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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

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.