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This work is dedicated to the memory of Stefan Miller, whose close cooperation in the remote past was decisive for the accomplishments described in these pages, and to all my friends in America, to whom I owe much gratitude, in particular: Robert Livingston, Neal Miller, Mortimer Mishkin, Hal Rosvold, Richard Solomon, Eliot Stellar, and Lucille Turner.

FIRST STEPS (1927-1931)

A reader who is for some reason interested in the biography of a scientist would want to know where and when he was born and how it happened that he became a scientist. To satisfy this curiosity, I shall mention that I was born on December 1, 1903, in Łódź, an industrial Polish city with a population of half a million, which at that time belonged to Czarist Russia. My father was a lawyer, I was the youngest of his four children. In 1910 I went to a Polish gymnasium, which was a combination of elementary and high school. At that time there were in Polish cities either governmental Russian gymnasia or Polish gymnasia, which were private or semiprivate. It was, however, considered utterly unpatriotic to send one's children to Russian schools, which were under social boycott.

I finished the gymnasium in 1921, after Poland had attained independence. As far as I remember, I already wished to become a scientist, although my interests were broad and undefined. While still in school I studied books in sociology, and since I was also interested in mathematics I dreamed of combining these two disciplines in some way.

Because of this lack of determination of which speciality to choose, I began to study mathematics at Warsaw State University, but I soon realized that I was not gifted in this field. Then I became interested in how the human brain works and thought that if I studied psychology I would be able to find answers to all the questions which bothered me at that time. Unfortunately, after one year of this study I became completely bored and disappointed, since I found no answers either in the classical textbooks of psychology or in the courses of human psychology taught in the University. I did gain, however, one great benefit during this year: not in the faculty of psychology but in the faculty of

law, where there were lectures by a famous professor of the theory of law, Ignacy Petrazycki, who had come to Poland from Petersburg. He had developed his own system of psychology, which was at that time completely original. According to his views, the essential processes of mental life were the emotions:
fear, hunger, thirst, curiosity, and many others. He considered these processes to be at the same time afferent and efferent; that is, they were thought to contain both the receptive experiences and the urge to react in a particular way. He considered that the perceptions and feelings, on the one hand, and volitional acts, on the other, derived from emotions. His book *The Foundations of Emotional Psychology,* published originally in Russian (Petrazycki, 1908) and then translated into Polish (Petrazycki, 1959), is now in total oblivion, although his ideas were very progressive and, as a matter of fact, most of them can be still considered quite modern. This was perhaps the first scientific book which influenced my thinking and helped to shape my ideas. I finally decided to study medicine, hoping that neurology and psychiatry would teach me how the brain works. I knew nothing about the physiology of the brain, except the meager facts presented in Polish textbooks of physiology.

My study of medicine was another string of disappointments. The study of anatomy contained a great number of data which had to be learned by heart and which were of no use to me. In particular, the anatomy of the skull and the brain contained innumerable details difficult to memorize, but completely devoid of any functional meaning. It was very frustrating for me that, being obliged to learn an extensive bulk of facts concerning human anatomy, physiology, and pathology, I still could not learn anything about how the brain works.

Rescue from this troublesome and seemingly hopeless situation came quite unexpectedly. This happened in 1927 during my third year of medical studies. I was then on very friendly terms with a colleague, Stefan Miller, whose scientific interests were exactly the same as mine. We both came quite incidentally across two of Pavlov's books on conditioned reflexes, which had just been published (Pavlov, 1925, 1926). The books were written in Russian, but Miller had spent his childhood in Russia and knew the language quite well. From these books we learned for the first time about conditioned reflexes, and we immediately realized that this was exactly the field of science we were looking for.

The extent of our excitement brought about by this discovery is difficult to describe. We became entirely involved in studying Pavlov, and only by some miracle, not quite clear to me yet, did we succeed in being graduated in medicine. We assigned to the obligatory courses in medicine a strict minimum of time and went to our exams after a few weeks of hasty learning. This is why my knowledge of the clinical disciplines is now almost nil. It was purely a matter of my short-term memory, and I completely forgot everything I had learned as soon as the exam was over. Instead of medicine, we read and reread Pavlov and discussed every detail of the experimental work of his coworkers. Moreover, we found in the library of the Nencki Institute of Experimental Biology the Russian journals in which the papers on conditioned reflexes were published.

After some time spent on these studies, we began to realize that Pavlovian conditioned reflexes are not sufficient to explain the whole acquired behavior of animals and men, as Pavlov had claimed. In particular, we became aware that the motor behavior established by way of reward and punishment could not be reduced to the paradigm of these reflexes. Accordingly, we began to ponder how it would be possible to incorporate the experiments involving motor behavior into the general scheme of Pavlovian experimentation.

After some consideration we came to the conclusion that the proper paradigm of experiments fulfilling this purpose should run as follows: If a given neutral stimulus, say the sound of a metronome, is combined with a given movement, say raising the leg, and it is then reinforced by food, whereas the same stimulus presented separately is not reinforced (in other words, when the movement is the necessary condition for obtaining food), then the animal should perform that movement in the presence of the stimulus. On the contrary, when the movement following a given stimulus is reinforced by a noxious agent, whereas the stimulus alone is not, then the animal should resist performing that movement. Since we considered that such conditioned reflexes differ essentially from those of Pavlov, we decided to call them conditioned reflexes of the second type, whereas the Pavlovian conditioned reflexes we denoted as first type.

After settling on this project of experimental work, we began to look for a laboratory where the experiments could be performed. Accordingly, we approached several professors of medicine, asking them to provide us with a room and facilities to start our experiments. Our inquiries were, however, completely unsuccessful. The reason for our failures was obvious, and I don't think these professors should be blamed. After all, here were two young students trying to persuade a distinguished professor that they planned to perform experiments constituting an essential complementation of Pavlov's research work, and the professor (not being acquainted with the area concerned) was rather suspicious of the supplicants and anxious to get rid of them. Therefore, we went from one distinguished man to another, telling them about our plans, and were always politely refused.

I don't remember who suggested that we approach Professor Jacob Segal, who had a chair of psychology in the Free Polish University, an institution somewhat analogous to the College de France in Paris. Unexpectedly, Professor Segal fully understood our ideas, became interested in them, and offered us a small room in his department, located on the third floor of what was actually an apartment house. We had no financial support, but we were allowed to utilize the meager equipment of the department, which was mainly concerned with education. The introduction into our first laboratory took place on February 1, 1928, the day which thereafter we celebrated as our anniversary day.

The first thing we had to do was to buy a dog. For this purpose we went to the marketplace, found the area where people sold dogs, and after long deliberations, chose a young and nice bulldog, which cost ten zlotys (about one dollar). We called him Bobek. He immediately became friendly with us and we brought him to our "laboratory." The housekeeper agreed to let him stay in her apartment.

Our next task was to organize a conditioned reflex laboratory in the room which was assigned to us. Putting together two square stools we made a "Pavlovian stand" and used cardboards for a screen. The bowl was made of tin and was fixed to the front part of the stand. Pieces of food were thrown from the small aperture in the screen by the experimenter. I do not remember how the horizontal bar above the stand was fixed in order to keep the animal in the harness during the experimental sessions. Since in the department of psychology there was a kimograph with a long tape, we utilized this for recording the dog's movements. For a long time we used a strip of toilet paper, which was both cheap and convenient, provided that it was relatively smooth and did not have transversal perforations. You can imagine the comical picture presented by two serious young men going to a paper store and asking to be shown all the possible varieties of toilet paper, scrutinizing them thoroughly, and choosing the one which fulfilled both conditions. Recording of movements was accomplished by a very primitive and crude arrangement made mainly out of pieces of wire.

The first experiment was performed in the following way. A band with electrodes connected with the induction coil was attached to the dog’s left hind paw. We presented a tone from a harmonium (luckily found in our lab), and after a few seconds a light electric shock was applied to the paw. When the dog lifted his leg, immediately a piece of sausage was thrown into the bowl. Occasionally the tone was presented without the shock and without reinforcement.

We considered it a great triumph when after a few days of such procedure, Bobek began to lift his left hind leg without the electric shock, turning immediately to the bowl and expecting food. At the beginning he did so both to the tone and in the intervals, but very soon the intertrial movements disappeared. Thus, we succeeded for the first time in establishing the type II conditioned reflex under experimental conditions.

The next step in our experimentation was an attempt to establish the type II conditioned reflex by using the passive lifting of the leg. The rationale of this experiment was the assumption that the indispensable condition for the formation of the type II conditioned reflex was that the proprioception of the movement should become the type I conditioned stimulus, that is, a signal of presentation of food. We turned on an electric lamp placed in front of the stand, and then we raised Bobek's left foreleg by pulling a string attached to the wrist;
this compound was reinforced by food, whereas the lighting of the lamp without the passive movement was not reinforced. After a short time Bobek began to actively raise his foreleg; at the beginning he did it throughout the experimental session, and then only in response to light.

Then by mere accident we discovered the following fact. When the band for recording was attached to the left hind leg, Bobek performed the movement of that leg in response to both the tone and the lamp; when the band was attached to the left foreleg, he raised that leg to both the conditioned stimuli. When the band was attached to one of the right legs, no movement was performed. We called this phenomenon "motor generalization" and were very much impressed by this unexpected finding.

I shall not describe all the experiments we performed during the first few months of our experimental work. We were very lucky with the dog, who was amazingly intelligent and learned very quickly all the conditioned reflex tasks with which he was presented.

In order to hasten our experimental work we decided to train the dog in two tasks simultaneously, one task involving alimentary conditioned reflexes being trained in the morning, and the other involving defensive conditioned reflexes being trained in the afternoon. The dog put up with all the difficulties presented to him and seemed quite happy. Only once, when we attempted to teach him to extend the hind leg in response to one stimulus in order to avoid the electric shock, and to raise the same leg in response to another stimulus in order to get food, did he refuse to work. Concurrently he developed an interesting experimental neurosis: when placed on the stand, he took a catatonic position, with his hind leg lifted continuously and his eyes half closed. However, when we stopped this type of experiment, he soon returned to normal.

Since at that time we did not consider it necessary to use more dogs—in fact, the results obtained on Bobek were most reliable and were repeated many times—we were able within a few months to make many interesting discoveries concerning the properties of particular varieties of type II conditioned reflexes. We were much impressed when we came across a phenomenon now called "avoidance conditioning." In accordance with our ideas we applied the following procedure: An auditory stimulus alone (a whistle) was followed by a puff of air into the ear (provoking a very strong defensive response), whereas this stimulus accompanied by a passive movement of the right foreleg was not followed by the air puff. Bobek began to raise the foreleg to the sound of the whistle and continued to do so consistently in spite of nonreinforcement.

After having obtained these first results we decided to do two things: one was to present a report before the Warsaw Branch of the French Biological Society in Paris, and the second was to write a letter to Pavlov presenting him with our achievements.

The first two papers, concerning the elaboration of the type II conditioned reflexes and their transfer, were delivered to the Society in the summer of 1928. As I remember it, the reaction of the audience was quite favorable, and after a few months these papers were published in *Comptes Rendus de la Societe de Biologic et de ses Filiales* (Miller and Konorski, 1928a, 1928b).

More or less at the same time, to our great joy, we received an answer from Pavlov. He congratulated us on our results, took them to be important, and asked for details. Thus we considered our first experimental season, which had lasted only five months, to be fully successful. This was just at the end of our fourth year of medical studies, and, to our great annoyance, we had before us still one full year at the University.

When the new academic year began, our position was much strengthened. We boasted of the letter received from Pavlov, and we also proudly displayed the reprints of our papers to everyone. As a result, Professor Czubalski, head of the chair of human physiology in the Medical Faculty of the Warsaw University, allowed us to continue our work in his laboratory. This was, of course, a great step forward. The physiological laboratory was equipped with all necessary instruments and there was, therefore, no difficulty in making salivary fistulas and recording not only motor acts but also salivation. Moreover, all the expenses connected with our work, which had previously weighed on our pockets, were now covered by the University. We repeated the experiments performed on Bobek on new dogs with salivary fistulas, calling our technique the "salivo-motor method." Moreover, we became engaged in a study of the relations between type I and type II conditioned reflexes, and we discovered that the classical positive conditioned stimulus inhibits type II responsing, whereas the negative conditioned stimulus does not. At that time this finding was quite unexpected. As a result, two new papers were published in the *Comptes Rendus de la Societe de Biologic et de ses Filiales* (Konorski and Miller 1930a, 1930b).

Since everything follows the rule of coming to its end, in the fall of 1929 we graduated in medicine and had to face the problem of finding jobs. This was, at that time, far from easy in Poland, because the positions in the universities were very scarce and we were ready to take only such jobs as would enable us to continue our work. As there were no vacancies in Professor Czubalski's Department of Physiology, we decided to work in psychiatry, judging that this specialty was not very remote from our main field of interest. Accordingly, we applied to Professor Luniewski, director of the big State Psychiatric Hospital situated in Pruszkow, near Warsaw. Professor Luniewski's reaction was most positive. He had already heard about us and favored our project of establishing a small laboratory of conditioned reflexes in his hospital.

In this way our practical problems were solved. We both received rather decent salaries and apartments inside the hospital. We were allowed to build and indeed were assisted in building, the laboratory of conditioned reflexes (the third so far in our brief careers).

However, our work in this field did not progress, because we were at that time so much concerned with psychiatry—a field quite new to us—that it was practically impossible to perform systematic experiments on conditioned reflexes. Even so, the knowledge of psychiatry which I acquired during that time has remained with me and has occasionally been of great help to me with regard to both my livelihood and my scientific work.

During our stay in Pruszkow we prepared a monograph written in Polish in which we presented all our experimental results on type II conditioned reflexes (Konorski and Miller, 1933). We claimed that these reflexes represent the physiological model of voluntary behavior and we considered that they possess a quite different mechanism from the Pavlovian (type I) conditioned reflexes.

In the meantime our contact with Pavlov was not broken and in one of his letters he proposed that we should come to Leningrad to discuss our results and perhaps perform some work. Now that we had graduated, this was possible.

At that time there was a true "iron curtain" between Poland and Soviet Russia, and contacts between the two countries were practically nonexistent. Therefore, the idea of our going to Leningrad seemed quite exotic to a number of our colleagues. The success of our enterprise was entirely due to Pavlov, whose authority in both Russia and Poland was so high that both Professor Czubalski and Professor Luniewski assisted us in obtaining passports to Soviet Russia, and we were easily granted the Soviet visa.

Late in 1931 on a foggy November morning we boarded the train going to the Soviet frontier, and then by a Soviet train we arrived in Moscow and from there the next day went to Leningrad. So it was that our big adventure began.

IN LENINGRAD (1931-1933)

It would be both unreasonable and impossible to depict chronologically my stay in Leningrad, telling how I gradually adapted myself to the difficult and unusual conditions of life, how I began to understand and master the Russian language and became familiar with the city and the people around me. The period of my stay in Leningrad resides in my memory as a compact entity with quite definite spatial dimensions but with no temporal coordinate. I cannot recollect just what was the sequence of events as my own experiences were developing. Therefore, I shall rather describe the atmosphere of the Institute in which I was working, my own work, and, perhaps most important, I shall tell about Pavlov as I remember him in those days.

When Miller and I arrived in Leningrad we didn't know how long we would stay. Miller had been married just before our journey and therefore he could stay in Leningrad for only a few months. As far as I was concerned, I was single and completely free to do whatever I wished, having no precise plans for the future. Just at the beginning of our sojourn, after a few discussions with Pavlov, it became clear that it would be most profitable if I remained in Leningrad and performed some systematic experimental work. Consequently, I was accepted by Pavlov as a member of his laboratory and thus became one of "Pavlov's pupils," with all the privileges of this little clan and with all its advantages and shortcomings.

Pavlov headed at that time two big laboratories situated in separate districts of Leningrad. One was the Department of Physiology of the Institute of Experimental Medicine. The Institute was founded mainly for Pavlov at the end of the nineteenth century and was situated in the outskirts of the city. In this department the famous studies of Pavlov on the physiology of the digestive glands had been performed, the studies rewarded by the Nobel prize. In 1910 a Moscow businessman provided Pavlov with funds that enabled him to erect for the study of conditioned reflexes a special laboratory with soundproof chambers, the famous Tower of Silence. This was a building with very thick walls, which could not be penetrated by noises or vibrations from the outside world. During my stay in Leningrad, one of the soundproof chambers in the Tower of Silence was allotted to me to share with a colleague. The second laboratory, situated near the center of the city, belonged to the Academy of Sciences of USSR. Although the two laboratories were administratively separate, they were united in respect to the scientists and the scientific work performed in them. About forty scientific workers worked in both of them.

Pavlov spent alternate days in the two laboratories. Wednesdays were free from experimental work, because before noon there were meetings of all scientific workers of both laboratories to discuss the current experimental work, and in the afternoons there were meetings in either psychoneurological or psychiatric clinics directed by two of Pavlov's coworkers; here the particularly interesting neurotic or psychiatric cases were analyzed and discussed from the point of view of Pavlov's ideas about brain pathophysiology.

During the morning meetings Pavlov was usually the only speaker. He was perfectly acquainted with the work of every one of his coworkers and presented the material from memory without the use of notes. Indeed, his excellent memory was famous and at the time that I was there—he was 82 years old—it had not deteriorated. Usually, after talking for about 20 minutes he would ask whether there were any questions or comments. Most often the discussion was rather limited and only occasionally would an argument develop.

Rarely was Pavlov opposed. The criticism, which was very feeble, would be circumlocuted by such phrases as: "How do you explain, Ivan Petrovitch, this or that fact?" Generally speaking, to openly criticize Pavlov was rather a shocking act and required some courage. First, Pavlov himself was a very strong debater, quite aggressive and not quite fair in discussion. Besides, if anybody dared to criticize Pavlov, the entire group would side against the critic. In such a situation, it was no wonder that Pavlov always prevailed. However, in spite of this, Pavlov truly esteemed those people who did oppose him and he highly respected the independence of one's views, as long as he did not consider them to be nonsense. After arguing strongly and defending his own viewpoint, and after cooling down, Pavlov would often accept the view of his opponent and would openly admit that he had been wrong.

Every morning, after arriving in the laboratory, Pavlov would sit down in a large open room, where everybody was free to join him. Most often people came to him to report their new experimental results.Pavlov listened attentively, then gave his comments and explanations of the data obtained. Here people were much more free to take part in the discussions than they were at the Wednesday meetings, and time and again a hot argument about some problem broke out. These discussions were, of course, exceedingly interesting, because of their informal character, and therefore I always attended them when I had finished my daily experiments.

In general, Pavlov was the "spiritus movens" of the work going on in his two laboratories. He assigned the problems to be worked out to each of his co-workers and he controlled all the stages of their research. Only in exceptional cases did a student follow his own line of research, but then it was rather difficult to win Pavlov's attention and appreciation.

From this description it is clear that scientific and intellectual life in the Pavlovian laboratories was very vigorous and that it was fully concentrated around Pavlov. In the absence of Pavlov we usually talked about him, quoted what he said, how he behaved, etc. Moreover, there was clear jealousy among Pavlov's pupils about who was the closest to him. People boasted when Pavlov spoke to them at some length, and, as a matter of fact, the attitude of Pavlov toward an individual was the main factor determining the hierarchy within the group. Another characteristic of "Pavlov's pupils" was that, although squabbling among themselves, they were united with regard to other scientific groups, having a feeling of superiority and self-importance. Of course, I fully shared this feeling. To sum up, the atmosphere reigning in the group reminded one of that usually encountered in royal courts, with Pavlov being the indisputable king.

Since the general character of the experimental work carried out in the Pavlovian laboratories is not well known by American psychologists, I shall briefly describe it:

Each member of the laboratory had several dogs (from 3 to 8) on which he performed his experiments. Usually the dogs remained in the laboratory for many years and they were trained in a great number of experimental tasks, all of them in classical salivary conditioned reflexes.

The routine of the experimental procedure was very rigid and essentially the same in the whole laboratory. Every day, except Sundays and Wednesdays, the animals were brought to the chambers from the animal house at exactly the same hour. All the dogs had fistulas of one parotic gland, which enabled measurement of salivary responses. Usually, when a dog was brought to the chamber, he jumped immediately on the stand and a small glass capsule was sealed to his cheek by the experimenter at the place of the fistula with a special sealing wax (whose formula was given by the great Mendeleev). The capsule was connected with a thin horizontal glass tube situated in front of the experimenter and filled with colored fluid. When the dog salivated, the meniscus of the fluid moved and thus salivation, in response both to the conditioned stimuli and to the unconditioned stimulus, was recorded. The reinforcement used in all experiments was prepared for the whole laboratory. It consisted of a powder made from minced crackers and minced boiled meat. Before a session the experimenter mixed the two powders in equal proportion and added a given portion of water. In this way, the powder had a consistency of moist sand and was easily chewed and swallowed by the animal, producing copious salivation. The constant portions of this cracker-meat powder were distributed in even quantities to the bowls, situated along the round disc mounted in the feeder and with one aperture just in front of the dog. By pneumatic control, the experimenter put a bowl with food into position so that the food was available to the animal. The act of eating lasted about 20 to 30 seconds.

Experimental sessions consisted of 6-10 trials separated by intertrial intervals of about 5 minutes each. In most experiments performed at this time the intertrial intervals were always the same in the given series of experiments.

The conditioned stimuli were auditory (the sound of a metronome, tones, the sound of bubbling water, whistles, buzzing, etc.), visual (continuous or rhythmic light, rotating or oscillating objects, etc.), and tactile (a small gadget was attached to the skin in such a way that by pneumatic control the experimenter was able to produce tactile stimuli). In well-trained animals the operation of a conditioned stimulus preceded the presentation of food by 20-30 seconds, so that the experimenter could record the rate of conditioned salivation over a relatively long time span. Besides the positive conditioned stimuli, negative conditioned stimuli were also used. These were similar to the positive stimuli but presented without food reinforcement: for instance, the sound of a metronome of different frequency, the tactile stimulus applied to another place of the body, and light of another intensity were among those used as negative stimuli. The training, consisting of the presentation of positive (reinforced) and negative (nonreinforced) stimuli, was called differentiation. Usually in experiments with each dog various conditioned stimuli, both positive and negative, were used. It should be noted that according to the habits of the laboratory, the negative stimuli were presented only once or twice per session, because it was found that if they were presented too often, the magnitudes of the positive conditioned reflexes became less regular.

Each experimental session usually lasted 30-45 minutes. At the end of the session a boy came to the chamber, took the animal out, and brought in the next one. In the animal house the dogs were fed at definite hours with fixed portions of cereal with bones. This highly stereotyped way of conducting experiments resulted in amazingly constant and stable responses to each of the conditioned stimuli and amazingly stereotyped behavior of the animals during the sessions.

When Miller and I began our work on type II conditioned reflexes in Pavlov's laboratory, we had to modify the stable routine. A number of arrangements had to be made to teach the animals to lift their legs in response to conditioned stimuli, to register their motor responses, and so forth.

We got five dogs, which had previously served for many experimental studies. We received their entire biographies, their age, the dates of their coming to the laboratory, the whole story of their conditioned reflex careers, and the lists of all positive and negative conditioned stimuli used in their training. Accordingly, what we had to do was to introduce some new stimuli and train the animals in type II conditioning.

The work in the Pavlovian laboratory was of utmost importance for me and certainly determined my entire future. Whereas our work on conditioning performed in Warsaw had been somewhat amateurish, here we could profit from the tradition and experience of this big scientific center. Moreover, since we received dogs with firmly established positive and negative classical conditioned reflexes, we were provided an excellent background for introducing type II conditioned reflexes and studying the interrelations between the two types. Finally, only in the Pavlovian laboratory were the methods of salivary conditioned reflexes well developed, and accordingly the relations between salivary and instrumental responses could be studied.

Beside this, I had the opportunity to get thoroughly acquainted with the whole bulk of the past and the present work on conditioned reflexes and thus to become a well-trained specialist in this field.

What were the main achievements during my stay in Leningrad? First, confirmation under more rigorous experimental conditions of the results we had found in Warsaw, to the effect that positive type I (classical) alimentary conditioned stimuli completely inhibit type II response, whereas negative type I conditioned stimuli may even increase this response. Second, we performed important experiments in which a given stimulus was reinforced by food, but this stimulus accompanied by passive flexion of the leg was not. As a result, the dog learned to extend his leg in response to the conditioned stimulus, in this way resisting passive flexion. Third, by applying as an unconditioned stimulus introduction of acid into the dog's mouth, we established avoidance conditioned reflexes and could study the relations between motor and salivary responses in these rather unusual conditions. All these results were presented in an extensive paper published in *Transaction's of Pavlov's Laboratories* (Konorski and Miller, 1936), and they laid the foundations for the further development of my ideas concerning the mechanisms of type II conditioning.

To end this description of my almost two years' stay in Leningrad, I would like to say a few words about my relations with Pavlov. As a matter of fact, they were far from being simple. There was no doubt that Pavlov highly appreciated the importance of our contribution to the field of conditioned reflexes, which, according to his own words, led to "physiological understanding of volitional movements." However, he strongly opposed our thesis claiming the existence of two types of conditioning and failed to see any difference between them. He was so sensitive about this point that when writing the above-mentioned paper for his journal we simply did not dare to use our own terminology and called type II conditioned reflexes "motor conditioned reflexes" or "conditioned reflexes of the motor analyser." Both of these terms were misleading.

It should be noted that this negative attitude of Pavlov toward the specificity of type II conditioned reflexes had a detrimental effect on the development of the study of these reflexes in Russia. In fact, had Pavlov accepted this specificity, the situation would have been clear and the experimental work on this type of conditioning would certainly have developed in Russia as it did develop, quite independently of our work, in the United States, where type II conditioned reflexes were called "instrumental" or "operant" responses. However, when the greatest authority in this field stated that type II conditioned reflexes simply do not exist, this was decisive and meant special investigations along this line, with insignificant exceptions, were not undertaken in Russia. Moreover, when in 1949 the orthodox Pavlovism was introduced in the Soviet Union, the term "type II conditioning" was denounced as a manifestation of my revisionistic tendencies and disapproved of.

BACK IN WARSAW (1933-1939)

In June 1933, there came a day when I packed all my luggage, consisting mainly of books, journals, and experimental materials, and returned to Warsaw. The first problem I was confronted with was how I should organize my future life. It was, of course, quite easy for me to return to the psychiatric hospital in Pruszkow, where Stefan Miller continued to work and where I had many friends; but I firmly decided to devote myself entirely to scientific work and knew already that this could not be combined with working in the hospital.

Strangely enough, the problem of my future was decided the second day after my arrival in Warsaw. Since the date of my return had been known in advance, Professor Jan Dembowski, one of the leading biologists in Poland, had arranged for me to give a lecture concerning my work with Pavlov. Among the audience there was a young woman who after the lecture asked me a number of competent questions concerning the views of Pavlov on the problem of inhibition and the relation between Sherringtonian and Pavlovian ideas in this matter. This was Dr. Liliana Lubinska; six months before my returning from Leningrad, she had returned from Paris, where she had spent eight years. She had studied biology at the Sorbonne, obtained a doctoral degree there, and was a pupil of a famous French neurophysiologist, Louis Lapicque. At that time there were very few neurophysiologists in Warsaw, and therefore we were very lucky to have met each other, even more so that while I was mainly trained in the higher nervous activity, she was competent in the lower parts of the nervous system.

From our first talk I learned that after returning from Paris she had obtained a position as a scientific worker in the Nencki Institute of Experimental Biology, in the Department of Physiology, whose head was Professor Kazimierz Bialaszewicz. Although his specialty was physiology and biochemistry of insects, he very willingly accepted Dr. Lubinska as a scientific worker in the department and encouraged her to continue her work in neurophysiology. In this way he considerably helped the opening up of research work in this discipline in Warsaw.

A few days later, Lubinska brought Miller and me to Bialaszewicz and we discussed with him the possibility of establishing a small laboratory of conditioned reflexes in his department. He gave his consent and offered us a room suitable for our experiments. Thus, in the fall of 1933 two important events occurred in my life. I established the laboratory on conditioned reflexes in the Nencki Institute, and Dr. Lubinska became my wife.

From the scientific point of view my collaboration with Lubinska was for me extremely valuable. As a matter of fact, from the very beginning of my scientific career I had engaged myself in the study of conditioned reflexes. However, my general knowledge of neuro physiology was very poor. On the other hand, Lubinska had an excellent knowledge in this field, having worked for a number of years in one of the best known centers of neurophysiology in Europe. Accordingly, she taught me a great deal about neurophysiology, with regard both to methods and to the theories.

This circumstance was decisive for the further development of my scientific ideas. During the time I worked in Pavlov's laboratory I was under the spell of his ideas and personality. In spite of my argument with him about type II conditioned reflexes, in all other respects I supported his views and was strongly convinced of the soundness of his ideas concerning the activity of the cerebral cortex. After my return to Warsaw I propagated these ideas very broadly in numerous lectures, seminars, and articles.

However, as I had learned more about Sherringtonian neurophysiology, I realized that the views of these two scientists were completely incompatible. According to Pavlov the general picture of the activity of the cerebral cortex was roughly the following: Excitatory and inhibitory processes are assumed to arise in particular "points" of the cortex as the effects of excitatory and inhibitory conditioned stimuli. Both these processes spread in a wavelike manner over the cortex, affecting larger or smaller areas, and mutually restrict each other. The greater the areas of the excitatory processes, the stronger the predominance of excitation over inhibition; the larger the areas of the inhibitory processes, the stronger the dominance of inhibition. If the inhibitory process spreads all over the cortex and subcortical centers, it gives rise to sleep. Often the excitatory area is surrounded by the inhibitory area, the phenomenon called by Pavlov negative induction; or vice versa, the inhibitory focus is surrounded by excitatory fields, the phenomenon denoted as positive induction. Pavlov imagined that the perpetual interplay between the excitatory and inhibitory processes was the essence of the normal activity of the brain—in other words, of the mental processes occurring in a subject. If there is a conflict between excitatory and inhibitory processes tending to occupy the same place in the cortex, then the pathological state called neurosis issues. All the experimental results obtained in Pavlov's laboratories on dogs, as well as observations of human patients in psychoneuro-logical and psychiatric clinics controlled by Pavlov, were explained by reference to the above theory.

The Sherringtonian idea concerning the functioning of the nervous processes was entirely different. It was based on the neuronal theory of the structure of the central nervous system advanced by Ramon y Cajal in his monumental work. According to this theory, the conveyance of nervous processes is always uni-directional, leading from the cell body through the axon to other neurons. Sherrington has shown that the nervous impulses arriving along an axon to a given neuron can either activate the neuron and cause it to discharge, or inhibit it, tli.it is, block energy brought to it through other axons. Accordingly, each neuron is a convergence point of both excitatory and inhibitory influences, which determine the intensity of discharges produced by this neuron and which in turn are conveyed by axons to other neurons.

It was quite clear to me that the Pavlovian and Sherringtonian concepts of the functioning of the nervous system could not be reconciled with each other, and it was even impossible to find a "dictionary" which would translate one set of notions into the other. Simply, one of the two theories should be rejected in toto, and the facts so far explained by the rejected theory should be reinterpreted in the framework of the other theory.

By that time I already had no doubt that it was Pavlov's theory that should be rejected. The more I pondered Pavlovian explanations of various facts in the field of conditioned reflexes and tried to analyze the explanations, the more I discovered inconsistencies and contradictions in the Pavlovian interpretation of the facts. Thus, the idea grew in my mind to try to explain the whole bulk of experimental work collected by Pavlov's school by the Sherringtonian principles of functioning of the central nervous system.

Besides this theoretical work undertaken during that period, I was intensely involved in experimental work on conditioned reflexes carried out in collaboration with Lubinska and Miller. Unfortunately, most of this work was performed before the war and the experimental material was lost. Perhaps the most important published work was that concerned with the problem of interrelations between alimentary and defensive type II conditioned reflexes. The procedure of these experiments was that, using dogs, type II conditioned reflexes, both alimentary and defensive, were established. Raising the foreleg was the alimentary type II response, and raising the hindleg was the type II defensive response (active avoidance). It was shown that the animals never exchange the defensive response for the alimentary one, and vice versa. On the other hand, when the animal was trained to perform two different movements in response to two different stimuli, *both* under food reinforcement, exchange of these responses occurred quite often. This work was published in Polish just before the war in a practically unknown scientific journal (Konorski, 1939).

It should perhaps be noted that during this time Miller and I became involved in an interesting discussion with Skinner. In 1935 Skinner published a paper entitled "Two Types of Conditioned Reflex and a Pseudo-type" (Skinner, 1935). In this paper he developed ideas somewhat similar to ours, showing the existence of two types of conditioned responses. Skinner called these type I and type II, but his type I was what we called type II, and vice versa. (Only later did he introduce the terms "respondent behavior" and "operant behavior," which are now in common usage.) It was because we did not agree with his approach to the problem of distinguishing between the two types of responses that this discussion developed (Konorski and Miller, 1937b; Skinner, 1937; Konorski and Miller, 1937a).

This discussion seems to me to be not quite obsolete. Skinner performed experiments based on his now well-known experimental procedure (the "Skinner box"). A rat was introduced into a box with a lever; time and again among his other activities the animal pressed the lever, and this was followed by presentation of food. As a result the animal learned to perform lever pressings with maximal frequency. Accordingly, Skinner considered that the formation of the operant responses consisted in the increase *of probability* of the performance of that movement. Since Miller and I worked with a quite different technique, in which we evoked a given movement either by passive flexion or by electrical stimulation of the paw, we argued that Skinner's explanation could not hold, because the probability of the performance of the movement of the animal before it was trained is in our experimental situation simply zero.

These were my main scientific activities in the thirties. They were abruptly cut short on September **1,** 1939, when Germany invaded Poland. In a few weeks our country was defeated, which led to a complete change in our way of life and to a long period of insecurity and hardship.

THE WAR (1939-1945)

I don't think it would be sensible to attempt to describe the days of the beginning of the War, which I remember very vividly and in great detail. The panic of the population, the smell and sight of the fires, the rapid advance of the German Army toward Warsaw—in five days they were already at the outskirts of the city—the bombardment by artillery of the Nencki Institute, which was situated on the western outskirts of Warsaw and which the Germans wrongly took for the military building situated in the vicinity, the gradual deterioration of Warsaw when the water supply, the electric power, the transport and finally the telephone contacts were extinguished—all this is beyond the scope of this narrative, which is supposed to be a scientific autobiography. Our only scientific occupation during that time was directed to carrying scientific books, reprints, experimental materials from one place to another, according to which place seemed to us for some reason most secure.

After a three-week siege of Warsaw, during which a heroic resistance was maintained in spite of the complete dominance of the Germans in the air and on land, the municipal authority decided that further resistance was absurd and the city surrendered. In the first months of occupation our main task was to put our scientific materials in order. In particular, since I had already written a few chapters of my prospective book on conditioned reflexes, we tried to type them in several copies in order to save them from destruction. The days in October and November were already short, and since there was no electricity we brought home accumulators from the laboratory; and this was the source of our light, which had, of course, to be maximally economized.

In October or November we received information from England through Belgium (which was not yet in the war) that a friend of ours in Cambridge had transferred a sum of money to Riga (the capital of Latvia) in order to help us come to London via Scandinavia. Many people used that route, which was not particularly difficult. Because of the great chaos on the frontiers, travel between the countries was not very dangerous then. Therefore, we decided to go east in order to reach Riga. However, a few days after our arrival Biatystok (a Polish city which was then in the hands of the Soviet Army) the frontier between Russia and Latvia was closed and there was no possibility, except with great danger, of crossing it.

Since we had to remain in Bialystok, I began to work in the large psychiatric hospital near the city. I got in touch with my colleagues at Pavlov's laboratory in Leningrad, and they lent us a helping hand. I obtained a formal invitation to come to Leningrad to attend a scientific session concerned with conditioned reflexes and to present a paper there. This invitation obliged the Soviet authorities to facilitate our journey, and so, in May 1940, I arrived with my wife in Leningrad, where we received good care and help on the part of my Pavlovian colleagues. By the way, this shows the great solidarity among scientific workers all over the world. The efforts of our colleagues in England to bring us to that country, for one, and the concern extended to us by people from the Pavlovian laboratories, for another, are excellent examples of this solidarity.

The helpfulness of my colleagues in Russia went even further, for they proposed that I be made head of the Department of Physiology in the Subtropical Biological Station in Sukhumi, Caucasus, a famous Primate Research Center. This was arranged so quickly that in the beginning of June 1940, we found ourselves in a most beautiful submontane place, situated on the shore of the Black Sea. The war was so remote from this place that one could almost forget about it, which was of course quite unthinkable in our situation. Dr. Lubinska and I obtained a very nice laboratory (whose previous head went to another place), and we had full freedom to develop scientific work according to our own plans.

The chief aim of the Primate Biological Station in Sukhumi was to breed monkeys and either to utilize them for scientific purposes in the laboratories of the station or to send them to other institutes concerned with experimentation on animals. In accord with the first purpose, the Station had laboratories of primate biology, psychology, physiology, higher nervous activity, immunology, and cancer research.

I decided to continue my work on type II conditioned reflexes, now utilizing monkeys instead of dogs. I had two good soundproof chambers and I equipped them with several instruments which played the role of manipulanda. There were levers to be pressed, chains to be pulled, buttons to be pushed. The animals were trained to perform various responses to various conditioned stimuli, or to establish chain conditioned reflexes, in which the responses had to be performed in a definite sequence.

Our relatively quiet life, disturbed only by our worrying about the fate of our friends, came abruptly to an end on June 22, 1941, when it was announced by radio that the German Army had entered Soviet Russia and that war was declared.

We understood at once that the continuation of our work along the same line would be completely inappropriate and we decided to work in the field, which might be of some value for practical war medicine. Accordingly, some quite new projects were begun, namely studies of traumatic neuroses and their treatment, and of nerve regeneration after the cutting of nerves, performed on dogs and monkeys. On the last topic we discovered that nerve regeneration occurs much faster than had so far been supposed (3 to 4 mm/day, not 1 mm), and that it is easy to find the tips of the regenerating fibers because of their strongly increased mechanical excitability (responses to very light hitting).

Although there were no apparent changes in our life at the beginning of the War, gradually the situation became more and more distressing. The swift victories of the German Blitzkrieg were most depressing, many young men from the Station were mobilized, and the economic situation went rapidly from bad to worse. There was a period when the German Army was quite near Sukhumi, and it was decided that the Institute should be evacuated partly to middle Asia and partly to Tbilisi, the capital of Georgia. Because of this I stayed for a number of months in Tbilisi, where I worked in the neurological wards of an army hospital. Since the front was very near at that time, there were many cases of traumatic concussion of the brain, and I studied their symptomatology in great detail.

Again I shall leave out the description of my most variegated experiences during this difficult time, when the fate of Soviet Russia and the whole world was at stake. I would only like to emphasize the most admirable attitude of the Soviet population and the Soviet Army during this difficult period. There was no tendency to panic in spite of the most dangerous situation, and the people's patience was infinite. I heard many stories about the extraordinary endurance of the Soviet soldiers, and I could confirm these from my contacts with them in the hospital. It was most amazing to see how the Soviet nation, which seemed to be on the verge of disaster, found enough moral strength to transform the impending defeat into victory. I think that only those who witnessed this transformation can properly appreciate it and give it full credit.

The first spark of hope was when, quite unexpectedly for all of us, in the middle of defeats the Soviet Army began a tremendous offensive at Stalingrad, which after a few months of bitter fighting ended with a smashing victory. However, at that time, the beginning of 1943, the situation was far from being clear and the outcome was still uncertain. Only in the summer of 1943, when the German offensive on Moscow was crushed and the Soviet armies began their magnificent advance, did it become clear that the war was won and that final victory was not very remote. The time was exceedingly hard. There was hunger all over Russia, but it was now certain that this hardship would eventually come to an end.

In the spring of 1945 we decided to end our stay in Sukhumi and go to Moscow, in order to return as quickly as possible to Poland. We already began to think about the new life in our country and about restoration of Polish universities and Polish science. In Moscow we met our good friend Professor

Dembowski, one of the previous directors of the Nencki Institute. After much deliberation we decided to restore the Institute. At the end of August we returned by Soviet train through Brest to Warsaw.

FIRST POSTWAR PERIOD (1945-195 5): IN LODZ

The return to Warsaw after six years of peregrinations was full of excitement, joy, sadness, and hope. Only then did we learn how many of our closest friends had perished during the War. Among them were Stefan Miller and his wife, both of whom had committed suicide during the extermination of Jews by the Nazis.

Warsaw was in ruins. Only the suburb of Warsaw, Praga, situated on the right side of the Vistula, was not completely destroyed, because it had been liberated earlier by the Russians. Therefore, almost all the people lived then in Praga and the government had its seat there. It was most impressive and encouraging to see how quickly the authorities and offices came into being and how energetically they began to work.

The first people whom we met were Dr. and Mrs. Niemierko, our old friends who before the war had worked in the Nencki Institute. Again we discussed together the problem of our future and decided that the Nencki Institute should be reestablished. With this project we went to the Ministry of Education, where we saw the director of the newly created Department of Science, Professor Arnold, a well-known Polish historian, and presented our plan to him. In half an hour the plan was accepted and two friendly couples, my wife and I and Dr. Niemierko and his wife, formed the organizational committee of the Nencki Institute. We chose Dr. Niemierko as chairman of the committee. Our decision was immediately accepted by the Minister of Education, Mr. Wycech, who signed the appropriate certificates. It is curious to note how quickly every new idea could be materialized at that time, because there existed no bureaucracy which could delay and hamper any new project, at best, or cancel it, at worst.

Since Warsaw was totally in ruins after the War and our prewar Institute was almost completely demolished, it was clear that we should place the Institute in another city, at least temporarily. After some hesitation we decided to settle in Lodz, which was still intact because the Germans withdrew from it very quickly, without a battle. Among other motives which determined our decision was that the new University was being organized in Lodz and Professor Kotarbinski, our very close friend and famous philosopher, was nominated its President. Again a problem was solved in five minutes; Professor Kotarbinski offered the chairs of animal physiology and neurophysiology to Professor Niemierko and to me, respectively. It was decided that we should have two positions, one in the University and the other in the Nencki Institute.

Soon we visited the city governor, who was very pleased to know that well-known scientific institute would be placed in Lodz. It should be noted that before the War Lodz was only an industrial city; and it was a sort of Cinderella with regard to cultural life. The new authorities of Lodz had very high cultural ambitions and set out to establish several higher schools, including the University, a higher technical school, and others.

The governor of Lodz offered us temporarily a small building where the Niemierkos, my wife, and I could have very modest apartments and where the germ of the Institute could be established. In one year the city authorities kept their promise and offered us a much more suitable building where regular laboratories could be arranged.

In 1947 the Institute was ready to begin experimental work. It consisted of three main departments: the Department of Biology, whose head was Professor Dembowski following his return from Moscow; the Department of Biochemistry, whose head was Professor Niemierko; and the Department of Neurophysiology, headed by me. Professor Dembowski became Director of the Institute.

I shall leave out a description of the seemingly insurmountable difficulties connected with the organization of our Institute. We had to work in a complete vacuum, having no equipment, no trained staff, and even no furniture. Still, however extraordinary our deed seems to me now, this was quite typical and usual for that time; everybody was confronted with the same situation. One cannot imagine the enthusiasm which at that time animated the Polish intelligentsia. There were no bureaucratic obstacles and limitations, and through energy and dedication one could bring every project into existence.

The difficulty of our situation was that between our generation (people from 45 to 60) and very young people (about 20 years of age) there was a generation gap, because most of the people who would then have been about 30 had been killed during the war or in the Warsaw uprising. Accordingly, when assembling the scientific staff, we simply took people who were in their first year of university studies. We were their teachers in the University and their bosses in the Institute. It was not usual that they were starting their scientific career so early, but there was no other choice. We were so anxious to begin our work that we had to expose our young colleagues to a not quite correct order of education.

It should be added that these people did overcome successfully these disadvantages and became highly competent scientific workers. Most of them have remained in the Institute and are working in it at present. After a short time a few previous workers of the Institute joined us, and this group came to constitute the core of its staff.

Returning now to my own history, it ran as follows: After I returned from Russia in 1945, my idea of writing a book that would present the whole field of conditioned reflexes "translated" into the language of modern neuro physiology grew completely ripe. Since in early 1946 there was still not very much to do in the Institute—the work began only in 1947—1 decided to devote my time mainly to writing the book. During my stay in Moscow I had spent all my days in the Lenin Library, where I studied all the experimental works on conditioned reflexes. Having these materials now at hand and having my ideas completely ripe, there was no great difficulty for me in organizing them, and in less than one year the Polish manuscript of the book was ready. I entered into correspondence with Dr. Waddington in England, who was editor of the Cambridge Biological Series, and explained the project to him. He readily accepted it and proposed that I find a competent translator in England with whom I would cooperate. I received from our Ministry of Education a modest fund to go to England, my travel expenses having been covered by the British Council. So in the early summer of 1946 I went to London, where I met a great number of my old friends, among them my elder brother, who had emigrated from Poland in 1939. He helped me considerably in my accommodation, and I was very lucky to find an excellent translator, Mr. Stephen Garry.

Since Mr. Garry was not a specialist in the field concerned, all of the technical translation rested with me, while his duty was to shape the sentences and phrases properly. Accordingly I spend most of my time in the libraries to learn technical English expressions from the books on conditioned reflexes. During that time I became so thoroughly acquainted with the terminology and idioms used in this field that since then I have been able to write all my publications directly in English, perhaps not quite correctly, but without special difficulty.

After four months of hard work, the translation was completed and submitted to Dr. Waddington. It appeared that it was indeed quite good, and only a few corrections were made by scientists who kindly read the typescript.

The book was published in 1948 by the Cambridge University Press. Its title was *Conditioned Reflexes and Neuron Organization* (Konorski, 1948). Its reviews were generally favorable, although living in Poland and having no relations with Western scientists I had no feedback about how it was accepted. My feeling was that while in England the book became quite popular and well known, in America it passed almost unnoticed, judging from the very sparse references to it in papers and monographs concerned with the problems of conditioning. My explanation of this fact is that at that time (1948) experimental psychology was strongly Skinnerian or Hullian, and physiological explanations of the mechanisms of conditioned reflexes were utterly unpopular. I suspect that either people did not read the book at all, not being attracted by its title, which to me seemed highly attractive, or else, if they had it in their hand, they rejected it.

Quite different was the attitude toward my book in Soviet Russia. After a big conference in Moscow in 1949, at which were present all of Russia's prominent scientists engaged in research on higher nervous activity, Pavlov's concepts were announced to be obligatory, and no deviations from them were to be tolerated. As a result, a number of first-class scientists, such as Orbeli, Beritoff, and Anochin, were denounced as revisionists and strongly criticized. Therefore it was no wonder that when my book became known in Russia it also drew bitter criticism and disapproval, which considerably reflected on my position in my own country. In spite of the difficult situation for our laboratory produced by this circumstance, our research work developed normally and we did not yield to any demands we were asked to fulfill.

While we were in Lodz there were a dozen scientific workers in our laboratory who began as undergraduates and only after several years received their degrees. During that time my chief aim was to perform experimental studies directly related to the hypotheses put forward in my book. The other objective was to resume the work on type II conditioned reflexes which was interrupted during the War.

Perhaps the most important accomplishment in the former field of investigation was a radical change of my views on internal inhibition, caused by completely unexpected results of our experiments (Konorski and Szwejkowska, 1950, 1956). The concept, put forward in my monograph of 1948, asserted that internal inhibition arising in the course of extinction of differentiation of conditioned reflexes was a synaptic process, and that inhibitory conditioned reflexes are due to the inhibitory synapses being formed between the center of the conditioned stimulus and the center of the unconditioned stimulus. These inhibitory synapses were supposed to grow side by side with excitatory synapses, so the inhibitory reflex was considered to be in essence an excitatory-inhibitory reflex, because the connections between the two centers were considered to be both excitatory and inhibitory. It was further assumed that inhibitory connections develop because of the prior existence of excitatory connections; accordingly, the strength of the inhibitory conditioned reflex was thought to be positively correlated with the strength of the excitatory reflex from which it was formed. However, to my great amazement, the results of our new experiments were just the reverse: it turned out that the stronger was the original excitatory reflex to a given conditioned stimulus, the weaker was the inhibitory reflex produced by that stimulus when it was not reinforced. In other words, the inhibitory connections were not enhanced, but prevented, from growing, when the excitatory connections were already there. Conversely, the strongest inhibitory conditioned reflex as judged by the difficulty of its transformation to the excitatory reflex, was established when a given stimulus from the very beginning of its presentation was not reinforced by the unconditioned stimulus. Of course, this finding compelled me to change radically my view on internal inhibition.

The second line of research, no less important than the first, was the study of the mechanism of instrumental (type II) conditioning. The first achievement in this study was the new paradigm of this conditioning proposed by Wyrwicka (1952). On the basis of her experiments she came to the conclusion that there are double connections linking the "center" of the conditioned stimulus with the "center" of the instrumental motor act. On the one hand, there are indirect connections running through the center of the unconditioned stimulus—in the case of alimentary conditioning, through the food center. On the other hand, there are direct connections linking the two centers. Both kinds of connections must work together in order that the instrumental conditioned reflex be produced. It has been shown that many experimental facts from the field of instrumental conditioning can be explained well by reference to this model.

In this period a new field of investigation was also opened up in our laboratory. I felt very strongly that we should broaden our research work by studying the functional organization of the cerebral cortex, using ablation techniques on animals trained in various types of conditioning. Unfortunately, I had never had the opportunity to learn neurosurgery. But again we were lucky, because a prominent Polish neurosurgeon, Dr. Lucjan Stepien, became strongly interested in our scientific work, and joined us. As a matter of fact, he taught brain surgery to the entire group, and he raised this important technique to a high level of performance in our laboratory.

The first study performed in this field was concerned with ablations of the prefrontal area of the cortex. The main reason for entering on this problem proved to be quite erroneous. I was disappointed by the fact that our dogs were too "clever" and would not behave like conditioned reflex machines, responding automatically to our stimuli, in a regular and predictable manner. On the contrary, quite often they reacted inappropriately or not at all to a given conditioned stimulus, according to its actual significance. Instead they responded to more sophisticated cues, based on the whole stereotype of the conditioned reflex sessions, which enabled them to determine whether the next stimulus would be positive or negative. Therefore, I thought that the prefrontal area might just be responsible for this higher order of behavior and that, when we removed this area, the animal would become a conditioned reflex automaton similar to the spinal reflex automaton obtained after decerebration in Sherring-tonian experiments.

This hypothesis was not confirmed; instead, we discovered the important fact that after prefrontal lesions, the inhibitory conditioned reflexes are strongly disinhibited while the excitatory reflexes remain unchanged. This finding was afterward examined in great detail and many experimental studies were performed to elucidate the mechanism of this phenomenon (Brutkowski et al., 1956).

Another project developed during this period was concerned with the role of the motor area in instrumental responding. The working hypothesis was that this area was a "site" of instrumental responses and that after its ablation these responses should be abolished.

This prediction again appeared to be wrong, because although the instrumental responses *were* usually impaired, or even absent after the operation, they were always recovered after some period of time without any additional training. Since we thought that this spontaneous recovery was due to the small extent of the lesions. We performed several operations on one subject in succession. All these operations, in general, did impair to a greater or lesser degree the *performance* of the movement, but failed to abolish the movement as a behavioral act. In plain language, the animal *knew* what he had to do, in spite of the fact that the execution of the movement was deficient (Stepien et al., 1961).

In this period I also became interested in the effects of lesions of the cerebral cortex in humans. Here the cooperation with Professor Stepien appeared to be extraordinarily fruitful. We became particularly interested in the problem of aphasia and we made an attempt to establish a classification of speech disorders on the basis of localization of lesions in the cerebral cortex. This work was further developed and extended after we moved back to Warsaw.

SECOND POSTWAR PERIOD (1956-1970): BACK IN WARSAW

There are at least two reasons why I can divide my postwar life into two distinct periods, with the demarcation point being 1955. First, 1955 was the year in which, two years after Stalin's death, the "thaw" began, when Khrushchev came into power in the USSR and dissociated himself sharply from the Stalinist period, denouncing it somewhat euphemistically as "the cult of personality." This was immediately reflected in all fields of cultural life in the USSR and even more so in Poland. In my own field the pseudo-Pavlovian indoctrination vanished completely, and I stopped being a revisionist and a servant of capitalism. On the contrary, I became even more popular than before, because my earlier protagonists were now able to openly take my side, whereas my antagonists were simply ashamed of their previous conduct and tried to apologize. The sign of this radical change of attitude came when Professor Dembowski, the President of the Polish Academy of Sciences, charged me with the task of organizing a symposium on brain and behavior, of which I was to be the main speaker. A big lecture hall at the Palace of Culture was fully attended by an audience which greeted me with an enthusiastic ovation. I was elected a member of the Polish Academy of Sciences, an honor which had been refused me, ostensibly as "punishment" for my sin.

By the way, the change which occurred then turned out to be permanent; we were no longer taught and instructed by incompetent persons as to what was and what was not correct in our thinking, and the atmosphere in science became quite normal. Whereas in the Stalinist period we were completely cut off from Western scientists, this barrier was now removed.

The second big change which occurred in 1955 lay in the fact that in that year our Institute was transferred from Lodz to Warsaw, where a new special building had been erected. After the war the Institute was established in Lodz because the prewar building had been destroyed by Nazis, but the appropriateness of moving back to Warsaw seemed self-evident to all of us. The decision of the government to this effect was certainly due to the fact that our Director, Professor Dembowski, played an important role in the organization of science in

Poland after the war and was first President of the Polish Academy of Sciences. Besides this, the high scientific reputation of the Institute and its long-time existence was an important factor facilitating this decision at the time, when a strong tendency to restore the past and revert to old traditions was predominant in all fields of culture, except, of course, politics.

Although it was because of Dembowski that the Institute was moved back to Warsaw, the main burden of making detailed plans for the transfer fell again to the Niemierkos, my wife, and myself. The difficulty of the job was that the type of building best suited to our needs was relatively new in postwar Warsaw and the engineers were completely incompetent to design and construct it properly. Moreover, since the work on erecting the Institute had begun during the Stalinist period, and we were not allowed to go abroad to see how such edifices were built we had to rely on our own rather meager and obsolete experience.

Generally speaking, the Institute was in many respects first class. Among other things, my Department possessed ten soundproof chambers for the experiments on conditioning in dogs, operation rooms for neurosurgery (planned by Professor Stepien), and a large animal house for animals subjected to recurrent experiments. I consider all these details to be a part of my autobiography, because truly every detail was planned or accepted by me. I can boast that the Department of Neurophysiology, with all its virtues and defects, is my own child.

Another important event, both for me and for 'our Department, was that very soon I was sent forth by the Polish Academy of Sciences to visit the United States for several months to become acquainted with scientific centers concerned with brain research. My visit came into being at the end of 1957 and was successful owing to one man, to whom I will remain grateful all my life. This man was Robert (Bob) Livingston. I don't remember how I got in touch with him, but I do remember that he sponsored my visit and organized it in an excellent way. He was at that time Director of Basic Research on Neurological Sciences and Psychiatry, National Institutes of Health, and the headquarters for arranging my visit were in Bethesda. He had planned all my itinerary, choosing those places which he thought would be particularly interesting for me. Since I had given him a list of the titles of ten lectures concerning our work in the Department, one or a few lectures were scheduled in each place I was to visit.

I left Warsaw in December 1957 and remained in America for three months. First, I visited Bethesda and explored all the laboratories I was interested in. Thereafter, I made a great tour around the United States, visiting all the important centers concerned with physiological psychology. I visited both the east coast (Bethesda, New York, New Haven) and the west coast (Los Angeles, Stanford, Berkeley), as well as Middle America (Ann Arbor, Bloomington, Chicago, Madison, Rochester, Urbana).

Although from that time on I was a very frequent visitor in America, I do remember most clearly my impressions from that initial visit.

My first impression was that of a most efficient organization of my trip and the great hospitality of my hosts. In each place I had a host who met me at the airport and took care of me during the period of my stay. As a matter of fact, I was the first scientist concerned with brain research to visit this country from Poland after the war, and probably the first from Eastern Europe, because during the previous era this had been practically impossible. Perhaps this was the cause of the curiosity which I aroused wherever I went. But everywhere I encountered a most friendly reception.

Second, to my great amazement and pleasure I learned that I was not unknown in the United States. Since so far almost all my papers had been published in Polish or Russian (except my monograph of 1948), I thought that people would not even have heard of my existence. Sometimes it was true, but not often.

Third, what I had expected was that most people whom I had to meet would be Skinnerian, and that consequently I would not find a common scientific language with them. I expected that people would argue bitterly with me and would not acknowledge the physiological approach to behavioral phenomena, the approach I represented. This again was not true. I had not realized what a great change had occurred in America in the preceding years and how much my approach was welcome. It was quite clear that a new era had already begun in America, an era of increasingly close cooperation between brain physiology and behavioral sciences. Consequently, instead of finding scientific adversaries and antagonists, I found in almost every place I visited scientific friends.

This being so, it was not surprising that my visits in various places immediately led to establishing close friendly relations with the people I met, at first scientific and then also personal. These relations turned out to be long-lasting, since they have remained up to the present time and have become even closer.

I was also happy to learn that in spite of the great distance between America and Poland, and the lack of direct relations, I was quite well acquainted with new American achievements: wherever I went, I had known in advance the research that was going on. This was due to the fact that we had established an excellent library in the Nencki Institute, and had all the important journals in the field.

This first visit of mine to America had very great significance for the further development of our scientific work and its relation to the work going on in the United States. Owing to my connections with American scientists, almost every member of our laboratory was able to visit the United States for one or two years, which gave him (or her) the opportunity to become acquainted directly with the American investigations; this significantly enlarged their scientific horizons. Consequently, our work became much better known by American scientists than it was before.

If the exchange of scientific information can be regarded as some sort of "intellectual market" in which the intellectual goods representing the results of research are sold and purchased, our laboratory—to my great satisfaction—was included in this market, and thus the long period of isolation came to an end.

Of course, the new political period, which began in 1956 and was characterized by the end of the Cold War, enabled the establishment of scientific relations not only with the United States but also with most countries of Western Europe.

There was also great improvement in the relations between our group and the scientists of the Eastern countries. In the previous period, these relations had been practically nonexistent, because of their boycott of me, which extended to my whole laboratory. Truly, the boycott was rather advantageous for me, because I could avoid pseudo-scientific discussions, which were characteristic for that period and had a rather negative effect on the development of the younger generation.

Now the attitude of Soviet scientists toward me changed almost overnight. They became friendly and began to invite an improvement in our relations, feeling that bygones should be bygones.

As a result of this change of atmosphere, three laboratories dealing with the physiology of the central nervous system—the Institute of Higher Nervous Activity and Neuro physiology of the Soviet Academy of Sciences, headed by Professor Asratian; the Institut of Physiology of the Czechoslovak Academy of Sciences, represented by Dr. Gutmann, and our laboratory—decided to arrange a common symposium, which was held in Poland in 1959.

This symposium was a great success, both scientifically and socially. We learned that there are in these other two institutes very nice and valuable workers in science. Friendship between the laboratories became stable, and we arranged similar symposia thereafter every three or four years.

Returning to our relations with America, the closest bonds were established between our laboratory and the Section on Neuropsychology of NIMH headed by Dr. H. E. Rosvold. At that time a very wise idea was proposed that the Polish debts to America, which were to be paid in zlotys, should be utilized for scientific purposes. This was done in such a way that certain Polish scientific projects in which American science was interested could be supported from these funds, according to Public Law 480. In 1962 we signed an agreement with the Section on Neuropsychology of NIMH. It was extremely valuable for us for financial reasons, and also led to very close contacts, both scientific and personal, between the scientists of the two groups. This agreement is in operation now and its range has even been enlarged.

The reason I say so much about the life of our laboratory is that all these things are a part of my autobiography and inseparable from it.

Owing to my frequent visits to the United States and to the visits to Warsaw, of many American students of brain research, I became increasingly popular in the United States. People realized that we (Stefan Miller and myself) were the first investigators to introduce instrumental responses into conditioning experiments. Perhaps this is the main reason why in 1965 I was elected foreign associate of the National Academy of Sciences. Of course, from an objective point of view, it does not matter at all who has made a given discovery; still it does matter to the person who made it. Although for a scientist the esteem and recognition of his colleagues do not play a decisive role in his endeavors, nevertheless they cannot be ignored. Accordingly, if somebody makes a discovery which he considers important but which is overlooked by other specialists, he feels frustrated. But he is even more unhappy if *he* was the first who did it, yet the discovery is ascribed to someone else. I think that these feelings are deeply rooted in all of us, and I would be astonished to find anyone lacking them. When Stefan Miller and I discovered that our conditioned reflexes of the second type were different phenomena from Pavlovian conditioned reflexes, we fully realized that this was an important discovery and clearly saw the vast perspectives which were opened up by the introduction of these phenomena into conditioned reflex studies. Since we were not in the center of the scientific market but on its periphery, and since we had published only a few papers in the prewar period in French or English, we considered it quite natural that we were unknown. Then the war broke out and our original work would have been even more forgotten. The fact that it was not, and that my monograph *Conditioned Reflexes and Neuron Organization* did find appreciation among American scientists, gave me great satisfaction. The only regret was that Stefan Miller died prematurely and did not live to see this appreciation.

Since the transfer of our Institute from Lodz to Warsaw, our scientific facilities have grown immensely. As I said above, there are in the new building in Warsaw excellent soundproof conditioned reflex chambers, the dogs are kept under excellent conditions, and the surgery is on a high level. The prewar journal of the Nencki Institute, *Ada Biologiae Experimentalise* was taken over by our Department and has been devoted to studies on the brain and behavior. The journal is published in English and its title is now *Acta Neurobiologiae Experi-mentalis,* to define the scope of the problems with which it is concerned.

The scope of research dealt with in our department has been considerably extended in comparison with the preceding period. I shall present here briefly only those lines of research in which I was directly involved and which have contributed to the further development of my own ideas.

In the studies concerning the mechanisms of instrumental conditioning, the important discovery was made by Gorska and Jankowska (1961) to the effect that deafferentation of the limb involved in a previously trained response does not abolish that response; this means that the proprioceptive feedback of the limb performing a given acquired movement is not indispensable for the execution of that movement. This result clearly contradicts the theory of type II conditioning, originally advanced by Miller and myself, according to which this type of conditioning if fully dependent on the proprioceptive feedback.

Another important result was obtained by Tarnecki (1962), who has shown that a movement of the hind leg elicited by stimulation of the motor cortex, followed by food reinforcement, cannot be transformed into an instrumental response, whereas a movement elicited by the sensory cortex can. The analysis of this fact, together with some other data, has led to the conclusion that only movements elicited by the afferent input (including stimulation of the sensory cortex) can be instrumentally conditioned.

Finally, the experiments of Ellison and Konorski (1965) have shown that the alimentary instrumental response (or rather its kinesthesis) does not necessarily elicit the salivary response as was postulated by our original theory of instrumental conditioning.

These facts have considerably clarified my ideas concerning the mechanisms of instrumental responding. Another group of findings helped me to better understand the organization of central mechanisms of conditioned reflexes in general. Here belong the results of the experiments on the hypothalamus and the amygdala performed by Wyrwicka (Wyrwicka et al., 1960) and Fonberg (1967, 1969), which allowed me to distinguish two parallel systems determining the animal's behavior: the thalamo-cortical system on the one hand and the hypo-thalamo-amygdalar system on the other.

All these facts—combined with the most daring idea advanced by Soltysik, claiming that consummatory food conditioned responses (mediated by the thalamo-cortical system) inhibit instrumental responses produced by the hunger drive and mediated by the hypothalamo-amygdalar system—led to a new view of the mechanisms of conditioning. This view is much different both from my previous concepts and from the concepts advanced by other investigators.

The other line of inquiry begun in the preceding period, namely the study of the functional organization of various regions of the cerebral cortex, also gained new impetus. The most important task consisted in the analysis of the effects of partial prefrontal lesions upon animal behavior. It was found that disinhibition of negative conditioned reflexes and impairment of delayed responses (which was studied in our laboratory in great detail by Lawicka on dogs and cats) depend on different prefrontal areas (Brutkowski and Dabrowska, 1966 Lawicka et al., 1966).

To conclude this very short report on the development of some of our investigations over the last fifteen years, I should mention the continuation of our studies on the effects of focal cerebral lesions on the behavior of patients, undertaken with Professor Lucjan Stepien and his group in the neurosurgical hospital. We concentrated on the problem of speech disorders and came to definite conclusions concerning the pathophysiology of various forms of aphasia. The reader interested in this problem should consult my recent paper on this subject (Konorski, 1970).

From all these bits of evidence a coherent picture of integrative cerebral activity began to crystallize, a picture which allowed me to understand a great number of data both in the field of behavioral studies of animals and in the field of human psychology. The principle cementing this knowledge and providing the foundation for the architecture of cortical activity was deduced or rather extrapolated, from the important discoveries made in recent years in the physiology of perceptual processes. Here belong in the first place the studies of Hubel and Wiesel concerning visual perception. According to these studies, visual stimulus patterns are represented in the cerebral cortex, not by complex assemblies of neurons, as postulated earlier by Hebb, but by individual neurons which react selectively to a given pattern. This selectivity is achieved by the principle of convergence, by which the elements of the given pattern are addressed to a given neuron, and lateral inhibition, by which the foreign elements of the pattern are filtered out. One step from these findings leads to the hypothesis that all "unitary perceptions" of the natural stimulus objects of various modalities (known visual objects, sounds, smells, etc.) are represented by separate units in the "associative" or "gnostic" cortical fields. In this way we obtain a general model for perceptual processes, while the interconnections between the units of various gnostic fields provide a basis for associations between various experiences.

After having arrived at all these ideas I felt that it would be reasonable to present them systematically in a special monograph. The decision whether to undertake this task or not was far from easy. On the one hand, I had a strong temptation to do so, according to a general shortcoming of human nature, so well expressed by Bernard Shaw, who said: "When a man has anything to tell in this world, the difficulty is not to make him tell it, but to prevent him from telling it too often" (Caesar and Cleopatra). On the other hand, since I was already over sixty, I was simply afraid I might not be able to fulfill such a difficult task, requiring a tremendous mental effort, a good memory (which was clearly deteriorating in me), and great powers of concentration. The decision was made even more difficult by taking into account my numerous duties, both scientific and administrative, connected with the running of the Nencki Institute and my own laboratory.

In the midst of these hesitations I received unexpectedly a letter from the University of Chicago Press asking me whether I had in mind writing a monograph in my field, and if so, proposing they publish it. How they came to this idea I do not know, but it was a strong catalyzer facilitating my positive decision. In the summer of 1963 during the International Congress of Psychology in Washington, I met Mr. Richter, the assistant director of the University of Chicago Press, and after a thorough discussion the matter was settled.

I worked on this book for three and a half years, having many ups and downs, many moments when I was elated and full of enthusiasm, and other moments when I was completely broken and strongly blamed myself for having decided to undertake this job. Certainly, the work *was* too difficult for me, and there were periods when I was completely exhausted. The difficulty was even greater because the book was written directly in English, and time and again the proper formulation of my ideas presented great difficulties. But I clearly realized that writing in Polish and then translating into English would be even more difficult and unsatisfactory, because of the great differences in the idioms of these two languages.

Finally, in the summer of 1966, the book was completed, and the typescript was submitted to the Press. In about one year the book was published (Konorski, 1967).

Contrary to my expectations the reaction to the book was rather poor. I had a feeling that many of my friends and colleagues simply disliked it, or had not read it, or even did not know about it, because its advertising was inadequate. I had an impression that with a few manifest exceptions, the book was received coldly or even in an unfriendly way.

What I think about the book now is this. I consider, just as I considered when I was writing it, that the book is good and important. Some of my hypotheses have proved to be either inadequate or wrong, but this shows that they were good starting points for further experimentation. This was, for instance, the fate of my hypothesis concerning the mechanism of the cerebellar function as presented in the book. Starting from this hypothesis I began with Tarnecki experiments on the cerebellum which showed that my previous concept was wrong, but these experiments did lead us to a solution of the problem which seems to be correct (see Konorski and Tarnecki, 1970). On the other hand, other hypotheses which were quite daring when I proposed them seem now to be confirmed. This would apply to my idea on the gnostic units (Charles Gross, personal communication), or my idea about two types of units in the lateral hypothalamus responsible for hunger and the taste of food, respectively (Gallistel et al., 1969).

The great drawback of the book is that it is too concise, since within one volume I have condensed material which should be presented in two separate volumes. This makes the book difficult and requires very attentive reading and rereading. I was also told by my colleagues that some paragraphs are not sufficiently clear.

I am very curious to know what will be the final fate of the book: will it eventually win general recognition, which I think it deserves in spite of its shortcomings, or will it have no important impact on the further development of behavioral sciences. I am rather afraid that the latter fate may prevail because the investigations concerning the mechanisms of conditioning are still in the hands of experimental psychologists, who simply do not care about the physiological interpretation of the phenomena of animal behavior and have quite different frames of reference from those applied in my book.

I am now approaching the end of my scientific biography. You have seen that it began almost fifty years ago, when I first asked myself the question: How does the brain work? The question was general, because my knowledge was nil, but it was not naive, because I knew very well what I had in mind.

At first I was completely in the dark, because I failed to come across those sources in which this question had already been coped with. The first beam of light came to me from Pavlov's work, which stimulated me to begin studies on this subject myself, in close cooperation with Stefan Miller. We thought at that time that by specifying and defining *all types* of conditioned reflexes, the answer to our question would be found. That is why we called the conditioned reflexes we studied type II, hoping that afterward we should discover conditioned reflexes type III, type IV, and so on.

When through Lubinska I came across the Sherringtonian physiology of the nervous system based on Ramon y Cajal's notion of its anatomical organization, it was possible to project the studies on conditioned reflexes upon the actual network of the brain. I accomplished this task by writing my earlier monograph (Konorski, 1948). Thereafter came twenty-five years of investigation in our postwar laboratory, during which time the scope of our work grew immensely, and I became acquainted with the effects of cerebral lesions not only in animals but also in men. This allowed me to make a new step forward in understanding the functions of the brain, since I was able to broaden the foundations of my ideas by including perceptions, associations, skilled movements, behavioral acts, and drives. In consequence, I was able to present a new synthesis of the integrative activity of the brain (Konorski, 1967) based on new facts and new concepts. Since, in my experience, the full cycle of renewal of my scientific ideas requires about two decades, this is probably the last version of my thoughts about the functioning of the brain. Therefore, my scientific biography seems really to have come to an end.

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