The Particle and the Man: Interview with Michael Peskin

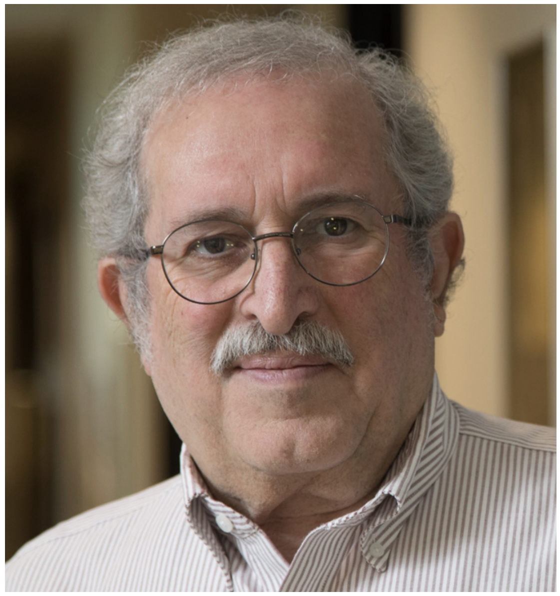

Fig: Michael Peskin SLAC

Michael Peskin is a high-energy physicist at SLAC, Stanford, California. Prof. Peskin is mostly interested in the fundamental interactions of elementary particles. He is the author of the famous textbook 'An Introduction to Quantum Field Theory' with Daniel Schroeder.

Once a PhD student of the Nobel Prize winner Kenneth Wilson, the founder of the Renormalization Program, Prof. Peskin has continued working on the major problems of theoretical and elementary particle physics. In this interview, he recounts his childhood and his life, and we discuss a wide range of subjects from Supersymmetry to quests in a scientific life.

Purnima Tiwari: Good evening, Prof. Peskin. I welcome you on behalf of Anveshanā. We are very glad to have you with us today. So let us begin with your childhood. How was your childhood and did the early formative years shape your interest in any specific field?

Michael Peskin: Well, I had a kind of childhood experience that I think a lot of my contemporary scientists have. We grew up in the suburbs, our parents grew up in the cities. I'm a fourth-generation immigrant from Lithuania or thereabouts. My great-grandparents came over, they worked on a farm. My grandparents owned some small shops in Philadelphia. My parents went to the University of Pennsylvania and became medical doctors, but I wasn't actually interested in becoming a medical doctor. I was interested in more ethereal pursuits and I studied a lot of maths and science and also, poetry and other things. I always thought that I would end up as a scientist. This was a time when the suburban high schools in the US were extremely good, and so a lot of very good scientists who grew up in that era were formed in that way. I always thought that I would become some kind of biological or chemical scientist. In high school, I tried very hard to become a biochemist. And I thought that was the area of science that would really open up as I grew up. Then, I was admitted to Harvard. For two years, I studied chemistry. I learned a lot about quantum mechanics, actually, and physical chemistry. I also learned that I was totally incompetent in the laboratory. So after two years, I decided to change my specialisation to mathematical physics. I started with fluid dynamics and then condensed matter physics. I had this really wonderful advisor named Alan Luther who was guiding me into some of the more advanced topics in condensed matter physics, the behaviour of electrons. At that time, there was a big breakthrough in the theory of phase transitions which was driven by a professor at Cornell named Kenneth Wilson. Wilson actually started out as a high energy physicist; he was a student of Murray Gell-Mann and he worked on problems having to do with many-body physics in nuclear interactions. During the 1960s he developed his own point of view towards quantum field theory, which turned out to be an extremely important breakthrough. And then at the end of the 1960s, he realised that what he had learned was very relevant to condensed matter physics and developed the theory of phase transitions which eventually won him the Nobel Prize.[1] So these developments were happening when I was an undergraduate. Luther suggested that I go to Cornell and work with him as a graduate student. Frankly, it was a fabulous idea, and so I went to Cornell and I was thinking about going into the theory of phase transitions. But just at that time, Wilson decided to go back into particle physics. There were a number of important breakthroughs then. The so-called asymptotic freedom of the theory of the strong interactions (Quantum Chromodynamics or QCD) had just been discovered. He was eager to apply his methods to really understand where strongly interacting particles, hadrons and such, came from. "It's time to solve the strong interactions." After that, there was no turning back. That's how I became a particle physicist.

Aayush Verma: So you didn't decide to do an undergrad in physics and you started with chemistry?

[1] K.G. Wilson, "Renormalization group and critical phenomena. 1. Renormalization group and the Kadanoff scaling picture," Phys. Rev. B 4 (1971), 3174-3183 doi:10.1103/PhysRevB.4.3174.
K.G. Wilson, "Renormalization group and critical phenomena. 2. Phase space cell analysis of critical behavior," Phys. Rev. B 4 (1971), 3184-3205 doi:10.1103/PhysRevB.4.3184.
https://www.nobelprize.org/prizes/physics/1982/wilson/lecture/

MP: Yes, actually, my degree is in a subject called Chemistry and Physics. Harvard gives this degree if you are half and half in both fields. So that was very convenient for me. I've taken a ton of chemistry courses.

AV: What physics courses you started taking there?

MP: Well, I took freshman physics. At Harvard, there were some really advanced freshman courses. There was *Math 55* where you plunge into Abstract Analysis – the abstract maths of Banach spaces and such things – more or less immediately when you walk in the door. There was a similar course in physics. And then there was a similar course in chemistry. So I took the ones in chemistry and maths but I thought taking all three would be too hard. I took a kind of second rank physics course. I can't say I worked on it very hard because I was taking these other really demanding courses. It wasn't until some time later that I caught up with physics. But as I said, in chemistry, I learned a lot of quantum mechanics. I had *Dudley Herschbach* as a professor. The chemists have their own intuitive way of understanding how quantum mechanics works. And I find that a little more congenial than the physicist's way, so I really enjoyed that. And then it turned out that I was really quite well prepared to do physics. (I needed to catch up on mechanics and electrodynamics, which I did on my own, over the summers.) When I went back into physics, what I wanted to do was to take the graduate-level quantum mechanics course. The department advisor, an elderly experimental professor, thought that I was crazy. But at Harvard, if you want to take advanced courses, they let you do what you want, because there are people who are good enough. If you are not smart enough to go right into the advanced courses, it's your problem, not their problem. So in my junior year, I took graduate-level quantum mechanics from *Arthur Jaffe*, someone who's very famous for doing quantum field theory with mathematical rigor. And I had a great experience with that. I really learned a lot. I was hanging on by my fingernails but it got me to where I wanted to go very fast.

AV: Did you start with Mathematical Physics?

MP: Yes, in my junior year. Then in my senior year, I was able to take Quantum Field Theory from *Sidney Coleman*, who's a very famous lecturer on that subject, and that was also a great experience.

PT: Physics often intersects with fields like computer science, chemistry and even philosophy for a fact. How valuable do you think interdisciplinary approaches are in advancing our understanding of the universe?

MP: Well, there are I think two ways that these fields intersect. One of them is through technology. Experiments in physics are extremely difficult and they require very advanced technology of very different kinds, so you really need to know a lot about chemistry, a lot about semiconductors, electrical engineering and computer science, if you are an experimentalis. Please remember though, that I graduated in 1973, so computers were very different then from what they are now. And it wasn't that everyone had a computer in their pocket that could do serious calculations. To be a theorist, you had to know something about computing. You also had to know a lot of maths because a lot of things that would be done now on computers were done analytically on pencil and paper then. I think all these fields have interesting intersections. Now, how these other fields intersect with physics, it depends a little on what you do. If you do condensed matter physics or semiconductors, chemistry is extremely important. I think in my field of particle physics it's much less important. Unless, of course, you're building a detector, which as a theorist is something that I don't do. Every field borrows from every other field. Those people who know how to work across fields often have advantages, but I think my main advantage was having a lot of experience in theoretical condensed matter physics when I went into particle physics because there are strong analogies between those two kinds of physics. At that time there weren't many people who were conversant in both fields. So that's how I got a little advantage.

DB: You went to do PhD at Cornell. Did you have any particular person in your mind to work with before going to Cornell?

MP: Well, as I told you, I wanted to go work with Ken Wilson and that was very interesting. He had a very unique approach to quantum field theory. Since then his viewpoint has become more canonical, but at that time, it was very unusual. And I learned a lot about quantum field theory. He had many unique insights. Working with him was a funny thing, though. I think we never really established a good working relationship. He gave me some problems. I would sweat for a week and try to work out the things he wanted me to work out. And then I'd go and meet him and I'd explain to him what I had done. And he started asking me questions. And the questions would typically have nothing to do with the paper that I was presenting to him. After going away and thinking about it for another day, I realised that those were the questions that I should have been asking given my results. So then, another week and more effort to try to answer the new questions. And again, the same thing happens. He was really on another level. It's maybe a missed opportunity that I didn't pursue that harder but that's the way it worked out.

DB: How exactly did you come in touch with Wilson?

MP: Well, I applied to graduate school. I was by that time a top physics student at Harvard. So in the university admission process, they paid attention to my application. I probably got a very strong letter of recommendation from Alan Luther. So they thought, I was an attractive prospect. And I really did do some good research at Cornell, but mainly it was learning many things, to get from where I began to becoming an expert in both the particle physics and the condensed matter literature. And maybe more learning than actually doing research. But a lot of interesting things were happening in physics at that time. The discovery of asymptotic freedom, semiclassical analysis, instantons, and, the development of QCD. Maybe for you this is all jargon, but it really was a very active period in theoretical particle physics when many

Kenneth G. Wilson (1936-2013) The Nobel Foundation

new ideas were being uncovered. And I really enjoyed just understanding what was the flow of the subject at that time.

DB: Did you work closely with any other faculty too?

MP: Well, I worked a little with John Kogut. Leonard Susskind was a frequent visitor, and I talked to him a lot. The other Cornell faculty were very strong. Kurt Gottfried, Tung-Mow Yan, also David Mermin over on the Condensed Matter side. So I had a great time there. As I said, I didn't really accomplish so much in research, but I learned a lot and it served me well.

AV: Who was on your thesis committee? And do you remember any questions in particular?

MP: It was Wilson, Kurt Gottfried, and Karl Berkelman, one of the experimentalists at the Cornell synchrotron laboratory. Through this lab, I spent a great deal of time interacting with mainly the graduate students, but also some of the faculty, to learn about experimental particle physics. I really enjoyed this. Particle physics experiments are very complicated and it's kind of a black art. But I could get insight by literally crawling around the lab and looking at all the devices and how people used them to measure things. That was a big part of my education. But Karl Berkelman and I never really got along well with each other. I took a particle physics course that he taught but somehow I ended up not doing all the homework. He gave me a project to do which I did in a very theoretical way that he didn't like. He was hoping that I would just find a good paper in the literature and summarise it for him but instead, I made some kind of abstract model of this phenomenon and he thought it was too simple. My committee mainly asked me about the questions in the theory of the pi and K mesons. This material was something that was very relevant to the subject of my thesis[2] but it was more, let's say, phenomenological, whereas my thesis was more mathematical. But I had studied up on that side and was prepared. And they passed me, so I was happy.

AV: You were basically interested in the theoretical aspects, if I'm right?

MP: Certainly I was interested in them, but that isn't what they asked me about on the thesis exam. They thought I had that down pat. So they asked me questions about the experimental consequences. Fortunately, I'd studied this, and knew how to answer them.

AV: And after Cornell, did you have any place in your mind where you wanted to work?

MP: I considered a number of exciting places. I was very interested in SLAC at Stanford. I had met *Sidney Drell*, the leader of the SLAC Theory group, a couple of years earlier at a summer school, and he was a very impressive figure. I was interested in Princeton, and also, actually, I had applied for a fellowship to go to Utrecht and study with *Gerard 't Hooft*. But I got into the Harvard Society of Fellows which is a very prestigious postdoctoral appointment, and that really seemed very attractive. *Steven Weinberg* had just joined Harvard. Sidney Coleman was there. Many very strong theorists were there. It was a great place. Sidney used to say that he was the 'Don Vito Corleone'[3] of particle theory. 'I make them the offers they can't refuse.' And it was certainly true for me. Oh! And just by accident, actually, because I didn't know this in advance… *Edward Witten* was a new postdoc there starting about a year before I got there, and he was a big influence on me and everyone else.

AV: Did you get the chance to talk with Witten?

MP: Yes. Really all the time. Except, he was so fast. We'd have a conversation about something, and a couple of days later, he'd come into my office and it was solved. So I had trouble keeping up with him.

[2] M.E. Peskin, "CHIRALITY CONSERVATION IN THE LATTICE GAUGE THEORY. 1. THE U(1) PROBLEM AND ITS RESOLUTION," CLNS-395.

M.E. Peskin, "CHIRALITY CONSERVATION IN THE LATTICE GAUGE THEORY. 2. DERIVATION OF LOCAL FIELD EQUATIONS," CLNS-396.

[3] One of the main characters in a novel 'The Godfather' by Mario Puzo.

AV: What questions were keeping him busy?

MP: Well then, he was working on non-perturbative aspects of supersymmetry and thinking about $\mathcal{N} = 4$ Super Yang-Mills theory and its consequences, central charges and formal aspects of supersymmetry. I didn't know much about supersymmetry when I went to Harvard. I had to learn because my officemate was Jim Gates. who was at that time one of the very strong young people working in the theory of supergravity. Warren Siegel, who was his collaborator, was also there. So I would talk to them a lot. But that wasn't the direction that I wanted to go in. What I was trying to do was to understand quark confinement much better and make models of that. So in the end, I didn't end up kind of collaborating with these people. But it was very interesting to follow them and I learned a lot of things from them. Again, the more different things you know, the more you can find a way to put these things together in some combinations that are original. I think that's the way that I've been working most of the time.

AV: After Harvard, you decided to join Stanford, if I'm right?

MP: Oh, no, there were a couple of years in between. First of all, the Society of Fellows appointment was three years, but they let you take one year somewhere else. My wife was studying German literature, so she wanted to go to Germany for a year. So I looked at a map of where she would be studying and started drawing circles. Her university was actually not so far from Paris. So I wrote to some people that I had met through my Cornell connections and I was able to spend a year at CEA Saclay, which is a big national laboratory just outside of Paris. At that time, the theoretical physics group at Saclay was very strong. If you know about this quantum field theory, you might know a famous textbook by Claude Itzykson and Jean-Bernard Zuber, and they were both on the staff then. The great physicist Édouard Brézin was also there. And so there were a number of people that I worked rather closely with. I think in the end, I wrote a paper with Itzykson and Zuber, the title of the paper was "The Roughening of Wilson's Surface".[4] And it was about a problem in lattice gauge theory where there's a phase transition but it is not a phase transition that has to do with quark confinement. It's a kind of epiphenomenon, but quite an interesting one. There's a strong analogy in condensed matter physics, and so we explained it. When I came back to the US, I gave a seminar at Columbia University and for the one time in my life, I met *I. I. Rabi*, the great experimenter and Nobel Prize winner. The title of my seminar was the same thing, the roughening of Wilson's surface, and Rabi, meeting me at coffee before the seminar said, "*Hey, Peskin, how do you polish a Wilson surface?*" So that's my experience meeting Rabi, but I think that the theorists did appreciate my seminar.

AV: I was interested to know what brought you to Stanford. Were there any particular works?

[4] C. Itzykson, M. E. Peskin and J. B. Zuber, "Roughening of Wilson's Surface," Phys. Lett. B 95 (1980), 259-264 doi:10.1016/0370-2693(80)90483-9

MP: After Saclay, I went actually for a postdoc for two years at Cornell, where my wife was finishing up her thesis. Actually, she never did finish her thesis. But we decided it was time to move on. And so I applied for a number of positions and I was offered this position at SLAC that I thought was very attractive. As I said, that place had always been on my list as a really good place to be. And I was a big admirer of Sidney Drell, who was the head of the group. So they offered me a position and I came. That was in 1982, and I'm still here, so I must have had a good time.

DB: Your book with Daniel Schroeder on Quantum Field Theory is very well-known and serves as a great piece of literature. What was the story behind the book?

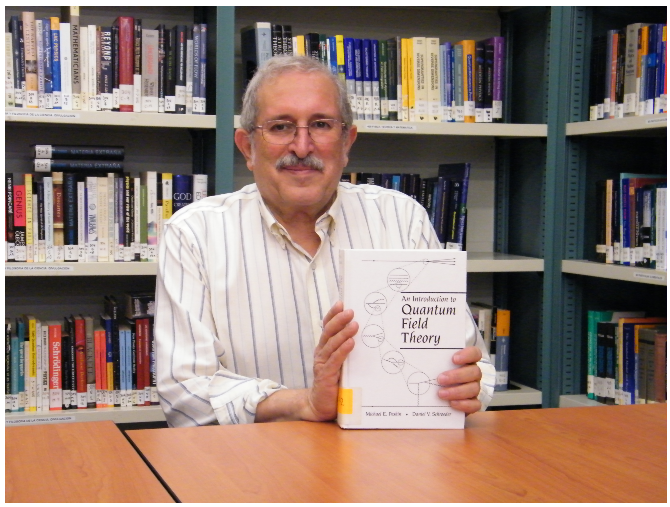

Fig: Michael Peskin in the library of the Institute for Particle Physics/Instituto de Física Corpuscular (IFIC, CSIC-UV) in Valencia Spain holding his classic textbook on Quantum Field Theory. The photo was taken during his visit in September 2016 to deliver a lecture on the Mysteries of the Higgs boson CERN

MP: I'll tell you a little about it. Quantum field theory is a very beautiful subject. I'd studied with the masters, so I really wanted to make an exposition of it. I taught the course at Cornell in those couple years when I was there after my year in France. I taught the course at Stanford, in the 1986 and 1987 academic years. At that time, there was no accepted book on Quantum Field Theory. There was the old book by Sidney Drell and James Bjorken in two volumes, Bjorken and Drell. That was a classic. But, as I said, the 1970s were an important period when people really understood quantum field theory much better and many, many things happened that advanced the field. There was no accepted textbook that was up to date and covered these developments. There was the textbook of Itzykson and Zuber. I tried that for a year at Cornell. My students hated it. It was somehow too French, maybe too dry and rigorous. There was the

Fig: 2018 visit to IISc Bangalore, Peskin having lunch with the students of the high energy physics group. By Michael Peskin

Landau and Lifshitz series book by Berestetskii, Lifshitz, and Pitaevskii. This is a great book with much unique material, but it is idiosyncratic, and my students also found it too difficult. (I'm not sure how Russian students dealt with it, I think by working extremely hard.) But somehow there was no solution for a general graduate textbook on quantum field theory. So I thought that maybe if I had one... I met Dan Schroeder through teaching at Stanford. He was a student in the SLAC theory group but his main interest was becoming a liberal arts physics professor. So I thought this would be a great project for him. I had this big pile of lecture notes, and I gave it to him saying, why don't you just type this up? And we'll be done in a year. In fact, it took us eight years to finish the book. The book came out in 1995, just at the time of the discovery of the top quark, and still, no one had managed to write a book that was a generally accepted modern treatment of quantum field theory. So the book, I must say, was enormously successful. It's still being used, after almost 30 years, and students seem to enjoy it. I travel all over the world and people tell me how much they love the book. It is very pleasing to put together something like that.

DB: Daniel Schroeder also has a book on thermal physics, and he also took that course from you, if I am right?

MP: Indeed, he developed his own approach to this subject. His book is meant for undergraduates. And it is really, I think, the best thermal physics and statistical mechanics book at that level.

AV: You learned Quantum Field Theory from masters like Arthur Jaffe and Sidney Coleman, but when you set out to write, what was the process for you while writing, let's say a book on QFT, where the chances of discouraging a newcomer is very high?

MP: Writing a textbook is very different from doing research. In writing a textbook you are

Fig: 2018 visit to ICTS Bangalore, Peskin giving a talk on topic *"The Search for New Particles at the LHC"*. ICTS

taking ideas that are already well understood by the experts and you're trying to explain them to students. That is a different kind of art. You have to remake the subject so that students can understand it more easily. And I suppose this is something that I happened to be good at. So I thought that if I wrote a book, it had a good chance. And as I say, there was a need in the marketplace for such a textbook, which this one now seems to have filled.

AV: Have you ever been to India? And do you happen to follow any works from the high-energy physics community from India?

MP: I have been to India, but not very often. I've been there three times. I was there for the 2011 international conference on Lepton and Photon Interactions at High Energy, which was held at the Tata Institute of Fundamental Research (TIFR). I gave the summary talk at that meeting. And so that was a very interesting experience. I'd also been there, I think some years before that, I was on a review committee for the Tata Institute and I spent probably three weeks in India between Mumbai and Bangalore. One of my close colleagues, unfortunately now just recently deceased, *Rohini Godbole* was at IISc Bangalore for a long time.

Later, I paid another visit to Rohini in Bangalore. On that trip, I also visited the Tata Institute of Fundamental Research and also the ICTS, which was founded by some of my friends from Tata [TIFR]. So those are the three visits. I never did much tourism in India, it was all about physics. On my first trip, I spent a weekend going to the Ellora and Ajanta caves, but I never, for example, went to the Taj Mahal. *Please excuse me*. Most of these Indian physicists that I know, I'd met in the US. *Spenta Wadia* was a postdoc at SLAC, so I met him in that way. Many of the other leading figures in India were postdocs or spent some time in the United States. Someone that I actually worked with when he was at SLAC was *Ashoke Sen*. Sen was a member of our group at SLAC, just at the explosion of string theory in 1984-85. I don't think we have any joint papers, but we were talking a lot at the time that he was moving

Fig: Rohini Godbole (1952-2024) ICTS

from what he was doing before, which was QCD, into string theory. And so I knew all these people outside of India. It was very congenial when I went to India to visit them.

DB: Can you share more about your interactions and collaborations with Prof. Rohini Godbole?

MP: I met Rohini Godbole at many meetings associated with future accelerators and searches for new particles beyond the Standard Model. In the 1990's, we both became advocates for an electron-positron collider as the next frontier accelerator after the LHC. Through thinking about these questions, we both appreciated the use of measurements of particle polarization to identify new particles and work out their interactions from experimental data. These polarization observables are difficult to measure at hadron colliders, though measurements of the properties of the top quark provide important counterexamples to this statement. But they are very straightforward to measure at e^+e^- colliders (where it is also possible to polarize the initial electron and positron beams), and this provides important and novel information about both Standard and beyond-Standard particles. When I met Rohini, we spent a lot of time discussing these issues. We have only one joint research paper[5] but certainly I learned

[5] R.M. Godbole, M.E. Peskin, S.D. Rindani and R.K. Singh, "Why the angular distribution of the top decay lepton is unchanged by anomalous tbW couplings," Phys. Lett. B 790 (2019), 322-325 doi:10.1016/j.physletb.2019.01.022, arXiv:1809.06285 [hep-ph]

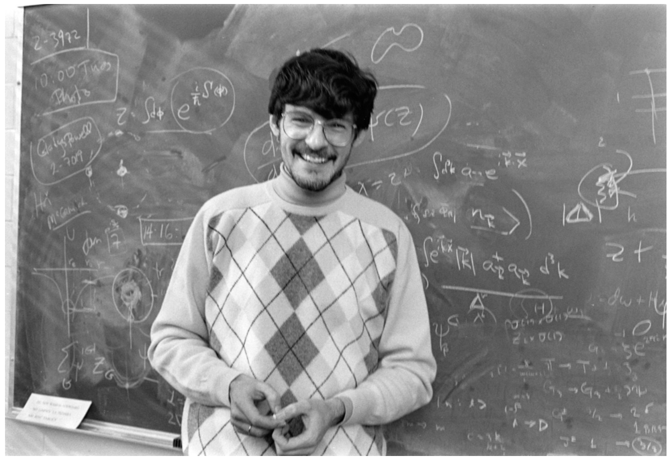

Fig: Emil Martinec in 1991. University of Chicago Photographic Archive

much more from her than this one paper would indicate. She was a gracious host to anyone who passed through IIS Bangalore. Rohini was a rather short woman, but also somewhat wide. I remember being a part of a group, coming home from a conference in Tokyo, that helped her to bring to the airport a pink suitcase that was approximately the same size as she was.

AV: Did Ashoke Sen or anybody try to convince you to do string theory? And what was your take when string theory was just starting?

MP: I had been interested in string theory for a long time. String theory was invented by *Gabriele Veneziano* in 1967 and for a while, it was considered the most interesting approach to the strong interactions. But then it was overtaken by QCD and in the early 70s, it went out of favour. But during the period when it was out of favour, I must say I found it very interesting, because it is an interacting system that you can quantize which generalises quantum field theory. And it's really a very profound theory. So I started studying string theory when I was a graduate student. In 1981, there were papers[6] by the Russian group of *Alexander Polyakov* which gave a new approach to quantizing strings. I got very interested in that, and I worked on this with one of my Cornell colleagues, Orlando Alvarez. Orlando wrote a bunch of papers[7] about that. And I encouraged one of my students, *Emil Martinec*, to work on the

[6] A.M. Polyakov, "Quantum Geometry of Bosonic Strings," Phys. Lett. B 103 (1981), 207-210 doi:10.1016/0370-2693(81)90743-7.

A.M. Polyakov, "Quantum Geometry of Fermionic Strings," Phys. Lett. B 103 (1981), 211-213 doi:10.1016/0370-2693(81)90744-9.

[7] O. Alvarez, "The Static Potential in String Models," Phys. Rev. D 24 (1981), 440 doi:10.1103/PhysRevD.24.440

supersymmetric version[8]. Emil, when he graduated, went to Princeton, and became a member of the Princeton "string quartet" that discovered the heterotic string. So it's a subject I've been interested in for a long time. When string theory came back in 1984-85, I worked hard on it for a few years, but I think I was hoping that there would be very interesting phenomenological applications, given the new understanding. That somehow did not happen, and instead, the subject got very mathematical. And at that point, I dropped out of it and went to work on other things. I'm still a fan of string theory. I think that string theory is likely to be the unique consistent regulator for quantum field theory, but it's going to be a long time before we can really test the stringy predictions of string theory.

DB: Do you believe that intuition plays a significant role in physics? And how do you balance it with the rigorous demand of mathematical proof ?

MP: Everyone has their own level of the relation between intuition and mathematics. There are people who say, 'I have an interesting mathematical problem. I'll do a calculation. Maybe I'll uncover an interesting result and then I can think about the consequences of that.' Other people like to think about the physics, the phenomena or the structures that we use to explain the phenomena, and come up with interesting ideas and then try and find the mathematics to express them properly. For me, it's something in between. I am a believer that these mathematical theories understand more than you do. And so if you do computations, you will uncover things that are surprising, which you can then develop intuition for. I also think that starting from an intuitive basis and trying to find a computation that matches the intuition is a good way to go. And I think in my career, I've done both kinds of things. There are people who are very talented at the purely formal approach. I think the great champion of that probably was Bruno Zumino, the inventor of supersymmetry. There are people who are 'very' intuitive and almost can't do a computation without help, but they know what the answer is. That is very impressive. Leonard Susskind is an example of that point of view. And everyone has to find his place in between.

DB: Well, I believe Feynman has a better combination of both.

MP: You need to realize, though, that these great physicists work extremely hard. We now have the privilege of getting glimpses of Feynman's notebooks. Everything he thought about, Feynman wrote down and seriously considered the consequences of it, and there are very detailed computations in those notebooks. For me, I guess I only do a really detailed computation when I want the numerical answer. I do that much less to try just to understand things. That's a failure of mine, I think.

DB: How has your role at SLAC evolved over the years? Can you share any particularly impactful moments from your work there?

[8] E.J. Martinec, "Superspace Geometry of Superstrings," Phys. Rev. D 28, 2604 (1983) doi:10.1103/PhysRevD.28.2604

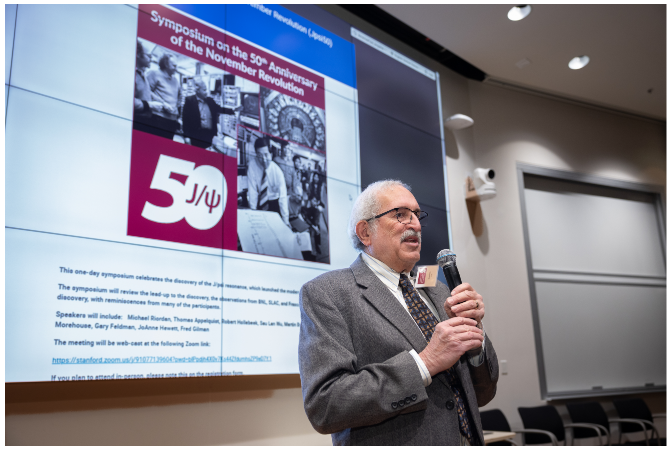

Fig: Peskin at 50th anniversary of the J/ψ discovery held at a week before the interview on Friday. SLAC

MP: The first thing I should say is that I came to SLAC late in the history of SLAC. The really great period for SLAC was in the 1970s. In the late 60s, my future colleagues at SLAC did the experiments called 'deep inelastic scattering', in which they discovered the internal structure of the proton. In 1974, they were doing e^+e^- colliding beam experiments. They discovered the psi particles. That was a tremendous moment when people began to understand that quarks were real. Actually, last Friday, we just celebrated the 50th anniversary of the discovery of the J/ψ particle with a symposium at SLAC. And many of the people who were 30 years old then and are 80 years old now came back to talk about their experiences. It was very moving. At the end of the 1970s, we did experiments on polarised electron deep inelastic scattering at SLAC. Charles Prescott was the leader of that group. He gave the final bit of proof that the weak interaction model of Glashow, Salam, and Weinberg – what we now call the standard model of weak interactions, was correct. And so SLAC was experimentally tremendously influential throughout the 70s. I think after that, much less so. But I came in 1982, so in some sense, I missed the big show. I will never be an old timer at SLAC, in the sense that I was not there during the greatest period. I did get interested in the experiments that people were doing when I arrived, in particular, the electron-positron experiments at the PEP accelerator at SLAC and an accelerator called PETRA (Positron–Electron Tandem Ring Accelerator) at DESY, the electron laboratory in Hamburg, Germany. One very important period for me was the running of a collider called the SLC, the Stanford Linear Collider. On the one hand, to measure the properties of the Z boson, one of the basic quanta of the weak interactions with high precision, and on the other hand, to basically validate the concept of a linear collider for future accelerators. So this is instead of a circular synchrotron, a situation where literally you shoot a beam of electrons and shoot a beam of positrons at it and collide those particles and then in this way, it's possible to reach much higher energies than with the synchrotron.

In 1986, Burton Richter, who was the director of the laboratory, said, 'We'd like to build a linear collider of much higher energy than the SLC, we'd like to understand what the physics is like for that machine.' And so with my colleague David Burke, we organised a study where we simulated events at higher energies, let's say a few hundred GeV, and tried to understand how you would do the experiments and what you would learn. And that's been a large part of my career ever since. One of the things that we learned is that you can produce the Higgs boson very easily, very copiously, and also in a way that it was very easy to recognize. You could measure the properties of the Higgs boson more easily in that setting than in any other that people are talking about. So I've been pushing this idea of studying the Higgs boson and other kinds of exotic particles, at e^+e^- linear colliders since the end of the 1980s. So that's a long time now. Unfortunately, we still haven't built one, but I'm always hopeful.

DB: Well, when did you start working on the Standard Model?

MP: Well, I guess you could say that I have been working on the Standard Model since I was a graduate student. The standard model was new when I was a graduate student. Around the time that the SLC opened, I spent a lot of time thinking about precision weak interactions. And one of the things that I'm known for is the way to use data from these precision experiments on the Z resonance, to try and look for signs of new physics at a higher mass-scale that would slightly perturb the Z. This is something I'm very interested in: How do you use data in a non-trivial way to look for deviations from the Standard Model, and how do you interpret those deviations in terms of possible new physics at higher energies.

DB: So what are your thoughts on the future of the Standard Model? Do you see any particular area that is likely to reshape it or possibly even lead to a new paradigm?

MP: I think that the standard model is obviously incomplete. Let's start with the Higgs boson. In the standard model, all of the particles, including the Higgs boson itself, get mass because the Higgs field is the order parameter of some symmetry breaking. The standard model has a very high degree of symmetry, but the most symmetrical point is unstable. When you go away from that point, the Higgs field gets, what we call an expectation value. It takes a constant value throughout space, and that constant value breaks one of the symmetries of the standard model. This is very analogous to what happens in a magnet or a superconductor. In the magnet, the equations of motion are rotationally symmetric but the spins of iron atoms all line up in the same direction, and so there's a preferred direction, which appears spontaneously. In a superconductor, you start with a normal conductor but then in certain circumstances at low temperatures, the electrons pair up and they provide you a condensed state which can transmute electric currents frictionlessly. This is also described by a kind of symmetry breaking. A long time ago, Yoichiro Nambu proposed[9] that there were such symmetry breakings that

[9]Y. Nambu, "Axial vector current conservation in weak interactions," Phys. Rev. Lett. 4 (1960), 380-382 doi:10.1103/PhysRevLett.4.380

take place in the nuclear forces. In the Standard Model, the whole structure of the model is built on the Higgs field being the agent of such a symmetry breaking. And there are ways to test that this is how particles get masses, for example, you can compare the masses of particles to their couplings to the Higgs boson, which you can measure by measuring the rates of the various Higgs boson decays. And people have done these measurements, to the at the level of 10% or 20%, at the LHC. It's working out extremely well, the Higgs really seems to be the agent of spontaneous symmetry breaking responsible for the masses of all particles. The thing we don't understand is why this happens. The explanation of it in the Standard Model, is simple, but it's totally ad hoc. You put in the values of parameters by hand that you need to get the observed results. And for me, that's just not physics. Somewhere up there should be a new force of nature which interacts with the Higgs boson and forces it to have an unstable potential and condense in a symmetry-breaking way. And for me, this is the most important question in particle physics, maybe even in all of physics. 'What is the new interaction that causes the Higgs to do what it does?' And bound up with that question is the question of the spectrum of quark and lepton masses, the CP violation, many other aspects of the Standard Model are bound up with the behaviour of the Higgs boson. As long as the Higgs boson is just something that we write in our equations without understanding it, we're never going to answer those questions. So there's something out there that we want to find out. I was very much hoping we would get clues to this at the LHC, the CERN Large Hadron Collider. But so far, it seems to be that, though we've discovered the Higgs boson, we haven't discovered clues to its nature beyond the standard model. We have to keep looking for those clues. As I've suggested, maybe you should build an electron-positron collider to measure the Higgs much more precisely. I really feel that the standard model has to break down, because it has missing ingredients, and these must exist in nature. We're physicists, so we have to find them.

DB: Regarding the International Linear Collider (ILC), how do you envision its role in advancing our understanding of fundamental physics? What are your thoughts on its current progress and prospects?

MP: I continue to believe that an e^+e^- linear collider should be the next step after the LHC. Questions about the Higgs boson — why does it have the mass and couplings that it does, why does it obtain a nonzero value throughout space? — are now the most important questions in particle physics. An e^+e^- collider would provide a setting to make very precise measurements of the properties of the Higgs boson, hopefully shedding light on these questions. It would be wonderful to build an accelerator with 10 times the energy of the LHC, but, today, we do not have any workable technology for such an accelerator. But we can learn more about the Higgs boson, and we must.

DB: Since you talked about symmetry breaking, it is a key concept in both particle physics and condensed matter physics, but its application seems to be different in the two fields. Could you explain how symmetry breaking manifests uniquely in

condensed matter physics compared to particle physics?

MP: I don't think it's really different, but in condensed matter physics, it's much easier to understand how spontaneous symmetry breaking works. Condensed matter physics is basically the physics of nuclei and electrons and those are things that we understand very well. So, for example, in a magnet, you can ask, 'Why is it that if you take a block of iron and you lower its temperature, the spins will all line up parallel to one another?' And the answer is that it comes from the atomic physics of iron atoms that are sitting next to each other in the middle that basically the electrons tend to repel. So if you have free electrons, then they like to sit as far from each other as possible. And if you have parallel spins, the Pauli exclusion principle prohibits electrons from coming together, whereas that's not true for opposite spin electrons. So the Pauli exclusion principle and atomic structure, then cause the ground state of a block of iron to have parallel spins. It's not an easy explanation, but it's a very physics-y explanation. Similarly, in a superconductor. *Leon Cooper* discovered the phenomenon of Cooper pairing: At very low temperatures, an electron passing a nucleus can deflect it a little and the nucleus will then have a different force with respect to the next electron that comes by, and that sets up an attractive interaction between the two electrons, which at extremely low temperature can cause them to form a bound state. That bound state is actually a boson, something that does not obey the Pauli exclusion principle, and so these bound states can condense throughout the metal and form a kind of conducting fluid in a metal. Again, it's not an easy explanation. Cooper, John Bardeen, and Robert Schrieffer actually won the Nobel Prize for coming up with this idea.[10] But it's real physics, it is an explanation based on the underlying understanding of the dynamics of electrons. In particle physics, we assume various kinds of symmetry breaking, but again, we don't know why they occur and there's got to be something behind that, I think. So, in some sense, we want to make particle physics more like condensed matter physics by finding the laws that lead to these symmetry breaking expectation values.

AV: What are your current interests and what are you working on nowadays? Moreover, what do you think about the current status of where theoretical high energy physics is and do you happen to follow any work outside of your working zone like black holes, gauge theories, etc?

MP: In the last 10 years or so, I've been pretty well concentrated in my research on the properties of the Higgs boson, experiments to probe the properties of the Higgs boson, and the accelerator physics of future accelerators that might do those experiments. Even more recently, I have been thinking about 10 TeV electron-positron colliders. That is to say, electron-positron colliders that can reach an order of magnitude beyond the energy reach of the LHC. And something that I'm very interested in is this question: How high luminosities can you

[10] L.N. Cooper, "Bound electron pairs in a degenerate Fermi gas," Phys. Rev. 104 (1956), 1189-1190 doi:10.1103/PhysRev.104.1189

get at those colliders? The answer is not so easy to find, because the electrons basically repel each other And electrons and positrons in small bunches strongly interact. So if you imagine such an accelerator, there's a large beam-beam interaction and it's something that actually affects how you design the accelerators, so it's something that I'd like to understand a lot better. More generally, 'What are theories that solve the problem that I asked about? What is the mechanism of the physics understanding of the spontaneous symmetry breaking of the Higgs boson? And how do you make models of that?' So those are the things that I've been working on.

Our group at SLAC is very diverse, and so there are a lot of interesting problems that are outside the area that I discussed. We have, twice a week, seminars where people come in from outside and talk about all these things. So it's great to go to those, I find them really fascinating and I'm trying to learn about these other areas. One of the areas in which we are particularly strong is computational QCD: How do you do calculations in QCD that are at, let's say, at the second, third, or fourth order of perturbation theory? Some of the big experts in that are my colleagues, *Lance Dixon* and *Bernhard Mistlberger*, and they bring in people that they know who work in this area, so I'm learning a lot about it. On the other side, the origin of the dark matter of the universe. I think we have some people in our group, in particular Philip Schuster, Natalia Toro and Rebecca Leane who are very interested in diverse models of the dark matter. They are thinking about how you test these models experimentally, both accelerators and with astrophysical observations. And so, you know, I follow those fields as best I can, although it's not what I'm working on. I think in both of those fields, there's really a lot of progress going on. So maybe we'll see some advances.

AV: What are your thoughts on this year's Nobel Prize in physics that went to John Hopfield and Geoffrey Hinton? Do you follow the works happening in Machine Learning? They are using machine learning in physics, and I believe some of them are using it in experimental high energy physics.

MP: Oh, yes. Actually, some of my close colleagues here at SLAC are interested in machine learning from a variety of points of view. I don't really have any intelligent comments on the Nobel Prize. John Hopfield, of course, is very well known for applying statistical mechanics to a wide variety of systems, including biological systems. It is very beautiful work, and it deserves to be recognized. As far as the computer science aspects, those developments are outside of my area of expertise. Today, AI has taken over many areas of science. One of them is the search for new physics at the LHC. I have a number of colleagues here at SLAC who with amazing ideas on how to use machine learning to better analyse the LHC data and to search for new physics. Personally, I have not played with machine learning tools very much. So I'm probably the wrong person to ask about that.

PT: So in the beginning you had mentioned that you also got into poetry and some philosophy?

MP: Maybe I should just say that I'm not a big fan of philosophy. I studied some philosophy when I was a student, I found it, I don't know… difficult and disappointing in the insights it gave. There's a famous book by *Hans Reichenbach* on the philosophy of quantum mechanics in which he postulates that something can be true, not true, or intermediate — three possibilities — and uses this for his philosophy of quantum mechanics. I always found this to be extremely misleading.

I had the experience when I was a postdoctoral fellow at Saclay of hearing a lecture by the well-known philosopher Bernard d'Espagnat. At the end of the lecture I asked him a question about what quantum spins "really" do. He answered, "You are the kind of person we call a dogmatic materialist." For a week afterward, it was a joke. My co-workers would wave their fingers at me and say, "You dogmatic materialist!". But I feel that, to truly understand quantum mechanics, you must believe that the Schrödinger wavefunction and associated objects such as electron spinors are real. So I am not a fan of philosophical approaches to understanding physics. I think you just have to grapple with the equations and make the best sense of them you can. And, I'm someone who actually believes in the reality of mathematical concepts, in a so-called Platonic point of view that *numbers are real, equations are real*. I believe the Schrödinger wave function is real, at least up to phase. And so this gets me in trouble with philosophers, but I find that it's a very effective way to try to move forward in physics.

PT: Coming back to the magazine, since Anveshanā is striving for a bridge between scholarship and human thought. We would like to know what your interests are, if any, than theoretical physics. Do you have a particular interest in any kind of art? Does it help you to draw inspiration into your work?

MP: I don't know. I've never found…As I've told you, I'm interested in literature. I'm interested in music very much, listening to music, I play the piano very badly but I'm interested in how music works. I see a lot of art when I travel and I enjoy that. I don't find a big relation between that and what I do in physics. It's more of a surface-level connection.

AV: So as a student of physics, or let's say broadly, sciences, one often encounters feelings of discouragement or dissatisfaction with the work and the learning process. So what are your thoughts about this? Do you have, in particular, any advice for young people who want to take a career in physics?

MP: That's a hard question. I think that like all of the sciences, physics requires real devotion. In the US, you know, people talk a lot about what kind of employment do I have after school? Can I get a good job? Can I make a lot of money? None of those things happen if you're very serious about physics. Maybe you can get a comfortable university position, that's about as well as you can do, I think, and it certainly doesn't make up for the hours and hours of work that you have to put in to become a real professional in that subject. So I always tell students that you have to feel that *physics is your calling*. That, the pursuit of science, the pursuit of knowledge is what you imagine your life is going to be. If you're not willing to make that kind of commitment, then you're not going to enjoy hours that you put in doing calculations, building apparatus, trying to do experiments. It has to be that this work gives you joy and brings you closer to what you aspire to. I am sure you appreciate that there is no way that you can do a 50-page calculation and feel joyous the whole time. But you have to understand that it follows a goal that you made for yourself, to improve human knowledge. If you don't feel that, you are not going to succeed. You might as well become a lawyer. So I guess that's the advice that I would give to young people. I think science is very beautiful. I think it's very important that people do it, but it is hard. You have to make a commitment to it.

PT: So we would like to thank you for the time you have given us. And for this beautiful interview that you have given us…

MP: Thank you very much for the opportunity. I hope you found some of this interesting.