Round Table Talk:
Conversation with Nathan Seiberg

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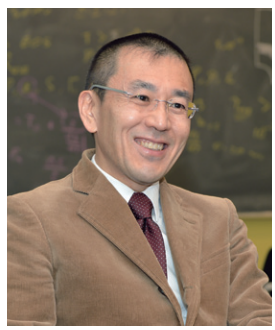
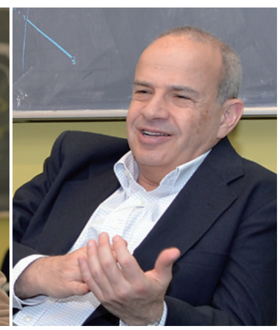

Hirosi Ooguri


Nathan Seiberg


Yuji Tachikawa

Ooguri: Over the past few decades, there have been remarkable developments in quantum field theory and string theory, and you have made significant contributions to them. There are many ideas and techniques that have been named after you, such as the Seiberg duality in 4*d* *N*=1 theories, the Seiberg-Witten solutions to 4*d* *N*=2 theories, the Seiberg-Witten map of noncommutative gauge theories, the Seiberg bound in the Liouville theory, the Moore-Seiberg equations in conformal field theory, the Affleck-Dine-Seiberg superpotential, the Intriligator-Seiberg-Shih metastable supersymmetry breaking, and many more. Each one of them has marked important steps in our progress.

Today, we would like to look back on the history and hear from you how you made these discoveries and your perspective on the future development of quantum field theory and string theory.

Seiberg: Before we start, I would like to thank you very much for your invitation and for your kind words. The hospitality at Kavli IPMU during my visit here has been fantastic. The two of you, the Director, the rest of the faculty and postdocs, and the administrative staff have gone out of their way to help me and to make the visit successful and productive – it is quite amazing. I don't remember being treated like this, so I'm very thankful and embarrassed.

Ooguri: Thank you for your kind words.

You received your Ph.D. at the Weizmann Institute in 1982 and immediately went to the Institute for Advanced Study in Princeton as a postdoc.

Seiberg: That's right, I came there for my postdoc.

Ooguri: That was before my time. I became a graduate student in 1984, and I remember reading your papers intensely after the so-called first superstring revolution since I wanted to catch up with what was known about supersymmetry. You started to work on supersymmetry almost immediately or maybe a year after you went to the Institute, is that right?

Seiberg: Almost immediately. I remember studying supersymmetry during the 1982/83 Christmas break.

Ooguri: So, you changed the direction of your research completely after arriving the Institute. I understand that, at the Weizmann, you were working on model building, something related to technicolor.

Seiberg: Yes, it was not successful. So when I moved to my postdoc position, I thought I should change direction and I thought that supersymmetry was interesting.

Ooguri: What did you find most interesting about supersymmetry at that time?

Seiberg: Supersymmetry looked like an interesting intellectual structure, which might also be relevant for phenomenology. Michael Dine had been working on supersymmetry before and he encouraged me to learn it. He recommended that I study the Wess and Bagger book, which was still in preprint form. (Julius Wess had just completed a series of talks at Princeton the year before. And Jonathan Bagger, who was a student at the time, took notes, which eventually became the famous book.)

Ooguri: Was Michael (Dine) also a postdoc there?

Seiberg: Michael was a 5-year member at IAS and I came in as a fresh postdoc. He took me under his wings and became my mentor. He had a huge impact on me. Our first project was about the dynamics of supersymmetric QCD. It's hard to imagine now how confusing it was at the time.

Tachikawa: Then came your paper with Ian Affleck and Dine on supersymmetry breaking.

Seiberg: The first paper we wrote was with Anne Davis, where we wrote the superpotential based on symmetries. Even that was not obvious at that time. Crucial earlier work on the subject had been done by Edward Witten.

Ooguri: Are you talking about Witten's paper on the index?

Seiberg: There were two papers. One about the index and the other was called dynamical supersymmetry breaking. I remember studying these papers very carefully because they were full of wonderful things. In particular, the behavior of supersymmetric Quantum Chromo Dynamics with massive quarks was analyzed. However, at the time there was no coherent picture about the behavior of the theory when the quark masses vanish. We wrote a paper with Anne about the analysis of the flat directions of the theory and we argued that a certain superpotential should be generated there. Our analysis was based on consistency conditions and the symmetries of the problem, but we did not prove that this superpotential is indeed generated.

Supersymmetry and Non-Renormalization of Superpotential

Seiberg: Then Anne left and we started working with Ian Affleck. That summer we wrote a paper showing that in some circumstances this superpotential is generated by instantons. This was very surprising at that time because people had believed that the non-renormalization theorem was exact and was also true non-perturbatively.

Ooguri: Were you convinced that the standard argument was wrong?

Seiberg: Most of the previous arguments for the non-renormalization theorem were based on Feynman diagrams and were intrinsically perturbative. They did not apply to instantons. I remember that the instanton computation itself took maybe a week. Then it took another two months to go through all the arguments in the literature and to debug them.

So we wrote a short letter and then we wrote a longer paper deriving the superpotential···

Tachikawa: This is the Affleck-Dine-Seiberg superpotential.

Seiberg: We showed that, depending on the number of flavors and colors, it is generated by instantons or by gluino condensation. For more flavors, no superpotential is generated and the vacuum degeneracy of the massless theory is not lifted. For me, this was the biggest surprise. These are quantum field theories with several ground states.

Ooguri: Continuously many ground states parameterized by the moduli space.

Seiberg: For me that was shocking.

Ooguri: So, some of the very important concepts in supersymmetric theory appeared within a year or so.

Seiberg: There were precursors of that in previous papers, primarily several papers by Edward Witten and in particular, a paper by Ian Affleck, Jeffrey Harvey and Edward Witten, which had analyzed the same problem in 2+1 dimensions.

We also spent some time trying to build models of particle physics phenomenology. So we were primarily interested in breaking supersymmetry. The theories with a moduli space of vacua were a nuisance. They could not be used for that purpose.

Then in the summer of 1984 Michael Green and John Schwarz wrote their revolutionary paper and the physics world changed overnight.

First Superstring Revolution in 1984

Ooguri: When did you hear about it?

Seiberg: I heard about it in late August or early September. Everybody was talking about this fantastic breakthrough, but none of us, the postdocs, had any idea what the breakthrough was about. There was some mysterious theory, string theory that we had never heard about. This was something that the older people had worked on and abandoned. They thought, "This was a failed attempt and younger people should not learn about it."

Ian Affleck left for a year in Paris, and Michael Dine and I had to decide, "Are we going to pursue our work on supersymmetric gauge theories, or are we going to study this new thing?" And we decided to study string theory. That fall David Gross gave a fantastic string theory course at Princeton University, mostly in order to educate the young people. The course was very popular and it was attended by all the postdocs at the university, all the postdocs at the Institute, all the students, and many faculty members. It was given in a huge lecture hall.

Tachikawa: Was it before the heterotic string was constructed?

Seiberg: During that fall the heterotic string was constructed, Calabi-Yau compactifications were discovered, and many other major developments happened. Many of us felt very difficult to keep up. As we were learning the basic material, like the bosonic string, many new discoveries took place.

Tachikawa: Did David Gross cover all of those latest developments during the course?

Seiberg: His course was mostly about the bosonic string.

Tachikawa: Were notes of this course ever published in some form? That might also be an interesting historical document.

Seiberg: I think David followed the available reviews at the time. Later, when I was at the Weizmann Institute in Israel and then at Rutgers, I taught string theory and I used my notes from that course, expanded them and updated them.

During the following years Michael Dine and I continued to collaborate on string theory.

Ooguri: Even though you put your work on supersymmetry aside, some of your works in string theory in the following few years were clearly taking advantage of tools that you had developed, such as your work on worldsheet instanton.

Seiberg: Sure, that is true, without doubt. In particular, thinking about moduli space of vacua, looking for non-perturbative effects that can lift the degeneracy, and controlling them by holomorphy were central in our work on string theory.

A typical interesting example of that occurs in various instanton computations. There the real part of the answer, which is the instanton action, is combined with an imaginary part, which is the topological charge, to give a holomorphic answer. This guarantees that the instanton can contribute to the superpotential. At that time, it looked like a complete magic. Affleck, Harvey, and Witten noticed it in 2+1 dimensional field theories, Affleck, Dine, and I saw it in 3+1 dimensional field theories, and then the same thing happened in worldsheet instanton contributions to the spacetime superpotential in string theory. Every time we saw that, it looked like a miraculous consistency condition, and we did not know a deep underlying reason why this occurred. Of course, today this is completely understood.

During that time Michael and I tried to understand various phenomena in string theory from a macroscopic perspective. We asked ourselves how to describe them from a perspective of a low-energy observer. And we tried to summarize many results obtained by worldsheet techniques in a spacetime effective action and in particular, in the spacetime superpotential. Our motivation was to see to what extent string theory can lead to phenomena that a low-energy observer would be surprised by. If I recall correctly, many of the "miraculous" results that people had found using microscopic (worldsheet) reasoning turned out to have simple macroscopic explanations.

In fact, this line of investigation helped uncover a number of interesting and subtle effects in the worldsheet technology. In hindsight, the main tool we used was the constraints from holomorphy of the spacetime superpotential, which is associated with spacetime supersymmetry. It is not manifest in the worldsheet computations and therefore, it leads to powerful constraints on the allowed answers.

Ooguri: I think one of your papers had a title which actually expressed that point of view: "Microscopic knowledge from macroscopic physics in string theory."

This was the opposite of what most of the other people were doing, to try to derive macroscopic results from a microscopic description. You advocated that generic properties of the low-energy effective theory can help elucidate some of the microscopic properties.

Seiberg: That's right because some of the symmetries are manifest in the low-energy theory, but are not manifest in the short-distance computation. An example of that is the string non-renormalization theorem. The original proof was based on worldsheet methods, but it was not sensitive enough to detect some subtleties associated with contact terms. Michael and I found another proof, which was based on the holomorphy of the spacetime superpotential. That macroscopic proof was totally conceptual and very elementary. And it pointed to special situations where nontrivial renormalization could take place. This was discussed in a later paper with Michael Dine and Edward Witten.

This was puzzling because the worldsheet methods suggested that there are no such special situations. A detailed analysis of this case has shed new light on the worldsheet methods and uncovered the significance of contact terms, which had been ignored in earlier work.

By having a simpler proof, you sometimes understand what really goes into it. If you have something very complicated with a lot of moving parts and it's not quite clear what is and what is not essential, it's hard to see how to go around a proof. But with a simple proof, it was clear. This was another example of how the low-energy description points to a loophole.

Conformal Field Theories and Topological Field Theories

Ooguri: Some of your works also had very deep and broad impacts in mathematics, such as your classification of conformal field theory and polynomial equations, which characterize fusion and modular invariance, with Gregory Moore.

These works also had impacts on physics, for example in topological phases in condensed matter physics. So, deep mathematical structures that came out from quantum field theory have had broad applications.

Your work with Greg on the classification of conformal field theory appeared just before I arrived at the Institute. Could you tell us how this work came about?

Seiberg: From my perspective it started with the work of Daniel Friedan and Stephen Shenker. (Greg Moore had related ideas independently.)

Ooguri: They advocated the idea of the space of all conformal field theories.

Seiberg: They had a picture in terms of a certain vector bundle over Riemann surfaces, which at that time I did not understand at all. I didn't even see what they had in mind or where they were heading. Then another piece of this story came with the work of Erik Verlinde. He visited Princeton and he gave a talk on what is known today as the Verlinde algebra and the Verlinde Formula. He described the fusion rules of a rational conformal field theory in terms of integers satisfying certain properties and he suggested that the modular transformation matrix S should diagonalize them. That was stunning. I still do not understand how he got this fantastic insight.

So, with Greg Moore, we tried to understand Verlinde's work. After making some progress we came across papers by Yukihiro Kanie and Akihiro Tsuchiya, which were very mathematical. (Yesterday I was very pleased to meet Tsuchiya for the first time.) But we managed to extract from these papers some simple concepts that we could use. We studied the properties of the transformations of conformal blocks in a rational conformal field theory and showed that they are characterized by a finite set of data. Furthermore, that data had to satisfy a number of highly constraining consistency conditions, which were given by some polynomial equations. In fact, these conditions were so over-constraining that it looked surprising that they have any solutions.

One outcome of these polynomial equations was that we could prove Verlinde's conjecture about the relation between the fusion rules and the modular matrix S. I am still amazed by his intuition. How did he come up with this conjecture?

Although we had a clear and consistent structure, at the time we had no idea what the proper mathematical setting for it was. Useful conversations with David Kazhdan and Pierre Deligne pointed us to the connection to category theory. Of course, at that point in time we did not know anything about category theory. I remember very vividly how I looked at the book that Kazhdan recommended and I said "What am I going to do with this?" And I left for the day. Fortunately, I was working with Greg... The following morning, he gave me a beautiful lecture summarizing all we needed to know from that book.

Ooguri: That's typical of Greg.

That was before Witten's Paper on the Jones polynomial. I remember reading your papers on the classification of rational conformal field theories just before I came to the Institute.

Seiberg: I don't remember all the details, but I think there were several different lines of investigation of seemingly unrelated problems, which came together. One of them was rational conformal field theory, starting with Friedan and Shenker, Verlinde, and our work. Witten was interested in the Jones polynomials. We wrote a series of papers in the spring and summer of 1988. Witten's paper came out in the fall of 88.

Ooguri: Were there interactions between the two directions of research?

Seiberg: I do not know to what extent Witten was influenced by our work. He definitely referred to it in his paper. But it is clear that his point of view, based on a three-dimensional picture was broader, more general and had deeper insights. We had a sequence of transformations in two dimensions,

but we did not realize that we should have thought of it as a theory in three dimensions.

Ooguri: In fact, when Witten calculated Wilson loop expectation values, he reduced it to the two-dimensional problem and used the modular transformation and fusion language.

Seiberg: But he also used certain intrinsically three-dimensional operations. And, of course, he also had a beautiful description of it in terms of a Chern-Simons Lagrangian.

Ooguri: But, nowadays this is used for theories without Lagrangians. So it's coming back to where you were.

Seiberg: Yes, but you still have to assume that there is a three-dimensional Lorentz invariant and even topologically invariant theory underlying the whole structure. In practice, when you compute something, it is not that different from the computations that we did. But we didn't have the better conceptual picture.

Then we tried to complete the dictionary between our earlier work on rational conformal field theory and Witten's Chern-Simons picture. That took another year. After that, Greg moved to Yale and there he did what I think is the nicest application of that body of work. Together with Nicholas Read they found the Moore-Read state.

Clarifying the Liouville Theory

Ooguri: It has had a direct application to condensed matter physics.

Another thing that I wanted to ask you about in this period, before the second superstring revolution, is about your work on the Liouville theory. This was a very confusing subject for many years, and many wrong things had been said about it.

I remember, you came to Kyoto and gave a set of lectures solving many of the confusions in Liouville theory, putting everything in order. You threw out many wrong statements, and what you picked turned out to be all correct. How did you do that?

Seiberg: It bothered me that this conformal field theory does not satisfy the general axioms of a conformal field theory. So it was natural to ask how to relax the axioms such that a coherent picture emerges.

Ooguri: The Liouville theory is non-standard in many ways. The notion of vacuum in the Liouville theory is very confusing, for example. There are states which do not belong to Hilbert Space but still have roles in the theory. You put everything in a meaningful package.

Seiberg: Thank you.

Ooguri: No, I'm not just complimenting you. I just wanted to hear how you did it.

Seiberg: Well, it was very confusing and I remember asking myself these questions.

Earlier Knizhnik, Polyakov, and Zamolodchikov wrote a beautiful paper about two-dimensional gravity. Immediately afterwards, David and Distler and Kawai gave a Liouville interpretation of their results. Then Douglas and Shenker, and Brezin and Kazakov, and Gross and Migdal used a matrix model to describe two-dimensional gravity and their answers were extremely simple and beautiful. In view of that I felt that even though Liouville theory might not satisfy all the standard axioms of a conformal field theory, it should still be a sensible theory. This motivated me to look into it in detail.

In addition to the data from the matrix model there was also a huge literature on the semi-classical Liouville theory. There were many things that had to work and so I just tried to make it work. Then I continued following on that with Greg – we had a very productive time trying to make this connection between the matrix model and Liouville theory more precise.

Of course, since then our understanding of the theory was completely transformed by the work of Dorn and Otto, and the Zamolodchikov brothers and others who found the exact solution of Liouville theory.

Power of Holomorphy

Ooguri: After that, you went back to the supersymmetric field theory in the early 1990s – maybe a couple of years before the second superstring revolution. Did you foresee anything coming? I am asking this since what you did in these few years in the early 1990s on supersymmetry turned out to play essential roles in the developments of the second revolution. What motivated you to come back to the supersymmetric field theory?

Seiberg: I was working on conformal field theory and two-dimensional gravity, and I felt that it was time to change. So I worked with Yossi Nir on model building – theories of quark and squark masses. And then I learned of a new paper by Michael Dine and Ann Nelson on gauge mediation of supersymmetry breaking. (Later they collaborated also with Shirman and with Nir.) They decided to take up the same question about supersymmetry breaking that Michael, Ian, and I had dropped when string theory came along.

Ooguri: So, this was a continuation of your work with Affleck and Dine. Was it roughly 10 years after that?

Seiberg: Indeed. They did their work in 1992-1993.

Ann visited Rutgers and we talked a lot. At that time, I was tired of supersymmetry and I didn't want to go back to that topic. But she succeeded to push me back into it...

Tachikawa: You didn't want to go back? That's surprising.

Seiberg: I did not want to go back because I thought "Okay, I've already worked on this, I want to do something new."

Ann visited us at Rutgers and gave talks about her work with Michael Dine, which were very stimulating for me. We started talking and eventually we wrote a paper about the relation between R-symmetry and supersymmetry breaking. (Some people refer to it as the Nelson-Seiberg theorem.) This followed an observation Ian, Michael, and I had made 10 years earlier, and it turned it into a more concrete and much clearer principle.

Ooguri: This was the beginning of the modern approach to supersymmetric field theory.

Seiberg: This was one element. The second element was influenced by my work with Yossi Nir, where we used spurions and the fact that the superpotential had to be holomorphic in them.

Ooguri: Was that the first time the spurion technique was used in supersymmetric theory?

Seiberg: Spurions had appeared earlier, especially in the context of supersymmetry breaking. I think the new point here was to view all the ordinary supersymmetric coupling constants as spurions by viewing them as background superfields. And the main application was to derive the non-renormalization theorem.

Ooguri: Were you the one who introduced this technique in supersymmetric theory?

Seiberg: I am not sure about that. But perhaps that was the first time all the coupling constants were viewed as background fields in the context of a supersymmetric theory. I remember that within hours or so after I thought of it many things fell into place.

I'd like to offer a historical perspective. Before the 1980s there was a clear understanding that the behavior of quantum field theory satisfies some genericity requirement. Murray Gell-Mann described it as the Totalitarian Principle: "Everything not forbidden is compulsory." 't Hooft described the same thing as "naturalness" – parameters take natural generic values unless there is a good reason, e.g. a symmetry, not to do that. But then, supersymmetric theories seemed to violate that principle. The superpotential is not renormalized and can be non-generic. This looked strange and the cancellations behind it seemed miraculous. Then, when it was realized that non-perturbative effects violate the perturbative non-renormalization theorem, it became clear that we need an organizing principle. In other words, to what extent are the genericity properties of quantum field theory true in supersymmetric theories?

The modern point of view, based on holomorphy of the superpotential vindicated Gell-Mann's principle and is consistent with 't Hooft's naturalness. The superpotential is subject to the same genericity properties as every other term, except that when we use this genericity we should also take its holomorphy into account. For this reason, I picked the title of my paper "Naturalness versus supersymmetric non-renormalization theorems."

For me that was very satisfying. Many different computations and many different phenomena were understood together using one organizing principle. I felt like I had been circling around this for years and all of a sudden it all came together. It was clear that that was the right way to think about it.

Ooguri: When did you come to appreciate the importance of holomorphy?

Seiberg: The appreciation of the power of holomorphy of the superpotential occurred to me gradually during these years. We have seen many examples of computations of a superpotential, where the real part and the imaginary part were computed independently, and surprisingly the result turned out to be holomorphic. At the time this looked like a miracle and a non-trivial consistency check of the computation.

Using this for a non-renormalization theorem was also not completely new. For example, Edward Witten argued for a perturbative non-renormalization theorem in the string worldsheet using holomorphy and Michael Dine and I used similar reasoning about string perturbation theory. In those cases, the holomorphic dependence was on fields, not parameters (although these were parameters in the worldsheet theory). Holomorphic dependence on parameters in 3+1 dimensional field theory also appeared in papers of Victor Novikov, Mikhail Shifman, Arkady Vainshtein, and Valentin Zakharov and in an unpublished work that Joe Polchinski and I did. And I must be forgetting other examples.

Ooguri: That was in the summer of 1993. But, in the late 1980s, you were essentially using it, weren't you?

Seiberg: Indeed, I use it. But at the time I did not view it as the underlying organizing principle.

Ooguri: That's interesting because that was also the year I wrote this paper with Bershadsky, Ceccotti, and Vafa on topological string theory, which also used the holomorphy and its relation to supersymmetry to derive the recursion relation, the so-called BCOV equation.

Seiberg: Your paper was a milestone. It clarified many issues and it led to many significant consequences. But this was a separate line of development. It is surprising and interesting that similar ideas came up independently in different contexts at more or less the same time.

As we discuss historical developments in science I am reminded of the famous Kurosawa movie Rashomon.

Tachikawa: It is based on a book by Akutagawa that we studied in school...

Seiberg: I am not familiar with the book, but I really like the movie. One lesson from that story is that different people view the same reality differently. This is common in the history of science. There are several lines of development and they are typically motivated by different questions. So if you interview another researcher who worked at the same time, you are likely to hear a completely different version of the events. And it is not that one description is more correct than another. They simply reflect different perspectives.

Going back to your question, at the time I was mostly interested in four-dimensional quantum field theory. The idea that you could say anything about the non-perturbative behavior of four-dimensional quantum field theory was totally unimaginable.

Ooguri: Right. You would not have expected that there would be analytic control over any non-perturbative physics in four dimensions.

Seiberg: With the understanding of holomorphy as an organizing tool all of a sudden things became clear and easy.

Ooguri: So, that became the guiding principle.

Seiberg: When you have a new tool you should be maximally ambitious. I remember that I thought, "We should be able to address all the open questions in quantum field theory using holomorphy."

As a first step I looked back at the supersymmetric version of Quantum Chromo Dynamics. With Ian and Michael, we had understood the behavior of the theory for small number of flavors, so I wanted to understand what happens with more flavors. Armed with the new perspective and new tools I realized that although the vacuum degeneracy of the classical theory is not removed in the quantum theory, extremely interesting effects are still present. For example, the complex structure of the moduli space of vacua can be deformed in the quantum theory. In other cases, there are new massless composite particles. That was surprising. Until then it had been believed that with strong dynamics the theory is gapped or has some massless Nambu-Goldstone bosons. Here, on the other hand, there can be massless composites that are not associated with spontaneous symmetry breaking. Later, this observation strongly motivated the understanding of the long distance behavior of N=2 theories (where there are massless monopoles) and other N=1 theories (where there are massless composite gauge fields, massless glueballs, massless exotics, etc.).

Then I started working with Ken Intriligator and Robert Leigh on increasingly more complicated models and we saw that the new techniques are very powerful leading to many new exact results.

Then Ashoke Sen wrote an extremely interesting paper…

Ooguri: Are you talking about his work on monopoles?

Duality and Seiberg-Witten Solutions

Seiberg: Yes. I am referring to Sen's paper establishing the existence of a charge-2 monopole in N=4 supersymmetric theories. That paper removed a real obstruction to duality. People had thought about duality before that paper. I think it was Witten who emphasized the importance of this charge-2 monopole. And it was believed that such a monopole does not exist and therefore the whole idea of duality had to be wrong. Sen's paper found that monopole and overnight it was clear that duality must be right.

Ooguri: Since Yuji is here, maybe one of us should explain it to him. In early 1990, not too many people believed in the electric-magnetic duality, the S-duality.

Tachikawa: Yes, I came too late to this part of the party.

Ooguri: There was of course the work by, I think, Montonen and Olive and then Olive and Witten, and···

Tachikawa: Goddard, Nuyts and Olive?

Ooguri: Right. But, when I read the Olive-Witten paper in the late 1980s, I had an impression that rather than giving evidence for duality, they were actually explaining why this miraculous formula works without duality – everything comes from supersymmetry.

Now we use it to motivate duality. But at that time, it was basically explaining that you don't need duality.

Seiberg: They considered states in small representations of the supersymmetry algebra. Today we refer to such states as BPS states. And they explained many of the special properties of these states as following from this fact. Since these special properties had a rational explanation, everyone thought that this should not be used as evidence for duality.

In addition, duality demanded the existence of a certain bound state of two monopoles in N=4 theories. And it was thought that such a state does not exist...

Ooguri: Yes, but then, Ashoke's demonstration of the bound-state was...

Seiberg: ···a phase transition. Before that paper duality was viewed as some technicality. Some things look as if they're dual, but there is no reason to believe this duality was an exact statement. Then, overnight it became obvious that duality is crucial.

Ooguri: Did it convince you that duality is actually a real phenomenon in quantum physics?

Seiberg: I was immediately convinced that it was true in N=4 theories. But I did not imagine that it will play such a crucial and central role as it later turned out to do.

And then I started collaborating with Edward Witten on the N=2 supersymmetric theory.

Ooguri: Were you interested in N=2 theory before you started collaborating with Witten?

Seiberg: Yes. I viewed it simply as another theory with N=1 supersymmetry. And as I was working out examples of increasing complexity with N=1 supersymmetry, this was a natural member of that list. Also, it was clear that the pure gauge N=2 theory has massless photons and the pure gauge N=1 theory was expected to confine. So I was hoping that a better understanding could explain the mechanism for confinement.

Ooguri: And, you did. Did you think that you had a better chance of doing that in the N=2 case, as opposed to N=1?

Seiberg: I was viewing it more as a special case of N=1. The specific properties of N=2 are nice and allow you to compute additional quantities. But for the question of confinement they are not essential.

If you go back some time – I think it was in the fall of 1987 – Witten wrote a paper explaining Donaldson theory as a twisted version N=2 supersymmetric Yang-Mills theory. It made Donaldson theory accessible to physicists. But it didn't solve that mathematical problem.

Ooguri: It gave a physical interpretation of the mathematical problem.

Seiberg: But I am told by mathematicians that from their viewpoint it didn't solve the problem, because at the end of the day you had to do exactly the same computations that Donaldson did anyway.

However, viewed more broadly, this is an amazing paper. In this paper Edward Witten introduced the notion of a Topological Quantum Field Theory. This is an extremely deep idea with far reaching consequences both in mathematics and in physics.

When I heard about that work I used my tool-kit of instanton technology to write a short note on the N=2 theory thinking about its moduli space of vacua and showing how the metric is corrected asymptotically. I didn't even want to publish that paper because I did not think it was interesting. Edward encouraged me to publish it. In fact, in his paper on Donaldson theory, he said that the ideas in that paper may well be important for further developments of the theory. He was completely right about that. That was in 1988.

Ooguri: That turned out to be the starting point and the boundary condition of what is now called the

Seiberg-Witten solution.
Seiberg: Yes. I wrote the paper on the "Behavior at Infinity" but I didn't pursue it. Then, in 1994 when I came back to supersymmetric theories, I remembered my 1988 paper. I knew that the vacuum degeneracy was not lifted and an infinite series of instantons corrected the metric on the moduli space of vacua. The question was how to evaluate it and how to sum up the series.

As I said, at the time Ken Intriligator, Rob Leigh, and I were collaborating. We wrote a paper studying various models in which the superpotential is given by an infinite sum of instantons. We succeeded to sum up the infinite series in an explicit closed-form formula using the knowledge of the singularities of the superpotential and its asymptotic behavior, combined with its holomorphy. So I was optimistic that the same thing could be done also in the N=2 theory.

When I started collaborating with Witten progress came in a stunning rate. Within weeks we had the complete solution of the pure gauge theory and the theory with matter. All sorts of interesting physical phenomena were elucidated including confinement and chiral symmetry breaking.

Witten immediately realized that it would help simplifying the problem with four-dimensional topology. But, I was more interested in understanding the dynamics of four-dimensional quantum field theory.

Immediately afterwards, I wrote the paper on duality in N=1. In that paper a dual description of N=1 supersymmetric gauge theories was presented. This work taught us many lessons. First, it was realized that electric-magnetic duality is ubiquitous. N=1 theories are more generic than N=4 and N=2 theories, and they exhibit similar dualities. Second, here one finds weakly coupled composite gauge fields. This underscores the fact that gauge symmetries are not fundamental. This point had been known before, mostly in the context of Abelian gauge theories, but here it was more dramatic.

In the meantime, Witten wrote the paper on the connection of our solution of N=2 supersymmetric gauge theories to four-dimensional topology.
Ooguri: Yes, which is now called the Seiberg-Witten equation. It turned out to be more powerful than the original Donaldson theory.
Seiberg: Physically, it is very clear because many of the complications of Donaldson theory were associated with small instantons. And every time you have a computation associated with a new manifold you have to control the same small instantons. What the renormalization group allows you to do is to compute the small instantons once and for all in flat space and to find an effective theory without them. Then you can place that effective theory on the curved space of interest. Since this effective theory no longer has these small instantons, many of the complications in the original theory are no longer present. So that's the reason it was...
Ooguri: In the effective theory it's already built-in, so you don't...
Seiberg: It's already built-in and you don't need to worry about that. In fact, the low-energy theory doesn't have small instantons. So that's what made these equations so much more powerful.
Ooguri: [Looking over to Tachikawa] Were you in high school in 1995?
Tachikawa: Yes. Well, that's about when I first heard your name.
Ooguri: You told me that you heard about the Seiberg-Witten theory by reading a popular mathematics magazine.
Tachikawa: Yes, there was an interview of Edward Witten by Tohru Eguchi in Kyoto. That was I think, early 1994.
Ooguri: That's correct. Witten came to give a public lecture in Kyoto sponsored by a Japanese company, and I was involved in coordinating that.
Tachikawa: Edward told Tohru that he was extremely excited about the work he was doing with you – without explaining much. But that was published in this popular Japanese magazine in the summer of '94···
Ooguri: Before the paper appeared. It was the first printed announcement of the result.
Tachikawa: Then, Japanese mathematicians like Fukaya got very interested in Seiberg-Witten theory. And Fukaya started to write a series of introductory articles about the Seiberg-Witten theory from mathematics point of view in that popular mathematics magazine. So for the first few years, I thought of the Seiberg-Witten theory as a purely mathematical thing.

I only learned about the physical part of the Seiberg-Witten theory after I started learning supersymmetric theory and finally I came across the review article by Peskin.
Seiberg: The TASI Lecture?
Tachikawa: Yes, TASI Lecture. And then I finally understood what Seiberg duality was, from the physics point of view. That was already 2003 or 2004. That's a long time to come.

Second Superstring Revolution in 2005

Ooguri: When did you hear about

Witten's breakthrough in string duality?

Seiberg: He told me bits and pieces of it as he was working. And then I heard the final version when he gave the talk at USC.

Ooguri: At the Strings 1995 Conference.

Seiberg: I was on sabbatical at the Institute at that time. We spoke the week before the conference and he told me that some of these things would actually work. But it was still very, very different from what he presented at the talks. I was stunned in the talk. I was supposed to give the talk after him and I felt, "What am I doing here?"

Tachikawa: What did you talk about?

Seiberg: I talked about field theory duality, which was a few months old and most people had not known about it at that time, so I thought I had a good talk to present. But after Witten presented his picture of string duality it seemed that my talk was already obsolete.

Ooguri: That was a stunning talk, I remember.

Seiberg: In his lecture he spelled out almost the entire picture.

I got on the stage and being very embarrassed I said, "I feel like I should drive a truck." Then I gave my talk. I don't remember the rest of it, but I am told that John Schwarz, who was the third speaker in the session, started his talk by saying: "If Nati has to drive a truck, I should drive a tricycle."

Ooguri: The field really made a phase transition in that year. Many of the ideas that you developed in quantum field theories, supersymmetric theories are now incorporated into it.

Seiberg: Witten described the many developments leading to this point as spokes of a wheel. So this was one spoke – supersymmetry, BPS, moduli

space of vacua, degrees of freedom at strong coupling, etc.

But there were also other lines of development. The work on 11-dimensional supergravity of Michael Duff, Christopher Hull, Paul Townsend, and others was crucial. And the study of supergravity solutions of various solitons and extended objects by Gary Horowitz, Andrew Strominger and others was also essential.

So there were many different developments that came together and the string duality picture put them all in a coherent picture. I think they enhanced each other because some aspects were clear from one point of view and other aspects were clear from another point of view. Together they combined to a complete and coherent picture.

Ooguri: Yuji, which year did you go to the Institute?

Tachikawa: That was 2006. I only joined this string theory community long after all of the things you were discussing. Whenever I hear about the glory days of 1984 or 1995, I always envy...

Ooguri: This will repeat itself. For example, when I was a graduate student, I studied Coleman's lecture notes, where he described – I quote, "the glorious victory parade, full of wonderful things brought back from far places to make the spectator gasp with awe and laugh with joy." These discoveries happed in the 1970s, and I missed all...

Seiberg: And in the 1970s, they talked about the glorious parade of ideas in the 1930s and 1920s. It is always like that.

There is no sign that this sequence of exciting discoveries will slow down. And based on past experience, I expect that this will happen again and again. And, as always, I expect it to happen in surprising ways.

There is always somebody working on a project that most people think is totally uninteresting and unmotivated. Then that project turns out to be a real breakthrough.

For that, we just need to keep an open mind and be accepting of other ideas. We should have this liberal point of view. Let everybody do what they are doing and encourage diversity. If everybody is working on the same problem, we will not have these ideas from left field that we really need.

Unity of Science

Tachikawa: Can I jump 20 years from 1996 to 2016?

Seiberg: Please.

Tachikawa: So you recently wrote a paper on condensed matter physics, which you will talk about tomorrow. What motivated you to get into this subject?

Seiberg: This is a fascinating topic. The condensed matter physicists have made incredible discoveries. I would like to understand them.

I'm a firm believer in the unity of science. I don't like it when people are put in boxes: one of them is a particle physics phenomenologist, another one is a string theorist, and another one is a condensed matter physicist. We are theoretical physicists and there are no clear boundaries between the different sub-disciplines. Instead, over the years we have seen a lot of cross-fertilization from one field to another.

Hirosi and I have just returned from a symposium in Chicago celebrating Yoichiro Nambu's career. The importance of interdisciplinary physics was very visible there. Nambu's work is a perfect example of ideas from one branch of physics imported into another branch of physics leading to enormous impact and to useful cross-fertilization between fields. This is clearly demonstrated by his most famous paper with Giovanni Jona-Lasinio, "Dynamical model of elementary particles based on an analogy with superconductivity."

Going back to your question about my current work on the topological phases of matter, I am simply trying to learn these beautiful ideas. These are fantastic phenomena that field theory exhibits, and I think that every field theorist must understand them. In addition, it is satisfying that these phenomena appear in the real world and are connected to real materials.

Ooguri: You can test these ideas with experiments.

Seiberg: And it is likely that these new ideas will also lead to new insights in quantum field theory, which then could be brought back to high-energy physics and string theory.

Many of the ideas in the study of topological phases of matter started in high-energy physics. For example, anomalies, Callan and Harvey's anomaly inflow, braiding statistics, Witten's topological quantum field theory and others are the main tools that are being used.

So I think there's a potential for high-energy theorists to do something useful here. There are clear indications that a lot more can still be done. And I hope that I'll be able to contribute. But at the very least, I'll learn something new and it is always refreshing to learn new things.

Ooguri: This time, non-supersymmetric theories.

Seiberg: I have worked in the past on related topics like topological theory in connection with rational conformal field theory. So I think I have some tools that could be helpful.

In general, there is no guarantee that any research project or direction will be successful. I always tell my students and postdocs, "Doing research is a risky endeavor. There is no guarantee of success. You must try many things hoping that one of them succeeds. But you should be ready to accept that most of them will fail. We do that for the few successful days in which we learn something new."

We Cannot Predict the Outcome

Ooguri: Before we finish, I have one more question to ask. You have been very successful in running research groups and mentoring students and postdocs. What do you think would make successful research groups?

Seiberg: First, I should not get the credit for that. Whenever I was in a research group I had many colleagues, who made essential contributions to the scientific atmosphere and to running the group.

Here at Kavli IPMU you clearly do the right thing. You have an excellent vibrant group here. Some of the world leaders are here at Kavli IPMU. These are people, whose papers I always study carefully. In the last few days I have watched the group function and I found it a real pleasure. I have attended interesting talks and I have participated in stimulating conversations. So all I can say is keep doing what you are doing.

As a general advice, I would suggest to create a stimulating environment by creating a diverse group of people. There should be people of different seniority level, of different talents, of different kinds of expertise, and of different backgrounds. For example, there should be mathematically oriented individuals, people with good physical intuition, good calculators, etc.

I would encourage everyone to interact with each other and to talk about their research. So that when a question arises, there will always be somebody who can find the answer.

There is also a question of how to select postdocs. I don't think there's a clear predictor for a postdoc success. Instead, I think we should not attempt to make the perfect selection, because this is impossible. We should simply attempt to collect a diverse group of researchers and to create for them the right atmosphere.

Another suggestion is to ask the postdocs what they think would help them.

Tachikawa: When I was at the Institute you suggested to me to have a meeting of the postdocs with Pizza...

Seiberg: I think this was your idea.

Tachikawa: Was it mine? Anyway, eventually we started a "Pizza Discussion" every week in the afternoons.

Ooguri: No faculty?

Seiberg: The faculty were not allowed.

Tachikawa: Right. That was a lot of fun.

Can you offer some vision for the future?

Seiberg: Some people think that researchers should have 5-year programs – like in the old Soviet Union – where everything is planned in advance. Solve this problem and then move to the next problem and then...

Tachikawa: We still have that system in Japan.

Seiberg: That might make some sense for experimentalists. But theorists' progress is more like a random walk. Theorists are stimulated by many sources. They listen to seminars, participate in informal discussions, read papers, etc. This impacts their research direction in unexpected ways. It might even lead them to abandon their existing line of research and to start another one. And even within a given project, in most cases the outcome of the research could not be anticipated from the beginning.

In our conversation about my research path I shared with you some examples of such unexpected results from my own experience.

So there is no way I could outline what I'll be doing in the next five years – this is ridiculous.

Ooguri: Well, your example shows that you followed your nose and pursued things you were interested in, like techniques in supersymmetric theory, which later turned out to be very useful.

Seiberg: I think I was lucky. I did not have a long-term plan, maybe a year in advance but not more than that. Well, I think the same is true for your papers. There was no way you could predict 2 or 3 years ago the topics you are interested in today.

The reason research is interesting is because we're surprised by the answers. If we could predict the answers, we would not be surprised by them. Almost by definition, we cannot predict the outcome. So we should not attempt to do that.

Pursue what you're interested in, keep working hard, pay attention to what's going on around you and be flexible – these are the rules. Sometimes it works, sometimes it doesn't.