Edward Witten

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ORAL HISTORIES

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Transcript

Zierler:

This is David Zierler, oral historian for the American Institute of Physics. It is May 15th, 2021. I'm delighted to be here with Dr. Edward Witten. Ed, it's great to see you. Thank you for joining me today.

Witten:

My pleasure. Thank you.

Zierler:

To start, would you please tell me your title and institutional affiliation?

Witten:

I'm the Charles Simonyi Professor in the School of Natural Sciences at the Institute for Advanced Study.

Zierler:

Ed, just as a snapshot in time right now personally, what are you working on, and what more generally in physics is most compelling to you right now?

Witten:

I have not succeeded very much in getting involved, but what I regard as the most exciting development in the last six or seven years, even a little more, is the role of quantum information theory in gravity. So, I'm looking for a way to become involved there, but I haven't been too successful as of yet.

Zierler:

Who are some of the people who are working in that field whose work you're following?

Witten:

Well, my colleague Juan Maldacena is leading the effort at the Institute. There are important centers elsewhere, for example at Stanford.

Zierler:

And for yourself, what are you working on these days?

Witten:

As I said, I'm trying to become involved in that area. There is something else I'm working on, but what I'm most excited about is what I told you. Actually, I'd like to tell you a little more detail. It's actually the third time in my career that I've suspected something really important might be happening.

Zierler:

This is the third?

Witten:

Yes.

Zierler:

I can take a guess, but of course, why don't you tell me yourself, what were the first two?

Witten:

Well, in the early 1980s, with the work of Green and Schwarz and to some extent Lars Brink, but Green and Schwarz mostly pioneered it, it was beginning to be clear that string theory provided a framework that could make sense out of quantum gravity, at least by eliminating the ultraviolet divergences in Einstein's theory. And it seemed clear that it had a potentially revolutionary impact. I was pretty young, and I hesitated to follow my judgment there. In hindsight, I had a lot of ideas that were on the right track. If I had had more confidence, I could have pursued it more. So anyway, that was the first episode. I've always believed that this period in the mid-1980's really should be called the second superstring revolution, with the first one being the period around 1970. But to my regret, in the accepted counting, the first one wasn't counted, and the period around the mid-1980s came to be called the first superstring revolution. So, the second period in which I sensed something big in the air was what came to be called the second superstring revolution, though I would have preferred to call it the third.

And what happened was that in the early 1990s, there were all kinds of clues of nonperturbative dualities in the air. I don't think I was one of the first to perceive the potential importance of that. But by around 1992 or 1993, I was perceiving it. I remember something that had a big impact on me was meeting John Schwarz at the Strings conference in Berkeley in 1993. At any rate, he was more excited than I had seen him for years. What he was excited about was his work with Ashoke Sen on nonperturbative dualities. And what they had done was interesting and suggestive. You could argue, I think, that they hadn't achieved decisive evidence for their conjectures. That was my view at the time. But there were a lot of things happening that were in that vein. Very suggestive, tantalizing clues about non-perturbative dualities. And this time, I had a little more confidence in my judgment than I had had a decade earlier, when I had had a similar feeling that something big was in the air.

And the third time has been the last six or seven years. It's actually hard to remember the evolution of my thinking (laughter). I reread an interview I had done in 2014 which told me what my thinking was in 2014 better than I could have remembered it reliably (laughter). And what I told the interviewer at that time was somewhat similar to what I'm telling you right now. So, this has gone on for a while, and despite that, I haven't really found the right way to become involved myself. But I do suspect that something big is happening.

Zierler:

What has happened since 2014, when you initially got excited about this?

Witten:

There have been various striking developments, but a particularly dramatic one came in 2019 when there was success in understanding what is known as the Page curve in black hole evaporation, by two groups of mostly younger physicists, although one of them (Don Marof) was a senior figure. Most of the people involved were younger. Lots of things have happened that show that there's a conspiracy between gravity and quantum mechanics. Somehow gravity at the classical level knows about quantum mechanics and statistical mechanics. It goes back to black hole thermodynamics as pioneered in the 1970s by Bekenstein and Hawking and

Gibbons and others. The discoveries in that decade were remarkable, but to me, for almost three decades after that, it did not seem that much was happening in that direction. That had changed by about 2010.

Zierler:

A question we're all dealing with right now, how has your work been affected by the pandemic, by working from home, by not seeing your colleagues in person? In other words, the caricature might be as a theorist you'd be happy to just sequester yourself. But to what extent is that not true?

Witten:

I think it's largely not true, because I think that the stimulation by other people is important. The idea of a theorist shutting himself or herself up in an ivory tower and coming up with an incredible insight is not the way it usually works. It's rare. I kind of illustrated that with what I told you about. In the second superstring revolution, where I was a little bit more successful personally, I got interested because of the stimulation from things that other people were doing. And on some occasions, it helped to talk to them in person. I mentioned one that had a big impact on me was meeting John Schwarz at that conference. Stimulation from colleagues is important. Especially as we get older, I would say, actually.

Zierler:

Is this making you miss, more and more, the intellectual environment of the Institute?

Witten:

Well, by now I'm not missing it, really. The Institute is open, and I could be going there five days a week. Recently, I've been going there three days a week, most weeks. The interactions with people are a little bit weird. When I do go there, I don't see people as much as usual. But if you arrange to talk to somebody, you can do that. And we can meet people casually at lunch. We've had two in-person seminars by now, which were weird, we had maybe twenty, twenty-five people in a huge lecture hall, way spread out. It was a weird way to have an in-person lecture.

Zierler:

Do you have any travel plans, or is that too far afield at this point?

Witten:

I don’t have any physics-related travel plans. I actually just a few days ago returned from a trip to Florida, to visit my father, who recently turned one hundred. And we're going in a couple of weeks to Italy. My wife’s family is having a reunion. There are some of her family we're anxious to see, especially.

Zierler:

So, it’s a little bit of a peek into life back to normal, then.

Witten:

Semi-normal. How about you? Are you close to normalcy?

Zierler:

Our kids are still remote learning, and I haven't been back to my office in fourteen months, but we're on our way. A little bit here, a little bit there.

Witten:

How old are the kids?

Zierler:

Four, six, eight, and ten.

Witten:

I see. Our granddaughters in New Jersey are full time in school. Well, every day in school by now. It’s not quite full time. They go home at 1:00, and they have an hour of remote schooling after that. But they're in person every day.

Zierler:

That’s great. We expect for the kids to go back full time in September.

Witten:

I see, okay. Good luck.

Zierler:

Ed, let’s take it back to the beginning. Let’s start first with your parents. Tell me about them and where they're from.

Witten:

They both were born in Baltimore, with immigrant parents from Eastern Europe. My mother grew up in a middle-class family, I would say. My father grew up in a very poor family and struggled to get an education and ultimately to become a physicist. So, the story of his life is first having to overcome poverty, and second, struggling to become a physicist.

Zierler:

Where did your parents meet?

Witten:

Well, they grew up in Baltimore. They met in Baltimore. I think they were probably introduced by mutual friends, but I'm a little murky right now.

Zierler:

Did your parents grow up in Jewishly observant homes at all?

Witten:

My mother, yes. My father, less so.

Zierler:

Did your father involve you in his career? In other words, when you were young, did you understand what it meant for him to be a physicist?

Witten:

Well, only slightly, I would say.

Zierler:

Where were you born, Ed?

Witten:

Also in Baltimore.

Zierler:

How long were you in Baltimore?

Witten:

With some gaps, until I went away to college.

Zierler:

In what neighborhood? Where did you grow up?

Witten:

It was called Summit Park. The family moved a couple times, but most of the time it was in Summit Park, which was a suburban area in Baltimore County, north of Baltimore. I guess Pikesville.

Zierler:

And in your own upbringing, how Jewishly connected was your family? Were you members of a synagogue?

Witten:

Well, we weren't members of a synagogue. I would say traditional but not observant.

Zierler:

Were you Bar Mitzvah'ed?

Witten:

Yes.

Zierler:

When did you start to engage your father in questions of physics?

Witten:

What I remember most are interacting in astronomy and math. I was very interested in astronomy when I was growing up. Well, I was not an exception; these were the days of the Space Race, so everybody was interested in astronomy. I was given a small telescope when I was about nine or ten. That's certainly a vivid memory. Another vivid memory is learning calculus when I was eleven. My father sort of taught me calculus or gave me materials from which I could learn it. But I didn't advance very much in math beyond that for quite a few years.

Zierler:

Was high school, the curriculum in math and science, sufficient for you? Did your teachers determine that you could go beyond what they had to offer?

Witten:

Well, I'd say in science, it might have been sufficient. In math, certainly not. And they did recognize that and tried to help. There were different philosophies about how children would best develop. I certainly could have learned far more math in those years than I did.

Zierler:

This is to say that you did not go to university-level math classes in high school?

Witten:

I didn't go to university-level math classes, but in my last year of high school, I was tutored at a university level. The last few months, I had a tutor who was possibly a graduate student at Hopkins; I don't remember for sure. I find this a funny discussion; I could have learned much more in those years, but I probably would have ended up in the same place. It's not that it's a missed opportunity I'm regretful about. I was just trying to give you a brief description.

Zierler:

Certainly. Here's an easier question. What schools did you apply to for undergrad?

Witten:

Not very many, but I went to Brandeis, as an undergraduate.

Zierler:

Why Brandeis?

Witten:

It's actually hard to remember at this point.

Zierler:

Did you go there with the intention of pursuing a major in mathematics?

Witten:

It’s actually hard to remember what I intended when I started, but I didn't end up pursuing such a major. I actually regret that more than I regret whatever did or didn't happen in high school. Because I think it would have made a difference. I suspect that I could have learned far more in my high school years, but I don’t think that would have made a difference. Whereas I think studying more math and physics as an undergraduate would have made a difference.

Zierler:

Who were some of the professors at Brandeis who you may have become close with or who were formative in your intellectual development?

Witten:

Really that question doesn't have a good answer.

Zierler:

On the social side of things, being an undergraduate in the late 1960s, were you political at all?

Witten:

Well, in theory, yes. In practice, I wasn’t really involved in anything. But in theory, yes. I was upset about the Vietnam War, for example.

Zierler:

Were there campus protests at Brandeis?

Witten:

There must have been, but I actually have very little memory of that.

Zierler:

What was your major in the end at Brandeis?

Witten:

Well, I don’t want to make you laugh, but I majored in history.

Zierler:

Sorry, you don’t want to make me laugh, but I'm laughing. (laughter)

Witten:

(Laughter).

Zierler:

Why history?

Witten:

Well, sometimes the choices made by young people are inscrutable.

Zierler:

So, at that point, you weren't looking to follow in your father's footsteps and become a physicist?

Witten:

No, it was a year or two later, I decided I wanted to do physics.

Zierler:

How much physics did you take as an undergraduate at Brandeis?

Witten:

Not enough.

Zierler:

What did you do once you graduated? You did not start in graduate school immediately.

Witten:

No, I worked on the McGovern campaign for a while, and then I was a graduate student in economics at Michigan. I quickly realized that that wasn't for me, and changed to applied math, and then eventually physics.

Zierler:

How did you get to work on the McGovern campaign? What was the connection?

Witten:

I started working at the local level, in Massachusetts, during the primary campaign.

Zierler:

What did you do for the campaign? What was your work?

Witten:

Nothing very memorable, to be honest with you. I will say that the most significant part of that was that I realized I wasn't good at it (laughter).

Zierler:

And then initially you thought you would go and become an economist?

Witten:

Yes.

Zierler:

What were your interests there? Did you think that your mathematical abilities would be applied well in that field?

Witten:

It's again hard to remember reliably, but I might have thought that. And I might have also thought that I could make a contribution to international development. But I realized- well, I came to the same realization I had come to when I was working on the McGovern campaign, that it wasn't a good match for me. I remember being very embarrassed when I told the people in the department at Michigan who had been quite kind to me, that I had decided to leave. But in hindsight, I understand something wasn't that clear to me at the time, that if a given graduate program isn't a good match for a given student, the department and the student are both better off if that's realized sooner rather than later. If I had understood that at the time, I would have been less embarrassed, probably, with what I told them.

Zierler:

So how did it dawn on you that it would be applied mathematics and at Princeton? What was that decision-making?

Witten:

You're asking too many questions about things that are too hard to remember, at this stage.

Zierler:

Did you apply broadly for applied mathematics, or only Princeton?

Witten:

Just a few places, I think.

Zierler:

The theme so far is that you were pursuing things that you realized you were not very good at. At what point at Princeton had you found your groove?

Witten:

I think by the time I was a graduate student, I found my groove. But I had to work very hard as a student. There’s a slight irony here. Because of the circumstances in which my father grew up, it was a tremendous battle for him to become a physicist. I potentially had all the advantages but made a little bit of a battle. Not as big a battle as my father had. But I definitely had to catch up when I was a student.

Zierler:

At what point in graduate school did you shift to physics?

Witten:

I think it was at the end of the first year.

Zierler:

Do you recall if it was a class or a professor or your own interests that caused the shift?

Witten:

Well, I took a variety of classes in the first semester, and I found the physics classes both more interesting and more accessible.

Zierler:

When did you first connect with David Gross?

Witten:

By the end of the summer after the first year, I felt I'd learned enough to look for an advisor. And I don't remember what made me go to David. I didn't know very much about what was happening among the professors and their research. I imagine whatever I heard from the other students convinced me to talk to David first. I can't be more specific than that. But I learned a lot from him in the succeeding two years.

Zierler:

Do you remember what David was working on when you connected with him?

Witten:

He and his student Frank Wilczek had discovered asymptotic freedom a few months before, they eventually received the Nobel Prize for that discovery, together with David Politzer. This discovery was pivotal because it suddenly lifted the confusion that surrounded the strong interactions. So certainly, David's interests during this period all had to do with the questions that arose in the aftermath of the discovery of asymptotic freedom, from understanding its predictions for high energy reactions, to dealing with the remaining mysteries like why quarks are confined. Anyway, that was the context for David's work at the time. I remember what he suggested to me as the first thing to work on, which was to refine a calculation he had done of anomalous dimensions of leading twist operators for large spin. And I did. I did a more precise calculation than he had done. I didn't publish it, and later it was done better by other people. Anyway, I learned something from doing that calculation.

Zierler:

Did you spend any time at the Institute as a graduate student?

Witten:

Only because we had joint seminars, between Princeton and the Institute. So, I would be at the Institute for seminars. So, I would have some exposure to people like Steve Adler and Freeman Dyson, when we had seminars. And Steve would come to seminars at Princeton, too. I don't remember if Freeman used to.

Zierler:

Did that first project that David had you do, that first calculation, did that inform what would become your thesis research?

Witten:

Not directly. The specific calculation I did- its application is to studying deep inelastic electron nucleon scattering, in the region close to what's called x=1, that means where the kinematics are almost elastic. Deep inelastic scattering had played an important role in the emergence of QCD and had pointed the way to asymptotic freedom. However, experimental data at the time weren't good enough to make the kind of calculation I was doing really relevant, which was probably part of the reason I didn't completely finish it and publish it. By now, certainly- well, experimental data are vastly better today. Much more refined things are done today in interpreting experimental data. The thesis work I did later, though, involved other applications of asymptotic freedom, which were fun, although not truly pathbreaking. The most significant was a calculation of what's called deep inelastic photon-photon scattering, where I discovered something interesting- well, a slightly surprising prediction of asymptotic freedom, for that reaction. And again, experimental data at the time weren't good enough to test the prediction. By now, it has been tested.

Zierler:

I wanted to ask, as a graduate student, when you say the experimental data was not sufficient, where was the data coming from? What were the experiments that you were aware of when you determined it was not sufficient?

Witten:

Well, it was clearly not sufficient. But to answer your question, how were QCD and asymptotic freedom discovered? They were discovered because of data on deep inelastic scattering, and also e^+e^- annihilation into hadrons. There was eventually a Nobel Prize for experiments that had already been done on deep inelastic scattering, and the data on e^+e^- annihilation had also been very influential. So, there was very important data, and it simulated the discovery of QCD and asymptotic freedom. But it wasn't refined enough to measure the subtle deviations from scaling that were predicted by Gross, Wilczek, and Politzer based on asymptotic freedom. Or maybe they were just barely beginning to see it, at quite a very crude level. But the calculation I did, the first one I mentioned, of the large angular momentum leading twist operators, the data was certainly not sufficient at the time to see delicate properties like that. The same was certainly true for deep inelastic photon-photon scattering, which I studied in my thesis.

Zierler:

In graduate school, were you aware of the earliest iterations of string theory? Were Green, Schwarz, Veneziano, Nambu- were these names that you were aware of in graduate school?

Witten:

The closest I came was the following: 't Hooft, in 1974, observed that the large N limit of gauge theory is governed by planar diagrams, Feynman diagrams that can be drawn on a two-dimensional surface. And 't Hooft observed that that was analogous somewhat to string theory and dual models and suggested that to the extent that dual models in string theory had had some success in describing hadron physics, the reason was that large N gauge theory was equivalent to a dual model. Now, David drew my attention to that paper soon after it appeared. Let me tell you that ‘t Hooft's idea, although unproved to this day, is actually largely believed to be correct. There have been many developments since then that support it, although I'd say it's unproved. More importantly, leaving aside proof, we don't have a useful precise version of the conjecture, just what string theory is supposed to be equivalent to large N gauge theory. But despite that, ‘t Hooft's paper has been extremely important in later developments, stimulating the work of Maldacena, for example. Not that directly, but interacting in an interesting way with the work of Maldacena, on gauge gravity duality. Anyway, it has been extremely important in the way it has influenced the field subsequently.

But anyway, what David asked me to do was too ambitious even today. So, I learned about it, but I didn't make much progress. Now, I knew that ‘t Hooft was drawing an analogy with dual models, but it didn't really inspire me to learn very much about dual models at the time. Therefore, the answer to your question is a little more complicated than a simple yes or no.

Zierler:

Besides David, what other professors did you work with as a graduate student at Princeton?

Witten:

I learned a lot from all the professors. But I didn't really work with anyone except David. In fact, I missed the boat at one point, frankly, because I was invited by David and Curt Callan to join a project which involved exploring the ‘t Hooft model in two dimensions. ‘t Hooft had had this grand vision in four dimensions, but he also had a model that he put forward in two dimensions that illustrated some of the ideas. He solved the model up to a point, but they went much farther in understanding it, with a student of Curt's named Nigel Coote. And I think I missed the boat by not joining that project as I was invited to. Not that what they learned was so important per se, although it was moderately important. But I think I would have learned a lot of physics by being part of that project.

Zierler:

Besides David, who else was on your thesis committee?

Witten:

It is hard to answer because in my mind, I am confusing the thesis committee with the generals committee. Tony Zee was on a committee, but whether it was the generals or the thesis committee, I can't tell you. I only remember a question he asked me, which luckily, I was able to answer.

Zierler:

Do you remember the question?

Witten:

Roughly. Well, I remember the way I answered it, a little better than I remember the question. It had to do with a resonance as a pole in the second sheet. Now that we're talking, I'm starting to remember another member, but I think perhaps again the generals committee. I can't remember his name. Although if you gave me a list of the department at that time, I'm sure I'd recognize it. I do remember the question he asked, which involved a process like an electron scattering from a helium atom, or hydrogen atom, perhaps, and ionizing the atom. Anyway, the answer involved what's called the Lippman-Schwinger equation. I remember I answered by starting to tell him about the multichannel Lippman-Schwinger equation. I recall now that Sam Treiman was on my thesis committee.

Zierler:

Not being a physics undergraduate, starting with math, did you compress all of the classes or knowledge that your contemporaries had from undergraduate, or did you simply skip over part of it, to graduate in time?

Witten:

I think I compressed it. I worked very hard at learning everything that I was supposed to know. I think I learned everything, roughly speaking, that you'd expect a graduate student to know about different areas of physics, for what was known at the time. I think if I had been a physics undergraduate, I would have learned more about many things, and it would have helped me later. But if you'd met me in 1975 or something and compared me to the other students, you wouldn't have said that my knowledge of physics was less. Definitely not, I don't think.

Zierler:

Was general relativity considered popular or interesting at Princeton at the time that you were a graduate student?

Witten:

Well, I was certainly interested in it. I learned general relativity in a very exciting period of about ten days, from the book of Steve Weinberg. I mean, I tried to learn more from the book of Misner, Thorne, and Wheeler, and I did learn more from it, but my opinion of the book was what it remains now, which is that it's got a lot of great stuff in it, but it's a little bit hard to use it to learn systematically. The book I found useful for studying systematically was by Steve Weinberg.

Zierler:

Did you talk to your father about these issues? Was he following what you were doing?

Witten:

Some.

Zierler:

Did you come to appreciate more his research as you understood these topics?

Witten:

Certainly, I came to an understanding of what he was doing in his papers.

Zierler:

Which was what, when you started to pay attention? What was some of the research your father did?

Witten:

Well, he was interested in general relativity, so he had done some work on exact solutions of the Einstein equations, for example.

Zierler:

After graduate school, what did you want to do next? What opportunities were available to you?

Witten:

Well, I went to Harvard as a postdoctoral fellow. I briefly flirted with the idea of going to Cornell, because I was very interested in what Ken Wilson was doing in lattice gauge theory. But finally, I had decided that Harvard was my first choice, so when Steve Weinberg called me to offer a postdoc, I accepted over the phone, without asking any questions, really. By the time the opportunity was there, I had already decided what I wanted to do.

Zierler:

So, it was more the prospect of working with Steve than being at Harvard that was attractive to you?

Witten:

Not only Steve. Steve was one of the attractions at Harvard, but I learned a lot when I was there from all of the faculty. I could really go over the faculty members at Harvard and tell you the things I learned from them.

Zierler:

Please.

Witten:

So, from Steve, I learned to understand current algebra. At Harvard, there were what they called family meetings, informal seminars. They also had formal seminars. But they had more informal seminars with more discussion. Whenever the subject of current algebra came up, Steve would give a little speech about how he understood it. I had been confused about current algebra as a graduate student. I learned a lot of the formulas, but I didn't understand the logic behind them. After hearing Steve's explanation of the logic a number of times, I came to understand that. So, I'd call that the most memorable thing I learned from Steve. From Shelly Glashow and Howard Georgi, I learned a lot about particle phenomenology and model building. And I would have gone in that direction myself if physics had proceeded differently.

From Sidney- what I remember best from Sidney Coleman is drawing my attention to important papers that in some cases were important years later in my work. In some cases, they were important in the more short-term period. But, important papers that I really wouldn't have noticed or known about except for him telling me. I can remember a few of those. There were two papers by Albert Schwarz that were important, that Sidney told me about. One was about the role of the index theorem. So 't Hooft had solved what's called the U(1) problem. I don't know how much you know about the physics backgrounds of these things. Are you familiar?

Zierler:

Of course. I know.

Witten:

He had solved the U(1) problem by using QCD instantons. And it depended on fermion zero modes that had been discovered by Jackiw and Rebbi. But physicists didn't really understand why they existed. Albert Schwarz wrote a paper explaining that these fermion zero modes, which had been so important, were a manifestation of the Atiyah-Singer index theorem. And this was the beginning of physics making contact with modern differential geometry. It also got Atiyah and Singer interested in physics, and it led to all of my later interactions with them. But anyway, I learned about this from Sidney. I also learned about another paper from Sidney, about another paper by Albert Schwarz that came out about a year later, that was eventually important in my work, but not for a decade. That was the paper in which Albert Schwarz reinterpreted Ray-Singer analytic torsion in terms of what would now be called a topological field theory.

Zierler:

Did you publish at all when you were at Harvard?

Witten:

Sure. I published various papers, some of which are not so memorable. Some are more memorable.

Zierler:

How long did you stay at Harvard?

Witten:

Four years.

Zierler:

At what point did you start to consider faculty positions, or did you want to pursue another postdoc before that point?

Witten:

Well, I was getting offers for faculty positions during those four years, so I didn't seriously consider doing another postdoc. I'm not sure what would have happened if the offer at Princeton hadn't come up. The offer at Princeton came up rather abruptly when- I'm forgetting, frankly, if it was because Murph Goldberger or John Hopfield left. One of them left, creating an opening at Princeton, and I got an offer unexpectedly. I think I had been considering offers from Berkeley and Caltech prior to that.

Zierler:

Tell me about your visiting position at Oxford.

Witten:

I visited Oxford, but I didn't have a visiting position there. I visited Oxford for about six weeks in the winter of 1977. Is that what you're asking about?

Zierler:

Yes.

Witten:

You might want to read the interview I did in 2014 because it would help you understand a bit better. But anyway, after the work of ‘t Hooft using instantons and the work of Albert Schwarz showing the role of the index theorem, Michael Atiyah and Is Singer and other mathematicians too, Raoul Bott, became very interested in physics. And I met Atiyah when he visited at Harvard, probably in the spring of 1977, and he invited me to visit Oxford in the following winter, which I did, I think for four to six weeks. I might be misremembering how long it was.

We talked about a lot of stuff. We talked a lot for several weeks. And then one day he showed me two papers, I think, which I hadn’t seen before, by David Olive and others- one was by Goddard, Nuyts, and Olive; one was by Olive and Montonen- on the beginnings of what we now call electric-magnetic duality in non-Abelian gauge theory. And he recommended that I go to London to talk to David Olive. So, I did.

And by the time I got to London, I would say, I was very skeptical about what they were saying. The more precise conjecture was by Montonen and Olive in the second paper, but technically what they were saying really couldn't be right for various reasons. So, by the time I got to London, I was very skeptical. But some of the technical objections didn't apply in supersymmetric theories. So, David and I ended up talking about that, at length. And by the end of the day, we had realized that their conjecture actually made perfect sense in the supersymmetric version of the theory. So, we wrote a little paper about that which probably was the most significant paper in the postdoc years. Anyway, that was memorable.

I drew the wrong conclusion from it, as I explained to the interviewers in 2014. We had actually been able to explain the Montonen-Olive formulas without assuming their main duality conjecture. Montonen and Olive had had some formulas which were striking, which they offered as evidence for electric-magnetic duality. In a non-supersymmetric theory, you could see that what they were saying couldn't really work. But in a supersymmetric version of the theory, we were able to show that the conjectures were actually true quantum mechanically, not just classically.

And a conclusion an optimist could have drawn from that was, “Oh, the prediction of the duality is true. Probably the duality conjecture is true.” But I was very conservative, and the conclusion I drew was the opposite. The conclusion I drew was since we're able to prove these formulas without assuming the duality, the formulas are not really evidence for the duality. There's no evidence for the duality, and it would be very hard to determine if this duality was right. So, I reached that conclusion and didn't continue with the subject for roughly fifteen years, until the period I told you about a little bit earlier, in the early 1990s when there were many new clues about electric-magnetic duality.

By the time I talked to John Schwarz at the Berkeley conference in 1993, one could have reacted the same way to his work as I had in the 1970s, to my own work with Olive. They had suggestive formulas but they didn't have decisive evidence. But by this time, I was a little bit more open to believing that these crazy ideas might be right, and maybe would be useful. Anyway, in that second period, I went back to it, finally.

Zierler:

Ed, did you enjoy the Harvard Society of Fellows, the social aspects of it?

Witten:

Well, I enjoyed it up to a point, but let's just say that many other people thrive on that more than I did.

Zierler:

(Laughter) Tell me about your initial work when you got to Princeton. What were you working on at that point?

Witten:

There were a few things. I got interested in supersymmetry, supersymmetric models. Well, I wrote a few papers in those years on supersymmetry that were influential. And I also got interested in Kaluza-Klein theory. So Kaluza-Klein theory is the idea of a unified theory where gauge fields arise from gravity in a higher dimension. The idea behind Kaluza-Klein theory in modern language is that the world is really perhaps five-dimensional, but in a kind of spontaneous symmetry breaking, one of the dimensions compactifies to a circle. And then at low energies, you get gravity and gauge theory. So, this was actually one of the ideas Einstein considered for unification of electromagnetism and gravity. Of the ideas Einstein considered, this is the only one that's considered of interest by contemporary physicists.

Einstein had another theory which you can read about in his book on the meaning of relativity. You'd find, if you asked around, that not many physicists are even familiar with it. But the idea of extra dimensions, that was Einstein's other idea, is interesting even today. But if such compactification occurs, why does it occur? You might ask whether maybe there's an energetic reason that this occurred. And that might make you start asking, "Well, what does energetics say in general relativity? And is Minkowski space in any dimension stable?" That's one of the most basic questions.

So that question leads to what's called the positive energy question in general relativity. Although you can define energy in general relativity and you can read in the textbooks how to do it—the modern version, I guess, was given by Arnowitt, Deser, and Misner- it's not straightforward to show that energy is positive. And there was a proof by this time, by Schoen and Yau, two mathematicians, Rick Schoen and S.T. Yau, but very few physicists understood it, and certainly I didn't. And since I didn't understand the proof, I couldn't find out whether it was applicable in other cases like Kaluza-Klein theories.

So, all that motivated me to look for my own proof, which I did. I had been aware of a paper a few years earlier by Jackiw and Deser. Basically, they said that in supergravity, energy would have to be positive, because the Hamiltonian was the sum of squares. And then they said, "Unfortunately, we can't take the classical limit of this statement to prove positivity of the energy in pure Einstein theory." In a later paper, Grisaru had come a little closer, perhaps, but had still not really tried to take the classical limit.

However, the idea that one cannot take the classical limit of this supergravity argument didn't sound right. I found that as soon as you asked the question, it wasn't too difficult to take the classical limit and prove positivity of energy in pure Einstein theory. So, this is part of the answer to your question of what I did in the early years at Princeton. There were too many papers to tell you about all of them, but this was one. And it had a cousin, which again had to do with being interested in the stability or instability of various compactifications of gravity. If positive energy was wrong, presumably there would be a catastrophic collapse at least at the quantum level, of Minkowski space. But since positive energy is true, that's counterfactual. But I looked for and found a case where the positive energy theorem is wrong, and there is such a catastrophe. And this is actually the original Kaluza-Klein theory- Minkowski space times a circle. The fifth dimension is rolled up to a circle, without fermions.

And as soon as you asked the question, it was actually rather easy to see that positive energy is false in that case, and you can find an exact solution that describes the catastrophe. The solution very simply is the Euclidean black hole as studied by Gibbons and Hawking, but looked at in a different way. So, I didn't even have to find a new solution. I just had to ask a different question about a known solution. That was the second somewhat notable paper.

There was another paper which eventually became important in the string duality era, in the 1990s, but at the time it was more like a field theory exercise and an interesting observation. I became interested in defining the charges of a magnetic monopole. I wasn't able to define the non-Abelian charges of the magnetic monopole in a theory like the real world, where at low energies, there’s a non-Abelian gauge field, in this case QCD. And the accepted answer, as determined a little bit later by Coleman and Phil Nelson, is that you can’t define the non-Abelian charges.

But at any rate, as a result of puzzling on that, I looked more carefully than people had done before at the ordinary electric charge of the magnetic monopole, and discovered something that had been overlooked, which involved the theta angle of gauge theory ‘t Hooft, Polyakov, and others had shown that realistic theories can have magnetic monopoles. And people had assumed that when they're electrically charged, their electric charge would be an integer multiple of the electron charge. But I showed that the electric charge of a magnetic monopole can actually be displaced from that by a constant, which depends on the CP violation of the theory. Anyway, I'm still trying to answer your question of what I was doing in the early years at Princeton, and this is one of the early papers which eventually was important.

Zierler:

To return to an earlier comment you made about your awareness of the inadequacy of experimental data as a graduate student, what was happening in the early 1980s experimentally that was interesting to you, where the data may have been better?

Witten:

Well, the data that I was referring to about QCD has been continually getting better ever since then, down to the present. Even nowadays at the LHC, some aspects of QCD are much better explored than previously. What were the most significant things that were happening in my early years at Princeton? Well, the bottom quark had been discovered in the late 1970s. So, in the early 1980s, one of the big questions was how the bottom quark decays. Is it a weak doublet? That eventually proved to be the case. In that case, another quark was needed, the top quark, although the search was prolonged- it wasn’t found until the mid 1990s. As well as exploring the bottom quark, experiment in this period was just reaching the capability to discover the W and Z particles, and to test the weak interactions at a much more fundamental level.

Zierler:

Were you interested in cosmological questions at this point at all?

Witten:

Well, I was, certainly, but I never contributed very much. Observation was advancing rather incrementally until the 1990s, when the cosmic microwave fluctuations were discovered. But of course, there was a huge theoretical advance in around 1980 with Alan Guth’s theory of the inflationary universe. I thought about that and considered to become involved with it. Anyway, I never really did become involved.

Zierler:

Did you take on graduate students right away when you got to Princeton?

Witten:

Within a couple years. It's hard to remember- my first student was Hidenaga Yamagishi, and the second was Jon Bagger. I don't remember exactly when they started. It wasn't long before I was taking students, for sure. And Cumrun Vafa was one of the first students, probably one of the first three or four. After the first two, I wouldn't be able to put them in order chronologically.

Zierler:

Ed, let's set the stage for 1984 and either call it the first revolution or the second revolution, however we understand it. Did these developments happen quickly, or was there a steady buildup to what became so exciting in 1984?

Witten:

For me, there was a clear buildup, because, as I told you, there were many premonitions. There were many foreshocks in the work of Green and Schwarz, primarily, and also with Lars Brink on some of the papers. You see, string theory had been developed up to a point in the 1970s, but then people had stopped before putting it in a completely consistent form. So Green and Schwarz were the first to formulate a version of string theory that we today believe is completely consistent. And they did it by assembling systematically ingredients that were known in the 1970s but hadn't been systematically assembled. So, they had closed strings with the left and right GSO projections. And they had open strings with the GSO projection. G, S, and O—Gliozzi, Olive, and Scherk—had conjectured that with what's now called the GSO projection, fermionic string theory would become supersymmetric. Green and Schwarz proved that. And by putting the theory in precisely the form that was space-time supersymmetric, they did with Brink the first completely sensible loop calculations.

So, all this I thought was incredibly exciting, and I spent the summer, I think in 1982—maybe 1983—anyway, I spent a summer reading John Schwarz's review article on superstring theory. In hindsight, it was an excellent but still too narrow source. After reading that article, I should have spent another month reading some of the older articles that he cited. Because he reviewed the subject but emphasizing naturally the more recent developments he had done with Green. And I realized later that some of the older things were also important. Anyway, this was extremely interesting and exciting. But I was wary of getting involved, because I thought it was too long-term a project, and that even if it was right, it might not really bear fruit for one hundred years.

Zierler:

What do you mean by bearing fruit? What might you have in mind? What would it look like for this to bear fruit?

Witten:

Well, ideally to describe the real world. Or at least to make some kind of major advance in understanding the theory. I'm not exactly sure what I would have said if you had asked me. There's no interview, so there's no record of my thinking in 1982 or 1983, and I won't be able to remember very well. But as I was telling you, I was interested enough to spend a whole summer reading John Schwarz's review article, but a little bit wary of becoming too involved in it.

But there was one thing that was obvious to me, which I pointed out to John and Michael, and everyone else who was interested. I think there were only a few who were interested. Steve Shenker and Dan Friedan come to mind, and maybe there were a couple others, but not a lot. Something that was obvious to me but wasn't immediately completely obvious to everybody was this. Green and Schwarz had put string theory in the form where there was a very strong case that there was a consistent quantum theory that described gravity together with other forces. And the other forces could be gauge fields, somewhat like in the Standard Model. But there was something extremely conspicuous that was wrong in terms of phenomenology, and that was that the weak interactions couldn't violate parity. It wasn't completely trivial at the time to explain to people that that was an important point. Because their theory could violate parity, but it predicted that the weak interactions had to conserve parity.

Now, to say it at a slightly more abstract level, in the Standard Model, the fermions have a chiral structure. The left and right fermions transform differently under the gauge group. And that's actually one of the most fundamental facts about the Standard Model. It's the reason that there are no fermion bare masses, and that fermion masses in the Standard Model come only from the Higgs mechanism. And it's our understanding of why the fermions are light compared to the Planck scale or the grand unification scale, or whatever are the truly high scales in physics. The fermion masses violate gauge invariance, and so they can only come from gauge symmetry breaking. That doesn't explain everything, but in all modern thinking, it's an important part of whatever we do understand.

And as it existed in 1982 and 1983, string theory was a consistent theory of gravity unified with other forces, but it completely missed the chiral structure. So, to me, that was a huge siren blaring. Anyway, to set the stage, I want to just point out to you that it was clear by 1982 or 1983 that there were an incredible variety of delicate things that fit together perfectly to make it possible to have a theory of quantum gravity based on string theory. It was unbelievable that it could all be a coincidence. Yet it was markedly wrong for describing the real world because of this question of the chiral nature of the fermions. But then in 1984, Green and Schwarz discovered a more general method of anomaly cancellation, and everything changed.

Maybe I should back up just for one more second and say that part of the problem had to do with this. The version of the theory that came closest to working was the type 1 superstring theory. Green and Schwarz had type I superstring theory and type II superstring theory. Those were the theories they could construct with the ideas at the time. There were two versions of type II: type IIA and type IIB. So, they had three superstring theories. Of the three, one of them, type I, had the chance to make chiral fermions. But if you tried to do that, you ran into anomalies, similar to the Adler-Bell-Jackiw anomalies, which are important in particle physics, and which, for example, were used to predict that the top quark had to exist once the bottom quark had been discovered. The anomalies make the theory inconsistent.

Even for type IIB, there was a problem with anomalies, but Alvarez-Gaumé and I had showed that somewhat miraculously, the anomalies cancel for that theory. Type IIA was manifestly anomaly-free, at least by the ideas at the time. But the theory that had the potential to get fermion chirality was Type I, and it looked like Type I was afflicted with intractable anomalies. So, anyway, what was really problematical for Green and Schwarz was the combination of fermion chirality and anomalies. Taking these together, it seemed that string theory could not work. But then, in August 1984, Green and Schwarz discovered a new mechanism for anomaly cancellation, and everything changed.

What Green and Schwarz did in August 1984 was to show that in a particular case, when the gauge group is SO(32), the hexagon anomalies disappear. And that really removed the only general obstruction to making a model based on string theory that would incorporate gravity together with at least what I would understand as the main features of particle physics, namely non-Abelian gauge theory with chiral fermions. So, it was immediately obvious to me, once they made their discovery, that you could make at least semi-realistic models of particle physics, in that framework. But also, to me, I had done kind of an experiment in the following sense. I had spent two years watching this, wondering, could it be? Can it be that all the coincidences that had been discovered that made string theory possible were just coincidences? As far as I was concerned, the discovery they made in 1984 was an empirical answer of “no” to that question. If the miraculous-looking things that had been discovered up to 1982 and 1983 were truly coincidences, you'd then predict there wouldn't be any more such coincidences. That had proved to be wrong when they made this miraculous-looking discovery about anomalies that enabled the theory to be much more realistic.

In explaining this to you, I'm trying to help you understand why this had so much of an impact on my thinking, watching from the outside for a couple years, wondering if this subject was as amazing as it appeared to me. And a “no” answer would have predicted there shouldn't be more miraculous discoveries. And that was, to my satisfaction, disproved in August 1984. So, after that, the hesitation that had kept me from becoming more heavily involved earlier evaporated. Now, I realized that in the physics world, there were plenty of people who hadn't lived through this two years of uncertainty that I had lived through, and in many cases they had never heard of the whole thing until August 1984. And they hadn't done the experiment I had done. So, they didn't react as I did.

Zierler:

How much cheerleading did you do among your colleagues, both near and far, after this revolution in 1984, that this is what people should concentrate on? That we can have this figured out in the near term?

Witten:

I wasn't intentionally cheerleading, but I was very enthusiastic. And I actually think I was right to be enthusiastic. I wasn't intentionally cheerleading, but to the extent that I encouraged other people to get involved, I've got no regrets about it at all (laughter).

Zierler:

To go back to your early comment about bearing fruits in one hundred years, where the goal would be to have a theory that explains the real world, well, here we are, forty years later. Are we about forty percent there?

Witten:

That's hard to say (laughter). It's impossible to say. It depends on what the answer is. But I think that theoretical physics has been far more productive during this thirty-seven-year period since 1984 than it would have been if we had been playing around with the same tools without string theory as a new perspective.

Zierler:

What was happening at the time or has happened since in the world of experimentation or observation that may get us closer to string theory being testable?

Witten:

Well, I'd prefer to just comment on what are the most significant observations that have been made, and what implications they might have. So, the biggest shock in physics for me since 1974, when the charm quark was discovered, was definitely the acceleration of the expansion of the universe. That was a tremendous shock. Because since the cosmological constant, since the vacuum energy is so incredibly small, it was generally presumed before it was observed that it was probably exactly zero for some unknown reason. And the prevailing view before the discovery was that there was something missing in our understanding of quantum field theory, quantum gravity, something, some symmetry, some new ingredient of some kind, who knows what, that would explain why the vacuum energy is exactly zero. And experiments showed that it isn’t exactly zero. That’s an idealization of what an experiment shows. An experiment doesn't really measure the vacuum energy; it measures the energy density of empty space in the recent universe. In principle, there might be, for example, a scalar field, with a very slow tilt so that it is slowly evolving toward a smaller energy. We don’t really know for sure that what we've measured is the energy of the vacuum. However, the most straightforward theoretical interpretation of what’s observed is that it’s the vacuum energy, and that’s why I refer to it like that in talking to you. Attempts at other models, and I have made some, actually, personally, aren’t very compelling. I actually think it’s extremely fundamental to measure better, to measure as precisely as we can whether what we interpret as the vacuum energy really is that. In other words, whether the expansion really exactly fits the exponential expansion due to the cosmological constant. Whether the parameter people call Omega is precisely equal to one, as accurately as we can.

It’s extremely fundamental because it strongly suggests that the world we're living in isn’t stable. I think there are not bulletproof reasons but there are good reasons to strongly suspect that a world with a positive cosmological constant might be exponentially long-lived but isn’t truly stable. And I believe if we're living in a vacuum state that isn’t truly stable, it’s likely to be much harder to understand. The extreme version of that is the possibility suggested by people like Weinberg, Bousso, Polchinski, Susskind, Rees, that what we're living in is part of a multiverse. If that’s right, I think it potentially makes the universe much harder to understand.

So, when you asked me what has moved this closer, I think the discovery of the cosmic acceleration might be what has most impacted our understanding of the universe during these thirty-seven years in terms of experimental findings. But it hasn’t made a detailed understanding closer, definitely not. The two other most important findings were certainly that cosmology became a precision subject with the cosmic microwave fluctuations and significant evidence developed for cosmic inflation. That’s one. And the second are the neutrino masses, which are plausibly a signal coming from something close to grand unification, though unfortunately we don’t know for sure that that’s the correct interpretation. Proton decay would be one of the most important things that hasn’t been seen, but there is still hope.

Zierler:

What about supersymmetry? What was some of the optimism in the 1980s about supersymmetry being seen?

Witten:

Well, we hoped it would be seen possibly at the electroweak scale. But more than that, almost everyone in the field at the time- I mean, I'm saying almost everyone, but I actually couldn't think of an exception; there must have been exceptions, but I can't think of one right now- believed that when we get to energies at which we could directly explore the weak interactions, we would discover a mechanism that accounts for why the weak scale is so much less than the scale of gravity or grand unification. So, it was generally believed that if we could directly probe the weak interactions, we'd learned the answer to what Steve Weinberg called the hierarchy problem.

Supersymmetry was a possible explanation of the hierarchy problem. It was far from the only one that people explored in those years. So, if you talked to me in the 1980s, I'm sure I would have expressed some hope about seeing supersymmetry as part of the answer of the hierarchy problem. But I would have expressed a lot of confidence about observing something that would have explained the hierarchy problem. Much more confidence on that. And I think that's what you would have found from practically anybody in the field. Supersymmetry was a possible approach to the hierarchy problem, but there were other possible approaches. But as experiment progressed, competing approaches had more trouble for a while than supersymmetric approaches. LEP experiments in the 1990's made most attempts to explain the weak scale without supersymmetry problematical. So, supersymmetry survived longer than other natural attempts at explaining the weak scale.

But ultimately, with the LHC, experiment has reached the point that it's extremely problematic to have what's called a natural explanation of the weak scale, a mechanism that would explain in a technically natural way why the Higgs particle is as light as it is, thus making all the particles light. It's actually a baby version of the problem with the cosmological constant. So, to the extent that the multiverse is a conceivable interpretation of why the universe accelerates so slowly, it's also a conceivable explanation of why the weak scale is so small. It might be the right interpretation. But if it is, it's not very encouraging for understanding the universe. When the multiverse idea became popular around 1999, 2000, and so on, I was actually extremely upset, because of the feeling that it would make the universe harder to understand. I eventually made my peace with it, accepting the fact that the universe wasn't created for our convenience.

Zierler:

You know, a rabbi could have told you that a long time ago (laughter).

Witten:

Well, I'm not a believer, so I won't comment. Have you interviewed David Gross?

Zierler:

I have.

Witten:

I don’t know if you got to discuss the multiverse with him. He reacted as I did. And I don’t know if he has made peace with it to the extent that I have.

Zierler:

(Laughter).

Witten:

I simply don’t know. I haven't discussed it with him for a number of years.

Zierler:

It’s a purely speculative question, but was your sense that had the SSC been built, supersymmetry would have been seen at those energies?

Witten:

Was my sense when? Then or now?

Zierler:

No, when there was the assumption that the SSC was going to come to fruition, in the late 1980s, early 1990s.

Witten:

I think I would have told you what I did tell you. I would have said I would have hoped that supersymmetry would turn up, but I would have been almost sure that some kind of natural explanation of the weak scale would turn up.

Zierler:

In the late 1980s, tell me about your work with topological quantum field theories. How did you get involved in that?

Witten:

It had to do with the influence of Michael Atiyah, who drew my attention to Donaldson invariants and Floer theory of four manifolds and three manifolds and the Jones polynomial of knots, as problems that he thought should be better understood through physics. So, I eventually got involved in those and had some success in interpreting them in terms of physics.

In the case of the Jones polynomial, there were a lot of clues that pointed toward what could happen. In string theory, what are called two-dimensional conformal field theories are very important. At the time, physicists were intensively studying soluble examples of two-dimensional field theories. They were soluble because they have a lot of symmetry, and they have what are called conformal blocks. Erik Verlinde had an intuitive picture of the conformal blocks, which was as if they were states in a quantum theory, with operators acting on them. I don't remember if he made that analogy explicitly, or if it just was that the language that he was using was as if that was true.

But then there was a math paper by mathematicians Tsuchiya and Kanie that I think was pointed out to me by Graeme Segal, who showed that what are called the Jones representations of the braid group, which Jones had used in describing his knot polynomial, can be expressed in terms of these conformal blocks that Erik Verlinde and other physicists were studying. And there was also work by Greg Moore and Nathan Seiberg, which was going on at the Institute, where I was at the time, so I was hearing a lot about it, also on these conformal blocks. And in hindsight, they were discovering things that were related to three-dimensional topological field theory.

So anyway, there were all kinds of clues that were going on. I've told you the main ones, at least for me, that pointed toward a reinterpretation of the conformal blocks of current algebra in two dimensions and also the Jones model in terms of a three-dimensional topological field theory. The right way to put it together hit me while I was at Swansea. There's an irony, because Albert Schwarz enters the story a second time. I told you that when I was a postdoc, Sidney Coleman had told me about a paper of Albert Schwarz. I told you about two papers, but one of them in particular was an early paper on what we now called topological field theory. He expressed it in a mathematical language that was so unappealing to physicists that not many physicists noticed the paper. I certainly wouldn't have, if Sidney hadn't shown it to me. Then also just before I left for Swansea, there was a paper by Sasha Polyakov who was interested in an Abelian Chern-Simons theory. I think he was motivated by high T_c superconductors. He discussed what I later reformulated as the framing anomaly for a Wilson line in Abelian Chern-Simons theory.

Anyway, there were all these things in my mind, and Albert Schwarz was supposed to come to Swansea, but these were the Cold War years, so he wasn't allowed to come, and his lecture was given by someone else, I believe by Krichever. My mind wandered. And while I was sitting there, it all popped into my head, that there was a Chern-Simons theory in three dimensions, the non-Abelian version of the theory Polyakov had been writing about. And if you used that theory, everything was going to work. You would explain the conformal blocks and the Jones polynomial. So anyway, that was how that got started.

Zierler:

Tell me about your decision to move to the Institute from the university in 1987.

Witten:

Well, the Institute was hard to say no to, because of the excellent conditions and the opportunity to concentrate on research full-time.

Zierler:

In what ways did you remain affiliated with the department, in terms of serving on committees, supervising graduate students, things like that?

Witten:

I supervised graduate students. As recently as about a year ago, I was still having graduate students.

Zierler:

What did it feel like to win the Fields Medal as a physicist?

Witten:

Well, it was a thrill, of course. It felt a little funny because I knew that obviously I was a non-standard selection. And I don’t like controversy about science, and I felt that I might have been a controversial choice in the math world. But on the other hand, I hadn’t selected myself, so I didn't feel any controversy was my fault.

Zierler:

Perhaps more fundamentally though, these distinctions between mathematicians and physicists are not as formal as they should be.

Witten:

That’s correct. But most of my papers do not have the precision of math papers, even when I discuss things of mathematical interest. That’s what I meant when I said I might have been a controversial selection. I didn't mean that I was formally a physicist rather than a mathematician. I'm sure that wouldn't be an issue.

What is an issue- I think it would be better if we weren't discussing this in the context of the Fields Medal, but just generally in the context of my relation to the math world. What's a little funny about my relation to the math world is that although some of my papers are of mathematical interest, they rarely have the detail of math papers. And I can't provide that detail. I simply don't have the right background. What I bring to the subject is an ability to understand what quantum field theory or string theory have to say about a math question. But quantum field theory and string theory are not in the precise mathematical form where such statements can usually be rigorous.

Zierler:

To compare and contrast the early 1980s to the early 1990s, where, as you say, there was a buildup to 1984 where you were thinking about these things in detail, was the same true in the early 1990s, in the buildup to M-theory?

Witten:

Well, in the buildup to non-perturbative string dualities. So, in the early 1980s, it had been mainly the work of Green and Schwarz, a little bit Lars Brink, that was providing the premonitions. In the early 1990's, there were other things. One paper that certainly influenced me was on what they called heterotic five-branes by Callan, Harvey, and Strominger. And then I was very interested in the papers of Mike Duff, who was groping around toward nonperturbative dualities. I had Mike send me a huge collection of his papers, and I more or less read all of them. I'm sure I'm forgetting some things. I wonder if in the interview in 2014 I did a better job of remembering what were the precise things that gave me premonitions at this time. Well, I told you about the conversation with John Schwarz, so certainly the work of Sen and Schwarz was one of the things that made me think something big was in store. I didn't know what, though.

But then Sen wrote another paper at the beginning of 1994 that to me definitely showed something big was in store. There had been this Montonen-Olive conjecture. I told you about my relation to it in 1977. They made an early conjecture of electric-magnetic duality. Technically, it didn't work. I went to London, and Olive and I improved it in a supersymmetric version. Later that was further refined by Osborn. The work of Sen and Schwarz gave some further hints in favor of this sort of conjecture. But I still was not sure what one could do with it.

But then Sen did something at the beginning of 1994 that really was fundamental. And it showed me that you could do more with these things. But also, I knew that what Sen had done was a brilliant idea, but it was also a brilliant idea that somebody could have had ten years before, if they had taken the whole idea of this duality seriously. So, it taught me that really, we could be doing better. In some of the same way that the Green/Schwarz work in August in 1984 had resolved some of my doubts- here, I didn't really have doubts, I wasn't holding back in the same way, but anyway, the Sen work inspired me to really believe you could do more.

What Sen did was to show that there's a bound state of two monopoles and an electric charge, that's predicted by the duality. And to me, it was by far the most striking advance in testing the duality since the early days. I'm not sure when exactly to date the early days, maybe my paper with Olive, maybe the refinement by Osborn. Anyway, the Sen paper was very dramatic and certainly the most dramatic advance since the early period.

During these months, Nati [Nathan] Seiberg and I had been talking on and off about N=2 super Yang-Mills in four dimensions. Nati was interested in it, I think, because it was part of his program of understanding supersymmetric dynamics. He had done some very interesting work on N=1. It was later overshadowed by work he did a year or so later on N=1, but anyway, what he had already done by 1993 was quite interesting. He wanted to do N=2. I think he thought I would be interested in it, because of the tie-in to my work on Donaldson theory of four manifolds. I didn't at the time take that tie-in too seriously, although it later turned out to be important.

Anyway, Nati and I were talking about that theory. And by early 1994- unfortunately, there's no way to date this. I remember what some of the turning points and the thinking were, but I don't know when they happened. But anyway, by early 1994, we understand that monopole condensation and duality was important in the story, and Sen's paper was certainly one of the inspirations, even though we were doing a more complicated case with less supersymmetry. So, by maybe late spring of 1994, we had understood what people now called Seiberg-Witten theory, where we had a quantitative description of the Coulomb branch.

You asked me what David Gross was working on when I was in graduate school. I failed to give you the obvious answer. I said things related to asymptotic freedom. But I didn't tell you that the most important unsolved question raised by asymptotic freedom was why quarks are confined and why hadrons are massive in the first place, and why chiral symmetries are spontaneously broken. There are those questions that were raised and not answered by asymptotic freedom. And as a graduate student, I was obsessed with those questions but really couldn't make any progress. 't Hooft's story about large N- I should have explained this better- was meant to be a framework where maybe you could make progress for those questions.

Well, it's a mixed bag to what extent this has happened even today, even though as I told you, people think 't Hooft was on the right track. Anyway, with the work with Seiberg, it was finally possible to make a model exhibiting quark confinement. Not in QCD as I would dearly love to do, but anyway, in some gauge theory in four dimensions. So that was some satisfaction personally.

Zierler:

And this puts us in 1994, roughly.

Witten:

Yes, 1994. In our second paper, we also had models of chiral symmetry-breaking and mass generation. We could give supersymmetric models where we could resolve the puzzles that still are not well resolved even today for ordinary QCD. In a supersymmetric counterpart of the theory, we were able to resolve those questions in 1994. Very understandably, people differ in how interesting this is. My attitude is that we'd rather do QCD, but in the meantime, it's nice to have models where you can understand similar phenomena. Other people think that the supersymmetric case is too unlike the real one to be exciting. It's not the kind of question that has a yes or no answer.

Zierler:

How then does this get us to M-theory?

Witten:

Well, a lot of these clues about nonperturbative dualities that were happening in the early 1990s were in the context of string theory. The Sen/Schwarz work was in that context, for instance. Really all the papers that I told you about that got me excited, except for the Sen paper on the two-monopole bound state, they were all about nonperturbative dualities in string theory. So that was part of the foreshocks in this period.

And there were a lot of papers where people would state a duality conjecture and then show one fact that it would explain. But if you're going to go out on a limb with a crazy conjecture, you wanted to explain more than one fact. You need a picture that fits together more tightly. And there were many different threads in the work of many different people. I should mention the work of Hull and Townsend in the spring of 1995, and maybe the previous year too- I don't remember the dates of all the papers.

Anyway, to be honest with you, what I did has been exaggerated by science journalists, because the story was too complicated to tell correctly. There were all kinds of threads coming from work of many people, and my role in the summer of 1995 was just to put them together. Sen had written that paper just at the field theory level, and Seiberg and I continued in that direction, or found a counterpart, in another case. There was also a paper by Vafa and me in the summer of 1994, again in field theory. By the end of 1994, we had vastly, vastly greater knowledge about the nonperturbative dualities in field theory than we had had a year before. But all this had been partly stimulated by developments in string theory. And that raised the question of whether we could make progress in string theory similar or analogous to the progress that had been made in field theory.

I know Nati Seiberg was one of the first to perceive this question clearly, but he concentrated on understanding the supersymmetric field theories better, and he discovered in maybe mid-1995 what people nowadays call Seiberg duality of N=1 super QCD. I concentrated instead on the string theories. But there was another thread in the story. I was bothered by the fact that there were five string theories. If we were going to describe the real world by one of them, what was wrong with the others? I was hoping there was some subtle inconsistency in some of the others.

I had two ideas in the first half of 1995, where I was going to show that one of the string theories was inconsistent. One of the two ideas, actually I wouldn't be able to tell you accurately now. I concluded it was wrong, and never wrote down my thinking, and can't remember right now very well what the thinking was. But the other idea involved looking at the strong coupling limit of type IIA superstring theory and supposedly finding a contradiction. I was hoping to show that the whole theory didn't exist, thus reducing the number of string theories a little bit, at least getting rid of one of them. But because of what was already known, maybe getting rid of more than one, getting closer to the goal of a single string theory.

However, it hit me that I was completely wrong. This is actually one of two important insights I had while sitting in an airplane. I was flying back from giving a lecture in Canada, maybe in Toronto or Montreal, I guess. It just hit me, that that was wrong. That the strong coupling limit did make sense, but it involved 11-dimensional supergravity. The only real difference from previous work was that I made it more quantitative. I think Hull and Townsend had tried to interpret supersymmetric black holes in terms of momentum around an 11th dimension. But for that idea to make sense, there's a simple quantitative check you have to make. You have to show that in the limit where string theory is weakly coupled, the 11th dimension is small, and in the limit where string theory is strongly coupled, the 11th dimension becomes large. It's a simple check, but nobody had done it, so I was left to do it. And once I did that, I found other similar checks for other dualities.

So, it was possible to make more precise statements completing and linking together numerous statements that others had already made. That description of what I did in the summer of 1995 might be less dramatic than some explanations you've read, but I think it's more accurate.

Zierler:

This is to say that you blame science journalists for calling it the second superstring revolution?

Witten:

Well, I think the terminology was coined by John Schwarz, and I think he should have called it the third superstring revolution.

Zierler:

Meaning because he would not have given himself credit for his role in the first revolution?

Witten:

I think John was an important person in what I would have called the first revolution around 1970. So, I've always been surprised he used terminology that I thought didn't fully capture his own contributions. But anyway, regardless of the counting, there definitely was a superstring revolution in the mid 1990s. Although I'm pleased with the contributions I made, I think they've been oversold, honestly. My contributions were more like combining what other people were saying, and making it more quantitative. It was important, but it wasn't-

Zierler:

Was part of the satisfaction that in combining these five discrete string theories into M-theory, that it would get us closer to string theory as something that represented the real world?

Witten:

Well, certainly. It's satisfying to know that there was only one candidate for superunification. There's only one reasonable candidate now for the theory that combines gravity and quantum mechanics. Before 1995, there was more than one. It's more satisfying to know that the theory seems to have a lot of possible manifestations, in terms of approximate vacuum states, but at a fundamental level, there's only one fundamental theory or system of equations, that we admittedly don't understand very well. That's got to be an advance of some kind.

As I told you, my work in 1995 involved largely simple quantitative tests, where these duality conjectures avoid contradictions. Because when one description is good, the other isn't. That's part of it. Otherwise, you get a contradiction. I had actually made one of those observations in 1988, but didn't take it seriously at the time. This was in 1987 or 1988. Maybe even a year earlier. Basically, of the five string theories, two of them have SO(32) gauge symmetry. So at least at the level of cocktail party chatter, people were wondering if it was conceivable that those two were equivalent. It's easy to see that they can't be equivalent in the region where they both are weakly coupled, because then you'd be able to see the equivalence with the naked eye, which you can't. But there's no obvious contradiction in claiming that they're equivalent in such a way that when one is weakly coupled, the other is strongly coupled.

It's actually quite easy to see that if they are equivalent, there has to be an equivalence of such a kind, not just to avoid a contradiction but to make the low energy supergravities match. And I had actually noticed that around 1980-something-six, seven, eight, I can't tell you. I didn't take this seriously enough to publish it. But it's the kind of observation I made in 1995. In fact, this precise observation I did publish in 1995. No one else had done it in the intervening years so it was left for me.

Zierler:

While we're on the topic of nomenclature, how do you understand M-theory within the context of what John Ellis called the "theory of everything"?

Witten:

Well, I don't like to be melodramatic, so I would never use the phrase "the theory of everything." If there's anything we know that might be the theory of everything, it's M-theory. But I've never used such a phrase.

Zierler:

It's dramatic, but the hope is that for better or worse, it is the theory of everything.

Witten:

It might be. And maybe humans will even understand it better one day.

Zierler:

Meaning what, exactly? What would that mean to achieve a theory of everything?

Witten:

Well, I don't like- see, I don't like to be melodramatic, so I don't want to answer it precisely in that context.

Zierler:

Well, regardless of the words- the words might be dramatic, but there's a meaning behind them.

Witten:

Okay, but I prefer to answer the question in terms of what would it mean to understand M-theory well. Einstein, when he got to general relativity, knew the ideas behind it before he invented the theory. He had the principle of equivalence and general covariance. And then he invented a theory that incorporated those ideas. What I said might be more strictly true for the principle of equivalence than for general covariance, which I believe he was confused about.

Not that I'm an expert, but just from what one reads in *Physics Today* and on the history of the subject, it seems clear that Einstein struggled more with that than he did with the principle of equivalence. But anyway, by the time he had the theory, he had the right mathematical framework of Riemannian geometry. At least by the time the theory was invented, he had the ideas it was based on, and some of them he had had before.

String theory and M-theory have always been different. From the beginning, they were discovered by people who discovered formulas or bits and pieces of the theory without understanding what's behind it at a more fundamental level. And what we understand now, even today, is extremely fragmentary, and I'm sure very superficial compared to what the real theory is. That's the problem with the claim that supposedly I invented M-theory. It would make at least as much sense to say that M-theory hasn't been invented yet. And you could also claim it had been invented before by other people. Either of those two claims is defensible (laughter). So I made some incremental advances in a subject that's far from being properly understood.

Zierler:

Tell me about when Juan Maldacena enters the scene and you understand the significance of AdS/CFT.

Witten:

He had this paper with a bold conjecture. I think he was bolder than I would have been. His conjecture was motivated by papers of Klebanov and others. Igor Klebanov, Amanda Peet, Steve Gubser were the authors of some of the papers. They were studying the scattering of low-energy particles, for example low-energy gravitational waves, from a system of D-branes. And they were trying- it's hard to explain what they were doing without [laughs] putting it in the language that we have after Maldacena. That's why I'm struggling. They were comparing the predictions of field theory on the D-branes to gravity in the space-time sourced by the D-branes.

Some things worked. Some things didn't work as simply as they expected. But they were getting very interesting results. Maldacena was bolder than any of us had been, and he said that there was a duality that the low-energy gauge theory on the D-branes is equivalent to the string/M/supergravity theory outside. In other words, he proposed that N = 4 super Yang-Mills theory, the gauge theory on the D-branes, which had been the right arena for the Montonen and Olive conjecture that we've discussed earlier in this interview—he suggested that it was exactly equivalent to type IIB superstring theory in a space-time created by the D-branes, which technically is AdS^5xS^5, anti de Sitter 5 space times a 5-sphere. So, he made that proposal.

I've struggled to fully understand that paper. Maldacena had the right idea, clearly, and he was bold in extrapolating to that idea from the detailed computations done by Klebanov and others. He didn't have a precise recipe for a map between observables in one theory and observables in the other side. And, well, if you want to take this duality seriously, you want such a recipe. And Maldacena's conjecture sounded wild and crazy at first. But if you tried to take it seriously, it wasn't too hard to see what the basic recipe had to be—at least for the simplest observables like correlation functions. So, I wrote a paper explaining that.

The paper appeared almost simultaneously with, actually a couple days later than, a competing paper by Gubser, Klebanov, and Polyakov. They did something somewhat similar, not quite as systematically but with more understanding of some examples. And that was because their work was more literally tied to the work Klebanov had already been doing. I hadn't understood very well the details of Klebanov's papers. But what I had extracted from them was what the recipe in the Maldacena correspondence had to be- a recipe for calculating correlation functions in the gauge theory in terms of what you would have to do in gravity. The basic statement was that you have to think of the gauge theory as living on the boundary of the Anti de Sitter spacetime.

That went to the title of my paper, "Anti de Sitter Space and Holography." Holography had been the idea articulated a few years earlier by people like 't Hooft and Susskind that a gravitational world would have some kind of holographic description on the boundary of the space time. I realized that if the Maldacena duality was correct, it had to be holographic. The gauge theory was a holographic description on the boundary of the world. And in that picture, I explained some simple statements about how to compute correlation functions.

Zierler:

Tell me about your subsequent work with Nati on string theory and noncommutative geometry.

Witten:

I think qualitatively it's known that open strings in some sense are related to some kind of noncommutative geometry. But it's very hard to get something simple and understandable out of it. The year before Seiberg and I worked together on this, Connes, Douglas, and Schwarz- Connes is Alain Connes, the mathematician who is the mathematical pioneer of noncommutative geometry and Schwarz is the same Schwarz I keep telling you about- they had suggested a slightly exotic limit in string theory in which the noncommutativity of string theory would simplify to something manageably understandable.

This was striking and interesting. It involved a limit that didn't get me that excited originally. But a few months later, there was another paper by Nikita Nekrasov and Schwarz, which got me more excited, actually. They described the instantons of the D0-D4 system in the presence of a gauge theory theta angle in terms of noncommutative geometry. Their result really bothered me, because it looked so beautiful that it must be correct, but it seemed to contradict what was known by more standard methods. So that contradiction got me to start working on it with Nati. Nati recognized this problem I've mentioned, but he was bothered more by something else, which to be honest I can’t remember right now. We each were puzzled about contradictions in the subject, but we were each motivated primarily by different ones.

Anyway, we worked together on it, and we got a better understanding that resolved the contradictions. I remember better how we resolved the contradiction that worried me more. But anyway (laughter). So, what I would summarize from the subject is what I told you at the beginning. String theory, in some sense, involves noncommutative geometry on a grand scale, but it’s difficult to tame that and put it in a form one can really understand. What Connes, Douglas, and Schwarz, and Nekrasov and Schwarz, and Nati and I did, was to isolate a corner of the theory where you can make the role of noncommutative geometry much more tangible.

Zierler:

Tell me about your decision to be a visiting professor at Caltech.

Witten:

Oh, well, we were seriously considering moving to Caltech at that time. We eventually decided not to, but we had two very nice years at Caltech.

Zierler:

Was that an opportunity to work more closely with John Schwarz?

Witten:

Well, a potential opportunity, of course. We did write at least one paper together during those years, but we didn't really work extensively. And it also led to the only paper I actually wrote with Michael Atiyah. Michael Atiyah visited at Caltech for a few months during those two years. His suggestions frequently influenced my work, but that was the only time we worked together.

Zierler:

You seriously considered making a permanent move to Caltech?

Witten:

Yes.

Zierler:

Why ultimately did you not?

Witten:

Well, life is simple in Princeton (laughter).

Zierler:

What was next in the 2000s? What did you work on next?

Witten:

Well, I'll jump ahead to about 2003. During the approximately ten years starting in 2003, there were four problems I worked on seriously, of which only the first was popular among my colleagues. The other three were things that I was really interested in, but they were outliers.

So, the first one had to do with scattering amplitudes in gauge theory. What I really wanted to do was to find a weak coupling version of the Maldacena duality for N=4 super Yang-Mills. In other words, one way to say what Maldacena discovered was that N=4 super Yang Mills is equivalent to a string theory. It's in line with what 't Hooft had said about QCD twenty-five years earlier. Like I told you, Seiberg and I had a model of confinement, but not for QCD, only for the supersymmetric version. 't Hooft said QCD is a string theory. We still today don't know if that's true for QCD. We only know for a supersymmetric version of the theory that it's true, as pioneered by Maldacena. But the Maldacena duality involved a string theory that was only understood for strong coupling. I wanted to understand it for weak coupling.

I also wanted to understand a lot of amazing results about gauge theory scattering amplitudes that had been discovered by people like Zvi Bern, David Kosower, and Lance Dixon. And these results which were not well known among string theorists at all. Anyway, the outcome was weird. I was successful up to a point, but I didn't really achieve what I really wanted, which was to find the weak coupling version of the Maldacena string theory. But I found what was called twistor string theory, which was a new way of looking at perturbative scattering amplitudes in gauge theory. It has definitely had an impact on people who do gauge theory, even people who did calculations motivated by the LHC. But I didn't completely achieve what I wanted. Just about a week or two ago, there was a paper by Gaberdiel and Gopakumar, who have a proposal which is maybe the beginning of doing what I had actually wanted to do when I was working on twistor string theory. I don't know, we'll have to see how that works out.

Anyway, for a couple years, my main interest actually was scattering amplitudes. Then, for a couple years, my main interest was a mathematical application of supersymmetric gauge theory. Back when I had visited Oxford in 1977, and this story is told in much more detail in that interview in 2014, Atiyah, when he gave me the two papers by Olive, also told me that he believed that this duality of Montonen and Olive was important because it was going to be a physics counterpart of something that is incredibly important in number theory, which I had of course never heard of, which was the Langlands program in number theory. I filed that away in the memory bank. I didn't try to worry about number theory and the Langlands program.

By the late 1980s- I'm probably forgetting bits of the story, I should tell you- but by the late 1980s, Sasha Beilinson and Vladimir Drinfeld had discovered what they called a geometric version of the Langlands program, and it involved ingredients of quantum field theory. Tantalizing. But it was tantalizing because they were using familiar ingredients of quantum field theory in a very unfamiliar way. It looked to me as if somebody had put the pieces at random on a chess board. The pieces were familiar, but the position didn't look like it could happen in a real chess game. It just looked crazy. But anyway, it was clear it had to mean something in terms of physics. I even worked on that for a while at the time.

I think I've gotten this slightly out of order. I think when I worked on it was actually before the work of Beilinson and Drinfeld, driven by other clues. And the Beilinson and Drinfeld work was one of the things that made me stop, because I realized that A, I couldn't understand what they were doing at the time, and B, there were too many things I didn't know that they knew, and that seemed to be part of the story. Anyway, as you can see, my memories from whatever happened in the late 1980s are pretty scrambled.

They wrote a famous paper that was never finished and never published. It's 500 pages long. You can find it online, if you like. They have an incredibly generous acknowledgement of what they supposedly learned from me, which is way exaggerated. Based on a hunch, I told them about a paper of Nigel Hitchin, but I didn't understand anything of what they attributed to me. At any rate, regardless, even if I didn't understand what they did with it, the fact that I was able to point them to the right paper was another sign of the fact that what they were doing had something to do with the physics I knew. But I couldn't make sense of the connection. And this kept nagging at me off and on for a long time. Are you familiar with the story I'm telling you at all, or is it completely new?

Zierler:

I'm familiar.

Witten:

Well, in about 2003, Edward Frenkel organized a conference at the Institute which was supposed to inform physicists about the geometric Langlands program. Edward Frenkel is a mathematician at Berkeley who had been very interested in the connections between the geometric Langlands program and quantum field theory and had developed some of them. He had the perspective of a mathematician, not of a physicist. So, he understood some sides of the story, but he thought maybe physicists would understand other sides. He organized this conference. I don't even know how he made it happen, not being at the Institute himself.

But anyway, they had a conference to educate physicists about the geometric Langlands program. All of my colleagues attended. Nati was there. Juan Maldacena was there. Probably a lot of others. I'm not remembering everybody right now. I don't know for sure, but I tend to believe that none of my colleagues there except me had heard of the subject before this meeting. What they got out of the lectures, I can't tell you. You could ask some of them. I could guess, but I won't. But I know that I had trouble getting a lot out of it. Because for example, Frenkel gave a series of lectures, where he was explaining what he understood of the relations to quantum field theory, but to me it sounded like the chess pieces at random on the chess board. I kind of already knew that story. Mark Goresky also gave a series of lectures trying to explain the Langlands program as it is in number theory to physicists, starting from no assumed knowledge except the notion that a field is a set of numbers where you can add and multiply and divide with certain standard axioms. He assumed no knowledge of algebra except, "What is a field?" And from there he tried to explain the Langlands program.

Well, I couldn't get much out of that either, because although I didn't really know anything about the Langlands program, I knew as much as you could explain in a few hours starting from nothing, from no assumed knowledge except "what is a field?" I mean, he assumed that even the basics of algebraic number theory weren't known, for example. His lectures were very well done but ultimately, it was impossible to do what he was trying to do. So that was another series of lectures I couldn't get much out of.

But then, just when I had given up learning anything from this conference—toward the end of the conference, there was a single lecture by David Ben-Zvi, who described what he called an approximation to the geometric Langlands program. I realized that in physics language his approximation was a duality symmetry of a two-dimensional field theory. And I started thinking that the reason he was calling it an approximation to geometric Langlands was that he was looking at the problem in the wrong complex structure, and that if one just rotated the complex structure, one would get the real thing. And that's correct, although it took a few years to understand it.

By the time I understood it, I was working with Anton Kapustin. That started because I had come to realize that Kapustin's work on what's called the B-model of two-dimensional topological field theory was going to be an ingredient in understanding geometric Langlands from the point of view of physics. And since he understood that better than I did, I started working with him.

Zierler:

But this was after you were at Caltech? You did not work with Anton when you were at Caltech?

Witten:

No. I started working with him in 2005, probably, at earliest, 2004.

There were a few steps in developing what I regarded as a good understanding of geometric Langlands. One idea that was important was to take whatever David Ben-Zvi had said and rotate it and deduce it by dimensional reduction from four dimensions. I don't remember accurately how long it took to understand that clearly. A second important point was to understand the role of the work of Kapustin and Orlov. Another bit that was important was this. Something that had driven me batty ever since I heard the work of Beilinson and Drinfeld in the late 1980s was their geometric Hecke transformations. I just couldn't understand them. For them, it's the main point of the whole theory, understanding geometric Hecke transformations, which are counterparts of Hecke transformations in number theory, that the number theorists consider incredibly important. I couldn't make heads or tails of a geometric Hecke transformation or the crazy-looking definition they used.

I told you I've twice had an important idea while sitting in an airplane. While flying back from Seattle--I think in 2004 or 2005, probably- it hit me that a geometric Hecke transformation is just an algebraic geometer's way of talking about an 't Hooft operator of gauge theory. Once that was clear, it was basically all clear, with the main ingredients all in place, super Yang-Mills theory compactified on a Riemann surface, look at the right complex structure, use the Kapustin-Orlov story for the B-model, and use the 't Hooft operators for the A model. It was basically all clear, although there were a lot of details to figure out, and it was hard to write a paper, because there was a lot to explain. That paper was written in 2006, and I was busy with things related to it in much of the time through 2008.

Zierler:

When did you start thinking about axions in string theory?

Witten:

Oh, I had always been interested in axions in string theory. It was honestly a completely trivial observation at the time, but as soon as Green and Schwarz had cancelled the anomalies, it was clear that a general property of string theory was to generate a lot of axions. I had pointed that out in a paper in 1984, I think the first paper I wrote after Green and Schwarz.

When you asked about axions and string theory, you might have been motivated in asking by my 2006 paper with Peter Svrcek, which has that title. But there's another paper you should consider, a much later one with astronomers Ostriker, Lam, and Tremaine. Honestly my contribution was modest. Ostriker and Tremaine have been very interested in this idea of "fuzzy" dark matter. The idea is that dark matter is made of a scalar particle with a De Broglie wavelength so long that the particle doesn't comfortably get compressed into a dwarf galaxy. This would be an important factor in limiting the dwarf galaxies that we'd see in the universe. And there are hints from observations that something like this might be happening, although it's highly contested. Astronomical data is murky, and part of what makes a great astronomer is being able to judge what data you should rely on, and what's important. That's a knack that Jerry Ostriker has, I've seen him to have it over the years.

My involvement came because he asked me if it was a totally crazy idea that there's a scalar that's so light that its De Broglie wavelength would be the size of a dwarf galaxy. And I said, "No, that's not crazy at all, because string theory naturally generates pseudoscalar or scalar particles with exponentially small masses." My contribution to the resulting paper was pretty minor. I wrote a section on why it would be plausible in particle physics. But I was not responsible for any of the astronomy. Jerry, however, kept telling me that he wouldn't be comfortable writing that paper without a particle physics coauthor. So, I guess I was the one.

Zierler:

(Laughter) Ed, when the LHC turned on, what were some of the hopes at that point? What were you following?

Witten:

Well, I was there, because we were on sabbatical at CERN from 2008 to 2009. Definitely we were hoping that the Higgs particle would be discovered. We were hoping the LHC would find a natural mechanism explaining the mass scale of the Higgs particle. But the hopes were not as high as they had been earlier. Because the precision of the LEP data had really presented obstacles for attempts at natural explanations of the Higgs particle, especially for nonsupersymmetric attempts, but even for the supersymmetric attempts. So, I think many of us by the time the LHC turned on were not as optimistic. We were definitely still hopeful that the LHC would discover a mechanism behind the Higgs particle, but one was aware by that time that there were counterhints. I think they literally turned on the LHC the day we arrived in Geneva. That was a coincidence; we had made our flight reservations long before they had scheduled the LHC opening. But then they had the famous accident ten days later, so we didn't end up having the fun we hoped we would have, of being in Geneva for the first year of the LHC experiments.

Zierler:

Intellectually, was CERN a good place to be during your time there?

Witten:

Oh, it was definitely stimulating.

Zierler:

Who were some of the people you worked with at CERN?

Witten:

I talked a lot to many of my colleagues. I learned a lot from many of the people there, but I didn't really write papers with others.

Zierler:

At this time, you were writing a lot about Yang-Mills theory as well.

Witten:

I'm always writing something about Yang-Mills theory. From roughly 2003 to 2005, I had been mainly interested in gauge theory scattering amplitudes, which means Yang-Mills theory. And the story about geometric Langlands also involved Yang-Mills theory, supersymmetric Yang-Mills theory in that case.

I think we should jump ahead to when I was back in Princeton after CERN. Around 1988 or so, I had interpreted the Jones polynomial of a knot in terms of three-dimensional gauge theory. But by this time, mathematicians had discovered a more refined invariant called Khovanov homology that you associate to a knot. In the three-dimensional picture, a knot is the orbit of a charged particle in three-dimensional space-time. And what you associate to a knot is the quantum amplitude for the particle to go around that orbit. In Khovanov homology, a knot is a physical object, and you associate to it a space of quantum states. And I was extremely frustrated that I had had this nice picture for the Jones polynomial, but it had this elaboration that mathematicians had discovered which I didn't understand physically. And there actually was a proposal to understand it by Gukov, Vafa, and Schwarz, the same Schwarz who keeps appearing in our story (laughter). You should interview him, if you haven't already. He has had a very interesting life. But maybe we shouldn't get into that right now.

Anyway, they had made a proposal, but they hadn't really developed it properly. It was obvious that it had some truth, but it was also obvious that it needed to be better understood. But by this time, I realized that to understand Khovanov homology was going to use the same mathematical ingredients that I had used to understand the geometric Langlands correspondence. I felt that my work on the geometric Langlands correspondence, in some sense, was the right answer of what that correspondence means, but that mathematicians weren't going to be able to understand it really. Because the ingredients of quantum field theory were too unfamiliar for them, and too difficult to make rigorous.

And it occurred to me that Khovanov homology was another important chapter in mathematics which could be understood using the same ingredients, but where there was a good chance that they could understand it and appreciate what I would do. And as a result of this and other things as well, for about two years, my main interest was to understand Khovanov homology from the point of view of gauge theory. It involved what I called a new look at the path integral of quantum mechanics. There was that one non-trivial step, and then beyond that, relatively standard steps involving, well, known string theory dualities applied in a new situation. Anyway, I did arrive at a satisfactory picture of Khovanov homology in terms of gauge theory. Mathematically, unfortunately, it's an unfinished story. I think that there's a good chance that mathematicians will be able to understand it rigorously, but that hasn't happened yet. The one who's most developing it is Cliff Taubes at Harvard. I hope his work will come to a conclusion soon.

This takes us up to about 2012, I think. And then for about two years, I worked on yet another slightly exotic problem. The original reason to take string theory seriously was that perturbative string theory gets rid of the ultraviolet divergences of general relativity. But perturbative string theory was not formulated in the elegant way that it deserved. And it was known that to do so, you should formulate it in terms of what are called super Riemann surfaces, rather than ordinary Riemann surfaces. And this is the last time I have to mention Albert Schwarz to you, because he had been one of the pioneers of super Riemann surfaces in about 1985 or so (laughter). I think it's the last time I'll mention Albert Schwarz. We'll see if- it was known by some of the pioneers that superstring perturbation theory should be formulated in terms of super Riemann surfaces. Albert Schwarz probably would have wanted to but didn't quite have the detailed physics background. Friedan, Martinec, and Shenker probably would have wanted to, but found it difficult. They found a shortcut, which enabled them to formulate string perturbation theory in 1985 in a way that's satisfactory for many purposes, but sort of hides some of the beauty of the subject. I thought the subject deserved to be developed with its natural beauty, and I actually spent about two years doing that. To remember which two years, I'd actually have to look at the papers, but maybe 2012 to 2014 roughly.

That problem was way out of the mainstream. I've now told you about two years each on three problems that were way outliers compared to the interests of my colleagues. One is the application to geometric Langlands, which I'm sure is important, even though I went into it knowing that the payoff was not going to happen anytime soon, because mathematicians would have great difficulty understanding that work. The second was Khovanov homology which I went into judging that a payoff might be accessible in a reasonable time. I viewed it as unfinished work. I had had twenty years before the story about the Jones polynomial, and clearly to finish it, you had to understand Khovanov homology. But I also saw it as a bridge. I thought mathematicians wouldn't be able to understand what I had said about geometric Langlands but might be able to understand what I could say about Khovanov homology.

And the third was on superstring perturbation theory. That was really a niche topic. I was the only one in the world, I think, who thought that it was worthwhile to spend two years to formulate superstring perturbation theory more elegantly. So, these three problems were extreme outliers compared to the interests of my colleagues.

Zierler:

In 2012, it was the twenty-fifth anniversary of the superstring theory books with Schwarz and Green.

Witten:

Yes.

Zierler:

Do you recall in the twenty-fifth anniversary edition what needed to be updated, and what was good from the beginning?

Witten:

We didn't update it. The book was just republished. Advertised as the twenty-fifth anniversary edition, but it was the same book. Updating it would have been a vast effort. It would have practically involved writing a new book.

Zierler:

Did you consider it?

Witten:

I did not.

Zierler:

But certainly, it sounds like the field needs a new book.

Witten:

Well, it's too big a task. I hardly even see it that it's humanly possible. Polchinski wrote two volumes on string theory. Are you familiar with his book?

Zierler:

Yes.

Witten:

It's actually quite a good book. The first book is excellent, and I think most people trying to learn string theory today would do well to start with volume one of Polchinski. Volume two is too sketchy to learn from systematically, I think. You would need to supplement it with other sources. Volume two covers the post-1995 period. That's why I'm mentioning it to you. Our books, of course, were pre-1995. Polchinski's books appeared in early 1998. Volume one deals with material that existed before 1995, but Volume two, he tried to be up to date. And the trouble is that you can't humanly do that in a book. It's too vast. There's also a book by Schwarz and the Becker sisters, which covers the post-1995 material. I think it's too sketchy to learn from systematically. But it would just be too much work to try to write a book. I have written some review articles, though, in recent years, on manageable topics.

Zierler:

Can you tell me about your interactions with Louise Morse and why that was important for you?

Witten:

(Laughter) Well, like most physicists, I had never heard of Morse theory, but at a meeting in Cargèse I think in 1979, Raoul Bott and Michael Atiyah took it upon themselves to educate physicists about Morse theory. I'm not sure, but I think it might be that none of us had ever heard of Morse theory before. Certainly, I had not. And I filed it away but didn't make anything much of it at the time.

Later, though, during my early years at Princeton, I was trying to understand why spontaneous breaking of supersymmetry is different from the spontaneous breaking of other symmetries. So, you asked me what I did in the early years at Princeton, that was part of it. There were enough other things that I didn't tell you about this. Anyway, I found it puzzling that supersymmetry was hard to break and supersymmetry breaking has a different flavor from breaking of other symmetries. And I kept thinking about simpler and simpler models, and the same thing was true in simpler and simpler models. And finally, I went all the way down to a quantum mechanical system of finitely degrees of freedom. In this model, supersymmetry breaking was still weird.

And finally, one day- I think I was in a swimming pool in Aspen- I realized that that had to do with Morse theory, and that I was stumbling onto a sort of physical interpretation of the Morse theory that Atiyah and Bott had tried to educate us about a few years later in Cargèse. So, I wrote a paper called "Supersymmetry and Morse Theory" where I tried to- well, there were two ways to look at the paper. One, that you were trying to explain for physics why supersymmetry breaking is strange. The other, that you were trying to explain Morse theory from a supersymmetric perspective. The paper could be read in either of those two ways.

Anyway, to answer your question, this led to my meeting Louise Morse and I guess we got to know her pretty well over the years (laughter). Yes, she used to host a gathering of the math department, after the Marston Morse lecture, which was an annual event at the Institute. So, while she would host that, I was often at those gatherings. Scientifically, Morse theory keeps coming back with new applications. Andreas Floer, a young mathematician who unfortunately died very young, in approximately 1985 or 1986, probably inspired in part by my work on Morse theory, developed what people called Floer homology of a three-manifold. It's a three-dimensional theory related to Donaldson theory of four manifolds.

And when I saw Michael Atiyah, I think in the spring of 1988, when he visited the Institute twice, he was very excited about Donaldson and Floer theory, and thought it was something a physicist should try to understand. I told you this before very briefly, but I'm telling you more about some aspects of it now. Anyway, I struggled with that. It didn't look like it made sense, because Floer had a supercharge that had spin zero. Well, in physics, supersymmetric charges have spin one half. So, it looked like that was an obstruction to making sense of Floer's theory as a physicist. But one day I realized that if you made a sort of topological twist, as I called it, of the N=2 super Yang-Mills theory, you could actually get Floer theory. So that was how I started working on Floer theory of three manifolds and Donaldson theory of four manifolds.

And for a while, I hoped that that would lead to rapid progress with four-manifold theory, but it didn't. I think Atiyah understood intuitively something that I struggled to understand for a couple years, which was that anything I could do for weak coupling would simply be to redo what the mathematicians had already done. But when Seiberg and I discovered a strong coupling understanding of N=2 super Yang-Mills, in the spring and summer of 1994, that did lead to rapid progress in four-manifold theory. That led to my work—I wrote a little paper called "Monopoles and Four-Manifolds." I think that's what it was. Anyway, developments related to Seiberg-Witten theory were very important in understanding four-manifold theory. I've tried to explain to you that it was linked to Morse theory. That was how we got into it. I connected Morse theory to supersymmetry, Andreas Floer generalized this approach to gauge theory, I took his work back into physics, and then, following my work with Seiberg, once we had a strong coupling picture, that led to progress with four-manifold theory.

But if we go back to Khovanov homology, the Jones polynomial looks like it's miles away from Morse theory. There was no supersymmetry in sight when one described it in terms of Chern-Simons gauge theory in three dimensions. But by the timeI had given a physical interpretation of Khovanov homology- well, I had used some dualities that were by this time standard, together with one novel idea, what I called the new look at the path integral. Anyway, Morse theory was part of the picture by the time I was explaining Khovanov homology. I've answered you, roughly speaking, about what Morse theory has meant in my career. I probably told you more about that than I told you about Louise Morse. The memory of Louise Morse that immediately comes to mind is the gatherings she would host after the Marston Morse lectures, which she continued to host until the age of about one hundred.

Zierler:

Tell me about your work on branes and supergroups.

Witten:

Well, it sort of should be considered in the general direction that led to this Khovanov homology story. Khovanov homology had to do with studying a certain configuration of branes, branes ending on other branes. It's a configuration that had been studied a lot by other physicists, and then a lot by Gaiotto and me in 2008. So, I knew a lot about this brane construction. And after a lot of struggling, I understood that Khovanov homology would be interpreted physically in terms of a configuration of three-branes ending on a five-brane. The work you are asking me about involves a generalization with three-branes on both sides of the five-brane.

Again, this had been studied by other physicists and also by Gaiotto and me back in 2008. But the work on Khovanov homology suggested new questions. And so, with the student Victor Mikhaylov, we investigated that. A one-sentence answer is that we did a two-sided version of what I had done a one-sided version of for Khovanov homology. To my great regret, Victor has left physics and is now working in mathematical biology. I think he knew a lot of things that nobody else knew, that he didn't publish before he left physics. I tried to ask him, frankly, to write up whatever he knew before he left. As well as being very brilliant, he's a perfectionist. So unfortunately, writing up what he knew would have been a huge amount of work for him.

Zierler:

I'm curious about your motivations in writing your paper "What Every Physicist Should Know about String Theory" and the extent to which part of that was the fact that many physicists have lost patience with string theory or think that it's a waste of time.

Witten:

That wasn't the motivation at all. Well, first I just gave a lecture with that title- I think the first time was in India- well, I had some practice giving lectures on that subject. String theorists make the assertion that string theory eliminates the ultraviolet divergences of standard Feynman diagrams. But where is that explained in a way that any physicist could understand who doesn't actually want to be a string theorist? I think it's a very fundamental observation, but where is it explained in such a way that anybody reading *Physics Today* could understand it? I felt nowhere, so I wrote that article. I literally wrote that article because I think there was something important that could be understood by a much wider community of physicists than did understand it. So, I took a crack at doing it. It's impossible for me to know if it was at all successful.

Zierler:

Well, people could have told you as much. People you may have surprised.

Witten:

Well, I got very little feedback from that article. Let me mention a few other articles that have gotten very little feedback, and then you'll see that I wasn't surprised. I was invited by the editor of the *Bulletin of the American Mathematical Society* to write an article for them on any topic I chose. This was in about 2005, maybe. And the *Bulletin* has articles that are more technical than *Physics Today* articles, but it's as close an analog as the math world has to *Physics Today*. In other words, not all the articles are extremely technical. There's also the *Notices of the American Math Society*. What I wrote was probably too long and technical for the *Notices*, but I am not sure.

Anyway, it occurred to me that the LHC was being built, and the Higgs particle would probably be discovered, and that was going to be news. And most people reading the news would have no idea what it was all about. But I could see there was a community of people who could potentially understand what the Higgs particle was all about, but who actually had no idea, namely mathematicians. I realized if you approach it the right way, you could explain the idea of gauge symmetry breaking for mathematicians. And so, I wrote an article explaining gauge symmetry breaking in three problems- superconductivity, four-manifold theory, and weak interactions. Now, literally I wrote it for the same reason I wrote that article that you asked me about in *Physics Today*. There was a community of people who could understand something they didn't understand, and I thought I could explain it to them. Neither of the two articles generated any feedback to speak of, so whether either one was successful, I can't tell you.

Zierler:

More recently, you wrote that perturbative superstring theory needed to be revisited. Why so?

Witten:

As I remarked earlier, I felt it had not been formulated with the beauty it deserved. A proper formulation involves super Riemann surfaces. I told you that formulating superstring perturbation theory in its natural habitat was my main interest for about two years. So, there was a series of papers. The one that you just asked me about was the most important in that series. There were a few follow-ups, and there were a couple papers on backgrounds, review-like articles. And then there were a couple follow-ups. But the one you asked me about was the main paper.

Zierler:

Even more recently, of course, it’s also going to be a speculative answer—what is your reaction to the g-2 muon anomaly experiment that so many people are excited about at Fermilab?

Witten:

Well, throughout my career, I have always been conservative, sometimes too conservative. I gave you the example where I didn't believe Montonen-Olive duality even after Olive and I had found what later was regarded as strong evidence for it. Because I interpreted the facts in the most narrow way. So, with that kind of outlook, you can maybe guess what I'm going to tell you. I'll believe that there’s a real anomaly when the lattice gauge theory improves and gives definitive results.

Zierler:

What will it take for the lattice gauge theory to improve?

Witten:

The very most recent lattice calculations suggest that there’s not a discrepancy. I'm not sure how familiar you are in technical detail. Conventional even computer-aided calculations of Feynman diagrams aren’t enough to calculate the muon g-2 with sufficient precision to compare to the incredibly precise measurement. You also need input about QCD effects. There’s a very small QCD effect due to photon-photon scattering which is not calculated accurately by any method. It’s believed to be small enough to not be important, but that isn’t rigorously known.

But even if we assume that it’s true that that’s small enough to not be important, there’s a QCD effect that’s big enough that it’s definitely important. It’s of the order of one hundred times the experimental effect. This effect can’t be computed reliably from first principles. It’s inferred from other experiments with the help of a lot of modeling. Lattice gauge theory is approaching the precision needed to reliably compute the QCD effect in question to sufficient accuracy. If lattice gauge theory reaches the precision of the experimental discrepancy, then that would make it extremely clear whether there’s a problem or not. Literally the very most recent lattice computation agreed with the Standard Model. *Science* magazine had an article that I thought was highly misleading about this anomaly. Then a week or two later, they had an article—maybe even by the same author; I don’t remember- that I thought contradicted the first article but told the story better, explaining that there are lattice calculations according to which there isn’t a g-2 problem.

Zierler:

That’s where you see this headed.

Witten:

I wouldn't say where I see it's heading, but I think if you want to be conservative, you'd say that that's definitely a possible outcome. I would say that there isn't a convincing case as of now that there's a discrepancy with the Standard Model. It's something to watch. There might be a discrepancy. But it will only be clear when the lattice calculations are more precise. They don't have to get much more precise. They're reaching the precision that will answer the question. On the time scale of the experiments, we'll know- an improvement of the experiment is expected to take a few years. I think it's quite likely that within a few years, we'll know more from the lattice.

I don't know if I've explained it adequately. People calculate Feynman diagrams as much as they can. That gets you eight digits or whatever. But the experiment is at ten- or eleven-digit precision. The experimental discrepancy is much smaller than a QCD effect that can't be computed reliably, even with any kind of conventional calculations, short of the lattice. And lattice is only now reaching the precision that's needed. So traditionally, that effect has been computed by taking data from other experiments from which you can extrapolate to what's needed for the muon g-2.

That obviously raises a lot of questions. How accurate were the other experiments they're taking the data from, and how reliable are the way they make these extrapolations? You have to remember that they're estimating something which is roughly a hundred times bigger than the experimental effect you're trying to pin down. I hope I'm correct with those numbers. So, to be confident there's a discrepancy, you have to have very high confidence in the precision with which the QCD effect has been extrapolated from other observations. Since lattice gauge theory is reaching the precision at which it can test that, and eventually supersede having to rely on a complicated series of interferences from other experiments, the picture is going to change.

You asked me that one question, but you could have asked me about discrepancies in B-physics. At any given time, there are discrepancies in B particle decay. The only thing is that they change from year to year. [laughs] Maybe it will stabilize at a clear indication of physics beyond the Standard Model. But you have to be a little bit, well, cautious. How should I put it? It's reasonable to be a little bit conservative. Experimental physics is really hard (laughter). And trying to understand the g-2 of the muon at the ten-digit level, when there are essentially incalculable effects at the seven- or eight-digit level, is an amazing thing to be able to do as accurately as they have.

Zierler:

If not B-physics, if not g-2, when over the course of your career has there been most optimism that we would find physics beyond the Standard Model?

Witten:

Well, we did find it with the neutrino masses. As I told you earlier, if you go back to the 1980s, there was very high confidence that at the weak interaction energies, one would see physics beyond the Standard Model that would explain the electroweak scale. That has proved to be wrong, as far as we can see, but in that period, there was very high confidence. There were many times when discrepant experimental findings that could have been physics beyond the Standard Model were published, but proved to go away with better analysis, or in some cases better experiments. It's difficult to remember now which of those were the most serious, if that was what your question was about. At CERN, during the period when they discovered the W and Z, there were discrepancies for a while, which were in that case largely resolved with better modeling, actually. In other words, more accurate modeling of the QCD effects.

Supposedly the Standard Model has been confirmed, but if you look at it in detail, there definitely are discrepancies that were not fully explained. SLAC measured the front back asymmetry in B particle production. It didn't agree as well as expected with the Standard Model. Nobody has had the capability to precisely repeat that experiment. That's sitting on the shelf.

There was a Fermilab experiment that measured deep inelastic neutrino scattering where it's incredible you can get the high precision they did. Namely they agreed with the Standard Model at the two percent level or something, whereas according to estimates of their errors, it should have agreed at better than one percent. Again, nobody has had the capability to precisely repeat that. You asked me a question about one possible discrepancy from the Standard Model, which was the muon g-2. It is a little misleading to discuss just this one example in isolation.

Zierler:

It's only because it's in the news right now.

Witten:

Okay. But anyway, there are things that are not in the news right now because they're not new, but they're sitting on the shelf and have never been explained. The big picture is that the Standard Model has survived an incredible number of tests. In detail, there are a few apparent discrepancies. They might be something important, but experimental physics is hard. It's difficult to estimate how likely it is to have- it's difficult to say how many independent tests of the Standard Model there are, and how many discrepancies there should be at the level of three sigmas or whatever. You can try to make such an estimate, but it's going to be unconvincing, because it's impossible to estimate the probability that systematic errors have been underestimated. So, you can say how many sigmas you're off, but in reality, that's not a good notion. The only way to be sure something is really a discrepancy is to repeat it, with a better experiment. And I mentioned to you a couple where nobody has had the capability to repeat with another experiment. B quark physics is better because it's constantly being improved, which is why I could tell you that at any given time, there are discrepancies, but they tend to not persist.

Zierler:

As you say, the Standard Model is remarkably resilient. To what extent can that be understood as a function that we don't yet have the capacity to test string theory?

Witten:

Well, we don't need string theory to discuss this question. The Standard Model has held up much better than its inventors expected, I'm sure. That's obvious. But also, I've seen it sometimes discussed in print by some of the inventors. Even in your interview with Howard Georgi, that was clear. Howard was reflecting the views that prevailed at Harvard in the 1970's, where some of the inventors were working. I came shortly after the period that I think he was talking about. Nobody in that environment expected the Standard Model would hold up at weak interaction energies.

Zierler:

To bring the conversation right up to the present, as we discussed right at the beginning, your interest in quantum information. And you said you don't yet know how you might break into the field. What might be some possible avenues?

Witten:

Well, when I was a graduate student, I sat down one day with piles of paper preprints. We didn't have the archive. I'd sit down with piles and piles of paper preprints, and go through them, trying to find something I might do. The most interesting calculation I did as a student- I told you about it- was this calculation of deep inelastic photon-photon scattering, which was inspired by a paper I saw by Roger Kingsley, who studied the question but not quite with the most modern QCD ideas. So, when I was a graduate student trying to break in, I would go through piles of preprints. I guess the equivalent now is to look at papers in the archive and try to see what I might do. And I have made some minor contributions, actually, but I don't feel like I've fully become engaged with the subject, as I have with other subjects in the past.

Zierler:

Given how conservative you say you are, in light of all of the optimism with the discovery of the Higgs that much more would be seen at the LHC, and in light of where we are now nearly ten years later, what are the big takeaways you have from that?

Witten:

Well, I think the big question is whether it's a miniature version of the cosmological constant problem. The historical view of particle physicists was that the weak interaction scale would have a natural explanation. Experiment seems to disprove that. And the existence of the Higgs at the mass it has, without anything else that would come with it that would give a natural explanation, looks like it might be a miniature version of the cosmological constant problem. Whether that's the right interpretation is one of the big questions. If that's the right interpretation, it does point toward a multiverse.

Zierler:

If not quantum information, what else are you currently working on?

Witten:

Oh, I am currently working on something else, but it's more like- I'll tell you about it in a second. But what I really think is the exciting direction in the field are the connections of quantum information and gravity. With that said, there was a new twist on geometric Langlands that was discovered by mathematicians in the last couple years. And with Davide Gaiotto, a frequent collaborator of mine since 2008, we're showing that it can be understood using a rearrangement of the same ingredients Kapustin and I had used in 2006 to understand the traditional formulation of geometric Langlands. We're actually writing a pair of papers. That's the second one. The first one is a little bit hard to explain technically, but it involves a new perspective on the general idea of quantizing a classical phase space.

Zierler:

Ed, now that we've worked right up to the present, I'll ask one last question that has both retrospective aspects to it and will also give a sense to what the future might hold. You've written before, "When one is doing research, the trick is to find a problem simple enough that one can solve it, but interesting enough that solving it is worthwhile." It's a striking sentence in its simplicity and its elegance, but it's also, I imagine, extremely difficult to execute.

Witten:

Yes.

Zierler:

If you can reflect over the course of your career, when has this dictum, if we can call it that, been most fully realized? And in what areas of your work, either because the problem is not simple enough but you can't help but work on it because it's interesting, or where you're interested in it but others may not find it worthwhile, what have you found is the most effective ways to perform up to that dictum?

Witten:

Well, as I said, it's the most difficult part of research, to do that. There were various times in my career where I lost a lot of time on things that were too hard. The most outstanding example was right at the beginning when I was obsessed with the problem of quark confinement, where- well, it was too hard to get the kind of understanding I wanted. Even today, we don't have it. As I told you, it wasn't until 1994 with the work with Seiberg that I was able to make even a small contribution there.

Well, the dictum you've quoted summarizes well the difficulty I've had getting involved with quantum information theory and gravity. I found a few things that I could do, but they were a little bit too narrow to really make me think that I was getting involved where I wanted to. And I haven't quite found the right avenue. But I haven't given up (laughter). I do have the feeling that's the direction where something big is most likely to happen. You see, there isn't a general understanding of what string/M-theory mean. And there's something missing in the general understanding of quantum gravity. The biggest hope would be that those two would somehow make contact with each other.

Zierler:

Do you think it will happen in your lifetime?

Witten:

Well, I hope so. Even better if it happens soon enough that I can contribute, which is possibly a stronger criterion.

Zierler:

Ed, it has been a great pleasure spending this time with you. I want to thank you so much for doing this.

Witten:

Oh, you're welcome.

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Quantum chromodynamics (/taxonomy/term/10066), Quantum gravity (/taxonomy/term/5716), String models (/taxonomy/term/7061), Superstring theories (/taxonomy/term/9241), Supersymmetry (/taxonomy/term/7106)

Additional Persons:

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