INTRODUCTION
to the Yuri Golfand Memorial Volume
MANY FACES OF SUPERWORLD

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Supersymmetry is almost thirty years old. The first supersymmetric model in four dimensions was found by Golfand and Likhtman in 1970. Yuri Abramovich Golfand was a very modest man who, unfortunately, did not gain much recognition when he was still alive. When I saw him for the last time in Haifa in 1992, he told me that he had a dream – to travel to the West, to the United States or France, to attend a physics conference or just “to see the world.” This dream never came true. He died in 1994, but you will look in vain for the obituary in Physics Today, CERN Courier, or any Russian physics journals. This Memorial Volume is a belated tribute to the man who was one of the discoverers of supersymmetry, which today, 30 years later, dominates theoretical high energy physics.

Basic Biographic Data

Yuri Abramovich Golfand was born in Kharkov, Ukraine, on January 10, 1922. Like many Soviet scholars of his generation, he started his education at the Kharkov University, Department of Physics and Mathematics. This was in 1938. The Second World War (WW II) interrupted his academic career; in 1941 Golfand becomes a cadet of the Military Airforce Academy. The end of 1944 found him at a front-line airdrome where he worked as a technician. After the end of the war, in 1945, Golfand resumed his studies, this time at the Department of Mathematics of the Leningrad University. He graduated in 1946 and got his PhD in mathematics within a year and a half. At the end of the 1940’s, Golfand worked for an electrical engineering research institute. In 1951, he joined the group of I.E. Tamm at the Theory Department of the Lebedev Physical Institute (FIAN) in Moscow. He stayed there for 40 years, with a long break, of which more will be said later. For a year or two, Golfand was marginally involved in the nuclear bomb project, like many of his colleagues at that time. Approximately at the same time he got interested

\[\text{[Some additional data were kindly communicated to me by Mrs. N. Koretz-Golfand.]}\]
in fundamental physics. In the 1950’s and 60’s, Golfand carried out several projects in quantum field theory, in particular, on applications of the functional methods. In 1959, he published a famous work on the method of renormalization, based on the assumption that the four-dimensional momentum space has a constant nonzero curvature. That was one of fascinating attempts to introduce elementary length to relativistic field theory.

In 1972, the Academy of Sciences conducted a routine campaign of personnel cuts. At the FIAN Theory Department it was decided that Golfand was the least worthy member of the group, whose work was unimpactful. As a result, he was fired from FIAN in 1973. This unfortunate turn of events left very little choice to Golfand – he decides to apply for the exit visa to Israel, which only aggravates his situation. In due time there comes a refusal. In those days such an application was considered to be high treason. Thus, Golfand becomes a refusenik – a nonperson, according to the Orwellian nomenclature – with all ensuing political consequences. His struggle lasted for many years. This chapter belongs to a different book, however, which has yet to be written. We will not touch it.

Golfand was unemployed for 7 years, until 1980, when he was accepted back to FIAN (but not to the Theoretical group), under strong pressure from the world physics community, and, in particular, the American Physical Society. It was only in June of 1990 – seventeen years after the original application – that the permission was granted to Golfand’s family to leave the Soviet Union, which at this time was rapidly approaching its demise. Within a few months his family moved to Israel. An official farewell letter from FIAN, signed by Academician L. Keldysh, the Director General, arrived a few days before the departure. The concluding paragraph of the letter reads: “I would like to express my deep and sincere regret for the damage which has been inflicted on you, and henceforth on the Institute, by your dismissal from FIAN.”

It will be fair to add that shortly before this, the Soviet Academy of Sciences awarded Yuri Golfand with the Tamm Prize for Theoretical Physics. This was the only award Yuri Golfand ever received.

Golfand spent the last years of his life in Haifa. Because of his age, he could not get a regular professorship, so he settled for a research fellowship at Technion, under a special program of the Israeli Government. Yuri Abramovich Golfand died on February 17, 1994, in Jerusalem, from complications of a brain stroke.
Work on Supersymmetry; Chronology

It is known that Golfand discussed the Bose–Fermi symmetry with his colleagues in the late 1960’s, trying to solve the puzzle of weak interactions, before the advent of the Glashow-Weinberg-Salam theory. That is why he was so much preoccupied with the problem of parity violation which is clearly visible in the first published work on four-dimensional supersymmetry. Evgeny Likhtman recollects that when he appeared in FIAN as Golfand’s PhD student, in the spring of 1968, Golfand had already found an extension of the Poincaré algebra by bispinor generators. (Today the extension found by Golfand is referred to as the super-Poincaré algebra, while the bispinor generators are called the supercharges.) In the review article, it is mentioned that the searches of the extensions of the Poincaré algebra conducted by Golfand in the late 1960’s were originally also motivated by the desire to bypass the well-known no-go theorems due to Coleman and Mandula, and Weinberg (or to establish new no-go theorems).

Golfand and Likhtman worked on various aspects of supersymmetry for several years. Their first published paper entitled “Extension of the Algebra of Poincaré Group Generators and Violation of $P$ Invariance” contained a field-theoretic model, which in modern terms can be described as supersymmetric quantum electrodynamics (QED) with the mass term of the photon/photino fields, plus two chiral matter superfields. (I suggest we call it the Golfand–Likhtman model.) Adding the photon mass term in the Abelian gauge theory does not spoil renormalizability. Alternatively, one can get this model from massless super-QED by adding a Higgs sector, and breaking U(1) spontaneously. The masses of the physical Higgs fields are then sent to infinity while the photon/photino mass is kept finite. The requirement of renormalizability was very important to Golfand who tried to follow as close as possible the pattern of the only respectable field theory of the time, quantum electrodynamics. On the other hand, the absence of massless particles was also a precondition – otherwise Golfand and Likhtman would have had settled for massless super-QED, which is significantly simpler than the model they found. This shows that Golfand kept in mind phenomenological applications in weak interactions.

The paper was received by the Editorial office of JETP Letters on March 10, 1971. To set the time scale, I should mention that the famous paper of Gervais and Sakita known to everybody, was received by the Editorial Office of Nuclear Physics on August 13, 1971. Golfand and Likhtman also prepared a detailed publication, which appeared in the I.E. Tamm Memorial Volume. The only date one can infer now with certainty in connection with this publication is that this Volume was sent to print on March 20, 1972. In fact, according to
Likhtman’s memoirs, both papers were prepared practically simultaneously in the end of 1970. For Western readers I should explain some essential details regarding the publication process in the Soviet Union. To publish a scientific paper was much more than just typing the manuscript and mailing it to the publisher. There was a long latent period, associated with getting all sorts of clearances. First, the so-called Expert Commission (a group of authorized fellow physicists in the given institution) was supposed to study the paper and recommend its publication. According to the official rules they had to certify that no new discoveries were reported, because if they were, the Expert Commission had to recommend to classify the paper right away. Of course, people tended to stretch the official rules, otherwise not a single breakthrough paper would have ever appeared in the Soviet Union.

At the next stage the paper would go to the so called Regime Department whose task was to check that no references to classified work or undesirable persons were made, no subversive ideas put forward, and so on. With all this paperwork done, the decision to allow (disallow) publication was to be made by the Director of the Institute. This is not the end of the story, however. All materials intended for publication had to be cleared through the so-called GLAVLIT, the almighty agency whose sole obligation was to ensure total Censorship in the country. If, at the previous stages the author would have at least some minimal control over what was going on with his (her) paper, GLAVLIT was a total black box.

The process of getting all clearances could extend anywhere from weeks to many months, and the paper was officially nonexistent until the very end. The author could not even refer to it in his/her further work. Thus, the Likhtman’s recollections that the paper was completed in 1970, and the official submission date of March 10, 1971, are not inconsistent.

In their second paper on supersymmetry, Golfand and Likhtman described in detail a recursive procedure of building supersymmetric models. By this time Likhtman, following Golfand’s instructions, worked out the free field representations of the super-Poicaré algebra in several practically important cases (today we would say, the chiral and vector supermultiplets were constructed). So, they knew how the numbers of the boson and fermion degrees of freedom in the supersymmetric Lagrangians should be balanced. This determined a starting point – the particle content of the models to be built. Then they suggested cataloguing all possible interaction terms in the Lagrangian, compatible with renormalizability, order by order in the coupling constant, and the corresponding terms in the supercharges, with unknown coefficients. The coefficients were to be fixed by imposing the anti-commutation relation \( \{ Q_\alpha \bar{Q}_\dot{\alpha} \} = 2P_{\alpha\dot{\alpha}} \), order by order in the coupling constant. (I use here the modern notation, \( Q_\alpha \).)
denotes the supercharge and $P_{\dot{\alpha}}$ the energy-momentum operator.) Needless to say that this was much more time- and labor consuming procedure than the superfield formalism of the present day. Note, however, that the work I describe now took place eight years before the invention of this formalism.

In the very same paper, in addition to the already established super-Poincaré algebra, Gol'fand and Likhtman presented (a limiting case of) the super-deSitter algebra.

Gol'fand continued to work on this range of ideas even after his forced retirement, through the years of unemployment. Misha Marinov recollects: “Soon after the Wess–Zumino preprint appeared, in January or February of 1974, I was invited to give a talk on supersymmetry at the Institute of Physical Problems. Gol'fand was in the first row and listened to my explanations very attentively. Then we talked about all details. Yuri Abramovich was greatly impressed by the Wess–Zumino work, though he said it was too technically complicated and that his approach was more elegant. It is curious to note that Abrikosov who attended this seminar too, strongly objected against the exploitation of the prefix “super” since it was already in use in another context in superconductors.”

Later Gol'fand, together with Likhtman, wrote an extended review for the collection Supersymmetry: A Decade of Development, edited by Peter West, where they summarized their own results and tried to indicate where they stood in relation to other numerous results on supersymmetry which were obtained by that time. This was the last paper written as a team by Gol'fand and Likhtman.

Likhtman

Under the spell of Gol'fand’s ill fate, the academic path of Evgeny Pinkhasovich Likhtman went astray. You will read this story in his memoirs published in this Volume. It should only be added that in the beginning of the 1970’s, before Wess and Zumino, Likhtman published several papers of his own devoted to various aspects of supersymmetry. In particular, on page 8 of Ref. 5 one reads: “As is known, in relativistic quantum field theory, in transforming the free energy operator to the normal-ordered form there emerges an infinite term which is interpreted as the vacuum energy. It is also known that the sign of this term is different for particles subject to the Bose and Fermi statistics. The number of boson states is always equal to the number of fermion states. From this it follows that the infinite positive energy of the boson states in any of the representations of the [super-Poincaré] algebra is annihilated by the infinite negative energy of the fermion states.” In one of his JETP Letters papers.
Likhtman mentions in passing that in supersymmetric theories the one-loop boson mass diverges not quadratically, but, like the fermion mass, only logarithmically. Thus, he apparently was the first to establish two fundamental properties of the supersymmetric theories, distinguishing them from all others – the vanishing of the vacuum energy and the absence of the quadratic divergences. It was Likhtman who gave a talk on supersymmetry at ITEP in the 1970’s. This was my only personal encounter with him.

E. Likhtman remains the employee of the Institute of Scientific and Technical Information in Moscow till the present. The only change is that back in the 1970’s the institute was referred to as “All-Union”, while now, with the fall of the Soviet Union, this part of the title is gone.

Missed Crossroads

It is natural to ask why the ideas of supersymmetry did not take root in the Moscow particle physics community right away, immediately after the discovery of Golfand and Likhtman. The community was strong, vibrant and versatile, and yet it missed a key turn on the pathway of theoretical physics.

Of course, it is hardly possible now to give a certain answer. I will still suggest a few conjectures.

One of the reasons might be a negative attitude to field theory in general which was prevalent in the community after Landau’s discovery of the “zero charge.” Even after the renormalizability of the Glashow-Weinberg-Salam model was proved by ’t Hooft in 1972, some of the elders of the community, whose opinions were highly respected, continued to openly express their animosity towards field theory. A radical turn occurred only in November of 1974, after the discovery of $J/\psi$, with the advent of quantum chromodynamics (QCD).

Perhaps, more importantly, Golfand was not taken seriously by many of his former colleagues. To this day some of them insist that “he himself did not understand what he did because he was not really a good physicist.” This is a quotation from a letter which I got about half a year ago, when the work on this Volume began. The author of the letter then continued: “I cannot remember a single interesting statement on physics which he ever made. Usually he was quite ironic about doing physics. He would occasionally come to a seminar, sit there and then disappear, without saying much or producing anything.” I hasten to add that this opinion is by no means shared by all of Golfand’s colleagues. Human memory is rather selective, and in many cases we see what we want to see... Still, it gives an idea of the general attitude.

The point of view that Golfand did not understand what he was doing is absolutely unsubstantiated either by the analysis of three papers on super-
symmetry produced by Golfand and Likhtman or by recollections of Likhtman and others. From these papers, and from the problems Golfand formulated for Likhtman’s PhD work, it is evident that Golfand clearly saw the contours of the theoretical construction they were building together with Likhtman, posed the right questions, and found adequate theoretical tools for their solution. Perhaps, some weakness was on the side of phenomenology. For instance, the issue of the parity nonconservation in the Golfand-Likhtman model, a persistent theme in Refs. 1, 4, was never elaborated in full. This is easily explained by the isolation in which they were working, and the lack of enthusiasm on the side of their colleagues. The soil fertile to the ideas of Wess and Zumino, provided by CERN in 1974, was totally absent in the case of Golfand and Likhtman.

**Glimpses**

Golfand was a frequent participant of the ITEP theory seminars. I used to bump into him in the corridors of ITEP regularly. At first I did not know who this small man, with warm eyes and a kind smile, was. So, I asked my thesis advisor, Prof. B.L. Ioffe. Ioffe lowered his voice to the level of whisper and replied that this was Golfand, the discoverer of supersymmetry. Later, whenever he spoke of him, Ioffe would automatically lower his voice even if we were alone in Ioffe’s office. This would emphasize, without any words, that Golfand was a *nonperson*.

Everybody who knew Golfand remembers his smile and his eyes. Usually he looked a little bit out of touch with reality, decoupled from the surrounding world, with thoughts directed inside rather than outside. Marinov wrote in 1994 in *Proceedings of the Israeli Physical Society* that the “Technion colleagues will remember forever Golfand’s smile and his quiet and sympathizing eyes.” Elsewhere he elaborates: “It was extremely interesting to socialize with Golfand. He was sparing with words, he listened more than he talked; his eyes, that were always alive and radiated warm energy, participated in the conversation.”

Here is how Lars Brink describes Golfand: “When Mike [Green] and I gave talks at Lebedev in 1984 he had managed to sneak in, and I met him in the shadow there. The last time I saw him was at the Sakharov meeting in 1991. He had emigrated then and was back, so I talked to him several times. He represented to me a character that I have only seen in Russia, the enormous warm heart, the sadness around the eyes, somewhat subdued, a person whom

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*An excerpt from the interview with N. Portnova, 1995, unpublished.*
you instantly like and feel complete confidence in...” Stanley Deser, who also knew Golfand personally, called him a man who came premature.

After I drafted this introduction, I sent it to a few colleagues whose opinion I value, soliciting comments. As a result, I got a letter from Prof. B. Ioffe which, to my mind, adds important touches to Golfand’s human and scientific portrait. I reproduce it below, with insignificant abbreviations.

Ioffe writes: “I knew Golfand from 1951. Very close friendly relations developed beginning in around 1957: we visited each other at home, which is very unusual for me. Once we celebrated together the New Year (1958 or 1959, I do not remember exactly), meeting the New Year midnight in a frosty and snowy forest (this was near Povarovo, 50 km from Moscow) by the fire we made. We were on skis, Golfand liked skiing as much as I did.

In the 1950’s and 60’s I often discussed physics with him; such discussions were very fruitful for me. Almost nobody knows now that Golfand invented the path integral formulation in field theory independently from Feynman. It was in the early 1950’s (probably, in 1952). He represented a field theory (he considered the scalar field theory) as an integral over the mesonic fields. Golfand did not follow Feynman’s route who started from path integrals in quantum mechanics. In fact, he did not know of Feynman’s work at that time. When later he gave a talk at the Tamm seminar, people were very skeptical, because nobody understood the subject – the presentation was different from Feynman’s, and only very few in the audience knew about Feynman’s paper, and nobody understood it either. After some time it became clear, that it was just the functional integral.

When Golfand was unemployed, there was a serious problem with getting permission for Golfand’s participation in ITEP seminars. As you remember, all “outside” participants were to be included in a “list”, which had to be cleared through the ITEP Regime Department. The permission would be granted only to people who had positions in physics institutes; the name of the institute had to be explicitly indicated in the list we submitted to the Regime Department before each seminar. Since I was responsible for the list and was signing it each time (formally till 1977 it was Berestetsky’s signature, but in fact I did it), I

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c I quote here from Lars Brink and Misha Marinov not accidentally. Lars (together with S. Deser, D. Gross, Y. Ne’eman, B. Zumino and some other Western physicists) was absolutely instrumental in Golfand’s survival through the years of unemployment. It was their victory when Golfand was reinstated in FIAN in 1980. Misha, a recent immigrant in Israel himself, did whatever he could to help Golfand to “blend in” into a complicated Israeli life during the most painful transition period. Golfand’s knowledge of Hebrew was rudimentary, and he would be essentially helpless without Marinov’s constant assistance. Later Marinov took the idea of this Memorial Volume close to heart; he helped to locate Golfand’s widow, Mrs. Koretz-Golfand, in Israel.
was committing fraud on a regular basis “putting” Golfand to some *ad hoc* institute, just to let him in. My whisper referred not to Golfand’s name *per se*, but to the fact, that he was unemployed. At this difficult time, we continued seeing each other, often exchanged phone calls, etc. After his divorce, our relations cooled off a little, though.

When I heard that Golfand was fired from FIAN, I expressed my dissatisfaction to a few FIAN people. Each time the response was: it was not me who did it. As I remember, Golfand was unemployed not all the time from 1973 to ’80; sometimes he had a part-time teaching job at a technical college. After he was taken back to FIAN, Golfand continued to attend the ITEP seminars on a regular basis, but he refused to participate in the FIAN seminars. That was the demonstration of how strongly he was offended.”

**About This Volume**

When the idea of this Memorial Volume came to my mind, I wrote a letter to my colleagues, fellow theoretical physicists. I sent it to about two dozen active members of the HEP community, those who determine the trends of the modern high-energy physics, and to several physicists from the younger generation – some of them I considered to be rising stars. The response was overwhelmingly positive. With one or two exceptions, all agreed to participate enthusiastically, and I got many very valuable suggestions as to the structure of the Volume. Its scientific part will, hopefully, represent a full picture of the huge tree into which supersymmetry grew today. The book will be used in the community in the years to come, and this is the best tribute to Golfand one can think of. I am sincerely grateful to all participants of the project, to whom I would like to say thank you. Together, we did a good job.

Also of importance is the first part of the book, which consists of the memoirs of Golfand’s widow, Mrs. Natasha Koretz-Golfand, and his former student, Dr. Evgeny Likhtman. They are both emotional and moving, precious evidence of the past, gone forever… I am very grateful to Mrs. Koretz-Golfand and to Dr. Likhtman for their willingness to share with us, and with those who will come after us, their personal recollections. Also included in the first part is a historical survey of the scientific ideas that paved the way to supersymmetry, written by Prof. M. Marinov, and the English translation of the Golfand-Likhtman paper from the Tamm memorial Volume.
Conclusions

Golfand’s career in theoretical physics spanned over 40 years. He was the author of several dozen papers devoted to aspects of field theory, out of which two opened to us the doors to the superworld, that will stay with us forever. I will risk to say that the discovery of supersymmetry was the single most important contribution of Soviet fundamental physics after WW II. I will go so far as conjecture that supersymmetry will play the same revolutionary role in physics of the 21-st century as special and general relativity in physics of the 20-th century. Treatises on the pioneers of supersymmetry – and Yuri Golfand definitely belongs to them – will be written by professional historians of science.

Minneapolis, August 24, 1999

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2. Yu.A. Golfand and E.P. Likhtman, “On \( N = 1 \) Symmetry Algebra and Simple Models”, in *Supersymmetry: A Decade of Development*, Ed. P. West (Adam Hilger, Bristol, 1986), p. 1.
3. J.-L. Gervais and B. Sakita, *Nucl. Phys.* B34, 632 (1971).
4. Yu.A. Golfand and E.P. Likhtman, “On the Extensions of the Algebra of the Generators of the Poincaré Group by the Bispinor Generators”, in I. E. Tamm Memorial Volume *Problems of Theoretical Physics*, Eds. V.L. Ginzburg et al., (Nauka, Moscow 1972), page 37.
5. E.P. Likhtman, “Irreducible Representations of the Extension of the Algebra of the Poincaré Group Generators by the Bispinor Generators”, Report of the Lebedev Physics Institute # 41, 1971, in Russian.
6. E.P. Likhtman, “Supergauge Renormalizable Theory of Massive Vector Field”, *JETP. Lett.*, 21, 109 (1975); “Supersymmetric Renormalizable Theory of Massive Vector Non-Abelian Field”, *JETP. Lett.*, 22, 57 (1975).

\[^d\text{See the list of Golfand’s publications at the end of this Volume.}\]
\[^e\text{In fact, one can view these two papers as a short and long version of one and the same paper. In essence, Golfand will be remembered as a one-work man. How many 200-paper collections would be gladly traded for a single paper, like that?}\]