ISSN 1330–0008
CODEN FIZAE4

REMINISCENCES OF MY WORK WITH PROFESSOR SUPER

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Talk at the Symposium

I am very glad that the celebration devoted to the birthday and the anniversary of the beginning of the scientific work of Professor Supek has been organized, and that I have a chance to take part in it. Professor Supek's contributions to the progress of science in our country are so enormous that it is at the same time a celebration of an epoch of physics in our country.

I began the study of mathematics at the Faculty of Natural Science and Mathematics in Zagreb in 1947. It was also my first contact with Zagreb, two years after the end of the Second World War. The situation was very difficult. Mathematics was a wonderful place for individual peace and happiness. In high school I had an excellent teacher in mathematics whose name was Stjepan Mintaković. So, my knowledge of mathematics was good and it enabled me to start my study successfully, which made my world bright in those days.

In 1947 a new system of education was introduced at our Faculty. To illustrate the change, let me mention that our generation had about fifty exams in comparison with only about half a dozen of the previous and earlier ones. Because of the war period, there were students four and more years older than me. They got together in classroom almost every day and had various discussions. Topics were mostly mathematics, physics, philosophy and similar fields. They served them for the preparation of their big exams, but not only for that. Behind these discussions there was, I would say, an inner desire for knowledge which brought them to study natural science and mathematics. These discussions were confrontations of different opinions, understandings, beliefs, exercises of rhetoric, youthful sophistries and revealed a deep tendency to get into the subject of the discussions. It was a place where one could hear news from science as well as from other fields of life. The discussions were sometimes so loud that they could be heard from distant corridors.

I went to this classroom very often. Naturally, I was invisible to those senior students, but they fascinated me very much. Even today I can't free myself from that influence. In this environment I heard the name of Professor Supek for the first time. It was related to new achievements in physics in this century, about which I knew little or nothing. Sometimes it was my impression senior students were not quite sure what it was all about. But everybody was sure that it was something quite different from the knowledge they had got in earlier education or ordinary life experience. This mystery attracted much of our attention. In those days I met Professor Supek for the first time. His impressive figure attracted my attention still more.

Professor Supek gathered a small group of young and very talented students. Among them were Ivo Babić-Gjalski, Gaja Alaga, Vladimir Glaser, Borivoj Jaksić and, I think, Sibe Mardesić who later on tied his interest completely to mathematics. Gradually I got acquainted with each of them. All of them were very proud of being coworkers of Professor Supek. It meant that they took part in the development of physics which took place at that time. Professor Supek helped each of them to become an excellent physicist and to reach their later achievements in life. Unfortunately, Ivo Babic-Gjalski lost his life very young in a tragic accident. I think it caused a great pain also to Professor Supek. Professor Supek could tell us much more about it. I think they were the first pupils of Professor Supek, and he was very proud of them.

When I think about that period of time today, then the enthusiasm of those young physicists is much clearer to me. It took 25 years for the first quantum ideas to get a closed form in nonrelativistic processes and almost 50 years to get a form in relativistic processes. The latter development took place just at that time. Professor Supek was involved himself in these investigations, and he transferred the excitement of these events to Zagreb, to our country. To be a coworker of Professor Supek meant involvement in that fascinating period of physics.

In order to illustrate that time, let me take a scientific paper published by Professor Supek and Ivo Babić-Gjalski in a journal of the Yugoslav Academy of Science and Arts in Zagreb in 1953. The title of the paper is *Correspondence of Classical and Quantum Electrodynamics*. Quantization of fields, quantum rules for particles, effects of these new laws, problems involved etc. were discussed in the paper. Let me quote an example: "We ask ourselves, is it possible to define unperturbed state (of an electron) in such a way that there is no self-interaction . . .", one of the questions in the paper. This is one of the fundamental questions in classical as well as quantum field theory. The question is which part of the electromagnetic field is attached to the particle and which leaves the particle. The attached part has to be connected with the particle's parameters, such as the observable mass. The answer in terms of the renormalization of the conventional quantum field theory is not satisfactory even today. Dirac said: "Some physicists may be happy just to have a set of working rules leading to results in agreement with observation. They may think that this is the goal of physics. But it is not enough. One wants to understand how Nature works. There is strong reason to believe that Nature works according to mathematical laws. All the substantial progress of science supports this view. In elementary particle physics we do not have these mathematical laws, only working rules. One should not be satisfied with them, but should continue to search for laws based on sound mathematics"[2]. Feynman said: "The shell game that we play (is) called "renormalization". But no matter how clever the word, it is what I would call a dippy process! Having to resort to such a hocus-pocus has prevented us from proving that the theory of quantum electrodynamics is mathematically self-consistent. It's surprising that up to now the theory hasn't been proved to be self-consistent in one way or the other; I suspect that "normalization is not mathematically legitimate. What is certain is that we do not have a good mathematical way to describe the theory of quantum electrodynamics"[3]. Schwinger said: "So spoke an honest man . . ,"[4].

Many physicists of the present-day generation do not agree with these opinions. Some of them think that these opinions are a nightmare of senile people. Some think that these infinities are an inherent part of Nature.

In its simplicity the question is: Does the mass of an electron, which is moving in a field, contain the energy of its attached electromagnetic field? The answer is clear: Yes, it does. Simply, in this process we do not have to know even its structure. The question of structure is just the place where difficulties begin. The difficulties appear whenever we are faced with source structures in classical electrodynamics. Experimental evidence of classical electrodynamics, however, does not indicate the source structure. Experimental evidence tells us about the behaviour of a charge in a given field, on the one hand, and about fields determined by motions of given charges, on the other hand. When one tries to unify this experience, as it does the conventional classical electrodynamics by the corresponding variational principles, the self-interaction appears and with it the infinities. So the question is: Is this full self-interaction a physical reality or a consequence of the unfortunate unification of this observational evidence?

For example, Gupta said: "... the self-energies of isolated electrons are physically meaningless, and our aim is to carry out a covariant subtraction of these self-energies in such a way that the other usual properties of the electrons are not affected"[5]. And Gupta did it.

Lopes and Schonberg[6] before Gupta, and Schonberg[7] himself expressed a similar idea of an invariant way of splitting the particle's field into two parts: one that reacts on it and accounts for the Larmor loss and the other that does not act on the generating particle, though influencing the motion of the other particles.

Rohrlich independently developed such considerations and constructed a closed consistent classical electrodynamics of a point particle without divergences. Rohrlich, among other, said: "It is a moot question today whether a satisfactory relativistic quantum electrodynamics is best obtained by quantizing classical electrodynamics as envisioned in the nineteen thirties. But the classical theory of Section 69 was not available in the past and is just the candidate that was wanted for this purpose. It is a point of particle theory and it is mathematically well defined".

Many physicists, who do not consider this approach fruitful, feel that we have made sufficient progress in quantum field theory to warrant an axiomatic formulation in favour of the inductive approach to quantization. Whatever the preferred approach might be, there is no doubt that quantum field theory and in particular quantum electrodynamics retains many important conceptual features of its classical counterpart. We can therefore learn from the classical theory a great deal that is relevant in the higher-level theories[8)].

There is an even more convenient theory than Rohrlich's. Therefore the critical points due to Dirac, Feynman and others are not shortcomings of old people, nor are infinities inherent to the laws of Nature.

No matter what is true, this shows that one of the problems in the paper by Professor Supek and Ivo Babić-Gjalski is still very actual today. All this shows that the investigations of Professor Supek with his coworker in the fifties were on the frontiers of physics at that time, but also at present. This explains why that paper was so attractive to young people.

My first, I would say, direct contact with Professor Supek took place at the beginning of my third year of study. Professor Supek gave lectures in the classroom of the Department of Theoretical Physics. Behind this classroom there was the working room of Professor Supek. My memory is still fresh of the moment when the door between these two rooms opened and Professor Supek entered the classroom. It was a question to me what these lectures would bring. As far as I am concerned, they brought about my decision to that my future be physics.

Professor Supek always devoted each lecture to one problem and talked about it in the form of a scientific discussion. It was very far from a classical school hour. It is not necessary to explain what it meant to us. We were learning quickly about the central problems of physics and their secrets. What effects these lectures produced might be illustrated by the fact that often when I raised my head after making notes, Professor Supek was no longer present in the classroom. He had already disappeared in his working room. I wondered how I didn't hear when the door closed.

Lectures of Professor Supek caused many storms in my head. A lot of time had passed until I realized in what a situation quantum physics was. As you know, even today we cannot say that quantum theory is closed.

I was very proud when Professor Supek asked me to work with him on problems of photoluminescence of metals at the end of my undergraduate study. Professor Supek had already done some research in metal theory, in particular on electron and phonon gases. The question was why frequencies of absorbed and emitted light were not equal and why the emission sometimes took a long time. An idea of Professor Supek was that the answer lies in the system of electrons, photons and phonons. It turned out to be true. Electron energies form bands. Light produces transitions of electrons to higher bands. The interaction of electrons with phonons then takes place causing the electrons to fall down in the energy band. This slows down the emission of light because of the selection rules and the Pauli principle. Thus the analysis was rather simple. It was necessary to make the right selection between more and less important factors. Professor Supek's ability to do that is manifested in all his works. That always impressed me very much. These investigations also indicated a possibility of measuring energy bands. Looking at these results today, I think they are not adequately appreciated.

Getting into the physcs of condensed matter did not make me very happy at that time. I didn't like complicated systems and necessity of simplifications, creation of models and similar things. I preferred simple systems which could be rigorously defined and analyzed. Many years later my feelings turned to the opposite. Condensed matter is a very real physical world and very rich in phenomena. Real in the sense that each model can be checked experimentally and the concepts verified. Let me illustrate it on phonon and electron gases. Phonons are excited states of crystal vibrations. Photons correspond to phonons. In the theoretical sense, photons come from quantization of the electromagnetic field. The ground state is a state without photons. The energy of this state is infinite. Is this a physical reality? There is an opinion that observable physics is only above this state. However, there are measurable consequences of this state, such as spontaneous emission, Lamb shift and so on. What about phonons? There is also the ground state of crystal vibrations. Is it a physical reality? The energy of this ground state is finite but not small. It is a part of the total energy of the system and it produces an important contribution to the binding energy. Therefore it can be measured. Measurements confirm this state. Furthermore, this state influences the motions of electrons and may produce effects like superconductivity. A comparison between phonons and photons is therefore very instructive. Modern Quantum Cavity Electrodynamics offers experimental insight into this question.

Many of the phenomena in metals take place in the vicinity of the Fermi level of electrons. The transition of an electron below the Fermi sphere to a state above produces a hole in the Fermi sea. This hole behaves like an electron with opposite charge. The electrons and holes in solid state correspond to the particles and antiparticles of particle physics. In relativistic physics, the state without a particle is a physical vacuum. Its energy in the Dirac field is infinite. Is it a real part of the physical world? In electron gas, the state without electron-hole pairs makes the Fermi sea. It is a "physical vacuum". Is it real? Of course, its energy is a part of the binding energy and therefore can be measured. The measurements confirm it. What does the parallel between the electron gas in metals and the Dirac field tell us? Such a comparison can be dangerous but, if made with caution, may be very instructive.

The next problem which I investigated under the supervision of Professor Supek was the quantum theory of electrical conductivity of metals. The theory which had previously been published was rather complicated. The idea of Professor Supek was to use methods of classical physics, such as free path or collision time. Professor Supek had done much of it. I had to finish it. This idea turned out to be very effective. It gave good qualitative results but also good quantitative agreement with experiments. I would like to emphasize a clear physical picture and not too complicated calculations. Some years later Professor Supek published an article concerning this matter.

Let me tell you an event in this connection. At that time I was in the U.S.A., within regular colloquia in the Department of Physics, University of Illinois, Professor Garcia-Moliner from Madrid came as a guest for one week. The title of his talk was *Theory of Electrical Conductivity in Metals*. At the beginning he mentioned that an article of fundamental value concerning this matter was published. Then he asked: "Do you know that name Supek?" Professor Frederick Seitz, the Head of Department, immediately answered: "Yes, we do, here is his student" and showed me.

A power of clear physical thoughts and adequate mathematics dominate in written materials of Professor Supek. When I give lectures to my students, I always point to it and advise them to read his papers. These seem to me of special importance at present. Many written papers are without this quality. Once at a dinner party in honour of Professor Fröhlich at the State University of Indiana (U.S.A.), Profesor Fröhlich mentioned with sorrow that there were so confused articles that you could not see a connection between the beginning and the end. Today this is also the case with many books.

My work on the electrical conductivity in metals was finished with my Ph. D. dissertation in 1954.

It seems to me worthwhile to tell you something about another event from that time. Investigating quantum mechanics, I had a feeling that the Planck constant was related only to the electromagnetic interaction. It appeared as if it were verified almost exclusively in systems where this interaction dominated. It was possible that an analogous but different scalar might exist in some other interaction. With these thoughts I came to Professor Supek. What was his reaction? Did he tell me: Leave it, it is a nonsense or something similar? Not at all! He listened carefully and advised me to look at experimental evidence: particle diffraction on crystal lattice. Very soon, no matter what particle was concerned, an electron, a proton, big or small, I found that the Planck constant was always the same. Therefore, the answer was negative. In spite of the fact that this idea sometimes comes back even today, because in most of the experiments the electromagnetic interaction dominates, I was excited by the reaction of Professor Supek. He expressed not only respect to personal dignity, which was always important to me, but also a need to be suspicious about what we say, state or teach. We must look back when we make advances, take other viewpoints or do similar actions. This attitude is always present in great people of scientific thought. In a lecture (1956) Feynman said: ". . . it is imperative in science to doubt; it is absolutely necessary, for progress in science, to have uncertainty as a fundamental part of your inner nature. To make progress in understanding we must remain modest and allow that we do not know. Nothing is certain or proved beyond all doubt. You investigate for curiosity, because it is unknown, not because you know the answer. And as you develop more information in the sciences, it is not that you are finding out the truth, but that you are finding out that this or that is more or less likely. That is, if we investigate further, we find that the statements of science are not of what is true and what is not true, but statements of what is known to different degrees of certainty. Every one of the concepts of science is on a scale graduated somewhere between, but at neither end of, absolute falsity or absolute truth". Professor Supek says: "Many things could be different and they are going to be different".

When the number of people involved in some activity becomes large, these views are very often pushed away. When many people tell you that something that is not good is good, it becomes a fact of people's knowledge. How many years and how many people were teaching that the potentials in electrodynamics are of no importance in physics, that they are not physical quantities, many still tell it, but today we know that the opposite is true. Every teacher tells children that they cannot make a sum of apples and pears, but many physicists, even today, make a sum of bispinors and pseudobispinors. There are other such examples. These views have power and sometimes rule scientific thoughts, as the law of great numbers.

Because of this, Professor Supek's views that I have mentioned above are of fundamental importance for the progress of science. Everyone who knows Professor Supek, who worked with him, listened to his words, or read his books, has felt these attitudes and, I am sure, learned very much from them.

After I got my Ph. D. in physics and finished the army service, I returned to Sarajevo. But very soon I came again to Zagreb and spent some time at the Ruđer Bošković Institute. At that time this Institute had contours of its present structure. Professor Supek was very much occupied with the development and building of the Institute. There was a tendency that nonscientific tasks be undertaken. It didn't happen thanks to Professor Supek. The activities at the Institute are such that they have brought appreciations all over the world.

In spite of the terrific involvement, Professor Supek found time for scientific work. He led the Theoretical Physics Department. Smaller or bigger groups were formed in various areas. Working conditions became better. The library was good and contacts with scientific world became better. Many foreign visitors came to visit the Institute. It helped to develop a scientific atmosphere, which is so important for this work. My personal investigations were in many-body theory. At that time there was a popular theory of collective motions of electrons in metals with the so-called subsidiary conditions. I spent a lot of time on it, and so did also some other members of the Institute. Unfortunately, it finished with disappointments in trends; one more illusion of youth in science. Trends are generally present in life. They are usually based on a part of the truth. This part can be small and may not be present at all, just an illusion. The difficulty is in that in the course of time many of them accumulate power in reciprocal relation to its value. Existential problems and inner attitudes of people contribute to it very much as in ordinary market. Stubbornness of protagonists makes the trends aggressive. Experience taught me of this danger.

At the end of the fifties Professor Supek helped me to go to the U.S.A. I spent two years as a Research Assistant Professor at the University of Illinois. Professor Supek helped me in two ways. He arranged for my stay with Professor Frederick Seitz, and he arranged for the financial support. At that time I had a full-time employment at the University of Sarajevo. Two big problems faced me. I couldn't get a passport. I had already had the signed contract with the University of Illinois and even an airplane ticket from Paris to New York. At the last moment this problem was solved in a way. Then I was faced with another problem. I needed about 100 U.S. dollars for travel expenses from Sarajevo to Paris and from New York to Urbana. All my efforts in Sarajevo gave no result. Then I talked to Professor Supek and in a couple of days I got the money through the Ruđer Bošković Institute. I must say that at that time it was not possible to get foreign currency from state institutions nor to use such currency if it was not supplied by state institutions. I tell you this in order to express my deep gratitude to Professor Supek for this help. But not only for that reason. I also wish to show how Professor Supek took care of his coworkers and, generally, scientific workers, and in this way he helped not only personal prosperity, but also the prosperity of science in our country. I am quite sure that many individuals deeply appreciate this help but I also think that the whole society does it, too.

By the time I went to the U.S.A. my contacts with Professor Supek became less frequent. When I returned, I spent most time in Sarajevo, although I took part as a lecturer in the graduate study at the University of Zagreb. I was involved in various problems in physics but also a great deal in the development of physics at the University of Sarajevo. However, the influence of Professor Supek on my work and life never stopped.

From time to time, the state authorities of Bosnia and Herzegovina invited Professor Supek as an advisor in problems of natural science. During such a visit an idea was expressed to allocate the building of the Faculty of Arts, which was under construction, to natural sciences, in particular to physics and chemistry. I personally was not involved in that discussion, but I inclined to that substitution. However, Professor Supek didn't support that idea. The people of natural sciences and mathematics ruled over the state institution. However, he considered that for the progress of a society, wide views are necessary as well as appreciation of ethics in social relations, which was also in question here. On various occasions, many years later, I found this view of Professor Supek deeply justified.

In many situations the written text by Professor Supek was not only a support, but also an inspiration to me. I have already mentioned that I always point it out to my students.

In order to illustrate the influence of Professor Supek, let me take one more example. In the first period of the Ruđer Bošković Institute, from time to time Professor Supek asked his coworkers to come to his room. During such a conversation, in which, I think, Vladimir Glaser and Borivoj Jaksić also took part, at a moment Professor Supek expressed his doubts in quantum physics. I didn't know exactly what Professor Supek meant, nor do I know it today. But it was a surprise to all of us.

Many years later, when giving lectures on (conventional) quantum field theory, I felt that I was telling something wrong. At these moments my thoughts came back to that conversation. It was not a question of understanding of quantum physics, but a question of its consistency. As the lectures were for students, I had some doubts whether I was misleading them.

A variational formulation of physical laws is not only a convenient mathematical method, but also a concise vocabulary of physics. It is of great value, in particular when the object of study is not sufficiently known. Breaking this formalism easily leads to a separation from the real material world. This statement may not be acceptable to many physicists. Modern methods are sometimes so far from their origin that these internal links become unclear. In a recent correspondence with a very well-known physicist, Professor Supek said that this position of thinking was so strange to him that he had difficulties in understanding problems in it. However, we cannot give up the vocabulary of macroscopic physics when we go to the microworld or to the cosmic world.

The Dirac field plays an exceptional role in relativistic quantum physics. It appeared in the period when nonrelativistic quantum physics had been raised to the relativistic level. In the thirties, as also today, there was a belief that almost everything had to be quantized. Canonical quantization dominates in nonrelativistic physics. It is also present in relativistic physics. In order to apply it, it is necessary to have a correct canonical description of the object. This is not the case in the conventional Dirac field theory. Lagrange's variables are proportional to their momenta. Consequently, there is neither a correct canonical formalism nor a correct canonical quantization. Many physicists have met with this problem and solved it in different ways. Some just neglect it. They use a method which is neither canonical nor consistent, and come to results which are acceptable. Some make quite general considerations, which have to serve for conviction of correctness, but when they come to the Dirac field everything breaks; however, they leap over it. Some clearly say that the situation is not clear, is complicated with many problems and that the best way is just to leave it and to postulate quantum rules with justification in applications. By all means, the last way is the most honest one.

Talking about these problems in one of my lectures and analyzing the mathematical structure of the Dirac and the electromagnetic fields it became clear to me that this required much more attention than is usually given to it. Both fields are described by a set of partial differential equations of the first order with several functions. This mathematical comparison concerning the Dirac field is present in the literature, but only as a statement that the relativistic field equations with the first-order time derivative should have more than one function. In Dirac's works, as far as I can say, this comparison is not present. It perhaps explains why the analysis is such as it is today, though it should be different. In nonrelativistic quantum mechanics, the energy is related to the time derivative. This fact also led Dirac in his analysis of the Dirac field. The mathematical similarity between the Dirac and electromagnetic-field equations has much deeper physical content. This knowledge led to the necessity of a more systematic analysis of the mathematical formulation of the Dirac field in terms of a physical scheme of the material world. I devoted much time to it. In the generation to whom I talked about it there was an extraordinary student. It is necessary, and I am very glad, to mention his name. He is Josip Brana. I suggested him one of these problems for his B.Sc. thesis. Since then we have become good coworkers and have obtained several very interesting results. Later on some other young physicists have joined us.

The schematism of the conventional quantum field theory is such that it is not easy today to see what it is all about, although more than sixty years have passed since the appearance of the Dirac equation. For that reason, repulsion to this knowledge is present very often. It is more psychological than scientific.

Let me clear up this problem by asking the question: Why do the potentials play a very important role in the electromagnetic-field theory, but are completely absent in the Dirac-field theory? Why are the equations of motion differential equations of the same kind? The electromagnetic potentials have been introduced into the theory in order to simplify mathematics. Why are they not used with the Dirac field? We know today, as I have already mentioned, that the electromagnetic potentials are not only a suitable mathematical device, but also a substantial element of the electromagnetic field as a physical object. Why are the corresponding quantities not present in the Dirac field?

Nowadays it is possible to express the content of this problem more simply by saying that there exists a system of relativistic differential equations the functions of which can form a bispinor and can form an antisymmetric second–order tensor and two scalars, with the electromagnetic field as a special case.

In a letter, Professor George Lochak said: That is true, I have checked it. It is interesting and even a little disturbing. Why disturbing? Let me make the following remark. When people are faced with a problem, they usually find some way out. It may be a wrong one, but by repeating it many times, one develops a feeling that it is really good and people begin to put confidence in it. We may find many examples of that kind in practical life. This occurs also here. But if something disturbs us, we must look at it openly. What comes out from this statement about the system of differential equations with two possible physical objects? The variational formulation is just a suitable way to get an answer. If the action integral is given for a system, all physical informations about the system can be obtained. Since the equations of motion of the above systems are equal, their action integral must also be equal, but that is not the case in the conventional field theory. Many problems arise from it, including fundamental concepts and models. Let me remark that the identity of the action integrals necessarily does not mean the identity of its physical objects. If that is clear, then it immediately follows that the conventional variational formulation of the Dirac field is not good and, consequently, so are many things derived from it.

It is often said that the relativistic laws for one particle cannot be formulated using the wave function of the particle, but it is necessary to use many-particle theory with infinite number of particles, and that this is realized in quantum field theory, where the symmetry of relativistic quantum mechanics in positive and negative energies is replaced by the symmetry with respect to particles and antiparticles. In a lecture dedicated to Dirac in Cambridge four years ago, Steven Weinberg said that almost everybody supported that opinion at present.

However, when the error that I have described is corrected, the content of the results is quite different. First, the Dirac field is determined by two equations, with positive and negative mass terms, not only by one of them. Second, separation of solutions with respect to space inversion (on spinors and pseudo-spinors) eliminates negative energies (from which the whole problem began). Finally, its physical observable manifestation is one particle and its antiparticle which satisfy the laws of motion known as quantum mechanics. We may say that the Dirac field is one of classical relativistic fields but with specific observable manifestations. Lowering it to the nonrelativistic level leads to nonrelativistic quantum mechanics as a consequence of the existence of such a relativistic field.

One of the fundamental assumptions in this formulation is a relativistic scalar which turns out to correspond to the Planck constant. Thus the mystery of quantum physics appears to be in this constant.

It is not easy to talk about this problem among physicists.

As I have mentioned, I don't know what Professor Supek meant when he said that he doubted quantum physics. I have selected one example which shows that this doubt is very much justified.

I would be glad to talk about some other problems. But I think I should now end discussing this theme.

Fourty years ago a green field was at the place of our meeting, a wonderful valley with nice views on the southern part of Zagreb. Now we see a big institute for fundamental science, a pride of Zagreb and our whole country. And here is the man to whom we have to thank for it.

At the memorial session in Cambridge four years ago Feynman said: When I was young, Dirac was my hero. On this occasion I may say: When I was young, Supek was my hero. But I have to add that also today Professor Supek is my hero. I am proud of it. It was my personal good luck to have met him, chance to meet Professor Supek. I want to thank Professor Supek for everything he did for me, and that was very, very much indeed, as you have seen.

At the end I want to express my best wishes to Professor Supek for many happy years with warm care of his family, coworkers and friends. Many thanks once again.

Thanks to all of you for coming and paying attention to these reminiscences of my work with Professor Supek.

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