THE GRIM HERITAGE OF LYSENKOISM: FOUR PERSONAL ACCOUNTS

III. HOW I BECAME A LYSENKOIST

ALEKSANDRA PUTRAMENT

Department of Genetics, Institute of Biochemistry & Biophysics, Polish Academy of Sciences, Warsaw, Poland

I HAVE A STRONG feeling that in discussions of the problem of Lysenkoism one important matter is overlooked, or at least underestimated: the element of plain, simple ignorance. Whatever else Lysenko was, above all else he was ignorant. This is very evident as I reread, nearly forty years later, his famous speech before the session of the Academy of Agricultural Sciences of the USSR on the thirty-first of July, 1948. Under the title of "Michurin's Teaching—The Basis of Scientific Biology," he wrote eleven pages of truisms, generalities, platitudes, and slogans. His attack on Vavilov in 1936, and on other geneticists in 1948, was surely dictated by envy and hatred alone.

I presume Western scientists are incapable of imagining the sheer enormity of the ignorance prevailing among the so-called scientists who were working in agriculture in eastern Europe, at least beyond East Germany and Czechoslovakia. I hope that my personal account may provide some idea of how it was in Poland, at least, directly after World War II. I am quite sure that the situation in the Soviet Union in the 1920s and the subsequent decades was no better.

When the Second World War started I was thirteen years old. The part of Poland we lived in was occupied by the Red Army. In February, 1940, together with my family, I was deported to Siberia. There we were put into a group of small settlements in the forest. Each settlement consisted of one or more barracks housing up to as many as thirty families. The nearest secondary school was in a village some 25 km distant, and there was no transportation. My parents decided that I should go to work in the forest, collecting resin. In that way I would earn some money and, what was more important, I would receive a daily ration of 800 g of bread instead of the 200 g allotted to nonworkers. Since bread was practically our only food, this difference was of the utmost importance. No school books were to be had, so that even had I been physically capable of learning anything after a very hard day's work, I would have been unable to do so. There were, in fact, no books of any sort. In the entire five years I spent in Siberia, I read about five novels. Occasionally, newspapers made their way into the settlements.

Until the summer of 1941, an NKVD representative watched over us. Anyone wanting to go to a nearby kolkhoz, for example, to barter clothes for potatoes, onions, or other food had to obtain a permit. Soon after the German invasion, however, we were "freed." That is to say,

Reprints of this article may be obtained for $3.00 from *The Quarterly Review of Biology*, State University of New York, Stony Brook, NY 11794-5275 USA. Please make money order or check payable to *The Quarterly Review of Biology*.

the NKVD person was withdrawn and we were treated as standard Soviet citizens. But irrespective of the NKVD, we had practically no social contacts. I became extremely shy.

During those five years in Siberia I received no political indoctrination. Yet gradually, after the beginning of the war with Germany, and the alliance of the USSR with Great Britain and later with the United States, the conviction grew in me that there was really only one enemy of all humanity, the Nazis. It followed that since the main opponent of the Nazis was the Soviet Union, and the Soviet political system was the most distant in nature from Nazism, therefore it must be the best. In comparison with the war, the fact that we were deported to Siberia seemed trifling. Thus I came to approve fully of Communism.

Even before the war my brother, sixteen years my senior, was connected with the Communist movement in Poland. In 1941 he escaped to Moscow, and later he participated in organizing the Polish Army in the USSR. After the war, he was for many years a member of the Central Committee of the Communist Party in Poland, but he completely avoided nepotism. He helped to support my mother financially, but we had no privileges connected with his position. We would not have accepted them, anyway. I do not think that my political attitude was affected materially by my brother's political views.

All universities and secondary schools were closed in Poland during the German occupation. Numerous secret schools were organized, but a considerable number of teenagers were unable to attend them. Thus, when we returned from Siberia in 1945 I attended special short courses set up for the "grown-up" pupils. In two years' time we were expected to complete the program normally requiring five years. That meant hard work, affected by the fact that practically no school books were available. In Poland, the war and the Nazi occupation lasted for nearly six years. During all of that time no school books— or in fact any other books—were printed. So we could learn only what the teachers told us in lectures. We took notes, and that was all we had to study. Before the war, in Poland, all pupils learned one foreign language, as a rule either German or French. After the war, we could choose Russian or English. Since I already knew Russian, I decided to take English. Our teacher, however, did not know how to teach the language, and no textbook of English was available. After two years I obtained the certificate of maturity that gave me a right to apply to enter a university. The certificate indicated I had received the best possible marks, but I knew myself that I was completely ignorant in every subject. In particular, I had no idea of the English language, and was sure that it must be too difficult for my mental abilities.

My family decided that they would continue to support me during my university studies. I did not know anything about such subjects as biology, chemistry, or psychology, among others, because I had never had an opportunity to discuss such matters with anyone. We were living in Cracow, and we had neither friends nor acquaintances there. I was very shy, and terribly ashamed of my ignorance. I decided to study agronomy, since I knew that Polish agriculture was extremely primitive and I hoped to be able to do something useful to improve it.

In the fall of 1947, to my great surprise, I passed the entrance examinations for the Faculty of Agronomy of the Jagellonian University. That university, founded in 1364, was one of the oldest universities in Europe. I was still sure that I would fail the first examination I had to take there, but no, I did not. As before, there were no books to be had. Again, we attended lectures, took notes, and when examinations were given we answered what we could on the basis of the lectures alone. For example, my entire knowledge of chemistry—general, inorganic, and organic—was limited to my notes in one notebook.

In the fall of 1948 we had about a 20-hour course on genetics. The lecturer, seriously disabled after several years in Auschwitz, presented the subject in its dullest and most formal aspects. At the end of the course he remarked, "Perhaps all this is wrong." I did not know what he meant, but I was impressed by the tone in which he said it.

In 1949, I obtained a copy of the famous speech delivered by T. D. Lysenko in the previous year and directed against "reactionary Mendelism-Morganism." I could understand only fragments of it, but it sounded wise and profound to a person too ignorant to understand it properly. I retained that conviction throughout the next several years. I did believe I understood certain passages. When Lysenko stated that the purpose of biology is to help in solving the practical problems of agriculture, to increase its productivity, and to feed the hungry, that made sense to me. As an example of pointless effort, Lysenko gave the scientific work of Dubinin, who determined the changes in a *Drosophila* population in a town that was heavily damaged during the war. Even now, I recall my consequent hatred of Dubinin. I thought, that man studied the population of *Drosophila* and ignored the fate of human beings. In this way, I became "hooked" on Lysenkoism.

Soon a "Society of Biologist Marxists" was founded. Among its members in Cracow were a few professors, a dozen lecturers, and some invited students. I was among the latter. Meetings were infrequent. I attended each one hoping to learn something, but the lectures and discussions were incomprehensible. Purely by chance, I have kept notes of one such discussion. The participants argued that the differences between individuals within a species are quantitative, while those between individuals of different species are qualitative. The speakers expressed a doubt: are differences, for example, between pinchers and hounds only quantitative?

During 1949-51, two or three short courses of the "New Biology" were organized. (Until 1956, the terms "New Biology," "Michurinism," and "Creative Darwinism" were treated as being synonymous. The term "Lysenkoism" was introduced in 1956.) In each of the short courses 200 to 300 students from all of the Polish universities participated. I do not know how they were selected. I remember vaguely that we were told about Michurin and Michurinism, Lysenko and his "stadial" development of plants, about Lepeshinskaya, Pavlov, Boshian, Oparin, and Williams.

A year before the end of my university studies, in 1950, the chairman of the Department of Plant Cultivation offered me a post as an assistant lecturer. I accepted it. The departmental library was very poor. We had no seminars and no scientific discussions. Some members of the department did experimental work, but I never learned what they were investigating. In order to obtain a Master's degree I had to carry out a small piece of research. It was on the effects of the synthetic plant hormone, the auxin 2,4-dichlorophenoxyacetic acid (2,4-D) on the development of roots in oat seedlings. I requested to see some scientific literatrure on 2,4-D. My scientific supervisor said, however, that because this compound could be used in biological warfare to destroy crops, all papers on the subject were top secret. Even the name "2,4D" thus acquired a sinister flavor. My teaching consisted of training students how to distinguish the seeds of different grass species.

It was easy, however, to obtain the *Collected Works* of Michurin. In each of those articles there were detailed instructions on the handling of different species of fruit trees. I did not even see the connection between these matters and genetics. I did think that there was some depth in his works that I failed to understand properly.

The works of J. T. Williams turned out to be very easy to comprehend. He discussed two principal problems. First, in the steppe regions of the USSR, and particularly in the southern part of the Volga Basin, crops are often destroyed by dry winds (sukchovey). Williams recommended that, in order to break the force of the winds and shield the crops against them, forest belts should be planted. Second, in order to keep the structure of the soil favorable for plants, the practice of growing crops in monocultures should be abandoned, and rotation of crops should be introduced. His first recommendation in particular appealed to me. Attempts to plant tree belts were indeed made in the early 1950s, but they were unsuccessful. The trees planted, usually oaks, simply could not survive on the steppes. No effort was made, it seems, to plant trees that could survive on the Ukrainian steppes.

As for crop rotation, I wondered what was new about it. In a primitive form, it had been practiced in Western and Central Europe, including Poland, from the Middle Ages. After potatoes, and later sugar beets and fodder crops, such as clover, were introduced in the 19th Century, crop rotation became much more refined. One very old professor — over 80 at the time — gave us extensive lectures on crop rotation and the effects of each plant species on soil structure, and similar topics. He mentioned in his lectures Russian specialists in the soil sciences — for example, V. V. Dokuchaev. This made me wonder why Williams presented his ideas as though they were novel. Not until

I began to write the present account did the revelation come to me. Williams was a sound, clear-headed, honest scientist, who wanted to modernize Soviet agriculture. In order to do so, however, he had to refrain from reference either to the "reactionary" Russian specialists of older times or to the "imperialistic" specialists of the West. For purely political reasons, he had to present the concept of crop rotation as if it were a purely Soviet production. The direct effect of introducing crop rotation would be to decrease the total area of wheat cultivation and thus, for a few years at least, its production. That created a political problem of the first importance.

In 1951 I obtained my Master's degree, along with a label as "young bright." I myself recognized the enormity of my ignorance, as well as my inability to find a proper research problem. I felt myself a fraud. So I collected my courage, went to the chairman of my department, and asked him what I should study. He swept his hand along the shelves filled with old German agricultural journals, and said: "This," and dismissed me. I wondered whether I should take his advice literally (first to learn German well enough to read the journals in order to find out what I needed to learn). Or should I take the advice metaphorically—that is, learn haphazardly, with the hope of eventually becoming erudite? I preferred the second alternative. It was easier.

Why did I not quit the job and turn to farming? During my studies I had actually spent a month on a state farm. There I learned two things: first, that I had no idea about practical farming at all; and second, that an important part of a farm manager's job is to give orders in such a way that they will be obeyed. I realized that I was completely incapable of giving such orders. Consequently, I was treated by the farm laborers with sympathy and patronizing tolerance, but not with respect or with fear. On the other hand, my classes with students went perfectly well. Whenever a student asked a question that I could not answer, I admitted freely that I could not, and my frankness increased rather than decreased their friendly respect.

Another year passed. Then a great event transpired. A course on the "New Biology" was organized and I was included among the 150 or so participants selected from all the Polish universities to take it.

The course began. I took notes of all the lectures and some of the discussions. As I have said already, up to this time all my learning had been based on listening to lectures and taking notes in longhand. That I could do well, provided I understood what the lecturer was speaking about. My notes from that course therefore reflect very well, I believe, the quality of the lectures themselves as well as my ability to follow them.

Before describing the course, let me first explain something of the problem with scientific degrees and titles in Poland. A university student is required to attend a certain number of lectures and to pass examinations afterwards. Then the student must do a minor piece of research in order to obtain the Master's ("Magister") degree. A more extensive piece of research is required for the Doctor's degree. After obtaining that degree, the research student must still pass a "habilitation" colloquium and deliver a "habilitation lecture" in order to obtain the level of "docent." Finally, whenever a docent proves to be active in research, the scientific community must provide an extensive analysis of his or her work, and recommend that the rank of "Professor" be awarded. All this follows the pattern formerly prevailing in Germany. The title of Professor is actually awarded by the President of the State.

During the war and the German Occupation, the educated fraction of the Polish population had suffered the most severe losses. After the war the Communist government, wanting to restore the life of science in Poland, went about it in a rather peculiar way. A certain number of people were chosen—I do not know by whom or just how—to be made professors. Some of them had doctor's degrees, but most of them had little, if any, experience in scientific research. Apparently the criterion was the title, rather than real knowledge. These professors acquired the right to teach university students, organize laboratories, and promote graduate doctoral dissertations. In 1956, they were named "professors from social promotion."

One such professor, a Professor P., was the spiritus movens of the course in the "New Biology." He had finished a university program in the early 1930s and attained his Ph.D. degree on the subject of ecological observations of spiders. The war years he spent with the guerillas. After the war, being a dedicated Communist, he held a variety of important jobs in the governmental administration. For a year or two he was an Under- Secretary of State on marine affairs. Then he was given the title of Professor and started to propagate the "New Biology." He was a kind man, really honest and very nice, and wanted neither power, riches, nor fame. His bequest to posterity was to be the introduction of the only "truly scientific" methodology: dialectical materialism.

Now, when I decipher my notes from that course, it seems possible to divide them roughly into three categories:(1) proofs of evolution, and the theory of Oparin on the chemical origin of life; (2) lectures and seminars with a more or less pronounced ideological inclination; and (3) four lectures on genetics. The lectures in the first category seem all right and need no further comment here.

The ideological lectures were given by Prof. P., two other zoologists, and a couple of other persons whose lectures I could not even begin to understand. I will use some excerpts from my notes to provide examples.

Prof. P. (an introductory lecture): "Science develops by collecting facts and constructing theories. . . . There are correct theories and wrong ones, such as the theories of phlogiston, preformism, and formal genetics. . . . It is not accident that at present the main front of struggle is between creative biology developed by Marxist scientists and the Western biology. . . . Darwinism has been developed and cleansed from errors by Michurin and Lysenko. . . ." In my notes I cannot pinpoint either the errors of Darwin or what the cleansing amounted to.

The two zoologists gave lectures on the history of evolutionary ideas and on the origin of life. They began with the ancient Greeks and went through the history up to Engels. Each philosopher or scientist was labelled as progressive or reactionary, materialistic or idealistic. According to my notes, Linnaeus included *Homo sapiens* in the Order Primates because in the 1750s there was a pre-revolutionary bourgeois atmosphere that was essentially progressive.

Prof. P. spoke—to judge from my notes—complete gibberish on the problem of species. He gave the following definition: "Species is a form of existence of living matter shaped in a historical process. . . ."

One of the zoologists, in a lecture on "Creative Darwinism," stated: "Creative Darwinism goes from practice to theoretical generalization and back to practice. . . ." I heard variations on this theme repeatedly, and I think I know how it originated. In Polish the word "practice" [praktyka; also Russ., praktika] has several related meanings, as in English. Lenin, in "Materialism and Empiriocriticism," stated that we check our sensory observation in practice, and he cited the English proverb, "The proof of the pudding is in the eating." (I had read this book in 1954, and was very pleased to understand the English sentence, and that is why I remember the context.) Obviously, what Lenin meant was that any physical activity, in contrast to mental processes, can show the reality of the outer world. Probably Lysenko, and certainly our mentors, understood this differently. They thought "practice" meant activities with an economic significance, such as agriculture. That is what Lysenko did himself, with the well-known results.

I turn next to the four lectures on genetics. An older professor of general biology (at least 70 years of age at the time) spoke about variation and mutations. He defined heredity as a norm of reaction of an organism to the environment, and mentioned the distinction between hereditary and non-hereditary variation. He also mentioned the Quetelet distribution and discussed the mechanisms of homeostasis. As I see it now, that was an honest lecture on the variation of quantitative characters.

Prof. X. spoke about heredity. He mentioned a number of examples intended to illustrate the inheritance of acquired characters (e.g., callosities on the legs of ostriches). He asserted that peach trees, when cultivated on islands of the Pacific, become evergreen. He mentioned phenocopies. He stated that variability is connected with changes of metabolism. Germ cells, he said, can be formed de novo (that is, from "acellular matter," vide Lepeshinskaya). Cells with new properties can originate by the assimilation of "feeding matter" from the environment.

Prof. Y. criticized the "chromosomal theory of heredity." I have only a few notes on this lecture: that Weismann's theory of germ cell lines was criticized long ago by the Polish biologist Nussbaum-Hilarowicz. The 3:1 segregation described by Mendel does not agree, he asserted, with statistical laws. The overdominance theory of heterosis is false.

Prof. Z. also spoke about heredity. To judge from my notes, I had no idea what he was speaking about. I find such sentences as the following: "The essence of inheritance is the type of metabolism, the type of relationship with environment"; "The inheritance of sex depends on the age of the females"; or "The ability to segregate is not restricted to hybrids."

After the lectures there was discussion. Prof. X.: Hemophilia does not depend on a single allele. Some of the horse-donkey hybrids are fertile. Someone said that Professor Nielson Eyle (Nilsson-Ehle?) found in Scandinavia a degeneration of oats (*Avena sativa*) into a weed (*Avena fatua*). Prof. P. stated that wild relatives of cultivated wheat are known, but not those of rye. Rye is constantly found in wheat fields as a weed. Thus wheat can degenerate into rye.

As a proof that the environment can provoke hereditary changes, colchicine and X-rays were mentioned. (Colchicine is a drug that inhibits chromosome divisions. X-rays have been known to induce mutations since 1927, a discovery made by H. J. Muller, who received a Nobel Prize for that in 1946.) I did not note who offered those examples, so do not know whether this was ignorance or cheating. It should be pointed out that at that time there were in Poland not more than a dozen people who really knew genetics. Among our teachers there were only two such persons, professors X. and Y. Prof. Z. was one of the "professors from social promotion."

There was also a lecture on the "biology of breeding." Here the term "vitality" was introduced. The speaker defined it ". . . as a property of an organism that regulates the volume and intensity of metabolism." In the following discussion an alternative definition was supplied: "Vitality is a force with which an organism demands the conditions for the realization of its heredity."

I can no longer remember how I learned that in Poland there was just one geneticist representing the reactionary Mendelism-Morganism. That was Wacław Gajewski. He had been invited to attend the course and spent several days with us. Presumably, under the force of the brilliant ideas being expressed by the brilliant speakers, he should have been converted to the progressive "New Biology." But he was not. Someone said to me, "Look, there is Gajewski." He did not look to me to be particularly vicious. Characteristically for the time, I never thought that he ought to be allowed to present his point of view.

In the fall of 1952 a circular came to our Faculty, with the information that special scholarships were now available. A person could apply for one and if granted it, could complete a doctoral thesis in three years, under the supervision of outstanding specialists. I was awarded one of them. The "outstanding specialist" to whom I was allotted, Prof. L., held two posts simultaneously: she was the chairperson of the Department of Plant Breeding at the College of Agronomy in Warsaw, and she was also Director of the Institute of Plant Breeding. When I met with her in Warsaw, she allowed me to choose the subject of my future doctoral research: either the degeneration of cultivated oats (*Avena sativa*) into the weed *A. fatua*; or the resistance of corn to corn smut. I did not question the possibility that one species can degenerate into another, but I did assume that it could not happen frequently. It therefore seemed risky to me to try to find such cases within the span of three years. On the other hand, the corn smut problem seemed prosaic and lacking in challenge; but it was safe. Fortunately for me, I was not ambitious.

Prof. L. sent me to another professor for instructions about how the experiments should be done. The directions given me were short, and as I found out three years later, they were not very good. I first faced the problem of acquiring a sufficient background. I knew what corn looked like, and I knew there was a parasitic fungus that attacked it, the corn smut. But I needed more than that to get started. An elderly lecturer from the Department of Botany told me there was a journal called *Phytopathology*. She prepared to show it to me so that I could begin to look up research papers on corn smut. As I knew no English, however, I had much difficulty in finding such papers, but I managed to find some. At first I had to look up each word in my English-Polish dictionary, published in 1904, and a gift from my father; and bit by bit

I translated each sentence. I could never know what to do with the "the's" and the "a's." It seemed best to ignore them, which I have done successfully ever since. In this way, very slowly and laboriously, I translated several papers on corn smut, and they made good sense to me.

In the fall of 1953, we moved from Cracow to Warsaw. There, at the College of Agronomy, I found more journals with papers on corn and corn smut. By then, when I knew what to look for, I could find it, read it, and understand it in spite of my linguistic difficulty. Of course, I was lucky that the corn smut problem was being studied by Americans. Had the authors been Japanese, my problem would have been much worse.

I was able to obtain an English translation of a book on phytopathology written by an eminent Swiss specialist, E. Gaumann. The book seemed enormous. It contained between 400 and 500 pages. I plowed along, understood most of it without translating every sentence, and it was really the first scientific book that I read and felt I understood fully. At that time I was 28 years of age, and half-way through my graduate study.

Prof. L. was rarely present in the Department. She did not show any interest in my work. At first I thought she didn't want to help me, but soon I changed my mind: she was simply unable to do so. She was, in fact, one of the professors from social promotion, that strange combination of fantastic cunning in dealing with people and an even more fantastic ignorance of matters of scientific research. As Director of the Institute of Plant Breeding, she bought people by offering them well-paid jobs and other privileges. From them she expected absolute obedience, and she had a well-organized network of informers. At the beginning of my third year of the scholarship, she told me she was not satisfied with the manager of the Laboratory of Plant Physiology at the Institute. She said that she intended to fire him, and she offered me the post. I told her that I just didn't know any plant physiology. She answered that I would learn. I had to refuse categorically, and then she lost interest in my future. News of my refusal spread through the Institute; soon quite a number of persons working there became very nice to me. They confided to me how Prof. L. had humiliated people and kept them in check.

I will give just one example of Prof. L.'s stupidity in scientific matters. In the early 1950s, groups of "peasants- Michurinists" were organized. Their assigned task was to search for new ways of increasing the productivity of Polish agriculture. Prof. L. told us about her meeting with some of these persons. One of them, in her opinion, was particularly interesting. He improved his pumpkins by watering them with skim milk. I was too ashamed to ask why, and what for. It is easy to imagine how the "peasant-Michurinist" enjoyed his little joke. It should be pointed out that at that time Polish peasants each had one or two woefully underfed cows, so milk was too highly valued to be squandered for watering pumpkins. Even if a "peasant-Michurinist" was crazy enough to commit such a folly, his wife would prevent him from doing it in no uncertain terms.

Doctoral students had an obligatory examination to take on philosophy. We had a few seminars on the subject. I understood them rather vaguely, since at that time we had access to no textbooks on the history of philosophy. The basic works we had to study were "Anti-Dühring," by Engels, and "Materialism and Empiriocriticism," by Lenin. I had no idea what the adversaries of Engels and Lenin had written, so in both cases studying the books was like listening to one side of a telephone conversation, part of which was in a foreign language. I did understand some fragments, and I still believed that philosophy would help me. This proved to be true, in a rather unexpected way.

After passing this examination with a moderate degree of success, I asked a lecturer of philosophy what I ought to study to improve myself. He told me to read an essay by L. Kolakowski, which had been published recently (January, 1955) in one of our so-called cultural weeklies. The article's title, if I remember correctly, was "Mythology and Realism." Having borrowed the magazine, I no longer have it, and can remember only vaguely what it was about. As a literary device, it described a dialogue between a Propagandist and a Philosopher.

Propagandist: This is a toadstool.

Philosopher: Is this a toadstool, or a poisoned chocolate?

After reading the essay, I saw Michurinism and other related problems in a new perspective; but it is hard to determine what change occurred in my mental processes. As I have stressed repeatedly in the present account, I knew I was ignorant, and in that respect I was clearly right. The essay, however, provided me neither with knowledge nor self-confidence. Yet I did change.

An example. In Soviet papers, the Western geneticists were described as "reactionary lackeys of rotting imperialism," and other similar epithets. (The most colorful of these was directed at William Bateson by Lysenko. He was called "mrakobies," literally, "Satan of darkness.") What I felt was that while "they" were bad, "we" were good — a nice, secure feeling. After reading Kolakowski's essay, I saw for the first time the absurdity of such epithets.

There was a journal with the characteristic title, "New Agriculture." I wrote a letter to the editor. In it, I stated that I was convinced that Michurinist genetics was essentially true; but there were too many assumptions in it unsupported by any data, and the total picture remained unclear. In particular, the evidence on which Mendelism was founded had never been accounted for within the framework of Michurinism. I also expressed doubt whether all Western geneticists were reactionary lackeys of rotting imperialism. I went into some detail in describing what Western geneticists presumably were not. After about two months, the editor invited me in for a talk. The typescript of my letter was crumpled and greasy from extensive thumbing. The editor told me that he would publish my letter, but I should discard the passage about the really vicious Western geneticists; the original text was too obviously satirical. I did as he suggested, and the letter was printed. At that time, the atmosphere was changing so fast that when it appeared in print it was no longer relevant.

In August, 1955, the last conference on the "New Biology" was organized. Professor P. spoke extensively about the administrative methods used in introducing Michurinism into Poland. There was also a lecture on the implications of Maoism for biology. After that lecture, we felt particularly gloomy. A friend of mine, Gustav, admitted that he understood nothing of it. We all answered in a chorus, "Neither did we." He said, then, "Perhaps this is all rubbish." Our gloom deepened.

In the fall of 1955, I collected the final results of my work and started writing my doctoral thesis. By then, I knew that all my experiments were just an unskilled, small-scale repetition of work that had been done in the United States some 20 years earlier. Still, I enjoyed making the analysis of my results and the task of writing them up. I could concentrate on the work in spite of the fact that my mother was dying, and every day I spent several hours with her in the hospital.

I was offered the post of Lecturer in the Department of Genetics at the College of Agronomy. In January, 1956, I started my new work. The staff of the Department was divided into two factions, and I was a member of the weaker one. My direct superior, in contrast with many persons I have previously mentioned, was very honest, kind and courageous. When she offered me the position, she admitted that she would be unable to give me any real scientific help. Next door to my own room, there was located a small laboratory of experimental systematics sponsored by the Polish Academy of Sciences. Four girls of my own age worked there. Their chief was W. Gajewski. They had regular seminars, and I was invited to participate in them. In winter the girls did karyological analysis of the plants they studied. Thus I had an opportunity to see how chromosomes actually look under the microscope.

At about 1950 there was founded a weekly magazine, *Po Prostu*, addressed to students and young intellectuals. It was usually very dull, but during 1955 it changed radically, became rebellious and interesting. (It was closed down in 1957.) Several young biologists, myself among them, wrote articles for it on the problem of Michurinism, which by then was called Lysenkoism. The term "New Biology" was abandoned. In my article I stated that I still considered the main ideas of Michurinism to be essentially correct, for example, the inheritance of acquired characters. I stressed, however, that one must understand scientific theories rather then believe blindly in them, and that the lack of criticism is most dangerous for science. These ideas were certainly not novel, but I had rediscovered them for myself. Even now, 32 years later, there are numerous scientists in Poland who are offended by any criticism of their work.

In April, 1956, the editorial committee of *Po Prostu* organized a public discussion on the subject of Lysenkoism. The speeches of the participants were duly authorized and published as a booklet under the title *Biology and Politics*. I have reread this booklet, and find the following points worth noting:

(1) Of the several organizers of the course on the "New Biology," only one was present at this public discussion. It was Professor P. He said that he remained convinced that the "New Biology" was essentially correct, but it had been treated uncritically and was forced into the curriculum by administrative action.

(2) Several professors expressed the opinion that dialectical methodology should be introduced with a better understanding of its principles. Obviously, they were still wary.

(3) Genetics was a principal topic of several speeches, although the speakers had no knowledge of it. For instance, my previous superior in Cracow stated that under the influence of the environment changes in protoplasm take place.

(4) Most of the younger speakers concentrated on recrimination and personal accusation directed against some professors. Two of us, Gustav and myself, declared that formal genetics has its weaknesses, and gave examples. The statements were clear enough to show that we had misunderstood the works we were citing.

(5) One, and only one, of the speeches makes as good sense today as it did at the time, that of W. Gajewski. He said, first, that the famous session of the Academy of Agricultural Sciences of the USSR in July of 1948 did not resemble a scientific session so much as a political putsch. Second, he affirmed that while a scientist can make errors, whenever conscious falsifying of the data begins science ends. I do not believe that at the time either my peers or I myself appreciated this speech. We were then in the very depths of our mistrust of all professors, and still much too ignorant to understand properly what he was saying.

I did not know at that time the meaning of the comment about "administrative methods of introducing the 'New Biology.'" In the summer of 1956, however, I was given a transcript of the proceedings of the Politburo from 1948 or 1949. One of the speakers there stated that Michurinism must be intensively introduced in Poland. I had supposed that it was introduced by professors who on their own accord had become enthusiasts of Michurinism.

In the newspapers and periodicals, discussions of Lysenkoism were not extensive. Professor Y., already mentioned in my account, declared in the press that he had been ordered by the Party to discredit Mendelism, and had to obey those orders. A couple of other writers ridiculed the "young Michurinists"—although never the professors—for believing in that doctrine. I was singled out by name, one writer saying that he would not be surprised if I entered a nunnery. Here, then, I should explain clearly in what sense I, and probably most of my colleagues, believed in Michurinism. As I have said repeatedly, from the time of my return from Siberia I recognized my own ignorance. During all the years from 1949 through 1954, I had full confidence in the knowledge and honesty of my teachers, as well as of Lysenko and other Soviet authors. Hence, whatever I failed to understand I attributed to my own ignorance. By 1954, I had formed a fairly coherent picture of Michurinism, its main idea being the modifying effects of environment on organisms, and the inheritance of such acquired characters. I had no idea yet that certain data, such as the results of the "vegetative crosses" made by Gluschenko, were either falsified, or had been based on faulty methods. Thus my "belief" in Michurinism had nothing in common with religious beliefs. As a matter of fact, in my own opinion religious belief, at least as I saw it in Roman Catholics, does not interfere with good science. My Roman Catholic friends believe in God, but keep an open mind in respect to secular matters. Among my mentors of the "New Biology" period, Professor P. can be described as a scientific mystic. I think, however, that the truth is much simpler. Among scientists, as among other people, some can neatly distinguish between what they do and what they do not understand, whereas others are unable to make such distinctions clearly. Professor P. belonged to the latter category. Under normal circumstances, this defect would not matter very much. The scientific community corrects its errors and straightens out equivocal statements. In a Western country, Professor P. would be a dedicated, amusing, nicely crazy zoologist, an active member of antiracist organizations. In the Poland of the period of which I have been writing, he fooled both himself and others.

In 1956 our newspapers became interesting.

I learned from them that the progress of our agriculture depended on economic rather than scientific factors. For instance, previously I had read that the peasants were too old-fashioned to use artificial soil fertilizers. In 1956, it turned out that only small quantities of the fertilizers were actually available, and they were sold preferentially to state-owned farms and to a few kolkhozes which had been organized in 1949 to 1954 (and which promptly broke down in 1957). So practically no artificial fertilizers were left for individual farmers.

Back in 1954, Khrushchev had promoted a novel panacea for our socialist agriculture: the growth of maize. In 1955, it was extensively advertized in Poland. In the next year, one of numerous political jokes going round was that Khrushchev had a new solution for the problem of the Suez Canal: fill it up and plant maize there.

I now started some experiments for the summer of 1956. However, I was never able to finish them. I was through with Michurinism, the "New Biology," Lysenkoism, but I was drifting along, interested only in politics. Late in the fall, Gajewski returned from a two-month stay in France and Great Britain. He brought piles of reprints of scientific papers. It was the first time I had ever seen such publications. He divided them among his coworkers, and gave one to me, too. It happened to be a review of the genetic mechanisms in plants that prevent self-pollination. The author was D. Lewis, of the Department of Botany of University College, London. I devoured it, then read a number of papers cited in the review, and at last knew what I wanted to do. I also knew how to go about it. To me, it was incredibly interesting. The old problem of whether to place confidence in what authors were saying ceased to exist. Here was evidence, and on the basis of that evidence I could make up my own mind. There were also descriptions of methods to be employed, and I could judge for myself whether they were suitable or not. I no longer felt any need for dialectical materialism. My only problem was to learn modern genetics and, as quickly as possible, to forget the past.

My account of my conversion to modern genetics would not be complete without an attempt to answer two questions: (1) What was the relationship between the approval of Lysenkoism by young scientists in the Eastern countries and their own political views? (2) How do I now view the role played at that time by my former professors?

As for the first of those two questions, I do not know what, if anything, was thought about Lysenkoism by those young biologists and agronomists who never participated in the courses and conferences I attended. My fellow participants were about 150 in number. When, in 1956, the time of Lysenkoism was over, we were faced with two alternatives: either we had to admit that we were stupid enough to have taken Lysenkoism seriously, or say that we had only pretended to so so. Many of us, sincerely or not, chose the second alternative. It should be pointed out that the majority of the Polish population disliked, or even hated, the existing regime, even those persons who did not know much about Marxism. When a population dislikes the authorities, it becomes a virtue to fool them. Between 1939 and 1958, the Polish people had passed through six years of Nazi occupation and eleven years of the Communist regime. It was time enough for duplicity to become a well-established tradition in any problem connected with our political life. And it was generally approved by every one.

Only two or three dozen of the participants of the conferences I had attended admitted that they took Lysenkoism seriously. I think, however, that all of us approved of Marxism. We liked the ideas of equality of all people and the internationalism it engendered. Yet certainly, ours was not a free choice of ideologies, based on any real understanding either of Marxism or any other political systems.

As to the second question I raised, I must confess that I am no expert on the professors of that period. I believe they could be roughly divided into those who were honest and brave and who openly disapproved of Lysenkoism, and those who were dishonest or cowards and who supported it. I cannot say how much courage was required of the former, nor how easily the latter submitted to political pressure or temptation in order to acquire privileges. In both groups, there were communists as well as anticommunists.

Among the dishonest and cowardly, there were numerous "professors from social promotion," such as the professor right out of musical comedy, my first boss at Cracow, Professor L., the female equivalent of a godfather mafioso and the supervisor of my doctoral thesis. Most of those in this category were agronomists, de nomine rather than de facto. They were in fact just ignorant persons who were devoid of even the vestiges of common sense insofar as professional matters were concerned. After 1956, all of these persons retained their posts: under socialism, ignorance is no reason for depriving a person of a job. The dishonest or cowardly biologists were not so ignorant as the agronomists. One of them, Professor X. in my account, in the 1960s wrote a textbook of genetics, and once even proposed to present a communication at a meeting of the Polish Genetics Society. At the last minute, so we were told, he fell ill. (I wonder whether that was from shame or from cowardice.)

Yet there were also some honest and brave professors—and among them, some members of the Communist Party. The story of a certain well-known Polish biochemist is worth relating. This man, Professor I. Reifer, had been a member of the Communist Party since the 1930s. Being Jewish, he could find no post in Poland, so just before the war he had emigrated to New Zealand. Directly after the war he returned to Poland and started teaching biochemistry at the College of Agronomy in Warsaw. In 1952, a group of young Party activists issued a denouncement against him, stating that he was politically unsound because he criticized Lysenkoism. The denunciation was addressed to the Commission of Party Control, whose duty it was to look into matters of Party loyalty and correctness. Professor Reifer was summoned to appear before this body, whose judge in the case was also an Old Communist, a tramcar driver. It seems that in order to drive a tramcar without a major disaster, the driver must retain his common sense intact. Hence, after ten minutes of the hearing, the denunciation was set aside and the "culprit" was exonerated. He retained a warm feeling for that tramcar driver, even as late as 1968, when he told me about that incident.

This account, as it stands, will I hope reveal the hard fate of a science in conflict with an authoritarian prophet of ignorance.