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THE GRIM HERITAGE OF LYSENKOISM: FOUR PERSONAL ACCOUNTS

I. FOREWORD

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THE FOUR contributions that comprise this collection of articles came to *The Quarterly Review of Biology* almost fortuitously. The first two we owe to the offices of David D. Perkins, Professor of Genetics at Stanford University and a former president of the Genetics Society of America. He wrote to me saying that he had received two short manuscripts relating the trials and vicissitudes that two geneticists in Poland, known to him personally, had undergone during the period of Lysenko's ascendancy in the Soviet Union and its allied countries east of the Iron Curtain. Would I be so kind, he asked, to look them over and tell him frankly under what auspices they might be published? One Polish geneticist was a senior scientist who had remained staunchly true to his scientific convictions about the validity of modern genetical work and the foolishness of Lysenko's claims, and had consequently paid a severe price for his intransigence. The other, a young woman who had grown up in science after the end of World War II, had begun by simply feeling ignorant and doing her best to find something reliable to believe in the confusing welter of Lysenko's doctrines, as they were forced upon Poland. Upon reading the two articles, I was deeply moved by their testimony of the death of biological science in their country, the scientists' isolation, and their struggles to endure, to learn to find scientific truth on their own, and to maintain their integrity.

The third of the four manuscripts came to us by a different route. Its author, S. M. Gershenson, was a well-qualified *Drosophila* geneticist whose training had been in the great Institute of Experimental Biology located in Moscow and founded and directed by N. K. Koltsov. As a young graduate student and postdoc who also worked in fruit fly genetics, I myself had read and appreciated Gershenson's work in the early 1930s. His personal reflections on the tribulations he endured in the Lysenko period came in the form of a manuscript sent to Professor Melvin M. Green of

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the University of California at Davis. After editing it for its English, Green submitted it to the *Journal of Heredity*. It was sent out to several referees, who expressed ambivalent opinions about its suitability for publication in the *Journal of Heredity*. As one of those readers, I suggested to the editor of the *Journal of Heredity* that the personal account written by Gershenson would fit much better into the QRB's group of articles on the aftermath in Europe of the Lysenko cataclysm in Russian genetics. With Melvin Green's concurrence, this transfer was made. Gershenson's account is even more a personal account than the contributions by the two Polish geneticists. That, however, it seems to us, is just the heart of the matter. What has been lacking in the historical treatment of Lysenko's war on "classical" genetics is precisely the personal element, the documentation of the destructiveness to the lives and work of individuals who had once been free in science to work at their own chosen problems, to reach their own conclusions, and publish them freely in the scientific journals of the world. Pruned of unnecessary duplication of already well-documented and treated accounts of Lysenko's victorious convocations under the aegis of Stalin and Khrushchev, there was firsthand information here, especially of the developments in the Ukraine, needed to round out a fuller historical picture.

The fourth of the papers in this collection might at first glance seem to be altogether different from the first three, for it deals with the fate of N. W. Timoféeff-Ressovsky. Timoféeff was also a product of the Koltsov Institute of Experimental Biology, but somewhat before the time of Gershenson. Timoféeff's fortune was to transfer to Germany in the late 1920s, in order to develop a strong genetical basis for the experimental work on human neuroscience and psychiatry at the Kaiser-Wilhelm Institute for Brain Research (K-W Institut für Hirnforschung), located in Berlin-Buch. There Timoféeff rose to become a recognized leader in world genetics by the early 1930s, and a deputy director of the K-W Institute for Brain Research as its founder and original Director, Oscar Vogt, reached semi-retirement, in the late 1930s. Timoféeff fell into the bad graces of the Russian Soviet regime when, more than once, he refused to leave Germany and return to Russia. To have acceded to the demand would have been, as both Vavilov and Koltsov wrote to him in secret, equivalent to committing suicide. Lysenko, Stalin, and the KGB were just waiting for Timoféeff to fall into their hands.

Nevertheless, Timoféeff walked a clever way among the Nazis. Without voicing any outspoken opposition, he refused to have anything to do with the development of their eugenics program, the program that in the end led to the genocide of millions.

In recent years, there has been in Germany, among certain geneticists and historians of science, a strong attempt to vilify Timoféeff. He has been blamed not only for encouraging Nazi eugenics policies by keeping silence, but even for justifying them by his genetical studies of mutations, giving support to the concept of the "genetic load" that was the excuse for the Nazi effort to expunge all harmful genes by simply eliminating their bearers. It seems scarcely to have entered the understanding either of the Nazis or the current vilifiers of Timoféeff that, inasmuch as virtually everyone in any population whatsoever is the bearer of some harmful recessive genes, the successful outcome of their policy could be reached only by exterminating everyone. The perverted logic reminds me of a grimly humorous verse that circulated underground in the days of 1933 when I was a postdoctoral fellow in Timoféeff's department in Berlin-Buch. A definition of the perfect Nordic, so it ran, was

So schlank wie Goering,	[As lean as Goering,
So stumm wie Goebbels,	As silent as Goebbels,
So blond wie Hitler,	As blond as Hitler,
So keusch wie Roehm.	As chaste as Roehm.]

Timoféeff endured some years of incarceration, first in the Lubianka and afterwards in a "camp of correction" in northern Russia, where starvation and vitamin deficiencies almost ended his life and cost him a severe loss of vision. He completed his sentence in Siberia, near Chelyabinsk, where he worked on radiation research for his native country and was widely recognized as their greatest authority on radiation hazards. Finally, Timoféeff was permitted to return to a laboratory near Moscow, in Obninsk, and there to establish a center for the study of radiation and population genetics, broadened into Vernadsky's concept of a total ecology, a "biogeochemistry" that Timoféeff had strongly promoted in his famous

seminars in the Urals. Yet this degree of forgiveness for his supposed disloyalty and aid to his country's enemies in time of war did not extend to a "rehabilitation" — that is, to an abrogation of the unjust sentence. Even today, long after the executed Vavilov has been rehabilitated — of course, post mortem — and has been honored among his country's great scientists by having his portrait placed on a Russian postage stamp, Timoféeff-Ressovsky remains in limbo.

The foregoing reasons, and especially the recurrence of violent attacks by the geneticist Benno Müller-Hill and the astute writer Karl Heinz Roth on the reputation of Timoféeff, led the editors of *The Quarterly Review of Biology* to seek an article to supplement the other three in this collection, an article that would defend the reputation of Timoféeff, an article written by one who knew him personally and had worked with him closely. The present editor has previously made a partial attempt to present such a defense in his biographical memoir of Timoféeff, written for a supplementary volume to the *Dictionary of Scientific Biography*, still in press; and also in a caustic review of the German book containing Karl Heinz Roth's article, "Schöner neuer Mensch," (*Q. Rev. Biol.*, 64: 175-180, 1989). The editors were fortunately able to find just the person with the desired qualifications to do this task — Raissa Berg. She is herself an expatriate Russian geneticist who suffered great indignities and tribulations during the Lysenko period, and she has already told movingly of her life's work and experiences in a recent autobiography, *Acquired Traits: Memoirs of a Geneticist from the Soviet Union* (Berg, 1988; see review in this issue of the QRB). Raissa Berg worked with Timoféeff-Ressovsky during his last years, in Obninsk, where they collaborated on one of his theoretical papers. Her "defense of Timoféeff-Ressovsky" makes a strong historical case to refute the slanders that have been charged against him, both in Russia by a recurrence of the charges that were lodged against him in the time of Stalin and Lysenko, and also in Germany, by the neo-historians who are so obviously seeking to find a scapegoat for the crimes of the Nazis against humanity.

None of these four articles is typical history of science. Instead, each one is the firsthand testimony of persons who lived through this century's most notorious debacle of science. It is the sort of primary documentation that historians, especially historians of science, need. Let us grant that personal emotions and failures of memory may obtrude in such documents. It is of course the historian's mission to check all conflicting evidence against other records, and so attempt to ferret out the truth. Yet such documents should be preserved, and if they seem to be especially revealing, they should be published for the sake of a truer understanding. For these reasons, the Editors make no apology for their decision to assemble these four samples of testimony regarding the persecution of genetics and geneticists in certain countries and during certain times, in this its century of greatest achievement.

My own acquaintance with the issues involved in Lysenkoism in Russia and the Soviet-dominated countries and with the twisted logic wherewith the Nazis composed their racist doctrines and justified their policies of "eugenic extermination" goes back to my days as a graduate student at the University of Texas, working under H. J. Muller, and as a postdoctoral fellow in Nazi Germany in 1933, when I worked in Timoféeff-Ressovsky's laboratory at the Kaiser-Wilhelm Institut für Hirnforschung in Berlin-Buch. It was in Austin that I met Vavilov, as he paid us a visit after field work in Peru and Mexico. With absorbing interest I listened to the account of his search for the origins of domestic plants throughout the world. There, too, in my final year of graduate study I became acquainted with S. I. Levit and I. I. Agol, who came from Moscow to spend a year with Muller. Levit was already the leading figure in human genetics in Russia, and Agol had participated in some of the much discussed *Drosophila* studies of Serebrovsky and Dubinin that led to the genesis of their subgene theory. I was even asked by Muller to tutor Agol in English for a time, but soon had to give up that attempt, since he was quite certain he already knew English well enough, and was sure that no American graduate student was competent to teach him anything on that score. Hence the news, in 1936, that Levit and Agol had been arrested and presumably executed in the early period of Lysenko's rise to power could not fail to affect me deeply, and Vavilov's subsequent arrest and disappearance were even more distressing.

In Berlin-Buch I not only came to know

Timoféeff well, but also found that Muller was there, spending a couple of months with Timoféeff before going on, as he had planned to do, to make an indefinite stay in Moscow at the Institute of Genetics. In fact, it was Muller's initiative that enabled me to transfer from the K-W Institut für Biologie across the city to the far northeastern suburb of Buch, where the K-W Institut für Hirnforschung was located, and to work there for five months with Timoféeff. I had been awaiting in vain the return of Curt Stern, with whom I had expected to continue my work on the nature and inheritance of dominant mosaic eye colors in the fruit fly *Drosophila*; but Stern, alarmed by the rise to political power of the Nazi Party and its outspoken anti-Semitism, continued to defer his return from America.

The Nazis came into full control of the government of Germany at the beginning of 1933. Already in May there were numerous assaults of mobs upon Jewish stores in Berlin, and often the proprietors and sales clerks were beaten up. By midsummer, a Nazi inquisition of the personnel of the Kaiser-Wilhelm Institutes began. Since they were scientific institutions, it had been assumed that they were sacrosanct. Not only all Jewish personnel, except the highest ranks, but every former socialist or communist was taken away for questioning. Some returned in a few days, bearing signs of beating during their inquisition. Others never returned, and their fate was often never learned at all.

It was a very strange period in which to attempt to keep one's mind on scientific problems. I remember some ardent arguments with Muller about the relation of a totalitarian government with science. Muller was quite sure that there was not even a faint resemblance between the Nazi attitudes and those of the Communist powers to the East. From its very beginning, Russian Communism had supported the freedom of science more fully than any nation on Earth, he claimed. Compared with the domination of science in America or Western Europe by suspicious political leaders who had no understanding that scientific advancement held the promise of the future welfare of mankind, Soviet leaders supported science fully and freely—or so it seemed to Muller, who had spent an earlier sabbatical visit in Russia and was greatly impressed by the power and enlightened development of scientific institutes and programs under the Soviet Academy of Sciences. On the other hand, I argued that there was essentially no difference between the domination of science by the Nazi leaders and by the Soviet leaders. Hitler and Stalin were equally untrustworthy and unenlightened, and viewed science only as a basis for technological improvements in military arms and economic resources.

We were never able to agree, although I retained the highest respect for Muller's idealistic devotion to communism; and he always treated me with kindness, thoughtfulness for my welfare, and good advice in my scientific problems. Time was to prove me right, but I could take no joy in the knowledge that when, in 1936, Muller had to flee from Russia because he had challenged Lysenko only to learn that Lysenko had Stalin's full support, my professor was a saddened and bitterly disillusioned man. The irony of the situation was that, while he had left quickly, under the pretense of serving as a volunteer medical aide in the struggle of the Spanish Republicans with the Fascist insurgents, and had made his way to Britain, he was unable to return to the United States at that time because of official suspicion that he was still a Communist and perhaps serving as an undercover agent.

To get back to Berlin in 1933. Not only was it the scene of monster parades and gatherings to hear Hitler proclaim his challenge to Europe, it was also a center of the most wonderful art and music a young American from Texas had ever imagined. It was a Wagner anniversary year and a Brahms anniversary, too, and I heard concerts and operas performed by some of the greatest musicians of our century. Furtwängler conducted the Berlin Philharmonic Orchestra in cycles of Brahms's symphonic and choral works. Max von Schilling directed a performance of Gluck's "Iphigénie in Aulis" on the very steps of the majestic Pergamon Altar in the Altes Museum. I heard every Wagnerian opera in chronological sequence, from his very youthful and virtually unheard operas "Die Feen" and "Das Liebesverbot" to the final opera "Parsifal"; and many of them several times over. The great German baritone Ludwig Hofmann and the magnificent Russian basso Alexander Kipnis were unforgettable, even though the sopranos and tenors of the time were not

quite first class. In pianoforte concerts, I heard Artur Schnabel and Edwin Fischer, and such a younger newcomer as Lili Kraus, incomparable in Mozart and Schubert, in what was possibly her debut season in Germany. Many a Sunday I spent almost the entire day in the splendid museums in the heart of Berlin, the Kaiser-Friedrich Museum with its great collection of Renaissance and early modern art, and the Altes Museum, with its huge and incredibly moving Egyptian, Greek, and Roman sculpture. Here was not only the world-famous colored bust of Nefertiti, but the huge classic Greek Pergamon Altar, perfectly preserved, and in some ways the equal of the Parthenon in its perfection. On other days I went to the Museum für Völkerkunde, where the cultures of all the world, it seemed, were represented by memorable collections. Even though the worldwide Great Depression was at its height, and the bank holiday in America had frozen my funds earlier in the year, an American dollar bought unimaginable riches in Berlin.

The contrast: one evening, when I had attended a Wagnerian opera at the Opernhaus in the heart of the city, I came out of the building to find a throng of people assembled in the open square. A great pile of books was assembled, some fifteen feet in height, and a uniformed Nazi was haranguing the crowd. By this time my understanding of spoken German was sufficient to enable me to get the gist of what he was mouthing: "These vicious books, all written by Jews who would like to destroy pure German culture—we have assembled them from stores and libraries of this city, and now they will pervert our youth no more. Evil philosophers who would degrade our Nordic purity, novelists who elevate Jews above all others, let us commit their corruption to the flames." Gallons of oil were poured over the great pile, and set afire. I believe I saw with my own eyes the very first "burning of the books" in Germany, of which there were many in the succeeding months.

Years later, one of my earliest reviews for *The Quarterly Review of Biology* was of a booklet coming from England and written by P. S. Hudson and R. H. Richens. It was entitled *The New Genetics in the Soviet Union* [see "Dialectical Materialism and Scientific Research," *Q. Rev. Biol.*, 23: 333-335, 1948]. Students of this troubled time in the history of modern genetics should not ignore that early effort to explain the new phenomenon.

In the winter of 1950–1951, I was requested by the U.S. State Department to spend two months as a consultant in regard to the state of scientific recovery in the Western Zones of Germany. Like many other Americans, I had supposed that by that date, over five years since the end of hostilities and after the seemingly complete economic recovery owing to the success of the Marshall Plan, the signs of war would have largely disappeared. To my amazement, vast destruction was still evident everywhere. In Berlin, new shops along Kurfürstendam stood beside blackened ruins. In Wiesbaden, my principal duty station, the entire center of the city remained in total ruin, except for the solitary hotel in which I lodged. Whatever recovery had taken place in academic areas was strictly local. There seemed to be no communication whatsoever between cities or between universities. No scientific societies had been reorganized; no scientific meetings were held. I had to make my way from one place to another, inquiring in each city what was known about geneticists or related biologists in the next place on my tour. In the noble university city of Göttingen, as elsewhere, a total lack of books for students to use was the common complaint of professors, and I saw a large human anatomy book actually chained to a pedestal so that it might be used but not easily stolen. In addition to finding out who was active in genetics, and where, I was expected to report on any signs of the "rehabilitation" of former Nazis now seeking to return to academic ranks. So secret was this report considered that after I had prepared it and sent it to Washington, my own security clearance was not sufficiently high to enable me ever again to examine it!

In Frankfurt, a visit to the Max-Planck Institut für Biophysik, which was one of the new successors to the prewar Kaiser-Wilhelm Institutes, led me to a conversation with M. Rajewsky, its Director. Rajewsky had been a good friend of Timoféeff-Ressovsky, as he was also a Russian who had migrated to Germany before the war. Rajewsky told me how he had made a desperate journey to Berlin in the last weeks before that city fell to the Russian Army, with the purpose of persuading Timoféeff to return with him to the relative safety of the West. But Timoféeff was not to be moved. He

refused on the ground that he, and he only, could save the institute at Berlin-Buch from destruction by the Russians, for as a native and compatriot he could speak their own tongue and explain the nature of the science done in that institution. He could save the lives and fortunes of all the scientists and research assistants under his care, as Deputy Director. Rajewsky returned to Frankfurt dejected; but Timoféeff did exactly what he had foretold. He saved the Institute from destruction and its personnel from harm. For some time thereafter, he served as its Director. Then, one day, he was arrested and carried off to Moscow for trial, as being a person who had refused to return home when his native country called him. The sequel has been told by Raissa Berg, as well as by Zhores Medvedev and others.

In 1960, when I attended a meeting in Moscow of the Conference on Science and World Affairs [more familiarly known as the Pugwash Conferences, from the site of the first meeting of this international scientific body], I inquired about the possibility of visiting Timoféeff, who was then already stationed at Obninsk. Excuses were made. It would not be possible to see him, as he was ill or on vacation, or whatever.

Meanwhile, a personal event occurred that I believe ought to be put on record, as it relates to the changing status of Lysenko's power over biology in the USSR. The 1963 Pugwash Conference on Science and World Affairs took place in London. By that time, I had been for some time a member of the Continuing Committee that organized the conferences, and had become quite well acquainted with Academician Igor Tamm, the noted Russian physicist whom I first met at the Second Atoms for Peace Conference (1958), of which he was the president, and which was held in Geneva, Switzerland. Tamm was undoubtedly one of the three or four most eminent Russian scientists at that time, a Nobel Prize winner equal in reputation to Peter Kapitza or the younger Andrei Sakharov. It was noticeable that at this Sixth Pugwash Conference, held in London, the Russian delegation moved together as one man, always accompanied by their Party watchdog, a pleasant fellow who attended a great many of the Pugwash Conferences, both before 1962 and later. The Soviet delegates were not supposed to meet individually or informally with representatives of the Western nations. I was therefore quite surprised when I received a surreptitious word that Igor Tamm would like to have a private conversation with me, if I would select a convenient time and place.

Accordingly, he met me alone in my hotel room one evening before dinner, and broached his question. Many Russian scientists of the Soviet Academy of Sciences, he said, were deeply concerned about the damage being done to biological science in Russia by Trofim Lysenko. What might I, as a geneticist, have to suggest as a suitable way to provide for a resurgence of genetics in the USSR without encountering the direct opposition of Lysenko and the political regime? Having thought about the matter briefly, I replied that it seemed to me unwise to attempt under present conditions any open support of modern genetical research. However, it would perhaps be feasible to establish a small group of bright young men within one of the established institutes under the Academy of Sciences, who might undertake within a year or two to acquaint themselves with the development of modern genetical research in the West and to pursue their research under the guise of "molecular biology," a term then not at all so familiar as now. I even suggested, if I remember correctly, that such a group could well be housed in the Institute of Biochemistry, under the distinguished biochemist V. A. Engelhardt, who was well known in America. Tamm nodded, appearing to accept the suggestion for consideration, and departed.

In 1966, a committee of geneticists appointed by the National Academy of Sciences of the USA nominated N. W. Timoféeff-Ressovsky for the Kimber Gold Medal and Prize Award. This prize, in its brief lifetime of fourteen years, was awarded only twice to non-Americans. Timoféeff was to be the next to last person to receive the award, which by many geneticists is regarded as an even higher honor than the Nobel Prize. [The last person to get the award, in 1967, was Barbara McClintock.] I had the honor to prepare and deliver the citation for Timoféeff, who of course, in spite of every effort by the president of the National Academy of Sciences, was not permitted to come to Washington to receive the award. Even Raissa Berg, who was so close to Timoféeff during his years in Sverdlovsk and Obninsk, does not seem to have known about this matter, for there is

no mention of it in her autobiography, which has so many other rich matters to say about Timoféeff. In the following year, when George B. Kistiakowsky, Vice President of the National Academy of Sciences, made an official visit to Moscow in order to conclude an agreement with the Soviet Academy of Sciences for the exchange of scientific personnel and information, he took with him the medal to be awarded to Timoféeff-Ressovsky. There was some difficulty in arranging for Timoféeff to be brought from Obninsk to Moscow, though it was no great journey, in order to receive the medal and cash award. Eventually he arrived, but the conferral was literally shoved into a corner room, and there was no public ceremony before the Academy members. This event indeed shows, as Raissa Berg has testified in her autobiography, that in Breshnev's time, until the late 1970s, the curse of Lysenko still lay heavy upon all biological science, even though Lysenko himself had been demoted in 1964, along with Khrushchev. Several incidents in which I was involved will show this to be so.

In 1969 I was again in Moscow, this time as a representative of the American Association for the Advancement of Science, of which I was the current president, for a visit with the Russian association "Znaniye" [to be translated simply as "Science"]. Znaniye, as the Russian counterpart of the AAAS, had in the previous year sent a delegation of its officers to Washington to attend the annual meeting of the AAAS. Znaniye boasted of well over a million members, since literally everyone in the Soviet Union of any political stature was compelled by Marxist philosophy to be interested in the progress of science, and with all other devotees of science it was a remarkably large and diverse organization. When asked by the officers of Znaniye what particular institutes in Moscow I might wish to visit, I asked to visit the Institute of Biochemistry. Academician Engelhardt gave me a cordial welcome, and shortly said, "There is a group of young men here I think you would like to meet. We have a very active group in molecular biology [did I detect a twinkle in his eye as he spoke?] — including modern lines of genetical research." I was truly impressed by those young scientists, who told me that in order to catch up and keep abreast of molecular biology — and genetics — in the West, they customarily spent at least half of every working day in reading the latest scientific literature, especially that from Great Britain and the United States. In this manner, Lysenko's domination was already undermined well before he was finally cast out.

In Znaniye's offices in Moscow, I was shown a list of all the publications the Society had published in recent years. Among them were several books of a popular nature on genetics and evolution, written by my friend Timoféeff-Ressovsky within the past five years. Naturally, I was delighted to see them, and inquired whether it might be possible for me to meet with the author, in whose laboratory I had once worked as a post-doctoral fellow — to meet either in Moscow or in Obninsk. My hosts said that they would look into the matter, but on the following day I was told that it could not be arranged so quickly, since I was scheduled to leave for Riga a day later. Perhaps when I returned to Moscow, after visiting Riga and Leningrad, it might be possible. Alas, when I did return to Moscow for a couple of days before taking my flight back to the United States, I was told — somewhat abashedly — that the requested meeting could not be arranged. Either Timoféeff was vacationing in the Crimea, or he was ill, but he could not be reached. I was never to see him again.

In Moscow, on that occasion, I was interviewed by a team of reporters for the state television service. When they pressed me to name some great Russian geneticist of whom they could be proud, I obligingly told them about G. D. Karpechenko, the Leningrad cytogeneticist who was the first person in history to produce a new artificial species. I had, of course, first learned about his work myself from the first edition of Th. Dobzhansky's classic work *Genetics and the Origin of Species*, in 1937. Karpechenko achieved his end by crossing together two cultivated plants, generally considered to be so different botanically that they were classified in different genera. The two plants were the familiar radish and the familiar cabbage. The result of this hybridization was, as might be expected, a sterile hybrid, with intermediate characteristics between radish and cabbage. With great patience, Karpechenko, in the late 1920s, succeeded in obtaining a hybrid with a doubled number of chromosomes: two full sets of radish chromosomes and two full sets of cabbage chromosomes. The initial hybrid had been

sterile because the two sets of chromosomes (radish and cabbage) were too different in nature to pair together in meiosis, and so no fertile pollen or ovules were made. In the amphidiploid, as the hybrid with the doubled sets of chromosomes is termed, each radish chromosome pairs with its radish homologue, each cabbage chromosome with its cabbage homologue, each pollen grain or ovule has one set of each parent species' chromosomes, and the resulting amphidiploid plant has four sets of chromosomes, two derived from each of the parent species. It is consequently perfectly fertile in crosses with its own kind, but when crossed with either a radish or a cabbage, gross infertility is the result. By any good definition of genetic isolation from its parents or other species of the family Brassicaceae, it is a new species, an artificially made species, and a Russian product. Karpechenko named it *Raphanobrassica*, from *Raphanus*, radish, and *Brassica*, cabbage. [It was unfortunately of no agricultural merit, since it had a spindly root like a cabbage, and prickly leaves like a radish. I did not inform the reporters of that fact. Nor did I tell them that Karpechenko was one of the very first victims of Lysenkoism.] The television team of reporters seemed very excited by this true tale of Russian scientific achievement, and promised that I would be on the screen the next night. Of course, I knew that would not happen. Never a sign of any kind followed my glorification of Russian scientific achievement in genetics.

A brief recent document, written by I. A. Sakharov of the Department of the History of Genetics in the Institute of General Genetics of the Academy of Sciences of the USSR, in Moscow, has come to me. This note relates that in 1988 the youth of the town of Vel'sk collected a sum of money in order to set up a local monument honoring G. Karpechenko, the creator of *Raphanobrassica*, and a victim of "the struggle for genetics in the USSR." On May 3, 1989, the monument's foundation was laid on the ninetieth anniversary of Georgy Dmitrievich Karpechenko's birth. The article recounts the course of Karpechenko's life, and especially his achievements in interspecific hybridization and polyploidy while working in the Department of Plant Genetics of the Institute of Plant Breeding in Leningrad. It mentioned also, facts of which I was unaware, that Karpechenko held a Rockefeller fellowship in 1929, in order to work in the laboratories of Thomas Hunt Morgan at the California Institute of Technology and of E. B. Babcock at the University of California, Berkeley. Karpechenko saw again his friends H. J. Muller and Calvin Bridges during that stay in America, both of whom he had previously met during their early visits to the Soviet Union. The article concludes with a summary of Karpechenko's defense of genetics when it was first assailed by Lysenko. "He defended the scientific truth." Early in 1941, after N. I. Vavilov had been arrested and his associates at the Institute of Plant Breeding were subjected to repression, Karpechenko was also arrested, along with the cytogeneticist, G. A. Levitsky, and the plant breeders, L. I. Govorov and K. A. Flyaksberger. They were imprisoned, Karpechenko in Moscow, where he died "under unclear circumstances," presumably on the 17th of September, 1942. It is no wonder that my account of Karpechenko's great achievement was not disclosed to the Russian public over television.

In the archives of modern genetics in the Library of the American Philosophical Society in Philadelphia I was made happy by seeing some fine photographs taken by the West German plant scientist Georg Melchers. They show a group gathered at a dinner party during the 1972 assembly in Moscow of an international meeting of plant scientists. At the table were both Timoféeff and his wife Elena, laughing happily with their friends Georg Melchers and Hans Stubbe, the latter from East Germany. I would like to have been there.

What irony lies in the fact that Timoféeff, who for so long a time was held in custody by the Russians and who was never fully rehabilitated as a scientist before his death occurred in 1981, now moves into the spotlight of the new German historians, who would make of him a scapegoat for the horrifying crimes committed by the Nazis in the pursuit of their eugenic goal of a "pure race." As if, in the first place, there really existed any such thing as a pure race among the vast intermixtures of migratory populations over thousands of years that have produced our modern peoples! But secondly, I must reemphasize the appalling example of false logic that claims that, because any scientist contributed to the undeniable evidence that exposure to high-energy radiation

produces an abundance of mutations, together making up a "genetic load" that becomes widely dispersed and requires generations to lessen significantly, that any such scientist consequently made inevitable the hideous empirical policy of genocide. Furthermore, this view holds that he is therefore morally as culpable as the perpetrators of that death to millions of innocent people. Such a travesty of reason is equal to that of certain philosophers who have claimed that the good is responsible for the evil in men's actions, inasmuch as without the good there would be no way of recognizing its opposite, the evil. In today's dedication to the elimination of environmental pollution and of the flagrant destruction of natural resources, is a person who discovers the fatal connections of cause and effect that an unthinking exploitation of our world has brought about to be regarded as a copartner in the crime of pollution and waste? Is a Rachel Carson to be banded with the manufacturers of chemical insecticides and fertilizers as an originator of our chemical problems in the environment? It is no whit more logical to hold Muller and Timoféeff, Julian Huxley and Haldane, among others, responsible for the errors of policy that cloaked a despicable bigotry in eugenics, as it developed in Germany under the Nazis and, we confess, in the United States between 1915 and 1930, when thousands of sterilizations of the "unfit" were carried out under laws that were based on the most dubious of suppositions. Publication of the present defense of Timoféeff-Ressovsky is therefore greatly needed just now, not simply in order to set the record straight, but especially to prevent any recurrence of the reasoning that searches for scapegoats to clear one's own national conscience, and that leads to further bigotry and to the destruction of true science.

THE GRIM HERITAGE OF LYSENKOISM: FOUR PERSONAL ACCOUNTS

II. LYSENKOISM IN POLAND

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POLAND IS a sufficiently typical example of the Soviet Union's "ideological offensive" against the Eastern European countries behind the Iron Curtain. The introduction of Lysenkoism there was but a small part of this offensive. To begin with, a foreign reader should have some idea of the situation that existed in Poland at the time when Communism was forced upon the country in 1945.

At the end of the 18th Century, Poland was partitioned between Russia, Austria, and Prussia. The Polish territory became simply provinces of those states. That situation was extremely unfavorable for the development of the country, for education, and for science in particular. In 1918, Poland regained its independence and, in spite of an extremely difficult economic and political situation, a rapid development of its universities and its research institutes began.

In 1929, when I entered Warsaw University as a student, I attended excellent lectures on cytology given by Professor Z. Wojcicki, on plant physiology by Professor K. Basalik, and on plant systematics and geography by Professor B. Hryniewiecki, and others. At that time there was no Department of Genetics at the University, but a number of lectures on Mendelism were given by Professor E. Malinowski, of the College of Agronomy in Warsaw. At four other Polish universities there was at that time no inclusion of genetics in the biological curriculum, owing to the lack of specialists in the subject. The situation in the faculties of agronomy and in the separate Institutes of Agronomy was rather better. At least some genetics was taught there, usually as a part of courses in plant and animal breeding, by such specialists, in addition to Malinowski, as Professor L. Kaufman and others. In Mory, near Warsaw, there was an Institute of Plant Breeding where Professor M. Skalinska, an excellent plant cytologist, headed a department. It should be stressed, of course, that Poland was an agricultural country. Plant and animal breeding was therefore of great importance, and Polish specialists had made significant achievements in the breeding of wheat, rye, sugar beets, and other crop plants. Of course, their work was at least to some extent based on genetics. Polish scientists were in good contact with Western European centers of research, and the level of science was constantly improving.

The outbreak of war in 1939 put an end to all of this. All universities — even all secondary

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schools—were closed. Underground teaching was organized, but only a fraction of the young people could attend such classes. Inevitably, its level was not high. Professors, lecturers, and the great majority of all educated persons lost thier positions. In Cracow, for example, 184 members of the faculty of the Jagellonian University and the Mining Institute were arrested and sent to German concentration camps. Few of these survived. Those scientists who were not imprisoned were, for six years, entirely cut off from all scientific institutions and international contacts, and had to earn their living at odd jobs. The scientific laboratories were pillaged by the Germans of all equipment. Even most of the books were taken from the libraries. At the Department of Botany of Warsaw University, even its herbarium was removed to Germany. Then the buildings were burned. Only ruins remained.

The worst losses were in the university staffs. Numerous scientists were killed while serving as soldiers or in the underground resistance (the Home Army). Many others were simply executed in the streets.

After the end of the war, in 1945, when I returned to Warsaw, I found that all the buildings of the Botany Department and of the Botanical Garden had been burned to the ground, and only a few members of the pre-war staff had survived and returned. That was because, during the six years of conflict and directly afterwards, there was a vast forced migration of Poles. The deportations to Siberia by the Soviet KGB numbered over one million persons. There had also been deportations from the Polish territories that were included in the German Reich from 1940 on, especially after the uprising in Warsaw in 1944; and finally, after the war, from the Polish territories taken by Russia. Most of these last deportees were moved into the former German teritories that in 1945 were incorporated into Poland. Altogether, it is estimated that about 6 million Poles—20 percent of the surviving population—lost their homes and were moved to new locations. The direct effect of this massive series of migrations was a break down of all sorts of social and community ties.

Directly after the war, consequently, the situation was extremely difficult and unfavorable for the reconstruction of scientific work. Yet persons with enthusiasm started to rebuild the country, both in general and in science in particular. In spite of our ruined laboratories and our decimated personnel, scientific activity was regenerated with surprising vigor.

My personal example is instructive. Before the war, I worked in the Department of Botany of Warsaw University. I took my Ph.D. degree in botany, but even then I was greatly interested in genetics and cytogenetics, so studied these subjects on my own. Just before the war broke out I had also become interested in experimental systematics, a subject developed strongly at that time by Julian Huxley in England, by Jens Clausen in Denmark [that was before he moved to the United States], and by Th. Dobzhansky in the United States; as well as by others. In 1938 I started to study interspecific hybrids between various species of *Anemone*. During the war, I earned my living by a sort of commercial gardening near Warsaw, and thus was able to continue my research. Directly after the war was ended, while participating in the reconstruction of the Botanical Garden, I continued my research on *Anemone*, and also started a similar study on interspecific relations between the species of the genus *Geum*. I wrote a popular book on genetics, published in 1948. At the University, I started courses in genetics and made a first draft of a handbook of genetics that was used by the university students.

In 1947 I obtained a grant and went to Lund, in Sweden, to continue my studies on *Geum* under the outstanding specialist in plant genetics, Professor Arne Müntzing. After my return from Lund, I hoped to organize a Department of Genetics in Warsaw University, with full support of the Faculty of Biology.

Soon after my return from Lund, however, the first news reached Poland of the "revolution" in genetics that had taken place in the Soviet Union. It was then that I heard, for the first time, the name of Trofim Lysenko. He was the leader of the "New Biology" faction that had achieved a victory over the forces of "reactionary formal genetics." Somehow, the name of the best-known Soviet geneticist, N. A. Vavilov, was not mentioned at all. Only later, word arrived that he had been arrested in 1940 and had died in prison in 1943. The decisive victory of Lysenko and his followers took place at a famous session of the Academy of Agronomic Sciences held in Moscow at the end of July, 1948. As is well known, at the end of that ses-

sion the participants "unanimously" declared that Western genetics was unscientific, idealistic, and metaphysical, whatever these terms might be taken to mean. The declaration was fully supported by Stalin as leader of the Communist Party, and Lysenko's "new biology" received Stalin's full support as the only truly scientific and materialistic theory of heredity constructed on a basis of dialectical materialism.

Soon the ideological offensive became extended to other countries east of the Iron Curtain. What a historically interesting phenomenon, not simply of forcing upon others a strictly political system and ideology, but also a philosophy of life, and even a scientific methodology (if the last can be considered applicable to Lysenkoism)! First, numerous speakers arrived in Poland to present the achievements of the "new biology." They delivered dogmatic lectures presenting Lysenkoist theories and the results of certain experiments mostly in plant breeding. These were delivered ex cathedra, to be accepted without reservation or discussion. The speakers used a stock of ridiculous epithets applied to all "enemies" of the "new biology," in particular to Western geneticists—such epithets as "reactionary," "backward," "antiscientific," "idealistic," "lackeys of imperialism," "lackeys of Wall Street," and the like. Particularly unforgettable, for me, was a lecture by G. M. Boshian. The numerous members of his audience were herded into a big room in Warsaw. They were predominantly biologists and agronomists, but there were also many political activists. Boshian presented the results of what he called his own research. He stated that viruses are formed spontaneously from unorganized organic matter. This term was often used, but was never defined. Viruses, he went on, in turn can give rise to bacteria, to demonstrate which he showed photographs of some kind of crystals from which viruses were supposed to be formed, and from the viruses, bacteria. Among other crazy statements, he averred that not only could *Penicillium* produce penicillin, but conversely from penicillin the mold *Penicillium* can be obtained experimentally. No questions or discussion was expected after the lecture. That was most embarrassing—the audience was treated as if composed of complete ignoramuses to whom these new, and brilliant, discoveries were now revealed. Afterwards, of course, those listeners at all acquainted with biology were horrified—it was simply inconceivable that such gibberish could be presented in the guise of scientific discoveries. I, for one, felt humiliated to be treated in such a way. I wondered whether Boshian himself was convinced by his own evidence, or if he claimed to believe all this nonsense just to support Lysenko in order to save his own skin. My impression was that Boshian was only semiliterate and had no conception of real research work, but was profiting from "the sensational discoveries" of acellular living matter, claimed at that time to have been made by O. Lepeshinskaya in the Soviet Union in order to make a career of her own by adding to the general muddle in biology. A considerable part of the audience, however, did not seem troubled by the lecture. Some of them seemed proud of the achievements of Soviet science. It needs to be emphasized that many of the organizers and participants in this and other Lysenkoist lectures were simply Communist Party activists with no biological education. Others, though biologists and agronomists, had had no training in laboratory research. They were taxonomists or ecologists or plant breeders. For the years of the Nazi occupation had not been favorable to learning. Young people could accept Boshian's revelations because they knew no better. They had been insufficiently trained.

It is to be understood that members of the Communist Party, whether in Poland or in the USSR, tended to adopt the attitude that party discipline requires from them a complete acceptance of whatever the Soviet Communist Party officially states to be so. On the other hand, any actual knowledge of biology, in general, or of "formal genetics," in particular was lacking—was indeed "spurious." For example, I. I. Prezent, the chief theoretician of the "new biology," was by training a party propagandist, a specialist in Marxism and dialectical materialism. He learned what he knew about genetics only from Lysenko's own papers. For such persons, to discuss genetics at all was unthinkable—tantamount to supporting the Western lackeys of Wall Street. In their jargon, Weismannism and Morganism were "reactionary" and "idealistic," and genes were simply unscientific myths. In such an atmosphere there was no room for discussion. Any form whatsoever of expressing one's personal views was

regarded as a declaration of hostility toward Soviet science.

Numerous communist party members who were engaged in propagating Lysenkoism in Poland showed a simply amazing ability to accept uncritically all Soviet statements, especially those lying outside their own specialties. Such was Professor H. Petrusewicz. I stress the fact that personally he was very kind and decent. We were good friends. Before the war, he completed his Ph.D. thesis on the ecology of spiders; and much later, in the 1960s, he became a good ecologist. The extremely negative role he played during the Lysenkoist period in Poland was attributable largely to two facts. First, he was a dedicated Communist, and had been ever since his university days in Wilno in the early 1930s. Second, he was a real believer in all the articles of Communist faith. The war years Petrusewicz had spent in the guerilla forces. Directly after the war, he did whatever the Party ordered. He rose to become Vice Minister of Approvisation, and then a Vice Minister of Marine Affairs. His ignorance of the problems with which he had to deal somehow troubled him not at all. I believe that in 1949 he was given the title of "Professor" and delegated to propagate Lysenkoism. He had no notion of what genetics was, but once again that did not bother him. He struggled vigorously against genetics and believed in Lysenkoism with never a doubt.

There were a few scientists who knew something of genetics, but nevertheless pretended to accept the "new biology" because they were scared. A very prominent example was that of Professor S. Skowron, who was educated in the West. In 1948, there appeared in print a textbook of genetics he had written (the wrong sort of genetics, for Poland at that time). I have been told that after Lysenko's victory in the summer of 1948, Skowron went through all the bookshops of Cracow, buying out all copies of his book. It was just at that time that Poland was full of rumors about the persecutions of "formal geneticists" in the Soviet Union, and about the disappearance of such personnages as Vavilov, Karpechenko, Koltzov, and others. This was the period of the severest Stalinist terror. Hundreds of thousands, if not millions, of people were imprisoned or killed. Thus, it is not surprising that people were frightened. On the other hand, my personal experience shows that the actual situation in Poland was far less dangerous than that in the Soviet Union.

After my return from Sweden in 1948, I still continued for some time to lecture on genetics in Warsaw University, and I made preparations to organize a separate department of genetics. Soon, however, the Lysenkoist version of genetics became official, and the Council of the Faculty of Biology asked me to abandon teaching the old, erroneous genetics and to introduce in its place the correct, new one. My answer was that there is only one genetics — that which is based on well-established evidence. Then a compromise was offered: I should teach both the "new" and the "old" genetics. I retorted that this could not be done, inasmuch as they were contradictory. I was then temporarily forbidden to teach genetics at all. Professor Petrusewicz wanted to convert me, so took me for an excursion to the Soviet Union. As a special privilege, he organized for me an official visit to Lysenko in his office at the Institute of Agronomy in Moscow, so that I could learn at the very source of enlightenment and come to change my views.

The meeting was quite strange. I was taken into a big office, where along one wall several gentlemen were seated in complete silence. They remained as mute witnesses of my visit until it ended. I have no idea why they were there. Lysenko greeted me with the statement: "If you will not believe in what I am going to say then your visit is pointless." I simply smiled. Then Lysenko started his monologue. It lasted about two hours. Since, he said, you are from the Botanic Garden, then you must have observed that in greenhouses there grow various species of plants that do not appear in the open. I attempted to point out that they were plants from warmer climates and needed to grow at higher temperatures. He retorted that that opinion was entirely wrong. The greenhouse plants never grew outside of greenhouses because they were formed there as a result of artificial conditions for growth. I did not argue any more. He continued in a similar way, with so many absurdities that I can remember only some of them. He said, for example, that plants do not take up minerals directly from the soil, but the process is mediated by soil microorganisms such as bacteria and fungi. People think that cuckoos lay their eggs in the nests of other birds, but they are wrong. In fact,

cuckoo chicks develop from the eggs of the host species. That is just one example of the transformation of one species into another, he stated.

Then he said, "If a pond was covered hermetically and its soil and water sterilized, I guarantee you that after some time there would appear in it frogs and other animals, and plants." Apparently, then, he believed in spontaneous generation. As he spoke, his mouth frothed, his voice became more and more aggressive, even though no one had contradicted him. I sat silently, since any polemics would have been pointless.

Lysenko seemed to pronounce revealed truths, to be possessed like Rasputin, and with the fanaticism of a Savonarola to be ready to send his opponents to death on the pyre. He impressed me as having some sort of mental illness, and to believe fanatically in what he was saying without any need to explain such completely unorthodox ideas. This was clearly not an auspicious introduction for converting an unbeliever into accepting his theories and views.

The next day I found out that at least some of his coworkers so much wanted to please their master that they simply falsified the results of their experiments, or described them in such a way as to substantiate his fantastic claims. At that time, O. Lepeschinskaya was claiming that cells can be formed out of "unorganized living matter," and Lysenko supported that crazy idea. From that position arose both spontaneous biogenesis and Boshian's ideas of the derivation of *Penicillium* from penicillin.

The day after my visit to Lysenko I visited the Department of Genetics at the Academy of Agronomy in Moscow. It was under the chairmanship of J. I. Gluschenko. He was a close associate of Lysenko, and also came from the Ukraine. I do not know what sort of education Gluschenko had received, but he impressed me as being a very self-confident and cunning man—so to say, a Kolchoz official. My visit having been prearranged, Gluschenko awaited me in a small office, where I was to be shown an experiment in which tomato shoots were decapitated, and the processes occurring at the cut surface that would give rise to a callus, then to new shoots, were studied. In this room stood a long table with a row of microscopes, at each of which there was a girl in a white coat. Gluschenko told me that under the microscopes, in sequence, I could see the different phases of formation of new tomato shoots. This was said to occur in the following steps:

1. At the beginning, on the cut surface of the shoot, amorphic (that is, acellular) living substance is produced.
2. At the next stage, cell walls are formed, but the cells within them have no organelles.
3. Next nuclei appear, but they contain no chromosomes.
4. Then chromatin is produced, so that the cells become complete.
5. At the final stage, the cells divide and callus is formed.

After these introductory explanations, I was invited to look at the slides. The microscope preparations shown to me were so poor that, in fact, nothing at all could be seen. One could imagine whatever one was supposed to see. Even a first-year student of biology would be ashamed of making such preparations. I asked Gluschenko what fixatives were used. He was not pleased by that, but responded that of course they used alcohol. To hear this unnerved me so much that I asked whether they used vodka. The question was rightly taken to be offensive and provocative, and quite typical of the self-confident supporters of formal genetics. So the visit ended. To me it had been very enlightening. I found out at first hand that the faithful followers of Lysenko prepared their scientific results just to support his fantastic theories. In this particular case, it was of course difficult to estimate where simple ignorance of appropriate techniques ended and conscious falsification of the results began.

At that period, Lysenko claimed that one species of plant can undergo transformation into another; for instance, rye (*Secale cereale*) may be transformed into the wild grass (*Agrostis spicaventi* L.) which, on wet, acidic soil can overgrow a field of rye. Lysenko also believed that trees can often be transmuted from one species to another. Just at that time, in a journal edited by the Institute of Botany of the USSR Academy of Sciences—an institution that had had a very long and brilliant scientific tradition—some followers of Lysenko described just such a case, a transmutation of pine to birch(?) in a forest near Leningrad. (I regret that I cannot remember the exact species which were involved.) To prove this, they published a joined photograph of the lower part of a pine tree at-

tached to the upper part of a birch. Years later, after Lysenko was overthrown, research students from that same institute found the original tree and photographed it from the other side. This new photograph was published in the very same *Botanischesky Zhurnal* (*Journal of Botany*). As might have been expected, there were actually two trees there, growing very close together, and even partly fused. Such was the crude falsification made to support the statements of the founder of the "new biology" in the USSR.

It may be safely assumed that Lysenko had many more such obliging coworkers. Perhaps most of them were ready to fabricate confirmatory results however stranger Lysenko's ideas became. That would of course be much more dangerous than just to promulgate unscientific theories. Along with ignorance and uncritical belief in those proclaimed theories there went also plain, cynical falsification of evidence.

Such was my deep conviction when I returned from my visit to the USSR. I expressed my views frankly to the authorities of Warsaw University and stated categorically that I would not teach my students crazy theories supported only by falsified evidence. The Scientific Council of the Faculty of Biology decided then to forbid me any further contact with students. Yet this was not a bad time in my life. I could devote all my time and effort to the research work on the cytogenetics and evolution of the *Geum* species. I was retained on the staff of the Botanic Garden, with no teaching duties. Since my research problem fascinated me enormously, I was really quite happy. I would not anticipate a fast career, but otherwise I suffered no harm.

In the years from 1949 through 1954 a vigorous Lysenkoist campaign was waged in Poland. Many books and pamphlets were published to further the campaign, with characteristic titles such as "On Creative Darwinism" or "Science in the Soviet Union, the Country of Socialism," (both of these by a Polish author, W. Michajlow), or "Against Reactionary Mendelism-Morganism" (tranlated from the Russian). Probably all of Lysenko's own publications were translated into Polish, along with five volumes of Michurin's writings. There were two books translated from English, "Soviet Genetics," by A. G. Morton, and "Lysenko Is Right," by J. Fyfe. The second of these seemed a bit mentally deranged; whereas Morton's book was a unique instance of Soviet propaganda written with British style and courtesy.

Of course the effects of the Lysenkoist campaign were widespread and particularly damaging to the development of biology, and especially to plant and animal breeding in Poland. First of all, the entire younger generation of scientists was strongly affected. Some of them became sincerely convinced that real biological science began with Lysenko, Michurin, and other Soviet scientists. Others came to a more cynical conclusion, that it does not matter what the truth is, but only that, in order to succeed in life, one must support the ideas adopted by the ruling authority. For many party members, to propagate even doubtful ideas was accepted as being Marxist, like the matter of bowing to party discipline, and was thus beyond any criticism. Lastly, weak persons, even if they realized that there was something wrong with Lysenkoism, preferred to suppress their doubts, or at least not to express them openly in the prevailing atmosphere of general terror. These persons were prepared to accept any compromise with conscience in order to achieve personal safety, or maybe advancement. This last class of persons, however, was not numerous. A majority of the young scientists and university students lacked any conception of Western genetics and found such notions as the inheritance of acquired characters intuitively appealing.

Among agronomists, the concept of the inheritance of acquired characters seemed to account beautifully for the adaptation of organisms to the environment, and was thus tacitly accepted. Early in the Lysenkoist period in Poland an Institute of Plant Breeding and Acclimatization was established. For many years, even after Lysenko's fall, the post of director of this institute was held by Professor J. Lekczynska, a devoted Lysenkoist and also an astonishingly ignorant person. She was nominated for membership in the Polish Academy of Sciences (although she was not elected). In her Institute she stimulated procedures of plant breeding according to Lysenko's recipes. That is to say, if inheritance means assimilation by an organism of environmental factors, then any plant can be forced to grow in any climate. That belief accounts for the term "Acclimatization" in the name of the Institute. Accordingly,

numerous attempts were made to grow rice in the Polish lowlands, while in the Tatra Mountains coffee bushes were planted. In both cases the results were easy to foresee. Fortunately for the country and its people, in most of the plant breeding stations traditional methods of crossing and selection predominated.

Much heavier were the losses in general and agronomic education. In the late 1940s and in the 1950s, new school books were printed, for the first time since the end of the war. The authors were handpicked by the Ministry of Education, which applied a sole criterion of political orthodoxy. The textbooks of biology were full of Lysenkoism; Mendelian genetics was mentioned only in certain derogatory remarks. Along with the school books, dozens of brochures were published, like those mentioned previously. Not only were the contents of the books on a shamefully low level, but even "politically neutral" subjects were full of errors. The achievements of Russian scientists were overstressed; those of scientists of other nations were diminished in importance, or simply omitted. The situation was neatly summed up in a joke current in 1956: the founder of geometry was the great Russian scientist Pietia Goras (i.e., Pythagoras).

In Poland the period of Lysenkoism ended in 1956, but some of the textbooks remained unchanged for several years afterwards. As a result, teenagers finishing the secondary schools were convinced (or pretended to be) that biology was developing only in the Soviet Union, while in other countries scientists were idealistic, dogmatic, or characterized by other opprobrious terms, the real meanings of which they did not know. In Poland, in order to be accepted into a university, one must pass examinations. The answers given to the questions on the entrance examinations for the Faculty of Biology were often quite embarrassing, silly, or amusing. The principal author of the biology textbooks for use in the schools was the Polish parasitologist, W. Michajlow, who for many years worked in the Ministry of Education. He started writing textbooks and many sorts of propaganda brochures in the late 1940s, and continued for many years to do so. He was often referred to by the candidates in biology as a "great Soviet scientist." Another wrote that the theory of evolution was created by two great Soviet scientists, Michajlow and Darwin. That poor student could not even realize how far apart Darwin stood from other persons he heard named in school.

The Lysenkoist propaganda extended from the primary school all the way up to the university. Young people had no chance to deal with it critically, on strictly scientific grounds. The only source of skepticism lay in the fact that the Lysenkoist propaganda came from the East, together with Communism. This realization was often expressed in jokes. For example, according to certain young Poles the most famous achievement of Michurin was to make a hybrid between an apple tree and a dog. The hybrid would bark whenever a thief tried to steal any apples, and it was capable of watering itself. Unfortunately, the few persons in Poland who could have supplied valid arguments against the "new biology" remained silent. Some were afraid, and pretended to approve it; others simply had no possibility of expressing their opinion openly.

At the very beginning of the Lysenkoist propaganda in Poland, the concept of the inheritance of acquired characters was vigorously supported by one outstanding biologist, Professor Dembowski. He was a well-known animal psychologist who, before the war, had worked in the University of Wilno. In 1946-47 he was given a post as scientific attaché in Moscow, and there he worked in the Institute of Experimental Biology of the USSR Academy of Sciences. Subsequently, he held positions in the University of Lodz and in Warsaw. In Warsaw, he organized the Institute of Experimental Biology, and participated in organizing the Polish Academy of Sciences, of which he served as its first president. In the years 1952 through 1956 he was also president of the Polish Parliament. Even before the war, he was known to be very critical of Mendelian genetics, and expressed his criticism in papers he published in the *Zeitschrift für Abstammungs- und Vererbungslehre* (now entitled Molecular and General Genetics). Thus, among Polish biologists, he was the first to commence propagating the "new biology."

I believe the first Lysenkoist conference in Poland took place in March of 1949. Dembowski was its organizer and was also the principal speaker. Officially, the organizers were a newly formed Society of Marxist Biologists—the very name speaks for itself—and the edi-

torial board of the journal *Nowe Drogi* ("New Ways"), which was the principal ideological periodical of the Communist Party. The participants in this conference were predominantly drawn from various academic schools in Warsaw, but there were also some from other places. They were biologists, agronomists, psychologists, and also some party activists only loosely, if at all, connected with biological subjects. The proceedings of this conference were edited by Dembowski under the title "On New Genetics". That may explain why no geneticists were invited to attend. The conference took place shortly after the famous session in Moscow where Lysenkoism was finally accepted as an integral part of Communist ideology.

Dembowski began his introductory speech by describing what great losses in Soviet agriculture were due to the wrongful ideas of Mendelian genetics. No details were given. Then he passed on to describing the great successes made in plant breeding by Luther Burbank, such as the production of onions with the fragrance of magnolias. He concentrated in more detail on Michurin's achievements. Then he described Lysenko's brilliant results and attacked Mendelian genetics. He claimed that Mendelian segregation need not in fact reflect statistical regularities and therefore led to no important successes. The long and elaborate lecture was closed by a statement that the speaker fully supported the opinion expressed by the Soviet "philosopher" Prezent that Western genetics was crumbling and had no future. Thus Dembowski presented the new Soviet genetics and contrasted it with the "formal" genetics of the West, which, he affirmed, was entirely in error. Dembowski said, for instance, that "Darwin's idea that the inheritance of characters can depend on material particles had adverse effects on the further development of genetical research. The ability of an organism to react to environmental factors in a specific way cannot consist of particles. Such views have no biological meaning." Instead, Dembowski supported Lysenko's opinion that "sex cells originate and are built from particles that are formed from substances coming from different tissues and parts of organisms and undergoing numerous (but regular) changes." [There seems to be some confusion here, since Darwin's long-abandoned theory of pangenesis fits the quoted opinion of Lysenko quite perfectly.] Obviously, Dembowski, being an animal psychologist, was not up to date in respect to the current concepts of Western genetics, but he was prepared to deal bravely with the rather unclear concepts expressed by Lysenko.

In the discussion that followed, a number of speakers who belonged in a political meeting rather than in a scientific conference, declared their loyalty to the party line. Typical was the speech by W. Michajlow, already mentioned above:

> . . . [This] Soviet experience must be used and creatively adapted by us. . . . We must correct the programs in the secondary schools, universities, and particularly in agronomic education . . . .

Other speakers, plant or animal breeders, more or less explicitly expressed their support for Dembowski's opinion and declared that they would introduce the new ideas into their respective fields of work. Still others, who were physiologists, botanists, or even psychologists, spoke without really adding anything. The only speaker who defended any aspect of genetics in this conference was Professor M. Korczewski, a plant physiologist from the College of Agriculture in Warsaw. He asked Dembowski, "Are there any attempts to clarify what is the material substrate and chemical transmitter of hereditary characters? . . . Are they compounds involving desoxyribonucleic acid, of which genes are supposed to be built, or other substances? . . . What is their relation with the chromosomal apparatus?" Korczewski also asked whether any attempts were made to find out what sort of substances were exchanged between the types used in "vegetative crosses," that is, in grafting experiments.

In his summary of the discussion, Dembowski stated that he had nothing further to say to most of the discussants. In answer to Korczewski, he said that he himself did not know what geneticists think the gene really is. "Geneticists think that the genes are enzymes. . . . In classical genetics enzymes are foreign bodies. . . . It was very unfortunate that chromosomes are stained easily by nuclear dyes. They are thus particularly noticeable in microscopic preparations and thus attract attention and look as if they were important. That was pure accident. . . ," and so on.

Thus did the highest level of Party and

Science resolve the controversy between the old and the new genetics. Thereafter, the "new biology" was to dominate exclusively and was to be developed not only by scientists but even by kolchoz members in Poland. In this way the development of genetics in Poland was affected seriously by the ideas of Dembowski, who had never even heard about DNA.

Not a few other biologists held similar views. For example, a zoologist who at this critical time was dean of the faculty of biology at Warsaw University described the phenomenon of chromosomes in a public lecture in the following way:

> When a nuclear dye such as gentian violet is used, the whole preparation is heavily stained. Chromosomes become visible at a certain point in the removal of the dye. But when this process is continued, the chromosomes simply disappear. Hence the chromosomes are just temporary pictures observed during removal of the stain.

This man was a zoologist specializing in taxonomy and zoogeography and, probably, for many years had not made, or even looked at, any cytological preparations. But he faithfully supported the regime.

Soon after the Dembowski symposium, W. Michajlow had conferred upon him by the government the title of professor, and he began writing textbooks for the schools, pamphlets and articles, as aforementioned. Largely owing to his zeal, Lysenkoism was included in the curricula of the lower schools and, though to a lesser extent, even in the programs of the universities and research institutes.

Of course, it was not sufficient to forbid the teaching of the ideologically wrong, reactionary, idealistic, and even racist Western genetics. It was necessary further to train properly the future university teachers. A number of courses in the "new biology" were organized, chiefly by W. Michajlow and K. Petrusewicz. The longest and most thorough of these courses was presented in the summer of 1952 in Dziwnow. Here, young biologists assembled from different universities were subjected for an entire month to an intense brainwashing. The main lectures given there were edited by Petrusewicz, Michajlow, and S. Skowren and printed under the title "Problems of Creative Darwinism." The book, 756 pages in length—a sort of bible of the "new biology"—appeared in print at the end of 1952. In Poland, at that time, only the most politically important books were printed as promptly as that. Over twenty lecturers participated in the course. Two of them, although in other fields of research, actually knew Mendelian genetics quite well; notwithstanding that, however, they vigorously supported Lysenkoism. The rest of the lecturers were drawn from such fields as evolution, taxonomy, embryology, or paleontology. They presented different kinds of data from their respective fields, carefully avoiding any genetical interpretation.

In his introductory chapter, Petrusewicz appealed for a struggle against idealism in science, particularly in genetics. This needs some comment for Western readers. At that time everything that was not accepted as a part of the official philosophy of dialectic materialism was, by definition, considered to be "idealistic." The meaning of this pejorative adjective did not correspond to any notion of idealism in the Western world. The struggle with "idealism" in genetics was a kind of slogan used by Lysenkoists against the notion of the gene as a hereditary particle that is not changing directly and adaptively under the influence of the environment. This resulted, of course, from complete ignorance or rejection of all experimental evidence of the nature and role of the genes in heredity. DNA was not even mentioned during the whole course. In reality the Lysenkoists should be called "idealistic" as they claimed preconceived ideas without any experimental proof.

Petrusewicz stated that the purpose of the course was to teach the young participants the principles of creative Darwinism, and in particular to present the achievements of Michurin and Lysenko, who had succeeded so brilliantly in directing and speeding up the evolutionary processes, so that they might "become a conscious and determined cadre prepared and ready for the battle for the new biology." Then he delivered a long lecture on the development of the idea of evolution, and the essential roles of Michurin and Lysenko in the foundation of "Creative Darwinism." This lecture was followed by a series of others presenting the evidence for evolution from various fields of biology. All speakers deftly avoided entering into the controversy over genetics. There

was, however, also a series of lectures fully approving the slogans of the "new biology." Such was Professor S. Skowron's lecture, "The Inheritance of Acquired Characters." In the beginning of his contribution, Skowron said ". . . the inheritance of acquired characters is an essential premise of modern genetics and of Darwinism as a whole. Recently this premise has been fully proven." Such a statement ex cathedra was intended to prevent any discussion of the subject. Next, this author presented a wide repertoire of "proofs" of the inheritance of acquired characters, on the basis of "experimental data" derived from the "new biology." He even mentioned the possibility that the nuclei of cells and the sex cells can be formed from acellular living matter. To do this was particularly reprehensible on the part of Skowron, who had done research in genetics in the laboratory of Winge in Copenhagen, and had subsequently written a textbook of genetics. Can one not say that Skowron proved himself to be a good example of adaptation by "assimilation and transformation of environmental factors" on the part of a living organism?

Other chapters, such as "Inheritance and Some of Its Regularities," by M. Birecki, and "Transformation in Inheritance," by A. Makarewicz and K. Kaniewski, consisted chiefly of incoherent slogans pertaining to the "new genetics." These authors were agronomists by education, had hazy ideas about biology, and no experience even in plant breeding. Their contributions resembled nothing so much as speeches made in a political forum, and assumed absolute truth on the basis of authority. They stated that genes do not exist, that Mendelian segregations happen only from time to time, and that all characters are acquired through assimilation of environmental factors. They gave supposed examples of the transformation of one species into another. The general trend of their argumentation was that it is not at all surprising that genes and chromosomes do not play any role in inheritance. Under bad conditions, rye degenerates into *Agrostis*; ". . . if there is no proof one should look for it; the conscious transformation of organisms should be achieved through one's own efforts." One may assume that with real faith any miracle can happen. Of course, as party members and good believers, these persons delivered their revelations with conviction and zeal.

Very curious was the fact that the course included lectures on Pavlovism. On this subject the contributors were Professor A. Jus and his wife. At that time Jus was a director of a large psychiatric clinic. It was well known that both the Professor and his spouse were actively involved in the shameful procedure of remanding the political enemies of the regime to psychiatric hospitals, a practice long and widely employed in the Soviet Union. In his lecture in the Dziwnow course, Jus gave the following reasons for presenting Pavlovism to the students: "Pavlovism is also connected with the theory of Michurin and Lysenko, in respect to its entire attitude. The purpose of research [in this field] is to increase the grip on the subject under investigation in order to modify and correct it." As a consequence of his activities in modifying and correcting humans in psychiatric clinics, Jus later found it expedient to emigrate from Poland.

Passing over certain other chapters that do not bear discussing, we come to the two final lectures: "On Soviet Creative Darwinism," by Petrusewicz, and "The Social Role of Science," by Michajlow. Both chapters were strictly propaganda, as they dealt mainly with the philosophical basis of Soviet "Creative Darwinism." The main conclusion they reached was that in capitalistic countries the theory of evolution had gone entirely astray and led to entirely wrong conceptions, while its practical applications led to war, starvation, and unemployment. Only Creative Darwinism, as developed in the USSR, is based on dialectical materialism and consequently leads to both theoretical and practical conclusions. These two speakers appealed to younger scientists to develop the "new biology" actively and to apply it to agronomy.

This short summary of the Dziwnow course may provide some idea of how much time and effort went into Lysenkoist propaganda in Poland. Numerous other, usually shorter, courses of a similar nature were created. The role of DNA in inheritance was never mentioned, even though in the 1950s such knowledge was well advanced. Consequently the young people were misled and biased by hearing such constant stress laid on the ideological aspects of science, to say nothing of the effect of the ridicule heaped on "scientific adversaries," namely, non-Marxist scientists who by very definition must be

wrong. Young students, moreover, had ample opportunities to realize that if they did not accept Lysenkoism, with all its slogans, they would be excluded from the local scientific community and would be unable to carry out any research. On the other hand, certainly Lysenko's slogans, his attacks upon recognized scientific authorities, and particularly the brilliant prospects promised by the "new biology" carried a real appeal to any person who completely lacked any idea of what science really is. Lysenkoism was supposed to be an integral part of Communist ideology; and Communism, while hated by a majority of the people, was fully approved by others. A former Michurinist of Jewish origin who, as a boy of 7 to 13 years of age had particularly terrible experiences during the war, recently wrote:

> After the war I perceived the world as a terrifying place . . . . The only ideology and the only force that promised a good and just world was Communism—destroyer of the Nazis. I believed that Communism is right, I wanted it to be right, it had to be right for me to go on living . . . . In 1948 I was a schoolboy and had no idea about genetics. I understood very little of Lysenko's speech . . . . But clearly that was my fault, I did not know enough, I did not understand.

As a result of the widespread propaganda, Lysenkoism took full control of biology and agronomy. In numerous plant and animal breeding institutions, Lysenkoist "methods" were introduced, at least nominally. Many experiments on "vegetative crossing" were performed, or were claimed to have been performed. Somehow, their results were never reported. I have no way to evaluate the amount of economic loss that resulted from the application of Lysenkoist methods, but certainly they were considerable, as in the USSR itself. Fortunately, a majority of our plant breeders mouthed the Lysenkoist rhetoric but, in actuality, applied strictly traditional methods of crossing and selection in their work.

The chief losses were certainly in general education. Several generations of young people who had finished secondary schooling during and after the war came to the universities with a poor knowledge indeed of biology. The later generations were, if anything, even more ignorant, since they were taught nothing but meaningless slogans. Agronomy students were conditioned by hearing repetitions of Lysenkoism. After such a course in agrobiology, the students were unable to do anything more than repeat a brainful of slogans.

In Poland, Lysenkoism was abandoned in 1956. At that time Professor L. Kaufman, an excellent animal geneticist, and I were able to visit France and Great Britain for a few weeks, in order to restore scientific and personal contacts with geneticists in the West. However, not until 1958 was I permitted to resume lecturing in genetics. The first modern textbook of genetics to reach us was *General Genetics*, by Adrian M. Srb and Ray D. Owen (W. H. Freeman, San Francisco, 1952). It was translated by my colleagues under my own editorship, and published in Polish in 1959. Only in the 1960s were courses in general genetics gradually introduced in other Polish universities. For ten years, then, teaching and research in genetics had been completely suspended in Poland, and not only that, but also information about the very rapid progress in genetical research going on in the world reached only a very few biologists, and in scanty amount.

At this point I would like to assure my colleagues and friends in the West that the entire Lysenko affair had nothing at all in common with true scientific discussion, differences in research results, or the opinions or interpretations of new experimental data. Those who have been exposed only to real science and real scientific discussions and have observed the phenomenon of Lysenkoism only from the outside can scarcely have any basis for understanding or imagining what went on in the Eastern part of Europe. It is simply incredible. Various discussions about Lysenkoism, in particular those involving leftist Western biologists, are so naive that one must laugh. First of all, it is necessary to understand that the majority of the so-called experiments cited to support Lysenko's claims were simply falsified by his coworkers. These persons, however well they may have known the elementary rules of scientific honesty and precaution against bias, had to present their superiors with whatever results they knew were wanted. This was essential if they were to survive. The experiments were performed without controls, and the results were not analysed statistically. Lysenko himself did not understand statistical methods and

simply hated statistics. To him, they seemed a whiff of the rotting West and were spurious for Soviet scientists. Besides, belief in what he proclaimed was required, so that persons saw what they were supposed to see, as in the earlier-mentioned experiments of Gluschenko. Lysenko himself had a great capacity for blind faith—he was possessed by this phenomenon, not so rare in Russian history—but whenever it seemed necessary he was quite prepared to cheat. Some years after his fall, I was told in Russia that when Lysenko had lost all of his high posts, he became chief of a milk farm in a kolchoz near Moscow. From there he sent annual reports indicating that the cows under his care were giving more and more milk every year. This, he claimed, was attributable to his methods, which he failed to disclose. A special commission set up to verify his reports found that the data had been fabricated. I do not know whether this tale is true, but it certainly seems probable. Afterwards, Lysenko was given a job as a gardener near Moscow, and in that post he did really take good care of the gardens around the dachas of Soviet party leaders.

The attitude of some Western scientists toward Lysenkoism is naive. In this great pseudoscientific discussion, even the philosophical implications of dialectical materialism for the "new biology" were in fact not really at stake. The entire philosophical setting was worked out by Prezent and some other "politicologues." Lysenko himself would have been quite incapable of exploiting such arguments. In my opinion, Stalin's approval of Lysenko's battle with academic circles was just a fragment of his design to split up society and destroy whatever social groups he considered to be ideologically alien.

In the Western nations there were also at the time, and since, persons who were Communists or sympathizers. Many of them leaned toward a support of Lysenkoism. In France, for example, Lysenkoism was strongly supported by the French Communist Party, and such ideas survived long after the fall of Lysenko in Russia. Disputes with such persons were made possible because they existed on the periphery of normal scientific life. It would be interesting to know what would have happened if the followers of Lysenko could actually have forbidden their opponents to carry on research on genetics and molecular biology in the Western countries. Fortunately, that is unimaginable in truly democratic states. Characteristically, a few years after the final end of the Lysenko episode in the Soviet Union, nobody in Moscow was even willing to mention it, so shameful did they feel it to be. It simply vanished.

It is my hope that this very personal account of my own experiences during the Lysenko affair will enable future historians to see it from a different point of view.

THE GRIM HERITAGE OF LYSENKOISM: FOUR PERSONAL ACCOUNTS

III. HOW I BECAME A LYSENKOIST

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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,

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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.

THE GRIM HERITAGE OF LYSENKOISM: FOUR PERSONAL ACCOUNTS

IV. DIFFICULT YEARS IN SOVIET GENETICS

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[This is a substantially revised and somewhat shortened translation by M. B. Evgen'ev (with the editorial assistance of M. M. Green) of an article published in Russian in *Ocherki istorii estestvoznaniya i tekhniki*, Vol. 35: 47–59, 1988.]

IN RECENT YEARS several publications have shed light on the difficult times in Soviet biology when T. D. Lysenko and his supporters were in power (Zh. Medvedev, *The Rise and Fall of T. D. Lysenko*, Columbia Univ. Press, New York, 1969; *The Medvedev Papers: The Plight of Soviet Science Today*, Macmillan (St. Martin's Press), London, 1971; *Soviet Science*, Norton, New York, 1978; R. Berg, *Acquired Traits: Memoirs of a Geneticist from the Soviet Union*, Viking-Penguin, New York). These books have raised many questions, not only in the minds of the younger but also in those of the older generations—in fact, among all who possess an interest in biology, yet were neither participants in, nor even witnesses to, the tumultuous events in biology during those difficult years. Even the most interested persons do not know to what disastrous consequences "Lysenkoism" led for the whole of biology, or what harm it did to the national economy.

Even now, several decades later, it is worth answering such questions, after the antiscientific ideas of Lysenko and his supporters have been unmasked and their theories shown to be completely erroneous. It is, in fact, not only worth doing, but in my opinion it is necessary. Contemporary Soviet scientists, especially the younger ones, definitely need to learn the lesson, how important it is, always and everywhere, to stand for scientific truth, and to what fatal results the violation of ethical standards in science may lead. Such violations were typical of Lysenko's supporters, who used every means in their power to eliminate their scientific opponents in order to establish their own careers and to reach their personal goals. It is necessary to understand clearly how dangerous ignorance can be when it is in power. Beyond taking notice of all this, it is necessary to speak out about it, because even now Lysenkoism continues to exist, and no one should pretend that now all is well.

All of these considerations, and more, have made me write down what I know about the lessons Lysenkoism has to teach us, even though it is not easy for me to remember all those difficult and bitter experiences endured by so many Soviet biologists (and I include myself), nor is it comfortable to recall the names of my dear teachers and coworkers who gave their lives for scientific truth.

In the Soviet Union, the golden age of genetics began soon after the great October So-

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cialist Revolution in 1917. In the mid-thirties, genetics in the USSR stood undoubtedly in second place in the world, behind the development of genetics in the United States. To document such a claim, it is sufficient to mention the names of N. I. Vavilov, who described parallel patterns of variability in plant species throughout the world; N. K. Koltsov, who proposed the matrix principle of gene reproduction and postulated the idea that all genes in a chromosome represent one giant molecule, in that way predicting one of the main postulates of modern molecular biology; A. S. Serebrovsky, first to demonstrate the complex structure and divisibility of the gene; S. S. Chetverikov, who may safely be called the father of experimental population genetics; Y. A. Philipchenko, who made outstanding contributions to the genetics of plants and of domestic animals; and G. A. Levitsky, a prominent cytogeneticist and author of a famous monograph in that field. At that time, the students and followers of those named above who were already working actively and were widely known included B. L. Astaurov, I. A. Rapoport, A. A. Prokofieva-Belgovskaya, M. L. Belgovsky, P. F. Rokitsky, G. D. Karpechenko, N. P. Dubinin, N. V. Timoféeff-Ressovsky, M. E. Lobashov, V. V. Sakharov, and many others, including the present writer. Many leading foreign geneticists considered it an honor to visit the genetical laboratories in the USSR: William Bateson and C. D. Darlington from England, Erwin Baur and Richard Goldschmidt from Germany; Calvin B. Bridges, Hermann Joseph Muller, and L. C. Dunn from the United States, S. G. Harland from Great Britain; and D. Kostoff from Bulgaria. Several of these prominent geneticists visited more than once and remained in our laboratories to carry on joint investigations for several years.

By the end of the 1920s, however, the situation in Soviet genetics had already begun to change for the worse. At that time several neo-Lamarckians, including E. S. Smirnov, E. M. Vermel, A. M. Kuzin, and Vladimirsky were actively defending the theory of the inheritance of acquired characters, that is, of modifications of the organism acquired during life. This theory was in opposition to the materialistic theory of Charles Darwin, according to which natural selection, acting upon a background of randomly occurring variability (in modern terms, mutations and their combinations), is the main force in organic evolution. The neo-Lamarckians, however, declared that evolution follows in accordance with Lamarck's laws, that is, by the direct adaptation of organisms to their environments and the subsequent inheritance of such "acquired characters." Such modifications, however, were later repeatedly shown, from the time of Weismann on, not to be inherited.

The neoLamarckians in the USSR obtained great support from a group of Russian philosophers, including especially M. B. Mitin and P. F. Yudin, who asserted that Lamarck's theory of the inheritance of acquired characters corresponds to the main postulates of dialectical materialism. Opponents of this view were accused of "idealism," in the sense that they denied the influence of environment upon heredity. Although that time the nature of mutations was not fully known, by the end of the 1920s, it was quite well established that mutations occur randomly among genes and chromosomes exposed to an environmental agent such as X-rays (Muller); and that the effect of a mutation upon the viability and fertility of its possessors depends upon the particular environment in which development of the individuals takes place, and in what genetic combinations the respective mutants exist (Timoféeff-Ressovsky).

To prove the correctness of their ideas, neo-Lamarckians often cited the experimental results of the Austrian biologist Paul Kammerer, who had worked with an ascidian (*Cione intestinalis*) and with the midwife toad (*Alytes obstetricans*). They claimed that Kammerer's experiments had proved convincingly that acquired characters may be inherited. Actually, Kammerer performed his experiments carelessly, without the necessary controls, and without any quantitative analysis of the results. He used only primitive tests and estimated his results only approximately. That is why his results were never confirmed by other researchers using appropriate methods. In all cases, when Kammerer declared that his experiments confirmed the inheritance of acquired characters, he was subsequently refuted. In fact the story of Kammerer's claims and the subsequent disclosures of invalidity and fraud is now very well-known to biologists. H. K. Noble, of the American Museum of Natural History, a great

authority on the Amphibia, went to Kammerer's laboratory and found that the enlarged and blackened thumbs of the midwife toads that had been reared under altered conditions were in fact injected with India ink! Kammerer was away at the time, and later claimed that the fraud was perpetrated by an assistant of his who wanted to "make things come out right for his master." Then, Kammerer committed suicide on the train on which he was going to the USSR to take up a high post in biological research.

Soviet scientists at that time supported Kammerer because of his leftist political views. On the basis of the story of his tragic death, the Soviet Minister of Culture, A. V. Lunacharsky, wrote a plot for the film "Salamandra," the principal thesis of which was the progressive role in evolution of the inheritance of acquired characters. This film appeared quickly after Kammerer's death and contributed much to the success of the publications of Smirnov and other neoLamarckians. Many Soviet biologists who were carrying out investigations of a descriptive nature and who were not familiar with genetics and did not read its literature were also sympathetic with Kammerer's ideas. Yet it is noteworthy that in those very years many data were published in the USSR genetical literature refuting the results obtained by Kammerer. Among such authors I mention N. K. Koltzov, A. S. Serebrovsky, Y. A. Philipchenko, M. L. Levin, S. G. Levit, and S. S. Chetverikov.

Among these, Chetverikov was attacked in the pages of *Pravda* for his criticism of the scientific views of the "progressive" Austrian investigator. Hence, even the first and relatively mild wave of repression, beginning in the late 1920s, affected Chetverikov. At that time, he was head of the Department of Genetics of the Institute of Experimental Biology in Moscow, the director of which was Koltsov. Chetverikov had been the first geneticist in the USSR to lecture on biometry and genetics at Moscow University. In 1929 he was arrested, spent several months in prison, and then was exiled from Moscow. He became a teacher in the secondary school of the town Vladimir, and later was appointed to the chair of genetics at Gorky University, where he studied the genetics of the silkworm. In 1959, several months before his death, he was awarded the prized Darwin Medal of the British Royal Society, an award given to a select number of scientists for their outstanding contributions to the study of evolution.

P. F. Rokitsky, one of Chetverikov's students, was arrested at the same time as Chetverikov, and spent several months in prison. After he was released, he became a professor at Minsk University and an academician of the Byelorussian SSR Academy of Science.

During the mid-thirties, intensive debates in genetics began because of the rapid rise of T. D. Lysenko. I must first indicate the postulates on which he based all his applied agricultural practices.

First, he denied the existence of genes and declared that they were a myth invented by bourgeois idealistic scientists. Furthermore, he stated that chromosomes have nothing to do with heredity, and consequently to study them is not worthwhile. Lysenko refused to accept Mendel's laws of heredity, and called them simply "the invention of a Catholic monk."

Second, Lysenko unconditionally accepted the inheritance of acquired characters and denied the leading role of natural selection in evolution. He considered natural selection to have been "Darwin's mistake." He did not understand at all how this fundamental idea in fact provides the material basis for adaptive evolution.

Third, Lysenko asserted that one species may suddenly become transformed into another, without any intermediate stages. Thus, a birch might be transformed into an alder, oats into wheat, cuckoos into another species of bird, and the like. He accepted the notion that in puddles appearing in the springtime, little fishes might arise by means of spontaneous generation rather than from the fertilized eggs carried by birds, as had been so clearly proven in the past.

Lysenko never tried to prove his ideas, either by quantitative experimental analysis or even by reading the scientific literature. He looked through only a small part of the Soviet biological literature and completely rejected the foreign literature in genetics and related fields. He declared that the works of Michurin and Timiriazev represented the major source for his theoretical considerations, yet even this was scarcely true, for he was accustomed to take out of context various unrelated passages from the works of these scientists in order to confirm his own ideas. Lysenko often stated that his concepts were based on Marxist dialectical

materialism. This statement was also not true although it was necessary, for ideological reasons, for him to claim it to be so.

On the basis of his antiscientific and simply ignorant statements, Lysenko asserted that the methods used in agriculture and recommended by geneticists were absolutely pointless and harmful, and should be immediately abolished and replaced by methods he suggested, those of "Michurin's biology," a term he introduced. It is interesting, however, to note that not a single one of Lysenko's statements, cited above, occurs in Michurin's work. Lysenko promised that his methods would bring about a rapid improvement in agriculture, for highly productive plant varieties could be developed by appropriate breeding in two or three years, instead of the ten to fifteen years typically required when the Weismann-Mendel-Morgan methods were used. The phenomenal, indeed magical, rise of Lysenko began with his report in 1933 at the All-Union Congress of Collective Farmers. In that report, he solemnly repeated his promises of a rapid progress in Soviet agriculture provided his revolutionary methods were used.

Stalin was present at the meeting. He applauded Lysenko's report, and in a speech gave high appreciation to that contribution. The proceedings of this congress were published in all the principal newspapers, and Stalin's approval of Lysenko was of course emphasized. Lysenko's rapid promotion followed. In 1934, he was elected an academician of the Ukrainian Academy of Sciences. In 1935, he became an academician of the Agricultural Academy; in 1938, its president; and in 1939, he became an academician of the Academy of Sciences of the USSR. After Vavilov was arrested, in 1940, Lysenko became the director of the Genetics Institute of the Academy of Sciences, which had previously been headed by Vavilov. From 1937 until 1966 Lysenko remained a Deputy of the Supreme Soviet of the USSR, and its Vice Chairman. He was made a State Prize Laureate, was decorated with the Order of Lenin no less than eight times, and in 1945 became a Hero of Socialist Labor.

From the beginning of his rise, Lysenko selected totally loyal assistants. He preferred uneducated people who lacked any serious training in biology, for they would do their best to ingratiate themselves with Lysenko in order to advance their own careers. Thus, Lysenko's chief assistant and supporter, I. I. Prezent, was a lawyer. Lysenko recommended him for a professorship of biology, and he simultaneously held chairs in both the Moscow and Leningrad Universities.

The first detailed public presentation of the antigenetical ideas of Lysenko took place in 1936, at a discussion arranged by M. B. Mitin, who at that time was the managing editor of the journal *Under the Banner of Marxism.* On the arranged program, the principal speech was given by the distinguished American geneticist (and future Nobel Prize winner) H. J. Muller, who at that time was working in the Institute of Genetics in Moscow. The speech was made in English, and translated by myself into Russian. After Muller had finished, Lysenko took the floor and presented his ignorant ideas, concluding with harsh cursing of Morganism-Weismannism-Mendelism and geneticists in general. As far as I can remember, there were two other presentations, both of them brief. One was delivered by A. R. Zhebrak, who defended classical genetics; the other, by Chairman Mitin, who praised Lysenko. Vavilov, who was also present, did not take part in the discussion. The next morning, however, he gathered his coworkers together in his study and told them, with indignation, about Lysenko's speech. Vavilov appealed to all the geneticists who were present to fight with vigor against the aggressive ignorance of Lysenko and his gang. He also emphasized the potential danger of Lysenko's ideas for the future survival of the whole of genetics in the USSR.

Two more discussions of the same sort took place in 1936 and 1938. I was not present on those occasions, but I know that several geneticists, including Vavilov, Dubinin, and Zhebrak presented their views. Lysenko and his clique dominated the exchanges and, in fact, during these years, were highly praised in many newspapers and magazines, whereas the Mendelists-Morganists were attacked more and more severely. To illustrate the attitudes of the scientific administrators toward genetics, the following personal example is sufficient. In 1936, while working in the Genetics Institute, I finished my doctoral thesis which dealt with

the genetic structure of the heterochromatic regions of the chromosomes. This dissertation was successfully defended at the Scientific Council of the Institute, Vavilov being the chairman. The distinguished geneticists Serebrovsky, S. Navashin, and D. Kostoff were the formal opponents at the defense. Afterwards, the dissertation was sent, as usual, to the High Attestation Committee. Already, before the decision of that Committee, I had moved from Moscow to Kiev, in response to an invitation of the Presidium of the Academy of Sciences of the Ukrainian SSR to become head of the Department of Genetics of the Zoology Institute. I soon received a call to attend a session of the High Attestation Committee meeting in Moscow. Professor Y. I. Polyansky, from Leningrad, was also summoned to attend, as a referee. He evaluated my work positively and took part in the subsequent session. The entire meeting turned out to be a farce. In the first place, I was not admitted to the meeting; only Polyansky was permitted to be present. After he had read his favorable review, I was invited to enter the Hall. The only other biologist to be present was Lysenko. He asked me two questions: "Why are you declaring in your dissertation ideas that are contrary to the concepts of K. A. Timiriazev? How do you describe in your work the nature of the gene?" I answered briefly, that my investigation was purely of a cytogenetic nature, and that Timiriazev had nothing to do with cytogenetics; hence, there could be no contradictions in my dissertation with his ideas. Furthermore, I mentioned that I had said nothing about the nature of the gene in my dissertation, but took it for granted that genes undoubtedly represent the material basis of heredity. After this statement, Lysenko in a 90-minute speech characterized me as a typical Morganist-Mendelist and requested that my dissertation be rejected. And so it was. My subsequent doctoral thesis was on the subject of genetic polymorphism and natural selection in natural animal populations. I successfully defended it seven years later, and it was unanimously approved during the absence of Lysenko. My first doctoral thesis was published in 1939, in Ukrainian, as a monograph. Many years later, in 1958, I received a reprint from the American geneticist D. L. Lindsley of an article of his published in the journal *Genetics*. In that article Lindsley wrote that he had had my monograph translated, had repeated my experiments, and confirmed all my conclusions.

A similar experience was met by the later distinguished Russian geneticist A. A. Prokofieva-Belgovskaya, in her own doctoral defense. After her dissertation was rejected by Lysenko, she had to defend another one several years afterward.

The widespread anti-genetics campaign launched in the press and headed by Lysenko and Prezent at first described geneticists as scientific enemies of "Michurin's biology." Later, geneticists were regarded as ideologically harmful personalities, and finally they were declared to be enemies of the whole Soviet system. Two outstanding biologists, Koltzov and Vavilov, were most severely criticized. In 1939, a long, fierce article about Koltzov appeared in *Pravda*. Following that, a commission of the Presidium of the Academy of Sciences, including Lysenko as a member, condemned the entire direction of investigation taking place in the world-famous Institute of Experimental Biology which Koltzov had organized. On the basis of the conclusions of this commission, Koltzov was dismissed from his directorship of that institute, and it was then totally reorganized. The persecution he underwent damaged the health of Koltzov, and several months later he died of a heart attack. His wife, and for many years his coworker, M. P. Sadovnikova, committed suicide on the same day. In 1940, Vavilov was arrested and sentenced to death. After two years in a death cell, and without ever seeing his family, the death sentence was commuted and reduced to twenty years of imprisonment. Less than a year later, Vavilov died of exhaustion in the Saratov prison and was buried in a common grave. The exact burial place of this outstanding biologist is unknown.

After Vavilov's arrest, several of his coworkers along with other brilliant Soviet geneticists were also arrested and died in Stalin's torture chambers. I shall mention only three of them, each of whom I knew personally. They were Levitsky, mentioned earlier in this article, who died in prison at the age of 66 years; Karpechenko, the first geneticist to create a new plant species by means of interspecific hybridization and polyploidization of the hybrid.

Later it was proved that this type of speciation takes place in nature in many plant groups. Karpechenko, an excellent scientist, died in the prime of life, at age 43. The third person I mention was G. K. Meister, an outstanding geneticist and plant breeder from Saratov, who created several wheat varieties and obtained hybrids between wheat and rye. The entire list of the executed, Vavilov's students and coworkers, would be far too long to record here. It is worth noting, however, that several geneticists were arrested and killed before Vavilov's arrest. During the wave of repression in 1937 to 1939, I was personally acquainted with N. K. Belyaev, and worked with him in Chetverikov's group studying the structure of natural populations of *Drosophila*. He was arrested and executed in 1937. At the same time, S. G. Levit, one of A. S. Serebrovksy's students, was also arrested and executed. He was the director, as well as founder, of the Medical-Genetical Institute in Moscow, the first of its kind in the USSR and one of the very first such institutions in the entire world. Several others, such as the outstanding geneticists I. Agol and M. Levin, were also arrested and executed.

The well-known session of the All-Union Agricultural Academy in August, 1948, became the apotheosis of Lysenko's monopoly in Soviet biology. The destruction of genetics at this meeting had been carefully planned and prepared. The entire procedure was a true farce, organized by Lysenko's closest collaborators, Prezent, M. A. Olshansky, and Lobanov. After Lysenko's initial speech, more than fifty persons took the floor to laud his ideas and to vilify and accuse the practitioners of genetics. Only eight persons attempted a defense. They were I. A. Rapoport, M. M. Zavadovsky, S. I. Alikhanian, I. A. Polyakov, P. M. Zhukovsky, I. I. Schmalhausen, A. R. Zhebrak, and V. S. Nemchinov. In a concluding speech, Lysenko once more demolished genetics and the geneticists. Then he stated that his report had been read and fully approved by Stalin. That led three of the geneticists who were present to take the floor and declare that they withdrew their previously expressed opinions favoring genetics. These three were Alikhanian, Polyakov, and Zhukovsky. In any case, after Lysenko's ideas had received the formal approval of Stalin it was no longer possible to continue the argument. Many prominent Soviet scientists who occupied positions of leadership in the Soviet Academy of Sciences thereafter began to praise Lysenko in speeches and articles, and to cast opprobrium upon genetics and geneticists. The list of such lip-servers included the head of the Biological Department of the USSR Academy of Sciences, A. I. Oparin; Academician Keller, Corresponding Members of the Academy, Koshtoianz and N. Y. Nuzhdin; and Professors A. N. Studitsky, P. P. Lobanov, V. N. Stoletov, P. A. Vlasiuk, N. V. Turbin, and others.

In the Ukraine, in September of 1948 and soon after that "famous" session just described had taken place, a meeting of the scientific public was called. Olshansky, one of the closest of Lysenko's supporters, made a long speech describing the August session and its outcome. In his report, praising Lysenko and excoriating the geneticists and their concepts, Olshansky accused the geneticists directly of inflicting great harm upon science and the national economy of Russia. He criticized four geneticists who were working in the Ukraine by name: Academician N. N. Grishko, Professors Delone and I. A. Polyakov, and the present writer. We were described as the representatives of a reactionary ideology. After he had concluded, many other Lysenko supporters followed suit in accusing us. These were for the most part persons unknown in scientific circles. Only at the end of the meeting, in witness of the atmosphere of that time, was the floor given to the four accused geneticists.

Before this meeting, I had been summoned by the Party Secretary of the Ukrainian Academy of Sciences, Comrade Isacovitch. He strongly suggested that in my speech I should criticize genetics and concede some merit to Lysenko's doctrines. Were I to refuse, I would be discharged from the Academy and expelled from the Party. He further reminded me that before coming to Kiev I had been a student of Vavilov and had worked with him—a man who had been arrested as the "people's enemy." Equivalent advice was given to the other "Mendelists-Morganists" who had been selected to be picked apart at the forthcoming meeting.

At the meeting, when they gave me the floor, I did not proceed to reject the main postulates of genetics, but simply admitted that Soviet genetics was, as a whole, open to certain just

criticisms. For example, there had been no criticism of certain attempts abroad to use genetics as a basis of reactionary eugenic theories and approaches. I admitted, moreover, that my own investigations had not been useful to agricultural improvement; and that Lysenko's efforts to use science to aid the national economy were correct. Comparable speeches were made by Grishko and by Delone. As for Polyakov, on the other hand, he completely rejected classical genetics and pronounced himself a follower of Lysenko. All these speeches were recorded in shorthand, but when we asked to see the records, we were refused. It followed that our remarks were misrepresented in the published account of the meeting. It seems important to mention this, because I have recently found, in one of the issues of the periodical *Ogonyok* (No. 2: 7, 1988), an article by V. Soyfer concerning Lysenko in which a distorted portion of my speech was quoted. This author had probably taken it from the published report of the meeting.

In his concluding comments, Olshansky mentioned that the struggle of the Michurinists with the Weismannists represented a form of the international class struggle of socialism with both capitalism abroad and with some surviving bourgeois ideology lingering in the minds of some Russian scientists. He also declared that a victory of Michurin's revolutionary doctrine over the reactionary ideas of the Weismannists-Morganists was of great importance for the strengthening of the scientific basis of Marxism-Leninism. All persons who continued to support the antiscientific doctrine of Weismann-Mendel-Morgan would be unmasked and ruthlessly persecuted.

After that August 1948 session of the Agricultural Academy and similar meetings held in various cities of the USSR, the victory of Lysenko's doctrine throughout the country was complete. All geneticists who had been teaching in universities or institutes of the USSR were dismissed as being enemies of the doctrines of Michurin, by order of the Minister of Higher Education, S. V. Kaftanov. I, too, was dismissed from Kiev University, where I had been head of the Department of Genetics and Darwinism. All laboratories headed by geneticists were either closed or transformed into the new Lysenko model.

The Department of Genetics in the Institute of Zoology of the Academy of Sciences of the Ukrainian SSR was among those closed, and all its employees were dismissed. I was transferred, in the simple rank of "scientist," to another department of the Institute, where the ecology of the silkworm was being investigated. Later, however, the Presidium of the Academy thrice raised the question whether I ought not to be expelled from the Academy altogether, as being one of the enemies of Michurin's doctrines. Only the support of my Party comrades, those with whom I had worked in the town of Ufa during World War II, saved me from dismissal. I was fortunate, for I emphasize that almost all geneticists in the Soviet Union had a hard time indeed during this period.

All positions formerly occupied by geneticists were then taken either by persons who were ignorant in the field of biology, or by persons who understood that Lysenko's theory was antiscientific, yet nevertheless supported him in order to build their own careers. The well-known sociologist I. B. Bestuzhev-Lada recently wrote about this situation in an article entitled "Truth and Only Truth," which was published in the newspaper *Nedelia*. To quote him exactly:

> T. D. Lysenko took advantage of the atmosphere of repression to make his pseudo-scientific career literally on the bones of real scientists, and destroyed an entire branch of Science. He replaced the real scientist with a gang of his own, some of whom are still alive today.

The level at which "genetics" was taught in the universities and institutes of our country after 1948 may be seen by a glance at a manual written by N. V. Turbin, entitled *Genetics and Selection*, and published in 1950. This book remained an offical manual until 1963, and in some places even until 1968. Such topics as the following were included in the *Manual*: "The struggle of the progressive Michurin theory in genetics with the reactionary genetics of Mendel and Morgan"; "Reactionary distortions in bourgeois genetics originating from the class ideology of the imperialistic bourgeois"; "Complete bankruptcy of modern Morganism in theory and practice"; "The Golden Age of Michurin's genetics and selection in the USSR"; "The August session of the Agricultural Academy

and its importance for the development of biological science"; etc., etc.

Lysenko's doctrines were propagandized in the press on an unprecedented scale, and many who were not biologists but belonged to the higher echelons of power took part in the campaign. So we find the Chairman of the Council of Ministers of the USSR, V. M. Molotov, publishing a paper praising Lysenko. It is sad to relate that many academicians and professors also joined in the campaign.

Lysenko's ideas, which were devoid of any scientific basis, were officially incorporated into agricultural practice. These included such ideas as the rapid production of new plant varieties by proper nutrition, the transformation of varieties of hard wheat into soft ones, the planting of potatoes in the summer, the transformation of plants by means of "vegetative hybridization," and increasing milk fat and the breeding of cows by intensive feeding of mothers. All these measures, pursued on the collective farms for many years, were discredited by their bad results, but Lysenko and his supporters did their best to conceal the real consequences and, by whatever means, to portray them as successes.

With each passing year it became more difficult to hide the complete failure, throughout the entire country, to improve agriculture by means of all the practices incorporated by the direct orders of Lysenko into plant breeding. Then, after the death of Stalin, scattered publications appeared daring to criticize Lysenko's dogmas. Interestingly, it was physicists and chemists who initially spoke out. Later, they were joined by biologists and representatives of progressive agriculture.

It finally became clear that Lysenko and his supporters had destroyed a vital branch of Soviet science and had done great harm to the national economy. The ultimate fall of Lysenko became clear when, in 1965, there appeared an article in *Vestnick [Messenger] of the Academy of Sciences of the USSR* giving an account of the report of a committee that had been checking on the investigations carried out on Lysenko's farm near Moscow. The purported aim of those experiments was to create a new breed of cows by appropriate husbandry. The committee established that there had been clear-cut falsification of records, quite incompatible with the standards of serious scientific work. It should not be overlooked, however, that real genetics began to be restored long before 1965. For example, M. E. Lobashov began to give lectures in modern genetics at Leningrad University in 1957; and the first postwar textbook in genetics, which Lobashov wrote, was published in 1963. Thus, by the early 1960s, investigations in genetics were being carried on in many scientific institutes of our country. In the Ukrainian branch of the Academy of Sciences, the Department of Genetics was restored to being in 1958. In that same year, Professor P. K. Shkvarnikov began to give a lecture course in genetics at Kiev University.

It seems desirable in closing to mention two additional points. First, it is necessary to consider the great harm done by Lysenko to other branches of the biological sciences besides genetics. Second, it is important to draw attention to the existence even today of echoes of the Lysenko doctrine.

The complete domination of Soviet biology by Lysenko's dogma, with its official sanction, excluded any criticism of it for more than twenty years. It led to the destruction of classical and modern genetics and had disastrous consequences for most other branches of biology in the USSR. Microbiology and epidemiology were particularly heavily damaged. Lysenko and his patrons strongly supported the absurd ideas of O. B. Lepeshinskaya, who postulated that new cells may appear not by division of parent cells, but directly from "cell-free" substance. Lepeshinskaya claimed that she had refuted Virchow's doctrine, *omne cellula e cellula* (every cell from a cell), which had been formulated by that distinguished scientist (on the basis of the work of Pasteur, Schleiden and Schwann, and many later cytologists) during the last half of the 19th Century.

Lepeshinskaya considered Virchow's dictum to be a myth invented by a bourgeois idealist. She attempted to prove her theory by experiments in which she ground up the tissues of freshwater hydras as a base for spontaneous generation of new organisms. All her experiments, however, were primitive in nature and were interpreted in ignorance of the vast amount of work done on this issue by other scientists over several centuries. When the results of her investigations were repeated by Soviet and foreign scientists, none of the results she claimed could be confirmed. Sad to say, Lepeshinskaya's absurd theory was praised

highly not only by Lysenko, but by many serious scientists who were in a position to appreciate its absurdity. For example, her theory was supported by Oparin, who had become one of Lysenko's allies; by Professor Makarov, who was working as a cytologist in Leningrad University and who included Lepeshinskaya's theory in his textbook; and by Professor Novikoff of Kiev University (also a cytologist), who understood the foolishness of her ideas but notwithstanding praised her highly in his own book and in public lectures.

In these times there appeared in microbiology a theory proposed by G. I. Boshian, and strongly supported by Lysenko and his patrons. Boshian asserted that under certain specific conditions viruses may become transformed into bacteria, or the reverse. On the basis of this theory he advocated drastic changes in modern medicine, in particular in microbiology and epidemiology. Had these ideas been realized in practice, they would have done great harm to many people. True scientists who tried to criticize Boshian's ideas were severely attacked in the press as enemies of Michurin's biology. Professor B. G. Drobotko of Kiev was among those who suffered. Boshian's doctrine was officially included in microbiology courses in the universities and institutes of the USSR.

Lysenko also brought harm to plant physiology, inasmuch as he denied the existence of the plant hormones that regulate the growth of plants. He called them simply the inventions of bourgeois scientists. This position led the Director of the Institute of Physiology in Kiev, Academician P. A. Vlasyk, to severely attack Professor N. G. Kholodny, who was one of the first scientists in the world to investigate phytohormones. A strong reason to remember the events of Lysenko's domination over Soviet biology is the lingering harmful effect of his ideas right to the present time. They have possessed strong viability.

Like many other university scientists, I often meet people in the USSR who, on account of their practical activities, have something to do with variation and heredity in plants, humans, or other animals and who have a very keen interest in those phenomena, even though scarcely able to understand them. These include such professional persons as agronomists, teachers, veterinarians, and medical workers. I assume full responsibility for stating that many of these workers in applied fields have very dim ideas about the nature of modern genetics and molecular biology. Many of them, indeed, have considered Lysenko's doctrine to be true and useful. One can hardly blame any of these persons, for they were taught by professors and teachers in Lysenko's time.

Even now, among Soviet biologists, one may meet persons who not only share Lysenko's ideas but are still trying to propagandize them in their lectures and articles. In the last two or three years, for example, there has appeared a series of booklets written by Professor B. T. Ioganzen, of Tomsk University, and Professor E. A. Logachev, of the Kemerovo Medical Institute. These authors have tried hard to rehabilitate the main ideas of Lysenko, by combining them with certain concepts of modern molecular genetics. These booklets were widely distributed by the authors to different parts of the country. Such efforts may be dangerous to the minds of unprepared readers, since they create views that do not correspond to the modern state of science and might even be harmful if applied to the practice of medicine or agriculture. Taking this reasoning to heart, three Soviet biologists, Academician L. A. Takhtajan, Corresponding Member of the National Academy of Sciences Y. I. Poliansky, and I myself published in the journal *Priroda* (Nature) a collective review containing a detailed criticism of the booklets written by Iogonsson and Logachev. After this article appeared, we received many letters from readers approving our critique, but also letters of quite another kind, in which the writers accused us of attacking the progressive ideas of Lysenko. The mere existence of such letters demonstrates the continuing vitality of Lysenko's doctrine.

Another example of the long persistence of Lysenko's ideas was recently provided by an article written by A. N. Studitsky and published in "Science and Life," a Russian magazine. During the time of Lysenko's supremacy in Soviet biology, Professor Studitsky was one of his supporters. At the present time, Studitsky has changed his position slightly. He now admits that "Lysenko retarded the development of Soviet genetics by forty years." At the same time, Studitsky continues to believe in the inheritance of acquired characters.

In his article, Studitsky once again cited Kammerer's experiments and other irrelevant

evidence to support his belief. Once again, he attacks modern genetics and highly praises "Michurin's biology." He mentions that he wrote this article under the influence of reading V. V. Dudintsev's novel *White Coats*, a story about Soviet biologists in Stalin's time. Studitsky considers the publication of that novel to have been a mistake, inasmuch as it is not necessary, in his opinion, to rehash the tragic events of forty years ago. I have sometimes heard other persons express similar opinions in ignorance of what happened to Soviet genetics.

The very best answer to all such persons, I think, is to be found in the speech of M. S. Gorbachev at the seventieth anniversary of the Great October Socialist Revolution:

> Even now we encounter attempts to hide away from sick questions of our history, to silence them and pretend that nothing wrong has happened. We cannot agree with such a position. To behave so means to neglect historical truth and [show] disrespect for the memory of the victims of lawlessness and tyranny.

THE GRIM HERITAGE OF LYSENKOISM: FOUR PERSONAL ACCOUNTS

V. IN DEFENSE OF TIMOFÉEFF-RESSOVSKY

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THE REPUTATION and integrity of the late great geneticist, N. W. Timoféeff-Ressovsky [N. V. Timofeev-Resovsky, 1900–1981], have been recently attacked by certain German and Soviet scientists, writers, or journalists. These persons have attempted to incriminate him as a chief supporter of Nazi theories of race and genetic deterioration as well as a supporter of Nazi eugenic policies.

In the West the most active accuser has been Benno Müller-Hill, a molecular geneticist who is a member of the Board of Directors of the Institute of Genetics, University of Cologne [Köln], in West Germany. He has recently been joined by Karl Heinz Roth (1986), a physician and historian of Hamburg, who contributed to a book entitled *Der Griff nach der Bevölkerung* an article entitled "Schöner neuer Mensch," in which he traced the roots of Nazi eugenic policies to American and British investigators of genetic mutation and population genetics, but he especially singled out Timoféeff as the principal German scientific supporter of Nazi eugenic policies. His arguments have been severely criticized and rebutted by Bentley Glass, in a review of the book containing the article (Glass, 1989). In the Soviet Union, the attack upon Timoféeff-Ressovsky was launched by the journalists A. Kuzmin and V. Bondarenko, who are adherents or members of an ultranationalistic, racist society known as "Memory."

The writer Daniil Granin (1987), in his narrative *Zubr* [The Bison], while pretending to follow Gorbachev's new principle of "openness" [glasnost] and to call in question the unjust attitude of those in power toward Timoféeff during the regimes of Stalin, Khrushchev, and Brezhnev, actually managed to clear those political powers of guilt in the matter. He produced a highly distorted picture of Timoféeff. Even the title of his narrative is ambiguous, for the nearly extinct European bison (aurochs) means in colloquial Russian a person who stubbornly follows obsolete principles, a fossil. Like Khrushchev's minions, who swayed the destinies of prisoners, Granin would permit amnesty, but not rehabilitation, for Timoféeff-Ressovsky. Granin accused Timoféeff of staying in Germany when ordered to return to the USSR and of refusing to collect documents that would have proved his non-involvement either in framing the racist theory of the Nazis or in carrying out experimentation upon human beings who had been doomed to death. According to the opinion of Kuzmin and Bondarenko, it was Granin's duty to have unmasked Timoféeff's services to the Nazis, in order to prove that he did not deserve amnesty.

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Nikolay Vladimirovich Timofeev-Resovsky, a descendant of a noble family, was truly fortunate to survive the bloody turmoil of the Revolution and Civil War, from 1917 to 1923. The latter was the year when he began his studies in genetics, under the guidance of N. K. Koltsov and S. S. Chetverikov at the Institute for Experimental Biology in Moscow.

N. K. Koltsov was a radiant personality, a representative of the Russian Renaissance, a brilliant teacher, and a founder of scientific institutions. He organized various university departments, experimental stations, and the great research Institute of Experimental Biology. He founded several scientific societies and the periodicals they published. In 1938, he was first to formulate the template principle of chromosome replication. In establishing eugenics as a branch of human genetics, he created a new science antithetical to the perversions of science that arose in Nazi Germany. Chetverikov became even more recognized internationally as a leader in creating the synthetic theory of evolution and as the real founder of experimental population genetics.

As a student under these scientists, Timoféeff demonstrated a marked ability to combine the comparative method of biological investigation, which had been the chief means used in the development of biology in the 19th Century, with the experimental methods introduced in the 20th Century. In Koltsov's footsteps, Timoféeff also exhibited an extraordinary ability to make contacts with scientists in other specialties and to work in little-explored boundary areas lying between divergent branches of science.

In 1925, the German neuroanatomist Oscar Vogt, director of the Kaiser-Wilhelm Institut für Hirnforschung located in Berlin-Buch, invited Timoféeff-Ressovsky to join the staff of his institute. Under the existing arrangement for the exchange of scientists between the Soviet Union and Germany, this was readily arranged. At first, Timoféeff occupied the lowly position of an assistant, since he had not yet completed his doctoral degree. Later he became a postgraduate student. Vogt's aim was to develop in his institute a strong genetical approach to problems of mental functioning and mental disorder and disease, and he had made such an exchange of personnel a condition of his acceptance of an invitation to perform a neuroanatomical study of the brain of Lenin shortly after Lenin's death. Timoféeff rapidly fulfilled this goal, and before 1930 was already head of a new Department of Genetics at the Institut für Hirnforschung.

To show the impact of Timoféeff's work in Germany, it is sufficient to cite a brilliant translation of genetic postulates into the language of physics, made by Erwin Schrödinger in his book *What is Life? The Physical Aspect of the Living Cell*, first published in 1944, and reprinted no less than seven times by 1974. In this classic of science, Schrödinger presented for the first time a cybernetic concept of the transmission of hereditary information from generation to generation of living organisms. He based his formulation largely on Timoféeff's investigations and conclusions (Schrödinger, 1967). He wrote (p. 45):

> The laws governing the induced mutation rate are extremely simple and extremely illuminating. I follow here the report of N. W. Timoféeff in *Biological Reviews*, v. 9, 1934. To a considerable extent it refers to the author's own beautiful work.

From the work of Timoféeff-Ressovsky and of his coauthor, Max Delbrück, Schrödinger visualized an organization of living matter that would be compatible with its nonstatistical but deterministic mode of action. The gene was presented as being a unique unit of a hereditary code. This constituted a step forward not only for biology but also for physics itself. Schrödinger wrote (p. 73):

> From Delbrück's general picture of the hereditary substance it emerges that living matter, while not eluding the "laws of physics" as established up to date, is likely to involve "other laws of physics" hitherto unknown which, however, once they have been revealed, will form just as integral a part of this science as the former.

In order to study the structure and variability of the hereditary material, investigators of the period commencing in 1927 commonly used ionizing radiation, especially X-rays of extremely short wave-length. In using this technique, Timoféeff followed H. J. Muller in the study of induced mutations in the fruit fly *Drosophila melanogaster*. The results obtained by many investigators along these lines pointed to the danger existing not only for patients ex-

posed to X-rays or to the gamma rays from radium used for diagnostic or therapeutic purposes, as well as to the unshielded or poorly shielded medical staff members, but also extending to the progeny and later descendants of exposed persons. Schrödinger phrased this matter as follows: "The Timoféeff report contains a particular hint which I cannot refrain from mentioning here." This "particular hint" was the concern expressed by Timoféeff about the "possibility of gradually infecting the human race with unwanted latent mutations" (Schrödinger, 1967: 47–48). Muller, and soon after him also Timoféeff, were the first geneticists to urge strongly the critical need for the protection of human genes from radiation hazards. These views were confirmed and extended by several international committees during the 1950s: an American committee of the National Academy of Sciences, USA; a British committee of the Medical Research Council; and a committee of the United Nations. All of these committees agreed that the genetic hazard of exposure to high-energy radiations is far greater than had been supposed in earlier times, and that strict measures should be taken to safeguard the population from unnecessary exposures, whether on account of medical diagnosis or treatment, or from the products of nuclear weapons tests. In the context of this paper and the current vilification of Timoféeff by such persons as Müller-Hill and Roth, it is to be stressed that this was the sole involvement of Timoféeff-Ressovsky in "Rassenhygiene." Every one of Timoféeff's contributions to radiation genetics, population genetics, and developmental genetics is to be seen as also constituting an input into medical genetics and preventive hygiene.

The invitation from Oscar Vogt to Timoféeff to come to Germany to work in the Kaiser-Wilhelm Institut für Hirnforschung saved Timoféeff, as a descendant of the nobility, from mortal danger when Lysenko rose to power under Stalin's favor, and when many geneticists were doomed to extinction. Timoféeff's own brothers were among the targets of this political repression. One of them was shot, the other exiled. In 1929, Chetverikov was exiled from Moscow without inquest or summons. Genetics and geneticists were subjected to an ideological attack that became ever more menacing. Genetics, indeed, moved toward imminent disaster. The most brilliant geneticists of Soviet Russia—Vavilov, Levitsky, Karpechenko, and many others—were imprisoned, then disappeared forever. It was clear that to return to the Soviet Union would have amounted to an act of suicide by Timoféeff. Quite reasonably, he made no plan to return to Stalin's empire.

Nevertheless, in 1929, Timoféeff-Ressovsky was actually thinking of returning to the USSR in order to attend the All-Union Meeting on Genetics and Selection. Learning of this, his friends at the Institute of Experimental Biology, being fully aware of the danger to him, persuaded him not to come. Again, in 1937, it once more became known that Timoféeff was ready to come back to Russia; but Vavilov entreated him not to do so, passing the message to him through H. J. Muller. Koltsov also wrote a letter to him, and passed it out of the country secretly in the Swedish diplomatic bag. Koltsov declared: "Of all the methods of suicide, you have chosen the most agonizing and difficult. And this not only for yourself, but also for your family." Zhores Medvedev has documented these events in *The Medvedev Papers* (Medvedev, 1971, p. 94). It was thus that eventually the graduate student who had left Russia temporarily to work at the Kaiser-Wilhelm Institute for Brain Research became, first, head of a department, and later, a vice director of that Institute.

Timoféeff, however, according to the official Soviet designation, became a "nevozvrashchenets," a person who did not return to his country after being sent abroad, and hence was an enemy of the people. Yet he did not thereby lose his Russian citizenship. Obviously he prized that highly, and refused to accept the alternative of becoming a naturalized German. His mighty intellect, his noble patriotism, his knowledge of history, and his faith in the dignity of the Russian people inspired in him a belief that the time of slavery would pass, and that eventually he would be able to return to his native land. Nor was he an escapee, so long as he had not forfeited his Soviet passport.

When war between the Soviet Union and Nazi Germany broke out, Timoféeff-Ressovsky, as a citizen of a hostile country, was in danger of losing his status in Germany. Yet he was not sent to a camp for detainees, nor even dismissed from his position of vice director of the

K-W Institut für Hirnforschung. Even after his son Thomas, who became a member of the underground resistance, perished in a Nazi detention camp, Timoféeff himself was not removed from his high position. Thus it came about that the ultimate victory of Soviet Russia over Nazi Germany turned into a fresh tragedy for Timoféeff-Ressovsky. According to a statement made in 1950 to Bentley Glass by Timoféeff's personal friend, M. Rajewsky, who at that date was Director of the Max-Planck Institut für Biophysik in Frankfurt-am-Main, he had himself made a hazardous trip to Berlin in the last weeks before the fall of Berlin in order to try to convince Timoféeff to flee to the West before it became impossible to escape the advancing Russian Army. Timoféeff's response was characteristic of the man. He refused to flee, saying that it was his responsibility to try to save the Institute for Brain Research from destruction, as well as to save its staff members from harm. Since he could speak Russian fluently, and was indeed a Russian, he felt that he could explain the nature of the Institute to the commanders of the advancing troops and see that it was preserved from harm. And that is exactly what he did accomplish. Only later, on orders from Moscow, was he imprisoned. [See Bentley Glass, in the Foreword to this collection of articles.]

In the words of Alexander Herzen, "from victorious swords are formed the strongest chains." Timoféeff was imprisoned, sentenced to ten years of labor in a camp of correction. Here Timoféeff was isolated from scientific work of any kind, as well as from those members of his family, especially his wife Elena, who had survived the abysmal collapse (a "Götterdämmerung") of the Nazi "Gross Deutsches Reich."

Half dead after two years of hard labor and inadequate food, and suffering from a partial loss of eyesight and from pellagra, Timoféeff-Ressovsky was transferred to another punitive establishment, ironically called by dissident intellectuals a *sharashka*. (The word *sharashka* actually denotes a very shabby business based on fraud and extortion.) In these special prisons, scientists were gathered to raise the war potential of the socialist state. Here Timoféeff set to work to determine whether or not a treatment of the seeds of plants with small doses of radioactivity would stimulate the productivity of the plants. Here too, in the Urals, Timoféeff's wife and younger son were allowed to rejoin him and to share his imprisonment. By the time Khrushchev initiated his policy of de-Stalinization, Timoféeff had almost served his full sentence of ten years. The new era was heralded by the release and rehabilitation of hundreds of thousands of persons who had been exiled or imprisoned. In 1955, Timoféeff too, was released, but for him no rehabilitation followed. The mercy of Khrushchev, when condemning the bloody past, did not extend so far as to pardon one who had refused to return to the Soviet Union lest he fall a victim of the very terror that the de-Stalinization policy had so unambiguously revealed and denounced.

After his amnesty, Timoféeff-Ressovsky was permitted to live in the city of Sverdlovsk, where he founded and headed a Department of Biophysics at the Biological Institute of the Ural Branch of the Academy of Sciences of the USSR, and also to establish a small experiment station in the southern Urals. Thither came scientists of many specializations in a sort of pilgrimage to learn of this new science previously undeveloped in Russia. Timoféeff's coworkers at the *sharashka* were also released to work with him at the experiment station. A series of articles written by various members of the group and devoted to the effects of the stimulation of plants by radiation appeared in the journal *Biophysika*, founded by the USSR Academy of Sciences in 1956, as well as in the *Botanicheskii Zhurnal* and in the *Doklady Akademii Nauk*.

In the spring of 1964 Timoféeff-Ressovsky moved to Obninsk, in the Kaluga region southeast of Moscow and quite near the capital city. Here he organized a laboratory of radiation genetics within the Institute of Medical Radiology, and commenced to work on the role of aquatic plants and animals in absorbing radioactive elements and thereby preventing the pollution of reservoirs. He thus became a disciple of the founder of biogeochemistry, V. I. Vernadsky, and Timoféeff transformed that science into an experimental one.

During all the years since first going from Russia to Berlin, Timoféeff had never completed a doctoral thesis, although already in the early 1930s he was widely recognized as one of the leading geneticists in the entire world. At long last, he had the opportunity to obtain the academic degree of Doctor of Biological

Sciences. Actually, in the end, he was awarded the degree in 1964 only because, at the very moment of his defense of the thesis, the extreme fluctuations in the attitude of the government toward genetics turned in his favor. Just when Khrushchev lost political power, genetics got a lift.

Timoféeff's interest in biogeochemistry was by no means narrowly restricted to the need for cleansing reservoirs. The biosphere as a whole, a system including humanity as a component, and providing humankind with everything needed for breathing, feeding, and creating a suitable climate, became the center of his attention. The delicate balance between the living and non-living components of the biosphere, and between different species of plants and animals and microorganisms needed study and control. Thus the laws of the evolution of the biosphere, which Vernadsky had elucidated—and primary among them the law of the increase during geological time of the numbers of atoms drawn into the various life-cycles—had to be applied to the improvement of human life as a safeguard against the alarming increase of human populations (Timofeev-Resovsky, 1968).

The replacement of Khrushchev by Brezhnev heralded some liberalization in the treatment of science. Lysenko's power was restricted; genetics was legalized and true geneticists were permitted to work; new institutes and periodicals and a Society of Geneticists and Breeders were founded; new textbooks of genetics and textbooks of biology for high schools and universities were published (Medvedev, 1969). Yet along with the resurrection of genetics, the positive results of Khrushchev's de-Stalinization program were buried. The persecution of freedom of thought—that genie freed by Khrushchev from the bottle—was actually begun by Khrushchev himself. Next, the desire of the powers that be to rescind the privileges allowed by Khrushchev's reforms became more intransigent with every passing day. Among these was the amnesty that had been granted Timoféeff in 1955. Some of these acts of baiting I have described in my book, *Acquired Traits* (Berg, 1988, pp. 253–254, 293, 302–308, 310). More is to be found in a marvelous narration by Zhores Medvedev in his books, *The Medvedev Papers* and *Soviet Science* (Medvedev, 1968, pp. 70–112; 1972, pp. 134, 191). Medvedev has there spoken from the personal knowledge of one who was a coworker with Timoféeff in the Institute of Medical Radiology in Obninsk.

The paradox of the increasing persecution of a geneticist at the very time when genetics was being rehabilitated as a science and when the participation of a geneticist of renown was so urgently needed in its reestablishment discloses that another malign force, besides Lysenko, was working in the political system to destroy genetics. This force was embodied in N. P. Dubinin, who successfully made his way to administrative power by scheming to undermine other geneticists, including the founders of genetics in the USSR. Slanderously he proclaimed them to represent bourgeois ideology. During Lysenko's rule, he was never strong enough to rival him in public, but as soon as *Lysenkovshchina* ceased to be an effective deterrent he returned to his insidious strategy. Timoféeff-Ressovsky became one of his victims.

In 1971 Timoféeff, at the age of 70 years, was forced to retire from his position. His laboratory at the Institute of Medical Radiology at Obninsk was broken up upon the direct order of the Party Committee of Obninsk. It was only after a noted physicist, Max Delbrück, a Nobel Prize winner in genetics and a collaborator with Timoféeff in notable genetics papers of the mid-1930s, came to lodge a protest with the Academy of Sciences of the USSR that Timoféeff was granted a position at the Medico-Biological Institute in Moscow. It was merely a desk position. No laboratory for him to head was granted. Timoféeff-Ressovsky died in 1981.

His true friends tried, after his death, to obtain from the Supreme Soviet a "rehabilitation" of his reputation and status. Instead, they were denounced, and Timoféeff was charged with having experimented with Soviet prisoners of war during his stay in Germany. The rehabilitation was denied.

Such was the background of the dramatic posthumous fate of Timoféeff-Ressovsky, a Russian geneticist who clearly ranks with Vavilov and Chetverikov as the greatest of them all. The Gorbachev era of *glasnost* and *perestroika*, a new era of de-Stalinization, seemed to be an appropriate time for the political authorities not only to pardon Timoféeff, but to go even farther, and find those persons who had persecuted him to be guilty of malevolent persecution. Nothing of the kind has occurred. In

1987, the monthly magazine *Novy Mir* ("New World") instead published in its first two issues a narrative written by Daniil Granin, and entitled *Aurochs* ("Bison"). It told the story of Timoféeff's life and fate. At first glance it seemed that the author was indeed daring to choose as a hero a man who was officially a *nevozvrashchenets*, a betrayer, whose criminal convictions had never been rescinded. That initial impression lured many readers into a false understanding, and Granin himself became something of a hero of the Gorbachev era. This I know personally from letters received from my former colleagues in Russia, as well as from three articles in the monthly magazine *Voprosi Istorii Estestvoznaniya i Tekhniki* ("Problems of the History of the Natural Sciences and Technology," Academy of Sciences of the USSR, 1987–1988.) These articles reported on a round table discussion devoted to "Certain Pages in the History of Soviet Genetics in Contemporary Literature."

Those who loved Timoféeff were thankful to Granin for his narrative, for it seemed to be at least a small step toward an official recognition of previously rejected values, such as one's freedom to live wherever one chooses to live, or wherever either lucky or tragic circumstances force one to stay, without being accused as a criminal; and above all else, the freedom to search for scientific truth without being subordinated to any political doctrine or ideology. Timoféeff-Ressovsky, in Granin's presentation, seemed to the majority of intellectuals to stand forth as a shining embodiment of these freedoms.

That was a deception. Seventy years of censorship, during which even a favorable mention of an officially condemned person was in itself a crime, prevented most of us from seeing the reality behind the facade. Not only had the State, by sentencing Timoféeff to undergo hard labor in a camp of correction, performed an act of legal justice but, according to Granin, Timoféeff had in fact pleaded guilty to the charges. Some of the participants at the round table expressed a view that Granin had made errors in presenting certain historical events, errors either affecting persons in contact with Timoféeff or in respect to Timoféeff himself, but they stopped with that criticism. In reality, the picture Granin presented was a reflection in a distorting mirror.

The big questions to be answered were the following: why was Timoféeff not liquidated by Hitler? And why, during the war, did Timoféeff retain his high standing at the Kaiser-Wilhelm Institute? Granin's answer is that he was a genius, and that was accepted as a fact by the Nazis. But Granin could not permit himself to give the right answer, even if he knew it.

The policies of Hitler and Stalin toward world-wide celebrities were identical. Had Granin told the truth, if it be assumed that he knew it, his narrative could have been interpreted as a barrage against the Soviet regime itself. Timoféeff was saved not because of his genius, for the degraded Nazi administration had often treated genius badly, or even with savage contempt. Rather, it was because of his world-wide fame. Thus he served Hitler's regime as a figurehead behind which it was thought the bloody reality might be concealed, so that world opinion might be cheated. The freer his own thought, the sharper his critical remarks, and the greater his own intolerance of deception, the more suited he was for the purpose of international deception.

The well-known Swedish geneticist Arne Müntzing, who in 1962 was visiting Leningrad as a member of some scientific delegation told me personally that he had been in Germany at a conference in 1936 or 1937, where Timoféeff was also present. The meeting was interrupted on account of a broadcast speech by Hitler. Everyone was supposed to stand and listen in silence. As everyone rose, amid the universal silence Timoféeff's voice thundered out:

> Wann wird denn dieser Wahnsinn endlich aufhören? [When will this madness finally cease?]

I have described this meaningful event in my memoirs (Berg, 1988, p. 304).

In the USSR, analogous persons to speak out included Ivan Pavlov, the poet Boris Pasternak, and V. I. Vernadsky. Another such person, the historian and academician E. V. Tarle, was saved by his friendship with Romain Rolland for, according to a rumor circulating among academicians, Rolland visited the Soviet Union and asked for a meeting with Tarle, who had been imprisoned. As a result, Tarle was freed.

In his book *Kettenreaktion: Das Drama der Atomphysiker*, Jost Herbig (1976, p. 54) wrote about the urgent request made by the Scientific Council of the German university where Heisenberg was teaching, to arrest him because of his open opposition to the racial policy of the Nazi regime. The request was denied by Alfred Rosenberg himself. That refusal was motivated by the world-wide recognition of Heisenberg's scientific merit. For Stalin as for Hitler, the major tool of deception was silence. Silence could then be broken to disclose that some obvious candidates for camps of correction were *not* imprisoned. That was good propaganda. Of course, to serve such a purpose, the persons would have to be of world-wide fame.

Those geneticists who expressed their gratitude to Granin at the round table discussion were ready to forgive him for the justification he made of Dubinin in his praise of Dubinin's book *Vechnoye Dvizhenie* ["Perpetual Motion"] (Dubinin, 1973, p. 351; 1975, p. 372). In that book, Dubinin accused Timoféeff not only, as the Soviet jurisdiction did, for staying on in Germany when ordered to return, but even for initially accepting the invitation to go to Germany. Timoféeff's crime in leaving the socialist motherland was never to be forgotten, never to be forgiven.

Granin has termed this diatribe which was directed by Dubinin against his own teachers, this apologia of Stalin's and Brezhnev's crimes against intellectuals, peasants, and citizens of Czechoslovakia, a "bald and honest recollection." In order to justify Dubinin's intrigues, Granin insinuated a slanderous accusation of racism at Koltsov. Not a single one of Koltsov's former students who were present at the round table raised a voice in Koltsov's defense. Granin knew exactly the narrow limits of freedom of speech, and the speakers at the round table knew them, too. Yet even Granin's extreme caution not to trespass upon those limits could save him from criticism. The most chauvinistic element of the press in the USSR, a chimerical scion of both adherence to communism and also to Great Russian Nationalism, attacked him. According to those journalists, a person who was an intellectual, who came from the nobility and was thus a class enemy, should not become a beloved, nor even a positive, personage in Soviet literature. From the orthodox Marxist-Leninist standpoint of group-accusation, the group affiliation of a person is a sufficient reason for persecuting him or her. Granin therefore, according to these journalists, ought to have exposed the criminal actions of Timoféeff beyond his mere refusal to return to the Soviet Union when ordered to do so. Those "patriots" condemned Granin for hiding the services Timoféeff-Ressovsky had rendered to the Nazi government. Let us hear their own voices.

Vladimir Bondarenko, in an article entitled *Ocherki literaturnikh nravov* ("Essays about literary dispositions") published in the magazine *Moskwa*, wrote as follows:

> I am interested in whether Timoféeff-Ressovsky collaborated, according to Granin, with the Nazis or not. It is known that in 1944 part of the physicists occupied in nuclear research were transferred under the leadership of the Bison. . . . Does that mean that if the atom bomb had been produced by the Germans and used against us, that would also have been meritorious on the part of the Bison? The narrative does not elucidate that question (Bondarenko, 1987, p. 190).

Bondarenko is entirely silent about the source of his information. His accusation that research on an atomic bomb was under Timoféeff's supervision is a final step in a chain of falsity.

In an essay *K kakomu khramu ishchem mi dorogu*? ("Toward what kind of cathedral are we searching for a road?"), A. Kuzmin attacked both Granin and Timoféeff-Ressovsky (Kuzmin, 1988). The article's title assumes that Kuzmin has knowledge both of what a perfect cathedral might be and also of what road must be taken to reach it. The metaphor of a cathedral was chosen to symbolize the ideal social order that was to be created by Gorbachev's "perestroika." According to Kuzmin, that social order, previously proclaimed by Lenin and realized by Stalin, is the building of socialism in one particular country. By Lenin, patriotism had been opposed to internationalism, which was equated with cosmopolitanism, and perceived as being a force hostile to the Russian people. The right way leading to the ideal cathedral then turned out to be a realization of the program chosen by the chauvinistic, anti-Semitic society known as "Memory." Kuzmin's socialism is thus opposed not only to internationalism and to Trotsky's idea of a world revolution,

but also to individualism. It followed that Granin, and his narration "Bison" (1987), became the targets of a severe attack.

Granin's guilt was not merely to justify an unpatriotic act. Nor was it simply to have written an apologia for a man who preferred to live in Nazi Germany when given a choice to leave. Granin's wrongdoing was to conceal Timoféeff's participation in Nazi crimes. Hitler's genocide of the Jews is not mentioned, but instead we hear that what is now taking place in the Gaza Strip embodies ". . . ideas of the slave-owning era [that] veil the genocide of the great Semitic Arab nation" (Kuzmin, 1988, p. 155). The postulated secret contract of "the Genetics Department of the Institute in Berlin-Buch with the War Ministry and with the Supreme Commissar over atom physics" is transposed from Bondarenko's pamphlet to Kuzmin's article (1988, p. 164). Research done in one of the laboratories under Timoféeff's supervision is depicted as part of Hitler's massacre of Hitler's political enemies, those whom he had declared to be biologically inferior. Kuzmin continued:

> Co-authors [Timoféeff, Born, and Zimmer] calmly narrate about experiments in humans, who were subjected to intravenous injections of thorium-X. The fascist Germany was for sure a sole country where experiments of this kind were not even veiled. Inferiors were not considered to be humans (1988, p. 164).

Through unforeseen circumstances, to be described hereafter, I became acquainted not only with these accusations but also with the publications used by the judges of Timoféeff-Ressovsky when bringing in a verdict of guilty against him. It is true that injections of thorium-X into human subjects were described by Wolf and Born (1941) and by Gerlach, Wolf, and Born (1942). Both of these publications are designated as coming from the Genetics Department of the Kaiser-Wilhelm Institute, Berlin-Buch, which was headed by N. W. Timoféeff-Ressovsky, as well as from the Radiology Department of the Auer-Society, Berlin, headed by P. M. Wolf. The work was actually done in the laboratory headed by Wolf. References given in these articles show that the use of radioactive substances as tracers to study blood circulation in animals and humans had already been begun in the 1920s by workers elsewhere.

Thorium-X was chosen by Wolf and Born (1941) particularly because of its short half-life, its low effective dose, and its low energy of decay, yet with radioactivity high enough to permit the signals to be picked up outside the body of the subject. Patients of the clinic served for a comparison of blood circulation in persons suffering from circulatory disturbances with those who had no circulatory problems. Thorium-X was injected in such small doses that it could not even be weighed (p. 342), but the dosage was estimated to be, per experiment, the equivalent of 0.03 mg. radium-equivalent (p. 346). The object of the experiments was simply to diagnose disturbances in the circulation of the blood.

The article by Gerlach, Wolf, and Born does indeed make reference to an article written jointly by Born, Timoféeff-Ressovsky, and Zimmer in 1941. It was entitled *Anwendung der Neutronen und der künstlich-radioaktiven Stoffe in Chemie und Biologie*. It was a short review article in the popular scientific magazine *Die Umschau*. This reference is the only indication that Timoféeff was in touch with Wolf and his coworkers.

The use of radioactive tracers in medicine and physiology was at that time worldwide, and not simply limited to Germany. Great caution was used to keep internal doses from such tracers far below any harmful level. From a standard reference work in the field, a *Handbook on Toxicity of Inorganic Compounds* (Seiler, Sigel, and Sigel, 1987), I learned the following:

> The availability of radium and X-rays since the beginning of this century led to the emergence of nuclear medicine and radiology, respectively. Both important medical fields make use of ionizing radiation for diagnosis or therapy. . . . Thorium as a contrasting agent was used because of its physical properties and despite its radioactivity. . . . Today pharmaceuticals play an important role in diagnosis, but the radiation from these activities in the general public is still only a fraction of the exposure caused by radiography (pp. 809–810).

In 1981, in the United States, I was myself injected for diagnostic purposes with a radioactive tracer and could observe on a television screen, together with the physician, the transport of the contrasting agent to the gall bladder and its removal from my body. Similar uses

of radioactive tracers are still in common worldwide use for diagnostic purposes.

Let us return to Kuzmin and his cathedral. Granin incurred Kuzmin's anger because Granin did not present properly the real hero, the knight of the ideological fight against the bourgeois intelligentsia, namely, Dubinin. Granin's flirting with Dubinin, his praise of Dubinin's memoirs, Kuzmin disregarded.

A hostile attack against Timoféeff-Ressovsky by non-Russian scientists I myself witnessed at the 16th International Congress of Genetics, held in Toronto, Canada, in 1988. It was at one of these that Peter Weingart, of the University of Bielefeld in West Germany, when speaking of the renunciation of humanitarian considerations by German race hygienists, asserted that mandatory sterilization and the later holocaust were "relatively well founded on the genetic and medical knowledge of the time" (Weingart, 1989, p. 897). In his abstract, no names were given of the persons who had created this "genetic and medical knowledge"; but in his verbal presentation Weingart did mention one name, and one only. It was that of Timoféeff-Ressovsky. I asked Weingart what he could mean by thus accusing Timoféeff. The answer I received was that Timoféeff had supported the Nazi racist theory.

In the two-page abstract of Peter Weingart's publication (1989), "Politics of heredity—Germany 1900–1940: A brief overview," not a single name was supplied of any person implicated in providing the basis of genetic and medical knowledge on which the Nazi racist ideology was founded. In speaking about the law for "protecting German blood and honor" by prohibiting marriages between Germans and Jews, Weingart wrote (p. 897):

> The law is now branded as a purely ideological anti-semitic measure, but historians have overlooked the fact that this law, at the time, reflected widespread concerns over the effects of race mixture that were not limited to German race hygienists.

He implicated Timoféeff-Ressovsky, without mentioning him by name, in the following sentence (p. 897):

> The major concern of the state . . . was the cleansing of the hereditary stock from so-called asocial elements. This concern was taken up even by modern geneticists to the extent that they propagated and undertook research in the identification of heterozygote carriers of recessive hereditary features.

Here the history of population genetics is quite falsified. The identification of heterozygote carriers of recessive mutations causing disease or disorder was already undertaken in Moscow in 1926 by the group of researchers under the guidance of S. S. Chetverikov and, during the last two decades, it has become a major enterprise of human genetics all over the world. It had nothing to do, then or now, with cleansing of the [human] hereditary stock. Timoféeff, as a follower of Chetverikov, started his population studies on *Drosophila* as soon as he arrived in Berlin-Buch.

I digress to examine the article by Weingart (1987), of which the abstract of the talk at the Genetics Congress purported to be a brief summation. That claim is in error, for the longer article itself is an analysis of relationships between science and politics long before the Nazis came to power. In his 1987 article, Weingart pointed to the existence of a conflict between eugenics, a precursor of modern human genetics, on the one hand, and race theory, on the other. To buttress that view, he quoted from a paper given by Raymond Pearl, a well-known American geneticist [and incidentally the founder and first editor of *The Quarterly Review of Biology*], a paper given at the Fourth International Congress of Genetics held in Berlin in 1927. The propaganda of the eugenicists, wrote Pearl,

> their public teachings, their legislative enactments, and their moral fervor are plainly based upon a pre-Mendelian genetics, as outworn and useless as the rind of a yesterday melon. . . . The literature of eugenics has largely become a mingled mess of illgrounded and uncritical sociology, economics, anthropology, and politics, full of emotional appeals to class and race prejudices, solemnly put forth as science (quoted from Weingart, 1987, p. 186).

Weingart also quoted the words of H. J. Muller from a review he wrote of the Baur-Fischer-Lenz textbook of human heredity, *Menschliche Erblichkeitslehre und Rassenhygiene* (published in Munich, 1932, 4th edition). Regarding the second part of the treatise, a part written by Fischer and Lenz, Muller commented:

> As they stray further and further from the fields in which rigorous genetic investigations have been conducted . . . Fischer and Lenz become less and less scientific, and we soon find them acting as mouthpieces for the crassest kind of popular prejudice (quoted from Weingart, 1987, p. 189).

From Weingart's article we further learn that Lenz regarded the real creator of the Nazi race theory to be Hitler himself. His words were (pp. 188–189):

> He [i.e., Fritz Lenz] wrote a lengthy review of Hitler's *Mein Kampf* in which he took the author's race-hygienic and racist theses completely seriously.

Weingart then tried to explain "the growing influence of race theories in Germany at a time when they were already drawn into question elsewhere" (p. 190) as follows:

> . . . [U]nder the influence of the party and its ideology the character of race-hygiene increasingly escaped the control of the scientific representatives, who steadily lost their influence on the government's population and eugenic policy (p. 191).

It was the Nazis themselves who were guilty; there was no need to accuse the guiltless. Timoféeff-Ressovsky was not mentioned. That was so different from what was said and implied at the Congress two years later, that I decided to write to Professor Weingart to call his attention to this contradiction. I wrote that I would be grateful if he would write me that his words at the Congress were not just and if he would permit me to refer to his letter in my articles. In a letter of August 23, 1990, Professor Weingart wrote that his words were misinterpreted: "My claim is that even a scientific genetics cannot prevent political abuse. . . . I did not . . . want to attack Timoféeff-Ressovsky for being racist nor do I have sufficient knowledge of his work and his personality to be able to do so. . . . I hope that this . . . clarifies what was an obvious misunderstanding and is sufficiently clear to serve your purpose for quoting it in other contexts."

At the Congress, following Weingart's remarks, Benno Müller-Hill caused a further sensation. From the podium he proclaimed that he could document Timoféeff's involvement in racist actions. He then reported that at Timoféeff's institute in Berlin-Buch a meeting had taken place, at which Alfred Rosenberg, the notorious Nazi leader of racist policies, made a speech. Müller-Hill's statement aroused an ardent controversy. It erupted during the session and continued during the breaks, and became more and more violent. Müller-Hill's accusations became more and more vigorous. Every geneticist from the Soviet Union who was present and who knew Timoféeff tried to persuade Müller-Hill that he was wrong, but he kept firmly to his accusations. Because of the implacable nature of our controversy, I was therefore surprised when, after returning to St. Louis, I got a letter from Müller-Hill containing his presumed documentation of Timoféeff's guilt. In our ensuing correspondence, I became a possessor of all the materials that Müller-Hill had scraped together in order to unmask Timoféeff as an active collaborator with the Nazis and, in particular, a supporter of their racial and eugenic policies.

Let me state at this point that I knew Timoféeff personally. On several occasions I had visited seminars at his biological experiment station in the Urals. Some of my own publications have dealt with Timoféeff-Ressovsky's presentations (Berg, 1957, 1958). I was also a coauthor of the first article written by Timoféeff after his release from imprisonment (Berg and Timofeev-Resovsky, 1961). In 1972, when he presided at a symposium on population genetics at the Second All-Union Congress of Genetics and had chosen its speakers, I had the luck to be invited. I also knew his published work while he was in Germany, both before and during the time of Hitler. In 1984 I spent a half year in the city of Mainz, in West Germany. There I immersed myself in the biological publications of the "thousand years" of the Nazis and had access to journals inaccessible in the Soviet Union. Indeed, I found monstrous manifestations of the moral degradation of the scientific community, but Timoféeff proved to be irreproachable.

Timoféeff himself told me how he had used the enormous, sluggish machinery of the Nazi administration to save from dismissal Jews employed in the Kaiser-Wilhelm Institute. I also knew from him directly his own disposition toward Lysenko and the destructive activities in

science, agriculture, medicine, and education of Lysenko and his followers and patrons. The *Lysenkovshchina* that began under Stalin had continued under Khrushchev to be a part of the official ideology. From *one* dictatorship over all branches of science, Timoféeff had fallen into *another* one. Discreetly he avoided every hint of support for political and scientific dictatorship, although he never openly attacked the powers under which he lived. Instead of arguing against quackery, he opposed it by himself following a relentless search for scientific truth, and by spreading scientfic knowledge to as many people as possible. From his subsequent behavior, after the return to Russia, it was simple to extrapolate what his behavior under the Nazis must have been. I was myself absolutely sure that Timoféeff did not collaborate with Hitler or his minions. Now, thanks to Müller-Hill, I had acquired documentary proofs that Timoféeff had not been involved in Nazi criminal deeds or in support of Nazi ideology.

The documents that were alleged to compromise Timoféeff-Ressovsky showed, in the first place, that the meeting Müller-Hill had mentioned at the International Genetics Congress in 1958 was actually held, not at the Kaiser-Wilhelm Institut für Hirnforschung, but at some educational center for local Nazi leaders. The periodical *Neues Volk*, in reporting the meeting, reproduced the speech of Alfred Rosenberg, truly a cynical attempt to falsify facts in order to advocate racism. It also provided a photograph that showed Timoféeff surrounded by the Nazi leaders attending the meeting. The caption under the photograph reads: "Members of special classes visit the Institute for Brain Research in Buch. Dr. Timoféeff (center) is one of the most noted geneticists." This piece of propaganda was clearly aimed to legitimate the nature of Nazi race policy, as being rooted in science and accepted by a world scientific elite. Obvious is also that mere attendance at a meeting of this sort, by the Vice Director of a Kaiser-Wilhelm Institute, could in no way have been avoided. It does not imply any harmony between Timoféeff and the views expressed by such persons as Rosenberg.

In order to provide evidence of Timoféeff-Ressovsky's support of fascist eugenics, Müller-Hill also enclosed an article published by Timoféeff in a magazine of medical genetics. The article was entitled "Experimentelle Untersuchungen der erblichen Belastung von Populationen" [Experimental studies of the hereditary load in populations] (Timoféeff-Ressovsky, 1935). Timoféeff first described how recessive mutations of a deleterious nature are hidden under the protection of their normal alleles in populations of certain species of flies and beetles. This was common knowledge among the geneticists of the 1930s. Timoféeff then wrote:

> It would be of paramount importance for human genetics as well as for race hygiene [the term Germans preferred to use in place of "eugenics" (R.B.)] not only to estimate the percentage of people affected with a particular hereditary disease, but also to find out the geographical distribution and the allelic frequencies of hidden mutant genes. This would not only facilitate race-hygienic control, but would also be helpful in avoiding certain difficulties in the classification of hereditary diseases. It is known that different mutations produce similar phenotypes and that there are genes which manifest themselves differently in combinations with other genes, so modifying considerably their phenotypic expression (p. 118).

It must first be said that there is not a single idea expressed in this paragraph which not only every informed geneticist of the 1930s would have unhesitatingly endorsed, but that the same holds true for informed opinion today. Differences of opinion arise only when geneticists consider what should be done to mitigate the genetic load. In the early 1930s many eugenists advocated such measures as compulsory sterilization of persons affected by extremely harmful genetic diseases and disorders, and even sterilization of the carriers of such recessive genes. Others advocated internment of such persons in state institutions. Still others felt that voluntary measures and genetic counseling would be adequate. It was only after the mid-1930s, when the harsh excesses of Nazi eugenic policy became widely known, and when geneticists such as J. B. S. Haldane showed the ineffectiveness of sterilization in lowering the genetic load, that genetical opinion swung radically away from compulsory eugenic measures, and the improvement of medical treatment and obliteration of environmental deficiencies seemed much better. In preparing an article for a journal of medical genetics, Timoféeff of

course used the term "race hygiene" in the same sense given it by such early German founders of eugenics as Wilhelm Schallmayer (see Sheila Faith Weiss, 1987) at the beginning of the 20th Century, and also as it was used by the founders of eugenics in the Soviet Union, Koltsov and Filipchenko. The two Russian founders of eugenics declared that the principal components of eugenics were (1) medical genetics, including medico-genetic consultation, aimed to estimate the probability of birth of affected offspring and to cure hereditary diseases on the basis of the correct diagnostic measures; and (2) voluntary refraining from reproduction in the case of a high probability of having an affected child, a restraint, or self-denial, to be based on a sufficient knowledge of human genetics.

Timoféeff's attitude toward the "purification" of the "Aryan Race" was exactly the same as the outlook of H. J. Muller, as expressed at the same time in Muller's book, *Out of the Night*, which was published in 1935 while Muller was in Russia, but was written many years earlier. Muller, in combatting the idea, then popular in the United States among some biologists, that forced sterilization was an effective way of lessening hereditary feeblemindedness (or other components of the genetic load), wrote as follows:

> As regards the eradication of feeble-mindedness, many of the so-called eugenists are laboring under a misconception, for (as Haldane has pointed out) sterilization of all the feebleminded would by no means prevent the reappearance of this trait in the next generation. . . . Important in this connection is the fact that a large proportion of the hereditary defects are probably inheritable in some concealed form . . . (pp. 78, 80).

Muller did insist that the cure of hereditary ailments is an urgent necessity and he stressed the need for the right diagnostics:

> Modern genetics shows that in many cases such ailments occurring in different families, may be so alike in symptoms as to be indistinguishable from one another, yet have a fundamentally different hereditary cause; and each causally different innate weakness may present its own special problems of treatment . . . . Such a problem . . . cannot be attacked with the greatest efficiency by the medical man before the ailment has been classified in relation to what the individual's heredity contained. The recent introduction of this point of view in pathology, neurology, etc., which we owe largely to Dr. Levit in Russia and to Dr. Vogt in Germany, vastly increases the work to be done in these subjects, and the need in them for a thorough understanding of genetics (pp. 70-71).

Oscar Vogt, whom Muller mentioned, was at that time and until 1938, when he retired, the Director of the Kaiser-Wilhelm Institut für Hirnforschung, where Timoféeff had studied the genetic load in *Drosophila* populations. S. Levit was at that time, and until 1936, the Director of the Institute for Medical Genetics in Moscow. In 1936 he was arrested and never heard of again. His was the sad fate that would have awaited Timoféeff, had he not been forewarned by Koltsov and Vavilov.

Muller, of course, was freer to express his views than Timoféeff was in Nazi Germany. Today it is routine practice in medical consultation to take into account the genetical data that Muller and Timoféeff insisted should be collected and used in genetic counseling.

Among the supposedly compromising materials Müller-Hill sent me were the two articles by Wolf and his coauthors dealing with the injections of thorium-X into humans (Wolf and Born, 1941; Gerlach, Wolf, and Born, 1942). Müller-Hill commented to me, in a letter of January 5, 1989, that to publish works of that kind and to participate in experiments on humans was "on the part of Timoféeff-Ressovsky 'abscheulich' [horrible; dreadful]," and he continued, "An expert [putative, R.B.] has written me that he could extrapolate from the data that the experiments were lethal." Such a misunderstanding of conventional tracer experiments used worldwide indicates either an abysmal lack of understanding or a willful distortion of the truth intended to mislead the ignorant.

Another allegedly "horrifying" crime committed by Timoféeff was connected with a paper written by S. R. Zarapkin, a coworker of Timoféeff in the KWI für Hirnforschung, an article entitled "Über die Variation der Kopfform bei einigen Menschengruppen" [On the variation of head-form in certain human groups] (Zarapkin, 1943). Four groups were compared with respect to head form: Jews, Eskimos, English, and Sicilians. Some differences

were found, and were reported without the slightest implication of any racist theory. It was demonstrated that although the head form of Jews changes with age, it was not affected by environmental conditions. No fresh measurements were made by Zarapkin, but Franz Boas's measurements were simply used to check Boas's own Lamarckian conclusions, and the author held that Boas's conception of the inheritance of acquired traits was disproved. Müller-Hill's comment, however, was how dreadful it was that Timoféeff's purpose [sic] was to show by means of this study that a Jew remains a Jew wherever he lives. That conclusion, so compatible with Nazi ideology, is a prime example of a dual fallacy of reasoning: first, to attribute to a particular person a point of view that might be held by an associate; and second, to extrapolate from a particular characteristic (in this case, head form) to a general all-embracing "racial" complex, social as well as biological.

Recent mail has brought me Müller-Hill's review of Granin's *Bison* (Müller-Hill, 1988a). Ironically, one can note a striking resemblance between the shameless propaganda of Kuzmin, adherent of the anti-Semitic group "Memory" and also a Stalinist, and what Müller-Hill has written. According to each of them, Granin has falsified Timoféeff's biography by concealing his crimes. Müller-Hill, to be sure, does not depict Timoféeff as the manager of the Nazi's atomic bomb project, but in other respects he shares Kuzmin's ardent wish to disparage Granin's target by making the same accusations. Müller-Hill has definitely tried, in this review, to persuade the reader that Timoféeff was devoted to Nazism. He has even stated that in 1933, when the Nazis came to power, "Muller fled to the Soviet Union, Timoféeff-Ressovsky stayed in Germany." The fact, according to the personal knowledge of Bentley Glass, who was with Timoféeff and Muller in Berlin-Buch in 1933, is that H. J. Muller came to Europe with the firm intention of going to the USSR, together with his wife and Carlos Offerman, a South American who was studying genetics under Muller, and that they merely stopped for a temporary visit to Timoféeff before proceeding onward.

Muller had long planned to go back to Russia for a second visit, for his first, in the 1920s, had been highly rewarding to him. He was indeed deceived by the Communist propaganda. Bentley Glass vividly recalls more than one somewhat heated argument he had during their joint stay in Berlin-Buch, Glass maintaining that there was nothing to choose between one kind of fascist dictatorship and another. Both Soviet and Nazi authorities were determined to dominate science completely, and to distort it to further their own political ideologies. Muller claimed there was a vast difference in that respect between Soviet Communism and Nazi ideology and policy. His first visit to the USSR had convinced him that in Soviet Russia science was free and was warmly defended in its conclusions (see Glass, Foreword). Muller at this time was elected an Associate Member of the Academy of Sciences of the USSR, and was invited to head the Department of General Genetics of the Institute of Genetics of the Academy of Sciences. The invitation came from the Director of the Institute, Nikolai Vavilov, who was a friend of Muller. It was not until four years later, in 1937, that Vavilov was to recommend to Timoféeff that he should stay in Germany, and at that same time asked Muller, for the sake of his own safety, to leave the Soviet Union; and it was seven years later, in 1940, that Vavilov was himself arrested, and in 1943 died while in prison.

Timoféeff's article of 1935 on the genetic load in populations of fruit flies and beetles was described by Müller-Hill as a demonstration of racism, as a philosophical acceptance of the propriety of death sentences for persons judged to be inferior. The actual language was as follows:

> In 1935 he [Timoféeff-Ressovsky] published an article on the mutational load in *Drosophila*, in which he commented that such a type of analysis would help greatly the "control" of human populations in race hygiene (*Der Erbarzt*, No. 8, pp. 117–118) (Müller-Hill, 1988a, p. 722).

In the same review, Müller-Hill once again mentioned the symposium, organized by the race headquarters of the Nazi Party on the problems of the *Weltanschauung* [world view], the meeting that he had mistakenly declared at the International Congress of Genetics in 1988 to have taken place at the Kaiser-Wilhelm Institute for Brain Research in Berlin-Buch. In this review, however, Müller-Hill merely concluded that Timoféeff, as a "member of the

symposium . . . in reality did nothing" (p. 722). He was, nevertheless, held responsible for the public impression created by "his picture among all the brown shirts." To strengthen this veiled charge, Müller-Hill informed his readers that Timoféeff "invited the participants to his institute" (p. 722). This grave charge, which could scarcely have been needed by party functionaries who went wherever they pleased, was not documented in the review, nor did Müller-Hill mention it in correspondence with me.

In the review, there was also a passage about the injections of thorium into human subjects.

> During the war Timoféeff-Ressovsky found an even better rationale for his research: Heisenberg's atom machine. He [i.e., Timoféeff] was certainly the best-qualified radiation expert in Germany at the time, and so he extended his research in this direction. His collaborators, Gerlach, Born, and Zimmer looked at the turnover of thorium-X (radium-222, an alpha emitter with a half-life of two days) in human beings (*Arch. f. exp. Pathologie*, 199: 83–88, 1942). The authors of this paper do not mention who were the individuals into whom they injected the thorium-X, nor do they say how large the dosage was. I take here their word that it was harmless (p. 722).

First, there is an error in the citation of the paper referred to by Müller-Hill. The publication in the *Arch. f. exp. Pathologie* is under the authorship of Gerlach, Wolf, and Born, not by Gerlach, Born, and Zimmer. Having access to the paper to which Müller-Hill refers, I found in it all of those data that Müller-Hill asserts are missing (see p. 24).

Both Kuzmin and Müller-Hill have contrasted the villain Timoféeff-Ressovsky with the hero Dubinin. According to Kuzmin, Dubinin needs to be protected against the slanders of Granin. Müller-Hill stated that this protection was needed against the State. Actually, Müller-Hill's sentence that mentioned Timoféeff and Dubinin together is most confusing:

> The charlatan Lysenko had been against eugenics. His former enemy, the geneticist Dubinin, was against it, too. But Timoféeff-Ressovsky was for it. So now when Dubinin had turned into a cantankerous old antisemite, does this not indicate that eugenics was and is unquestionably a good thing? (Müller-Hill, 1988, pp. 721–722).

This puzzling comment seems to mean the following: Granin used the official permission of pardon for Timoféeff-Ressovsky to imply a sanction by the state of Timoféeff's racist, fascist eugenical views. For in a state where everything is sanctioned by the authorities, the mere permission to publish a book about Timoféeff must signify that the state has accepted the views of the rehabilitated person and has decided to put those views into practice. How marvelous that line of reasoning, which ignores the fact that Timoféeff was never actually rehabilitated, and which furthermore completely misunderstands the significance of *glasnost*. Behold! Timoféeff-Ressovsky was an adherent of bloody Nazi eugenics. Dubinin, the enemy of eugenics, is slanderously exposed to public dishonor, and silenced. Granin's book heralds the onset of a new era of fascism.

In Müller-Hill's review, to continue, he described the imprisonment and release of Timoféeff in the following way:

> Timoféeff-Ressovsky was transferred to a secret laboratory . . . and the work went on. . . . He now had to do secret research. . . . Apparently nothing was published during these years. Finally when the problems were solved, Timoféeff-Ressovsky became director of a small biological station in the Ural Mountains (Müller-Hill, 1988a, p. 721).

What a distorted picture of Timoféeff's experiments on the radiostimulation of plants, which were published in several journals! of his unqualified release owing to Khrushchev's de-Stalinization policy! and of his founding of a biophysics department in Sverdlovsk at the Biological Institute of the Academy of Sciences. Müller-Hill's aim seems to be to represent Timoféeff as not only a participant in Hitler's holocaust, but also an attendant of Stalin's war machine.

To grasp the aim of Müller-Hill to ascribe to Timoféeff a conduct so alien to his nature, I had finally to read Müller-Hill's book, *Tödliche Wissenschaft: Die Aussonderung von Juden, Zigeunern, und Geisteskranken, 1933-1945* (Müller-Hill, 1984; Eng. trans., 1988b). This little book is a useful reminder of Germany's tragedy of 1933 to 1945, and aims to prevent a relapse. The author undertook an enormous labor in order to reconstruct the history of Nazi crimes against humanity.

According to Müller-Hill, the guilty persons include all the inhabitants of Germany, except for those who themselves perished in hospitals, in prisons, internment camps and ghettos, or in "scientific institutes" where they were being used as laboratory animals for experiments. The criminals in Germany numbered not only all scientists, especially the anthropologists and geneticists, but also all physicians, and especially psychiatrists. Science itself is guilty, not just the persons who applied scientific data in criminal ways, but science as such. The accusation against science starts with the very title of the book. It is science that is murderous. Müller-Hill wrote:

> It seems to me that the intrusion of science into the field of the human being, endowed with speech and a means of signalizing, the intrusion that started in the 18th century, was a fundamental error. . . . Because man observed in this way turns out to be reduced to an object or to an animal doomed to subservience (Müller-Hill, 1984, p. 100).
>
> [Note: this passage and all the following quotations have been translated by the author from the original book in German (R.B.).]

If one takes Müller-Hill's expression of compunction literally, no medical care would ever be acceptable, since it always represents an "intrusion" upon the body of the patient! The reasonable ethical issue is to consider precisely where to draw a line between warrantable intrusion on the body of a human and unwarrantable intrusion.

Müller-Hill went on to express his view of the guilt of the Germans as follows (Müller-Hill, 1984):

> Hitler came to power, because he threw it open for German citizens to use biologically well-grounded science for their dreams to become murderers (p. 94). More important than a "General Plan" [i.e., an order from a superior authority (R.B.)] was the will to wipe out the Jews, Gypsies and mentally ill persons. . . . The "will," liberated by Hitler, was the desire of the "hidden and undisguised murder-lovers to kill" (p. 96).

Anthropologists and psychiatrists Müller-Hill held to be guilty because they created "the ideology, or more precisely, the religion of fascismus. Many were at that time of the opinion that this religion is needed to save the motherland and capitalismus" (ibid., p. 94). Some of them repeatedly and deliberately "provoked the death of their patients with whom they experimented. . . . The controlling scientists considered certain categories of humans as some kind of experimental animals" (ibid., p. 98). Müller-Hill concluded with a question, "whether there were anthropologists or psychiatrists in Germany who did not match the delivered image." His own answer was the following: "As to anthropologists and specialists in human genetics, I affirm my right to say that there was not a single person who would differ considerably from those whom I have mentioned" (ibid., p. 100) and he continued, "I have named only the most active ones, so as not to be accused of dealing with insignificant persons" (ibid., pp. 100–101).

To hold that a person is guilty because of group affiliation is a doctrine of every revolution, of each forcible reconstruction of society. Proceeding in his review of Granin's book to reaffirm this depraved idea, Müller-Hill implicitly accused Timoféeff, for the "Bison" was lucky enough to be among the "insignificant" persons whom Müller-Hill did not mention in his book. Müller-Hill's group accusation of all Germans as harboring murderous intentions naturally evoked a protest from those Germans who had attended his public address. He has described those attacks on him in an article entitled "Genetik nach Auschwitz" [Genetics after Auschwitz] prepared for inclusion in a volume called *Das Weltbild der Biologie* (I quote two examples of these attacks upon him [translated from the German manuscript (Müller-Hill, 1982, p. 29)]:

> You were not there. To speak about this history is possible only for those who witnessed it.
>
> You despise your audience. Even your clothes show how you despise us.

The few persons present who sympathized with the speaker dared to demonstrate their approval of his words only after the lights had been turned off in the auditorium. Müller-Hill conveniently ascribed the hostile demonstrations not to the indiscrimate nature of his accusations, but as a sign of a coming relapse into Nazism.

One final misrepresentation of what was go-

ing on in Nazi Germany prevented Müller-Hill from making a correct explanation of the freedom accorded to Timoféeff-Ressovsky under Hitler. To aggravate the guilt of the Germans and to justify his sweeping condemnations, Müller-Hill denied that the bloody business conducted in the Third Reich was done secretly. He wrote (Müller-Hill, 1984, p. 94):

> Everyone knows that Jews and mentally ill persons are being killed, but nobody dares to tell it. The essence of German fascism, annihilation, was an open secret and had to remain an open secret. . . . Like the name of God, so was the name of annihilation not to be pronounced. . . . Hitler gave to German citizens an opportunity to carry out their desire to murder, by being able to tell that they were forced to do it [or] did not know anything. He transformed them back to children, who forget and [then] tell that they did not know anything [about it].

Müller-Hill's charge that everyone knew that Jews and mentally ill persons were killed, simply does not correspond with the facts. During my own stay in Germany in 1984, many German acquaintances told me that they knew about the dismissals and deportation of Jewish people but not about the annihilation. A blockade of information existed within Nazi Germany; outside the country, misinformation prevailed. The American wife of one famous geneticist, Curt Stern, who was one of those who supplied the most convincing proofs of the chromosome theory of heredity, told me how she persuaded her husband not to return to Germany after the International Genetics Congress of 1932, which Stern had attended in America. His life would indeed not have been endangered had he gone back to Germany at that time. Other distinguished German Jewish geneticists, such as Richard Goldschmidt, were not forced out of their positions and exiled until the mid-1930s. Reliable information about the persecution of the Jews, which began in 1933, was so scarce and so contradictory that Stern actually decided at one time to return. Evelyn Stern had to go to Germany herself to learn that Jews were being dismissed from all jobs and professional positions. The professorship offered to Curt Stern in München would clearly not have been his for very long, so he decided not to return at all, after the term of a fellowship awarded to him by the International Education Board had expired.

It goes without saying that Müller-Hill did not mention the deceitful Nazi propaganda that depicted before the public opinion of the world a favorable image of a free and generous country. Timoféeff-Ressovsky's world fame and reputation for defending the freedom of scientific thought are not consonant with Müller-Hill's accusations. Had Müller-Hill drawn a correct conclusion from his unsuccessful search for evidence of Timoféeff's services to Nazi ideology and racial policy, he would not have accused Granin of making a hero out of a Nazi collaborator. Instead, he would have countered Granin's arguments by disclosing the criminal actions of the Soviet State against a man who had defended scientific integrity at the risk of his life, in the totalitarian hells of both Hitler and Stalin. For any person who knew Timoféeff-Ressovsky personally, his devotion to the freedom of science is no mystery. His world fame, combined with his refusal to take rewards from the government, provided him independence, both under Hitler and afterwards in the Soviet Union. In Nazi Germany a world reputation was his shield; in Russia, even more important was his noble asceticism.

In my own life, I have known several persons of that kind: Vernadsky, Astaurov, Lyubishchev, Dmitri Filatov, Ukhtomsky, Rapoport, Efroimson, and my father, Lev Berg. However paradoxical it may seem, their independence was rooted in the destitute condition of Russia in their time. The scarcity of resources, held at the disposal of the ruling authorities, and the prerogatives of the same powers to bestow positions and to grant promotions, as well as to give permissions to go abroad, served to stratify society and used the prevalent misery as one of the means to govern. Privileges become hooks to fish for those who might prefer a replete slavery to a hungry freedom. Are those who might prefer a hungry freedom not unrulable?

It was my original intention to conclude my defense of Timoféeff-Ressovsky at this point, but the march of events has been rapid and inexorable. The debate continues over whether Timoféeff in fact deserved his ten-year sentence to camps of correction, or whether the sentence was simply one of Stalin's countless crimes against the population of the Soviet Union.

Those who believe the former alternative to be true are aided by the new German accusers. Müller-Hill and Karl Heinz Roth are proving themselves to be of great advantage to the most reactionary elements of Russian society. It seems that the debate divides its participants cleanly into those who desire a liberalization of society in the USSR, on the one hand, and on the other those who oppose the recent liberalization and who justify the bloody deeds of Stalin's "oprichnina." In the second category we find the two authors, D. Ilyin and V. Provorotov, of an article entitled "Ktovy, Doktor Timofeev-Resovsky?" that was published in the magazine *Nash Sovremennik* ["Our Contemporary"] last year (Ilyin and Provorotov, 1989).

Although D. Ilyin is not identified for the reader, Provorotov is listed as being the major-general of justice, senior assistant of the chief military prosecutor, and Honored Lawyer of the R.S.F.S.R. According to the opinion of these two writers, the recent attempts to prove the innocence of Timoféeff-Ressovsky, an obvious criminal, take their root in the "new thinking," in "democracy," and in "pluralism." All of these terms, it must be emphasized, have been brought into current use by Gorbachev. To show their disgust at such innovations, Ilyin and Provorotov always enclose the opprobious words in inverted commas, to make quite sure the reader will discern to whom they are to be referred. These words, according to the writers of the article, are now used to "revise and to pervert in accord with self-seeking interests the sacred, unquestionable principles, traditions, legends [predania]. . . . The 'socialistic market' is not yet created, but the morals are going to become marketable goods" (Ilyin and Provorotov, 1989, p. 173).

We learn from the article that in 1987 the son of Timoféeff-Ressovsky, Andrey Nikolayevitch Timofeev, applied to the Supreme Court of the USSR for an official "rehabilitation" of his father. That appeal was supported by several members of the USSR Academy of Sciences, by several associate members, and by various professors and cultural workers, to the total number of twelve.

As a consequence, the Supreme Military Procurator's Office instituted new proceedings in the case. The report of this reinvestigation exhibits incredible tendentiousness and ignorance. The new inquiry then confirmed the conclusion of the previous court-martial that Timoféeff was a war criminal. The evidence given of participation in the war against the Soviet Union was, in particular, that his genetic studies of the influence of X-rays on humans were pointed toward the use of X-rays to destroy Soviet troops. Rehabilitation of Timoféeff-Ressovsky was consequently denied.

Ilyin and Provorotov have not limited themselves to describing this new juridical farce. The article by Müller-Hill (1988a) published in *Nature* has been used as valuable evidence of Timoféeff's guilt. A letter written by Professor G. Sereda to the editor of the magazine *Nash Sovremennik*, and devoted to Müller-Hill's accusations, is mentioned with great sympathy. Sereda repeats the lie that Timoféeff invited the participants of the symposium on the Nazi *Weltanschauung* to his institute. As quoted by Ilyin and Provorotov, Sereda's final words were as follows:

> To the description of Müller-Hill . . . I can add that half a month after the meeting (November 10, 1938) the Nazis organized an enormous Jewish pogrom called "the imperial crystal night." These were the results of the racist "Symposium" in practice.

Thus, according to Sereda, Timoféeff-Ressovsky is among those who bear the responsibility for the pogrom.

The thorium-X experiments on humans, again interpreted by Ilyin and Provorotov in a highly ignorant way, are among the further accusations directed at Timoféeff-Ressovsky. For this purpose, Ilyin and Provorotov reintroduced Professor Sereda, as a specialist in radiochemistry. This specialist then stated that "the international sanitary regulations prohibit injections of any dosages of radioactive substances." What utter nonsense! According to Sereda, the dosages used in the experiments of Timoféeff's coworkers exceeded the lethal dose by 14 to 20 times. Thus was proven the murderous nature of the experiments.

I possess copies of the articles cited by Sereda. Tests of thorium content in tissues of the reportedly injected organisms were carried out by the authors to reveal the differential accumulation of radioactive substances in different tissues. The experimental animals were of course sacrificed to make these tests. These were rats. But according to Sereda, experiments were

done on humans, execution being routine. Sereda must assume, obviously, that not a single person in the Soviet Union has read these articles, the falsified account of which is then used by Ilyin and Provorotov as a damning documentation of their charges. They concluded the thorium-X topic gloatingly by stating that among Timoféeff's adherents no one has commented on Sereda's publication, and then ask, "Does this silence not give consent?"

The other source used by Ilyin and Provorotov to "expose" Timoféeff-Ressovsky was the publication by Karl Heinz Roth (1986), previously referred to by Bentley Glass (1989) and by myself, in the earlier part of this paper. One should not overlook the subtitle of the book in which Roth's article was a major contribution. It was "The Actuality and Continuation of Nazi Population Genetics."

First permit me to mention a good turn that Roth actually did to Timoféeff's reputation. He has reported that in Timoféeff's institute the salute "Heil Hitler" was prohibited by him, on pain of punishment (Roth, ibid., p. 45), and further that "Timoféeff was in 1943 denounced because of his 'defeatist' expressions about the war potential of the Soviet Union. The executive agent of the Kaiser Wilhelm Society (KWG) remedied the situation." The latter action refers to a letter of general administration of the KWG addressed to the head of the State Ministry of Education (Roth, p. 61, footnote 75). [The quotation marks are Roth's.]

Karl Heinz Roth is by no means an adherent of the Nazi measures aimed to protect the "Nordic race" from the progressive accumulation of deleterious hereditary traits. The very title of his article, "Schöner neuer Mensch," [The beautiful new man] reveals his sarcasm. Roth has denominated the Nazi ideology, with its chief ingredient of racism, as a *Wahnsystem* [a mad system]. His goal, instead, was to find what persons were guilty in laying its theoretical foundations and thereby of justifying it.

Roth further stated that Müller-Hill missed the truth in this matter because he disregarded the results of investigations in population genetics.

> Müller-Hill has therefore drawn a conclusion that the Nazi scientists were possessed by an irrational Baal cult of annihilation. . . . I disagree with Müller-Hill at this point. The annihilation policy of Nazi anthropologists, human geneticists, and psychiatric specialists in hereditary mental disorders had at its disposal plenty of rational and consistent motives. . . . The rationality of the annihilation thought, thanks to recent change in biogenetic thinking, roots in the universal panic about the alarming deterioration of the human genepool. The catastrophic nature of the deterioration resulted in visualized and theoretically justified extreme changes [*Radikalisierungen*] in dealing with humans. There is an immanent logic in the conclusion that every step toward the application of biology and genetics to alter humanity implies annihilation (*Vernichtung*). The route towards utopia, combining harmoniously creative and murderous trends, was paved in the thirties and forties by leading geneticists of those days. The utopia was substantiated in its cognitive and methodological aspects (Roth, 1988, pp. 51, 52).

From Roth we also learn that it was not concern for the welfare of mankind nor the search for scientific truth that propelled the efforts of the leading geneticists. No, it was the desire to rule over the life of every individual, and also to save capitalism.

That statement about capitalism jars severely the credence of any reader who might be even remotely aware of the views of the most famous geneticists of the time, such as H. J. Muller and J. B. S. Haldane. It is no simple deviation from the facts, but an outright rejection of the truth. Take Muller, for example. His initial sympathy for communism and his hostility to capitalism are very well known. Let the doubter read his famous address to the Eugenics Congress in New York in 1932, which was entitled "The Dominance of Economics over Eugenics" (Muller, 1933). It was further expressed in his book *Out of the Night* (Muller, 1935). The evolution of his political views after his four-year stay in the Soviet Union in the 1930s has been described by Elof Carlson in his biography of Muller, *Genes, Radiation and Society: The Life and Work of H. J. Muller* (Carlson, 1981). Muller's sympathies for Communism were later shattered, but that did not mean for him any reconciliation with capitalism. As Carlson wrote: "Muller never chose the path of being a vocal anticommunist. He did denounce communism when he had to, but he never closed eyes to the abuses and inadequa-

cies that still existed in the United States" (p. 432). During the civil war in Spain, both he and Haldane served in the International Brigade, offering medical services to the troops of the socialist army.

Haldane, on his side, expressed his adherence to Marxism in his book, *The Marxist Philosophy and Sciences* (Haldane, 1938). Until 1948 he remained a member of the Communist Party in England, but he left the Party then in protest against the ruthless use of power by Stalin, and because of Stalin's decision to give Lysenko's quackery the status of an official ideology. In spite of this, Haldane never forsook his own communist ideal. Because of his well-known Marxist views, Haldane had difficulty even until his last years in getting entry into the United States as a visitor. He was denied an entry visa to attend an International Symposium on the Origins of Physiological Systems held in 1963 in Florida, and again in 1976 was denied an entry visa to visit the University of North Carolina, which had invited him to come (Feldman, 1976). Nor was the Nazi government grateful to persons who, according to Roth, had supplied the underpinnings of its annihilation policy. A "Special Search List for Great Britain," drawn up by the Central Security Office during the time of the projected invasion of Britain, listed persons it was thought important to incarcerate at once. Along with Churchill was J. B. S. Haldane (Shirer, 1960).

How then could Muller and Haldane be imagined to be ardent supporters of capitalism? Roth supplies an answer. The sympathy of geneticists toward capitalism was by no means disinterested. Roth wrote:

> The international union of geneticists [*die Genetiker-gemeinde*] was world-wide subsidized and controlled by U.S. Capitalism. Grants were distributed on an international scale and German institutions, the Kaiser-Wilhelm Institutes among them, were not excluded from these subsidies. Scientific day-dreams about power and economic claims for power [*ökonomischer Machtanspruch*] mutually fortified one another (Roth, ibid., p. 19).

Roth even expressed the thought that it was by no means accidental that the Soviet geneticists who emigrated to the West (Timoféeff-Ressovsky and Dobzhansky) in the 1920s became the leaders of radiation genetics, population genetics, and the accepted evolutionary synthesis (Roth, ibid., p. 22).

Bentley Glass, to whom I have already referred in my introductory paragraphs, in his review, "The Roots of Nazi Eugenics," commented on these views held by Roth, as follows:

> It was clearly (in some minds) a gigantic international conspiracy to make poor Germany the site of a field experiment in human genetics and evolution. And, of course, it was financed by the Rockefeller Foundation, which supplied considerable funds to the Kaiser-Wilhelm Institutes (Glass, 1989, p. 178).

The irony of his comment may perhaps not be perceived by some readers. In any case, Glass vigorously rebutted the following statement made by Roth:

> Until the outbreak of war, the traditional eugenic movements of the world applauded the compulsory sterilization and asylum laws of the Nazis.

Among those who forcefully and in a timely way repudiated Nazi eugenics Glass lists H. J. Muller and Julian Huxley, both of whom Roth reckons among those who bear the responsibility for Nazi eugenic (read, "extermination") policy. To these names Thomas Hunt Morgan and J. B. S. Haldane ought to be added. (In respect to Morgan, see Garland Allen, 1978, pp. 282-283.)

As a population geneticist myself, one who has been studying the genetic load of *Drosophila* populations since 1937, I cannot refrain from commenting that Roth's description of the methods used to unmask the concealed recessive mutants that constitute the genetic load is simply wrong. In ignorance, Roth then makes the following judgment:

> We thus have good reason to put forward a well-founded supposition that no extraordinarily intense, harmful mutation load exists in nature, but was brought about by [human] experiment itself (Roth, ibid., p. 56).

According to Roth, the chief person in Germany responsible for Nazi crimes is thus shown to be Timoféeff-Ressovsky. Many pages of Roth's treatise are devoted to a description of Timoféeff's experiments in radiation and population genetics. One example is sufficient

to reveal his perverted logic. As has already been mentioned, Timoféeff, in an article published in *Erbarzt* (Timoféeff-Ressovsky, 1935) spoke about the necessity of making careful diagnoses of hereditary human diseases and emphasized the variability in their manifestations. Both internal and external factors are responsible for this variability. Diseases caused by different mutant genes sometimes have a similar manifestation, while some mutations manifest themselves differently in different individuals. The right classification of hereditary disorders, so urgently needed for cure or for genetic counseling, is hindered by this variability of manifestation. Is it then not obvious that for the extermination of harmful genes by the annihilation of their carriers a detailed classification of diseases that neglects this variability is of no value at all?

Again, according to Roth, Timoféeff deliberately forced a fear of human degeneration upon the German leaders and thus promoted the policy of annihilation. In this context, Roth wrote:

> Needed were new legitimate arguments to sharpen and to make more precise the measures of correction applied to counteract the putative increasing genetic danger for the population. Timoféeff-Ressovsky provided the arguments willingly (Roth, ibid., p. 37).

Those who are guilty must be punished. The following passage concludes Roth's article:

> Some experts in human genetics and some physicians were, after 1945, court-martialed. . . . While some persons were punished, those who were responsible for eugenic and demographic-genetic massacre were left out of reach, together with their scientific ruling programs (Roth, ibid., p. 59).

It seems that, in accordance with Karl Heinz Roth's conviction, Timoféeff would be impeached in the courts of justice side by side with Muller, Chetverikov, Dobzhansky, Julian Huxley, and J.B.S. Haldane, to say nothing of almost all other geneticists and evolutionists, West or East. That did not happen. Instead, Timoféeff alone was court-martialed by Stalin's court of military justice. The article by Ilyin and Provorotov supplies an account of the recent rehearing of the original court's verdict.

Roth's delirious vision of an international conspiracy against Germany supported by the Rockefeller Foundation, with Timoféeff-Ressovsky playing the role of mercenary in fulfilling the diabolical plan, coincides in every respect with the world concept fostered by the obscurantist stratum to which Ilyin and Provorotov belong. In their lampoon, they do not restrict themselves to an attack upon Timoféeff-Ressovsky, but warn every defender of any person whose persecution in the time of Stalin they do not consider to be unjust. Such defenders are warned that their very defense of such persons will be regarded as an attack upon the sacred principles of the past and will be subjected to a vigorous counterattack. The poet Boris Pasternak and the writer Vassily Grossman are named as examples of those who, like Timoféeff, should be condemned even more severely than they were the first time, just because faithful supporters have dared to raise the question of their posthumous rehabilitation. Ilyin and Provorotov declare that it would be better for the defenders to be silent and bring about no further disclosures that would damage their idols.

I learned from Ilyin and Provorotov, to my amazement, that the list of names of those who signed the appeal for a rehabilitation of Timoféeff-Ressovsky had, at its top, the name of N. P. Dubinin. Dubinin, however, has been eager to correct the "mistake" that included him among the signers. He has sent a note to *Nash Sovremennik*, the same magazine chosen by Kuzmin, Bondarenko, and Ilyin and Provorotov for their broadsides, to protest that "It is my duty to declare that I did not sign any petitions to the Supreme Court about the rehabilitation of Timoféeff. . . . I always considered that his work for Germany during 1941–1945 is immoral" (Dubinin, 1990). Dubinin referred to a book by D. Irving to document the charge that the work of Timoféeff-Ressovsky in coauthorship with Born and Zimmer was aimed at using irradiation as a military weapon. Dubinin also quoted memoirs he wrote, issued in 1973 and 1975, to express his continuing affirmation of his opinions of those years, in which he claimed that the unforgivable guilt of Timoféeff was to accept an invitation to go abroad (Dubinin, 1990, p. 191). It must be said that in these days, when *glasnost* goes far beyond the limits set by Gorbachev, such a statement sounds both obsolete and

frightening. The barometer points to bad weather ahead, to a collapse of freedom of speech. Dubinin concluded his commentary by commending Ilyin and Provorotov. In fact, they did not really need to refer to the old inquiry of 1946 on Timoféeff because the new one "has shown in relief who Timoféeff-Ressovsky really was."

Two recent publications in defense of Timoféeff-Ressovsky have come out of Russia. One, written by A. Malenkov and V. Ivanov (1989) was published in *Nature*. It is a response to Müller-Hill's review of Granin's book *Zubr* and bears the same title as Müller-Hill's review, "Heroes and Villains." Malenkov and Ivanov knew Timoféeff personally. They were members of his unofficial "university" in the Urals, where Timoféeff was the sole professor. They have stated that "according to the opinions of people who knew him well, Timoféeff-Ressovsky—descendant of the princely Vsevolzhskies—never betrayed his ancestral motto: 'Honor above all'" (p. 612).

The other publication presents materials collected by S. Bura in defense of Timoféeff-Ressovsky and appeared in the newspaper *Moskovskie Novosti* [*Moscow News*] (Bura, 1990). It was timed to appear on the ninetieth anniversary of Timoféeff's birth. This report describes the findings of the Special Commission summoned by the Eastern Branch of the Academy of Sciences in Germany to study the activities of Timoféeff-Ressovsky during the war years 1941-1945 and to find whether he was involved in any war projects. The commission rejected all the accusations made by the Prosecutor's Office of the USSR, at whose request the Commission had been instituted. Further evidence, also published in the newspaper account, was from two letters Bura had received from various persons. One of these letters was written by Nikolaus Ril, who was one of the supervisors of the military uranium project of the Nazis. He simply stated that Timoféeff's research had nothing at all to do with measures of warfare. Timoféeff did nothing he would live to regret. He had survived among the Nazis only thanks to friends who sheltered him.

The second letter was from E. Feinberg, a German physicist, who had knowledge of a French prisoner of war who worked in Timoféeff's Department. The name is given as "Sharl Peir," which probably stands for "Charles Peyre." This man testified under oath that Timoféeff was a convinced anti-fascist. Timoféeff's assurances to the Nazi administration that his radiation experiments with fruit flies were important for the war were simply camouflage. The words used by Bura as the title of his communication, "I was born Russian and I don't see any means to change that fact," were uttered by Timoféeff when he rejected the offer of German citizenship.

The entire ideology of the recent detractors of Timoféeff's reputation may be regarded as equivalent to the doctrines of the society "Memory." It is therefore of particular interest that the Russian language newspaper of New York, *Novoye Russkoye Slovo*, of February 22, 1990, reported, on the basis of an official Soviet press release, that the Procurator's Office has brought legal action against the society "Memory." This action cites the society's Program, which was published in the little-known Moscow newspaper *Energetics*, and which demands that the Government implement a law depriving Jews of the right to hold any of the leading positions, to join the Communist Party, or to be honored with any scientific degree. The outcome is not clear.

Let me conclude my defense of Timoféeff-Ressovsky, then, by saying that in my own opinion he needs no "rehabilitation" by the Soviet government. He himself never applied for that, a rejection that I believe to be in true accord with his personality. He believed in an eternal hierarchy of moral values, and remained confident that in the future the perverted scale of such values adopted by his prosecutors would be reversed. History would justify him. In that respect, he stands beside Galileo and other great scientists persecuted by the authorities of their time. Like Gregor Mendel, the scientific forebear of all geneticists, perhaps he too died saying, at least in his heart, "Meine Zeit wird schon kommen." [My time will indeed come.]

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