

**The 4th Vernon W. Hughes Memorial Lecture
Brookhaven National Laboratory
21 May 2009**

**MY JOURNEY AS A SCIENTIST:
FROM THE USSR TO THE USA AND DIPOLE MOMENTS
IN
STORAGE RINGS**

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AN INVITATION

In the Fall of 1987, Vernon invited me to join his muon (g-2) collaboration.



Hardly anyone expected me to be able to do science any more, because my previous 13 years had been spent rather far from the physics community.

1974-76—unemployment because of human rights activity

1977-84—KGB prisons and hard labor camps (did smuggle out a couple of scientific papers)

1984-86—Siberian exile (visit from Y. Golfand, co-discoverer of supersymmetry)

Oct. '86 —deported to USA; Feb. '87 went to Cornell as Senior Scientist.

AND AN ABSOLUTELY SERIOUS ONE

Vernon's invitation wasn't an act of charity. He was familiar with my earlier work on muon ($g-2$) and spin in general.

- He knew the 1962 article I had written with Simonyan—perhaps the first publication ever dedicated to ($g-2$) rings.

SOVIET PHYSICS JETP VOLUME 18, NUMBER 1 JANUARY, 1964

ANOMALOUS RESONANCE SPIN FLIP OF A PARTICLE IN A MAGNETIC FIELD

Kh. A. SIMONYAN and Yu. F. ORLOV

Physics Institute, State Atomic Energy Commission, Erevan

Submitted to JETP editor December 13, 1962

J. Exptl. Theoret. Phys. (U.S.S.R.) 45, 173-176 (August, 1963)

It is shown that if a particle rotates in a constant magnetic field, a weak high frequency radial or azimuthal magnetic field with a frequency equal to the anomalous part of the spin precession frequency should induce resonance spin flip.

- He had in his files a copy of a 1968 letter I wrote to the CERN muon ($g-2$) team, "The stabilization of the mean muon orbits in the ($g-2$) experiments." (Alikhanyan, director of the Yerevan Physics Institute, hand-carried it to the team on one of his visits.)
- And he had a copy of an article by Bailey & Picasso of CERN that refers to the 1968 letter. Indeed, Vernon gave me his copy at our first meeting.

THE BAILEY-PICASSO ARTICLE

J. Bailey and E. Picasso, The Anomalous Magnetic Moment of the Muon and Related Topics. Progr. Nucl. Phys. 12, 43 (1970)

Bailey and Picasso describe two methods for (g-2) measurements.

The first is the one I proposed in my 1968 letter:

"The first method, due to ORLOV, is effectively to force all muons to have the same $\bar{\gamma}$, by feeding radio-frequency power into a cavity in the ring. Under the influence of the RF field, each muon will execute synchrotron oscillations in energy about a common mean energy $\bar{\gamma}$. With a crystal-controlled RF oscillator r is extremely well determined, since the muons remain bunched (so one could use a high field-index n_B), and with field-mapping one gets a precise value of (g-2). . . This method has been studied in some detail at CERN." (p. 69)

"Finally, we ask: Should one use the Orlov method or the electric-field method?

The answer is: Both are good experiments giving the product $a_\mu (e/m_0)$ to great accuracy. There are many small corrections which must be considered for both methods, but we shall not discuss them." (p. 72).

INVITATION ACCEPTED

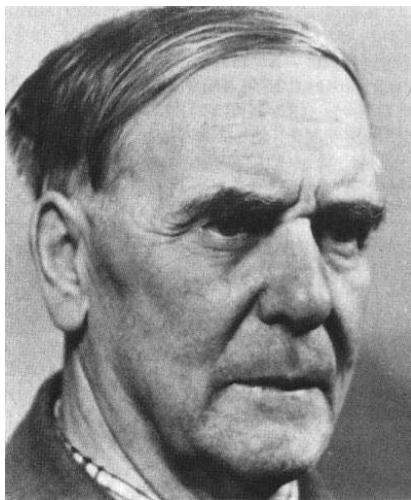
Vernon's invitation plunged me back into science.

In 1987, twenty-five years after writing the 1962 article with Simonyan, I wrote the first of many (g-2) notes: Tolerances on turns of (g-2) magnets around azimuthal and radial axes.

Thus began the American portion of the scientific journey described in my title—a journey that began in 1947 in Moscow.

EDUCATION AT PHYS TECH, 1947-52

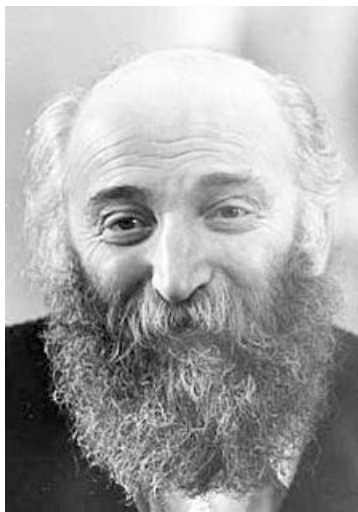
My physics education began at the Physics and Technology Institute ("Phys Tech"). Kapitsa, Landau, Budker and other great physicists were my first teachers.



P.L. KAPITSA (1894-1984)



L.D. LANDAU (1908-1968)



G.I. BUDKER (1918-1977)

SOME INTERESTING DETAILS

- **KAPITSA** worked at Cambridge with Rutherford in the '20s and '30s. He was an artist at the blackboard, erasing with his left hand what he had just inscribed with his right. He wrote fearless letters to Soviet leaders in defense of arrested scientists V.A. Fock, L.D. Landau and many others. Toward the end of the Stalin era, he was dismissed from his own Institute for Physical Problems and put under virtual house arrest in his dacha.
- **LANDAU** worked at Kapitsa's Institute for Physical Problems. He spent 1938-1939 in prison and was saved by Kapitsa.
- **BUDKER** worked at the Kurchatov Institute, called Lab #2 at the time. Kurchatov protected him.
- Most of our Phys Tech professors had their primary appointment at different research institutions and to some degree were involved in defense research.

- We were assigned work at these institutions up to several days a week, helping experimentalists in actual laboratories. I was in the group working at ITEP, called Lab #3 at the time.
- We were free to attend or not to attend lectures, even ones on Marxism-Leninism.
- We were often given a list of math and physics problems, each with points assigned to it, reflecting its difficulty. You could choose which problem(s) you wanted to be graded on. So, for example, you could choose a single, very difficult problem worth 100 points, or 10 less difficult ones worth 10 points each.

ITEP

1948-1952 (as student), 1953-56 (as researcher)

In my second semester at Phys Tech, I began working a couple days a week at ITEP and ended as a researcher with Berestetsky in Pomeranchuk's theoretical section. The atmosphere at ITEP was intense with round-the-clock thinking, calculating, experimenting—all to a very high standard.

A.I. ALIKHANOV (director of ITEP), when only one student in our group had done well on a theoretical exam: "You are shit! Either you intend to be researchers or not. If you do, then please be so good as to study theoretical physics thoroughly—whether you intend to become theoreticians or experimentalists. If you do not, then we'll bid you good-bye."



(Right) A.I. ALIKHANOV, director of ITEP; (left) his brother, A.I. ALIKHANYAN, director of Yerevan Physics Institute.



I.Ya. POMERANCHUK (1913-1966)
Chief of Theoretical Section, ITEP
"Science is the most jealous of mistresses"

In 1954, when I was already on the ITEP staff, ITEP began to design the 7 GeV strong-focusing proton-synchrotron. During a visit to ITEP, Budker warned that non-linear effects might not permit us to get the needed beam intensity.

Pomeranchuk assigned me to investigate whether Budker was right: "I am throwing you on these nonlinear nails!" he declared.

So on those nails I became an expert in accelerator physics, especially in the area of resonances and nonlinearity. (Contrary to Budker's dark prediction, I showed that there exist stability regions between and below the nonlinear resonance borders.)

1956

- That was the year of Khrushchev's famous anti-Stalinist speech at the 20th Party Congress.
- That was the year of political speeches given by myself and three others at an ITEP Communist Party meeting, directly inspired by Khrushchev's speech.

—My basic demand was democratization on the basis of socialism, meaning political democracy and socialist economy.

—Robert Avalov, a former classmate at Phys Tech, advised arming the people in order to prevent a repetition of Stalinism.

WHAT HAPPENED?

It wasn't good, but could have been a lot worse. We were fired and expelled from the CP and became notorious throughout the Soviet Union, through being vilified in Pravda and elsewhere. (Now all our speeches have been published together with other Central Committee archival documents.)

SOME REACTIONS. . .

KHRUSHCHEV (when Alikhanov phoned to ask him to reverse the decision): "I could not avoid this. Other Politburo members demanded their arrest."

ALIKHANOV (to the four of us): "If you knew what you were doing, you are heroes. If you didn't, you are fools!"

BUDKER (to me): "You are a hero. Do not hang your head."

LANDAU (upset, to me): "It's difficult to help you, if you don't know yourself what you want."

PONTECORVO (when I accidentally met him on Gorky Street): "What did you demand?"

"Democratization on the basis of socialism." "But bourgeois freedoms are incompatible with socialism!"

Much later, in 1976, Pontecorvo told me that he was not a member of the Soviet Communist Party, but a member of the Euro-communist Italian Party.

Nevertheless, he added, "If you organize some party independent of the Communist Party, you and I will be enemies."

* * *

Probably for the first time in all of Soviet history, many Soviet scientists were supporting scientists suffering political persecution. Support for us came from my immediate boss at ITEP, V.B. Berestetsky; B.V. Chirikov, then at Budker's Institute; Lev Okun at ITEP; and scientists in Kharkov, Leningrad and elsewhere. Their financial help, delivered in person or discreetly through third-parties, enabled us and our families to survive during that difficult period. (And I got a lot of opportunity to think about whether I was a fool and whether bourgeois freedoms are indeed bourgeois.)

ARMENIA, 1956-1972

Alikhanov asked his brother Alikhanyan to give me a job in the Yerevan Physics Institute, which he generously did. The locals in Yerevan were pretty friendly to me. It was from Yerevan in 1968 that I sent the private letter that finally landed in Vernon's files.

Restrictions on my traveling and communications even inside the country were finally softened at the end of the 60s. However, attempts to give me a job either in Moscow (ITEP, Moscow University) or in Leningrad (the Ioffe Institute), made by Alikhanov, Pomeranchuk, Leontovich and others, failed. But I could work far from these centers, which I did in 1963-64 with Budker in Novosibirsk.

Overall, this was a fruitful period:

- I (and later my lab) was responsible for the design of the 6 GeV strong-focusing electron synchrotron which began operation in 1967, so I became an expert in synchrotron radiation.
- Among other things, ITEP's E.K. Tarasov and I found (independently of Robinson) the damping decrements sum rule for arbitrary rings, as well as the decrements themselves. Later I was nominated for a state prize for this cycle of work.
- Later, V.N. Beier from Budker's Institute and I discovered the effect of spin diffusion induced by synchrotron radiation.

- In 1968-72, my Yerevan lab was busy designing a 500-100GeV e+e- collider, some 15 years ahead of the CERN e+e- collider design. But 1972, it became clear that the design would not get any state support. The only useful result of several years work was the proof (on paper, at least) that such a collider was possible.

Still, my years in Yerevan were very fruitful. However, the KGB was not sleeping!

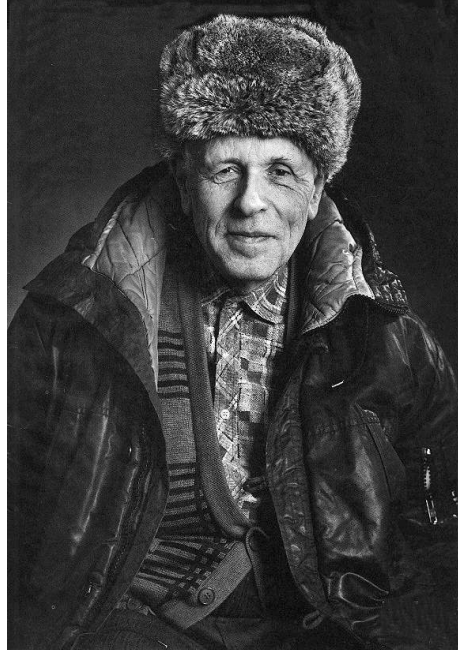
1968

This was the year of a peaceful uprising of Czechoslovakia against Stalinist Soviet rule, followed by huge Soviet military intervention.

And it was the year of a small but famous demonstration against that demonstration. On August 25 at noon in Red Square, Moscow, Larissa Bogoraz and a handful of other dissidents stepped into history during their only 15 minutes of free expression, before being immediately arrested and then, with one exception, sent to prison, exile or psychiatric hospital.



L. BOGORAZ (1929-2004)



A. SAKHAROV (1921-1989)

1968 was also the year of Sakharov's famous "Reflections on Progress, Peaceful Coexistence and Intellectual Freedom," arguing the necessary convergence of the opposite sides of the Cold War.

The Red Square demonstration and Sakharov's pamphlet had a tremendous impact on me. I, too, wanted to make a stand in opposition to the regime. But I wanted it to be an essential one, since it would inevitably lead to arrest. The relative backwater of Armenia was not exactly the place for a significant gesture, and I was forbidden to work at ITEP and Moscow University. So I continued work as before in Armenia, and waited.

1972-73

In 1972, after a conflict with the Atomic Energy Commission, and with the help of Lev Artsimovich (well-known for his construction of the first tokamaks) I finally left Yerevan for work at an institute near Moscow whose full name I can never remember: the Institute of Terrestrial Magnetism and Dissemination of Radio Waves.

1973 was a notable year:

- I was nominated for the 1973 State Prize (with Sokolov, Ternov, Kolomensky and Lebedev) for my work on synchrotron radiation.
- I met Solzhenitsyn in his hiding place.
- I became a founding member of the first Soviet Section of Amnesty International.
- I wrote and distributed a letter to Brezhnev in defense of Sakharov (whom by now I knew well).
- And for these last two, I was fired from the institute whose name I can never remember. I remained jobless to the end of my Soviet life.

THE MOSCOW HELSINKI GROUP

- In May, 1976 I organized the Moscow Helsinki Group to monitor human rights violations of the Helsinki Accords in the USSR.

- In February 1977 I was arrested.

- In Nov. 1980 my old teacher, Kapitsa, send a long letter to the KGB Chief, Y.V. Andropov:

"Much esteemed Yuri Vladimirovich,

Along with many other scientists, I am deeply disturbed by the situation and fate of our outstanding scientists, physicists A.D. Sakharov and Y.F. Orlov. The current situation can be described rather simply: Sakharov and Orlov produce big benefits by their scientific activity, while their activity as dissidents is considered harmful. . . .If we ask scientists, they will definitely say that when such outstanding scientists as Sakharov and Orlov are deprived of the chance to engage in normal scientific activity, then this deprivation brings a loss to mankind."

- Another old teacher of mine, Budker, tried to help from his hospital death bed. He sent a telegram to my wife, Irina, asking her to come talk with him. But she never received it. I read it later in my indictment file, after his death.

● My old friend Pierre Lefevre, together with Dieter Moehl, Georges Charpak and others at CERN, organized the "Orlov Committee" there.

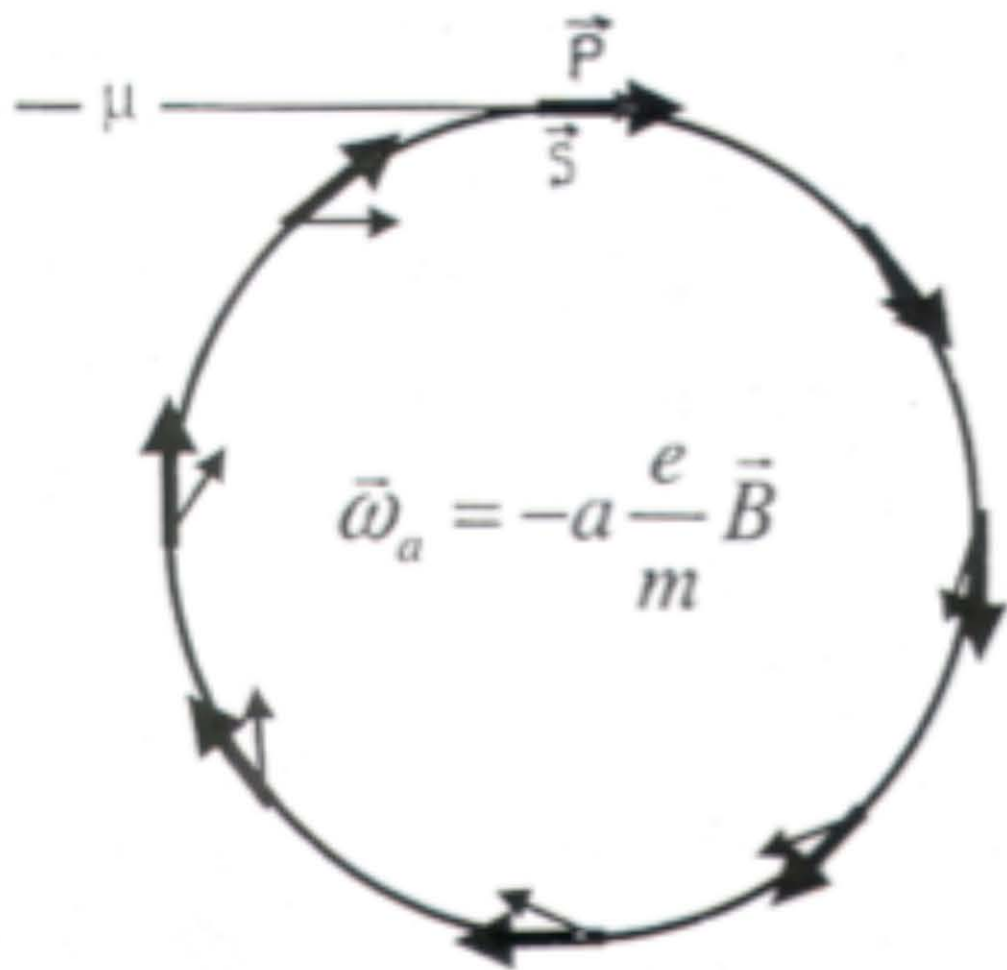
● In the US, the "SOS (Sakharov, Orlov, Shcharansky) Committee" was organized by M. Pripstein, A. Sessler, J.D. Jackson, K. Gottfried and others. The SOS Committee bothered the Soviet authorities considerably. In 1979 under the heading "top secret" the following was recorded:

RESOLUTION

Of the secretariat of the Central Committee of the Communist Party
of the Soviet Union

On the question of the appearance of the so-called group "SOS" (USA)

1. To agree with the proposal of the USSR Academy of Sciences concerning publication in Pravda of an article by Soviet scientists, dedicated to the current state and future development of Soviet-American scientific cooperation.
2. To order the Academy of Sciences and editors of Pravda to finalize the text of that article.



BACK TO (G-2)

Let me return to 1987, when I began work with Vernon Hughes, Bill Morse, Yannis Semertzidis and others, on the muon ($g-2$) as a theoretical expert on both spin and beam dynamics.

I think that my main contribution to that experiment was to discover and explain coherent betatron oscillations (CBO) and a stroboscopic method of dealing with them.

This “jumping windows” method was developed and implemented with Yannis Semertzidis. It was completely different from other approaches and confirmed the ($g-2$) value obtained by them.

This fact convinced Vernon Hughes that the final result of the whole ($g-2$) experiment—the new ($g-2$) value—was correct.

(G-2): TWO NEW IDEAS

There are several new ideas on how to perform the next--more precise-- (g-2) measurement. Here are two:

- The first idea, developed with Yannis Semertzidis, departs from the so-called “scraping” technique developed at CERN and the BNL (g-2) experiment. It uses information about coherent betatron oscillations (CBOs) received from fiber beam monitors. If the radial and vertical CBOs are known, then after every injection they can be quickly damped by a specially designed oscillating electric field. After this damping, the beam can be placed in the center of a vacuum chamber with a very clear, good gap between the beam and any physical borders.

- The second idea is not to use the CERN method of $(g-2)$ measurement at all, but instead to use the resonance method first proposed in the article that I wrote with Simonyan in 1962. This method is conceptually different from the CERN and previous BNL methods.

Vernon referred to it and our article in one of his 1987 reports. But it was too late to change the concept of the already developing $(g-2)$ work at that time.

Now, both Yannis and Bill Morse agree, we need to investigate the method anew: it is extremely important from a physical point of view to confirm the existing $(g-2)$ value by using a technique completely different from the one already used. Fortunately, the resonance method can be implemented in the existing $(g-2)$ ring with only small additional installations.

CURRENT EDM INVESTIGATIONS

The collaboration led by Yannis Semertzidis extends Vernon's search for the muon magnetic dipole moment by using storage rings, into a search for the electric and magnetic dipole moments of different particles. At the moment, we plan to measure the electric dipole moment of the proton, by using a combination of Yannis' and Bill Morse's ideas: so-called "frozen spin"; magic energy ($p=0.7\text{GeV}/c$); and clockwise and counter-clockwise injections imitating the time-reversal effect.

As for the EDM of the deuteron, I personally believe that my resonance method, if properly developed, will work. The investigation continues.

In all these projects, we are inspired by our many years of work with Vernon—by his energy, his passion for science, and perhaps especially by his tenacity.