CONVERSATIONS WITH STEFAN POKORSKI (SP AND ANDREW KOBOS)

Conversation 1

BUT ON THE OTHER HAND…..

AMK – *Stefan, what would you like to say, please, on the occasion of your anniversary, perhaps a bit annoying after all?*

SP – Indeed, Andrew, a bit annoying… Well, if you wait for a summary, then I tell you: it is too early [laughing]. But as an occasion for a little reflection – that's fine. The bottom line is that I have always liked what I have been doing. I am happy to see the Warsaw Group in theoretical particle physics present on the scientific map of Europe. Actually, I am proud of my former students in Warsaw and in a couple of other places in the world, and of what we have achieved as a group – hopefully, we made small but useful contributions to the research in particle physics. I am happy to have a few good friends in the world; with some of them we made some useful research, too.

I may quote here the title of a famous popular book on psychology by Thomas Harris, *I am OK, you are OK* – it's a wise life maxim. Indeed, given the time and the place I was born, I feel alright. Everybody should put his bar just a bit up, but not too much higher than he can jump, and should enjoy every successful jump.

AMK – *What do you mean by "the time and place you were born"?*

SP – Surely, it could have been worse… for example being born in Poland a few years before the second world war. Still, the early post-war period here was not particularly great, either. The family's war wounds, the fears of the late forties and the early fifties – those weighed a lot. And on the scientific side – the war had almost totally destroyed Poland's research potential, particularly the human one. It took several decades to "repair" the damage, particularly for a pretty isolated, at the time, society.

But on the other hand…. it seems to me that in the atmosphere of those years, for many the value of science and, in general, of intellectual life was higher than it is now. I mean here, obviously, my parents' generation. Moreover, obstacles often mobilize and stimulate… – now I mean myself.

AMK – *On the one hand… but on the other hand – how often would you say this phrase looking in retrospective at your professional life?*

SP – For sure, a couple of times; In my research, I missed a couple of opportunities. But on the other hand… Therefore, I have no reason to regret. You and I had a long conversation about four years ago, included in a volume of recollections by the members of the Polish Academy of Arts and Sciences (PAU), addressed to a broad, mainly non-physicist public. Since you are going to include longer excerpts from this conversation translated by you into English for the present volume, I apologize to my fellow-physicists for the personal touch of that conversation and for various quite popular comments on physics. Anyway, "but on the other hand" is mentioned there several times.

AMK – *The purpose of the previous publication was to document in a way the scientific life in Poland in the last 50 years. Your younger colleagues asked me now to collect and edit the present volume. Why? To please you?*

SP – As far as I know my colleagues and as far they know me, they mainly want to please themselves [laughing]. Speaking seriously, we all, I think, look to seeing this coming volume in the same way: as a documentary, a very informal one and with a lot of many personal perspectives on the research in theoretical particle physics in Warsaw during the last 50 years. But not only that: we also want to transfer to others a message of love for scientific curiosity and for passion in research, of the importance of the basic research and of particle physics in particular, as one of the most challenging domains in physics.

AMK – *So, it is a success story and a love story…*

SP – [silence and no sign of objection]

AMK – *You say the last 50 years, so let us start again with your memories from your student times.*

SP – As I said in the earlier conversation with you, I began to study physics in Łódź and then I moved to Warsaw in the fall of 1961, for the third year of university. I clearly reiterate in the present conversation of ours: Leopold Infeld's name was behind my decision to move to Warsaw. And I am in particle physics thanks to Wojciech Królikowski. The first Chair of theoretical particle physics, just for Wojciech Królikowski, was established at University of Warsaw in 1960.

Leopold Infeld was a man of classical general relativity but he cared about developing other branches as well. Still, if I remember correctly those years, the atmosphere in the Institute was not so much in favour of particle physics. The time was pre-Standard Model, and there were many speculative ideas floating around, as such are floating again in the recent post-SM time, lacking precise mathematical formulation, etc. Nothing to be liked by the mathematically minded Infeld school.

Every Thursday afternoon, Wojciech Królikowski and Józef Werle were running seminars on theoretical particle physics. As a third and later fourth year student, I attended them regularly. I remember it very well – it was usually Wojciech Królikowski who suggested the topics for talks (Józef Werle was closer to theoretical nuclear physics) and most of the new ideas were discussed (the eightfold way, the Cabibbo angle, algebra of currents, ways to deal with non-renormalizability of the Fermi theory, etc). Wojciech Królikowski followed closely the literature and felt strongly the need for new ideas, even if speculative ones. By all means, he was the founder of theoretical particle physics in Warsaw, at the University, and in the Institute for Nuclear Research, where he headed a division, too.

AMK – *Did he have PhD students?*

SP – Of course he did. The particle physics group, spread over the two institutions, was slowly growing. Andrzej Jurewicz, Michał Święcki, Leszek Łukaszuk and Grzegorz Białkowski – later my thesis advisor – were all his students. In spite of a broad spectrum of seminar subjects, the research of that younger generation was mostly focused on the so-called S-matrix theory of strong interactions. At the end of 1963, as a fifth year student, I approached Professor Wojciech Królikowski, whom I also knew from his excellent lectures on quantum mechanics and on the 2nd quantization, and asked him for a subject for my MSc thesis. He sent me to Grzegorz Białkowski, who had just come back to Warsaw from his long-term stay at several universities in Italy – and this is how my time at The Hoża began.

AMK – *In our conversation almost four years ago, you described those early years in some detail. Would you, please, add more to that?*

SP – Yes. I would like to stress how important it has been for my research to have, as Białkowski's student, very close contacts with the Warsaw experimental Group. That "formed" me as a physicist, not subjectwise but it did form my way of thinking about physics. One cannot overestimate the "simple" questions asked by experimentalists. And my lasting friendships go back to that time. The picture would not be complete without mentioning also a "nightmare" of my life: the joint exp-theory seminar held on Fridays at 9 (!!) a.m.

Another point I want to emphasize is the impact on my scientific life of my first one and one-half years stay at CERN in 1968–1969. Poland was then not a CERN member-state but experimentalists from Cracow and Warsaw participated in CERN's experiments. Also theorists happened to be accepted there for long term visits (Andrzej Białas, Leszek Łukaszuk). I got accepted at the CERN Theory Division, merely on the basis of the strong support from Warsaw. I remain most grateful to Maurice Jacob, Leon Van Hove and Jacek Prentki for their extreme hospitality extended to me at CERN. The first two made my stay there scientifically fruitful, while Jacek Prentki as Theory Division Leader eased the shock the 26 year old scared person was exposed to. My friendship with Jacek, getting stronger and stronger with years, was interrupted by his death in 2009.

Maurice Jacob became a close friend of mine, too, for many years, after my first stay at CERN. Our relations and our family's relations became very private. Much more formal were my contacts with Leon Van Hove, later on the Theory Division Leader and the Director General of CERN. For many, a very tough boss; for me he was an older colleague for whom I had a lot of sympathy and appreciation.

AMK – *You said earlier that your first, as we would call it now, post-doc at CERN was not a breakthrough yet it was very important for you. Why was that?*

SP – It was not a breakthrough because I was still working on strong interactions and multiparticle production. However, it was very important because I was able to prove that I could offer something intellectually to the international research community. Now, I don't remember much now but, I guess, it must have been a bit more than just a good work because later, in the 1970s, (already having a permanent position in Warsaw) I was offered a couple of more long-term associateships at CERN and a number of several months-long visits to various places in Europe and in the United States. In order to remain a welcome, or at least an acceptable visitor, then it was more or less obvious that you had to bring some new ideas and some stimulation in particle physics; especially if the host had to offer a bit more than just a desk, as was always the case with us in the early days. Of course, this is an international science so you also profit a lot from that. No doubt. But it cannot be a one way avenue.

AMK – *Was it also like that with your former PhD students?*

SP – Sure. Usually I was helping them to get their first post-doc positions, but the rest depended on them.

AMK – *You said a lot about international collaboration. How would you describe the research conducted in Warsaw, its atmosphere, its intensity, etc. in the years when the group of your students and post-doc young collaborators was growing?*

SP – My driving force always was to have a strong group in Warsaw, in spite of all the well-known pre-1989 obstructions. I think it went pretty well because in Warsaw we were collaborating with each other and generating a lot of new ideas. It seems to me, Andrew, you want to confront my perspective with the perspective of my younger colleagues. A clever trick! They would probably tell you that they were forced to a heavy, "unhuman", labour. I can tell you I see it differently: it was always for me to work too hard and they were always too slow! But, I guess, the effects have after all been not too bad and we have been able to forgive each other, even to like each other personally. I think we had (in fact, I hope we still have) a common curiosity in research and common goals; also common ambitions to create something.

AMK – *Let me, please, come back to your earlier remark about some similarity between the situation in particle physics in your student times and in the recent years.*

SP – Indeed, there is a similarity but a profound difference, too. At the time there was no theory of weak and strong interactions. In the quest for such a theory many very speculative ideas were considered and seriously discussed, similarly as in the last twenty years for Beyond the Standard Model theory. There was, however, plenty of experimental data to guide and control the theorists' imagination. Hence, the final synthesis, the Standard Model, has emerged quite quickly. In the last thirty years, in the post-Standard Model era there has been no such empirical guidance for the theorists because – as my friend Hans Peter Nilles from Bonn used to say – "The greatest puzzle of the Standard Model is that there is no puzzle". Although we clearly see some theoretical imperfections of the Standard Model, the available experimental data – except for a couple of astrophysical facts (dark matter, matter-antimatter asymmetry) did not press for the Standard Model's extension.

Here comes the great breakthrough opened to us by the Large Hadron Collider, which performs so successfully. Finally, theoretical ideas will confront experiment and, instead of asking what is possible, we are able to begin asking what is true. The next 15–20 years will be great for the physics of elementary interactions. The Great Expectations will – I believe – become true.

Warsaw, January 2012

Conversation 2

WE ARE AT A TURNING POINT IN PARTICLE PHYSICS

Stefan Pokorski and Andrew M. Kobos

On SP's young years

Andrew M. Kobos [**AMK**] – Stefan, would you please tell me about your young years and about your road to physics, in particular?

Stefan Pokorski [**SP**] – Eh, Andrew, you're forcing me to embark on a sentimental journey in time…

AMK – You may call it that way, but I mean your formative years.

SP – I was born in 1942, during the Second World War. After the war I lived with my parents in the city of Łódź and it was there I went to elementary school and later to high school.

My road to physics was perhaps trivial. Retrospectively, going as far back in time as I can remember my elementary school, it seems to me that I attended it just to engage one day in scientific research. It puzzles me, of course, where from such thoughts come to my mind. It's been so not only since I very recently thought a little bit about our pending conversation. It seems to me that this supposition resulted from what my parents had dreamt about my future. This desire of theirs somehow permeated me subconsciously.

My father, Longin, developed in me the spirit of sport-like competition. My mother, Frances, was a psychologist. She dealt with the so-called difficult teenagers and it, too, somehow affected me. On the one hand her ambitions and dreams about my future were transmitted to me in a way, yet – on the other hand – she left it to my own responsibility for my own future that they come true. I can’t recall any specific help from my parents in actual realizing their ambitions. I think it was good that they left it this way.

I just said my road to physics was trivial. It means simply that all my young years pointed to exploring things. But my interest in physics was my own, originated spontaneously in me in the final years of my seven year-long primary education.

On undergraduate and graduate studies

AMK – *Was it University of Warsaw you studied physics at?*

SP – I began my undergraduate studies in physics in 1959, at the University of Łódź – with no quandary. I studied at Łódź for my two first undergraduate years (out of five). When I look back at that period of my life, I appreciate that people who then lectured physics at Łódź University made quite heroic efforts to teach us something meaningful. Yet, after two years I moved to University of Warsaw to continue my studies in the third year.

AMK – *Why was that? Did you not like the style or the quality of physics courses at the University there?*

SP – No, it was not for such a reason. The courses were not bad. People at University of Łódź have actually taught us quite a lot. My two colleagues from the same year, Maria Giller and Stefan Giller later became professors at University of Łódź.

In my case, the decisive factor was the myth of Leopold Infeld. Although I am not sure if at this early point of my undergraduate studies I thought about theoretical physics but surely I did so a year later. First, Professor Infeld actually created the institute of theoretical physics at University of Warsaw and, second – although there have been differing opinions on this – he certainly promoted physics, at least or particularly in the minds, in fact the brains, of young people. I do know this from my own example. I read his books on famous physicists and mathematicians, his popular articles on physics, and – most importantly – Infeld was surrounded by an aura of being formerly Einstein’s collaborator. Leopold Infeld’s myth most certainly was at the heart of my decision to move to University of Warsaw.

Incidentally, it is a frequent road to good physics: a quest for the best scientific centres in physics… Just that way I found myself in Warsaw. And later… there is basically not much to talk about.

AMK – *Well… Don't tell me that, Stefan…*

By the way, Professor Infeld did not have then much time left; certainly not enough to be your master…

SP – He did not, it was too late for this. In physics, generations last short. I was a student of Professor Grzegorz Białkowski, and a "scientific grandson" of Professor Wojciech Królikowski, who in turn had been a student of Professor Wojciech Rubinowicz.

Andrew, here on the wall you can see a drawing that shows the genealogical tree of the of the physicists in the Institute of Theoretical Physics of University of Warsaw. There are two main branches of this tree: one Rubinowicz's and one Infeld's. Both Infeld and Rubinowicz educated and guided groups of their students who themselves had broad spectra of their interests in physics. Infeld himself worked in relativity but shaped, for example, Iwo Białynicki-Birula who has worked in quantum field theory, and the late Maciej Suffczyński who worked in solid state physics.

AMK – *Was it thanks to Professor Grzegorz Białkowski that you found your way to elementary particle physics?*

SP – During two last years of my undergraduate studies my interest in physics clearly crystalized. At that time Białkowski was abroad, in Italy if I remember correctly. I got to particle physics mainly because of Professor Wojciech Królikowski. He and Professor Józef Werle run a seminar on theories of elementary particles and atomic nuclei. It was around these two professors that young physicists interested in these theories focused.

It happened purely by a random chance that I found myself under the supervision of Grzegorz Białkowski who returned to Warsaw in the fall of 1963, when I just started my fifth undergraduate year. There were two us who wanted to specialize in elementary particle physics. Wojciech Królikowski decided by tossing a coin. I went to Grzegorz Białkowski and the other person to Wojciech Królikowski. I defended my MSc thesis in 1964 and my PhD thesis in 1967, both supervised by Grzegorz Białkowski. My PhD thesis was on phenomenology of strong interactions.

The fact that Białkowski closely collaborated with experimentalist was very essential to my early scientific career. His research topics were very similar to those that at the time dominated in Cracow. It was the phenomenology of strong interactions between particles in collisions at high energies. In Warsaw the leading particle experimentalists were Ryszard Sosnowski, Andrzej Kajetan Wróblewski, Janusz Zakrzewski, Lech Michejda and Grzegorz Bialkowski was the theorist. It was among them that my scientific existence came to being.

AMK – *Your habilitation – DSc thesis?*

SP – After I had graduated with PhD I was sent for six months to the Joint Institute of Nuclear Research in Dubna, USRR, perhaps as a reminder to me that I should not be thinking too high about myself… However soon later, in 1968, I received a postdoc position in the CERN Theory Division. No doubt, in getting this position, I had strong and warm support from Grzegorz Białkowski in the first place, as well as from Wojciech Królikowski and from the Warsaw experimental group. Professor Marian Danysz support was particularly important. My stay at CERN was very essential to me but then at CERN I worked on subjects different to those on which I would later spend my life in science.

As to the heart of the matter – i.e. physics – I reckon that after I got my PhD I always had to manage myself – in accordance – after all – with the healthy principles of one's scientific development. My habilitation (DSc) thesis in 1971 was less phenomenological, more theoretical, but basically on the same subjects. It bordered the problems that later on turned out very important in a totally different context: it concerned the Veneziano model which in later years became the foundation of string theory.

Here we come to theoretical physics of elementary particles as it stood in the years when I made my undergraduate studies, I prepared my PhD thesis, and – finally – to what in this physics we have to offer now.

I remember perfectly well my discussions in the mid-1960s with Grzegorz Białkowski, my PhD thesis advisor, in which he maintained that during our lifetime there would be no chance for a reasonable theory of elementary interactions to emerge. Such opinions were perhaps the reason for the then very strong trend in world's research aiming at phenomenological descriptions of strong interactions, multiple particle production in high energy collisions including. Such an approach resulted mainly from the progress in experiments using new accelerators and new experimental techniques that allowed for the detection of very many particles and for analyzing their interactions. The general expectation was that – with time – it all will result in a fundamental breakthrough in particle theory.

Yet, parallel to the phenomenological approach to strong interactions there was another stream – somewhat a hidden one – that eventually turned more important – as we can see from today's perspective. It was research on weak interactions, toward major advancements in quantum field theory.

However, in Poland – similarly to many other research centres in the world – people, who at the time were well prepared to join that stream, could have been counted on the fingers of one hand. And not even those able to join this stream, but those to appreciate it, and to understand that it was so important. From the standpoint of the then theory of elementary interactions this second stream – now clearly a fundamental one – was underestimated and as a matter of fact it was difficult to plug oneself into it.

Polish physicists did not participate early enough in the development of the theory that now is called the Standard Model. On the one hand it is regrettable… But on the other hand… The research carried at the time in Poland on the phenomenology of strong interactions was vital to achieving by "Polish" elementary particle physics a strong international stature; it was our window on the world that itself, too, was intensely engaged in this kind of research. In my case, I want to mention at this point my scientific collaboration – extremely important and valuable to me – with Professor Leon Van Hove who later was elected the Director General of CERN.

One should also remember that – although later the phenomenological research on strong interactions turned out less fundamental than it was expected to be – plenty of important and very useful results were obtained. These results allowed for good descriptions of the effects in the collisions of elementary particles at high energies. Without those results it would simply be impossible to interpret the experiments currently performed with contemporary large accelerators.

I would like to expand on my earlier thoughts I touched upon in our conversation. The dawn on my scientific existence occurred in physics of strong interactions stemming from the S-Matrix theory. Neither in Warsaw nor in Cracow was there a sufficient pressure to become preoccupied with quantum field theory. That is why I said earlier that my first stay at CERN – although extremely essential to my coming into being as a physicist – was not a breakthrough in my interests and in my scientific research activity.

I would rather say that such breathroughs happened during my next two stays at CERN in 1973 and 1978, for one year each. In 1973 at CERN I began to seriously study quantum field theory. A turning point of the particular importance to me was the stay in 1978 when I finally turned myself from a phenomenologist into a theorist. From that point in time I became preoccupied with quantum field theory in the context of what was still to be done in the Standard Model – more precisely in quantum theory of strong interactions, i.e. in quantum chromodynamics (QCD). By that time the theory of electroweak interactions had, in principle, been completed.

AMK – *Quantum chromodynamics on lattices from its onset involved huge numerical computations. At CERN there were very powerful computers: the advanced CDC's and the first Cray supercomputers. But in Poland, there was a Cyber-72 at the best…*

SP – At that time computers in CERN were not too impressive either. But it was just the early stage of QCD's development, laying the theoretical foundations of QCD, and one still could calculate many things analytically. Together with Wojciech Furmański then from Cracow, we two worked out several papers which still matter in QCD.

The most important was that since the late 1970s quantum field theory became the basic tool for our research carried in Warsaw with a group of my younger colleagues.

AMK – *Let us continue, please, the thread of a missed opportunity. Do you think, Stefan, that there could have been some Polish names in formulating the Standard Model, had Polish physicists worked in this field sufficiently early?*

SP – Yes. There could have been Polish names in this, but such work should had been started early in the 1960s. Nevertheless, there are Polish names in the Standard Model, but placed not so prominently, as they could have been. As to the missed opportunity, Polish names are – as I said earlier - very prominent in the phenomenology of strong interaction. I've told you that earlier, Andrew.

On the Standard Model

AMK – *It was the 1970s when the Standard Model was formulated…*

SP – Not quite so, to be precise. The concept of quarks as the consequence of symmetry, with no dynamics, was conceived in 1962. The electroweak part of the Standard Model, i.e. the theory of electroweak and electromagnetic interaction, was completed in the second half of the 1960s. In 1967 Steven Weinberg wrote his famous paper – for which he received the 1979 Nobel Prize in Physics, together with Sheldon Glashow and Abdus Salam. And 1967 was the year I wrote my PhD thesis on a totally different subject…

Quarks as physical objects emerged at the end of the 1960s. I remember a conference in Lund, Sweden, in 1969 – the first conference outside Poland I attended. At this conference people were told about experiments at SLAC in Stanford, CA, in which – for the first time ever – it was noticed that quarks were physical particles.

QCD, the theory of strong interactions was formulated in the years 1972-1974. In 1973 while working at CERN, I came across this problem for the first time. Then it was still a chance – for me at least – to jump somehow into this. However, I joined real research on this subject only a few years later.

AMK – *Was anything of fundamental importance added to the Standard Model in later years?*

SP – The fundamentals of the Standard Model were formulated in the 1960s and the 1970s. Starting from the late 1970s there have been no doubts that it is the correct theory. No new fundamentals have been added to the Standard Model in the later years. However, more and more precise verification of the Standard Model was and continues to be fundamental. I mean the experiments at CERN in the 1980s that led to the discoveries of several bosons predicted in the Standard Model, like the W- and the Z- bosons. Insofar, the Higgs boson remains elusive.

Simultaneously, the quantum field theory that underlies the Standard Model was tested very precisely – that was also fundamental.

AMK – *There are physicists who question the mathematics of quantum field theory, the renormalizations thereof…*

SP – In my mind quantum field theory constitutes above all a physical picture of interactions. In the framework of this picture a duality occurs between particles and fields in which the interactions are carried by particles. The mathematical divergencies result from an intermediate process, solely a calculational one, that is being done in convenient manner from the technical standpoint but not necessarily a reasonable one from the physical point of view and not necessarily just the only one possible. The calculations can be made in such a manner there are no explicit divergencies at all. Quantum field theory provides us with physical predictions in terms of some physical parameters that are measured in experiments. Looking at it this way, there are no divergences in quantum field theory.

But you asked about renormalization and I talk about divergences. I guess, you mentioned renormalization in the context of divergences in QFT, didn't you? But renormalization is something more general, necessary also in finite field theories. It's linked to the fact that the physical input parameters, like e.g. the coupling constants, are experimentally measured at some energy scales. In a sense, it's a beautiful by-product of the renormalization procedure to remove the divergencies. I would not pick at the mathematics of renormalization. Summing up very briefly: I can see nothing wrong with quantum field theory.

As you know, Andrew, some people are dreaming about theory of everything. It is a dream that everything be finite, be no free parameters, everything be predictable from … eh, just from what? I don't know. These are fantasies, I think.

AMK – *Did the Standard Model emerge more clearly from quantum field theory or from phenomenology?*

SP – It is an interesting question. The Standard Model emerged from – no doubt about it – a very strong interplay of theory with the then existing experimental results. Apart from strong interactions, for which there were lots of results, there were weak interactions, also with many experimental results on them. The structure of the Standard Model has emerged in order to describe experimental observations. E.g. weak currents of specific properties were seen, etc. On the other hand, a huge and very substantial element of theoretical thinking was also involved. First of all, gauge symmetries have emerged as a fundamental physical principle. The renormalization problem you brought up here is another good example of theoretical problems the Standard Model faced. The final shape of the Standard Model has been imposed by the desire to have a renormalizable theory, i.e. such one that would predict results of various measurements in a function of a finite number of measurable parameters.

The definition of a renormalizable theory is not its ability to remove infinities but as a scheme under which all the predictions of such a theory (even infinite number of those) can be expressed by a finite number of measurable parameters. Speaking less precisely, a non-renormalizable theory differs from a renormalizable one in that in the former one has to increase the number of measured parameters in order to come out with new predictions. The gauge symmetries as an underlying physical principle and the requirement of renormalizability, bound with the existing experimental data – i.e. a very strong interplay of theory and experiment- resulted in formulating the Standard Model.

AMK – *Andrzej Białas believes that the Standard Model will survive in physics for hundreds of years or more, similarly to the Newton theory.*

SP – Of course, it will. With one reservation though, depending on what we mean precisely by the Standard Model. If we mean a renormalizable theory with an elementary scalar field, usually called the Higgs boson, then not necessarily so. But if we're talking about the unified theory of weak and electromagnetic interactions based upon the gauge symmetry SU(2) x U(1) and the Brout–Englert–Higgs mechanism of this symmetry's violation, then surely so.

In such, a more general, sense the Standard Model is true in the same way as Newton theory is true as an approximation to special theory of relativity. The Standard Model, in this general sense, is most probably an approximation to a larger theory which – so far – we do not know. It is an effective theory for phenomena occuring up to a certain energy scale, a theory immersed in the hierarchy of theories.

One may ask the following question: What does the Standard Model need its renormalizability for if – after all – it is only an effective theory? Indeed, renormalizability is not a necessary characteristics of a "correct" effective theory, but it is a convenient property of it, which makes it possible to conceal our ignorance of physics at higher energies by the use of few free parameters measured experimentally. However, it may well occur that the results of experiments carried with the use of the Large Hadron Collider will soon point to a way, in which the Brout–Englert–Higgs mechanism is being realized, different than the one through the existence of an elementary scalar field. And then the effective electroweak theory may prove to be a non- renormalizable theory.

On the Higgs boson

AMK – *Stefan, you've mentioned the Large Hadron Collider. And if we talk about the LHC, questions about the Higgs boson are inevitable. Is the Higgs boson the only yet unconfirmed*[1] *cornerstone of the Standard Model?*

SP – The short answer is yes. Its longer version may be as follows. I frequently say that we are at a turning point in particle physics. It is only the Higgs boson that is still missing to fully verify the Standard Model. All this is a logically inferred consequence of the discovery by Antoine Henri Becquerel, Marie Curie Skłodowska and Pierre Curie of strong and weak interactions via the discovery of radioactivity. Step after step we have been seeing the harmony of experiments and theoretical concepts. All these have nicely fallen in place enforcing the evolution in 20th century physics. The formulation of the Standard Model and the results of experiments in the colliders at CERN that brought the discoveries of the W- and the Z-bosons – all that – I think – was at some point, well before it has actually happened, expected. These were great discoveries but not unexpected ones, the direction was more or less clear.

But now we are at a turning point in particle physics. First of all, the Brout–Englert–Higgs mechanism is needed to finally solve the mass problem, to make W and Z bosons massive and to leave the photon massless in the unified theory, and to give masses to quarks and leptons. Isaac Newton codified mass; he said that mass is inertia. But we still do not know what the source of mass of elementary particles is, how the Brout-Englert-Higgs mechanism is realized in Nature. The simplest version of the Brout–Englert–Higgs mechanism realization predicts the existence of a scalar particle – the Higgs boson.

AMK – The Higgs field is supposed to be responsible for violation of symmetry in electroweak interactions. And this is thought to give rise to mass.

SP – This is – roughly speaking – a rather formal explanation. One can also say that the Brout–Englert–Higgs mechanism explains the generation of mass by the existence of a fifth interaction. In its simplest formulation, it is the interaction of particles with the Higgs field that yields the primordial mass to particles. It is the same as to say that symmetry is violated – but using a different language, more physical, more vivid, I should say. The consequence

[1] This conversation, or at least its core, was carried out in Polish in 2007. Its, somewhat revised and shuffled, English version was written in early 2012. (AMK)

of the Brout–Englert–Higgs mechanism is then the existence of a new particle – the Higgs boson – having the mass somewhat more than 100 GeV. But this would be so only in the simplest formulation; the Brout–Englert–Higgs mechanism could act even without the existence of a physical particle. Let's not go here into difficult details.

Be or not be the Higgs boson – there must exist a mechanism that would explain mass… This fifth interaction is very much needed but it strongly implies venturing beyond the Standard Model.

AMK – *First, please, a question what happens if the Higgs boson is not found?*

SP – First, surely the Standard Model shall prevail. In this case it will survive in such generalized meaning I talked about earlier. There are strong arguments, based not on model speculations but on fully basic theoretical aspects, for expecting that a reason for the existence of mass should be found at energies accessible with the LHC, with or without a Higgs particle . Yet, frankly speaking, it is the most probable that the Higgs boson will be found.

AMK – *At the 2007 Epiphany Conference in Cracow you talked about possible versions of the Brout–Englert–Higgs mechanism, other than the simplest one…*

SP – The Higgs or no Higgs – it does not mean very much to the Standard Model but it is of great importance to the direction of thinking beyond the Standard Model.

On the one hand the task facing the LHC is to complete the Standard Model by understanding the mechanism of mass generation. On the other hand, the LHC mission is to open experimentally a new chapter about which at the moment we, as a matter of fact, know nothing.

We touch here upon the situation we have been facing so far, in contrast to going beyond the Standard Model. As I said, while to date the evolution in elementary particle physics was the logical consequence of experiments and theoretical considerations of problems linked to those, then we have so far no experimental indications as to what physics exists beyond the Standard Model. In other words, to the same extent that, within the last several dozen of years, the experimental results from all those big accelerators were confirming the theoretical expectations, the results from experiments with the LHC – apart from the expected discovery of the Higgs boson – are totally unpredictable. However, any understanding of the mass generation mechanism will be a solid experimental indication as to which of the theoretical speculations – that we, among others, are working on, have a chance of proving true.

Venturing beyond the Standard Model

AMK - *Apart from the unconfirmed mechanism of mass generation, are there any other unexplained or open problems within the Standard Model?*

SP – From the experimental viewpoint the Standard Model agrees with everything that so far has been measured in the laboratories on the Earth. Empirical indications that there must exist physics beyond the Standard Model come from cosmology and astrophysics. Clearly, these are extremely interesting aspects of physics.

AMK – *You have entered the Standard Model at an advanced stage of its development. Did you, in the late 1970s, already think about venturing beyond it?*

SP – I have to admit once again that in 1978 at CERN I discovered for good the beauty of quantum field theory – not of the axiomatic one, but the one applicable to the theory of elementary interactions. You may remember, Andrew, that earlier in our conversation I called it a breakthrough in my scientific activity. After I had returned to Warsaw, while trying to understand it further, I decided to write up in a textbook what I had learned and understood of quantum field theory. But, as it usually happens, I began actually writing the book only two years later, during martial law in Poland, when there were no telephones, no scientific and non-scientific journals available. In these circumstances I started writing my *Gauge Field Theories*. I finished the book in 1985. It was published in 1987 by Cambridge University Press; its revised edition appeared in 2000 [2].

It is my pleasure to mention here the strong support I enjoyed from Peter Landshoff of University of Cambridge, who at the time was the Editor at Cambridge University Press. He patiently read the subsequent pieces of the manuscript. Also, I should like to acknowledge gratefully the very thorough reading of large parts of my book by Howard Haber of University of California, Santa Cruz, and – obviously – the great help I received in the

[2] Stefan Pokorski, *Gauge Field Theories*, Cambridge University Press 1987, 2000.

process from my younger colleagues of the Institute of Theoretical Physics, University of Warsaw.

Now, returning to your question… This book, *Gauge Field Theories*, consumed four years of my life. It was time spent very well because in the process I learned a lot concerning physics beyond the Standard Model. I realized that, beside quantum chromodynamics, I had not valued enough the unified electroweak theory that appeared fascinating. Consequently, quantum chromodynamics receded into the background in my research. True, one of the chapters of my book is on quantum chromodynamics but the rest of the book is on quantum field foundations as applied to elementary interactions. For me, trying to make any refinements to electroweak theory would be too late in the sense of being original. It became obvious to me that – having learned as much in depth as it was possible about the unified theory of electromagnetic and weak interactions – the natural thing would be to work on the expected extensions to this theory.

AMK – *For years now, you and your group – the Pokorski Group – have been working on physics beyond the Standard Model. Would you please, Stefan, to elaborate on this?*

SP – We were lucky! As usual, one has to be lucky to achieve something…
As a counterbalance, it is perhaps worthwhile to return first to the late 1960s and the early 1970s. As I already mentioned it several times, joining the constructing of the Standard Model was not easy then. To tell the truth, I remember well situations in which I could have entered quantum field theory much earlier – but I did not do it. For instance, in 1969 during my first long term stay at Cern, I overlapped there with Chris Llewellyn-Smith and David Gross [3], similarly young post-docs. We were good friends with Chris and even collaborated a bit. However, Chris and David already mainly worked on something that later proved to be fundamental, i.e. on nucleon structure functions and deep inelastic scattering whereas, at the same time, I was preoccupied with phenomenology of strong interactions while collaborating with Leon Van Hove. I repeat what I've already said here earlier: Leon Van Hove was a fantastic man, my collaboration with him played a fundamental role in my scientific life but… But sometimes I regret he did not work on the deep inelastic scattering!.

Sometimes, also regretfully, I recall that just after my master, in 1965, I was intensively learning weak interactions and current algebra for a long series of seminar talks in Warsaw. I even wrote on that subject an article in Polish, in a journal published by the

[3] The winner of the Nobel Prize in Physics in 2004. (AMK)

Polish Physical Society. But, either I did not recognize the importance of that research or had not enough courage to pursue it alone ….I joined the multiparticle Warsaw party.

AMK – *I recall the late Jan Łopuszański regretting his missed chance in 1971 to theoretically discover supersymmetry while at Stony Brook, NY.*

SP – Unfortunately, such missed opportunities happen and they happen not so rarely. The problem is that sometimes one does not have enough courage, imagination, or he/she lacks the ultimate thought. One has to forget such defeats and go on. On the other hand one has to do justice to those who click in the right moment, those who immediately understand the deeper meaning of some facts.

Returning again to your question about the situation 20 years later, i.e. in the late 1980s and early 1990s… As much as we had really missed the Standard Model in the late 1960s and the early 1970s, we were lucky later and in the right moment we joined theory beyond the Standard Model. I worked on these subjects with my present colleagues who in those years wrote their MSc theses or started work on their PhD theses. It was then that we worked mainly on supersymmetric theory.

AMK – *Supersymmetry was introduced in the early 1970s. By Wess and Zumino if I remember correctly…*

SP – True. However, in 2012 supersymmetry still remains an open problem. One thing should be said once again. I can see a fundamental difference between the situation at the time when Standard Model was being developed and the situation when we went beyond the Standard Model. Then there were experimental data available in the relevant energy range, while for going beyond the Standard Model there have been none.

AMK – *Simply because there has been nothing yet to collect the data indicating the presence of a new mass scale in particle physics?.*

SP – That's right. There are only theoretical arguments. This will hopefully change, though certainly slowly, with the advent of the Large Hadron Collider.

On Physics Beyond the Standard Model

AMK – At energies higher than those covered by the Standard Model things should happen. The answer to the question "Is there 'life' beyond the Standard Model is, I understand, "Definitely, yes". There are scales of different names spreading over many, many orders of magnitude, in length, energy, time, etc.

SP – Affirmative. That's the whole point. That's why physicists are so excited with the physics of elementary interactions. It is damned interesting… Perhaps for the first time in the centuries-long development of physics, basically nothing is known what happens there. What is known is that t h e r e i s something there. I repeat: If the existence of the Higgs boson is confirmed it will be a clear indication as to which way the thinking about physics beyond the Standard Model should proceed. Everybody's got a bet on some numbers – as in roulette gambling.

AMK – *Would you please sketch out the relation of all that to cosmology. It is a fascinating aspect of elementary particle physics.*

SP – At this stage of theory's development the relation of particle physics with cosmology is obvious; we could talk long about it. The Big and The Small are combined, bound together. Simply because at one "point" (here I use the term "point" colloquially) The Big was The Small and it was the laws of elementary interactions that have determined the present structure of the Universe.

There are two cosmological observations available to us: the existence of dark matter and the lack of antimatter in our Universe. These two facts are very difficult to explain. There is no explanation for these within the laws of the Sandard Model. In particular, the existence of dark matter, confirmed by many independent analyses of various astrophysical observations, indicates that in Nature we rub shoulders with there must be a particle which does not appear in the Standard Model. Dark matter in the Universe implies the existence of a matter of yet another kind – not yet discovered by us; a matter that interacts weakly or is too heavy to have been discovered in the experiments we've been able to carry out to date. Dark matter particle is as stable as proton is – it does not decay. The trouble with it to us is that it does interacts only weakly and not electromagnetically, and thus we cannot trace it so easily. Supersymmetric theories came out with a candidate for such a particle, frequently called

WIMP[4], suitable to being dark matter. If supesymmetry is sensible such a particle should be discovered in experiments with the LHC. No certainty, however – as generally nothing is certain…

AMK – *We're talking here a totally new physics…*

SP – Sure thing, We're talking here a totally new physics. Most probably, totally different interactions or totally new symmetries exist.

Andrew, let me start with a question which you've clearly been orbiting here: why since more than thirty years do people think about physics beyond the Standard Model? When I was an undergraduate, in my textbooks someone was frequently quoted, as saying (at some time and somewhere) that electron is inexhaustible. As metaphoric as it may sound, that man was right…

First of all, there are those two most important astrophysical indications… Apart from this, people think about physics beyond the Standard Model because many of us cannot imagine that we will ever accomplish the comfort of the theory of everything. One may expect the next theory in the chain of theories but not the ultimate one – it is perhaps a general philosophical statement. A more solid argument is that there exist in the Standard Model unresolved problems – theoretical ones, not experimental. An expansion to supersymmetric theory resolves – to a large extent – the theoretical problems in the Standard Model.

AMK – *What about supersymmetric particles other than the WIMP? Are there any predictions for those?*

SP – Supersymmetric theories predict that all supersymmetric particles other than the WIMP quickly decay.

AMK – *All right, it is supposed to be a single one stable particle… In "our"matter, however, stable particles are more numerous: proton, electron, neutron bound in atomic nucleus and in neutron stars. Even free neutron decays only after 10^3 seconds. Why "our" particles are privileged?*

[4] for Weakly Interacting Massive Particle.

SP – Believe me, in each case there are good resons for that! But the majority of „our" particles, described by the Standard Model, is short-lived and they do decay. After all these are not the particles which *sensu stricto* "our" matter is built of.

AMK – *But… the effect thereof is that „our" matter does exist…*

SP – You know, Andrew, it is a difficult question. Some of "our" particles need these other particles to combine everything into a consistent theory. And this has been confirmed experimentally. On the other hand, there is lingering the famous question asked by Isidor I. Rabi in 1937 when the muon was discovered: "Who ordered the muon?". There still is no answer to this question. It can now be reformulated: "Why are there whole families of particles looking very similar that are not necessary for the structure of the Standard Model?". However, without the existence of at least three such families there would have not been CP symmetry violation in Nature. But that is another matter…

AMK – *If some particles are not needed to explain elementary interactions, then – perhaps – their role is to enable the particles that "stay" just to stay…*

SP – In a very indirect way. We touch here upon one of the fundamental questions about the reason for which three families of leptons and quarks exist – three sets or three almost identical matrices – templates that differ only in masses of elementary particles. There is no answer to this question in the Standard Model. And there will be no answer to it in any extension of the Standard Model that the results from the LHC may point to. Most likely the answer to this question would have to be sought at much higher energies in a Grand Unification Theory. Such question could also be asked in a more limited range, within one of these three matrices, the lightes one, that even does not exist in full in our body. These particles, experimentally confirmed except for the Higgs, exist to ensure a coherent theory of interactions.

Supersymmetric particles constitute or rather imply the existence of a matter of a new kind. Supersymmetry, should it exist, would allow avoiding some theoretical problems in the Standard Model. One supersymmetric particle, the WIMP, would explain dark matter in the Universe but it emits no electromagnetic signal.

AMK – *That's why we cannot detect it directly…*

SP – But indirectly we could. Neutrinos do not interact electromagnetically either. However, we can see neutrinos indirectly, i.e. in the context of the missing energy of particles produced in a final state. Methods of such indirect neutrino detection have been well developed .

The evidence for dark matter is indirect, too. The LHC will access the supersymmetric scale. If supersymmetric particles really exist – in the forms we now think they do – with all the reservations, of course – they should be discovered in LHC experiments.

AMK – *It seems most likely that the Standard Model is an approximation of a broader theory. It is almost obvious if one ponders inductively on the ladder of physical theories. But, but… is there yet any real framework of such a broader theory?*

SP - We come back here to the turning point in particle physics I mentioned. There is a whole set of theoretical concepts. However, since there are no indications other than cosmological ones, that are quite general anyway, as they say there is something but unspecified something , the problem is not whether or not there is a solid framework of such a theory but the problem is that there are too many such frameworks… And none of them is convincing beyond reasonable doubts. This notwithstanding, I bet on supersymmetric theory as being most likely. But just a bottle of wine, not more!

AMK – *People say quantum electrodynamics is a low-energy approximation to the Standard Model. If so, one floor above the Standard Model there is…*

SP – Why not? It is precisely the way of thinking of those who apply to physics the "bottom up" approach.

AMK – *OK. Yet where will it end?*

SP – Oh! Why should it end anywhere. I see no reason that it should end at all.

AMK – *True.*

SP – A caveat, however. There are two ideas floating around among some theoretical physicist, which personally I don't share and don't like. One is the anthropic principle and another is quest for theory of everything.

On the anthropic principle and string theory

AMK – *Well, Stefan, if you brought those up, let us, please, talk about the two. What is your opinion about the anthropic principle? It was Steven Weinberg, after all, who affirmed it in a paper of his in 1987.*

SP – I think the anthropic principle is a destructive approach to scientific research. Let's imagine that Copernicus believed in the anthropic principle or that since his times the anthropic principle was widely believed in. Then our civilization, based upon science, would simply not developed at all.

I think the anthropic principle is an erroneous approach to science because the paramount goal of science, including psychology, is to try to explain possibly the largest number of processes, observations, events we encounter in our world, to explain those with the smallest possible number of assumptions and to attempt explaining the largest possible number of casual relation between the processes.

The argument being used by the followers of the anthropic principle goes along lines like this: "In order for us to exist and understand this world the relations in Nature must be as they are." It is true that otherwise we would not exist, but it cannot be inferred from this that the relations must be just such, because we do exist.

We exist because the laws of physics are such that we could emerge given just such Nature constants. This does not imply at all that the Nature constants must be such as they are and that we must exist. We can try to understand why it happened just so – at least we have made a huge progress towards understanding of this – but claiming that it must be so, because otherwise we would not be around, makes no sense.

Some are saying: "Let us explain all we cannot understand with the anthropic principle." This is not serious to me.

AMK – *Someone called it an "intellectual surrender".*

SP – That is on the one hand. On the other hand, I do not think that explaining everything to the very end is the heart of the matter. I think, cognition, understanding things is an advancing process but the dream of understanding everything is an Utopian idea.

String theory was to be the theory of everything. It was a great discovery of the 1980s. Its beginning seemed fascinating, it had its roots in theory of strong interactions, in the Veneziano model I already mentioned here. However, later string theory detached from its genesis and stood alone as a theory of elementary interactions in which gravitation is included in the entire scheme of elementary interactions *en par* with the remaining three interactions.

Obviously, it would be fascinating to solve the quantum gravity problem in such a context, i.e. to unify all interactions. Looking at the evolution of understanding the elementary interactions, I still believe that gravity should be considered in the context of all other interactions and that one day it will be shown that gravity is part of the quantum picture.

AMK – *Grand Unification may be an Utopy, too. Simply by being behind the threshold of difficulty.*

SP – May be. For me, however it seems most natural that at some moment in future we will be able to describe Grand Unification.

String theory seemed to have proceeded in this direction but the problem with it is how to derive from it the Standard Model, or in simpler words how to get predictions as to what is seen in experiments. Even if – one day – a final version of string theory would appear – would it be a theory of everything? At the moment it is seen to the contrary: string theory cannot be a theory of everything. It has been calculated that there are 10^{60} or 10^{100} possible string theories. Therefore string theory is a totally ambiguously defined theoretical scheme. If one were to find it with the "first principles" one would have to have good reasons to pick one of these myriads of string theories.

The anthropic principle has been fashionable in string theory, because there is almost an infinite number of possible string theories. Hence some people talk about using the anthropic principle to select some of them.

Generally speaking, such peculiar intellectual fluctuations appear time to time in science, in physics in particular. They appear not only in science…

AMK – *Some physicists endorse the title of an Internet blog on string theory: „Not even wrong".*

SP – A real problem actually exists with string theory because it has not produced any predictions for experimentally measured quantities. String theory is a great concept but it is scholastics of some kind. And – as we all know – at some moment scholastics did not suffice…

AMK – *Roughly speaking, scholastics was, I guess, a long-lived attempt to reconcile theology with the philosophy of observed reality*

SP – This applies very well to string theory. Frankly speaking, believing in string theory is some sort of theology.

AMK – *Well, Stefan… Andrzej Staruszkiewicz once told me just this…*

SP – Eh, Andrew. I can see I am in a hopeless situation. You have already been indoctrinated… but at least in the right direction.

AMK – *Stefan, having heard all that from you, I can't refrain from asking you perhaps a delicate question… Why did you go to string theory at all? In the late 1980s a couple of your graduate students made their PhDs on string theory.*

SP – The answer to your question, Andrew, is trivial. I was interested in what string theory was about, I wanted to learn something new. However, quite quickly I realized that it was not for me – simply, it was too difficult for me. So, first I was curious and then I found it too difficult. And that's all.

On constructing a theory

AMK - *I remember, Andrzej Staruszkiewicz once pointed out that it was only when in the 1920s several people had a fresh look from a "distance" at the available thousands of experimental data on atomic spectroscopy quantum mechanics was born.*

The late Barbara Skarga, a philosopher, told me that frequently the existing philosophical thinking had to be taken to pieces, deconstructed, in order to build a new one.

Is it possible that that in elementary particle physics we face one of these two scenarios?

SP – I think that in every of these two statements there is a large fraction of an objective evaluation of the situation prevailing in physics. I would add here a paramount condition: physics is an awfully difficult subject and one has to really love it not to get disappointed that he/she understands nothing at first. To be specific, in the development of the theory of elementary interactions – even when the Standard Model was being formulated – the first type of synthesis you're talking about played an enormous role: redundant information was to be neglected to filter out the really needed one. Secondly, it was not only a simple decomposition into prime factors but also a great involvement of new ideas and intuition. One must not get stuck in details, but it is not always immediately clear what is essential and what is not.

By the way, the example with atomic spectroscopy is not fully true. After all, it was this spectroscopy that gave clues which resulted in quantization. The spectroscopy of the hydrogen nucleus was essential in that, while the spectroscopy of many other atoms was inessential. The contributions of great discoverers consist in their still unknown sense to notice that what is essential. Not with complicated calculations, not with precise reasoning, but with a stroke of genius of some kind. This happens very infrequently, but gives rise to real breakthroughs.

AMK – *To overcoming the seemingly not crossable thresholds of difficulties…*

SP – Just that. Without Einstein, Feynman and several others there would not be contemporary physics.

AMK – *Let's go a little bit further. Are you a Platonist? Do you think, for example, the unified theory of all interactions does exist as a Platonic Being. Has it to be only reached intellectually or has it to be constructed?*

SP – I mean here but the methodology. I dislike philosophy, my approach to theory in physics is perhaps primitive. From my own experience, I can see – *toutes proportions gardée* – how I solve scientific problems with hard work. Hard not necessarily in the sense that I always think about the solution and work on it, but hard in the sense that I explore, create in my mind the entire background and then in one instance something dawns on me. For months

I seemingly have known almost all elements of the solution, but suddenly the missing one clicks.

AMK – *Is your concept of dimension deconstruction a good example of such a line of thoughts?*

SP – It is one of my better known papers[5]. The term "dimension deconstruction" was suggested by another team od physicists who independently had basically the same idea at the same time[6]. The idea turned out to be quite useful in various approaches to the physics beyond the Standard Model.

On the Length, Time and Energy Scales

AMK – *In one of your review articles, rather a semi-popular one, I found a diagram of the time, length, and energy scales corresponding to basically different physics of elementary interactions. One thing puzzles me. Why there is a broad gap expected between the supersymmetry and the neutrino scales, spreading across roughly ten order of magnitudes in energy and length. The neutrino scale has already been confirmed...*

SP – Here we touch upon fundamental questions that divide particle physicists. There always was the question of the complexity or the structure of matter. Note that physics of elementary interactions developed through discoveries of more and more fine constituents of matter: atoms, atomic nucleus (from which proton was inferred), electron, neutron, quarks. Many people believed (still 25 years ago; their number has declined) that this process will not end, that quarks consist of something tinier, that the series of elementarity continues toward ever smaller entities. However, another part of the physics community believed that there was no good reason to think that the elementary objects are similar in nature to the Russian "matryoshka" doll. These people have maintained that the Universe is eventually built of finite elementary constituents, being the elementarity limit. It now seems that these people are right.

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[5] S. Pokorski, C. Hill, Jing Wang, *Phys. Rev.* **D64** (2001) 105005.
[6] … A ???, Cohen, Georgii…..

AMK – *Anyway… The four „martyoshkas" seem to be too small a number to extrapolate into a longer array…*

SP – True. Moreover, there are basic differences between the "earlier" and the "current" structure. When the elementary objects, from which a larger object is composed, are heavy enough that the mass of the larger object is roughly sum of the masses of these smaller objects, the latter are bound together rather weakly. As to quarks, neither there is any theoretical concept that would indicate an internal structure of the quark, nor there is any experimental indication pointing to it. It is hard to imagine that quantum physics would ever be able to explain such a structure. This also implies the limit of elementarity.

However, it does not mean that more and more particles and interactions do not exist. Among other things, quantum physics infers that the higher the energy and the shorter the length the heavier objects may be virtually produced although they live shorter and shorter. This means that by increasing energy we will be peeping into a "new world", not in the sense of a substructure but in the sense of virtual, shorter and shorter in time, existence of new entities and new interactions.

AMK – *Is it not simply the Heisenberg uncertainity principle formulated for energy and time?*

SP – Of course, it is. The natural consequence of quantum physics is that the higher the energy the larger the number of objects and perhaps of interactions. And it is in this sense that I do not envisage the bottom up approach (not the scholastic one!) come to an end.

When we reach the Planck scale, i.e. a length scale of the order of 10^{-35} m, the gravitation becomes a strong interaction and the dominating one, and we may find there something new. But experimentally we'll never get there. At the most we may "earlier" see proton decay. If we observe proton decay, e.g. $p \to \pi^0 e^+$, we may learn something about the grand unification scale.

AMK – *I've asked you about the possible desert or a jungle above the supersymmetric scale…*

SP – OK. We come back to the question about physics beyond the Standard Model. In the particular diagram you refer to the concept of the supersymmetric scale is shown and not much above it for many orders of magnitude. Up to the neutrino scale there is a desert. But in an opposing concept there is a jungle there. As you mentioned, I talked about these two concepts in my talk at the 2007 Epiphany Conference in Cracow.

The Large Hadron Collider is expected to indicate which one of the two scenarios – a desert or a jungle – has actually been realized in Nature. If the elementary Higgs boson does exist – it is rather the desert, if there is something more complicated it may be the jungle.

And why do I say that in the supersymmetric concept there is a desert above it? Obviously it is a hypothesis, yet one that consistently results from some aspects of supersymmetric theory. Namely, supersymmetric theory has this – let's call it – virtue that quantum field theory can be applied up to very high scales and still produce clear predictions. One of such predictions is the grand unification, i.e. the unification of electroweak and strong interactions – but on the condition that below is a desert. Should there be other interactions and particles in this lower region those would spoil the simple picture of the unification of all interactions apart from the gravitational one. As I said, should we observe proton decay it would be strong argument for grand unification.

AMK – *Perhaps you would, please, also comment on the neutrino scale?*

SP – Again it is still a hypothesis. Experimentally, it was discovered that neutrinos have non-zero masses, different for different neutrino flavours, and that their masses are much, much smaller than the masses of other elementary particles.

The most elegant theoretical explanation is via the so-called see-saw mechanism that in the simplest words says this: the reason for something being very light and hence very high on a see-saw is that there exists something else, very heavy, that keeps the other see-saw end very low. Assuming the see-saw mechanism, the neutrino scale is determined by the neutrino mass.

AMK – *And what is that „heavy" mass, called M_R, supposed to be?*

SP – This heavy mass is supposed to be a chargeless particle, similar to neutrinos apart from its very large mass. Such a particle would fit as a supplement to the Standard Model.

However, every new mass scale in Nature, particularly so different from the known ones, bothers us also from the philosophical point of view (although I said I disliked philosophy) simply because it imposes on us troubling questions: “Where do so many and so different mass scales come from? Where do such a variety comes from?” For example, why do the W-bosons and the Z-bosons have such masses just as they have? So, not only why do they have masses at all? The latter question may get answered if the Higgs boson (or something else) is found with the LHC. But the question, “Why do they have just specific masses?”, will remain. The quest for understanding the values of the masses is at the core of all those crazy but interesting ideas going beyond the Standard Model that currently engage the majority – I think – of particle physicists.

We have elementary particles – quarks, leptons and bosons – that have some mass distribution; and, in particular, one of the quarks is heavy. And this is what theorists try to explain one way or another. But suddenly the neutrino appears that is a million or more times lighter than the electron, and, together with the neutrino, another problems surfaces: Why is there a so much different mass scale? The see-saw mechanism attempts to explain it by saying: the small neutrino mass (m_ν) is the quotient of a mass of the order of the squared W-boson mass (M_W^2) and an yet unknown heavy (large) mass M_R.

AMK – *Do you think grand unification is attainable to us, or perhaps it is beyond the threshold of our cognition capability?*

SP – Andrew, this is a very interesting question. Any answer to it depends on what “windows on the world” we have at our disposal. One such a window is to build bigger and bigger accelerators and look with a “naked eye” at what happens at higher and higher energies.

AMK – *Right so, but then we should increase the reachable energies by 10-12 orders of magnitude. C’mon, Stefan, we’ll never get there…*

SP – Just that! Just that! With bigger and bigger accelerator we’ll certainly get beyond the Standard Model. The question remains whether we have or will find a “window on the world” allowing us to probe the world that far away… It is now possible to look quite systematically through those windows. It is already known how to look at this world. And this is being done by physicists. It is extremely promising that we, indeed, have such a window… The mass of neutrinos is such a window on the far-away world, but not sufficient to make a great progress… Proton decay would however be another such window…

In our conversation we've been returning several times to your somewhat earlier questions. Here we do it again, as to possibilities to construct grand unification theory. All this is related to the violation of various symmetries that are conserved in the Standard Model . So once again venturing beyond the Standard Model. If there exist processes that under some conditions violate some symmetries of the Standard Model – and such a symmetry is the baryon number conservation enforcing stability of the proton – it would be one of these windows on the world that could tell us something on the far-away scales.

If one day we find experimentally that proton does decays it would be a real breakthrough. Then theorists will start constructing – within quantum field theory a consistent grand unification theory. Contrary to appearances, it would not be terribly difficult because proton decay would be such strong a constraint allowing for only a very small number of possibilities.

On organizational matters

AMK – *Let us dwell for a minute on the more standard matters… For nine years you were director of the Institute of Theoretical Physics of University of Warsaw. You were then the leading force in the expansion and the refurbishment of the Institute of Theoretical Physics building at Hoża Street.*

SP – It was a sidetrack activity. I am much more proud of my nineteen former graduate students whom I promoted to PhDs. Sixteen of them keep working in academia.

AMK – *You have created here a scientific Group working in theory of elementary particles. You say, you acted as thesis advisor to 19 postgraduate students. Clearly you have established here your school of research in this field.*

SP – You see, Andrew, I would prefer that you ask this question other people, not me. Let them answer such a question. For sure, I created here a rather coalesced research group, but please don't use big words. They are very talented and outstanding people, known all over the world for their results in this kind of physics. This group of my younger colleagues clearly exceeds critical mass needed to start good and significant research. And they did it. I have to admit that working closely with them has given me a truly great pleasure, even greater because we have the comfort of the extended office space you mentioned. The existence of this group brings me pride.

AMK – *At present, what is your most important international collaboration?*

SP – Since many years physics of the kind we cultivate has been very „international". To stay afloat in this business, one has to collaborate basically with the whole world. To keep proper proportions it should be made clear that experimental and theoretical collaborations greatly differ in means and funds. Poland's experimental collaboration in high energy physics goes mainly through CERN.

I am a particle theorist. For a particle theorist the most meaningful is the Theory Division at CERN but the whole networks of international collaborations are of great significance, too. The collaboration ties between theorists are exceptionally strong.

On theoretical physics schools and conferences

AMK – *Going back in time, please let me ask you about the Cracow Schools of Theoretical Physics in Zakopane and on the Symposia at Kazimierz upon Vistula.*

SP – After I had graduated with MSc in 1964, I attended the Cracow Schools of Theoretical Physics in Zakopane quite regularly. I still remember well my first talk there. In later years, I attended this School rather infrequently, but in the recent years I again go there more often to lecture. Since 1977 the Symposia organized by our Warsaw group at Kazimierz upon Vistula were in conference style, rather than in school style.

In the early times when both the Cracow and the Warsaw particle physics theory groups were being developed, these two serial meetings served to build bridges to physics in the West. The Cracow Schools of Theoretical Physics in Zakopane that started in 1961 were for some years the only formalized way to start and maintain contacts with research groups in Western countries. Very good, knowledgeable and famous lecturers came invited or on their own initiative. These Schools were absolutely essential to us, both in Cracow and in Warsaw.

Starting in 1977, our Warsaw Group began organizing Symposia on Physics of Elementary Interactions held in the charming little town of Kazimierz upon Vistula. For many years these provided a very significant promotion to us. To the Kazimierz Symposia we owe appearing and getting known in the international community of theoretical particle physicists.

At some later point in time both the Cracow-Zakopane Schools and the Kazimierz Meetings lost their primary importance to the Polish community of particle physicists who have already became known, while – as a result of the political changes in Eastern Block – the international contacts and travels we no longer difficult.

After a few year break in the Kazimierz Symposia, in the early nineties we organized in Warsaw a couple of large international conferences. In 1998, we initiated a series of annual European conferences called in short “PLANCK”. Sequentially, it is being organized in all major particle physics centres in Europe. The Tenth Planck Conference was held in Warsaw in 2007 and the 15th will again be held in Warsaw in 2012.

On personal interests

AMK – *Stefan, What are your interests other than physics? Any hobby?*

SP – In the first approximation, my hobby beyond physics is physics. Frankly speaking, I am thinking about the problems we touched upon here ceaselessly – non-stop. Secondly, physics delivers to me a variety of emotions, sport-like emotions too, because there always is a very strong competition in physics; it drags you in strongly, gives rise to flaring emotions. It does not mean of course that I’m brain-washed in all other aspects.

Sometimes a member of our family asks me: „What does your occupation consists in?’ And I don’t know the answer to it… It is very difficult to explain this. And then comes the next question: “What do you actually do?” It is awfully imprecise what physicists, particularly theorists do. Perhaps the answer should be: “I think. I keep thinking”.

Perhaps, I inherited it from my Mother… I am much interested – but at an amateurish level – in psychology of individuals and societies – in their mutual relations. Sometimes, I read books on this subject and ponder on various phenomena in groups of humans. This is surely the symptom of my side interest. Time to time it helps me in my life.

Based on the conversation on April 12, 2007;