

*“There is a truth and again there is a truth...
The truth about us is endless. As are the lies.”*

Phillip Roth, The Human Stain

Thinking of R.B. Khesin*

S. M. Mirkin

Department of Molecular Genetics, University of Illinois at Chicago, USA

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I'm writing this essay because I really loved R.B. The decision to write it was almost instinctive, from the moment I read the E-mail from Vladimir Gvozdev¹ and Lev Kisselev² stating that it could be “free form.” I realized that this was my chance to tell the world what I thought of R.B. being a 20-year-old enthusiastic grad student in Russia during the Stagnation period, and what I think of him now in the U.S., as a far from naive forty-something university professor.

One would wonder, who needs all this? I've told all my students in Russia and in America about R.B. Truth be told, however, once again I use his name and his authority almost instinctively to emphasize the now unconventional views of science, which I still happen to share. Specifically, that all the major discoveries are made by outsiders, that all the best experiments are done by hook and crook, that there is nothing worse for a scientist than publishing an erroneous paper, etc. Thus, R.B. became a somewhat legendary, albeit old fashioned, figure for my students, who had never seen him and hadn't read his papers. The legend of R.B. became an intricate, I'd even say essential, component of my lab. And this legend, say my exhausted students, should be carved in stone so it can again and again be used to educate young scientists in these progressively more pragmatic times.

Truth also lies in the fact that all of us, in time, must repay our debts. After losing so many loved ones this became clear: one may not have time enough to pay back. I remember one Indian Summer day I bumped into R.B. in front of the Biology Faculty of the Moscow State University, and we sat down to

smoke. We discussed my work and R.B. suddenly said sadly, “Now you'll publish your articles, become a professor and forget your teachers.” I haven't forgotten anyone, this essay is my debt of remembrance to my teacher.

When I took on the task though, I realized that I really overestimated myself. I've never written a literary essay, only research articles. Worst of all, especially for a scientist, I don't, and probably never will, have time to investigate R.B.'s life systematically. Everything that you will find below is nothing but my personal, entirely emotional account of some events in R.B.'s life, and some of R.B.'s words for which I have no supporting documents, much less proof that I remember it correctly. In other words, this is my legend of R.B., which might have very little to do with his real life. I ask you to not be offended by the many possible discrepancies in dates, names, and events. If the image of R.B. is at least emotionally correct, that's already not bad.

THE FIRST ENCOUNTER

The first time I saw R.B. was in the Main Biological Auditorium at the Moscow State University, where he taught a course in molecular biology. He was wearing a brown suit, made by Moskvoshveya, and a red checkered button-down with an unbuttoned collar. I was amazed: he looked like either a technician or a ministry's chauffeur. This wasn't merely my impression. Many years down the road, R.B. told me the following story. He was dropping off Alexander Aleksandrovich Baev³ at a meeting of the Biochemistry, Biophysics and Chemistry of Physiologically Active Compounds Division of the Russian Academy of Science. As he walked into the Academy the receptionist

¹ Vladimir A. Gvozdev: Ph.D., D.Sc., Professor and Head of the Laboratory of Molecular Genetics of Animals at the Institute of Molecular Genetics, Russian Academy of Sciences.

² Lev L. Kisselev: Ph.D., D.Sc., Member of the Russian Academy of Sciences, Professor and Head of the Laboratory of Molecular Basis of Oncogenesis at the Institute of Molecular Biology, Russian Academy of Sciences; Editor-in-chief of “Molekulyarnaya Biologiya”.

* Authorized translation from Russian by Vera Mirkina.

³ Alexander A. Baev (1904–1994): Ph.D., D.Sc., Member of the USSR Academy of Sciences, at the time was Academician-Secretary of the Biochemistry, Biophysics and Chemistry of Physiologically Active Compounds Division of the USSR Academy of Sciences.

asked, "Alexander Aleksandrovich, is the chauffeur with you?"

By that time, my fourth year as a biology major, I had listened to many university professors. A couple of them were really excellent orators. For example, Thomas Tikchonenko⁴, who taught general virology. His speech was exuberant. He would throw in a detective plot every lecture, even at the expense of straying from the specific topic. Or Nickolai Kaden⁵, who taught freshmen the morphology of plants. Remarkably proper Russian without a single inaccuracy or slang term, and an occasional use of Latin or German, made him look a true Russian intellectual.

The lectures of R.B. were decidedly unlike those mentioned above, because they entirely lacked any showmanship. Markedly dry academic stories, in monotone, about the state of a problem on that day. A lot of facts, with barely any emotion. A scrupulous examination of every, including the very latest, scientific article devoted to that topic. The goal of the listener, after sorting through the mass of facts, was to figure out what "the point" of all of this was, as my American colleagues would say. Gradually from lecture to lecture, it became clear that there was a point, though it wasn't spelled out for you, but as a rule it was original. In the whole huge auditorium not only students, but also many teachers and faculty sat enchanted and tried to decode the charades. Strictly speaking, decoding them based only on the lectures of R.B. was impossible. One had to read textbooks, review articles, and sometimes even original papers. Thus, everything that you managed to understand, you would never forget.

All this, of course, happened before the time when biology textbooks became a set of simple schemes, which one could easily grasp without reading the book. Even long before the Internet, which allows one to dispense with textbooks altogether. This was a time when students were honored to analyze written text without pictures. Even for those times, the manner of R.B.'s presentation was slightly crude. Only in the very rare emotional moments could one hear R.B.'s real passionate voice, unrestrained by scientific boundaries. I will remember it for my whole life. "DNA, with all its genetic information, is dead without RNA polymerase. Life begins with transcription." But this was very rare. Otherwise, it was just science.

During my senior year I was still far from sure what science was and if it was worth pursuing as a career. I vividly remember how, leaving one of R.B.'s lectures, I was thinking "So this is how it is: this

molecular biology... an endless discovery of facts, and only occasionally, when everything comes out right, the opportunity to speak out your hypothesis, but even this comes with uncertainty." There was something fatal in this, suicidally appealing; I was captivated. It's not surprising that for my master thesis study I chose the laboratory of R.B. The advisor of our group, Sergei Yanushkevich⁶, with whom I had shared my plans, simply said, "If he takes you for diploma work, consider yourself lucky, and if he leaves you on as a grad student you will be a scientist." R.B. took me for my diploma, and as a grad student.

THE LABORATORY

The lab was the most important part of R.B.'s life, its sense and reason. When I came to work on my diploma in 1976, there were at least thirty people in the lab, if not more. It was a very complex, mobile, and curious organism. It was specifically an organism rather than a laboratory in the American sense of the word, where employees come for short periods of time to solve specific, albeit difficult, problems. This very organism was brought to life to endlessly evolve at the will of, and together with R.B. Its outcome, like with any other evolutionary process, was never quite predictable, which made life very interesting. In this lab you couldn't just work, you had to live. To live was to become an integral part of the organism, and to resist the environment. I was part of this evolutionary process from 1976 to the death of R.B., and these were the most interesting years of my life, both in scientific and personal respects.

The lab consisted of three layers of scientists, conditional, of course, since they were heterogeneous in age and perspective, which led to many unexpected encounters. These layers were: senior scientists, approximately nine people; junior scientists, grad students and scholars, about fifteen people; and lab technicians, who formed a state within a state, of which there were approximately ten. In addition, the neighboring lab of Vladimir Gvozdev, which, while formally independent, came out of R.B.'s lab and had common interests with us, seminars and the like. This was another fifteen people. Overall, a huge scientific collective. If you talk about the scientists, they were all very exceptional people with concrete, grasping minds, talented experimentalists, unbelievably hard working, and devoted to science. I vividly remember my impression after my first lab meeting: "There couldn't possibly be so many smart people in one room. What am I going to do? The second I open my mouth everyone will see what an idiot I am."

⁴ Thomas I. Tikchonenko: Ph.D., D.Sc., at the time was Professor of the Department of Virology, Faculty of Biology, Moscow State University; currently resides in the USA.

⁵ Nickolai N. Kaden (1911–1975): Ph.D., D.Sc., at the time was Professor of the Department of Higher Plants Morphology and Systematics, Faculty of Biology, Moscow State University.

⁶ Sergei I. Yanushkevich (1932–1997): Ph.D., at the time was Associate Professor of the Department of Genetics and Selection, Faculty of Biology, Moscow State University.

How could one possibly control forty talented and ambitious scientists? This is where the complex evolutionary nature of the lab organism came into play. On the one hand, there existed formal subdivisions, groups. For example, I was in the biochemical genetics of bacteria group. But not one of these groups was actually autonomous, or even semi-autonomous. All the employees of the group, starting with the leader and ending with the undergrad helper, acknowledged only one authority, R.B. Thus, he could instantly change the direction of a group and similarly the fate of its every member.

These groups, consequently, had little effect on the general development of the lab. I would identify four phenomena that in a much greater measure determined lab life. The first phenomenon was the lab meeting. It took place every other Monday, started at 9 a.m. sharp, and often ended late in the afternoon. Each employee was required to present a progress report several times a year. Exceptions were not made for anyone, even R.B. himself.

To explain the greatest importance of this event I have to say a few words about the overall scientific style of R.B. In karate, in which I took a slight interest in my youth, there is a method where a pupil does knuckle push-ups ten, twenty, thirty times. He is totally exhausted, and then the teacher says, "just another five times... and now another two... and now just another one, and another one, and another," and it goes on like this for eternity. The fundamental approach of R.B. was that a scientist, even doing his experiments extremely carefully (reckless people didn't stick around the lab), has to mistrust his results as a matter of principle. From this stemmed a necessity to consider more and more flawless controls. As a practical matter, this led to two obstacles. First off, since the time was still pre-biotechnology, to assemble and isolate everything necessary for these complicated control experiments took weeks, or months at that. Second, the accumulation of experimental data in biology, as a rule, results in an increasing amount of minute inconsistencies that R.B. was a master at finding. Only in rare instances when everything came together perfectly was one given the "go ahead" to write a paper. Other works stretched over periods of years, and often ended in nothing, seeing as how no clarity was obtained.

Meanwhile, in the surrounding scientific community, even in those fairly purist times, the criteria weren't nearly as strict. R.B.'s associates, especially the young ones, used to get hurt at seminars of colleagues from other institutes, where a slew of relatively sloppy data was presented as a scientific breakthrough. I remember a time after a famous scientist's presentation where indirect evidence was used to build an incredibly elegant and simple hypothesis. I was noticeably crushed. R.B., with his characteristic

insight, noticed this and asked if I had liked the presentation. When I answered that it was very elegant and impressive, R.B. after a brief silence said "Elegant, yes, but was it accurate? I at one point became interested in the work of this scientist where cells were irradiated by X-ray. I irradiated the same cell line with the same dose and all the cells died, contrary to what was stated in his paper. I have repeated this experiment many times with the same result. Finally I called the author. He said, "Well, maybe I misstated the dose. Try ten times less!" I asked, "Maybe 20 times?" "May as well be twenty," he answered. "I never read one of his papers again," finished R.B.

And so we return to the issue of meetings. Its main function was to find every possible reason for the presented data and/or their interpretation being wrong. The position and title of the speaker had no bearing, only his arguments did. The speaker had to defend himself from his peers to the best of his ability. R.B. very carefully regulated this process so no one got chewed to death, but no one got off easy either. The lab meeting was the decisive body that allowed papers to be sent for publication, the final stage was a personal go-ahead from R.B.

Since everyone took their turn as predator and prey, this was in principle an intra-laboratory system of peer review, which was even more severe than one from the outside, since it concentrated almost exclusively on problems, not strengths. These meetings taught us remarkable things. Humbleness before the mercilessness of a scientific argument. That a serious counterargument cannot be defeated by charismatic speech, born arrogance, or an elegant run around. That no matter how painful it is (because a paper or thesis is almost done) one has to occasionally roll up his sleeves and start all over.

Another major function of the meetings was to study the art of the scientific presentation, to stand your ground and put the others in their place, "to take a blow."

There was yet another function of the meeting, which in later years I found to be the least attractive. In the huge lab of R.B., one couldn't, of course, avoid the occasional argument and revolutionary situation. If nothing else helped, R.B. would gently say "I'm going to embarrass you in public." And this he would do at the very next meeting. I would never forget how in one of these situations R.B. sharply said, "So let this senior scientist X go and learn from the technician Sinyakova how to run a protein gel." This was very unpleasant, but luckily didn't happen too often.

Let me go off on another tangent. Twenty years later, after 11 years of working in Chicago, I can't help but ask myself "I wonder if R.B.'s approach would be compatible with science in the States." This philosophical unhurriedness, this self-absorbing attention

to detail, this unwillingness to publish until the last possible moment. With the system leaning more towards grant dollars, where to continue a grant from the N.I.H. one needs to publish six to eight papers, where competitors nearly step on your toes and so on. To tell it straight, this is very hard. But having said this, I'm still convinced that the scientific method of R.B. was not only correct, but the only possible one. R.B. once said to me, "Papers, Sergei, you have to publish of such high quality that your colleagues think, upon just seeing your name, that it should be read." This, of course, is a very high standard, but much more respectable than the above-mentioned grant dollar. And isn't it us who today in the West drown in a sea of papers, a good half of which simply can't be believed while only a fraction of the rest is truly original?

All of us, to an extent, are replicas of our teachers, and with time we resemble them more and more. I don't want to say that I fully embody the principles of R.B. in my laboratory in the States: that would be impossible. At least I gave it my best shot. Just recently there was a paper accepted for publication on a project that had been stretched over seven years. Apparently we had really perfected it, since there was not one criticism from the outside review. When I proudly told a colleague about this remarkable review I mentioned the fact that this project had been in the works for seven years. He was shocked: "Who could allow themselves more than two years for a paper?" He's right too. Using R.B.'s approach in the States you'd leave yourself no room to expand, but you could survive. Through prayer and fast, so to speak.

A second phenomenon was the Institute's seminar, which was held interchangeably with the lab meetings on Mondays. Although it was called the Institute's, in reality R.B. was entirely in control of its direction and character. All the practical work was done, with great tact, by Eza Kalyaeva⁷. I would say that the direction of this seminar was dependent on two things. The first was the ever changing scientific interests of R.B., so that one year we listened to numerous presentations about recombination, another about replication, and so on. The other factor was the idea that a seminar must present all major studies completed in our field in the USSR. I'll add right away that because of the nature of the closed off system overall and our Institute, in particular, as a part of the Institute of Atomic Energy, foreign speakers were a near impossibility. Seeing as R.B. treated everything, including seminars, quite seriously, the seminars were always very interesting. Being privileged enough to work in R.B.'s lab, we were freely granted complete information about various aspects of molecular biology as laid out by the

best scientists in the country. This of course was priceless.

There was yet another aspect to these seminars which gave them a cutting edge. Our poor speakers didn't know what awaited them. We were well trained at our lab meetings, so no mistakes or inaccuracies were forgiven. And everything would start out so quietly. An elegant speaker would arrive, fresh from a trip abroad, to talk about a wonderful chromosome organization model. Gracefully and confidently he would speak of his work, but suddenly a grim bearded type would stand up and ask a question about the neutralization of charges in DNA at a physiological ionic strength. And after him would follow series of questions, one after the other, while the speaker was pouring sweat, blushing and dodging the questions. But the situation was bad, and soon, his model was tearing at the seams. It became apparent that the fate of a work would be decided then and there, and not in the distant Boston. Further, the reputation of the scientist himself would be determined today, once and for all. This didn't fully correspond with reality, but it sure felt like it. This was a characteristic feeling of students in any leading scientific school: we make the science. Thus, surviving one of R.B.'s seminars, or, as they were called, Khesyatniks, unscathed was a task at which most didn't succeed. Our compensation for "suffering" caused by the specifics of R.B.'s lab and personality was an indisputable elitism.

Having warmed up in this fashion at the beginning of the week, R.B.'s employees went to work on their experiments. We worked really hard. I suppose that the next generation of young scientists couldn't even imagine the pre-biotechnological days: without "kits," without reliable isotopes, without disposable test tubes and Petri dishes, without automatic pipettes, and prior to PCR. A time when proteins were labeled with the hydrolysate of Chlorella, and the Klenow fragment had to be isolated by each lab for itself. Another problem consisted in the fact that no matter how global the tasks that R.B. placed before his lab, it was equipped relatively poorly. Consequently, things like quartz cuvettes for the spectrophotometer, centrifuge tubes, scintillation vials, etc. became hard currency. The only way to compensate for all these shortcomings, and complete work which would meet R.B.'s standards, was back-breaking labor in combination with a soldier's resourcefulness. This was the third phenomenon illustrated below with a couple of examples out of lab life.

Grad student Igor Zaitsev⁸ determined the dependence of transcription initiation *in vitro* from the temperature. This involved performing transcriptional

⁷ Eza S. Kalyaeva (1937–2000): Ph.D., was Staff Scientist of the Laboratory of Molecular Genetics of Microorganisms at the Institute of Molecular Genetics, Russian Academy of Sciences.

⁸ Igor Z. Zaitsev: Ph.D., currently Director General of the "Bacterial Supplies" Unit of the Russian Ministry of Health.

reactions in a wide range of temperatures with a smooth gradient. How, practically speaking, would one do this? Igor single-handedly welded a metal chamber, to be used as a water bath, which was divided by tin strips into many connected compartments. The heater was attached to one end of the camera while the small turbine for water circulation was at the other end. Through many experiments, he managed to select the speed of the heating and circulation at which the water in neighboring compartments varied by only 0.5°C. Thus, he obtained a remarkably beautiful and convincing temperature curve for promoter “opening.” If I recall correctly, this was the only experimental curve included in a great textbook on transcription written by R.B.’s associates Vadim Nikiforov⁹ and Yurii Zograf¹⁰**.

Another example concerns the group of Irina Bass¹¹, which included grad student Sergei Mekhedov¹². This group studied the regulation of RNA-polymerase gene expression. A typical experiment looked like this: intracellular protein was labeled in various bacterial cultures using the already mentioned *Chlorella* hydrolysate. Upon isolation, labeled proteins were separated by gel electrophoresis in individual glass tubes. (I have no idea whether modern students have ever seen modules for gel electrophoresis in tubes.) Gels were then stained, sliced into thin pieces (approximately 0.5 mm thick), and each slice was individually placed into a scintillation vial to measure its radioactivity. (There were no phosphorimagers at that time.) Since both Irina and Sergei were highly responsible people, they made a schedule for the next business day on a huge piece of graph paper which was then placed on the inside of the lab door. As a result of such scientifically organized labor, every minute of the 10–12-hour workday they were pouring, shaking, loading, cutting, and measuring. So as not to get confused, Irina’s lab contained approximately 10 timers that each sounded different, sending signals only they could understand. It is not surprising that when after seven years of such work Sergei wrote his Ph.D. thesis, one would observe him sitting with his back to anyone who came in the room, with a sign attached to his shirt. “Please don’t bother Mekhedov.” It was signed “Mekhedov.”

⁹ Vadim G. Nikiforov: Ph.D., D.Sc., Professor and Head of the Laboratory of Molecular Genetics of Microorganisms at the Institute of Molecular Genetics, Russian Academy of Sciences.

¹⁰ Yurii N. Zograf (1936–1985): Ph.D., was Staff Scientist of the Laboratory of Molecular Genetics at the Institute of Molecular Genetics, USSR Academy of Sciences.

** Nikiforov, V.G. and Zograf, Yu.N., in *Itogi Nauki i Tekhniki. Ser. Mol. Biol.*, Moscow: VINITI, 1977, vol. 13.

¹¹ Irina A. Bass: Ph.D., Staff Scientist of the Laboratory of Molecular Genetics of Microorganisms at the Institute of Molecular Genetics, Russian Academy of Sciences.

¹² Sergei L. Mekhedov: Ph.D., currently Staff Scientist at the National Center for Biotechnology Information, National Library of Medicine, National Institutes of Health, USA.

The last example is about me personally. For my work I needed to measure the speed of DNA and RNA synthesis in a temperature-sensitive mutant in DNA gyrase. This is relatively simple: control and mutant cells are grown to the early exponential phase at 30°C, each culture is then split in half to be incubated at either 30 or 42°C. At various time intervals, samples are taken and pulse-labeled, followed by radioactivity measurement in the TCA-insoluble material. As simple as it sounds, there are two cultures, two temperatures, many time intervals. Overall, about 60 samples per experiment. Unfortunately, I couldn’t measure radioactivity in all of them simultaneously since I only had 20 scintillation vials. Thus, I had to do my measurements in groups of 20, washing the vials in between. (I still remember the procedure for washing scintillation vials in R.B.’s lab. One hour in the detergent solution, 30 rinses in hot water, 15 rinses in cold water, 5 rinses in distilled water, and dry.) Once, infuriated by this procedure, I selfishly asked R.B. for extra vials. These quartz vials were hard currency and were kept in R.B.’s personal lab desk. R.B. listened to me sympathetically, but curtly said, “No.” “Why not?” I asked astonished. “All your experiments are turning out anyway,” was his answer. Arguing with this was difficult, and I left empty-handed.

In general, R.B. reacted to various “minor” scientific difficulties with irony. This isn’t surprising, in his life he had to deal with much more serious issues than a lack of equipment and supplies. On top of this, he was truly convinced that if his associate came up with a truly good experiment, he would find some way “by hook and crook” to stage it. And if not, the experiment, and possibly the experimentalist, is worthless.

Around 6 p.m. it was time for the laboratory tea. This was a big occasion, and the fourth major phenomenon. Two huge pots of water were heated in the dishwashing room, the tea was made, and the employees gathered there. It goes without saying that the teatime allowed for a much needed break after a long day of work, before the final evening stretch. The main attraction, though, was something entirely different. Discussing science: who read, heard, thought of what; discussing one’s endless flow of problems as they happened: won’t digest, won’t label, won’t isolate; to show yourself and look at others in raging debate, and, if R.B. wasn’t sick, to interact with him informally.

To be fair, I must say that in R.B.’s presence everything, of course, revolved around him. He in turn saw this as yet another educational experience, so one could never fully relax. He would walk into the room, filled with young people sipping tea, and from the doorstep ask, “Quickly, in which literary work is the discovery of bacteriophages depicted?” I challenge readers to answer this question independently.

My strongest impression about the lab teatime was somehow quite similar to my impression of the Institute's seminars: this is where and when science is made. It's funny to say, we were just a bunch of kids, and we didn't seriously think or talk about anything other than science. In the relaxed environment of teatime some really amazing things could happen.

I'll give just one such example, concerning what was perhaps the greatest discovery in Soviet molecular biology in the 80s: the discovery of the mobile genetic elements in *Drosophila*. These elements were cloned in the lab of Georgii Georgiev¹³ and their multiple localizations were demonstrated in two ways. Nick Tchurikov¹⁴ in Georgiev's lab had shown it using Southern hybridization, while Eugene Ananiev¹⁵ in Gvozdev's lab did it using *in situ* hybridization with *Drosophila* polytene chromosomes. There was, however, a strange observation that Eugene shared with us over evening tea. Parts of conjugated homologs in polytene chromosomes occasionally separate. Eugene noticed that multiple elements hybridize with these separated portions differently, but wasn't sure how to explain it. Truth be told, no one particularly wanted to explain it, since polytene chromosome asynapsis is an extremely rare event, almost an artifact. During this teatime, however, there was a grad student Victor Bashkirov¹⁶, who was, without exaggeration, a geneticist with a godly gift. He started asking Eugene about the genetic structure of the *Drosophila* strain used for *in situ* hybridization. It turned out that a hybrid strain was used, and it somehow became clear that differential hybridization at asynapsis sites could be caused by individual differences between homologous chromosomes of the hybrid. Eugene went back to work, and two weeks later we learned that multiple elements have different chromosomal localization in various *Drosophila* strains, i.e., they are mobile.

I must digress on two notes. I am far from sure that all those involved in the discovery of mobile genetic elements would agree with my interpretation of the event. I am, however, forgiven by virtue of the fact that I heard this discussion with my own ears. It still remains evident that for a teatime this was an exceptional breakthrough, yet the story accurately conveys the spirit of R.B.'s lab.

¹³Georgii P. Georgiev: Ph.D., D.Sc., Member of the Russian Academy of Sciences, Founder and Director of the Institute of Gene Biology, Russian Academy of Sciences.

¹⁴Nickolai A. Tchurikov: Ph.D., D.Sc., Professor and Head of the Laboratory of Genome Organization at the Institute of Molecular Biology, Russian Academy of Sciences.

¹⁵Eugene V. Ananiev: Ph.D., D.Sc., currently Plant Genomics Research Manager at Pioneer Hi-Bred International, USA.

¹⁶Victor N. Bashkirov: Ph.D., currently Staff Scientist of the Laboratory of Neurogenetics and Molecular Genetics of Development at the Institute of Gene Biology, Russian Academy of Sciences.

Now, how was this all possible? Why had we agreed to work almost twenty four-seven for a measly wage, surrendering our personal business for the sake of a doubtful privilege: to not publish false work? And why is this no longer present, not in my lab, nor in any lab I'm aware of in the States?

The answer to this question is fairly simple: the personal example set by R.B., since a lab is a logical extension of its supervisor. R.B. spent his whole life at the bench, no matter what his title or salary. He staged experiments nearly to his very death. He lived by science, and thought and spoke only of it. He was endlessly honest and thorough. One of the most amazing things is that he never wrote himself into others' papers. For example, I have practically no collaborative works with R.B. although I was his grad student. To all my offers to be a co-author, R.B. would respond in one way: "I didn't do one experiment for this paper."

It's not an accident that with this attitude toward work, his private life, softly put, wasn't that great. Even this he thought to be a normal aspect of a scientist's life. I remember during my divorce, in the heat of grad studies, R.B. said "Tried once, and enough. Still, for a scientist it's hopeless." This advice I didn't follow.

Who of us is ready to serve science in such a way? To be such an ascetic? Decisively, not me or my colleagues, comfortably living "in the country of dentists." Our employees really can't be blamed for anything, but we can.

JOKES

A young scientist, upon reading the previous section, would exclaim "What horrors!" How true: a strict professor, hurriedly working employees, constant drilling and lack of a personal life. I wouldn't want this first impression to stick. The lab was filled with young, happy people, who in their spare time joked around and entertained themselves to the utmost. Of course, there wasn't a lot of spare time, but good health allowed us to save on sleep. And R.B., with all his air of strictness and seriousness, was actually quite jovial and personable. In photographs of him his mischievous, mocking eyes always stood out. I would like to believe that the following chapter will change this lasting impression.

R.B.'s lab was established in the Institute of Atomic Energy somewhere around 1959. By my arrival in 1976 it was overgrown with traditions and legends. These legends were passed on from generation to generation of grad students, with time becoming intertwined with new folklore. Since the Russian way of thinking, as it is well known, has a mythological character, new arrivals to the lab nearly drowned themselves in these stories:

The legend of Michael Shemyakin¹⁷, who could measure the pH of a solution to the tenth with his tongue.

The legend of Josephine Shmerling¹⁸, who in 1950 gave up half her salary so R.B. would not get fired during the layoffs.

The legend of Sophia Mindlin¹⁹, who was offered a lab, or even a department, in Alikhanyan's²⁰ new Institute, but turned it down to stay as a regular senior scientist with R.B.

The legend of Alex Dmitriev²¹, who, knowing that R.B. liked to make a lot of corrections in manuscripts, printed out the first draft of his thesis so it only took up the middle third of the page leaving the rest of the page blank. In response to R.B.'s question about the wide margins he flatteringly said, "Those are for your comments." There were almost no corrections.

The legend of Alec Goldfarb²², who turned out to be an American spy, and on the verge of discovery fled the country without having defending his thesis. Eza Kalyaeva usually added, "Now, if he had only talked to me first, none of this would have happened."

The legend of Eugene Khurges²³, who was unconditionally loved by all the women in the lab, starting with the lab technicians and ending with the senior scientists. They all lovingly called him "Khu."***

The legend of Andrei Chernyshev²⁴, who during his seventh year of grad studies came into work in the dead of winter wearing sandals. To the surprised and amused question of R.B. he gravely answered, "Oh, R.B., I've had prosthetics down there for ages!"****

The legend of Vadim Nikiforov, who, having worked for many years as a junior scientist, came to R.B. and said, "I don't want to hide it from you, I was offered a senior position elsewhere." "Why didn't you

say something before, Vadim?" R.B. exclaimed, and immediately signed for the promotion. As it turned out, Vadim was being given the title of Senior Lieutenant of the Army Reserves.

I could go on forever, but I'll end with the fact that I recently found out: there exists a legend about me. A group of grad students I had recruited from Moscow came to our University. When their supervisors in Moscow found out they would be working with me, the reaction was such: "Oh, Mirkin, he's the one that can wind DNA around his finger!"

Of course, we also took every chance we had to joke around. There were even special locations designated for jokes. For example, there was a laminar flow hood used to work with yeast in Olga Danilevskaya's²⁵ room. The vast smooth surface of the laminar was covered with little jokes, which were renewed daily. One morning, a short rhyme written by Andrei Lebedev²⁶ appeared on the laminar: "What happened to the laminar? In the morning it breathes like a drunkard." We drank rather heavily back then, so the joke more than corresponded with actuality.

Another featured place for jokes was the lab's open mic nights, which took place either on New Years or on R.B.'s birthday. I'll give you just one example, related to the fact that R.B.'s lab kept the Soviet record for the number of Josephine employees (Josephine Shmerling and Josephine Gorlenko²⁷), who, on top of this, used to be R.B.'s closest confidants. And so, during one open mic night, to celebrate R.B.'s sixtieth, in the restaurant "White Stork," in front of the entire scientific community of the country the following musical satire was performed:

They say Roman is getting old,
That's a fable and a joke.
Like a young Frenchman Khesine
In many ways loves Josephines.

A separate matter, which was particularly controversial, was R.B.'s sense of humor. Here the opinions of even his closest associates split. I personally think that R.B.'s sense of humor was just fine, although its direction was often reminiscent of the phrase about how "I and my buddy General Zhukov loved a good-natured joke even on the very toughest days."**** Let's say R.B. is addressing Alex Gragerov²⁸ who hasn't quite completed some task. "You, Alex, should

¹⁷Michael F. Shemiakin: Ph.D., D.Sc., prior to his retirement was Vice Head of the All-Union Research Institute of Agricultural Biotechnology.

¹⁸Josephine G. Shmerling: Ph.D., Scientist-Emeritus of the Laboratory of Molecular Genetics of Microorganisms at the Institute of Molecular Genetics, Russian Academy of Sciences.

¹⁹Sophia Z. Mindlin: Ph.D., D.Sc., Professor, Senior Staff Scientist of the Laboratory of Molecular Genetics of Microorganisms at the Institute of Molecular Genetics, Russian Academy of Sciences.

²⁰Sos I. Alikhanyan (1906–1985) Ph.D., D.Sc., was Founder and Head of the All-Union Research Institute of Industrial Microorganisms at Glavmikrobioprom USSR.

²¹Alexander D. Dmitriev: Ph.D., currently Staff Scientist at the Institute of High Nervous Activity and Neurophysiology, Russian Academy of Sciences.

²²Alex D. Goldfarb: Ph.D., currently Professor of the New York Public Health Research Institute, USA.

²³Eugene M. Khurges: Ph.D., currently Staff Scientist at the AJINOMOTO-Genetika Research Institute, Russia.

*** "Khu" sounds like the vulgar Russian word for the male organ.

²⁴Andrei I. Chernyshev (1952–1986): Ph.D., was Staff Scientist at the Institute of General Genetics, USSR Academy of Sciences.

**** His legs were fine, but after seven years with R.B. it sure didn't feel like it.

²⁵Olga N. Danilevskaya: Ph.D., currently Research Scientist at Pioneer Hi-Bred International, USA.

²⁶Andrei N. Lebedev: Ph.D., Staff Scientist of the Laboratory of Molecular Genetics of Microorganisms at the Institute of Molecular Genetics, Russian Academy of Sciences.

²⁷Josephine M. Gorlenko: Ph.D., Staff Scientist of the Laboratory of Molecular Genetics of Microorganisms at the Institute of Molecular Genetics, Russian Academy of Sciences.

**** The phrase allegedly belonged to Joseph Stalin.

²⁸Alexander I. Gragerov: Ph.D., currently Director of Biochemistry at Primal, Inc., USA.

either put your mind to it and do it, or don't put your mind to it, but I wouldn't recommend that either."

I'll end this section with the best joke I ever heard from R.B. Here's how it happened. During the teatime one day, the Union leader of our institute by the name of Vladimir Nezavibat'ko²⁹ kept peeking his head into the room. (To get the joke you'd have to know that "Nezavibat'ko" is roughly translated as "don't call father.") He obviously had something very urgent to discuss with R.B., but he didn't dare come in, since first of all, he was very shy, and second of all, he felt inferior to R.B. R.B., on the other hand, was surrounded by his colleagues and didn't notice him. Finally, Josephine Shmerling very loudly said, "R.B., Vladimir Nezavibat'ko is calling." R.B. looked around, saw Nezavibat'ko's overwhelmed face in the door, and instantly asked, "Vladimir, did you call or didn't you call?" Like it or not, you had to submit yourself to the speed of R.B.'s reaction and his unquestionable awareness of his position.

GENOME INCONSTANCY

One of the most remarkable features of R.B. as a scholar was the enormous breadth of his scientific knowledge. Part of it should surely be credited to his teachers, geneticists of the old Russian school, such as Alexander Serebrovskii³⁰, who had a truly encyclopedic knowledge. Another important factor was that throughout his career, R.B. worked in various and sometimes remote scientific areas. His Ph.D. thesis was in the field of *Drosophila* genetics, while his doctorate, after the destruction of genetics in the Soviet Union, was in the area of protein synthesis. During his work on the Doctor of Sciences degree, R.B. wrote a book whose title was quite unusual for a geneticist, "Biochemistry of Cytoplasm."

After getting his own lab at the Radiobiological Department of the Institute of Atomic Energy, R.B. dedicated himself to the problem of transcription regulation, where he could fully use his expertise as both geneticist and biochemist. Soon to follow was the discovery of early and late genes of T-even bacteriophages, unquestionably the major discovery in his life.

R.B.'s manuscript describing this discovery was submitted to the prestigious *Journal of Molecular Biology*, but was not accepted, since it was at odds with the predominant view at the time: the operon model. Finally R.B. published it in the Russian journal *Biokhimiya*, which was not even translated into English. Consequently, few scientists in the West

know that R.B. should be credited for the discovery of bacteriophage early and late genes. Even here it was not without a legend. In R.B.'s words, Sir John Kendrew, then editor-in-chief of the *Journal of Molecular Biology*, repeatedly stated that the only mistake made by his *Journal* throughout the years of its existence was the rejection of R.B.'s paper. Upon Alec Goldfarb's emigration to the U.S. and his popularization of R.B., the truth was partly restored. At least in several textbooks, including "Molecular Genetics" by Gunter Stent, the honor of discovering early and late genes in bacteriophages was given to R.B.

Subsequently, the major topic in R.B.'s lab became the studies of *E. coli* RNA polymerase structure and function. Among the lab's major achievements one could mention the isolation of the first RNA polymerase mutants, characterization of the mechanisms of temperature-sensitive suppression of nonsense mutations, unraveling the mode of action of transcription inhibitor rifampicin, and finally, in collaboration with Yurii Ovchinnikov's³¹ lab, decoding the nucleotide sequences of the genes and the amino acid sequences of protein subunits of RNA polymerase. I've only mentioned a few studies. This was a mammoth work for which R.B., Yurii Ovchinnikov, and their collaborators were awarded the USSR State Prize in mid-80s.

In parallel with his work with bacteria, R.B., together with his student Boris Leibovitch³², studied the mechanisms of X-chromosome dose compensation in *Drosophila*. This is an interesting system, since, contrary to mammalian cells, where one X-chromosome is inactivated altogether, both X-chromosomes in *Drosophila* females are partially active. R.B. believed that female X-chromosomes in *Drosophila* are more condensed than that of males and consequently are less active. Boris and he tried to prove it by measuring the activity of exogenous RNA polymerase added to male and female polytene chromosomes.

By the end of the 70s, however, R.B.'s ardor for transcription studies cooled noticeably. One possible reason was that R.B.'s lab, being an unchallengeable leader in the field in the Soviet Union, gradually lost its leadership worldwide. Thus, R.B. started to look for a new, big scientific direction where he could apply his encyclopedic knowledge and profound experimental expertise. I suspect that the topic of my Ph.D. thesis, DNA gyrase and supercoiling, was a reflection of the early stages of this search: something new, though still peripherally related to transcription.

²⁹Vladimir N. Nezavibat'ko (1937–1997): Ph.D., was a Head of the Laboratory of Biopolymer Synthesis at the Institute of Molecular Genetics, Russian Academy of Sciences.

³⁰Alexander S. Serebrovskii (1892–1948): Ph.D., D.Sc., was Professor and Head of the Department of Genetics and Selection, Faculty of Biology, Moscow State University.

³¹Yurii A. Ovchinnikov (1934–1988): Ph.D., D.Sc., Vice-President of the USSR Academy of Sciences, Head of the Institute of Bioorganic Chemistry, USSR Academy of Sciences.

³²Boris A. Leibovitch: Ph.D., currently Staff Scientist of the Department of Biology at Washington University, Saint Louis, USA.

But this field was not paramount enough for R.B. It was a minor episode for him, while I continue studying these and related questions to this day.

In the end, R.B. decided to concentrate on the problem of genome instability. This decision was in line with the time. Recent discovery of bacterial transposable elements lead to reconsidering Barbara McClintock's studies in maize. Eukaryotic mobile genetic elements were just characterized, and the mechanisms of their transposition were aggressively studied. The concept of a selfish gene had emerged, and the role of repetitive elements in genome structure and functioning was actively discussed. In the bacterial world, there was a rapid spread of antibiotic resistance. The classic Mendelian genetics was falling apart and the concept of the lateral gene transfer became imperative. This fundamental area was chosen by R.B. for the final turn of his career.

This is all quite clear in retrospect, but for us, R.B.'s students, his decision to abandon precise quantitative studies of RNA polymerase in favor of semi-intuitive studies of lateral gene transfer came as quite a shock. I am not certain of the year when R.B. presented his first lecture on his new topic at the Molecular Biology School in Mozhinka. In the preceding months, he repeatedly called in sick with serious heart problems, so we didn't see him for a while and were not sure what he was going to talk about. It was R.B. at his best: in 90 minutes without a trace of emotion he depicted numerous cases pointing out the possibility of lateral gene transfer. And it became instantly clear that this phenomenon was real and needed to be studied. If I am not mistaken, the text of R.B.'s lecture was later published in the journal *Molekulyarnaya Biologiya*.

Easier said than done: needs to be studied. How are you going to do it? Nothing even remotely similar was done in his lab at the time. R.B. addressed this problem in two ways. First, he decided to find a natural system where one could experimentally analyze lateral gene transfer. Bacterial antibiotic resistance didn't look like a good choice, since the selection was carried out in a clinical environment. R.B. chose to study natural deposits of heavy metals. His logic went along the following lines. Free-living bacteria at high concentrations of heavy metals should develop metal resistance. This resistance could then be transferred to bacteria residing in animals, and so on and so forth. I deliberately simplify, but hope that the main idea is clear: to follow geographic and evolutionary distributions of metal resistance determinants from their apparent origins. One could even notice here a certain parallel with Vavilov's origins of cultured plants.

R.B. started to send expeditions to mercury mines. There he collected samples of water and soil, feces and dead bodies of rodents, birds, etc. This was followed by isolation of mercury-resistant bacteria from

these sources, followed by cloning and analysis of mercury resistance determinants. This enormous work was only started while R.B. was still alive, the very first mercury-resistant transposons were just cloned. This work is still continued in R.B.'s old lab by Sofia Mindlin, Elena Bogdanova³³, Josephine Gorlenko, Gene Kholodii³⁴, and others.

Secondly, R.B. has decided to collect and analyze all the literature on genome instability in order to give it a most scholarly review, establishing once and for all lateral gene transfer as the central process in genetics. One should know that R.B. didn't consider it possible to cite an article unless he had studied it in detail. Thus, his office and a couple of neighboring rooms became storage for Xerox copies of thousands of scientific papers. In the final version of his book "Instability of the Genome," he cites more than 3500 references, each of which, I guarantee, he carefully studied.

Rough drafts of the book's chapters were given by R.B. to his pupils to read. I commonly read them on my way to the lab and back home (50 minutes each way between subway stations at Belyaev and Shchukinskaya). Besides it being difficult to concentrate, R.B.'s writing, in his usual style, overwhelmed the reader with facts and details. My head was ready to explode! Out of my misery, I wrote comments on the margins of the manuscript saying, "R.B., a human's mind can not possibly comprehend this!" Apparently this was not only my opinion, since in the book's preface R.B. thanks several colleagues for "sometimes quite thorny comments."

The last chapter I've read was chapter 8, "Homologous Genetic Recombination and the Role of Gene Amplification in Evolution." I couldn't believe my eyes. It was a stylistically sparkling, elegant and perfectly logical depiction of the main concept of the book: from gene duplication and divergence toward lateral gene transfer. It was literally impossible to lay it down, it was so well written. Returning it with my comments to R.B., I couldn't help but ask: "How come this chapter reads like a novel while previous ones were so lackluster?" R.B. chuckled and said, "I simply learned how to write a book only by its very end." I don't think this is actually the case. I feel that this last chapter came out of his heart. And in my mind, it is the only chapter that deserves careful reading, while the remaining chapters can be used as a comprehensive handbook.

At the end, I cannot resist but cite R.B.'s dedication on my copy of "Instability of the Genome." "To my

³³Elena S. Bogdanova: Ph.D., Staff Scientist of the Laboratory of Molecular Genetics of Microorganisms at the Institute of Molecular Genetics, Russian Academy of Sciences.

³⁴Gennadiy Ya. Kholodii: Ph.D., Staff Scientist of the Laboratory of Molecular Genetics of Microorganisms at the Institute of Molecular Genetics, Russian Academy of Sciences.

dear Sergei Mirkin. I think that the future of my lab will in many ways depend on you. I have a faith in you and hope that you will carry high the banner of biology in general and genetics in particular throughout your whole life! March 19, 1984. R.B. Khesin.” Such a high style: a banner to carry high throughout life.

THE FINAL DAYS

By the irony of fate, the illness and untimely death of R.B. turned out to be linked to his study of genome instability. Somewhere early in 1984, R.B. developed a problem with his intestine. He decided that he hadn't been careful enough in his expeditions and had caught a pathological strain of *E. coli* or some other enterobacterium. Since in Russia you could buy antibiotics without a prescription, and in the lab there were plenty, he occupied himself with self-medication. The situation didn't get better: neither the antibiotics nor the herbal remedies, brought to him by horrified employees, helped. He got thinner and thinner before our eyes, started zoning out at seminars, and once, on his way home from work, passed out at the wheel and ended up in the opposite lane of the tunnel near the Sokol Metro (thank God that it was late at night so there was no on-coming traffic). It was time to see a doctor.

It needs to be said that the details of R.B.'s illness were known only to his closest friends and confidants and were not discussed in the lab. Therefore, I only had snippets of information. R.B. had gone to the city hospital, where his father had worked and where he had himself gone for treatment after being wounded in the war. This is an appropriate time to remember a conversation I had with R.B. long before the aforementioned events. R.B., with his connections and position, could be treated at any hospital, including the Central Kremlin Hospital, but preferred ordinary medical establishments. To my question of why, R.B. replied: “In the Kremlin Hospital, Sergei, doctors are hired through their connections, while truly talented physicians work in the ordinary city hospitals.”

That's how R.B. got hospitalized with a diagnosis of intestinal cancer. As I understand it, the surgery was unsuccessful: they cut him open and sewed him back up, found metastases, and it was too late to do anything. R.B. spent a lot of time in the hospital, so the lab took turns visiting. My turn came too. The hospital was located in an old estate, and R.B. was in a separate room with high molded ceilings and huge windows that overlooked Strastnoi Boulevard. I brought some home-cooked food with me and tried to feed him, he even managed to eat something. He wasn't feeling too great, but he was in a good mood, joked a lot, and thought he was going to get better. Apparently, he was told that the surgery was a success, and I, knowing the

situation and seeing before me only a shadow of R.B., understood that there could be no talk of getting better. Still, I joked, lied a little, and did everything one should to support someone who is gravely ill.

After some time, R.B. came back to work, but he was very weak, still losing weight, and gradually withdrawing. Even his smile was somehow beleaguered. He continued his treatment and I found out from Simon Shnoll's³⁵ book that it was being supervised by Gary Abelev³⁶, therefore, everything possible, and impossible, was done for him. The situation was bad anyway.

The last time I saw R.B., not long before his death, was purely by accident. At this time I had begun to work heavily with Victor Lyamichev³⁷ and Maxim Frank-Kamenetskii³⁸ on the structure of homopurine repeats in DNA. It was already apparent that under the influence of DNA supercoiling they adopted a new conformation, but details of the new structure were unclear. I was staging experiments on the localization of single-stranded segments in this structure using nuclease S1. It happened on a weekend, the lab was almost empty, and suddenly R.B. glanced into my room. He asked what I was working on, and since I hadn't seen him in a while, I, with great interest, started telling him about the H-form DNA, and about my other work with the detection of cruciforms in *E. coli* cells. R.B. looked terrible, but he listened with interest and asked very intelligent concrete questions—he was still in great form intellectually. When I finished, R.B. said, “This is very interesting and promising. There are some very interesting connections with your thesis.” And after a short pause, “And well, I'm dying Sergei!” I couldn't find the courage to tell him that I knew, that I loved him and would remember him the rest of my life. Instead, I mumbled that everything would be fine, that the chemotherapy should work, that we would discuss these problems many other times, and other nonsense.

R.B. was about to leave, when my young wife Lena³⁹ called to tell me she had arrived and was waiting in the lobby. R.B. and I went down to the entrance, he said good-bye and left, and I winked at the security guard and brought Lena inside. (Security was of course lenient because we were, after all, as Sergei

³⁵Simon E. Shnoll: Ph.D., D.Sc., Professor and Head of the Laboratory of Physical Biochemistry at the Institute of Theoretical and Experimental Biophysics, Russian Academy of Sciences.

³⁶Gary I. Abelev: Ph.D., D.Sc., Professor and Head of the Laboratory of Immunochemistry at the Institute of Carcinogenesis, Oncology Center of the Russian Academy of Medical Sciences.

³⁷Victor I. Lyamichev: Ph.D., currently Director of Research at the Third Wave Technology, Inc., USA.

³⁸Maxim D. Frank-Kamenetskii: Ph.D., D.Sc., currently Professor at Boston University, USA.

³⁹Elena Yu. Siyanova (1958–2000): Ph.D., was Instructor of the Department of Molecular Genetics at University of Illinois at Chicago, USA.

Nedospasov⁴⁰ used to say, “Chemists who worked at institutes with alcohols.” “Who was that old guy?” Lena asked. “You’re insane!” I exclaimed (Lena had listened to his lectures while at the University just a few years ago), “That was R.B.!” He was unrecognizable. Sixteen years later in Chicago, Lena was dying a painful death of cancer too, and not everyone recognized her either. I sat at her bedside and thought how the most beautiful, talented, and decent people suffer untimely deaths, while we are left to wrestle with guilt, memories, and pain.

I will only share with you one observation about R.B.’s funeral, brought to my attention by R.B.’s last grad student, Olga Lomovskaya⁴¹. “In my whole life, I have never seen so many men cry.”

In concluding this very sad section, I would like to tell you two stories which took place not long after. The first one, I heard about a year after R.B.’s death under the circumstances that leave no room to question its validity. As I previously briefly mentioned, R.B.’s personal life, as a whole, would leave one unsatisfied. He was a strong, handsome, and charismatic man nonetheless, and had a lot of women in his life. A couple of these relationships were very steady, and lasted for many years. R.B. used to jokingly say to one such girlfriend. “Will you marry me?” She in turn, being a married woman, would respond, “Buy me a calf and I will.” In his will, R.B. left her money to buy a calf.

The second story occurred during the burial of R.B.’s urn, several months after his death. Everybody from our lab, as well as many scientists from other institutes, were present. After a couple of formal speeches, one of R.B.’s closest friends, the one-legged, unkempt David Goldfarb⁴², came to the podium and said an incredible thing. “The scientific way of thinking is an unconventional way of thinking, and my friend R.B. was a dissident in the full sense of the word.”

R.B. AND THE SYSTEM

I came back from the cemetery quite shocked. The thing is that, for reasons outside the framework of this story, I had a considerable amount of personal experience in dealing with Soviet dissidents. R.B., at least as far as I could see, in no way fell into this category.

Here was my train of thought: although R.B. had suffered at the hands of the system twice, once in 1948

⁴⁰Sergei A. Nedospasov: Ph.D., D.Sc., Professor and Head of the Laboratory of Cytokine Molecular Biology at the Institute of Molecular Biology, Russian Academy of Sciences.

⁴¹Olga L. Lomovskaya: Ph.D., currently Director of Biochemistry at the Essential Therapeutics, Inc., USA.

⁴²David M. Goldfarb (1918–1990): M.D., D.Sc., at the time was a Professor at the Institute of General Genetics, USSR Academy of Sciences.

as a geneticist, and again in 1951 as a Jew, by the time I met him he played a notable role in the system: Corresponding Member of the Russian Academy of Sciences, Head of the largest laboratory in the Radiobiological Department of the Institute of Atomic Energy, founder and permanent director of the School of Molecular Biology in Mozhinka, member of the editorial boards of many biological journals, member of many scientific councils, and the list goes on. Although one could imagine that if he were Russian and a Communist Party member, like for example Alexander Baev, who had also previously suffered because of the system, his position would be even more significant, it still wasn’t half bad.

Further, although by a few subtle comments one might have been able to guess that he didn’t love the system, it hadn’t been apparent. National politics were never discussed in the lab. The one time a conversation did break out over tea, about either Sakharov’s⁴³ exile or the Soviet troops in Afghanistan, R.B. quickly disrupted the conversation by saying, “Why does this even interest you?”

Moreover, while disliking the system, he used it plenty pragmatically, if not for personal goals then in the interest of the lab. For example, one of the privileges he had was a yearly meeting with the Director of the Institute of Atomic Energy and President of the Academy of Sciences, Anatolii Aleksandrov⁴⁴, during which he could ask for anything. The result of one of these requests was the recruitment of Alex Gragerov at the peak of State anti-Semitism.

Even concerning questions of the emigration of Soviet scientists to other countries he took a very typical stance for the time’s scientific establishments. Alec Goldfarb told me how when he shared his plan to leave the country with R.B. he was very uncompromising. “First quit, then in six months apply for emigration.” Alex Varshavski’s⁴⁵ sensational escape in 1977 was condemned by R.B. on the grounds that Alex had let down Vladimir Engelhardt⁴⁶, who had given his honest word that he would return.

There were also other things. In my youth, I had learned to type very quickly on a typewriter using all ten fingers, I typed tons of Samizdat and Tamizdat and gave them out to my acquaintances. (This, by the way,

⁴³Andrei D. Sakharov (1921–1989): Ph.D., D.Sc., Member of the USSR Academy of Sciences, the most prominent Soviet human rights advocate, laureate of the Nobel Peace Prize.

⁴⁴Anatolii P. Aleksandrov (1903–1994): Ph.D., D.Sc., at the time President of the USSR Academy of Sciences and Director of Kurchatov Institute of Atomic Energy.

⁴⁵Alexander Ya. Varshavsky: Ph.D., D.Sc., currently Professor at California Institute of Technology, Member of the National Academy of Sciences of the USA.

⁴⁶Vladimir A. Engelhardt (1884–1984): Ph.D., D.Sc., Member of the USSR Academy of Sciences, Founder and Head of the Institute of Molecular Biology, USSR Academy of Sciences.

fell under the Russian Penal Code Article 190'). The more innocent things, like Brodsky's poems, I brought to work. R.B. once borrowed my retyping of the Brodsky's book "Part of Speech." Some time later I asked him if he had liked the poems. "No," he answered. "It seems that upon emigrating he is doing poorly, and anyway, his youthful poems were much better."

Finally, let's not forget the banner which I have to carry high for the rest of my life. All this somehow doesn't come together in the image of a dissident. And what exactly was meant by David Goldfarb, remained unclear. I figured it out many years later, already in the States, from conversations with direct participants of some crucial events, Ed Trifonov⁴⁷, Maxim Frank-Kamenetskii, and David Goldfarb.

In the mid-1970s, Ed Trifonov, a senior scientist in the Radiobiological Department of the Institute of Atomic Energy and a faculty member of the Moscow Physical-Technical Institute, applied for emigration to Israel. The situation was unusual considering that both institutions were secure. That's why it was decided that the "horrible" actions of Ed would be condemned. This was done at a closed conference for Communist Party members from the Institute of Atomic Energy. Although Ed was not a Party member he was to be present, since he could not complete his emigration paperwork without the resolve of the conference. And so, putting a valium pill under his tongue, he had to hear well-acquainted intellectual people call him a traitor, a rootless cosmopolite, a bird soiling its own nest, and many more things commonly said at such conferences. To be honest, the final verdict was positive in regard to Ed's departure, seeing as how such offenders apparently had no place among Soviet scientists on Soviet land.

The ordeal didn't end there. In about two weeks an exact translation of the minutes from the closed conference of a top-secret Soviet institute, including all first and last names of the speakers, appeared in the Los Angeles Times. The rest of the Western press didn't hesitate to make many anti-Soviet and plenty sarcastic comments.

The scandal was tremendous. Needless to say, the Institute of Atomic Energy developed A- and H-bombs. Many employees of the Institute, including those who had spoken at the conference, had top security clearance. You couldn't carry a single sheet of paper out of the Institute without prior clearance from the local KGB office (the so-called "First Department"). How could such a leak occur?

KGB agents worked relentlessly. The case wasn't closed until the beginning of the Perestroika. There were 3 suspects. Ed Trifonov himself, who, however, insisted that he'd never laid eyes on the minutes and

could do without the scandal, since he was planning on leaving the country as soon as possible. The second suspect was Ed's best friend, Maxim Frank-Kamenetskii, but he couldn't have gained access to these documents, being Jewish and a non-party member. The third and final suspect was Yurii Lazurkin⁴⁸, the Head of the lab and department where both Ed and Maxim worked. As a communist and Department Head he clearly had access to the minutes, but vehemently denied having passed them along to anyone. Yurii was a World War II veteran who had, along with Igor Kurchatov⁴⁹, demagnetized Russian war cruisers during battles for Sevastopol and Stalingrad. His portrait was on exhibit at the State Historical Museum. It was difficult not to believe him. It was no longer Stalin's era, and suspects were not tortured for information. The investigation came to a standstill.

The events inevitably had their consequences. Maxim Frank-Kamenetskii and Yurii Lazurkin had their teaching rights suspended for a year. My American colleagues think I'm joking when I tell them this. To them not teaching for a year seems more a glorious reward than a severe punishment, allowing them to commit themselves solely to research. Neither of the two was ever allowed out of the country. (Maxim's first foreign trip was granted just in 1988 in the heat of the Perestroika at the personal request of Alexander Spirin⁵⁰.)

Ironically the situation played right into Ed's hands. He arrived in the West a hero, which didn't hurt his job hunting.

About a week before his death, R.B. asked Maxim to come visit him at the hospital. He said to him approximately the following. "Maxim, I am very guilty before you. You were investigated, denied your right to teach, not allowed out of the country, while the minutes were sent to the West by me. Forgive me, if you can." By Maxim's account, he went numb when he heard this.

What had happened? R.B., as Head of the largest lab in the Radiobiological department of the Institute of Atomic Energy, had to read the minutes of Party conferences. The procedure was such: a special courier brought the minutes to R.B., who had to read them on the spot, sign that he had read it, and return it to the

⁴⁸Yurii S. Lazurkin: Ph.D., D.Sc., Professor Emeritus, at the time was a Head of the Department of Biophysics at the Moscow Physical Technical Institute and Head of the Department of Molecular Biophysics at the Institute of Molecular Genetics, Russian Academy of Sciences.

⁴⁹Igor V. Kurchatov (1903–1960): Ph.D., D.Sc., Member of the USSR Academy of Sciences, directed the Soviet Union's nuclear weapons program from its inception in February 1943 until his death. Founder and Head of the Institute of Atomic Energy.

⁵⁰Alexander S. Spirin: Ph.D., D.Sc., Member of the Russian Academy of Sciences, Founder and Head of the Institute of Protein, Russian Academy of Sciences, Head of the Department of Molecular Biology, Moscow State University.

⁴⁷Eduard N. Trifonov: Ph.D., D.Sc., currently Professor at the Weizmann Institute of Science, Israel.

courier. This time, a mix-up had occurred. The courier had brought the minutes late in the evening and left them with R.B. overnight. In those times, there was no free access to Xerox machines, the only Xerox machine in the Institute was located in the KGB office. R.B. spent the whole night rewriting the minutes by hand, and in the morning had brought them out of the Institute by putting them inside the waistline of his pants. (This was exactly the same fashion in which we smuggled alcohol out.) That same morning he gave the manuscript to David Goldfarb. He, in turn, gave it to a friend of his son, Anatoly Scharansky⁵¹, who was already in serious opposition to the system and socialized with the Western press. That's how the minutes had ended up in the L.A. Times.

This whole case is not simple. I won't spend a lot of time discussing it or giving you a moral judgment, but will rather leave this to the interested reader.

CONCLUSION

What conclusion can one draw from this whole story? In Simon Shnoll's book "The Heroes and Tyrants of Russian Science" there is a section on R.B. In it, he is presented as an unquestionable victim of a totalitarian system: he suffered as a geneticist, a Jew, didn't fully realize himself as a scientist, and didn't have contact with the worldwide scientific community. I know and love Simon, but this point of view is somewhat simplistic.

R.B. was indeed a victim of the Soviet system for the above-mentioned reasons. The story with the Party conference minutes is a sign of his inability to tolerate such abuse. At the same time, he was a beneficiary of the system as well.

What other system would allow him to conduct his research on such a large scale at such a slow pace? Could one imagine R.B. in the environment of the overly competitive United States scientific system, under constant pressure to publish more and get more and more grants? Further, wasn't it his halo of martyrdom that attracted the best students to his lab? Isn't it

⁵¹Anatoly (Nathan) Scharansky: Ph.D., prominent Soviet human rights advocate, currently President of the Yisrael Ba-Aliya party, Minister of Industry and Trade, Israel.

because his associates knew his personal history that they were ready for slave labor and the daily drill-like routine? Isn't that why they were unconditionally loyal? Finally, wasn't it his martyr-like air in which his enormous authority in Soviet biology was grounded?

Russia is exactly such a country: it tramples its children into the dust, but there they somehow become folk heroes and mind setters. This type of relationship, as strange as it sounds, sometimes turns out to be productive. R.B., no matter how much the system degraded him, was appreciated and loved by his colleagues and pupils. What more could one ask for? Is this sort of relationship even possible in the far from sentimental U.S. scientific system? I highly doubt it.

Finally, a strong scientist does science not because he wants to answer some specific question or to assume some specific position, but because he cannot live otherwise, he has to reach his full potential. He does this in whichever niche he may find himself in, if even by hook and by crook. Only if this becomes completely impossible will he find a new niche. Let everyone reach the same level of potential as R.B. did. Yes, the system beat him to the ground, but he, being a strong man, managed to stand up and get from it all that was required to sustain his one true love: Science.

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