HOW I BECAME A PHARMACOLOGIST

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I was born in St. Petersburg in 1892 and there in 1909 I graduated from school. During my last years in school I took a great interest in biology. Books on zoology and botany as well as books treating general problems of biology (heredity, variability, and origin of species) were among my favorites. This interest inspired my decision to enter the biological faculty of St. Petersburg University. However, I soon began to doubt my decision. To carry on both scientific work and a profession, I decided to obtain higher medical education. There was no medical faculty at Petersburg University then so young people who wanted a medical education entered the Military Medical Academy where not only future military but also civil physicians studied medicine.

The Military Medical Academy was famous for its former professors: surgeon Pirogov, therapeutist S. P. Botkin, neuropathologist V. M. Bekhterev, physiologist I. M. Setchenov, chemists Zinin and Borodin—such were the names. Borodin was not only a prominent chemist but also a composer who had created the opera *Prince Igor.* It seemed to me that there I would be able to master my beloved biology. I entered the Military Medical Academy.

There were zoological and botanical chairs at the academy, although at that time neither the academy nor any Russian university had special chairs or departments of general biology.

Being a first-year student I asked the head of the zoology department Professor N. A. Kholodkovsky, to allow me to work in his department. Kholodkovsky, an outstanding zoologist and parasitologist, was also well known for his Russian translation of Goethe's *Faust.* Kholodkovsky received me rather amiably and turned me over to his assistant. I was given a place to work, a microscope, and the task of drawing helminth ova. I labored quite assiduously, but the work was too far off from the general problems of biology I was keen on. Lectures also disappointed me. According to the tradition of the academy, most of my first year was spent on anatomy which required only memorizing without thinking. Histology was read by Professor A. L. Maximov, a well-known Russian histologist and a brilliant lecturer.

However, his lectures were of a descriptive morphological character and bore no relation to general biological problems. Lectures on botany delivered by Professor Varlich were of a soporific kind. By the end of the first year I had doubts about my choice of a higher school and I began to think about moving to another school.

However, my mood changed radically when I became a second-year student and began to attend lectures on physiology delivered by I. P. Pavlov. Pavlov was not an eloquent orator, but his lectures were amazingly interesting. He did not deliver the whole course of physiology but only those aspects in which he had worked himself and had made his famous discoveries: physiology of the heart, digestive tract, and brain.

He spoke vividly and gesticulated expressively. The expressiveness of his hands is imprinted in his portrait by a Russian painter, Nesterov. Pavlov illustrated most of his lectures by experiments on animals with subsequent analysis and discussion of the phenomena observed. Ideas underlying the experiments were so consistently and convincingly demonstrated that it seemed to the listeners that they themselves took an active part in discovering and explaining facts shown in the experiments. Those lectures of Pavlov's inspired me with a feeling of joy in scientific work.

When discussing the experiments demonstrated during his lectures Pavlov always paid attention to negative results in case the course of experiments was at variance with the expected evidence. Pavlov tried to find out a cause for the unforeseen results and, by analyzing the "failure," used it to widen the scope of knowledge and notions of his listeners. Pavlov often said that unexpected results were the most exciting feature of an experiment because these unexpected results and their analysis led to discoveries. Pavlov believed that a good lecture should necessarily stimulate questions. It was a rule at his lectures that any student could raise his hand at any time and ask a question. Having completed a part of his lecture he even made a special pause to encourage students to ask questions. It grieved him to hear none; in such cases he considered his lecture inadequate and would plead: "Questions, questions, I don't hear questions."

Pavlov's lectures did away with my doubts concerning my chosen career and inspired me with a wish to follow Pavlov and to occupy myself with physiological work. My zeal did not go unnoticed, and in my third year I was given a small grant for making the best progress in physiology and was permitted to carry on experimental work in Pavlov's department.

Thus I was very fortunate to begin my scientific career in the laboratory of the great physiologist whose benign influence I felt throughout my life.

Those aspects of physiology not covered by Pavlov's course were delivered by his assistants: B. P. Babkin, L. A. Orbeli, V. N. Boldyrev, G. P. Zelenyi, who later became famous physiologists. All of them attended Pavlov's lectures, and after each lecture used to surround him and discuss scientific and general problems.

Boldyrev became known because of his discovery of the so-called periodical activity of the empty digestive tract. In 1904 when working in Pavlov's laboratory he discovered that the empty digestive tract did not remain inactive but showed strong periodical contractions of the stomach with simultaneous secretion of pancreatic juice and bile into the duodenum. The exact physiological role of such

activity still is not known but Boldyrev attached great importance to it and supposed that enzymes entering the intestine during this process were absorbed by mucosa and took part as catalysts in tissue metabolism. According to Boldyrev's hypothesis, the enzymes participating in tissue metabolism were not synthesized in tissues but were a product of the pancreas for the use of the whole organism. This hypothesis proved to be quite fantastic, but as a young student, I thought it probable and readily agreed to Boldyrev's offer to participate in experiments designed to support the hypothesis and to extend them to the isolated rabbit heart. Boldyrev assumed that weakening of the heart activity after many hours of perfusion by Ringer-Locke fluid depended on washing away of enzymes from tissues. So he proposed to add blood to the nutritional fluid when heart contractions were weak to provide it with enzymes brought, according to his hypothesis, with blood from the intestine to tissues.

These experiments were a failure, because coagulated blood contains many biologically active substances that are toxic to the isolated heart. This fact, however, was then not known either to Boldyrev or especially to me. Still, those experiments benefited me as they made me learn isolated organ methodology.

An experiment which I planned and completed in 1912 as a third-year student in Pavlov's laboratory proved more successful. This work was meant to elucidate the cause of different positions of the frog body during strychnine convulsions, this difference depending on the site of injection. When I showed the results of my experiments to Pavlov he said: "When analyzing the action of drugs think always of possible participation of reflexes."

I reported this, my first experiment, at the Student Scientific Society of the Military Medical Academy headed by a professor of pharmacology Kravkov. Thus, I became known to him as a student interested in physiology and pharmacology.

The program of the meeting, which was posted in the student dining hall, read as follows: "Report of student S. V. Anichkov 'On the question of strychnine action on the frog *(Rana temporaria)*.' " A witty student added in pencil "and potassium cyanide action on the flea." He evidently wished to show the senselessness of such a subject. However some years later the insecticide action of cyanides was considered quite sensible.

The summary of my report was published in spring of 1912 in the *Proceedings of the Military Medical Academy,* and this paper gave a start to my scientific publications.

One can infer from this small episode that although physiology was the first great passion of my student years, my first experimental work was done on pharmacology.

The same spring I finished my third course, but for political reasons I was forced to leave the academy and even Petersburg, and only in 1913 did I get a chance to continue my medical education at Kazan University.

This is one of the oldest Russian universities, where such prominent scientists as N. I. Lobatchevsky, creator of the non-Euclidean geometry, A. M. Butlerov, builder of the modern theory of molecular structure, and a well-known physiologist I. A. Mislavsky had been professors. Young V. I. Lenin studied law at Kazan University and was expelled from it for participating in the student revolutionary movement.

As for myself, I had every reason to enter the University: my teacher N. V. Boldyrev had just been given the pharmacological chair there.

In Russia of those times there was a shortage of pharmacologists, and vacant pharmacological chairs were often occupied by physiologists and pathophysiologists. Thus Pavlov had occupied the pharmacological chair for several years before being appointed to the physiological chair at the Military Medical Academy. Later on he was succeeded by N. P. Kravkov, the pupil of a pathophysiologist V. V. Pashutin.

Thus, when I entered Kazan University it happened that Boldyrev had no experienced assistants, so he invited me, a fourth-year student, to help him during lectures with demonstrations and to carry on practical lessons. In this manner, while executing my duties as assistant I also enlarged my knowledge of pharmacology.

But my heart still belonged to physiology, and I enthusiastically set about the physiological theme given me by Boldyrev. He charged me with the task of studying motor activity of the empty stomach on myself. By that time an American physiologist, Carlson, had already shown that contractions of the empty stomach found by Boldyrev in dogs, might also be observed in men. He named them hunger contractions. But as he did only acute experiments he did not determine whether the same periodical alternating activity and rest periods as observed by Boldyrev in dogs took place in the empty human stomach. Also still unknown was whether secretion of pancreatic juice proceeded simultaneously with contractions of the human empty stomach. To study thoroughly the activity of an empty digestive tract it was necessary to swallow two thin probing sounds: one ending in a small rubber balloon for recording stomach contractions, and the other for collecting pancreatic juice. For both sounds to reach the stomach and duodenum I had to swallow them in the evening, eat a good supper and retain them overnight. In the morning I had to start experiments on an empty stomach. Very soon I learned to swallow the probing sounds easily and got accustomed to them to such an extent that I could hold them for 24 hours running and even longer without any unpleasant sensation.

In the evening, with the sounds sticking out of my mouth I usually went to supper with some of my girl acquaintances who studied at the so-called female higher school where women obtained higher education when coeducation of men and women in Russia was forbidden.

Most of the girls I knew came to Kazan from small towns and villages; their parents sent them farm products: chickens, geese, ducks, and pork, so I could always count on a tasty and substantial home cooked supper, which Boldyrev considered important for the strong contractions of the empty stomach to occur the next morning.

My work went on successfully, and several months later I could show that in men contraction periods 15–25 min long could be observed every 50–60 min and that during these periods pancreatic juice and bile discharged in the duodenum. The results of these experiments, which I reported at the sitting of the Kazan Medical Society, were then published in the Kazan Medical Journal in 1914.

The time spent at Kazan University is notable for me also because of the friends I made there. These were young people enthusiastically devoted to science. Every-

day contact with them strengthened my desire to occupy myself with experimental work. Our so-called leader was a young assistant of the histological chair, A. I. Mislavsky, son of the physiologist I. A. Mislavsky, a future professor of histology in Kazan. At the same chair worked B. I. Lavrentjev, a final-year student, later a prominent Moscow histologist, as well as a second-year student V. M. Karasik, my future colleague, a known Leningrad pharmacologist. At the physiological chair worked a young assistant K. M. Bykov who moved to Leningrad after the Revolution to work in Pavlov's laboratory and became famous for his works on corticoviscceral relationships. A. D. Speransky also became Pavlov's pupil some time after, but he had just graduated then from the Kazan University and was working at the chair of anatomy. Later on, already an academician, he became a widely known pathologist. But at Kazan, Speransky was experimenting on dogs in Boldyrev's laboratory implanting spleen into lungs to form anastomoses between the greater and lesser circulatory systems. Boldyrev permitted him to use his operating room for experiments but asked him in return to take part in my experiments. Boldyrev tried to introduce forcibly a rigid sound into our duodena. The X-ray control was accomplished by an inexperienced roentgenologist, and, as a result, Speransky and I got radiation burns on the skin of our backs, the traces of which I still retain.

All these young students at Kazan University were working at neighboring chairs. Meeting every day, we became friends and we retained our friendship for the rest of our lives. As I write these words, none of these friends is alive.

My tale of Boldyrev's chair and my Kazan friends relates to 1914. Next year I was to graduate from the University and dreamt of staying there as assistant to my teacher Boldyrev. But my life unexpectedly took quite another turn.

In summer of 1914 World War I broke out, and I went to the front, first, as a physician assistant and then, as a combat infantry officer. I was seriously wounded in January 1917, i.e. a month before the Tsarist government was overthrown. In summer of 1917, after a stay in the hospital, I returned on crutches to my native town.

After the February revolution, prohibition of the joint education of young men and women was abolished and young men had a chance to be admitted to the Petrograd Female Medical Institute equally with women. I entered the last, 5th course, and thus was the first male graduate of the former female institution. I had to study assiduously to make up for the lost years.

It was the time when great events were happening, the events which changed my country into a mighty advanced state with a new social system: the October Revolution broke out. When crossing the Neva by the bridge on the 25th of October (November 7, nowadays) I heard and saw a gunshot from the cruiser "Aurora" as well as crowds of rebellious soldiers gathering at the Winter Palace where the last session of the Provisional Government was being held.

Civil war and foreign intervention provoked by counterrevolutionary forces caused hunger and epidemics. I worked as a physician at cholera and typhus hospitals. I could then only dream of returning to Pavlov's laboratory and resuming my scientific work.

The situation in Petrograd was very grave; many of its inhabitants left for the south. But most of Pavlov's co-workers and young people in particular united even more closely round their teacher who bore hardships heroically. There were no vacancies in his laboratory then, while there was a shortage of assistants in the Military Medical Academy at the pharmacology chair headed by N. P. Kravkov. At the beginning of the 1918–1919 academic year, Kravkov invited me to work with him, and soon I was appointed his senior assistant. Thus my profession was determined at last, and I began to advance along the pharmacological road. I never regretted it for I consider pharmacology to have the advantage over other related physiological sciences: it is an active science that not only observes and explains vital processes but strives to influence them and to restore them to the norm. But I always wished to be a physiologist, too. Many of the prominent pharmacologists of the first half of the twentieth century, such as Dale, Loewi, Magnus, Heymans, and Liljestrand were interested in physiology.

Professor Kravkov was known by his works on vessels of isolated organs; he developed the isolated rabbit ear preparation. He offered me the task of studying the effect of drugs on vessels of isolated human fingers and toes. We began with fingers and toes amputated during operations and then turned to those taken from dead bodies. At the beginning of Ringer-Locke perfusion through finger and toe vessels they invariably went into spasm, and studying the effect of drugs on them was impossible. However, Professor Kravkov encouraged me to continue my experiments. My patience was soon rewarded, for within a few hours the vessels relaxed and a constant rate of perfusion was established, enabling one to observe the effects of different drugs. I had to procure the material for my experiments from hospitals, availing myself of the cases where a finger or a toe could be taken just after the death of a patient and quickly transported to our laboratory. In 1919 city transportation was far from good; trams were overcrowded and one often had to travel on the steps of the tram holding on to the handrail. Once during such a trip I was robbed of my purse containing instead of money a finger taken from a corpse. It was a loss to me, though I believe the thief did not consider my purse a happy acquisition.

The results of my experiments on isolated finger vessels were published in Russian and German and attracted the attention of pharmacologists. These experiments were used as the basis for my doctoral thesis, defended in 1922.

Having become a doctor of medical sciences I was appointed reader at the First Medical Institute, from which I had graduated four years before. The pharmacology chair was occupied then by Professor A. A. Likhatchev, a highly educated person. He knew modern European languages to perfection and old ones to such an extent as to take pleasure in reading Latin classics in the original. He was an exceedingly kind and noble man and was always a model of worthiness for me. Likhatchev gave me complete freedom in my work and helped me with his valuable counsel. I was then engaged in study of physiology and pharmacology of the digestive tract and used a combination of the operational Pavlov methods with the isolated intestine method of R. Magnus. The neighboring chair of physiology was occupied by L. A. Orbeli who had at his disposal many talented young scientists, for example, then a student, A. G. Gynecinsky, who studied the influence of sympathetic nerves on fatigued muscle. His name is known in connection with the so-called Orbeli-

Gynecinsky phenomenon. Professors Likhatchev and Orbeli were good friends, and their young co-workers formed a single community. Civil war and foreign intervention were by that time done away with, and the Soviet government took every step to improve the living and working conditions of scientific workers. In 1921 V. I. Lenin signed the well-known decree providing I. P. Pavlov and his co-workers with better working conditions. On Maxim Gorki's initiative the House of Scientists was founded, which supplied scientists with additional provision.

This period, one of the happiest in my life, ended with my unexpected promotion, which gave me a wider field of action.

In 1924 after the death of my first teacher in pharmacology, Professor N. P. Kravkov, I was appointed professor of pharmacology. To be the head of the chair considered as the best pharmacology chair in Russia was a very responsible and difficult task for a young man like me (I was 32 then). At that time the physiology chair at the Military Medical Academy was occupied by I. P. Pavlov who was succeeded by L. A. Orbeli. The pathophysiology chair was occupied by the famous N. N. Anichkov, my namesake. He was the author of the cholesterol theory of atherosclerosis. The outstanding Russian histologist A. A. Zavarzin read a course of lectures on histology, while the chemistry course was delivered by Professor S. V. Lebedev, the author of rubber synthesis. They were all not only prominent scientists but brilliant lecturers as well, so I had to work hard to be equal to the occasion. As a result of this difficult experience I learned to sympathize with young scientists who succeeded their outstanding teachers.

Next year, I was sent abroad for some months to study and visit laboratories. I went to the town of Rostock where the meeting of the German Pharmacological Society (Tagung der Deutschen Pharmakologischen Gesellschaft) took place the same year. At that time Germany was the world center of pharmacology, and not only German but many famous pharmacologists of other European countries participated in the meetings of the German Pharmacological Society. Two outstanding American pharmacologists, Reid Hunt and Carl Voegtlin, were there, too. Of all the lecturers in Rostock the famous English physiologist Starling impressed me most. He demonstrated the transplanting of an isolated kidney into his heart-lung preparation. I reported on the work of my chair that was the continuation of the last works of N. P. Kravkov who was the first to use his favorite method of perfusion of isolated organs to study endocrine glands. This method was especially useful when studying the action of drugs on an isolated adrenal. Starling's report made me think of transplanting an isolated adrenal of a dog into the heart-lung preparation.

After Rostock, I made a tour of Germany to visit pharmacology chairs, i.e. pharmacological institutes as they are called there.

Visiting pharmacology chairs of Germany and becoming acquainted with their leaders enriched me with the knowledge of the achievements of our science and introduced me to the methods which were new at that time.

I was greatly impressed by my German colleagues. Their attitude towards me, a young professor from a country with quite another social and political system, was irreproachable. Among the German pharmacologists I owe most of all to Paul Trendelenburg who then occupied the pharmacology chair at Freiburg in Bresgau, a wonderful small town in the South of Germany. P. Trendelenburg, a gifted and

learned scientist, was a person of high intellectual culture. He came from an old medical family: his grandfather was a physician, his father a well-known gynecologist, and his brother a professor of physiology. Among the German pharmacologists Trendelenburg was noted for his excellent works and it seemed only natural when he was invited in 1926 to head the pharmacology chair at Berlin University.

When in 1927 I visited Germany for the second time I worked at his chair for a month. His brilliant lectures were perfectly illustrated by experiments. One day we were talking during a dinner break in a café not far from Dorotheen Strasse (Klara Zetkin Strasse nowadays) where the Institute of Pharmacology is situated. I remember Trendelenburg's telling me that the pharmacotherapeutic action of glucosides of the digitalis group could not be adequately demonstrated because there was no method for obtaining a satisfactory failing heart. I referred to my work on the transplantation of an adrenal gland into the heart-lung preparation and noted that in a prolonged experiment a heart in the Starling preparation showed signs of failure. Thus our collaborative work on the action of strophanthine on the failing heart was conceived. We started our experiments together but continued them separately—I in Leningrad, P. Trendelenburg in Berlin—and we discussed our findings in letters. This work, the results of which were included in textbooks, can be considered an example of the friendly contacts between two laboratories in foreign countries. I must add that although the test object was proposed by me, the leading role in this work belonged to my teacher and friend Trendelenburg.

During my stay in Berlin I became acquainted with a number of Trendelenburg's pupils: Otto Krayer, Martha Vogt, Edith Bülbring, G. Kushinsky, and others. We have kept up our friendship to this day. Trendelenburg, a determined advocate of international scientific contacts, advised me when I was at Freiburg to visit Rudolf Magnus in Holland and Henry Dale in England and gave me letters of recommendation to both. In Utrecht I stayed in Magnus' laboratory for only two weeks but I got more out of them than from many years elsewhere. Magnus, one of the most important pharmacologists of the first quarter of our century, was extraordinarily thoughtful toward his co-workers, especially young ones.

The door of his study stood permanently ajar, and because the library of the chair was also kept there, all his co-workers could call on him any time for information and advice. Magnus also showed regard and consideration for his foreign guests. His assistants introduced me to various new methods that his laboratory was famous for. He himself spared much time for me, discussing scientific problems and questions of organization of work. Among other things, he said that he thought it useful to give young scientific workers not separate but large, well-equipped common laboratories so that they could learn from one another.

At the very beginning of the twentieth century Magnus became famous for his investigations on the isolated intestine, but by 1925, he had turned to his celebrated experiments on the position reflexes (Stellung Reflexen) and effect of drugs upon them. Magnus told me that every great pharmacologist sooner or later turned to pharmacology of the central nervous system. I was rather disappointed because at that time I worked exclusively on isolated organs but wanted very much to be a great pharmacologist.

From Holland I went to England. Diplomatic relations between the Soviet Union and England were then temporarily broken off, and I got but a seven-day visa and that only through the solicitation of Starling. My aim was to visit the laboratories of Starling and Dale. Henry Dale was not yet a Nobel laureate and baronet, but his works already ensured him world fame, and many young pharmacologists strived to work in his laboratory. My short stay in England did not allow me to become acquainted with all the methods applied in his lab, but I got an opportunity to establish friendly contact with Henry Dale, and our correspondence and exchange of scientific papers continued until his death.

Professor Clark, who is rightly considered to be father of general pharmacology, made a great impression on me. To my surprise he turned out to be a modest and even shy man. I spent one day at Cambridge where I visited a well-known pharmacologist, Dickson. At Cambridge I was a guest of my countryman and fellow student, P. Kapitza. He was a pupil of Rutherford. As a guest of Kapitza, who was a fellow of the Trinity College, I attended a dinner at the College. I still remember its gothic hall, the former hall of the medieval monastery, where clad in cloaks, fellows of the College sat with the master of the College, Rutherford himself, at the head of the bare oak tables. At Cambridge I got to know young Dr. Adrian, later the world famous physiologist Lord Adrian.

All my time in London was spent visiting pharmacological and physiological laboratories. I thought I had no right to spare even a few hours on museums and sightseeing. I did not even go to the British Museum, putting my visit off to some next time. But there was no next time: I have since visited many cities and countries of the world but I have had no occasion whatever to visit London again. I still cannot forgive myself for ignoring the world famous treasury of art.

The role of art in the life of a scientist was demonstrated to me by the prominent Austrian pharmacologist Otto Loewi when in 1935 he attended the fifteenth International Physiological Congress in Leningrad. Loewi took a most active part in the Congress but all his free time he spent at our famous Hermitage picture gallery. When going there he took a folding stool to be able to admire world masterpieces at his leisure.

In 1935 I had been head of the pharmacology chair for ten years, I had gained some experience, and the main line of my work was clearly defined: pharmacology of the carotid body. This I owe to my interest in the pharmacology of nicotine and also to remarkable works of C. Heymans on the carotid body. Heymans showed that nicotine stimulated respiration through reflexes from carotid chemoreceptors. When I think of this mechanism of drug action I recollect the words of Pavlov addressed to me, a young student then: "Think of a possible participation of reflexes."

I reported the results of my investigations at the pharmacological section of the fifteenth Congress. The Congress played a significant role in the fate of the Soviet physiologists and pharmacologists. Our government made it possible for us to receive our foreign guests in a fitting manner. Among the guests, besides the most outstanding, were young pharmacologists who became famous scientists later on: Gaddum, W. Feldberg, O. Krayer, E. Bülbring, Van Dyke, Claus Unna, Z. Bacq, G. Kushinsky, J. Levi. Free intercourse with these representatives of the young

pharmacological guard was for Soviet pharmacologists a very valuable scientific experience.

When opening the Congress at the Tavricheski Palace, its chairman I. P. Pavlov made an excellent speech in which he called war a brutal means of settling an argument. At a reception at the Moscow Kremlin in honor of the delegates, Pavlov raised his glass to the members of the Soviet government as great social experimenters. The banquet in honor of the delegates given at a suburban palace built at the time of the empress Katherine II was unforgettable. In the splendid halls there gathered the flower of world science. During the war fascists plundered the palace and burned it down. The Congress took place not long before World War II.

The war interrupted scientific work. Privations and hardships suffered by the Soviet people were incredible. My wife and children stayed in the blockaded Leningrad. The children exhausted by starvation were near death. My father-in-law died of hunger. My country lost millions of its sons and went through devastation and great distress. Still greater was the joy of victory and enthusiasm of postwar reconstructive work. The Soviet government spared no expense to restore scientific institutions and build new institutes and laboratories.

I was fortunate to stand at the head of a newly established pharmacology department at the Institute for Experimental Medicine in Leningrad, one of the oldest medical research institutes in Russia.

The main concern of our department became neuropharmacology, and we devoted our attention not only to the direct action of neurotropic drugs on the nervous system but also to their action through the nervous system on the whole organism, too, in particular on metabolism and the function of the endocrine glands both in the norm and during the so-called neurogenic dystrophy.

This was meant as an extension of Pavlov's ideas to pharmacology, Pavlov being one of the founders of the Institute for Experimental Medicine. Thus, I have come to pharmacology of the nervous system, i.e. of the sphere considered by R. Magnus to be the most important in our science.

In the course of the history of pharmacology the characteristic feature of the postwar period was the development of international contacts, first, in the form of a pharmacological section of the physiology union (SEPHAR) and then, in the form of an independent international union of pharmacologists (IUPHAR). I had many occasions to speak at international pharmacological congresses making not only scientific reports but also short speeches at banquets and official ceremonies. I always put forward two main ideas: according to the first, pharmacology is the best science in the world. This assertion always found support among the listeners, people who devoted themselves to this science. But still greater enthusiasm always met my other idea—that peace and international friendship are the best mode of life on our planet. The enthusiasm with which scientists regard these my words testifies to the fact that they sympathize with the idea heartily.

The leader of the Swedish pharmacologists, the late professor Liljestrand said to me once during a friendly talk that he believed himself to be a happy man because his life had been devoted to science. I want to repeat these words of my late friend: I, too, have lived a long and happy life.