fermionic relations, in particular the analogue of integration over fermionic variables. Unfortunately I was unable to grasp the scope of this and I have forgotten all details. What I do remember is that Berezin took me on an unusual tour through Moscow. We met at some bus station and then walked for hours through areas of Moscow which one usually does not show to tourists. The slums we passed illustrated Berezin's bitterness against the regime under which he had been forced to spend his life. The terror had ended with Stalin's death but the feeling of imprisonment remained. In 1975, we succeeded in obtaining an invitation for Berezin to spend a year at CERN. But it was too late. We learned that Berezin had drowned in a river in Siberia.

The two persons whose work was most influential in propagating the idea of supersymmetry were Julius Wess and Bruno Zumino. They developed the formalism for a supersymmetric, Poincaré-covariant quantum field theory.

Supersymmetry had several good aspects to its credit. Some divergent graphs in perturbation theory cancelled. This was not very relevant since other bothersome divergent terms remained. More important was that the energy was represented by a positive operator. Thus no extra assumption was needed to ensure the positivity of energy. Furthermore it appeared that supersymmetry allowed a natural inclusion of exterior (non-geometric) symmetries. Though I was aware of these features and met Bruno Zumino at CERN almost daily I was first attracted to work on supersymmetry when my friend Jan Lopuszanski from Poland visited CERN and asked me why there couldn't be any fermionic charges. Indeed why not. He had just spent some time at the institute of Wess in Karlsruhe and together with Martin Sohnius, a graduate student of Wess, he had written a manuscript in which they introduced so-called "central charges". This gave some enlargement of the existing supersymmetric scheme. I got interested and proposed to Jan that we should find all supersymmetries which are compatible with a Poincaré-invariant S-matrix. It was the analogous question for supersymmetries which Coleman and Mandula had settled for ordinary symmetries [Coleman 1967]. This problem was not very hard and the result was mildly interesting because it gave a final answer. However the resulting scheme was not very beautiful. While the fermionic charges generated the space-time translations they did not generate the Lorentz transformations. These had to be added separately. A much more elegant scheme could be obtained when one allowed fermionic charges which did not commute with the linear momenta. Then the fermionic charges generated not only the full Poincaré group but also dilations and conformal transformations as well as a $U(n)$-group of external symmetries. This was somewhat contrary to our original aim since, unless the dilation symmetry is broken, it allowed only zero mass particles and no non trivial S-matrix. But as an algebraic scheme it looked very natural. During the time of computation, involving the checking of hundreds of generalized Jacobi identities, I had an extensive and very fruitful correspondence with Martin Sohnius. Then I went to see Jan Lopuszanski in Poland to talk it over with him. We had now enough material but I was somewhat tardy in writing it up. So ultimately Jan lost his patience and commanded: "Now you sit down at this desk and start writing." The ensuing paper [Haag 1975] attracted quite a lot of attention among the fans of supersymmetry.

Our friendship hat started 1968, when Jan Lopuszanski invited me to Wroclaw, where he was professor of physics.

My first visit to Poland in Winter 1968 had started in Wroclaw where Lopuszanski invited me to stay at his house. It proceeded to Karpacz, a mountain resort at the foot of Sniezka, the former German "Schneekoppe". There the University of Wroclaw owned a large house which could serve as a conference center. The physics department had organized a winter school there to which some colleagues from Western countries were invited as lecturers, among them David Ruelle, Nico Hugenholtz and myself.

There was a Russian delegation and a fair number of interesting colleagues from Poland.

On the way back after the workshop Ruelle and I had to go via Warsaw and spend a night there. The University of Wroclaw had booked a room for us at the hotel Bristol but when we got there the clerk at the reception desk told us that they could not accept the request and we would have to pay the rate for foreigners which was twice the rate of Polish citizens. It exceeded the amount of money we carried with us. David Ruelle blamed me for this because I had once said "Breslau" instead of "Wroclaw". Anyway we decided that we had to activate the international brotherhood of theoretical physicists and we called Bialynicki-Birula, a colleague in Warsaw. He came, settled the affair with the receptionist and joined us for dinner at the hotel where an orchestra played evergreens by Emmerich Kálmán and many modestly dressed couples enjoyed dancing. I asked Birula "Who are the people who can afford to come for dinner and entertainment to such an expensive hotel?" His answer "Oh, just ordinary people who save money for a couple of months to be able to go out once in a while in luxury".

After the year at CERN, I intended to continue working on supersymmetry and invited Martin Sohnius to come to Hamburg for some period. There was one obvious task: to arrive at a local version where the global fermionic charges were replaced by spinorial charge densities, possibly accompanied by some local fermionic gauge principle. We were not successful in this.

These years brought also two memorable extended stays abroad. Among them my first visit to Japan lasting six weeks. It was based on the Institute for Mathematical Sciences in Kyoto but the contract under which I was invited stipulated that I should visit a number of other institutions in the country, talk with the people there and give some lectures. Araki who was my host, took me to many places and was eager to introduce me to aspects of Japanese life such as the traditional Japanese Inn for staying overnight. He delegated the very competent and charming secretary Toshie, who had been to America and spoke fluently English, to see to it that I got something to eat in Japanese restaurants and to introduce me to the marvels of Japanese garden architecture around temples and imperial palaces. Araki also persuaded two young mathematicians to take me on a hiking tour in the Southern island Kyushu. So altogether I left Japan with vivid pictures and the wish to return soon. It took, however, more than 15 years till I could visit Japan again.

The other long visit was a three months stay at Berkeley, California following an invitation by I.M. Singer. He was a man of widely ranging interests who had contributed significantly to several branches of mathematics, co-author of the celebrated Atiyah-Singer index theorem. I had met Iz Singer on several occasions since he was a very old friend of Dick Kadison. We felt that it might be worth while to engage for a period in intensive discussions, hoping that once again the mutual inspiration between mathematics and physics might work and that our joint background could lead us to produce results in an area which could vaguely be called "quantum geometry". This hope did not materialize. We produced nothing tangible together but at least for me the discussions were very fruitful.

Statistics claim that after the age of 35 or at most 40 the productivity of theoretical physicists quickly drops to zero. I do not believe the relevance of such statistics and therefore was not concerned about this message. But it is quite amusing to recall how various colleagues reacted to it. Harry Lehmann took it from the positive side and declared: "Now let the young people do something". He saw his task now in keeping informed about new developments to be able to judge and give advice without the ambition to produce new ideas. Freeman Dyson also believed the verdict of the statistics and decided to change the field to astronomy or rather astrophysics where no age limit was proclaimed. Leon van Hove argued that it depended on the