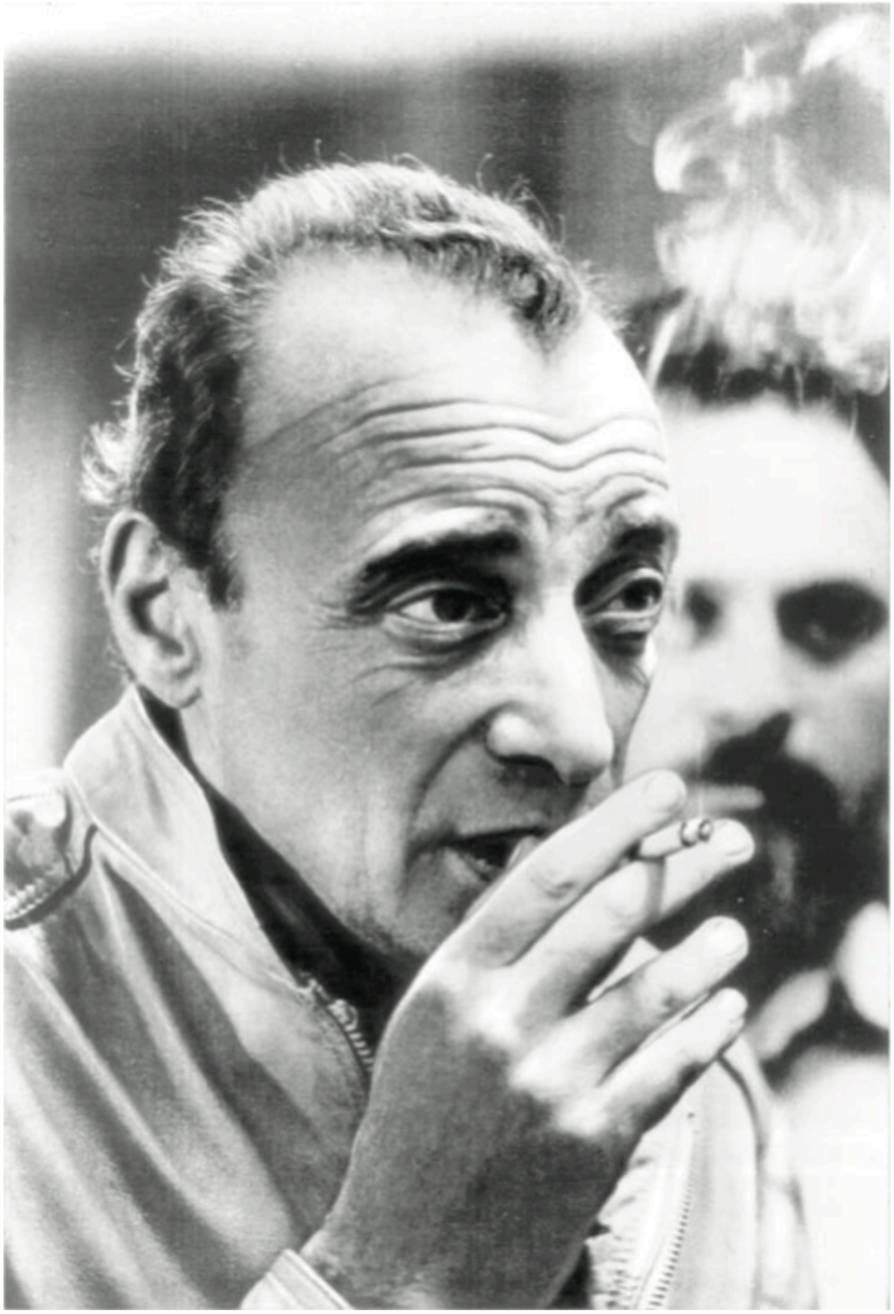

Vladimir Gribov

1930 - 1997

VLADIMIR NAUMOVICH GRIBOV*

Gribov was an outstanding theoretical physicist, a deep thinker. His profound insights and results, powerful theoretical constructions lie at the heart of theoretical description of soft particle collisions at high energies. They continue to be used all over the world, both by theorists and experimentalists.

V. N. Gribov was born in Leningrad on March 25, 1930. In 1947 he enrolled at the Physics Department of the Leningrad State University. He graduated with honors in 1952, with specialization in theoretical physics. At first, until 1954, he had to work as a teacher at a vocational school, carrying out physics research only at spare time, at home, and attending Shmushkevich's seminars at the Physico-Technical Institute of the Academy of Sciences of the USSR in Leningrad (PTI). In 1954 I. M. Shmushkevich and K. A. Ter-Martirosyan succeeded in getting him a job at the Theoretical Department of PTI, playing on a relative decline in state-sponsored anti-Semitism. Thus, he became a research assistant which allowed him to entirely focus on research.

The first paper of V. N. Gribov, *Interaction of Two Electrons*, was published in 1953 in the journal *Vestnik Leningradskogo Universiteta* [Leningrad University Bulletin]. This work, on the theory of ionic dielectrics and hydrodynamics, was carried out in cooperation with L. E. Gurevich. Then Gribov's scientific interests shifted, under the influence of L. A. Sliv, K. A. Ter-Martirosyan, and I. M. Shmushkevich, to nuclear and elementary particle physics. His PhD dissertation on neutron excitation of the rotational levels of non-spherical nuclei was completed in 1956 under the supervision of K. A. Ter-Martirosyan.

*This compilation is based on two articles published in *Sov. Phys. Usp.* 33, 872-873 (1990), and *Phys. Usp.* 41, 407-408 (1998).

In a number of papers published in 1957–1959 Gribov developed a phenomenological theory of near-threshold reactions that produce several particles. He worked out what later became a classic method of determining pion-pion scattering lengths. This work was followed by a large number of papers on analytic properties of the scattering amplitudes in quantum field theory. V. N. Gribov's lectures on quantum field theory – a limited release of PTI – became the standard textbook for an entire generation of Soviet physicists.

The decisive phase of Gribov's scientific career started at the end of the 1950s, with regular journeys to Moscow, to attend the seminars run by L. D. Landau and I. Ya. Pomeranchuk and to discuss physics with them. Both were of extremely high opinions of Gribov's talent. Landau treated him as his successor. Pomeranchuk's interest in hadron collisions at asymptotically high energies can be traced back to Gribov's influence. In 1962 Gribov became the Head of the PTI High Energy Theory Department.

In the 1960's, V. N. Gribov, together with I. Ya. Pomeranchuk, was instrumental in the development of the theory of complex angular momentum, the so-called Regge theory. With remarkable virtuosity, Gribov used the analyticity and unitarity of the S matrix and predicted that the diffraction cone in elastic hadron scattering must contract asymptotically with increasing energy; this corresponds to a logarithmic growth of the interaction radius. The American theorists G. Chew and S. Frautschi came to similar conclusions almost simultaneously, and the French theorist M. Froissart derived the limit for the rate of the asymptotic growth for hadron cross sections (the so-called Froissart limit).

In 1962, Gribov and Pomeranchuk, and independently the American theorist M. Gell-Mann, showed that the Regge pole exchange leads to the so-called factorization, and established asymptotic relations between the cross sections of various processes. For example, the squared cross section of the pion-nucleon scattering must be equal to the product of the pion-pion and nucleon-nucleon cross sections.

The Regge pole with the quantum numbers of the vacuum was called the Pomeranchuk pole or Pomeron. It led, in a natural way, to the Pomeranchuk theorem on the equality of the particle and antiparticle cross sections for the scattering off a given target. Then a straightforward path led from the Pomeron to poles with other quantum numbers, such as those of the ω meson, ρ meson, nucleons, and so on.

Research of the multiparticle unitarity constraints on complex momenta that began with Gribov's seminal 1964 paper co-authored with I. Ya. Pomeranchuk and K. A. Ter-Martirosyan, culminated in 1967 with the development of the Gribov Reggeon field theory. Gribov's analysis of scaling laws, carried out together with E. M. Levin and A. A. Migdal in the tight-binding version of the interacting Pomeron theory, played an important role in the evolution of modern theory of type II phase transitions. In 1972 V. A. Abramovskii, V. N. Gribov, and O. V. Kancheli published their famous AGK paper in which they derived the relation between multi-Pomeron contributions to the elastic scattering amplitude and the inclusive spectra in multiparticle production processes, the so-called AGK cutting rules, that underpin modern theory of the soft inclusive processes. For a long time Gribov's papers on the Pomeron theory and inclusive processes topped the list of the most cited works by Soviet physicists.

To understand what impact Gribov had on the Regge theory it suffices to list a number of classic results that bear Gribov's name: The Froissard–Gribov partial wave decomposition; factorization theorem of Gribov and co-workers, the Gribov–McDowell symmetry of the fermion trajectories, the Gribov–Volkov conspiracy relations, the Gribov–Morrison selection rules in the diffractive dissociation: the Gribov–Pomeranchuk–Ter-Martirosyan Reggeon unitarity conditions; the Gribov Reggeon field theory; the Abramovski–Gribov–Kancheli cutting rules for the inclusive processes, etc. His studies of the Regge theory propelled V. N. Gribov into the leaders' club of high-energy interaction theorists and brought him worldwide acclaim.

The 1965 paper by V. N. Gribov, B. L. Ioffe, and I. Ya. Pomeranchuk deserves special mention. In this paper they were the first to discuss a possible increase of longitudinal distances relevant in strong interaction dynamics. This paper and Gribov's subsequent 1973 lectures on the parton model and its connection with the Reggeon field theory played a key role in the development of the current space-time picture of the inclusive processes. The idea of a relativistic increase of the secondary particle production domain is now a fundamental ingredient in theory of multiparticle production in nuclear collisions.

Gribov also made significant contributions to the theory of interactions between nuclei and high-energy particles. In 1969 he developed a field theory for such processes, based on the theory of inelastic screening. Inelastic screening is of crucial importance in the estimate of the neutron cross-sections from data on interactions with deuterons. Also in 1969, Gribov formulated a model of generalized vector dominance that is now widely used in analyzing relations between photo-absorption and deep inelastic lepton scattering off nucleons and nuclei.

In the late 1960s Gribov initiated an impactful program of studies of the Reggeization in quantum field theory (Quantum Electrodynamics, QED). This is despite a general disbelief in field theory which descended on the Soviet theoretical community after Landau's discovery of zero charge (i.e. infrared freedom of QED).

In a number of groundbreaking papers written with V. G. Gorshkov, L. N. Lipatov, and G. V. Frolov devoted to this topic Gribov and co-authors obtained a fundamental result: in QED the vacuum singularity – Pomeron – lies in the plane of the complex momenta with $j > 1$ and corresponds to an increasing total cross-section. For the purpose of this investigation the authors basically invented a double logarithmic approximation, which became a standard tool in hard processes of this type.

In 1972 V. N. Gribov and L. N. Lipatov constructed a consistent field formulation of the parton model and proposed a computational technique for evaluating deviations from scaling that occur in deep inelastic scattering and electron-positron annihilation into hadrons. In these papers, which preceded quantum chromodynamics, they were the first to derive equations for the evolution of parton distributions, which today are known as the DGLAP equations. With the advent of QCD the Gribov–Lipatov program was generalized to cover non-Abelian gauge theories, and gave rise to a huge direction of theoretical activities. In 1977 Yu. Dokshitzer and independently G. Altarelli and G. Parisi presented a generalization of the Gribov–Lipatov 1972-1974 results, the DGLAP equation. This was a beginning of an avalanche. Of special interest is the kinematic region of soft partons which was thoroughly studied at the electron-proton collider HERA (DESY). This kinematic region was theoretically analyzed by Lenya Gribov, V. N. Gribov's son, in his turn an exceptionally talented theoretical physicist. The death of their only child in 1984 in a mountaineering accident in the Pamir mountains was a tragic blow for his parents.

The 1969 paper by V. N. Gribov and B. M. Pontecorvo on neutrino mixing anticipated many results of the wide-ranging debate on neutrino oscillations that started a decade later and culminated in the discovery of the neutrino oscillations. This was in 1969, long before the τ-lepton discovery.

With the advent of QCD Gribov's interests shifted again. He concentrated his efforts on non-Abelian field theories in nonperturbative regime and, in particular, on the phenomenon of confinement. He was the first to point out that instantons correspond to tunneling transitions between different pre-vacua in Yang–Mills theories. Unfortunately, this paper remained unpublished. In fact, that was his habit, he never rushed to publish leaving his ideas up in the air. Another example of this type is as follows. Long before S. Hawking, Gribov insisted, in discussions with Ya. Zel'dovich, that black holes must emit particles via quantum tunneling. Now this phenomenon is known as the Hawking radiation.

In 1977 Gribov discovered a non-uniqueness in the quantization of the non-Abelian gauge fields, the so-called Gribov copies. The problem of the Gribov vacuum copies has not yet been completely solved till this date.

In the last decade of his life Gribov invested enormous efforts in understanding quark confinement from a non-conventional standpoint.

After PTI's split, when the Leningrad Institute of Nuclear Physics was created in 1971, Gribov became the Head of the Theoretical Section of this Institute. Now, the theoretical section of the Leningrad Institute of Nuclear Physics is staffed almost exclusively by two generations of his students. Gribov had many students in Moscow, Tbilisi, and elsewhere. Gribov's lectures and evening seminars at the annual Schools of Nuclear and Particle Physics held by the Leningrad Institute of Nuclear Physics always attracted an enormous audience.

In 1980 Gribov moved to Moscow and became the Head of the Theoretical Physics Section at the L. D. Landau Institute of Theoretical Physics of the USSR Academy of Sciences.

The scientific achievements of V. N. Gribov brought him wide recognition. In 1971 he became the first recipient of the L. D. Landau Prize of the USSR Academy of Sciences. In 1972 he was elected as a Corresponding Member of the USSR Academy of Sciences and a Honorary Member of the American Academy of Arts and Sciences in Boston. In 1978 he was awarded the *Badge of Honor.*

Gribov's books and monographs:

- *Strong Interactions of Hadrons at High Energies*, (Cambridge University Press, 2009);
- *The Theory of Complex Angular Momenta: Gribov Lectures on Theoretical Physics*, (Cambridge University Press, 2007);
- *Quantum Electrodynamics: Gribov Lectures on Theoretical Physics*, with J. Nyiri, (Cambridge University Press, 2005);
- *The Gribov Theory of Quark Confinement*, Ed. J. Nyiri, (World Scientific, Singapore, 2001);
- *Gauge Theories and Quark Confinement*, (Phasis, Moscow, 2002).

V. N. GRIBOV
1930–1977 *

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This is not an obituary.

This note is intended to make the physics world aware of the loss it suffered on August 13 when professor Vladimir Gribov passed away, all of a sudden, in Budapest where he was steadily recovering after a mild stroke.

Vladimir Naumovich for youngsters, Volodya for friends, BH (his Russian initials taken as Latin letters) for colleagues world-wide.

His devotion to physics was so intense, his knowledge, shared with anyone willing and prepared to listen, was so deep, that I feel one can still seek his advice, discuss problems with him, trying to probe new ideas against the incredible physical intuition of this man, to match them with his "picture". I am sure that many physicists, from St.Petersburg, Moscow and Novosibirsk, as well as those western theorists who knew him well, share this feeling.

Gribov graduated from Leningrad University in 1952 when for a young man with Jewish blood there was not a slightest chance to get a decent job. After Stalin was gone, the paranoid antisemitic wave receded. With the help of Ilya Shmushkevich and Karen Ter-Martirosyan, Gribov, having served his term as a teacher at an evening school for adults, was able to start his scientific career in Russia's first research institution — the Physico-Technical Institute

*A preliminary version of this article appeared in 1998 in [1]. A full version is ArXiv: physics/9801025.

(later, Ioffe PTI) in Leningrad. Soon he was recognised as an informal leader of the theory group created and cherished by Shmushkevich. This group, under Gribov's lead, was to become one of the centres where the world-class physics of the 60's-70's was being developed, later to be known as the "Leningrad school". In 1971 the theory group became a part of a new Nuclear Physics Institute (LNPI) in Gatchina, near Leningrad.

In the late 50's Gribov was brought to Moscow and introduced to Lev Landau. It did not take long for Dau to form a high opinion of Gribov. A special fund was created to allow a young physicist to commute from Leningrad (400 miles one way) to participate in the weekly Moscow Landau seminars.

There Volodya was to meet Isaak Pomeranchuk who became his close friend and collaborator and made a great impact on Gribov the physicist. Gribov always referred to Pomeranchuk as his true Teacher. He admired Chuk's intuition, style of doing research and his attitude to life and to physics.

BH belonged to a generation of physicists, now almost extinct, for whom physics, in all its variety and complexity, was still felt as a single subject, who "had a picture", in his words. "He has a picture", was Gribov's highest compliment, a universal formula ranging from appreciation to admiration.

Gribov was always open to discussion. He never refused, as far as I know, to discuss a physical problem, be it of nuclear physics or elementary particle physics, cosmology or radiophysics, solid state physics or atomic physics. Not only did he know quantum physics as deeply as one can know it, he *felt* quantum mechanics, he *thought* quantum-mechanically. FSU physicists remember Yakov Zeldovich saying at the plenary session of the annual Academy meeting: "What a fool I was not to listen to what Volodya Gribov was telling me, long before Steven Hawking's work, on why and how black holes should radiate via quantum tunneling". Gribov was the first to interpret an instanton — a classical solution of non-linear Yang-Mills equations, found by Polyakov and collaborators — as an under-the-barrier trajectory linking vacua with different topology of the non-abelian field. This interpretation has become a common wisdom. He also came to the conclusion that classical fields (instantons, monopoles, etc.) are of little relevance for the long-standing problem of QCD colour confinement (which wisdom still awaits acceptance by the community).

"I am not smarter", BH used to say, "I just think more"...

* * *

For decades he was not allowed to travel abroad: a free-minded person was not the KGB's idea of a loyal citizen. One can only guess how much harm Gribov's isolation has done to theoretical physics. Given an ever-red traffic-light on the road from LNPI to the West, many western physicists visited Leningrad in the 60's-70's to discuss new ideas with BH and his colleagues, to learn, and to go through the beneficial ordeal of a notorious "Gribov seminar".

This was a legendary seminar. It had no time limit and would go on as long as was necessary to establish the truth. Some visitors hated it and would never repeat that most dreadful experience of their life; others loved it: finding the truth of the matter was at stake and the speaker would be the first to benefit.

For a speaker it was a test of self-confidence, of the depth of his or her knowledge of the subject. Equally was it a challenge for the audience: to participate during seminars ("to work on seminars") was one of the two unquestionable duties of the members of the Gribov theory department. (The second one being: "never refuse help to an experimentalist".)

To understand the spirit of the seminar you have to accept the notion of "aggressive friendliness". No merits counted, no excuses were given: a newcomer and a renouned academician were treated equally, that is equally amicably and aggressively. After 5 minutes of a smooth introduction BH would jump to the blackboard and make his three points: what this guy is trying to tell us, why it is "all wrong", and how the problem had to be approached. This would trigger a hot discussion involving all the audience (including the speaker; though, markedly, there were historical exceptions when a speaker would leave the seminar room).

BH as a speaker would be given the same friendly treatment. That is, the story goes, how Lev Lipatov, now a world-known theoretician and an academician, became a co-author of the famous Gribov & Lipatov work of 1970-71 which laid the basis of a field-theoretical description of deep inelastic scattering and e^+e^--annihilation. Gribov was presenting his work, and the young man made a couple of "killing" comments. Gribov got stuck trying to answer Lipatov's questions: "Lev, you are a co-author already, help me", was the solution.

Many a difficult problem was cracked in this fashion, at the blackboard, in the noisy (and, in early days, smoky) atmosphere of PTI/LNPI seminars.

* * *

Gribov was never an icon, and a rosy picture of this character would be unphysical and therefore false. He had a strong personality, strong both in its rights and wrongs.

It was not easy, to say the least, to argue with BH. In spite of his mind being fast, flexible and receptive, a prejudice of his could be stone solid. You would not dare to start arguing with him before making absolutely clear for yourself that the man was wrong. Such a dispute could eventually rise to a fight, sometimes reaching heights which any socium with a minimal awareness of good manners would classify as absolutely unacceptable. To shout at your boss, though, was pretty safe: Gribov and his Leningrad colleagues always remembered the heritage of Ilya Shmushkevich: "a scientific argument cannot lead to administrative conclusions" (sounds much better in the original Soviet newspeak).

Neither was Gribov always right in his vision. It took a good 10 years for him to accept quarks as the true basis of hadron physics. He encouraged, though, his young students to play with a new hypothesis and discussed with them applications of the quark picture to hadron scattering. Hence, the famous Frankfurt-Levin ratio of the pion-proton and proton-proton cross sections of the early days of the quark model.

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"When I was young I was happy to see the pieces of a lengthy calculation cancel and produce a zero result. This told me that I had been smart and had not make a mistake. Only later did I recognise that this was stupid: a good physicist should know a priori that the answer will be zero." This recollection of Gribov's can tell you much about his research style, a very special way of attacking a difficult theoretical problem that he developed and used with brilliance. He had a profound knowledge and skill in using mathematical methods in physics. However, describing his results Gribov would not stress the mathematical difficulty, not even the mathematical beauty, of the solution he found. What mattered most was, once again, "a picture". He would approach the problem from different angles, abstracting its essential features and illustrating them with the help of simplified models and analogues from different branches of physics, solid state physics being his favourite source of inspiration.

You were led to see that the answer is correct because there is a clear physical picture behind its structure and its properties, not merely because it has emerged as a result of a derivation following the mathematical deduction rules. People unfamiliar with this style were often confused. After Gribov's talk some felt they were being cheated: a couple of chalk drawings, a strain of hand-waving arguments, and — here you are: that's the answer? Such listeners were not aware that they fell victim of the speaker's generosity: for Gribov it went without saying that the receiving party is capable of reproducing the necessary mathematical calculations and analysis, this being a default professional quality. He was talking physics. Even when a mathematical framework to envelope a foreseen physical answer was not developed, and thus the problem not solved, this would not stop him from sharing his ideas and arguments with anyone willing to listen. Physics was given top priority, ambitions put aside. "Physics goes first" was the motto.

One inside story to illustrate the point. A project BH was pursuing with his student had run at some point into a rather difficult mathematical problem. The student was given a page of notes where the basic idea of how to approach the problem was briefly explained, followed by few lines of calculations. He was shocked to find out that the very first equation that the maître had written was wrong. Having done the job and having noticed that the other nine equations that followed were dead wrong as well, the student arrived at the answer. He compared it with what was written on the bottom of the Gribov note, and the answer there was the correct one. Weird though it might seem, it was neither a miracle nor an accident. According to Alexei Anselm, for many years Gribov's collaborator and friend, — "Working with BH you had a strange feeling that numbers were his personal friends: all those factors of 2 and π simply knew their place in Gribov's formulae".

* * *

Gribov left Leningrad in 1980, on the eve of turning 50. It was a hard blow for the LNPI theory laboratory — the Gribov laboratory. It remained a group of top-class theorists but was never again the unique team that it had been. The loss for BH proved to be comparable, if not stronger. Having moved to Moscow for personal reasons, he found himself pretty much in isolation. The Landau Institute for Theoretical Physics in Chernogolovka, with which he formally became affiliated, had its established orderly way of things. It goes without saying that everyone respected Gribov, a "ring-bearer" of the Landau tradition. At the same time, the community as a whole was not ready to accommodate such a disturbing and virulent force: he did not fit into the style of Chernogolovka seminars.

Later he lived permanently in Budapest with his new family and, in a wider world, was being warmly received in the US and Sweden, France and Italy. Recently Gribov, as a Humboldt awardee, enjoyed the hospitality of the Nuclear Physics Institute in Bonn. However, no place was to be found in the West for a man about to turn 60, where he could start a new school and work in a team — a natural Gribov environment.

Many a year went under the strain of personal tragedy. Lenya Gribov, the son of Volodya and his first wife Lilya Dubinskaya, perished in a mountaineering accident a few months after defending his Ph.D. in theoretical physics. Volodya kept cursing himself for having infected Lenya with his passion for mountains. Neither time nor the loving attention of wife Julia and step-son Palik could help to heal the wound.

When asked by Julia what physics meant for him, Volodya said that he had realised quite early that if he made an effort he could find the truth. And therefore, he had decided, he must. He kept working, working on the most challenging problem, working with unmatched persistence and intensity which has only doubled after the loss of his son.

Being a perfectionist, BH would not write a paper before he had the final solution of the entire problem that he had set for himself. August 13, 1997 caught Volodya Gribov in the process of writing up the work concluding his 20-year-long study of the problem of quark confinement in quantum chromodynamics.

* * *

Gribov's contribution to physics deserves a special study. It suffices to say that his name is attached to many a key notion of modern theoretical physics: Gribov-Froissart projection and the Gribov vacuum pole (Pomeron), factorisation, Reggeon calculus, Gribov diffusion, the AGK cutting rules, the Gribov bremsstrahlung theorem, Gribov-Lipatov evolution equations, and many more.

Gribov's impact on modern physics is deeper than it is known to be.

One of his jewels "Interaction of photons and electrons with nuclei at high energies" where the space-time picture of particle interactions at high energies was established, found its way through the iron curtain. The key elements of this work were incorporated into the famous Feynman book which laid the foundation of the parton model. The Feynman-Gribov parton model, that is.

Gribov with Alexander Migdal developed an ingenious technique for analysing dynamical systems with long-range fluctuations, which triggered a breakthrough in solid state physics. The physics of solids near the critical temperature proved to be similar to that of the so-called strong-coupling regime of high-energy hadron-hadron interactions. The subsequent works of the "two Sashas" — Polyakov and Migdal — and a contemporary more general treatment suggested by L. Kadanoff and K. Wilson have established the scaling solution of the problem of the second order phase transitions.

Gribov's QCD studies produced a brilliant physical explanation of asymptotic freedom, based on an early observation of the anti-screening phenomenon made by Julij Khriplovich in a pre-historic 1969. In 1977 Gribov demonstrated the inconsistency of the standard field-theoretical treatment of gluon fields (Gribov copies, the Gribov horizon). Later he suggested the quark confinement scenario based on super-critical binding of light quarks by a quasi-Coulomb colour interaction.

His last works remain to be discovered, understood and developed.

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Vladimir Gribov believed in the Truth in physics. Not that he was a naïve man, but he could not (or rather did not want to) understand how some people calling themselves physicists would politely listen to and applaud "nonsense". He thought that everyone shares his "physics goes first" belief and is ready to put aside any political, mercantile considerations when a physical issue is at stake. In our pragmatic world such a scenario does not look very realistic. However, since his commitment to physics was close to religious, we can consider it as Gribov's prophecy for the physics world of the future.

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1. Yuri Dokshitzer, *V. N. Gribov, 1930-1997,* in A. V. Smilga (Ed), *Continuous Advances in QCD 1998*, (World Scientific, Singapore, 1988) pp. xv-xxi.

ON V. N. GRIBOV

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No prophet is accepted in his own country

Vladimir Naumovich Gribov was undoubtedly the greatest theoretician of the postwar generation in the USSR. Even a short list of his major scientific achievements is impressive: the theory of threshold multiparticle reactions, Gribov-Froissart projection; shrinkage of the diffraction cone at high energies; Gribov–Pomeranchuk factorization of the contribution of Regge poles; Gribov–Morrison selection rules; Glauber–Gribov theory of diffraction scattering on nuclei; Gribov Reggeon diagram technique; Abramovsky–Gribov–Kancheli rules; the Bjorken–Gribov paradox and Gribov generalized vector dominance; Gribov-Pontecorvo neutrino oscillation; the theorem of bremsstrahlung at high energies; the Gribov–Lipatov evolution equations of structure functions; Gribov copies, and much else besides.

In particle physics, he did more than anyone else in our country. But just as it's held true for all ages and peoples that "No prophet is accepted in his homeland," in the USSR (and abroad) during his lifetime, his achievements were valued much less than they warranted. Very belatedly, much later than a number of other theoretical physicists, he was elected a corresponding member of the Academy of Sciences, but for the rest of his life no place was found for him among the Academy's full members. Of all the awards, prizes, etc., possible in the Soviet Union and Russia, he received only one – the Landau

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Medal. True, he told me this was the only award he really wanted to receive. And only very rarely was Gribov invited to make prestigious rapporteur presentations at major international conferences (in the USSR only at the Dubna Conference in 1964).

But there were people who immediately appreciated his talent: Pomeranchuk and Landau. In the late 1950s, N.N. Bogolyubov received a Lenin Prize nomination for work on dispersion relations. Landau took part in reviewing the nomination materials. In his review, Landau wrote that the Lenin Prize for work on the given topic should go to Gribov, not Bogolyubov. A short time before this, Gribov did work on spectral representation of the vertex function in field theory, and Landau saw this achievement as much greater than Bogolyubov's proof of dispersion relations. Note that this was at the dawn of Gribov's career, before his famous works on Reggeistics (Regge theory) and all the rest! Of course, Landau's review didn't influence the Lenin Prize Committee: the prize went to Bogolyubov. It's not hard to guess what consequences this review had for Gribov. He felt them for a very long time, perhaps even until the end of his life.

Pomeranchuk was not only highly appreciative of Gribov; he just loved him. I remember the heroic time of starting work on Reggeistics, the joint works of Gribov and Pomeranchuk. (They did 14 joint works.) For Pomeranchuk, and for all of us at ITEP, Gribov coming from Leningrad was always a cause to celebrate. Discussions began in the morning and continued into the late evening. A column of smoke would fill Pomeranchuk's small office. Both of them, Gribov and Pomeranchuk, were desperate smokers. And after several days of work, out of chaos would come truth those were real holidays for the heart!

Pomeranchuk considered Gribov's opinion extremely important, almost as much as Landau's. A typical example was my joint work with Gribov and Pomeranchuk on the behavior of the annihilation cross-section of e^+e^- into hadrons at high energies, Pomeranchuk's last work. This work is exceptional in his career. After the proof of zero charge in quantum electrodynamics and meson theories, Pomeranchuk believed, as did Landau, that "the Lagrangian is dead and should be buried with all due honors" (Landau's words). For 10 years, Pomeranchuk developed phenomenological and analytically based methods in particle physics (the Pomeranchuk theorem, Regge theory, SU(3) symmetry, etc.). In the work in question, Pomeranchuk returned to the methods of quantum field theory, that is, the Lagrangian. This return was difficult for him, and he wanted to be sure Gribov fully shared his views. Pomeranchuk was already seriously ill (cancer of the esophagus) and couldn't swallow; he spoke through an amplifier. But he worked, wrote formulas! We carried on discussions, sometimes Gribov came from Leningrad. And then, in one discussion this was about two weeks before Pomeranchuk died he and I (Gribov wasn't in Moscow) concluded the work was done, the result obtained.

"But," Pomeranchuk said, "call Gribov, and if he agrees with everything, start writing the article."

I called Gribov, and he said he had doubts about the argument. I relayed this to Pomeranchuk. He responded that, although he, Pomeranchuk, had no doubts, as long as Gribov had at least a shadow of a doubt, there was no question of going forward. I called Gribov again and asked him to come immediately to Moscow. On his arrival, he and I took a couple of days to discuss our evidence and finally found a solution that settled all doubts. After this we went to see Pomeranchuk. It was Sunday, December 12, 1966.

"Volodya," Pomeranchuk asked, "do you have doubts?"

"No," Volodya said, and it seemed to me that I saw a trace of relief on Pomeranchuk's face. But the conversation wasn't a long one, Pomeranchuk didn't feel well. He died on the night of Tuesday, December 14, 1966. The article had to be written without him.

Scientific (and not only scientific) discussions with Gribov were always full of the intense heat of creativity, of its combustion. (I can't find anything better than such hackneyed words.) At the same time, he was uncompromising in science. When his opinion was that a work was incorrect, it was impossible to convince him to accept or even keep quiet about it. But if as a result of discussions (often very lengthy) Volodya agreed, you could be 100% sure the work was correct. Of course, this had its downside. Sometimes Volodya was mistaken and refused to accept a correct and sometimes even very good idea. And because his arguments were persuasive (but, as it sometimes turned out later on, incorrect) and his authority was great, people gradually relented to him.

One example, very important (and distressing) for me. In early 1972, after 't Hooft proved the renormalizability of non-Abelian gauge theories, I realized the Landau–Pomeranchuk arguments about the internal contradictions of Yukawa theories (the unphysical pole of the effective charge at high energies) don't apply to non-Abelian theories. This was the logic of my reasoning: the Landau–Pomeranchuk arguments were actually based on the Kallen–Lehmann representation for the photon propagator in quantum electrodynamics (or the meson in meson theories). According to this representation, since the imaginary part of the propagator is positive, it should grow with the growth of energy, and then a pole's appearance is inevitable. But in non-Abelian gauge theories, the gauge boson propagator is not gauge-invariant, and therefore it's impossible to make such claims. However, I didn't know the calculation technique for non-Abelian theories.

Just then Vainshtein arrived from Novosibirsk; he knew the technique. I tried to convince him to do the relevant calculations, worked on convincing him for two days and on the third day convinced him. And here's the catch: Gribov arrived from Leningrad and in a couple of hours made Vainshtein change his mind; Gribov argued with great confidence that non-Abelian theories will have the same pole (i.e., zero physical charge) as in quantum electrodynamics. To my embarrassment, I must confess that I overlooked the Khriplovich's earlier, published work where the calculations I needed were already performed, and Vainshtein, surprisingly, told me nothing about it. Studying the calculation technique for non-Abelian theories took time, which I didn't have: I was due to leave for Czechoslovakia soon to start up a nuclear power plant.

Gribov had a way of approaching a problem, a phenomenon, from a new, unexpected side, as a rule deeply physical, and the phenomenon started to sparkle with new colors. Many examples can be cited: instantons (the idea that instantons in Minkowski space tunnel between vacuums with different topological multipliers belongs to Gribov), Gribov copies, etc. Or closer to home for me: the sum rule for γN and eN scattering (work done by Gribov, Shechter and myself). Here Gribov managed to look at this problem from the viewpoint of Yang-Mills theory, and this made it much easier to understand. Another similar example: Gribov's work on the interaction of photons with nuclei and the connection of deep inelastic scattering with e^+e^- annihilation the Gribov–Bjorken paradox. Finding and formulating a paradox, as Gribov was able to do, is the best way to achieve the advancement of science.

At seminars, when Gribov made his presentations, he spoke while he thought (always without papers), as if inviting the participants to solve the problem along with him. In this respect he was like Pomeranchuk, who also seemed to improvise when he lectured or spoke at seminars. (With Landau it was different: it was obvious that for him the problem was solved and that he was giving us, the ignorant, its outlines.) And seminars at ITEP, when Gribov spoke, and in the theory department at LNPI, as far as I know, almost always ran on until late in the evening. In mentioning the theory department of LNPI, I have to say that it was basically Gribov's creation. Although I.M. Shmushkevich laid its groundwork, and this was a solid foundation, Gribov erected the whole building, and his traditions are still alive at LNPI (now PNPI). No major theoretical work, not only in particle physics, but also in other areas of theoretical physics, could leave the walls of LNPI without discussion with Gribov, and these discussions were always very productive for the authors. He also had a strong influence on experimental research at LNPI.

The situation changed when Gribov moved to Moscow. I think this was (at least for the first few years after the move) a difficult, maybe even dramatic period in his life. Life in Moscow was completely different than in Leningrad: here a greater role was played by various relationships peripheral to science and sometimes by even intrigues, the scientific hierarchy. One thing or the other was forbidden. Gribov wanted to stay free of this, but on the other hand, a life that totally ignored all this was impossible. His tie with the school he'd established in Leningrad weakened, despite supportive efforts on both sides. On the other hand, his scientific contacts in Moscow, although established, weren't as close as in Leningrad. Finally, in Leningrad, Gribov belonged to the overall intellectual elite, not only in physics or science: he knew and met with many different people, and many knew him. In Moscow there was none of this. Here in general the notion of an intellectual elite is much less clearly defined a lot depends on a person's closeness at a given time to those in power.

On top of all this there was the tragic, senseless death of his son Lenya in the Pamir Mountains: he fell, broke through into a crevasse on a peaceful glacier, and was already dead when they pulled him out. I feel my own share of the blame for this accident. For several decades I spent time in the mountains, and then my son started to do the same. We were friends of Lenya's. Our example may have somehow influenced him, and he took this up as well, although physically his preparation was worse.

And here I want to return to where I started. All of us, close friends and colleagues of Volodya Gribov, should feel a sense of guilt that in Russia he never had the esteem or recognition his achievements deserved. This lack of recognition, of course, affected his mood. And I would like what I've written here to have resonance as my belated words of repentance.

IN MEMORY OF A FRIEND

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It is really hard to write about Volodya. There are no words that can express my admiration for his talent and the charm of his outstanding personality. Time can not soothe the pain of his untimely death. It only makes it worse, especially for the scientific world.

The achievements which make Gribov a remarkable person are very well described in the previous articles of Yu. Dokshitzer and others. So, I will dwell more on our personal contacts and impressions.

The first time I saw Volodya in 1957 or 1958 at the seminar hosted by Landau, who was my post graduate supervisor at the time. I remember a young man, whom I had not seen before, stand up during the talk and start firing questions at the speaker. And the latter found it difficult to satisfy the young man with his answers. Landau took the young man's side and agreed with his objections to the speaker. This inquisitive young man was Volodya.

I got to know him closer when in 1958 after doing my doctorate degree I came to work at Leningrad Physico-Technical Institute (LPHTI). The atmosphere at the Theoretical Department was really pleasant. The Head of the Theoretical Department Ilya Mironovich Shmushkevich, the friend of I.A. Pomeranchuk, was sticking to Landau's school and followed its traditions.

One of the important elements of the activity of the Theoretical Department was a seminar. Shmushkevich tried to clarify every aspect of the issue in depth. Therefore the seminars went on for hours and involved lots of argument. Gradually Gribov took the leading role and was the one who could clearly define the problems and on most occasions find solution to them.

The seminar used to attract not only those who worked in the Theoretical Department. Ludwig Faddeev was one who attended them and used to give a comprehensive explanation to arising mathematical problems. People in the Department worked in a very warm atmosphere of understanding. We were young and tied by interest in science and close friendship. I remember our amicable feasts. Sometimes we were invited by Shmushkevich to his place. I still remember those friends who are alive and those who passed away. Besides Volodya there were V. Shekhter, A. Anselm, I. Dyatlov, S. Maleev, Yu. Petrov and others.

We made friends with Volodya at once. Neither of our scientific career was smooth. Upon graduation from the University we had a hard time finding a job in a scientific institution. Volodya started as a teacher at an evening school, and I became a teacher in a village school 100 km from Moscow. After Stalin's death the situation gradually changed. L.D. Landau (who was the one to examine me on his theoretical minimum) was able to take me to a postgraduate school, and K.A. Ter-Martirosyan has demonstrated his strong will by making Volodya accepted by LPHTI.

Apart from our professional problems we had a similar family situation. We both had young sons and enjoyed exchanging our experiences.

Volodya had a very rare talent for evaluating new works. He showed interest in other topics, not always connected with his work. He would go deep into the matter, find good qualities of the work (if there were any) and its weak points. His remarks, if the author listened to them, often led to new turns in research. Kind-hearted, he at the same time was intolerant both towards scolism and unreasonable results. In this respect he took after Landau. A few years later when Volodya started working with I.Ya. Pomeranchuk, the latter said: "You can hardly imagine the pleasure of working with Volodya. He very much reminds me of Landau".

Volodya himself set high standards to his works. He never wanted to publish them as fast as possible, searching for additional proofs of his findings. I remember when in 1958 I arrived in Leningrad and met Volodya, he was discussing the representation that determined analytical properties of the scattering amplitude over two variables, i.e. energy and momentum transfer, and was looking for convincing arguments in favour of the one. What he told me (and it was not published) coincided in essence with the Mandelstam representation that appeared the same year. Hence Volodya was well prepared to use it and obtained the results, considered nowadays classical, on the asymptotical behaviour of hadron scattering amplitudes at high energies. Yu. Dokhitzer and L. Frankfurt wrote about it in the Preface to the book *V.N. Gribov: Gauge Theories and Quark Confinement* (1) .

In 1960 I started to work in the Joint Institute for Nuclear Research in Dubna. Several experiments on weak interactions that were rather interesting for me, were being prepared there. However, Volodya and I kept in touch. He often visited Moscow and stayed there for long periods of time.

The results obtained by Volodya greatly impressed Landau and Pomeranchuk. Landau thought that Volodya's approach was a way out of the tight corner for the field quantum theory, where the latter found itself after the discovery of the "zero-charge". From his point of view unobservable quantities should have been taken away from the theory, among them were field operators ψ, and consequently, the Hamiltonian, which could be constructed only from the field operators. Following Landau's ideas the theory should be based on scattering amplitudes with their properties: analyticity (causality), unitarity and crossing symmetry (relativism) (2). Volodya's results quite obviously pointed to the fruitfulness of such an approach and gave extremely interesting predictions (for example, the one pointed to the shrinkage of the diffractive cone in hadron scattering, i.e. to the growth of hadron interaction radius with energy).

Landau highly appreciated Volodya's talent, his possession and devotion to science. I remember him saying many times that he together with Volodya would continue writing his course on theoretical physics. It happened when Landau gained consciousness in the hospital after the accident.

Everyone who knows Volodya, marks his terrific intuition. There is a lot of examples: the physical interpretation of the instanton, his remarks that in non-abelian theories anti-screening was quite possible, his space-time picture of interactions, that was the predecessor of the quark-parton model, etc. Not all of such remarks were timely appreciated by Volodya's colleagues.

In 1965 S. Alliluev, A. Logunov and I managed to explain the behaviour of hadron scattering at large angles, which was established by Orire, with the help of the model of scattering on Gaussian potential. In this model perturbation theory approximations grow at first, but then they decrease. Summing them up we have got the required law. When in Leningrad I asked Volodya if it was possible to obtain the required result in the ℓ-plane. "No problem" — was the answer. "These are branchings, they should be able to explain this phenomenon perfectly well."

Our conversation took place in a small room right before a theoretical seminar. There were a lot of people, but Volodya's remark did not seem to interest anybody. It was only in a few years, that A. Anselm treated this question in his doctorate thesis and derived the Orire law summing branching in the ℓ-plane. (He did not even remember Volodya's remark.)

Volodya not only had an amazing intuition in physics, he created also the necessary mathematical apparatus.

In 1968 my ten-year old son and I together with Volodya, his wife Lilya and their son went to the Caucasus on a guided tour. Our way was to the Chegemskoe clove not far from Nal'chik. Volodya took a tiny wireless with him. And one night we heard over the radio the news about American astronauts on the Moon. We were so happy and proud of this achievement. That over-the-top news was followed by alarming news – clouds started to gather over the "Prague spring" and people's hope for the "socialism with a human face" began to melt. Our march was over at the other end of Chegemskoe clove at the foot of the main Caucasian range. I sent my son back to Moscow by plane, where my mother was to meet him. I myself started for a mountaineers' camp near the Adyr-Su, a tributary of the Boksan. We started for the neighbouring Adyl-Su clove to begin our ascend. (Between these two cloves there is the famous Boksan neutrino observatory, where experiments on solar neutrino detection were later carried out.)

On my way back I dropped in the post office on Boksan and was given a cable. "We are in Mestya. Volodya has had a heart attack." I was just shocked. The cable was sent about a fortnight ago. I decided to start for Mestya immediately. It would have taken too much time to get to the place by any means of transport. That meant to go by peripheral roads. I returned to our camp to take my papers and the same night I left the camp to pass Adyr-Su passage. I did not have any mountaineer's equipment, so I had to literally crawl over shabby ice bridges across clefts. Finally I passed the cleft, but had to walk along a big ice-flow Leksor. It was only late in the evening that I reached the path leading to Mestya. In the suburbs of Mestya (the capital of Svanetya, Georgian region) I met with Yura Petrov. It was quite unexpected. On learning from a mountaineers group who left the camp a few hours earlier, that I was on my way to the town, he decided to meet me.

I would like to tell a few words about Yura Petrov. He was one of the most faithful friends of Volodya. In the winter of 1941–1942 his parents starved to death in the city under siege, and the twelve-year boy was evacuated from Leningrad together with other children of the orphanage to the Caucasus. In the summer of 1942 the Caucasus was occupied by the Nazi, and the boy ran away and became a homeless child. After the war he attended an industrial school, then it was the time at a technical college, and university. He was hired in memory of his father-physicist by the Physico-Technical Institute, but the level of his knowledge was not sufficient. He was in charge of making numeric calculations of a nuclear reactor, which was going to be constructed. However, in a short time Yura brilliantly mastered physics of reactors and in the end became one of the most qualified specialists. In the 1970s together with Konoplev K. he designed the "Pik" reactor with a unique $10^{15}\text{cm}^{-2}\text{sec}^{-1}$ neutrino flux. Now this reactor is going to be launched.

Learning about Volodya's disease, Yura immediately took a plane from Leningrad. When I arrived, Volodya felt quite well. He was given a separate room in a tourist camp. People around him treated him very warmly. The woman-doctor who visited him was an excellent specialist, and Volodya said that the pain in his heart ceased when she came. Svanetya used to be an isolated area behind a remote dale at that time. But when enlightment reached it, many people were carried away with it. (For example, there was a worker in the camp whose name was Edison, and his brother's name was Newton). Volodya and I continued discussing a recently published work by Veneziano, which we began to do in Chegem.

We kept discussing politics too. It seemed that everything was going to the better. But there was the first warning bell... I returned to Moscow on the 21st of August and got a shock when I saw a newspaper. It said that the countries of Warsaw Pact moved troops into Czechoslovakia.

In March 1980 I was going to visit Leningrad to celebrate Volodya's 50th anniversary, but I heard that he was in Dubna. I came to Dubna and saw a completely happy, a bit embarrassed Volodya and learnt about the changes in his life. I was very glad about his happiness and the fact that he was able to win it. I had known Julia for a long time, meeting her in Hungary at the neutrino conferences and I was glad about their union. On the 25th of March, Volodya's birthday, besides me there was Leva Okun, Arkady Migdal and his wife Tanya, Volodya's sister Inna and Julia's friend Livia. We happily celebrated Volodya's 50th birthday.

I am sure that it was Julia who was able to save Volodya from a terrible shock connected with the death of his son, Lyonia. Volodya loved him very much and Lyonia himself had been doing very well and had carried out a brilliant work in theoretical high energy physics shortly before his death.

When Volodya and Julia settled in Moscow, I often visited them when they came back from lectures in Moscow Physico-Technical Institute. Volodya's head was crowded with new ideas. We spoke a lot about his quark confinement model, the role of massless quarks, the importance of non-abelian symmetry and the nature of the chiral anomaly. I think it was easy for him to discuss these things with me because I never keep to any doctrine and always try to understand a physical idea. Besides, Volodya knew about our work with Yakov Borisovich Zeldovich on supercritical nuclear charges. And appealed to it. To explain the nature of the supercritical vacuum he developed an analogy with a remarkable effect known in condensed matter physics: the Andreev reflection. But he did not hurry to write practically ready works, thinking them over and over thoroughly again. He liked a phrase said, I think, by Bergman: "My film is practically ready. All I have to do is to shoot it". Unfortunately, Volodya did not have time "to shoot" a lot of things, and Julia and Yu. Dokshitzer had to do a heroic work: to write his articles according to his notes.

I liked to visit Gribovs' hospitable house, discuss different questions about life, talk to growing Pal, who was so loved by Volodya.

I remember our meeting at the university in Minnesota, where Volodya came earlier than he had planned because he learnt that I had to go away. He had just recovered from his illness, but as usual he smoked a lot (lighter cigarettes, though). In the morning we went to the workshop where Volodya showed his typical features of character. He could not be indifferent as far as science was concerned. He hated pseudo-science and was able to criticize a speaker severely. The way the question was put and hence the received result seemed to him absurd. He felt so nervous that he left the workshop.

That is how I remember him: talented, full of passion and wise.

LIVING IN TRUTH: V. N. GRIBOV AND THE POST-WAR GENERATION OF SOVIET PHYSICS*

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What does it mean to live in truth?
Putting it negatively is easy enough:
it means not lying, not hiding, and not
dissimulating. – Milan Kundera [1]

Vladimir Gribov (1930–1997) was one of the leading figures in post-World War II Soviet theoretical physics. He and his colleagues worked at the cutting edge of quantum field theory, plasma physics, nuclear and elementary particle research at a time when mediocrity or decay (by international standards) ruled in many other fields of science, art, and industry. Yet, physics was in a special position: it offered both a tolerable living and an officially sanctioned exemption from ideological make-believe. Theoretical physicists like Gribov neither lived in ivory towers nor were willing accomplices in the states nuclear project. Instead they carved out a niche for themselves in which lifestyle and values were substantially influenced by their belief in physical truth.

Window to freedom

The fate of theoretical physicists of Gribov's immediate postwar generation was shaped by a totalitarian state that needed them in order to modernize and assert itself but was deeply suspicious of and

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hostile to their intellect a state that offered them optimal working conditions, and at the same time wiretapped and killed them.

When Gribovs generation entered university, the Cold War had just started, and physicists were in urgent need. Their salaries were increased fourfold, and ample research funding made available [2]. More physicists were inducted into the Academy of Sciences, which meant faster access to a bearable flat or a dacha and special food deliveries. Yet salaries stagnated under Khrushchev, and an average physicist in the seventies earned less than the bus driver that took him to his institute. Families of physicists, like everyone else, spent considerable energy in order to satisfy their basic needs, not to mention obtaining luxuries.

Nevertheless, it was not just the euphoria for science and privileges that made young people study physics after World War II; it was also the restrictive intellectual climate of the Soviet Union. The post-Stalinist generation of physicists who had come of age in the last war years lived in a society that denied them opportunities to express the horrors they had experienced under Stalin. To critically minded people physics offered a rare ideology-free niche in which one was officially allowed, even compelled, to search for the truth.

In the 1930s and 40s, physics had faced attempts of ideological cleansing. Relativity and quantum mechanics had to be defended from philosophers accusations that they were bourgeois and idealistic theories. Many of the key physicists in Leningrad, Moscow, and Kharkov were arrested in the purges of 1938-39. In 1948, the threat of a massive attack on physics arose again, as plans were made for a congress at which leading physicists were to be denounced for ideological mistakes. If the congress had taken place, the fate of Soviet physics may well have mirrored that of Soviet genetics: Stalin's protégé, the agricultural "scientist" Trofim Lysenko had just succeeded in having genetics banned in the Soviet Union (see [2], pp. 162-163). Two days before the physics congress was due to begin, however, it was cancelled. Lavrentii Beriia, the chief of the secret service, had learned just in time that work on the bomb was based on the bourgeois theories that were heading for damnation. According to an anecdote recorded by Gorelik, Stalin responded with the words,

"Let them go. We can always shoot them later."

Landau called this the first case of nuclear deterrence in history [2]. Despite such threats, ideology had little impact on physics. Although textbook passages about the Heisenberg uncertainty principle proclaimed that the principles interpretation should follow Lenins thought, only a score of opportunists and mediocre scientists were actually impressed with the attacks on "idealism."

Boris Altshuler, today a professor of theoretical physics at Princeton, recalls his decision to study physics in the 1960s: "Career choices in the Soviet Union were obviously limited. Many careers either didnt exist or offered no intellectual freedom. The natural sciences were the only profession that secured relative independence both in intellectual and economic terms."

Consequently, many of those who had actually wanted to pursue other professions also ended up in physics. When Lev Okun, who today divides his time between the Institute of Theoretical and Experimental Physics (ITEP) in Moscow and CERN in Geneva, finished secondary school in 1947, he wanted to study literature:

"My friend and I went to Moscow University, to the Department of Philology...and we wanted to talk to the dean. But before that, a professor [appeared] in this position [bent forward], and he opened the door as if a god was there, and he was very frightened and humiliated.... And my friend and I looked at each other, and we turned back and never entered this building again.... I never saw anybody [in physics] who behaved like this."

The desire for freedom played a role in prompting many of the most talented students to specialize in theoretical physics in their senior years of study. Experimentalists were vulnerable to material conditions, whereas the only thing theoreticians needed to work was their brains. In the country in which they lived, this was a big advantage: Landau performed the calculations for his theory of the shock wave in prison; later, physicists who did not find jobs in science could work at home. Not until the 1970s was the Soviet Union's lag in computerization felt in theoretical physics.

While work on projects related to bombs and nuclear power stations was expected, outside that obligation physicists enjoyed wide-ranging freedoms and little pressure to achieve quick results. Boris Altshuler describes the informal work style of theoreticians at the Leningrad Institute for Nuclear Physics in Gatchina, where he worked between 1978 and 1989, in this way:

"We had no particular obligations. We didnt have to teach, and we were basically free to decide what we wanted to work on. People in the U.S. cant imagine that kind of freedom. Here, you spend a lot of your time writing applications for grants that you may or may not get. In Leningrad, if you wanted to switch from solid-state to particle physics, no problem: all you had to do is perhaps move to another group."

From the beginning, Soviet ideology had supported a close relationship between research and industry. Science was to serve the people, not be confined to ivory towers. Yet in practice, the economy was unsuited to make use of most scientific innovations. Nonetheless, arguing that advances in physics are often based on unexpected discoveries, physicists at that time, i.e. before the late 1970s, succeeded in convincing the leadership to let them conduct the research they wanted.

Gribov's circle

After the war, the exhausted Soviet people experienced a few years that were free from the campaigns and nightly arrests of the 1930s. Soon, however, the respite was over, particularly for Jews. In the twenties and early thirties, Jews, along with the rest of the disenfranchised populations of Russia – peasants and workers – gained access to education and upward mobility. Some of Landau's students, including Isaak Pomeranchuk and Leonid Piatigorskii, came from shtetls.

In 1937, however, Stalin's nationalities policy made a clean break with pluralism. Later, war propaganda used Russian nationalism to raise morale, and after the victory, it took on a new life as a tool of oppression. The campaign against "deference to Western bourgeois science" [3] launched in 1947, was linked to the massive anti-Semitic campaigns of Stalin's last years. Most of Gribov's university friends were convinced atheists, but according to their identity documents, they were Jews.

Gerasim Eliashberg, who belonged to the circle, notes:

"[I]n 1950- 52, the cruelties of the late 1930 appeared to repeat themselves. Those who were older had already developed a system of survival ... and were very closed to outsiders. But it took us freshmen a while to realize that we had to be, too ... Our little group enabled us to survive and to stay human. [A]s a Jew I wouldnt get the permission to specialize in nuclear physics. So in my second year at the university I made a cynical decision. I went to the Komsomol leader. After the guy had understood what it was about, he said: 'There is this Tito clique at the university. Write an article unmasking them for the wall newspaper.'

And I did. But then Volodia Gribov and his friend Lionia Altshuler came along ... and said:

'Do something like this one more time and we won't say hello to you again.'"

Tania Altshuler, Lionia's later wife, was one of the friends.

"We had a toast," she recalls. "We used to say: 'To it!' And that stood for 'To (Stalin's) kicking the bucket.'"

Gribov graduated from the university in 1953. The Ministry of Middle Machine-Building – Soviet-speak for the Ministry of Nuclear Energy – assigned him to teach at a school in the town of Rzhev, Kalinin Province. But, in a textbook case of bureaucratic absurdity, he succeeded in exchanging that post for one at an evening school for workers in the Rzhevka neighborhood of Leningrad's Kalinin District. During the day, Gribov went to seminars at the Physico-Technical Institute. "Volodia was very good," recalled Karen Ter-Martirossian, "and I encouraged him to sit for Landau's theoretical minimum," a unique examination that only 43 candidates passed.

Landau, Gribov's teacher, was not just a top physicist who got the 1962 Nobel Prize for his theory of superconductivity and whose nine-volume course of theoretical physics remains a standard text worldwide; he was also an electrifying personality whose impulsive habitus stood in contrast with the conformism of Soviet society. At the seminars, he was always ready for battle, initiating wild brainstorming sessions during which everyone interrupted and chased everyone else from the blackboard. Often offensive and brusque, he demanded the kind of uncompromising search for truth from his students that drove his own quest. He preferred to work at home, on the couch in his study. Lying there, he received fellow physicists, and when he tired of them he simply turned to the wall (see [4]).

In the 1960s, Gribovs seminars became to the physicists of the "new" Leningrad school what Landau's had been to his own generation. Volodia Anisovich, a physicist at LNPI, recalls his first meeting with Gribov towards the end of his student years:

"There are some ten people and someone is talking, and I even understand what he is saying. Suddenly a man with black hair and a sharp narrow face jumps up and says something, and I see that I understand nothing. I am even a bit irritated: everything has been fine, why did he have to jump up! Suddenly a second man...jumps up, a bit older and starts arguing...Volodia [Gribov] and Karen [Ter- Martirossian]. After this a total mess sets in. The presenter disappears, Volodia and Karen shout at each other, pick up pieces of chalk, write something. At the end, Volodia is left alone at the blackboard, explaining something.... I have understood nothing of the whole thing ... so I go home."

Landau and Gribov were alike in another way. Both "felt" physics as a unity. Yuri Dokshitzer, today a professor at the Université Paris VI, describes [5] Gribov's approach to physics in this way:

"He had a profound knowledge and skill in using mathematical methods in physics. However...what mattered most was...a picture. He would approach the problem from different angles, abstracting its essential features and illustrating them with the help of simplified models and analogues from different branches of physics.

People unfamiliar with his style were often confused ... some felt they were being cheated: a couple of chalk drawings, a strain of hand-waving arguments, and – here you are: that's the answer? Such listeners were not aware that ... for Gribov it went without saying that the receiving party is capable of reproducing the necessary mathematical calculations. 'I am not smarter, I just think more,' Gribov once said."

Physics and the struggle for truth

The Leningrad physicists around Gribov and the "Moscow Lenigradians" at the Landau Institute and ITEP shared more than a style of thinking and working: they also comprised communities of lifestyle and values. Physics was for them far more than a profession: it was a vocation and a way of life. When they were not at the institute, the theoreticians worked at home, thinking, smoking, and talking: "making physics," as Gribov's second wife Julia Nyiri, herself a physicist, called it. Summer and winter schools of theoretical physics were orgies of undiluted physics-making. Events of the Leningrad Physico-Technical Institute (later the LNPI) took place in the countryside holiday homes of the Academy of Sciences. Yuri Dokshitzer, whose father had made him suffer through a rigorous musical education, played songs by Okudzhava, Vysotskii, and Galich on his guitar. Alexei Kaidalov from ITEP sang. The lifestyle of physics-making was punctuated by mountaineering and kayak trips and flavored by samizdat copies of poetry by Mandelshtam, Solzhenitsyn's prose, or Agatha Christie and Irving Stone novels bought during trips to the West. Physicists' flats housed readings by actors and concerts by bards Bulat Okudzhava and Vladimir Vysotskii, members of an emerging alternative to the totalitarian uniformity of culture.

Intellectual exchanges were of a particular intensity. Otherwise a mild man, Gribov could be harsh when he felt that someone was not honestly trying to get an answer. Intense curiosity and belief in the meaningfulness of ones work, enjoyment of the creative process for its own sake, and a sensual pleasure in being able to express a piece of physical reality in a clear form – all these may be particularly characteristic of theoretical physicists anywhere. But Western physicists too found the intellectual intensity of the exchanges that went on in the Leningrad school fascinating.

Partly, the explanation lay in the oppressive nature of Soviet society, which gave any niche culture a particular intensity. Whereas any questioning of an official statement to the outside was dangerous, discussions in the "inner circle" were endless and passionate. "How could brains seized by fear and ideological pressure at the same time think independently and creatively in their professional fields? Apparently the matter is that work was salvation, a sort of internal emigration," writes Evgenii Feinberg [6]. In addition, there were simply fewer material distractions. As Vitalii Ginzburg put it [7], "Work and science was everything to us: a perfume and even a narcotic."

Moreover, the search for truth in physics carried a broader meaning. It was the defense of a moral stand against falsehood that could not be publicly displayed in other domains of Soviet life. Independently of each other, both Evgenii Feinberg and Gerasim Eliashberg said the same sentence: "Physics was the only way to maintain ones human integrity." A third physicist, Yurii Petrov, a close friend of Gribov's, emphasized: "Numbers cannot lie." The rationality and objectivity of "pure science" offered natural scientists a way out of irrationality and ideological license.

Physicists and the state

The mistrust that characterized the relations between the power holders and the scientists they needed but whose work they could hardly control remained unchanged until the perestroika. Ella Ryndina, an experimental physicist and Landau's niece, always had a pillow on the telephone to muffle the sounds picked up by the bug. Everyone knew to speak openly only during outdoors walks. One physicist who was giving an enthusiastic account of his trip to the West, added, for the ears of the spies: "But to think of it that they have to live under capitalism!"

As division head in Gatchina, Gribov should actually have been a Party member. But he was reluctant to join, and among the 70 physicists on his staff there were just four or five members. This was unusual, even taking into account that physicists generally had very low Party membership rates, that rates among theoreticians were even lower than among experimentalists, and that ITEP in Moscow and LNPI in Leningrad had lower rates than other research institutes. Only some 10% of around 2,000 scientists in Gatchina were in the Party. So low were these rates that some of the leaders themselves grew concerned. Lev Okun recalls that Pomeranchuk, who was not a Party member, repeatedly tried to persuade him to join. "He said, 'Look, there are no party members in our department, that's bad, and for the benefit of others could you do this?' And I would tell him, 'Please go ahead, and I will follow you.'"

Within the division, there was an intuitive understanding. Ioffe recalls:

"Pomeranchuk used to come to my room and ask, 'Have you read this morning's Pravda?' 'Yes,' I would say. 'Aha! And did you notice anything in particular?' 'The article about the meeting of the Party committee in this and this province.' 'Aha! What about it?' 'The order in which the members of the Politburo were listed.' 'Aha!' And that was the end of the conversation. Both of us understood that the order of the names pointed to some shift in the power configuration in the Party leadership."

Few Soviet physicists became open dissidents like Andrei Sakharov or Yuri Orlov. "Most of us were dissidents at heart and in the kitchen, but public resistance was more infrequent than with the biologists, who were being hindered in their work," says Ilya Roizen, a student of Vitalii Ginzburg. Very few physicists were directly confronted with the choice between professional renown and human integrity that every artist or academic in the humanities had to face. They admired Sakharov's courage but saw that political engagement barred one from the practice of really good science. Even theoreticians, who continued thinking under arrest and in camps, could not produce consequential work under persecution. "I understood that I had to choose between doing science and fighting with the KGB and the government.... I thought that my first obligation is to do physics as well as I can," says Okun.

There were subtle ways to resist. Every now and then physicists would be requested to sign a state-sponsored letter of solidarity or protest. Those who did not want to often disappeared for a few days. When the President of the Academy of Sciences, the physicist A. P. Aleksandrov, was asked to criticize Sakharov in an official letter, he had supposedly gone on an extended drinking binge and was "unfortunately" unavailable.

Contacts with the West

Soviet physicists suffered from the limits on contacts with their Western colleagues. Until the mid-thirties, they had regularly published in foreign journals, but then contacts became sparser and broke down almost completely during World War II, a state that lasted until the mid-fifties. With the exception of that period, Soviet physicists did have access to the main periodicals such as Physics Review, but even the short delay with which they arrived could be frustrating. In the early 1950s we used to calculate how much we are delayed by because of being separated from world science, says Yurii Novozhilov at LNPI.

Contacts resumed in 1959, the first time a Rochester Conference, a major physics meeting, took place in the USSR. Soviet physicists were gradually allowed to go abroad again. But some remained "nevyiezdnye," a term that could roughly be rendered as "unabroadable." Gribov, for example, was not allowed to go abroad for a long time despite the fact that several conferences in the West were devoted to the Gribov copies in the 1970s. "In 1968, I wanted to invite Alexei Anselm from Gribov's team to London," recalls the British physicist Elliot Leader. "The usual reply to invitations was: 'Thank you very much for inviting Professor X. Unfortunately, he is unable to go, but we will send Professor Y.' That was someone politically correct." The first time everyone invited to a major conference in the West was actually allowed to attend was in 1988.

But, says Lev Okun, "Maybe the lack of communication with the West was in a certain sense a blessing, because it gave originality to what we did. Many serious things were first done in Russia, like Regge calculus by Volodia [Gribov], CP violation by ITEP people and Landau. These were trend-setting for the West." Three Soviet physics journals were translated into English in the United States; but by the time the scientific community was convinced of the correctness of an idea and all officials had approved it, the same idea may already have appeared in the West.

With the collapse of the Soviet Union, the symbiosis of theoretical physicists and totalitarian regime ended. Military budgets shrank; in the Academys institutes, cables fall from ceilings and paint from the walls. One can no longer live on an institute salary; there is no money for periodicals; and under President Putin, publishing is once again regulated. In the unlit corridors of ITEP, and on the wooded alleys of Chernogolovka and Gatchina, one still encounters physicists of the old guard, but most of them are visitors from abroad. Gribov, too, spent most of the time before his death in 1997 in such places as Princeton and Bonn.

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