Chapter 3
Alexei A. Abrikosov: The Magnetic Structure of Type II Superconductors

"I went and read Feynman's paper, and understood that it was exactly what I had proposed two years before. And I came and said to Landau, "Why do you accept this from Feynman, and you didn't accept it from me?""

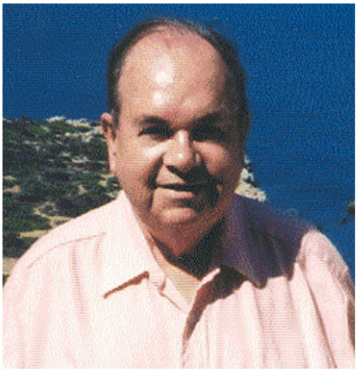

Fig. 3.1 Alexei A. Abrikosov

3.1 Biographical Notes

Alexei A. Abrikosov shared the Nobel Prize in physics for 2003 with Vitaly L. Ginzburg and Anthony J. Leggett "*for pioneering contributions to the theory of superconductors and superfluids*."

Abrikosov was born in Moscow in 1928. Already at the age of ten he was convinced he would become a scientist. He graduated from high school at the age of 15, in 1943. He had great talents in mathematics, but enrolled at this young age as a student at the Institute for Power Engineering. Still only 17 years old he was accepted as a student by the great Lev Landau, who understood what talents were at hand. Not yet 18 he passed Landau's famous test, "the theoretical minimum," and stayed close to him for many years to come. Eventually, he did his PhD with Landau, and was later a postdoc in his group.

During his long scientific life, Abrikosov has explored successfully many fields, but mainly the theory of solids: superconductors, metals, semimetals and semiconductors. He is famous for the theoretical discovery of what he called *superconduc-*

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tors of the second kind, later mostly referred to as *type II superconductors*, and their magnetic properties, where magnetic field penetrates the superconductor by quantized amounts as vortices, in periodic arrays, called the *vortex lattice*. This magnetic structure in type II superconductors now bears his name as the *Abrikosov lattice*.

His main discovery was published in 1957, but the results had already been achieved in 1953, without Abrikosov being allowed by Landau to publish them. The reason was that his boss, Landau, did not initially believe the theory, and was not convinced until he learned that Richard Feynman had published an article where quantized vortices in superfluids were predicted to drive the so-called lambda transition in helium from the superfluid liquid phase, called helium II, to the normal liquid phase. The importance of Abrikosov's discovery of the quantized magnetic structure in superconductors can best be characterized as huge. In 1962 Abrikosov, together with all Russian physicists, and the world of science, suffered the loss of the creative mind of their great mentor, Landau, who was very seriously injured in a car accident, and later died, in 1968.

Abrikosov has had a distinguished scientific career. Around 1960 he worked with Khalatnikov and Gorkov on various aspects of superconductivity. With Gorkov he discovered gapless superconductivity. With Khalatnikov he did much work on superfluid He^3 employing the Fermi liquid theory of Landau. Together with Gorkov and Dzyaloshinskii, in 1961, he published a widely used textbook, *Quantum field theory methods in statistical physics*. Abrikosov has been a very active teacher during almost all his career. He has held several different professorships in Russia, later also in the US and the UK.

In 1991 Alexei Abrikosov moved to the US and joined the Materials Science Division as an Argonne Distinguished Scientist in the condensed matter theory group of the Materials Science Division, where he continued to be active. In Argonne he has worked on the theory of high-T_c superconductors, properties of colossal magnetoresistance in manganates and, together with experimentalists there, discovered the so called *quantum magnetoresistance* in silver chalcogenides. Abrikosov has been elected a member of the National Academy of Science in the USA, and of the Russian Academy of Sciences, Foreign Member of the Royal Society of London and the American Academy of Arts and Sciences. He has received numerous Russian and international awards, and the Honourable Citizenship of Saint Emilion, France. He was awarded an honorary doctorate from the University of Lausanne, Switzerland.

3.2 His Own Story

3.2.1 Childhood in Moscow

Both of my parents were medical doctors. And for reasons that I don't understand, my mother told me that under no conditions must I become a medical doctor. I don't know why she said that, but nevertheless she did. So therefore I excluded a medical career from the very start. You know kids listen to their mother, without any doubt.

I think that in religion it is the same, the kids get the religion from their parents. That is the usual thing. But on the other hand I had no doubt that I would become a scientist eventually, so I read various books about great scientists and inventors. This was when I was maybe less than ten years old. I read books about Michael Faraday, Bessemer, Edison, about various kinds of inventors, and other people.

I had a kind of a dream, not a realistic one; it was if I were a hero of ancient mythology, then I would fight various kinds of monsters and so on. And I had two other such unrealistic dreams. One was to become a member of the Royal Society, and the other was to get the Nobel Prize. Again this was approximately the age of ten, and of course I didn't consider that to be realistic. With my consciousness I understood that it was absolutely impossible. Afterwards, both of these were fulfilled. But that I couldn't expect. It says something about the power of children's dreams.

On a similar note, there is the story of the Frenchman Jean-François Champollion who was able to interpret the Egyptian hieroglyphs. He studied Egyptian writing from childhood on, among them the Rosetta stone and its translations. The Rosetta stone is in the British Museum and it has scripts in Egyptian hieroglyphs, in Greek and in demotic Egyptian language. Therefore, although hieroglyphic script is very different from ancient scripts like Latin and so on, he managed somehow to guess it. He had such a dream, and he achieved it.

I grew up in Moscow, and I worked in Moscow most of my life. You may say I was privileged due to my parents, my father mostly. He got the Golden Star of Socialist Labour Hero, and the Stalin Prize. He was the vice president of the Academy of Medical Sciences, and a very well—known person. He was one of the rare scientists who after the revolution did not emigrate from Russia, and also he did not oppose the Soviet power. He was a neutralistic person. He said, "If we have some power which allows us to work, why should we interfere with its actions? We should be grateful that we have the opportunity to work." And that was his view always.

Because of all this he was called when Lenin, the founder of the Soviet state, died. He was called on and performed the autopsy of Lenin, and his assistant made the first sort of balsamation of the body in order that people who came from far away in huge numbers could actually see the dead man and say goodbye to the person whom they considered like a god. Many rumours exist regarding Lenin's body, and his brain, but I heard nothing from my father.

3.2.2 School and Education

I liked going to school, of course, and I had many friends. Also, I was very successful with my lessons, in particular when it came to mathematics. So mathematics was very easy for me. I would say I had an intention of becoming a mathematician, but I did not.

When the war started in Russia, I was 13 years old, so I was much too young to be drafted, and when the war ended in '45, I was still only 17 years old, and not yet of the draft age. But on the other hand I was very much advanced in my studies.

I graduated from high school at the age of 15, and then I went to the Institute of Power Engineering (IPE). From that place they did not draft students, they gave them a delay until they graduated. I went to that school during the war and then I transferred to Moscow University. They created special groups on nuclear physics and engineering. This was in 1945, when the Americans exploded their nuclear bombs.

3.2.3 Entering the Landau Group

I always had such an idea that while studying at the university I must simultaneously start work at some research institute. In the Soviet era university teaching was separated from research institutions. And so research was done in a different place. There was some research at universities, but much less than in the research institutes.

So already when I was at the Institute for Power Engineering I wanted to do some research. It happened that my mother knew a physicist by the name of Vul at the Lebedev institute in Moscow. He invited me to work in his lab, and so a few days a week I came to his lab and was working there on the physics of semiconductors and insulators. We were measuring the dielectric constant of barium titanate. And simultaneously I went to the university and to the IPE. My department was called the Electrophysical Department. My real goal was to study physics.

I chose that institute both because at first it delayed the draft to the army, and also because at my young age the university didn't want me. I was only 15 years old, and IPE took me. But when I went to the university and compared what we were learning and what they were learning, I found that they learned much more. I therefore actually took the notes from the lectures and the textbooks and prepared for some exams. And we arranged so that I could make it, and I could pass the exams for the university, for the Physics Department. I had what I would call a triple load, at the Power Engineering Institute I took courses, while carrying out experimental research with Vul at the Lebedev institute, and then at the same time I was also preparing for, and passing the exams at the Physics Department at Moscow State University in 1945.

But then, next year it was declared that the country needed nuclear specialists, and they created special groups at the Physics Department at the university, and these students would also have a delay of the draft, and they would learn nuclear physics in what I would say was some advanced manner. They first wanted us to graduate earlier but this didn't work out. But anyhow I didn't lose any time. I came to the university and had these ideas that I must combine it with some research work, and the only name I knew in physics—since I came from a doctor's family—was Kapitza. So I asked my father whether he knew Kapitza, because my father was a member of the Academy. And, yes, he knew Kapitza and spoke with him. Kapitza became interested when he learned that there existed such a young and able guy and he wanted to see me.

When I came to his institute he told me, “You are such a young person, and you must not become very narrow from the start; you must take a broader approach.” And in this connection he spoke with two people, one was an experimentalist, the other was a theorist. The theorist was Landau. And so I spoke with both of them.

I was around 17 when I came the first time to Landau and rang at the door. His wife—she describes it in her memoirs—saw a boy, a small boy, and she said, “Who do you want to see?” and I said, “professor Landau.” And she said, “You have to be mistaken.” “No, no, I’m a student of the university and I want to see him.” And we made the appointment. Landau gave me his program which he called the “theoretical minimum.” It consisted of nine subjects, which I had to learn and to pass personally with him. And so I passed these exams.

Landau was actually, I understand now, very kind to me, but he had principles and the principles were hard, and therefore he should not show his kindness. That was always his way, and he would have the same requirements to everyone. But actually, since I passed successfully every exam, I was never turned down. So he was happy with me.

Then next, in order to pass the graduation of the university I would have to go to some nuclear plant or nuclear research institution. I wanted to go to Landau and become his PhD student, I should say, the Russian system is somewhat different from the West. So therefore, in order to achieve that I had to somehow get out of the nuclear group. And this was done, but it was very hard, very hard.

3.2.4 With Landau

But nevertheless, I managed. As I said, I successfully finished the theoretical minimum and became his post graduate student. Landau preferred to work alone. He never proposed a topic for research. But he needed the young people, they were welcomed very much. He needed them for several purposes. He needed them first of all because he hated reading other people’s papers. And therefore all students periodically gave reports at his seminar about somebody else’s work so that he could reproduce everything himself.

For a very long time I was the secretary of that seminar. I therefore brought some journals and he marked the papers which he considered interesting. I wrote cards and put them in some box. Then everybody could choose a card, and there was a group of people who were his pupils who participated in those seminars.

In alphabetical order they had to give the reports. When a person’s time approached he searched in the box, chose a card and studied that particular work and reported it at the seminar. And although I was the secretary, nevertheless I had to do the same kind of thing. This was the first thing which he needed, but he also expected that we would somehow invent various topics for research and inform him on our progress. And so his knowledge would increase. But he never tried to put his name on what we did. It was very difficult to convince him to put his name on anything, I would say. Of course he would put it on the work he did himself, but then he had only his own name. I have something like four or five papers together with him.

It happened that I and Khalatnikov, my colleague, first got some ideas we were developing, and then Landau got interested, but he was not actually an expert in these methods. Therefore we taught him these methods for a month or more. And eventually he understood what we were doing, and he said that it was all rubbish, and then he thought about it. Then he invented some principles, and after that we were quite conscious and we applied these principles. And practically doing it, writing equations, solving them and so on, that was my task. What he had called rubbish was good for inspiration, but not good for really constructing a theory. And I agree with that. He had a good judgement.

3.2.5 Superconductivity: Discovery of Type II

Superconductivity I bumped into. Landau never invited me into that. As I said in my Nobel lecture, when I first asked him how to find a topic for my own research, his main advice was to talk to experimentalists. I did, and I had with me my university mate, who was in the same group as myself, Zavaritsky. He later died young with some type of cancer. We talked with the experimentalist and Zavaritsky did the main experimental work, checking the Ginzburg-Landau theory for the critical magnetic field of thin films. He did just that, and he got a beautiful agreement with Ginzburg-Landau theory and so it was quite well. However, his boss, Shalnikov, who was a perfectionist, said: "Your samples are just nothing, they are dirty. And you prepare them so that you take a drop of metal and you heat it up, and then the drop is evaporated and the fumes fly and condense on a glass substrate." "However," he said, "your glass substrate is kept at room temperature, pretty warm, and when the atoms come onto the glass, they can move. There they agglomerate, and what you have is actually a plane covered with drops, instead of a continuous film. In order to prevent that, you have to keep your glass substrate at helium temperature and never heat it before you make your measurements."

Now, that was a little bit hard, but still Zavaritsky managed to do it, and when he did, there was absolutely no agreement with Ginzburg-Landau theory! That's how it was. And so he said, "Look, now I cannot get agreement with Ginzburg-Landau." Then we started to think, both of us, to think what can be the reason. The Ginzburg-Landau theory looked so beautiful, it was much better than anything existing at that time. So it couldn't be wrong. That was how convinced we were. So therefore we had to search for opportunities within that theory, and that was what we did. And we found a possibility there.

And since it was theory, I did a corresponding comparison, and when I calculated and compared, it fitted completely. What had to be done was the following: You express the quantities entered in the Ginzburg-Landau equations in dimensionless way, introducing corresponding dimensionless units, for instance, instead of magnetic field, introduce magnetic field divided by some value. If you do such a procedure, then all material units disappear from the equations. They become universal. Except for one quantity. There remains one quantity which was later called the Ginzburg-Landau parameter, with the Greek letter *kappa*. The value of this parameter can be defined from the intermediate state, a periodic structure of distribution of magnetic and non-magnetic regions in the superconductor depending on the sample geometry.

This so-called intermediate state was known long before the Ginzburg-Landau theory. Ginzburg-Landau theory was published in 1950, while the Landau theory of the intermediate states had been published already in 1937. So that was known, but there existed some quantity which Landau introduced quite empirically: The surface energy. The Ginzburg-Landau theory predicted the surface energy. And the surface energy had a direct connection with the Ginzburg-Landau parameter *kappa*. So therefore knowing surface energy, which was defined from the period of the intermediate state, you could define the parameter kappa, and for conventional superconductors it always appeared to be very small.

And that simplified the theory considerably. The main idea of the Ginzburg-Landau theory is that they introduced as the order parameter some type of wave function in the Landau theory of second order phase transitions. That was the central point of that theory. This was a big change of thinking.

The application of the wave function started just with my work. The phase played no role in the work of Ginzburg and Landau. Of course, in principle they kept it in. In principle the wave function is a complex quantity, but in all the calculation, when they calculated the surface energy, the critical field, the critical current, there was no phase. I started with, first of all, solving the problem of critical field of thin films with large values of the GL parameter *kappa*. They had considered only small values. When I considered large values I got agreement with experiments.

The possibility that the order parameter would be suppressed by the magnetic field, so that it wouldn't be constant when you turn on the field, was not considered at this stage because in thin films it was constant. It changed with the film but it was constant over the whole thin film. So at this moment it did not require any phase, only the absolute value. Nevertheless, in this case it was written in the Ginzburg-Landau work that if the GL parameter *kappa* is large, the surface energy between normal and superconducting phase becomes negative. And that was an argument in their paper, not to consider large *kappa* values because negative surface energy would be unphysical, since everybody knew that there existed an intermediate phase in superconductors known at the time. This did not require considerations regarding the phase of the wave function.

However, it showed us that there really exists substances with big *kappa* and negative surface energy. *And that meant that there exists a new type of superconductor*. So Zavaritsky and I called them superconductors of the second group in our papers which were published in the same journal. For some reason, unknown to me, he did not want to write a paper together with me, and so we wrote two different papers. And they were published in the same issue of this journal, which was never translated into English and is totally unknown, I think. And that was the first time, I think, that the idea that there exists two types of superconductors was published.

Now, de Gennes attributes the discovery, the practical discovery of type II superconductivity to Shubnikov. The reason is that de Gennes did not know my paper, because my paper was never translated to English. I described this too in my Nobel lecture.

It was so that there was de Haas, and he was a very well known physicist. de Haas with the wife of Casimir, whose name was Casimir Jonker, published a paper in 1935 were they measured the magnetic properties of superconducting alloys. I don't remember what alloy it was, but they got a gradual transition from a superconducting to a normal phase, with gradual entrance of the magnetic field into the superconductor. That was the first paper on this subject. And they said that there exists a special state with two critical fields. That's what they got from their experiments. Now Shubnikov was a former student of de Haas. He looked tentatively at what de Haas was doing and decided to repeat this experiment, but with much better samples. de Haas had written that he attributed the behaviour I mentioned to inhomogeneities, that the superconductor consists of pieces with different critical parameters, so first some of them turn normal, and then the others.

This was in 1935. Shubnikov decided to repeat the same experiment, but with better samples. He wanted to get rid of inhomogeneities, and study homogeneous material. And so what he did was that he heated the samples for a long time, very close to the melting temperature. And after that he made an X-ray, and if the material was inhomogeneous, where different parts had different properties, he could expect that there would be lines in the X-ray spectra, which belonged to different pieces. He didn't find any of that, but his measurements were done at room temperature, for some reason, I don't know for what reason, but that is experimental stuff. He could not make X-ray photos at low temperatures.

So he got extremely smeared out curves, despite this homogenization. And so he wrote that he could not explain that in any way other than by inhomogeneity. Therefore he wrote, "We don't see inhomogeneity with the X-rays, however it nevertheless must happen, and probably it is due to precipitation of another phase at lower temperature." That was written in Shubnikov's paper, so therefore Shubnikov was not the first person who observed such a gradual transition for a superconductor to another phase. This was probably not known to de Gennes. Shubnikov definitely considered inhomogeneity, and therefore not another type of superconductor.

However, Shubnikov was accused of attempting to organize an anti-Soviet strike, and was arrested and immediately killed by the KGB. So he is a tragic figure. And furthermore I can tell you, there was this guy, Kurt Mendelssohn, quite well known. And Kurt Mendelssohn invented a description of this "inhomogeneity" which was called "Mendelssohn's sponge." It was described as a kind of sponge of a superconductor with higher critical temperature which was embedded in a superconductor with lower critical temperature.

He worked in England. He was a German who had emigrated to England. Now, after my work was accepted he so hated it, because my work undermined the Mendelssohn sponge, and therefore he did a very sly thing. Namely, it was known already that there is a different phase and that it appears in type II superconductors. And now he proposed to call this new phase with the vortices the "Shubnikov state." First of all Shubnikov had nothing to do with it, and secondly it was not *for* Shubnikov, it was *against* me.

So then I decided to see what happens in bulk type II superconductor. I started with the vicinity of the second critical field, because of what I already knew. So the question is: How does the transition happen? From the films I saw that it is a second order transition.

So therefore one can reason as follows: If you have a nucleus of some different phase, and the field is too large, then the nucleus will gradually disappear, and will decay. But if the field is too weak it starts to grow, and it will grow and increase with time. And therefore; a stationary nucleus that does not grow and does not decay must define the field which is the transition point. And such a field was actually described in the work by Ginzburg and Landau. Just for mere curiosity they found that, and that was considered an additional proof that large values of the GL parameter *kappa* are impossible.

However, I took a nucleus which could exist at higher fields, only one nucleus at some certain point. Then I imagined the following: It can appear indefinitely, so therefore you can derive the linear combination of such nuclei. And because I saw through it homogeneity of space, in an infinite sample, it must be periodic.

However, one can't write such a periodic function as a mere sum, you have to introduce phase factors, because the particular form of one nucleus depends on the calibration of the vector potential. The vector potential enters the Ginzburg-Landau equation, and therefore to do that, the only way was to write a linear complex function. And therefore I wrote that complex function, and it described what happened close to the second critical field.

Now I wanted to know what happens at lower fields, so here I tried many constructions, and afterwards I saw all of these constructions in different papers. But I was not satisfied. Landau agreed with some of them, but I was not satisfied. And then I thought, "Why must I invent something? I must analyse. The wave function that I got close to H_{c2}, maybe will teach me something." And so I found that it has zeros. I did not introduce these zeros, I did not do that on purpose. It somehow appeared by itself. And just thinking about that, I understood that there is no other way to compensate the growth of the vector potential. But in reality the magnetic field, in average, doesn't grow, and the absolute value of the wave function does also not grow. So therefore the growth of the vector potential has to be compensated. And it can be compensated only by the phase. And so here the phase came in. It is also in my Nobel lecture how really that compensation happens. Afterwards, in the so called gauge theory, they found that they have also no way of compensating the growth of the vector potential, except for these singularities. Then I also understood that the lower the magnetic field, the larger is the distance between these singularities. And in the limit of a small magnetic field I can see only one of them, and this one of them was the point vortex. After that, after I made the field theory, the structure and so on, I could calculate the magnetization as function of the magnetic field. Then this was easy to measure, and I could compare with the best experimental work. And that was Shubnikov's work. And when I compared my theoretical results with Shubnikov's results, it fitted perfectly. Note that it doesn't depend on pinning. Pinning becomes important only if you send currents through the structure. Then the vortices start to move, but if you measure static magnetization, nothing moves. So therefore pinning is absolutely of no importance, not interesting here.

3.2.6 Difficulties with Landau

Landau was not happy, and I had already experiences like that with Landau. When I brought him something that was unusual, he required some simple argument for the theory. When I made my PhD thesis, for example, it was on thermal diffusion in plasmas. I found in partially ionized plasmas which has ions of different charge, that thermal diffusion could change its size and reach huge values. He said; I don't believe that. And so he said; I will only believe it if you give a simple argument. So then I disappeared for a week and I tried to work it out. And I succeeded. I brought it to him, and he immediately accepted it.

But here I could not give him such a simple argument. Therefore he disagreed. This was quite unfortunate for the work. Now, at that moment we had an interesting problem in quantum electrodynamics, the behaviour of Green's functions at high energies, and I put my theory on superconductors of the second kind in the drawer, and there it stuck for four years. I understood that my theory was correct, and thought I will be able to convince him one day. At this point it was not the most important issue.

And at this time the world became interested in the work by Feynman on vortices in rotating helium. Since Landau constructed the theory of superfluids, dealing with helium, he believed he must know everything about liquid helium. So he read Feynman's paper and he came and he said: "Of course Feynman is right, and what we published with Eugene Lifshitz is absolutely wrong." They had considered a system of concentric cylinders in rotating helium. And so I went and read Feynman's paper, and understood that it was exactly what I had proposed some years before. And I came and said to Landau, "why do you accept this from Feynman, when you didn't accept it from me?" Landau said I had done something different. Then I gave it to him and he understood that it was exactly the same. His attitude was, "You should have explained it to me."

This paper of mine was translated into English, but nevertheless, it did not attract attention. And people were accustomed to what Mendelssohn published, the inhomogeneous model. Then, however, they found alloys with very high critical magnetic fields and started to investigate their properties. And so, after that there existed an experimentalist who worked in Grenoble in France, Bruce Goodman, an Englishman. He published his theory, which was something like an intermediate state. And then probably somebody drew his attention to my paper, and then he published another paper, very strange, where he gave a more thorough explanation of his own theory, and my theory. Then he compared both with experiments, and came to the conclusion that my theory fits the experiments much better than his own, and just to demonstrate that, he published his paper.

3.2.7 Acceptance

You understand that such things never happened, they never happened before, and never happened afterwards. So after that people started to refer to both papers, and after a while Goodman's paper was absolutely forgotten. So that was how it developed. Nevertheless, experimentalists did not believe in vortices and vortex lattices. Then came experiments with neutron diffraction by the group of Cribier in France, but still experimentalists did not believe it. Neutron diffraction was not actually, at that time, something what everybody knew. So they didn't really believe that neutrons could find the diffraction.

Only after two Germans, Essmann and Träuble, did decoration experiments in 1967 where you could see the lattice, could people get accustomed to the fact that there exists some periodic structure. After this my theory was accepted. This happened ten years after my work was published, in '67, 14 years after I did it. So when people say that I got the Nobel prize too late, I reply that it was very hard to get recognition. And I can tell you what I'm doing now, which is the theory of high temperature superconductor. I can explain everything, and nobody recognizes that. And that was all my life so. Therefore, I behave very peacefully, that's why I'm still alive, have no heart attack, because I consider it as natural that people don't want to understand my theories. I mean, they are very simple to understand, but people don't want to understand.

So now, in the sense, finally and gradually there was confirmation, and everybody agreed that I was right. And then people started thinking about pinning and various other aspects. Eventually the work on these type II superconductors, in particular on vortex lattices and even more so for high T_c vortex liquids, all became so popular that now such lattices are everywhere.

I should add that Anderson and Kim did their work in the early sixties, and they believed in me, but they were theorists. They believed my work.

Now you see people speak about vortex matter as some special stuff, which in some sense permits us to understand new problems. The vortex state and vortex lattice is really something beautiful.

3.2.8 Side Issues, the Bomb, KGB, the Prize

I had no contact with Sakharov; I was too young. Sakharov was also not in nuclear physics from the start. He joined it when it came to the fusion bomb, mostly. And it was so that he actually was a student with Tamm, Igor Tamm, who got the Nobel Prize for the Cherenkov radiation together with Pavel Cherenkov and Ilya Frank. And Sakharov told Tamm that if he would be allowed to participate in this work, he had some ideas. Then Tamm proposed to include him in this work and it was successful. So Sakharov was very active.

I cannot tell exactly what was the reason why the Soviets came ahead of the Americans. It is a bit complicated. One of the things was that it was Ginzburg who actually had the idea, not the Americans who relied mostly on deuterium and tritium for the fusion bomb. There were various complications because tritium was unstable, and so Ginzburg proposed lithium and hydrogen. Both of these substances were common and if combined, they could actually decay into helium nuclei in a fusion process, producing very large energies. But also some major difficulties were absent with that method. This was in the late 1940s. They had the Landau group performing calculations. I was at the same institute now. Landau wanted to attract me towards this work, but he didn't get the permission from the KGB. It is true that Landau had been jailed in '38. But he was released after some time, because Kapitza behaved very smart and insisted on it. Now it was a different situation. Already they had made some calculations, and Landau wanted to attract me to these calculations. But KGB did not permit it. And only afterwards I learned, although not with complete definiteness, that this was because of the brother of my father, whom I had never seen and even did not know about his existence. The revolution had happened when he was an assistant ambassador in Japan. He never returned to Russia. He stayed in Japan until the end of World War II, and then he moved to the US and died shortly after. His name was Dimitri Abrikosov. But he wrote very interesting memoirs. I got these memoirs much later, and found it to be an extremely interesting book. It was originally just a manuscript which was found at Colombia University by a specialist on Russia of that period, by the name of Larsen. That person edited the manuscript and published it under the title "Revelations of a Russian diplomat." That is a very interesting book, but somehow I didn't know that person, my uncle. I didn't know about his existence. Nevertheless the KGB considered me not suitable to work on the bomb.

Anyhow, the Landau group made their calculations, and that helped a lot. So therefore, when they made practical tests of the fusion process, everything worked exactly as predicted in these calculations. This was half a year in advance of the US. The US was working on a static, on-the-ground device. But the Soviet bomb was already transportable, thrown from a plane, and everything worked perfectly.

You might think that the propaganda machine of Stalin would have influenced me. My answer is it did not, even though I became a member of the communist youth. But so was everybody. That was a condition in order to get into a university. But I never entered the communist party. I am reminded of a saying; that a person who at twenty years of age is not a member of the communist party has no heart, but if he is still a member at the age of fifty, he has no brain.

To answer your question, how long I have been waiting for the Nobel prize, I can tell you that somewhere in the early seventies Professor Roald Sagdeev who was the head of Institute for cosmic research in Moscow, told me that he got an invitation to nominate a candidate for the Nobel prize, and he wanted to nominate me. That probably was the first nomination. So one can say I have been waiting for thirty years.