MEMOIRS

Andrei Sakharov

TRANSLATED FROM THE RUSSIAN
BY RICHARD LOURIE

Alfred A. Knopf New York 1990

6

THE TAMM GROUP

I FIRST HEARD of the splitting of uranium nuclei just before the war, from my father, who had attended a lecture on the subject. A short while later, I read an article on nuclear fission in the journal *Achievements of the Physical Sciences.* I'm ashamed to admit that I did not fully grasp the importance of this discovery—this despite the fact that both my father and the article made mention of the theoretical possibility of a chain reaction (although I don't recall any clear distinction being made between a *controlled* chain reaction such as takes place in a nuclear reactor and an *explosive* chain reaction of the sort that occurs when an atom bomb is detonated).

By now, the basic principles of a controlled chain reaction have been treated throughly in the non-classified literature, and the physics of nuclear explosions has also been discussed, albeit with some deliberate inaccuracies inserted and details withheld.

In both the controlled and the explosive chain reaction a free neutron is captured by the nucleus of a fissionable isotope, whereupon the nucleus splits in two. This process releases energy, as well as two or three free neutrons; these may, in turn, cause more nuclei to split. This process is termed a chain reaction. It can proceed even at room temperature, since neutrons, being electrically neutral, are not repelled by atomic nuclei.

Of greatest importance are the chain reactions that take place in the rare uranium-235 isotope and in plutonium-239. Let us recall that atomic nuclei consist of protons, which carry a positive charge, and neutrons, which carry no electrical charge. The number of protons in the nucleus—equal to the number of the shell's electrons—determines the atom's chemical properties, as well as its size, optical properties, and so forth. Nuclei with the same number of protons but different numbers of neutrons are known as "isotopes" of a chemical element. The number of neutrons determines the atomic weight (or, to be more precise, the mass number) and the atom's behavior in nuclear reactions. For example, natural uranium contains 99.3 percent nuclei of uranium-238 (92 protons and 146 neutrons in the nucleus) and 0.7 percent nuclei of uranium-235 (92 protons and 143 neutrons). The mass number of the isotope is the sum of the protons and neutrons combined ($238 = 92 + 146$, $235 = 92 + 143$).

At low neutron energies, below one MeV (one million electron-volts), a fission reaction occurs only in uranium-235 and plutonium-239—so-called fissionable isotopes. Uranium-238 nuclei will split if struck by high-energy neutrons, but the reaction is not self-sustaining, since fission does not produce "fast" (high-energy) neutrons. A "forced" fission reaction using U-238 is possible, however, if fast neutrons are delivered from an external source such as a thermonuclear reaction (the fusion of two deuterium nuclei produces free neutrons with energies of 2.5 MeV; the fusion of one deuterium nucleus and one tritium nucleus produces free neutrons with energies of 14 MeV). [Deuterium and tritium are isotopes of hydrogen.] Inside a nuclear reactor, a chain reaction can be maintained using the mixture of uranium isotopes that occur in nature. This reaction can be controlled quite easily, since some of the neutrons are released after a delay and not at the moment when the uranium atom splits.

In 1939–1940, much of this was still unknown. The last prewar discussion of chain reactions was contained in a paper by Zeldovich and Khariton.[1] Foreign journals stopped publishing papers on the subject, although, as we know, intensive nuclear research continued in secret throughout the war. As for myself, I simply forgot about the subject until February 1945, when I read (in *The British Ally,* a magazine for Soviet citizens published by the British embassy in Moscow) a description of a heroic British-Norwegian commando raid on a cache of heavy water in Norway that the Germans had intended to use in an "atomic bomb"—an explosive device of fantastic power utilizing nuclear fission. I believe this was the first mention of an atomic bomb in the press, and the "leak" may well have been intentional, perhaps designed to discourage the German program. The psychological warfare conducted by many countries at that time was far too complex for ordinary mortals to fathom.

I immediately recalled everything I'd ever heard about fission and chain reactions. Then, during the next few months, I began to hear occasional

[1][An important paper by Zeldovich and Khariton was delivered at the Conference on Questions of the Physics of the Atomic Nucleus held in Kharkov, November 15–20, 1939, and published later that year. For the early history of atomic physics in the USSR, see Arnold Kramish, *Atomic Energy in the Soviet Union* (Stanford University Press, 1959).]

references to a "Laboratory No. 2," but paid little attention. Later I was to learn that "Laboratory No. 2" was a major scientific research institute headed by Igor Kurchatov—the establishment now known as the Kurchatov Institute of Atomic Energy.

As I became absorbed in the wider world of theoretical physics, my interest in atomic matters again ebbed. May 1945 was marked by an unforgettable event—V-E Day. In Europe, fascism had been defeated; the war was over. But in the Pacific it was still going on.

On my way to the bakery on the morning of August 7, 1945, I stopped to glance at a newspaper and discovered President Truman's announcement that at eight A.M. the previous day, August 6, an atom bomb of enormous destructive power had been dropped on Hiroshima. I was so stunned that my legs practically gave way. There could be no doubt that my fate and the fate of many others, perhaps of the entire world, had changed overnight. Something new and awesome had entered our lives, a product of the greatest of the sciences, of the discipline I revered.

The British Ally began serial publication of the Smyth Report, an account of the development of the atom bomb that contained an abundance of declassified information on isotope separation, nuclear reactors, plutonium, and uranium-235, and a general description of the structure of the atom bomb.[2] I would snatch up each new issue of the *Ally* and scrutinize it minutely with an interest that was purely scientific: But I was also eager to put my talents as an inventor to the test. But everything I dreamed up was either old hat (the lattice effect for reactors, which had been known for three years) or impractical (the method of isotope separation based on Knudsen flow[3] into the mouths of configured rotors). My old school friend Akiva Yaglom said, "Andrei proposes at least two new methods of isotope separation a week." After the final installment of the Smyth Report appeared, however, I gave little thought to the atom bomb for the next two and a half years.

FATE CONTINUED to weave its web around me (I recall the holiday scene from *Faust* which Oleg had read aloud to me).[4]

[2][The formal title of this report was "Atomic Energy for Military Purposes. The Official Report on the Development of the Atomic Bomb Under the Auspices of the United States Government, 1940–1945." It was written by the physicist Henry Smyth and released by President Truman on August 11, 1945.]

[3][Knudsen flow takes place when the mean free path of a particle is longer than the length of the chamber.]

[4][In this scene Mephistopheles, in the guise of a black poodle, circles around Dr. Faust.]

Toward the end of 1946, I received a mysterious letter requesting me to come to Room 9 of the Peking Hotel at a designated time. Some implausible pretext was provided, although I can't remember what. The Peking Hotel was on Mayakovsky Square, not far from my parents' house. Room 9 was furnished like a typical government office—the T-shaped table, the portrait of Stalin, and so on. The man at the table rose to greet me, invited me to be seated, introduced himself as General Zverev, and then outlined the true reason for the meeting: "We [he never explained whom he meant by "we"] have been following your progress in science for quite a while. We'd like you to work with us on state projects of the greatest importance after you complete your graduate studies. You'll have the best of everything for your work—libraries with scientific literature from all over the world, big accelerators, the best pay and living conditions. We know you have a housing problem; if you agree to work for us, you'll be given an apartment in Moscow that will be reserved for you even if you're assigned elsewhere for a while."

I turned the offer down: I hadn't left the munitions plant for FIAN and the frontiers of physics, only to abandon everything now. I told General Zverev that for the time being I wished to continue doing theoretical research with Tamm. Zverev said he hoped my decision was not final.

What if I *had* agreed? Several years later, at the Installation, I met Dmitri Zubarev, a theoretical physicist, who told me that he, too, had been summoned to Zverev's room, and had accepted the offer (he too had a housing problem). Zubarev ended up working near the Black Sea with scientists brought from Germany. The project's director, Avraami Zavenyagin, had pinned great hopes on the German scientists, but they weren't trusted and little serious work was done. Zubarev got bored and used his connection with Nikolai Bogolyubov to get transferred to the Installation, where he remained until 1953.

IN 1947, after completing my thesis, I was invited to discuss it at "Kurchatov's place"—Laboratory No. 2, by then renamed the "Laboratory for Measuring Instruments" (LIPAN). I spoke in a small auditorium, after which I was asked many questions by the physicists present, Kurchatov among them. Afterward, he took me to his spacious office, where he seated himself behind a large desk piled high with scientific journals and equipped with telephones of every color. As we talked, Kurchatov stroked his bushy black beard, his expressive brown eyes gleaming. On the wall facing me hung a larger-than-life oil portrait of Stalin with his pipe and the Kremlin in the background. The painting, clearly an original by one of the "court" artists, symbolized Kurchatov's high standing in the state hierarchy; it remained in place for some time even after the Twentieth Party Congress.

Kurchatov suggested that I switch to his institute and work in theoretical nuclear physics after finishing my graduate studies. I knew that Arkady Migdal and Isaak Pomeranchuk (the questioners at my dissertation defense) had accepted similar offers: Migdal worked at LIPAN, Pomeranchuk at the Institute of Experimental and Theoretical Physics, which was headed by Abram Alikhanov. Kurchatov supported basic scientific research of every type, but when the occasion demanded, he would also devote his facilities—and the brains of his scientists—to solving practical problems. He always managed this with such tact that no one felt coerced or offended. It was on Kurchatov's initiative that the science city of Dubna was built and two giant accelerators constructed there.

Kurchatov evidently had liked my talk, or me, or else Migdal had put my name forward. In any event, I turned down the offer, again citing my desire to work with Tamm. Thus, in 1946 and 1947, I twice rejected attempts to entice me away from FIAN and the frontiers of theoretical physics. But the third time, in 1948, nobody bothered to ask my consent.

TOWARD THE END of June 1948, Tamm, in a rather furtive manner, asked me, along with another of his charges, Semyon Belenky, to remain behind after his Friday in-house seminar. As soon as we were alone, Tamm shut his office door and announced his startling news: by decision of the Council of Ministers and the Party Central Committee, a special research group had been created at FIAN. Tamm had been appointed to lead the group, and Belenky and I were to be among its members. Our task would be to investigate the possibility of building a hydrogen bomb and, specifically, to verify and refine the calculations produced by Yakov Zeldovich's group at the Institute of Chemical Physics. (I gave it no thought at the time, but I now believe that the design developed by the Zeldovich group for a hydrogen bomb was directly inspired by information acquired through espionage. However, I have no proof of this.[5])

A few days later, after recovering from shock, Belenky remarked lugubriously that: "Our job is to kiss Zeldovich's ass!" Belenky's recent dissertation had been on electromagnetic shower processes in cosmic rays, but during the war he had worked at the Zhukovsky Aerohydrodynamic Institute, involved in research on supersonic flow and jet flight. That was probably why he had been

[5]David Holloway writes in "Soviet Thermonuclear Development," *International Security* 4:3 (1979/80), p. 193: "The Soviet Union had been informed by Klaus Fuchs of the studies of thermonuclear weapons at Los Alamos up to 1946. . . . It is true that Fuchs's account of these early discussions of the superbomb would have been misleading rather than helpful to Soviet scientists in a scientific sense, because the early ideas were later shown not to work." This provides some evidence for my conjecture.—A.S. July 1987

included in our group—no one else at FIAN had experience in the field of gas dynamics.

As to why *I* had been selected, I was told that the director of FIAN, Academician Sergei Vavilov, had said: "Sakharov's got a housing problem; we'll be able to help him if he's included in the group." The fact that I was working on nuclear physics and plasma theory and had some ideas about mu-meson catalysis no doubt also played a role. Vavilov may have known about the interest I'd shown in isotope separation three years before. All in all, I imagine the chief reason for my inclusion in the special group was Tamm's strong recommendation.

Vavilov was true to his word. In May 1948, I was assigned two rooms on Twenty-fifth of October Street, in the very heart of Moscow. It was not a "posh" place, despite its location: the rooms were off a long common corridor, and wood was still used for heating. At the last moment, a deputy director of FIAN appropriated one of our two rooms for his mother (a very old and quite amiable woman—she and Klava got along wonderfully). Our remaining room measured only 150 square feet, so we had no place for a dining table, and ate off stools or the windowsill. The ten families living on our corridor were served by a single small kitchen, and the toilet, which was located off the staircase landing, served two communal apartments. There was neither bath nor shower. But we were delighted. We had our own place—no more noisy hotels or capricious landlords who could kick us out whenever they pleased! And so began four of the happiest years in our family life, the source of some wonderful memories.

Klava's difficult relationship with my mother, which had upset me a great deal, improved considerably. Tanya was developing into a good, cheerful little girl who didn't lack for admirers among the boys in the building. We became friendly with our neighbors in the apartment and at the dacha (we had rented a room in a cottage in the village of Troitskoe on the Moscow–Volga Canal). Klava and Tanya spent the summer of 1948 there. Each Sunday I would bring out provisions from the city, and would stay for a day or two. I remember the sparkling water, the sun, the lush greenery, the boats gliding over the reservoir (I was, however, banished from the sailboats because of my ineptitude), and the warmth of our friendship with our neighbors, the Obukhovs, the Rabinoviches, and the Shabats. A colleague from FIAN, Moisei Markov, had rented a place close by, and I had a gentle, teasing relationship with his wife, Lyuba.[6]

I spent at least five days a week at FIAN in the room assigned to our special group. There were two more members of our team—Vitaly Ginzburg, ex-

[6]Obukhov, a future academician, was specializing in the physics of the atmosphere and turbulence; Rabinovich was a fellow graduate student; Shabat a mathematician; Markov, another future academician, was a theoretical physicist.

tremely talented and one of Tamm's favorite students, and Yuri Romanov, a young researcher who had recently joined the department. Ginzburg apparently was included on a part-time basis, and dropped out when the group was transferred to the Installation.

Despite summer's distractions, we worked with a fierce intensity. Our world was bizarre and fantastic, a striking contrast to everyday city and family life, and to normal scientific pursuits.

THIS SEEMS a good time to describe our approach to the ethical and human aspects of our work. My own attitude, which was initially influenced by Tamm and others around me, has continued to evolve over the years; but in this chapter I shall confine myself to the period prior to the 1955 thermonuclear test.

In 1948, no one asked whether or not I *wanted* to take part in such work. I had no real choice in the matter, but the concentration, total absorption, and energy that I brought to the task were my own. Now that so many years have passed, I would like to explain my dedication—not least to myself. One reason for it (though not the main one) was the opportunity to do "superb physics" (Fermi's comment on the atom bomb program). Many people thought his remark cynical, but cynicism ordinarily presupposes duplicity, whereas I believe Fermi was quite sincere, although he may have been begging the real question. It should not be forgotten that Fermi's complete sentence—"*After all,* it's superb physics"—implies the existence of another side to the matter.

The physics of atomic and thermonuclear explosions is a genuine theoretician's paradise. The equation of state of matter at moderate pressures and temperatures cannot be calculated without introducing simplifying assumptions into the theoretical equations (otherwise the computations involved exceed the capabilities of the most advanced computers), but it is possible, using relatively straightforward calculations, to describe what happens at temperatures of millions of degrees centigrade, under conditions resembling those at the center of a star. The equation of state can be expressed by the formula $p = aDT + bT^4$, where p is the pressure, D the density, and T absolute temperature, and a and b are easily calculated coefficients. The first term (aDT) is the pressure of the ideal, completely ionized gas, and the second (bT^4) is the radiation pressure. In old days, Lebedev had to use refined techniques to measure it, but here the radiation pressure is enormous and pivotal for the process. The calculation of the pressure of the gas is also simplified at very high temperatures: ionization is complete, and the interactions of the particles can be neglected.

Similarly, formulas to determine the thermonuclear reaction rate become straightforward. Its value is easily calculated by elementary integration if the reaction cross section is known from experiment as a function of the energies of the colliding particles. I began my work with the Tamm group by calculating these integrals, using the saddle-point method known to every student of physics and mathematics. A few days later I submitted my first secret report, which was labeled S-1 (Sakharov 1).

A thermonuclear reaction—the mysterious source of the energy of sun and stars, the sustenance of life on Earth but also the potential instrument of its destruction—was within my grasp. It was taking shape at my very desk. But I feel confident in saying that infatuation with a spectacular new physics was not my primary motivation; I could easily have found another problem in theoretical physics to keep me amused—as Fermi did, if you will pardon this immodest comparison. What was most important for me at the time, and also, I believe, for Tamm and the other members of the group, was the conviction that our work was *essential.*

I understood, of course, the terrifying, inhuman nature of the weapons we were building. But the recent war had also been an exercise in barbarity; and although I hadn't fought in that conflict, I regarded myself as a soldier in this new scientific war. (Kurchatov himself said we were "soldiers," and this was no idle remark.)

Over the course of time we devised or borrowed a number of principles, including strategic parity and nuclear deterrence, which even now seem to justify intellectually, at least to some extent, the creation of thermonuclear weapons and our role in the process. Our initial zeal, however, was inspired more by emotion than by intellect. The monstrous destructive force, the scale of our enterprise and the price paid for it by our poor, hungry, war-torn country, the casualties resulting from the neglect of safety standards and the use of forced labor in our mining and manufacturing activities, all these things inflamed our sense of drama and inspired us to make a maximum effort so that the sacrifices—which we accepted as inevitable—would not be in vain. We were possessed by a true war psychology, which became still more overpowering after our transfer to the Installation.

I have read that on August 6, 1945, Robert Oppenheimer locked himself in his office while his younger colleagues ran around the Los Alamos laboratory shouting Indian war whoops, and that he also wept at his meeting with President Truman. Oppenheimer's personal tragedy disturbs me deeply, all the more so because I believe he was acting in good faith, for reasons of principle. Of course, the whole sad story of Hiroshima and Nagasaki which so affected his soul was even more troubling. Nuclear weapons have never again been employed in battle, and my fervent and paramount dream continues to be that they will be used only to deter war, never to wage war.

Have Soviet and American atomic scientists helped to keep the peace? After more than forty years, we have had no third world war, and the balance of nuclear terror—the threat of MAD (mutual assured destruction)—may have helped to prevent one. But I am not at all sure of this; back then, in those long-gone years, the question didn't even arise.

What most troubles me now is the instability of the balance, the extreme peril of the current situation, the appalling waste of the arms race. Thermonuclear weapons could end human civilization; they have become so frightening that the very thought of using them seems unreal. Their credibility as a deterrent has thus decreased, while their threat has increased enormously.

Is there a way out? That question will soon be answered. Each of us has a responsibility to think about this in global terms, with tolerance, trust, and candor, free from ideological dogmatism, parochial interests, or national egotism. I believe the time has come for nuclear deterrence to be replaced first by parity in conventional weapons, which, in the ideal case, will in its turn be succeeded by an equilibrium reached through farsighted statesmanship and compromise. I know I'm not alone in this conviction; I was pleased to find similar ideas in an article by Dr. Wolfgang Panofsky. One word of caution: I am convinced that the transition from nuclear deterrence to parity in conventional weapons must be managed with care and executed in stages. (This view is, of course, based on my assesssment of the present-day situation.)

AT ABOUT the same time that we Soviet scientists were beginning our calculations, Robert Oppenheimer, then chairman of the General Advisory Committee of the Atomic Energy Commission, was trying to apply the brakes to the American hydrogen bomb program in the expectation that the USSR would then refrain from developing thermonuclear superweapons of its own.

Oppenheimer's judgment was challenged by Edward Teller. Teller had experienced firsthand the 1919 Communist revolution in his native Hungary, and he had a deep-seated mistrust for the socialist system. He insisted that only American military strength could restrain the socialist camp from an expansion that would threaten civilization and democracy and might trigger a third world war. That is why Teller believed it necessary to speed development of an American H-bomb and continue nuclear testing despite the genetic damage and other nonthreshold biological effects that implied. (Later on, I was to object to his position on testing.) And that is why he testified against Oppenheimer. Teller has been ostracized ever since by many American scientists, who consider his testimony and his overall position to have violated ethical norms binding on the scientific community, as Freeman Dyson, for one, makes clear in his memoirs.[7]

[7][*Disturbing the Universe* (Harper & Row, 1979), p. 90.]

What are we to make of the tragic conflict between these two extraordinary individuals, now that we can view it through the prism of time? In my opinion, both men deserve respect. Each was certain that truth was on his side and that he was morally obligated to see the matter through to the end—Oppenheimer by behaving in a way that was later construed as a breach of his official duties, and Teller by disregarding the tradition of "good form" in the scientific community. Issues of principle were further complicated by technical and policy questions. Oppenheimer apparently believed (and had impressive evidence to back his view) that the designs that had been concocted for a hydrogen bomb were not very promising. Teller believed that a practical solution would be found sooner or later; he may already have had some idea of the eventual design; and he was, of course, right in this respect.

The dispute over their opposing stands continues to this day, but the facts that have come to light about the state of affairs in the late 1940s support Teller's point of view. The Soviet government (or, more properly, those in power: Stalin, Beria, and company) already understood the potential of the new weapon, and nothing could have dissuaded them from going forward with its development. Any U.S. move toward abandoning or suspending work on a thermonuclear weapon would have been perceived either as a cunning, deceitful maneuver or as evidence of stupidity or weakness. In any case, the Soviet reaction would have been the same: to avoid a possible trap, and to exploit the adversary's folly at the earliest opportunity.

Still, Oppenheimer's position was not without merit. His assumption was that it would be exceedingly difficult to build a hydrogen bomb, and he hoped an American moratorium would lead the USSR to abandon further research on the grounds that: "The Americans have failed, so let's not waste our time. Even if we succeed, they'll catch up and pass us before we know it, and we'll end up losers again." Oppenheimer surely realized that for his plan to work, several conditions had to be met: consensus within the American administration; skillful American diplomacy; Soviet H-bomb research had to be at a point where the USSR would be ready to call it quits (and this was probably not the case); and the United States had to be willing to accept some risk. All this must be judged in the context of the times: it was the period of maximum mutual distrust—the Cold War, the Berlin blockade, soon the Korean War—and Moscow enjoyed superiority in conventional arms, just as it does now.

Oppenheimer felt he had little hope of convincing his opponents that he was right, and so he acted in a roundabout manner. He must have realized that more conventional, seemingly safer policies were likely to prevail; and in that case he was prepared to quit the game. He had every moral right to do so, and this is indeed what happened.

I cannot help but feel deeply for and empathize with Oppenheimer, whose personal tragedy has become a universal one. Some striking parallels between his fate and mine arose in the 1960s, and later I was to go even further than Oppenheimer had. But in the 1940s and 1950s my position was much closer to Teller's, practically a mirror image (one had only to substitute "USSR" for "USA," "peace and national security" for "defense against the communist menace," etc.)—so that in defending his actions, I am also defending what I and my colleagues did at the time. Unlike Teller, I did not have to go against the current in those years, nor was I threatened with ostracism by my colleagues. I had to overcome some resistance on technical questions, but I was not without support; the struggle for the "Third Idea" [see Chapter 12] arose for different reasons and was conducted in different circumstances than in Teller's case.

How did these directions find expression in my life? This book is my answer to that question.

If I am right in believing that the thermonuclear weapon model on which Zeldovich, Kompaneyets, and their team were working in the 1940s and early 1950s was the fruit of espionage, then Oppenheimer's case is strengthened, at least in theory. It would then be plausible that if the Americans had not initiated the whole chain of events, the USSR would have pursued the development of a thermonuclear bomb only at a much later date, if at all. A similar scenario has been repeated with other weapons systems, including nuclear-powered submarines and MIRVs [missiles carrying several warheads which can be independently targeted]. Now isn't it once again time to stop and think before it's too late? I have in mind SDI, the Strategic Defense Initiative.

However, it is clear now that the situation was already out of control by the time the Teller-Oppenheimer dispute erupted, and neither the USSR nor the United States could then have pulled back. We have been building thermonuclear weapons ever since; but so far, at least, we have avoided the abyss of a third world war.

Before moving on to another subject, I would like to note that Teller's colleagues seem quite unfair (and rather mean-spirited) in their condemnation: Teller was, after all, taking a stand based on principle. The very fact that he was willing to maintain a minority stance on an issue of such critical importance should be viewed as evidence in his favor. It is surely ironic that in 1945 Teller and Leo Szilard strongly favored exploding an atom bomb at some uninhabited site in hopes that a demonstration of its power might end the war without use of the new weapon against a Japanese city, while Oppenheimer persuaded them (Teller himself says in retrospect, too easily) that the decision should be left to soldiers and politicians.

After this lengthy but vital digression, I want to return to my starting point—"superb physics." Research on the atomic bomb no doubt involves "superb physics," but it is essentially "consumer-oriented." Exploring elemen-

tary particles and other phenomena through fission and fusion explosions is a very different matter from working in laboratory conditions, although from the point of view of elementary processes there is little difference. These processes involve atomic nuclei and electrons and photons with energies in the 20 KeV range, easily attainable in laboratory conditions, and the behavior of elementary particles at that energy level has been thoroughly investigated. To learn something new, one needs high-energy levels at each elementary interaction, and not massive explosions consuming immense resources and producing great destruction. These high-energy levels are sought in cosmic rays or through the use of particle accelerators. Physicists have also turned to cosmology in their study of high-energy processes; and it is from these sources, and not from nuclear explosions, that new discoveries in fundamental science will come.

Perhaps the sole contribution of nuclear explosions to fundamental science has been the opportunity they provide to study the properties of the transuranic elements. The hypothesis is now generally accepted that all elements with atomic numbers greater than that of iron are products of thermonuclear reactions in supernovas and other stars. Similar processes could be initiated in specially designed thermonuclear charges. I don't know whether such "scientific" explosions have been conducted in the USSR or the United States, but I remember reading somewhere that the element californium (atomic number 98) was discovered in the course of testing a certain type of American thermonuclear weapon. Research into the transuranic elements is a rather narrow specialty, however, and has had little impact on physics as a whole: in this sense, the mountain has brought forth only a mouse. In the future, it may be possible to use nuclear explosions to accelerate elementary particles, but that is only a dream so far, and perhaps not a very practical one.

I WAS INVOLVED in top-secret work on thermonuclear weapons and related research for some twenty years. I was a member of Tamm's special group at FIAN from June 1948 until March 1950, when I was assigned to the Installation [*obyekt*]—the secret city where those developing atomic and thermonuclear weapons lived and worked. I was employed there until my clearance was revoked in July 1968.

I shall remain silent about some aspects of my life and work in the period between 1948 and 1968. No matter what fate may have in store for me, I consider myself bound for life by a pledge not to divulge state and military secrets, a commitment I undertook of my own free will in 1948.

The task of Tamm's special group, as he himself formulated it, was to analyze, refine, correct, and extend the calculations of Zeldovich and his team, and to assess the whole project of building a thermonuclear weapon (i.e., just what Belenky had said so colorfully). I spent two months studying Zeldovich's reports and improving my meager knowledge of gas dynamics—we were all using Landau and Lifshitz's volume on this subject—and of astrophysics (the physics of stars and the physics of a nuclear explosion have much in common). Once, as I was standing in line for the cashier at the public baths, mulling over certain questions in gas dynamics (I couldn't stop thinking about them), I realized that an explosion in an ideal, cold gas can be described hydrodynamically by a function with a single variable if certain simplifying assumptions are made. I later discovered that my solution had been anticipated by Leonid Sedov, and still earlier by Theodore Taylor, but I went on to work out other self-similar solutions that were useful for the qualitative and semi-quantitative description of processes that interested us.

Two months later, I radically changed the direction of our research by proposing an alternative design for a thermonuclear charge that differed from the one pursued by Yakov Zeldovich's group in both the explosion's physical processes and the basic source of the energy released. I will call this the "First Idea."

Vitaly Ginzburg soon suggested a "Second Idea" which constituted an important addition to my proposal. The main feature of our design, as compared to the Zeldovich team's, was that the question of theoretical feasibility did not arise; there were also some essential engineering and technological differences. (After the 1953 thermonuclear test, the new design was further improved by the "Third Idea," of which I was one of the chief authors.)

Tamm supported the First Idea from the moment I told him about it in 1948; he'd been skeptical from the start about the earlier approach. At Tamm's suggestion, I paid a visit to the Institute of Chemical Physics, where I met with Zeldovich's deputy, Alexander Kompaneyets. Zeldovich, in addition to his duties at the Institute, had been associated with the Installation since its founding. He was spending much of his time there with the top-secret atom bomb group which he headed, since our first test of a fission device was in the offing.[9] Kompaneyets did not accept my ideas immediately, in part because he mistrusted my calculations. A week later I spoke directly with Zeldovich, who at once saw the merit of my proposal. It was our second encounter; we'd first met at a seminar where the discovery of a whole new family of elementary particles had been announced. Professor Alexander Shalnikov from the Institute of Physical Problems had asked sarcastically: "How much will each particle cost?" The speaker replied gloomily: "A lot." What he should have answered

[9][The first Soviet atomic explosion detected by the United States occurred on August 29, 1949.]

was "An infinite amount": all the particles were the product of experimental error, and so the divisor in the true cost equation would be zero.

After our second meeting, Zeldovich invited me back to his home, which wasn't far from the Institute of Chemical Physics. He introduced me to his family, light-heartedly remarking that the greatest blessing in life is a good-natured wife. His wife's smile seemed to me rather tense. We discussed both our projects at length and agreed that Tamm's group would concentrate on the new proposal, while his team would continue work on the earlier design, at the same time providing any help we might need, since there were still many gaps in our knowledge. Though he did not say so, I think Zeldovich decided then to ask for my transfer to the Installation, a request which had to be referred to the highest level. At one of our follow-up meetings, he questioned me about my family and my medical history. I was taken by surprise, but I guessed what was afoot. I told him that for all practical purposes I enjoyed good health, which was essentially true.

From the beginning, the Tamm group operated under conditions of strict secrecy, something we weren't accustomed to. Only members of the group could enter the room assigned to us, and the key was kept in the security office. All notes had to be made in special tablets with numbered pages. At the end of the working day, they were placed in a suitcase, sealed, and handed in for safekeeping. I suppose we were flattered at first by all the rigmarole, but it soon became routine—although it could, on occasion, become the cause of tragedy.

Once, some years later at the Installation, one of my colleagues sent a note with a problem to be solved to the Institute of Applied Mathematics, which performed numerical computations for us. A typist at the Institute apparently burned the sheet of paper when it was no longer needed, but she neglected to record its destruction. That prompted a visit from our Ministry's head of security to investigate the "extraordinary incident." The man's appearance alone, his fixed gaze from under drooping eyelids, evoked a physical dread in me. He had been chief of state security in Leningrad when some 700 ranking officials were executed there during the "Leningrad Affair" [the purge of the Party organization there in 1949]. He talked for an hour with the head of the Institute's security department, who spent the next day, Sunday, with his family, by all accounts cheerful and affectionate with his children. On Monday morning he arrived at work fifteen minutes early and shot himself before anyone else arrived. The typist was arrested and spent more than a year in prison.

In the fall of 1948, my salary was increased, and if memory serves me right, my status as a senior scientist was approved. Two months after the First Idea was accepted as the basis for our group's research, I was invited to meet with General Fyodor Malyshev, a state security official, who was the authorized representative of the Council of Ministers and Party Central Committee at FIAN. In point of fact, Malyshev represented the Beria machine: similar posts had been created in all scientific institutions conducting significant secret work, as well as in many other projects and enterprises; this was Beria's means of exercising direct control over all military research and development. Malyshev's small but impressive office was next to the security department. It contained a safe and the requisite number of telephones.

Malyshev complimented me on my work and then urged me to join the Party, saying that a Party member could do more for our people and for mankind in its progress toward a radiant future in which there would be no room for war; Party membership is not a privilege, not a guarantee of an easy life, but rather a great responsibility before the people and a readiness to serve the Party whenever and wherever needed; the compensation you receive is the feeling that you are part of a great cause. Malyshev offered to recommend me for membership.

I replied that I would continue to do everything in my power to ensure the success of our work, but I could not join the Party, because a number of its past actions seemed wrong to me and I feared that I might have additional misgivings at some future time. Malyshev asked me which actions I thought mistaken. "The arrest of innocent people and the excesses of the collectivization campaign," I replied. Malyshev argued: "The Party has severely condemned the mistakes committed during Yezhov's purges [in 1937–1938], and they have all been rectified. As for the kulaks, what were we supposed to do when they came at us with shotguns?"

Malyshev asked me to give very serious consideration to his suggestion. Had I agreed to join the Party, I imagine I would have been appointed to a major administrative post in the atomic research program, that of scientific director at the Installation, say, or something similar, and that wouldn't have done the program much good! What kind of an administrator would I have made?

Early in 1949, Tamm and I were summoned to the office of Boris Vannikov, head of the First Main Directorate of the Soviet Council of Ministers. (This was the provisional designation given the agency responsible for the entire atomic program; it had for some time exceeded a normal ministry in size. It was subsequently renamed the Ministry of Medium Machine Building, and the Committee on the Peaceful Uses of Atomic Energy was carved out of it.) Vannikov (not his original name, which was identifiably Jewish) was a very colorful personality. When I met him, he was no longer young; he had joined the Party in the early years and served it with distinction during the Revolution. Then, during the 1930s, when any slip could mean destruction, he gained considerable experience managing industrial and scientific projects for the military. With this background, it is no wonder that he was cautious, clever—

and cynical. He had been arrested during the war, but released after a week or so and appointed to a top post in the defense industry.

Vannikov received Tamm and myself in his spacious office. A man by the name of Nikolsky—presumably Beria's representative—was also present. After cracking a few jokes, Vannikov came to the point: "Sakharov should be transferred to work on a permanent basis with Yuli Khariton [meaning to the Installation, where Khariton was the scientific director]. It's necessary for the project."

Tamm became agitated and said I was a very talented theoretical physicist who could accomplish a great deal in key fields of science (he was so excited he forgot to say "*Soviet* science"); to limit me to applied research would be a great mistake, and not in our country's best interest. Vannikov seemed to be listening closely, a somewhat forced smile on his face. The direct Kremlin line rang. Vannikov answered and then tensed up. "Yes, they're here with me now," he said. "What are they doing? Talking, arguing." There was a pause. "Yes, I understand." Another pause. "Yes sir, I'll tell them." Vannikov hung up and said: "I have just been talking with Lavrenti Pavlovich [Beria]. He is *asking* you to accept our request."

There was nothing left to say. As soon as we left Vannikov's office, Tamm commented: "Things seem to have taken a serious turn." In reality, the serious turn had occurred a lot earlier.

the hour we were unpacking in the two rooms assigned to us until the cottage became available.

At first, like everyone else, we found it difficult to settle into a daily routine. It was a particular problem to find milk for the children. But things gradually fell into place.

8

FOUR SCIENTISTS

Tamm, Pomeranchuk, Bogolyubov, Zeldovich

IT WAS my destiny to meet four great theoretical physicists who would in their own ways and in different degrees influence my views and my work as a scientist and inventor. As is customary in memoirs, I will not essay detailed portraits, but will limit myself to the broad brushstrokes.

Igor Tamm played the largest role in my life, and was the only one of the four to influence my opinions on—or, more precisely, my fundamental approach to—social questions.

Tamm worked at the Installation from April 1950 until August 1953. This was the period of our closest collaboration, and I grew to know him in ways that would have been impossible in Moscow. We worked together uninterruptedly throughout the day, had breakfast and lunch together in the canteen, and ate supper and relaxed together in the evenings and on Sundays.

When Tamm moved to the Installation he was already fifty-five, and had many brilliant scientific achievements to his credit. (He was to make a further significant contribution with his work on isobaric resonances [or isospin], and after this came his heroic attempt to develop a nonlocal theory—his approach *seems* incorrect now, but who can be certain?)

Tamm came late to his scientific career; earlier, his activist bent and his socialist convictions had led him to concentrate on politics. At one 1917 congress, Tamm, then a member of the Mensheviks, had been the sole member of his party to vote in favor of immediate peace ("Bravo, Tamm!" Lenin thereupon exclaimed). During the civil war, Tamm had some narrow escapes, crossing the front several times on dangerous assignments. Only after these adventures did he begin to study science, influenced and greatly aided by Leonid Mandelshtam, whom he met in Odessa during the final phase of the civil war. Tamm talked about his life and many other things during evenings we spent in his hotel room or wandering along the deserted forest paths of the Installation (one of them was the local "Lover's Lane"). We would discuss the most sensitive questions: the repressions, the camps, anti-Semitism, collectivization, the ideal and real faces of communism.

Above, I emphasized that Tamm influenced primarily my *approach* to social questions: many of my specific opinions, especially now, differ markedly from his. I once heard Mikhail Leontovich remark with affectionate irony: "Despite everything, the member of the Elizavetgrad Soviet Executive Committee lives on in Tamm." That, of course, was only part of the truth. Tamm was capable of a critical self-analysis, and he frequently cursed his past follies. (Evgeny Feinberg, in his wonderful memoir of Tamm, relates one such incident concerning the Comintern's dogmatic position on the social democrats, which Tamm debated with Bohr in the 1930s.) What remains significant for me are the underlying principles by which Tamm was guided: absolute intellectual integrity and courage, willingness to reexamine his ideas for the sake of truth, and readiness to take action. Instead of brooding about the state of affairs within the confines of his own circle, he would relentlessly pursue his goals. In those early years, Tamm's every word seemed a revelation to me—he already understood so many things I was just beginning to notice, and he was more knowledgeable and astute about them than almost anyone else with whom I could talk freely.

Tamm had been imprisoned by both Denikin's counterintelligence and the Cheka.[1] (One of Tamm's fellow prisoners had a habit of reciting the pornographic verse of Ivan Barkov, which reinforced Tamm's existing aversion to that sort of literature.) His survival was apparently due to chance. The Chekists were executing half a dozen prisoners every morning, but Tamm's number never came up; he was freed on Felix Dzerzhinsky's orders. The Chekist commander who personally released him remarked in evident frustration: "But all the same you're a White Army spy!" When asked what basis he had for this accusation, he showed Tamm a school photograph of Tamm's wife-to-be which had been confiscated during a search. The photograph was inscribed: "We're all your agents." In the 1930s, Tamm was saved once more by a stroke of luck, and by the fact that after quitting the Mensheviks he had not joined any other party, not even the Bolsheviks. By then, he had established a scientific reputation in the USSR and abroad, and that may also have been a helpful factor.

[1][General Anton Denikin commanded the anti-Bolshevik forces from 1918 to 1920. Cheka is the Russian acronym for the Extraordinary Commission for Fighting Counterrevolution and Sabotage, established by the Bolsheviks on December 7, 1917, and first headed by Felix Dzerzhinsky. Members of the KGB—the Cheka's present-day successor—are often called *chekisty*.]

We talked a great deal about the repressions of the 1930s. Tamm recalled how one of his favorite students, Semyon Shubin, would repeat the stock line: The NKVD doesn't arrest people for no reason; I've done nothing anti-Soviet, so they won't touch me. (Such words were spoken by so many people at the time. What lay behind them? Blindness? Hypocrisy? Self-deception as a means of surviving psychologically in an environment of universal terror? The sincere delusions of doomed fanatics?) Tamm and Shubin's final argument began one evening in 1937, and it lasted almost until dawn. The next day Shubin was arrested, and he died not long afterward in a camp. Inquiries as to the cause of his death elicited the answer (any response at all was a rare concession) that Shubin had died from a "chilling of the epidermal integument." Many other physicists, including Alexander Vitt and the brilliant young theoretical physicist Matvei Bronshtein,[2] were arrested and perished in the 1930s.

JUST ABOUT the time we moved to the Installation, a campaign against "kowtowing to the West" was unleashed from above and roared through the press, scientific and cultural institutions, and the academic world. Russians were advertised as the first to have discovered or invented anything and everything. "Russia, homeland of the elephant," ran the stock joke. But the campaign didn't always run smoothly. In one instance, a biography of Admiral Alexander Mozhaisky, who was supposed to supplant the Wright brothers as the inventor of powered flight, was rushed into print so fast that his portrait and his achievements were confused with those of his brother.

The battle against "kowtowing" fused with an anti-Semitic campaign, thinly veiled as "anti-cosmopolitanism." Vannikov, a Jew himself, entertained his high-ranking colleagues with anti-Semitic jokes. "If you don't want to be known as an anti-Semite," he remarked, "say 'cosmopolitan' when you mean 'kike.' "

Tamm's opinion on this subject was categorical; he voiced it often and with passion. For him there was no "Russian"—let alone "Soviet"—science any more than there was "American" or "French" science: science is universal. It is a vital part of the world's cultural heritage, worth pursuing as an end in itself, but it also offers mankind hope for a better future. As for anti-Semitism, Tamm declared that there was "one foolproof way of telling if someone belongs to the

[2]Bronshtein's work on the quantization of weak gravitational waves and on the stability of photons remains significant. In his last paper he argued that the "tired light" hypothesis [a now discredited theory which asserted that photons lose energy because of their interaction with intergalactic matter en route to earth] was not the correct explanation for the red shift phenomenon observed in cosmology.

Russian intelligentsia. A true Russian *intelligent* is never an anti-Semite. If he's infected with that virus, then he's something else, something terrible and dangerous."

In the fall of 1956, I asked Tamm what he thought of Khrushchev (adding that I admired him very much since he was so different from Stalin—our conversation took place some months after the 20th Party Congress). With no hint of amusement at my enthusiasm, Tamm replied, Yes, he liked Khrushchev, and of course he was no Stalin—but it would be better if he were even *less* like Stalin! The uprising in Hungary took place that October, but I don't recall discussing it with Tamm; our meetings were becoming less frequent.

In 1968, I published *Reflections on Progress, Peaceful Coexistence, and Intellectual Freedom.* Tamm, by then seriously ill, was skeptical about my ideas, and especially that of "convergence." He remained faithful to the ideals of his youth, to a belief in a pure, undistorted socialism as the only means of resolving mankind's problems and ensuring general happiness. He held back from any discussion of ways to prevent a nuclear or ecological catastrophe in a divided world, but he did acknowledge that I'd posed some critical questions.

Whatever our differences, they never altered the respect and deep affection, even love, that Tamm and I felt for each other. I recall with pride that he asked me to stand in for him in 1968, when he received (along with Powell of Great Britain, discoverer, with Lattes and Occhialini, of the pi-meson) the Academy's highest scientific award, the Lomonosov medal. A recipient traditionally delivers a lecture after the award ceremony, but Tamm, by then kept alive only with the aid of a respirator, was unable to attend. He did write out his Lomonosov lecture and discuss it with his students, myself included. Typically, it was devoted not to his past achievements, but to new scientific ideas that excited him. It was with great emotion that I read his lecture from the podium at the Academy's General Assembly.

IN AUGUST 1968, Soviet tanks rolled into Prague, shocking many in the USSR and abroad. I can't recall now who proposed to Tamm that he add his name to a collective letter of protest. He did sign, but later withdrew his endorsement at the urging of a colleague and favorite student, who argued that this was necessary to protect the theoretical department at FIAN, to which Tamm had devoted his life. I was sorry to see this happen, for I believe that Tamm's signature would have been of enormous significance and would also have given him the satisfaction of adding another valiant deed to a glorious life. The concern for the future of FIAN's theoretical department seemed grossly exaggerated, but people continually justify their failure to act in times of crisis by arguments of this sort.

I HAVE DESCRIBED my feelings about working on nuclear weapons during the period 1948–1956, but I can't speak with the same degree of certainty about Tamm's attitude. I don't recall any conversation with him that really got to the bottom of the issue; I simply assumed that his views on the matter were similar to mine. Tamm once told me about Pyotr Kapitsa's refusal to participate in the development of nuclear weapons. [See p. 302 for Sakharov's conversation with Kapitsa on this subject.] When Kapitsa was called to come to Beria's office, he replied that he was extremely busy with scientific work, but if Beria needed to speak with him, he was welcome to visit the institute. In telling me this, Tamm didn't seem to be praising Kapitsa for his courage; on the contrary, he said something to the effect that "Of course, Beria was a far busier man than Kapitsa." I took this remark at face value as a criticism of Kapitsa, since I then saw Beria as no more than a part of the state machine, engaged like the rest of us in "a project of the utmost importance." And it seemed to me that Tamm shared my opinion. Now I am almost certain that Tamm was being ironic; he may have overestimated my ability to catch his drift.

Around that time, Zeldovich asked me: "Do you know why it was Tamm and not Dau [Lev Landau] who proved so useful to the project?" Zeldovich answered his own question: "Because Tamm has higher moral standards!" (meaning a readiness to devote himself singlemindedly to "the cause"—to my mind, neither the whole story, when it came to Tamm's stance, nor a completely sincere statement on Zeldovich's part).

I know little about Landau's own thoughts on the subject. Once, in the mid-1950s, I paid a visit to the Institute for Physical Problems, where Landau headed the theoretical department and also a separate team performing calculations for nuclear weapons. After we'd finished our business, he and I took a stroll in the garden. It was the only private and candid conversation we ever had. Landau said: "I don't like any of this" (meaning nuclear weapons in general, and his involvement with them in particular).

"Why not?" I asked, perhaps a little naïvely.

"Too much noise."

The toothy grin that so often lighted up Landau's face was missing on this occasion; he appeared quite grave and melancholy.

DURING THE YEARS we spent together at the Installation, Tamm and I naturally had many discussions about science. He was fond of saying that all areas of knowledge interested him—with the exception of philosophy and law.

I completely agreed with him about law; despite my later personal involvement in that murky subject, I've never really accepted it as having anything to do with the real world. As for philosophy, I suspect that Tamm had in mind dogmatists and those who, as Feynman put it, "flit around" the edges of science. He was hardly dismissing the great philosophers of the past, or the present-day contributions of sophisticated philosophical analysis.

Tamm often talked about biology in those days. I was in complete accord with his negative opinion of Lysenkoism (and of Lepeshinskaya, Boshian, and Bykov,[3] who were much in the news at the time). But I didn't share his view that a satisfactory explanation of the phenomenon of life would require entirely new principles in biology, possibly in physics as well—a break with the past comparable to the emergence of quantum mechanics. I argued that stereochemistry (with its "lock-key" principle), augmented by electrochemistry, was adequate to "explain" the origins of life (just as a primitive alphabet can be used to express the most complex notions). I consider that the scientific discoveries of recent decades, beginning with the breaking of the DNA code, tend to confirm my opinion. Yet the structures now being uncovered have proved infinitely more complicated, varied, and intricate than anyone could have imagined thirty years ago. So much remains unclear, so many crucial questions must still be formulated accurately, so many details will have to be resolved.

Tamm was convinced that the main thrust of science would shortly switch from physics—the cutting edge of progress in the first half of the twentieth century—to the life sciences. I agreed with him in this and, indeed, the proportion of intellectual and material resources then devoted to the life sciences was utterly inadequate and did not correspond to their practical and theoretical value. Now they have been accorded a greater priority, but the exact sciences have not surrendered their position in the front rank; interest in them has not diminished as they continue to produce unexpected discoveries of enormous significance. In choosing their specialties, young scientists should be guided by their own inclination and their instinct for what is new, something that is mysteriously reborn in every generation.

In the 1950s Tamm used to say that if he had to choose his specialty over again, he would pick biology. I never took this literally, for fundamental physics was his true passion: it both tormented him and gave his life meaning. A few years before his death, at a time when he was already seriously ill, he declared that his dream was to live to see (and to be mentally capable of understanding)

[3][Influential authors of sensational but scientifically unsound biological theories: Lepeshinskaya's "life substance," Boshian's "discovery" of virus-to-bacteria transformation.]

a new theory of elementary particles, one that answered all the "accursed questions." (Living to see answered such questions as the secrets of the human brain, the differentiation of embryonic cells, and the origin and evolution of life wasn't mentioned.)

Evgeny Feinberg has remarked that if Tamm hadn't concentrated on the most difficult problems on the frontiers of physics, he could have done valuable work elsewhere simply by virtue of his erudition, his professionalism, his phenomenal capacity for work, and his talent for exact mathematical calculation. This is evident from his work on magnetically confined fusion [see Chapter 9], from all his applied work, and from the papers he wrote during his rare periods of "scientific depression" when he became discouraged by his failures on the cutting edge of physics.

Indicative of Tamm's true passion was the work of his final years, in which, on the basis of Snyder's ideas, he sought to construct a theory with curved momentum space. The work demanded an immense effort on the part of Tamm, whose life by then was entirely dependent on the use of a respirator. Others in his situation might have sunk into apathy or despair and given up the ghost. The thrust behind this last undertaking was his conviction that the theory of renormalization—then regarded as the definitive solution to the problem of "ultraviolet divergences"—was in fact no more than a temporary and partial solution, or at best, a phenomenological one at low energies. Only a few people (among them Dirac) were of this opinion, especially before the discovery of the "Moscow zero." It seems to me that Tamm was correct in principle and incorrect only in expecting too much from his theory of curved momentum space. Nowadays great hopes are invested in gauge supersymmetry theories, and especially in "superstrings." But the matter remains far from resolved.

LET ME RETURN to the story of our life in the 1950s. Tamm, Romanov, and I normally ate breakfast and lunch together. Tamm would report any news (politics, sports, or odd items of interest) he had picked up from foreign radio broadcasts. He listened regularly to BBC programs in English and Russian, something very few people did at the time. It was Tamm who told us of Hilary and Tenzing's ascent of Everest in 1953. (I thought of this recently when Tamm's son, Evgeny, led a Soviet expedition to Everest. Back then, Tamm cursed himself for whetting his son's interest in mountain climbing, a dangerous passion.) But what mattered was not so much the content of Tamm's reports as his approach to the news (as to everything else): intelligent, passionate, and open-minded.

He never let us get down in the dumps. He himself was enthusiastic and sociable, and he encouraged us to have fun in our spare time. We played chess in the evening, sometimes with four players, or without looking at the opponent's pieces, or in some other variation. Tamm taught us the Chinese game Go and another game, Taking the Stones, which can be solved using an algorithm based on the "golden section" (we racked our brains over that one!).[4]

We skied and went hiking, and in the summertime we swam. (I was hopeless in the water, but Tamm tactfully spared me unnecessary embarrassment.) Pavel Guryanov, our driver, joined in these activities on an equal footing with the rest of us; in Tamm's company this seemed perfectly natural, but later I saw bosses treat subordinates in an entirely different fashion.

Guryanov once saved Tamm's life, and mine, when an army truck passing on a narrow, winding road came hurtling at us. With the split-second reactions of a former tank driver, Guryanov avoided a head-on collision by swerving onto the sidewalk, threading his way among pedestrians. Sadly, he later took to drink, and was sent to work as a railroad engineer.

Tamm had money problems for the greater part of his life. His Stalin Prize helped, but he earmarked a portion of the money for talented individuals in need. He would ask people to identify potential recipients (who were never told where the money came from). I wish this idea, or something like it, had occurred to me. I only learned of Tamm's generosity after his death.

I agree completely with what Evgeny Feinberg wrote in his memoir of Tamm:

> In late 19th century Russia there existed something of fundamental importance—a solid, middle-class, professional intelligentsia which possessed firm principles based on spiritual values. That milieu produced committed revolutionaries, poets, and engineers, convinced that the most important thing is to build something, to do something useful. That was the milieu which produced Igor Tamm, and he shared its virtues, and its shortcomings. Perhaps most important of all was his independent spirit in matters large and small, in life and in science. . . .[5]

Tamm's wife, Natalya Vasilievna, possessed similar virtues. Things were probably not always easy for her—life, after all, is a tricky thing. Once, in an attempt to allay the doubts (absolutely groundless) that tormented Klava, Natalya told her: "A man's love is often inconstant: it can fade and almost

[4][In Taking the Stones, there are two piles of stones. Each player in turn must take an equal number of stones from each pile or else any number of stones from one pile. The player who takes the last stone loses.]

[5][Evgeny Feinberg, ed. *Reminiscences about I. E. Tamm* (Nauka, 1987).]

vanish, but the flame returns." I have only Klava's report of this conversation and I can't be certain that Natalya was referring to her own marriage, but her words are evidence of considerable experience and a kindly wisdom. In the many years they spent together (Natalya survived Tamm by nine years), she always supported him, both in his moments of triumph and during the periods of depression to which active and sensitive individuals are prone.

Much has been written about Tamm, but I hope I have managed to add a few brushstrokes to the existing portraits of a man who played a major role in my life. Perhaps the great fortune of my early years was to have had my character molded by the Sakharov family, whose members embodied the generic virtues of the Russian intelligentsia described by Feinberg, and to have then come under the influence of Igor Tamm.

ISAAK (YUZIK on his passport) Pomeranchuk was a completely different sort from Tamm, but another uncommonly fascinating individual. He was extremely upset by his assignment to the Installation in the summer of 1950: we were tearing him away from his important work on elementary particles and field theory—and from his young wife. He had just remarried and was very much in love. His previous wife or wives (there may have been more than one) had left him in short order. This time the story was reversed: he'd stolen the wife of a general. Night after night he'd waited beneath her window, hoping for an occasional glance from behind the curtain. (Gossip based on hearsay; but it accords well with Pomeranchuk's character.)

I recall his pacing the small yard of his cottage, ruffling his black hair, and humming a song that ran something like this: "I grew up and blossomed before seventeen, then a girl in love starts wilting." (I presume he was casting himself as the "girl" in that song.) He said to me once: "You know, I guess I'm just an old-fashioned man, for whom such odd notions as love are most important."

In spite of his emotional distractions, Pomeranchuk quickly and brilliantly solved the problems in theoretical physics Tamm and I assigned him. He was a virtuoso of theoretical technique, and knew many methods that were new to me. And yet he regarded this work with utter disdain. Someone told me that he'd once grabbed the director of a large physics institute by the lapel and asked him: "Do you have a 600 MeV accelerator? . . . You don't! Then you're a building superintendent, not a director!"

This was not a pose, but an expression of Pomeranchuk's true character and his consuming passion. He believed that the fundamental laws of nature could be discovered in their "naked" undistorted form at very high energies. The problem was simply to determine the energy level necessary, and then conduct your experiments with elementary particles at those levels. The progress of science over the next thirty years confirmed his intuition, but Pomeranchuk didn't live to see these developments: he died in 1966. (And none of us is likely to live long enough to see all the important questions answered either.)

After Pomeranchuk had moped around the Installation for a few months, the administration realized it would be better to let him return to his own work and to his wife.

In the 1960s, Pomeranchuk's zeal was rewarded when he made a number of fundamental discoveries in high-energy physics. Justice triumphs—sometimes. One of his successes—the well-known "Pomeranchuk's theorem"—states that the cross sections of collisions between a particle and a target and an anti-particle and the same target become asymptotically equal at high energies. His name is also associated with the Regge trajectory with zero quantum numbers. But this, of course, is only the tip of the iceberg, a mere token of his accomplishments. He spent much time working with talented students—Vladimir Gribov, Lev Okun, Boris Ioffe, Karen Ter-Martirosian, and Igor Kobzarev among others—and enjoyed his collaboration with them.

I saw Pomeranchuk quite often during his last years, when he was trying to return to fundamental science. He was still on fire with scientific projects, and I remember his excitement and his doubts about quarks, the elementary particles posited by Gell-Mann and Zweig. Pomeranchuk's wife had died, and he himself was suffering from cancer of the esophagus. A wise and sympathetic doctor, the late Professor Kassirsky, who knew his patient well, advised Pomeranchuk to make liberal use of painkillers if he wished to live out decently what remained of his life. He heeded this advice, and as a result was able to go on working until the day he died.

Just before his death, he discussed with his students their last joint article, on scaling. Björken's well-known paper on the same subject had appeared at about the same time, followed by Feynman's work. All three articles had been born of the spectacular results obtained from the giant Stanford Linear Accelerator. Pomeranchuk was still on the cutting edge.

The last time I saw him, he was terribly ill and emaciated. With grim humor, he told me that he now confined his walks to the nighttime so people wouldn't be scared at the sight of him. Apart from that one remark, he spoke only of science. Pomeranchuk is remembered by all who knew him as the shining knight of theoretical physics.

I FIRST HEARD Nikolai Bogolyubov's name in 1946. Akiva Yaglom, a member of my school's math club and later my classmate at Moscow University, told me that an exceptionally gifted young physicist had come from Kiev, a stray pup overflowing with so many ideas that he was handing them out right and left.

Later, at FIAN, I attended a remarkable lecture by Bogolyubov on superfluidity. Of course, it was just a model theory, and, moreover, one that relied on perturbation theory, but it was the first theoretical investigation that derived the surprising phenomenon of superfluidity from first principles, without relying on a specially postulated spectrum of elementary excitations. Unfortunately, certain scientists did not appreciate his approach, and Bogolyubov, to say nothing of his students and associates, engaged in some rather dubious conduct during the squabbles that followed. Ten years later, however, when articles on superconductivity by Bardeen, Cooper, and Schrieffer appeared, Bogolyubov had a suitable theoretical framework ready, and he capitalized on it brilliantly.

He strengthened the mathematics department at the Installation by finding a new director to replace Agrest, and recruiting a large team of talented associates. He also did some work on Installation topics that coincided with his own interests, and on these occasions proved to be a topnotch problem-solver. But he had no interest whatever in technical, design, or experimental work.

Once, when Bogolyubov happened to attend a meeting on engineering problems in Khariton's office, he left with a baffled expression on his face and was heard to say, half in earnest and half in jest, that he had "got tangled in the nuts and bolts." That expression became proverbial with us. Bogolyubov made no secret of the fact that he devoted most of his time to topics that had nothing to do with the Installation—much later I did the same thing myself—and to writing monographs on theoretical physics. This was the principal reason he brought Klimov, Shirkov, and Zubarev to the Installation. He achieved his greatest success with Dmitri Shirkov, the youngest of the three; their joint monograph on quantum field theory was universally and justly acclaimed. Bogolyubov also produced an excellent monograph in collaboration with Zubarev, but Klimov and Bogolyubov did not hit it off; after Bogolyubov left the Installation, I took Klimov into my department.

Tamm and I got along well with Bogolyubov after working hours. We would occasionally drop by his hotel room, where he welcomed us and offered us "what God has provided" (and He provided some very tasty things). Bogolyubov would pace the room, talking and gesticulating. I always found our conversations interesting even though we never touched on sensitive subjects. Bogolyubov was erudite in the most diverse fields, had an excellent command of several languages, and possessed a sharp, original mind and a sense of humor. Half-jokingly, he predicted that in short order my chest would be so thickly covered with medals there wouldn't be room for them all. It was he who first drew my attention to cybernetics and the work of Norbert Wiener, Claude Shannon, and John von Neumann (this supplied ammunition for my arguments with Tamm about the nature of life), and to the enormous potential of computers

Bogolyubov quit the Installation at the same time as Tamm, after the 1953 test. We met only sporadically after that, even though we lived on the same floor of a building in Moscow for a while.

It seems to me that the years Bogolyubov spent in relative isolation at the Installation laid the foundation for his outstanding work on quantum field theory and elementary particles in the 1950s and 1960s; those achievements are widely known and there's no need to discuss them here.

Bogolyubov has many disciples among physicists and mathematicians, including both genuine scientists and those he has just chosen as his sycophants. He heads the theoretical physics and mathematics departments at several institutes and has become something of a scientific "general." What he needs all that for is beyond me, but "status" is apparently an integral part of his style and his sense of well-being. I prefer to remember how his expression would light up with excitement at word of a new development in science, and the spate of ideas he would immediately generate.

MY LONGEST RELATIONSHIP—spanning four decades—was with Yakov Zeldovich. I write of him with mixed feelings. He played an important role in my work in the 1950s and an even greater one in my theoretical work of the 1960s. For many years I valued our friendship highly, regarding it as close and congenial. When Lusia and I were beginning our life together in 1971 and she asked me who my friends were, I answered: Zeldovich. Even now, I don't doubt he was sincere when he telephoned that same year on my fiftieth birthday to say that he loved me. All the same, in retrospect, I detect a touch of the "operator" in the way he behaved on certain occasions. And in the 1970s and 1980s, some of his actions (or failures to act) were not friendly at all.

Zeldovich was seven years older than I. I don't know much about his family background; I believe his father was an accountant. When we first met, Zeldovich occasionally wore a hat inherited from him: round, green-tinged, with a brim—it reminded me of turn-of-the-century photographs of Jewish life in the Pale of Settlement. It's my impression that his parents lived in straitened circumstances. He never spoke of his childhood and youth, although once he mentioned an "inferiority complex" he claimed to have overcome (but, who knows, perhaps he spent his whole life battling it). He was short in stature, but had been exceptionally strong as a youth.

Zeldovich was not a university graduate; he was, in a sense, self-educated. He worked as a laboratory assistant in several scientific institutions in Leningrad, after moving there from Belorussia around 1930, and published his first papers at the age of seventeen, highly original work, primarily in physical chemistry. His formula pertaining to surface phenomena is well known, and his work on the kinetics of chemical reactions contains the seeds of a theory for a chemical chain reaction. His reputation soon earned him a master's (*kandidat*) degree and then his doctorate (his dissertation concerned the production of nitrogen oxide from fuel gas) without his ever bothering about a bachelor's degree.

The physics of combustion, of explosions, and of other chemical phenomena continued to engage him throughout his life, but his scientific interests kept expanding, and he was invariably among the pace-setters in new fields. He worked on fission chain reactions and atomic technology, jet propulsion, and thermonuclear weapons, then made an abrupt switch to elementary particles, and finally turned his attention to cosmology and astrophysics. Few scientists are competent in such a wide range of fields. As a sideline, Zeldovich published survey articles and monographs, as well as his very interesting *Mathematics for Beginners*. Of course, he wrote most of his books with coauthors, but his hand can be felt in all of them, and they reflect his ideas. (He later found himself at odds with some of his coauthors; it's difficult to say who was to blame.)

Among Zeldovich's many published works (I shall mention only a few highlights of his distinguished and prolific career), his papers written with Khariton and published in 1939 and 1940 on the theory of fission chain reactions were landmarks. During the war Zeldovich worked on jet propulsion, and in 1945 he was sent to Peenemünde to gather information on the Germans' V-2 rocket program. He traveled in the uniform of a Soviet army captain, and was once invited to dinner by the KGB boss of the Soviet zone, who in effect exercised power over half of Germany. Zeldovich recalled their meeting with some fear, but also with a tinge of admiration, a sin of which we were all a bit guilty at that time.

In the 1950s, his best-known work on elementary particles was the article he coauthored with S. Gershtein in which they introduce charged current and formulate the law of conservation of vector current. This paper anticipated the idea of "current algebra" and provided a basis for formulating the theory of weak interactions. But Zeldovich and Gershtein failed to take the final, decisive step—the introduction of a parity-breaking operator in the interaction of currents. This was left to Marshak and Sudarshan, Gell-Mann and Feynman. But what would appear to be the final theory of weak interactions was not constructed until much later, by Glashow, Weinberg, and Salam. ["Final" is too strong a word. Much remains unknown: the masses and other properties of neutrinos, the mechanism of CP-violation, etc.—A.S. 1987] I shall deal in Chapter 18 with Zeldovich's work on cosmology and the theory of elementary particles in the 1960s, which served as a stimulus and starting point for my own research into those fields.

* * *

I GREW CLOSER to Zeldovich when I was transferred to the Installation in 1950. We worked in adjacent offices. (In the early days, all of us had to share offices; I shared mine with Tamm and Romanov.) The cottages in which we lived were always close by. In 1949–1950, Zeldovich lived with the Zababakhins; his room, actually an enclosed terrace, was nicknamed "Storage for a Corresponding Member of the Academy of Sciences." We visited each other's offices several times a day, to share a new idea, discuss a problem, or simply joke and talk. Certainly we discussed serious scientific matters, but we also amused ourselves with "brainteasers" in mathematics and physics, competing to find the fastest and most elegant solution. It never entered my mind that we might engage in any form of rivalry other than this sort of battle of wits.

One evening in the spring of 1950 on my way home from work, I caught sight of Zeldovich. The moon was out, and the bell tower cast a long shadow on the square in front of the hotel. Zeldovich was walking deep in thought, his face somehow radiant. Catching sight of me, he exclaimed: "Who would believe how much love lies hidden in this heart?"

In many respects, the Installation was a big village in which nothing remained secret. I knew Zeldovich was having a love affair with Shiryaeva, one of the prisoners, an architect and artist by profession. Her husband had renounced her after her arrest on charges of anti-Soviet slander; such stories were common in those days. It was Shiryaeva who had painted the murals in the VIP dining room, in our theatre, and in the homes of the Installation's bosses. She had been granted trusty status, apparently as a reward for her services.

A few months after our encounter in the moonlight, Zeldovich woke me in the middle of the night. Romanov, in the other bed, looked up for a moment, but then turned over and remained silent: he never asked questions. Zeldovich was agitated. Could I lend him some money? Fortunately, I had just been paid, and I gave him everything I had. A few days later I learned that Shiryaeva's term had expired, and that she was being sent to Magadan, far to the east, for "permanent resettlement." Zeldovich managed to get the money to her, and after some months I learned from him that Shiryaeva had given birth to their daughter in a building where the floor was covered in ice an inch thick.

Zeldovich managed to obtain some improvement in Shiryaeva's situation, and twenty years later, at a conference in Kiev, he introduced me to Shurochka, the daughter born in Magadan. She looked amazingly like his other daughter—by his wife, Varvara Pavlovna. (Zeldovich had a number of other affairs—too many—most of them strictly sexual liaisons. I don't like some of the stories I've heard.) He dreamed of someday bringing all his children together. I hope he succeeded. Time is a great healer, provided there's complete honesty.

IN THE MIDDLE of 1950, a commission visited the Installation to check up on senior scientific personnel. We were all called in, one at a time. Among other questions, I was asked what I thought of the chromosome theory of heredity. (After the Academy of Agricultural Science's meeting in the summer of 1948 and Stalin's endorsement of Lysenko, belief in Mendelian genetics was regarded as an indication of disloyalty.) I replied that the theory seemed scientifically correct. The commission members exchanged glances but said nothing. Evidently, my position and reputation at the Installation disposed them to overlook my sins.

A couple of weeks later, Zeldovich came to me and said that we had to help Lev Altshuler, the head of one of the experimental departments. He was a longtime acquaintance of Zeldovich and had played a major role in the development of fission charges and in research on physical processes at high pressures. It turned out that Altshuler had been asked the same question by the commission and, with his typical forthrightness, had given the same answer I had. He, however, was threatened with dismissal.

"Zavenyagin is at the Installation," Zeldovich informed me. "Andrei Dmitrievich, if you appeal to him on Altshuler's behalf, they might leave him alone. I've just been talking with Zababakhin. It would be best if you went together with him." Within the hour, Zababakhin and I were in the director's office, where Zavenyagin received us.

At the time, Avraami Zavenyagin was nominally Vannikov's deputy, but since Vannikov spent a lot of time on matters of state outside the bailiwick of the First Chief Administration, in practice Zavenyagin decided many matters on his own. He was from "Ordzhonikidze's team,"[6] and at one time had been director of the Magnitogorsk industrial complex. He came under attack in the 1930s but was not arrested; instead, he was sent to run the Norilsk construction conglomerate in Siberia. The town and the ore-processing facilities were built from scratch in the tundra by zeks working in conditions of permafrost, blizzards, and, for most of the year, polar night. It was impossible to escape, but desperate convicts would occasionally try to do so. A pair of professional criminals would find a "pigeon" to take along with them; if their supplies ran out, they could kill and eat their companion (and I don't think this was just a tall tale). The mortality rate at Norilsk was almost as extreme as at Kolyma: the temperature in the mines was a bit higher, but still below freezing. After Zavenyagin's death in 1956, the Norilsk mining and smelting complex was named in his honor.

[6][Grigory Ordzhonikidze was Minister of Heavy Industry in the 1930s.]

Zavenyagin was a tough, decisive, exceptionally enterprising chief. He heeded the opinions of scientists and understood their role in the project. He made some attempt to study the concepts involved in our work, and from time to time would come forth with technical solutions, usually quite sensible. He was a man of great intelligence—and an uncompromising Stalinist. He had large, black, melancholy Asiatic eyes, a reminder of his Tatar ancestry. After Norilsk, he always felt cold, and he wore a fur coat draped over his shoulders even in a warm room. Surprisingly, given his background, there was a gentleness apparent in his relationships with some people— I was later to find myself included in this select group. He held the rank of lieutenant general in State Security; behind his back we called him "the General" or "Avraami."

I sometimes wonder what motivates such people. Ambition? Fear? A thirst for action and power? Conviction? I have no answers. Back in 1950, we simply accepted Zavenyagin as a top official.

He heard us through and then said: "I'm aware of Altshuler's hooligan conduct. You say he's done a lot for the Installation and he'll be useful in the future. Fine. We won't take official action now, but we'll watch how he behaves."

Then, before dismissing us, Zavenyagin asked how the work was going in our department. He was pleased that we knew all the figures by heart and remarked that Beria's favorite method for checking a person's professional competence was to ask him for facts and figures.

Things turned out well, but now, all these years later, I ask myself why Zeldovich didn't go himself to see Zavenyagin, or at least accompany us. Possibly he feared that Zavenyagin was aware of his personal relationship with Altshuler and would discount his plea, but it seems more likely that this was an example of Zeldovich's tendency to hide behind others when trouble was in the offing, even relatively insignificant trouble. The pattern was repeated later when he asked me to write a letter about literary matters, and to speak out in defense of a mutual acquaintance of ours who was under arrest, a story I'll tell later.

The fact that Zeldovich was a Jew undoubtedly had something to do with all this; perhaps he felt vulnerable and feared his interventions would be ineffective. But I know Jews and members of other nationalities who, lacking Zeldovich's standing and virtual immunity, still behave very differently, and are able to look beyond their group affiliation in matters of civic responsibility. There may not be many of them, but they do exist.

Up to a certain point, I was inclined to regard these aspects of Zeldovich's behavior as minor failings. Everyone does what he can, and Zeldovich did a great deal in his professional work and in popularizing science and introducing young people to it. He and I discussed social issues, though I don't know whether he was always candid. But since I myself was often slow to appreciate the true state of affairs, why should I assume that others understood everything and were engaged in deliberate deceit? And yet, could Zeldovich have been serious when he claimed to like *The Morning of Our Motherland*, a painting that depicts Stalin standing before a blue-green background of kolkhoz fields and construction sites? It is, of course, possible that he was sincere. . . .

Still, Zeldovich could be an interesting conversationalist who spoke with intelligence, sincerity, and emotion. It was at his home that I first saw certain samizdat publications, including Tvardovsky's *Tyorkin in the Other World* and Akhmatova's "Requiem." I often felt a certain warmth in Zeldovich's relationship to me—in both word and deed. That made all the more bitter some of his actions in the 1970s and 1980s.

In 1973, Zeldovich telephoned me after a tendentious newspaper article denounced a letter I'd written with Alexander Galich and Vladimir Maximov in defense of Pablo Neruda [see p. 389]. Lusia answered the phone and said: "We're so happy; my daughter Tanya had a little boy!" Zeldovich interrupted her: "You'd better look after your other 'little boy'!" When I came to the phone, he attacked me in such clichés that I couldn't take him seriously. Two years later, after my Nobel Peace Prize was announced, Zeldovich phoned once again and urged that I refuse the award, calling it a provocation. He sounded like the newspaper *Trud*, which had termed the award "thirty pieces of silver" (and had also made an innuendo about my wife's Jewish extraction). In both instances Zeldovich must have known that my telephone was tapped. He followed up his 1975 call with a letter, again surely aware that the KGB reads all my mail.

I don't understand why Zeldovich felt obliged to make such a display of his loyalty to the regime and to demonstrate my isolation. I thought I had the right to count on him—and other colleagues whose positions shielded them from reprisals—to defend my rights after I was illegally exiled to Gorky in 1980. In 1981, I wrote to Zeldovich and Khariton, asking them to intercede (privately, not publicly) on behalf of my stepson's fiancée, Liza Alexeyeva, who had become a hostage because of her link to me. I made plain my distress and how much I was relying on their help; Khariton did not respond at all, while Zeldovich cited the shakiness of his own situation in refusing to intervene, complaining that he wasn't allowed to travel any further than Hungary! This was the excuse offered by an academician, a three-time Hero of Socialist Labor, a man who had never made good use of the considerable power that comes with such status. And that status, I am convinced, was very secure indeed. In essence, I was asking him to do no more than what I'd done in the Altshuler affair. (Whether his intercession would have helped is another matter.)

* * *

THE PRECEDING description of my complicated, contradictory relationship with Yakov Zeldovich was written in 1982. The bitter taste left by Zeldovich's inaction in Liza's case and by other unpleasant incidents lingered on and colored the tone of my account. Now, I would like to take a more tolerant view of a complex personality. Not long ago, Zeldovich came up to me at an Academy of Sciences meeting (in a rush, as always) and said: "A lot happened in the past. Let's forget the bad: Life goes on!" Of course it does. . . .[—A.S. 1987]

ON DECEMBER 2, 1987, Zeldovich died of a heart attack.[7] Any chance of our meeting and talking again was lost. The petty and superficial aspects of our relationship have faded; what remains is his enduring, truly immense contributions to science. And all those whom he helped to enter the realm of science.

At times I still catch myself carrying on a mental dialogue with Yakov Borisovich on scientific matters. . . .[—A.S. 1988]

[7] [Sakharov was listed among the scientists and Party officials who signed Zeldovich's obituary, which appeared in *Pravda* on December 5.]

9

THE MAGNETIC THERMONUCLEAR REACTOR

MY WORK WITH Igor Tamm on the problem of a controlled thermonuclear reaction belongs to the early period at the Installation, 1950–51.

Although these questions were already on my mind in 1949, as yet I had no concrete, intelligent ideas. Then, in the summer of 1950, Beria's secretariat sent us a letter from Oleg Lavrentiev, a young sailor in the Pacific Fleet, who noted the importance a controlled thermonuclear (i.e., fusion) reaction might hold for future energy production and then offered a proposal to create a high-temperature deuterium plasma by means of electrostatic confinement. Specifically, he proposed that two or three metal grids be used to surround the reactor volume. An electric charge of several dozen KeV applied to the grids would create an electrostatic field that would deflect deuterium ions and electrons and prevent them from escaping the reactor. I wrote back that Lavrentiev had raised an issue of immense significance, and had displayed initiative and creativity that merited all possible support and aid. His specific plan, however, struck me as impracticable: there was no way to ensure that the hot plasma would not come into contact with the grids, which would inevitably result in enormous heat loss and render such means incapable of attaining sufficiently high temperatures for thermonuclear reactions. I probably should have mentioned that Lavrentiev's idea might prove fruitful in conjunction with other ideas, but at the time I had nothing specific to suggest.

My first vague thoughts on magnetic rather than electrostatic confinement occurred to me as I read Lavrentiev's letter and wrote my reply. The fundamental distinction between a static magnetic field and a static electric one is that the force lines of a static magnetic field can be closed outside material bodies (or form closed "magnetic surfaces"); in this way, the "contact problem" might in principle be solved. The appearance of closed magnetic force lines

19

THE TURNING POINT

THE YEARS 1965–1967 were a turning point in my life. I was heavily involved in demanding scientific work, even as I was approaching a decisive break with the establishment.

I still spent most of my time at the Installation, where attention was shifting from the development of nuclear devices to new ventures such as underground "breeder explosions" (which produce radioactive substances when uranium and thorium atoms capture free neutrons and then split) and a nuclear propulsion system for space flight. We spent much of our time developing specialized nuclear charges for nonmilitary applications, including copper mining in Udokan and other strip mining projects, building dams and canals, releasing underground oil reserves from shale, and capping accidental blowoffs of oil and gas wells. The first and second Installations vied with each other in theoretical and experimental research into peaceful uses of nuclear explosions, but the serious risk of contaminating the soil, groundwater, and atmosphere continually thwarted the practical application of our ideas.

Both Installations began to concentrate on problems requiring an "operations research" approach. The first problem of this kind we tackled was an investigation of antiballistic missile (ABM) systems and ways to counter them. In the course of many heated discussions, I, along with the majority of my colleagues, reached two conclusions which, in my view, remain valid today:

1) An effective ABM defense is not possible if the potential adversary can mobilize comparable technical and economic resources for military purposes. A way can always be found to neutralize an ABM defense system—and at considerably less expense than the cost of deploying it.

2) Over and above the burdensome cost, deployment of an ABM system is dangerous since it can upset the strategic balance. If both sides were to possess

powerful ABM defenses, the main result would be to raise the threshold of strategic stability, or in somewhat simplified terms, increase the minimum number of nuclear weapons needed for mutual assured destruction.

These findings, which were apparently shared by American experts, probably helped pave the way for the 1972 Treaty on the Limitation of Antiballistic Missile Systems. I have continued to refine my ideas on ABM systems, and the evolution of my views can be traced in my written comments on the topic, especially *My Country and the World* (1975), my 1983 letter to Sidney Drell ("The Danger of Thermonuclear War"), and my talk on SDI [the Strategic Defense Initiative] delivered at the 1987 Forum for a Nuclear-Free World.

During the second half of the 1960s, I became involved in discussions of a still broader range of problems. I read economic and technical studies concerning the production of radioactive substances, nuclear weapons, and delivery systems, visited several secret military facilities (or "mailboxes" as we called them), and attended one or two conferences on military strategy. Inadvertently, I picked up quite a bit of information (I am thankful that I was not told everything, despite my high-level security clearance). What I learned was more than sufficient to impress upon me the horror, the real danger, and the utter insanity of thermonuclear warfare, which threatens everyone on earth. Our reports, and the conferences where we discussed a strategic thermonuclear strike on a potential enemy, transformed the unthinkable and monstrous into a subject for detailed investigation and calculation. It became a *fact of life*—still hypothetical, but already seen as something possible. I could not stop thinking about this, and I came to realize that the technical, military, and economic problems are secondary; the fundamental issues are political and ethical. Gradually, subconsciously, I was approaching an irrevocable step—a wide-ranging public statement on war and peace and other global issues. I took that step in 1968.

ADDING MY SIGNATURE to a collective letter opposing the rehabilitation of Stalin was one of the significant harbingers of my 1968 essay.

In January 1966, Boris Geilikman, a neighbor formerly associated with FIAN and now at the Institute of Atomic Energy, escorted a short, energetic man to my apartment. He introduced himself as Ernst Henri, a journalist. (I later learned that Geilikman made this introduction at the request of Academician Vitaly Ginzburg.)

After Geilikman left, Henri came straight to the point. There was a real danger, he said, that the forthcoming Twenty-third Party Congress might adopt a resolution rehabilitating Stalin. Influential military and party circles—alarmed by the decay of ideology, the breakdown of values, and the loss of confidence following the failure of Alexei Kosygin's economic reforms—were pushing that idea. But many other Party members understood that Stalin's rehabilitation would have devastating consequences, and prominent representatives of the Soviet intelligentsia should support these "healthy forces."

Henri said he was aware of my stand on genetics, my major role in defense, and my authority. I read his draft letter, found nothing objectionable in it, and added my signature. Pyotr Kapitsa, Mikhail Leontovich, and five or six others had signed before me, but Henri's own name did not appear, because he wanted to limit the list to "celebrities." In total, twenty-five people eventually signed the letter, including the famous ballerina Maya Plisetskaya.

Rereading the letter now, I still agree with its assessment of Stalin's crimes, but I find the line of argument overly influenced by tactical considerations and the tone too deferential. At the time, however, my discussion of the letter with Henri and others greatly advanced my understanding of social issues.

Henri mentioned that foreign correspondents in Moscow would be briefed on the letter, and I made no objection. He asked me to pay a visit to Academician Andrei Kolmogorov, whose authority reached beyond mathematicians to Party and military circles.

Kolmogorov was then engaged in reorganizing the teaching of mathematics, but, in my opinion, his innovations were not constructive. Set theory and mathematical logic are too sophisticated for young students; they complicate the learning of practical mathematical techniques without inculcating a deeper understanding of mathematical principles. I prefer the traditional approach (after all, Euclid served many generations well before the advent of Bourbaki), and would add to it only the study of differential equations and other useful mathematical tools.

Kolmogorov agreed to see me, but made it clear that he was in a hurry. Although he was no longer young and his hair was streaked with gray, he appeared fit, suntanned, and vigorous. He was gentle in manner and speech, pronouncing his "r's" in the old, aristocratic way, and holding himself somewhat aloof. He read the letter, but refused to sign it, because, he said, the soldiers with whom he dealt all idolized Stalin for his wartime leadership. I replied that Stalin's role had been determined by his government post and not the other way around, and that he had committed numerous crimes and had made costly mistakes. Kolmogorov didn't disagree—but he still wouldn't sign. (A few weeks later, however, after foreign broadcasts reported our appeal, Kolmogorov did sign a similar collective letter opposing Stalin's rehabilitation.)

In retrospect, I now realize that Henri's letter was probably inspired by his influential friends in the Party machine or the KGB. Henri became a frequent visitor to my apartment and told me something of his past, leaving (I suspect) much unsaid. His real name is Semyon Rostovsky. In the 1920s he was a

Comintern agent working underground in Germany, and he saw at first hand the absurdity of the Comintern's (that is, Stalin's) policy, which held Hitler's fascism to be a lesser evil than Social Democracy since the popularity of the latter's pluralist philosophy threatened the Communists' monopoly of the working class. Stalin believed he could come to terms with Hitler on spheres of influence (or, if need be, destroy Hitler), but he feared the liberal center: it seemed dangerously beyond his control. Rostovsky wrote a number of articles warning against fascism, and his book *Hitler Over Europe?*[1] brought him fame under the pseudonym Ernst Henri, which he has maintained ever since.

Henri showed me a samizdat article he'd written on Stalin, and some correspondence with Ilya Ehrenburg on the subject. But Henri was never a "dissident."

IN THE FALL of 1966, two men (one of whom, I think, was Geilikman; I don't recall the other) asked me to sign an appeal. Typed out on onionskin paper and addressed to the Supreme Soviet of the Russian Republic (the RSFSR), it opposed the impending enactment of Article 190-1 of the RSFSR Criminal Code [Circulation of Fabrications Known to Be False Which Defame the Soviet State or Social System; maximum sentence: three years labor camp] which would open the way for the prosecution of many more dissidents. Articles 70 [Anti-Soviet Propaganda; adopted in 1960; maximum sentence: ten years labor camp plus five years internal exile] and 190-1 subsequently served as the principal juridical weapons for the suppression of dissent [until they effectively were revoked in 1989].[2]

A prosecution under Article 70 required proof, at least in theory, of the defendant's anti-Soviet intent; Article 190-1 did not. On the other hand, the 1971 *Commentary on the Criminal Code* asserts that "the circulation of fabrications which are not known to be false by the party responsible, as well as the expression of mistaken opinions or suppositions do not constitute crimes under Article 190-1." In practice, however, no attention was paid to this commendable declaration. The courts regularly convicted dissidents for their beliefs, for expressing their opinions, and for reporting information they sincerely believed to be accurate. The statements at issue in such proceedings were based on actual fact (with the exception of a few accidental misunderstandings), and usually exposed human rights abuses such as dissident trials, conditions in labor camps, and the deportation of Crimean Tatars; or—even

[1][Simon and Schuster, 1934.]

[2][Harold Berman's *Soviet Criminal Law and Procedure*, 2nd edition (Harvard University Press, 1972), contains English translations of Article 70 (pp. 153–154) and Article 190-1 (pp. 180–181). The translation reflects the imprecise language of the Russian original.]

more telling—the secret additional protocol to the 1939 Soviet-German Nonaggression Pact, the mass executions of Polish officers at Katyn, and so on.[3]

Courts rarely bothered to try to demonstrate that a defendant's allegedly defamatory statements were false; it was enough to show that they were "anti-Soviet." Moreover, no attempt was made to prove that the defendant *deliberately* distorted facts.

Although the above comments are based on my subsequent experience in defending human rights, even in 1966 I realized that the alarm triggered by Article 190-1 was justified, and I signed the letter. In this case, it was clear that its authors were acting on their own initiative and accepted the responsibility for any repercussions. I not only signed the joint letter but, a couple of days later, sent a personal telegram to Mikhail Yasnov, chairman of the RSFSR Supreme Soviet, expressing my concern. There was no reply. (When I told the physicist Boris Ioffe what I had done, he said: "Andrei Dmitrievich, you really are a brave man!")

Since then, I have sent many other letters and telegrams to officials. With a few insignificant exceptions, I have never received a reply, and these efforts have produced little in the way of immediate results. Some people therefore regard them as a form of naïveté, while others condemn them as a dangerous and provocative "game." But I believe that statements on public issues are a useful means of promoting discussion, proposing alternatives to official policy, and focusing attention on specific problems. They educate the public at large, and just might stimulate significant changes, however belated, in the policy and practice of top government officials. Appeals on behalf of specific individuals and groups also attract attention to their cases, occasionally benefit a particular individual, and inhibit future human rights violations through the threat of *glasnost* [public disclosure].

In dealing with civic issues and individual cases, it is most important that appeals be open. Private interventions are sometimes useful as a supplement—not a replacement—for public actions.

IN THIS SAME YEAR, 1966, I made an important new acquaintance—Zhores Medvedev's identical twin, Roy—who helped broaden my understanding of social problems. Roy, a historian by profession, visited me in my Moscow

[3][The Crimean Tatars are a Turkic people who since the early fifteenth century have considered themselves a national entity distinct from other descendants of the Mongols, a fact recognized by the Soviet authorities in 1921, when they created the Crimean Autonomous SSR. It was formally dissolved in 1945. See Alan Fisher, *Crimean Tatars* (Hoover Institution Press, 1978). For the Secret Protocol of the Molotov-Ribbentrop Pact, see *Nazi-Soviet Relations 1939–1941*, Department of State, 1948.]

apartment, and told me about the book on Stalin he'd been working on since the Twentieth Party Congress in 1956.[4]

Roy's father, Alexander, a professor of philosophy, had been a member of a Communist opposition group in the early 1920s. He was arrested during the purges of the 1930s, and died in a labor camp. Roy told me he maintained close relations with many Old Bolsheviks, and their eyewitness accounts and unpublished memoirs provided a wealth of previously unknown details for *Let History Judge*.

Roy left several chapters of his manuscript with me. He was to come back many times, bringing me new chapters in return for the old. He loaned me other samizdat manuscripts, including Eugenia Ginzburg's *Journey into the Whirlwind*,[5] one of the better-known memoirs of Stalin's camps. He also brought me news about dissidents and events of social significance. Some of his reports may have been biased or slanted, but when all is said and done, they did help me escape from my hermetic world. One story he told me (I believe during our first meeting; I can't vouch for its reliability) was about Pyotr Yakir visiting Zeldovich to get his signature on the collective letter protesting Article 190-1. When asked whether he himself had signed, Yakir admitted he hadn't. "After you," he was told. Yakir signed, and Zeldovich followed suit. I'm not at all sure he would have done so later on.

I was fascinated by Medvedev's book on Stalin. I hadn't yet read Robert Conquest's *The Great Terror*, or any other works on the subject, and was largely ignorant of the crimes of the Stalin era. An example of the kind of material Medvedev succeeded in uncovering was the report of the special commission established by Khrushchev to investigate the 1934 murder of Sergei Kirov—a detailed description of the assassination of Stalin's rival and of the subsequent elimination of all eyewitnesses to the crime and to the cover-up.[6] The information contained in *Let History Judge* stimulated the evolution of my views at a crucial time in my life.

Even in 1966, however, I couldn't accept Medvedev's tendency to attribute all the tragic events of the 1920s to the 1950s to the idiosyncrasies of Stalin's personality. Although Medvedev agreed in principle that more fundamental causes were at work, his book failed to explore them. We must still look to the future for a satisfactory analysis of our history, one free from dogmatism, political prejudice, and other forms of bias.

[4][Published in a slightly abridged English translation as: *Let History Judge* (Knopf, 1971) and reissued in a revised version by Columbia University Press in 1989.]

[5][Eugenia Ginzburg, *Journey into the Whirlwind* (Harcourt Brace & World, 1967). A second volume was published posthumously by Harcourt Brace Jovanovich in 1981.]

[6][*Let History Judge* (pp. 157–166) suggests Stalin's complicity in Kirov's murder, the act which triggered the bloody purges of the 1930s.]

Roy Medvedev and I continued to meet for some years, but then our paths, both public and private, diverged, until we broke off relations after 1973.

ON DECEMBER 3 or 4, 1966, I found an envelope in my mailbox containing two sheets of onionskin paper. The first sheet was an anonymous report on the arrest and confinement in a psychiatric hospital of Viktor Kuznetsov, an artist who had helped draft a model constitution for our country—Constitution II—which the authors hoped would spark discussion about the introduction of democracy.

The second sheet announced a silent demonstration on December 5, Constitution Day. It proposed that interested persons arrive at Pushkin Square a few minutes before six P.M., assemble near the monument, and then at the stroke of the hour remove their hats and observe a minute of silence as a sign of respect for the Constitution and support for political prisoners, including Kuznetsov. (I learned much later that Alexander Esenin-Volpin was the author of this Constitution Day appeal, and of several other original and effective ideas to promote respect for human rights.)

I decided to attend. Klava didn't object, though she did say it was an odd thing to do. I took a taxi to Pushkin Square and found a few dozen people standing around the statue. Some were talking quietly; I didn't recognize anyone. At six o'clock, half of those present, myself included, removed our hats and stood in silence. (The other half, I later realized, were KGB.) After a minute or so we put our hats back on, but we did not disperse immediately. I walked over to the monument and read the inscription aloud:

> I shall be loved, and the people will long remember
> that my lyre was tuned to goodness,
> that in this cruel age I celebrated freedom
> and asked mercy for the fallen.[7]

After that, I left the Square with the others.

SOMETIME LATER, Yuri Zhivlyuk—another new acquaintance of 1966—told me that my "escapade" had been filmed by the KGB using infrared film (it had been dusk on the Square) and then shown to high officials. I have never completely understood Zhivlyuk. He was working at FIAN when I met him, and people there told me that in the mid-1960s he had been Komsomol

[7][Alexander Pushkin, "Unto myself I reared a monument," [illegible]

secretary in one of the laboratories. A samizdat document, Valery Skurlatov's "A Code of Morals,"[8] was circulating among Komsomol members at the time; I don't know whether this fascist program represented Skurlatov's own views or was a trial balloon launched by some faction using him as a stalking-horse. In any event, Zhivlyuk wrote a bitter complaint to the Komsomol Central Committee, which proceeded to punish both parties—Skurlatov for his essay, and Zhivlyuk for "washing dirty linen in public."

Despite this rebuke, Zhivlyuk maintained some sort of relations with the Komsomol Central Committee, and was sent on a fact-finding mission to Bratsk and the far north in 1969. He told me about the economic and ecological problems of the region and described the deliberate corruption of Siberian trappers. A government procurement official would fly into a settlement in a helicopter loaded with vodka. After a few days the trappers and their parents, wives, and children would all be drunk, and the helicopter would fly off with furs for export.

Zhivlyuk was of Ukrainian descent and had ties to dissidents in the Ukraine. He introduced me to Ivan Svetlichny, a poet and Ukrainian activist. Zhivlyuk also had contacts among Moscow dissidents, Andrei Tverdokhlebov in particular. I suspect Zhivlyuk may have had some sort of link with the KGB (perhaps with its "progressive circles"). In the 1970s he apparently became ensnared in this tangled skein of relationships, and he disappeared from my field of vision.

Early in 1967, Zhivlyuk told me about the case involving Alexander Ginzburg, Yuri Galanskov, Alexei Dobrovolsky, and Vera Lashkova, and the demonstration organized in their defense by Vladimir Bukovsky and Viktor Khaustov. These events, like the February 1966 trial of the writers Andrei Sinyavsky and Yuli Daniel, played a critical role in shaping public consciousness and in forging the human rights movement in our country.[9]

When I heard of the case of Ginzburg and the others, I recalled that in mid-1966 Ernst Henri had shown me Ginzburg's letter of recantation which had appeared in the newspaper *Vechernaya Moskva* [on June 3, 1965]. I still don't know who was behind Henri's attempt to scare me away from Ginzburg, but in February 1967, I decided to ignore his warning. I used Zhivlyuk's

[8][English translation of Skurlatov's "A Code of Morals," in Stephen F. Cohen, ed., *An End to Silence* (Norton, 1982), pp. 171–174. Skurlatov worked in the propaganda department of the Moscow Komsomol.]

[9][For information on the Sinyavsky-Daniel trial see: Max Hayward, editor, *On Trial* (2nd edition, Harper & Row, 1967); on the Ginzburg-Galanskov trial: Pavel Litvinov, ed., *The Trial of the Four* (Viking, 1972); and on the trials resulting from the Bukovsky-Khaustov demonstration: Pavel Litvinov, editor, *The Demonstration in Pushkin Square*, (Harvill Press, 1969). For a general history of the diverse dissident movements, see Ludmilla Alexeyeva, *Soviet Dissent* (Wesleyan University Press, 1985).]

information to write Leonid Brezhnev in defense of Ginzburg, Galanskov, Lashkova, and Dobrovolsky. Although I neither circulated my letter in samizdat nor publicized it in any way, it was a milestone for me in that it was my first intervention on behalf of specific dissidents. (During the Sinyavsky-Daniel trial I was still "out of things," and I paid little attention to Mikhail Sholokhov's speech at the Twenty-third Party Congress recalling that "in the memorable twenties . . . scoundrels and turncoats" like Sinyavsky and Daniel were shot.)

The Ministry learned of my letter. Friends reported that Efim Slavsky had told participants in a Party conference held at the second Installation in March: "Sakharov is a good scientist. He's accomplished a great deal and we've rewarded him well. But as a politician he's muddleheaded, and we'll be taking measures."

Measures were indeed taken. I lost my post as department head, even though I remained deputy scientific director of the Installation. My salary was reduced from 1,000 to 550 rubles a month. (This wasn't the first reduction.) The Chief of Administration, Tsirkov (who had switched from experimental work on magnetic cumulation to administrative work) reportedly said that he didn't understand how anyone could live on so little, even though by ordinary Soviet standards I still enjoyed a high salary.

In April or May of 1967, Academician Vladimir Kirillin, then chairman of the Committee on Science and Technology and a deputy of Premier Kosygin, invited me to his office. At the appointed time, a dozen or so prominent scientists and engineers, including Vitaly Ginzburg, Yakov Zeldovich, and Ilya Lifshitz, sat down to a table set for tea. Kirillin told us that futurological studies were enjoying a vogue in the United States. Although some of the articles being published were trivial, sophisticated futurology could provide a long-range perspective useful for planning. Kirillin asked us to set down our thoughts on the development of science and technology in the coming decades. We should write without constraint, concentrating on the scientific fields we knew best, but touching on more general questions if we so desired.

I carried out Kirillin's assignment with enthusiasm and managed to include quite a few flights of fancy in a relatively short article. On the plane en route from the Installation to Moscow, I exchanged manuscripts with Zeldovich, and he complimented me on my essay. It was published in 1967 in *The Future of Science*, edited by Kirillin and distributed on a restricted basis.

The work I did on this article had a profound psychological effect on me, and turned my thoughts once again to global issues. Some of my propositions resurfaced later in *Reflections on Progress, Peaceful Coexistence, and Intellectual Freedom* and in the article "The World after Fifty Years."

* * *

THAT SAME YEAR, I tried my my hand at writing once more. Henri suggested that he and I collaborate on an article about the present-day role and responsibility of the intelligentsia: he would ask questions and I would respond. I agreed and we set to work, but my answers were more radical than he had anticipated—I was by now approaching the ideas which were to find expression in *Reflections.*

I took the manuscript to a typist who lived near the Sokol metro station, three bus stops from my Moscow apartment—a woman I had been employing for several years to type up scientific reports. When she handed back the last section of this new article, she was visibly upset. There had been a change in her family situation, she told me, and she could no longer work for me. It was obvious she was hiding something—a visit from the KGB, I suspect. After this experience, I had *Reflections* typed at the Installation.

The editors of *Literaturnaya gazeta* told Henri they needed permission from above to publish the article. Apparently, I'd gone further than they'd expected when they agreed in principle to the project. At Henri's request, I sent the manuscript to Mikhail Suslov via the Ministry. Two or three weeks later I received a letter from his secretary stating that Suslov found the manuscript interesting, but unsuitable for immediate publication since its ideas might be interpreted incorrectly. I returned the manuscript to Henri, visiting his apartment for the first time. It was spacious, evidently a bachelor's establishment, crammed with books and mementos of his years abroad. At that point, I promptly forgot the whole affair.

But that was not the end of the story. Some time later I discovered that the article had been included in a typescript periodical, *Political Diary,* rumored to be a KGB publication, or samizdat for officials. Several years after that, Roy Medvedev announced that he had been its editor. To this day, I wonder how my article ended up in his hands.[10]

IN JUNE or July 1967, at Leontovich's suggestion, I was given Larisa Bogoraz's letter describing the desperate situation of her husband, Yuli Daniel, and an account of her visit to the Mordovian labor camp where he was serving his

[10][The Sakharov-Henri article, "Scientists and the Danger of Nuclear War," appears in *An End to Silence,* pp. 228–234. Roy Medvedev claims in his foreword (p. 20) that Sakharov was a regular reader of his monthly bulletin which circulated among a few dozen intellectuals from 1964 to 1971; but he did not recognize the name *Political Diary,* which was used only for foreign consumption.]

sentence. I was about to fly back to the Installation and took the letter with me.

I called Yuri Andropov on the high-frequency telephone from my office at the Installation. Andropov told me he'd already received eighteen requests to look into the Daniel case (even back then, I had some difficulty in believing his statement), and that he would do so. He urged me to send him the original of the letter, and when I asked why, he answered, "For my collection." I pretended I had misunderstood, and mailed him a retyped copy

Six weeks later, Deputy Procurator General Mikhail Malyarov (the same man who in 1973 would warn me about my conduct) phoned my Moscow apartment. He said that he'd checked on Daniel at Comrade Andropov's request and that both Daniel and Sinyavsky would be released under an amnesty slated for the fiftieth anniversary of the October revolution. I thanked him for this information, which, however, turned out to be false. As usual, the amnesty did not apply to political prisoners. (Roy Medvedev later assured me that the decision to exclude political prisoners had been made at the last moment, but, as always with Medvedev, I don't know where he got his information, and I have some doubts about its accuracy.)

IN 1967, I became involved in the effort to save Lake Baikal. The deepest lake in the world, it is an immense reservoir of fresh water. Even more important, the Baikal region is a unique phenomenon of nature, an area of surpassing beauty which has become for many a symbol of our nation. For several years, *Komsomolskaya pravda, Literaturnaya gazeta,* and other newspapers had been publishing alarming—and convincing—reports on threats to Baikal from industrial construction along its shores, the felling and rafting of timber, and the discharge of chemical wastes into its waters. Though our efforts to protect Baikal were unsuccessful, I did gain valuable insight into environmental problems, both in general and in the particular context of Soviet society. (Later on, in Gorky, I read Boris Komarov's *The Destruction of Nature,*[11] a comprehensive discussion of environmental problems in the USSR, including Baikal, and a work I recommend highly.)

Let me describe my part in the Baikal campaign. Early in 1967, a student at the Moscow Institute of Energy visited me on behalf of the Komsomol's Committee to Save Baikal. He invited me to attend the committee's meetings, to study the issue, and to join in the defense of Baikal. I took the matter

[11][*The Destruction of Nature in the Soviet Union* (M.E. Sharpe, 1980). Boris Komarov is the pseudonym of Zev Wolfson, an ecologist educated at Moscow University who emigrated to Israel in 1981.]

seriously, and a few days later I visited the Komsomol building on Serov Passage where the meetings were held.

Among the committee's members, I recall Academician Igor Petryanov-Sokolov (the inventor of the Petryanov dust filter); the aircraft designer Oleg Antonov; the journalist Oleg Volkov, a former inmate of Stalin's camps; a member of the RSFSR State Construction Board whose name I have forgotten (he and Volkov were the two best informed and most active members); the limnologist Nikolai Nikolsky; and finally the student who had invited me and who represented the Komsomol on the committee. I was shown a number of startling documents on Baikal and on other ecological problems. Petryanov spoke about his specialty, industrial air pollution, which in some localities was catastrophic. Data on air pollution was classified and, as far as I know, remains so. I also learned about the long-term harm done by the flooding of arable land behind hydroelectric dams built in relatively flat regions.

I conducted some research on my own, meeting with Professor Rogozin, a specialist in the cellulose industry. I learned that in the late 1950s, Orlov, the minister in charge of the paper industry, had ordered construction of a large cellulose complex on the shores of Lake Baikal. This facility was designed to produce a particularly durable viscose rayon cord for airplane tires. It was assumed that polymerization would be facilitated by the pure Baikal water, and the resulting fibers would be stronger, but the plant's actual output showed that this hypothesis was unfounded. More important, the aviation industry switched from rayon cord to metallic cord. Thus, whatever rationale the Baikal complex may once have had—and it never, in any case, offset the potential harm to the lake—vanished. Construction nevertheless went ahead, and whole armies of officials, defending their unfortunate decision and their "regimental honor," continued to insist on the importance of the complex for the defense of the country, the usual clinching argument.

The story goes that Orlov had chosen the site by simply pointing to a place on the shoreline while cruising in a motorboat with his cronies. Building was already under way when Baikal's defenders discovered that this was the precise spot where the famous Verninsky earthquake had caused the lake to swallow up thirty-five acres of shoreline in the last century; it was a seismically active region. Telegrams were duly dispatched to Moscow, but instead of canceling the project, the only reasonable course of action, the authorities transferred responsibility to a new contractor—the Ministry of Medium Machine Building. (Petryanov taunted me: "Do you know who's in charge of the murder of Baikal? Your own Slavsky!") New plans were drawn up for earthquake-resistant multistory aluminum and glass buildings supported by steel piles. It was an engineering miracle, but construction costs had multiplied, and the buildings are still vulnerable to the major earthquakes that have occurred there once or

twice a century. As its reward, the Ministry of Medium Machine Building was permitted to cut timber in the Baikal preserve!

The big problem now was treatment of toxic waste. The appropriate institutes worked out a scheme for biological purification, after which the effluents were to flow through a canal into the Angara River, bypassing Baikal. The scientists defending the lake pointed to flaws in the proposal, and their fears proved more than justified when the complex began operating. The Academy of Sciences appointed a commission of experts chaired by Academician Nikolai Zhavoronkov, a chemist with little competence in this particular field but responsive to the wishes of the Academy's president and the State Planning Committee.

Our committee had assembled extensive documentation on the damage to the lake and its surrounding area which could come about through human activity. The pollution caused by floating logs down the rivers which empty into the lake kills the spawn of most fish, including the Baikal *omul,* which a century ago rivaled beef as a source of food for all Russia. The accidental discharge of effluents, deforestation, and fire are other hazards threatening the fragile ecological balance of the Baikal region. We proposed that the lake shores be closed to new industry and that existing enterprises be moved. We calculated the expense of such relocation and showed that it was not excessive—far less than had already been spent on the Baikal project. Our report, signed by us and also by a secretary of the Komsomol Central Committee, was sent to the Party Central Committee together with a sampling of the seven thousand letters on Baikal received by *Literaturnaya gazeta* and *Komsomolskaya pravda.*

For good measure, I decided to telephone Brezhnev personally; it was the last conversation we ever had. He was friendly and courteous, but complained of overwork and suggested that I talk to Kosygin, who was handling the Baikal matter. Unfortunately, I failed to follow up. I had never dealt with Kosygin, did not know him personally, and feared that without preliminary spadework, a call would be useless. I knew nothing about the relations between Brezhnev and Kosygin and didn't realize that Brezhnev was in fact shifting responsibility for an unpleasant task onto someone else. My call to the head of state, I thought, was all that was needed: if Brezhnev and Kosygin were interested (I made no distinction between them), they would take appropriate action. I was wrong.

I soon learned that a final decision had been made at a meeting of the Council of Ministers attended by Mstislav Keldysh, President of the Academy of Sciences, and, I think, Zhavoronkov. Kosygin asked Keldysh: "What does the Academy recommend? If the safeguards aren't reliable, we'll stop construction."

Keldysh reported the Zhavoronkov commission's conclusion: the water purification system and the other safeguards for Baikal were completely reliable. He may, of course, have been acting in good faith. Possibly he felt that he was choosing "the lesser evil"; the ecological peril most likely seemed less threatening to him than it did to our committee. Still, my feeling is that his stand and his general outlook were greatly influenced by the Academy's administrative dependence on the bureaucratic machine headed by the Central Committee, the State Planning Committee, the ministries, etc. Keldysh and the Academy's presidium were predisposed to respect the wishes of this machine and to ignore the warnings of whistleblowers, dismissing their arguments *a priori* as demagogic, exaggerated, impractical, and generally nonsensical.

Only a couple of years after these events, a Komsomol expedition brought back photographs showing the massive destruction of Baikal's fish and plankton caused by toxic wastes. But in accordance with standing instructions, no accidental discharges had been logged. As always, everything was fine on paper.

20

1968

The Prague Spring.
Reflections on Progress, Peaceful Coexistence, and Intellectual Freedom.

BY THE BEGINNING of 1968, I felt a growing compulsion to speak out on the fundamental issues of our age. I was influenced by my life experience and a feeling of personal responsibility, reinforced by the part I'd played in the development of the hydrogen bomb, the special knowledge I'd gained about thermonuclear warfare, my bitter struggle to ban nuclear testing, and my familiarity with the Soviet system. My reading and my discussions with Tamm (and others) had acquainted me with the notions of an open society, convergence, and world government (Tamm was skeptical about the last two points). I shared the hopes of Einstein, Bohr, Russell, Szilard, and other Western intellectuals that these notions, which had gained currency after World War II, might ease the tragic crisis of our age. In 1968, I took my decisive step by publishing *Reflections on Progress, Peaceful Coexistence, and Intellectual Freedom.*

My work on *Reflections* happened to coincide with the Prague Spring. A year earlier, I'd finally bought a short-wave receiver, and I listened once in a while to the BBC and Voice of America, especially to programs on the Six Day War. In 1968, I began tuning in regularly to the news from Czechoslovakia, and heard Ludvík Vaculík's stirring manifesto, "2,000 Words"—and much more besides. Zhivlyuk and Roy Medvedev supplied additional details during their increasingly frequent visits.

What so many of us in the socialist countries had been dreaming of seemed to be finally coming to pass in Czechoslovakia: democracy, including freedom of expression and abolition of censorship; reform of the economic and social systems; curbs on the power of the security forces, limiting them to defense against external threats; and full disclosure of the crimes of the Stalin era (the "Gottwald era" in Czechoslovakia). Even from afar, we were caught up in all proposals ought to be discussed. I'll call Alexei Rumyantsev and have him organize something at his institute."

"Of course, Turchin and Medvedev should also participate," I replied. To this Trapeznikov made no response.

Academician Rumyantsev was director of the Institute of Applied Sociology. I'd met him twice at the Academy of Sciences, where he was a member of the Presidium. What I didn't know was that his position was becoming shaky: he was regarded as overly sympathetic to reform and democratization. During our conversations he seemed ill at ease, as if I posed a mortal threat to him. And maybe I did.

I still don't understand why Trapeznikov invited me to his office. To have a look at the troublemaker in his diocese? To reeducate me? To neutralize my "malign" role in the Academy's elections? (Incidentally, I never spoke against Trapeznikov's candidacy, although I made no secret of my opinion that he wasn't qualified. When Trapeznikov's first nomination failed, Keldysh telephoned Brezhnev in a panic. Brezhnev is supposed to have answered calmly: "Well, so what? I'm not an Academician, either.") All three motives may have played a role, and there may have been a fourth one at play—to undermine Rumyantsev. That was Zhivlyuk's theory (he referred to confidential sources). In any event, Rumyantsev refused to host a discussion of the Letter at his institute, insisting that he hadn't received any instructions from Trapeznikov.

I saw Trapeznikov once again when Keldysh was running for reelection as president of the Academy. Trapeznikov walked over, shook my hand, and spoke to me in familiar terms as if I were "one of their own." He asked if I intended to vote for Keldysh. I said yes. Satisfied, Trapeznikov walked away.

22

THE DISSIDENTS AND THEIR WORLD

Valery Chalidze. The Grigorenko affair. Rescuing Zhores Medvedev.

VALERY CHALIDZE phoned me in May 1970, introduced himself, and asked if I knew his name. When I told him I had heard about his samizdat journal *Social Problems* from Roy Medvedev, he said, "That will make things easier."

We agreed to meet, and he asked me if I'd be willing to cosponsor a complaint to the Procurator's Office protesting Pyotr Grigorenko's involuntary confinement in a psychiatric hospital. I was happy to do so, and then delivered the complaint myself to the Procurator's Office.

I had not met Grigorenko, but I had heard a good deal about him, and had been moved by the letter he wrote me in response to *Reflections*. A man of remarkable courage and integrity, he played a leading role in the dissident movement. He has told his story in detail in his fascinating *Memoirs*,[1] so I shall note here only the bare essentials.

Grigorenko fought in the Second World War as a professional army officer and later taught at the Frunze Military Academy, where he was promoted to Major General. After he criticized Khrushchev's mistakes at a Party meeting in 1961, warning that he was planting the seeds of a new personality cult, Grigorenko was demoted and transferred to the Far East. In 1964 he was arrested and confined in a prison psychiatric hospital for circulating leaflets which called for a return to "Leninist principles." He was released in 1965, but reduced to the rank of private, expelled from the Party and left without a job. Grigorenko wrote an informed, persuasive, and widely circulated samizdat article on Stalin's responsibility for the defeats we suffered during the first months of the war.[2] He was an energetic supporter of the Crimean Tatars' efforts to return to their homeland. In May 1969, he was arrested in Tashkent

[1][Pyotr Grigorenko, *Memoirs* (Norton, 1982).]
[2][*The Grigorenko Papers* (C. Hurst, 1976), pp. 12–51.]

when he traveled there to defend arrested Crimean Tatar activists, and he was again confined to a prison psychiatric hospital.

Our complaint, drafted by Chalidze and delivered by me to the Procurator General, pointed out serious procedural violations committed during the preliminary investigation and at Grigorenko's trial.[3]

A young psychiatrist, Semyon Gluzman, studied the available evidence, and affirmed Grigorenko's sanity.[4] After Vladimir Bukovsky collected documents on his case and sent them to the West, a worldwide protest campaign helped to win Grigorenko's release in 1974. He was still energetic, but his health had been undermined. In 1976, he joined the Moscow Helsinki Group. In 1977 he flew to the United States for surgery and to visit his son Andrei, who had emigrated in 1975. While there, Grigorenko was stripped of Soviet citizenship, barring his return to the USSR. He continued to take part in public affairs and, after a long illness, died in New York in February 1987.

My collaboration with Chalidze, which began with the Grigorenko case, soon led to a whole series of joint projects and to friendly personal relations.

ON MAY 29, 1970, Roy Medvedev telephoned. He was extremely agitated; his brother Zhores had been forcibly confined in the Kaluga psychiatric hospital, and diagnosed as suffering from "creeping schizophrenia." Zhores Medvedev's work in two disparate fields—biology and political science—was regarded as evidence of a split personality, and his conduct allegedly exhibited symptoms of social maladjustment. In fact, his detention was the Lysenkoites' revenge for his book attacking them; they were then still capable of mustering significant power inside the establishment.[5]

The Grigorenko affair had sensitized me to the political abuse of psychiatry. I had been battling the Lysenkoites for a long time, and my work with Roy Medvedev on the Memorandum had created a bond between us. I entered the fray even though I was not well. I had sharp pains in my lower abdomen and was running a temperature of 101°; a month later I was operated on for a hernia. (In later years, Lusia and I often faced situations where it was necessary for us to take action despite our poor health.)

On May 30 I went to the Institute of Genetics, where an international

[3][According to the *Chronicle of Current Events*, no. 14, the complaint was signed by Sakharov, Chalidze, Mikhail Leontovich, and Valentin Turchin.]

[4][In 1972, after the Soviet authorities discovered the author of this analysis, which circulated anonymously in samizdat, Gluzman was arrested and sentenced to ten years labor camp and exile.]

[5][*The Rise and Fall of T. D. Lysenko* (Columbia University Press, 1969). For the story of Zhores's detention, see Zhores and Roy Medvedev, *A Question of Madness* (Knopf, 1979).]

symposium on biochemistry and genetics was in progress. Many scientists had come from the socialist countries and twenty to thirty from the West. Before the session began, I walked to the blackboard and wrote the following announcement:

> I am collecting signatures in defense of the biologist Zhores Medvedev, who has been forcibly and illegally placed in a psychiatric hospital for his writings. Contact me during the break or reach me at home. *A. D. Sakharov.* (I added my address and telephone number.)

No one stopped me. I went outside and waited in the corridor.

Nikolai Dubinin, director of the Institute and a member of the Academy, was one of the last to notice my announcement. He erased it, and in his opening remarks criticized me sharply for mixing science and politics. Dubinin had been accustomed to sending me holiday greetings in commemoration of our common struggle against the Lysenkoites, but a year or so earlier had ceased to do so.

Two or three scientists signed the appeal during the break, as did two who were working in a laboratory. Others came later to my apartment.

Later, much of the dissident world assembled at Chalidze's room. On this occasion I met for the first time Tanya Velikanova, Grigory Podyapolsky and his wife Masha, Sergei Kovalev, and many others. Everyone there signed the appeal I had drafted, and Kovalev had authorization to sign for Alexander Lavut as well. All those I've named became my friends. Kovalev was distinguished, even among that company, by a deliberate quality of thought. He was tardy, but that was excusable in light of his heavy responsibilities.

The authorities, taken by surprise by my initiative, became alarmed. The poet Alexander Tvardovsky (an acquaintance of Roy Medvedev), the writer Vladimir Dudintsev, and other artists and scientists joined in the protests. I was called in by Keldysh, president of the Academy, and reprimanded for my behavior. I argued with him, and he promised to talk to the Minister of Health, Academician Boris Petrovsky.

On June 12, I was invited to a meeting at the Ministry of Health along with Academicians Boris Astaurov and Pyotr Kapitsa, who had also interceded in behalf of Medvedev. Keldysh was represented by Academician Anatoly Alexandrov. Petrovsky opened the meeting. Georgi Morozov, director of the Institute of Forensic Psychiatry, delivered a carefully worded medical report on Medvedev's condition. Kapitsa spoke, witty and cautious as ever. Then Astaurov and I argued for his release. After I spoke, Alexandrov retorted that my appeals to the West showed that I myself was in need of psychiatric attention. Petrovsky closed the session with a promise to resolve the matter through appropriate channels.

Medvedev was discharged on June 17. The rapidity of his release was unprecedented for a case of involuntary confinement in a psychiatric hospital in such circumstances.

23

THE HUMAN RIGHTS COMMITTEE

The Kiev conference. Pimenov and Vail.
I meet Lusia. The Human Rights Committee.
The Leningrad "skyjacking" affair.

MOST OF JULY 1970 I spent in a hospital, where my hernia was corrected by surgery. Once back on my feet, I decided to attend the international Rochester Conference on elementary particles, which was meeting in Kiev that year.

Before going, I dropped by to see Tamm at his dacha (in 1956 the government had given us adjacent dachas in Zhukovka, not far from Moscow, in a special section of the town reserved for academicians). Tamm spent spring, summer, and fall there, confined to his bed. As mentioned earlier, he had been kept alive on a respirator for several years, but he continued working and managed to stay in touch with a multitude of people. When I entered his room, I saw he had other visitors: Evgeny Feinberg, our colleague from FIAN, and Victor Weisskopf, the author of major contributions to quantum field theory, and for many years director of CERN, the European center for research on elementary particles.

Weisskopf recounted an incident from the 1930s, when he was living in Switzerland. The Swiss police, who worked closely with their German counterparts, accused him of being a Soviet spy. When he asked the basis for their accusation, the response was: "You visit Professor Tamm. He's been given a new apartment in Moscow; with their housing crisis, that's proof enough that he works for the NKVD."

Explanations were unavailing—Weisskopf had to leave Switzerland and was told he could never return. But the ban was revoked after the war, and he was allowed to live there again when he was appointed director of CERN in 1961.

The foreign department of the Academy reprimanded Feinberg after our encounter: "What right did you have to arrange a meeting between Sakharov and Weisskopf?"

"In the first place, I didn't 'arrange' anything. In the second place, what