Zhores Medvedev

Personal Details

Name	Zhores Medvedev
Dates	Born 14/11/1925
Place of Birth	Georgia (USSR)
Main work places	Obinisk (USSR), London
Principal field of work	Radiation genetics
Short biography	See below

Interview

Recorded interview made	Yes
Interviewer	Peter Harper
Date of Interview	21/02/2006
Edited transcript available	See below

Personal Scientific Records

Significant Record set exists
Records catalogued
Permanent place of archive
Summary of archive

Biography

Zhores Medvedev was born in Tiflis, Georgia, then part of USSR and was brought up in Leningrad. After army service during the war he studied at Moscow Agricultural Academy and after obtaining his PhD in 1950 worked on problems of aging in relation to error accumulation in proteins and nucleic acids. In 1963 he was appointed head of the molecular radiobiology laboratory in Obninsk. His 1962 book ‘The Rise and Fall of TD Lysenko’ and subsequent books and ‘samizdat’ activities led to his arrest and psychiatric hospital detention in 1970, but he was released following protests by many eminent scientists. After coming to London for research at National Institute for Medical Research, Mill Hill, London in 1972, he was stripped of his Soviet citizenship. He remained at Mill Hill until retirement in 1991 and continues to live in London. His Russian citizenship was restored in 1900 by President Mikhail Gorbachev. His twin brother is Roy Medvedev, historian and activist.

Interview with Dr Zhores Medvedev Tuesday 21 February 2006 at his home in Mill Hill, London.

PSH. What I would like to do mostly, if it's alright, is to concentrate on what you can tell me about Human and Medical Genetics in Russia. How it was first destroyed and then anything about how it was kept alive and then brought alive again, and I am very pleased to note that other people at the British Library have done a biographical interview. I won't try to do that, but could you perhaps tell me what was it that first interested you to go into the field of science at the very beginning.

ZM. I was interested in science since I was actually a teenager. The first books in biology I read when I was 13 or 14 years old, before the war. I was very impressed by a book by Academician Bogomoletz which was called Prolongation of Life, and it was published in 1938. So I became interested in this field and in 1940 I read a very impressive, large book, really not popular like that of Bogomoletz but a serious book, the problems of ageing and longevity by Nagorny, a Russian professor from Kharkov. So this developed my interest in this field, but for many different reasons, practical, during the war I was in the Army, I was wounded. In 1944, I was able to apply for higher education, and I considered to go to Moscow University for general biology. But I did find that Moscow University was kind of elite type of university and was very little chance, despite the fact that there were some privileges for war veterans etc. So I immediately started to feel they might consider me as not politically suitable, because my father was arrested and died in the camps, so I was in the group of so-called members of families of enemies of the people.

PSH. May I ask, was your father a scientist or medical person?

ZM. No, he was military. Here is his portrait here, on the top. He was in the Army. He was high ranking officer. He was arrested in 1938. There was a very serious purge in the Army during Stalin.

PSH. I read that.

ZM. You probably read about this. So he was a victim of this purge and I, as a member of the family, in Soviet time, during Stalin's time, because of all this, where you go to university, school, job. You fill questionnaires which ask your biography. Who was your mother? Who was your father? etc etc. And in my case this was a kind of a liability. So I selected agricultural academy in Moscow. Quite old, and very, very good from the point of view of educational standards, and much better actually than university, I discovered later. And I selected the faculty of agrochemistry, so it gave me biochemical background into chemistry etc. University probably gave me more wide knowledge of histology, general biology, anatomy, all the things that were probably better. But agricultural academy in this faculty was alright. My wife graded from the same academy as well. So after this education, again I felt that I might not be accepted into the postgraduate or graduate student, again for political reasons. So I started research early with Professor Zhukovsky. Again he was in the middle, a botanist. He supported me very much. He suffered

himself from Lysenko, and he did help me to get my degree quite quickly without any official graduate 3 years study. So I got my degree quite quickly.

PSH. Before you did your research, the teaching in the agricultural academy, was there any teaching of classical genetics, or had it all been removed by that time?

ZM. No there was classical genetics and Professor Zhebrak, it was essentially plant genetics, animal genetics as well. It was an official course of genetics. We were recommended textbook of Dobhzhansky, and Dobhzhansky was the kind of textbook on genetics before1946 approximately, 1947. And Zhebrak was an agricultural geneticist. He became well-known for his works on creation of polyploid wheat, polyploid crops. New crops. It was popular at the time. After1948, when Lysenko took over with his famous meeting of the . . . Zhebrak was dismissed and this department, or this chair of this course was given to Lysenko himself. So Lysenko became Professor of the Timiriazev Agricultural Academy and he started to teach genetics. It was Lysenko genetics, but I got through this course before Lysenko took over so, in my case I did study genetics as it was in Dobhzhansky book.

PSH. Was Dobhzhansky's book then written in Russian at that point?

ZM. No it was translated. Dobhzhansky defected in America. I don't know whether you know his story?

PSH. I know it approximately.

ZM. Dobhzhansky was a very bright young geneticist and at the beginning of the thirties or end of the twenties, Russia was more liberal and there was an exchange programme with the Americans and the Rockefeller foundation subsidised several young Russian geneticists to go to United States to study genetics. All of them, including Zhebrak, was there as well. Dobhzhansky decided that he does not want to return back, so he was considered as a traitor, etcetera etcetera during Stalin's time. But nevertheless his textbook on genetics was considered the best in the thirties. I don't know about later time.

PSH. And did you study chromosomes, for instance?

ZM. Yes I did. It was part of the actual first year in botany, so the plant cytology and botany was part, so we did study microscopes, all this preparation of meristems and plants in genetics, yes. The chromosomes were included, of course.

PSH. To me that is interesting

ZM. It was 1945/'46. It was a very long year so the course of botany, which was, the first year you studied the general discipline like botany, chemistry, organic chemistry inorganic chemistry etc etc, and the botany was practical and general botany, and the botany did include, this is my Professor Zhukovsky textbook. It is a later edition 'Botanika' and you find a lot of things here, structural yes, cytology is part of the . . . so you study all this meristem, cell division, chromosomes.

PSH. Because this was quite a few years after the problems, say around 1937, when Levit and colleagues, a lot of persecution had already happened by then.

ZM. Yes that's right, but nobody yet considered chromosomes as something which was not linked to inheritance. In the 30s there were two fighting camps and each camp got his own base, institutions, laboratories. The total ban on classic genetics, including chromosomes, came only in 1948 after Lysenko took over and everything was forbidden. People stopped to consider chromosomes as part of the inheritance and the field became unpopular. But when I was a student of the first year and second year, I did study chromosomes, all this meiosis, mitosis and all the things linked to the inheritance. So polyploidy was a very popular field of research, so this was not forbidden. People were working in this field.

PSH. You must have been one of the last people to be able to have a training before it became impossible.

ZM . I don't know. In medical science probably it was a complete ban, but in botanical science you can't actually try, because this is descriptive science. It describes, cell division, how can you describe cell division without chromosomes or meiosis. So nobody tried to tell that meiosis doesn't exist.

PSH. Meiosis.

ZM. So the descriptive part did remain in, let us say, in botany and plant physiology. In medical, I don't know how in medical institutes they teach this field, because in medical descriptive, it includes probably meiosis as well.

PSH. But I think in those years nobody really could work on human chromosomes still for another 10 years, because the techniques for looking at human chromosomes didn't really – well I'm wrong there because in Russia they did exist actually and maybe that was going to be one of my questions which maybe I could ask at this point and this was that while in Western countries human cytogenetics only developed in the 1950s, in Russia in Levit's Medical Genetics Institute there was very strong medical cytogenetics, with people like Andres, Navashin, and I was wondering whether you had any information on that group of people. What happened to them?

ZM. Most of them were arrested, as I think I did write in the Rise and Fall of Lysenko because Professor Lerner from California University who translated the book, he dropped quite a few parts of the original and so the English version is not actually a full translation.

PSH. Was that because he was afraid it might cause damage to people?

ZM. No, just because he wanted to concentrate on genetics so medical genetics was already not general genetics, so he dropped parts linked to agriculture and many other fields, several other fields. So it's about 60% of the original text actually.

PSH. Was it ever translated, that other part?

ZM. No the full Russian text was published in Russia only in 1993, about 13 years ago and it was not translated any more, because nobody was interested to translate now Lysenko is gone.

PSH. But still anything about those early workers, especially the ones in the Levit Institute would be very interesting to me. So maybe I should get, the Russian version is in full?

ZM. It's full, yes, but as far as I know the history of human genetics was not written yet in Russian. I don't know whether Bochkov and others can tell you about this, but my very good friend, Efroimson, who died in 1990 at the age of 80, he did publish 3 books on human genetics and I have one of his books Immunogenetics, which doesn't discuss this problem. But his large book on human genetics and general, in Russian is available, not unfortunately here, but you can get it. I can give you the name and you can try to find it through the British Library – its Russian edition.

PSH. That's very helpful, because I have some friends who are Russian speaking and can translate at least parts, a chapter or something like that. That is very interesting. But did you receive any personal information about the people in that group of the medical genetics institute in Moscow?

ZM. Not really, because when I became interested in History of Genetics which was late fifties, there were very few people left, because I have been friendly with son of Agol. I don't know whether he is alive or not, but he gave me some pictures, some photographs.

PSH. His father was shot, is that right?

ZM. Yes. So all this group had been liquidated, but when I did write about this I didn't try to write about negative parts of this group, because this group actually was active in the twenties in eugenics. They created the Russian eugenic journals. You probably can find it somewhere. It's a quite interesting journal. For several years it was published in Russia, and it is full of such nonsense about, they tried to advocate the possibility to select better human beings than we have at the moment, by the use of genetic methods. So you can see it there. And the last person who made this absolutely stupid decision was Professor Muller, who came to Leningrad to work with Vavilov, and then Moscow as well, and he was obsessed with the idea actually at the time, that the socialist country was very good to select the better human being. And he prepared in 1936 a very thick letter. I may be able to find it later in my files here. It was in Russian, it was translated from English original but you can find it in Muller archives I believe. Yes Muller archive was deposited with University of Illinois.

PSH. I heard about that letter and he rashly sent it to Stalin.

ZM. Letter to Stalin. It's a report about 25 pages long, and where he explained that socialist country with dictatorship, with this system when people do what the government tell them, it's very good to select the superior human

beings. He was explaining how to do this. With all the details, so we can select the sperm from the best people, like him probably, yes. There were some jokes and there were some critical papers in newspapers about this approach. Ideas that, who would be considered the best, so eventually we will see. The one person everywhere yes. So this was a spark but the problem was they got such bad publicity because they advocated this approach. I am not surprised that when terror really started they'd been considered as the people who were racial. When Hitler emerged with his crazy ideas of Nazi Germany etc etc, this whole group became vulnerable because they did advocate similar ideas. There's no question about it. So I am not surprised that during the Stalin terror that somebody did write that they have the same fascist ideology, Nazi ideology the creation of artificial, superior etc etc. We are all working class people, would be considered as rubbish. So this did happen. You don't find it in my book because it's anti Lysenko, the book. But if you write objective history, you can't afford not to look into this and Muller was very instrumental because he was most famous at the time. He was not yet a Nobel prizewinner, but nevertheless he was a famous geneticist. You heard about his letter.

PSH. I heard about it, yes.

ZM. Letter to Stalin.

PSH. Yes I had. In fact I have read Muller's biography by Carlson.

ZM. It was published in Russian. It was published in Russian in a Journal which published most interesting parts of central party committee archives. They did find Muller's letter to Stalin and they published it. So it was published in Journal 'Istochnik'. Again I have archives of this, partly in Obninsk, where I have a flat, partly here. I can check and find it yes.

PSH. I would certainly be very interested if there is an English version, but maybe I can find that in the American archives.

ZM. No, in Russia it was Russian version, because you can't send to Stalin the English version. It was translated. Not by Muller himself but somebody else. So to central committee, to Stalin, a Russian version was sent officially and was kept in archives, and recently was published again in Russian. But Muller apparently did prepare the English version and he did publish actually something, I remember I read in Science long, long ago, something along similar lines. Yes he did publish in the sixties or . . .

PSH. I remember that and he wrote a book called "Out Of The Night" in which he proposes eugenics. Leaving eugenics and thinking just of the science, of the people who were working at that time along with people like Muller and Vavilov and Levit, the only person who I can find any record of in recent years is Raissa Berg, and I read her book and somebody told me that she is still living in Paris.

ZM. Raissa Berg, she was my very good friend actually, yes, and I met her in Leningrad just because she was wife of my close friend Kirpichnikov. She has changed three husbands and one of the husbands was Kirpichnikov, a

geneticist and, she had two daughters from him; then he married somebody else, but her two daughters are from Kirpichnikov. And when I was already here, it was 1977, being in correspondence - not many people continue to correspond with me, but Raissa Berg was not afraid. Efroimson stopped writing me letters, to answer my letters. For some reason he was afraid, because he was in prison, so people who have been in prison they have this complex of fear.

PSH. That's forgivable I think.

ZM. Yes. This is forgivable, but Berg was always considered kind of very important because her father was famous, a famous scientist. She was in Novosibirsk and then she moved to Leningrad back. She was unhappy that they didn't want to
carry some kind of great experiment she designed on Drosophila and she was offered to retire, because she was already 61. You can't do anything if you are 60 because it is legal age. You can, if officials will allow you to continue, you can continue. Here you have to retire anyway if you are 60 and a woman. I have some science friends from my Institute and the women scientists retired at 60 at my Institute, and men were allowed at 65. So she was retired, but she was full of energy, so she decided to emigrate and to continue her research in America. I did write to her 'it's a crazy idea. In America you never get any good job at your age, so you will be unhappy'. But she didn't listen, she did emigrate as Jewish immigration. I think she is not Jewish. Berg is German mostly, more than Jewish. But it doesn't matter. She emigrated through Italy and she was treated quite well. In Madison University she was given a grant for 3 years. They told her, we can't give you any position. We will give you a grant for 3 years. She continued to work with Drosophila and after three years she didn't publish anything, so they told her we can't extend your grant because you didn't produce anything. She told them: I am preparing, I will do this. But nevertheless her grant was over and she was very angry. She was writing angrily. I met her there, I had been in America at Madison University. It was a very nice department of Genetics, Professor Crow.

PSH. I've met and spoken with him. He is still very active. He is more than 90.

ZM. Yes, he is a very old man now. So she moved from there to St Louis University, where she got a grant for 3 years as well. So she continued some work and again didn't publish it. Because in America molecular genetics is already, old kind of classic Drosphila genetics,

PSH. Something of the past.

ZM. After three years she was given very small social security benefits, small flat in St Louis but not in the best part of the city because she was not working there, so she wasn't happy because she couldn't drive. People around her were mostly blacks and so after she had been in correspondence . . .

PSH. So she moved to Paris?

ZM. We did correspond but then, for some reasons I don't know, age maybe, she stopped writing. Answer letters yes, but maybe she changed her address, but from Paris she didn't write. Because her daughters emigrated, her both daughters emigrated. One lives in America. The other lives in France. They have been here, Raiisa Berg was in London and her daughter was here. It was 15 years ago, long ago. She is still alive.

PSH. I spent quite some time in Paris.

ZM. If she had been in '77 she was 61 so now she is . . .

PSH. About 90.

ZM. Yes about 90.

PSH. Coming back to your own career, I've rather diverted you from that, but after your spell in research at the Agricultural Institute in Moscow, what was your next piece of work and post.

ZM. My first job actually was in Yalta, in the Botanical Gardens. In the Soviet Union when you graduate from higher education, there is so-called distribution, official distribution. You have to work 3 years in a place where you will be sent. So because I had some kind of privilege, I had already published some papers on plant physiology, plant biochemistry and my Professor Zhukovsky was kind of botanical supervisor of the Nikitski Botanical Garden in Yalta, so he arranged for me from this botanical garden gave a request for Medvedev. I had been there in '48 as a student, so I get quite nice appointment not to collective farm, state farm to be agronomist, which could happen, but to botanical garden, and I had been there for two years and returned back to agricultural academy in Moscow and did find a job there at the biochemistry department, plant biochemistry. After my book on Lysenko it was typewritten version, circulating

PSH. In the Samizdat.

ZM. Samizdat yes, there was some pressure on me to retire, to resign because Timirayezev Academy was vulnerable. They didn't dismiss me, because I was successful in this field. But they started the pressure because you are in our department. We were asked by party officials to explain what you are doing here.

PSH. What year was it that you first wrote the first version of the book in Russian?

ZM. It was in 1962. And it was circulating quite widely. It was published in the Russian language in Germany, in one of the émigré journals. The final text I sent to America myself through the joint friend, actually Åke Gustafsson [University of Lund], geneticist, was in Leningrad in 1967 and he took a microfilm of my book.

PSH. Was this Gustafsson from

ZM. Genetics.

PSH. From Copenhagen.

ZM. Not Copenhagen, from Sweden.

PSH. Sweden. I'm trying to think. I know a Gustafsson. I think it must be the same one.

ZM. Now he is old man. Probably have his address here. (Looks for papers). He is quite a famous plant geneticist.

PSH. I think it's a different one. The one I know is more human cytogenetics.

ZM. Åke Gustafsson, Institute of Genetics, University of Lund, so it's Sweden.

PSH. By that time were you already then working in Obninsk?

ZM. Obninsk yes. I started to look at other jobs with some promotion as well. I considered work in Kiev. I had an invitation from Kiev, from Novosibirsk and Obninsk, all in medical science, because this was the time after Khrush-chev's fall, when molecular biology was popular, and in many other academies around the country they tried to create laboratories of molecular biologists, and Obninsk was quite close to Moscow, a small, very nice town, scientific town. Like academic town in Novosibirsk.. And it was a new institute, a very large one and very generously subsidised, so I was offered this job to create a laboratory of molecular radiobiology and I did accept. So from 1963, I was in Obninsk, at first just organising labs and in 1965 it was organised and I became head of the laboratory and it was part of genetics, a division of genetics which was headed by Timofeef-Resovsky, very famous scientist and Bochkoff and others were all there. So we have been very very friendly with Bochkoff and all these people you met. They probably didn't tell about me.

PSH. Oh they did, in fact they remembered you very warmly and specifically said, would I please give their regards when I met you, because I met them, all three, in Moscow.

ZM. They are not human geneticists, this is the problem.

PSH. That is very interesting to me, because one of the questions which I have asked them but I would also ask you, is, how medical and human genetics managed to grow from this rather specialised root of radiation genetics?

ZM. Just because there was nobody available who would know human genetics, and radiation genetics was considered human, being a kind of effect of radiation on humans first, but Timofeef Resovsky's group was classical genetics. Timofeef Resovsky himself, all his life he worked with Drosophila and nothing else. And Ivanov was Arabidopsis, plant genetics. He created a

small laboratory that studied genetics of Arabidopsis, and at the second point it was 1964 and Timofeef Resovsky who was head of the Division. The Division included my lab, the laboratory of radiobiology, the laboratory of genetics and immunology as well. So we are kind of senior people sitting withTimofeef Resovsky, and Timofeef Resovsky said that he is under pressure as head of the division for two reasons. First reason is there is no party member in the whole division and this is absolutely unacceptable for the local officials, and so the director of the institute and medical academy pressed him that there should be at least one party organiser. It's necessary, compulsory, and he said I don't know anybody who is a party member and a geneticist as well. So they started a kind of search, who would be acceptable to invite to work, and I don't know who did find, that there was such person as Bochkov, who works in Sukhumi with the primates. There is a kind of research unit in Sukhumi into the genetics of primates and primates are close relatives of humans. So we should invite him and he is a member of the Party, he is good standing PhD and, he has a degree, so we will help him. So Bochkov was invited and he accepted very happily the invitation from Timofeef to come to Obninsk and to start a kind of medical genetics. It was not called human genetics. It was medical genetics. He created a small group and he was sent to America for one year to some institute and the decision was made, I think it was 1987/88 to create an institute of medical genetics. It is called now human genetics?

PSH. I think it's still medical genetics.

ZM. To create institute of medical genetics in the academy of medical sciences. There was nobody available to be the director of the Institute, so the President of the academy or vice-President of the Academy, I don't remember his name now, was appointed as director and Bochkov as his deputy. So Bochkov moved to Moscow and became deputy director of this institute and this academician, I think died soon after and Bochkov became director of this institute, and when Timofeef Resovsky was sent into retirement, when he was 66 or 67, the genetics department started to disintegrate and the people from this department started to move to Bochkov. Because Bochkov again was Director of the institute and he had problem to find, to recruit people with some knowledge of genetics, because nobody knew human genetics, so he invited all this, Ivanov, Ginter and everybody else from genetics in Obninsk. So genetics in Obninsk was completely destroyed.

PSH. That's very interesting to me.

ZM. And how they are doing there I don't know.

PSH. They have done well.

ZM. Bochkov, I met him here. In Britain there was a conference in, actually in Cambridge, and I was invited to attend or I came to attend as I was a member of the Genetics Society and Biochemical Society for some time here. I'm still a member but not Genetics. And someday I met Bochkov there, and he was friendly, but he refused to go for dinner and he told, Zhores, it was still Soviet

time. So he was afraid to talk really. Old friend as he was. So I was very disappointed. But he was a member of the Party actually.

PSH. Did it cause some difficulty in the Department to have somebody who was a Party member and therefore might be sending back reports or something.

ZM. We considered Bochkov, that he doesn't do this. I don't know exactly, but Timofeef Resovsky trusted him, to a certain point, but I could feel always that he was restricted to a certain extent. He was not a free man to discuss all the problems. I don't know whether he was writing reports or not. It was all possible. Anyway I was dismissed from this Institute myself after Lysenko book was published in America in 1969, I lost the job anyway. And the last two years before my trip to Britain I worked not far, Institute of biochemistry, physiology and biochemistry of farm animals. It was protein laboratory there. Studying proteins and I got the job as Senior Scientist. I'd been there for 2 years before I left.

PSH. How did it happen that you came to Mill Hill?

ZM. Oh, again by accident. I used to get invitations from some other areas but most had been denied by officials. Among gerontologists I became known in 1963. I did publish a review, a theoretical paper in Russian, which was called the molecular mechanism of ageing, where I developed the theory that ageing is accumulation of errors of protein synthesis, as a result of other errors, in DNA, RNA etc, but essentially we accumulate errors and I tried to speculate how it might happen. So this review was translated and published in the United States by the Institute of Ageing now, Gerontology centre at the time. And I was in correspondence with quite a few people, including Robin Holliday, who was head of the division of Genetics.

About the same time in 1963, Orgel, American scientist, published in the proceedings of the National Academy of Sciences, the paper which calls ageing an error catastrophe, the error catastrophy of ageing, which became better known, first of all because its kind of dramatic error catastrophe as he tried to explain his theory, how its after a certain point error catastrophe happened, and Robin Holliday was obsessed with the theory and when he became head of the division here, of genetics, he tried to prove, with, you know Hayflick limits? The divisions.

PSH. Yes the generations come to an end

ZM. So Robin Holliday was convinced that this was error catastrophe, so molecular and cytological pictures look similar. So Robin Holliday started to try to prove, but he was a plant geneticist. There was nobody in his division who knew proteins, protein biosynthesis, worked in protein biosynthesis field as experimental field, so he did write me a letter. I sent him a reference in English and some other reprints which we published in English about nucleic acids and possible error accumulation in nucleic acids. So he sent me a letter of invitation to be ready to come to his lab and work with him for one year etc etc because he needed somebody who has experience in protein biosynthesis research for his project. I did write that I would be quite happy,

but it was quite difficult, and eventually I got an official invitation from the Director of the Institute, Professor Arnold Burgen, but it was denied. I asked permission to travel to London for one year research. But after some developments, after I published a book about, I was in a mental hospital for some time and I published some other works, in the West, when I still worked there on history and political problems. Macmillan here published 'Medvedev Papers' in 1971, so the officials realised that I would continue to do such writings and it's better if I was outside than inside. So I expected I was invited to the Director's office and he told me, sometime ago you were invited to work in the Mill Hill Institute, and we considered this as negative, now we have changed our minds and we would allow you if you are still interested. I told them no. There was some episode at the Kiev Biological Congress in '72 where I was refused to give a lecture which was in the programme. I was invited by the International Association to present, actually to give a lecture, not paper, but lecture but officials didn't want me to go. I was detained for a couple of days to prevent me, so there was very strong conflict between American gerontologists and Russian about Medvedev, so I think officials decided that Medvedev was an irritant and it was better that he would go, because there was no reason to arrest me or to do something else, and they probably considered that Medvedev was already very prominent, which was not the case, but nevertheless within the gerontological groups Medvedev was already known for his works. So they decided that it was better Medvedev be given permission and they did this, then sent me my passport, so this is the general outline of the story.

And here I came to Robin Holliday, and he wanted me to prove that ageing is a kind of error catastrophe. I explained to him that error catastrophe was fiction . In proteins errors are, I got a grant for one year and I told one year was not enough. First of all there was not a single old rat or old mice in the whole of Britain. I didn't mind that I can't work to compare young and old. I told him that I am not interested to work with Hayflick's cell culture. Hayflick is good friend of mine yes, but I told him that I don't consider this as real ageing, it's cell culture. It's cell culture ageing, something happens, but it's not ageing of the complex system. Now they try to link Hayflick with telomere and other things. So I prefer to work with the real mice and rats. But there was no colony of old mice, it takes time. I told him that it is very difficult to prove accumulation of errors because they are random. You take the proteins, you analyse the amino composition, it would be the same. But because one molecular level is changing, the other molecular level changing in other ways, the protein is not the same. But he himself made the kind of discovery, because he did find that specific activity of some enzymes change with age and he considered this was a result of errors. He wanted biochemical proof, but biochemical proof is too difficult to get, nearly impossible. Now with new techniques it may be possible in DNA but I am not certain about this.

PSH. Can I bring you back to Russia and ask you about one part of the field which has interested me, and this is how, after 1948, geneticists were protected by people working in the radiation field and the involvement of people like Sakharov.

ZM. No, Sakharov was not involved in protection. Sakharov was a very isolated figure at the time. Because Kurchatov was a kind of Tzar of the nuclear science and nuclear bomb project, his institute became very very prominent and in Kurchatov's centres, secret centres in Ural . . .(speaks to wife).

PSH. So it intrigues me how . . .

ZM. All the nuclear centres, nuclear bomb centres, they had very poor protection from radiation and all the kind of industry works used to produce radioactive isotopes. There were no regulations, there was no protection and in Kurchatov Institute as well as in the secret centres where they produced the bomb, the uranium and plutonium etc, they had an increased level of cancer, radiation sickness and many other things happened there, and they did realise that they needed radiation biology, radiation genetics. So it came as a result of actually very high level of mortality and cancer among the scientists and workers and engineers and everybody else involved in this. So eventually there was certain result of discussion. Professor Dubinin was instrumental to a certain extent, Khesin who has died now, instrumental to approach Kurchatov with some advice and Kurchatov decided that he will create the department of biology in his Kurchatov Instititute. And because of unlimited funds available, they built a beautiful building, the biological centre of the Kurchatov Institute, where they invited quite a few geneticists to work with radiation genetics and general genetics as well. So it was a result essentially not because they loved genetics so they wanted to support them, but because they needed it.

PSH. And were they able to do this despite Lysenko? Did they hide this from him or was . . .?

ZM. No.

PSH. It must have been then from a very high level.

ZM. Lysenko, and then after Lysenko, Olshansky, one of his supporters, was president of Agricultural Sciences, so his administrative influence had reached this whole system of agricultural sciences and through appointments made after 1948, he was able to make appointments at the Leningrad University, his followers and some editorial boards and some publishing houses as well. But nevertheless in the Soviet Union the system works in a very peculiar way. Lysenko himself was not able to exercise his influence directly. He was able to direct his influence through the central Committee, where in the science department and in the agricultural department were people who work from his kind of clique or appointment. Kurchatov is entirely different. It is so called Minister of atomic energy, which was responsible for nuclear power stations, nuclear bombs and it was entirely outside of Lysenko's influence, and outside of Central Committee Departments, because during this time there was such importance of atomic energy, nuclear bombs, space science, rocket science, so each of these fields was controlled only by the whole Government. In Soviet Union they created a kind of mini Soviet system. Each field had to have some undisputed leader who was the boss, who was the Tzar, so Lysenko was the Tzar within his kingdom, but this

kingdom was different and here the Tzar was Kurchatov. You can't do anything. Even Khrushchev was not able do anything if Kurchatov tell it's, we can't do this. We will suffer or the project would suffer as a result.

So Kurchatov had the power to appoint and he had unlimited access to the money. The decree was passed after the Americans dropped the atomic bomb. It was, I think, October 1945. Stalin, the Central Committee and all the Government, they passed special decrees where they gave the head of the atomic and the space, rocket establishments the power to make appointments which they considered necessary without any approval anywhere else, because all these appointments were secret and you can't actually ask Central Committee permission "I would like to appoint Mr such and such to such and such position", because then Mr name will be revealed. So until the death of Korolev nobody knew he designed all these rockets and sputniks. So nobody knew who was in charge of the sputnik, or Gagarin, who prepared him to fly. Nobody knew. Kurchatov's name became known only in 1956 when suddenly Khrushchev decided to take him to Britain on his trip. And this made Kurchatov known. Before this Kurchatov was unknown name. Nobody knew that Kurchatov does exist. But for some reason Kurchatov became known as the head of the atomic project, but the head of the missile project, Korolev was not known until he died, in 1966, I believe.

PSH. So apart from Timofeef-Resovsky, who were the other geneticists who came to work under this scheme related to radiation and atomic energy. Were there other previous senior geneticists who came in?

ZM. No, the most senior was Dubinin, academician who got an appointment first in Novosibirsk and then he moved to Moscow, and I don't know anybody else there who is considered as very famous, because I was very friendly with Khesin. He was a biochemist. He was interested in protein biosynthesis. He was not a geneticist.

PSH. What was his name?

ZM. Khesin. Roman Khesin. He has died now. He published a very interesting book, The Biochemistry of Cytoplasm. Prokofiev-Belgovskaya was quite prominent who was in this group, and who else? Prokofiev-Belgovskaya you know?

PSH. I do, because she was very much involved in the development of cytogenetics.

MZ. And to a certain extent a beneficiary of this programme was Astaurov. You know Astaurov?

PSH. I know his name.

ZM. Academician Astaurov. He is partly cytologist, partly geneticist. He created the Institute of Developmental Biology within the Academy.

PSH. And were they also working in Obninsk?

ZM. No. They had been in Moscow. They had all been in Moscow. Timofeef-Resovsky was invited to Obninsk just because he was former German citizen. He was never rehabilitated, so Moscow was closed for him, otherwise he would be working in Moscow.

PSH. Can I ask one thing which I read only recently. I managed to find a copy of your book on the Ural disaster and that interested me in terms of, well to start with, it was something even now I did not know about, but it interested me in terms of how this disaster might have provided a stimulus for research into radiation genetics.

ZM. It didn't, because they started to invite people to work in this area five years later, after that disaster. Before that it was secret, so eventually it was decided because of the human problems of populations who lived inside, outside, probably some kind of problems emerged with the cancer, radiation sickness, and so on. It was decided to create a kind of biological station which would study genetics, effect of radiation on humans and plants and animals, on ecology etc etc, and actually the first person who was considered as appropriate as a head of this research was Timofeef Resovsky, because Timofeef Resovsky at that moment lived in Sverdlosk and was head of the Department of Genetics of Ural Division of the Academy. He was in the camp, in Gulag, but then he was released in '55 together with the Germans and he was left there and continued to work there. So when it happened Timofeef Resovsky was the only qualified person to study this. But even Kurchatov was absolutely helpless as far as if the security assistant told him that this person was a security risk. You can't appoint him to head this project. So Timofeef Resovsky was quite enthusiastic. He did actually make many projects himself. He did write that this gave an opportunity to study the effect of radiation on everything, so his reports were considered at different levels and probably stimulated the creation of this project, but when it was discussed who should be head of this project, Timofeef Resovsky was not approved, because they considered him as a security risk.

(Telephone rings)

PSH. I was admiring both your books and your collection of carvings.

ZM. Oh, the carvings are my older son's, who died unfortunately. It was his hobby.

PSH. They are beautiful.

ZM. He was left in Russia. It was his hobby. This is commercial, others are not.

PSH. So I was asking again about the Urals. You said in your book really, that all the studies started more than five years after the problem had happened, so that must have limited their value.

ZM. Yes they missed the highest level.

PSH. The things which I had down, I have made a few notes, but we have covered most of them. The first thing was, I mentioned Levit earlier, but do you know, is there any biographical article about him either in Russian or English?

ZM. Only a very short piece. You can find it in a book. I have it, the biologists, biographies of all prominent biologists of the world and Levit is there, yes.

PSH. Is this recent?

ZM. Published maybe ten or fifteen years ago yes. So there is a small half a page.

PSH. But not more than that.

ZM. Not more no.

PSH. Because I was at the 1978 International Genetics Congress in Moscow and one of the things I noticed there was, they had great biographies of Vavilov, which were very long up until about 1935, and then after that there was about one sentence which said he did not work any longer, and even though they had rehabilitated him, they were not saying anything.

ZM. Well this biography published, it's not a recent biography. There is now a whole book about Vavilov in prison and this kind of thing. His KGB file was presented there. How he had been in prison, now a lot of material about him in prison which is not always accurate. They publish protocols of interrogation which were falsified and authors now, young authors, who have access to this archive, they present this as a document, they don't realise this all that protocols are falsified.

PSH. I read the book of Popov

ZM. Yes Popovski, which is very inaccurate. Very inaccurate book. It was an English translation?

PSH. English yes. I don't speak Russian unfortunately, but it was an English translation. Then the other thing which I was . . .

ZM. This book is an English translation of Soyfer.

PSH. Soyfer, I have read his book also.

ZM. It's also a very poor book because he doesn't present Vavilov. For some reason he wants to reduce their status, I don't know why, the same as Popovsky. They don't want to, Soyfer was one of Lysenko's followers for quite a few years. Then he worked with Dubinin. He is not a very good geneticist himself, but nevertheless he has kind of complex: he didn't want to acknowledge some great scientist or important scientist. He wants to be considered himself as very very important, which is not the case. So it's very strange.

PSH One of the people who I find very difficult to assess is Dubinin. It always seems that there is some ambivalence about him.

ZM. Yes, Dubinin considers genetics is OK for animals or plants, but not for humans. Very strange. He did publish his own autobiography where he considered that human genetics is not really science. It's quite strange to understand, yes. I can't understand Dubinin.

PSH. Am I right . . .

ZM. He has kind of complex of, maybe you know the word, that he is the most important geneticist in the whole of Russia, and this was not acknowledged properly so he dismisses everybody else.

PSH. Was he writing, he was working, am I right, before 1937?

ZM. Yes he was a young geneticist. I don't know whether he is still alive or not because he is well over 90.

PSH. I think people would have said when I visited if he was still alive, but the other person who I know very little about but who is mentioned a lot is Chetverikoff.

ZM. Chetverikoff, yes. He died long ago so when I became scientist he was already not alive, but he was considered as the founder of population genetics. You will probably find about him in the Dobhzhansky book.

PSH. I must go back to that book, but I never quite understood how he met problems and was imprisoned at such a very early stage. Was he very outspoken or . . . ?

ZM. No, it's not due to his genetics because he was always a prominent scientist before the revolution. He was an old man, and like Koltzov, he was treated badly in early thirties because he was either Menshevik or he did belong to the wrong party before the revolution and this was the problem. So with Stalin terror in the thirties, people who had been in the wrong political movement before the revolution were considered as enemies or liabilities or not trusted. So his first problems were not linked to research. He was the professor who Timofeef-Resovsky started to work with in the very beginning, but again, like Timofeef-Resovsky he belonged to anarchists before the revolution. So this was the wrong political group. Something he did during the civil war. Maybe he was in the wrong unit. Very complex.

PSH. The other person whose name comes up again later in relation to medical genetics is Davidenkov.

ZM. Davidenkov I know very little about.

PSH. He was in St Petersburg is that right? Because the information I have got was that he was more a neurologist, but very much involved in work on hereditary diseases.

ZM. Maybe, I'll reach for this book of references about these people. You can borrow this book if you find it useful.

PSH. It's in Russian though.

ZM. It is in Russian yes. This is short biographies with pictures, so Davidenkov, I hope he is here. Yes, Davidenkov, but no photographs. Davidenkov, Sergei, Nickolaivitch. He was born in 1880 died in 1961. Soviet scientist in the field of medical genetics. Member of the Academy of Medical Sciences from 1945, was born in Riga, graduated from Moscow University, then worked in the hospitals of Moscow and Kharkov, then in 1912 already Professor of Kharkov Medical Institute. In 1920-32, worked in Azerbaijan Research Institute on professional pathology, and in Medical Biological Institute in Moscow. In 1932-1961 in Medical Institute, Advanced Medical Institute, where doctors come to improve their skill. Yes he worked there in Leningrad and his main research works were on the pathology of the nervous system and medical genetics. He studied the pathology of the nervous system, the control of neural infections, muscle atrophy and neurosis, and he prepared the list of inheritable diseases. He prepared kind of, because during Stalin's time the concept that some illnesses are inheritable was trouble. It was a problem. And he also considered some psychiatric pathologies from point of view of genetics, and small mutations which affect the nervous system, that's all of it.

PSH, Still if you were prepared to lend it to me for a week or two, I could make then a photocopy of the pieces on some of the most interesting people and then I could probably get my friends, from that photocopy, to make a translation of that page.

ZM. Yes, because all the people we talked about are probably here. Who else are you interested in?

PSH. People like Navashin and of course Levit.

ZM. Levit is certainly here. Navashin is here. Sergei, Mikhail, two Navashins.

PSH. Yes. I could check that from publications.

ZM. One died in 1973 and they are actually father and son I believe. Yes because one is Sergei and the other Sergeiwitz, his patronymic, so there is a son and a father. Two Navashins.

PSH. Well that would be extremely helpful if I might.

ZM. And you will find, I think even Raissa Berg is here, maybe not, because she emigrated. It was published in 1984 when people who left Soviet Union were not considered. Bochkov was here.

PSH. And do you appear or not?

ZM. No I'm not here, since 1984 I was not here. Berg, her father might be here.

PSH. I managed to find a copy of his book, which surprised me that I was able to, it was translated it into English

ZM. I have Russian edition. It's not extremely interesting. Berg, Lev. Her father is here. Not Raissa.

PSH. Well it's been very kind of you spending this time and are there any things?

ZM. Chetverikov is here. Chetverikov, this is his portrait. So you can find at least main data about these people. I sometimes use it when I need to find out about some people. It's called 'Biologists' and it's also foreign biologists.

PSH. I read Russian a little but only badly.

ZM. You will find Morgan, Mendel, all be here. Some very minor figures like some people who were not actually important scientists are here, Soviet scientists, because as soon as a person is an academician, I'm trying to find him, I think he is probably here as well.

PSH. That will be interesting.

ZM. No because you can't, Lubishchev who never was a prominent scientist but he is well known among Russians because he was a mathematician, he criticised Lysenko. Lysenko is here yes.

PSH. Well that's very, very kind for you to lend it to me and may I ask, I have a rather old copy of your book but it would be lovely if you could write in it for me. That really would be nice. I appreciate it and meanwhile I will turn off the machine and thank you very much for sparing the time.