In Conversation with Sergio Cecotti

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Biographical Sketch. Sergio Cecotti is an Italian theoretical physicist, currently at BIMSA, Beijing, and formerly at SISSA, Italy. After graduating from University of Pisa in 1979, he worked at Harvard University, UCLA and CERN, before becoming permanent at SISSA. He paused his academic career in 1993 to jump into politics. He was first president of Friuli-Venezia Giulia for almost one year in 1995/1996 and then became mayor of Udine for the decade 1998–2008. Then, he returned to SISSA and restarted his academic activity.
Sergio Cecotti works in string theory. He applies geometric methods to understand and classify quantum field theory. His most famous achievements include his works in the long-lasting collaboration with Cumrun Vafa which led to the discovery of topological/anti-topological fusion and the Kodaira–Spencer theory of gravity.

We met Sergio Cecotti in his interim office at the Villa 11 of the Yanqi Hotel in Huairou (Beijing). It was a quite large room with just one desk in the middle and an arm chair and a tea table at the window. The three of us were very nervous about how the interview would go, as this was already his third within a few month, and a researcher has certainly more important things to do than to just giving interviews. And indeed it took two or three questions till a natural flow of the conversation was established. But once that point was reached, Cecotti gestured and joked and told all sorts of old stories. He is a fantastic story teller and his eyes started to glow when he started to talk about his research work. It is probably impossible to convey all the enthusiasm he expressed during the interview in written form, but nevertheless we tried.

LH: *Professor Cecotti, thanks for conceding us this interview. How is your Chinese experience going? How is your adaption to China going? You have been at BIMSA for some months now.*

SC: It's 6 months. I arrived in September, and joined BIMSA right after the quarantine period.

LH: *Are you getting used to the life in China, now?*

SC: Yes, it's interesting. I live at the International Talent Apartments provided by BIMSA. The apartments are new. There were very few small problems at the beginning, and they were solved in five minutes. Four workers arrived at the apartment and fixed it immediately.

LH: *What do you miss the most from Italy?*

SC: The coffee, undoubtedly. There are two things that are very hard to find: coffee and cheese. Real coffee you may find, it will cost twenty times the price in Italy and is twenty times worse. But cheese is almost impossible to find. I understand that if I go to downtown Beijing there are places selling cheese. I have been to an Italian restaurant and they have proper cheese, but otherwise, even in big, furnished supermarkets, I cannot find cheese.[1]

LS: *And do you drink wine, to accompany cheese?*

SC: I don't drink wine, generally speaking, but sometimes I enjoy a glass. It is not impossible to find that, even in the market of the international community near BIMSA they sell a number of wines. A selection of wines can be found almost in every supermarket, but proper cheese, on the contrary, is almost impossible.

LH: *What do you like the most of your new life here?*

SC: Well, the best thing is BIMSA I guess. The institute. I find that there are many people, all of them very helpful. The institution is very organized—well, every country compared to Italy would seem—but here it is surely a nice place to work.

LH: *Do you go often to Tsinghua University from BIMSA?*

[1] **Note added:** After the interview took place, we have been contacted by S. Cecotti, who wrote us that "the problem with the availability of good cheese and coffee was brilliantly solved by the BIMSA staff" [after reading about it in a preprint version of this interview]. Cecotti declares that he is now fully satisfied.

SC: No, I have been to Tsinghua only four times, and always for dinners or events. I went once to attend to the lecture by Kontsevich, but I was going anyway that day for a dinner.

I will have to go next week to give a lecture on the history of physics. I am not an expert, so I need to prepare.

LH: *When I gave the history lecture last semester, it was the same for me. I had to prepare much more for those two hours than what I would do for an ordinary course. Do you have collaborators here?*

SC: At the moment I am not working with anybody at BIMSA. I hope that sooner or later I will start to work with collaborators here but first one needs to find a problem that is of interest for both parties.

LH: *We know that you were an athlete when you were young.*

SC: When I was young, I was in the national team of athletics.

LH: *Wow. And do you still practice sport?*

SC: No. I competed at the international level for a few years when I was young, I did enough sport at that time. When I decided to stop practicing athletic to do physics, the coach of the national team said that they will cut my legs. They said that they couldn't accept the idea that a candidate to the following Olympic Games would not want to compete. Eventually, they spared me [laughs].

LH: *You are Italian, you were faculty in Italy, and after so many years you decided to move to BIMSA. Can you tell us what the tiebreaker was that made you decide to accept the offer?*

SC: It was a challenge! You see, it is a totally different world, and I was tired of being in Italy. It was, as you said, a long time. Then, here I know the institute was run by Yau, I knew that his name would be a guarantee of quality, scientifically but also in general in the way the institution will work. I believe BIMSA will become in short time one of the leading institutions in the world, so I thought: why not go there and start this adventure that is very promising.

LH: *What's the best surprise here? What went far better than you figured it?*

SC: Most of things are better than I expected. I mean in China in general, and also at BIMSA. The organization, the fact that you have many people that help. In Italy is not like that. There is a lot of staff there as well, but you don't know what they do exactly, and in practice each researcher has to do everything by himself. Here instead the secretary staff helps on everything. It is more necessary for me because most of the paperwork is in Chinese, so if one does not know the language, it would be impossible. But here they are very helpful. They even hired a secretary who speaks Italian, in order to make the relations easier for me.

LH: *You have a secretary who speaks Italian?*

SC: At BIMSA, yes. The secretary speaks perfectly well. In order to simplify life, they even hired that person.

LH: *Do you have any tips for newcomers?*

SC: The most important thing is: come! There are people that still don't know if they want to join BIMSA or not. They received a proposal from Yau but they have not decided yet, and my tip for them is: come, you will be happy!

LH: *People is lacking a bit of information. There should be more transparency about all the help they would get once they arrive at BIMSA.*

SC: My sensation is that in many places, people thinks of China as some strange place were you wouldn't want to live. Instead, this is a very pleasant place to live, at least for us doing mathematics, which is what I can judge.

LH: *Are you having Chinese classes, to learn the language?*

SC: It has been set up, but it has not started yet. The institute hired a teacher, and the course should start once we move to the new campus.

LH: *I don't know how much you have seen about the Chinese academic system. Can you compare it to the Italian one, maybe?*

SC: I don't know the Chinese academic system very well yet, because BIMSA is a very special institution. Also in Italy I was in a special institution, which is not how academia generally works. So I know little of Italy and essentially nothing of China, in that regard.

What I can say is that here, the government is, generally speaking, much more willing to promote science. There is much more investment, from that point of view.

LH: *Did you have contacts with students?*

SC: Not yet, but I will teach at the Quizhen College next semester.

LS: *What course will you teach?*

SC: Classical mechanics. The idea is that they start with classical mechanics in the first semester, and then go on to quantum mechanics and quantum field theory in the second and third semester. At least that's what I understood from Yau. There will be other professors teaching quantum mechanics, another teaching general relativity. You can teach quantum field theory at various levels.

LH: *As a mathematician, I associate the expression "quantum field theory" to something very difficult.*

SC: The logic is not difficult. The computations are difficult, but you can teach it without doing the hard computations.

LH: *How does the optimal institution look like, for you?*

SC: The optimal I don't know. As I said, here is much better than the other places I have been. Maybe in the future it will improve even more. I am not going to say that anything is optimal because I always want to leave room for improvement.

LH: *But BIMSA is close to optimal. Is that what you are saying?*

SC: Yes, at least from my perspective.

LH: *Now about your job. How did you decide to become a researcher? Why not being an athlete, for example?*

SC: I decided to become a physicist much earlier than doing athletics. I practiced sport at high level because I tried and won a competition, then another one and so on, but not because I wanted to be an athlete in my life, it was not my dream.

On the contrary, I decided to be a physicist very young, when I was around fourteen. I started studying quantum mechanics at that age. Then I read very deep things, and decided I wanted to do that. The sport I did because I succeeded in it. My teammates in the athletics team called me "the Martian", because I was not training. I was just going to the competitions, four or five times per year, without attending the training or stuff like that. That's why the others made the joke that I was from another planet, going to international competitions without training. Of course, I was studying physics the rest of the time, instead of training. physics has always been the first choice, even if I did many other things during my life.

LH: *Have you ever thought of a plan B? Anything you would have liked to become, in case being a physicist had not worked out?*

SC: Maybe a mathematician.

LH: *Have you ever thought of moving fully into mathematics?*

SC: Actually, around ten years ago, I saw that there were more positions in mathematics than in physics. Thus, I decided to try to apply as a mathematician. However, I was denied because I came from physics. After the selection process was completed, I looked up on the list of those who got those positions, and all of them had made their career working on BCOV. "... but I am also working on BCOV!", I said [laughs].[2]

LS: *What type of questions and problems you more interested in nowadays?*

SC: Nowadays I am working on the most geometric aspects of quantum systems, as I did for almost all my life. However, the older one gets, the more complicated geometries one is willing to consider. Now I am studying these very subtle, minimal details of the story.

LS: *As in your recent work on the two distinct notions of Kodaira singularities.*

SC: Yes, for example. Let's say that I am interested in these details that most people overlooked. Mathematicians of course defined this quantity, but for physicists it was too subtle an invariant. Because, you see, if an invariant is topological, it is easy to compute. This is the old story: it is possible to deform the system into a position where you can do the computation. But when it comes to the classification of singularities, that can jump like crazy as you change the parameters of your system, even though it is an invariant in a suitable and more sophisticated sense, physicists will not catch it. In fact, the strange thing is that this invariant has a simple physical definition, that physicists would not guess before reading the mathematicians' papers.

LH: *So you had to learn a lot of math, right? When did you start learning math?*

SC: That is a good question. It was 1984, and the string revolution was taking place. After the famous paper by Green and Schwartz, Witten published perhaps more than 10 papers in a couple of months, a very large amount of papers, using

[2] The acronym BCOV refers to a mathematical theory developed after the influential works [1, 2], which together sum up to more than 1400 citations. The letter C in 'BCOV' is after Cecotti.

mathematical techniques that were not common tools for physicists at the time. Calabi–Yaus for instance, and other aspects of algebraic geometry. Then, I said to my colleagues in Pisa [where Cecotti was at the time] "We are on the losing side of the competition, because we do not even have the tools to keep the pace. We need to learn the related mathematical language".

Then, I took those papers and looked at the names: Calabi and Yau. Yau I did not know who he was, at the time. But, regarding Calabi, his main historical collaborator Visentini was in the Mathematics Department in Pisa. I said "we should ask him to teach us complex geometry". We thus went to Visentini, who was having the opposite problem. His problem was that he had to give classes but nobody wanted to attend his courses. So we proposed to set up a course of complex geometry for physicists. He accepted to do a course, once per week, three hours every Friday afternoon. So I convinced my colleagues in physics to attend the classes, to make sure there was an audience, and the course started. It became a five or six years course on complex geometry, one afternoon per week. We learned from sheaf theory to Kodaira–Spencer. It was very very good, very neat, sometimes with a very formal language that was already in disuse at the time. Everything was covered in great detail. I have to say, it was analytic geometry, not really algebraic geometry.

LS: *How do you figure out the research questions you work on? Do you have a long-term program and proceed a step at a time, or how do you find your inspiration?*

SC: For a long time in my life the questions were posed by Cumrun Vafa. He proposed me to try this and that.

LS: *And how did this fruitful collaboration with Vafa started?*

SC: It was 1991, and he had just written a paper about Landau–Ginzburg models in two dimensions as a conformal field theory in which the superpotential is set to zero. From that point of view, Vafa and his group only knew the holomorphic part of the story. They were able to compute the Yukawa couplings, but only without their absolute normalization. My group and I were working on a similar story, but we were interested precisely in the absolute normalization. We wrote and published a paper with the result in the case in which the superpotential is quasi-homogeneous. To have a quasi-homogeneous superpotential is equivalent to have a two-dimensional conformal field theory. Our result was not a surprise: we got the same answer that one would get from solving the equation you get from the conformal algebra. In essence, the outcome of our work was a confirmation that, assuming a conformal theory, if you do your computation with the Landau–Ginzburg model you get the exact same formula you would obtain from other conformal field theory techniques.

Then, Cumrun Vafa wrote to me, asking about the case in which the superpotential is not quasi-homogeneous. Physically, this means that the theory is gapped, not conformal. He asked me how to compute the normalization in that other case. I answered: "of course the same formula is true". He didn't believe me, he objected that I deduced the formula with the additional assumption on the superpotential, thus assuming conformal symmetry. He asked me how could that be true even without the conformal symmetry. “It is true” I replied. So, he suggested that I took the superpotential x^3+x and computed the normalization with my formula. He would then check if the result was correct or not.

I started working on that. I did not have an immediate answer, because I had a differential equation, but I needed to solve it for the particular case x^3+x he proposed. Luckily enough, after some rewriting, it turned out that the equation was one of the Painlevé equations. A lot is known about their solutions, so I could finish my computation and show the solution to Cumrun. He looked at it, and was surprised by the fact that it was correct. Yet, he wanted to find a solid justification for the formula. Clearly the answer was correct: it showed all the physical properties that one would expect. We therefore started working on a different argument. This is how we constructed this idea of topological/anti-topological fusion, to justify that result, and we gave more examples to support our result. From there, we started applying these techniques to different problems.

LS: *After the success of the topological/anti-topological fusion, you worked on the idea of holomorphic anomaly, that led to the famous BCOV work. Can you tell us about the step from one breakthrough to the other?*

SC: The step is easy to explain, because there are a few papers in between. In particular, there was this paper about topological/anti-topological fusion in the Ising model. There, it is shown that, for a two-dimensional theory with $(2,2)$ supersymmetry which is gapped, the topological/anti-topological equations become the isomonodromic equations studied by Jimbo, Miwa and others. There was a huge mathematical theory about isomonodromic deformations, and in particular a very central object is the τ-function. We had the same equations, so we were able to define the same quantity, in abstract. There was this big story about the τ-function, we asked ourselves what was the physical meaning of the τ-function in our context, in terms of the Landau–Ginzburg model.

To explain that idea, we wrote a new index. At the time, we wrote many papers with new indices, in the title or in the text, but this newest index was indeed the τ-function. Later, this was related to the Ray–Singer torsion of the Calabi–Yau which is the target of the sigma model, which in turn is related to the genus 1 topological partition function. The latter is the origin of the story of the holomorphic anomaly. This is the explanation of how the holomorphic anomaly came about.

I have given a seminar at the Moduli Space Conference last week, at the Yau Mathematical Sciences Center, because it is always the same story. You can compute the geodesically complete metric of your Calabi–Yau using these techniques.

LS: *You also work on the classification program of supersymmetric field theories. What do you think is the most interesting aspect of this program?*

SC: The idea behind every story of classification is that, a classification is interesting if it is like the classification of Lie groups. Some infinite families, easy to understand, and then finitely many exceptional cases. You are interested in understanding these ones. So, you have predictable infinite families to which the obvious construction applies, and a few, but only finitely many, situations of theories that cannot be constructed in the standard way.

That was the idea about classifications in four dimensions. We also have an earlier paper on classification in two dimensions. It has the title "On Classification…", because it was not an actual classification, but it was the structure of how the classification should work. That was more important, because the physics behind this classification was the first place, where the idea of wall-crossing phenomena appeared. In that paper, we classified explicitly only two-dimensional $(2,2)$ theories, which have at most Witten index 3. This is because you get Diophantine equations, with the degree of the equation given by the Witten index. For quadratic, you have the solution. For cubic, in general you don't have explicit solutions, but that particular cubic we needed was solved, so we could use that solution. In degree four and higher you cannot expect to have explicit solutions to the differential equation. For degree higher than three we could draw some families, containing theories with this or that symmetry, with this or that property, but you have to add assumptions in that case, to simplify the equation. The equation would depend on too many integral quantities, thus you have to ask for symmetries to reduce the number of variables or reduce the degree.

After that, Cumrun and I made the classification of complete theories in four dimensions. That was a cheap result. I mean the paper has more than a hundred pages, but was done in less than an afternoon. It required a few days to write, but it contains many pictures. It is based on just one theorem, taken from mathematics, and saying "Oh, there is this classification!". The mathematical theorem was on the classification of cluster algebras with finitely many quivers in the mutation class. The idea is that they, in turn, are in one-to-one correspondence with four dimensional $\mathcal{N}=2$ quantum field theories. And so we could make the classification of theories with certain properties, which means simply to take the classification from the theorem. We changed the name of the object we wanted to classify, but there is a one-to-one correspondence with supersymmetric quantum field theories, and the classification of cluster algebras was complete, so there was really no problem.

This kind of game is interesting. There is one type of algebras, finite dimensional associative algebras that are called quasi-quaternionic that are symmetric. There was a theorem in algebra, stating that the classification of such quasi-quaternionic algebras was given by a certain list. Later on, it was realized by some physicists that there is a one-to-one correspondence between four-dimensional $\mathcal{N}=2$ superconformal field theory which have a Lagrangian formulation and have Coulomb branch of dimension two and the quasi-quaternionic algebras. At that point, this observation was a problem, because the official classification of these algebras by mathematicians gives a list. All entries are associated to a quantum field theory, but there are many more quantum field theories. Many many more! This was a problem that caused panic in the mathematical world working with these algebras, because one of the central results of that area was challenged by physicists, claiming that there were many more.

LH: *How did that play out, eventually?*

SC: They had to correct the theorem. In the end, a new theorem replaced the previous one, and the final classification agrees with the one expected from physics.

There was a big subtlety that was overlooked by both physicists and mathematicians, but in two different ways. You see, from the physical point of view, you get a quiver quantum mechanics, and the BPS states of the four-dimensional theory are the vacua of the quantum mechanics. Now, you can compute two different things: you can compute the supersymmetric vacua of the quiver theory either perturbatively or non-perturbatively. You get the answer to two different questions, under some conditions. At this point, everybody would say "Of course the correct solution is the non-perturbative one".

LS: *I would definitely say so, yes.*

SC: However, the exact, non-perturbative solution would give you the first classification, the incomplete one. On the other hand, it turned out that you need to use the perturbative vacua. If you use the perturbative answer for the classification, you get all the structure that was predicted by Gaiotto, Moore and Nietzke. For mathematicians, this means that the correct thing to do is not to work in the standard topology but in a p-adic topology. In practice, that means to expand in series, in this case, which precisely corresponds to a perturbative expansion. Indeed, a mathematician—not the one of the first theorem, a different one—realized that you need to go p-adic. The problem was that the algebra defined with the standard analytic topology with respect to the coefficient is infinite dimensional. The other, the p-adic one, becomes finite dimensional.

This kind of game, we played it a lot. The result was worked out by a mathematician, but it came from the a story where you can understand everything from the physics side, in four dimensions with $\mathcal{N} = 2$ supersymmetry.

LS: *What in, in your opinion, your major contribution to Science?*

SC: Well, the major, I don't know.

LH: *Which one do you like the most?*

SC: My contribution I like most is the multi-faced object that is called Special Geometry. It can be topological/anti-topological fusion, it can be geometry in the sense of supergravity, the Hodge structure and Hodge number of the Calabi–Yau used for string compactifications, it can be holomorphic integrable systems and algebraic integrable systems in the sense of Liouville, Seiberg–Witten geometry which is again the same story, and has yet many other incarnations. Indeed, I have written, I don't know how many, different inequivalent definitions of what Special Geometry is.

LH: *And what is your favorite one?*

SC: The one that is still to be written. I always find that the latest definition is not good enough, in the sense that it is not as abstract and general as I would like. In one of my last papers I have given a generalization in which the underlying manifold is not even complex, it is real. It is a challenge, because you want the so-called "power of holomorphicity", so to have some kind of Cauchy theorem and so on, but without having a complex structure around. It can even be of odd real dimensions. It can be very far, but still have all the good. You need to relax the definition, and there are in fact physical systems that realize that geometry.

But I will try to generalize further. Even in my last paper, about two different notions of types of Kodaira fiber, there is a generalization with respect to the previous idea of Special Geometry in the sense of integrable systems à la Seiberg–Witten, which is a fibration with a section. But in this last work I threw away the condition of having a section. In fact, I will throw away much more assumptions. For now, you have a symplectic manifolds which is fibered with fibers that are abelian varieties. My idea is that abelian varieties are not general enough, and you need to consider anti-affine varieties, of which abelian varieties are a sub-class.

LS: *Where do you find your ideas?*

SC: There is no place where you can find the ideas, where you can just look at the place and find the correct answer.

However, when I was young, it used to work like this. I was sleeping, and I dreamed that I was attending a seminar, in which the person would write some formulas and so on on the blackboard. Then, there was it, the formulas I was looking for. The problem was that, when I woke up, I remembered that the solution of the problem was on the blackboard in my dream, but I could not recall the details. If, somehow, I could recall the claim, the thing to be proven, and said it to someone "I know that this fact is true", if the other person would say that it was not possible, then I would immediately recall all the details. But only if somebody says to me they believed the result was wrong, in that moment, all the details resurfaced from the dream [laughs]. That thing happened a few times at the beginning of my career, but it was a short story, only until I was thirty or so.

In fact, the most important result of all, nobody questioned it with me, so I never remembered the details! There was this guy in my dream that was explaining on the blackboard the exact renormalization group of Yang–Mills theory in four dimensions.

LS: *You really should have found someone to challenge your claim.*

SC: Well, but nobody would tell me that it is false that there is a renormalization group for Yang–Mills theory [laughs]. But it was many many years ago. Now, sometimes I wake up from my dreams thinking "Oh my God, I have written something wrong in a paper!"

LS: *Are there other areas of mathematics or physics you would like to know more about?*

SC: Yes, there are some things that I always wanted to know, but I find them hard. I am interested in all subjects that are supposed to be deep, but they are also typically very hard. Number theory and algebraic geometry are the two topics I would be very happy to know. As I said, I attended classes of analytic geometry, but not of algebraic geometry. I know something, enough to ask questions to an expert when I need.

LH: *Would you still go to classes, if there was an algebraic geometry class?*

SC: Well, if there was an algebraic geometry class for physicists, who want to learn but don't want to get things too complicated, then yes. It is like Chinese, I agreed to attend lectures of Chinese, but only at a very basic level.

LS: *How would you describe the interplay between physics and mathematics?*

SC: When I was a freshman in the university, one of my teachers was Bombieri. He told me that there is no such a thing as distinction between physics and mathematics. There is only deep and trivial. If something is deep, it is deep both in physics and in mathematics. If something is trivial, it is trivial in both.

It is just a matter of style. The style for saying things, for writing papers, is different. Mathematicians usually do not like the physicists' style. I remember Visentini talking about Enriques, saying of him that his reasoning was that of a theoretical physicist, as if it was equivalent to say that the reasoning was that of an alien. However, as Mumford said, Enriques never published a wrong theorem, even though his way of thinking was that of a theoretical physicist. So, there is no clear-cut distinction. Well, except that in physics there are those crazy people [smiles] that do experimental physics. They do experiments that are totally irrelevant and give us our bad fame. For what concerns theoretical physicists, there is no deep distinction with a mathematician, except that the physicist's style is more relaxed. In a physics paper, you can find statements like "this fact can only be true". However, this also is changing. Theoretical physicists are taking more and more rigorous approaches. For example, in a recent paper of mine, I wrote a statement that, to me, was completely clear and obvious from the physics of the problem. The referee of that paper complained that it was unclear whether I was claiming that the result was proven in full mathematical rigor. Therefore, I decided to add a 30-pages long proof of my claim, in full detail. But usually physicists do not like to read papers with these long proofs. Indeed, a mathematician friend of mine, Barbara Fantechi, said to me that us physicists never write anything that is absolutely true, but only make statement that are highly likely. I have to defend the category and say that this is not entirely true. Most of the time, we do have a proof, but prefer to swipe the technicalities under the rug. We check the proof on our own, to make sure everything we say is correct, but do not publish it. There is this sensation that is inelegant to bother the poor reader with the technical proof. For a mathematician is typically the other way around: the proof is what matters most.

LH: *Do you get intuition from physics?*

SC: In my opinion, physical intuition is a mythological creature. Of course researchers have intuition, but I wouldn't say it comes from the physics of the problem. Intuition means that some things are obvious, and there is no need to annoy the reader with the details of the proof, but this is true also for mathematicians.

It is important here to distinguish between two aspects: the technical proof to show that a statement is true, and the deep reason of why that statement should be true. Usually, mathematicians write the paper with emphasis on the technical proof, and then shortly remarking why a statement is necessarily true otherwise the whole branch of mathematics would collapse. The physicist's style tends to put the emphasis on the fact that, if a statement weren't true, a whole branch of knowledge would collapse, leaving the more boring proof to the reader.

LS: *If I may, I would like to change gears a bit. I have read your Wikipedia page, where it is written that, I quote, "Sergio Cecotti is an Italian politician, former major of Udine". This is certainly true, but what do you think about this definition? What are your feelings about being described as a former politician, rather than as an active researcher who also did politics for a period.*

SC: My parenthesis in politics lasted almost fifteen years. Many people know me only as a politician. Besides, the Wikipedia page was written by the staff of the political party I belonged: it was for electoral reasons. When I finished my political career and went back to actively doing physics, an Italian newspaper wrote that, no matter how good I was as a scientist, I could not possibly be better at it than at doing politics. This is just an example.

LS: *I bet that, when you stopped your academic career for the first time to move into the political activity, many people said the opposite.*

SC: When I communicated my decision to Cumrun Vafa, he said that the theoretical physics community would miss me, but he was glad that I could play an active role in improving the situation of my country. He wished he could do the same for his own country.

LS: *Would you define yourself as a physicist or as a politician?*

SC: I am a physicist. During my life, I happened to do other things, and in particular I worked as a politician during about fourteen years, but I was trained to be a physicist. The political career was a long parenthesis, that was initially intended to be short.

When I was young, I thought that, becoming old, I would be less brilliant and less energetic for physics, and I would then step into politics. I ended up doing the exact opposite! [laughs] When I was young and mentally efficient, I worked as a politician, and when I grew old, I stepped back into physics.

LS: *Was it hard to get back to physics?*

SC: It was not very hard. After a couple of weeks I started doing physics again, I read a paper by Ashoke Sen and wrote to him with some ideas and comments. He figured out what to do with those ideas, and we wrote a paper. Therefore, just about two weeks after having started looking again into physics problems, I had a paper with Ashoke Sen. Many people in the community were surprised, but the paper come out spontaneously and almost casually.

Some weeks later, I read a paper by Cumrun Vafa and collaborators. It was about Yukawa couplings, and remember that this is the precise same subject for which we started collaborating in the first place in the nineties. I wrote an email to him with the answer to a question they asked in the paper, where I calculated the absolute normalization of the Yukawa couplings. To my surprise, Vafa answered that my computation was not the answer to the question they were asking: they were looking for the computation before the normalization, so they were looking at one level less. This was unexpected, as I did not sent to him the answer to the question they were after because I considered it trivial. I thought they already knew, and wanted to compute the absolute normalization for that quantity. Thus, I emailed him the answer to his question and, exactly as the first time we collaborated, he was surprised by my answer but observed that it passed all the consistency checks and had all the expected properties. We then started collaborating again. We promoted to four dimensions many of the ideas we developed years before in two dimensions, in particular the wall crossing formulas.

LH: *Were you following the research developments during your parenthesis in politics?*

SC: Not at all. You see, in that period, I missed the so-called duality revolution, a change in paradigm in theoretical physics. The way of doing theoretical physics, especially string theory, changed, changed completely during the time I was in politics. The concepts, the tools, the techniques, were completely different when I came back. I felt it is a very strange world.

To my advantage, however, all the young researchers had lost the memory of the old tools and techniques. So, when there was a concrete problem that required to master these older computational techniques, I had an advantage over them. It was a very small niche on which I had this advantage, but I was essentially alone in that peculiar situation and the rest were many thousands of researchers. Having arrived from a different epoch in a time capsule turned out to be useful to address the problem the younger collaborators of Vafa were stuck on.

LH: *Have you learned the newer techniques?*

SC: No.

LS: *What are the best and the worst moments of your career?*

SC: The best moment was when we discovered the holomorphic anomaly, and then understood its connection with gravity. That period was really exciting, we were able to find explicit solutions that seemed totally out of reach. The problem in enumerative geometry of counting curves of various degrees on algebraic varieties was considered very hard. In the famous example of the quintic threefold, it was known that the number of genus one curves in the first three degrees was zero, but the first non-zero number was not known. With our formula, we were able to compute the exact number in many many degrees. We found the generating function and expanded it up to degree 12 to showcase our formula, but with our formula, finding the number in arbitrary degree was just a matter of computational time. You just need to expand the generating function up to the desired order.

A group of five mathematician working on this problem had a different approach. After a very long and computationally-expensive calculation, based on an enumerative geometric algorithm, they derived the first number, which of course matched our prediction. Our holomorphic anomaly technique to obtain the generating function was extremely general. You can simply choose your variety, click the button in our algorithm, and produce a table with many enumerative invariants. In the follow-up paper we extended this formalism to genus higher than one.

Needless to say, there were a lot of details we had to understand and fix beforehand. But once we understood the mechanism, the outcome was simply amazing. The power of the technique, and how easy it was compared to the previously known methods, was incredible.

LH: *Let's talk about students and education. What do you think would be the ideal education, starting from undergraduate courses?*

SC: I have never thought about that. I know that in Soviet Union they started studying mathematics and physics very early. The student were requested to know everything in Landau's book, which means essentially to know everything about physics. The idea should be to know everything, which however is almost impossible. I am not fully aware about the Chinese system. In Italy, one problem that I know is that, in the physics department, there are curricula in various sectors of physics but in no one there is exhaustive mathematics. In a board meeting when I was still at SISSA [Italy], I insisted on teaching more courses in pure mathematics. Dubrovin complained about my proposal, because, he insisted, only trained mathematicians should teach pure mathematics courses, not physicists. I agreed with him in principle, as long as they set up a course and teach it. However, the reality was that courses offered by mathematicians to undergraduate or graduate students in physics were not receiving any attendee. The situation was blocked, because Dubrovin and the group of mathematicians were against us physicists teaching math courses, but they were not going to teach to physics student either. The consequence of the situation was the big problem that students in physics were not receiving any solid background in mathematics, unless they attended extra-curricular courses. This could be a hobby of an individual, but it should not be an institutional directive.

LH: *And what other subjects do undergraduate students need to learn, if they wish to become successful mathematical physicists? Let's say, what do they need to know to become a researcher in your field?*

SC: There are many interesting stories that young physicists should know, in my opinion. For instance, traditionally, in the physics degree, students are taught Lie groups, but not other types of groups. For most physicists, there can only be two kinds of groups: finite groups or Lie groups. However, in many applications and physical problems, many more groups appear that are not of these two kinds. It is important to enlarge the vision and teach the broader concept of groups, not just for the sake of a more abstract presentation, but because these other types of groups do appear in many applications. There are plenty of such examples.

In general, I would say the basic concepts that a young theoretical or mathematical physicists should learn are the differential and algebraic geometry, and a solid knowledge of representation theory. Some other professors would include functional analysis in the list. I have never used functional analysis in my research, but other people with a more classical viewpoint — that is to say, older people—would say that functional analysis is a fundamental tool. Many physicists, though, are not interested in existence theorems, for example. A physicists would rather think: “Clearly, something is happening in this system, therefore a solution should exist.”

In the last couple of years, functional analysis is reviving. The study of von Neumann algebras seems to be relevant to define what is meant by an observable in quantum gravity. But, as someone said, for most physicists, the only analysis you will need in your research is a good knowledge of the Latin and Greek alphabets [smiles].

LH: *You said you will teach next semester. What is you teaching philosophy?*

SC: I don’t have a great experience, honestly. I started many years ago in SISSA, where only PhD courses were offered and we had around three or four students per course. Besides, typically they would not show up to my class after the second lesson [laughs].

LH: *We now move into our final questions. What is the most exciting discovery in recent years in your field?*

SC: There is more than one, all connected to duality. If I have to pick one, I would say that a very exciting discovery is the result of Strominger and Vafa that string theory correctly reproduces the thermodynamics of a black hole. It is the first time in which you have a theory which is not an *ad hoc* construction, but a first principles derivation of a physical theory that includes quantum gravity. String theory satisfies the most unlikely and stringent conditions for it to be a theory of quantum gravity. There are other theories of quantum gravity, but it is unclear whether they can reproduce the entropy of black holes. These alternative theories for now essentially reproduce Minkowski spacetime, which is a necessary condition to be acceptable, to have Minkowski as a solution, but I do not consider it as a critical argument in favor of those theories. On the contrary, thanks to the result of Strominger and Vafa, we are starting to understand quantum gravity. It shows that there exists at least one model, namely string theory, in which the two things, quantum and gravity, can stand together. The consistency conditions are restricted but we know that it is not the empty set. In fact, having a smaller set of solutions is better. Ideally, if we find that only one consistent theory can exist, that must be it.

LH: *Now about the future of your research field. Where is string theory headed?*

SC: I will quote Gregory Moore, who said that making predictions is hard, especially on the future. I cannot really answer this question. When I was young, I was doing quantum field theory and, all of a sudden, the boom of string theory took place. String theory was known about twenty years earlier, but it was forgotten because it was not believed to apply to the real world. Eventually, it came back in its full glory, with what is called the second string revolution. In our sector, you can never tell. Maybe next week a paper will appear that will shake our understanding of physics, or maybe no change of paradigm will happen for the next forty years. physics develops with jumps. Mathematics is smoother, you pile up one theory after the other. Physics, instead makes huge jumps, because if someone finds a new result that explains a deep feature of reality, everything will change. In mathematics, if a deep theorem in a given subject is published, researchers who study the other subjects will not care. In physics is not like that. If there is a change in the way we understand the universe, all disciplines of physics should go in that direction, or try to confute that result if you believe it is wrong. In this case, you will build your minority group telling that the correct story is a totally different one. But still, everything will revolve around the new insight.

In physics, you have revolutions, starting with the Copernican one. In mathematics, you have evolution, possibly with change of languages. I do not believe that in a few years from now mathematics will be totally different. In physics, on the contrary, it may well happen. Imagine if you were in politics for the ten years during the transition from classical to quantum physics. Coming back, you would think one of the two: either everyone else is crazy, or you are crazy. I experienced this sensation myself. When I came back to physics from politics, the string duality revolution had taken place. My colleagues then were talking a totally different language and asking totally different questions.

LH: *Do you have a scientific hero?*

SC: Yes, Dirac was my hero, when I was young. I read Dirac's book *Principles of Quantum Mechanics* and, at the time, I understood nothing. But I understood that the guy had a very deep way of thinking, of organizing the idea, of making arguments.

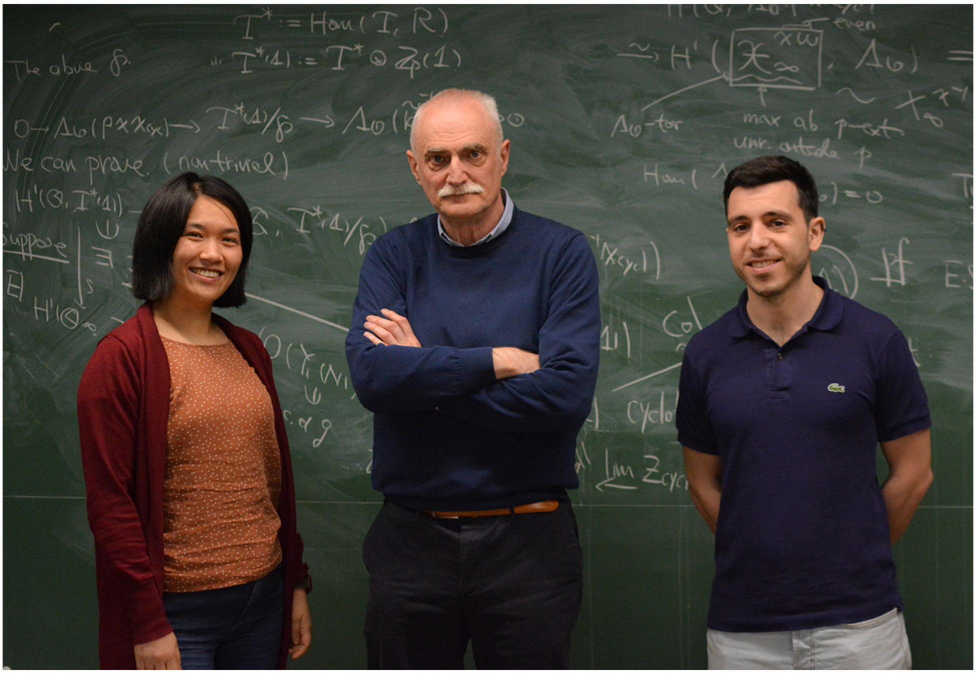
LH: *We have arrived at our last question: What book would you recommend people to read? Would you still recommend Dirac's book to a teenager interested in physics?*

SC: Yes. There are two aspects: one is the topics written in the book, the other is the way the book is written. In the case of Dirac's book, which is possibly the most fundamental book in the history of physics, what is most remarkable is the explanation of quantum mechanics. There are many other books that explain the same contents, but not with the same depth and clarity of thought.

Nowadays, the questions in physics have changed. For a perspective on these more modern topics, I recommend a book by L. Susskind *The Black Hole War*. It is not a technical book, but is not hand-waving either. It has few equations and pictures, and lots of explanations. For quantum gravity level 0, I warmly recommend this one. It presents no technicalities, no thorough mathematical knowledge is required, but the idea and the meaning are clearly explained.

LH & LS: *Professor Sergio Cecotti, thank you for your time. It has been a pleasure. But before we leave, can we take a quick picture in front of a blackboard?*

LH & LS: *We wish you all the best for your China adventure and for your research.*



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