the internal mechanism of the action or of the inaction of the signallising properties of the objects, and they represent the physiological basis of the finest reactivity of the living substance, the most delicate adaptation of the animal organism, to the outer world.[6]

I desire to express my deep conviction that physiological research in the direction which I have briefly outlined, will be highly successful and will help us to make great advances.

Only one thing in life is of actual interest for us—our psychical experience. Its mechanism, however, has been, and remains, wrapped in deep mystery. All human resources—art, religion, literature, philosophy, historical science—all these unite to cast a beam of light into this mysterious darkness. Man has at his disposal one more powerful ally—biological science with its strictly objective methods. This study, as we all see and know, is making great advances every day. The facts and conceptions which I have given at the close of this lecture are typical of numerous trials to make use of systematic application of a purely naturalistic method of thinking in the study of the mechanism of the highest vital expression of the dog—this faithful and friendly representative of the animal world.[7]

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[6] This description of conditioned reflexes is practically the same as that given in chapter i (see footnote 7). The conditioned stimuli were admitted to be signals, arising from the simultaneous excitation by the important unconditioned and the unimportant conditioned stimuli, and the next logical test of this suggestion was its realisation. In the preceding chapter Prof. Pavlov states that such experiments have been performed successfully. A detailed description of these experiments will be found in the next lecture delivered about two years after the present one. *Cf.* footnote 3 of the next chapter.—*Translator.*

[7] Here one may notice that Prof. Pavlov intends to give an experimental basis for the comprehension of our psychical experience by investigating the mechanism of this complicated process in a higher animal—the dog.—*Translator.*

CHAPTER IV

SCIENTIFIC STUDY OF THE SO-CALLED PSYCHICAL PROCESSES IN THE HIGHER ANIMALS[1]

(Read in honour of Thomas Huxley, at the Charing Cross Medical School, London, October 1, 1906.)

ATTITUDE OF THE INVESTIGATOR—THE UNCONDITIONED SALIVARY REFLEX—THE NEW CONDITIONED REFLEX—RESULTS OF THE EXPERIMENTAL STUDY OF THE CONDITIONED SALIVARY REFLEX—REACTION OF WARNING AND SIGNALS—ELEMENTARY NATURE OF THE CONDITIONED REFLEX—CONDITIONS NECESSARY FOR ITS FORMATION—SIZE OF CONDITIONED REFLEX DEPENDENT ON INTENSITY OF STIMULUS—COMPLEX CONDITIONED STIMULI—CONDITIONED REFLEXES ARE LAW-OBEYING AND THEY CAN BE STUDIED OBJECTIVELY—MEDICINE AND PHYSIOLOGY.

THE subject of to-day's address, delivered in honour of Thomas Huxley, an eminent representative of natural science and a most energetic champion of that greatest biological principle (the doctrine of evolution), is the naturalistic investigation of the psychical processes in the higher animal.

I shall begin with an actual case which occurred in my laboratory a few years ago. Among my collaborators was a young doctor with an active mind capable of appreciating the joys and triumphs of investigation. Great was my astonishment when this loyal friend of science became profoundly disturbed on hearing of our plans to investigate the psychical activity of the dog in that same laboratory and by the same means which we had been using for the solution of physiological questions. All of our arguments were ineffective; he prophesied and hoped for only failure. The cause of this, as far as we could understand, was his idea that the psychical life of man and that of the higher animals was so individual and exalted that it not only did not lend itself to investigation, but would even be sullied by our rude physiological methods. Although this, gentlemen, may have been a somewhat exaggerated example, I believe it is characteristic and typical. In dealing with the highest vital phenomena, the fact must not be overlooked, that a systematic appreciation of natural science to the last limits of life will not

[1] In the expression "the so-called psychical process" the reader will see that the author has taken a new position. Now he considers all of his results as purely physiological. And he rejects definitely the further possibility of investigating subjective states. He begins to bring out a mass of evidence in favour of the purely physiological nature of his data. The artificial conditioned reflex treated of in this chapter strengthened his view, and gave new possibilities for thorough experimentation.—*Translator.*

be able to avoid misconception and opposition from those who are accustomed to regard these phenomena from another point of view and are convinced that this point of view is unassailable.

This is why I feel it obligatory first to explain exactly and clearly my point of view concerning the psychical activities of the higher animals, and secondly to pass from the preliminaries to the subject itself as soon as possible. I have referred intentionally to psychical activities as "so-called". If the naturalist hopes to make a complete analysis of the activity of the higher animals, he has not the right to speak of the psychical processes of these animals, and he can not so speak without deserting the principles of natural science. This is natural science—the work of the human mind applied to nature, and the investigation of nature without any kind of assumption or explanation from sources other than nature itself. Were the investigator to speak of the psychical faculties of the higher animals, he would be transferring ideas from his own inner world to nature, repeating the procedure of his predecessors who were accustomed, on observing nature, to apply to its inanimate phenomena their own thoughts, wishes and sensations. The naturalist must consider only one thing: what is the relation of this or that external reaction of the animal to the phenomena of the external world? This response may be extremely complicated in comparison with the reactions of lower animals, and infinitely complicated in comparison with the reaction of any inanimate object, but the principle involved remains the same.

Strictly speaking, natural science is under obligation to determine only the precise connection which exists between a given natural phenomenon and the response of the living organism to that phenomenon, or, in other words, to ascertain completely how a living being maintains itself in constant equilibrium with its environment. This assertion can hardly be contested, and is further supported by the fact that it receives daily more and more general acceptance in the investigation of the lower and intermediate stages of the zoological scale. The question is simply whether this rule is already applicable to the examination of the higher functions of the higher vetebrates. A serious endeavour to institute enquiries in that direction is, as it appears to me, the only reasonable answer to the question. I and my many collaborators began this work some years ago and we have recently devoted ourselves to it almost exclusively. I would now ask your attention to an account, first, of the most important results of this enquiry, which seem to me to be very instructive; and secondly, to an account of the inferences which may be drawn from it.[2]

[2] The hope stated in the next to the last paragraph of chapter i, as will be seen from the fourth paragraph from the end of this chapter, is now far in the future.

Our experiments have been performed exclusively on the dog, in which the particular reaction used was an unimportant physiologic process—the secretion of saliva. The experimenter was always working with a perfectly normal animal, *i.e.*, an animal which was not subjected to abnormal influences during the experiment. Exact observations on the work of the salivary glands could be made at any moment by means of a simple method. Saliva flows, as we all know, when something is given the dog to eat or is introduced forcibly into his mouth. Both the quality and the quantity of the saliva, under these conditions, is strictly dependent upon the quality and quantity of the substances brought into the dog's mouth. In this well-known physiological process we have before us a reflex. The idea of reflex action as a special elementary function of the nervous system is an old and established truism of physiology. It is the reaction of the organism to the external world, effected through the nervous system, by which an external stimulus is transformed into a nervous process and transmitted along a circuitous route (from the peripheral endings of the centripetal nerve, along its fibres to the apparatus of the central nervous system, and out along the centrifugal path until, reaching one or another organ, it excites its activity). This reaction is specific and permanent. Its specificity is a manifestation of a close and peculiar relation of the external phenomenon to the physiological action, and is founded on the specific sensibility of the peripheral nerve endings in the given nervous chain. These specific reflex actions in normal life, or to state it much more accurately, in the absence of abnormal vital conditions, are constant and unchanging.

The responses of the salivary glands to external influences are, however, not limited by the above-mentioned ordinary reflex actions. We all know that the salivary glands begin to secrete, not only when the stimulus of appropriate substances is impressed on the interior surface of the mouth, but that they also often begin to secrete when other receptive surfaces, including the eye and the ear, are stimulated. The actions last mentioned are, however, generally considered apart from physiology and receive the name of psychical stimuli.

We shall take another course, and shall endeavour to restore to physiology what properly belongs to it. These exceptional manifestations unquestionably have much in common with ordinary reflex action. Every time that such a flow of saliva begins it is attributable to the occurrence of some special stimulus among the external influences that may be

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Now the author disputes the right of psychology to handle those facts which are easily and successfully accessible to physiological investigation. As to the relation between his own work and psychology, his point of view is clearly expressed in the beginning of chapter xxiii.—*Translator.*

recognised. On very careful exercise of his attention the observer perceives that the number of spontaneous flows of saliva forms a rapidly diminishing series, and it is in the highest degree probable that those extremely infrequent flows of saliva, for which no particular cause is at first sight apparent, are, in reality, the result of some stimulus invisible to the eye of the observer. From this it follows that the centripetal paths are always stimulated primarily, and the centrifugal paths secondarily, with the interposition, of course, of the central nervous system. Now these are actually all the elements of a reflex action, the only missing points being exact data as to the operation of the stimulus in the central nervous system. Are we familiar with this last mechanism in the ordinary reflexes? Speaking generally, then, our phenomena are reflexes, but the difference between these newly recognised reflexes and the long-known ones is certainly immense, for they have been assigned to quite different departments of science. Physiology has, therefore, before it the problem of evaluating this difference experimentally, and of establishing the essential properties of the reflexes which have been newly recognized.

In the first place they arise from all the body surfaces which are sensitive to stimulation, even from such regions as the eye and the ear, from which an ordinary reflex action affecting the salivary glands is never known to proceed. It must be mentioned that usual salivary reflexes may originate not only from the cavity of the mouth but also from the skin and the nasal cavity; the skin, however, produces this effect only when it is subjected to some destructive process such as cutting or erosion by caustics, while the nasal cavity produces this effect only through the contact of vapours or gases, such as ammonia, which cause local irritation, but never through the agency of usual odours. In the second place, a conspicuous feature of these reflexes is that they are in the highest degree inconstant. All stimuli applied to the mouth of the dog unfailingly give a positive result wtih reference to the secretion of saliva, but the same objects when presented to the eye, the ear, etc., may sometimes be efficient and sometimes not. In consequence only of the last-mentioned fact have we provisionally called the new reflexes "conditioned reflexes," and for the sake of distinction we have called the old ones "unconditioned".

The further question naturally arose whether the conditions which determine the occurrence of the "conditioned reflexes" could be investigated, and whether a complete knowledge of the conditions would make it possible to impart to these reflexes a character of constancy. This question must be regarded, it seems to me, as answered in the affirmative. I will remind you of some well-established laws which have already been published from our laboratory. Every conditioned stimulus becomes totally ineffective on repetition.[3] The shorter the interval between the separate repetitions of the conditioned reflex the more quickly is this reflex extinguished. The extinguishing of one conditioned reflex does not affect the operation of the others. Spontaneous restoration of extinguished conditioned reflexes does not occur until after the lapse of one, two, or more hours, but there is a way in which our reflex may be restored immediately. All that is necessary is to obtain a repetition of the *un*conditioned reflex, as, for instance, by pouring a weak solution of acid into the dog's mouth and then either showing it to him or letting him smell it. The action of the last-mentioned stimulus, which was previously quite obliterated, is now restored in its full extent. The following fact can be regularly observed: If for a long time, such as days or weeks continuously, a certain kind of food is shown to the animal without it being given to him to eat, it loses its power of stimulating from a distance,* that is, its power of acting from the eye, the nose, etc. These last-mentioned facts show plainly the close connection which exists between the stimulant effects of various properties of the substance—namely, the effects of the properties which excite secretion of saliva when the substance is in the mouth—and the effects of other properties of the same substance acting upon other receptive surfaces of the body. This material gives us the ground for assuming that the conditioned reflex in some way originates owing to the existence of the unconditioned reflex. And at the same time we may perceive the main features of the mechanism which gives rise to the conditioned reflex. When an object is placed in the mouth of the dog, some of its properties excite the simple reflex apparatus of the salivary glands; and for the production of our conditioned reflex that action must synchronise with the action of other properties of the same object influencing other receptive regions of the body whence the excitation is conveyed to other parts of the central nervous system. Just as the stimulant effects due to certain properties of an object placed in the mouth (unconditioned reflex) may coincide with a number of stimuli arising from other objects, so all these manifold stimuli may by frequent repetition be turned into conditioned stimuli for the salivary glands. Such stimuli may arise from the man who feeds the dog or who forcibly introduces certain articles into the dog's mouth, or they may owe their origin to the general environment in which this takes place. For this reason the above-mentioned experiments, by which the laws of the conditioned reflexes must be determined, require for their performance a well-trained experimenter who can really investigate only the action of the given conditioned stimulus or a definite

[3] For an illustration of this phenomenon see record of the first experiment in chapter ii.—*Translator.*

* Experiments made by Tolochinov and Babkin.

number of such stimuli, without unconsciously introducing new stimuli with each successive repetition. If this last condition is not realised the laws in question will naturally be obscured. It must be remembered that in feeding a dog or forcing something into his mouth, each separate movement and each variation of a movement may by itself represent a conditioned stimulus. If that is the case, and if our hypothesis as to the origin of the conditioned reflex is correct, it follows that any natural phenomenon chosen at will may be converted into a conditioned stimulus. This has, in effect, been proved to be true. Any visual stimulus, any desired sound, any odour, and the stimulation of any part of the skin, either by mechanical means or by the application of heat or cold, have never failed in our hands to stimulate the salivary glands, although before they were all ineffective for that purpose. This was accomplished by applying the stimuli simultaneously with the action of the salivary glands, their action having been evoked by the giving of certain kinds of food, or by forcing certain substances into the dog's mouth. These artificial conditioned reflexes,[4] the product of our training, showed exactly the same properties as the previously described natural conditioned reflexes. As regards their extinction and restoration they followed essentially the same laws as the natural conditioned reflexes.[5] Thus we have the right to say, that our analysis of the origin of conditioned reflexes is proved by the facts.*

[4] The possibility of establishing these artificial conditioned reflexes was of great importance. First of all it proved that the circumstances which give rise to the conditioned reflex, as mentioned in chapter iii, footnote 4, are adequately explained by the proposed theory. Besides this, the intentional formation of conditioned reflexes made possible a more thorough and conscious experimentation.—*Translator.*

[5] Professor Pavlov says (*Activity of the Cerebral Hemispheres* (Russian) p. 49): "Formerly we made a distinction between 'natural' and 'artificial' conditioned reflexes; 'natural' reflexes being those which appeared to be formed spontaneously as a result of the natural association of, for example, the sight and smell of food with the eating of food itself, or of the procedure of introducing acid or some rejectable substances with the acid or the rejectable substance itself, while 'artificial' reflexes were those which could be formed as a result of artificially associating with the food or rejectable substance, stimuli, which in the ordinary course of events, have nothing in common with food or the rejectable substance. At the present time, however, we know that there is not the slightest difference in properties between all these reflexes. I mention this fact here because the numerous experiments of the earlier period of our work were carried out with the 'natural' conditioned reflexes, and it is from these that I shall draw many examples in the present lecture. All the numerous artificial stimuli which we now use every day in our experiments were important to us at the time of those experiments because they provided easily controlled, exact, and regularly reproducible stimuli, and because they could be applied to check the correctness of our conception of the mechanism by which natural conditioned reflexes are formed. At present the artificial stimuli predominate in importance because of the vast field of research they have unfolded to us and because they came ultimately to provide the most important material for our investigation."

* Experiments made by Boldirev, Kacherininova and Voskoboinikova-Granstrem.

Now that so much has been adduced on the subject we may advance further than was possible at the outset in the understanding of conditioned reflexes. In the manifestations of nervous energy which have up to the present time been submitted to careful scientific examination (our old specific reflex), the stimuli with which we had to do were comparatively few in number, but very constant in their action, and there was abundant evidence of a constant connection existing between the external influences and definite physiological effects. Now, however, in another more complicated part of the nervous system we encounter a new phenomenon, namely, the conditioned stimulus. On the one hand, this nervous apparatus becomes responsible in the highest degree, *i.e.*, it is susceptible to the most varied external stimuli, but, on the other hand, these stimuli are not constant in their operation and are not definitely associated with certain physiological effects. At any given moment we find comparatively few circumstances favourable for these stimuli becoming active in the organism for a longer or shorter time and producing distinct physiological results.

The introduction of the idea of conditioned stimuli into physiology seems to me to be justified for many reasons. In the first place, it corresponds to the facts that have been adduced, since it represents a direct inference from them. In the second place, it is in agreement with the general mechanical hypotheses of natural science. In many kinds of apparatus and machinery, even of simple construction, certain forces can not develop their action unless at the proper time the necessary conditions are present. In the third place, it is completely covered by the ideas of facilitation (*Bahnung*) and inhibition, ideas which have been sufficiently elaborated in recent physiological literature. Finally, in these conditioned stimuli, looked at from the point of view of general biology, we have a most perfect mechanism of adaptation, or, what amounts to the same thing, a very delicate mechanism for maintaining an equilibrium with the surrounding medium. The body has the capacity to react in a sensitive way to the phenomena of the outer world which are essential to it, because all other phenomena of the outer world, even the most insignificant, coinciding even temporarily with the essential become their indicators or, as they may be called, their signalling stimuli. The delicacy of the reaction shows itself both in the production of the conditioned stimulus and in its disappearance when it ceases to be a proper signal. There must be assumed to exist at this point one of the chief mechanisms for further discrimination in the nervous system. In view of all this, it is permissible, I think, to regard the idea of conditioned stimuli as the fruit of previous labours of biologists, and to consider my present report as illustrating the result of this work on a most complicated subject. It would be unreasonable to attempt to determine at present the limits of the immense field thus opened and to partition it. The following must be regarded as, and nothing more than, a provisional arrangement of material that has been collected, giving only the points indispensable for purposes of explanation.

There are reasons for considering the process of the conditioned reflex to be elementary, namely, a process which consists only in the coincidence of any one of the innumerable indifferent external stimuli with a state of excitation of a point in a definite part of the central nervous system. Now a path is established between the former indifferent stimulus and this given point. The first argument in favour of this hypothesis is the repeated occurrence of this phenomenon: the conditioned reflex may be obtained in all dogs, and it may be produced by all imaginable stimuli. In the second place, there is the certainty of its occurrence; under definite conditions it is reproduced inevitably. We see, therefore, that the process is not complicated by any other (and unknown) conditions. It may here be mentioned that various conditioned stimuli which had been rendered effective were applied at a distance, as from another room; the experimenter, who for the purpose of obtaining the conditioned reflex usually either gave the dog something to eat or put a substance of some kind into the dog's mouth, was not now in close proximity to the animal, but the result of the stimuli was, nevertheless, the same.

It has already been stated that every imaginable phenomenon of the outer world affecting a specific receptive surface of the body may be converted into a conditioned stimulus. After conditioned reflexes had been obtained from the eye, the ear, the nose, and the skin, it was a matter of interest to know what relation the cavity of the mouth had to the general question, and whether a conditioned reflex originated in the mouth. The answer to the inquiry could not be a simple one, because in this case not only the receptive surfaces for the stimuli of the conditioned and of the unconditioned reflex, but also the stimuli themselves were all brought together. Careful observations, however, have made it possible to separate the conditioned stimulus from the unconditioned stimulus even in this instance. When inedible, irritant substances were many times in succession forcibly introduced into the dog's mouth, we could observe the following facts:

If, for instance, a certain amount of acid was poured into the dog's mouth many times in succession, on each fresh repetition of this procedure, there was regularly a greater flow of saliva; the same thing was repeated on a series of successive days until a certain maximum was attained, whereupon for a considerable time the secretion remained constant. If the experiments were stopped for some days the quantity of saliva secreted became much less. This fact could be very simply explained as follows. On the first administration of the acid solution the secretion of saliva depended principally, or even exclusively, on the unconditioned reflex which the acid caused, while the subsequently occurring increase in secretion pointed to a conditioned reflex gradually formed under the influence of the same acid, and having as its receptor surface also the mouth cavity.*

We will now consider the conditions which determine the formation of conditioned reflexes. This question taken comprehensively is naturally a vast one. The following account will serve to give you only a slight idea of the full compass of this vast subject.

Although there are great differences in the time required for the establishing of a conditioned reflex, some relations have been seen to exist. From our experiments it is evident that the intensity of the stimulus is of essential importance. We have some dogs in which the cooling or warming of a definite place on the skin acted as a conditioned stimulus for the salivary glands. A temperature of zero or 1° C. in an experiment repeated 20 or 30 times caused saliva to flow, whilst a temperature of 4° or 5° C. in an experiment repeated 100 times gave no effect whatever. Exactly the same thing occurs with high temperatures. Heat of 45° C. applied as a conditioned stimulus showed similarly no action after even 100 applications; a temperature of 50°, on the other hand, caused a secretion of saliva after from 20 to 30 applications.** In contradistinction to this we must state with regard to acoustic stimuli that very loud sounds such as the violent ringing of a bell did not, in comparison with weaker stimuli, quickly establish a conditioned reflex. It can be assumed that powerful acoustic stimuli call out some other important reaction in the body (*e.g.,* motor), which hinders the development of the salivary response.

There is another group of related phenomena which deserves mention. When an odour not naturally exciting the salivary reflex—that of camphor, for instance—is by means of a special apparatus diffused, this diffusion of the odour must be made to coincide 10 or 20 times with the action of the unconditioned stimulus, such as acid poured into the dog's mouth. But if some of the odouriferous material is added to the acid, the new odour after one or two administrations acts as a conditioned stimulus. It should be asked whether the important circumstance in this experiment is the exact coincidence in time of the conditioned reflex or something else.***

For the sake of brevity I will entirely omit the technical details, such as by what methods the conditioned reflexes are best obtained; whether

* Experiments made by Zelheim and Boldirev.

** Experiments made by Boldirev, Kasherininova and Voskoboinikova-Granstrem.

*** Experiments made by Vartanov.

with food or with non-food substances; how many times the various stimuli may be applied in a day; with what length of intermissions, and so on. Next in order comes the important question, What are the stimuli that the nervous system of the dog recognises as individual phenomena in the outer world? Or, in other words, What are the elements of a stimulus? With regard to this a good deal of evidence is in existence already. If the application of cold to a definite area of the skin (a circle having a diameter of from four to five centimetres) acts as a conditioned stimulus for the salivary glands, the application of cold to any other portion of the skin causes secretion of saliva on the very first occasion. This shows that the stimulation by cold is generalised over a considerable part of the skin, or perhaps even over the whole of it. But the application of cold to the skin is very clearly distinguished as such from the application of heat and from mechanical stimulation. Each of these stimuli must be elaborated separately in order to give a conditioned reflex. Just as in the case of cold, the application to the skin of heat as a conditioned stimulus, also generalises itself. This is equivalent to saying that if an application made to one place on the skin stimulates the salivary glands, an application made to another cutaneous area will also produce a secretion of saliva. Totally different results were yielded by mechanical stimulation of the skin, such as rubbing with a coarse brush (by means of a special apparatus). When this treatment applied to a certain area of the skin had become converted into a conditioned stimulus, the same treatment applied to another place on the skin remained completely ineffective. Other forms of mechanical stimulation, such as pressure with a sharp or a blunt object, proved themselves less effective. Apparently the first mechanical stimulus formed only a small part of the latter.*

Stimulation by musical sounds or by noises is remarkably convenient for determining the discriminating or analytical faculty of the nervous system of the dog. In this respect the precision of our reaction is very great. If a certain note of an instrument is employed as a conditioned stimulus, it often happens that not only all the notes adjoining it but even those differing from it by a quarter of a tone fail to produce any effect.** Musical timbre (quality) is recognised with similar or even with much greater precision.[6] An external agent acts as a conditioned stimulus not only when it comes on the scene but also when it disappears, so that either its beginning or its end may be made the stimulus. Of

* Experiments made by Boldirev, Kasherininova and Voskoboinikova-Granstrem.

** Experiments made by Zeliony.

[6] In the following chapters the reader will meet with these phenomena under the name of *differentiation*. Chapter xvi is devoted especially to this subject, and it contains an analysis of the mechanism of the differentiation of various stimuli.—*Translator.*

course, a separate analysis has to be undertaken in order to explain the nature of such stimuli.*

We have hitherto spoken of the analytical ability of the nervous system as it presents itself to us in, so to say, a finished state. But we have now accumulated material which contains evidence of a continuous and great increase of this ability if the experimenter persists in subdividing and varying the conditioned stimuli. Here, again, is a new field of enormous extent.

In the material relating to the various conditioned stimuli, there are not a few cases in which can be seen an evident connection between the intensity of a stimulus and its effect. As soon as a temperature of 50°C. had begun to provoke a flow of saliva, it was found that even 30°C. had a similar but lesser effect. An analogous result may be observed in cases of mechanical stimulation. A diminished rate of rubbing with the brush (5 strokes instead of 25 to 30 strokes per minute) gives less saliva than the ordinary rate of rubbing, and accelerated rubbing (up to 60 strokes per minute) gives more saliva.

Furthermore, combinations consisting of stimuli of the same kind and also of stimuli of different kinds were tried. The simplest example is a combination of different musical tones, such as a chord consisting of three tones. When this is employed as a conditioned stimulus, the tones played in pairs and each separate tone of the chord produce an effect, but the pairs produce less saliva than the three together, and the notes played separately less than those played in pairs.** The case becomes more complicated when we employ as a conditioned stimulus a combination of stimuli of different kinds, that is, of stimuli acting upon different receptive surfaces. Only a few of such combinations have been provisionally experimented with. In these cases, one of the stimuli generally became a conditioned stimulus. In a combination in which rubbing and cold were employed the former was preponderant as a conditioned stimulus, while the application of cold taken by itself produced an effect hardly perceptible. But if an attempt is made to convert the weaker stimulus separately into a conditioned stimulus it soon acts energetically. If we now apply the two stimuli together, we have before us an increased effect resulting from the summation of stimuli.***

The following problem had for its object the explanation of what happens to an active conditioned stimulus when a new stimulus is added to it. In the cases that we examined we saw that the action of the previously formed conditioned stimulus was disturbed when a new stimulus of like kind was added to it. A new similar odour inhibited

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* *Ibid.*

** Experiments made by Zeliony.

*** *Ibid.*

the operation of another odour which was already a conditioned stimulus; a new musical note likewise impeded the action of a note employed as a conditioned stimulus, and just previously applied. It is not without interest, I think, to mention that we started with these experiments with another object in view. We were intending to form a new conditioned reflex with the aid of another conditioned reflex which had been previously formed. We accordingly experimented with combinations of dissimilar stimuli. Researches in this direction are well advanced. We have to discriminate between different cases. Some examples may be given. Scratching (or rubbing with a brush) may be a ready and effective conditioned stimulus. When we add to it the ticking of a metronome, applying both stimuli simultaneously, the scratching immediately loses its efficacy as a stimulant during the first applications (first phase); and this loss extends over some days but returns again, notwithstanding the addition of the metronome, and now our double stimulus has nearly the same effect as the scratching alone (second phase); later, scratching, when applied simultaneously with the metronome, ceases to act and the influence of this double stimulus now comes to an end altogether (third phase). When the glare of an ordinary electric lamp is added to scratching * which is a conditioned stimulus, the scratching at first produces exactly the same effect as before when it was without the lamp, but afterwards the combination of scratching and the luminous stimulus ceases to act.[7] Apparently a phenomenon of the same kind was observed when the action of other mechanical stimuli was experimented with instead of the scratching which had been made to play the part of a conditioned stimulus. In the first place, secretion of saliva was caused by pressure with a sharp as well as with a blunt object, but to a less degree than by scratching; on repetition, however, the effect of the pressure stimulus became progressively less, until finally, it altogether disappeared.

We may assume that a part of the stimulation by the sharp and blunt objects was identical with scratching, and that this component was responsible for the action of these objects during their first applications. But a part of the action was special; it led in the course of time to a destruction of the influence of the first. In these inhibitions we see the following phenomenon which in all experiments of this kind is

* Experiments made by Vassilyev.

[7] As in both these cases the action of the new agent which checks the activity of the conditioned agent, must be especially elaborated by coincidence of the conditioned stimuli with this new agent (in the cases cited, metronome or light) and with lack of activity, *i.e.*, elaborated in a direction opposite to that of the conditioned stimulus, this new agent was designated as a conditioned *inhibitor* and the corresponding process as conditioned inhibition. The building up of this conditioned inhibition will be more clearly discussed in chapter viii.—*Translator.*

regularly repeated. After a conditioned stimulus had been applied together with another one which inhibited its action, the effect of the first one tried alone was greatly weakened, and sometimes even arrested completely. This is either an after-effect of the inhibiting stimulus which was added, or it is the extinguishing of the conditioned reflex because in the experiment with the added stimulus the conditioned reflex had not been strengthened by the unconditioned reflex.

The inhibition of the conditioned reflex is observed also in the converse case. When you have a combination of agents acting as a conditioned stimulus, in which, as has been already stated, one of the agents by itself produces almost no effect, then frequent repetition of the powerfully acting stimulus alone, without the other one, leads to a marked inhibition of its action, almost to the point of its annihilation. The relative magnitudes of all these manifestations of stimulation and inhibition are closely dependent on the conditions under which they originate.

The following is an example. We assume that the stimulus of scratching is acting as a conditioned reflex in the following manner: In the first place nothing but scratching was employed for 15 seconds, then acid was poured into the dog's mouth, scratching being continued up to the end of one minute. If you now apply scratching for a full minute, you get a copious secretion of saliva. Try to keep up this reflex, that is, continue the scratching for a second minute, and only then pour acid into the dog's mouth. If you do this several times in succession, the effect of the scratching will quickly diminish during the first minute and will ultimately cease altogether. In order that the scratching may regain its efficiency during the first minute, it is only necessary to repeat the experiment several times; indeed, its effect will be even greater than it was in the previous experiments.

We have observed a similar course of events also in the exact measurement of the inhibitory effect.

Finally, it may be mentioned that the attempt was made to form conditioned reflexes from the traces [8] of the latest remnants or after effects, both of a conditioned and of an unconditioned stimulus. This was accomplished by allowing a conditioned stimulus to act for one minute immediately before the unconditioned stimulus, or by even three minutes earlier. There was always an interval of a few seconds to several minutes between the stimuli. In all these cases the conditioned reflex

[8] *Trace reflexes* are the remnants of the excitations in the central nervous system. The traces of the excitation are supposed to function as signals for the setting in of the action. As stated in the text, these conditioned trace reflexes have quite peculiar properties. The reader will meet with references to them at several places throughout the lectures. For some possible explanation of the mechanism of this phenomenon see the next to the last paragraph in chapter vii.—*Translator.*

developed. But in the cases in which the conditioned stimulus was applied three minutes before the unconditioned one, and was separated from the latter by an interval of two minutes, we obtained a result which, although quite unexpected and extremely peculiar, always occurred. When scratching, for example, was applied to a certain spot on the skin as a conditioned stimulus, after it became active we found that scratching of any other place also produced an effect; cold or heat applied to the skin, new musical sounds, optical stimuli, and odours—all these had the same effect as the conditioned stimulus. The unusually copious secretion of saliva and the extremely expressive movements of the animal attracted our attention. During the action of the conditioned stimulus the dog behaved exactly as if the acid which served as the unconditioned stimulus had been actually poured into its mouth.*

It may appear that this phenomenon is of a different order from those with which we have hitherto been occupied. The fact is that in the earlier experiments at least one coincidence of the conditioned stimulus with the unconditioned one was necessary; but in these experiments, phenomena which had never occurred simultaneously with an unconditioned reflex were acting as conditioned stimuli. Here an unquestionable difference naturally comes to light, but at the same time there is seen an essential property of these phenomena which they have in common with the former ones, that is, the existence of an easily excitable point in the central nervous system, to which, as a result of its condition, are directed all the essential stimuli from the external world that affect the cells of the highest parts of the brain.

I now bring to a close my cursory and very incomplete summary of the data which have been obtained in this new field of research. Three characteristic features of this subject deeply impress the investigator. In the first place, these phenomena are easily accessible for exact investigation, being in this respect scarcely inferior to the ordinary physiological phenomena. I refer to the ease with which they may be repeated—beyond all expectation—to their uniformity under similar conditions of experimentation, and to the fact that they are suitable for experimental analysis. In the second place, there is the possibility of considering this subject objectively. The introduction of a few subjective considerations which we admitted now and again for purposes of comparison seemed on further reflection to be an act of violence or an affront to a serious intellectual endeavour. In the third place, the subject involves an unusual number of stimulating questions for the investigator.

Under what heading is the subject to be classified? To what part of physiology does it correspond? The reply to this question presents no difficulties. It corresponds partly to what was, in former days, the

* Experiments of Pimenov.

physiology of the special sense organs, and partly to the physiology of the central nervous system.

Up to the present time the physiology of the eye, ear, and other receptor organs has been regarded almost exclusively in its subjective aspect; this presented some advantages, but at the same time, of course, limited the range of enquiry. Investigation by the method of conditioned stimuli in higher animals avoids this limitation, and a number of important questions in this field of research can be at once examined with the aid of all the immense resources which experiments on animals place in the hand of the physiologist. Owing to the shortness of the time that remains it is impossible to give illustrations of such questions. The investigation of conditioned reflexes is even of greater importance for the physiology of the highest parts of the central nervous system. Hitherto this department of physiology, throughout most of its extent, has been cluttered with foreign ideas, borrowed from psychology, but now there is a possibility of its being liberated from such harmful dependence. The conditioned reflexes disclose before us the vast field of the relations and reactions of animals to nature; this is a subject of immense extent and one that must be treated objectively. The physiologist can and must examine these reactions, using in connection with them progressive and systematic removal of parts of the central nervous system in order that he may ultimately arrive at an exact knowledge of the mechanism involved. And here arise at once some urgent and practical questions.

Still one point remains. What relation is there between psychological data and the facts just described? What points of mutual correspondence are there? Who will occupy himself with these relations? and when? This relationship may be interesting even now, but it must be confessed that physiology has at present no serious reason for discussing it. Its immediate problem is to collect and to analyse the endless amount of objective material which presents itself. But it is plain that the conquest which physiology has yet to make consists for the most part of the actual solution of those questions which hitherto have vexed and perplexed humanity. Mankind will possess incalculable advantages and extraordinary control over human behaviour when the scientific investigator will be able to subject his fellow men to the same external analysis as he would employ for any natural object, and when the human mind will contemplate itself not from within but from without.

Must I say something about the relationship which exists between medicine and the subject of my address? Physiology and medicine are fundamentally inseparable. If the physician is in his actual practice, and even more important, in his ideals, a mechanic of the human organism, then inevitably every fresh discovery in physiology will sooner or later increase his power over this extraordinary machine, his power to conserve and repair this mechanism. It is extremely gratifying to me that in honouring the memory of a great naturalist and man of science I am able to make use of ideas and facts which from this single successful point of view promises to throw light upon the highest and most complicated part of the animal mechanism. I am fully persuaded of, and boldly express my confidence in, the ultimate triumph of this new method of research and I avow it the more fearlessly because Thomas Huxley, who is an example to all of us, fought with rare courage for the freedom and the rights of the scientific point of view.

CHAPTER V

CONDITIONED REFLEXES IN DOGS AFTER DESTRUCTION OF DIFFERENT PARTS OF THE CEREBRAL HEMISPHERES [1]

(Read before the Society of Russian Physicians, St. Petersburg, and published in *Transactions of the Society of Russian Physicians,* 1907-1908.)

PAVLOV'S CONTROL EXPERIMENTS DO NOT CONFIRM THOSE OF BECHTEREV'S LABORATORY.

The aim of this communication is to give a preliminary summary of the experiments, performed by my co-workers and myself, concerning the relation of the conditioned salivary reflex to the hemispheres. We consider the assertion of Dr. Belitsky, that the existence of conditioned salivary reflexes depends upon the presence of a certain cortical area, and after extirpation of this part of the hemispheres, all the conditioned reflexes disappear. The experiments of Dr. Tichomirov, described in his dissertation, and those of Dr. Orbeli completely refute the results of Belitsky. On the one hand, I am showing you the extirpated parts of the hemispheres in the dog containing the centres proposed by Belitsky, and on the other hand, Dr. Orbeli demonstrates before you a dog from which these centres have been removed, and which, as we can see, gives a prompt and strong reflex to the sound of crushing dry bread. Orbeli repeated the same experiments on another dog with the same results.

After receiving negative results in the control experiments of Belitsky, Tichomirov repeated similar experiments of Dr. Gerver with a conditioned reflex of the gastric glands, and obtained also negative results. I carried out these experiments myself a second time and saw the same results as did Dr. Tichomirov. From the brain of my dog and from the protocols it could be seen that, notwithstanding the removal of part of the cortex four times as extensive as that removed by Gerver, conditioned reflexes of the gastric glands were, nevertheless, present

[1] Near the end of chapter i Pavlov says: "After having established the possibility of analysis and systematisation of our phenomena, we come to the following phase of the work: the systematic division and destruction of the central nervous system in order to see how the above established relations will be changed in this process." Prof. Pavlov intended to use for this purpose the researches of Bechterev's laboratories. But here he was wholly disappointed, as all this work Pavlov failed to verify experimentally. So he had to start anew with these questions. The first of these experiments were performed by Orbeli and Tichomirov and are described in chapters v and vi. They were made for the purpose of verifying the researches of Bechterev's laboratories (Belitsky, Gerver and Gorshkov).—*Translator.*

on the sixth day after removal of the imaginary centres of Dr. Gerver, and these reflexes remained for some days. The findings of our experiments leave no doubt that Dr. Gerver became a victim of a mistake based upon the illness of his dog after operation and a consequent loss of appetite.

In my laboratory nearly all parts of the cerebral hemispheres have been removed in stages, and conditioned salivary reflexes repeatedly tried. From these experiments I must conclude that there is no special part of the hemispheres on which, in a general sense, the existence of the conditioned salivary reflex depends.

But this does not exclude special relations of different parts of the hemispheres to the conditioned salivary reflexes. Tichomirov showed that the arc of various conditioned reflexes in certain of its parts is located in the cerebral hemispheres. Artificial conditioned reflexes to the salivary glands from the skin disappeared completely and could not be re-formed if the part of the cortex corresponding to the so-called motor region was removed. Similarly, on removal of the occipital lobes, the natural optical reflex to the salivary glands disappeared. Other conditioned salivary reflexes persisted, and even new ones could be formed. The same relations have been seen in other dogs in our laboratory, besides those described in the thesis of Dr. Tichomirov. From our experiments we see that for the building up of the conditioned reflexes certain cortical connections from various specific receptors are necessary—from the eye, the ear, the nose, the skin. There is ground for assuming that the same is true for all other conditioned reflexes. Thus we have the right to state that: *the cerebral hemispheres are the organ of conditioned reflexes.*

Finally we may add that in our material there is no indication for belief in the existence of parts of the hemispheres especially engaged in the building up of conditioned reflexes (other than particular regions containing the various paths from specific receiving surfaces), *i.e.,* centres corresponding to the so-called association centres do not exist. A definite conditioned reflex disappears forever only when there are removed definite regions including the cortical conductors from its specific receptor organ.

CHAPTER VI

THE CORTICAL TASTE CENTRE OF DR. GORSHKOV

(Abstract from a lecture, read before the Society of Russian Physicians, and published in *Transactions of the Society of Russian Physicians*, 1907-1908.)

In 1901 Dr. Gorshkov inferred that the taste centre is situated in the frontal region of the brain. After the bilateral removal of these cortical gyri of the dog, according to Dr. Gorshkov, the animal quietly eats meat covered with salt, citric acid, or quinine, or meat soaked in the following solutions: salt 32 per cent, acid 9.5 per cent, or 5 per cent quinine. Dr. Tichomirov, repeating these experiments, could not obtain the same results. I also removed in a dog the same parts on both sides of the brain as Gorshkov; and could not see any loss of taste. It is evident that the inference of Dr. Gorshkov is a result of prejudice and of inexact observations. For example, Dr. Gorshkov drew his conclusions from operated dogs which died two or three days after the operation. And his experiments on dogs operated on one side of the brain, who "quietly ate with the opposite side of the tongue" meat mixed with vegetable substances mentioned above, shows that he is throughout fantastic.

I. P. PAVLOV, LECTURES ON CONDITIONED REFLEXES

CHAPTER VII

MECHANISM OF THE HIGHEST PARTS OF THE CENTRAL NERVOUS SYSTEM AS SHOWN FROM THE STUDY OF THE CONDITIONED REFLEXES[1]

(Read before the Society of Russian Physicians, and published in *Transactions of the Society of Russian Physicians*, 1908-1909.)

DIFFERENCE BETWEEN UNCONDITIONED AND CONDITIONED REFLEXES—TEMPORARY NATURE OF THE CONDITIONED CONNECTIONS, AND THE POSSIBILITY OF AN INFINITE NUMBER—COLOR BLINDNESS IN THE DOG—CONCENTRATION OF INDIFFERENT STIMULATIONS—IRRADIATION.

Six or seven years ago, with my collaborators, I made the first attempts to subject the entire nervous activity of the higher animals (of the dog) to an objective investigation, excluding absolutely any conjectures concerning the activity of the experimental animals based on a comparison with our internal world. From our point of view all nervous activity of the animal could be considered as a reflex activity of one of two forms—the usual reflex, which had previously been studied for many decades, and which we called an *unconditioned* reflex, and a second, new reflex which embraces the whole remaining nervous activity, and which we designate as *conditioned.*

At present, we can state with assurance that our experiment has been fully justified by the facts obtained; for the scientific material which our method has produced grows steadily and falls naturally into a certain system. These facts allow us, on the one hand, to systematise to a certain degree the process of the highest nervous activity, and on the other hand, to throw light upon some general, but real points in the mechanism of this function.

The organ upon whose activity in our experiments the different influences of the outer world are brought to play, is the salivary gland. A group of definite external stimulating agents, acting from the oral or nasal cavities or from the skin, were seen to call forth the usual reflex activity of the salivary glands—an activity which we called the unconditioned reflex. All external stimuli from these same receptors, and also from the eye and the ear, which are first directed to the receptor centres of the cortex, and from here into the medulla oblongata, provoke other reflexes—the *conditioned* reflexes. The paths along which

[1] It is to be noticed that at this stage of the investigation Pavlov selected from the multitude of facts confronting him the four most general features of the work of the cerebral hemispheres. They are discussed in this chapter, and the general scheme stands unchanged up to the present.—*Translator.*

the excitation of the first order travels, are fixed, and are always, under the circumstances of normal life, open and permeable. The paths for the excitations of the second group are paths which are formed under certain conditions of life; under other conditions, they are closed and impassable; thus these latter paths are now open, now obstructed. In this second group we have to do consequently with a *temporary coupling or connection* of different conduction paths, and this we must consider as a fundamental activity of the highest parts of the central nervous system, as a central point in its mechanism.

Any external stimulus, if only capable of being transformed on the body surface of the dog into a nervous process can, according to our experiments, be conducted to the salivary glands by means of the higher parts of the brain, thus becoming their stimulus. In this fact lies the second important point of their mechanism—the *universality of possible connections* in the higher departments of the central nervous system. The single fact which apparently contradicts this generalisation, is that up to this time we have not been able to form conditioned salivary reflexes by means of different refracted rays of light (*colours*). But we have good reason to consider this as not a conduction. Stated exactly, this fact signifies only that the generally accepted opinion about the dog, *viz.*, that he reacts to the different wave lengths of light, or, subjectively speaking, distinguishes between colours, is not based on experiment, but is a prejudgment arising from a superficial analogy with man.

The third point in the mechanics of this action which we have analysed is manifested in the nature of the method itself, by means of which the conditioned reflexes are formed. In order to make a stimulus for the salivary glands from any phenomenon acting on the receptor surfaces of the dog, it is necessary that the action of this phenomenon on the dog should occur simultaneously with the unconditioned reflex of the salivary glands provoked by the introduction of food or of some inedible stimulating substance into the mouth of the animal. From this fact it is evident that if very strongly excited foci arise in the nervous system (in the given case, in the salivary reflex centre), the formerly indifferent stimuli from the external world, acting on the receptors and thus exciting the receiving centres of the cortex, are conducted toward these strongly excited foci; in this way the impulses concentrate and open a path leading to these foci. This fact can be designated as the *mechanism of concentration* of direction of indifferent stimulations.

Finally, the fourth part of this mechanism. This is to be found in facts relating to a special group of conditioned reflexes. They have been investigated in our laboratory by Dr. Pimenov. If some external agent, which we intend to use as a conditioned stimulus of the salivary glands, does not coincide exactly with the unconditioned salivary reflex but always precedes it, and if the end of its action is separated from the beginning of the unconditioned reflex by a certain interval (in the experiments of Pimenov by a pause of two minutes), then the following happens. When the conditioned reflex has thus been elaborated, there also act as stimuli various other external phenomena, and these stimulations by the external phenomena develop only gradually in a certain definite order. If, under such conditions, the original mechanical irritation of a certain skin area has been made a stimulus of the salivary glands, then also the mechanical stimulation of other skin areas begins at first to call out the salivary reflex, contrary to the law of specificity of the ordinary from mechanical stimulation of the skin-conditioned reflexes. Afterwards thermal skin stimulations show this effect, and finally the irritation of other body surfaces, for example, of the nose, the eye and the ear. The internal mechanism of this fact can be understood thus: A stimulation entering the cerebral cortex which is not directed to a certain active nervous point begins to spread and disperse itself over the surface of the brain. If now a strongly excited point arises later, it will attract to itself cortical stimulations not only from the point originally irritated, but from all other points over which the stimulation had gradually spread. This is the law of dispersion, of irradiation of the stimulation in the cerebral cortex.[2]

Obviously, the formulation of these four points in the mechanism of the highest parts of the central nervous system must in many respects be considered provisional.

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[2] For a more detailed study of these processes of "irradiation" and "concentration," see chapter iv.—*Translator.*

CHAPTER VIII

FURTHER ADVANCES OF THE OBJECTIVE ANALYSIS OF COMPLEX NERVOUS PHENOMENA, AND ITS COMPARISON WITH THE SUBJECTIVE CONCEPTION OF THESE PHENOMENA [1]

(Based on the experiments of Dr. P. N. Nikolaev. Read before the Society of Russian Physicians, and published in *Transactions of the Society of Russian Physicians*, 1909-1910.)

ALL NERVOUS ACTIVITY IS A REACTION TO THE EXTERNAL WORLD BY MEANS OF UNCONDITIONED AND CONDITIONED REFLEXES—DETAILS OF AN EXPERIMENT WITH CONDITIONED REFLEXES—REFLEX OF SECOND ORDER AND COMPLEX CONDITIONED STIMULI—INHIBITION—DETAILS OF EXPERIMENT—DEDUCTIONS FROM THE EXPERIMENT—EXPERIMENTAL PROOF OF THE HYPOTHESIS—INHIBITION OF INHIBITION—DISCUSSION OF THE MEMBERS OF THE COMPLEX STIMULUS—COMPARISON OF THE SUBJECTIVE AND OBJECTIVE ANALYSES OF THE EXPERIMENTAL FACTS—PHYSIOLOGY AND PSYCHOLOGY.

THIS address has to do with the so-called conditioned reflexes—the objective investigation of the activity of the central nervous system of the dog. I shall ask you to recall some of the fundamentals of this study. From the standpoint of objective research, we hold that all the nervous activity of the dog, without reservations, is a reflex activity, a reaction of the animal to the external world effected through the nervous system. In this reaction, we can distinguish two kinds of reflexes. The simple and well-known reflex, which we call "*unconditioned,*" is one in which certain phenomena of the external world are associated with definite responses of the organism through a constant and unchanging connection in the central nervous system. For example, if a mechanical body impinges on the eye of an animal, it is always followed by a defensive movement of the eyelid, or every time a foreign body enters the larynx

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1 Even at this stage of the investigation Pavlov was aware that in this fine work of the hemispheres one must take into consideration the individual peculiarities of each different nervous system. In chapters xxxv and xxxviii one will find more facts on this point. The matter is discussed systematically in chapter xvii of Pavlov's *Activity of Cerebral Hemispheres.*

In special experiments it proved really to be true that the stimulus which often coincides in time with the state of inhibition in a point of the hemispheres becomes able, when applied, to produce this state of inhibition.

The extinction of the conditioned reflex is considered as brought about by a process of inhibition. The production of this inhibitory process is hindered by every extraordinary or new agent added to the extinguished conditioned reflex; so, for lack of inhibition, the conditioned reflex becomes again efficient. This process, viewed as a lifting of the brake (inhibition), received the name "dis-inhibition." In chapter x, describing the first cerebral mechanism, and in chapter xi, there are given examples of dis-inhibitors.—*Translator.*

and irritates it, coughing results. From these old reflexes we can differentiate a new group in which the connection of the external phenomena with the responsive reaction of the organism has only a temporary character. This connection forms under only certain conditions, continues only in the presence of certain conditions, and disappears under definite conditions. Thus we distinguish between *constant* and *temporary* reflexes. In this way can we comprehend and understand many complicated relations of the dog to the outside world as *temporary reflexes.*

At present, as shown by numerous reports already presented, our knowledge concerning the conditioned reflex is based on a great body of facts, which, we can assert without exaggeration, multiply themselves each day. Besides, the appearance of the rules and laws which embrace and unite this multitude of facts insure for our investigation an uninterrupted progress.

Here we would present for your consideration an instance of the complex nervous activity of the dog, an instance in which, as we think, the analysis is deeply penetrating and yet as a matter of particular interest, preserves, despite this depth, a great accuracy. In order to render perfectly clear everything that I have to say I shall report the concrete case of a dog upon which we performed the experiments, and trace the results from the very beginning. It is necessary to add that some of the facts of this exposition, obtained from this dog, were also obtained from many other dogs; and the last observation which I shall present, and which forms the new material of this report, was repeated on a second dog with exactly the same results. Consequently the facts in this address cannot be considered as accidental.

A light (L), in the tables, has been made a conditioned stimulus for the salivary glands by means of the food reflex. This is accomplished in the following manner. The dog is put into a dark room, and at a certain moment a bright electric light is switched on. We wait for a half minute, and then give the dog food and allow it to eat for a half minute. This procedure is repeated several times. Finally the electric light, which at first was an indifferent agent for the animal, and had no relation whatever to the function of the salivary gland, owing to repeated coincidence of eating with salivary activity, becomes endowed with the property of acting as a special stimulus for the salivary gland. Every time the electric light appears we have a salivary secretion. Now we can say that the light has become a conditioned stimulus of the gland. The activity of the salivary gland in such a case serves as a simple index of the reaction of the animal to the external world. This reflex gradually grows until it finally attains a certain limit, in the present case, ten drops of saliva in half a minute. We now add to the light a definite tone (of about 426 vibrations per second); the simultaneous action of the two, as you will see in the tables, is represented by L plus T. The combination of light and tone lasts a half minute. This combination of stimuli is never accompanied by feeding. For the first few applications of this combination there is no change in the original effect of the light, *i.e.*, the light plus tone gives the same salivary secretion as the light alone did (ten drops in half a minute). I wish to emphasise that this combination is never accompanied by food. We ask ourselves, however, the following question: Although apparently there is no outward change, may it not be possible that there has taken place in this process some intrinsic transformation? Has not the tone which we have joined to the light and which formerly had no relation to the salivary gland become something other? And after four or five applications of this combination (without feeding), the tone had acquired the property of acting as a stimulus of the salivary secretion. It is true the effect was very small, only one or two drops. But what does this signify? Why has the tone become a stimulus? Why has the tone which has never been accompanied by feeding taken on the character of a stimulator? It is evident that the tone acquired its exciting effect by being applied simultaneously with the light, and it has actually gone through the same process as occurred when the light received (from its association with eating) its stimulatory effect on the salivary secretion. In the action of the tone we see the action of a new conditioned reflex, and as in the given case the effect of the tone came about owing to its coincidence with a conditioned stimulus (light), and not to coincidence with an unconditioned stimulus (food), this new stimulator (tone) can be designated as a stimulus of the second order, and the new reflex as a *reflex of the second order*.

This effect, it is necessary to note, is in most cases very weak, only one or two drops, very transitory, and not fixed. If the experiments are continued for some time, the tone will lose its action. This period comprises the *first phase* of the different changes through which the tone effect in the combination passes. The secretory action itself is so small and it requires such exact conditions for its manifestation, that doubt may arise even as to its very existence. But there is a circumstance which greatly facilitates the control of this experiment. Among the experimental dogs one finds special types of nervous systems; in particular there are dogs with weak nervous systems in which this phenomenon is clearly expressed. This phase remains comparatively stable, so that the conditioned reflex of the second order in such dogs may last for weeks, and disappear very reluctantly.

So the first result of the combination (tone plus light), which is never accompanied by eating, consists in this; the tone also becomes a conditioned stimulus. Repeating this double stimulation ten to twenty times and never supporting it by feeding we arrive finally at the next phase. If this combination during the first four to five applications gave the same effect as the light alone, then afterwards the action of this combination begins to decrease, and instead of ten drops, it produces eight, five, four, three, and finally no drops. So the light (L) alone yields ten drops, and the light plus tone (L + T), zero. This last state remains stationary; repeat this double stimulation as much as you will, you see no change. What does this mean? Is there some sort of internal mechanism at work? And if there is, can it be discovered, and by what means? Obviously, we must try the component elements of this double stimulation. It is not necessary to try the light, as we know that it always gives ten drops. There remains for us only to try the tone alone. We saw that the tone in the first phase gave one to two drops, but if we try it now, it gives nothing. How shall we explain this zero? There are two possible explanations: it can be either a true zero, that is, it may have no effect whatever, or it may have a negative value, *i.e.*, it may not only be indifferent but actively inhibiting. This problem must be solved, but how? We have a series of experiments which decide this question finally and absolutely. The tone is not zero in the combination (double stimulus); it has a negative value, it is actively inhibiting.

This can be shown in the following way. Let us take, besides the conditioned light stimulus, some other conditioned food reflex, for example, mechanical irritation of the skin, which also calls out salivary secretion. Now let us unite this scratching (skin irritation) with a tone. It was shown that the tone destroyed the effect of the scratching. From this one can see that the effect of the tone is not zero, but an active minus. It has become an inhibitory agent. Thus it is evident that if the tone is joined to some other conditioned stimulus, the action of this stimulus is destroyed.

On the basis of these facts we are convinced that at the foundation of our complex conditioned reflex there lies a definite internal mechanism. This mechanism consists in the following: If we join to a conditioned reflex any other indifferent stimulus, and then do not support this double stimulus by an unconditioned reflex (food), the new agent will pass through two phases of activity. At first, for a short time, the new agent appears as an active stimulus of the unconditioned action, but afterwards, in the second phase, it plays another rôle—that of an inhibiting agent.

The above statements were made by us some time ago. Now I shall pass over to entirely new facts. My collaborator, Dr. Nikolaev, who has just completed work on this problem, is responsible for them. I shall now present them to you, and analyse them. To the double stimulation of light plus tone, we add a third, a metronome (L + T + M in the tables). This trio (L + T + M) is always followed by food, and we maintain the same time condition, *i.e.*, the trio is given alone for half a minute, and then it is given for another half minute accompanied by eating. Therewith is developed a very long and interesting series of phenomena. The gist of our report today is contained in the analysis of this picture, which is shown in Table I.

TABLE I

EFFECT OF THE TRIPLE STIMULUS
L + T + M

Flow of Saliva in Drops	*Number of Recurrences of the Same Result*
0 0	2 times
2	once
4	16 times
6–9	5 times
10	16 times
10	22 times

This table represents the stimulating effect of our trio (L + T + M) and the separate periods of its action and the quantity of saliva. In the table zero occurred twice. This means that the influence of the triple stimulus was at first the same as that of the double stimulus, *i.e.*, zero. This obtained, however, in only the first two experiments. From the third experiment a change set in. In place of zero, the trio now gave two drops, and that only once; afterwards it began to give four drops, and this occurred sixteen times. So this first long period lasted sixteen days. Thus the triple stimulus on its fifth trial produced a definite salivary secretion, *viz.*, four drops. Now may we rightly ask, what does this mean? And what is its internal mechanism? and why did we receive precisely four drops and no more nor less? Our problem is now complicated because we have three agents, each one of which has a different significance. In order to explain the conjoint action of these three agents it is obviously necessary to try their action singly and also in various combinations.

As a result of these investigations, we obtained certain facts which lead us to a definite conclusion. We have three agents; from these it is possible to make seven combinations. Light, tone, and metronome each separately, light plus tone, light plus metronome, tone plus metronome, and finally light plus tone plus metronome. Now we must try out all these combinations, and the result will yield us some answer. We are already acquainted with three of these seven combinations. The light alone gives ten drops; the light plus tone, zero; light plus tone plus metronome, four drops.

TABLE II

L =	10 drops
L + T =	0
L + T + M =	4
T =	0
M =	0
T + M =	0
L + M =	6

It must be expressly stated that all these combinations are repeated every day, and that they gradually become strengthened in their special effect. Now we must try out the other four combinations, which usually are not applied, and which we try only occasionally, for the purpose of making an analysis. The metronome alone is without any effect, also the tone alone, as well as the tone + metronome. The single effective combination we find to be the light + metronome. But we note from the beginning a certain peculiarity: the light + metronome yields six drops, but the light alone, ten drops. This fact may be explained in the following manner: the metronome becomes an inhibiting agent; for in the combination of the light and the metronome, the effect is smaller than from the light alone. Now we come to the conclusion that during the first period of the application of our trio, the metronome has taken on the rôle of an inhibiting agent, as the light in combination with the metronome gives less saliva than the light alone.

Now two questions arise: first, how does the metronome in this trio receive its inhibitory action? and secondly, how can the metronome, being inhibitory, produce a secretion of four drops of saliva in the triple stimulus? The first can be answered only hypothetically, because we have not, as yet, pertinent experiments. Our hypothesis is the following: when to the light + tone we add the metronome, and allow this trio to act for a half minute and follow it for the next half minute with feeding, then during the beginning of the period of the coincidence of the metronome with the light + tone, in the nerve cell of the animal there exists an inhibitory process. In this case the action of the metronome coincides with the process of inhibition in the nerve cell, and therefore it is quite natural that it should acquire the character of an inhibitory agent—the colour, so to speak, of the process with which it had been constantly involved.

In this instance a phenomenon appeared, analogous to that mentioned above when, in interpreting the mechanism of the double stimulation (L + T), we noted that the tone added to the light borrowed from the latter its stimulating action; so here also the process predominant in the nerve cell lent its own colour to the agent coinciding with it. Thus we must explain how the metronome has itself become an inhibiting agent due to its coincidence with an inhibitory process. We cannot suggest any other explanation.

I repeat that this explanation is in the highest degree probable, but probability is one thing and fact another. Therefore, we decided to perform a series of new experiments in order to confirm our supposition. Thus we came to the decision of our second question: how can the metronome, which in the triple stimulus received its inhibiting action, have transformed the trio into a stimulating agent, now yielding 4 drops of saliva; *i.e.*, how has it become now a stimulator?

This effect might appear to us as entirely incomprehensible, if we had not exact data about a certain nervous process which we have analysed every day for many years. This is the so-called *inhibition of inhibition*. It consists in the following: If you have some conditioned stimulus, and if you add to this any other agent having a certain effect on the dog (for example, if the dog turns toward it), this agent will inhibit the conditioned stimulus. The inhibitory process is an habitual and well-known phenomenon in the physiology of the central nervous system. But also the following can be observed: if you, having to do with an inhibitory process in the nervous system, join to this inhibited stimulus some new extra agent, the inhibited stimulus now manifests its own effect. This fact may be understood thus: the new extra agent inhibits the inhibition, and as a result there is a freeing of the previously inhibited action, *i.e.*, a positive effect. If we take light, our conditioned active stimulus, and join to it an extra agent, for example, a whistle, the light effect is inhibited. But if, having extinguished the effect of the same light by repeating it without feeding, we now join to this inactive light some extra agent, the light will show its earlier stimulating effect. This is the phenomenon of inhibition of inhibition. The process of inhibition of inhibition is as often observed in the activity of the central nervous system as are the processes of stimulation and inhibition.

Now if this is true, the appearance of four drops of saliva after our triple stimulus ($L + T + M = 4$ drops) must be understood as follows: the metronome, having attained an inhibitory effect, acted on the nerve cell, which was itself involved in an inhibitory process, *i.e.*, the metronome inhibited the action of the tone, and in this way liberated from its inhibiting influence a part of the effect of the light. On the basis of the fact that the metronome is an inhibitory agent, and in consideration of certain definite processes of the nervous system, we must interpret this period of the action of our combination during which it gives 4 drops. In this period it appears that our newly added stimulus (metronome), falling on the soil of inhibition, inhibits only the inhibition, and liberates from its influence the conditioned stimulus (light).

Now we would call your attention again to Table I. You have seen that sixteen of these experiments done with the triple stimulation yielded the same result. Further we have seen that on the twentieth trial, a change sets in, and the process passes over from the first phase to the following: the effect of the trio increases to 6, 7, 8, and 9 drops, and, in the twenty-fourth experiment, to 10 drops. Thus the effect of the trio becomes equal to the action of the light alone.

TABLE III

L =	10 drops
L + T =	0
L + T + M =	10
T =	0
M =	4
T + M =	4
L + M =	10

Here in this Table III we find ourselves in the second period of the process. Now we must explain to what circumstance we owe the effect of the triple stimulus in producing 10 drops instead of the previous 4, and the significance of all the agents taking part in this effect.

Let us endeavour to make some analysis of this, *i.e.,* let us test the significance of all possible combinations. The following three are already known to us: light with its effect of 10 drops; L + T, zero; L + T + M, 10 drops. Further, it was proved that the tone remained zero, that the metronome now has an effect of 4 drops, and that the tone plus metronome gives also 4 drops; the metronome when joined to the light does not change the action of the latter, and consequently the metronome has lost its former inhibitory effect. Thus the metronome in the second phase passes over from the rôle of an inhibitor to the rôle of a stimulator of moderate strength. It itself gives 4 drops, and together with the tone it also gives 4 drops, and when joined to the light it gives the same effect as the light alone.

Here I must remark that when active conditioned stimuli are combined, there is never a summation of the individual actions in the combination. That is, if you have several conditioned stimuli acting in different degrees of strength, then the combination of them, one with the others, gives a quantity of saliva corresponding to the action of the strongest stimulus. In our case, the maximum quantity was given by the light, and therefore, in the combination with the metronome, the double stimulus gave exactly the same effect as the light alone.

Thus, in the triple stimulus we see a process analogous to that which we observed in the double stimulus, with only this difference, that the process occurs here in a reversed order. There we observed two consecutive phases; the first, when a newly added agent takes on the character of the conditioned stimulating process, and the second, when this new agent is changed into an inhibiting agent, owing to the fact that it is never accompanied by food. A similar process is observed in the case of the triple stimulus: here we see that the new stimulus, the metronome, during the first period of the application of the trio, has become an inhibiting agent, having been influenced by that process which was at the given moment predominant in the nerve cell. With the continued application of the triple stimulus, owing to its being accompanied by eating, the metronome acquired a stimulating action. Consequently, we see that the same regular recurrence of the two phases has been repeated here.

Now arises the interesting question concerning the significance of the other combinations. You see that the tone alone always remained zero; notwithstanding the fact that the tone in the trio was accompanied by eating, it did not receive any stimulating action. This means that the tone during its presence in the trio did not become a stimulus. On the other hand, while in the trio it was not an inhibitory stimulus; for the metronome alone and the metronome in combination with the tone evoked the same 4 drops.

Thus you see that the rôle of the tone is an exceedingly interesting and peculiar one. Under different conditions the tone has a different effect; in the double stimulus, it acts as an inhibiting agent, and in the trio, it has no effect.

If we consider all the above facts, we come to the conclusion that we have to do with certain regularly recurring events, which bear a certain relation to one another. In other words we see the summated action of different agents, which under stated conditions come to have a definite plus or minus effect, and thus a certain *equilibration* among them is attained. That is, we have to do with some not yet clearly definable equilibrium of the nervous process. You see that our ciphers remained exact and constant, and that thereby each agent has a special and definite significance. If these phenomena were accidental, then our figures would be very fluctuant and confused. Our facts, however, were not at all of this character. This is the first logical account showing that we have really to do here with a definite equilibrium.

Another more direct proof appears from the work of Dr. Nikolaev. His comparison of figures revealed a certain definite relation, a mathematical connection one with the other. In Table IV you can see that the double stimulus (L + T) was never accompanied by food, and was strengthened in its rôle of a zero agent, and the triple stimulus (L + T + M), on the other hand, was always accompanied by eating and became reinforced in its rôle as a stimulator. What does all this imply? It shows that in order that these rôles may not be interchanged—that the double stimulus will always give zero and the triple stimulus 10 drops—

there must obtain definite mathematical relations between the repetitions of the combinations. Namely, the double stimulus which is not accompanied by eating must be applied exactly twice as many times as the trio because as soon as the trio began to be repeated more frequently, the double stimulus lost its *nil* effect, and became positive.

TABLE IV

Date	*Saliva*	*Number of trials and their order* L + T	*Number of trials and their order* L + T + M	*Ratio:* (L + T) / (L + T + M)
Jan. 21	0	28	14	= 2:1
Jan. 31	0	32	16	= 2:1
Feb. 3	2	35	18	< 2:1
Feb. 5	0	45	19	> 2:1
Feb. 12	0	63	26	> 2:1
Feb. 16	0	74	32	> 2:1
Feb. 26	0	85	40	> 2:1
Mar. 2	2	92	47	< 2:1
Mar. 4	0	100	50	= 2:1
Mar. 5	5	103	52	> 2:1
Mar. 10	0	122	56	= 2:1
Mar. 13	0	120	60	= 2:1
Mar. 17	0	126	63	= 2:1

From the above table it is evident that every time the double stimulus was repeated twice as many times as the trio its action was always zero. Only under such conditions could the inhibiting agent manifest its effect. But as soon as the trio was repeated oftener than the double stimulus, the relation failed, the inhibiting action of the tone became weaker and the double stimulus came to have a positive effect, as shown in the experiments for Feb. 3, Mar. 2, and Mar. 5.

Here you see that for these combinations to maintain their definite significance, there must be preserved their mathematical relations, *viz.*, that the double stimulus must be repeated exactly twice as many times as the trio.

Such are the facts to which we would call your attention. We undertook an analysis of three agents, and we saw that the action of the agents developed with certain regularity. There appears the law of the action of the newly added agent; this agent passes through two phases, and finally a certain equilibrium was always reached in the nervous system, a definite plus or minus influence of the agents one upon another.

After these instructive facts were obtained, we desired to know whether the investigation of the corresponding nervous phenomena could be made with similar exactitude by means of subjective analysis. For this purpose I sought to acquaint myself with the conditions—I addressed myself to books on the subject. I did not find in the books what I was looking for, perhaps because it is difficult to become a specialist in a short time. Therefore I turned with the following question directly to specialists. To what facts of subjective psychological analysis do our facts correspond, and how are they to be analysed? Unfortunately, in this, as well as in many foregoing cases, my trials were unsuccessful. Some answers were received from which it was impossible to extract anything positive, and this is easily understood. The comparison of the results obtained by objective analysis of the complex nervous phenomena with the results of subjective research met with extraordinary difficulties of two sorts. All our reasoning had to do with facts obtained by a strictly objective method, and are of a special character; they are conceived of in terms of space and time, *i.e.*, they are purely scientific facts. Psychological facts are thought of only in terms of time, and it is conceivable that this difference in mode of thinking might create an incommensurability between these two ways of thinking. This is one of the circumstances that causes difficulty.

The other circumstance consists in the fact that it is impossible to compare the complexity of our phenomena with the complexity of the psychological phenomena. It is evident that the activity of the human nervous system greatly exceeds in complexity that of the dog. Therefore psychologists are embarrassed to say to what phenomena of experimental psychology our analysis corresponds. I have received from psychologists statements that they have no parallel analysis, and I think that, owing to the indicated difficulties, for a long time we shall pursue a different road from that of the psychologists. We physiologists are not sorry for this. We are by no means placed in a difficult position. Our scheme of understanding nervous activity is simpler than that of the psychologists; we build only the foundation; they, the superstructure; and as the simple and elementary is always conceivable without the complex, whereas the complex cannot be conceived of without the elementary, our position is more favourable, for our investigations and our success do not depend in any way upon their researches. On the contrary, I believe that our investigations may be of great importance for the psychologists, as they must in the future lay a basis for psychological knowledge. Psychological knowledge and psychological investigation are very difficult; they have to do with exceedingly complicated material, and besides this they are accompanied by an extremely unfavourable condition which is absent from our work, and from which we do not suffer. Such a highly unfavourable condition of psychological research is to be seen in the fact that the investigation does not deal with a continuous and unbroken series of phenomena. Psychology has to do with conscious phenomena; and we well know that psychical life is composed in a checkered fashion of conscious and unconscious elements. The psychologist in his investigation is, it seems to me, in the situation of a man wandering in the darkness with a small lantern in his hand, which is able to illuminate only a small section of the way. You can understand that it is impossible to study the whole region with such a lantern. To every one who has been in such a situation it is well known that the view which he can obtain thus bears absolutely no likeness to that which he sees in bright sunshine. Thus we physiologists are in much more favoured circumstances. When you consider all this you will see how different are the chances of objective and of psychological investigations. Our researches are still very limited and are carried on only in a few small laboratories; indeed, one can say that they have only begun. Notwithstanding this we have a serious experimental analysis, of deep penetration and of great exactness in all its parts. If one would wish to find, however, the laws of psychical phenomena, he must admit that he is in confusion as to where to look. For how many thousands of years has man elaborated psychical facts, facts dealing with his own spiritual life? And not only do psychologists strive, but all art and all literature seek to represent the mechanics of the spiritual life of man. Millions of pages have been written to describe the internal world of the human being, but with what result? Up to the present we have no laws of the psychic life of man. Until now the proverb is true that, "The soul of another is a riddle."

Our objective investigations of the complex nervous phenomena of the higher animals fill us with a reasonable hope that the fundamental laws underlying the fearful complexity in which the internal world of man is manifested to us can be discovered by physiology, and in the not far-distant future.

CHAPTER IX

SOME GENERAL FACTS ABOUT THE CEREBRAL CENTRES

(Read before the Society of Russian Physicians, and published in *Transactions of the Society of Russian Physicians*, 1909-1910.)

THE REFLEX PATH—SENSORY (RECEPTOR) AND MOTOR (EXECUTIVE) PARTS—THE ANALYSER—PROOF THAT THE MOTOR CENTRE IS ALSO RECEPTOR.

THE brain is indeed an immense theme. By virtue of its structure and its functions it will undoubtedly attract and occupy many generations of investigators. It is too early to speak of any definite plan or functional type of brain. Consequently one should now limit oneself to the collection of facts. However, at any given time one must have a general conception about a subject, in order to have a framework on which to hang the facts, to have something on which one may build, and in order to have an hypothesis for future investigations. In scientific work such conceptions and hypotheses are indispensable.[1]

For several decades I have been occupied with the study of the nervous system; with the central nervous system especially for ten years, and with the extirpation of parts of the brain for the purpose of explaining its functions for the last five years. Thus I have collected a mass of material, and I feel the need of referring it to certain general conceptions. One of these conceptions which has formed itself in my mind, I now have the honour to present for your attention.

The basic idea in our conception of the functioning of the central nervous system is that of reflex activity in a certain nervous path along which an external stimulation, falling on the central nervous system, reaches this or that organ. This conception is, it is true, old; but, nevertheless, it is the only scientific one. It is now time, however, to pass from this primitive form of the conception to a variation of it, somewhat more elaborate. The old hypothesis, it is evident, cannot embrace all the facts which have now been gathered together. I shall endeavour in a few words to supplement the former conception.

The most important point which must be emphasised and especially

[1] It is well to call attention here to Pavlov's point of view. In his lectures he is not so much interested in publishing his results as he is in evolving his ideas in his own mind. The discussion of his experiments and schemes with others, he observes, is a great aid to him in the further development of his thoughts and plans. See also the introduction to chapter xviii. Before an audience Pavlov says he is so stimulated that he is able to criticise his own ideas better than he is ordinarily.—*Translator.*

explained concerns the central part of this nervous path. As is known, the reflex path consists of the centripetal nerve, the central apparatus, and the centrifugal nerve. We will call attention especially to the central part of the nervous path. It has been noted for many years that the central apparatus must be represented as a duplex system, *i.e.*, according to the old terminology, it consists of the sensory part and the executive or centrifugal part. It was thought that the stimulation flowing along the centripetal nerve enters the nervous system, goes into the sensory cells, and from these passes over into cells of the centrifugal nerve, and so reaches that organ in which it calls out a certain response. This duplex function of the central nervous system has not been sufficiently emphasised. In many books and articles one can read about the central nervous system, but can find no adequate explanation concerning the central part of this path, of what cells it consists, etc.; on this point, there is confusion and lack of clarity. As I reviewed my collected material, it became evident that precisely here there should be no obscurity. The matter demands that this point be brought into the light, in order that it may be clearly seen that the central section of the nervous path must always consist of these two parts. Thus, in all cases we must conceive that the stimulation first of all goes along the centripetal fibres into the cells, formerly called sensory, but better designated as receptor cells; then it passes into a connecting part, and finally into the cells of the centrifugal nerves—the efferent or executive cells. I repeat that all this is not new in the scheme of cerebral construction; it has always been mentioned, but it has never been systematically emphasised. This is the most essential point that must be kept in mind in all further investigations of the various nervous phenomena. All achievements and perfections in nervous activity are located in the receptor cells, *i.e.*, at that neglected point. All the extraordinary intricacies of function and the complicated perfections of the apparatus are evidently situated in that part of the central apparatus, and not in the centrifugal part. The latter is always simpler, more stationary, and less changeable than the centripetal.

The working centre, the effector centre, is a simple one, but the receptor centre, from which the impulse is transferred to the effector centre, is highly complicated, and in location is widely diffused. If one begins at the lower parts of the central nervous system and ascends, he will be convinced that in its construction precisely the parts of the receptory centres become more and more predominant. Into these receptor centres flow all stimulations, external as well as internal, and these centres analyse everything entering the central nervous system. Wherefore the entire reflex arc must, I think, be divided into three chief parts. The first begins in every natural peripheral end of the centripetal nerve and ends in the receptor cells of the central organ. This part of the reflex arc I propose to consider and to designate as an *analyser;*[2] for the task of this consists directly in decomposing the entire world of stimulating influences falling on the organism from the outside, and the higher the animal, the finer is this decomposition. This is the first part. Next comes the part which must join the brain end of this analyser with the effector apparatus. This part may be called the coupling, connecting, or locking apparatus. Finally, the third part, which can be called the effector or working apparatus. Thus I represent the nervous path of the old reflex arc as a chain of three links—the *analyser*, the *connection* or lock, and the *effector* or working part of the apparatus.

From this point of view I turn to the centres of the cerebral hemispheres. I am inclined to suppose that the brain represents chiefly and probably exclusively (the last provisionally) the cerebral end of the analysers. Consequently, according to the old terminology, the cerebral hemispheres consist of the sensory centres; or, according to the terminology which I propose, the receptor centres; *i.e.*, the brain endings of the analysers. To support this there are reasons enough. That a considerable part of the hemispheres is composed of these analysers is clear; the occipital and the temporal regions are the centres for the eye and the ear. Those parts about which there is most controversy are the so-called motor regions, the frontal parts. Based on all that I have seen and considered concerning these latter areas I am inclined to the view that in their plan of construction they present no exception to that of the other parts of the hemispheres. Both contain receptor centres. This conception is not altogether my own. It originated in the year 1870 when Fritsch and Hitzig made their brilliant discovery.

During forty years this view has been upheld by many physiologists,

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[2] Under the name "analyser" Pavlov includes as a functional unity the surfaces of the body receiving the stimulation from the outer world (sense organs), the nerve or nerves conveying the impulse to the central nervous system, and the cells in the central nervous system to which this process flows. Although the important part of the physical analysis of the phenomena of the outer world belongs to peripheral structure of the analyser, the greatest physiological interest attaches to the cells of the central part of the analyser, especially in the highest parts of the brain. This part is supposed to be in close relation with the process; here the connections upon which the conditioned reflex depends are temporarily made and broken. A description of the analyser as a physiological unity will be found in the second part of the next chapter and in the first part of chapter xxi. More recent investigations show that the definition of the word analyser does not cover all the functions of this physiological apparatus; for it not only decomposes the outer world into its elementary and minute phenomena, but it also is endowed with the ability to unite several elementary phenomena into one complex stimulus, *i.e.*, it performs synthesis as well as analysis.—*Translator.*

and I myself am in favour of it. All of the formerly so-called motor region should from this standpoint be considered as a receptor centre like the occipital and the auditory areas, with this difference, that they are centres from another receptor surface, which has a special relation to movement. Therefore, it is not by chance that all physiologists agree that the region of the centres for the skin receptors and for the apparatus for movement coincides with the motor area. These several regions are entwined and entangled one with another. Certainly there are at present many contradictions in the facts. It is a subject of controversy, particularly with regard to the data of clinical observations. But I think that, discarding everything of a doubtful nature and judging strictly by the facts of physiological experiments, there will be no contradictions, if we accept the view that the motor region of the cerebral hemispheres is a place of receptor centres in exactly the same way as the occipital region is for the eye, and the temporal region for the ear.

No one has ever succeeded in producing a true paralysis by removal of the so-called motor region, as has been done by destruction of the spinal cord. In experimental animals, as the dog, no such paralysis appear; as soon as the operation is ended, even though it is very extensive, the animal, when free from the narcosis, begins to make movements in all of his extremities—all of his muscles are in activity, and not a single one is paralysed. One notices only that there is lack of order and co-ordination in the movements. In higher animals (primates) we see paralysis after such an operation; and in man paralysis is often observed clinically. But this circumstance does not dislodge me from the position which I hold. A paralysis, *i.e.*, the inability to move some member, as the hand or the foot, does not in the ape or man signify the presence of true paralysis. One must take into consideration the following: first, the higher the animal, the more complicated are its movements, and secondly, these movements do not exist pre-formed when the animal comes into the world, but must be elaborated by practice, *i.e.*, learned. Those reactions which we now call conditioned motor reflexes, are movements which are gradually formed—paths in the brain developed during the life of the individual. Consequently it is clear that the sudden loss of a large mass of external stimulations by means of which this or that movement is realised, results in the failure of the animal or man to make any special movements. We often meet the phenomenon of the apparent inability to move one or another muscle, *i.e.*, a seeming motor paralysis, which is in reality a paralysis of the analyser.

If we take a stand on the uniformity of construction of the brain, and if we closely consider our facts observed after removal of the so-called motor region, I believe there will be no incontestable proof of the existence of true motor centres in the cerebral hemispheres.

These few considerations represent general conceptions comprising all our accumulation of facts. They will be presented in separate reports, supporting my point of view.

CHAPTER X

NATURAL SCIENCE AND THE BRAIN

(Read before the Congress of Scientists and Physicians, Moscow, December, 1909.)

SCIENCE FACED WITH THE STUDY OF ITS CREATOR, THE HUMAN BRAIN, SHOULD NOT ABANDON ITS OBJECTIVE ATTITUDE—THE BEGINNINGS OF THIS SCIENCE OF THE BRAIN—TWO MECHANISMS, THAT OF TEMPORARY CONNECTIONS AND THAT OF THE ANALYSER—UNCONDITIONED AND CONDITIONED REFLEXES—THE FOOD REFLEX AND ITS SIGNALS—CONCENTRATION ABOUT A FOCUS OF EXCITATION—THREE KINDS OF EXTERNAL INHIBITION—INTERNAL INHIBITION—THE ANALYSER, ITS ANATOMICAL PARTS AND ITS FUNCTION (ANALYSIS)—DIFFERENTIATION BASED ON INHIBITION—THE OBJECTIVE INVESTIGATION ADVANCES—THE ACTION OF THE ANIMAL IS A SERIES OF EQUILIBRATIONS WITH THE OUTER WORLD—PAVLOV'S ATTITUDE TOWARD SCIENCE AND SPIRITUAL THINGS.

One can truly say that the irresistible progress of natural science since the time of Galileo has made its first halt before the study of the higher parts of the brain, the organ of the most complicated relations of the animal to the external world. And it seems, and not without reason, that now is the really critical moment for natural science; for the brain, in its highest complexity—the human brain—which created and creates natural science, itself becomes the object of this science.

But let us approach the matter more closely. Already for many years the physiologist has persistently and systematically investigated, according to the strict rules of biological thought, the animal organism. He has observed the vital phenomena which appear before him in time or space, and has endeavoured by means of experiments to define the constant and elementary conditions of their existence and course. His predictions and his control of vital phenomena increase steadily, just as the control of science over inanimate nature increases. If the physiologist deals with the fundamental functions of the nervous system, with the processes of stimulation and conduction, even though these phenomena still remain obscure in their nature, he maintains his methods of natural investigation, successively studying the different external influences on this general nervous process. And even more! If the physiologist deals with the lower sections of the central nervous system, if he asks how the organism by means of this apparatus responds to this or that external condition, *i.e.,* if he studies the changes of the living substance under the influence of this or that external agent, he remains the same natural investigator. The constant reaction of the animal organism to the external world, realised with the help of the lower parts of the central nervous system, is called in physiology a reflex. This reflex, as we might expect from the biological point of view, is strikingly specific—a certain external phenomenon calls forth a definite change in the organism.

But now the physiologist turns to the highest parts of the central nervous system, and suddenly the character of his research sharply changes. He ceases to concentrate his attention on the connection between the external phenomena and the reactions of the organism; and, instead of adhering to actual relations, he begins to make suppositions about the internal state of animals, based on his own subjective state. Up to this moment he had used general scientific conceptions. Now he changes front, and addresses himself to foreign conceptions in nowise related to his earlier ones, to psychological ideas; in short, he leaps from the measurable world to the immeasurable.[1] This is a step of extraordinary importance. What has brought it about? What deep-seated reasons have forced themselves upon the physiologist? What conflict of opinions preceded it? One is forced to give an unexpected answer to these questions. There is no precedent for this step in the history of science. The scientist, in the person of the physiologist, investigating the highest parts of the central nervous system, has unconsciously and without himself noticing it, adopted a usual and conventional habit—that of thinking of the complicated activity of animals as analogous to his own feelings and thoughts.

Thus the physiologist abandoned his strong scientific position. And what has he profited by this? He borrowed his conceptions from that body of knowledge concerning the human intellect which, as the workers in this field themselves admit, has not yet the right, in spite of its antiquity, to call itself a science. Psychology as the knowledge of the inner world of the human being is still at sea concerning its own essential methods. And the physiologist has taken over the thankless task of divining the inner world of the animal.

One can understand, therefore, why the study of the most complicated nervous activity of the higher animals has not made any remarkable progress, though it is about 100 years old. Since 1870, the work on the highest parts of the nervous system has, it seems, received an impetus to go forward; but, this has not placed the investigations on the great highway of science. Some basic facts were discovered during the first few years, and then the progress of investigation was again halted. Although the subject is a vast one, for more than 30 years the same old themes have repeatedly been worked over and there has hardly been any new conception. The objective physiologist of to-day must admit that the physiology of the brain is still uncertain. Thus, you see, psychology as an ally has not justified itself in the eyes of physiology.

[1] This idea is expanded in the last paragraph of chapter xvii.—*Translator.*

In such a state of affairs, reason demands that physiology return and proceed along the way of natural science. But what is it to do, then? In the investigation of the higher parts of the central nervous system it must remain faithful to those methods which it used in the study of the lower parts, *i.e.*, it must state exactly the changes in the external world and the corresponding changes in the animal organism, and discover the laws of these relations. But since these relations are evidently so fearfully complex, is it possible to make an objective record of them? To this essential question may be given only one serious answer—an assiduous and determined effort in that direction. And thus, purely objectively, the relation of the changes in the external world to the corresponding effects in the organism, is now being studied by many workers, using various species of animals.

I have the honour to present before your highly esteemed attention this attempt to investigate the most complicated activity of the higher animals, especially of the dog. Further I shall base my statements on the decade of work from my laboratory, in which a number of young workers have joined their efforts to mine in endeavouring to follow in this new path. This decade of effort now obscured by racking doubts, now (and more often) inspired by the feeling of assurance that our struggle would not be in vain—this work, as I am now convinced, has passed beyond this first hesitant stage and offers a certain and positive answer to the above question.

The entire activity of the highest parts of the central nervous system, as revealed to us by our point of view, stands before us in the form of two main nervous mechanisms: first, as the mechanism of a *temporary union, i.e.*, the establishment of a new connection in the conducting paths between the phenomena of the external world and the answering reactions of the animal organism; and, secondly, as a mechanism of *analysers*.

Let us consider these mechanisms separately. As I have mentioned above, physiology in the lower part of the central nervous system established years ago the mechanism of the so-called reflex, *i.e.*, the mechanism of a definite connection by means of the nervous system between certain phenomena of the external world and the corresponding definite reactions of the organism. As this is a constant and simple connection, it was natural to designate it as an *unconditioned reflex*. From our facts we concluded that in the higher parts of the central nervous system the mechanism of a temporary connection is realised. By means of this part of the nervous system the phenomena of the external world now excite the organism to activity, now fail to call forth a reaction, as if they did not exist. These temporary connections, these new reflexes, in contradistinction to the old ones, were called *conditioned reflexes*. What does the organism profit by the mechanism of this temporary connection? When does the temporary connection, the conditioned reflex, appear?

Let us proceed from an actual example. The most essential connection between the animal organism and the environment is that brought about by certain chemical substances which must continually enter into the composition of the given organism, *i.e.*, the connection through food. In lower animal forms a direct contact between the food and the organism leads to assimilation. In the higher animals, these relations become more numerous and far reaching. Now odours, sounds and pictures attract the animal to food substances. And in the highest of all animals, the sounds of speech, and the sight of written and printed characters disperse the human race over the whole surface of the globe in search of daily bread.

In this way numberless, various and remote external agents act as *signals* for food. They direct the higher animals to seize it, impel them to realise the food connection between themselves and the external world. *Pari passu* with this variety and remoteness is the change brought about by substitution of the temporary for the constant connection between the external world and the organism; first, because the remote connections are essentially changeable and therefore temporary, and secondly, owing to their variety and number they cannot be included in even the most comprehensive scheme.

The given food object may be now in one place, now in another, it may be at one time accompanied by certain phenomena, at another time by entirely different ones and may be a part of one or another definite system of the external world. And therefore the movement reaction of the organism towards this object must be united by a temporary connection now to this, now to that external phenomenon.

In order to render more comprehensible the second thesis—that it is impossible for the remote connections to be constant—let me make a comparison. Suppose that instead of the temporary communication which is effected for us through the central telephone station, this connection became an unchangeable one, and that all subscribers became thus permanently connected one with the others. It would be expensive, awkward, and utterly impossible. Everything which is lost in this case by a certain conditionality of the connection (one cannot be connected with every subscriber every moment), is richly compensated for by the variety and number of the possible connections.

How is this temporary connection established? How is the conditioned reflex formed? For this purpose it is necessary that the new indifferent external agent coincide in time once or oftener with the influence of another agent which is already in connection with the organism, *i.e.*, with an agent which can manifest itself in some activity of the animal.

If the condition of this coincidence is fulfilled, the new agent comes into the same connection, manifests itself in the same activity as the old one. A new conditioned reflex is thus formed through the assistance of an old one. In the higher nervous system, where the process of formation of the conditioned reflexes occurs, the procedure is as follows: If a new, formerly indifferent stimulus, entering into the cerebrum, meets in the nervous system at the moment, a *focus* of strong excitation, this newly arriving stimulation begins to concentrate, and to open a road, as it were, to this focus, and through it onward to the corresponding organ, becoming in this way a stimulator of that organ. In the opposite case, *i.e.,* if no such focus of excitation exists, the new stimulation is dispersed without any marked effect in the mass of the cerebrum. Such is the formulation of the fundamental law of the highest parts of the central nervous system.

Let me now, as briefly as possible, illustrate by facts what has just been stated about the mechanism of formation of conditioned reflexes.

Our entire work, up to the present, has been performed exclusively on a physiologically unimportant organ, the salivary gland. This choice, although at first accidental, proved in our further work to be a serviceable and happy one. Above all, it satisfied a fundamental demand in scientific thinking, *i.e.,* to begin with the simplest case; and, secondly, in this organ it was easy to distinguish between simple and complex forms of nervous activity, so that they could be readily separated for study. This has led to an understanding of the matter. Physiology had already known for many years that saliva begins to flow when food or some other stimulating substance is introduced into the mouth, and that this relation is established by means of certain nerves. These nerves receive the stimulations arising from the mechanical and chemical properties of the introduced substances, conduct them at first into the central nervous system and then to the gland, causing there a production of saliva. This is the old reflex, according to our terminology, the *unconditioned*—a constant nervous connection, a simple type of nervous activity which takes place in the same manner in animals from which the higher parts of the brain have been removed. And not only the physiologist, but every one knows that the salivary gland has very complex relations to the external world; for example, in a hungry animal or person the sight or even the idea of food causes a flow of saliva. According to the old terminology the salivary secretion is excited psychically. For such a complicated nervous activity the highest parts of the brain are necessary.

Our analysis concerning this point has directly shown that at the foundation of this complex nervous control of the salivary gland, this complicated relation to the external world, lies the mechanism of the temporary connection—the conditioned reflex, which I have described before in a general way. Our experiments brought out clear and indisputable facts. Every event in the external world, every sound, picture, and odour, everything could be brought into a temporary connection with the salivary gland, could become a stimulating agent of salivary secretion, provided it coincides in time with the unconditioned reflex, with the salivary secretion provoked by the presence of substances in the mouth. In short, we can form as many and as varied conditioned reflexes on the salivary gland as we wish.

At the present time the subject of the conditioned reflex, only on the basis of the work of our laboratories, fills an extensive chapter, with a mass of facts and a number of exact rules connecting them. The following is only the most general outline, only the headlines of this chapter.

First of all, there are numerous details concerning the speed of formation of the conditioned reflex. Then follow various sorts of conditioned reflexes, and their general properties. As the conditioned reflexes are in the highest part of the central nervous system, in which there is a constant collision of innumerable influences from the external world, it is comprehensible that among the different conditioned reflexes there is an incessant struggle, a choice among them at any given moment. Consequently there are constantly arising cases of inhibition among these reflexes.

Three kinds of inhibition have now been established: *simple inhibition*, *extinguishing inhibition*, and *conditioned inhibition*. Altogether they form the group of *external inhibitions*,[2] for they are based on the addition of an external agent to the conditioned stimulus. On the other hand, a previously formed conditioned reflex, owing to the effect of its internal relations alone, is subjected to continual fluctuations, even to temporary complete disappearance, *i.e.*, it is inhibited internally; and this constitutes *internal inhibition*. If, for example, even a very old and strong conditioned reflex is repeated several times without being accompanied by the unconditioned reflex by help of which it was formed, it immediately begins to lose its strength, and more or less quickly, but gradually, falls to zero; *i.e.*, if the conditioned reflex as a signal of the unconditioned signalises falsely, the former immediately and steadily loses its stimulating effect.[3] This loss of effectiveness of the conditioned reflex comes

[2] At this stage of the experimental work the basis for classifying the several kinds of inhibition was the presence or absence of an external stimulus. All inhibition which was produced by external agents was put in the class of external inhibition. Further work brought about some changes in this scheme—for example, "conditioned inhibition" proved to belong to the group of internal inhibitions. For the present classification of inhibitory processes the reader may consult the first part of chapter xix.—*Translator.*

[3] The reader will remember that this process is called *extinction* of the conditioned reflex.—*Translator.*

about not by its destruction, but by its internal inhibition; for a conditioned reflex which has been extinguished in this way, after some time becomes restored *per se*. There are still other cases of internal inhibition. In further experiments a new and important side of the problem has been clarified. It has been proved that besides stimulation and inhibition of stimulation there is as often an *inhibition* of *inhibition*, in other words, "*dis-inhibition*."[4]

It is impossible to say which of these three acts is the most important. One must simply state that all the highest nervous activity, as it manifests itself in the conditioned reflex, consists of a continual change of these three fundamental processes—*excitation, inhibition,* and *dis-inhibition.*

I pass now to the second of the above mentioned fundamental mechanisms, the mechanism of the *analyser.*

As mentioned before, the temporary connection proves to be necessary as soon as the relation of the animal to the external world becomes complex. But this great complexity of the relations presupposes the ability of the organism to decompose the external world into separate parts. And in fact every higher animal possesses manifold and delicate analysers. They are what until now have been called the sense organs. Physiological teaching about these latter, consists, as the naming of the organs themselves shows, for the most part of subjective material, *i.e.,* it springs from observations and experiments on sensations and conceptions of the human being, and thus it has been deprived of those extraordinary means and advantages of objective science afforded by animal experimentation. It is true that this region of physiology, thanks to the interest and participation of some brilliant investigators, belongs in many respects to the most elaborated branches of physiology, and contains much data of great scientific importance. But these elaborations of research concern chiefly the physical side of the phenomena in the organs, for example, they have to do with the conditions for the formation of a clear picture on the retina. In the purely physiological part, in the investigation of the conditions and kinds of irritability of the nerve endings in a given sense organ there is a mass of unsolved problems. In the psychological part, *i.e.,* in the teaching which concerns itself with the sensations and the perceptions produced by the stimulations of the organs, in spite of the accurate observations and the ingeniousness of the investigator, there have been established only elementary facts. Evidently, what the genius Helmholtz referred to as "unconscious conclusion" corresponds to the mechanism of the con-

[4] The English prefix *dis* seems to correspond best to the Russian prefix *ras* which Prof. Pavlov combines before the word for inhibition. Prof. Pavlov's term is *rastormazhivanie,* which literally translated, means *unbraking.—Translator.*

ditioned reflex. When, for example, the physiologist says that for the formation of the conception of the actual size of an object there is necessary a certain length of the image on the retina and a certain action of the internal and external muscles of the eye, he is stating the mechanism of the conditioned reflex. When a certain combination of stimuli, arising from the retina and ocular muscles, coincides several times with the tactile stimulus of a body of certain size, this combination comes to play the rôle of a *signal*, and becomes the conditioned stimulation for the real size of the object. From this hardly contestable point of view, the fundamental facts of the psychological part of physiological optics is physiologically nothing else than a series of conditioned reflexes, *i.e.*, a series of elementary facts concerning the complicated activity of the eye analyser. At present, there is here, as everywhere in physiology, much more unknown than known.

The *analyser* is a complicated nervous mechanism beginning with the external receiving apparatus and ending in the brain, now in its lower, now in its higher sections; in the last case, it is much more complex. The fact on which the physiology of the analyser is based is that every peripheral apparatus is nothing more than a special transformer of a certain given external energy into a nervous process. And there follows a long series of questions entirely undecided or only touched upon. What processes are involved in this transformation? Upon what depends the analysis? Which part of the activity of the analyser is to be attributed to the construction and process in the peripheral apparatus, and which part to the construction and process in the cerebral ending of the analyser? What consecutive phases does this analysis show, starting from its simplest, and proceeding to its highest stages? And finally, what are the general laws governing this analysis? At the present time all these questions have been put to a purely objective test in animals by means of the method of conditioned reflexes.

In establishing a temporary connection between a given phenomenon of nature and an organism, it is easy to determine in what degree the corresponding analyser of the animal is able to decompose the external world. For example, in the dog it can be shown without special difficulty that his ear analyser can differentiate the finest timbres and the separate parts of composite tones, and not only differentiate but retain this differentiation (in man called "absolute pitch"), and that his ability to distinguish high pitches is much greater than that of man; he reacts to vibrations of 70 to 80 thousand per second, whereas the limit of the human ear is not higher than 40,000-50,000.

Besides this, in the objective investigation, there appear the general rules according to which this analysis is performed. The most important of them is the gradation of analysis. It is, therefore, in the conditioned reflex, in the temporary connection, that the given analyser takes part at first as a whole in its more general and gross activity, and only afterwards, thanks to its gradual differentiation, does its activity become more delicate and refined. For example, if a bright figure appears before the animal, then at first every reinforcement of the illumination acts as a general stimulus and only afterwards is it possible to elaborate a special conditioned stimulus from the size, form, intensity, etc., of the figure.[5]

Further, in such experiments on animals with conditioned reflexes, the fact has been brought out that differentiation comes about as a result of an inhibitory process, probably through a suppression of all other parts of the analyser except the definite part concerned. Upon the gradual development of this inhibitory process depends the gradual analysis. That this is true can be proved by many experiments. I shall give one such as a convincing example. If one breaks down the balance between the excitatory and the inhibitory processes in favour of the former, by introducing into the animal a stimulant, such as caffein, then a well elaborated differentiation is immediately and seriously damaged, and in many cases it completely disappears, although temporarily.

Objective investigation of the analysers has also manifested advantages in experiments with partial extirpation of the hemispheres. These experiments disclosed an important and exact fact; the greater the damage to the cerebral end of the given analyser, the less delicate is its work. It continues to enter into a conditioned connection as formerly, but only through its more general activity. For instance, after considerable destruction of the cerebral end of the eye analyser, one or another intensity of light can easily be made a conditioned stimulus, but definite objects, combinations of light and shadow, lose their specific stimulating effect.

In concluding the presentation of some of the facts of this new subject, I feel constrained to mention certain characteristics of the work in this field. The investigator constantly feels that he is standing on very fertile and solid ground. Questions besiege him on all sides, and his task consists in reducing them to a logical and natural order. Notwithstanding the speed of the research, its progress is sure. One who has not proved these facts himself can hardly believe how often these relations, enigmatic as they seem from the psychological point of view, are subject to a clear and successful objective physiological analysis, easily controlled at every step by suitable experiments. Again and

[5] This process in which the power of stimulation belongs only to that single agent coinciding with the unconditioned reflex and which is lost by all the neighbouring stimuli is called *differentiation*. See footnote 4, chapter iv, and the accompanying paragraph.—*Translator.*

again the worker in this field is struck by the incredible power of objective investigation in this new domain of complex phenomena. I feel assured that every worker in this field will be seized by an extraordinary interest and passion for investigation.

Thus, on a purely objective scientific basis the laws of complex nervous activity are elaborated, and the secrets of its hidden mechanism are gradually revealed. It would be an unjustifiable pretension to assert that by the two mechanisms described above all the higher nervous activity of the higher animals is once and forever exhausted. But this is not important. The future of scientific investigation is always obscure and is replete with surprises. The essential point is that on a purely scientific basis, under the guidance of purely scientific conceptions, there is instantaneously opened up an unlimited territory for investigation.

With this conception of the complex nervous activity of the animal organism, the most general statements concerning it are thoroughly compatible. As a part of nature every animal organism represents a very complicated and closed system, the internal forces of which, at every given moment, as long as it exists as such, are in equilibrium with the external forces of its environment. The more complex the organism, the more delicate and manifold are its elements of equilibration. The analysers and the mechanisms of constant as well as of temporary connections, serve for this purpose, they establish the most precise relations between the smallest elements of the environment and the finest reactions of the animal organism. In this way then is all life, from that of the simplest to the most complex organism, including man, a long series of more and more complicated *equilibrations* with the outer world. The time will come, be it ever so distant, when mathematical analysis, based on natural science, will include in majestic formulæ all these equilibrations and, finally, itself.

When I say this, I should like to anticipate what might be misunderstood in these statements concerning my views. I do not deny psychology as a body of knowledge concerning the internal world of man. Even less am I inclined to negate anything which relates to the innermost and deepest strivings of the human spirit. Here and now I only defend and affirm the absolute and unquestionable right of natural scientific thought everywhere and until the time when and where it is able to manifest its own strength. And who knows where its possibilities will end!

In conclusion allow me to say something about the accoutrement of the investigator in this new region.

He who would venture into this new field, who would register the influences of the environment on the animal, has need of an exceptional equipment. He must hold in his hands all the external influences. Wherefore it is necessary for this investigator to have a special and new type of laboratory: first, one in which there are no accidental sounds, no sudden light changes, no accidental air draughts, etc., where in short he may govern as far as possible the constancy of all external conditions; and secondly, one in which he has at his command a supply of the generators of many kinds of energy, capable of being varied by corresponding analysers and measuring instruments. Here must ensue a rivalry between the contemporary technic of the physical apparatus and the perfection of the animal analyser. Here will be a close alliance between physiology and physics, from which, I think, even physics will gain not a little.

Under the conditions of our present laboratories, not only is the work of which we speak often limited against our will, but it is also almost always difficult for the experimenter. For weeks he may have prepared for an experiment, and at the decisive moment, when he impatiently awaits the answer, an unexpected vibration of the building, or a noise from the street, etc., destroys his hopes, and the desired answer must be postponed for an indefinite time.

A proper laboratory for this investigation is of great scientific consequence, and it is my wish that in this country, where the foundation of such an investigation has been laid, there could be erected the first appropriate laboratory, in order that this highly important scientific undertaking should become our own and to our credit.

With a feeling of pride, I announce that my native country has answered promptly my call for a new type of laboratory. The Ledenzov Society has reacted so energetically that the Institute of Experimental Medicine has now begun the construction of such a laboratory.[6]

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[6] The work on this laboratory was interrupted by the war and revolution. During the worst year, although Prof. Pavlov's attendance was carried out with military punctuality, even when he had to walk through snow and ice, few successful experiments could be performed on account of the lack of light, fuel and food. Starving animals had to be fed to the surviving ones, which were often carried home with the investigator to share with him his meagre ration and to keep them from freezing. Pavlov alludes to these difficulties in the Preface to the first edition. After 1918 all laboratories have been supported by the State. Since 1923 increasing funds have been allocated to Pavlov's laboratories so that now they are excellently equipped and well provided for.—*Translator.*

CHAPTER XI

THE TASK AND THE ARRANGEMENT OF A LABORATORY FOR THE STUDY OF THE NORMAL ACTIVITY OF THE HIGHEST PARTS OF THE CENTRAL NERVOUS SYSTEM IN THE HIGHER ANIMALS

(Read before the Ledenzov Society for the Advancement of the Experimental Sciences and Their Practical Application, Moscow, 1910.)

THE FUNCTION OF CONDITIONED REFLEXES, THEIR RISE AND FALL—EXPERIMENTAL PRODUCTION OF SLEEP—THE SLEEP REFLEX; GENERAL INHIBITION—THE FOCUSING REFLEX—EXTINCTIVE INHIBITION—SIMPLE INHIBITION—INTERNAL INHIBITION—THE CONDITIONED INHIBITOR—DIS-INHIBITION—EXPERIMENTAL DIFFICULTIES—THE ANALYSERS—ANALYSIS; TIME AS A CONDITIONED STIMULUS—THE EAR ANALYSER—REQUISITES OF THE CONSTRUCTION AND EQUIPMENT OF A LABORATORY—GRATITUDE EXPRESSED TO THE LEDENZOV SOCIETY.

In the following short account, which is prompted by the desire to describe to you a new type of laboratory, it is impossible to present even in its most general features the entire new chapter of animal physiology, and to demonstrate the most important landmarks in the analysis of the most complicated vital phenomena. But the groups of facts with which we are concerned in this lecture testify, I think, in a high degree to the positive and exact knowledge of the animal organism derived from the laboratory investigations.

An enormous part of the external visible activity of a normal higher animal appears to me in the main as a series of countless conditioned reflexes—temporary connections between the activity of the skeletal musculature and the most diverse and minute elements of the external world upon which this activity is directed, in order to introduce food into the organism, to remove destructive influences, etc. I shall not dwell, however, on this part of the most complicated vital activity, *i.e.,* on the circumstances of formation of the conditioned reflexes and their properties, but I address myself directly to the other part of this activity. The external world perpetually calls out, on the one hand, conditioned reflexes, and on the other hand, continually suppresses them, submerges them through the action of other vital phenomena. This rising and sinking of the conditioned reflexes responds at any given moment to the demand of the fundamental law of life—*equilibration with surrounding nature.* This is adjusted and accomplished through the different kinds of inhibition of the conditioned reflexes. It is precisely these inhibitions which concern us to-day.

The constant subject of our investigation has been the conditioned reflex, the temporary connection of various external agents with the activity of the salivary gland—an organ which is at the entrance to the digestive tract of the animal organism, and bears the same relations to the external world as skeletal muscle, but is infinitely simpler in its rôle, and in its connections with the organism. Wherefore its advantages for investigation. Various external agents, various sounds, lights and pictures, various odours and all kinds of mechanical and thermal stimuli for skin receptors—all these which were formerly indifferent towards our gland (*i.e.*, left it in a state of rest) could be transformed into temporary stimuli, into agents which cause it to elaborate its usual secretion. This is attained by uniting exactly simultaneously several times the action on the animal of one of these agents with the action of the usual physiological stimulus of the organ; such physiological stimuli are all kinds of food or various inedible substances forcibly introduced into the mouth of the dog. And now, under what external conditions or in the presence of what internal state of the animal does our conditioned stimulus lose its habitual elaborated action? The number of these conditions is certainly very great, even though they are not all known. And I shall treat only of those facts which have been established with more or less certainty.

For many years one or another of my collaborators has complained during his work with conditioned reflexes that the experimental animal became sleepy. This state rendered impossible further study of the investigated phenomena for the simple reason that they disappeared. The difficulty was especially marked when as a conditioned stimulus we used thermal irritation of the skin—either heat at 45°C. or cold at about 0°C. In the last case the experiment ended in a deep sleep and complete cessation of the complex nervous activity of the animal. There even grew up a prejudice in the laboratory against working with thermal agents. But the difficulty could only be neglected temporarily, for the matter in its very nature directly related to our problems.

As we concentrated our attention on these phenomena, we at last discovered their mechanism. Many years ago we had been surprised by the contrast between the great animation and liveliness of some of the dogs before the experiment and the drowsiness which appeared soon after the beginning of the experiment. It is clear that something during the experiment causes the sleepy state. But the experiment consisted in nothing more than feeding the dog at very short intervals with small amounts of food, or in putting weak acid into his mouth during the thermal cutaneous stimulation. As neither the food nor the acid could produce the sleepiness, the cause is to be sought only in the action of the thermal agent. As a result of different forms of experiments, it became evident that action by one and the same degree of heat or cold on one and the same area of the skin—if these agents act for a short time but are often repeated, or better still if they act continuously for a long period—leads sooner or later to a drowsy state of the previously lively dog and even to a deep sleep. It became clear that a definite agent in the outer world may condition the state of rest of the animal and the suppression of his higher nervous activity affecting in the same sure way as other agents evoke one or another manifestation of his complex nervous function. In other words, besides the different active reflexes there is also a passive *sleep reflex.*

The outer world forces the animal at one time into all sorts of activity, necessarily connected with a destruction of the living substance, but at another time when such an activity owing to the conditions of the moment is superfluous the same external world with the same imperativeness compels the animal to rest, which insures the restoration of the living substance destroyed during activity. And only in this way does the perpetually changing physico-chemical system of the organism remain intact and preserve its identity. That sleep as an inhibition of the higher nervous activity can be conditioned, not only through the accumulation of the products of activity, but also by a particular reflex stimulus, is supported by our observations on other kinds of proved inhibitions which pass over in a truly astonishing manner into drowsiness and sleep. I believe that on this way of investigation, and not behind mountains of obstacles, lies the solution of all the unexplained mysteries of hypnotism and its related states.[1] If ordinary sleep is an inhibition of the whole activity of the higher parts of the brain, then hypnotism must be a partial inhibition only of its different departments. This episode of the sleep reflex is one of many examples met with during investigations by the objective method, *i.e.,* a method showing that it takes into consideration all the influences of the external world on the organism, no matter how minute or fleeting they may be, and that investigation by this method already partly embraces, and will finally completely embrace the activity of the organism.

The *sleep reflex* is only one kind of inhibition of conditioned reflexes. Inhibition which is induced by the sleep reflex is called by us *general inhibition,* for it inhibits also other complex nervous phenomena besides those concerned here. At every moment there is manifested in our experiments another fact of directly opposite character, *viz.,* a positive active response of the animal to every fluctuation in the environment. Every sound, be it ever so small, appearing in the midst of habitual sounds and noises which surround the dog, each weakening or reinforc-

[1] This turned out to be true. For the results of Pavlov's studies in hypnotism, see chapter xxx.—*Translator.*

ing of these constant sounds, each change in the intensity of the room illumination (the sun becoming hidden by the clouds, a sunbeam suddenly breaking through, a flickering of the electric lamp, a shadow across the window), the appearance of a new odour in the room, a warm or cold current of air, something touching the skin of the dog, as a fly or a falling speck of plaster from the ceiling—in all these and in endless like cases, there fatally begins an activity of one or another of the skeletal muscles of our animal, as of the eyelids, eyes, ears, nostrils; or the head or the trunk or some other part of the body will turn and take a new position; and these movements are either repeated and reinforced, or the animal becomes fixed in a certain pose.

We have before us again a special reaction of the organism, a reflex of the simple kind which we call an *orienting* or *focusing* reflex. If in the surroundings of the animal there appears some new agent (by this I include changes in the intensity of previously acting agents), then the corresponding receptor surfaces of the organism become focused on it, in a manner which will bring about the most favourable stimulation. This focusing is accomplished through the activity of points in the central nervous system. The stimulated points in their turn, according to the general law of *reciprocal* action of the nervous centres, as already established for the lower parts of the central nervous system, inhibit our conditioned reflex. All the current activities of the organism must give way to these extraordinary demands of the external surroundings.

In our present laboratory this is the most troublesome, the most insurmountable cause of disturbance of our basic phenomenon, the conditioned reflex. Of course, this phenomenon must itself be studied in detail, and it is being so studied; but on the other hand, it is a great impediment to the examination of the various other sides of our chief phenomenon, making it either more difficult or even impossible.

Now every new factor arising in the surroundings, if repeated at short intervals and unaccompanied by any further direct influence on the animal, becomes more and more indifferent. The orienting reflex which it calls out becomes weaker and finally disappears, and with it also disappears the inhibitory action on our conditioned reflex. Therefore, this kind of inhibition we call *extinctive inhibition*. On this extinction is based the fact that the constant composition of the surroundings remains without apparent effect on the animal. In certain kinds of experiments we intentionally apply a repetition of the stimuli which produce the extinctive inhibition in order to render these agents indifferent. But it is evident that they cannot be removed in this way altogether and forever; they are countless, and after a certain length of time, if not repeated, they are restored.

In the same class with the extinctive inhibitions must be placed the effects of many agents of the external world having a special relation to the organism, *i.e.*, definite inborn reflexes or other conditioned reflexes. All extremely strong stimuli, strong light, sudden noises, etc., provoke special reactions, as, for example, shivering or trembling of the animal, the reaction of running away, trying to break away from his stand, or the opposite cataleptic-like state; on the other hand, the sight and sound of persons having a certain relation to the experimental animal, or the sight and sound of other known animals, and various things of the same sort, condition every previously elaborated response on the part of the animal. All these reactions are certainly connected with the activity of definite parts of the central nervous system, and this activity inhibits, according to the aforementioned law, the activity which we are investigating.

The above mentioned reactions are often stronger and more constant than the simple orienting reflex, though they also lose by repetition their inhibitory action; they must therefore be regarded as a kind of extinctive inhibition. In order not to be disturbed by this sub-group of extinctive inhibitions, it is necessary as a rule to avoid them; for the gradual weakening of their effect through repetition requires much time.

But there is one more essential point; one cannot always understand the real significance of the given stimulus for the animal. Is it possible to become acquainted with all the accidental connections with the outer world which our dog has formed before he enters the laboratory? Furthermore, it is impossible to find in any reference a full enumeration of the inborn reactions of the dog. In the majority of cases there arises the question, Is the given reaction inborn or acquired?

There is in addition a number of external influences which have in greater or less degree a destructive effect on the organism. If the fixation of the animal on the stand is connected with very strong pressure on any part of the body, or if the thermal or mechanical apparatus attached to the skin damages its integrity (slight excoriation or burning), if the introduction of some irritant into the mouth causes an injury of the mucous membrane, even to a minor degree, in all these and similar cases our conditioned reflex will suffer and finally disappear entirely. Evidently any threatening destruction of the organism provokes a *defence* reaction on the part of the animal, in the form of one or another movement to get rid of the destructive agent, and, according to the general rule of the reciprocal action of the nervous centres, it inhibits our special complicated nervous activity, our conditioned salivary reflex. This kind of inhibition we call *simple inhibition,* because it arises at once as soon as the cause is present, and it remains constant, and disappears with the cause. As inhibitions of this sort may be considered certain, other internal physiological phenomena, having at a given moment a predominant importance for the organism, such as overfilling of the bladder, stimulate the nervous apparatus which controls its emptying.

The most thoroughly studied members of this group of inhibitions are the physiological factors acting upon that organ with which we are constantly concerned in our investigations, *viz.*, the salivary gland. This gland serves for the physical and chemical elaboration of food as well as for cleansing the mouth of inedible and injurious substances. The activity of the gland differs in these two cases, and is stimulated from special nervous centres under the influence of the corresponding stimuli. Between these two centres there is the same antagonism as between all others. The unconditioned reflex on inedible substances inhibits the conditioned reflex on food, and vice versa. This inhibition arises at once and also remains constant as long as the cause is effective.

As one can see from this brief review, a long series of external and internal influences are entangled with that complex nervous activity under investigation—the conditioned reflex. But in order to appreciate in its full extent the significance of the enumerated moments for this activity, it is necessary to examine in some detail other sorts of phenomena which are closely connected with the conditioned reflexes.

If the formation of a temporary connection between external phenomena and the corresponding reactions of the organism be considered an expression of the perfection of the animal machine, a manifestation of the more exact equilibration of the organism with the external world, even higher is the perfection manifested in those fluctuations to which this temporary connection is subjected through the internal mechanism of the nervous system.

If a certain agent, our conditioned stimulus, replacing and signallising food and provoking the corresponding reaction of the organism—in our case the salivary secretion—proves to be in contradiction to reality, *i.e.*, if it fails to coincide several times with eating, it gradually loses its stimulating effect. This result is brought about not through the destruction of the salivary reflex, but through its temporary inhibition by means of a special internal process. Likewise, if a conditioned stimulus coincides with the unconditioned (from which it received its stimulating effect) only during a certain moment of its presence, its stimulating action is also inhibited until this certain moment arrives. The physiological meaning of this is very simple: why should an activity take place at all, if under the given circumstances it is unnecessary? This inhibition of the temporary connection, of the conditioned reflex, we call *internal inhibition*, in contradistinction to those inhibitions which we have described as external.

We must consider a special condition in the presence of which internal inhibition occurs. If some absolutely indifferent agent coincides several times with a conditioned stimulus, when the latter happens not to be accompanied by the unconditioned reflex by means of which it had been formed, there develops an internal inhibition, *i.e.*, the given combination gradually loses its stimulating action which belonged to the conditioned stimulus alone. This additional, formerly indifferent agent, owing to which the conditioned stimulus in the combination has gradually lost its stimulating effect, we call a *conditioned inhibitory stimulus,* or an inhibiting agent, or *inhibitor.* This agent is now truly an inhibiting agent; for when joined to every other conditioned stimulus which is based on one and the same unconditioned stimulus, it inhibits it from the first test. One may suppose that the conditioned inhibiting agent is, in a certain degree, a stimulator of the process of internal inhibition, and that the entire mechanism of the conditioned inhibition is, in a certain measure, a mechanism of a negative conditioned reflex. That this is actually true is shown by our latest experiments, in which, owing to repeated temporal coincidences of the indifferent agent with the processes of internal inhibition, the indifferent agent becomes a conditioned inhibiting agent.

Internal inhibition, as appears from our work, plays an important part in the manifestation of the most complex activity of the central nervous system. It always accompanies, for example, the discriminative activity of the nervous system.

What this internal inhibition really is, remains obscure; but the obscurity does not give sufficient ground for doubting the possibilities of its detailed study. Here, as everywhere in natural science, investigation begins with the statement of the fact itself, and the systematisation of its various modifications under different conditions. This will later give us material on which may be based real conceptions of its mechanism. Thus we know at present that the process of internal inhibition is a much less stable process than that of excitation. Already we have some information about the quantitative relation between the intensities of these two processes.

This process of internal inhibition can itself be inhibited, just as the process of conditioned stimulation can. So we have inhibition of inhibition, in other words, *dis-inhibition, i.e.,* a freeing of the inhibited processes of the conditioned reflexes. As such, inhibitors of the process of internal inhibition (*dis-inhibiting agents*), appear to us all those agents which I have before described as inhibiting agents of the conditioned stimulus.

I fear that this frequent repetition and compounding of the word "inhibition," this accumulation of "inhibitions," will produce an unfavourable impression, and will obscure the real nature of the matter. Let us clarify it by a concrete example. Take one of our conditioned stimuli, an organ tone of 1,000 vibrations per second. Thanks to its repeated coincidence with the feeding of the animal the tone produces now of itself a flow of saliva; it is a conditioned stimulus of our gland. Now I repeat it several times without accompanying it with feeding. As I have mentioned, it gradually loses its stimulating effect, and becomes indifferent for the gland. The mechanism of internal inhibition has made it ineffective, it is *internally inhibited.* At last I add to the tone which has been made thus ineffective, some new agent, for example, the flash of an electric lamp before the eyes of the dog. This stimulus has never before had any relation to the secretion of the gland. And I immediately see that the extinguished conditioned stimulus again recovers its action; saliva flows, and the dog, which during the sound of the tone, was indifferent or even turned his head away from the experimenter, now turns toward him and licks with his tongue as he is wont to do before eating. One can understand this only in the following way: the flash of the lamp inhibited, removed, the internal inhibition, and thus dis-inhibited and restored the conditioned reflex. In exactly this way occurs dis-inhibition in other cases of inhibition. Thus is conditioned inhibition dis-inhibited. It is a special case of internal inhibition.

But here there may arise some misunderstanding: if, as I have claimed, both the reflex and its inhibitors can be inhibited, what is the result of dis-inhibition, *i.e.,* what can be set free if our inhibitory stimulus inhibits the reflex itself?

A simple solution of the matter consists in the following: as I have mentioned, the process of internal inhibition is more labile (unstable) than that of stimulation; and hence there can always be found such intensities of the new external inhibitory agent as are just sufficient to inhibit internal inhibition, but not strong enough to suppress the constant, more stable process of the conditioned excitation. In this case, then, only dis-inhibition occurs. In other words there is a graduated series of inhibition intensities—an *ineffective,* a *dis-inhibiting,* and an *inhibiting.*

I cannot enter into particulars here, but allow me to testify that the study of the complex nervous phenomena at this point, with their regular changes wholly dependent on the strength of the stimuli, has produced in me some of the strongest impressions I have ever experienced in my scientific life. And I only assisted in these experiments; they were performed by one of my young and active collaborators, Dr. I. V. Zavadsky.

As all of the above-mentioned inhibiting agents of the conditioned reflexes, at a certain degree of intensity become agents of inhibition of internal inhibition, *i.e.*, dis-inhibiting agents, their importance for the study of the complex nervous activity of the animal is doubled. In order to manage the investigation completely, in order not to be dependent every moment upon accidents, one must hold these inhibiting agents constantly under his own control.

Here one must take into account the *extinctive inhibitions;* for their presence may be accidental and entirely independent of our wishes. Notwithstanding great care in making observations, it is always difficult to locate in the mass of stimuli falling on the animal that new agent having the inhibitory effect. Without doubt the receptor processes in the animal are finer, more exact, and more extensive than they are in man; for his higher nervous activity, having to do with elaboration of the incoming nervous material, suppresses the lower nervous processes, which are concerned with the simple reception of the external stimuli.

We noted that the new unexpected agent always influenced either the conditioned reflex or its internal inhibition, and in this way diverted the course of the experiment. If only a separate isolated factor is involved, the loss is not great. You can repeat this experiment at the next time, hoping that there will be no trouble. But if you are conducting a long experiment, consisting of successive stages, the loss is considerable. A series of phenomena has to be formed in a certain order, and therefore a long period is necessary to prepare for the repetition. But this is not the worst case! Often when one has prepared weeks and months for an experiment, in the critical moment when one is awaiting the decisive fact, the result is obscured through an accidental inhibiting agent. Only the repetition of the experiment, after waiting for weeks, and with new conditioned reflexes, may rectify the matter. The nervous phenomena which we are studying are characterised by their transitoriness; every moment and with every condition they take a new turn. And therefore it may happen that the new combination under examination, which was disturbed during the first trial, does not occur in the same virgin state when the experiment is repeated after weeks of waiting. All these phenomena which we have just considered belong to one group.

Now let me ask your attention to the work of the *analysers.* These are nervous mechanisms whose duty it is to decompose the complexity of the external world into its elements, and to receive these elements as well as all their combinations. I shall illustrate by the ear analyser because it is the one on which we made the most numerous investigations. In a previous address I have mentioned that this analyser easily differentiates very delicate fractions of tones, and that the dog's range of discrimination (70 to 80 thousand vibrations per second) is much greater than that of man.

Especially marked is the ability to discriminate between different intensities of tones. It is not difficult to make from one and the same pitch many conditioned stimuli; for example, if a small intensity of a certain tone is made a conditioned stimulus, a greater intensity of the same pitch is without the least effect. These intensities may be so slightly different that the human ear can hardly note the difference when they are repeated with a very short interval between them, or it does not discriminate between them at all, though the analyser of the dog can differentiate them when they follow one another at an interval of hours.

Unfortunately the imperfection of physical instruments sets a limit to this sort of experiment. We could not be certain, using our imperfect apparatus, whether only the strength of the tone changed or also its pitch and its quality; further, we could not control the absolute intensity of the sound. And, as I have already said, this point is of great importance for the ear analyser.

Certainly the analysis of intensity, the measuring of the strength of the external agent, is the most elementary analysis, and as we know from general physiology of the nerve, it is peculiar to the simplest element, the nerve fibre. One may suppose that in the animal the analysis of intensity lies at the basis, certainly in part, of the perception of time. The following statement may be made: whether an external agent of uniform and constant intensity acts on a given analyser of the animal, or whether the trace of an interrupted real stimulation gradually becomes obliterated in the nerve cells, every intensity of the stimulated state of the cell at any separate moment is a special element which is differentiated from all grades of intensity preceding it as well as all grades of intensity following it. Through these elements as units, time can be measured, and each separate moment of time can be signalled in the nervous system. Time itself, however, must be investigated, as we shall constantly use it in our experiments as a conditioned stimulus.

Not less delicate is the differentiation of the intervals of time, *i.e.*, the pauses between the separate sound stimuli. The ticking of the metronome (100 per minute) can be made a conditioned stimulus. After some practice the ear analyser of the dog can differentiate even after a lapse of 24 hours between a metronome beat of 104 and one of 96, *i.e.*, an interval of 1/43 of a second. The human ear cannot distinguish between these two metronomes without counting, even after the interval of one minute.

The investigation of the ear analyser of the dog has been still further varied; differentiation was elaborated with different orders of succession of one and the same tone, and secondly, with the introduction of different long pauses between the same tone and between different tones. I shall say a few words about the first case. A conditioned stimulus was made in the dog by a series of four ascending tones. A differentiation was elaborated between this series and a series of the same tones used in descending order (of pitch). From four tones one can make, as is known, 24 permutations. An interesting question arose: how will the ear analyser react to the remaining 22 permutations of these tones? It proved that the analyser of the dog divided them into exactly two equal groups; to one, the nervous system reacted as to a stimulus, to the other, it remained indifferent; the first were referred to the group of ascending tones; the latter, to the group of descending. The examination of the tones in these permutations showed that whilst in one group the number of ascending tones was predominant, in the other, the descending tones were preponderant.

However, this is only the beginning of the study of the analyser! As a final ideal all these countless manifestations of the external world falling on the ear analyser (which are made use of by the organism in its most refined adaptations and relations to its environment) must be studied and systematised. Also the same must be done with the other analysers of the animal.

I have now finished the enumeration of facts necessary for the solution of our problem. The question arose: what means and equipment must the investigator have at hand to proceed along this new path without serious setbacks and with a chance of success? I have so chosen my facts that the answer based on these facts does not present difficulties. The first and chief condition consists in an entirely new type of laboratory. Above all, and this is by far the most essential, this building must be insulated from outside sounds, from both the street and the neighbouring rooms. And this must be carried out in spite of the many connections that are necessary between different parts of the building. I do not know how far it is technically possible, but the ideal requirement of this building or at least of some of its rooms would be complete exclusion of all extraneous sounds.* Even an approach to these ideal conditions would considerably lessen the difficulties of the investigator. The other prerequisites of this building do not represent such great obstacles. It should be uniformly illuminated. This can be done by constant and equal artificial lighting, or by changing the natural illumination so as to compensate for weather conditions and to make it correspond to the artificial lighting. Finally, there must not be any draught during the course of the experiment which brings an odour, cold, or warmth.

* Compare the famous *camera silenta* of Zwaardemaker.

Only such a building can free the mind of the investigator from the continual anxiety that some accidental and unexpected stimulus will wreck his projected experiment; only in such a building can a needless loss of time and trouble and worry be saved, and the experimenter be given the possibility of investigating his problem with exactitude.

The second demand concerns the equipment of the laboratory with exact instruments of wide variety by which it should be possible to stimulate the receptor surfaces of the animal in numerous ways. The apparatus should be capable of regulating the intensity, duration, and frequency of the conditioned stimuli, and should include electrical, mechanical and thermal agencies placed in a special room of the laboratory or in a special building in the neighbourhood of the laboratory. Other parts of the apparatus must be within the experimental room, such as different sounds, lights, pictures, odours, thermal influences, etc. In short, the experimenter must be able to reproduce before the dog the external world and its variety. This is an immense technical task which one must execute if he would work under ideal conditions. But if accomplished, it will bring its rewards in future results.

The third requisite is simple, and is easily carried out, but none the less essential. After the above conditions have been fulfilled, *i.e.*, when every slightest sound, every fluctuation of the illumination has been excluded, it is clear that for our further success provision must be made for the health and welfare of our experimental animals. Often under the present working conditions they fall into one or another kind of disease. A glaring inconsistency exists when we devote every attention to the exclusion of foreign stimuli, but disregard the state of the animal itself; for example, the dog may be suffering from some skin malady, or rheumatic pains. To our sorrow it may happen that we are forced to discard an animal having many elaborated reflexes (and their formation often demands months or years of time and work), because it has been badly housed or looked after. For the success of our experiments it is necessary to have a large, bright, warm, dry and clean building for our animals, and such does not now exist in physiological laboratories.

If the scientific rights of our new field are granted, and it seems to me that the facts already obtained sufficiently demand it, the laboratory just described is an urgent necessity for the advance of our research. This is my conviction, the conviction of one who has for many years constantly considered and thought over this matter. I was very happy and deeply grateful when my conviction, my wish, and my scientific efforts in this society to which I have the honour to speak, met with such an enthusiastic response.

A society which has already expended great sums for scientific undertakings and their practical applications, a society which is in a favourable position for future growth on account of its material means, a society with a vital programme and with practical methods, a society whose activity is conducted by eminent representatives of both theoretical and technical branches—such a society appears to me as a factor of great importance in Russian life. The vast territory of Russia with its incalculable resources and natural strength cries out for an enthusiastic and well-supported experimental study of nature, and the application of the results of this experimental activity to the advancement of human welfare. This society must become a powerful lever in such work.

More and more there is an active faith in the power of the human mind and its special weapon—experimentation. There is a new impulse, the highest which has yet arisen, expressed in this society; it is the highest impulse underlying a general human interest (and not only a Platonic one), an impulse coursing through the whole cultured world—the interest in experimental science and its applications. Only recall the tremendous expression of this interest in America, in Stockholm, in Paris and in the Jubilee of the University of Berlin. And I believe in the future Moscow will have reason to be proud of its "Society for the Advancement of Experimental Science" and of its founder, Christopher Ledenzov, as much as it is now of its statesmen.

CHAPTER XII

A LABORATORY FOR THE STUDY OF THE ACTIVITY OF THE CENTRAL NERVOUS SYSTEM IN THE HIGHER ANIMALS

(Built on the plans of I. P. Pavlov and E. A. Hanike, with funds received from the Ledenzov Society.)

DESCRIPTION OF DIAGRAMS OF THE NEW LABORATORY.

THIS laboratory is a part of the Institute of Experimental Medicine in Leningrad. Its façade is shown in Fig. 4. It has three floors as shown in Fig. 5. The first and third floors are for the experiments on animals, and are shown in Fig. 6, altogether eight working rooms ("a" designates the experimental rooms, and "b" the corridors in which are the electrical and other apparatus). The middle or second floor has the same plan except that its rooms are not so high, and it has not the 4-corner rooms for the dogs. It serves for the arrangement of hydraulic and other apparatus.

The following measures were used to eliminate vibrations and the conduction of sounds into the rooms where the dogs are at work.

1. A moat surrounds the entire building, the upper part of which is filled with straw.

2. The eight working rooms on the first and third floors are separated one from the other by the intermediate floor, and by the cross-shaped corridor.

3. The underpinning of the building, *i.e.,* the beams, are immersed in sand-filled rooms.

4. The windows in the working rooms are small and consist of one piece of the thickest ground glass. The doors from these rooms leading to the staircase are double, iron, hermetically closing, and with special layers impermeable to sounds.[1]

[1] War and revolution delayed the equipment of this new laboratory, and only in 1925 did the work in this building, with mechanical registration of the salivary reflex, start on the large scale aimed at by Pavlov.—*Translator.*

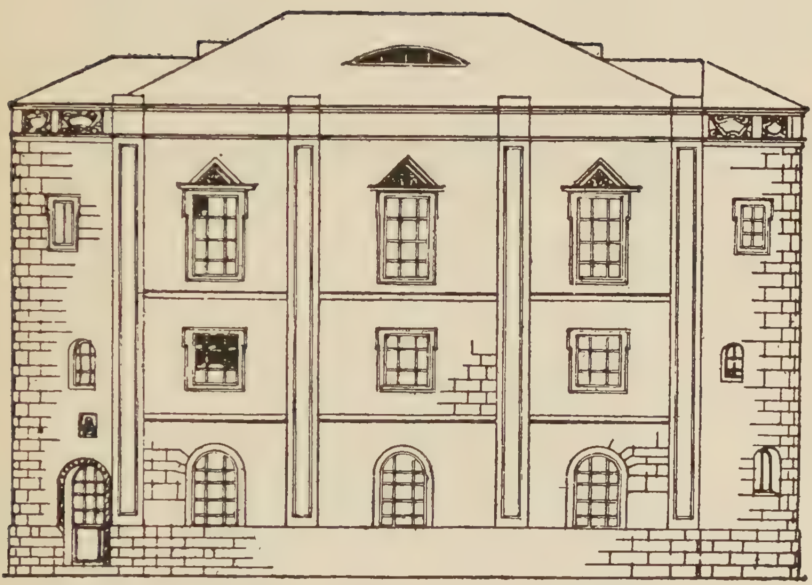

FIG. 4

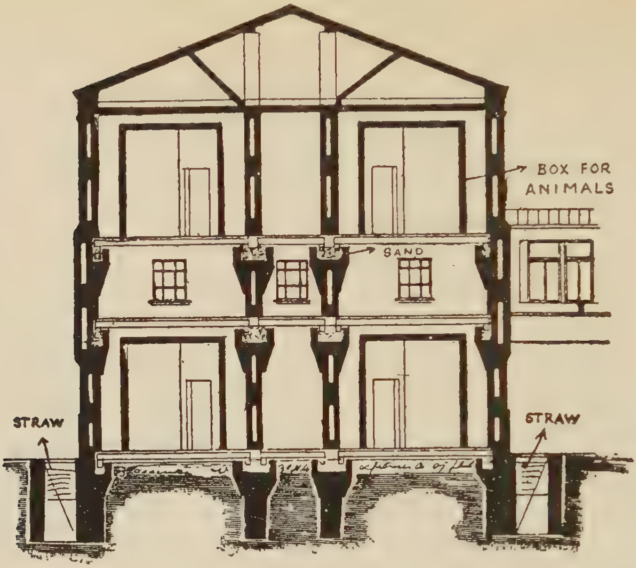

FIG. 5

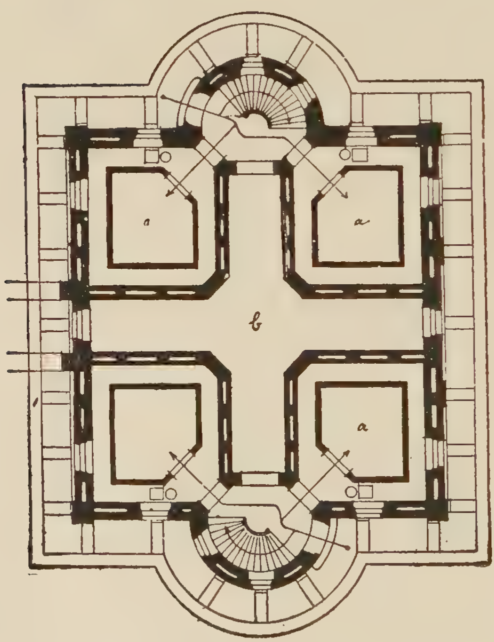

FIG. 6

CHAPTER XIII

THE FOOD CENTRE

(Read before the Society of Russian Physicians, and published in *Transactions of the Society of Russian Physicians,* 1910-1911.)

THE ANALOGY BETWEEN THE FOOD CENTRE AND THE RESPIRATORY CENTRE—AUTOMATIC STIMULATION OF FOOD CENTRE BY "FASTING" BLOOD—DELAYED CONDITIONED REFLEX AND DIS-INHIBITION BY DIFFERENT STATES OF THE FOOD CENTRE—LATENT EXCITATION—RECIPROCAL ACTION OF CENTRES—ACTIVITY OF FOOD CENTRE PRODUCES SENSATIONS OF APPETITE AND HUNGER—PHYSIOLOGICAL BASIS FOR APPETITE AND TONICS—REFLEX INHIBITION OF FOOD CENTRE EXPERIMENTALLY AND IN LIFE—THE REFLEX ARC AND THE FOOD CENTRE—LOCATION OF THE FOOD CENTRE.

WITHIN these walls the conditioned salivary reflexes have been spoken of many times. In our knowledge of the conditioned reflexes, there has been one point which has remained in obscurity, though it is indissolubly connected with them, and without it none of our conditioned reflexes can take place. This point concerns a part of the central nervous system as real as the respiratory centre, with which it is also fully analogous, although one rarely meets with its description in any text-book. When you light upon some interesting question, paradoxically enough, it is not to be found in new but in old books!

What is this point? It concerns the study of the food centre. On the basis of our material dealing with the conditioned reflexes, a food centre exists as surely as does a respiratory centre. As I have just said that it is analogous to the respiratory centre, I must begin with some remarks about this latter. The activity of this centre is manifested in the movement of certain skeletal muscles belonging to the thorax. You know that the first impulse for respiration comes from the chemical properties of the blood, overloaded with carbonic acid and other products of metabolism; further, the activity of the centre is conditioned by reflex stimulations from various peripheral organs, particularly from those where breathing occurs—from the lungs. We have an analogous fact relating to the food centre.

How is the activity of the food centre manifested? It is manifested in the exercise of the whole skeletal musculature, when it directs the body of the animal to the nutritive object, and also in the activity of that part of the skeletal musculature which transfers the food from the outside world to the digestive tract. The food centre sets into activity the upper secretory parts of the digestive canal, especially the salivary and gastric glands, simultaneously with the stimulation of certain movements of the skeletal musculature. These two different functions, the secretory and the muscular, are excited by the food centre in a parallel manner, so that by following the activity of one of these functions the experimenter can judge of the activity of the other. Consequently, the work of the salivary glands as studied in the conditioned reflexes is closely connected with the manifestations of the activity of the food centre. By limiting our observations to its secretory activity we lose nothing, but on the other hand we gain in exactness and clearness; for the skeletal muscles serve other masters besides the food centre, and therefore their phenomena are very complicated. The gastric glands are deeply situated, and their activity is not directly and exclusively dependent on this centre but is also conditioned by some other internal stimulations. Only the salivary glands serve as a special representative of the activity of the food centre.

What do we know about this activity? By what is it stimulated, varied and checked? It is clear that the first impulse to the activity of the food centre—by which the animal is set in motion, led to the food, and impelled to take it, by which it secretes saliva and gastric juice—arises from the chemical composition of the blood of the animal which has not eaten for several hours. In such an animal the blood acquires "hungry" properties. This finds a close analogy in the respiratory centre. Just as the respiratory centre regulates the inhalation of oxygen, so the food centre controls the entrance of solid and liquid foodstuffs into the organism. If it is admitted that the chief stimulator of the respiration centre is an internal automatic stimulus, then the same must be accepted in regard to the food centre. Besides the analogy there are facts which support this view.

In general, every centre can be stimulated either automatically or by external stimuli affecting the centripetal nerves in the different peripheral organs. Up to the present time there has been no single testimony to the fact that for the development of the activity of the food centre there is absolutely necessary a reflex stimulus, although this question has been thoroughly studied. Although various nerves leading from the gastro-intestinal tract have been cut, no one has ever seen a disappearance of the positive movement reaction of the animal to food, or, using the usual terminology, a loss of appetite. I, too, have performed many such experiments; I have cut the splanchnic, the vagus, and both pairs of the receptor nerves of the tongue, and the animals felt well, lived long, and took food as normally. We see here the same result as in the case of breathing, after cutting all centripetal nerves, the activity of the centre remains as before.

Consequently, the chemical composition of the blood of a hungry animal is a stimulus for the food centre. This internal automatic stimulation exists at first in a latent form, and later begins to manifest itself in the movement of the animal toward the food, in salivary secretion, etc. I shall devote some moments to this latent, automatic stimulation, as it has appeared many times in our salivary reflexes. Some experiments of Dr. P. M. Nikiforovsky will illustrate this.

A salivary conditioned reflex was elaborated in a dog, using a light as the conditioned stimulus; in a darkened room a bright flash of light was given before introducing an acid solution into the mouth of the animal. After many repetitions of this combination every flash of the light caused a secretion of saliva. Now the experiments were so altered that the introduction of the acid was delayed for three minutes after the flashing of the light. In such a case there is formed the so-called retarded or *delayed conditioned reflex, i.e.*, in the first and second minutes there is no saliva, and only in the third minute, just before the introduction of the acid, does saliva appear.

Analysis of this phenomenon showed that the delay of the salivary reflex developed because of internal inhibition, for during the first two minutes the action of the bright light is inhibited, is delayed by some internal condition.

It can be easily proved that this is really so. The internal inhibition itself may be paralysed or inhibited; in other words, the reflex may be *dis-inhibited.* Every unusual stimulus from the external world may be a paralysing agent of this inhibition, *i.e.*, a dis-inhibiting agent. Thus, if between the flashing of the light and the third minute any stimulus appears, it will inhibit the inhibition, and produce the salivary secretion.

Now, after reminding you of the nature of this *delayed salivary reflex*, I shall cite a fact always observed in this dog. Our dogs are fed usually at 5 P.M. If the experiment with the light is begun at 10 A.M., and the delayed reflex tried, then the salivary secretion begins only during the third minute after the flashing of the light. If the same experiment is made between 3 and 4 P.M., then one does not see the phase of delay, but otherwise there is nothing unusual in the behaviour of the animal, and he conducts himself as he did in the morning. It is clear to us that the latent stimulation of the food centre acts on that centre which is in relation to the acid reflex. We know, however, that between the different centres there is some relation, and that one centre can inhibit another. And as in our case there is during the first and second minute an inhibitory process in the acid centre, the increase of the latent excitation of the food centre paralyses this inhibition, as would any other new stimulus; the increasing latent stimulus dis-inhibits the acid reflex during the first two minutes.

Further I shall give many facts testifying to the existence of this *latent excitation* of the food centre. The question arose, What is the basis for this latent stimulation? One might think that the stimulation did not reach an intensity sufficient to produce an effect. Certainly this can and must be true, but it hardly answers the question. There is apparently an internal inhibition, which up to a definite moment does not permit the activity of the food centre, as manifested by the secretion of saliva. Several facts substantiate this.

We have before us a dog; there are no manifestations of the activity of the food centre, the dog makes no movements toward the food and secretes no saliva. I introduce into his mouth acid, which certainly is not food, and indeed the motor reaction of the dog is quite different from the food reaction. When the reaction to the acid is ended, the dog begins to make marked motor reactions, particularly toward food, sniffing the air, and prancing on the table, *i.e.*, it is unquiet, and if there is any apparatus near which has been used as a conditioned food stimulus, the dog turns toward it and even licks it, etc. Here we have a positive expression of the activity of the food centre.

Only in this way can I understand that the excited acid centre acts on the food centre, and according to the general law of *reciprocal action* of centres, inhibits it. As the food centre is in a certain degree of inhibition, the inhibiting effect of the acid falls upon this inhibition and inhibits it; the stimulus is freed and the reaction appears. This is the phenomenon of dis-inhibition, with which we meet constantly; it is a striking reality of which we are freshly convinced every day.

Here is another example, from the work of Dr. Kudrin. We have a dog with the posterior parts of the cerebral hemispheres removed. The variation from the normal expresses itself, among other things, in this, that the processes of inhibition are weakened as a usual result of any considerable operation on the cerebrum. If you take a normal dog which has not been fed during the day of the experiment, and begin the séance, giving it meat powder, you get a flow of saliva. After this, there sets in a certain excitation of which I shall speak further. The excitation passes off in about five minutes, the dog becomes quiet, the salivary secretion stops, and some dogs even go to sleep. In the dog upon which we had operated and which had a weakened inhibition, we see the following: As long as the animal is kept without food, it remains quiet, but as soon as it is fed, it is thrown into great excitation; this excited state, with salivary secretion, lasts a long time, sometimes one and a half hours or more. It disappears slowly. An irregular wave-like secretion can be seen, now weaker, now stronger. From physiology we know that if the secretion is wave-like we have to do with a conflict of antagonistic processes; for example, reciprocal action of the pressor and depressor apparatus. If we transfer this conception to our case then we must assume that in the food centre, when there is a state of latent *excitation*, there is also an element of inhibition.

In order to apply what I have said to human life, I shall add the following: It is clear that the food centre, besides affecting the skeletal musculature and the secretory glands of the first part of the digestive tract, also has another action, with which we, as animal organisms observing ourselves, are also acquainted—it is the production of the sensations of appetite and of hunger. When we speak of appetite in man this is an indisputable fact, but when we refer to the animal world, in order not to indulge in fantastic speculation, we must limit ourselves to registering and comparing the visible phenomena.

Thus is the activity of the food centre represented in our sensations. The fact that the activity of the food centre can be made to manifest itself in dis-inhibition, is observed in human beings; it lies at the basis of therapeutics. If the appetite becomes poor, in order to stimulate it there are often administered not nutritive, but inedible and disagreeably tasting substances; the patient receives something bitter, sour, etc., as a tonic, and the result is the same as that obtained in the dog, when stimulation of the acid centre increases the inhibition of the inhibited food centre, thereby dis-inhibiting it, and causing its energetic activity.

For the respiration centre, there are besides the automatic excitants different reflex stimuli. If both vagus nerves are cut (which bring impulses from the lungs to the respiration centre), then marked and lasting changes in respiration occur. Also in the activity of the food centre, the sensory centripetal nerves play a colossal rôle, especially the taste nerves, the nerves of the chemical receptors in the oral cavity.

Here are some experiments bearing on this. You try in a dog the natural conditioned food reflex, *i.e.,* you let the sight or odour of some food morsel act on the dog for a definite time, say one-half minute, and you note a certain effect—3 to 5 drops of saliva. The amount of secretion can serve as a measure of the irritability of the food centre. After this you let the dog eat, and when it has finished, you see the beginning of an excitation which was not present before; the dog licks with its tongue, sniffs, prances, and begins to whine. If now, immediately after all this is over and the dog is quiet, you repeat the experiment, showing the dog something to eat, you will receive not 3 to 5 drops of saliva, but 10 to 15. With the first feeding you sent reflex impulses into the food centre, and this activity was greatly increased; for the same stimulus produced a much larger secretion.

In our daily life this phenomenon is often seen. It may happen that at the dinner hour there is no appetite and we are indifferent to food, but it is enough to eat one morsel to stimulate the taste nerves, and an appetite comes immediately. *L'appetit vient en mangeant.* This is evidently an excitation of the food centre through a peripheral reflex stimulus.

But the food centre as well as the respiratory centre is excited not only by these peripheral stimulations (*e.g.*, from the mouth), but is reflexly regulated, both positively and negatively. Every day we see this in our experiments. The following will serve as an illustration: In the beginning of the experiment as I have said I applied the natural conditioned reflex, *i.e.*, showed food to the dog, and received 3 to 5 drops of saliva, and then fed the animal. In the second experiment there was a stronger action, 10 to 15 drops appeared, as a result of joining the stimulation from the mouth to the internal excitation of the food centre. When I repeated this experiment a third time, I received not 10 to 15 drops but 8; in the fourth repetition still less, about 4 drops, and on the fifth trial only 2 to 3 drops. The conditioned reflex gradually disappears, every time I give only a little food, and in the nature of the experiment there arise conditions which inhibit the food centre.

Why is this, and what does it mean? The action certainly starts from the stomach, either from its contact with the food, or from the initial phase of its secretion; in general because food has just entered the stomach. Consequently, in this case there is a reflex inhibition of the food centre. The explanation is clear. When the food has entered the stomach, the food centre must temporarily stop its work until the introduced food is digested. How can it be proved that this is really a reflex from the stomach? The experiments of Dr. Boldirev give an answer. His dog had a fistula of the œsophagus so that the swallowed food did not enter the stomach, and in this case the described inhibition of the conditioned reflex did not develop, and from frequent repetitions of the conditioned reflex, one always received the same amount of saliva.

And who of us is not acquainted with the following fact of our daily life! At a certain time of day you feel the paroxysms of a good appetite, and if you eat only a small amount, the appetite becomes sharper, but after 5 to 10 minutes it completely disappears. Every mother knows this, to her sorrow. Children wait for the dinner hour unwillingly, and beg for something, even though a little, to eat beforehand, but the mother admonishes them with the words, "You will ruin your appetite." And it actually happens that if the child has eaten something earlier, he does not eat at dinner; there has developed a reflex inhibition of the food centre.

It would seem that this is a defect in the organism, but we know of many such cases. And again arises the question, Are these really imperfections in the animal machine? The entrance of a small amount of food in the stomach temporarily suspends, or at least weakens, the activity of the food centre. There is not much harm in this. If there is a great lack of food material in the organism, then this small amount introduced is quickly digested, and appetite soon returns. The state of affairs might be worse; if the irritability of the food centre began to decrease only when the animal became stuffed with liquid and solid food, there would be, as a consequence, overeating and immoderate overfilling of the stomach.

Thus we see a complete analogy between the respiratory and the food centres. As I have described these analogies, it might seem that they are not so many, but we meet with these facts every day in the laboratory, and we are constantly persuaded that the food centre is a perpetually acting machine just as is the respiratory centre.

Now the question arises, How is this food centre to be described, what are its component parts, and what is its activity? It is certain that it must be counted as a part of the nervous system which regulates the chemical equilibrium of the body. Food must be understood here in a very general way; if a child breaks off and eats a piece of chalk with a sensation of pleasure, it is the work of the food centre.

One must consider this centre as highly complicated and consisting of several parts. Some months ago I expressed the idea that in the central part of the reflex arc, there can be constantly differentiated two halves. This fact is often overlooked. In physiological books there is much written about the centres, but little explained: of what parts do they consist, and do the given cells belong to the centripetal or to the centrifugal nerves? A strange regression has occurred in regard to this. When in the investigation of the spinal cord, knowledge of the reflex arc arose, the view was expressed very clearly that in the central part of the arc there is the central end of the sensory nerve and the beginning of the motor nerve, and this was based on histological findings in the cells of the dorsal and ventral horns. As investigations proceeded and as one penetrated deeper into the central nervous system, he got further and further away from this original and correct conception, and finally did not observe of what cells the given centre was composed. The cells which have been designated as "sensory," I shall, according to the terminology used at present, call receptor cells.

The main point of nervous activity is located, I believe, in the receptor part of the central station; at this point is to be found the impetus for the full development of the central nervous system, realised in the cerebral hemispheres of the brain; for these constitute the basic organ of that most perfect equilibrium with the external world which is incarnate in the higher animal organisms. The centrifugal portion of the reflex path is purely executive; the same muscles may be employed for a thousand different purposes, and these purposes are determined by the activity of the receptor apparatus; this latter apparatus conditions what functional combinations will be formed by the cells of one or another motor nerve.

I shall return to the food centre. Of what cells does it consist? I state positively that it consists of receptor cells; for they receive different stimulations, from internal as well as reflex stimuli. The nerve centres of the organs in which the activity of the food centre is manifested are, however, simplified to a high degree. In the case of the conditioned reflex we can excite the food reflex through an endless variety of stimuli, and the salivary secretion always proceeds from the same centre, the salivary centre.

As the food centre is a receptor centre it is comprehensible that it must be extremely complicated; like every receiving centre, it performs the most varied reactions; it forces the muscular system to move, now in response to the acid stimulus, now meat, now bread, chalk, etc.; the centre receives the stimulation, and transfers it as an impulse to the executive organ. In short, it is as complicated as is the cortical centre of the optic or auditory nerve.

Where is this centre situated? Physiologists consider the question of topography more indifferently than do pathologists. For physiologists the question of the function and of the activity of the centre is of more importance. That the exact location of a centre is not an easy matter may be seen from the example of the respiratory centre. At first one thought that this centre was in the *medulla oblongata,* and about the size of a pin-head. At present its limits have broadened, it has ascended into the brain, descended into the spinal cord, and no one can exactly define its boundaries. In the same way, one must accept the fact that the food centre is widely spread out. To define exactly its limits is at present impossible. At present we have only a few indisputable facts bearing on the answer to the question.

We must admit that the food centre is situated in more than one part of the central nervous system. Let us recall the pigeon from which the cerebral hemispheres have been removed; he remains motionless for hours, and even though surrounded by mountains of grain, he can not transfer a single one to his mouth. In such a pigeon, however, the activity of the food centre is clearly manifested. Five to seven hours after he has been fed by putting grain into his mouth, he begins to walk, and the more energetically the greater the length of time after feeding. Evidently, this is the activity of the food centre calling out the work of the skeletal musculature. That this is so can be proved by again filling the crop of the bird with grain, whereupon he again becomes quiet and motionless. From this it follows that a part of the food centre lies lower than the hemispheres. On the other hand, it is also evident that a part lies in the hemispheres, and that the taste centres also are there. Our taste, be it pleasant or disagreeable, undoubtedly represents a nervous stimulation which is reflected in our consciousness. Of course, such a phenomenon can be attributed only to the cerebral hemispheres. The food centre must consist of different scattered cell groups, and in the hemispheres there must be a special large group of such cells. Thus for the solution of this problem we have a certain amount of material at hand, but it is not entirely sufficient. As regards the respiratory centre, however, the state of affairs is not better.

After all I have stated to you it is clear that the food centre is a nervous apparatus for the regulation of the intake of solid and liquid substances necessary for the chemical processes of life. It is just as real and it works just as incessantly as the centre for breathing.

CHAPTER XIV

SOME FUNDAMENTAL LAWS OF THE WORK OF THE CEREBRAL HEMISPHERES

(Based on experiments of Drs. N. I. Krasnogorsky and N. A. Rozhansky. Read before the Society of Russian Physicians, and published in *Transactions of the Society of Russian Physicians*, 1910-1911.)

DISCOVERY OF INHIBITION—LAW OF IRRADIATION—TRACE REFLEXES—LAW OF CONCENTRATION—THE SLEEP REFLEX—SLEEP MAY BE CAUSED BY EXPERIMENTAL EXCITATION—CONDITIONED INHIBITION—EXPERIMENT IN WHICH SURROUNDINGS ACT AS CONDITIONED INHIBITOR—EXPERIMENT SHOWING SPEED OF IRRADIATION OF A PROCESS THROUGH THE BRAIN—CONCENTRATION—PRESENT FACTS JUSTIFY OBJECTIVE POSITION TAKEN ON THEORETICAL GROUNDS—FALLACY OF PSYCHOLOGICAL REASONING.

Nervous activity consists in general of the phenomena of excitation and inhibition. These are, so to speak, its two halves. I shall not commit a great error if I liken these two phenomena to positive and negative electricity.

The first conception of inhibition in the nervous system belongs to the Weber brothers, and concerns the peripheral nervous system. In 1863, twenty-four years after the discovery of the Webers regarding peripheral inhibition, it was shown that inhibition is a constant phenomenon in the activity of the central nervous system. That was the work of I. M. Setchinov, and the first contribution of Russia to physiology. By the brilliant discovery of the inhibitory centers in reflex activity he laid the cornerstone of Russian physiology. From that time on, central inhibition has attracted much interest and an increasing number of research workers. This inhibition has been established for many kinds of nervous activity, and it may be asserted now that the inhibitory process has the same frequency and importance as the excitatory.

My present report has to do precisely with inhibition, and how it manifests itself in so high a part of the organism as the cerebrum.

As most of you know, the activity of the hemispheres is now being studied by us in an objective way, *i.e.,* without making use of any psychological conception in the analysis of the phenomena, but by comparing only external facts, *viz.,* the phenomena of the external world with the reaction of the animal. The reaction which we use is that of the salivary glands. The conception of the conditioned reflex is the central one in this objective study of the activity of the nervous system. Our conditioned reflex represents a temporary connection between external phenomena and the activity of the organism, in the given case with the activity of the salivary gland; the usual reflex represents, on the other hand a constant connection. We may not only easily observe the origin of this temporary union, but we may also note that it is a highly sensitive and constantly fluctuating reaction, now stronger, now weaker, now vanishing, so that the study of nervous activity by the objective method is reduced to the study of all the conditions which affect the conditioned reflex. In our case the conditioned salivary reflex is formed by the coincidence of some indifferent phenomenon with the feeding of the animal, or by the introduction of an irritating substance into the mouth. I shall now proceed to add some facts about the physiology of the conditioned reflexes.

I shall give a description of inhibition as it occurs in the activity of the cerebral hemispheres. In previous articles I have already referred to the process of excitation in the brain. The essential feature of this part of nervous activity consists in the fact that when stimulation originates in the hemispheres it must spread and *irradiate* over the cerebrum. We call this the *first law of excitation.*

A mass of facts speaks for this. If you form a conditioned reflex, for example, to the ticking of a metronome, and then try other sounds, you will find that these other sounds at first also produce the salivary flow. Consequently the stimulation from a certain group of cells irradiates over a large part of the cerebrum, and therefore every other auditory stimulus provokes the secretion of saliva. If you make a conditioned stimulus from a tone of 1,000 vibrations, and afterwards try other tones of various vibrations, all of them have an effect. The same holds true for other conditioned stimuli. If you repeatedly combine mechanical stimulation of the skin (pricking) with feeding, finally this pricking calls out every time a secretion of saliva. Now when you subject other parts of the skin to the same pricking, they all cause the salivary secretion. This is because the stimulation has spread over the hemipheres, so that all the points of the skin region of the brain act the same as the first point excited. There is a form of experiment in which we do not connect the activity of the salivary gland with the present stimulus, but with its remains, or *trace, i.e.,* we give the stimulus and then allow an interval to elapse after its termination before putting acid into the mouth of the dog, or before feeding it. In the *trace reflexes,* the excitation spreads still further. After forming them to the given stimulus, you will find that saliva flows in response to many different stimuli.

Besides the law of irradiation, there is another law, that of *concentration* of excitation, *i.e.,* the irradiated excitation gathers along certain lines and towards certain foci. This is a fact which is seen in the laboratory every day. If you have formed a conditioned reflex to the metronome and then repeat this reflex many times, other sounds gradually lose their effect, and at last only the metronome calls out the excitation. And this concentration of the excitation proceeds still further: if you repeat the stimulation with the metronome long enough, it finally happens that only the metronome with the number of strokes you have constantly used will be effective; the dog may react to your stimulation of 100 ticks per minute, but not to one of 96. If in another dog you repeat for many times the mechanical stimulation of the skin at one and the same place, occasionally using it at other places, you will find that the action at these other places becomes less and less and finally disappears, and the former diffused skin irritation becomes concentrated. If you form a conditioned reflex to a tone of a certain strength, then only this one tone, and in the given strength, is effective; tones of greater or of less strength have no action. In these cases of high concentration of the excitation, besides repetition of the given stimulus it is also important to repeat the other neighbouring and related stimuli, but without feeding (*i.e.,* without the corresponding unconditioned reflex).

Now I shall pass over to the study of the other half of the nervous activity, the inhibition process. As you will see from experiments, the same laws that apply to excitation also hold here; inhibition likewise irradiates and concentrates. I shall first speak of sleep, as this state plays an important rôle in experiments with inhibition.

For many years we noticed that our dogs became sleepy; this interrupted our work, for the conditioned reflexes weakened and disappeared. We noticed especially that sleep occurred during thermal stimulation when thermal stimuli were connected with the stimulation of the salivary glands. It proved that thermal stimuli are special producers of sleep, *i.e.,* they condition and cause sleep just as other stimuli call out other activities of the animal. It is interesting that for the production of sleep one may use either warm or cold stimuli, but one must apply them on the same spot on the skin and keep their temperature constant. If you change the spot stimulated or the temperature, the sleep is light and uncertain. On the basis of these experiments we have the right to speak of a *sleep reflex,* and it became obvious to us that the state of sleep is a kind of inhibition of the activity of the hemispheres. Why do we say it is inhibition? Because this state of drowsiness, this sleep reflex, acts on our other conditioned reflexes in exactly the same way as well-known inhibiting agents, in all details as inhibition in general; there is a complete analogy between their actions. I shall bring before you further facts showing that other undoubted cases of inhibition pass over gradually into sleep, evidently on the basis of their mutual relationship.

We now come to other phenomena of inhibition. I form a trace reflex by scratching the skin of the dog for one minute in a definite place, waiting one minute, and then introducing acid into the mouth. Consequently I am undertaking to elaborate a conditioned reflex on the *trace* of the scratching irritation, from what remains of it in the nervous system. After many repetitions, I note the fact that I receive no effect on the scratching; but after I stop the scratching, at the end of another minute, during which I am waiting, the salivary secretion begins; therefore, I have formed a trace reflex out of the remains of the mechanical stimulation which is in the nervous system. But when the experiment lasts longer, there is the following interesting phenomenon: during the scratching, the dog becomes more and more quiet and drowsy and finally falls into deep sleep. If until the moment of the scratching he was awake, then immediately when you begin the scratching he becomes sleepy. The sleep grows deeper and deeper and lasts for an increasing length of time. At last we must abandon our experiments because the dog on the stand is constantly fast asleep. Apparently this is an unaccountable and unexpected process; you repeatedly give the dog acid, which should stimulate him strongly, but instead he sleeps, *i.e.*, the acid becomes a producer of sleep. On the contrary, in the same dog, the same acid reflexes, if they are not present as trace reflexes, *i.e.*, if the conditioned stimulus is given simultaneously with the unconditioned, produce not the slightest sleep.

How is this to be understood? During the scratching we never put the acid into the dog's mouth, consequently the inhibition must develop in this time. This is a peculiar and difficult situation for the nervous system. The inhibitory process must be connected with the presence of the stimulus, but the acid reflex must be excited by the trace of this stimulus. As the inhibition is connected with a strong stimulus, and as the process of excitation is connected with a weak stimulus (the trace stimulation), the inhibition at last prevails and there ensues a diffused effect of this inhibition, which goes over into drowsiness. With the appearance of these phenomena the conditioned reflex itself vanishes.

If you observe these experiments many times, and carefully consider the results, you can find no other more natural explanation for the curious relationship. At first, gentlemen, this explanation may seem artificial, but in the future you will make acquaintance with other facts which will convince you of its truth.[1]

In the next case the relation is simpler. You have formed some conditioned reflex, say, to the ticking of a metronome and this stimulus evokes a constant salivary reaction. Now I add to the metronome an

[1] The large body of evidence showing that sleep is a state of inhibition is given in chapter xxxii.—*Translator.*

odour, say camphor, and at this moment I do not reinforce the metronome, *i.e.,* I do not feed the dog while the metronome and odour are present. At first the metronome causes a flow of saliva in spite of the effect of the odour. But if we repeat this several times the combination becomes ineffective. The metronome with the odour of camphor does not produce a flow of saliva. Such a state we call *conditioned inhibition,* and the agent which we add to bring it about, we call a *conditioned inhibitor.*

There are some interesting details of conditioned inhibition. I begin my experiments by trying the metronome as a stimulus, and it yields not less than 10 drops. Then I try the combination of the metronome and the camphor, and get no effect. If one to three minutes after I have applied the conditioned inhibitor, I try the metronome alone, it evokes only 1 or 2 drops. What does this mean? It means that the inhibition, which developed in the central nervous system when I applied the camphor and the metronome, has spread out over the large hemispheres and remains thus irradiated; some time must elapse before it disappears. Therefore, if I try the metronome ten to thirty minutes after the combination, then it acts as well as usual.

This fact of conditioned inhibition explained one point for us which for a long time we could not reconcile with our results. When we came across experimental animals which were lively and sprightly and with which we thought the work would proceed quickly and smoothly, we found that in actual use they brought us only despair; on the experimental stand they continually slept, and we could get no kind of conditioned reflex. What is the cause of this? You have a lively animal which loses no opportunity to play, jump around, and lick every person and thing which it sees. You take such an animal and put him on the stand, in a light harness for a support; at first he behaves in the same way as he did when on the ground, and then he tries to get free, pulling and struggling. You combat his efforts, bind his paws, tie his head tighter, etc., and at last you attain your wish—the dog becomes quiet, but at the same time he begins to get drowsy and finally falls into a deep sleep. What does this signify? By vigorous methods you have suppressed, inhibited, the normal reaction of the animal to the external surroundings. In the nervous system of the dog inhibition has arisen, grown stronger, and spread from the motor region over both hemispheres, as sleep. All the surroundings have become transformed into conditioned inhibitory stimuli (inhibitors).

This can be proved as follows: You may gradually reduce the elements of the surroundings, and you will note that at the same time the inhibition becomes gradually less. In the following table from Rozhansky's work, the results of one of his experiments are presented:

EXPERIMENT OF FEBRUARY 22, 1912 (DOG "KABILL")

Time	*Stimulus*	*Number drops of saliva from parotid fistula during 30 sec.*	*Remarks*
3:50	Metronome	½	On stand in harness
4:00	"	2	On stand without harness
4:12	"	4	On another table
4:25	"	7	On the floor
4:35	"	3	On new table
4:47	"	0	On stand without harness
4:56	"	0	On stand in harness

You let the dog down on the floor, apply a conditioned stimulus, and receive 7 drops of saliva. If you put the dog back on the table, but without the stand and the harness, 3 drops. On the stand, zero drops.

In the case before you, gentlemen, you see this fact. You call out through the total effect of the surroundings, which acts as a conditioned inhibitor,[2] an inhibition of muscular reaction to the external world, but by inhibiting that you lose the conditioned salivary reflex. Here you have an inhibition which is not limited to those bounds necessary for you, *viz.*, the muscles; the inhibition spreads further and is expressed as a general state of rest of the nervous system. These cases show us that a nervous inhibition called out in a definite place, does not remain in this place, but spreads—irradiates.

Should this not be convincing, we shall present such facts in conclusion as leave no room for doubt, and substantiate the law of which I have spoken. The following experiment has been performed by Krasnogorsky. We have apparatus for pricking the skin fastened at three points on the lower part of the left hind leg; the first on the paw, the second 3 cm. above this, and the third 22 cm. higher. The lowest is ineffective, as this one has never been accompanied by food, and it has ceased to stimulate. The other two have always been accompanied by feeding, and they are therefore positive conditioned stimuli. From former experiments we know that the differentiation of the skin spots is based on the development of inhibition in these places. If pricking at the lowest point has ceased to be effective in provoking a salivary flow, this is because inhibition has developed in this place and prevents stimulation. You can clearly see how the inhibitory process irradiates over a certain distance and you can follow it exactly, and state the distance. When you have applied the lowest stimulus (the inhibitory one) and have received zero drops, and you now try the stimulus at the middle point and another time at the highest point, you see an enormous difference.

[2] This word is used to designate an inhibiting agent. Prof. Pavlov uses the word *tormoz*, meaning "brake."—*Translator.*

If after a certain time following the application of the inactive stimulus, you apply the one lying next to it, it proves to be inhibited, showing that the process of the inhibition has spread to it. If after the same time and under the same conditions you apply the highest one, you find there no inhibition.

In this way you can follow the nervous process and the movement of the inhibitory wave with the eye, and you see that after reaching a certain limit it does not spread further.

Now one can see also with what velocity this inhibition wave spreads in the nervous system and how far it travels. If one and a half minutes after the application of the inactive stimulus, whose inactivity is based on the development of inhibition at this point, you try the effect of the other stimuli, you will see that at the distance of 3 cm. the inhibition is clearly present, but at 22 cm. it is absent. Consequently one and a half minutes after the application of the inactive stimulus, the inhibitory process is not present at the point corresponding to the highest stimulating apparatus. If, however, you try the effect of this uppermost apparatus not one and a half minutes, but only a half minute after the inactive apparatus, you will find that the inhibition is present there; you thus see clearly how the wave of inhibition spreads out over the nervous system, and how it contracts. This fact seems to me an unmistakable illustration of the law of *irradiation* of inhibition; no other explanation can be given.

In conclusion, one is forced to say that inhibition spreads over the hemispheres in the same way as does excitation.

We also have many facts which show that inhibition is concentrated just as is stimulation.

We have, for example, a conditioned reflex (metronome), and a conditioned inhibitor (camphor). If the latter has been applied for only a short while as an inhibitor, and if you try the metronome five to ten minutes after the camphor, the metronome produces no effect. But if you continue the experiments further, *i.e.*, if you always reinforce the metronome by the unconditioned stimulus, but do not thus reinforce the combination of metronome and camphor, you will see how the process of inhibition is more and more concentrated. If now the metronome alone is tried five to ten minutes after the combination, it acts exactly as formerly, giving a full reaction. An apparently similar phenomenon is observed in the following fact. If you have a tone of 1,000 vibrations, and try to make a differentiation of a note varying from it by only one-eighth tone, *i.e.*, you accompany the tone of 1,000 vibrations by feeding, but not that which differs by one-eighth tone—finally these tones are differentiated; the one is effective, the other is not. This differentiation depends upon the inhibition process. If you try the tone of

1,000 vibrations very soon after trying the differentiated one-eighth tone, the former will be inhibited. If a longer time has elapsed after the differentiation, then the inhibition concentrates, *i.e.*, the trial of the differentiated (negative) tone after a short interval has no more an inhibitory effect on the active (positive) tone (1,000).

Similar facts were observed accidentally in other dogs with which we worked. We cannot yet bring these facts into any scheme, we are only observers, but apparently they have to do with the law of irradiation and concentration of inhibition.

Here is a series of dogs. One of them, you notice, has developed a condition of drowsiness which affects all the activity of the cerebrum. Then there is another type.[3] He does not sleep on the experimental stand. Consequently, inhibition did not reach its high point and manifest itself in a general inactivity of the hemispheres. In this dog inhibition is manifested by the inactive state of the muscles; the animal stands there like a statue. The inhibition is not limited to the muscular system, but passes over to the salivary gland. Now the last type of dog: It is lively as long as it remains on the floor. In the stand it does not sleep, but there is a state of muscular rest; it stands as if cut out of wood; however, the inhibition is limited to the muscular system and does not affect the salivary glands, which are strongly excited. In different dogs we have different degrees of irradiation of inhibition, and a certain definite concentration of this inhibition in consequence of one and the same inhibiting influence of our surroundings. The last dog has an ideally elaborated nervous system; inhibition remained at that point where we desired it, affording muscular rest to the dog, but not going any further than that; the salivary reflexes were unaffected and intact.

Even though the last-mentioned facts represent only observed material, their meaning is unmistakable; at one and the same time you see the phenomena of conditioned inhibition, and the phenomena of the definite limitation of this inhibition. The above facts give us reason, I think, to say that inhibition bears the same relation to its basic law as does excitation. Just as excitation at first irradiates and then concentrates, so does inhibition.

These facts offer substantial ground for the belief that excitation and inhibition are two different sides, two manifestations of one and the same process.

This is all, gentlemen, that we wanted to demonstrate and communicate to you. In conclusion I count it as not without interest to tell you of some further and more intimate facts which we collected in our

[3] For several types of dogs, see chapter xvii of Pavlov's book, *Activity of the Cerebral Hemispheres,* and chapter xxxix of this volume.—*Translator.*

investigations by this new method and which can be understood only much later.

When ten or eleven years ago we started to apply the objective method of studying the nervous activity of the dog, our situation was difficult. We were accustomed, as were others, to represent the dog as willing and thinking. As we were considering the objective point of view, it seemed highly improbable that there could be success. But we made our decisions on theoretical grounds and began working objectively, although on the one hand, the field of investigation appeared infinite, and, on the other hand, there were almost no general facts to begin with. Our position was a hard one; for we had no facts, no basis, to show us that our decision was right. We could only hope to attain to something, but at the same time there was the doubt whether our work would be recognised as worthy of science. Afterwards moments of success gave us courage.

For years we collected many facts. Our assurance began to increase. Still I must admit that doubts also increased, and did not leave me even up until a short time ago, although I did not speak of them. Many times I asked myself the question, Have I taken the right position in considering the facts from only the external aspects, or had I not better return to the old point of view? Such a predicament was repeated and filled my thoughts, but finally the matter became clear. Every time there was an appearance of a new series of facts—and they were difficult ones, almost incomprehensible from our point of view—doubts became greater. Why was this? What was the matter? It is plain enough. Because in these new facts we had not yet found any causal relations, we could not then explain what kinds of connection existed between the phenomena, by what they were conditioned. But when we had explained these connections, when we saw that from a certain cause follows this or that effect, at that self-same moment we felt calm and satisfied.

Why had we formerly, like cowards, returned to the old subjective methods? The secret is simple: because the subjective method is the method of thinking without considering real causes, because psychological reasoning is indeterminate reasoning, recognising phenomena, but knowing not whence they come nor whither they lead. I say, "the dog thought," "the dog wishes," and I feel satisfied by that explanation. But this is fiction. This is no cause for the phenomena. The psychological explanations were fantastic and without basis. Our objective explanation is truly scientific, always based on facts, always seeking for the cause.

CHAPTER XV

DESTRUCTION OF THE SKIN ANALYSER

(Read before the Society of Russian Physicians, and published in *Transactions of the Society of Russian Physicians*, 1910-1911.)

EXPERIMENT WITH DOG WITHOUT MOTOR REGION OF BRAIN; CONDITIONED REFLEXES AFTER SUCH AN OPERATION MAY BE NORMAL BUT LACK ADAPTATION BECAUSE OF DESTRUCTION OF SKIN ANALYSER—LOCOMOTOR ACTIVITY A CHAIN OF REFLEXES—MOVEMENT ANALYSER—PSYCHOLOGICAL ANALYSIS OF THE GIVEN CASE LEADS ONLY TO CONFUSION.

Our report to-day consists mainly in the demonstration of a series of experiments based on observations by Dr. N. M. Saturnov. At first please notice and consider the following phenomena: The dog before you is placed on the floor, and, as you see, remains for a long time in the same position as if his legs were frozen. One, five, ten, or twenty minutes may pass without his changing this attitude. You see the dog moving his head, but his legs very seldom if at all. There must be some special reason for this. The next symptom: I stroke the animal very gently, and he barks and growls. I can keep this up for an hour or more, and a threatening reaction in the form of barking is provoked. And thus again and again for months. Formerly, when the dog was in a normal state, a series of conditioned reflexes was elaborated; skin reflexes of two sorts (thermal and mechanical), and afterwards a sound reflex. The mechanical irritation of the skin was the oldest of these conditioned reflexes; every time pricking was applied to the skin, saliva flowed. Later, we extirpated some parts of the so-called motor region of the cortex, and then this condition now present gradually developed.

Now that we have described and you have seen the behaviour of this dog, we shall inquire into the state of his conditioned reflexes. First, let us try out our conditioned mechanical reflex from the skin, which, as we have said, was present for a year or more before the operation, and which was always exact and unfailing in its action. Dr. Saturnov, who has worked with this animal, will perform the experiments before you.

Now as the skin is pricked you notice that the dog exhibits no movements connected with eating, and no drop of saliva appears. This is a result of the operation; the conditioned skin reflexes have disappeared, in spite of the fact that these were our oldest conditioned reflexes, and that even after the operation mechanical stimulation of the skin was combined with eating more than 500 times. Thus, the conditioned skin reflex is impossible for this animal. This fact is plainly in accord with the behaviour of the dog we have seen on the floor, as described above. From what we have seen in this dog we might think that his higher nervous activity has been completely destroyed. We shall now see his other characteristics and you will learn that they are quite different.

Before the operation the sound of a bell was connected with the activity of the salivary glands and was their conditioned stimulus. This reflex was quickly restored after operation—after six combinations of the bell with eating. We formed a new sound reflex to a tone of 300 vibrations; on the twentieth combination the saliva appeared, and by the fiftieth trial the reaction had become constant. Consequently, new conditioned reflexes of hearing are easily formed, and the old ones (formed before the operation) readily restored. Here is an experiment illustrating this. You see that the dog is now quiet, and that saliva does not flow spontaneously. The bell begins to ring. The dog moves and looks for food, and 9 drops of saliva appear—clearly a normal reaction. The bell is obviously a conditioned stimulus. This demonstration is so evident that it is not necessary to show you other sound reflexes.

With the same facility a conditioned reflex to the smell of camphor was formed after the operation. This odour *per se* does not provoke saliva, *i.e.*, it is not an unconditioned stimulus for the salivary gland. It acts only when it is brought into temporary connection with the secretion. At the twelfth combination of the camphor with feeding there is a movement of the dog (the food reaction), and on the twenty-second, a salivary reaction is observed. Now we will demonstrate the experiment. In this hermetically sealed bottle there is camphor. We will break the seal during the experiment, and by means of a rubber bulb, diffuse the odour under the nose of the dog. We begin the experiment. The dog stands quietly, and saliva is not flowing. Now we liberate the vapour of the camphor. The dog gives a positive movement reaction (food), and there are 5 drops of saliva. It is evident that the odour of camphor is conditionally connected with the salivary reflex.

These are the facts which we wished to show you to-day. As you can see, they are exact and very clear phenomena. Let us consider them in more detail.

First you note a peculiar behaviour of the animal. The dog does not move spontaneously, but whenever he is touched, he exhibits a threatening reaction, *i.e.*, he snarls, growls, and shows his teeth. If you had seen only this much of his behaviour you would have said that he was maimed or injured. But, on the other hand, when we put him on the table and test his complicated nervous activity by delicate methods, we find him perfectly normal. How is this to be understood? What has happened? The analysis of this question is rather simple.

Comparing all the facts before you, you will find no difficulty in arriving at an explanation. The strange behaviour must be considered as an absence of the signals which normally come in from the skin. If you observe the animal more closely, you will see that when the dog is forced to move among hard objects which might strike against him, there is a lack of adaptation to the surroundings. The normal activity of the skin analyser has been destroyed.

From the standpoint of our knowledge of conditioned reflexes—the objective method of investigating the activity of the higher nervous system—we conceive of two mechanisms: first, the mechanism of a temporary connection; second, the mechanism of the analyser, *i.e.*, that nervous apparatus which has the task of decomposing the entire complexity of the external world into its elements. Thus we have the ear analyser, the eye analyser, etc. The skin analyser in this dog is destroyed, *viz.*, its central end in the highest part of the central nervous system has been extirpated, and therefore the delicate, exact, and accommodating connection of this analyser with the outer world is absent. Stroking, which normally brings about a reaction of contentment, produces in our dog without the upper end of the skin analyser an opposite reaction—one of defence, issuing from the lower-lying centres. That this is actually so is proved by the disappearance of the skin reflex after operation, and the consequent loss of delicate connections with the external world through the skin analyser. There have remained only the skin reflexes of the lower centres, and these have been constant under varying conditions for several months. We have repeated the experiments hundreds of times, probably a thousand times, and the effect is always the same.

One is inclined to think that the first symptom to which I called your attention, *i.e.*, the position of the dog for some time in an unchanging attitude, is dependent on the same cause. There are data showing that the entire locomotor activity is a chain of reflexes in which the end of one reflex is the beginning of the next; this chain begins with the normal stimulation of the sole of the foot through its contact with the ground, etc. It is natural to suppose that those stimuli which are the initial stimulation for walking are lacking in this dog, and he therefore remains motionless.

The behaviour of this animal can be explained thus: one of the chief stimuli and regulators of movement, *viz.*, the skin, having been greatly restricted in its receptor action by the operation, affects only the lower centres; therefore, the more complicated connections for motion are absent, and only the gross and unrefined ones remain. Everything relating to the higher activities effected through the other analysers remains undisturbed; for these analysers are intact. From the nose and the ear you can produce a normal reaction, and differentiation by means of them is unimpaired. For example, the bell had an effect, but another sound (the metronome) did not (differentiation), as the latter had not been accompanied by food. Odours and sounds produced not only the salivary, but also the corresponding movement reaction. If the dog is standing on the floor as usual, and if you now begin to stimulate it by the bell or the odour, moving them from place to place, the dog is set into motion and follows them (as signals of food), behaving as a normal animal.

Besides the aforementioned facts, there is another point of interest. Though this dog has lost the central part of the skin analyser, and has therefore a marked defect, he shows no symptoms of ataxia; he walks easily, scratches energetically, and can even deftly scratch the back of his ear with his hind foot, and can get himself out of difficult situations; if there is any ataxia, it is very slight.

If these are the facts, then we have luckily come upon a case in which the function of the skin analyser is impaired, but not that of the movement analyser. One must now add to those analysers generally referred to as eye, ear, skin, nose and mouth, the *analysers* of *movement,* which have to do with centripetal stimulations arriving from the motor apparatus—from the muscles, bones, etc. Thus besides these five external analysers, there is a sixth, the delicate internal analyser of the motor apparatus, which every moment must signallise in the nervous system the simultaneous position and tension of all the separate co-ordinating parts engaged in the given movement. For this analyser there is a special place in the brain—the motor region of the cortex. Our dog is interesting because he represents an example of an isolated defect of the skin analyser without impairment of the movement analyser. Further investigation must proceed in this direction—it must examine the individual peculiarities of these two analysers. Such a study will guide us, I think, in the orientation of those peculiarities shown in dogs with destruction of the frontal parts of the brain.

I now ask your attention to the following: Every experiment, like the one shown to-day, gives us the possibility of making a certain test, of comparing the psychological with the objective point of view concerning the observed phenomena. If you consider the given animal psychologically, you will be seriously perplexed. When you see the dog only on the floor you must conclude that he is stupid and inert. Stroke the dog as much as you will, without doing him any harm (we are doing him no harm—we are only feeding him), and yet the animal always reacts as if he wished to attack you. Put upon the table, the same animal becomes clever and normal, as shown by his numerous and delicate connections with the phenomena of the surrounding world. A sound after being several times repeated and accompanied by feeding, becomes the signal for food, and the same is true with the odour of camphor. Here is an apparent contradiction; at one time the dog is stupid, at another, clever. I come to the same conclusion if I compare the movement of the dog's head and feet. The head moves constantly and makes the customary orienting motions, but the legs are motionless—again a contradiction; judged by his head and neck the animal is normally active, but judged by his legs, he is as if paralysed.

The activity of the animal is conditioned by the corresponding stimulations. In those parts of the animal where the stimulation apparatus remains whole, we see normal and complicated relations; in those parts whose corresponding stimulating signals have been injured, there is always an absence of some of the normal activity.[1] From the nose and the ear there are complex reflexes, but from the skin, only the reflexes of the lower brain centres remain.

This is comprehensible because the central portion of the signallising apparatus of the skin, resident in the hemispheres of the brain, has been destroyed. The peculiar movement phenomena of the head and feet can also be understood. The impulse for locomotor activity, for movement of the feet, is lacking, but it is present for movement of the neck and head; for during the operation the upper part of the brain, corresponding to the lower parts of the body, is destroyed, but not the lower part of the motor region corresponding to the upper part of the body, the head and the neck.

For me it is obvious that the confusion and perplexity arising from consideration of the behaviour of such an animal from the psychological standpoint disappears after the physiological analysis. Ultimately, we may be able to state exactly what is lost in a given animal and what remains.

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[1] This is very strong evidence in favour of the physiological point of view.—*Translator.*

CHAPTER XVI

THE PROCESS OF DIFFERENTIATION OF STIMULATIONS IN THE HEMISPHERES OF THE BRAIN

(Based on the experiments of Dr. V. V. Belyakov. Read before the Society of Russian Physicians, and published in *Transactions of the Society of Russian Physicians*, 1911-1912.)

MECHANISMS OF THE ACTION OF THE CENTRAL NERVOUS SYSTEM BASED ON TEMPORARY CONNECTIONS AND ANALYSERS—ANALYSER AND ANALYSIS—DIFFERENTIATION BASED ON INHIBITION—EXPERIMENT SHOWING DIS-INHIBITION AND DIFFERENTIATION—SEAT OF INHIBITION DURING DIFFERENTIATION—COMPARISON OF PHYSIOLOGICAL AND PYSCHOLOGICAL EXPLANATIONS OF THESE EXPERIMENTS SHOW LATTER IS INADEQUATE—PHYSIOLOGY OF THE ANALYSERS; LOCALISATION OF CENTRES.

THE objective study of the higher nervous activity of the animal, the doctrine of conditioned reflexes, has arrived at a conception of two of the chief mechanisms of the central nervous system, *viz.*, the mechanism of *temporary connections* and the mechanism of *analysers*.[1] The present report concerns the physiological rôle and the activity of the analysing mechanisms.

I remind you that by analyser we mean a nervous apparatus consisting of the following parts: a certain peripheral end (eye, ear, etc., commonly called ''sense organ''), the corresponding nerve, and finally the brain terminus of this nerve, *i.e.*, the group of cells in which this nerve ends. We have to do with the uppermost part of this nerve, lying within the cerebral hemispheres. This apparatus is rightly called an analyser because its function consists in the decomposition of the complicated external world into its smallest possible integral parts. Its activity may be divided for study into two parts: on one side the limits of analysis are defined, and on the other the mechanism of analysis is investigated. To-day we shall deal with the second part of this problem, *i.e.*, with the mechanism.

In order to explain to you how we represent the mechanism of analysis, I shall give a detailed experiment. We take some agent of the external world acting on one or another analyser, some sound, smell, mechanical stimulus of the skin, etc., and try to bring it into temporary connection with a certain physiological activity, always in our experiments with the activity of the salivary gland. We introduce the given agent into the connection we desire by combining it with the usual physiological stimulus of this organ.

[1] For definition of "analyser" see chapter ix, footnote 2.—*Translator.*

After some repetitions we attain what we wished; the agent which formerly had no effect on the organ, is now brought into relation with its activity and soon becomes its stimulus. Every time it acts, it awakens the activity of the organ, in our case the secretion of saliva. Now when the combination is formed, if we try other stimuli from the same receptor surface of the body, they act effectively, although they have never before coincided with the activity of the organ. For example, if I have connected a definite tone with the activity of the salivary gland, and then try other tones or sounds, they also are effective. But this is only a stage, a definite phase. If we repeat many times our chosen agent, we notice that our stimulus, which had at first a general character, gradually becomes specific. Formerly, the most diverse tones and noises were effective, then many of these became inactive, and finally the sounds which function as stimuli become fewer and fewer and consist only of those tones closely related to our original tone. We are now convinced that this gradual transition from a diffuse, widely spread excitation to a special, narrowly limited one, this differentiation, comes about owing to the development of an inhibitory process at some point in the nervous system.

What are the grounds for our conviction? It rests upon a basis of constantly repeated facts. They are the following:

I choose, for example, a tone of 1,000 vibrations; this has become a stimulus of the salivary glands, and by means of many repetitions I have brought about a condition in which 1,000 vibrations stimulates, but not a tone even so close as 1,012. Consequently, the field of stimulating sounds has narrowed, and a tone which differs by only 12 vibrations, *i.e.*, one-eighth of a note from the original, does not stimulate. Such differentiation of the stimuli has taken place through the development of an inhibitiory process, as I have said, and the proof is as follows: I take a tone of 1,000 vibrations and it calls forth a flow of saliva; then another, of 1,012 vibrations, is applied and there is no saliva; complete differentiation between the two tones has occurred. If immediately after the 1,012 tone, I apply my original one, it is inactive and I must wait some minutes before it becomes effective again.

This is to be understood in the following way: When the differentiated tone is applied, there results an inhibitory process in the nervous system, and if during this time I apply my originally active tone, the inhibitory process suppresses its action. It is necessary for some time to elapse before the tone will be effective, in order that this inhibitory process may disappear in the nervous system. The fact that inhibition develops here is unmistakable.

The process of analysis and the process of differentiation must be presented thus: If our chosen special agent is brought for the first time into connection with a definite physiological function, then the stimulation called out by this agent, coming to a certain point of the cortex, irradiates or spreads over the corresponding receptor centres; and thus not only the single point in the brain end of the given analyser enters into the definite connection, but the whole analyser or a greater or smaller part of it. And only later, owing to the opposition of the inhibitory process, does the field of influence of the stimulation become smaller until at last an isolated action is obtained. This is the important fact explained by the foregoing experiment.

This is, obviously, only the beginning of the matter, and manifold questions arise. Some of these are answered in the experiments of Belyakov, the protocols of which I shall give. The first experiment shown is as follows: If we are right in saying that differentiation has for its basis the process of inhibition, then we should be able to destroy this differentiation at any moment by destroying the inhibition. Why? Because in the investigation of the complicated nervous activity we constantly encounter the process of *dis-inhibition*.[2] If differentiation is really based on inhibition of all the neighbouring stimuli which were formerly active, it should be possible to dis-inhibit them and to make them again effective. We can show this in the accompanying protocol.

TABLE I

DOG "DOGONYAI," MAY 9, 1911.*

Time	*Conditioned Stimulus*	*Quantity of Saliva in Drops*			
		First ½ minute	*Second ½ minute*	*Third ½ minute*	*Total*
10:58	Trumpet	Dog barking	Dog barking	Very excited	Shivers
10:58.30	⅛ tone	6	3	2	11
11: 3.—	⅛ "	1	1	1	3
11: 7.—	⅛ "	3	1	1	5
11:11.—	⅛ "	1½	1½	—	3
11:15.—	⅛ "	trace	—	—	trace
11:20.—	⅛ "	½	—	—	½
11:24.—	Original tone	1	Feeding with meat powder		—

These are the results of our experiments on our dog "Dogonyai," in which, during the course of many months, we had elaborated and used a differentiated tone varying from the original by ⅛ of a note.

[2] The reader will remember what has been said concerning *dis-inhibition* in footnote 1, chapter viii, and in chapter xi. All the processes of internal inhibition can be dis-inhibited by extra stimuli, and as the process of differentiation is based on internal inhibition, the differentiated (negative) stimuli can also be dis-inhibited, *i.e.*, in the presence of extra stimuli they will manifest an excitatory action instead of the usual inhibitory one.—*Translator.*

* The trumpet is applied as an unusual and strong stimulus (extra-stimulus); ⅛ tone means ⅛ note higher pitch than the original tone used as the conditioned stimulus. The ⅛ tone is the negative or differentiated tone.

That is, the differentiated tone had no effect in calling out a secretion, while the original promptly stimulated a flow. Now we allowed the sound of another musical instrument to act on the dog—a trumpet with a very shrill sound composed of many overtones. It produced a marked effect on the animal, which began to bark, break loose from the stand and to tremble. When he had become quiet after the sound of the trumpet had ceased, we tried the differentiated (negative) tone 1,012, and there was no trace of differentiation. First, 6 drops were obtained during 30 seconds, exactly the same as received from the original tone (1,000 vibrations), and in the next two consecutive half minutes, 3 and 2 drops respectively, a total of 11 drops. After five minutes we repeated the same tone, and it acted, giving 4 drops in one minute. Four minutes later its action had not entirely ceased. If one examines the last column of the table, showing the total amount of saliva secreted, he sees that it is considerable. It looks as if the differentiated tone acted as a usual stimulus; for on being repeated, its effect was gradually extinguished. This dis-inhibition lasted for 10 to 15 minutes, and there was no trace of differentiation. Of such experiments we have many. Here we have given one which clearly illustrates the fact that the differentiated tone is extinguished just in the same way as is a well elaborated and old conditioned reflex.

And further. If inhibition lies at the basis of the process of differentiation, then it should be possible to reinforce, accumulate, and summate this inhibition. How? By several successive repetitions of the differentiated stimulus. Table II describes such an experiment:

TABLE II

Dog "Krasavets," June 1, 1911 *

Time		*Stimulus*	*Number drops of saliva for ½ minute from*	
			Parotid Gland	*Submaxillary Gland*
1:45		Positive tone	9	10
1:53	1 minute	Negative tone	0	0
1:54		Positive tone	8	7
2:10		Positive tone	8	7
2:25		Negative tone	0	0
2:28		Negative tone	0	0
2:31	1 minute	Negative tone	0	0
2:32		Positive tone	5	3
2:55		Positive tone	10	8

In "Krasavets" a certain tone had been used as a conditioned stimulus. The first row of figures in Table II shows a usual size of the con-

* Positive tone = the one which has been accompanied by feeding.
Negative tone = the one which has not been accompanied by feeding.

ditioned reflex—9 drops from the parotid and 10 from the submaxillary gland. Now we try the differentiated tone which is about ½ note lower. It has no action. We apply it once and one minute later repeat the original (*positive*) tone, and we see that if there is any inhibition here it is very small—instead of 9 drops we have 8 and 7. Now we repeat the same differentiated (*negative*) [3] tone three times in succession, *i.e.*, we accumulate the inhibitory action, and we see that the habitual (positive) tone applied (as before) at the same interval after the application of the differentiated (negative) tone, now becomes sharply decreased, giving 5 and 3 drops instead of 8 and 7. If we allow some time to elapse for the dispersion of this inhibition, and again try the positive tone, we see that it has recovered its usual effect—10 and 8 drops. Thus can we say that inhibition, which lies at the basis of differentiation, can be summated by repeating the differentiated (negative) stimulus.

Here is another fact following from this. If inhibition lies at the foundation of differentiation, then the more difficult the task of differentiation, the greater will be the inhibition. It is obvious that it is more difficult to distinguish between two tones differing in pitch by only one-eighth of a note than it is to distinguish between two tones differing by two full notes. One may suppose that also the intensity of the inhibition process will vary. The more delicate the differentiation, the stronger will be the inhibition, and vice versa. Here is an experiment:

TABLE III

Dog "Dogonyai"

Date		*Time*	*Conditioned Stimulus*	*Drops of saliva for ½ minute*
June 11, 1911		11:25	Positive tone	4
		11:40	⅛ Negative tone	0
	10 minutes	11:44	⅛ Negative tone	0
		11:54	Positive tone	1
		12:15	Positive tone	3
July 6, 1911		1:20	Positive tone	5
		1:40	Negative tone	0
	10 minutes	1:44	Negative tone *	0
		2:54	Positive tone	4
		2:10	Positive tone	4

Under normal conditions we get in the dog "Dogonyai" 4 drops of saliva from the positive tone (1,000 vibrations). Afterward we try

[3] It seems convenient to refer to these two tones as positive and negative, using positive to mean the stimulus which has been accompanied by feeding and produces a flow of saliva, and negative to mean the stimulus applied later, which is not accompanied by feeding and which usually produces no flow of saliva (the differentiated tone.)—*Translator.*

* The negative tone differs from the positive by two musical steps.

the negative tone (1,012), which does not produce saliva. For two successive trials it gave zero drops. Ten minutes later we try the positive tone again, and it is inhibited. The inhibition of the differentiated tone lasts for some time and is expressed in the decreased effect of the positive tone. Let us compare this experiment with the second (*see* experiment of July 6, 1911, Table III). In the first row, we see a normal size of the conditioned reflex—5 drops. Then we try a differentiated tone, but one which is easily distinguished, *i.e.*, one which differs from the positive tone by two whole notes. This negative tone is repeated twice. After ten minutes we try the positive tone. It has not changed in the least; it gives 4 to 5 drops. Thus we see that a fine differentiation (with the one-eighth, negative, tone), caused an intense inhibition, but a coarse differentiation (the negative tone differing by two notes) did not evoke any marked inhibitory effect.

There arises an interesting question: Where does this inhibition which lies at the basis of the differentiation take place? Naturally one thinks that it develops in the corresponding analyser, *i.e.*, in that place where the stimulations are analysed. But this must be proved. Now I will give an experiment which leads to the conclusion that the inhibition occurs exactly in that analyser to which the given inhibitory stimulus belongs. We tried to dis-inhibit the differentiation by applying various stimuli coming through different analysers, with the results as shown in Table IV.

TABLE IV

Dog "Krasavets"

Date	*Time*	*Conditioned Stimulus* *	*Quantity saliva in drops for ½ minute from*	
			Parotid Gland	*Submaxillary Gland*
June 24, 1911	1:20	Positive tone	9	11
	1:40	Negative tone plus gramophone	3 + 2	5 + 3
	1:55	Positive tone	10	12
	2:05	Positive tone	12	14
June 25, 1911	2:35	Positive tone	8	10
	2:45	Positive tone	12	13
	3:00	Negative tone plus light	½	trace
	3:20	Positive tone	10	12
June 28, 1911	3:25	Positive tone	10	12
	3:45	Positive tone	12	13
	70	Negative tone plus odour of camphor	trace	0
	4:00	Positive tone	10	12

* The negative tone was ½ note lower than the positive.

The first row in the table shows the usual normal secretion to the positive tone, 9 to 11 drops. Thereafter, together with the negative tone is applied a new stimulus which should produce the orienting reaction of the animal. The stimulus used was the music of a gramophone and it gave a considerable dis-inhibition. Instead of the usual zero which we should expect, the differentiated tone together with the gramophone gives 3 and 2 drops (for each 15 seconds) from the parotid gland, and 5 and 3 drops from the submaxillary. Thus the gramophone dis-inhibits the negative tone. In the following experiment (Table IV, June 25, 1911), we applied a light stimulus as the dis-inhibitor; it had almost no effect. Differentiation remained. The light stimulus has not dis-inhibited—it has not destroyed the differentiation. Finally, in the third experiment (Table IV, June 28), we applied camphor odour as the dis-inhibiting agent. This also had no effect. Thus we used three different stimuli; light, gramophone, and camphor, for the eye, ear, and nose analysers respectively. Our differentiated (negative) tone affects the ear analyser, and the gramophone which affects the same analyser, proves to be a strong dis-inhibiting agent; but the stimuli falling on the eye and nose analysers are without effect. Although the light is only a weak stimulus, the same is not true of the odour. Odours are strong stimuli, but, as you see, the odour did not dis-inhibit our negative tone.

We have other experiments which directly prove that the inhibition takes place in the analyser of the differentiated stimulus. Such an experiment is given in Table V.

TABLE V

Dog "Dogonyai"

Date	*Time*		*Conditioned Stimulus**	*Quantity saliva for ½ minute*
June 2, 1911	11:05	10 minutes	½ Negative tone I	0
	11:15		Whirligig	2
			Whirligig	2
June 4, 1911	11:10	10 minutes	½ Negative tone I	0
	11:20		Positive tone	1½
	11:40		Positive tone	4
June 14, 1911	10:40		⅛ Negative tone II	0
	10:44	1 minute	⅛ Negative tone II	0
	10:45		Whirligig	½
	11:10		Whirligig	3
June 15, 1911	10:55		⅛ Negative tone II	0
	10:59	1 minute	⅛ Negative tone II	0
	11:00		Positive tone	trace
	11:40		Positive tone	4

* Negative tone = that not accompanied by feeding.
Negative tone I = ½ step variation from positive tone.
Negative tone II = ⅛ step variation from positive tone.

Here we compare two conditioned reflexes, one on a tone and the other on a whirling object (whirligig). After the applications of the negative tone there remain in the central nervous system the inhibitory traces the action of which is compared by measuring the size of the reflex to a positive stimulus, a tone (of the same analyser as the negative stimulus, *i.e.*, the ear analyser) with the size of the reflex to a positive stimulus (whirligig) from another analyser (the eye analyser). At first a coarse differentiation was tried out (a half note). This differentiation by "Dogonyai" resulted in a weak inhibition; for in him we had elaborated a much finer differentiation, one on an eighth tone. The effect of the inhibitory traces from the differentiation we tried on a reflex through the eye analyser, *viz.*, on the whirligig. This reflex was not inhibited by the application of the negative tone, but gave the same number of drops as it had given on a previous trial that same day, *i.e.*, 2 drops. Consequently, a weak differentiation (*i.e.*, an insignificant inhibitory process) in the ear analyser under the given conditions (of time) showed no effect on the excitation process in another (eye) analyser. Now turn again to Table IV. The same differentiation (½ tone), *i.e.*, the inhibitory process of the same strength and under the same conditions, gave a marked inhibitory effect on a conditioned reflex of the same analyser (ear). On that day the salivary secretion to the positive tone was 4 drops. The application of the coarse differentiation (½ tone) gave a zero effect. Ten minutes later the application of the positive tone produced the secretion of 1½ drops of saliva instead of 4. Thus it is proved that one and the same differentiation, an inhibitory process of one and the same analyser, has an inhibitory effect for reflexes of the same analyser; but for reflexes of other analysers it has no inhibitory action. The location of the inhibition arising from differentiation is, therefore, to be found in the very same analyser as in the one to which the differentiated (negative) stimulus was applied.*

But as you remember from foregoing reports, the nervous processes in the highest parts of the central nervous system constantly flow, irradiate, and concentrate. This is the reason for believing that the inhibitory process coming from a given analyser, may spread over the entire hemispheres. In order to prove this, instead of a simple differentiation (which demands only a weak inhibitory process) one must use a higher differentiation, or accumulate the differentiated inhibition; for then the

* That is, if differentiation has been elaborated for sounds, then the inhibition resulting from the negative sounds affects not all positive conditioned stimuli, but mainly those of the sound analyser; if differentiation has been elaborated between stimuli coming in through the eye analyser, then the inhibition resulting from negative optical stimuli affects principally positive conditioned stimuli of the eye analyser, but not, for instance, sound stimuli.

inhibitory wave is not limited to the given analyser, but embraces the neighbouring and the distant analysers.

In the same dog "Dogonyai," we apply now a higher differentiation, one-eighth tone, and we repeat this. And you see clearly that its action is not limited to the same analyser, but spreads to other analysers. In Table V (June 14) you have an experiment showing the effect of the differentiated inhibition aroused in the ear analyser on the reflex of the eye analyser—on the reflex from the whirligig. After the application of the negative tone the whirligig produces only one-half drop; but if we wait 25 minutes, so that the wave of inhibition has had time to disappear, it gives the full effect—3 drops instead of one-half drop. Obviously, the same thing occurred when both reflexes were in one and the same analyser, *viz.*, in the ear analyser; thus, if the positive tone is tried after the repeated application of the (negative) one-eighth tone, the former has no effect (see Table V, June 15, 1911). If the inhibitory process had attained a certain strength in distant analysers, it is clear that it should have a much greater effect in the analyser where it arose.

Such are the facts which we have established with Dr. Belyakov. From them it is evident that one may advance, *i.e.*, that one may propose very penetrating and profound questions about this mechanism, and may obtain definite answers. We are able not only to establish the fact of the differentiated inhibition, but we can actually direct it into certain paths experimentally, strengthen or weaken it, and find out where it originates.

In reviewing these results, it is interesting to propose a comparative judgment on our objective point of view, a point of view which is maintained without difficulty. You see that I am not merely imagining, that I continually stand on a basis of facts. I control all my propositions by experiment, and in this way I always subject my ideas to the decision of facts. In order to know the power of this objective physiological point of view, I ask you, gentlemen, to try to understand and explain the above-mentioned facts from the psychological point of view. You will see a remarkable difference. Let us take one or two examples. I make a conditioned stimulus from a definite tone. We will imagine, and say that the dog well remembers that this sound is a signal for eating and that it will be followed by eating, and in expectation of this, the dog secretes saliva. Now, when I apply, after this tone, another differing by only one-eighth of a note, the dog cannot distinguish them immediately but confuses them, and consequently secretes saliva. He remembers badly. Thereafter I repeat many times the usual (positive) tone and the unusual (negative) tone, and I bring it about that the dog well remembers that the positive tone is accompanied by eating and the negative tone is not accompanied by eating. When I apply the positive tone, the dog secretes saliva and is ready to eat, but when I apply the negative tone he is quiet and does not expect food. Now immediately after the negative tone, I apply the positive, and it has no effect. Why is this? The dog well understands the tones; it remembers which tone is the signal for eating, and which is not for eating. Why does it not secrete saliva now on hearing the positive tone? How can this be explained? And further. I repeat for the second time the negative tone; there is no saliva. This means that the dog remembers that this tone is not followed by eating. I repeat the negative tone the third time with the same result, which proves that he remembers accurately. But why has he forgotten the positive tone? This is impossible to understand when considered psychologically. It is even more incomprehensible that, as shown in the experiment, he is able to recollect the positive tone 15 minutes after the negative one. But from our physiological point of view the matter is simple. If the differentiation is an inhibition, if a repetition of the differentiation is a summation, an accumulation of inhibition, then one must wait a certain length of time for the inhibition to disappear and thereupon the normal relations return.

To examine all psychological conceptions, and to show that in comparison with our objective data, they are crude, empirical and fantastic, and that their properties are an insurmountable obstacle for an analysis of the most delicate phenomena of the highest nervous activity—this is the task that lies before us.

Now I revert to the question of the analysers. We have already collected and systematised facts concerning the activity of the analysers. Further, we have data as to how their activity changes under certain conditions. If we destroy parts of the hemispheres of the brain, which represents a complex of analysers, this destruction is manifested in the way we should expect, judging from the aforementioned facts. If we damage a part of an analyser, this is immediately reflected in its function. The degree of impairment of function is conditioned by the size of the lesion and the time which has passed between the operation and the moment of the observations. These disturbances are, as is known, to a certain degree gradually compensated for, but they never entirely disappear.

Further, there is the task of explaining which disturbances of the functions of the analyser are to be attributed to injury and which to removal of its parts. Certainly this is a far-reaching question and I do not know when it will be solved, but I hope that in the experiments which we have done, we have certain clues to its answer. We have evidence, for example, that the disturbance in differentiation is dependent on a certain distortion and interruption of the normal current of the process of inhibition.

So you see, gentlemen, that the highest activity of the nervous system, the activity of the cerebral hemispheres, their analysing function, can be subjected to a strict physiological investigation with absolutely no aid from the conceptions of psychology. And this analysing function is the chief task of the cerebral hemispheres.

The present report of facts and fragmentary knowledge, humble as it is, gives us, I think, some indications for the solution of those deep secrets concerning the physiological activity of the analysers. One of the phenomena before which we stand in great bewilderment is the fact that after removal of considerable portions of the cerebral hemispheres, you cannot always discover, after a lapse of some time, any defect in the activity of the nervous system. It seems that you are dealing with a precious and extraordinarily important mechanism; but, on the other hand, you destroy and ruin a mass of it, and you see therefrom no consequences. I should like to emphasise that the remarkable compensating ability of the brain demands attention. Thus you see what was stated about the great hemispheres as a whole nearly 100 years ago and then rejected, now stands forth as a living fact relating to their separate parts. The physiology of the brain had its beginning in the observations of the French school, which asserted categorically that in the hemispheres there is no localisation, and that in destruction of the hemispheres, if only a part is left, the conditions return to normal.

These views were entirely abandoned in 1870 when the famous experiments of Fritsch and Hitzig were made, giving a basis for the doctrine of localisation of centres. Then the former views seemed a great mistake; now, however, when we are making a detailed study of the analysers, this abandoned idea comes to light again. When one has removed large parts of the cerebral hemispheres, in the beginning it seems as if the analyser has been annihilated; its action is hardly expressed. But after weeks and months have passed, these injuries have become so compensated for that it is with difficulty that one notices in what way the animal is not normal.

The fact of the existence of a localisation, as regards the great part of the cerebrum, is hardly subject to doubt. But how is it with the localisation inside of separate regions of the brain—this is a difficult and colossal task which confronts physiology. How is it to be explained that a structure can be broken and destroyed, and yet we can see no results of this damage? For the separate analysers apparently one must admit as an undoubted fact that there is some substitution, some compensation of functions. How is this to be understood, what surmises is one to make here? Our conception must, of course, be mechanistic.

Some hope, some approach to this subject is already evident. It is probable that the statement with which I began to-day has significance. I refer to the fact that when the conditioned reflex is just formed it becomes generalised. From this it is apparent that the brain end of an analyser represents a common mass in which all parts are in close connection, and one part can be substituted for another. One may suppose that at the peripheral end of the analyser there is a strict differentiation of the stimulating elements, each element separate and distinguished one from the other; at the same time at the brain end of the analyser there is a connection extending over the whole cerebrum so that all the peripheral stimulations are conducted to every point of the brain. Thus there is the possibility of replacing a great part by a small part of the analyser.

What I have said is not so much a proposal as a presentiment as to how these extremely complex and important questions can be solved. Finally, I should like to give expression to the thought, how far we are from any sort of real conception of the mechanism of the cerebral hemispheres!

CHAPTER XVII

SOME PRINCIPLES OF THE ACTIVITY OF THE CENTRAL NERVOUS SYSTEM AS SHOWN FROM THE STUDY OF CONDITIONED REFLEXES; INTERACTION OF CENTRES

(Read in commemoration of I. M. Setchenov, before the Society of Russian Physicians, and published in *Transactions of the Society of Russian Physicians*, 1911-1912.)

WHITE AND GREY MATTER—SETCHINOV'S INHIBITION—GOLTZ'S EXPERIMENT—THE CONDITIONED REFLEX—DESTRUCTIVE (PAIN) STIMULI—FLOW OF ENERGY AND NERVOUS IMPULSES FROM WEAK (PAIN) CENTRE TO STRONG (FOOD) CENTRE—LAW OF CONCENTRATION AND IRRADIATION—ROLE OF PAIN AND FOOD CENTRES IN THE STRUGGLE FOR EXISTENCE—"WARM" AND "COLD" CENTRES AND THEIR RELATION TO THE ACID CENTRE SHOWN BY EXPERIMENT—RELATION OF CENTRES BASED ON IRRADIATION AND CONCENTRATION—INTERRELATION OF MEAT AND SUGAR CENTRES SHOWN BY EXPERIMENT; ITS APPLICATION TO DIETETICS—PHYSIOLOGICAL CONCEPTIONS ARE THOSE OF TIME AND SPACE.

THE extent of our knowledge of the two principal parts of the nervous system—*viz.*, of the peripheral part, the nerve fibres, on the one hand, and on the other hand, of the central part, the grey matter, consisting mainly of nerve cells—differs greatly. In the physiology of the peripheral nervous system there have been established, as is well known, many exact laws relating to both irritability and conductivity. Certainly the nervous process as such remains a mysterious secret; but this applies in equal measure to the central nervous system, for the process in both cases is exactly the same. But, as you know, this process is being energetically attacked by scientific minds and their efforts will probably not be unavailing.

Concerning the central nervous system, the grey substance, the grouping and connections of the cells one with the other, our chief knowledge is limited to topographical data. There are many investigations and many assertions about the location of this or that centre. The facts relating to the chief question, their functions are, however, very poorly elaborated. We know that the essential function of the central nervous system is performed through the so-called reflex activity, *i.e.*, the transference of the stimulation from the centripetal to the centrifugal paths. Our knowledge is certainly too elementary, too general. One can readily understand that after these general statements, there arise at once important questions as to the special path along which the transference of the stimulation occurs and the laws governing this transference. Concerning the activity of the central nervous system, however, our knowledge is very limited, and one can say that the subject has just begun to be worked upon. During the last ten to twenty years questions of this sort, but relating to the lower parts of the central nervous system, *i.e.*, the spinal cord, have been considered systematically.[1] Such inquiries pertaining to the activity of the highest parts of the central nervous system were begun in my laboratory for the first time by use of physiological and not psychological methods.

At first there might be doubt as to whether such methods really possess advantages for the solution of this question when they are extended from the lower to the higher parts of the central nervous system. If the lower parts are complex, how infinitely more complicated are the higher parts! In spite of this obstacle, there are in the study of the brain certain favourable circumstances, and the most favourable is this: Reflex activity with all its complexity is already complete and fixed when we meet it in the spinal cord. Thus in these pre-formed connections we have no means of seeing how they are established. The physiology of the higher parts of the central nervous system is in quite another position. Here we see the process of formation of the reflex act, and we have the possibility of observing fundamental properties and elementary processes, thanks to which this formation occurs.

In order to explain the matter, permit me to make some comparisons. Take a factory turning out finished articles from raw stuffs. If you know only the materials used and the end products, great wisdom is required to divine what is done in the factory, and to know through what processes these products pass in their elaboration. Such a problem in many cases might forever remain unsolved. It is another matter when you enter this factory and can see how these substances are worked over, into what combinations they enter, and how they pass from one department to another. Then you more or less easily understand the principles. The same holds good for the physiology of the highest sections of the central nervous system. Here before our eyes is formed a reflex act. Its mechanism is thus disclosed and laid bare before us.

The members and guests of our Society are well aware that we have now accumulated a great deal of material relating to the physiology of the normal activity of the higher parts of the central nervous system, material which consists not only of separate facts, but which can be ordered and systematised. To-day I shall make an effort to add new facts to our former generalisations, or, more exactly, to introduce new material so that another series of facts will be embraced, facts which have been obtained not only from a study of the higher parts of the central nervous system but also from investigations of the lower parts, *viz.*, the spinal cord.

[1] Pavlov refers to the researches of Sherrington and his collaborators.—*Translator.*

One of the chief facts in the activity of the central nervous system is that of a peculiar inhibition, which I shall now consider. As the initiator and promulgator of this conception, we with full justice acknowledge I. M. Setchenov, to whom to-day's meeting is dedicated. It is just fifty years ago since the appearance of his famous work, *The Inhibitory Reflex Centres.* This article and its facts may be counted as the first victory of Russian thought in the realm of physiology, its first independent original contribution to that science.

The fact of inhibition is shown in the following experiment. The speed of reflex action was determined by immersing the hind foot of a frog in an acid solution of known strength, and measuring the time between the beginning of the immersion and the beginning of the responsive reaction, the contraction (the so-called Türck's method). In such frogs the hemispheres had been extirpated, and on the exposed parts, *i.e.,* on the optic thalami, were put crystals of sodium chloride. Under the influence of this chemical stimulus the reflex weakened greatly, as was shown by the longer time between the immersion in the acid and the contraction.

The latter phenomenon can probably be explained thus: the irritability of the lower parts of the central nervous system, *viz.,* of the spinal cord, through which the reflex takes place, is markedly decreased, in consequence of which a greater interval of time must elapse before the stimulation can reach that strength which will produce an effect, the withdrawal of the foot from the acid. This observation must be considered as the starting point for the collection of a large number of other facts relating to the central nervous system.

At about the same time appeared the first reports on the so-called *Quakversuch* of Goltz which consisted in causing a frog from which the cerebrum had been removed to emit croaking sounds by lightly stroking its back. The reflex could be repeated with machine-like regularity. If at the same time one stimulates another point, for example, presses on the foot, the croaking reflex is inhibited.

At present we have a series of similar facts. As an example, I shall take another experiment of Goltz. In experiments on dogs with the spinal cord cut between the thoracic and lumbar vertebræ, he showed that many reflexes in the muscles and genito-urinary system, which occur with mathematical exactness, immediately cease if at the same time some place on the hinder half of the animal is stimulated sufficiently to call out another reflex. The latter reflex inhibits the former. These facts have been proved repeatedly and have been worked out systematically. Here is an example:

The posterior roots of the seventh, eight, ninth and tenth spinal nerves are exposed in a frog, and the contraction of the gastrocnemius muscle is registered. When the ninth nerve root is stimulated, this muscle contracts. But if simultaneously other centripetal roots, *viz.*, the seventh or eighth thoracic are irritated, leading to the contraction of other muscles, the reflex contraction of the gastrocnemius weakens or entirely disappears.

In other words, if together with a certain reflex another is produced, the former suffers in strength or is completely destroyed. In the physiology of the conditioned reflexes, *i.e.*, in the influence of such stimuli as are brought into temporary connection with the activity of the salivary glands, we have seen a number of such facts, showing the interaction of two stimuli originating at separate points. If during the time a conditioned stimulus is being tried, another stimulus acts—for example, some new tone, the sight of a new figure, an odour, or some thermal stimulation of the skin, etc., or in fact anything that can evoke a new reflex—then the conditioned reflex weakens and may vanish.

This is one of the most common facts which we meet in studying the function of the central nervous system. Now I shall dwell some moments on the mechanism of this phenomenon. How is it to be explained? What does this fact show to us, what properties or elementary processes? What conception does it bring to us? I should like to emphasise the following situation. Let us take a certain reflex, *i.e.*, there is excitation of a certain point in the central nervous system. If at the same time another reflex is evoked, another point in the central nervous system is stimulated, and the first reflex becomes weaker and may disappear. One may suppose that the exercise of the second reflex withdraws a certain amount of energy into its own centre at the expense of the energy of the first reflex centre; consequently less energy remains in the first reflex centre and its manifestation is weaker, or, if the diversion of energy is considerable, entirely absent. Other explanations can be given, but this one can not be gainsaid, as it corresponds well to the actual facts.

If the foregoing facts are understood in this way, then another phenomenon of the activity of the central nervous system has an identical internal mechanism. This phenomenon is the so-called conditioned reflex, *i.e.*, the temporary connection between a given external stimulus and a certain organ.

How does this phenomenon which we call a conditioned reflex originate? In our experiment we feed the animal or introduce acid into his mouth, and thereby stimulate either the food or the acid receptor centre. From these centres the excitation is conducted to the centres of the corresponding organs; in the case of food, from the food centre to the centres of movement and secretory reactions; or, if we have to do with the acid centre, to the centre of defensive movements (by which the animal rids himself of the acid), and also to the centre of salivary secretion. We have here a certain focus in the central nervous system—a focus of great activity. When such conditions exist, then all other stimulations formerly indifferent that enter at that time the central nervous system, are attracted and conducted to this active focus. If a certain stimulus is often repeated without being accompanied by any kind of effect which is more essential for the organism than the stimulus itself, then this stimulus becomes neutral and indifferent. We are surrounded by a multitude of sounds, sights, etc., but if they call out no specially important stimulations, then we react to them as if they did not exist. If now these neutral stimuli coincide several times with the stimulation of the activity of our centre, then they no longer spread and irradiate over the hemispheres as they did when they were not attracted to a specially excited focus; instead, they open up a persistent narrow path to the active centre, connect with it, and thus the formerly neutral stimuli become definite stimulators of the centre.

If one accepts this explanation, then two important groups of facts may be considered from one and the same point of view. In both cases we see that the excitation flows from one place to another. That this is not fantasy but is really so, is proved by the experiments of Dr. Yerofeyeva. I will consider her results from a new point of view to-day, and I think it will be plain that our conception is strengthened in a high degree by them.

What are our facts? We take a dog with a chronic salivary fistula—our usual laboratory animal for these experiments—and let a strong electric current act on his skin. This, according to the subjective terminology, is a *pain stimulus;* but according to the objective term, a *destructive stimulus.* It is obvious that the answer to such a stimulus is a usual reflex, a *defensive* reaction of the animal; he protects himself with all his might against this stimulus. He tries to break loose from the stand, he bites the stimulating apparatus, etc. The stimulation passes into the centre of the defence reaction; it is expressed in defence movements. If you repeat this experiment for several successive days, the irritability of the animal increases with each repetition, and the defence reflex becomes reinforced. But let us perform this experiment in another way. If you give the dog food during the action of the destructive stimulus (if he will not eat the food, forcibly introduce it into his mouth in order to stimulate the taste cells), you will notice that the defence reaction becomes weaker and weaker, and in the course of time may vanish. This means that you have before you a fact of the first category, an inhibition. The stimulation of the food centre leads to inhibition of the centre for pain reflexes.

If feeding is often repeated simultaneously with the pain stimulus, finally you will not only fail to have the defensive reaction, but, on the contrary, with the application of the electric current, you will see that the dog develops the food reaction; he turns toward you, looks toward the place from which the food is brought, and saliva flows. The stimulation which entered into the centre for defence reaction, now passes over to the food centre, *i.e.*, the centre which governs the movements and secretions relating to food. This is an illustration of the second group of reflexes; it is a conditioned reflex.

From this example you witness how one phenomenon passes inevitably over into another; and thus their relationship is clearly established. First, as you have seen, the pain centre was inhibited, and then the stimulation was transferred to the food centre. Hence follows the logical conclusion that the processes are essentially one and the same, that there is merely a transference, an alteration of direction, an attraction of energy from one centre to another. And if the new centre is the stronger, as in the given case, all the energy of the first centre passes over to this stronger centre, and the previously active centre becomes entirely quiescent.

Let us go further. What does this mean, that the energy of one centre can be conducted to another? This occurrence can be related to a great group of facts about which I have spoken earlier. One year ago at an anniversary meeting of this society, I discussed the laws of irradiation and concentration of the excitation processes. The law of concentration consists in this, that the excitation is collected and directed to certain points of the nervous system. This law is founded on the following observations. By the method previously described you have made a conditioned reflex from some tone; *i.e.*, you have repeated the tone always at the same time with feeding or with the introduction of acid into the mouth, and at last you have the corresponding reflex and the corresponding secretion. Let us suppose that we have made this reflex from a tone of 800 vibrations per second, and the tone evokes constantly its conditioned reaction. Now we try other tones. And it turns out that they are all effective, though they are very far apart, for example, pitches of 100 and 200 vibrations and even 20,000 to 30,000. This fact—*viz.*, that we have united the food center only with a single stimulus and that the excitation has become generalised—furnishes a direct basis for speaking of a law of irradiation, and for representing the phenomenon thus: the excitation arriving at certain cells of the hemispheres, does not remain exclusively in those cells which it first entered, but overflows, *irradiates,* into the neighbouring cells.

The second half of the experiment is as follows. As you repeat the reflex on 800 vibrations, supporting it by feeding, it becomes more and more specialized, the range of effective tones becomes narrower and narrower, and if you employ your given tone for a long period, you may reach an extreme grade of specificity.[2] From a tone of 812 vibrations there may be no reflex. The excitation which was at first spread out, irradiated, is now collected and concentrated.

This justified the proposal of the related law of concentration. It is clear that the facts which I mentioned earlier are in perfect correspondence with the law of concentration, and that in the experiments with inhibition and formation of conditioned reflexes, the law of concentration of the excitation, of focusing it at a certain point, is made manifest.

These are our facts; this is what we have already done. It is to be understood that this is only a general formulation, only the beginning. Further, in each of these laws—of irradiation and of concentration—there must be more detailed points established. To work out these details must be the task of future investigations. My laboratory is at present occupied with this problem, as I shall show.

In the experiments of Dr. Yerofeyeva we find facts which prove that the law of concentration under special conditions may be expressed differently, *i.e.,* in different cases it appears in individual forms. As I have shown, the excitation in the centre for defence movements may be diverted to the food centre. This experiment is easily carried out in all animals. If, however, you try to direct this excitation into the acid centre, *i.e.,* if you wish to make from the electric stimulus a conditioned stimulus for the acid centre, you will fail. Hence follows a supplement to the law of concentration: the direction of the stimulation is conditioned by the relative strength of the interacting centres. The food centre is obviously a powerful physiological centre; it is the protector of the individual's existence. It is evident that in comparison with the food centre, the defence centre is of less importance. You know that during the struggle for food the different parts of the body are not especially defended; among animals there are fierce tussles and fights for food in which occur wounds and serious injuries. Thus the destruction of separate parts of the body is a sacrifice to the most important requirement of life, the obtaining and capture of food. It is manifest that the food centre must be considered as the most powerful physiological centre, and therefore we have a clear reason why this centre may attract excitations from other centres. The acid centre certainly has no such importance; its activity is a special one, and it is plain that in comparison with it the defensive centre is of greater

[2] In later work it proved to be necessary not only to repeat the conditioned stimulus followed by the unconditioned, but also to apply the similar stimuli without the unconditioned reflex in order to bring out the contrast. See chapter xxxi of this book, end of paragraph 10, and the last part of chapter vii in Pavlov's book, *Activity of the Cerebral Hemispheres,* where differential inhibition is discussed.—*Translator.*

moment; consequently an excitation can not be drawn from the centre of defence into the acid centre. This is in reality the case.

Finally, I can give you experiments which well illustrate the law of irradiation. Experiments with thermal stimulation of the skin have been performed in our laboratory by Dr. P. N. Vassilyev, and the following unexpected fact was observed. For a long time, since the beginning of our studies, conditioned reflexes on thermal stimuli have been developed. Either warming or cooling may be made a conditioned stimulus for the food or for the acid centre. In this respect the thermal stimuli do not differ from others. But in the following respect there is a considerable difference; it is very difficult to obtain simultaneously various conditioned responses from cold and warm stimulations. For example, if you have made from a warm stimulus acting on a certain area of the skin a conditional stimulus for the acid centre, *i.e.,* you obtain the corresponding movement and secretion, and if this is well developed, you may be assured that even though you do not further apply this conditioned stimulus the conditioned reflex will remain intact for weeks and months depending upon how well and how long you developed and reinforced it. In the same way you may form a conditioned reflex from cooling a certain part of the skin and feeding the animal. Such a reflex can also be stable and remain intact for weeks and months. But if you wish to apply these stimuli together during one and the same experiment, then you will encounter insurmountable difficulties. Let us begin the experiment with the cold reflex, and suppose that cold as a stimulus is connected with the food centre. You note the corresponding food reaction; the dog turns toward you, looks for the food, and saliva begins to flow. You repeat this, once, two or three times, and every time you see a uniform reflex. If, after this you try the warm stimulus which is connected with the acid centre, then instead of the motor-acid reaction and the corresponding salivary secretion, you see the same cold reflex, *i.e.,* the food reaction. In other words, the dog confuses the warm-acid reflex with the cold-food reflex. If you begin the experiments in reversed order, you will obtain analogous results, *i.e.,* if you begin with the warm-acid reflex, then the cold-food stimulus will be confused with the warm-acid stimulus.

This phenomenon may be explained by assuming that there is an easy irradiation from the warm to the cold centre, and vice versa. If, for example, you repeat several times the cold stimulus, the thermal nerve cells (cold and warm) become fatigued, and the stimulus becomes generalised; the stimulation overflows and spreads equally over both centres, and if you now pass over to the other thermal stimulus, the reaction is the same as from the first thermal stimulus. No other interpretation, it seems to me, is plausible. It is necessary to admit that both thermal centres are very close together, that they are intimately interwoven, just as are the warm and cold points of the skin, and hence the phenomenon of irradiation is seen especially clearly; the irradiation readily passes from one centre to another, and to separate them is a matter of great difficulty. It would be interesting to see how quickly this separation could be brought about. At any rate we see here a brilliant example of irradiation.

Further arises the question, what are the relations between the laws of irradiation and of concentration? These laws, it is evident, are of opposite nature. In the first case we have to deal with an overflowing, with a spreading of the excitation in the brain; in the second case, with a grouping, a collection of the excitation around a separate distinct point.

Thus we see that the problem of the interrelations of these two chief laws is of the highest importance for the whole mechanism of the central nervous system. Certainly the solution of the problem is very distant, but, nevertheless, valuable material may be collected. Two investigations in my laboratory give some hints toward a solution. One year ago experiments were completed, showing that different reflexes (food) can be compared with one another (Yegorov). Up to that time only the conditioned acid and the conditioned food reflexes, and consequently the stimuli connected either with the acid or with the food centre, had been set in opposition. In this investigation the first attempt was made to define the mutual effect of the various food reflexes on one another. The examination was made in this way: Certain indifferent stimuli were combined with various kinds of food, one stimulus with cheese, another with milk, a third with bread, a fourth with meat, etc., the influence of one of these reflexes on the others was noted. In these experiments, the fact was brought out, that the stimulations by various food substances is often accompanied by an extremely long-lasting after effect.

In the physiology of the conditioned reflexes we have many instances showing that a stimulation in the form of its *trace* can manifest itself in the central nervous system for a long time after the cause of the stimulation has been removed and its visible effects have ceased. We formerly had to do with traces after ten-minute intervals, and we had not dealt with longer lasting traces. In the experiments of Yegorov the traces (after-effects) were of much longer duration, not only of hours but of days. This coincides with the experience of actual life, when, for example, some taste, especially if unpleasant, is remembered for a long time.

The peculiarity of the facts of which I shall now speak is probably dependent upon a long duration of this *trace* (after-effect). The experiments were done as follows. We have a conditioned reflex, *i.e.*, we have a certain stimulus connected with feeding, say with meat powder. The conditioned stimulus gives a more or less constant effect. Afterwards another conditioned reflex is elaborated from another stimulus, for example, the eating of sugar. We may for brevity designate these as the "meat reflex" and as the "sugar reflex". What will be the result if I allow one reflex to act during the trace of the other? The following answer was received from the experiments of Dr. Yegorov. If you have a "meat reflex" of a certain size (measured by the number of drops of saliva—one of the effects of the stimulation of the food centre), say 10 drops, and if now you apply the "sugar reflex," and soon afterward try again the "meat reflex," the latter will be markedly decreased. The stimulation of the "sugar centre" (using this expression for the sake of conciseness), *i.e.*, of a group of nerve cells which are stimulated through their corresponding fibres from the peripheral apparatus by sugar, inhibits the "meat centre," *i.e.*, the group of cells which are stimulated by meat in the mouth.

If this experiment is repeated many times, and all the details noted, the highly interesting fact given below is observed. If soon after (five to ten minutes) the application of the conditioned sugar reflex, you apply the conditioned meat reflex, you now receive a fairly large reflex response, 7 to 8, or even 10 drops, *i.e.*, nearly the same quantity as was obtained before the sugar reflex. Only in the next trial will the meat reflex be entirely inhibited. At the third and fourth repetition it slowly regains its strength. On the following day the meat reflex may still be somewhat inhibited, and only on the third day be completely restored.

The existence of this lasting effect of one taste reflex on another is well known in practical life. You know the annoyance of a mother when her children eat something sweet before dinner, because after this they do not want their usual food. Apparently now they do not relish other foods.

I ask your attention especially to the course of this phenomenon. I reiterate that the sugar reflex undoubtedly inhibits the meat reflex, not only for some hours but for some days, and that this inhibition sets in not at once but after some time. Immediately after the sugar reflex the meat reflex gives a nearly normal effect, and only on the second and third repetitions does it become inhibited. This unexpected state of affairs can be explained, I think, in only one way. One must suppose that when the sugar reflex—a reflex of considerable strength—is applied, its influence is not confined to the cells of the sugar centre, but spreads and overflows a large region of the food centre, *i.e.*, the excitation arising from this reflex can be detected in other parts of the food and taste centres. If you use the meat reflex very soon after the sugar reflex, the former is effective; for in the meat centre there still exists the excitation which has spread out from the sugar centre. If, however, a certain time has elapsed after the sugar reflex, according to the law of concentration the excitation has begun to collect in the sugar centre; then to this strong centre the excitation from the meat centre is attracted, and the reflexes proceeding from the meat centre are inhibited.

In the experiments cited we can see the mutual influence and a certain exchange of work by centres acting according to these two laws. In the first phase you have irradiation; the stimulation overflows, covers a large region, and this is the reason why the meat reflex is at first unaffected and exists at the expense of the sugar reflex. Some time later this stimulation of the sugar centre collects, concentrates, and the meat reflex becomes weaker for a long time. That this is really so is shown by the experiments of Dr. A. A. Savitch. If you try the meat reflex twenty-five minutes after the application of the sugar reflex, the former is more or less effective; but if you wait thirty to forty minutes after the sugar reflex, there is a marked weakening of the meat reflex, because during this time the irradiation wave has already contracted and is concentrated at the sugar centre. Consequently, the energy from the meat centre has been diverted also to the sugar centre.

In this way these experiments point to a new wide territory for investigation. The questions before us concern a fundamental point, *viz.*, the mutual relations of two fundamental laws of the central nervous systems, the law of irradiation and the law of concentration.

When you behold a series of such facts, I believe you will arrive at the conception which for me is the only true one. As all the experiments presented show, the study of the reflex mechanism, which forms the basis of the activity of the central nervous system, is here reduced in its essence to a study of space relations, of the definition of the paths along which the excitation at first spreads, and then concentrates. If this is so, then it is comprehensible that a sure probability of mastering the subject in all its extent is given only by conceptions which are characterised by notions of space. This is the reason why it must be perfectly clear that it is impossible by means of psychological conceptions, which essentially are not spatial conceptions, to penetrate into the mechanism of these mutual connections. You must be able, so to say, to point with the finger where the excitation process was at a given moment, and where it has gone. If you conceive of these relations as they are in reality, then you will understand the truth and power of that science which we are vindicating and developing—the science of the conditioned reflexes. It has absolutely excluded from its domain psychological conceptions, and has to do always with only objective facts—facts existing in time and space.

CHAPTER XVIII

SUMMARY OF RESULTS OF REMOVAL OF DIFFERENT PARTS OF THE CEREBRAL HEMISPHERES

(Read before the Society of Russian Physicians, and published in *Transactions of the Society of Russian Physicians,* 1912-1913.)

POINT OF VIEW—CONDITIONED REFLEX DEPENDENT UPON THE CEREBRAL HEMISPHERES—HEMISPHERES ARE THE SEAT OF THE CENTRAL END OF THE ANALYSERS—ANALYSER OF MOVEMENT AND SKIN ANALYSER—"MOTOR" CELLS ARE SENSORY—MUNK'S EXPERIMENT; "PSYCHICAL" BLINDNESS—PAVLOV'S ANALYSIS OF THIS—EXPERIMENT WITH DAMAGED EAR ANALYSER—LOCATION AND FUNCTION OF THE ANALYSERS—FUNCTION OF POSTERIOR AND OF FRONTAL LOBES—EXPERIMENTAL DIFFICULTIES IN BRAIN OPERATIONS.

WHEN the question of a subject for to-day's report came up, I thought to myself, what shall I do, take a small part of the subject and discuss the results of a single series of experiments, or review the whole of our work? I decided on the latter; for it seems to me that this will be both instructive for my hearers and not entirely useless for us. It will be of great value to us to consider what we have attained by our labour of many years, to draw some conclusions therefrom, to weigh the results, to contemplate them, to define more clearly our shortcomings, and to set our goal and tasks for the future.

We in my laboratory have now for seven years been occupied with the extirpation of the whole of the hemispheres as well as of their separate parts; for this purpose we have used many scores of dogs, which afforded us exceptionally significant data for the basis of the present survey.

Many years ago we stated our special point of view regarding the highest nervous activity as it is manifested in the highest animals. In the investigation of this activity we rejected subjective psychological conceptions, and chose the external, objective, physiological point of view, *i.e.,* that which the biologist uses in all of his science. From this standpoint, the whole complex nervous activity, which was formerly interpreted as psychical, appeared to us as the expression of two chief mechanisms; first, the mechanism of the formation of temporary connections between the agents of the external world and the action of the organism, *i.e.,* the mechanism of the conditioned reflex; and, second, the mechanism of the analysers, *i.e.,* an apparatus whose purpose it is to analyse the complexity of the external world, to decompose it into its separate elements and moments. All our results up to the present fit into these conceptions. This, of course, does not exclude the possibility of further development and enlargement of conceptions relating to the subject.

Although our researches on the complicated nervous activity have been carried out on organs of small physiological importance, on the salivary glands, the two mechanisms at work in the cerebral hemispheres are nevertheless clearly manifested in the activity of these glands.

I shall present my material not in chronological order, that is, not in the order in which we came into possession of the facts, but in logical sequence, disposing and grouping the data so as to make clear to you the essence of the matter.

The first question which we must decide here is the question of the relation of the cerebrum to the above mentioned mechanisms—to the mechanism of the formation of conditioned reflexes and to the mechanism of the analysers. The outstanding fact which was constantly manifest to our many collaborators in a large number of dogs was this: that the cerebral hemispheres are the seat of the conditioned reflexes, and that one of their chief functions is the formation of these transitory connections. We have many proofs thereto, although our subject is of such a nature that new evidence may always be of service. Investigators who have extirpated either the whole or a part of the cerebral hemispheres have observed a disappearance either of all of the conditioned reflexes, or of special groups of them, depending upon whether the whole or a part of the hemispheres was removed. Various and exact measures were used in this work, in order to obtain the facts, and the results were always the same. Under certain conditions there was constantly a loss of all or of only some of the conditioned reflexes. Great perseverance was used in this research; many times we tried for years to restore a lost conditioned reflex before we concluded that it could not be established again. With one dog we not only tried to form conditioned food reflexes in the experimental room, but we went so far as to accompany all of his feeding with a certain tone, in order to ascertain whether in this manner it was not possible to form the conditioned reflex again. But once the organ of a given conditioned reflex was annihilated (as by removal), the conditioned reflex never reappeared. After these constantly recurring facts, it must be acknowledged that the hemispheres are, in effect, the organ of the temporary connections, the birthplace of the conditioned reflexes. Certainly one may categorically ask whether these temporary conditioned connections can not be formed also outside the hemispheres, but in my opinion there is no good ground for considering such a question. All the knowledge we have at present brings us inevitably to the conclusion that the temporary connections owe their formation to the cerebral hemispheres, and that they vanish with their removal. But it is conceivable that sometimes under extraordinary conditions, conditioned reflexes may be formed outside the hemispheres, in some other part of the brain. In this respect one can not be too positive, because all our classifications and laws are more or less conditional, and are significant only for the given time, under the circumstances of the given method, and within the limits of the given material. Fresh in the minds of us all is a well known example—the indivisibility of the chemical elements, which was considered for a long time as a scientific axiom.

Thus I repeat that in various experiments by many workers the fact was constantly met that the temporary reflexes occurred only in the presence of the whole or a part of the hemispheres. Consequently, we may accept without misgivings the statement that one of the most *essential functions of the hemispheres* is the elaboration of the conditioned reflexes, just as the main work of the *lower parts of the nervous system* is concerned with the simple, or according to our terminology, the unconditioned reflexes.

The second mechanism belonging to the cerebral hemispheres is the mechanism of the so-called *analysers.* In this case we started from the old and well known facts, changing somewhat the conception of them. We designate as analyser that apparatus whose function it is to decompose the complexity of the outer world into its separate elements; for example, the eye analyser consists of the peripheral part of the retina, the optic nerve, and the brain cells in which this nerve ends. The union of all these parts into one functional mechanism called analyser has its justification because physiology at present has no data for an exact division of the work of the analyser as a whole. We can not assert that a certain part of its function is performed by the peripheral section and other parts by the central end.

Thus the cerebral hemispheres, according to our understanding of the matter, consist of a number of analysers; of the eye, ear, skin, nose and mouth analysers. An examination of these analysers brought us to the conclusion that their number must be increased, that besides the above cited ones relating to external phenomena, to the outer world, there must be recognised in the cerebrum special analysers, whose purpose it is to decompose the enormous complexity of the inner phenomena which arise within the organism itself. Certainly not only an analysis of the external world is important for the organism, but of the same value is a *signallising upwards* and analysis of everything happening within the organism itself. Besides the external analysers there must be internal analysers. The most important of these inner analysers is the *analyser of movements.* We know that from all parts of the motor apparatus, from the joints and their surfaces, from the tendons, ligaments, etc., there originate centripetal nerves which signallise every moment the exact details of the act of movement. All these nerves unite above in the cells of the hemispheres. The most diverse peripheral endings of these nerves, together with the nerves themselves and their terminal cells in the great hemispheres, form a special analyser, which decomposes the motor act with its enormous complexity into a large number of the finest elements, whereby is attained the multiplicity and exactness of our skeletal movements.

Bound up with the conception of such an analyser is a marked interest in the physiology of the brain. In 1870 Fritsch and Hitzig demonstrated, as you know, that an electrical stimulation of a definite part of the cortex in the frontal half of the hemispheres called forth a contraction of certain groups of muscles. This discovery furnished a basis for recognising in these places particular motor centres. But then the problem came up as to how these parts of the hemispheres could be represented. Are they motor centres in the full sense of the word, *i.e.*, cells from which impulses proceed direct to the muscles, or are they merely sensory cells to which peripheral stimulations course and from which they are dispatched into the active motor centres, that is, those centres from which motor nerves go out directly to the muscles? This controversy, begun by Schiff, has not yet ended.

We also were forced to take a part in the decision of this question, and came to the following conclusion. We had been inclined for a long time to assume that those places in the cortex through the stimulation of which certain movements result are aggregations of sensory cells, *viz.*, cerebral endings of centripetal nerves, which go out from the motor apparatus. But how were confirmations of the correctness of this view to be found? Besides the data which had been adduced previously, and which were used by the defenders of this view, we succeeded in finding a new proof, and one seeming to us to be especially convincing.

If the so-called motor region is in reality the analyser of the motor apparatus and is entirely analogous to the other analysers (ear, eye, skin, etc.), then a stimulation brought to the analyser can be directed into any centrifugal path, *i.e.*, this stimulation can be connected with whatever activity we desire; in other words a conditioned reflex may be elaborated from a motor act. This we have succeeded in doing. Dr. Krasnogorsky, using, on the one hand, one of our usual stimuli, acid, and, on the other hand, flexing some joint, formed a temporary connection between the flexion and the work of the salivary gland. The definite movement called out a salivary secretion as well as did the stimuli from the ear, eye, etc. Then the question arose, how true is this interpretation of the given phenomenon; is this actually a reflex of flexion proceeding from the motor act, or a reflex from the skin?

In answer to this question Krasnogorsky succeeded in giving a proof which seems to me to be almost incontestable. When he formed on one leg a skin reflex, and on the other a flexion reflex, and then removed various parts of the cerebral hemispheres, it was proved that if the sigmoid gyrus was removed, the flexion reflex was lost, but the skin reflex persisted and might be demonstrated. On the contrary, when the gyri coronarius and ectosylvius were extirpated, the skin reflex disappeared and the flexion reflex remained. There was no room for doubting that the skin and the movement analysers are not the same, and that the latter analyser has its location in the motor region of the brain.

After these experiments I think we may speak specifically of the motor or the movement analyser with the same scientific justification as we do of the eye and ear analysers.

It remains for us to explain why movement results from electrical stimulation of those parts of the hemispheres in which are, as some authors assert, the motor centres. The sensory cells of the motor analyser are situated here, according to our opinion, and, consequently, from here in normal life stimulations stream out to the definite motor centres; hence it is conceivable that with such well worn paths, the stimulation of these places by electricity, evokes the usual result, *i.e.,* the impulse passes out from here along the customary ways to the muscles.

Thus from all our experiments we can say that the cerebral hemispheres represent a central station of all analysers, which may serve, either as do the eye and ear analysers for the analysis of the external world, or as the motor analyser, for an analysis of internal events, for example, movement. It is, however, obvious that our knowledge of the analysis of all other internal phenomena taking place through all the remaining internal analysers is much more limited. Besides the motor analyser we have not studied any other analysers of this sort by the method of conditioned reflexes. Undoubtedly, however, such phenomena will eventually enter into the physiology of the conditioned reflexes.

Now let us pass over to a consideration of the function of the analysers. What do they do? As indicated by their name, they have the purpose of decomposing complex phenomena into separate elements. But what do we know more particularly about their uses, and what have the experiments by the method of conditioned reflexes taught us? Here, I think, the objective point of view has served us admirably. General facts concerning the analysers were observed many years ago. The researches of Ferrier and Munk brought out a number of details bearing on the work of the analysers. These facts, however, were interpreted from a confused and unscientific point of view. When Munk removed the occipital and temporal gyri he noticed certain abnormalities of hearing and vision in the operated dog. Such peculiar behaviour of the animal towards the external world he called "psychical blindness" and "psychical deafness." What does this mean? Let us consider psychical blindness. It means that after extirpation of the occipital parts of the brain, the animal does not lose the ability to see. He avoids objects which are in his way, distinguishes between light and dark, but at the same time he does not recognise his master and the people he formerly knew. The dog fails to react to them; if they exist for the dog, they are only common optical stimuli. Now Munk and others assert that the dog "sees" but does not "understand." But what does this mean—he "understands," "he does not understand"? These words do not express any definite notion of the process and they, too, must be explained and interpreted.

The method of conditioned reflexes, after refusing all psychological conceptions, has brought a solid foundation and understanding to the matter. From the objective point of view, the removal of this or that part of the cerebral hemispheres was considered as a complete or partial destruction of this or that analyser. If the given analyser remained intact, if its brain end was uninjured, then the dog by means of this analyser differentiated separate elementary phenomena as well as their combinations, *i.e.,* the dog behaved normally in this respect. If, however, an analyser was destroyed or damaged in more or less degree, then the dog could not distinguish finely these phenomena of the external world. And this defect of analysis is proportional to the destruction of the analyser. If the analyser is completely destroyed, there is no trace of an analysis, even of the most elementary phenomena. If fragments of the analyser are left, if a part of it is still intact, then the connections between the organism and the environment remain, but in the most general form. Further, the more of the analyser which is saved from destruction, the better and finer its power of analysis. Briefly, if we consider injuries of the analyser as injuries of a mechanism, then it is evident that the more this analysing apparatus is damaged, the poorer its work. Such a conception makes the subject completely clear, and provides a basis for further investigations, while the psychological point of view confronts an insoluble problem, and contributes nothing by the expressions, "the dog understands," "the dog does not understand."

Now let us examine the experiments of Munk from our standpoint. We destroy the occipital parts of the hemispheres, *i.e.,* the brain end of the eye analyser. If after this operation only a minimal part of the analyser is uninjured, the animal can make only coarse analyses, distinguish between light and dark. In such an animal, you cannot elaborate conditioned reflexes to the form of objects, nor to the sight of their movement, but very easily to stimuli of light and dark. If, for instance, you repeatedly stimulate the dog with an intensive light just before feeding him, then later every time this light appears before the dog, the secretion of saliva begins; this is the work of that portion of the analyser which was left after the operation. This is the reason why Munk's dog did not collide with objects in his path. He distinguished between darkness and light, and passed around objects. In such a restricted way the eye analyser functioned well. But where a fine analysis was required, where it was necessary to distinguish between combinations of light and shade, forms, etc., the analytical ability was insufficient, for the damaged analyser did not function. It is plain why such a dog is unable to recognise his master—because he cannot discriminate between objects. There is no mystery about the matter. Instead of saying that the dog has ceased to "understand," we say that the analyser is damaged, and that it has lost the capacity to form conditioned reflexes on the more delicate and complex visual stimuli. And now we approach the great task of investigating this analyser step by step in order to see how it acts when it is whole and in good order, and what disappears gradually from its ability when we destroy one or another part of it.

We have exact and clear data bearing on this problem. If after our operation, there is left only an insignificant part of the eye analyser, the animal can form conditioned reflexes only to the intensity of light. If less damage is done to the analyser, then you can work out a conditioned reflex to the sight of moving objects, and later also to their form, etc., up to the point of normal activity, these different stages depending upon the extent of injury to the analyser.

The same holds for the ear analyser. If there is much damage, or if its activity is temporarily inhibited, then the animal can distinguish only between silence and sound. For such a dog there is no difference between various sounds. All sounds, noises and tones, whether high or low, are for him the same. The animal reacts only to the intensity of the sound; its detailed properties do not exist. If the destruction of the analyser is less, you can form independent reflexes to noises and musical tones, which signifies that there is some qualitative analysis yet present. If the injury is still less, then there is differentiation between separate tones, and the less the detriment to the analyser, the better is the analysis of tones. With a considerable destruction, the animal differentiates only between great intervals of pitch, for example, between octaves; if the injury is moderate, it differentiates between tones, and finally between fractions of a tone. We have a gradation from complete inability of the analyser up to normal activity.

I shall now present to you the highly interesting experiments of Dr. Babkin. As the dog he worked with lived for three years after removal of the posterior part of the cerebrum, one may consider that the condition became stationary. The dog easily distinguished not only between noise and sound but between tones of different pitch. To one tone there was a positive conditioned reflex, to another tone, no reflex, and hence in this respect the dog was entirely normal. But he had an irremediable defect; he could not differentiate between difficult sound combinations. Conditioned stimuli were made from a series of ascending tones, *do, re, mi, fa.* After some time the corresponding conditioned reflexes were obtained. But now reverse the order of the tones to *fa, mi, re, do.* A normal dog can distinguish between these combinations, but this one could not make so exact an analysis; try as much as we would, we obtained no differentiation. He had such an injury of his ear analyser that it became unable to perform this task. In close connection with this last phenomenon is an old and well-known fact, to which usually the expressions, he "understands," he "cannot understand," are applied; such dogs cannot recognise their names. The dog in this experiment was named "Ruslan," but after the operation his name produced not the slightest effect, though it was repeated a thousand times. His ear analyser was in such a state that he was incapable of distinguishing between sound combinations. If the dog cannot discriminate the groups of tones, *do, re, mi, fa,* from the same tones in another order (*fa-mi-re-do*), then he certainly cannot recognise his name—for "Ruslan" is a more complicated combination of sounds. Such an analysis is beyond the ability of his damaged ear analyser.

I reiterate that in the investigation of the function of the analysers there is great merit in the method of conditioned reflexes—the objective method. This method has entirely removed all mystery from our subject, rejected all meaningless expressions as "he understands," "he does not understand," and has replaced all these by a clear-cut and productive programme for the study of the analysers.

The problem before the investigator is the exact delimitation of the functions of the analysing apparatus, and the tracing out of all variations in its functions in the cases of destruction of its different parts. From the mass of facts obtained in this way, one may make an attempt to reproduce the structure of the analyser, and to decide of what parts it consists and how these parts mutually interact.

So much for the work of the analysers. Concerning their topography and their arrangement it must be said that an exact localisation, as proposed on the basis of earlier facts, is now unsuitable. As to this point there had been even previously many objections. Our experiments show the formerly established boundaries of the analysers are incorrect. Their limits are much wider; they are not so sharply separated, but are interlaced and interwoven one with another. Certainly it is a difficult task to define exactly how the analysers are distributed in the brain, how and why they are connected with one another.

From the standpoint of the conditioned reflexes the *cerebral hemispheres* appear as a *complex of analysers,* whose functions are: to *decompose* the intricacy of the outer and inner worlds into their separate elements and components, and further to *connect* all these with the manifold activity of the organism.

The next question with which the method of the conditioned salivary reflexes is closely related and which without this method cannot be decided or even strictly defined is this: Is the activity of the hemispheres limited to the mechanism of the formation of temporary connections and the mechanism of the analysers, or must there be recognised some higher mechanism for which at present we have no name? That is a question which is not based on fancy, but arises from experiment. If you cut out all the posterior part of the hemispheres behind the gyrus sigmoideus and along the fissura sylvia, you will have an apparently normal animal. With the help of his nose and skin he will recognise you and the food and all the objects he meets. He will wag his tail when you stroke him, and he will show joy on recognising you by sniffing, etc. But this animal will not react to you if you are at a distance because he does not use his eyes in a normal fashion. If you call him, he will not come. You must conclude that this dog uses his eyes and ears very little, although otherwise he is entirely normal.

But if you remove the frontal parts of the cerebrum along the same line as in the above operation, extirpating the regions which you left in the other dog, you get a completely abnormal animal. He has no correct relations to you, or to other dogs, to his food, or to any part of his environment. It is an entirely demented animal which apparently shows no sign of purposeful behaviour. Thus there is a great difference between the two animals—the one without the hinder, and the other without the frontal half of his cerebral hemispheres. About the first, you would say he is blind and deaf, but otherwise normal; about the latter, that he is an invalid and a helpless idiot.

Such are our facts. An important and pertinent question arises, Is there not something special, some higher function in the frontal parts of the brain as compared with its posterior sections, is there not here concentrated the essential functions of the cerebral hemispheres?

The method of the conditioned salivary reflexes gives a clear and definite answer to this question, such as can be obtained from no other method of research. Is it actually true that an animal without the frontal half of his hemispheres shows none of that higher nervous activity which characterises the normal animal? If you adhere to the former methods, if you observe only the activity of the skeletal muscles, then you will be inclined to answer in the affirmative. But if you turn to the salivary glands with their conditioned reflexes, the matter appears in an entirely different light. The value of our method depends not only upon the use of the conditioned reflex, but also upon the employment of the salivary gland in this reflex. If you observe the work of the salivary glands in such an operated dog, which seems at first sight to be demented, you will be astonished to see in what degree the complicated nervous relations of these glands are preserved. In the function of the gland you can find not the slightest disturbance. In such a dog (deprived of the frontal part of the hemispheres) you can form temporary connections, inhibit them, and dis-inhibit them. That is, the salivary glands exhibit the whole complexity of relations which is observed in a normal animal. You see plainly that there is an unexpected discrepancy between the activity of the salivary glands and that of the skeletal muscles. Whilst the muscles perform in an abnormal and disorderly manner, the salivary gland functions perfectly.

What does all this signify? First, it is unmistakably clear that in the frontal lobes there are no mechanisms which are dominant for the whole cerebrum. If such mechanisms were present there, then why did not the removal of this part of the brain destroy also all the delicate and complicated functions of the salivary glands? Why does everything proceed as normally with these glands? Obviously we must admit that all the peculiarities which we observed in this dog are phenomena relating to the skeletal musculature. Our task is thus reduced to finding out why the action of the skeletal muscles becomes so disturbed. There is no ground for assuming a general mechanism situated in the frontal lobes. In these lobes there cannot be any important arrangements which condition the highest perfection of nervous activity.

A simple explanation of these disturbances in the work of the skeletal musculature as a whole is this—the activity at every *moment* is dependent upon the skin analyser and the motor analyser. Thanks to these analysers, the movements of the animal are continually co-ordinated and accommodated to the surroundings. In this dog without frontal lobes, both the skin and the motor analysers have been destroyed, and it is natural that the general activity of the skeletal musculature should be impaired. Consequently, we have in the destruction of the frontal lobes only a partial defect (analogous, for example, to the injury of the eye analyser), but not such a general disorganisation as might result from the removal of the influence of some hypothetical higher mechanism of the brain situated in these lobes.

A series of experiments were performed in view of the importance of this question, in collaboration with Drs. Demidov, Saturnov and Kurayev. The experiments were so conducted that the entire frontal lobes, including the olfactory parts, were removed. In such a dog the conditioned salivary reflexes could be elaborated only from the mouth cavity, the so-called water reflex; if as the unconditioned stimulus, a solution of acid was poured into the mouth, then it happened that the introduction of water, which by itself is without effect on the salivary glands and formerly was indifferent for them, caused a secretion, showing thus the production of a conditioned reflex. But as this water reflex might not be convincing enough to every one, it was necessary to prove in such a dog the existence of other reflexes. Therefore, Saturnov extirpated the frontal lobes but left the olfactory. In this dog there could be formed after the operation a conditioned reflex from the olfactory nerves.

After these experiments we must consider the subject clarified, and come to the final conclusion that a dog without frontal parts of the cerebral hemispheres loses only some special mechanisms, *i.e.,* some of its analysers, but not a general or higher mechanism.

When we thus examine the activity of the brain by the method of the conditioned reflexes, we get an absolutely definite answer. Maintaining our exact facts as a foundation, we can state that the cerebral hemispheres represent an accumulation of analysers, which decompose the activity of the external world into its individual elements and moments, and afterwards connect the phenomena thus analysed with the special activities of the organism.

Can we be satisfied with the results obtained? Without doubt we can, for our experiments afford a starting point for the further successful investigation of the subject. At the same time it is clear that this study has just begun, and its magnitude and difficulties stretch far out into the future. If we would outline the further investigation of this subject we must pay the greatest attention first of all to our present method, the dismemberment of the apparatus under investigation (*i.e.,* the hemispheres) into its component parts. A method inherently combined with infinite difficulties! The more we experiment with extirpations of the brain the more we wonder at the results obtained with this method by former investigators. Owing to the extirpation we never attain a stable condition, it constantly changes. You put your hands on the brain, you touch it with your heavy hands; you have wounded it, and removed certain of its parts. This damage irritates the brain, and the action of the injury lasts for an indefinite time and spreads to uncertain limits. You cannot say when this detrimental effect will end. That such a harmful effect exists, however, is proved by many well known experiments, which it is not necessary to review. Finally, the looked-for moment comes when the irritation from the wound passes. But now appears a new stimulus—the scar. And there are only a few days during which you can work with the certitude that all the observed changes depend only upon the absence of the extirpated portions of the hemispheres. Then the following course of events sets in: At first there appear the phenomena of depression, and you know that it is the effect of the cicatrix. Such a condition continues for some days, and then appears a convulsive attack. After the irritation, after the depression, after the convulsion, there ensues a new period of depression or an entirely special state of the animal. After the convulsion you cannot recognise your dog as the same one you had before; he is much more upset now than he was immediately after the operation. Evidently the scar has not only irritated, but has exerted pressure, drawn and wrenched the tissues, in a word, done damage again.

I must add that this effect of the scarring never ceases, at least I have never seen it end. The affair sometimes lasts for months and years. Convulsions usually begin after one to one and one-half months, and then they recur. We have operated on many scores of dogs, and I can categorically state that not a single one escaped convulsions, and if they were not repeated it was only because the animal died during the first attack.

Try under these discouraging conditions to analyse successfully so complex an activity as that of the brain. Without doubt the contemporary investigator of the cerebrum should above all be scrupulous as to how to adapt his manipulations in relation to this organ. This is a most important question, because under present conditions there are lost an enormous amount of human labour and a multitude of animals. Endeavours have already been made to reduce this waste. A German experimenter, Trendelenburg, has tried local freezing of the brain, and in our laboratory Dr. Orbeli is using the same method. The near future will show us in what degree this procedure will be of avail, and what good it will bring to us.[1]

These are our results, our complaints, and our hopes.

[1] The difficulties and complications following operations on the hemispheres brought Pavlov almost to despair of this method as a means of physiological investigation. And until now he has not used it systematically for investigation of the physiological mechanisms in the brain, having recourse to it only occasionally. In his other book, *Activity of the Cerebral Hemispheres*, this point of view is expressed even in the titles of the chapters dealing with the experiments on destruction of the hemispheres: chapters xix-xxi are entitled "Pathological Disturbances," etc.—*Translator.*

CHAPTER XIX

INTERNAL INHIBITION AS A FUNCTION OF THE CEREBRAL HEMISPHERES

(Article published in the Anniversary Volume of Prof. Ch. Richet, Paris, 1912.)

REVIEW OF THE TWO CHIEF MECHANISMS IN THE HEMISPHERES—DISCUSSION OF THE FORMATION OF THE CONDITIONED REFLEX—EXTINCTION; RETARDATION; DIFFERENTIATION; CONDITIONED INHIBITION: ALL TYPES OF INTERNAL INHIBITION—LAW OF IRRADIATION AND CONCENTRATION—DIFFERENTIATION OF INHIBITION—DIS-INHIBITION —INHIBITION ALWAYS FOLLOWS EXCITATION.

More than ten years have passed since I decided to study the complicated nervous relations of higher animals (the dog) to the outer world. Usually these relations were understood and analysed on the basis of an analogy with our own subjective internal world, and considered as psychical. I undertook their objective external investigation just as physiologists study the other functions of the organism. Now for more than ten years I and my collaborators have applied ourselves energetically to this problem. We have collected considerable material on the subject, but it has been published only in Russian in doctors' dissertations or in reports to scientific societies. I refrained from publishing in foreign languages because I intended to advance and systematise our inquiry in order to present reasonable and acceptable physiological conclusions. So it happened that I postponed a complete and systematic review of the attained results, and permitted to be printed only short, generalised communications regarding our investigations. But at present, wishing to express my esteem to one of the creators of contemporary physiology, I ask your attention to a group of phenomena constituting an independent chapter on the subject of conditioned reflexes.

As I explained in my report in Moscow in 1909,[1] we investigate and consider the higher nervous activity of the dog as the work chiefly of two nervous mechanisms: the *mechanism of temporary connections* between external agents and certain activities of the organism, *i.e.*, the mechanism of temporary reflexes, which we call conditioned reflexes in contrast to those better known and recognised reflexes which we designate as unconditioned; and another mechanism, the mechanism of *analysers*, whose function it is to decompose the complexity of the surrounding world into its elements. The analyser consists, according to our scheme, of a receptor surface (for example, the retina), of the

[1] See chapter x of this book.—*Translator.*

corresponding nerves (for example, optic, auditory), and the brain ends of these nerves located at various levels of the central nervous system, including the hemispheres. The work of these two mechanisms embraces a countless number of simple and complicated nervous relations of the animal to the outer world.

In 1910 [2] I endeavoured to systematise the phenomena of inhibition which appear in the work of the above-mentioned mechanisms. First, we considered a group of inhibitions which can be easily defined and characterised, and which we termed *external inhibition.* The mechanism of this inhibition is apparently the following: if some other point of the central nervous system is set into activity through the corresponding external or internal stimuli, this causes an immediate diminution or complete disappearance of our conditioned reflex—the conditioned reflex becomes weaker or vanishes.

Besides external inhibition there exists another group of inhibition phenomena whose mechanism is quite different. The conditioned reflex, which is a temporary connection of some external, previously indifferent agent with a certain function of the organism, originates because the action of this indifferent agent on the receptor surface of the animal repeatedly coincides in time with the action of an already existing reflex stimulus of one or another activity. Owing to this coincidence, the indifferent agent becomes a stimulus of the same activity. All our experiments have been done on the salivary gland, which, as you know, reacts to psychical stimulations, using the old terminology, and is consequently in complicated relations to the external world. Food and other stimulating substances entering into the mouth of the animal cause an unconditioned reflex; *a conditioned reflex, however, may be called out by any agent of the external world if it is capable of acting on any receiving surface of the organism.* It is clear that a pre-formed reflex must exist as the basis of this formation of the new reflex. Now, if the conditioned stimulus acts for some time alone, unaccompanied by the unconditioned stimulus with the help of which it was formed, then the action of the conditioned stimulus becomes weaker, or, in other words, is inhibited.

The first clear example of inhibitory action is the phenomenon which we call the extinguishing of the conditioned reflex. If a well elaborated and stable conditioned stimulus be repeatedly applied at short intervals, alone, without being followed by the unconditioned stimulus, then it begins to decrease and gradually becomes inactive. This is not a complete destruction of the conditioned reflex, but only a temporary suspension. That this is so is proved by the spontaneous restoration of the reflex after an interval, unless during this time something acts

[2] See chapter xi of this book.—*Translator.*

unfavourable to its restoration. That this extinction of the conditioned reflex is not a matter of fatigue is proved by its restoration without the assistance of the unconditioned stimulus. How this reappearance comes about will be referred to on later pages of the present paper.

This *extinguishing* was the first instance of the different kinds of inhibition which we have studied. We have met with other examples, however, since this first one.

If we have established a conditioned reflex by giving an unconditioned stimulus (for example, food) shortly (for example three to five seconds) after the beginning of the action of some indifferent agent, then the indifferent agent becomes a conditioned stimulus, quickly manifesting its effect: now if this conditioned stimulus is applied alone, saliva begins to flow after a few seconds. But let us slightly alter the experimental relations. We systematically join the unconditioned stimulus to the conditioned stimulus three minutes after the beginning of the latter; in this case, the conditioned stimulus soon becomes weak, and disappears for a certain period, and then the following state of affairs sets in: during the first minute, or one and a half to two minutes, there is no effect seen from the conditioned stimulus; its action becomes manifest only at the end of the second minute, at first weakly, then increasing in strength, and attaining its maximum at the moment of the connection with the unconditioned stimulus. Such a conditioned reflex is *delayed,* and the phenomenon itself we call *retardation.* What kind of phenomenon is this? The apparently effective conditioned stimulus remains without action during the beginning of its application. The analysis of this fact showed us that in the case of the delayed reflexes inhibition takes place, because disappearance of this state of inhibition may be brought about immediately by special interference, and the conditioned stimulus will then be as active at the beginning as at the end of its three-minute application.

The third kind of inhibition is manifested during the differentiation of stimuli. Let us elaborate a conditioned stimulus from a tone of some musical instrument of 800 vibrations: finally, we have a stable and considerable action. Now let us apply for the first time the nearest tones; we get at once an effect somewhat proportional to the proximity of the second tone to that which was originally used as the conditioned stimulus. But if you constantly and systematically accompany the original tone of 800 vibrations with the unconditioned stimulus (for example, food), *i.e.,* reinforce your conditioned reflex, as we are accustomed to say, and if you repeat the other neighbouring tones without the unconditioned stimulus, then these neighbouring tones gradually become ineffective.

It is plainly seen that the *differentiation* of these tones is effected through the development of an inhibitory process set up by the neighbouring tones. To prove this, you begin the experiment by trying the tone of 800 vibrations. It shows its usual significant effect. You reinforce it by the unconditioned reflex. After establishing the conditioned reflex you may be sure that subsequent repetitions of the experiment give a uniform result. Instead of the usual experiment, you now apply soon after the first trial (*i.e.,* first for that day) of the tone of 800 vibrations, one of 812 which you have well differentiated, never having given food after it; the action of the latter is null. But now try again your tone of 800 immediately after the negative tone (812) and you will find that this time it has either no action or a very slight one. If, instead of applyng the positive conditioned stimulus immediately after the negative, you apply it 15 to 20 minutes later, then it has its usual effect, *i.e.,* produces the same flow of saliva. Consequently, in order to abolish the action of the neighbouring tones, inhibition must arise, and this inhibition only slowly disappears from the hemispheres.

Finally, the last type of inhibition. We take some indifferent agent having no marked effect on the animal and add this to a well elaborated conditioned stimulus, not accompanying this combination of two agents by the unconditioned stimulus (food) ; the indifferent agent will gradually become an inhibitor of the conditioned stimulus, *i.e.*, the combination of the conditioned stimulus with the indifferent agent is always null, although the conditioned stimulus used alone is as active as before. This phenomenon we call *conditioned inhibition.* Here, too, we have an after effect of the inhibition, just the same as we have described in the case of differentiation of the stimuli.

All these types of inhibition we have united under the term of *internal inhibition.* This group is rather a natural one, because all its members are characterised by the same general, sharp features.

In 1870 the experiments of Fritsch and Hitzig laid the foundation for an exact and successful physiology of the brain, and physiologists made acquaintance with the important and not sufficiently appreciated fact that the stimulation of a certain point of the hemispheres always tends to irradiate quickly: the initial contraction of a definite group of muscles under the influence of a continuous or strong stimulus passes over into tonic convulsions of the whole body. This is a characteristic of the mass of the cerebral hemispheres, of the most reactive and most labile part of the central nervous system.

It is a well known fact, which may be observed with all agents, that when they have just become conditioned stimuli their action, during the first days, is very general, *i.e.,* there now act as conditioned stimuli (calling out the same effect as the original stimulus) all phenomena similar or akin to the elaborated conditioned stimulus. Only by and by, under the influence of definite circumstances, does the conditioned stimulus become specialised (specific), *i.e.*, the action of all these accessory stimuli, which do not coincide with the conditioned reflex, is obliterated by the process of inhibition, and only the conditioned stimulus remains active. Now it is quite natural to conceive the first effect as a phenomenon due to irradiation.

These considerations and our additional data give us the right to accept the law of *irradiation* and concentration for impulses arriving in the hemispheres; first these excitations spread, and flood the whole cerebrum, and later they collect in certain, definite points.

This law of irradiation and concentration is even more clearly manifest in the process of the internal inhibition than in the process of excitation. Here are some illustrative facts. Suppose that we have several conditioned stimuli connected with the same unconditioned stimulus. According to the method mentioned above, let us extinguish one of these conditioned reflexes. Immediately after, we can see partial or complete extinction of all other conditioned stimuli, even those belonging to other analysers. If you now vary the experiment so that after the extinction of one of the conditioned stimuli, you try the other ones, not at once but after some minutes, you will see that the latter, at this time, operate with full effect, but the one which you have extinguished remains for a long period inhibited. From these cases of extinction one may suppose that the inhibition originates in the analyser to which the extinguished stimulus belongs, whence it irradiates to other analysers; afterwards it disappears from these other analysers first, and concentrates in the initial point (experiments of Horn).

Similar relations are observed in differentiation of inhibition. Let us form a conditioned stimulus from a definite tone and differentiate from it other tones. Suppose that the positive tone is one of 800 vibrations, and that one of 812 vibrations is ineffective (negative). Besides this we prepare several conditioned stimuli from agents affecting other analysers, using the same unconditioned stimulus which was connected with the tone of 800 vibrations. In order to attain a strong inhibition, a fine differentiation was elaborated, and, therefore, after the application of this differentiated (negative) tone, the positive tone of 800 vibrations, as well as the positive stimuli of other analysers, were ineffective. If the differentiation were not so delicate (if the negative tones used were two or three full tones higher or lower), only a weak inhibition is developed, wherefore after application of this negative tone only the positive tone stimulus is directly inhibited, and the positive stimuli of other analysers remain active (experiments of Belyakov).

The same relations are strikingly corroborated in experiments with the skin analyser (work of Krasnogorsky). We apply as a conditioned stimulus the mechanical irritation of the skin. For this purpose we arrange on the hind leg of the dog a series of four pricking apparatuses at a certain distance from one another, and we see that the stimulation of these points gives a regular and uniform effect. Now we differentiate from these stimuli the action of a fifth apparatus, placed at the lowest part of the leg, and always unaccompanied by food, *i.e.,* a negative stimulus. The effect of all our stimuli, as I have said before, is measured by the number of drops of saliva. Suppose we get 10 drops during thirty seconds from each of the upper four (positive) conditioned stimuli. Now we apply the fifth and lowest apparatus and receive a null effect—full differentiation. Thirty seconds later we try the action of the four upper apparatuses (positive), and we find that they are all inert. If now the same trial is made one minute after the action of the negative conditioned stimulus, we get quite another result. The following number of drops is received, respectively, taking the apparatuses in order from above downward (the last figure showing the apparatus next to the inactive one): 5, 3, 1, 0; after waiting two minutes, 10, 8, 5, 2; after three to four minutes, 10, 10, 10, 4; and after five to six minutes the normal and equal size of reflexes from all of the four positively acting apparatuses. It is to be understood that all these experiments must be performed under equal conditions, during the course of several days, etc.

From this experiment it is clear that the inhibition, which resulted from the lowest apparatus, irradiated over a great region of the skin analyser, and afterwards gradually concentrated around its point of origin.

The group of internal inhibitions is distinguished by the following highly characteristic features. For the sake of complete clearness I shall give a concrete case. Suppose we have a delayed conditioned reflex, *i.e.,* the conditioned stimulus calling out this reflex does not exercise its effect immediately but only one to two minutes after its beginning, during the third minute of its action. If now during the first ineffective phase of the conditioned stimulus (the first two minutes of its action), there acts on the animal some agent of moderate strength, producing an external inhibition, for example, an agent calling out a slight orienting reflex, saliva will begin to flow immediately; the conditioned stimulus has, now, become effective earlier. Certainly this additional agent *per se* has no relation to the salivary gland, and is incapable of producing a salivary secretion.

As this agent has an inhibiting effect on the same conditioned stimulus during the active phase, we may conclude that during the inactive phase it becomes effective by inhibiting the internal inhibition, and thus freeing the stimulation (experiments of Zavadsky). Such *dis-inhibition* is met with in other cases of internal inhibition.

If we have produced extinction of a conditioned stimulus to a certain degree, or even to zero, we can at once restore its action, to a greater or less degree, by joining to it an agent from the group of external inhibition (experiments of Zavadsky). In this way one may cause to disappear the inhibition of all kinds of differentiation (experiments of Belyakov), as well as of the conditioned inhibition (experiments of Nikolayev).

As I have said in previous publications, *dis-inhibition* is manifest only under certain conditions, *viz.*, if the *dis-inhibiting* agent is of average strength (not very strong and not too weak). But if this agent is of great intensity it inhibits the conditioned stimulus itself, and consequently there is nothing to be liberated from internal inhibition. It is essential that this agent be of definite strength, neither too powerful, lest it inhibit the stimulus, nor too weak, lest it can not inhibit the internal inhibition. Only under these conditions is there complete dis-inhibition. If our explanation of the facts is accepted, then one must come to the conclusion that the process of internal inhibition is less stable than that of excitation. I do not exclude the possibility of other explanations of what we call "internal inhibition," but I see no serious objection to our understanding of the phenomena. We must admit that at the present time we know altogether nothing of the real nature of internal inhibition.

If you employ a process of internal inhibition which is already present, you can obtain thus a new negative inhibitory conditioned reflex just as you may receive a new positive conditioned reflex with the aid of well elaborated conditioned reflexes (experiments of Volborth). This is done in the following manner: we apply a well established conditioned reflex, and by the method described above, cause its complete extinction. To the extinguished stimulus an agent is added which must be indifferent to the extent that it does not act on the extinguished stimulus, *i.e.*, it should not dis-inhibit it. Such a combination is repeated several times, whereby the indifferent agent receives the potentiality of the conditioned inhibition, *i.e.*, if it is now joined to the active conditioned stimulus which gives a full effect, it weakens this effect; this weakening may be considerable and may lead to the complete disappearance of its action. Consequently, the formerly indifferent agent, which coincided many times wth the process of internal inhibition, became connected with the inhibition, and thereupon its application called out this process.

One must notice that the three above-mentioned characteristics of internal inhibition are also properties of the excitatory process. This harmonises well with the view, which occupies an ever larger place in physiology, that *inhibition always follows excitation,* that it is in a certain sense the *reverse side of excitation.*

More and more material must, evidently, be collected in order finally to give a firm foundation for a more or less proper understanding of the mechanism of the central nervous system.

When many years ago I began to devote my energies to the objective study of the highest parts of the central nervous system, I was continually astonished and impressed by the infinite complexity [3] of the existing relations. But it also seemed to me that the higher sphere of nervous activity, in comparison with the lower parts of the central nervous system, offers to the experimenter many advantages. In the spinal cord we find pre-formed connections; we can not see nor assist in their elaboration, and consequently we do not know what elementary properties and what most general and simple laws, manifesting themselves in the central nervous system, played a part in their formation. It is otherwise with the higher parts. Here we observe the phenomena in unbroken succession, and we continually see the elaboration of new relations and the analysis of stimuli, and thus we have the possibility of watching and learning how these phenomena develop and upon what elements they rest.

[3] See chapter xxvi, footnote 3.—*Translator.*

CHAPTER XX

THE OBJECTIVE STUDY OF THE HIGHEST NERVOUS ACTIVITY OF ANIMALS

(Read before the Society of the Moscow Scientific Institute, March, 1913.)

BEHAVIOUR AND AMERICAN PSYCHOLOGY—PSYCHICAL REACTIONS ARE CONDITIONED REFLEXES—SENSE ORGANS AND ANALYSERS—WAKING AND SLEEPING STATES—THE SLEEP INHIBITION; EXTERNAL INHIBITION; INTERNAL INHIBITION—THE DEFENCE REACTION IS OVERCOME BY THE FOOD REACTION BUT NOT BY THE ACID REACTION—IRRADIATION OF EXCITATION ILLUSTRATED BY EXPERIMENT—PYSCHOLOGICAL EXPLANATIONS HOPELESS—THE AGGRESSIVE REFLEX—EMOTIONS—CONDITIONED STIMULUS MUST PRECEDE THE UNCONDITIONED—INTERACTION OF CENTRES—CONSCIOUSNESS.

IN to-day's illustrious festival of Russian science permit me to call your attention to the work of Russian investigators in one of the most interesting regions of contemporary research. The theme of my report is the objective study of the higher nervous system of animals.

With full justice Charles Darwin must be counted as the founder and instigator of the contemporary comparative study of the higher vital phenomena of animals; for, as is known to every educated person, through his highly original support of the idea of evolution he fertilised the whole mentality of mankind, especially in the field of biology. The hypothesis of the origin of man from animals gave a great impetus to the study of the higher phenomena of animal life. The answer to the question as to how this study should be carried out and the study itself have become the task of the period following Darwin.

Since 1880 more and more frequent have become investigations of the influence of surroundings on the movements of animals, or according to the American terminology, *behaviour*. First the attention of biologists was turned to the lower animals. Simultaneously, with a purely physico-chemical explanation of the external reactions of the animal, as, for example, in the study of tropisms and taxises, there were also attempts at an objective, realistic description and systematisation of the facts which constitute the behaviour of animals, as well as the psychological understanding of the phenomena (these latter more rarely). These studies continually widened, and embraced an ever increasing number of animals all along the zoological ladder. Most of these investigations at present belong to North America, the new home of science. But in American researches on the behaviour of higher animals there is, in my opinion, a gross defect which prevents the success of the work, but which, I have no doubt, will sooner or later be removed. I refer to the application of psychological conceptions and classifications in this essentially objective study of the behaviour of animals. Herein lies the cause of the fortuitous and conditional character of their complicated methods, and of the fragmentary and unsystematic character of their results, which have no well planned basis to rest on.

Twelve years ago, I with my collaborators, to whom I send friendly and grateful greetings, decided to study the higher nervous activity of the dog, strictly objectively, absolutely excluding psychological conceptions in the analysis of our material.

The present report, although sketchy, will include a complete review of our work and our conclusions. I shall enumerate our chief facts, show how they may even now be systematised, and what conclusions may be drawn from them.

Definite, constant, and inborn reactions of the higher animals to certain influences of the external world, reactions taking place through the agency of the nervous system, have for a long time been the object of strict physiological investigation, and have been named reflexes. We call these *unconditioned reflexes*. The apparently endlessly complicated, seemingly chaotic (forming and disappearing during the life of the individual), constantly vacillating reactions of the higher organism to the countless and eternally changing influences of the outer world—in short, what are generally called *psychical functions* we recognise also as reflexes, *i.e.,* regulated responses to the external world. As these reactions are dependent upon a multitude of conditions, it appeared logical to us to designate them as conditional, as *conditioned reflexes.*

Numerous and infinitesimal phenomena of the external world become stimuli of the several functions of the organism under only one condition: their action must coincide one or many times with the action of other external agents which call out the activity of the organism, and then these new agents themselves begin to provoke this activity. Food, the chief bond between the animal, the living organism, and the surrounding world, stimulates (through its odour, and its mechanical and chemical effect on the surface of the mouth) the food reaction of the organism—*i.e.,* the approach of the animal to the food, the introduction of it into the mouth, the flow of saliva, etc. If with the action of food on the animal there coincides several times the presence of any indifferent agent, then this latter will call out the food reaction. The same holds true for the remaining activities of the organism, the defensive, the reproductive, etc. These functions begin under the influence of constant as well as of temporary stimuli. In this way these temporary stimuli become signals—the agents of the constant stimuli—and they make much more complex and delicate the relations of the animal to his environment.

But it is clear that the organism must possess mechanisms which are capable of decomposing the environment into its elements. These it has in the form of the so-called *sense organs*, which, in our objective analysis of life, correspond to the scientific term, *analysers*.

The action of the mechanism through which the temporary connections, or conditioned reflexes are formed, and the more delicate work of the analysers lays the basis of the higher nervous activity. It has its seat in the cerebral hemispheres; the coarser analysis and the unconditioned reflexes being functions of the lower parts of the central nervous system. It is clear that these complex and delicate relations of the animal organism to the outer world are constantly vacillating and changing. We have already become acquainted with three different kinds of inhibition which lead to the weakening or complete disappearance of the conditioned reflexes.

Sleep, or, as we call it, the sleep inhibition, divides the life of the organism into two phases: the *waking* state and the *sleeping* state, or the *externally active* and the *externally passive* state. Under the influence of inner causes and also under certain external stimuli, drowsiness and sleep set in, during which the activity of the higher parts of the central nervous system, as manifested in the conditioned reflexes, is either weakened or entirely disappears. By means of this inhibition an equilibrium is maintained between those parts of the organism which are directly concerned with the external world, an equilibrium between the processes of destruction and of restoration of the reserve material of organs during their states of work and of rest.[1]

The second kind of inhibition, which we call *external inhibition*, is an expression of the concurrence of different outer, as well as of inner, stimuli on the predominant influence in the organism during every separate moment of its existence. This is an inhibition which we meet as often in the lower as in the higher parts of the central nervous system. Every new agent at the outset of its action on the central nervous system enters into a struggle with those other agents operating there; sometimes it weakens the effect of the latter, at other times it routs them completely, and in other cases it retreats and leaves the field to the agents formerly present. Translating this into the language of neurology, we may say in the given case that a strongly excited point of the central nervous system decreases the irritability of all surrounding points.

The third sort of inhibition of the conditioned reflexes, we call *internal inhibition*. This is a rapid loss of the positive effect of the

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[1] Here sleep is considered as a peculiar kind of inhibition. Further investigation proved that sleep is to be regarded as widely irradiated internal inhibition. The chapter, "Inhibition and Sleep—One and the Same Process," contains the whole body of evidence strengthening this view.—*Translator.*

conditioned stimuli when they do not act as true and exact signals and agents of the unconditioned stimuli. This, however, is not a destruction of the conditioned reflex, but only a suspension.

While some agents of the external world can condition the above-described kinds of inhibition, others, on the contrary, may remove an existing inhibition. We thus have the phenomenon of *dis-inhibition*—the freeing of the stimulation from the effect of the inhibition.

This kaleidoscope of the conditioned reflexes, with their fanciful and apparently irregular and incomprehensible course, is in reality exactly determined, *viz.*, through the strength, duration, and direction of the movement of the nervous process in the mass of the great hemispheres. I shall now illustrate this through examples taken from actual experiments. You have before you two kinds of external agents; on the one hand, various substances, edible and inedible, which, introduced into the mouth of a dog, are accompanied by corresponding reactions (certain movements, certain secretions); and on the other hand, an electric current which, directed to one or another point of the skin, calls out also a corresponding reaction of the animal—the *defence* reaction. If you allow both agents to act simultaneously there begins in the central nervous system a conflict. If the electric current is confined only to the skin, and at the same time food is put into the mouth of the dog, the struggle ends with the victory of the nutritive agent, and the electric current, be it ever so strong, becomes the signal, the representative, of food, and a conditioned stimulus for the food centre. The electric stimulus now provokes not a defence, but a food reaction: the animal turns toward the experimenter, makes licking motions, etc., as before eating. The same is observed if the electric current is supplemented by burning and wounding of the skin. In other words you have an actual diversion of the nervous process from the paths of the defensive centre to the paths of the food centre.

But if you take some other combination—the same electric current and the introduction of acid into the mouth—you never succeed in forming a conditioned reflex for the acid from the electric stimulus, no matter how often this combination is repeated. The nervous process aroused by the acid in the mouth is not strong enough to overwhelm the nervous process set up by the action of the electricity on the skin. Let us go further. If you apply the electric current on such parts of the body that it penetrates to the bone, then in spite of your patience, with certain intensities of the current you will never receive a conditioned reflex from the electricity, even taking food as the unconditioned stimulus. Now the nervous process from the electric stimulation is more intense than the nervous process from food. We know subjectively that bone is more sensitive to pain than the skin. The nervous process is thus despatched in the direction of the most powerful stimulation. It should not be difficult to show the practical meaning of the relations brought out in this experiment: for example, we often see in the struggle of the animal for food that the integrity of the skin is sacrificed. The danger to the existence of the organism in this case is not so great, and the animal prefers the danger involved in the acquiring of food to the safety of his skin. But if the bones are broken, the animal, to preserve himself from total destruction, must neglect for a time the calls of nutrition.

Thus the relative intensity of the nervous processes determines the direction of the nervous impulses, and the connection of the agents with the various activities of the organism. These relations of intensities comprise a large chapter in the physiology of the conditioned reflexes: and the exact definition of the comparative intensity of nervous processes set up by the action of various stimulating agents forms a most important point in the present study of the activity of the cerebral hemispheres.

The latent after-effects of foregoing stimulations at any given moment have enormous significance for the activity of the hemispheres. For this reason it is necessary to make a careful study of the duration of such latent effects. In this direction, too, the physiology of conditioned reflexes furnishes abundant material. For example, the ticking of a metronome, indifferent because it has not been connected with any activity of the organism, has an effect on the conditioned reflexes for some seconds, or even a minute, after its cessation. The introduction of acid into the mouth of the dog alters the conditioned food reflex for 10 to 15 minutes after its application. The eating of sugar can change for several days the conditioned reflex on meat and bread powder. It is a great, though entirely feasible, task to calculate the traces of the stimuli which have previously fallen upon the animal in question.

Not less important is the establishment of a general rule for the movement in the cerebral hemispheres of the nervous processes, excitation as well as inhibition. Forty years ago in the first exact experiments on the hemispheres it was noticed that the stimulation of a certain point on the cortex of the brain, if it be of short duration, calls out a contraction only of a certain group of muscles. If, however, the stimulation lasts for some time, it spreads over to other muscles, indeed over the whole skeletal musculature, leading to convulsions. Obviously, the fact which confronted the physiologist was a characteristic of the brain as a part of the central nervous system, *viz.*, the fact that excitation easily spreads from its original point over a large area of the cerebral cortex, the fact of irradiation of the nervous process over many groups of nerve cells. This *irradiation* of the excitation is constantly met with in the physiology of the conditioned reflexes.

If you have made some special tone a conditioned stimulus of the food reaction, then not only this tone, but in general all sounds, will call out the same conditioned reflex as that produced by the special tone. Or if you have a conditioned reflex from the pricking or stroking of some particular spot of the skin, then at first in the early part of your work, pricking of all remaining points of the skin also evokes the conditioned reflex. This is a general fact. We must suppose that in all these cases the excitation arriving at a certain point of the cerebral hemispheres spreads and irradiates over the corresponding part of the brain. It is only in this way that all the stimuli of the given category could become connected with a definite activity of the organism.

The fact of irradiation of the nervous process is even more clear and salient in the case of internal inhibition. The following is an experiment remarkably convincing. You arrange on the leg of the dog a series of apparatuses for the mechanical stimulation of the skin. The action of the upper four is accompanied by feeding. After some repetitions, the mechanical stimulation of these four points produces the food reaction; the animal turns toward the operator, makes licking motions, secretes saliva, etc. Due to the process of irradiation, the fifth and lowest apparatus, when for the first time put into action, also calls out a flow of saliva although its action was never accompanied by feeding. But if you repeatedly use it without feeding, you finally reach the point where it produces no visible effect (negative conditioned stimulus). How has this been brought about? It has occurred in consequence of the development of the inhibitory process in the corresponding point of the central nervous system. The proof of this is obvious. If you now apply the fifth (negative conditioned stimulus) apparatus, then for some time afterwards all the upper (positive conditioned stimulus) apparatuses are without effect. The inhibitory process has irradiated from its original point to neighbouring regions of the cerebral hemispheres.[2]

The irradiation of the nervous processes forms thus one of the fundamental phenomena of the activity of the cortex of the brain. Related to this process is its counterpart—the *concentration* and collection of the nervous process in a certain point. To save time I shall demonstrate this new phenomenon with the same experiment. You apply for a long while the action of the lowest apparatus (negative conditioned stimulus). If you try the upper apparatuses after a short interval, they also will be ineffective. The greater the interval of time between the trial of the lowest (negative conditioned stimulus) and of the upper four (posi-

[2] See also preceding chapter for a description of this experiment.—*Translator.*

tive conditioned stimuli), the more is the inhibition freed, and in strict succession from above downwards, until, when the interim is sufficiently long, there is no inhibition from the application of the lowest of the four apparatuses.[3] Before your eyes the inhibition wave contracts and returns to its starting point, in other words, it concentrates. After many repetitions of the ineffective stimulus, the inhibition concentrates more and more quickly, first during minutes and then during seconds, and, finally, during a hardly perceptible period. In this way the separate phenomena of the nervous activity of the cerebral hemispheres are subject to two general laws (or they may be spoken of as one law)—the law of irradiation and of concentration of the nervous process.

Hence it follows that the pivotal point in the scientific investigation of the activity of the brain lies in the delimitation of the paths along which the nervous process spreads and concentrates—a problem of space relations. This is the reason why from the strictly scientific point of view, it seems to me that the position of psychology as a study of *subjective states* is completely hopeless. Certainly these states for us are a reality of the first order, they give direction to our daily life, they condition the progress of human society. But it is one thing to live according to subjective states, and quite another thing to analyse purely scientifically their mechanism. The more we work with conditioned reflexes, the more we are persuaded how deeply and radically the decomposition of the subjective world into its elements and their grouping by psychologists differ from the analysis and classification of nervous phenomena by physiologists who think in terms of spatial relations.

Partly to give an example of this, partly to show how wide are the boundaries of our investigations and what they include, I shall describe some more experiments.

Our experimental animal seems to be a watchdog, and in addition, one of a nervous temperament. He reacts aggressively to every person who enters the room where, with the experimenter sitting close beside him, he is on the stand. This pugnacity becomes pronounced if the intruder makes some threatening gesture, or if he strikes the dog. In the objective study of the central nervous system, this represents a special reflex, the *offensive reflex.* The internal mechanism of the nervous system of the given dog is revealed in the following experiment. The intruder—the cause of the continued and energetic aggressive reaction of the animal—sits in the place of the experimenter, and puts into

3 At first all four skin spots give no effect, then after a greater interval, the uppermost is active but the other three inactive (inhibited); a little later, the upper two active, and the lower two inhibited; then only the lower of the four inhibited, and at last they are all free.—*Translator.*

action a formerly well elaborated conditioned stimulus of the food reflex. Contrary to expectations this stimulus evokes now an enormous effect, much greater than the usual experimenter was ever able to obtain when he performed his experiments on the quiet animal. The dog secretes more saliva than in any of the preceding experiments and eats the food greedily out of the hand of him whom he has just attacked so ferociously and whom he will again attack after the feeding. How can this be explained?

Before answering this question I shall mention other peculiar facts. The object of the dog's animosity—the newcomer—remains in the seat of the regular experimenter, behaves himself faultlessly, making not the slightest movement, not even the most indifferent, and limits his conduct to repetition of the conditioned stimulus accompanied by feeding the dog. Gradually the animal becomes quieter; he still barks but not so fiercely, and after some time becomes entirely calm, although he never turns his eyes away from the new experimenter. The aggressive reaction has apparently become weaker. A most remarkable state of affairs it is that when the conditioned stimulus begins to act again, there is no drop of saliva, and the dog takes the food only five to ten seconds after it is offered and eats it slowly and apathetically. But if the new experimenter stands and behaves more freely, the aggressive reaction recurs and at the same time the food reflex is reinforced. How are these phenomena to be understood?

From the standpoint of our previously known facts, the mechanism of these strange phenomena is no puzzle. When the most pronounced offensive reflex was manifested, the excitation flowed from a certain point in the cerebral hemispheres over a large area, probably over the whole of the hemispheres, embraced many centres, and among them the food centre. All this resulted in a general, extremely heightened activity of the hemispheres. This is the reason why the food stimulus produced such an extraordinary effect. Supposedly, this is the nervous mechanism of what we subjectively call an "*affect*"; what we have seen in our dog must be designated psychologically as the affective state of *anger*. In the weakening of the external stimuli (the movements of the stranger), the reflex gradually diminishes, and the nervous process contracts and concentrates in a certain part of the cerebral hemispheres. When this concentration has reached a certain degree there results an isolation of the centre of the offensive reflex, and there follows, according to the above-mentioned law of conflict of centres, a decrease in the excitability of all the remaining centres, including that for food. This, I think, is a beautiful illustration of the laws of irradiation and concentration of the excitation and of the interaction of these processes.

In conclusion, I shall present one of the latest facts from our laboratory. We have always developed the conditioned reflexes in the following manner. The new agent, with which we had chosen to develop a conditioned reflex, was brought into action, and five to ten seconds or even later the dog was fed. After several recurrences of these combinations, the agent itself called out the food reaction of the animal, became a conditioned stimulus. But when this method was slightly varied, an unexpected result cropped out. If we began with the feeding and then five to ten seconds afterwards added a new agent, then in spite of innumerable combinations, we failed to develop any reflex.

Whether under such circumstances it will be possible to form a conditioned reflex is a problem for further enquiry. But the extraordinary difficulty of its formation is an indisputable fact. What does this mean? Judging again from our known facts, the answer to this is not intricate. When the dog eats, the food centre is in the state of excitation (for this is a very strong centre), and according to the law of the conflict of centres, all the remaining parts of the cerebral hemispheres are in a state of considerably decreased irritability, wherefore the incoming stimulations will be without effect.

Allow me to take this opportunity to express in a few words how we represent physiologically what we call "*consciousness*" and "*conscious.*" Certainly I will not discuss this question from the philosophical point of view, *i.e.*, I shall not touch on the problem of how the brain substance creates subjective phenomena, etc. I shall only endeavour to answer provisionally what kind of physiological phenomena, what sort of nervous processes, proceed in the hemispheres of the brain when we say we are "conscious" and speak of our "conscious" activity.

From this point of view, *consciousness* appears as a nervous activity of a certain part of the cerebral hemispheres, possessing at the given moment under the present conditions a certain optimal (probably moderate) excitability. At the same time all the remaining parts of the hemispheres are in a state of more or less diminished excitability. In the region of the brain where there is optimal excitability, new conditioned reflexes are easily formed, and differentiation is successfully developed. That area is at the given moment the creative part of the hemispheres. The outlying parts with their decreased irritability are incapable of such performance, and their functions at best concern the previously elaborated reflexes arising in a stereotyped manner in the presence of the corresponding stimuli. The activity of these areas is subjectively described as unconscious, automatic. The area of optimal activity is, of course, not fixed; on the contrary it is perpetually migrating over the whole extent of the hemispheres, being dependent on the relations which exist between the different centres as well as on the influence of external stimuli. The borders of the region of lowered irritability obviously change in conformity with those of the area of excitation.

If we could look through the skull into the brain of a consciously thinking person, and if the place of optimal excitability were luminous, then we should see playing over the cerebral surface, a bright spot with fantastic, waving borders constantly fluctuating in size and form, surrounded by a darkness more or less deep, covering the rest of the hemispheres.

Let us return to the last experiment. If an external stimulus of moderate strength impinges on the brain of the dog, when at the moment there is no definite, sharply circumscribed focus of excitation, this stimulus conditions the appearance in the hemispheres of a region of increased irritability. If afterwards on the same hemispheres a more significant stimulus acts—for example, a stimulus aroused by food stuffs—which creates a new and more energetic focus of excitation, then there arises between these two foci certain connections. The nervous process, as we have seen, is directed from the area of lesser excitation to that of greater excitation. If, however, the process begins with strong stimulation, as, for example, that called out by feeding, the resulting increase of irritability at a certain point in the cerebral hemispheres is so strong and great, the inhibitory process arising in the other parts is correspondingly so intensive, that all impulses falling at the given moment on this point can not open paths or enter into new connections with any activities of the organism.

I do not argue for the unconditional acceptance of the last hypothesis; it should only show how the objective study of the higher parts of the central nervous system gradually reaches into the realm of the most complicated nervous activity, as far as we can judge from the provisional comparison of our subjective states with facts from the physiology of the conditioned reflexes.

I have finished my communication, but I should like to add what seems to me to be of great importance. Exactly half a century ago, in 1863, was published in Russian the article "Reflexes of the Brain," which presented in clear, precise, and charming form the fundamental idea which we have worked out at the present time. What power of creative thought was necessary under the then existing physiological knowledge of nervous activity to give rise to this idea! After the birth of this idea, it grew and ripened, until in our time, it has become an immense force for directing the contemporary investigation of the brain. Allow me at this 50th anniversary of the "Reflexes of the Brain" to invite your attention to the author, Ivan M. Setchenov, the pride of Russian thought and the father of Russian physiology!

CHAPTER XXI

THE STUDY OF THE HIGHER NERVOUS ACTIVITY

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

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

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

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

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

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

The time has come, gentlemen, to add something to this old notion of the reflexes, to admit that, parallel with this elementary function of the nervous system to repeat pre-formed reflexes, there exists another elementary function—the formation of new reflexes. If in machines made by human hand there are present certain conditions by which may arise new and appropriate combinations of the mechanical parts, why should one deny this elementary property to the nervous system—the most perfect of all regulators in the most complicated of all constructions? The reason is not because of lack of scientific facts or of a formula—both of these have been known for a long time—but what fails is the general acceptance and systematic application of this formula in the study of the higher parts of the nervous system. The phenomenon which we consider is perfectly clear: it is the property of living substance to adjust itself, or, as I prefer to say, to come continuously into equilibrium with the external world, *i.e.*, in the interest of the integrity and welfare of the given system of living substance, to enter into connections with new conditions—in other words, to respond to formerly indifferent agents by a definite activity.

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

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

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

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

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

Thus the mechanism of the formation of the new reflexes gives a complete possibility of examining the activity of the analysers. This activity in higher animals is uninterruptedly at work, just as is the process of the formation of new reflexes. In the present imperfect knowledge of this activity we can hardly guess its far-reaching significance in the life of the animal, and what we consider as very complicated processes is only the most delicate and exact analysis. The real need is for a systematic study of the activity of the analyser. First of all, we must state what the analysers of the given animal isolate as units of the external world. By this I mean all the qualities of the stimuli, all their intensities, their limits, and their combinations. Then we must study those basic rules which govern the analysis. Partial destruction of the peripheral or of the cerebral end of the analyser should make us gradually acquainted with the separate details of the analysers; and only from the combined activity of these parts will there finally be evident all the functions of the analysers as they are performed in the animal.

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

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

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

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

A conditioned reflex can be made not only from an indifferent stimulus, but from a stimulus which may be firmly connected with a certain centre, even though it is an inborn connection. A striking instance of this occurs with *destructive* stimuli, or, according to the psychological terminology, *pain* stimuli. Their usual result, their constant reflex is *defence,* the struggle of the muscular system against the stimulating agent, its annihilation. Systematically repeating the feeding of the dog, *viz.*, the excitation of his food centre, combined with an electrical stimulation of the skin, even though it is injured, will lead without great difficulty to an entire cessation of the defence reaction and to its substitution by the food reflex, *i.e.*, the corresponding movements, secretion of saliva, etc. Now you may cut, burn or in any way destroy the skin, but instead of the defence reaction you see only the signs of the food reflex, or, subjectively speaking, of a strong appetite—the dog turns toward the experimenter, makes licking motions, and the saliva flows. This fact has been so often demonstrated in my laboratory that it may be accepted without question. What is its meaning?

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

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

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

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

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

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

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

The elaborated conditioned reflexes, as mentioned previously, are highly sensitive, and therefore under ordinary conditions of life they incessantly fluctuate in degree and even decrease to zero. I cannot but see here a convincing justification of our methods of investigation. As sensitive as the extent of the conditioned reflex is, the experimenter at present still has control over it. The changes in size of the conditioned reflex occurs in both directions. We have made a special and thorough study of the negative (decreasing) variation of the conditioned reflexes, which we consider as phenomena belonging to the generally accepted physiological conception of inhibition. The facts compel us to recognise three separate forms of *inhibition—sleep, external,* and *internal.*[1]

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

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

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

1 See footnote 1, chapter xx.—*Translator.*
2 See previous chapter.—*Translator.*

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

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

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

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

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

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

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

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

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

To conclude this aspect of the conditioned reflexes, I should like to remind you that *time*, as such, proves to be also a stimulus; we may make a conditioned stimulus of it, and investigate it with regard to differentiation, inhibition, and dis-inhibition.[4] I am convinced that directly along this path of exact experimentation lies the solution of the problem of time, which has occupied philosophers for countless generations.

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

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

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

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

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

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

And further. Conditioned reflexes may be formed from stimuli which come from the skeletal muscles, for example, from bending the leg at some definite joint, when the movement reflex has been differentiated from the pure skin stimulus. The ultimate proof that this differentiation can really be obtained is afforded by the extirpation, now of one, now of another, portion of the anterior lobes, and the consequent disappearance of the skin reflex and preservation of the movement reflex in one cases; and in the other case, the loss of the movement reflex and preservation of the skin reflex.

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

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

I will conclude with a fact that seems to me especially instructive for our study. We have before us a dog the front half of whose hemispheres has been removed. All the conditioned reflexes formerly elaborated have disappeared. In all vital respects he is completely helpless, he has lost all normal relations to the external world; he cannot take food which lies near him, he notices neither lifeless objects, men, nor animals; in walking he collides with objects and gets into most uncomfortable situations. And what do you think, gentlemen! In such an animal one can find a path to the discovery of entirely normal and complicated nervous activity. With the salivary glands of this dog may be formed the so-called "water reflex." When a normal dog drinks or has water put into his mouth, there is no flow of saliva or at most only one or two drops. If one has previously put some substance stimulating the salivary secretion, for example, acid solution, into the mouth, then the introduction of water also calls forth an abundant flow of saliva. Apparently the different stimulations which make up the entire act of introducing the liquid into the mouth and which accompany the reflex effect of the acid become a conditioned stimulus of the acid reaction; and as such they are manifest in the introduction of the water. This salivary secretion has all the properties of a conditioned reflex. In the dog which I have described one can promptly form, with the help of the acid reflex, the conditioned reflex to water, having all the general properties of conditioned reflexes. This result was confirmed in another dog in which the frontal half of the hemispheres was removed but the olfactory lobe left intact. This dog was similar in all details to the preceding one, but he could form not only the water reflex but odour reflexes. As the autopsies showed, the posterior parts of both hemispheres in the two dogs were atrophied. Consequently, in the removal of the frontal parts there was a destruction of the conduction paths to the hinder parts. Speaking psychologically our animals became idiots judged by their movements; but judged by the activity of the salivary gland, they were at the same time intelligent.

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

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

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

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

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

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

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

CHAPTER XXII

THE INSTABILITY (LABILITY) OF INTERNAL INHIBITION IN CONDITIONED REFLEXES

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

THE KINDS OF INTERNAL INHIBITION—EXPERIMENT.

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

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

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

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

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

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

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

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

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

CHAPTER XXIII

THE PURE PHYSIOLOGY OF THE BRAIN

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

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

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

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

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

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

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

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

The whole way along which the nervous impulse travels is, as is well known, called the reflex arc or path. In the lower central nervous system there are recognised three parts of this arc: the receptor (a receiving apparatus), conductor (conducting apparatus), and the effector (the apparatus which exercises the special activity). Add to "receptor" the word "analyser" (decomposing), and to "conductor" the word "connector" (linking or coupling apparatus), and you have the expressions for the corresponding anatomical structure for the two fundamental activities which characterise the higher part of the central nervous system.

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

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

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

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

Concerning the activity of the analysers, the first fact to be observed is the following. All phenomena in the beginning link into the conditioned connection as entirely general stimuli, and not until later do they become specialised, *i.e.*, so that only definite phenomena provoke the conditioned response. For example, if you make a conditioned stimulus out of some given tone, then at first not only the tone you have used but also other tones, and even noises, etc., provoke the same reflex. Afterwards, when your given tone has been repeated many times, the number of sounds which act becomes smaller and smaller, and finally only the selected tone produces the conditioned reflex. In this way the limits of the activity of the analyser are marked off, being in some animals capable of an almost inconceivable delicacy of differentiation, and presenting possibilities of wide development. I must omit a mass of particulars relating to these facts.

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

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

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

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

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

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

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

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

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

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

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

Let us continue the experiment still further. After an interval of thirty seconds between the irritation of the two skin points we get not a zero effect but 3 to 5 divisions. At an interval of forty seconds, we have again 15 to 20 divisions; at an interval of fifty seconds, 20 to 25 divisions; and with an interval of sixty seconds we obtain the customary full effect of 30 divisions. During this whole period of sixty seconds, and even longer, the irritation of the site of the primary extinction remains zero. We may choose for comparison of the primary and secondary extinctions any two of our twenty skin points (on which equally strong conditioned reflexes have been elaborated) and we always get the same series of figures, provided the distance between the skin points is the same in the different cases. If the distance between the stimulated points is lessened, the difference consists in this—the decrease of the effect and the zero effect on the secondary extinguished point sets in sooner, the zero effect lasts longer, and the return to the normal effect is delayed. If certain precautions are observed, these experiments proceed with astonishing exactitude, as shown by our results from five dogs used by two workers during the course of a year. The repetition of these experiments yields such stereotyped results that I can say without exaggeration that for some time I could not believe my eyes.

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

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

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

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

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

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

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

Thus you see that the horizon of the strictly objective study of the highest nervous activity continually widens. Why should physiology strive to delve into the hypothetical and fantastic internal world of the animal? During the thirteen years of our investigations I have not once used with success any psychological conception. The physiology of the brain should not for a single moment leave the ground of natural science which every day proves its solidity and productiveness. One may rest assured that along this path, upon which the strict physiology of the brain has set out, astonishing and marvelous discoveries await us, and that there will result such power over the higher nervous functions as is in nowise inferior to the other achievements of natural science.

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

CHAPTER XXIV

SOME FACTS ABOUT THE PHYSIOLOGY OF SLEEP

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

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

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

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

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

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

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

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

TABLE I

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

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

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

Now I shall call your attention to some actual figures. As soon as the dog is prepared on the stand some of our conditioned stimuli are applied, and we obtain a salivary secretion—37 divisions of our scale, which is the normal reaction. A certain precaution has to be observed in order to obtain exact results. The chamber itself has a hypnotising effect on the dog; for as soon as the animal, although wide awake and active, was brought under the spell of the room, he became entirely changed, even before he was put upon the stand; on the stand sleep became, of course, more marked and increased. In order to fix a certain moment for the passage from the waking to the sleeping state, as soon as the dog was put on the stand and preparations for the experiment began, we endeavoured to prevent sleep by calling him, stroking him, slapping him, etc. When everything was prepared we would go quickly from the room and begin the experiment immediately. In this way we were able to obtain the above-mentioned secretory reaction of 37, and also the motor reflex. In the next experiment we allowed the surroundings to act two minutes after preparing the dog and before beginning the conditioned stimulation. When we used our conditioned stimulus the result was zero drops of saliva, but the dog ate at once. Next we let the surroundings act for four minutes; we obtained 20 divisions of saliva but the dog began to eat only forty-five seconds after the application of the conditioned stimulus, and then not unless the food was brought into contact with the mouth. If the surroundings were allowed to act for one-half to one hour, all the reflexes disappeared.

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

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

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

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

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

CHAPTER XXV

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

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

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

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

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

First, it was necessary to determine the external conditions and the stimuli which produced and developed the guarding reflex. This task was not especially laborious. The chief stimuli of the reaction were unmistakable. The prime condition is the closed and isolated room with the customary experimenter. As soon as the animal leaves this room, he changes entirely his behaviour toward strangers and toward his master. There is not a trace of the aggressive reaction; on the contrary the dog is friendly toward strangers. But at the same time he treats his master (the experimenter) with indifference and apathy, and now you may not only approach the master, but may strike him and the dog does not interfere.

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

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

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

Three conditions thus take part in the development of the guarding reaction. When the reaction is still weak the presence of all these conditions is required to make it manifest. If the master leaves the experimental room, there is no aggressive reaction to the stranger although the dog is fastened on the stand. If the dog is released and put on the floor there is no aggressive reaction, although the master is present. If the guarding reaction has already been reinforced by the repetition of all of these three conditions, then only two of them are required to bring it out. However, when the guarding reaction has reached its highest tension, then the sight and voice of the experimenter taken alone are insufficient for the manifestation of the reaction. In another room and out of the stand, the dog no longer guards his master.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

CHAPTER XXVI

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

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

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

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

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

Now I shall proceed with the discussion.

We take some higher animal, for example, the dog. Although he is not at the top of the zoological ladder (the monkey takes that position), he is the closest to man; for there is no other animal which has accompanied man from prehistoric times. I have heard the late Modest Bogdanov, the zoologist, in reviewing prehistoric man and his companions, chiefly the dog, use the following phrase: "Justice compels us to say that it was the dog who helped man to emerge from savagery." Thus he appraises the dog! This animal is, therefore, no ordinary one. Consider the dog —a guard, a hunter, a domestic pet, a servant—and in all this higher manifestation of activity, you see what the Americans are wont to call *behaviour.* If I intend to investigate this higher activity of the dog, *i.e.*, to systematise the phenomena of his life and to discover the laws and conditions under which these phenomena arise, before me looms up the question: how must I find, how choose the way?

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

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

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

Next rises the question, what is the basis of such a fine co-ordination, and what is its mechanism? For this the physiologist—and that is my specialty—has an answer ready. The properties of the food act on the nerve endings, stimulating them. These nervous impulses are conducted into the central nervous system to special points, and there cross over to the fibres leading to the salivary glands. Thus there is evidently a connection between what enters the mouth and the work of the glands. The details of this union are explained as follows: the several nerves from the oral cavity, where the substances act, are separately excited by acid, sweet, rough, soft, hard, hot, cold, etc., and these impulses travel along different nerve fibres to the central nervous system. From there these impulses can reach the salivary glands along different nerves. The one calls out one kind of activity; the others, other kinds. Consequently, different properties of the food stimulate separate nerves, and in the central nervous system there is a transfer to the corresponding nerves calling out each its particular function.

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

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

But how is this to be studied? Taking the dog when he eats rapidly, snatches something in his mouth, chews for a long time, it seems clear that at such a time the animal strongly desires to eat, and so he rushes to the food, seizes it, and falls to eating. He longs to eat. Another time the movements are slower, less avid, and therefore we say the dog does not want so strongly to eat. When he eats, you see the work of the muscles alone, striving in every way to seize the food in the mouth, to chew and to swallow it. From all this we can say that he derives pleasure from it. When on the contrary an inedible substance happens to get into the mouth, and the dog ejects it, spews it out with the tongue, shakes his head, then we involuntarily want to say that this is unpleasant for the animal. Now when we proceeded to explain and analyse this, we readily adopted this trite point of view. We had to deal with the feelings, wishes, conceptions, etc., of our animal. The results were astounding, extraordinary; I and one of my collaborators came to irreconcilable opinions. We could not agree, could not prove to one another which was right. For some decades before, and also afterwards, we could settle all our questions, we were able to decide one way or another, and the dissension ended.[1]

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

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

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

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

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

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

Analysis has shown that the constant form of stimulation of the salivary glands by food at a distance, in the usual case with which we are all familiar, is characterised also by the formation of a new nervous path resulting from setting up connections. Dr. Tsitovitch, in the laboratory of Prof. Vartanov, performed the following interesting experiment. He took a new-born puppy, and for several months fed it only on milk, so that the animal had never experienced any other kind of food. After making a salivary fistula in order to observe the work of the glands, he showed the dog ordinary food, not milk. No kind of food at a distance[2] had any effect on the salivary glands. This means that when different foods act on you from a distance the reaction is a reflex which was first formed when, in childhood, you experienced these foods—saw them and then ate them. The matter is thus: when a piece of meat is placed before a puppy several months old, it has no action whatever on the salivary glands—neither its appearance, nor its odour. It is necessary for the meat to enter the mouth, where it evokes a pure, simple conducting reflex, and only then is there consequently formed a new reflex from the appearance and smell of the meat.

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

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

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

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

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

In the investigation of the analysers, situated in the cerebral hemispheres, very important facts are disclosed. For example, such a one: At first when a new reflex is formed from some sound, as a rule, the new stimulus appears in a general form, *i.e.*, if you have formed a conditioned reflex from a certain tone, say of 1,000 vibrations, and now if you try other tones, for example, of 5,000, 500, 50 vibrations, in the beginning you will get an action from each of them. Always at first the greater part of the analyser enters into the reflex. Only later with the repetition of the reflex does there occur *specialisation*. This is one of the important laws. It is clear that also this fact can be investigated without having recourse to any extraneous conception. It is well here to refer to our investigation of the limits of the analysing faculty. It has been proved, for example, that the analyser of the dog can recognise one-eighth of a tone. The sensitiveness of the auditory apparatus of the dog for tones is comparatively much greater than ours. We distinguish sounds up to 50,000 vibrations, while the apparatus of the dog functions for sounds as high as 100,000. I shall remind you of the following interesting fact. Where there is an injury in the cerebral hemispheres at the site of the corresponding terminals of the visual, auditory, etc., analysers there is of course a break in the apparatus. A dog having a damaged eye analyser, for example, does not recognise his master. But he will avoid colliding with his master just as he would avoid striking against a chair. Regarding this, it has been said that the dog sees but does not understand. It must be admitted, however, that this phrase is difficult to comprehend if we consider it carefully.

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

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

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

In the study of the above kinds of activity it has been proved that the first important property of the higher cerebral mass is a peculiar movement of the nervous processes in this mass. I shall describe it later and in detail. Another exceedingly important property appeared, *viz.,* that if in the higher parts of the cerebral hemispheres, there is an element quite isolated functionally and if upon it beats a certain excitation proceeding from some agent, then before long there inevitably results the *Inactive State,* the state of sleep or of hypnosis. The fundamental property of the higher nervous mass is this extreme reactivity, but if there is temporary isolation, if the excitation instead of following off to the sides concentrates for a time, *i.e.,* if the excitation acts continuously at one point, then this element unfailingly passes over into the sleeping state. Many things are explained from such a relation of the higher nerve cells to the stimuli. This relation may be conceived of either as a sort of preservation of the precious substances of the cerebral hemispheres, of the substances which must constantly respond to all the influences of the outer world; or it may be understood in the biological sense, *i.e.*, that if the stimulus changes every moment, you must react to it by a definite activity, but if it becomes monotonous, then without important consequences you may rest, recuperating for a new expenditure. I shall not enter into details.

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

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

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

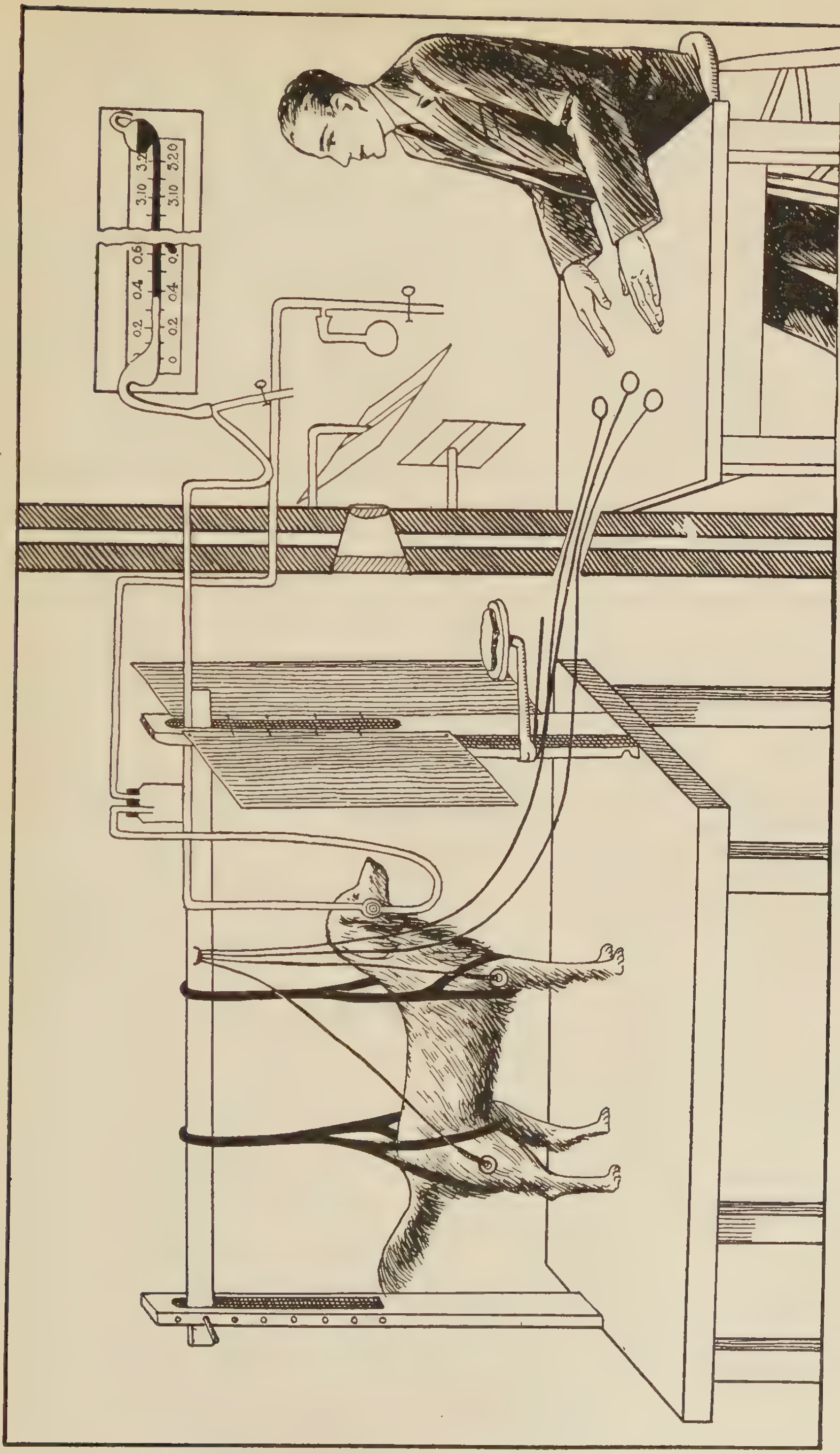

Fig. 7.—Diagram Illustrating Experimentation on the Dog

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

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

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

I shall complicate my experiment as follows. I have a dog in which our reflex has been elaborated at several places, let us say three. After the mechanical stimulation of each of these places there appears the same acid reaction, measured by a definite flow of saliva. This is the simplest way to measure the reaction; the measurement of the motor component would be more difficult. The motor and the salivary reactions go together, they are parallel. They are the components of a single complicated reflex. Now we have several skin reflexes formed. They are all equal, they act with absolute exactness, they give the same number of divisions of the tube used to measure the salivary secretion, for example, 30 divisions for one half-minute stimulation. I stimulate the place on the front leg in the way I have just said, *i.e.,* I do not combine it with the influence of the acid, and so after about five or six times the mechanical irritation does not show any action. To the physiologist this means that I have obtained a complete inhibition of the reflex. When this has happened to the place on the front leg, I can stimulate another spot on the hind leg. And there developed such phenomena. If now I take the mechanical stimulation on the thigh—just as I did on the front leg, where I got zero—so that there is no interval between the end of that stimulation and the beginning of this, then at the new place I obtain a full action, 30 divisions on our tube, and the dog behaves as if this were the first application of the stimulus. Saliva flows freely, the motor reaction occurs, the dog acting as if he were rejecting acid from the mouth with the tongue, although there is no acid present—in short, the whole reaction appears. If in the next experiment I try the effect of the irritation on the front leg until again there is no secretion (by repeating the mechanical stimulation without giving acid), and then irritate the place on the hind leg, not after zero seconds but after five seconds, then I receive not 30 divisions from the new place but only 20. The reflex has become weaker. The next time I use an interval of fifteen seconds, and I get a slight action from the new place,—5 divisions. Finally if I stimulate after twenty seconds there is no action whatever. If I go further and employ a great interval, thirty seconds, then again I get an action from this place. With an interval of about fifty seconds, there is considerable secretion, 25 divisions, and with an interval of sixty seconds we see the full reaction. On the same place, on the shoulder, after we obtained zero result, if the irritation is repeated with an interval of five, ten, fifteen minutes, then we get zero (I do not know if I have made this clear to you). What does this mean?

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

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

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

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

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

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

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

CHAPTER XXVII

THE REFLEX OF PURPOSE

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

When the negative features of the Russian character—laziness, lack of enterprise, and even slovenly relations to every vital work—provoke melancholy moods, I say to myself, No, these are not our real qualities, they are only the veneering, the damning inheritance of slavery. It made a parasite of the master, freeing him, through the unpaid work of others, from the practice of natural and normal striving to obtain his daily bread for himself and family, from the necessity of making his way in life; and it left the reflex of purpose without exercise in the fundamental habits of living. Of the slave it made a completely passive creature, without any vital perspective; for continually in the way of his most natural aspirations arose an insurmountable obstacle in the form of the powerful egoism and caprice of his master. But I venture further: A spoiled appetite and poor nutrition may be *restored* by a careful régime and special hygiene. The same can and should happen with the reflex of purpose which has been suppressed during Russia's history. If every one of us will cherish within himself this reflex, as the most precious part of his being, if parents and instructors of all ranks will make their chief problem the strengthening and developing of this reflex in the plastic masses, if our society and state will provide a full opportunity for the exercise of this reflex, then we shall become that which we should and can be, judging from many episodes of our history and from some strokes of our creative strength.[2]

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

CHAPTER XXVIII

THE REFLEX OF FREEDOM

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

We again insist on the necessity of describing and enumerating the elementary inborn reflexes, in order gradually to understand the whole conduct of the animal. Without such a classification we have only the usual empty conceptions and words: "the animal forms and breaks habits, remembers, forgets, etc.," but we never arrive at a scientific study of the complex activity of life. There is no doubt that a systematic study of the fund of inborn reactions of the animal will greatly favour an understanding of ourselves and the development in us of the ability of self-guidance. It is clear that together with the reflex of freedom there exists also an inborn reflex of slavish submission. It is a well known fact that puppies and small dogs often fall on their backs in the presence of larger ones. This is the surrendering of self to the wishes of the strong, the analogy of the man's falling on his knees, or prone on his face—the reflex of slavery. This has, of course, a use in life. The intentional passive attitude of the weak leads to a natural decrease of the aggressive action of the strong, while even an ineffective resistance tends to increase the destructive ambition of the strong. How often and varied appears the reflex of slavery on Russian soil, and how useful to recognise it! Let us take an example from literature. In Kuprin's story, *River of Life,* there is described the suicide of a student who was tormented by his conscience after having betrayed his companions to the police. From a letter of the suicide it was evident that he was made a victim of the reflex of slavery inherited from his mother, who was a *prijivalka.*[2] If he had had an insight into his condition, he would first have understood his limitation, and secondly he might by systematic measures have developed control and successful suppression of this reflex.

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

CHAPTER XXIX

HOW PSYCHIATRY MAY HELP US TO UNDERSTAND THE PHYSIOLOGY OF THE CEREBRAL HEMISPHERES[1]

(Read before the Society of Psychiatry, Petrograd, and published in the *Russian Physiological Journal*, 1919.)

PAVLOV STUDIES TWO INSANE PATIENTS FROM THE PHYSIOLOGICAL OBJECTIVE POINT OF VIEW—BOTH ARE CASES OF CATALEPSY WHICH PAVLOV ANALYSES AS INHIBITION OF THE MOTOR REGION OF THE BRAIN—DISAPPEARANCE OF THE CHRONIC INHIBITION IN SENILITY.

My earlier researches on the circulation of the blood and on digestion convinced me that the physiological mode of thinking may derive great help from the study of clinical cases, *i.e.*, from the unlimited number of 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 auxiliary to this study. The usual physiological method, which, as a mode of analysis, consists in destroying parts of the brain, is very crude in comparison with the delicacy of the mechanism to be investigated. In brain diseases one might expect in some cases to come across a much more obvious, permanent, and detailed decomposition of the elements participating in the complete activity of the brain, and a separation of its single functions in consequence of pathological causes, the effect of which sometimes reaches a high degree of differentiation.

In the summer of 1918 I had at last an opportunity of studying * a number of cases of insanity. My former hopes were realised. In some instances, I found excellent examples of facts concerning points more or less explained in physiology; in others, new details of the action of the brain were brought to light, new questions and problems for laboratory investigation arose.

My way of looking at the psychiatric material was however greatly different from the usual point of view adopted by specialists. Owing

[1] Being deprived, in the summer of 1918, of the possibility of taking his usual two months' vacation in the country, Pavlov rested by changing his work. He went to the Hospital for Mental Diseases to observe personally some clinical cases. This article is a discussion of the two principal cases that came under his personal observation.—*Translator.*

* I am greatly indebted to Dr. M. K. Voskresensky, director of the asylum for insane in Udelnaya, for admission to the asylum, and to Dr. V. P. Golovina, who spent a great deal of time in showing me the patients.

to many previous years of experience in the laboratory I reasoned on a purely physiological basis. As I always tried to explain to myself in physiological conceptions and terms, the psychic activity of lunatics, I did not experience great difficulty if I concentrated not on the details of the subjective state, but on the principal features and phenomena of the pathological state of the insane. How this was done will be seen in part from the following account.

I shall give in this paper a description and analysis of two cases. The first was an educated, well-bred girl, age about twenty-two. We find her in bed in the garden of the hospital lying motionless with her eyes half closed. At our approach she does not speak. The physician who accompanied me, told me that this was her usual behaviour. She refuses to eat without help, and is untidy. When questioned about her relatives and home, she appears to understand and remember everything quite well. She replies correctly but with extreme effort, and answers slowly. The cataleptic state of the patient is marked. For some years, periodically, she has been either nearly recovering, or falling ill again with various symptom-complexes. Her present state represents one of these complexes.

The second case was a man aged sixty. He has spent twenty-two years of his life in the hospital, lying like a living corpse without making the least voluntary motion, without pronouncing a single word; he is very untidy, having to be fed through a tube. During the last few years, as he approached sixty, he began more and more to make voluntary movements. At present he gets out of bed without help, goes to the lavatory, talks freely and quite reasonably, and eats without assistance. Referring to his former state, he declares that he understood everything around him, but experienced such an extreme and insuperable heaviness in his muscles that he could hardly breathe. This was the reason why he neither moved, ate, nor spoke. He suffered the first attack of the disease at the age of about thirty-five. The history of the case records tonic reflexes.

How are the described conditions of these two cases to be characterised from the physiological point of view?

In order to answer this question let us consider one prominent motor symptom, occurring in both cases. I refer to the catalepsy of the first patient and to the tonic reflexes of the second. When do these symptoms manifest themselves in the most striking manner in animals? Schiff observed long ago that cataleptic phenomena appear in rabbits after the removal of the cerebral hemispheres. Decerebration, introduced by Sherrington, is also a simple method by which there may be obtained tonic reflexes in cats. Poisoning by some anæsthetics, as urethane, also produces cataleptic phenomena. In all these cases there is an elimination of the activity of the cerebral hemispheres *without suppression of the lower parts of the brain.* The last circumstance is due either, as in the case of rabbits and cats, to a specific property of the cerebral tissue of these animals and to the absence of further reactive phenomena, because of the recentness of the operation; or, as in the case of poisoning by urethane, to the presence in the latter of the ammonium radical, which stimulates the lower motor centres. Such an isolated elimination of the cerebral hemispheres, the central organs of so-called voluntary motions, leads to a manifestation of the normal activity of the lower parts of the nervous apparatus for motion. This activity is first of all designed for equilibrating the organism and its parts in space, and represents the reflex of equilibration, which in normal conditions always functions, but is always masked at the same time by voluntary motions. Catalepsy is thus a normal and habitual reflex which in the above-mentioned conditions manifests itself distinctly by virtue only of the inhibition of the action of the cerebral hemispheres. The tonic reflexes are the elements of that compound reflex (plastic tonus of Sherrington, and labyrinthine and neck reflexes of Magnus).

In these patients the presence of the same kind of mechanism may be supposed to exist, *i.e.*, exclusion of the activity of the cerebral hemispheres. But, as seen, they are characterised by the exclusion of the activity of only the motor region of the cerebral hemispheres. In fact, our patients are not able to make any voluntary motions, or at least they suffer an extreme impairment of this function. This is clear to the observer, and is even stated by the patients themselves. But at the same time they understand what they are told, remember everything, and are conscious of their state, *i.e.,* they work quite satisfactorily with the other parts of the cerebral hemispheres.

A strictly limited suppression of the motor area of the cortex of the cerebral hemispheres is known also in other human or animal conditions. A person in a certain state of hypnosis understands perfectly well all that he is told, remembers it, and willingly executes commands, but he has no voluntary power over his skeletal muscles, and is compelled to retain the pose assigned him, even though it is uncomfortable and undesirable for him. The essential feature of this condition obviously lies in an isolated suppression of the motor area of the cortex of the cerebral hemispheres, a suppression which does not extend either over the whole hemispheres, or deeper into the mass of the brain. Working in the laboratory with conditioned reflexes I have observed a similar state in dogs. In one of our cases, in collaboration with Dr. Voskresensky,[2] we studied the conditions most accurately and systematically. For weeks and months the dog was often left for a long while alone in

2 See chapter xxiv.—*Translator.*

the room, in a wooden frame and without experimental influences. In consequence of such a proceeding the whole environment of the room became for the dog a soporific agent to such a degree that it was enough to bring him into the room to have all his behaviour changed immediately. By varying the duration of the action of this agent we could see the separate phases in the development of drowsiness and sleep. The following results were obtained. A conditioned reflex for sound and food (association) was formed, that is, at the production of a definite sound the dog exhibited the phenomena of the feeding reaction: he secreted saliva and made appropriate movements, licking his lips, turning his head toward the place where he was usually fed, and immediately starting to eat as soon as food was offered.

At the first signs of drowsiness the conditioned salivary reflex to the sound disappeared, but the motor reflex to the sight of food remained normal, *i.e.,* the dog started to eat the proffered food without the slightest delay. This first phase was followed by the second, which was quite unexpected and very significant. The conditioned salivary reflex to the sound was again present, and became stronger with the addition of the natural conditioned reflex to the food itself, but the motor reflex was absent—the dog did not take the food, and even turned away from it and resisted its introduction by force. In the following phase a deep sleep ensued, and all the feeding reactions, of course, disappeared. When the animal was intentionally awakened (by means of some strong stimulus) the phases described above appeared in reversed order as the drowsiness disappeared. The second phase could be explained thus: the sleep inhibition was already present in the motor area of the cortex, but the remaining portions of the hemispheres still functioned normally and exhibited their activity on an organ quite independent of the motor region—on the salivary gland. There is here a complete analogy to an awakened person who understands (and admits), that you are rousing him by his own request, but who, unable to overcome the power of sleep, begs you to leave him alone; or gets angry and even opens hostilities against you, if you persist in fulfilling his earlier request and continue to trouble his sleep.

The first phase and its substitution by the second as the sleep grows deeper can be explained in the following way. Since in our dog all the interior of the room, *i.e.,* all the stimuli going to the eyes, ears and nose, was acting as a soporific agent, the corresponding regions of the cerebral hemispheres were subjected to the inhibition of sleep, which, though superficial, was strong enough for the suppression of the conditioned action of the stimuli. At the same time the soporific influence was not sufficient to inhibit the predominant region—the motor cortex. But when the monotonous skin and motor stimuli, resulting from the limitation of the motion in the frame, were added to the soporific action of the room, then the sleep inhibition spread over the motor portion of the brain. Now this portion being the strongest, attracted to it the sleep inhibition from all the other regions according to the law of concentration of the nervous process, thus releasing them temporarily from inhibition, until with the development of the action of other soporific agents, the inhibition invaded with an equal and sufficient intensity all the portions of the cerebral hemispheres.

In the patients described above we have enough evidence to affirm the existence of a concentrated and isolated inhibition of the motor cortex of the cerebrum, as a result of the cause which brought about the disease.

What objections from the clinical point of view may be raised against our explanation of the symptoms in these two cases? Here I shall give the apparent inconsistencies with clinical reasoning, which were pointed out by the psychiatrists when I reported to them these results of our analysis. Some of them saw in the cases cited by us a stupor-like state as a consequence of emotion. But first, this concerns not the mechanism but only the cause of the symptoms. Apparently instances of stuporousness, or of a kind of cataleptic state, may occur under the influence of strong, unusual agitation, brought about by extraordinary sounds, by strange pictures, etc. A strong irritation of certain regions of the hemispheres may bring about inhibition of the motor cortex, and thus create conditions favourable for a manifestation of the equilibrating reflex. Secondly, there are not indications in the patients of such a mechanism—the presence of extraordinary stimuli could not be detected, while one of the patients plainly refers only to the difficulty, indeed the impossibility, of voluntary motions.

Further, it was pointed out that destruction of the cerebral hemispheres in progressive paralysis is proved on pathological anatomical grounds, although catalepsy is absent. But even then there is no complete exclusion of the motor activity of the hemispheres. The patients make many voluntary motions though badly co-ordinated; and on the other hand, in the form of convulsions they often show phenomena of an abnormal motor irritation of the cortex. Therefore in progressive paralysis the chief condition for the development of the pure equilibrating reflex is lacking.

It was also pointed out that thromboses and extravasations in the cerebral hemispheres are followed by paralysis and not by catalepsy. But the conditions necessary for the production of catalepsy are absent. In these cases one observes the disappearance of even spinal reflexes. The inhibition, resulting from the destruction, spreads and reaches even the spinal cord. Inhibition should manifest itself to an even greater degree in the regions of the brain nearest to the cerebral hemispheres.

Thus in the clinical diseases of the cerebral hemispheres one does not meet with facts inconsistent with our analysis of the pathological condition of the patients. Therefore, in certain cases, one is forced to acknowledge the existence of the mechanism of pathological functioning of the cerebral hemispheres as here proposed. In the second of the cases described above the following fact also leads one to consider the symptoms as an inhibition of the motor cortex. After more than twenty years of illness the patient has begun to return to his normal state. It means that all the time his condition was of functional rather than of organic nature.

Proceeding further in the analysis of the states of the two patients it is necessary to call attention to yet another circumstance. Although the motor elements of the cortex corresponding to different movements (for instance, of the skeleton, eye-ball, organs of speech, etc.), are localised, according to physiology, in different portions of the hemispheres, and are scattered, so to speak, nevertheless in both the patients these elements are all united by a common inhibitory process, in strong contrast to the other elements of the hemispheres, which remain at the same time more or less free. This leads to the important conclusion, that *all the motor elements resemble one another* in structure or chemical constitution, or, most probably, in both. This is why the motor elements all respond in a similar way to the cause producing the symptoms of the illness, thus differing from the other elements of the cortex, such as those of sight, hearing, etc. This difference between the elements of the cortex appears most sharply in the phases of hypnosis and sleep, when some elements are in one state, and others in another,* though the actual cause is the same.

Let us now answer the question, What, in effect, is the determining cause of the given symptoms? One may think of several things. There may be a definite toxic action, whose sphere of influence is limited by the individual peculiarities of the separate cortical elements. One may also suppose that there is present an exhaustion of the elements of the cortex, resulting either from the general condition of the organism, or from overfatigue of the brain. The exhaustion might be concentrated in some definite elements of the cerebrum either on account of

* This difference between the cellular elements of the cerebral cortex should be considered unquestionable and indisputable. In the physiology of the peripheral nerves, we constantly meet with a distinct individuality (in irritability, relative strength, etc.) of the nerve fibres and of their peripheral ends of different functions. This individuality affords us a fundamental means of differentiating these different fibres in a mixed anatomic trunk, as, for example, the method of separating vasoconstrictor from vasodilator fibres.

the particular part which these elements have taken in the work producing the exhaustion, or as the result of their specific nature. Finally there is the possibility of direct or indirect (the last resulting from local changes in the blood circulation or in the general nutrition) reflex influences which may affect injuriously the different elements of the cortex. Therefore, in different cases, in spite of the similarity of the mechanism of the given complex of symptoms, the causes producing them may not be the same.

It will not be altogether amiss to put one more question: What is the explanation of the case of the second patient, in whom the inhibition of the motor region of the cortex was at last disappearing after having remained during twenty-two years nearly on the same unchanging level. This may depend only on the age of the patient. He was returning to a normal condition at the approach of the age of sixty, at which age a sharp decline in the activity of the organism is usually pronounced. How is this relation to be interpreted? If in this case, some toxic agent had been active, then one might think of a quantitative diminution of the agent which had produced the condition, a lessening of its action as the result of the senile changes in the body metabolism. If the principal cause of the disease was a chronic exhaustion of the nerve mass, then in the presence of changes in the brain accompanying old age and manifesting themselves in restricted activity and less functional disturbance of the brain (the sharp weakening of the memory for current events), the exhaustion would be less pronounced. If it is granted that sleep and hypnosis are a kind of special inhibition, then the second patient would present an example of chronic partial sleep or hypnosis. In senile talkativeness, eccentricity, and in extreme cases imbecility, one sees a more considerable decline of the processes of inhibition. From this point of view I take it that the recovery of the patient was due in reality to the senile decline of the inhibitory processes.

The physiological analysis of the above cases suggests, I think, many new problems for the laboratory investigation of the physiology of the brain.

CHAPTER XXX

HYPNOTISM IN ANIMALS

(Read before the Russian Academy of Sciences, November 9, 1921.)

MODE OF PRODUCTION OF HYPNOTISM—AN ANALYSIS SHOWS IT TO BE MOTOR INHIBITION THROUGH FEAR, A SELF-GUARDING INHIBITORY REFLEX.

THE so-called hypnotism of animals (the *experimentum mirabile* of Kircher) is produced by some energetic influence which suppresses every resistance; for example, the animal is brought into some unusual position (laid upon the back), and thus held for some time, etc. Then when the restriction is removed, the animal remains motionless for many minutes or hours. Different authors, noticing now one, now another, detail of the phenomenon, have given varying explanations. Thanks to the systematic study of the normal activity of the brain which has been conducted in my laboratory I can at present indicate the biological significance of the phenomenon and exactly explain its physiological mechanism, thus combining all the separate facts of the several investigators.

This reaction represents a self-guarding reflex of an inhibitory character. In the presence of the overwhelming power which the animal meets, there is no escape in struggle or in flight, and the only chance to save himself consists in remaining immobile in order not to be noticed (for moving objects attract especial attention), and in order not to provoke in this annihilating agent an aggressive reaction. The fixed position is brought about in the following manner. Extraordinary, extremely intensive (or unusual), external stimuli quickly bring about a reflex inhibition of all the motor region of the cortex, which governs the so-called voluntary movements. This inhibition is either limited only to the motor region, not passing into other areas of the cerebral hemispheres or to the mid-brain, or, on the other hand, it may irradiate over all these parts, depending upon the duration and strength of the stimulus. In the case of limitation of the inhibition to the motor region, there are present reflexes from the eye muscles (the animal follows with his eyes the movements of the experimenter), from the glands (saliva begins to flow when food is given, although no skeletal movements are made in the direction of the food), and finally also tonic reflexes from the mid-brain to the skeletal musculature for the retention of that position into which the animal has been put (catalepsy). In the second event, with wide spreading of the inhibition, all the above-mentioned reflexes gradually disappear, and the animal passes into a completely passive state, the state of sleep, with general relaxation of the musculature. The described course of events confirms the conclusions to which I came in my previous laboratory work, *viz.*, that the so-called inhibition is nothing more than sleep, but partial and localised. Our rigidity and stupor in the face of great fear is nothing else than the reflex which I have just described.*

* I must add that as I could get no physiological literature until 1922, when I obtained it in Helsingfors, this lecture was written in ignorance of the fact that concerning hypnosis in animals some authors had come to this same conclusion.

CHAPTER XXXI

THE NORMAL ACTIVITY AND GENERAL CONSTITUTION OF THE CEREBRAL HEMISPHERES

(Read before the Society of Physicians of Finland, Helsingfors, April, 1922.)

THE SIX PRINCIPAL NERVOUS PHENOMENA—REFLEXES, INSTINCTS, ASSOCIATIONS—ROLE OF THE CONDITIONED REFLEX IN ESTABLISHING AN EQUILIBRATION—EXTINCTION, RETARDATION, CONDITIONED INHIBITION, DIFFERENTATION ARE FORMS OF INTERNAL INHIBITION—EXTERNAL INHIBITION IS ANALOGOUS TO THE INHIBITION OF SPINAL REFLEXES—DREAMS—THE WAKING STATE AND SLEEP—THE MOTOR AREA OF THE CORTEX IS ALSO A SENSORY (RECEPTOR) REGION—LOCALISATION IN THE CORTEX STUDIED BY SURGICAL OPERATION AND CONDITIONED REFLEXES—EACH SENSE ORGAN HAS A CENTRAL CORTICAL AREA AND A DIFFUSE AREA—POST-OPERATIVE COMPENSATION—CONTRASTING EFFECTS OF THE ANIMAL'S BEHAVIOUR AFTER REMOVAL OF (a) FRONTAL LOBES (b) POSTERIOR LOBES—SLEEP INHIBITION AND FATIGUE.

In order to analyse successfully the function of any organ it is necessary to know first of all its normal activity. This holds true also for the cerebral cortex. For the last twenty years I and my co-workers have busied ourselves mainly with this problem, using the dog as an experimental animal.

All the nervous activity and all the behaviour of the higher animals may be included in a scheme containing six principal nervous phenomena: (1) excitation; (2) inhibition; (3) the wandering or spreading of both excitation and inhibition; (4) reciprocal induction—inhibition by the excitation process (negative phase), and excitation by the inhibition process (positive phase); (5) the opening and closing of paths between different points of the system, and finally; (6) analysis—the decomposition by the organism of the external *milieu* and its own internal world (everything which proceeds within it) into their units. Here I can give only a brief outline of this activity, in more or less dogmatic form, and then a brief discussion of the general functional construction of the cortex, with a description of some of our experiments.

The chief fund of nervous activity consists of a mass of *reflexes*—constant, inborn connections of internal or external stimuli with certain activities of the executive organs. *Instincts,* as detailed analysis reveals, are the same as reflexes, but in their composition they are more complicated. The complete register, the detailed description, and the natural systematisation of all these complex reflexes is the next important task of the physiology of the central nervous system.

The next highest step of nervous activity is occupied by the so-called *associations* or *habits, i.e.,* connections formed during the life of the individual owing to the coupling or combining function of the cortex of the cerebrum. The formation of associations proceeds on the principle of signalling. When some indifferent stimulus accompanies once or several times an inborn definite reflex, then this indifferent stimulus acting alone later has the power of calling out that reflex with which it coincided. In the presence of definite conditions the associations are formed regularly and inevitably. Thus we have the right to consider the associations as pure reflexes, though acquired, and to investigate them exclusively from the physiological point of view. I and my collaborators call both sorts of nervous activity reflexes, and designate the inborn as *unconditioned*, and the newly formed ones as *conditioned*, and the corresponding stimuli which provoke them as, respectively, unconditioned and conditioned.

Obviously conditioned reflexes favour enormously the safety and welfare of the organism. Thanks to these temporary connections, diverse and complex agents become conditioned stimuli, which call out the conditioned reflexes. Centres become related functionally and synthesis of stimuli occurs. Probably the site of this coupling, combining activity is to be sought for in the points of union, the synapses of the neurones, especially in the cortex of the cerebrum; for after removal of this part of the brain all conditioned reflexes are destroyed and new ones can not be formed.

A further stage of nervous activity is characterised by an incessant correction of the conditioned reflexes. If a conditioned reflex does not correspond to reality—if under certain conditions the conditioned stimulus is not followed by the unconditioned stimulus, or is followed but not immediately—then the conditioned reflex is temporarily or constantly (in the case of constant circumstances) inhibited. The following example will explain these relations.

According to the described procedure we establish a conditioned reflex from an indifferent tone, *i.e.*, we make it the conditioned stimulus for the food reflex, one of the most important of all unconditioned reflexes. This means that this tone gives the same reaction as the food itself. The animal produces the corresponding movements and secretions (action of the gastric and salivary glands). This reaction can be most simply and exactly measured by the amount of the salivary secretion. Now, I apply the conditioned stimulus (tone) and get the usual salivary secretion, though I do not allow the dog to eat. In the further application of this stimulus after a pause of several minutes the effect is decreased, and if I repeat it, I finally get a zero effect. This is inhibition. The inhibition after a certain time vanishes of itself without our acting in any way on the animal. We call this *extinction* of the conditioned reflex.

Another case: that of the formation of a conditioned reflex which almost coincides with the unconditioned, *i.e.*, the unconditioned stimulus (in our laboratory, usually feeding) is applied very soon (three to five seconds) after the beginning of the conditioned stimulus. Under these conditions when we apply the conditioned stimulus alone, it begins quickly to act. Now let us alter the arrangement of the experiment; let us give the dog food only three minutes after our conditioned stimulus has begun to act, instead of after three to five seconds. Then the effect of the conditioned reflex is soon entirely lost, but it will reappear with this difference: its effect is manifest only in the second or third minute after the beginning of the conditioned stimulus. Thus only the terminal part of the conditioned stimulus is effective, but not the initial part. This is what we have called the *retardation* of the conditioned reflex, and, to all appearances this, too, is an inhibition.

In the next case we combine our conditioned stimulus (tone) with another stimulus (mechanical irritation of the skin) without accompanying the combination by feeding. Thereby the conditioned stimulus gradually loses its effect in the combination. This is also an inhibition, and we call it *conditioned inhibition.*

Now the last case: we have made a conditioned stimulus from the mechanical irritation of a certain place on the skin. At first after this conditioned reflex is established other points of the skin also show the same effect when they are stimulated, and the closer they lie to the first point, the stronger their effect. This spontaneous generalisation of the stimulus has a special biological significance, and is the expression of the irradiation of the excitation in the mass of the cortex. By repeating the stimulation of our chosen point in the skin and accompanying it with feeding, and not accompanying the stimulation of the other points by feeding, these latter become inactive; now they are differentiated, negative, conditioned stimuli. This kind of inhibition we call *differential inhibition.*

This form of inhibition gives us the ability to analyse, which is the most delicate relation of the organism to the elements of the external and internal worlds. The original basis for this analysis is given by the peripheral apparatus of the various centripetal nerves. These are transformers, by every one of which a definite form of energy is changed into a nervous process. Through isolated nerve fibres the nervous process is conducted to certain points of the central nervous system, and from here it is either sent out directly again through isolated paths to the periphery, where it produces some definite activity of the organism (for example, the investigating, orienting or focusing reflex, to use our terminology); or, as we have already shown, if it is more or less irradiated, it only gradually attains again a high degree of isolation by means of the differential inhibition.

Differential inhibition fulfils a still more complicated task; it forms the foundation of the differentiation, delimitation, or separation of the compound stimuli which have been previously extended in the cortex of the cerebrum by means of the coupling activity.

All the above cases of inhibition we put into one group, to which we give the name of *internal inhibition*. The process at first spreads over the cortex, and then again gradually contracts, as does the excitation process—irradiation and concentration. Concentration of the excitation as well as of the inhibition is effected and especially reinforced by *reciprocal induction*, which confines both the excitation and the inhibition processes within strict limits of time and space.

Now, after a long period of collecting facts, of doubting and testing our hypotheses, we have arrived at the conclusion that internal inhibition and sleep are in reality one and the same process.[1] In the first case this process is strictly localised and, as it were, partitioned, whereas in sleep it is continuous and widespread. Owing to the lack of time I regret that I can not enter into the details of this important theme. I shall point out, however, a fact of great significance. A more or less long-lasting stimulation, regardless of its vital meaning for the life of the animal, falling on a certain point of the cerebral cortex, if it is not accompanied by simultaneous stimulation of other cortical points, earlier or later inevitably leads to the inhibition of that point and then to a generalised inhibition and sleep.

Besides internal inhibition there is another kind which does not develop gradually as does internal inhibition, but acts immediately on the conditioned reflexes, weakening or suppressing them—*external inhibition*. It is called out by every new activity of the cortex which produces an automatic or reflex stimulation. It is completely analogous to the inhibitory phenomena recognised long before for the lower parts of the central nervous system. We are now investigating the relations between internal and external inhibition. It is probable that they are both parts of one and the same process.[2]

Thus we see that the cerebral hemispheres are an organ of extraordinary complexity, hardly comprehensible in all its details. Together with the spread of the excitation or inhibition processes arising in consequence now of strong stimuli, now of the establishing of new relations (corresponding to new combinations in the external or internal

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[1] For a more detailed discussion of this question, see the following chapters.—*Translator.*

[2] The suggestion that all kinds of inhibition are based on the same process is met with for the first time at this place, but since then Pavlov has brought out many facts to strengthen this view.—*Translator.*

milieu) during the active waking state, there are in this organ constantly more or less formidable boundaries made up of innumerable, closely intermingled, and interchangeable (from a state of excitation to one of inhibition) points. These boundaries can quickly be effaced temporarily under the influence of the excitation or inhibition resulting from strong stimuli of a positive or depressive character. On the other hand, the boundaries disappear periodically, but only temporarily, when general and diffuse inhibition (sleep) sets in. Wherefore arises the marked discrepancy between reality and dreams, the traces of former stimulations, which now combine in the most unexpected manner.

The *waking state* is maintained by means of stimulations falling upon the cerebral hemispheres chiefly from the outside world, intermingling, and more or less quickly alternating; but also by means of that movement of the excitation in the brain mass occurring as a result of the existing connections between the traces of earlier innumerable stimulations, as well as of the establishing of new connections between the present and the old stimulations. *Normal periodic sleep* sets in as a consequence of a more and more preponderant inhibitory state in the hemispheres which is related to a growing exhaustion of the organ in its entire mass proceeding during the working day. It should be added that as Verworn in his theory of inhibition as a fatigue phenomenon brought out a number of facts showing the relation of the two states, in the same manner we in our conclusions regarding inhibition as a sleep, have met with many cases in which inhibition coincided with exhaustion.

With such a formula for the activity of the cerebral cortex, there opens before the physiological investigator an unbounded horizon, and there appear an endless series of questions which can be solved by purely physiological methods.

Now I come to the *general constitution* of the *cerebral hemispheres.*

First, how shall we explain the motor region of the cortex? Has it receptive or executive functions? We endeavoured to determine this in the following way. Conditioned reflexes were formed from the flexion of the leg of a certain joint, and also from the mechanical stimulation of the skin in the corresponding region. Then in one of the dogs the gyrus sigmoideus (motor region) was removed, and in another the gyri coronarius and ectosylvius (skin area, according to our experiments). The first animal retained the conditioned reflex from the skin stimulation, and lost the conditioned reflex from the flexion (motor act). On the contrary, in the second dog, the reflex to the skin stimulus was lost and that from the flexion was preserved. Wherefore we conclude from these and also from experiments of other investigators, that the *motor region,* like the eye and ear areas, has a *receptor* function, and that the motor effect from the stimulation of the cortex is in reality of reflex character. Thus the uniformity of the whole cortical region of the brain may be considered as established. According to this view the cortex is only a receptor apparatus, which in various ways analyses and synthesises the incoming stimulations. These stimulations reach the purely effective apparatus by means of descending connecting fibres.

The next important question confronting us is that of *localisation* in the cortex. On the basis of Munk's experiments it became evident that the projection of the retina lay in the cortex of the occipital lobe. Not long ago this was confirmed by Minkovsky in the laboratory of Monakov. We also have seen this in many dogs. Munk showed that stimulations from the ear bore a corresponding relation to the temporal region of the cortex. On the other hand, the Luciani school has for a long time advocated a more expansive area for the location of these centres.

At the present time Kalischer, using the method of conditioned reflexes, or as he calls it, the training method, shows that these reflexes from the eye and the ear can be obtained after removal of the visual and auditory areas. The clinicians possess a mass of material which also can not be reconciled with the theory of narrowly limited centres. We have endeavoured to elucidate this indefinite situation by the following experiments. The conditioned stimuli which we used consisted either of elementary or of various complex stimuli. For the ear experiments we employed at one time a series of four successive ascending tones, and another time a chord of two extreme tones and one middle tone from the third octave. The first stimulus of the ascending tones was easily differentiated from the same tones in descending order by the normal animal. After the formation of the conditioned reflex to the chord, its separate tones called out the same reflex but the reaction was weaker. Now in the animal having the reflex to the chord, one-half of the auditory area of Munk was removed. After this operation one of the extreme tones of the chord lost its effect when used alone, although a conditioned reflex could be formed anew on this separate tone. In the dog with the conditioned reflex to the series of tones, the posterior half of the cerebrum, all the part behind the gyrus sigmoideus at the level of the apex of the fissura fossæ sylvii and behind this fissure, was extirpated. Now it was impossible to differentiate this series of tones used in ascending order from the same series used in descending order. The separate tones of the series, however, employed as conditioned stimuli, could be easily differentiated by this animal.

In the experiments in which we used optical stimuli, after removal of the hinder part of the hemispheres along the line described in the preceding paragraph, the animal could differentiate not only various degrees of general illumination, but also equally lighted figures of various shapes, such as squares from circles. Complicated pictures, however, could not be distinguished one from the other. Obviously in this category belong the facts long since recognised: that after extirpation of the temporal and occipital regions of the hemispheres, the dogs lose forever their conditional reactions to objects and words, as well as to complicated auditory and visual stimuli.

From all these facts we conclude that each *peripheral receiving apparatus* (the "sense" organ) has in the cortex a *special central territory,* its own *terminus,* which represents its exact *projection in the brain.* Here—thanks to the special construction of this area (probably the more condensed distribution of cells, the more numerous connections between them, and the absence of cells with other functions)—can be effected highly complicated stimulations (the highest syntheses), and also their differentiation (the highest analyses). However, the given receptor elements *transcend this central area,* extending out over a great distance, probably throughout the entire cortex, but the farther they are from their centre, the more unfavourably they are disposed (in regard to their function). In consequence of this, the stimulations become more elementary, and the analyses less refined. In conformity with this view, the motor region, too, must be considered as a receptor one, as a projection of the whole movement apparatus; the receiving elements of this system, however, may be farther distributed from their central territory.

Before physiology looms up the immense, though promising, task of systematically investigating the state of synthesis and analysis at different distances from the nucleus of the projection area. The conception presented here concerning the cortex explains in the most natural way the mechanism of the gradual and slow restoration, to a greater or less degree, of functions lost after extirpation of parts of the brain, excepting of course those disturbances which are the direct and immediate effects of the operation, as pressure changes, derangements of the blood circulation, etc.

In conclusion, I ask your attention to the following. We have many examples of the compensating ability of the organism. It is obviously the highest perfection of the machine. It is evident that this property must be especially developed in the nervous system, which regulates and controls the whole organism. The most frequent threat from the external world comes in the form of mechanical energy. Consequently, the nervous system must be especially adapted to this danger. On this ground, it seems to me, one can account for all those decussations in the nervous system, the puzzling course of its fibres, the apparent superfluity of its elements, etc., as a factor of safety, a provision for a more or less efficacious neutralisation of menacing destruction.

Finally, the last question about the general functional construction of the cortex: do there exist besides the receptor parts of the brain which we have considered, still higher regions with general executive functions?

We have had two groups of cases classified according to which part of the brain was removed. We extirpated either the frontal and smaller half in one group, or the larger posterior half in the other. There was a marked difference between the two sets of syndromes. The dogs without the posterior sections appeared at first sight as perfectly normal. They were well oriented to the surroundings, chiefly by means of stimulations from the skin and from the nasal mucous membrane.

It was entirely otherwise with the dogs the forepart of whose hemispheres had been removed. They were completely helpless, and could not live without attention. They had to be fed by putting the food into the mouth or directly into the stomach, and they had to be protected against all sorts of injuries. They made no purposeful movement. It seemed as if nothing remained of the normal functions of the hemispheres. But this is not so. Our salivary reflex served us well here. Allow me to remind you that we observed the reactions of the animals, not through their muscular movements, but through their salivary secretions. In the given case it proved that these animals, judged by the skeletal muscles, were complete invalids, but judged by the work of the salivary glands, they were capable of a complicated nervous activity. They were able to form conditioned reflexes as are normal animals, and exactly to correct them, as mentioned in the beginning of this report. In one dog thus operated on it was possible to form conditioned reflexes only from the receptor surface of the mouth cavity upon which acted at the same time the unconditioned stimulus. In another dog the frontal lobes had been removed but the olfactory region left intact; in this animal the higher nervous activity could be studied by the influence of olfactory stimuli (odours). As a postmortem examination showed, during our operation the conducting paths of the posterior lobes of the brain had been seriously damaged; for they were markedly atrophied. This was the reason why no reflexes to the salivary glands could be formed from eye and ear stimuli. Yet from these receptor organs it was easy to form conditioned inhibition, and sleep ensued when the stimulations were continued.

This fact was constantly observed in partial destruction of various regions of the cortex. It was impossible to form positive conditioned reflexes from the body surfaces corresponding to the removed area, whereas conditioned inhibition could readily be obtained. Drowsiness and sleep developed very soon when these stimuli were applied. These phenomena as such form one of the foundations for the conclusion that *sleep and inhibition are identical* in character, and are *related in some way to fatigue.*

From the foregoing experiments it follows that in the destruction of the forepart of the hemispheres, together with great damage to the posterior parts, the remnant of the underlying cortex is capable of performing higher nervous activities. On this fact is founded *the law of equivalence of all parts of the hemispheres* in regard to the mechanism of their function. Already H. Munk had insisted on this.

In concluding, it is interesting to refer to the activity of the skeletal musculature in the above-mentioned animals. There was a great difference between the animals with hemispheres *entirely* removed and those with only a part extirpated. The former can, as is generally known, stand and walk, if only some days have elapsed since the operation. But our animals, with only the frontal lobes lacking, could not stand until some weeks had elapsed and they began to walk only after a month or more, and even then they assumed very unusual positions and often fell. And this continued during all their laboratory life. It was especially marked that these animals undertook simultaneously such movements as are inconsistent with the equilibrium of the body, in other words, they lacked the ability of combining movements efficiently.

How can we understand this condition? I think only in the following manner. We had removed by our operation the central receptor region of the skin and motor apparatus, by means of which proceeds the efficient and normal combining of movements. In the remaining part of the hemispheres there are only more disconnected and isolated receiving elements of the same functions—elements which can synthesise gradually and very slowly and in a very limited way. In those animals from which all of the hemispheres had been removed, the lower locomotor centres begin to function quickly, being unhindered by an unbalanced activity of the upper sections.

This report is based on the results of more than one hundred investigations from my laboratory and the work of seventy collaborators.[3]

[3] Most of these have been published separately in Russian.—*Translator.*

CHAPTER XXXII

INTERNAL INHIBITION AND SLEEP—ONE AND THE SAME PROCESS

(From the Anniversary Volume, dedicated to the President of the Russian Academy of Sciences, A. P. Karpinsky, 1922)

GENERAL DESCRIPTION OF EXPERIMENTATION WITH CONDITIONED REFLEXES—PRODUCTION OF SLEEP BY SPECIAL AGENTS OR BY DELAYING THE UNCONDITIONED STIMULUS; DEPENDENT ON THE TYPE OF DOG—THE LAW OF SLEEP—THE FOCUSING REFLEX—CELL EXHAUSTION AND SLEEP—RELATION OF SLEEP TO INHIBITION—ILLUSTRATIVE EXPERIMENTS SHOWING TRANSITION OF SLEEP INTO INHIBITION AND VICE VERSA—SLEEP ARISES AND VANISHES AS DOES INHIBITION—SUMMATION OF SLEEP AND INHIBITION—DISTINCTION BETWEEN SLEEP AND INHIBITION (SLEEP A WIDESPREAD INHIBITION)—LIMITATION OF INHIBITION BY EXCITATION; AN ILLUSTRATIVE EXPERIMENT—SIMILARITY BETWEEN THE MOVEMENT THROUGH THE BRAIN OF SLEEP INHIBITION AND OF ORDINARY INHIBITION—PREVENTION OF SLEEP—PROOF OF THE IDENTITY OF SLEEP AND INTERNAL INHIBITION—THE IDENTITY EXPLAINS MANY FACTS—EFFECT OF FAMINE ON SLEEP—REST.

In the beginning of our objective study of the higher nervous activity of animals by the method of conditioned reflexes (behaviour), we met with an undesired phenomenon in the experimental object—drowsiness and sleep. The animal (dog) was usually put on a stand on a table, fastened by loops suspended from a beam of the stand, and his head held up by a cord around his neck. Thus our animal had his movements restricted. The stand and the dog were confined in a special room where only the experimenter was present. In our laboratory later on the experimenter was removed to a position outside the chamber, and by a specially arranged apparatus he gave the stimuli and noted the reactions with the door closed. In the course of our experiments two different unconditioned reflexes were used; either the food reflex by feeding some more or less dry substance, or the defensive reflex by pouring acid (0.5%-1.0%) into the mouth of the dog. The reaction was studied and measured not by the motor effect, but by the secretion of saliva from the submaxillary or parotid gland. By means of a definite procedure (coincidence in time) conditioned reflexes are formed with the help of the unconditioned; various previously indifferent agents which had no relation to the unconditioned reflex, now call out the corresponding food or defence reaction both in the motor and the secretory activities of the animal.

If now, after the conditioned reflex has been elaborated, the conditioned stimulus be allowed to act alone before the unconditioned stimulus (feeding or putting acid into the mouth), even though it acts only a few seconds (15 to 30), then after some repetitions of this procedure, drowsiness and sleep begin to manifest themselves both during and after the action of the conditioned stimulus. The sleep may be so deep that it is necessary to shake the animal before he will take the proffered food, and this in spite of the fact that the dog may have had nothing to eat for 24 hours, may be very greedy, or may react very strongly to the injection of acid into the mouth.

Now the following three circumstances had been noticed at an early stage of our work. *First,* there are certain special agents out of which we form our conditioned stimuli that are particularly conducive to sleep. Of these the chief are thermal skin stimuli, warm as well as cold; mechanical skin stimuli, light scratching or pricking; and weak stimuli in general. The *second* especially noteworthy condition is the length of time during which the conditioned stimulus acts before the unconditioned stimulus. Suppose that in a certain dog we always give the conditioned stimulus 10 seconds before reinforcing it, *i.e.,* adding the food or the acid. In the course of these 10 seconds we have an extreme degree of both the motor and the secretory reaction. It is surprising how quickly the situation will change when there is apparently only a slight modification—if the unconditioned stimulus is applied not 10 seconds but 30 or 60 after the beginning of the conditioned stimulus. Drowsiness set in soon after the beginning of the conditioned stimulus, the conditioned reactions disappear, and the animal that never before slept on the stand, during every experimental séance falls asleep after the first application of the conditioned stimulus. The *third* factor: drowsiness and sleep arise under the above mentioned conditions in strict dependence upon the individuality of the dog, upon his type of nervous system. In order to avoid the obstacle of sleep, it is interesting to mention that in the beginning of our experiments with conditioned reflexes, we fell into a paradoxical error. We endeavoured to choose such dogs as would not fall asleep, as were very lively and active when outside the experimental room; but the results we obtained were exactly opposite to those we desired. These dogs under the above mentioned conditions proved especially susceptible to sleep. On the contrary, the animals we considered as stolid and inactive were particularly fit for our experiments when on the experimental stand, and even under most favourable conditions for a long time they did not sleep.

The above sleep-producing conditions finally led us to the scientific investigation of this phenomenon. What then is sleep and what is its essential relation to our experiments?

Not only practically but also theoretically my laboratory has been concerned with this question for more than ten years. We have tried and rejected five or six different hypotheses, and now at last we have come to what I think is a final conclusion, *viz.*, that the inhibition with which we had become acquainted as a recurring phenomenon in our work with the conditioned reflexes,—that this internal inhibition and sleep are fundamentally one and the same process. This conclusion conforms to the multitude of facts which we have collected during our twenty years of work with the conditioned reflexes, and also to new experiments designed to throw light on the subject.

The general law is this: a more or less enduring stimulation falling on a certain part of the hemispheres, whether or not it is of vital significance (and especially if it is without such significance), and no matter how strong it may be,—every such stimulation, if it is not accompanied by simultaneous stimulation of other points, or if it is not alternated with other stimulations, leads inevitably sooner or later to drowsiness and *sleep*. In the first place this statement is well illustrated by the fact that the conditioned stimulus which acts on a certain point of the brain, although it may be connected with the most important stimulator of the organism, food, leads, in spite of this to sleep, if it continues for a long time, or in some cases even for a few seconds if isolated and if it is not followed by the simultaneous mass of stimulations which constitute the act of eating. It must be added that there is no exception to this rule, not even in that case where the conditioned stimulus for the food reaction consists in a powerful electrical current applied to the skin of the dog. This fact is well known in its general form, though it has not been subjected to scientific investigation. *Every monotonous and continuous stimulation leads to drowsiness and sleep.* It is hardly necessary to mention every-day instances of this sort.

Once engaged in the study of this subject, we investigated the above state in other cases than those of conditioned reflexes. If there arises in the surroundings some new stimulus, *i.e.*, if there is a change in the situation, the animal responds with a general reaction during which he turns his corresponding receptor surfaces in the direction of the stimulus (looks, listens, etc.), provided the stimulus does not call out through its peculiarities some other special action. We call this general response the *orienting, investigating* or *focusing* reflex. If we repeat this stimulation at short intervals or let it last for a long time, the investigating reflex becomes gradually weaker, and finally vanishes, and afterwards, if no alternating stimulations affect the animal, he becomes drowsy and sleeps. If this is repeated several times, then the experimental sleep can be reproduced with the same exactitude as the reaction of a hungry dog to a piece of meat (experiments of S. I. Chichulin and O. S. Rosenthal). This fact is so constant that there can be no doubt regarding it. An isolated and continuous stimulation of a definite point of the cerebral hemispheres leads infallibly to drowsiness and sleep. It is most reasonable to consider the mechanism of this phenomenon as conforming with what we already know about living tissues, as a phenomenon of fatigue, and the more so because normal periodic sleep is without doubt the result of exhaustion. Thus, thanks to the continuous stimulation of the given point, it becomes fatigued and "somehow" in dependence upon this exhaustion there develops a state of inactivity, of sleep. I say "somehow" because it is not possible to understand clearly the whole phenomenon without a special insight into the series of chemical changes occurring in the given cell. The following details of the phenomenon speak in favour of this view. The inactivity, in the form of sleep, which has arisen in a given cell does not remain only at its point of origin but spreads farther and farther until it embraces not only the hemispheres but also the lower lying parts of the brain, *i.e.,* the state developing in a certain cell which has been working and has expended its energy passes over also to such cells as have not worked or been active. This is at present an obscure point in the understanding of the phenomenon. One must grant that exhaustion in the cell produces a special process or substance which stops the activity of the cell, as if to prevent an extraordinary, threatening and annihilating over-exertion. And this peculiar process or substance may be carried over to the surrounding cells which have not participated in the given work.

Now we come to the relations which exist between sleep and internal inhibition of the conditioned reflexes.

Internal inhibition develops when the conditioned stimulus is not attended by the unconditioned, whether this be once or always, but in the latter event, only under certain circumstances. Thus comes about extinction, retardation, conditioned inhibition, and differential inhibition. We see then that the same conditions are necessary for the occurrence of sleep as for the development of internal inhibition. It is impossible, therefore, not to consider this fact of great significance for the question as to the relation of internal inhibition to sleep, and the more so because in all our cases of internal inhibition we meet with an admixture of drowsiness and sleep. In retardation, when we delay the beginning of the unconditioned stimulus for some time after the beginning of the conditioned stimulus, in proportion to the length of this interval we have, as mentioned before, the state of sleepiness. In a dog with an elaborated conditioned reflex, if one repeats stimuli related to the conditioned stimulus (which were previously active due to irradiation of the stimulation) without accompanying them by the unconditioned stimulus, then there is a loss of their action simultaneous with the setting in of drowsiness and sleep. Exactly the same phenomenon is observed in the elaboration of the process of conditioned inhibition from a stimulus; except that the drowsiness rarely passes over into complete sleep.

Likewise, in the extinction of the conditioned reflexes, if the extinguished stimuli are repeated several times during the same experiment, drowsiness and sleep are clearly manifested. If one continues applying the extinguished stimuli during some days, then the animal that formerly had no inclination to sleep, becomes so somnolent that it is difficult to work further with him. It should be added that there are apparently some peculiarities in the various kinds of internal inhibition which affect the speed of the occurrence of sleep and its stability.

Now a further question: what special relations are observed between sleep and inhibition? We meet here with many variations. They are now the transition of inhibition into sleep and vice versa, now the alternation of sleep and inhibition, now the summation of sleep and inhibition.

We have a dog in whom the unconditioned stimulus is added to the conditioned after 30 seconds. A conditioned reflex was elaborated: the salivary secretion commences 5 to 10 seconds after the beginning of the conditioned stimulus. We repeat this experiment for weeks or months, depending upon the individual animal, always accompanying the conditioned stimulus with the unconditioned. Now we can see that the latent period of the conditioned stimulus gradually increases; 15 to 20 seconds pass, then 20 to 25 before the conditioned reflex begins; and finally the conditioned reflex does not start until the exact end of the 30 seconds or perhaps 1 or 2 seconds earlier. This is internal inhibition, *retardation,* an exact adaptation to the moment of the action of the unconditioned stimulus. Later on, the effect of the conditioned stimulus is entirely absent during the first 30 seconds of its action, but it becomes manifest if its application is continued for longer than 30 seconds. But there follows a stage, when you can not obtain any effect from your conditioned stimulus, and together with this the animal becomes drowsy and goes to sleep, or becomes quite motionless (*i.e.,* falls into a cataleptic state).

An opposite case: we have elaborated a delayed reflex in which the unconditioned stimulus followed the conditioned only three minutes after its beginning. Here the three-minute period of the conditioned stimulus is divided into two phases—the initial, inactive; and the second, active. And often during an experimental séance we observe that in the first trial of the conditioned stimulus, the animal becomes immediately sleepy, and toward the end of the three-minute period, when the conditioned reflex should begin to appear there is only a minimal, if any, effect. Further, the active effect of the conditioned stimulus increases with each repetition, fills the greater part of the stimulation period, and the state of sleep is more and more dispelled. Finally, there is no sleep and drowsiness at all, and the whole period of the application of the conditioned stimulus divides into two equal parts, or into parts having a relation of 2 : 1, the first without effect, the second with an effect which gradually increases toward the end.

Thus, we see that in the first case, inhibition passes over into sleep, and in the second, sleep changes into pure inhibition.

A similar transformation of inhibition into sleep is observed in the orienting or investigating reflex. This reflex, as has often been noted, disappears during a long duration or constant repetition of the stimulus. It is interesting that, as the experiments of Prof. G. P. Zeliony show, such a reflex provoked by a sound, in a dog having the hemispheres removed, is not lost even though it be repeated many times. Hence the reason for thinking that the cells of the hemispheres and of the lower parts of the brain bear a very different relation to their stimulations. In what way is the destruction of the investigating reflex attained in the normal animal? The experiments of N. A. Popov proved that the process lying at the foundation of the suppression of the investigating reflex is in all its details similar to the extinction of the conditioned reflexes, and is a manifestation of inhibition. Later this inhibition passes over into sleep.

Sometimes, for example in the experiment with delay of the unconditioned stimulus for about 30 seconds after the beginning of the conditioned, it happens that an animal which is usually wide awake when on the stand begins to fall asleep immediately at the commencement of each separate conditioned stimulus, going into a passive state, hanging his head, and even snoring; but after the conditioned stimulus has continued for 25 seconds, the dog awakes and gives a markedly positive reflex. This state of affairs may last for a considerable length of time. It is evident in the given case that sleep replaces inhibition, arising and vanishing exactly as does pure inhibition.

There is further the constant fact of simultaneous disappearance of sleep and internal inhibition. We have a well elaborated delayed (three-minute) reflex which in the waking state of the animal becomes effective only after one and a half to two minutes. If we now apply our conditioned stimulus after the animal has fallen asleep, the stimulus awakens it, dispelling the sleep and together with it the internal inhibition; the conditioned stimulus gives its effect immediately, and the inactive phase disappears.

Here is a case of *summation* of sleep and inhibition. We have again a well elaborated delayed reflex (three-minute). The effect begins only after one and a half minutes and attains a maximum at the end of the third minute. Now, together with the conditioned stimulus, we apply some new, rather weak, indifferent stimulus such as a hissing sound. During its first application it dis-inhibits, allows the conditioned stimulus to manifest its effect in the inactive phase, and we see the orienting reaction at the start of this new stimulus; on the second application there is no orienting reaction, the conditioned stimulus is not manifested during all three minutes, and drowsiness can be observed. The conditioned stimulus applied alone continues to give a pure, delayed reflex (experiments of D. S. Fursikov). Thus two inhibitions summated produced the state of sleep.

The same phenomenon is to be seen in the following modification of the experiment. The reflex is delayed for 30 seconds. The effect begins 3 to 5 seconds after the start of the conditioned stimulus. Now we introduce a new, additional stimulus, and repeat it until it ceases to produce the orienting reaction, but provokes drowsiness. If we apply it now together with the conditioned stimulus, we get a more delayed reflex, its action beginning after 15 to 20 seconds (experiments of S. I. Chichulin). Thus in the first case two kinds of inhibition gave drowsiness. In this second case the state of drowsiness arising from one stimulation reinforces the inhibition of the other.

All the foregoing facts strengthen our conviction that internal inhibition and sleep are one and the same process. But what is the difference between the two states and how does this difference come about? At first sight these two states seem to differ widely. Internal inhibition always takes place in the waking state of the animal, and particularly in his most exact adaptation to his surroundings, while sleep is a state of inactivity, the repose of the hemispheres. The distinction is as follows: *inhibition* is a partial, fragmentary, narrowly limited, strictly localised sleep, confined within definite boundaries under the influence of the opposing process—that of excitation; sleep on the contrary is an inhibition which has spread over a great section of the cerebrum, over the entire hemispheres and even into the lower lying midbrain. From this point of view the above cases can be easily understood: either the inhibition spreads and sleep sets in, or the inhibition is limited and sleep disappears. Let us take, for instance, the case in which during an experimental séance the formerly predominating sleep becomes gradually replaced by the appearance of pure inhibition. Here under the influence of repetition of the unconditioned stimulus, little by little the stimulation limits the inhibition process, confining it within narrower boundaries and to a shorter period: together with this, sleep disappears, and there actually occurs an equilibration of the stimulation and inhibition processes.

From this point of view in order to limit inhibition and to prevent its change into sleep, or, vice versa, to transform sleep into pure inhibition, it is essential to form in the hemispheres points of stimulation which can resist the spread of the inhibition. For a long time we applied such a procedure, although empirically. When, from a more or less delayed conditioned reflex drowsiness developed and sleep set in, we formed new conditioned reflexes out of stronger agents and made them more strictly coinciding reflexes, *i.e.*, reflexes in which the unconditioned stimulus was joined after a shorter interval to the conditioned. This often helped. Sleep was removed, and the formerly delayed reflex restored.

Recently Dr. Petrova performed experiments extending over a long period as follows. Conditioned reflexes were elaborated in two dogs: the first a very lively animal, and the second phlegmatic. In the first the conditioned stimulus was begun 15 seconds before the unconditioned, and in the second, three minutes. But soon after the formation of the conditioned reflexes both dogs became sleepy on the stand, and later to such an extent that no further experiments could be performed. The following changes were at this time made in the procedure. The unconditioned stimulus was added to the conditioned 2 to 3 seconds after the beginning of the latter, and besides the earlier reflex, which had been elaborated from the ticking of a metronome, conditioned reflexes were formed out of five new agents—a bell, a tone, the sound of air bubbling through water, the flashing of a light before the eyes of the animal, and mechanical stimulation of the skin. The reflexes formed very quickly and sleep disappeared; in the experimental séances each stimulus was applied only once, whereas formerly the metronome was repeated six times. Afterwards all the coinciding reflexes were delayed by removing the unconditioned stimulus every day five seconds further from the beginning of the conditioned. Correspondingly the effect of the conditioned stimulation was little by little retarded. When this interval between the conditioned and the unconditioned stimuli reached three minutes, a striking difference was noted between the two dogs. The experiments with the phlegmatic dog proceeded smoothly, good delayed reflexes were elaborated from all stimuli, and were retained as such though all the stimuli except the original metronome were discontinued, and the interval between the stimulus of the metronome and its unconditioned stimulus, food, was lengthened to five minutes. In the lively dog it was quite otherwise. When the unconditioned stimulus was delayed to three minutes, the excitement of the dog reached a high degree; during the stimulation the dog barked desperately, struggled energetically, developed dyspnœa, and the salivary secretion did not stop, *i.e.*, in the intervals between the separate stimulation, as is usual in dogs which display a state of strong excitation. Then all stimuli were discontinued except the metronome, which remained as a delayed stimulus. The animal slowly became quiet, but at the same time drowsy, and fell asleep,—and the reflex disappeared. In order to keep the dog awake it was necessary to apply all the stimuli again and coincidentally, *i.e.*, to follow the conditioned stimulus quickly by the unconditioned. This we did. Afterwards the unconditioned stimuli were delayed. Now the delayed conditioned reflex developed without excitation, and the reflex to the metronome, when it was again alone, did not pass into sleep, but retained its delayed character.

This experiment is of interest in many respects: here I ask you to observe that the application of many stimulation points without frequent repetition of the stimulation of one and the same point during the experimental séance caused the disappearance of sleep and the limitation of the inhibition and its inclusion within definite boundaries. The following experiment of Fursikov leads to the same conclusion. From mechanical stimulation of the skin at one end of the body a delayed conditioned reflex was elaborated, the unconditioned stimulus being retarded for three minutes. Then drowsiness set in, and the reflex disappeared. Afterwards a coincident conditioned reflex from the mechanical stimulation of the skin at the other end of the body was formed. The delayed reflex was restored, but it still showed retardation. Thus it happened that the stimulation of a new point in the skin region of the hemispheres caused a delimiting of the inhibition issuing from the first point, and simultaneously sleep disappeared.

The same thing occurs in every differentiation. If those stimuli which are closely related to the conditioned stimulus are applied repeatedly, without accompanying them by the unconditioned stimulus, then the borrowed effect of the original irradiation gradually decreases; they are inhibited, and deep sleep supervenes during the effect of these differentiated (negative) conditioned stimuli and extends beyond the period of stimulation. But through the application of such (negative) stimuli alternating with the well elaborated conditioned (positive) stimulus, which latter is always accompanied by the unconditioned stimulus, for example, food, it comes about that sleep passes off and the negative conditioned stimuli are entirely ineffective, inhibited. Consequently the stimulation of a certain point limits the spread of the inhibitory process from the neighbouring points, concentrates it, and in that way banishes sleep.

The same phenomenon that is seen in differentiation may be seen in conditioned inhibition, if the inhibiting combination is constantly alternated with positive stimuli.

A similar process is observed in extinction. If the extinction is repeated on many successive days or many times during the same séance, the affair ends in drowsiness and sleep. If the extinction is not done every day, and not often, only once or twice during a single experiment, then it proceeds quickly and there is no drowsiness. Apparently the oft-repeated stimulations which are reinforced (*i.e.*, followed by the unconditioned stimulus) do not permit the spread of the inhibition: this is the concentration of inhibition.

The foregoing explanations and conclusions embody the idea that inhibition and sleep are processes which move through the mass of the cerebrum. And it is so in fact. Many experiments in my laboratory have made it clear that the internal inhibition which is called out at a given moment persists in the nervous system for some time after the cessation of the provocative agent, and only later gradually concentrates in time and adapts itself more and more closely to the given moment. The same holds for concentration in space. In the skin one can follow exactly how far and with what speed the inhibition at first irradiates and later concentrates about its point of origin.

The same facts are known from ordinary observations on sleep. Falling to sleep as well as awaking, the overwhelming of the brain by sleep and its release from sleep, proceed more or less gradually. I have noted this process (with L. N. Voskresensky [1]) in a dog that fell asleep as a result of the action of the whole environment of all the objects in the experimental room. One can easily distinguish between some of the successive stages of sleep manifested in various parts of the brain. It is interesting that the velocities of the spread of inhibition and sleep are of the same order. Falling asleep and awaking can be measured in minutes, few or many, and the same is true of irradiation and concentration of internal inhibition. The similarity goes even further. As is well known, human beings vary widely in the quickness with which they fall asleep and awaken; some go to sleep and wake up very rapidly, others slowly. The same is true of the movement of inhibition processes. Among the dogs that have been compared up to the present (three), the difference between the extremes was as 1:10. In one dog the hither and thither movement of inhibition (irradiation and concentration) took place in one and a half minutes; in the other extreme case, in 15 minutes. From the point of view of the extent of the spread of the inhibition, one can understand the following difference, which is seldom met with in animals. In the majority of dogs it happens that the expansive irradiation of the inhibition is manifested in complete sleep and relaxation of the skeletal musculature; the inhibition reaches those parts of the brain which lie below the hemispheres and which govern the equilibration of the animal in space. In rarer cases the inhibition is confined to the hemispheres and their motor region, and does not penetrate downward; in this latter case the result is that the animal becomes stiff and immobile, but preserves his active pose.

As the above mentioned experiment of Dr. Petrova showed, the careful and gradual development of localisation of inhibition in one of her dogs,

[1] See chapter xxiv.—*Translator.*

so operated that the previous irradiation of inhibition to the extent of sleep was finally prevented and only a narrowly localised sleep, a pure inhibition, remained. In some cases the internal inhibition, *viz.*, in differential inhibition and conditioned inhibition, drowsiness and sleep, although they occur, are of shorter duration than in other sorts of inhibition (*i.e.*, the localisation of inhibition proceeds more easily and quickly). Therefore, in order to prevent the development of sleepiness, we elaborate, usually during the period of the preliminary work, not only several conditioned reflexes but also differential inhibition or conditioned inhibition. And the desired effect is generally obtained by this procedure.

In accordance with the foregoing statements is the following fact. As an agent producing internal inhibition acts more surely and quickly on repetition, so it is with sleep; for any indifferent agent, or our conditioned stimulus which causes it, also becomes more effective on repeated application.

Here is a fact from one experiment, which seems worth mentioning although it has not yet been repeated. Sleep which appeared at the beginning of the development of differentiation, and later with the differentiation well established became imperceptible in the general behaviour of the animal, was again manifest when we undertook to destroy the differentiation by accompanying the negative conditioned stimulus by the unconditioned stimulus (food).* It was as if sleep had been temporarily liberated from its confines. But this is not so easy to imagine.

Finally an additional proof of the identity of sleep and internal inhibition is evident from the following fact, which we have often met before. It is the development of general excitation in some cases of inhibition. We are elaborating, for example, a conditioned inhibition, and when it commences to manifest itself, we see that our dog begins to be much excited, struggling, barking and panting. In some dogs this is only a short phase; in others it is an obstinate condition which remains for a long time. This has already been described by us in one of Dr. Petrova's dogs. In this dog, during the development of retardation arising from six simultaneous stimuli, there was an extraordinarily intense and persistent state of excitement, disappearing only on the removal of five of these stimuli. A like state of excitation is observed in some dogs under the influence of often repeated indifferent stimuli which then lead to sleep. Dogs that are not fastened in the experimental room make many movements, scratching, barking, etc., before lying down to sleep.** In dogs having a delayed conditioned reflex and manifesting

* Experiments of V. V. Stroganov.

** Experiments of I. S. Rosenthal.

the retarding inhibition as sleep during the inactive phase, the following characteristic sequence of phenomena is observed: as soon as the conditioned stimulus begins to act, the waking dog, which has until this time been quiet, executes some disorderly movements, and only later does rest return, now accompanied, however, by slumber (by which we mean passive position of the body, hanging of the head, and closing the eyes). Later at the approach of the active phase, the animal again makes indefinite movements, and only then commences to give the specific motor reaction to food.

Thus, the dissolving of excitation by inhibition as well as the transformation of the waking state into sleep, is attended by a temporary general excitement. Probably this is the positive phase of induction; *viz.,* the initial inhibition immediately provokes in the remote regions excitation, which is suppressed, however, through the continuous effect of the inhibiting or sleep producing agent.

This explanation of sleep and internal inhibition as essentially one and the same process has thrown light upon many formerly obscure facts. Here are the chief ones. After extirpation of the cortical projection of some receptor organ, it is impossible for weeks or even months to form a conditioned reflex from stimulation of the corresponding organ, although the same stimuli can readily appear as conditioned inhibitors. The possibility of external inhibition in these cases was excluded by special experiments. When some time has elapsed after the operation, it is possible to form a conditioned stimulus but only if the unconditioned stimulus is almost coincident, *i.e.,* following no later than three to five seconds after the conditioned. With a greater separation of the conditioned and unconditioned stimuli, the conditioned reflex vanishes. These facts are especially evident when part of the projection area of the skin is extirpated. On a certain part of the skin where the retardation may be 30 seconds, the dog stays awake and the reflexes are maintained, whereas on other skin areas with the same retardation the dog becomes drowsy and sleeps, and the reflexes are lost. In the first phase after this operation the stimulation of the parts of the skin which correspond to the extirpated parts of the projection area in the brain (and which have, therefore, lost their earlier positive conditioned action) inhibits from the start the reflexes simultaneously stimulated from other parts of the skin which correspond to areas of the brain not injured in the operation. Furthermore, the stimulation of these inactive places calls out no orienting reaction. Finally, stimulation of only these places, though of short duration, causes drowsiness and sleep, even in dogs that never before slept on the stand. Now we have no difficulty in understanding all the facts. When after the operation you stimulate the corresponding points of the receptor apparatus, then all the cells which remain—cells weakened by the operation or cells which during the existence of the cells now removed had never been stimulated or had been stimulated only together with the cells now removed—are quickly fatigued. They fatigue even during the latent period of the stimulation and therefore they provoke inhibition from the outset, and with the wider extension of inhibition, sleep as well.

A fact relating to this was observed in our laboratory in the hard years, 1919 and 1920, when we had to work with starving and exhausted animals. Even slightly delayed reflexes quickly disappeared, provoking sleep, so that further work was impossible.* Apparently the general exhaustion made itself especially manifest in the nerve cells of the hemispheres. One can understand in a similar manner the previously mentioned fact that under the conditions of our experiments lively dogs are especially susceptible to sleep. One may suppose that the vivacity and restlessness of these animals is such that with their ready excitability there is rapid exhaustion of the given stimulated points; this results in inhibition, and this in turn produces by induction a general excitation. This stimulation by impelling the dog to move to and fro generates new excitations in other cells, whereby in the state of freedom a more extensive development and spread of inhibition (sleep) is prevented. On account of the impossibility of such spread of excitation while the dog is on the stand, and with the unavoidable uniformity of external and internal stimulations, in these animals with their weak nervous systems, sleep quickly develops.

Probably it is possible to explain the temporary initial excitation which arises under the influence of the soporific stimuli during the waking state, as a means of avoiding sleep under unfavourable conditions of time and space. This avoidance is effected if the animal is constantly exposed to new external stimulations, or if stimulations are produced by the movements of his own body.

After we had seen that with a well developed delayed reflex the conditioned stimulus acting on a drowsy or sleeping animal gives immediately at the time of waking the conditioned effect without an inactive phase, it was natural for us to modify our view concerning what we call *dis-inhibition* of conditioned reflexes. Certainly dis-inhibition is a prominent and important phenomenon when internal inhibition, although it may be well elaborated, suddenly disappears under the influence of any unusual stimulus. If in analogy with the inhibition of conditioned stimuli by unusual stimuli (external inhibition), dis-inhibition were explained as a possible inhibition of inhibition, this would make more involved the understanding of an already very complicated nervous relation. Now we can give a simpler explanation. As in the

* Experiments of N. A. Podkopayev, I. S. Rosenthal and U. P. Frolov.

above mentioned case, where inhibition disappeared together with sleep, so also in all other cases one may suppose that a new, incoming, irradiating stimulation removed the inhibition just as it dispels sleep; for inhibition, according to our analysis, is a partial sleep.

After what we have said, if you accept the fragmentary nature of sleep in the cerebral hemispheres, the phenomena of human hypnotism are comprehensible on the basis of the partitioning and complexity of the great hemispheres.

In conclusion, I venture to make a general deduction from the cited facts and their comparison. If one agrees with us that sleep and internal inhibition are essentially one and the same process then we should have a striking instance of the economic principle of the organism, *viz.,* that the highest manifestation of life, the most exact adaptation of the organism, the constant correction of its temporary connections, the unceasing establishment of a moving equilibrium with the surrounding world, rests upon the inactive state of the most precious elements of the organism—the nerve cells of the cerebral hemispheres.

CHAPTER XXXIII

CHANGES IN THE EXCITABILITY OF VARIOUS POINTS OF THE CEREBRAL CORTEX AS ONE OF ITS FUNCTIONAL CHARACTERISTICS

(From the *Schweizer Archiv für Neurologie und Psychiatrie*, 1923, vol. xiii.)

THE CONDITIONED REFLEX MAY BE FORMED TO ANY INDIFFERENT AGENT—POSITIVE AND NEGATIVE CONDITIONED STIMULI—EXTERNAL INHIBITION; AN EXPERIMENTAL EXAMPLE—INDUCTION—RECIPROCAL INDUCTION, A MEANS OF STABILITY—AN EXAMPLE—MUCH REMAINS TO BE EXPLAINED.

UPON the physiologist devolves the colossal task of explaining the functions of the cortex of the cerebral hemispheres. At present only preliminary experiments can be undertaken and only an attempt to characterise some functions of this nervous mass from certain facts. On the basis of my many years of research I take the liberty of describing to you some of the characteristics of these functions.

For some time past we have occupied ourselves with the study of the reflexes formed under certain conditions during the life of the individual —the conditioned reflexes. The existence of such reflexes is dependent upon the presence of the hemispheres, which means that they are a special function of this part of the brain. In the investigation of these reflexes, material has been collected which has indicated to us one of the characteristics of the brain cortex.

Every agent of the external world which can be transformed into a nervous process by the special receiving apparatus may, if it stimulates a certain part of the cortex, call out the activity of one or another organ. This is effected by means of the conduction paths to the executive nervous elements (cells and nerve fibres) of the given organ. The essential condition for the formation of this new reflex is the temporal coincidence of the effect of this agent (conditioned stimulus) on the organism with the action of that stimulus (unconditioned stimulus) which provokes an inborn unconditioned reflex (including here what is usually called instinct), or which provokes well elaborated and stable conditioned reflexes. For example, all agents which formerly had no relation whatever to food, if they coincide one or several times with the act of eating, afterwards are capable, when they act alone, of calling out the food reaction —a series of definite movements and corresponding secretions. Conditioned stimuli elaborated in this way, are connected with certain definite points of the cortex. This gives us the possibility to trace exactly the changes to which these points are subjected during various activities of the brain. In the present account I shall refer to these changes as variations in the irritability of these points.

As has already been shown in our experiments, every well elaborated conditioned stimulus, if it is repeated temporarily or constantly (if constantly under certain conditions), without being accompanied with the unconditioned stimulus by the help of which it was formed, quickly loses its stimulating effect and may even become an inhibitory agent. Thus the point of the cortex stimulated by this agent loses its former irritability and acquires a new one. This inhibitory agent, if the conditions which produced it remain uniform, may express its effect, may call out the state of inhibition, directly and immediately, just as a positively acting stimulus provokes an excitation process. Also our inhibitory agent will occasion, according to its duration, different grades of an inhibition process, internal inhibition, as we call it. In this way one may speak empirically of positive stimuli (producing the stimulation process), and negative stimuli (producing the inhibition process). We have employed for some time the terms positive and negative reflexes (from the experiments of G. V. Volborth). The advantage of this conception of our facts is that it enables us to understand all different states of the nervous elements in any circumstances and under the influence of any stimulating agents, as a continuous uninterrupted succession of processes; and this corresponds to the facts.

The conditions which give rise to inhibition points in the cortex are as frequent as those which produce excitation points, and, consequently, the entire cortex represents an immense complex of positively and negatively excited areas intermingled and scattered in a checkered fashion. In this system of more or less fixed points there arise changes of irritability depending upon the changes in the internal or the external environment of the animal. And this occurs in various ways, as will be described.

A simple and constantly occurring case is the following: as soon as there is produced by a novel external or internal stimulus some new nervous activity, expressed in the work of one or another organ, our conditioned stimulus loses in strength or even becomes ineffective: that is to say, under the influence of the newly arising foci of stimulation in the cortex, the irritability at the point corresponding to our positive conditioned stimulus is decreased or reduced to zero—*external inhibition.*[1] This is apparently analogous to similar relations in the lower lying parts of the central nervous system.

Such a form of inhibition occurs only with stimuli of moderate strength. If the new stimulus is very strong and is accompanied by a

[1] The reader met with this phenomenon in the foregoing chapters under the name of external inhibition by extra stimuli. See chapter xi.—*Translator.*

FIG. 8 PROFESSOR PAVLOV AT THE OPERATING TABLE

violent reaction of the animal, then our special stimulus not only does not lose its effectiveness, but on the contrary its action is enhanced, *i.e.*, the positive irritability of the point upon which the stimulus falls is increased. Here is an example: a conditioned food reflex was elaborated, having a certain definite effect, and at the same time in this dog the guarding reflex was strongly expressed. In the presence of the person who is accustomed to experimenting with him in an isolated room, the dog remains quietly in the stand permitting without resistance all the necessary manipulations. If, however, the usual experimenter is replaced by another, the dog exhibits a markedly aggressive reaction, and if during this time the newcomer applies the conditioned stimulus, he obtains a sharply augmented effect. But it is only necessary for him to sit motionless and the aggressive reaction gradually disappears, though the dog steadily watches the new experimenter; and if the conditioned stimulus is now tried, it produces an effect well below the normal. This result can be repeatedly obtained. (Experiments of M. Y. Besbokaya.)

If the conditioned inhibition points become fatigued under the influence of such strong new stimuli, they lose their inhibitory action, and are transformed into positively acting points (our dis-inhibition). With very weak, newly added stimuli, by which the positively acting points are not perceptibly influenced, only the inhibitory points suffer a transformation; they are dis-inhibited; *i.e.*, their negative irritability passes over into a positive irritability. The above described changes in irritability arise spontaneously, without elaboration. Further, however, there are also slowly developing fluctuations, which we shall now discuss.

In relation to these phenomena, I shall describe experiments with conditioned mechanical skin stimuli; for the skin is a great and easily accessible receptor surface upon which all the phenomena interesting us may be clearly manifested. If we have developed conditioned reflexes to the same kind of mechanical stimuli, of uniform strength on many spots of the skin, then we have in the cortex a region which is easily controlled in regard to its excitability. Now if in a series of mechanical skin stimulators fastened to the body of the dog, we have made positive conditioned stimuli from all except the outer one, and from that we have made a negative conditioned stimulus (*i.e.*, by not accompanying it with feeding it has become differentiated), then with every application of this negative stimulus, the inhibitory process spreads from its point of origin over the positively acting points, and then contracts around the original point; it first irradiates and later concentrates. This phenomenon was observed and described long ago by us (Krasnogorsky, Kagan, Anrep, and others).

Even at that time one of these authors (Kagan) noticed the following phenomenon, though only in a few cases and not marked; immediately after the cessation of the inhibitory stimulation, and indeed in points remote from the origin of the inhibition point, an increased excitability could be observed; in other words, the conditioned stimulus produces a greater effect than before. Recently, our attention has been especially directed to this phenomenon, and several of our collaborators have investigated it in different cases of internal, *i.e.,* elaborated inhibition. It proved to be a striking and constantly recurring fact. Let us first consider that inhibition which develops in the case of the differentiated conditioned stimulus. The more the differentiated (negative) agent is repeated without the unconditioned stimulus, the more quickly its inhibitory effect begins, and the stronger it is; finally, pure inhibition results. At the same time, this inhibition, even though it was formerly widely spread, concentrates more and more, under the influence of the stimulation of points with a positive action, *i.e.,* the positive conditioned stimuli. But now a new phenomenon appears. Immediately and very quickly, in the course of some seconds or minutes, after the cessation of the action of the inhibitory agent (negative conditioned stimulus) there is observed in the neighbouring points with the positive action a marked increase of irritability. In those points lying nearest to the point corresponding to the inhibitory agent this phenomenon appears as a phase, which is followed by a decrease of the irritability and finally by a return to normal. In the remote points there is only the phase of increased irritability which is followed directly by the normal (experiments of Bykov). In some dogs the increased irritability in all the observed points is replaced by the irradiating inhibition (experiments of Fursikov and Kreps). These variations are determined apparently by the degree and velocity of the irradiation as well as of the concentration of the inhibition process, and by the strength of the positively acting points. In conformity with Sherrington's term, we designate these phenomena as *induction.* Induction of the stimulation process by the inhibition process appeared, in the case described, not in those elements * in which the inhibition occurred, but in the neighbouring elements. This is induction from a distance.

It was interesting to trace the state of excitability in the neighbouring and distant points during the action of the inhibiting stimulus. This has been done for another kind of internal inhibition in the experiments of N. A. Podkopayev. If the positive conditioned stimulus without being accompanied by the unconditioned stimulus is repeated several times in succession at intervals of some minutes, it quickly loses its stimulating effect. The conditioned reflex sinks, as we express it, to zero. This occurs in consequence of the development of an inhibition process

* Induction may now, however, be stated to exist also in the same elements where inhibition originated.

at the stimulated point. As we have seen in the process of differential inhibition after the cessation of the stimulus, this process, too, spreads—it irradiates. If through the development of extinguishing inhibition, the stimulating effect from a certain place on the skin has fallen to zero, and this zero effect is maintained by continuous stimulation (of course, without the unconditioned stimulus), Podkopayev has seen that the stimulation of other points of the skin is manifested in a very particular manner. The stimulation of all other points of the skin, neighbouring as well as distant, acts positively, but with certain peculiarities. The latent period is clearly shortened (1 to 3 seconds instead of 4 to 5), but the general effect is less in comparison with the normal. The most simple explanation of this fact is that the sharp decrease of the latent period is a sign of an increase of the irritability of the stimulated points; but as both the inhibitory and the positive impulses fall simultaneously on the effector centre, the resultant action is their algebraic sum.

We have reason to believe, however, that the opposite conditions also exist—that the excitation process can induce and reinforce the inhibition. This also results when both processes are well elaborated. We arrive at such a conclusion from the following experiments. Some years ago K. N. Krzhishkovsky had studied the rate of extinction of that kind of inhibition which we call conditioned,* in the case where the inhibited combination was followed by the unconditioned stimulus and so converted into a positive one. It proved that the extinction of this inhibitory stimulation is peculiar and that its speed depends upon whether the extinction (application of the inhibited combination followed by the unconditioned stimulus) has continued uninterruptedly or has alternated regularly with the application of one of the positive conditioned stimuli accompanying the unconditioned stimulus. If the combination followed by the unconditioned stimulus, *i.e.*, the procedure for the extinction of the conditioned inhibition, is alone tried, the annihilation of the inhibition effect appears during the first or second trials; if it has alternated with the conditioned stimulus, then the annihilation is not evident for a long time. This phenomenon can be explained by saying that the positive stimulus induces the inhibition process, and prevents in this way its destruction. The experiment of Krzhishkovsky has recently been repeated by V. V. Stroganov and the differential inhibition studied in greater detail. A certain frequency of the metronome was made a positive conditioned stimulus, and another frequency a differen-

* Conditioned inhibition results from a combination of a conditioned stimulus with an indifferent agent which has never been accompanied by the unconditioned stimulus; the conditioned stimulus in this combination is, therefore, inhibited, and the new agent becomes the conditioned inhibitor, while the conditioned stimulus taken alone has its usual action.

tiated (*i.e.*, formerly inhibiting) stimulus. Then the differentiation was extinguished by giving food together with the (formerly) negative conditioned stimulus. When this procedure was constantly alternated with the application of the positive stimulus, the disappearance of the differentiated inhibition and the formation of a positive stimulus (with the second frequency of the metronome) proceeded very slowly—after twenty or more trials. But in the case of continuous extinction, this result is obtained after the first or second trial.

Thus we have a negative of *induction;* an inhibition process is called out by the process of stimulation.

The process of induction develops in our case under the influence of the persistent action of the corresponding stimuli; it does not exist from the outset. The whole matter may be summed up thus—*for the formation of the isolated foci of stimulation and inhibition in the cortex there is necessary, first of all, the presence of the corresponding stimuli; but once these foci have been formed, induction appears in the rôle of a supporting mechanism for their maintenance and stability.*

In our present experiments with the conditioned reflexes induction manifests itself almost exclusively in the neighbouring cortical regions, but not at the points where the primary processes occur. We happened to see this last fact only in another form and during accidental observations. It will not be superfluous, I think, to mention the most marked case of this kind. The dog concerned showed a highly developed reflex of slavery (servility or obedience); it was investigated and described by U. P. Frolov. The animal had an isolated stomach pouch for study of the activity of the gastric glands. When he was put on the stand he remained wide awake and also became motionless, not even changing the position of his feet. If, however, he had been on the stand for some time and we now proceeded to free him from his harness in order to remove him from the stand, the dog fell into a state of marked excitation; he barked furiously and tried by every possible means to break away. Now it was absolutely impossible to make him return to the stand, either by calling him, whipping him, or placing a chair on which he could jump up to the stand as he always had done on entering the experimental room. When, however, the dog is taken out of the experimental room for a rest and brought again into the laboratory, he immediately rushes to his own place and springs into his stand. It is not difficult to explain this mechanism. The stand and its harness act in this very submissive animal as a strong conditioned inhibiting agent for his motor system, even though the uncomfortable position and fatigue of his limbs demand movement. And now when there is liberation from this inhibiting agent, an extreme excitement sets in through induction in that part of the motor cortex which has been for a long time inhibited. This phenomenon is observed to some extent in many dogs, but in this case it was especially prominent.

The existence of a positive and negative induction phase favours the fine and exact delimitation of those positively and negatively excitable points formed in the cortex during the life of the individual. This in the main is accomplished in the interest of preserving the organism as a separate system in its environment, by a constant and efficacious activity of that organ which maintains the finest connections of the animal with the external world—the activity of the cerebral hemispheres.

So much for the actual relations. Concerning their interpretation, the possible representation of their internal mechanism, nothing definite can be said other than that these are the general and special properties of the cortex of the cerebral hemispheres. And we do not even consider the question as to the elements of the central nervous system that are concerned. Obviously more facts are needed. For the present all remains in darkness—the spread of the inhibition process, as well as the phenomenon of the elaborated reciprocal induction, and many other of the above mentioned phenomena, above all the fact of the transformation of a positive excitation into the opposite negative process, and vice versa.

CHAPTER XXXIV

ANOTHER PROBLEM IN CEREBRAL PHYSIOLOGY

(From the *Reports of the State Medical Institute*, Moscow, vol. i, Part I.)

DUPLICITY OF THE CEREBRAL HEMISPHERES—THE DIFFERENTIATION OF SYMMETRICAL POINTS OF THE SKIN.

A NEW question in the objective study of the brain is that of the two cerebral hemispheres, their existence as a pair. What does this duplication signify? How is the simultaneous activity of the hemispheres to be understood and explained? What sort of compensating action is there here, and what is the advantage of a united activity of both hemispheres? On the basis of the existing scientific knowledge we know that there is a certain division of labour between the two. But also it is known from extirpation experiments on animals that the absence of one hemisphere can be compensated for after some time, either partly or in full, by the activity of the remaining one. In the physiology of the conditioned reflexes there is a series of experiments which categorically raises the question of the duplex activity of the hemispheres. In this short report allow me to discuss a few experiments pertaining to this question.

It was shown in our laboratory that the positive conditioned reflexes, as well as the negative ones, which had been elaborated on the skin surface of one half of the body are obtainable to exactly the same degree from the stimulation of corresponding symmetrical points of the other half of the body. This was first brought out by N. I. Krasnogorsky in his brilliant dissertation, "The Process of Inhibition and the Localisation of the Skin and Movement Analysers in the Cortex of the Brain" (St. Petersburg, 1911). It has been shown to be a constant and exact occurrence, and has been confirmed in detail by one of our later collaborators, G. V. Anrep. This author established for the first time the so-called stationary irradiation of the conditioned excitation, which consists in the following: if we have elaborated a conditioned reflex from mechanical stimulation of a certain point of the skin at one end of the body, then on the first tests of mechanical stimulation of other points of the skin we also note a conditioned effect, whose strength is proportional to its distance from the original point. Exactly the same relations hold for the symmetrical points of the other side of the body, though they have never been experimented with before. The experiments of Krasnogorsky and Anrep were confirmed by our co-workers, I. S. Rosenthal and D. S. Fursikov.

K. M. Bykov has made highly interesting and remarkable supplements to these experiments. He has not succeeded at the present writing, in spite of persistent attempts, in differentiating two symmetrically placed skin points. It had been previously proved in our laboratory that with mechanical and thermal agents, used as positive and negative conditioned stimuli, the differentiation of various points of the skin on the same side of the body proceeded easily; nevertheless, Bykov found it impossible to obtain the slightest differentiation between two symmetrical points on opposite sides of the body. On one side of the body positive conditioned reflexes were elaborated from the mechanical stimulation of certain points of the skin, which we shall designate by the Arabic figures 1, 2, 3, 4, 5, etc. One of the extreme points (point 1) was differentiated, *i.e.*, its earlier positive effect, which existed thanks to irradiation, was transformed into a negative effect, into inhibition, by repeatedly stimulating it without accompanying it with the unconditioned stimulus (food). The same relations between the corresponding points on the other side of the body were produced spontaneously. We shall represent the points on the other side of the body corresponding to 1, 2, etc., by the Roman numerals I, II, III, IV, V, etc.; thus if point 1 is on the knee of the right leg of the dog, point I will be situated at the same place on the left leg. On this side it came about that point I, like its corresponding point 1 of the other side, became negative (gave no saliva on stimulation), while points II, III, etc., remained positive, like their corresponding points 2, 3, etc. Now we began to differentiate one of the positive points (for example, point III) on the new side of the body, *i.e.*, we repeatedly stimulated this point without accompanying it by feeding. The following came to pass. As this point III gradually changed from positive to negative, *i.e.*, gave a constantly decreasing salivary secretion on stimulation, its corresponding point on the other side of the body (point 3) showed a parallel transformation. If now point 3 were restored to its normal positive condition by combining stimulation of it with feeding, then the positive action of point III also became restored in like manner. And this condition remained, notwithstanding that the symmetrical point III was repeatedly stimulated a hundred times without feeding; it continued to give a positive effect because point 3 continued positive. There was no sign of its becoming negative, *i.e.*, differentiated, and it was obvious that further trials would be unavailing. The same relation held between point 1 and its symmetrical point I on the other side of the body; it was impossible to differentiate them, keeping the one positive and the other negative. How is this enigma to be understood? We know from our own experience as well as from observation on animals how easily and exactly symmetrical points on the two sides of the body can be distinguished.

We have projects for further experiments by which we hope to solve this problem. Probably experiments on animals after destroying the commisural connections between the two hemispheres of the brain will help us toward an explanation.

CHAPTER XXXV

THE LATEST SUCCESSES OF THE OBJECTIVE STUDY OF THE HIGHEST NERVOUS ACTIVITY

(Read at the Anniversary Celebration of the Lesgaft Scientific Institute, Petrograd, December 12, 1923.)

NO NECESSITY FOR AN ANIMAL PSYCHOLOGY—THE ORIGIN OF A PHYSIOLOGY OF THE BRAIN—THE CONDITIONED REFLEX AND THE CEREBRAL HEMISPHERES—SYNTHESIS AND ANALYSIS (BEHAVIOUR) FOUNDED ON EXCITATION AND INHIBITION—IRRADIATION AND CONCENTRATION—EXAMPLES OF IRRADIATED EXCITATION (EMOTION) AND INHIBITION (SLEEP)—LIMITATION BY RECIPROCAL INDUCTION—BEHAVIOUR DEPENDENT UPON A BALANCING OF THE PROCESSES OF INHIBITION AND EXCITATION—THE CONFLICT AND THE FAILURE TO BALANCE MAY EVENTUATE IN A PREDOMINANCE OF EITHER EXCITATION (NEURASTHENIA) OR OF INHIBITION (HYSTERIA)—AN EXPERIMENTAL STUDY OF SENILITY AND OF THYROID INSUFFICIENCY—MEMORY—A CERTAIN CEREBRAL IRRITABILITY IS NECESSARY FOR THE FORMATION OF CONDITIONED REFLEXES—SENILE GARRULITY AND SENILE DEMENTIA—ASSOCIATION AND HIGHER ORDER REFLEXES—THE FUTURE OF BRAIN PHYSIOLOGY.

Why is it that physiology is just beginning to master the secrets of the animal organism? Because the most complicated and important part of the animal, the highest section of the nervous system, the hemispheres of the brain, were, in spite of their great interest, considered beyond the scope of physiology.

Why is this?

The answer is that the rôle of physiology in this domain was contested by psychology—a branch of philosophy which does not even belong to the group of natural sciences. Certainly psychology, in so far as it concerns the subjective state of man, has a natural right to existence; for our subjective world is the first reality with which we are confronted. But though the right of existence of human psychology be granted, there is no reason why we should not question the necessity of an animal psychology. What means indeed have we to enter into the inner world of the animal! What facts give us the basis for speaking of what and how an animal feels? The word "zoöpsychology" is, it seems to me, a misnomer, the result of a misunderstanding. That this is so is exemplified by the following: In a 300-page book by an American author the analogies between the imagined internal worlds of various animals and that of man are discussed, and there constantly recurs the conditional phrase "if they have consciousness". But what kind of discipline is this for a science? Suppose the animal has no consciousness, then all this is only empty babbling, a flow of words.

But though zoöpsychology as a science is to be condemned, the data which zoöpsychologists collect is worth while. These data are derived from a study of the influence of the external world on animals and of their responding reactions. The facts obtained are of course valuable, and will be of service in the future. As long as we have no definite knowledge of the internal world of the animal, zoöpsychology, I repeat, has no right to existence. And all this material must fall to the lot of the physiology of the higher sections of the nervous system, a physiology which, as I have said, has just begun to develop. Only a quarter of a century ago did investigators in Europe and America assume a true scientific attitude in this regard.

Although physiology of the brain had an energetic beginning in 1870, it has not even yet developed, and it remains fragmentary. The obtained facts have borne almost no relation to the manifestation of the highest nervous activity (*behaviour*) of animals. For example, movements of various groups of muscles were seen to result from the stimulation of the corresponding areas of the brain, but what explanation did this give of the highest nervous activity of the animal, how was it to be applied to the reaction of the organism to the outer world?

At last, a quarter of a century ago, a pure physiology of the brain appeared which on the one hand, treated the matter scientifically and objectively, and, on the other hand, considered the behaviour of the animal toward his surroundings. In spite of the newness of this physiology its borders are so extensive that it affords us the possibility of understanding the mechanism of the general conduct of the animal.

The central conception of this physiology is the so-called *conditioned reflex.* Besides the word, conditioned, an adjective describing other of its properties may be used to designate it, such as temporary, individual, etc. The phenomena of the conditioned reflex consist in the following: The basis of the higher nervous activity rests upon the inborn connections of the animal with the external world. A *destructive* stimulus calls out a defence reaction; *food,* a positive reaction—grasping the substance and chewing it. In this group of inborn connections of the animal are included all the reactions which usually are termed *reflexes,* or if they are complicated, *instincts.* Such reflexes are the function of the lower parts of the nervous system.

The cerebral hemispheres, on the other hand, have to do with the formation of the conditioned, *temporary reflexes;* their function is to combine certain external agents, which formerly were isolated, with some physiological activity. All these new unions are formed on the basis of the inborn reflexes. If some agent—which, thanks to an inherited connection, calls forth a definite response—acts on the animal, and if simultaneously with this, a new agent acts, then, after several such coincidences, the new agent begins to call out the same reaction as the original agent did (*i.e.*, the agent calling out the inborn reflexes). Thus, food, for example, is an agent which has an *inborn* connection with a certain reaction of the organism; the dog tries to get to the food, to seize it, and to eat it. There is also an accompanying glandular reaction—saliva and other secretions begin to flow. And now if with this unconditioned agent, food, some other stimulus influences the animal—for example, a picture, sound, odour, etc.—this last stimulus becomes an exciter *per se* of the food reaction. The same law applies to all other unconditioned connections—the defensive reflex, sexual reflex, etc.

Thanks to this fundamental phenomenon of the higher nervous activity, there is a favourable, or even unlimited, opportunity to study the entire activity of the cerebral hemispheres, to investigate the analyses and syntheses which the animal makes of both the external and the internal worlds. However, the whole *behaviour* of the animal is included in this *synthesis* and *analysis*. In order to maintain an equilibrium with the surroundings, it is essential, on the one hand, to analyse as well as to synthesise the external world, because not only simple separate agents act on the animal but also their combinations: and, on the other hand, to analyse and synthesise the corresponding activity of the organism.

The basic processes upon which this synthesis and this analysis are founded, are, on the one hand, the excitatory, and on the other hand, the inhibitory process,—this latter a kind of opposite to the excitatory process. I say "a kind of opposite," because we do not know exactly the nature of either of these processes. We have only hypotheses concerning them, which have not led to definite results. The formation of conditioned reflexes rests upon the process of excitation, but the matter is not settled by this statement. In order to attain to a proper relation of the organism to the surroundings, there is required not only the elaboration of the temporary connections, but also a continual and rapid adjustment of them. For if they do not correspond to reality under the given circumstances, then they are suppressed. And this abolition of the temporary connections comes about as a result of the inhibitory process.

Thus both processes, that of excitation as well as of inhibition, participate in this incessant maintenance of equilibration with the surroundings. And a multitude of reactions of the animal become comprehensible once we are acquainted with the characteristics of the two processes. Arising under the influence of definite stimulations, these two processes move through the mass of the hemispheres with a speed which is measured not only in seconds but also in minutes. At present we have no accurate information of the inter-relations of the velocities of these reciprocal processes. It is possible that the inhibitory process moves the more slowly.

We know that this movement proceeds in two directions. Both excitation and inhibition first spread out over the cortex of the hemispheres—they *irradiate*. During the next phase, they collect around a definite point—they *concentrate*.

The processes of excitation and of inhibition with these their properties condition all the activity of the hemispheres. The chief function—the formation of temporary connections—is based on the ability of the process of excitation *to concentrate*. The mechanism of the elaboration of the conditioned reflexes, the mechanism of association, may be considered to proceed as follows. If a strong stimulation, for example, that proceeding from food, occurs, then all other stimuli falling simultaneously on other parts of the brain, are drawn to this point of intense excitation (food center), *i.e.,* they concentrate here.

The process of inhibition concentrates in a similar way, giving rise to the formation of conditioned inhibitory reflexes.

Irradiation may be seen in a very important manifestation of the higher nervous activity. Let us take some strong stimulation: the resulting excitation spreads far and wide in the hemispheres, and this is expressed in the immediate heightened activity of many functions of the organism. We have such an occurrence in the case of the *emotions.* I recall the instance of a dog which had an intensely developed aggressive reflex toward strangers. He recognised only one master, the experimenter, and guarded only him, reacting to the appearance of every other person in the experimental room by fierce barking. When I myself took the place of the usual experimenter and tried the conditioned food reflexes, I obtained, not a decrease, but an extraordinary reinforcement of these reactions. The food which I offered was eaten with extreme greed. So we are compelled to conclude that the original excitation in the aggressive center irradiated, and also charged the food center.

I shall now show you, on the other hand, a striking case of *irradiation of inhibition.* Detailed investigations have proved that the inhibition which exists always simultaneously with excitation and controls it, is, in essence, the same process as sleep. Sleep is no more than an extreme irradiation of the inhibitory process. In order to prevent sleep, it is necessary to limit the inhibition by opposed stimulations. If the inhibitory process does not meet with resistance (from an excitatory process) it irradiates and overflows the cerebral hemispheres, even passing over to the lower parts of the brain, producing a completely passive state—the *sleeping* state of the animal.

In this way, the reciprocal limitation of both processes gives rise to an enormous *mosaic* in the cerebrum, consisting of excited points and inhibited or temporarily sleeping points. And by the presence of these closely intermingled, now excited, now sleeping points, the entire behaviour of the animal becomes determined. To some stimulations the animal reacts by activity, to others by inhibition.

This delimitation of the processes is greatly favored by another phenomenon, that of *reciprocal induction.* There exists such a relation—an excitation arising in a certain place causes an inhibitory process around this region and owing to this the spread of the original excitation becomes limited. On the other hand, the inhibitory process induces an excitatory process, and this in turn checks the spread of the inhibition. Thus the whole cortical area is partitioned off into excited and inhibited points.

The above is a hasty account of our earlier work. Passing over now to our recent investigations, I feel constrained to say that it is not my personal work, but for the most part that of my collaborators. I have not only made use of the hands of others, but I have also amalgamated our ideas together.

The entire *behaviour* of the animal, as is evident from what I have said, is dependent upon the balancing of the excitatory and the inhibitory processes, and upon the adaptation of these two processes to the various agents of the external world.[1] However, this balancing is no light task for the animal, and is often accomplished at the cost of great fatigue and strain. This can be clearly seen in our laboratory animals.

If I have produced a process of excitation and now limit it with one of inhibition, this is trying on the animal; it begins to whine and bark and attempts to free itself from the stand. The only reason for this is that I have brought about a difficult *balancing* of the processes of excitation and inhibition. Let any one of us consider his own personal life and experiences and he will find many similar examples. If, for example, I am occupied with something,—*i.e.,* I am under the influence of a definite process of excitation—and if some one suddenly proposes to me to do another thing, it is unpleasant for me. For it means that I must inhibit the strong excitatory process in which I was engaged, and only after this can I start a new one. "Perverse" children are classical examples belonging here. You order such a child to do something, *i.e.,* you wish to inhibit the excitatory process present and to start another. And a scene often follows, the child throwing himself on the floor, stamping his feet, etc.

And even more. A stress of such nature, this difficult conflict between the two processes, can, as we have already seen in many of our dogs, produce painful results, *i.e.,* marked disturbances in the normal nervous activity. In all probability these cases explain the genesis of the illnesses which we often see in actual life as a result of very strong processes of excitation or of inhibition, *e.g.,* as when you experience a powerful

[1] See chapter xxviii.—*Translator.*

stimulation, but are compelled by the circumstances of your life to suppress, to inhibit, it. Now there may follow a derangement in the normal activity of the nervous system.

We are now engaged in the more detailed investigation of the above phenomena. The deviations from the normal may be in either one of two directions, depending upon the type of dog; in one, the processes of excitation suffer, in the other, those of inhibition. Dogs in which the latter processes are affected show it very plainly. The animal that was usually quiet becomes extremely nervous and restless. We see from our experiments, moreover, that the inhibitory processes have disappeared; the animal gives the impression of having lost his ability to inhibit. In the conflict between the two processes, we see the excitatory one has taken the upper hand. I recall such an animal that had to be removed from experimental work for three to four months before there was a return of the normal relations. Only then could we, by exercising great care and proceeding gradually, re-establish the process of inhibition.

So much for the deviation from the normal activity toward the predominance of the excitatory process. There are other cases, however, in which the disturbances are characterised by an excess of the processes of inhibition. In these one can see a decrease in general, positive activity of the dog, a tendency toward sleep and inhibition out of all proportion to the circumstances.

If we turn our attention now to human pathology we find here analogous conditions. There are, on the one hand, *neurasthenics,* who are incapable of even weak inhibitions, and, on the other hand, the various kinds of *hysteria,* where inhibition takes the form of anæsthesias, paralyses, increased suggestibility, etc. I now believe that these pathological conditions correspond to the deviations from the normal which we have observed in our experimental animals.

Here, in the investigation of these deviations (in the direction of the preponderance of the inhibitory processes and the weakening of the excitatory) I am constrained to mention one of the discoveries of our distinguished, though deceased, physiologist, N. E. Vvedensky.[2] Vvedensky did much to advance the physiology of the nervous system, and although he succeeded in bringing to light obscure facts, for some reason he has not received recognition in the foreign scientific literature. In one of his books, entitled *Excitation, Inhibition and Narcosis,* he described the changes in the nerve fibres caused by strong stimulation and he distinguished several phases. It now seems that these peculiar phases are entirely reproduced by the nerve cells if there is an intense collision be-

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[2] Compare with next to last paragraph of chapter xxxvi. The pathological states are discussed in detail in the chapters, "Normal and Pathological States," etc., of *Activity of the Cerebral Hemispheres,* I. P. Pavlov, 1926.—*Translator.*

tween the processes of excitation and inhibition. I have no doubt that Vvedensky's researches will finally receive the recognition they merit.

Besides the observations which I have just described, we have recently been able to study the functional changes in the highest parts of the brain in *senility* and during disturbances in the metabolism of the given organism. Simultaneous experiments were carried on by two of our collaborators: in the one case on a very old dog, and in another on a thyroid ectomised animal. It is known that a complete removal of the thyroid gland in man impairs the function of the cerebral hemispheres and causes cretinism.

What happened in our cases? In order to form conditioned reactions we generally use the food reflex. But with this we were totally unable to form a definite temporary connection. Months passed, and still there was no temporary connection. In the old dog there was not the slightest sign of a conditioned food reflex. In the animal that had been deprived of the thyroid, the reflex appeared but only toward the end of every séance, and on the next day we had to begin all over. This indicated that there was a great deficiency in the activity of the hemispheres of the animal.

What is the significance of this? Upon what changes in the brain does it depend? Our conclusion is that in both cases we have to do with a very lowered irritability of the hemispheres. We old people know all this very well; for with increasing years memory [3] for present events falls sharply, and in order to remember a thing we must keep the attention on it for a longer time, and only then is the excitatory process retained. Thus in the case of our dog, the normal cortical activity can be restored by some method which increases the irritability of the hemispheres. Therefore we replaced the food stimulus by a stronger one. As a rule, during the experiment we give only small portions of food, and the main feeding comes only when the work is ended and the dog is returned to his kennel. It seems that the feedings during the séance were too little to produce a sufficient excitation, and we substituted for them the defence reflex by putting acid into the dog's mouth. The motor reaction of the animal had taught us that this reflex was connected with a stronger excitation in the brain. Our supposition proved to be true. After we had thus increased the irritability of the brain, it was possible to form the conditioned acid reflex. An important fact comes to light; in the state of decreased irritability there was an insufficient activity of the hemispheres, but to restore this activity it was only necessary to increase the irritability.

We went still further. Having obtained the conditioned reflex to acid we decided to see how the inhibitory process was affected. We began

[3] For an account of Pavlov's memory, see biographical sketch.—*Translator.*

to elaborate a differentiation, which, as you know, depends upon inhibition.

The conditioned reflex had been formed to a metronome of 100 beats per minute; the differentiation was formed to one of 50 beats. In the other dog we used two tones one octave apart. It turned out that for both animals the elaboration was an impossibility. In the dog without the thyroid the negative conditioned stimulus was applied 600 times, and still there was no differentiation. The old dog died during the course of the experiments, but the other one survived. We come to the conclusion that these animals are incapable of differentiation, *i.e.*, of inhibition. For a normal animal such differentiations are easily accomplished.

Then we thought that perhaps the inhibitory process might be dependent upon the excitatory, and that it was possible that we had not increased the irritability, the tonus of the hemispheres, sufficiently. Thereupon, instead of the acid stimulus we used a strong destructive one, *viz.*, induction shocks of the electric current applied to the skin. A strong reaction ensued, which continued for some time after the cessation of the current. The animal repeatedly jerked up the foot which had been shocked. Now the tone we had used quickly became a *conditioned destructive stimulus*. No sooner was it applied than the animal began to scuffle and whine, etc.

And now the differentiation could be formed easily. If we applied the higher pitched tone without accompanying it by the unconditioned stimulus, the dog readily distinguished it from the lower tones; to the latter he reacted by a violent defence reflex, while to the higher tone there was no reaction.

Thus we had increased the irritability of the brain by the application of electric shocks, and what was impossible for the dog became possible. Evidently there is some kind of relation between the processes of excitation and of inhibition; if the former is diminished, then the latter becomes weaker or even fails altogether.

From this point of view we can understand the *garrulity or dementia of senility*. Whence comes this talkativeness? Normally a person speaks only what is sensible and appropriate. If he commences to talk without any special reason he is obviously unable to restrain himself, to inhibit. Dementia is to be understood in the same way except here the thoughts have no correspondence to reality. In the normal person all such mental processes are suppressed or rejected. In cases where inhibition is seriously interfered with everything comes out together in a confused mass without discrimination.

After these experiments a case which I saw five years ago in the asylum for the insane is now clear to me. There was an old man who had lain like a corpse for twenty years. From the age of thirty-five or forty to sixty he did not make a single movement or speak a word. After the age of sixty he gradually began to make ordinary movements, to speak and to walk. In conversation with him we found that during the whole time he had been completely conscious, seeing, hearing and understanding everything, but he had been unable to respond—to move or speak. Thus for all this time his nervous system, especially the motor region of the cortex, had been inhibited, and only in old age, when these negative processes weakened, did the inhibition at all give way or diminish.

Now you can see some of the facts in the normal and pathological behaviour of the human which become clear from the point of view of this new pure physiology of the nervous system.

I shall add another instructive example. Our ability to understand is based chiefly upon a long chain of excitations, upon *association.* And in our experiments we had to deal with this phenomenon too. It was interesting for us to see whether we could form a new conditioned reflex not with the help of an unconditioned reflex (ordinarily the food reflex), but with the help of another conditioned reflex (although one which had been well elaborated). For example, if we have formed a conditioned food reflex to a metronome of 100 beats per minute, then the sound of the metronome will regularly call out the food reaction; the metronome is a positive conditioned stimulus, *i.e.*, it has always been accompanied by feeding. Should it not be possible to form a new conditioned reflex not directly on the food reflex, but on the metronome of 100 beats unaccompanied by feeding? Such would be a reflex of the *second order,* or a *secondary* reflex. Now it happened, if we applied a new stimulus, for example a slight mechanical irritation of the skin, simultaneously with the formerly used metronome of 100 beats, that the skin stimulation alone finally became able to evoke the food reaction. Furthermore, the following happened: for a long time we were unable to form a reflex of the third order; we could never go beyond a reflex of the second order. Upon what does its formation depend? It appears that you have only to augment the general irritability of the brain, and that then you can form conditioned reflexes of the third order. When we used a stronger unconditioned stimulus (the electric current, a destructive agent) than the unconditioned food stimulus, it was easy to elaborate a *tertiary* conditioned reflex.

This short report about our latest experimental results, will, I think, show you how the behaviour of man and his most complicated reactions to the outer world can be included, analysed, explained by physiology. Along this path of research it is my belief that great achievements and conquests await the human mind. Even at my age I hope to see something of this, but it is beyond doubt that many of the younger generation will be witnesses to these extraordinary attainments.

Such is the value of the objective and scientific method when it attempts to enter into the most complicated of all regions, which until now has been studied only from the subjective point of view.

CHAPTER XXXVI

RELATION BETWEEN EXCITATION AND INHIBITION AND THEIR DELIMITATIONS; EXPERIMENTAL NEUROSES IN DOGS

(Dedicated to the memory of my revered friend, Robert Tigerstedt, to whom physiology owes so much, not only for his investigations, but for his promotion of physiological knowledge and research.)

INTRODUCTION—FIRST LAW OF RELATIONSHIP BETWEEN EXCITATION AND INHIBITION —SECOND LAW (DELIMITATION)—THE CONFLICT, AND THE DESTRUCTION OF NORMAL NERVOUS ACTIVITY—TWO EXPERIMENTAL EXAMPLES (DIFFICULT DIFFERENTIATIONS)— THE RESULTING NERVOUS DISORDER CAUSED BY THE COLLISION OF EXCITATORY AND INHIBITORY PROCESSES—AN EXPERIMENT SHOWING FOUR STAGES IN THE ABNORMAL RELATIONS BETWEEN THE EFFECT OF STRONG AND WEAK STIMULI RESULTING FROM A COLLISION OF EXCITATION AND INHIBITION—NERVOUS DISORDERS WERE PRODUCED IN THE EXPERIMENTAL ANIMALS BY THE LENINGRAD FLOOD—LESIONS OF THE CORTEX FOLLOWING EXTIRPATION—SENILITY—THYROID INSUFFICIENCY—CLASSIFICATION OF THE CORTICAL STATES—EXAMPLES OF PARTIAL SLEEP OR ISOLATED INHIBITION (SLEEP WALKERS, THE MILLER AND THE MILL, ETC., CATALEPSY, SUGGESTIBILITY)—PHYSICO-CHEMISTRY OF THE NERVE FIBRE IS NECESSARY.

ALL the following facts have to do with the functions of the cerebral hemispheres and have been obtained by the method of conditioned reflexes, *i.e.*, reflexes formed during the individual life of the animal. As the significance of conditioned reflexes even now has not become well known and recognised among physiologists, to avoid repetition, I refer the reader to my recent lecture on this subject (see chapters xxxi and xxxii).

By the great difference in facts we were compelled to assume in the work on the cerebral hemispheres two different kinds of inhibition, and we called them *external*[1] and *internal*.[2] The former appears in our conditioned reflex at once; the second develops in time and is gradually elaborated. The first is an exact repetition of inhibition, well known for many years in the physiology of the lower parts of the central nervous system when stimuli, acting on various nervous centres and provoking various nervous activities, meet; the second may relate only to the cerebral hemispheres. Probably, however, the difference between these two inhibitions has to do only with the conditions of their origin, but not necessarily with the process itself. We are still investigating this question. Here I shall speak only of internal inhibition, referring to it, however, without its adjective, simply as inhibition.

[1] See especially chapter xxxii.—*Translator.*

[2] See especially paragraph 7, *et seq.*, chapter xxxii.—*Translator.*

There are two conditions, or better one condition, the presence or absence of which determines whether the impulse brought from the outside into the cells of the cerebral hemispheres will excite in them a process of stimulation or a process of inhibition; in other words, whether this impulse will become positive or negative. This fundamental condition consists in the following: if a stimulus entering into a cerebral cell coincides with some other extensive stimulation of the hemispheres or some lower parts of the brain, then this stimulus is a positive one; under the opposite conditions (*i.e.,* when it acts alone), it becomes sooner or later a negative, an inhibitory stimulus. Relating to this indisputable fact there arose the question, Why is this so? But until now there has been no answer.[3] Therefore we must begin with this fact without having analysed it. Such is the *first basic relation between excitation and inhibition.*

Physiologists have for many years been familiar with the spreading of excitation processes. The study of the higher nervous activity led us to a conclusion concerning the spreading also of the inhibitory process from the point where it first originates. The facts from which this conclusion is drawn are simple and obvious.[4] Now, if from one point an excitatory process spreads, and from another an inhibitory, then they limit each other, each confining the other to a definite space and within definite borders. In this way it is possible to obtain a very delicate functional delimitation of the separate points in the cerebral cortex. If we have to do with such separate points in the cortex which under corresponding conditions are subjected to stimulation, then this result can be easily explained by the plan of the cellular construction. This interpretation meets with difficulties when we have to deal with an excitatory or an inhibitory process corresponding to various intensities or other similar variations (for example the different frequencies of the strokes of a metronome) of one and the same external stimulating agent. In order to explain this according to the simple cellular scheme, it is necessary to presuppose as a point of application of this agent not a single cell, but a group of cells. In every case, as a matter of fact, it is possible to associate with a certain intensity of a known elementary agent the excitatory process, and with another intensity of the same agent the inhibitory one. Therefore, the second general relation between stimulation and inhibition in the cortex is their mutual limitation in space, their *delimitation.* The most evident demonstration of this is obtained in the experiments with mechanical stimulation of various points on the surface of the skin.

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[3] Some suggestions concerning this point will be found in chapter xxxix, paragraph 13.—*Translator.*

[4] See chapter xxi, paragraph 27.—*Translator.*

From this we are forced to presuppose some struggle between two opposing processes, ending normally in a certain equilibrium between them, in a certain balance. This conflict and this balancing are not too easy for the nervous system. We have seen this from the very beginning of our work and we see it constantly even until now. The animal often expresses this difficulty by motor disquietude, by whining and dyspnœa. But in the majority of cases the equilibrium is at last established, each process is allotted its proper place and time, and the animal, after becoming perfectly quiet, reacts to the corresponding stimuli by the process of excitation or of inhibition.

Only under special conditions does this conflict of the two processes lead to destruction of the normal nervous activity and then there originates a pathological state which may last for days, weeks, months, and perhaps even years. This may of itself gradually return to the normal when the experiments are interrupted and the animal is given a considerable rest, but if the conflict is too violent, it can only be removed by definite means, and the animal must be given regular treatment.

These special cases at first arose accidentally and unexpectedly, but afterwards we reproduced them intentionally in order to study them. Their description follows in chronological order.

The first of these cases we came upon many years ago (in the experiments of M. N. Yerofeyeva). The conditioned food reflex was elaborated not from an indifferent agent but from a destructive one, evoking an inborn defensive reflex. The skin was irritated by an electric current and at the same time the dog was fed, although at first the feeding had to be forced. A weak current was applied which was later increased to the maximum. The experiment ended thus: with the strongest current, as well as with burning and mechanical destruction of the skin, there could be provoked only the food reaction (the corresponding motor reaction and the salivary secretion) and there was no trace of any interference by the defensive reaction, there were no changes in breathing or heart beat, characteristic of this last reaction. It is clear that this result was attained by the transference of the external excitation to the food center and that simultaneously with this an inhibition of the centre for defensive reactions must occur. This special conditioned reflex persisted for some months and probably might have remained stable under the given conditions had we not changed them so that the electric irritation was systematically transferred at every excitation to another new point on the skin. And when the number of these points became considerable, then in one of our dogs the condition suddenly changed. Everywhere, beginning with the first location of the skin stimulus and even with the weakest current there was manifested only the strongest defensive reaction, and not a single trace of the food reaction.

By no means were we now able to reproduce the former results. The dog which in the former experiments had been quiet became very excited. In another dog this result came about only when—in addition to the considerable number of places on the skin from which we evoked only the food reaction in spite of the very strong current—during one and the same experiment the irritation was often and quickly moved from one place to another. Both dogs had to be rested for some months, and only in one of them were we able by proceeding slowly and cautiously, to restore the conditioned food reflex to the irritating agent.

The second case of the same sort was observed a little later (experiments of N. R. Shenger-Krestovnikova). A conditioned food reflex as elaborated in a dog to a circle of light projected on a screen in front of it. Differentiation of the circle from an ellipse of the same size and intensity was afterwards tried, *i.e.,* the circle was always accompanied by feeding; the ellipse, never. Differentiation was thus elaborated. The circle called forth the food reaction, but the ellipse remained without effect, which is, as we know, a result of the development of inhibition. The first ellipse applied was markedly different in shape from the circle (the proportion of its axes was as 2:1). Afterwards as the form of the ellipse was brought closer and closer to that of the circle, we obtained more or less quickly an increasingly delicate differentiation. But when we used an ellipse whose two axes were as 9:8, *i.e.,* an ellipse which was nearly circular, all this was changed. We obtained a new delicate differentiation, which always remained imperfect, lasted two or three weeks, and afterwards not only disappeared spontaneously, but caused the loss of all earlier differentiations, including even the less delicate ones. The dog which formerly stood quietly on his bench, now was constantly struggling and howling. It was necessary to elaborate anew all the differentiations and the most unrefined now demanded much more time than at first. On attempting to obtain the final differentiation the old story was repeated, *i.e.,* all the differentiations disappeared and the dog fell again into a state of excitation.

After these observations and experiments we recently undertook the investigation of the described phenomena more systematically and in more detail (experiments of M. K. Petrova). As it is possible to assume from the above facts that the destruction of the normal relations occurred as a result of the collision between the excitatory and the inhibitory processes in certain difficult circumstances, we performed in two dogs of different types—the one very lively, the other inactive and quiet—special experiments with various inhibitory agents and their combinations. Together with the conditioned reflexes which had been retarded for three minutes, *i.e.,* when the unconditioned stimulus was followed by the conditioned only three minutes after the beginning of the latter, as a consequence of which the positive effect appeared only after a preliminary period of inhibition of one to two minutes, there were applied other kinds of inhibition (differentiations, etc.). But this problem was solved by these different nervous systems, although with varying facility, yet without damaging the normal relations. Then we began to elaborate the conditioned food reflex with a destructive agent. Now it was sufficient, having formed this reflex, to repeat it several times on the same spot on the skin, in order that the pathological state might appear. This deviation from the normal proceeded in the two dogs in opposite directions. In the lively dog the elaborated inhibitions suffered to a considerable degree or even disappeared entirely and changed into positively acting stimuli; in the quiet dog, on the contrary, the positive conditioned salivary reflexes markedly decreased in strength and disappeared. And in both cases these changes were very stable, they lasted for months and did not alter or vanish without special treatment. In the lively dog with the weakened inhibitory process, there was in the course of a few days a permanent return to the normal, brought about by means of rectal injections of potassium bromide. It is interesting to observe that together with the appearance of normal inhibition, the strength of the positive conditioned action not only was not decreased, but was even somewhat augmented. On the basis of these experiments we are, therefore, compelled to think not of a decrease of one nervous excitability under the influence of bromide, but of a true regulation of the nervous activity. In another dog we failed to restore a permanent and measurable salivary reflex, in spite of various means which we employed for this purpose.

The following experiments done on a third dog for another purpose, however, gave similar results but more instructive details (experiments of I. P. Razenkov). Many positive conditional reflexes were elaborated on the animal, from various receptors or from the same receptor with varying intensities of one and the same agent. Among others a reflex was obtained to a definite frequency of a mechanical stimulus of a certain place on the skin. After this we began to elaborate differentiations from the same place on the skin, but with a mechanical stimulation of another frequency. This differentiation was attained also without great difficulty, and without noticeable changes in the general nervous activity of the animal. But when immediately after the application of completely inhibited rhythm of the mechanical skin stimulus, there was tried without delay stimulation by a positively acting rhythm, the dog manifested a peculiar disturbance, lasting five weeks, and only gradually disappearing. Restoration to normal was perhaps aided by our special measures. For the first days after the experiments in which there was a collision of the nervous processes, all positive conditioned reflexes disappeared. This state lasted for ten days. Afterwards these reflexes began to reappear, but in a peculiar order; contrary to normal, strong stimuli were without effect, or acted minimally, and considerable effect was obtained only from the weak stimuli. This state continued for fourteen days. Again ensued a special phase. All stimuli now acted equally, and with about the same force as strong stimuli in the normal animal. This extended over a period of seven days. Finally came the last period before the normal, characterised in that the stimuli of moderate strength were greater than in the normal state, strong stimuli were less than in the normal, and weak ones had lost their action entirely. This too lasted for seven days, and then there was a return to normal. With the repetition of the foregoing procedure which evoked the disturbance described above, *i.e.*, the repetition of an immediate transition from the inhibitory-acting mechanical stimulation of the skin to the positive-acting stimulation, there occurred the same disturbance with the same phases, but running a shorter course. With further repetitions the derangements became more and more fleeting, until finally the same application provoked no disturbance. The decrease of the pathological state was expressed not only in the shortening of the duration of the abnormal condition but also in the reduction of the number of phases, and the disappearance of the more abnormal phases.

Thus with the collision of the excitatory and the inhibitory processes, there appears either a predominance of the stimulating process, disturbing the inhibition (it is possible to say, a lingering increase of the tonus of the excitation); or in other cases a predominance of the inhibitory process, with its preliminary phases, disturbing the excitatory process, *i.e.*, an increase in the tonus of the inhibition.

But we have seen the same phenomena under other conditions than the above. Under the action of exceedingly unusual directly inhibiting stimulations affecting the animal, there occurs a chronic predominance of the inhibitory process. We observed this in a high degree in some dogs after the great inundation in Leningrad of September 23, 1924, when our experimental animals were rescued with great difficulty and subjected to quite exceptional conditions.[5] The conditioned reflexes disappeared for some time and only slowly reappeared. For a considerable period after this reappearance each more or less strong stimulus, which was formerly followed by a considerable conditioned effect, as well as the application of an earlier elaborated and even well-

[5] For a detailed account of this case, see chapter xxxix, paragraph 4.—*Translator.*

concentrated inhibition, provoked again this chronic state of inhibition; either as a complete inhibition or as its preliminary phases mentioned above (experiments of A. D. Speransky and V. V. Rikman). In less degree and for a shorter time the same is often observed under more usual conditions, as the removal of a dog to a new environment, or his transference to a new experimenter, etc.

On the other hand, a slight change in the application of a well-elaborated positive conditioned reflex, *viz.*, an unconditioned stimulation following immediately the beginning of the conditioned, so increases the tonus of the stimulation that if the elaborated inhibitions are now tested, they are seen either to have entirely disappeared or to have suffered a great loss in constancy and regularity. Also a frequent interchange now of positive and negative reflexes leads, especially in lively dogs, to a high degree of general excitement (experiments of M. K. Petrova and E. M. Kreps).

The facts given above do not exhaust our material concerning the relation between excitation and inhibition. In the course of our work we have met with many quite peculiar cases of just this very same kind.

It was noticed in many instances that in certain phases of drowsiness in normal dogs there occurred a distortion of the effects of conditioned stimuli. The positive stimuli lost their effect, but the negative became positive (experiments of A. A. Shishlo). In the light of this knowledge, we can understand the frequent fact that in the drowsy state of the animal an apparently spontaneous salivary secretion sets in, which is absent in the waking state. The explanation consists in this—that at the beginning of the elaboration of the conditioned reflexes of a given animal many accessory stimuli, indeed the whole *entourage* of the laboratory, become conditionally connected with the food centre, but later all these accessory stimuli are inhibited, owing to the adaptation to which we subject the conditioned stimuli. In drowsiness these inhibited agents recover, as we are inclined to think, temporarily, their original activity.

The temporary transformation of the elaborated inhibitory stimuli into positive ones is observed also in pathological attractions of the cerebral cortex produced by post-operative cicatrisation, especially during the intervals between convulsions. It is interesting that along with these elaborated inhibitory stimulations during this time only the weakest positive stimuli act, *viz.*, light, whereas all other positive conditioned stimuli of moderate or of considerable strength remain without effect (experiments of I. P. Razenkov). Here belongs the former fact, frequently reproduced by us, that new stimuli provoking one or another reflex of moderate strength during their action convert the conditioned inhibitory reflex into a positive reflex (our so-called inhibition of inhibition—dis-inhibition).[6]

In lesions of the cortex, on the other hand, which follow extirpations, the positive conditioned stimuli belonging to the injured cortex become inhibitory. This I have already mentioned in my foregoing article on sleep. The phenomenon is especially marked in the skin region of the hemisphere, where it has been best studied (earlier experiments of N. I. Krasnogorsky and newer ones of I. P. Razenkov). If the lesion is inconsiderable, the previous positive conditioned mechanical skin stimulus produces an effect less than the normal, and if repeated during the course of one experiment soon becomes inhibitory, *i.e.*, being joined with other effective stimuli it weakens their action, and applied alone produces in the animal a state of sleepiness. If the destruction is more serious, it has, under usual circumstances no positive action, being purely inhibitory, and causes after its application a complete disappearance of all positive conditioned reflexes in other parts of the brain.

But this inhibiting agent, being now inhibitory, can nevertheless, under certain conditions manifest a positive effect. If the animal becomes sleepy, then this stimulus, as well as the elaborated inhibitory agent, as mentioned above, manifests a small positive effect. But this effect can be brought about in the animal by still another procedure. If this stimulus is repeated several times, with only this slight change, that it acts alone for 5 seconds instead of the usual 30 seconds (*i.e.*, if the unconditioned stimulus is added 5 seconds instead of 30 seconds after the beginning of the conditioned), then after displacing it to 30 seconds, we may have a positive effect, which, however, is very unstable. When it appears quickly enough after the beginning of the stimulation, it begins to fall rapidly during the continuation of the stimulation and finally entirely disappears (a markedly weakened excitability). Such a transitory effect may be obtained also by a preliminary injection of caffein, or by many other similar measures (experiments of I. P. Razenkov).

Somewhat different but still related to this theme are the following facts. With a very weak general excitability of the cortex, as is observed in senility of animals (experiments of L. A. Andreyev), and also in animals from which the thyroid has been removed (experiments of A. V. Valkov),[7] or in certain states brought about by convulsions in post-operative scarring of the cortex (experiments of Razenkov), the inhibitory process is either much weakened or becomes impossible. In these cases only by an increase of the irritability of the cortex by

[6] Discussed in detail in chapter xi, paragraph 22, and in chapter xxii.—*Translator.*
[7] See chapter xxxv, paragraph 27.—*Translator.*

the application of stronger unconditioned stimuli can we sometimes provoke an inhibitory process.

Here belongs the phenomenon of reciprocal induction, which I have mentioned in the previous chapters (experiments of D. S. Fursikov, V. V. Stroganov, E. M. Kreps, M. P. Kalmikov, *et al.*).[8] And finally the last fact consists in the following: if by a corresponding procedure, separate points of the cortex are reinforced for a long time, some always as points of excitation, some others always as points of inhibition, then they gradually become highly constant in their effects, and stubbornly resist the influence of the opposite processes. Extraordinary means are sometimes required in order to bring about a change of their function (experiments of B. N. Bierman, U. P. Frolov).

All these facts permit us, I think, tc classify the various conditions to which the cortex is subjected in a certain consecutive order. At one pole of this system stands the state of excitation, an extraordinary increase of irritability, when an inhibitory process becomes very difficult or impossible. After this is the normal waking state, the state of normal equilibrium between the processes of excitation and of inhibition. Then follows a long but also consecutive series of states transitory to the inhibition, of which the following are the most characteristic: a state of equalisation, when in contradistinction to the waking state all stimuli, independent of their intensities, act exactly equally; the *paradoxical phase* when only weak stimuli act and when strong stimuli either have no action at all or have a barely noticeable effect; and finally the *ultra-paradoxical phase,* during which only the previously elaborated inhibitory agents have a positive effect. After this follows a state of complete inhibition. The explanation is not yet clear of that other state in which the excitability itself is so low that inhibition in general becomes very difficult or impossible, just as in the case of the state of extreme excitation.

At present we are occupied among other things with the experimental decision of the question: In all cases of normal transition from the active state to that of inhibition as in the state following sleep or in the process of elaboration of negative conditioned reflexes, etc., are there to be found transitory states which are so sharply expressed in pathological cases? Already we have some clues to the answer to this question. If this should prove to be so, then only as a prolongation of the transition from one state to another, a certain isolation and fixation of those transitory states which normally change quickly or almost imperceptibly, can be considered as pathological.[9]

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[8] See chapter xxxiii, paragraph 12.—*Translator.*

[9] Detailed discussion of the recent work relating to this will be found in chapter xxxviii.—*Translator.*

The above facts open the way to the comprehension of many phenomena of both the normal and pathological activity of the higher nervous system. I shall give some examples. In foregoing articles I have shown how normal conduct is based on the elaborated delimitation of the points of excitation and of inhibition, on that magnificent mosaic in the cortex, and how sleep is to be considered as irradiated inhibition. Now we may add some details showing how it is easy to understand certain variations of normal sleep as well as separate symptoms of hypnosis when they are considered as different degrees of extensiveness and intensity of the inhibitory process.

Cases of sleep while walking or riding horseback are well known. In other words inhibition is limited to the confines of the cerebral hemispheres, and does not spread below over the subcortical centers established by Magnus. Further we know of sleep with partial waking in relation to special stimuli, although they may be feeble: the sleep of the miller who wakes when the noise of the mill ceases; the sleep of the mother who awakes on the faintest sound coming from her ill child, but whose rest is not disturbed by much stronger sounds from other sources, *i.e.*, sleep in which easily excitable points stand on guard.

Catalepsy in hypnosis is evidently an isolated inhibition of only the motor regions of the cortex, not spreading to the centers of equilibrium, and leaving free the remaining parts of the cortex.[10] Suggestion in hypnosis can also be considered as such a phase of inhibition, in which weak conditioned stimulation (words) acts more effectively than stronger direct external stimuli. The symptom established by Pierre Janet of the loss of the sense of reality during many years of sleep can be explained as a chronic inhibition of the cortex interrupted only for short intervals, and especially in the presence of feeble stimuli (usually at night), this inhibition concerning particularly the skin and motor regions, which are the most important for the influence of reality on the organism and for the action of the organism on the outer world. Senile talkativeness and dementia too find a simple explanation in an extraordinary weakening of inhibition resulting from the feeble excitability of the cortex. Finally our experiments on dogs give us the right to consider those changes which we produce—the chronic deviations of the higher nervous activity from the normal—as pure neuroses, and some light can be thrown on the mechanism of their origin. In this way the action of an exceedingly strong and unusual stimulation (for example the flood of 1924) on dogs with a weak nervous system, having a predominant inhibitory process, in other words, on a central nervous system with an increased tonus of inhibition, reproduces the etiology of a special traumatic neurosis.

[10] For details of this statement see chapter xxix.—*Translator.*

It is obvious that the time has not yet come for a theory to explain all the enumerated phenomena and to assign them a common basis, although many hypotheses have been proposed, each one of which has a certain justification. In the present situation, one may use various conceptions in the work if only they permit a systematisation of the material, and suggest new and detailed problems.

Thus in our experiments we think of different phases which develop under special conditions in the cortical cells, from extreme excitation to the deepest inhibition, and which depend upon the intensity and duration of the corresponding stimulation and upon the conditions under which this stimulation occurs. The manifest analogy between the changes which we have observed in the function of the cerebral cortex and in the changes of the nerve fibre inclines us to this view. The latter changes occur under the influence of various strong influences; they are aptly described by N. E. Vvedensky, in his well known work, *Excitation, Inhibition, and Narcosis.* We do not agree with his theory, but we have grounds on which to refer all the observed transitions from excitation to inhibition to one and the same elements, the nerve cells, as Vvedensky has rightly done in the case of the nerve fibre.

One can hardly deny that only a study of the physico-chemical processes taking place in nerve tissue will give us a real theory of all nervous phenomena, and that the phases of this process will provide us with a full explanation of all the external manifestations of nervous activity, their consecutiveness and their interrelations.

CHAPTER XXXVII

EFFECT OF INTERRUPTING THE EXPERIMENTATION IN DOGS WITH CONDITIONED REFLEXES

(From the volume dedicated to Charles Richet, May 22, 1926.)

DESCRIPTION OF FIVE CASES.

An interruption of two months in experiments with several dogs having conditioned reflexes gave us an opportunity to observe peculiar changes in their behaviour.

Dog No. 1.—This very active animal, when experimentation with it was begun after an interruption, showed a hyper-excitability, continually wagging his tail and reacting very briskly to the slightest movement of the experimenter.

The conditioned food reflexes, however, disappeared and the animal would not take the proffered bread mixture.*

After several ineffectual attempts to provoke the conditioned reactions, we resorted to a differentiation ** which was well elaborated, *i.e.*, it represented a strong inhibitory process. After this the vanished reflexes reappeared, and the dog took the food.

The explanation of these phenomena is based on the fact that the dog possessed a pronounced reflex of freedom.[1] By the systematic use of conditioned and unconditioned stimuli for the food reflex, before the experimentation was interrupted, the reflex of freedom had been inhibited. During the interruption of the work the conditioned food reflexes were weakened, and so the reflex of freedom, formerly present, became strengthened and prepotent. But it has been established in our laboratory that internal inhibition, owing to induction, strengthens the excitation process. By applying the strong internal differential inhibition, the action of all of the positive conditioned food reflexes was reinforced; consequently the reflex of freedom vanished.

Dog No. 2.—This animal was very strong and excitable. There had been established, although with much difficulty, a differentiation of two frequencies of pricking the skin (mechanical stimulus), one frequency

* This consists of a mixture of one part of meat powder and three parts of black bread crumbs. The meat powder is prepared from finely ground roast beef (previously dried), and the mixture is moistened before feeding.

** For this one uses two stimuli, one of which is always accompanied by feeding (positive), and the other of which is never associated with food (negative). In the latter case the salivary reaction is inhibited.

[1] See chapter xxvi.—*Translator.*

was a positive stimulus, and the other frequency a negative stimulus. Then during an experiment, the negative pricking stimulus was immediately followed by the positive pricking stimulus. This change proved to be too great a strain for the nervous system of our animal. The differentiation began to weaken, and continued to weaken inversely to our efforts to maintain it. Finally not only the mechanical excitation of the skin, but also all the surroundings became conditioned stimuli of this painful state of the animal's nervous system. All other conditioned reflexes became weak or unstable, or even disappeared completely. The dog refused all food, was uneasy during the experiment, and strove to free himself from the stand.

After the interruption, the conditioned reflexes from pricking the skin disappeared, but those from visual and auditory stimuli became reestablished and were augmented. A single application, however, of the positive pricking of the skin was sufficient to throw the animal into the state which existed before the interruption of the work.

Dog No. 3.—This animal had a very strong passive defensive reflex, and it was therefore extremely timid.

After the interruption of the work the conditioned reflexes, although they were old and well formed, disappeared, and the animal, in spite of his voracity, refused food. One may suppose that the environment of the laboratory, having ceased to be familiar for this dog after two months' interruption of the experiments, had become, consequently, inhibitory. The presence, during the course of the séances, of the familiar experimenter close to the dog (ordinarily the animals and the experimenter are separated—in two rooms—during the séance), and the application of the differentiation (as in Dog No. 1) permitted the reestablishment of the conditioned reflexes little by little; and gradually the dog commenced to take his food.

Thus the social excitant (the experimenter) and the stimulation induced by the internal inhibition of differentiation overcame the external inhibition produced by the surroundings.

Dog No. 4.—This was a sluggish animal which became quickly drowsy during the employment of weak conditioned stimuli. By appropriate measures we succeeded in overcoming the state of somnolence, and the conditioned reflexes became perfectly regular.

After the interruption of the experiments all these reflexes disappeared entirely and the dog refused the feedings.

The somnolent state, it is evident, reappeared owing to the inhibitory, hypnotising surroundings of the laboratory and the application of weak conditioned stimuli. From numerous experiments it appeared that after the interruption, the positive conditioned reflexes were more affected than the negative ones.

When, by the means described above, we had again banished the drowsy state of the dog, the positive reflexes were quickly restored and the dog greedily ate the proffered food.

Dog No. 5.—This dog had a great number of positive and negative (inhibitory) conditioned reflexes which functioned regularly.

After the interruption, they were tried anew. None of them had diminished in strength. But the next day, during the experiment, the animal became very restless and dyspnœic and without any apparent external reason the positive reflexes were weakened.

Evidently the great nervous strain imposed upon this dog by the first séance after the interruption, undertaken without any preliminary training, provoked this painful state of his nervous system. This condition continued, and that constitutes an indirect proof of our hypothesis.

These observations show that the presence or the absence of such and such conditioned or unconditioned factors determines exactly and continuously the behaviour of the animal. In connection with these conditions, there were observed certain modifications of the general tonus of the nervous system, now in the direction of excitation, now in the direction of inhibition, and, on the other hand, there is a like transformation in the relations among the special reactions to the surroundings.

In other words, these facts argue in favour of the conception of a strict and absolute determinism operating in the higher nervous activity of the animal.

CHAPTER XXXVIII

NORMAL AND PATHOLOGICAL STATES OF THE HEMISPHERES

(Read at the Sorbonne in Paris, December, 1925, on the occasion of the conferring of an honorary degree on Prof. Pavlov by the University of Paris.)

DIFFERENCE BETWEEN UNCONDITIONED AND CONDITIONED REFLEXES—THE TWO FUNCTIONS OF THE CEREBRAL CORTEX—POSITIVE AND NEGATIVE CONDITIONED STIMULI—THE BRAIN AS A MOSAIC—TRANSITORY PHASES BETWEEN THE WAKING AND SLEEPING STATES—TWO EXPERIMENTAL EXAMPLES—THE PARADOXICAL PHASE AND HYPNOSIS—ANOTHER CONFIRMATION OF THE IDENTICAL NATURE OF SLEEP AND INHIBITION—TWO EXPERIMENTS SHOWING A CHANGE IN THE INHIBITORY PROCESS (ONE WITH AN ELECTRIC CURRENT AS THE CONDITIONED STIMULUS; THE OTHER WITH DIFFICULT DIFFERENTIATION)—RECOVERY WITH REST AND BROMIDE—OTHER EXPERIMENTS SHOWING PREDOMINANCE OF THE INHIBITORY PROCESS—A DIFFICULT COLLISION IS THE CAUSE OF THE PATHOLOGICAL DISTURBANCE—HYPOTHESES CONCERNING THE SEAT OF FORMATION OF CONDITIONED REFLEXES AND THE NATURE OF EXCITATION AND INHIBITION—FUNCTIONAL SENSITIVENESS OF THE CORTICAL CELLS; ILLUSTRATED BY THE RUSSIAN FAMINE—TWO TYPES OF NERVOUS SYSTEMS.

I HAVE the high honour to direct your kind attention to the recent results of the investigations in my laboratory. This report of research will, I trust, stimulate considerable interest. The experiments have been carried out on animals, *viz.*, on the dog, the friend of man since prehistoric times. For twenty-five years now we have made researches on this animal in order to understand the entire higher nervous activity of his organism from a purely physiological point of view, discarding all the while psychological explanations and terms.

The chief organ of this higher nervous activity is, of course, the cerebral hemispheres.

The important phenomenon in the activity of the cerebrum, about which centres all our experimental material, is what I call the *conditioned reflex.* The conception of reflexes in physiology, the gift of Descartes' genius is a purely scientific comprehension. It is now sufficiently established, we may say, that so-called instincts[1] are the same phenomena as reflexes, though often of a more complex kind. It is preferable, therefore to use the one term reflex for all these constant and regular reactions of the organism, and for the sake of precision we add to this word the adjective unconditioned or conditioned.

Let us take one of these unconditioned reflexes, a most common one, of daily occurrence, the food reflex. A definite motor and secretory reac-

[1] See chapter xxvii, first paragraph and footnote 1, for a discussion of the relation between instinct and reflexes.—*Translator.*

tion to food as a stimulus occurs when it is placed before the dog or when it gets into his mouth. If a few seconds before the food is in the mouth of the dog, there acts, for example, on his ear, the sound of a metronome, and if such coincidence takes place one or more times, then the metronome will call out the same reaction as the food, *i.e.*, there will appear the same movements and the same salivary and other digestive secretions. This new food reaction can become as exact as if the food actually were in the mouth, and it may exist for an indefinitely long time.

These reactions are what I call *conditioned reflexes.*

Why should this not be considered a reflex? The mechanism is the same—a definite external agent, the conduction of the impulse along a certain afferent nerve, and a central connection with special efferent nerves to the muscles and glands. The *difference* is not in the mechanism of the reaction but in its perfection. The mechanism of the unconditioned reflex is ready in all its parts from the day of birth. The conditioned reflex is elaborated during the course of the individual's existence in one region of the central nervous system, *viz.*, in the cerebral hemispheres; for with their removal the conditioned reflexes disappear from the activity of the nervous system. As in the normal animal this elaboration of the reflex mechanism arises inevitably under definite physiological conditions, there is absolutely no basis for considering it other than physiologically. The establishment of the mechanism in the conditioned reflex consists evidently in a *coupling,* in the formation of a functional union in the path in which the excitatory process moves. And now we possess the facts which permit us to consider this act of coupling as a physiological process and even as an elementary one.

Conditioned reflexes may form on every conceivable agent of the surroundings if only there exists for them a receptor apparatus in the given animal, and they may form on any of the unconditioned reflexes. Their biological significance is enormous, for they alone make possible the establishment of the most delicate and exact equilibrium between the complex organism and the surroundings. The innumerable conditionally acting agents signal, as it were, for a few, near agents, directly favourable or harmful to the organism. Even very fine and distant conditioned stimuli acting on the eye, ear or other receptor organs can provoke the movement of the animal—on the one hand, *toward* food, the opposite sex, etc., or, on the other hand, *away from* all harmful and destructive agents.

From such a point of view the physiological rôle of the cerebral cortex is either (1) a connecting, combining or *coupling* function (according to the mechanism), or (2) a *signalling* function (according to its significance). And the signallisation is adaptable in strict correspondence with the external agents.

Just a few words here about our method. For the formation of conditioned reflexes we employ two unconditioned reflexes almost exclusively: the *food* reflex and the *defensive* reflex (the latter is seen upon introducing into the mouth of the animal not food but substances which the dog rejects, such as a weak solution of acid). We register not the motor component of the conditioned reflexes but the secretory, the secretion of saliva; for the reaction is easier to measure in this way.

The above described reaction is a *positive conditioned* reflex; for the conditioned stimulus calls forth in the cortex of the cerebral hemispheres a process of excitation. But along with the positive there exist *negative*, inhibitory conditioned reflexes when the conditionally acting agent calls out a process not of excitation but of inhibition. We elaborate, for example, a positive conditioned reflex to a tone of 1000 vibrations per second. Now if we try other tones we find they also have a positive conditioned action. If, however, we repeat them without supporting them by the unconditioned stimulus (food or acid), then they gradually not only lose their positive effect, but become inhibitory agents. Their inhibitory nature can be clearly demonstrated, for shortly after their application, during the next minute or even many minutes, the positively acting tones are weakened or completely inactive.

At the present time our investigations of conditioned reflexes have proceeded so far as to permit me to discuss them more or less fully. In addition to the foregoing necessary introduction, I must mention briefly two or three other details before discussing the special subject of my communication.

Both the process of excitation and that of inhibition execute a certain movement over the cerebral cortex, at first *irradiating* more or less widely from the point of origin, and then *concentrating* around this point. With the concentration of these processes there occurs a very fine definite localisation of them, and owing to this the whole cortex becomes reduced to a huge *mosaic* of points of excitation and of inhibition closely intermingled.

This mosaic is formed and reinforced partly by the reciprocal crowding in of the opposed processes of excitation and of inhibition, directly called out by the corresponding *external* agents; partly, however, by *internal* relations, in particular by *reciprocal induction*, when one process leads to the strengthening of the other.

In a recent lecture (chapter xxxii) I reported a long series of experiments, proving, I think beyond doubt, that sleep is the same inhibition which co-exists side by side with the process of excitation during the waking state, not delimited, however, but continuous and irradiated not only over both hemispheres, but over the neighbouring and lower parts of the brain.

Recently we have investigated the *transitory phases* between the waking and the sleeping state. In our experiments the dog is placed in the stand in such a way that his movements are restricted and he is alone in the experimental chamber, isolated even from the experimenter. Under the influence of these conditions, together with the definite character of the applied stimuli there developed in the central nervous system of the animal a special state, tending, so to speak, toward sleep. Partly owing to the individual peculiarities of the nervous systems of different dogs, partly as a consequence of some special procedure in our experiments, we are able to observe and investigate certain fixed, as it were, stages in the transition from the waking state to that of complete sleep. Of such phases we can clearly recognise several.

Here I shall consider only two of them.

When the conditioned reflexes are formed from different external agents with the help of one and the same unconditioned stimulus, then the effects received show a wide quantitative variation in spite of the definite elaboration of all reflexes. To the ordinary thermal and mechanical stimuli of the skin, as well as to light stimuli, the conditioned reflexes are usually smaller than are those to sound stimuli. As we have been taught by our recent experiments, this depends upon the absolute energy of each stimulus—the greater the energy of the stimulus the greater is its effect. In a special phase during the transition from the waking state in the direction toward sleep this normal relation of the effects disappears. Either all the effects become equal (the equality phase), or the relation becomes reversed, so that the effects from weak stimuli are greater than those from the strong, or the latter may even remain without any effect (*paradoxical* phase).

Here are examples. A dog formerly showing differences in the size of the conditioned reflexes corresponding to stimuli of different strength falls, in the course of prolonged experiments, into a barely perceptibly drowsy state and the conditioned reflexes, compared by the size of their effects, all become equal. It was only necessary to give a small subcutaneous injection of caffein in order to have the animal fully awake; and immediately thereafter all the reflexes, measured by the size of their effects, returned to the proper order.

In another dog, always remaining wide awake during the experiments, we often repeated and continued the application of the inhibitory stimuli for a considerable time. Thus we brought him into the drowsy state. Trying now a weak positive conditioned stimulus we found it inactive, and after this we gave the dog a little food. This, of course, lessened to a certain degree the sleeping state. On repeating the conditioned stimulus a second time we had a slight action from it. We fed the dog again. For the third time the conditioned reflex to that stimulus attained its usual size or even exceeded it. The conditioned stimulus was now accompanied by food. In a further application one of the strong conditioned stimuli showed a smaller effect than the previously applied weak stimulus. With the continuation of the experiment in this manner, we finally succeeded in establishing the normal relation between the stimuli, corresponding, as in the former case, to their strength.

It is evident that the excitation produced by repeated acts of eating gradually overcame the sleepy inhibitory state of the cerebrum which we had brought about in the beginning of the experiment and from which state it passed by the same consecutive stages back again into the fully waking state.

Here is another instance. This is a dog in which it was possible to elaborate quickly many reflexes to agents of varying strengths. In the elaboration of one more new reflex (to a weak stimulus) the stimulus was applied several times in succession in each experiment and also for several consecutive days. This led to a marked change in the general state of the animal. He gradually became less restless on the stand and stolidly remained in a given pose as if he were cut out of marble: and at the same time, of the formerly elaborated stimuli only the action of the weak ones was preserved. With weak stimuli the full secretory effect was received for the whole period of their action, and the dog began now to eat the food immediately upon its presentation. With strong stimuli only at the very beginning of their action did there flow even a small quantity of saliva, but this secretion diminished and the dog did not turn toward the offered food. If we entered the room and stimulated the dog in some way, by stroking him, calling him by name, etc., then after this all the conditioned reflexes became re-established, and the stimuli, compared by the strength of their action, had their normal effect. When, on the contrary, the dog for several days received no special stimulation on the stand, all the conditioned reflexes finally disappeared, and the animal did not take the food given him. But you had only to free the animal from the stand and to put him on the floor and he fell to eating greedily.

Almost without doubt in the experiments described we have to do with a special *hypnotic* phase. I think that this paradoxical phase is the actual analogy of a specially interesting phase of hypnosis in man, the phase of *suggestion,* in which the strong stimuli of the real world give way to weak stimuli, the words of the hypnotiser. The paradoxical phase explains many cases of *abnormal sleep,* interrupted or continued. It lasts sometimes for years. During this time the subject returns only for short intervals to the waking state, and then especially in the absence of strong stimuli, which always occur in the day. Therefore the waking intervals come oftener during the night. The case of five years' sleep observed by Pierre Janet and that of twenty years' sleep seen in St. Petersburg are examples.[2]

Thus the *transitory* phases between the waking state and sleep appear as different degrees of extensiveness and intensiveness of the inhibitory process in the hemispheres. The so-called *animal hypnotism* (known for a long time) is a real hypnosis, one of the transitory phases between the waking state and sleep, an inhibition affecting chiefly the motor area of the cortex, by virtue of the peculiarities of the procedures by which it is produced. *Catalepsy* which sets in during hypnosis evidently arises owing to the manifestation of the activity of the equilibrating centres of the brain (discovered by Magnus and Klejn), now freed from the masking influence of the motor area of the cortex. Our experiments showed that various transitory phases and sleep can be produced from weak as well as from strong stimuli, and also from unusual ones. Thus the waking state is established in general by an average strength of customary stimuli, which is especially the case for some types of nervous system.

Especially interesting is the fact that we were able to observe the paradoxical phase in other states in addition to those described above in the dog. After each single application of the inhibitory conditioned stimulus, particularly soon after the reflex had been elaborated, there was seen a long successive period of inhibition over the whole of the hemispheres. And during this period too, we could detect the characteristic paradoxical phase. Our former conclusion that sleep and inhibition are one and the same process received here another confirmation.

Let us pass over to the other series of experiments. At first we met accidentally, but later we were able to produce voluntarily, pathological functional changes in the dogs' nervous systems analogous to neuroses in the human.

In two dogs the conditioned reflexes were gradually formed not from indifferent stimuli but from a very strong electric current applied to the skin. With this shock there was no howling nor exhibition of the defensive reflex whatever, but the animals, on the contrary, turned toward the place from which they were accustomed to receive food, licked their lips, etc., in short, they responded by a lively food reaction and there was an abundant flow of saliva. The electric current could be replaced by cauterisation and wounding of the skin, but the effect remained the same.

This conditioned reflex continued for a long time without change. Then we began to move the site of the stimulation, *i.e.*, to apply the current every time to a new place on the skin. For a long time the effect remained unaltered. But in one dog, when a certain number of spots

[2] See chapter xxix.—*Translator.*

had been tried, the results underwent a sudden and radical change. The conditioned food reflex to the electric current disappeared completely, and now a much weaker current even when applied to the first elaborated spot called out only the strongest defensive reaction. In another dog the mere transference to a new skin spot did not bring about the same result; but when in one and the same experiment we stimulated these different places consecutively there was exactly the same effect as seen in the first dog. Both the animals became very excited and restless. It was necessary to let them go three months without any experimentation, and even then it was possible in only one of the dogs and by beginning very slowly and with a weak current, to re-form the old reflex. In the other dog we did not succeed in doing this.

In the next dog a conditioned reflex was formed on the projection of an illuminated circle on a screen close in front of the animal. Then an ellipse was differentiated from the circle. In the beginning it also gave a positive conditioned action, but on repetition without support by feeding it became an inhibitory agent. This first differentiated ellipse was in the same position and received the same illumination as the circle but its shape was very different (elongated). Later there were successively differentiated ellipses, each one more nearly circular. These new differentiations were well elaborated and stable. When, however, one of the most nearly circular ellipses, closest in form to the original circle, was now tried, there began a reversal; the differentiation (absence of effect owing to differential inhibition), which after some few trials had become effective, when repeated again and again failed to maintain its inhibitory character, *i.e.*, this last ellipse began anew to have a positive action, which increased with every application though it was never accompanied by feeding. And at the same time so did all the earlier and less delicately elaborated though more stable differentiations. Now we had to do the elaboration of the ellipses all over again, and to operate more carefully and slowly than the first time, beginning with the ellipse whose form was farthest removed from that of the circle. On the application of the most nearly circular ellipse which could be differentiated, the same history recurred. And in this dog too there was after the experiments a marked alteration in the general conduct; he was no longer gentle and docile, but became very excited and irritable (nervous).

It was evident that in both cases,—in the experiments with the conditioned reflexes to electric shock as well as in those with the circle and differentiated ellipses,—it was the inhibitory process which suffered permanently. In the first case, in order that the food reaction should be connected with the electric shock, it was first necessary that the defensive reflex to the current be inhibited. In the second case, as has been shown above, the differentiation was also based on the inhibitory process.

These observations were begun long ago in our work, and they remained as detached and unused facts. We could not make heads or tails out of them. Only in recent years have we been able to work out special themes with them on a larger scale and thus to fathom their relations.

In many dogs we have obtained the same results.

Under the influence of similar circumstances their nervous systems markedly lose their inhibitory functions. From a large number of different cases of inhibition there remained intact only a few of the most stable ones, and even in these there were defects. The pathological condition lasted sometimes for months and often remained stationary. It is interesting to mention that in some cases a marked improvement was quickly brought about by several daily injections of bromide.

But in other dogs, evidently of another nervous type (*see* preceding chapter), we got entirely different results. In them our experiments caused a *predominance* of the *inhibitory* process. The positive conditioned reflexes either entirely disappeared or exhibited the peculiar properties of the above described transitory phases between the waking and the sleeping state.

Here is such an experiment. There was elaborated a conditioned reflex to a rhythmical, mechanical irritation (having a definite frequency) of the skin. From this conditioned stimulus there was differentiated a stimulus almost identical with it except in the frequency of the rhythm. One frequency was made a positive conditioned stimulus and the other a negative one (a conditioned inhibitor as a differentiated stimulus). When both these reflexes had become stable, the positive conditioned stimulus was applied immediately, *i.e.*, without any interval, after the action of the conditioned inhibitor, the differentiated stimulus; in other words, one frequency of the mechanical stimulus of the skin was replaced by the other. This led to a striking pathological state of the nervous system, which only after many weeks, and then aided perhaps by some of our special procedures, returned to normal. We observed the animal every day without exception. The trouble began by the disappearance of all the conditioned reflexes. Then we observed them returning by and by, passing through those definite phases already described: but the interesting point is that they remained in each phase for several days, in some of them up to ten. Among the other phases there again appeared, and especially characteristically, the paradoxical phase and the phase of equality.

Thus there occurred in the pathological cases the same nervous phenomena as we observed in the normal. But in the latter they lasted only a short while, whereas there they were constant. This refers to the predominance of the positive (excitatory) as well as to that of the inhibitory processes.

What are the general characteristics of all the pathological instances? Upon what depends the persistent and marked deviation from the normal brought about by our procedures? We have the right to answer, I believe, that it is a *difficult collision, an unusual confronting of the two opposing processes of excitation and inhibition (be it in time or intensity relations or even in both together)*, which leads to a more or less permanent destruction of the normal balance existing between these two processes.

It is necessary to add, however, that some of our procedures by which we brought about the pathological states did not prove effective in all dogs. There are some animals which undergo them well, without damage. We can not extend this statement to the applications of the electric shocks as conditioned stimuli, as such experiments have been too few. We can describe all the above facts as characteristic of the physiological work of the cerebral hemispheres, according to the following hypothesis, which gives us a scheme for conducting further experiments. The coupling, the elaboration of new unions (new paths for the nervous processes in the hemispheres), we attribute to the function of the intercellular membrane, if it exists, or simply to the fine ramifications between the neurones, between the separate nerve cells. The fluctuation of the excitability and its transformation into inhibition may be attributed to the very cells themselves. This distribution of function appears to us as a probable explanation in the light of the fact that when the new connections are well elaborated they are preserved for a long period, whilst the alteration of the excitability, the change into inhibition, is a vacillating phenomenon. The processes of excitation and inhibition appear to us to be different phases in the activity of the cortical cells of the cerebrum. We are compelled to assert that these cells have a high degree of reactivity and, consequently, sensitiveness and destructibility.

This quick functional sensitiveness is the chief impulse to the origin in the cells of a special process of inhibition—an economical process, which not only limits a further extreme functional destruction, but also assists in the restoration of the depleted excitatory substances.

Thus do we explain the most constant and striking fact with which we have to deal in the work with conditioned reflexes. The fact itself is as follows: If the conditioned stimulus is applied even some ten seconds not supported by the unconditioned reflex, there inevitably sets in sooner or later, but in all cases, and in some dogs with astonishing rapidity, a state of inhibition in the cell; and beyond the cell the state spreads over the cortex and even into some of the lower-lying centres of the brain until complete sleep will ensue. That the prompt addition of the unconditioned stimulus near the beginning of the action of the conditioned stimulus prevents this occurrence is not a contradiction of our explanation of the facts. Our recent experiments show that during the action of the unconditioned stimulus the positive conditioned stimulus loses its effectiveness, becomes inhibited. The most wide-awake signal plays its responsible rôle,—and then when its part is finished and its work is no more needed, its rest is thoroughly secured and cautiously guarded.

The value of the excitatory substance of the cortical cells and the limited nature of it is clearly shown by the following experiments. A few years ago when we suffered a great food shortage our experimental animals, too, were, of course, in a starving condition. With such it was almost impossible to perform researches on conditioned reflexes. A positive conditioned stimulus, in spite of all our corrective measures, rapidly passed over into an inhibitory one. All the researches converged to one theme—the effect of the famine on conditioned reflexes. It is necessary to add that this tendency of the stimuli to go over into the state of inhibition has been manifested not only by the conditioned food reflexes but also in the same degree by the conditioned reflex to acid. This fact once more speaks for the great delicacy of the method of the conditioned reflexes in the physiological study of the cerebral hemispheres.

In the light of the facts just discussed one can readily understand the existence of different types of nervous systems which we have encountered in our dogs. Evidently it is the same with our own nervous systems. It is easy to find nervous systems which, from the day of birth, or in consequence of trying tasks during the individual's life, possess only a small store of excitatory substances in the cortical cells, and therefore easily pass over into the inhibitory state, in some one of its phases. They can, indeed, persist in one of these phases.

I have finished, and I should be very happy if any of my highly esteemed listeners would come to me for explanations or with objections. Such a vast and complex subject can hardly be discussed satisfactorily in so short a time.

CHAPTER XXXIX

THE INHIBITORY TYPE OF NERVOUS SYSTEMS IN THE DOG

(Read before the Psychological Society, Paris, December, 1925, on the occasion of the election of Prof. Pavlov to honorary membership.)

DESCRIPTION OF AN INHIBITORY TYPE OF DOG—EXPERIMENTAL ANALYSIS OF THE EFFECT OF THE LENINGRAD FLOOD ON THIS DOG—PASSIVE DEFENSIVE REFLEX (FEAR) APPEARS—INTERPRETATION OF THE EXPERIMENT—STIMULATORY SUBSTANCES AND FATIGUE—RESEMBLANCE BETWEEN THE PASSIVE DEFENSIVE REFLEX AND INHIBITION—PHOBIAS—ANALOGY OF TEMPERAMENTS (SANGUINE, EQUILIBRATED, MELANCHOLIC) TO PAVLOV'S TYPES OF DOGS—DISCOVERY OF THE SOCIAL REFLEX.

I WISH to express my sincere thanks for the great honour you have shown in giving me this opportunity to address you. In the study of the nervous system physiology and psychology must, I am confident, be sooner or later united in an intimate and friendly work. Now, however, let every one of us try in his own way, to marshal his special resources. The larger the number of approaches, the greater the chances that we shall finally unite and proceed together, each helping the other.

In the study of the activity of the brain in the higher animals (in particular, the dog), I and my collaborators, as you know, have adopted a purely physiological point of view, and the terms and explanations we use are exclusively physiological.

The more we investigate, by our method, the higher nervous activity of dogs, the oftener we come upon considerable and striking differences in the individual qualities of the nervous systems of these animals. These differences, on the one hand, added difficulties to our investigation and often disturbed the complete reproduction of the results in other dogs: on the other hand, they were extremely advantageous, as they strongly emphasised a certain aspect of the nervous activity.

Finally, we have been able to distinguish several definite types of nervous systems. To one of these types, then, I take the liberty to direct your attention. This type of dog is one which judging by his behaviour (especially under new circumstances) every one would call a timid and cowardly animal. He moves cautiously, with tail tucked in, and legs half bent. When we make a sudden movement or slightly raise the voice, the animal draws back his whole body and crouches on the floor. We now have in the laboratory an extreme example of such a type. The dog—a female—was born in the laboratory and has lived there five or six years. Never have we subjected her to anything unpleasant. The only thing required of her was this: we put her periodically on the stand and offered food in the presence of certain signals—our conditioned reflexes. But even up to this time, at the sight of any of us, although constant members of the laboratory staff, she starts, and slinks off as if from dangerous enemies. Such an animal is very useful for work on conditioned reflexes, but not at once. At the beginning it is exceedingly difficult to form conditioned reflexes; the animal resents being placed on the stand, the attaching of the various pieces of apparatus and especially the feeding, etc. But when all this difficulty is at last overcome, the dog acts like a perfect machine. Especially notable is the stability of the inhibitory conditioned reflexes—when conditioned agents call forth not the process of excitation but of inhibition. In dogs of other types, on the contrary, it is the process of inhibition which is the more labile and the more easily destroyed. When on a dog of this type there falls, under the usual experimental conditions, some inconsiderable new stimulus, for example the presence of a stranger outside the door of the experimental chamber, only the negative conditioned reflexes are fully maintained; the positive ones immediately weaken or vanish.

I shall now discuss a dog of this type. My collaborator, Dr. Speransky, performed the experiments. Six positive conditioned reflexes were formed: to a bell, a metronome, a whistle, an increase of the general illumination, to a circle of white paper, and to the appearance of a toy rabbit. Differentiations were formed, *i.e.*, inhibitory stimuli were elaborated to a metronome of another frequency, to the decrease of the illumination, to the shape of a paper square, and to a toy horse. The size of the positive reflexes varied as follows: All the auditory reflexes were one and a half times or twice as great as the optical. The bell occupied first place among the sounds, next came the metronome, and the weakest of all was the whistle. The optical were all of nearly the same size. As has been already stated in general (and this dog worked perfectly), all the described relations could always be reproduced uniformly.

In September of last year (1924) there was in Leningrad a great flood. The dogs were saved only with difficulty and under extreme circumstances. Five to ten days later, when they were returned to their usual kennels, the dog under discussion was, to all appearances, perfectly healthy, but in the experimental room it perplexed us not a little. All the positive conditioned reflexes were completely annihilated; not one drop of saliva flowed, and the dog refused to take food offered in the customary manner. For a long time we could not guess what was the matter. All our first suppositions about the cause of this phenomenon could not be substantiated. Finally we came upon the idea that the strong effect of the scene of the flood still persisted.

Then we adopted the following course. Our experiments with conditioned reflexes are now usually conducted so that the dog remains alone in the experimental chamber, while the experimenter is seated outside the door in another room. From here the various agents are made to act on the dog: by a certain mechanical device the vessel of food is swung under his nose, and here on the outside of the door are registered the results of the experiment. For this dog we altered the circumstances. Dr. Speransky sat quietly inside the room with the dog, but did nothing else, while I, instead of him in the outer room, performed the experiment. The conditioned reflexes, to our great satisfaction, reappeared, and the dog began to take the food. We repeated this experiment (at first rarely and then more often) over a period, and then gradually weakened or modified it by sometimes allowing the dog to remain in the room alone: in this way we restored the animal to his normal condition. Next we tried the effect of a certain component, so to speak, of the inundation, by reproducing it in miniature. Under the door of the experimental chamber we allowed a stream of water to trickle. Perhaps the sound of the running water or its reflection threw the dog into the former pathological state. The conditioned reflexes vanished as before and their restoration had to be brought about by the means employed previously.

Moreover, when the dog had recovered, it was impossible to elicit an effect from the former strongest of all the conditioned stimuli, *viz.*, the bell. It inhibited itself, and afterwards there was inhibition of all the remaining conditioned reflexes. A year elapsed after the flood, and during this time we carefully protected the dog from every kind of extraordinary stimulus. Finally in the autumn (of 1925) we were able to get the old reflex, even to the bell. But after the very first time the reflex began gradually to decrease, although it was employed only once a day; and at last it disappeared entirely. At the same time all the remaining reflexes suffered, now temporarily vanishing, now passing into various hypnotic phases ranging between the waking state and sleep, although in this dog the latter state was never fully attained. During this condition of the animal we again tried two methods in order to restore the normal reflexes. The inhibitory reflexes in this dog were, as has been said, stable to an unusual degree. But concerning the well-inhibited stimuli, we know that they are able by induction to strengthen the process of excitation. Therefore we applied those differentiated stimuli (negative, inhibitory) mentioned above. And we actually saw many times that after this the reflexes reappeared and the dog took the food, although before the reflexes had been absent and the food refused, or in the presence of transitory hypnotic stages, under influence of induction, the phase was transposed toward the normal state.

The other method is only a variation of the one described previously. Into the dog's chamber we introduced not the experimenter in *toto* but only a part of his clothing. This was sufficient to increase markedly the reflexes. As the piece of clothing was not visible to the dog it was evidently the scent of it which acted.

To the experimental facts, which I have purposely kept clear of suppositions, it is necessary to add the following. If we turned our attention to the movements of the dog when the conditioned reflexes had disappeared and he refused to eat, then we saw not the food reaction but the *passive defensive,* according to our terminology, or as it would be called ordinarily, the reaction of *fear.* This is particularly striking when the dog falls into one of the hypnotic stages, or as we call them, *paradoxical, i.e.,* when only weak conditioned stimuli act, but not the strong ones. With optical stimuli (these are in general weak) there is an evident motor-food reaction, while immediately after with auditory (strong) stimuli there is a striking passive-defensive reflex: the animal moves the head uneasily from side to side, crouches with head hung down, and does not make the slightest movement toward the food box. Nevertheless, the animal when outside the experimental room is very lively and greedy.

The animal described, however, is by no means an exception. We have had several dogs of this type, as I mentioned earlier, upon which the flood and its variations had a similar influence.

I shall now pass on to our interpretation of all the above facts.

For us it is perfectly clear that this type must be the opposite of all other types, in which it is frequently impossible to elaborate full inhibitory reflexes, or in which, although they may be well elaborated, they are very unstable and are quickly impaired. This means that in the described type the inhibitory process is predominant, while in all the remaining types the process of excitation either prevails or is in more or less equilibrium with that of inhibition.

How can we approach to the understanding of this type, and of its deeper mechanism? We recognise as the most constant and general law in the physiology of the conditioned reflexes the fact that an isolated conditioned excitation conducted into the cerebral cells inevitably leads, sooner or later, and sometimes astoundingly quickly, to an inhibitory state of the cells and perhaps even to its uttermost degree—to the sleeping state. This fact can best be understood thus: these cells being extremely sensitive and quick to react, rapidly expend their *excitatory substances* under the influence of stimuli, and then there sets in another process, in a certain degree conserving and economic, the process of inhibition. This process cuts short a further functional destruction of the cell, and thereby accelerates the restoration of the expended substances. In favour of this speaks our fatigue after the day's work. It is removed by sleep, which, as I have shown before, is an overspread inhibition. Evidently the same is proved by the striking fact, demonstrated in our laboratory, that after damage of certain parts of the hemispheres, for a long time it is impossible to obtain positive conditioned reflexes from the receptors (sense organs) connected with these parts, their stimulation producing only an inhibitory effect. And when later a positive effect of these stimuli appears, it lasts for only a short time and quickly passes over into inhibition. This is an effect typical of so-called nervous exhaustion. Here should be mentioned an observation which we made during the recent difficult years of my native country when the state of exhaustion which the dogs shared with us caused them to fall quickly into different stages of inhibition, and finally into sleep, under the influence of the conditioned stimuli, so that there was no possibility of carrying on researches with the positive conditioned reflexes.

We may conclude, therefore, that the cortical cells, in the type of dog we have described, possess only a small reserve of the excitatory substances, or that these substances are extraordinarily destructible.

A state of inhibition in the cortical cells may be produced by either very weak or by very strong stimuli; only with stimuli of average strength may the cells continue for a long time in a state of excitation without passing over into different degrees of inhibition. With weak stimuli the process of excitation passes over into inhibition only slowly, but with strong stimuli the change is rapid. These degrees of strength of stimulus are, of course, relative, *i.e.*, a strong stimulus for one type of nervous system may be only of average strength for another type. The great inundation produced inhibition only in the type under discussion, while on the other dogs it had no perceptible influence. A bell did not act as a strong stimulus on the subject under consideration until the neurosis appeared (which can be put in a class with human traumatic neuroses), but after the flood, which caused the neurosis, the bell acted definitely as a strong stimulus—as an inhibitory one. The same may be stated in regard to one of the normal hypnotic phases, the paradoxical, when only the weak stimuli acted positively, and the strong led to inhibition.

Then it is impossible not to be struck with the resemblance between the passive defensive reflex in dogs and the inhibitory process. And our dog, we observed, possessed a nervous system in which the process of inhibition predominated. The constant presence of this passive defensive reflex is a common and constant characteristic of the general behaviour of such dogs. At the height of development of the neurosis with all conditioned stimuli, and afterwards in the paradoxical phase during the action of only the strong stimuli, the passive defensive reaction constantly occurs. A remarkable thing! Even in dogs which do not as a rule manifest the passive defensive reflex this reaction characteristically appears, nevertheless, during the time of the paradoxical phase in the presence of strong conditioned stimuli.

On these grounds, we may, I think, assume that at the basis of normal *fear* (timidity or cowardice) and in particular of the pathological fears (*phobias*), there lies a predominance of the physiological process of inhibition, an expression of the weakness of the cortical cells. In this connection I ask you to recall the aforementioned case of induction, when a purely physiological application temporarily removed the inhibition and with it the passive-defensive reflex.

As I gradually analysed the types of nervous systems of various dogs, it seemed to me that they all fitted in well with the classical description of *temperaments*, in particular with their extreme groups, the *sanguine* and the *melancholic*. The first is the type for which there is continually necessary varying stimuli, and furthermore this type indefatigably seeks such stimuli and is itself under these conditions capable of expressing great energy. With monotonous stimuli, however, it quickly and easily falls into a state of drowsiness and sleep. The melancholic type is the one with which we experimented. You recall the extreme representative of this group described in the beginning of my lecture. Is it not natural to consider this as melancholy and to term it so, when at every step, at every moment, the surrounding medium calls forth in the animal always the same passive-defensive reflex?

Between these extremes stand the variations of the balanced or *equilibrated* type, where both the process of excitation and the process of inhibition are of equal and adequate strength, and they interchange promptly and exactly.

Finally, we have come across in our dogs a definite *social* reflex, operative under the influence of an agent of the social surroundings. Dogs and their wild ancestors, wolves, are herd animals, and man, owing to their ancient, historical association together, represents for them a "*Socius*." Dr. Speransky, who always brought this dog into the experimental room, who played with him, fed him and petted him, represented for him a positive conditioned stimulus, heightening the excitatory tonus of the cortex, and dispersing and overcoming the inhibitory tonus. That Dr. Speransky acted on the dog only as an external synthetic stimulus, consisting chiefly of optical, auditory and olfactory components, was proved by our recent experiment in which the scent alone of Dr. Speransky had the same effect (though, of course, weaker) on the nervous system of the dog as he did himself.

This experiment taken together with other similar experiments, brings

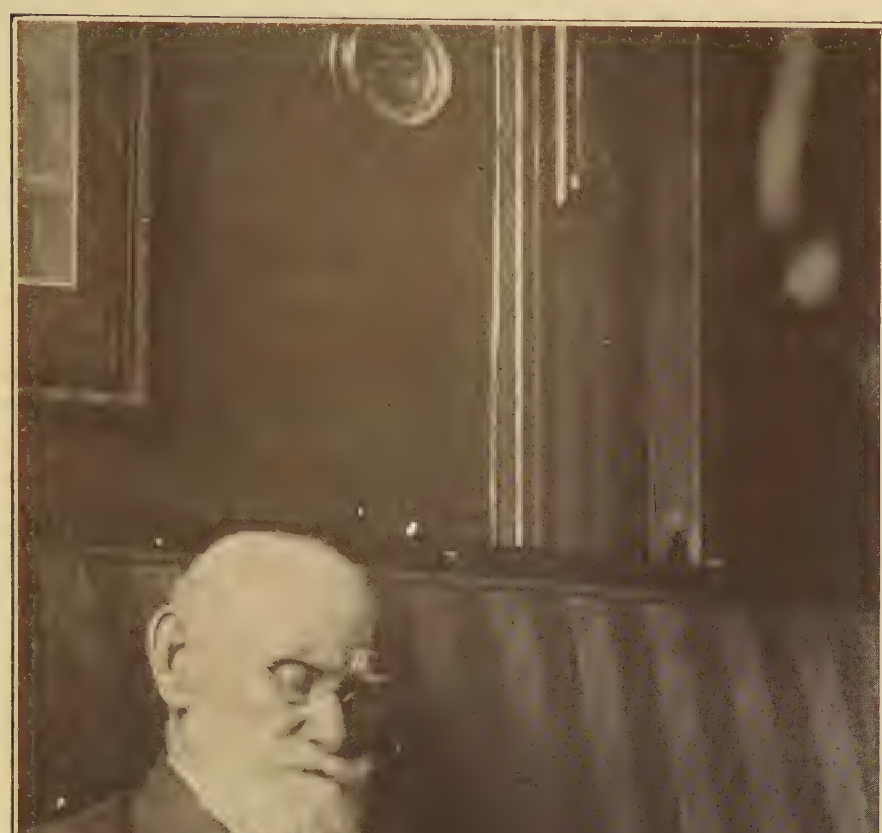

FIG. 9: PROFESSOR PAVLOV IN HIS STUDY

us, finally, into the field of social reflexes, which we shall include in the future program of our experiments. It is hardly possible now to doubt that by the help of the method of conditioned reflexes, for the purely physiological investigation of the activity of the cerebral hemispheres, there is opened a limitless region.

CHAPTER XL

A PHYSIOLOGICAL STUDY OF THE TYPES OF NERVOUS SYSTEMS, *i.e.*, OF TEMPERAMENTS

(Delivered before the Pirogov Surgical Society, December, 1927, and rewritten February, 1928, for this volume.)

PHYSIOLOGICAL STUDY OF TEMPERAMENTS RESULTS FROM THE METHOD OF CONDITIONED REFLEXES—ADAPTABILITY—REVIEW OF THE METHOD OF CONDITIONED REFLEXES—THE CONDITIONED AND UNCONDITIONED REFLEXES—REACTIONS FORMERLY KNOWN AS PSYCHICAL CAN NOW BE STUDIED PHYSIOLOGICALLY—SIGNALLING REFLEX—FORMATION OF THE CONDITIONED REFLEXES AND THEIR IMPORTANCE IN THE ANIMAL'S LIFE—THE POSITIVE AND THE NEGATIVE CONDITIONED REFLEXES, EXCITATION AND INHIBITION—OBSERVATION ON DOGS SHOWS THEY FALL INTO THREE GROUPS,—ONE HAVING A PREDOMINANCE OF EXCITATION, THE SECOND OF INHIBITION, AND A THIRD CENTRAL OR EQUILIBRATED GROUP—EXAMPLE OF HOW EXCITATION AND INHIBITION MAY COLLIDE AND PRODUCE NEURASTHENIA—TREATMENT OF DOGS WITH NEUROSES BY REST, AND IN EXCITATORY CASES, BROMIDES AND CALCIUM—THE CENTRAL TYPE UNDER THE SAME EXPERIMENTAL CONDITIONS REMAINS HEALTHY AND EQUILIBRATED—THE CENTRAL, NORMAL GROUP HAS TWO SUB-GROUPS—APPLICATION OF THE ABOVE FACTS TO THE HUMAN FINDS AN ANALOGY IN TEMPERAMENTS, TO THE CLASSIFICATION OF HIPPOCRATES OF CHOLERIC, MELANCHOLIC, SANGUINE, AND PHLEGMATIC—DESCRIPTION OF THESE HUMAN TYPES—EXPLANATION OF NEURASTHENIA AND HYSTERIA IN MAN, AND THEIR ANALOGS IN THE CYCLIC PSYCHOSES AND SCHIZOPHRENIC FORMS OF INSANITY—ISOLATION OF PATHOLOGICAL POINTS IN THE CORTEX OCCURS IN HYSTERIA, AND TO A GREATER EXTENT IN SCHIZOPHRENIA, WHICH IS AN EXTREME DEGREE OF HYSTERIA—THE CONTRIBUTION OF THE LABORATORY IN SOLVING THIS QUESTION.

AT this festival, dedicated to the memory of a great Russian physician, as a mark of deference to the talent, scientific services and life of Nicholas Ivanovitch Pirogov, I am permitted to make a report of the experimental work done together with my collaborators, which, though not strictly surgical, has a physiologico-medical significance.

Temperament forms an important part of constitution, and as constitution occupies an obviously important place in the attention of the medical world, my communication will not be untimely.

The physiological study of temperaments is the result of a new method used in the investigation of the higher nervous activity. But as this study has not yet been included in textbooks and in the physiological teaching from which we draw our basic information of the animal organism, it devolves upon me, in order to be better understood, to touch upon a few general points before passing over to the special subject of my communication.

The general characteristic of living substance consists in this, that it responds with its definite specific activity not only to those external stimulations with which connections have existed from the day of birth, but to many other stimulations, connections with which have developed in the course of the individual's life; or in other words, that the living substance possesses the function of adaptability.

For the sake of greater clearness, I shall pass directly to the higher animals. In them specific reactions are known as reflexes, and by means of these reflexes there is established a constant correlation of the organism to the surroundings. This co-ordination is obviously a necessity, for if the organism did not especially conform to the environment, it would indeed cease to exist.

Reflexes are always of two kinds: the constant reflex to a definite stimulus, existing in each animal from the day of birth, and the temporary reflex, formed to the most diverse kinds of stimuli which the organism meets during its life. Concerning the higher animals, for example the dog, to which all our investigations refer, these two sorts of reflexes are applicable to the various parts of the central nervous system. The constant reflexes, those which have always been known as reflexes, are connected with all parts of the central nervous system, even with the cerebral hemispheres. But the hemispheres are especially the seat of formation of temporary connections, of transient relations of the animal to the surrounding world, the organ of conditioned reflexes.

You will know that until recently, until the end of the last century, these provisional relations, the transitory connections of the animal organism with the surroundings, were not even considered physiologically, and they were designated as psychical relations. Recent work has shown, however, that there is no reason whatever to exclude them from the scope of physiological investigation.

From these general statements I now pass over to a series of special facts.

Take some injurious influence, some harmful agent such as fire, which the animal avoids and which burns the animal if he happens within its sphere of action or comes into contact with it. This, of course, is a usual inborn reflex, the work of the lower parts of the central nervous system. But if the animal is guarded by the distance from a red light and the representation of the fire, then this reaction, formed during the life of that animal, will be a temporal connection. This impermanent, acquired reflex may be present in one animal, but in another which has not come into contact with fire, it may be entirely absent.

Consider another kind of stimulation, such as the food reflex, *i.e.,* the seizing of food. First of all, this is a constant reflex and children and new-born animals make special movements to take the food into the mouth. But there is also the response seen when the animal runs toward food at a distance on account of some of its aspects, perhaps a sound which is emitted, as, for example, from small animals serving as food for others. This is also a food reflex, but one which is formed during that individual's life with the help of the cerebral hemispheres. It is a temporary reflex which from the practical point of view might be called a signalling reflex. In such a case the stimulus signals the real object, the actual purpose of the simple inborn reflex.

At present, the investigation of these reflexes has gone far. Here is a common example which we constantly see: You give or show to a dog food. A reaction to this food begins: the dog tries to get it, seizes it in his mouth, saliva begins to flow, etc. In order to call out this same motor and secretory reaction, we can substitute for the food any accidental stimulus, whatever we will, as long as it has with the food a connection in time. If you whistle, or ring a bell, or raise the hand, or scratch the dog—whatever[1] you will—and now give the dog food and repeat this several times, then each one of these stimuli will evoke the same food reaction: the animal will strive toward the stimulus, lick his lips, secrete saliva, etc.,—there will be the same reflex as before.

Obviously it is highly important for the animal under the circumstances of his life to be physiologically connected thus distantly and variedly with the favourable conditions which are necessary for his existence or with the injurious influences which threaten him. If some danger, for example, is signalled by a sound from a distance, then the animal will have time to save himself, etc. It is clear that the higher adaptability of animals, the most delicate equilibrations with the surrounding medium, are unfailingly connected with this kind of temporarily formed reflex. The two kinds of reflexes we are accustomed to designate by two adjectives: the inborn, constant ones we call unconditioned reflexes, but those which are built up on the inborn reflexes during the individual's life, conditioned reflexes.

We connect and disconnect the electric light and the telephone many times every day. It would be inconceivable that in the multitudinous connections of the nervous system, uniting the organism with the surrounding infinite world, this technical principle were not utilised, if this were not also a common physiological occurrence. Theoretically there is no ground for objection; and physiologically this view receives full confirmation. The conditioned reflex is formed and exists under definite conditions and laws, as indeed is true of every other nervous phenomenon.

Let us take still another fact concerning the conditioned reflex. A tone, for example, of 1000 vibrations per second has been made a conditioned food stimulus by means of the usual procedure, *i.e.*, its simul-

[1] Prof. Pavlov has previously qualified this with the statement that the signalling agent must be to a certain extent indifferent, or at least not call out some other too strong reaction.—*Translator.*

taneous application with food. This reflex in which the conditioned stimulus evokes in the cortex a process of excitation is a positive food reaction. Such a reaction we call a positive conditioned reflex. But alongside of the positive conditioned reflex we have the negative, which calls out in the central nervous system not a process of excitation but one of inhibition.

If now after the elaboration of this reflex to a tone of 1000 vibrations per second, I try another tone, differing from the original by an interval of 10 to 15 notes, then the new tone is also effective, but less so in proportion to the extent of its variation from the original tone. Proceeding further, if as previously, I always accompany the original tone with food, and never give food with the other tones, then the latter, which acted at first, by and by completely lose their influence as conditioned food stimuli.

What then! Have they become indifferent? By no means. Instead of showing a positive effect they have taken on an inhibitory one. They call out in the central nervous system a process of inhibition. The proof of this is very simple. You try a tone of 1000 vibrations per second. It produces as always a positive reflex, the food reaction. Now apply one of those tones which have ceased to act. Immediately after this trial, the 1000-vibration tone also temporarily loses its effectiveness. Consequently the new tone has evoked in the central nervous system inhibition, and some time is necessary for the inhibition to pass off. Thus you see that it is possible for these temporary agents to produce in the central nervous system the process of inhibition as well as that of excitation. You can readily see that this is of the greatest importance in our life as well as in the life of animals; it amounts to this, that under any given circumstances and at any moment a certain activity must be manifested, but in another situation, inhibited.

Upon this principle is founded the highest orientations of life. In such a way, a continual and proper balancing of these two processes lays the basis of a normal life for both man and animal. These two opposite processes, it is necessary to add, are coexistent and equally important in the nervous activity.

So much for the preliminary explanation. We can now pass on to the main theme of our report.

In the elaboration of conditioned reflexes, both positive and negative, we observe in dogs enormous differences in the speed of formation of these reflexes, in their stability, and in the degree of absoluteness they attain. In certain animals it is easy to produce positive reflexes, and these reflexes are stable under varying conditions; but in these dogs, it is very difficult to elaborate inhibitory reflexes, and in some animals it is impossible to obtain them in pure and exact form, for the reflex will always contain an element of positive activity. This is the characteristic of one group.

At the other extreme, however, there are animals in which the positive conditioned reflexes are formed with great difficulty, are highly unstable, and become inhibited by the slightest change in the surroundings, *i.e.*, they lose their effectiveness. The inhibitory reflexes, on the other hand, are quickly formed and remain stable.

Thus certain animals are specialists, as it were, in excitation, but failures in inhibition, while others are specialists in inhibition, but weak in excitation. Between these extremes there is a central group which can inhibit well, and at the same time easily form positive conditioned reflexes, and both the positive and negative reflexes remain constant and exact. Consequently all dogs fall into three chief groups: an *excitatory group,* an *inhibitory* group (extreme groups), and a *central* group. In the last, the processes of excitation and inhibition are equilibrated. As conditioned reflexes are referred to the cerebral hemispheres, the peculiarities of the indicated groups concern three kinds of character and the corresponding activities of the cerebral cortex.

But we have an even more convincing proof of the existence of these three types of nervous systems.

If there is a very difficult meeting of the excitatory and inhibitory processes, then there will be a complete change in the relations of these three kinds of central nervous system. I shall describe to you in more detail the means which we usually employ, and which are undoubtedly the highest proof of the adaptability or strength of the nervous system. We apply an apparatus with which we mechanically, by rhythmical tactile stimuli, irritate the skin, for example, every second, and we make this a conditioned stimulus. This stimulus can be differentiated, *i.e.,* the nervous system can be made to respond differently to other frequences of the mechanical stimulus. Let us suppose that instead of 30 irritations in a half minute as formerly, I make 15, then it will come about that when I apply the first stimulus there will appear a positive food reaction, but with the stimulus of 15 per half minute the reaction will be inhibited. The first stimulus (30) is of course always accompanied by food, while the latter (15) is not.

In such a way, two stimuli, differing only slightly from each other, produce in the nervous system two opposite processes. And if these two processes are made to follow one immediately upon the other, to collide, as it were, then very interesting results ensue. Let us say I begin with the stimulus of 15 taps; there is no food reaction. If I now exchange the 15 frequency for the 30, this will be a test of the nervous system which will clearly distinguish the three mentioned types. If the experiment is done with the dog of the excitatory type, standing at the extreme pole of the group, in which there predominates excitation and in which the inhibition is always weak, then the following occurs: either at once or after several repetitions of this procedure the dog becomes ill. He retains only excitatory processes, and all the inhibitory ones disappear almost completely. Such a condition we in our laboratory call *neurasthenia,* and it sometimes lasts for months.

If in a dog standing at the opposite pole to this type, I apply the same procedure, conversely the excitatory processes are weakened, while the inhibitory prevail. This state we think represents *hysteria.*

In both cases the normal relation between excitation and inhibition has disappeared. We call this a nervous breakdown, and these destructions of equilibrium in the nervous system we consider as neuroses. They are real neuroses, one showing a predominance of excitation, the other of inhibition. It is a serious illness, continues months, and is one for which treatment is necessary.

The chief curative measures we apply are interruption of all experimentation, but sometimes we have recourse to additional methods. For dogs of the inhibitory type we have discovered no other cure than to let them rest for five or six months or even more from the experiments. But for the other neuroses we have found bromides and calcium salts very useful; in a week or two the animals return to normal.

Thus it plainly appears that different dogs subjected to the influence of one and the same condition develop diseases of opposite nature.

But besides these extremes there is a *central* type. The same application to the latter type of animal has no effect whatever; they remain healthy.

Now it becomes quite evident that there stand out three well-defined types of nervous systems; the central or equilibrated, and the two extreme types, the excitatory and inhibitory. The extreme types use different sides, as it were, of the nervous system, employing only one-half of its function. We might call them half types. Between them stands the whole type, in which both processes are constantly active and in equilibrium.

The following moreover is interesting. The central type has two forms, externally very divergent one from the other, but judged by our criterion the difference is slight. In one form the various balancings of the opposite nervous processes are accomplished easily, but in the other with some difficulty and just avoiding pathological breakdown. Now if we turn our attention to the general external behaviour of our dogs, we observe the following: The excitatory type in its most characteristic form is made up largely of animals having an aggressive nature. For example, if the master, to whom they are well accustomed and obedient, beats or whips such dogs, they may lose control and bite him.

With members of the extreme inhibitory type it is necessary only to threaten them—raise the hand, shout, etc.—and they will tuck in the tail, crouch, or even urinate. These are what we call cowardly animals.

Regarding the central type, there are two forms: quiet, self-contained, sedate animals, ignoring everything about them; and on the other hand, animals which in the waking state are very lively and active, running here and there, sniffing at everything, etc. But paradoxically the latter animals easily fall asleep. When they are brought into the experimental chamber and put on the stand, just when the surroundings cease to stimulate them, they begin to doze and sleep. This is certainly an astonishing combination of the processes of excitation and inhibition.

Thus all our dogs fall into four definite groups. Two extreme groups of markedly excitatory and inhibitory animals; and two central groups of well balanced, equilibrated animals, but also different—one being quiet and the other exceedingly lively. We may consider this an exact statement.

Now can we apply this to man? And indeed, why not? I do not think it will be an insult to man if it is shown that his nervous system has the same common characteristics as that of the dog. Our education has already proceeded so far that against this view no one will seriously protest. We have a perfect right to use the facts established concerning the dog's nervous system for the human, and there is a close parallel. These types of nervous system are, when existing in people, what we call *temperaments*. Temperament is the most general peculiarity of every person, the most basic essentiality of his nervous system, and the type of nervous system colours all the activity of the individual.

The question of temperaments is an empiricism dating from the observation of human beings by the genius Hippocrates, who, it seems, came closest to the truth. His ancient grouping of temperaments divided them into choleric, melancholic, sanguine, and phlegmatic. This classification, it is true, has been now much elaborated: One says there exist only two temperaments; another three; another six, etc. However, during the course of 2000 years the majority are inclined to recognise four forms. We think that the old view is essentially correct. That some of the latest authors have erred in their opinions, I may mention as an example the following instance of a Russian psychiatrist. He proposed six temperaments—three normal and three pathological. The normal were subdivided into lively, serene, and phlegmatic; and the pathological into choleric, melancholic, and sanguine. It seems strange that the sanguine temperament, for example, should be classified as pathological simply because sanguine people have a vacillating nature.

If one accepts the old grouping of four temperaments then one can not fail to see its analogy to our experimental results in dogs. Our excitatory type is the choleric; and our inhibitory is the melancholic. The two forms of the central type correspond to the phlegmatic and the sanguine temperaments. The melancholic temperament is evidently an inhibitory type of nervous system. To the melancholic, every event of life becomes an inhibitory agent; he believes in nothing, hopes for nothing, in everything he sees only the dark side, and from everything he expects only grievances. The choleric is the pugnacious type, passionate, easily and quickly irritated. But in the golden middle group stand the phlegmatic and sanguine temperaments, well equlibrated and therefore healthy, stable, and real living nervous types no matter how different or contrasted the representatives of these types may seem outwardly.

The *phlegmatic* is self-contained and quiet,—a persistent and steadfast toiler in life. The sanguine is energetic and very productive, but only when his work is interesting, *i.e.*, if there is a constant stimulus. When he has not such a task he becomes bored and slothful, exactly as seen in our sanguine dogs, as we are accustomed to call them. Such animals are extremely lively and active as long as the surroundings stimulate them, but they begin to doze and sleep when they are not stimulated.

We may speculate and consider somewhat further such as touching the clinic of nervous and mental diseases, although our knowledge does not extend beyond that of the textbooks. These clinics draw their material chiefly from the extreme, unstable types or temperaments, as we believe; both forms of the central type remain more or less inviolable before the waves and storms of life's sea. It seems correct to think of the excitatory choleric type as corresponding to the pathological form known as neurasthenia; and the inhibitory melancholic as hysteria, for this is properly an inhibitory disease. And further when these illnesses attain to the degree of the so-called psychoses, may not one think that the two chief groups of constitutional endogenetic forms of insanity—the cyclic psychoses and schizophrenia—are only more marked developments of the same diseases?

The neurasthenic, on the one hand, may accomplish much in life, produce some great work. Many prominent people have been neurasthenics. But together with these periods of intense activity, the neurasthenic also has to live through times of deep depression, during which his abilities are curtailed.

But what about the cyclic psychoses? There is a striking similarity. Before the onset of actual insanity, they either reach states far beyond the normal limits of excitation, or they become plunged in periods of deep depression and melancholy.

On the other hand, our laboratory cases of hysteria, our dogs, evidently have very weak cortical cells easily passing over into various degrees of a chronic inhibitory condition. But the basic features of human hysteria are also a weakness of the cortex. Malingering, suggestibility and emotionality (I take these psychical characteristics of hysteria from the pamphlet *Hysteria and Its Pathogenesis* by L. V. Blumenau) are manifestations of this weakness. A healthy person does not hide behind a cloak of disease to attract sympathy or interest in his illness. Suggestibility obviously has at its basis a readiness of the cortical cells to pass over into inhibition. But emotionality is the predominance of a flood of very complicated unconditioned reflexes (aggressive, passive-defensive, and other functions of the subcortical centres) with weakening of the cortical control.

There is thus basis for considering schizophrenia as an extreme weakness of the cortex, as a marked degree of hysteria. The basic mechanism of suggestibility is destruction of the normal unification of the activity of the whole cortex. Therefore the inevitable conclusion is that it arises in the absence of the usual influences, coming from the other parts of the cortex. But if this is so, then schizophrenia is the highest manifestation of such a mechanism. How may we consider the extreme, general weakness of the cortex, its abnormal and pathological fragility? In our inhibitory, hysterical dogs, by applying the functional difficulties presented by our experiments, we can make completely isolated pathologic points and foci in the cortex; in schizophrenia, in the same manner, under the influence of certain experiences of life, acting perhaps on an already organically pathological condition, gradually and constantly there appear a larger and larger number of such weak points and foci, and by degrees there occurs a breaking up of the cerebral cortex, a splitting up of its normally unified function.

And in consideration of all the above mentioned facts, it seems to me that in this thousand-year-old question of temperaments, the laboratory by virtue of the elementary and simple nature of its experimental objects, has an important and unequivocal contribution to make.

CHAPTER XLI

CERTAIN PROBLEMS IN THE PHYSIOLOGY OF THE CEREBRAL HEMISPHERES

(The following is the Croonian Lecture which was delivered before the Royal Society, May 10, 1928. It was translated into English by G. V. Anrep, and is printed here by the permission of the Royal Society.)

FUNCTION OF THE CENTRAL NERVOUS SYSTEM—CONDITIONED AND UNCONDITIONED REFLEXES—SYNTHESIS—CONDITIONED TRACE REFLEXES, SIGNALS—EXTERNAL INHIBITION AND NEGATIVE INDUCTION ARE THE SAME—EXPLANATION OF THE ORIGIN OF THE CONDITIONED REFLEX—ANALYSIS—APHASIA—VICARIATION OF FUNCTIONS OF THE HEMISPHERES—SUMMATION OF CONDITIONED STIMULI—TYPES OF NERVOUS SYSTEMS (EXCITABLE, INHIBITABLE, AND CENTRAL, THE LATTER BEING FURTHER SUBDIVIDED INTO STOLID AND LIVELY)—THESE TYPES CORRESPOND TO THE HIPPOCRATIC CLASSIFICATION OF TEMPERAMENTS—CONCEPTION OF THE CORTEX AS AN ISOLATED AFFERENT AREA FOR ANALYSIS AND SYNTHESIS; THE SPINAL CORD IS BOTH AFFERENT AND EFFERENT—PLASTICITY OF THE CORTEX IN THE SOLUTION OF DIFFICULT PROBLEMS—EXPERIMENTATION ON THE HIGHER NERVOUS ACTIVITIES OF ANIMALS WILL POINT THE WAY TO SELF-EDUCATION.

It is a great pleasure for me to take this opportunity to offer my hearty thanks to the Fellows of the Royal Society for the help which they gave me during the difficult years through which my country has passed. I wish to thank the Society also for the grant which enabled my last scientific work to be published in English, and for inviting me to deliver the Croonian Lecture.

I believe that physiology has at last reached a stage at which it is possible to give a general outline of the activity of the entire central nervous system, including that of the cortex of the hemispheres, though as yet, of course, without deep analysis or detailed knowledge of this activity. The primary function of the nervous system is obvious. It is continuously to maintain a dynamic equilibrium between the functional units within the self-contained system of the organism and between the organism as a whole and its environment. The pre-eminent function of the lower parts of the central nervous system is to integrate the activities of the separate parts within the organism. The rôle they play in maintaining the higher animal in equilibrium with its environment is only subsidiary, the most delicate adjustments of this equilibrium being pre-eminently the function of the hemispheres.

A clear and definite proof of this is provided by the old and repeated observation on dogs, in which the cerebral cortex has been extirpated. Such dogs remain in flourishing health, and can probably live as long as normal animals, so high is the co-ordination between the various internal activities of the organism. This, however, can happen only if the animal is under the constant care of man, who must bring food to his mouth and shelter him from all sorts of harm; otherwise he must inevitably perish. His powers of adaptation to the environment are very limited. The parts of the nervous system which still remain are insufficient to break up the environment into its elementary units, and to make correlation with its perpetual changes, by establishing temporary connections with the various activities of the organism—for instance, with those of the skeleto-muscular system. The activities of this latter system itself, which is the one chiefly concerned in confronting the environment, now fail to be analysed and synthesised to the same degree as takes place in the presence of the hemispheres. As a result, the dog without hemispheres loses the capacity for fine and precise correlation of each separate act with the separate events occurring outside it.

As a result of these observations it is truly legitimate to distinguish a lower from a higher nervous activity, relating the latter to the hemispheres. An unlimited field opens before the physiologist for investigating the analysing and synthesising aspects of this higher nervous activity of the higher animals, and the mechanism underlying them. This fact of nervous analysis and synthesis has confronted the inquisitive mind of man for a long time. Nervous analysis was the subject of the physiology of the sense organs or receptors of the nervous system, which obviously by their nature also serve the organism as analysers of the environment. The synthesising activity was first formulated by psychologists in the form of the law of association. Thus analysis and synthesis first attracted attention as subjective phenomena. Since then, with the co-operation of many biologists, a method was evolved of strictly objective investigation of these phenomena—a method which can be successfully applied to animals.

The fundamental nervous phenomenon, the use of which renders such an investigation possible, is what I call the *conditioned reflex*. The phenomenon itself was known long before. It is an act of *synthesis* by the hemispheres of the animal. Given that there is a coincidence in time of any external stimulus whatever with some definite activity of the organism, this activity tends to become evoked by that stimulus. I, in co-operation with a great number of co-workers—to whom I send from here warm and sincere greetings—founded on this fact a systematic investigation of the functioning of the hemispheres under both normal and pathological conditions.

We have concerned ourselves mainly with two activities of the organism, namely, its reaction to food and its reaction to substances which are rejected on introduction into the dog's mouth—that is, with the alimentary and with one of the defence reactions—and we connected with these all sorts of stimuli that occurred to us. Food, as a stimulus which acts in its own right from birth, evokes a definite reaction of the animal. The animal takes it into its mouth, masticates and swallows it, and at the same time a secretion of saliva occurs. This reaction we call an *unconditioned reflex.* If, during the act of eating, some sight or sound or touch affects the animal on each of several occasions, we find that these stimuli become signals of food evoking the same movements and the same salivary secretion. In our experiments we measured only the secretory reaction.

During the last twenty-seven years we have collected an immense number of observations, which it would be impossible to describe even in the shortest form; nor is it necessary, since this would merely be a repetition of what has been said in my recent book. I shall therefore restrict myself to those problems in the physiology of the hemispheres concerning which we have obtained new facts since the appearance of the book.

As the foundation of the activities of the hemispheres we recognise the processes of excitation and inhibition, their movement in the form of irradiation and concentration, and their mutual induction. At present we are obliged to refer special cases of the activity of the hemispheres to one or other of these heads, but no doubt this classification will have to be modified and probably simplified.

Before discussing the actual problems of the present lecture, I wish to emphasise one important point. More and more observations are being accumulated which show that the establishment of new nervous connections takes place entirely in the hemispheres. The implication is that not only neutral stimuli—*i.e.,* stimuli which are not connected with any activity of the organism—but unconditioned stimuli also, come into communication with definite points of the cortex, pertaining to the respective stimuli. I cannot discuss the evidence for this statement now, but must proceed to the problems which are our immediate concern.

We now know quite well all the conditions under which the conditioned reflex is necessarily established. It follows, therefore, that its establishment is governed by physiological laws, as definite as those which regulate other phenomena in the nervous system. A full and stable conditioned reflex develops when the stimulus which is to become conditioned slightly precedes that activity (unconditioned reflex) with which it is to be linked. The stimulus may also, without ill-effect, terminate a short time before the activity begins (*conditioned trace reflex*); but if the stimulus is introduced *after* the beginning of the activity, then, although, as our present experiments seem to show, a conditioned reflex may also develop, it is insignificant and evanescent; on continuing the procedure the stimulus, which in this connection we term the *neutral agent,* becomes inhibitory. This fact, which is at present under careful investigation, is sometimes strikingly manifested. If during an experiment we simply repeat short feedings of the animal, not combining them with any external stimulus, no influence is produced either on the general condition of the animal or on the previously established conditioned reflexes. This is shown by tests carried out during the intervals between the feedings. If, on the other hand, some extraneous stimulus is introduced during the actual time of eating, and this is repeated many times, then, after a period varying in different animals, a general inhibition develops: conditioned reflexes weaken conspicuously, and finally disappear completely, the dog even declining food—in fact, there supervenes a hypnotic state. The extraneous stimulus itself, when tested outside the time of feeding, in combination with a positive conditioned stimulus, is found to have become strongly inhibitory. This inhibition can be observed whether the positive stimulus is applied concurrently with the extraneous stimulus or within the period of its after-effect.

Where, under the ordinary method of establishing conditioned reflexes, the conditioned stimulus preceding the neutral stimulus is continued together with it, this, as has been observed from the very beginning of our experiments, never weakens the reflex: on the contrary, this frequently strengthens it.

How are these facts to be understood? From a biological point of view of machine-like reactions of the organism, the interpretation of all these relations does not seem difficult. Since conditioned reflexes play the rôle of *signals,* they must obviously acquire significance only when they precede in time the physiological activity of which they become signals; and since they act on the extraordinarily responsive cells of the cortex, it would be natural to expect that these cells would not be stimulated longer than necessary, and their energy thus dissipated, but that they should be left to recuperate for another phase of activity. (This suggestion has already been made in my book, *Conditioned Reflexes.*) But how should these facts be explained in terms of the general properties of the cortical tissue? How is it that an overlapping in time, given that the neutral stimulus begins to act first, renders this agent an excitatory stimulus, while a similar overlapping when the unconditioned reflex precedes the neutral agent makes the latter an inhibitory stimulus? The following interpretation may be possible.

Negative induction or external inhibition (more and more observations are available to show that these are identical) consists in this, that a stimulation of the cortex at one point leads to inhibition of the rest of the cortex. This would explain how the cells when they are affected by the neutral stimulus, after some definite activity of the cortex has already been started, undergo inhibition; the neutral stimulus, therefore, cannot under these conditions acquire excitatory properties. The mechanism of the development of the conditioned reflex under ordinary conditions can be pictured as follows: the excited state of the cells of the cortex acted on by the neutral agent (when this begins to act first) resists the inhibitory influence of the unconditioned stimulus, and it is only under these conditions that a fusion of the effect of the stimuli takes place, leading to the establishment of a connection between the two points. In other words, the mechanism is based on the confluent irradiation of excitation arising at the two points. This interpretation of the facts, however, leaves many questions unanswered. Why does not the neutral stimulus, when it acts first, evoke inhibition of the points pertaining to the unconditioned stimulus? Why does it not produce the same effect as that produced by the unconditioned stimulus when this operates first?

While it is difficult to answer these questions, one can to some extent understand the position by remembering the relative strength of the stimuli; an unconditioned stimulus is usually much more powerful and more extensive in its effect than the neutral stimulus. There is abundant evidence that the relative strength of stimuli is a factor of the utmost importance in the activity of the cortex. Moreover, as I have already mentioned, even where the unconditioned stimulus precedes the neutral agent, an abortive conditioned reflex may appear. Why then, in this case, do the cells pertaining to the neutral stimulus, which is already on its way to becoming a positive conditioned stimulus, invariably pass into a state of inhibition? A fact of special interest is that, while a neutral stimulus which is introduced at the time of the operation of an unconditioned reflex sooner or later becomes strongly inhibitory, other points of the cortex, which are not stimulated at that time, do not become centres of a strong and protracted inhibition. At the same time, as I have already said, when an established but weak conditioned stimulus overlaps the unconditioned stimulus, its effect becomes, if anything, stronger. The fact that a neutral agent acquires powerful inhibitory properties when it is introduced during the unconditioned reflex (in our experiments on alimentary reflexes) is quite unintelligible from a general biological point of view. It might be suggested that our mode of administration of stimuli is artificial, and that therefore our observations disclose only a sort of pathological exaggeration of a normal mechanism. As against this, however, is the fact that all the temporal combinations of stimuli, which have just been described, frequently occur under normal conditions of life. A satisfactory solution of the problems presented cannot be given without further experimentation.

The second problem with which I propose to occupy your attention relates to the *analysing* function of the hemispheres. It is obvious that the analysis is based, in the first instance, on the peripheral endings of the various afferent nerves. These peripheral apparatus are a collection of special transformers, in which different forms of energy are changed into nervous energy. Each single afferent nerve fibre, running from some definite element of the peripheral receptive field, must be regarded as a conductor to the cortex of some definite element of one or other form of energy. In the cortex a special cell must stand in connection with the fibre, the activity of the cell being related to some definite element of one or another definite form of energy. This interpretation of the structure of the cortex rests on definite experimental indications: as a result of investigation of functional disturbances of the cortical cells, such a fragmentation of cortical functions is revealed as we could never dream of obtaining by any operative procedure. In my recently published lectures an observation was mentioned showing that it is possible to derange a point pertaining to a separate conditioned stimulus, namely, the sound of a metronome, leaving points corresponding to other auditory stimuli undamaged. Succeeding experiments have confirmed that it is similarly possible to create a localised disturbance of the cortex, corresponding to a definite point in the tactile analyser, without impairment of the normal functioning of any other points. The *mosaic* construction of the cortex becomes more and more tangible. The further question, however, immediately arises: How far does this spacial differentiation extend, for instance, in the case of different auditory stimuli? We have started, and are continuing, the following series of experiments.

After having produced impairment localised at the cortical point related to a metronome, we proceeded to produce similar impairment at the point related to a particular tone: the selected tone then also ceased to produce a normal effect. It is interesting that in this case the impairment of function involved, to a certain extent, the rest of the tonic scale, so that the reflexes to other tones, which were not used in the experiments, also lost their normal stability—*i.e.*, they easily underwent inhibition. Reflexes to other auditory stimuli, such as buzzing, hissing or bubbling sounds, remained normal. How can we interpret these results except as indicating a precise localisation of different auditory stimuli in the cellular net of the cortex? The facts which I have mentioned are to some extent analogous to some of the various phenomena observed in *aphasia* in man.

The localised disturbance of the activities of cortical elements can be achieved in two ways. We employ a definite stimulus, which we have reason to believe is related to a definite cortical element, as both excitatory and inhibitory—that is, we develop a differentiation either of frequency of stimulation or of its intensity, and then bring these opposite reflexes into acute *collision* by applying one frequency or intensity immediately after the other: in certain nervous systems a pathological state of the corresponding cortical point results. The same thing happens when an attempt is made to transform a long-established excitatory stimulus into an inhibitory one, and vice versa. In both cases, as illustrated by instances in my book, the disturbance is the result of a difficult encounter between the opposite processes. Moreover, by mere repetition of a conditioned stimulus for a prolonged period it is possible to render the cortical point more or less permanently inhibited. For instance, on repeating an auditory conditioned stimulus day after day many times in each experiment, it finally became null and void, a condition which lasted for some time. Other auditory conditioned stimuli, however, which were only used infrequently or were temporarily disused, remained entirely unaffected.

I will now refer to another point bearing on the structure of the cortical end of the analysers, namely, *vicariation* of functions. We extirpate some definite convolution of one hemisphere: a generalised cutaneous conditioned reflex suffers definite impairment, conditioned reflexes from some points of the skin lose their positive effect, and stimulation of these places now produces inhibition of all other conditioned reflexes when these are evoked simultaneously or after a short interval. The stimulation of these places may even lead to profound sleep in an animal, which up to this point never slept during the experiments—*i.e.*, it leads to an irradiation of inhibition, not only over the cortex, but over the lower parts of the nervous system. Within weeks or months after the extirpation the positive effect of stimulation at these places returns, but is transformed with extreme ease into inhibition. A few repetitions of stimuli in the same experiment may lead to complete inhibition. In these cases there is no evidence of the possibility of stable differentiation, according to the localisation of stimuli at any of the affected places. Some differentiation is obtained fairly rapidly, but the positive effect soon becomes weak and then vanishes.

The same results of extirpation are at present under observation in a dog which was operated on nearly three years ago. This case is specially instructive, because the operation was not followed by any sign of immediate or late complications in the form of convulsions. I previously advanced the conception that, in the cortex, there are, besides the special areas representing the different analysers, certain elements so to speak in reserve which are dispersed over the whole mass of the cortex. I mentioned also that these dispersed elements do not participate in any of the higher synthesis and analysis, functions peculiar to the special areas. As a result of the experiments just described, we are now able to add that the dispersed elements are not even *capable* of reaching the state of functional perfection with which the special areas are endowed.

The next problem in relation to which we have collected new data is that of *fluctuations* in the excitation of the cortical cells, their transition into an inhibitory state, and the *summation* of conditioned stimuli. The positive effect of various conditioned stimuli often undergoes considerable fluctuations in strength, even when the conditions apparently remain constant. As we push our investigation further, the necessity of determining the precise cause of every fluctuation becomes more and more imperative. The following is a typical case of which the significance has only recently been appreciated. For a long time it was impossible to find the cause of the fluctuations of different conditioned reflexes evoked during a particular series of experiments. None of the already recognised causes of fluctuation would explain the case in question. Finally, attention was focused on one of the stimuli as a source of the prevailing disorder in the strength of the reflexes. We began to notice that this stimulus, on being applied first in an experiment, evoked a conspicuously large response, compared with those to other stimuli. If, however, it was repeated in the experiment a second time, its effect was then conspicuously small. Next we noticed that it was just after the application of this stimulus that the irregular fluctuations appeared in the strength of the other conditioned reflexes, and that, in addition, the animal became excited.

All this inclined us to think that the stimulus was a very strong one for the cortical cells of the particular animal: the verification of this supposition was not difficult. It was sufficient to decrease the intensity of the stimulus in order to make the condition of affairs change abruptly. The positive effect of this stimulus diminished somewhat, but it now became considerably more uniform in strength on repetition. Sometimes, even, it did not change at all during the whole of an experiment. The other reflexes also ceased to fluctuate in strength, and the animal quieted down. In order to collect further evidence some of the other stimuli were in turn somewhat increased in strength, and, as a result, the same fluctuations were observed as had been produced by the original strong stimulus. Experiments showing the effects of increased and decreased strength of stimuli can be repeated several times over in the same animal. Having acquired this information, we often, when beginning work with a new animal, tested the strength of various conditioned stimuli. In every separate experiment we repeated the same stimulus several times. In the usual course of things, after several repetitions of a conditioned stimulus in the same experiment, its effect diminishes to some slight extent towards the end of the experiment. The extent of this diminution and the amplitude of the fluctuations during the experiment definitely indicate those stimuli which are excessively strong, and therefore unsuitable for further experiment (unless, of course, the object of the experiment is to test stimuli of excessive strength). In the case of the repetition of excessively strong stimuli the progressive diminution in their effect is very considerable towards the end of the experiment, and during the experiment the fluctuations are remarkably great.

In different animals the agencies which act as extraordinarily strong stimuli may be widely different from one another, as regards their *physical* strength. Every animal, therefore, has a certain limit to what may be called normal excitability, for there is a definite optimal strength of each stimulus. As soon as the individual limit of normal excitability is reached, the corresponding cortical cells become more and more inhibited, and this state is reflected in other cells which are stimulated by other stimuli, with resultant variations in the strength of the reflexes in one direction or another, on account of irradiation or induction. It is therefore obvious that we have constantly to ensure that our conditioned stimuli should remain within the limits of their optimal strength.

In close conjunction with the question of the limits of normal excitability stands that of the summation of conditioned stimuli, which has interested us for a long time, but has hitherto not lent itself to solution or experimentation. As suggested in my lectures, the magnitude of the conditioned reflex is determined, *ceteris paribus,* by the amount of energy transmitted from the stimulus to the cortex. The greater the energy, within certain limits, the greater is the conditioned response. If two weak conditioned stimuli are applied together, their summated effect approximates to that of a strong stimulus. At certain strengths of weak conditioned stimuli an exact arithmetical summation of effect can be observed. If a weak stimulus is combined with a strong, their summated effect is nearly always equal to the effect of the strong one alone. Finally, the summation of two strong stimuli produces an effect which is usually somewhat smaller, and only very seldom greater, than that of either singly. In the variation of the experiment already described, when, in the course of one experiment, a single conditioned stimulus was repeated many times, the following results of summation were observed: first we obtained several curves expressing the fluctuations in the strength of conditioned reflexes for a weak, a medium and an excessively strong stimulus. Next we combined the action of the weak and medium stimuli, and repeated this summated stimulus the same number of times as the separate stimuli: the curve so obtained is identical in type with the curve for the strongest stimulus, exhibiting very considerable variations during the experiment, and ending in a profound diminution during the last few applications.

There are other phenomena concerned in summation. In the first place, it has a certain after-effect. In the case of a single application of a summated stimulus, the after-effect involves the subsequent reflexes in the same experiment—not only those in response to the component stimuli, but also to all others. The after-effect is obvious for several days. Most conspicuous is the inhibitory after-effect left by the summation of strong stimuli. The conception of the limit of normal excitability of cortical cells throws much light on the details of the fact of summation, but there still arises the further and very difficult question of the point where summation takes place. The results of summation of weak conditioned stimuli might naturally be regarded as a fusion of the effect of both weak stimuli in that point of the cortex with which, in all these particular experiments, the conditioned stimuli are brought into relation, *viz.*, the chemical analyser in the cortex. But on the other hand, the summation of the weak conditioned stimulus with the strong, and of the two strong together, definitely points to the cells pertaining to the conditioned stimuli themselves. We have every right to regard the inter-relation of processes in summation as taking place somewhere within the above-mentioned sets of cells, but what is the share of the chemical analyser, and what the share of the cells pertaining to the conditioned stimuli, must be answered by the investigations on which we are now engaged.

When, in the same experiment, we apply alimentary conditioned reflexes in conjunction with the reflexes to acid, the inter-relations become still more involved, because they are complicated by the interactions of the different regions of the chemical analyser itself. The problems thus arising are also under investigation.

Our final problem concerns the types of nervous system. The experimental material, collected from dogs which we used for our observations, is so large that we have a certain basis for defining at least the main types of nervous system. The difference as regards the development and the character of excitatory and inhibitory conditioned reflexes in our animals may be striking. There is one group of dogs in which the positive conditioned reflexes develop with ease, quickly reaching and persistently remaining at their maximum strength, often in spite of various inhibitory influences, *i.e.*, interference from extraneous reflexes. In the case of these animals an attempt has been made to reduce the effect of strong or weak conditioned stimuli by means of unbroken repetition, a method usually very effective for this purpose, but the reflexes remained very steady. The inhibitory reflexes, on the other hand, develop in these animals with great difficulty, and it seems as if the animals' nervous system opposes a barrier to their establishment. Much time must usually be spent in order to establish them firmly, if, indeed, this is possible at all. Some of these dogs fail to develop fully inhibitory reflexes, such, for instance, as those involved in the establishment of absolute discrimination of stimuli. In others fully inhibitory reflexes can be established, but they should not be repeated during a single experiment, or even once a day, otherwise they again lose completeness, and they are very easily disinhibited by extraneous stimuli. This type of animal may be called the *excitable type.*

At the other extreme is the type in which the positive reflexes develop under our conditions very slowly, slowly reach their maximum strength, and are extremely liable to diminish and disappear for considerable periods of time, in the presence of quite insignificant extraneous stimuli. Frequent repetitions of the excitatory reflexes also lead to their diminution and disappearance. The inhibitory reflexes, on the other hand, are developed extraordinarily quickly, and well maintain their strength. This type of animal may be called inhibitable. As regards the cortical cells of these two groups of animal, it may be presumed that in the excitable type the cells are vigorous and richly provided with "excitable substance," while in the *inhibitable type* the cells are weak and poor in that substance. For these weak cells the usual strength of stimuli is super-maximal, and hence leads to inhibition.

In between these two extreme types is the *central type.* This easily acquires both positive and negative conditioned reflexes, which, after development, are stable. Since the normal nervous activity consists in a perpetual equilibration of the two opposing nervous processes, and since in the last type this equilibration is more or less easily achieved, we may call this the "well-balanced" animal. We have at our disposal several criteria for comparing different animals as regards their conditioned activity, and the grouping of animals which I have just indicated finds constant confirmation. Of course, there are several gradations between these primary types. This classification also finds support in the fact that all the characteristic differences become exaggerated under the influence of various prolonged nervous disturbances (experimental neuroses) which develop as the result of excessively strong stimulation, or of unresolvable conflict between the two nervous processes. The balanced type more or less quickly, and at any rate without lasting disturbance, overcomes these difficulties; the extreme types show definite neuro-pathological symptoms differing in the two types. The excitable type entirely loses all capacity for inhibition, and enters a state of strong and continuous excitation, both under the conditions of our laboratory environment and at large; the inhibitable type, on the contrary, loses almost completely the positive conditioned reflexes, and, in response to conditioned stimuli, passes through various phases of the hypnotic state. Treatment is needed in order to restore these animals to the normal—prolonged rest and interruption of the experiment, or pharmaceutical remedies, or both.

It is of interest that the balanced type, as judged by means of our tests, is represented in two groups of animals, varying greatly in their general behaviour—one *stolid* and quiet, peculiarly indifferent to external happenings, but always on the alert; the other extraordinarily *lively* and mobile under ordinary conditions, and showing continual interest in whatever happens around them, but under monotonous conditions—for instance, when left alone in the experimental room—surprisingly apt to fall quickly asleep. These dogs, like the quiet ones, though not so easily, overcome the difficulties presented to them.

It is obvious that these types of nervous system are what is usually defined as "*temperaments.*" Temperament is the most general characteristic of an individual, whether man or animal. It is the most fundamental characteristic of the nervous system, a characteristic which colours and pervades all the activities of every individual. This being so, we cannot fail to see that our types correspond to the ancient classification of temperaments, the choleric and melancholic types being our extremes—excitable and inhibitable; while the phlegmatic and sanguine correspond well to the two forms of balanced type—the quiet and the lively. It seems to me that our classification of temperaments, which is based on the most general properties of the central nervous system, namely, the relations between the two aspects of nervous activity, inhibition and excitation, is the most simple and the most fundamental possible.

Now since, in our experiments with conditioned reflexes, we are concerned with the properties of the hemispheres, we can go a step further and say that temperament is determined mainly by the properties of these. That the facts of temperament are not attributable to special complex peculiarities of the unconditioned reflexes, usually known as instincts or tendencies, is shown by the fact that the unconditioned alimentary reflex may be very intense in extremely inhibitable animals. Complex and special manifestations of unconditioned reflexes, such as the alimentary, the defensive both in its active and passive form, and others, depend of course on the activity of the higher sub-cortical centres, which serve as the basis of the elementary emotions. The sum total of the vital expressions, however, will be mainly dependent on the type of the activity of the cortex, which may be predominantly excitatory or inhibitory, or both in different proportions, and which modifies the sub-cortical activity accordingly.

The conception of the preponderant significance of the fundamental properties of the cortex, as determining temperament, should be accepted as applicable to man.

Having completed this survey of our recent experiments, and of the series of new problems arising therefrom in the physiology of the hemispheres and of the brain generally, I will attempt to draw two general conclusions—one purely physiological, the second more practical and of a certain general application.

If the central nervous system is to be divided into two parts only, the afferent and the efferent, then I would regard the cortex of the hemispheres as constituting an *isolated afferent area.* In this area only does the higher *analysis and synthesis* of the inflowing excitations take place, and it is only from here that ready-made combinations of excitation and inhibition can flow into the efferent areas. In other words, only the afferent part is the active or, so to speak, the creative part, while the efferent is the obedient executive. In the *spinal cord* the afferent and efferent parts are intimately connected: the investigator always carries away the impression of a unified activity of both parts, and is, as I believe, precluded from giving a self-contained description of the peculiarities of the afferent part. For instance, the law of forward conduction of the nervous process is substantiated by experiments demonstrating the uninterrupted progression of the spinal reflex act from start to finish. But is this law valid for a purely afferent organ? In the cortex of the hemispheres we continually observe both progression and regression of the excitatory and inhibitory processes. Is this the result of two-directional conduction along the same paths or, in order to preserve the principle of the law of forward conduction, must we understand that special complicated constructional devices come into play?

Coming to the more practical of the two conclusions which I have mentioned, I am led to it under the influence of persistent impressions, formed over a long series of years devoted to this work. These multitudinous experiments on the activity of the hemispheres reveal the astounding plasticity of this activity. Many problems depending on the nervous function, which may seem for a given brain entirely impossible of solution, nevertheless, by means of gradual presentation and careful method, become in the end satisfactorily solved: and if they are to be solved, the type of nervous system of the individual animal must never be ignored.

I trust that I shall not be thought rash if I express a belief that experiments on the higher nervous activities of animals will yield not a few directional indications for education and self-education in man. I, at any rate, can say, looking back on these experiments, that for myself they have made clear many things, both in myself and in others.

THE END

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