Oxford University mathematicians Xenia de la Ossa and Philip Candelas in conversation with SMRI Director Geordie Williamson on their careers and the origins of mirror symmetry

This interview explores general characteristics of research in mathematics and physics, however some comments describe more technical aspects of these fields. We have left these in, in the hope that our lay audience will forgive these inclusions as these details may delight and interest researchers in mathematics and physics.

Geordie Williamson: It's a real pleasure today to have Xenia de la Ossa and Philip Candelas speaking to us, who are visiting from the University of Oxford. I was wondering if you could give us a brief biographical sketch, including how you become interested in maths and physics?

Xenia de la Ossa: I was born in Costa Rica, from a Costa Rican father and a Salvadorean mother. I travelled quite a lot because of my father's work, so I lived in Mexico, Colombia, Guatemala, and all my studies were in different places. I ended up doing my undergraduate degree in Guatemala at the Univerisdad del Valle de Guatemala. I had fantastic teachers there and an inspiring time. Although I started my first year in biology, I was very taken by the maths and physics courses.

It was really my first time doing proper mathematics and physics problems, and I enrolled in physics the following year. It was that first year with extremely inspiring teachers that was the reason I made the switch. I still love biology though! So then I did my master's in Costa Rica in theoretical physics and then a PhD in Austin, Texas. From physics, I moved more and more into the mathematical side of theoretical physics. At the time, string theory was booming.

G: Which year did you begin your PhD?

X: I started in 1983. I met Philip there, we got married and had a child in the middle of my PhD.

After the PhD, it was postdoc time, which was hard because our younger daughter was then a baby. I went to Costa Rica right after my PhD because I had a deal with them that I was supposed to go back. After that I did more post docs, until the year we moved to Oxford [1999]. We had a two-body problem because Philip already had a permanent position, in Austin, and finding two jobs in similar areas wasn't easy. At one stage, while I was at Princeton, Philip was commuting back and forth from Austin essentially every week.

Philip Candelas: I was brought up in London. In the beginning I wasn't much taken with maths but by the time I was about 15 or so I think it was clear that I was going to pursue some sort of career in mathematics. I was lucky enough to be admitted by Cambridge. It's scary looking back how one makes decisions based on no knowledge. I decided that general relativity was the area that I wanted to move into as a graduate student and I followed Dennis Sciama. He was a cosmologist who had moved recently from Cambridge to Oxford.

In those days, the application process was a lot less formal than it is now. You spoke to a potential supervisor, and somebody made a recommendation, and it was decided. I went to Oxford to do a DPhil, which was officially in astrophysics. It was the time of Hawking and the Hawking-Penrose theorems. Then there was Hawking's discovery of the Hawking radiation of black holes. I started looking at that, and wrote a thesis about it.

While there were no formal postdocs at the time, it was very important to go to the US for a year.

One day, I expressed this desire to Dennis and he spoke to John Wheeler, who was about to leave Princeton for Austin. John Wheeler said that there was money for me to go also to Austin. I went initially for a year, thinking that I would not enjoy it, but it was a wonderful place. The science was extremely exciting, and there was a wonderful group in relativity when it was still a small subject, including people that were interested in quantizing gravity. Though I did not know this until later, there was also a faculty post that was part of the deal to attract John Wheeler to Austin. The two people to whom it was first offered turned the offer down, and there was some fear that the position would revert to the administration. I think that I was the next warm body to walk down the corridor at that point, and I was asked if I would like to stay in Austin. I returned to Oxford, over the Summer, wrote up my thesis and returned to Austin as an assistant professor in September. I was still there in 1999. It was one of those situations where you go for a year, but you stay for 21 years!

G: It wasn't difficult to move from the Oxford environment to Texas?

P: Oh, it was very different, very exciting. Austin is a great place to live and people really are friendly. The weather is good most of the year. And when it's not good, it's because it's too hot That's a liberating thing coming from the UK!

It coincided with a heroic period in Austin, kicked off by the rapid increase in the oil price. This price increase, which was so harmful everywhere else, meant that it became very favourable to drill. As Texas is the centre of the oil exploration industry, there was suddenly a lot of money in the State and in the University. The University was keen to use the money to better itself. It attracted people like Steven Weinberg. Steve came, he brought people, and he was very interested in gravity at that time, and how one might be able to incorporate gravity into the standard model of elementary particles to perhaps produce a theory of everything.

G: Before you went to Cambridge, did you already envisage having an academic career?

P: I think I was one of the last of a generation where you didn't have to think so much about pursuing an academic career. I simply said I would like to go to the states for a year and then, somehow, it was set up. Similarly, I didn't formally apply for my job in Austin. All that was done without really thinking hard about whether I wanted to pursue an academic career.

G: So as a 15 year old, you just liked doing math and physics?

P: Yes, I knew I was going to do maths or physics and I thought: quantum gravity–that's the big problem. And, and of course, general relativity is beautiful, particle physics is fascinating and putting those together is a great challenge. But I didn't have to make some of the policy decisions that people do now.

G: Xenia, did you see a career in academia?

X: The idea grew slowly. When I started in biology, I wasn't thinking about that because I was thinking that I would live in Central America. When I did my master's in Costa Rica, I had a very good supervisor who encouraged me to go to the US. My MSc supervisor was quite encouraging.

G: It's so unsettling how you can have a few conversations with people as an undergraduate that have such an enormous influence on the rest of your life. Those little discussions are so crucial.

X: It's only later that you understand the impact of those conversations. I started doing the master's part-time. At the time I was working in a lab, in chemistry. In the end, I did go on and do a PhD and then I liked the idea of being in academia.

G: Did you feel the pressure to publish that postdocs feel today?

X: Yes, absolutely. Perhaps it's worse now, there is a lot of pressure to publish. You see this in committees too, when people say, "Oh, this person only has three papers, but this one has five". It's perhaps not as bad in math as it is in theoretical physics.

G: It feels to me that there's less objective reality in theoretical physics. In maths we have some conjectures and no matter who proves it, it's going to be an Annals paper.

X: In theoretical physics, there is a lot of fashion-driven work. Very often grants are given, at least in part, on that basis. Of course, if you're an early career researcher, you really want to get into those areas. However, that doesn't mean fashionable areas are not interesting!

G: The first time that I heard both of your names was in the famous quintic paper. For many mathematicians, this is just an incredibly inspiring story. Could you explain for the layperson what happens in that paper?

X: There is a precursor to that paper which Philip and I wrote discussing the moduli space of Calabi-Yau manifolds. We discussed the moduli space of the Kähler-class, and moduli space of the complex structures. Nowadays, it is a looks like a trivial thing that everybody should learn, but at the time it wasn't. A few years earlier mirror symmetry had been conjectured. Philip and his student had been working on the experimental evidence for mirror symmetry.

For the paper, we said if mirror symmetry is true, then there should be an isomorphism between these two moduli spaces, and this became the mirror map. But this was not a classical statement. In physics, what we knew at the time is that you had to include the quantum corrections. On the Kähler-class side the corrections come from the rational curves of the manifold. We knew that the structure of the complex structure moduli space was correct classically, that is, it didn't have any quantum corrections. If mirror symmetry existed, one should be able to construct the mirror map and compute the correlation functions on each side of the theory. The relevant three-point correlation function is what physicists call the Yukawa coupling, and this is what we computed. We computed the coupling classically on one side and then, using the mirror map, one obtains the quantum-corrected answer on the other. This story started with this paper, and then we implemented that idea for the quintic paper, in 1990/91.

G: So in the earlier paper you said that such an isomorphism has to exist, but in the quintic paper you could actually work it out, and give concrete predictions for the numbers of rational curves of each degree?

X: That's right, it was quite a chunk of work.

P: There was an idea that perhaps there was mirror symmetry, because space time is 10 dimensional, and to have a supersymmetric world, four of the dimensions are the familiar dimensions of spacetime, and six of the dimensions are a Calabi-Yau manifold. The properties of the Calabi-Yau manifold tell you about the high energy physics in our world. It's really low-energy physics in the in the big picture, but it tells us about the physics in the four-dimensional world.

In particular, the Hodge numbers, or the topology of the Calabi-Yau manifold is reflected in the number of families and antifamilies of particles. This meant that you could change the Euler number of the Calabi-Yau manifold by a sign and nothing would change in the physics. The thought was that perhaps Calabi-Yau manifolds always come in pairs. It was a weak argument, but it turned out to be true. For every manifold with certain Hodge numbers, there's another with the Hodge numbers interchanged!

Mathematicians tend to produce examples by the traditional means of algebraic geometry—you write down a quintic equation that acts in complex projective four-space, and you can generate lots of examples like that. These always have a negative Euler number. If you give an equation, as with the quintic, you have four-dimensional projective space with one Kähler form. So you have one parameter family of projective spaces and a many parameter family of quintics. There are many terms in the general quintic, and the coefficient of each term is, basically, a parameter. You get 101 parameters from the complex structure, and just one parameter of the projective space. The Euler number for the quintic is twice the difference of these numbers, so -200.

There are many examples like this where you give some polynomials in a product of projective spaces. You have a few Kähler parameters corresponding to the projective spaces and a lot of parameters corresponding to the polynomials, so you produce lots of examples with negative Euler number. Meanwhile, some physicists had seized on examples that you get from orbifolds. Those always have positive Euler number.

There was one group of people who said that it's clearly the case that there are more Calabi-Yau manifolds with positive Euler number, and then there was a group of algebraic geometers who said no, it's clearly the case there are more Calabi-Yau manifolds with negative Euler number! At the beginning, it didn't look as if there were equal numbers of these things at all. We had to generalize the constructions and start looking at polynomials in weighted projected spaces. This is a sort of hybrid object where you take a projected space, you divide by groups and blow up the fixed points.

Suddenly, when you started constructing lots of these and you start to ask how many of examples like this can one construct? Thousands! When you plot the Hodge numbers of these spaces on a graph, you see that the plot suddenly looks symmetric. That was a wonderful thing. This was done by two of my graduate students Rolf Schimmrigk and Monika Lynker.

G: When did you first start seeing that plot? Was that in the 90s, or earlier?

P: That was in the 90s. At that same time, Xenia and I were working on the moduli spaces because if these two spaces of Calabi-Yau manifolds are the same, then their moduli spaces, the parameter spaces must be the same. Again, that didn't quite fit because there's natural geometry on the complex structures and there's a natural geometry on the Kaehler classes, and it's different. But we know that the Kähler classes get corrected through the quantum corrections.

From another way of looking at it, ultimately, you're analysing path integrals, and one way to study it that is by steepest descent. You have a series that counts the number of stationary points in the integral. It turns out that in the supersymmetric cases this sum, which you would think is just an approximation, in fact is exact. The terms in the series have a very beautiful interpretation as sums over the rational curves in the mirror manifold.

G: We've probably lost the lay audience at this point! But, in summary, you made some predictions about things that mathematicians really cared about, and initially the predictions were basically met with disbelief.

P: The numbers of rational curves were known in degree one and degree two. The number for degree one was calculated, by Schubert, as 2875. He calculated that on two different occasions in his career, by different methods. Sheldon Katz, in his thesis, calculated the number of rational curves of degree two on the quintic, which is 609,250.

The way we did the calculations was simple manipulation of periods. If you have a computer, you can just carry on and keep calculating more. There was a third number, the number of cubics and this number is 317,206,375. And we were just at the stage where Norwegian mathematicians Geir Ellingsrud and Stein Arild Strømme were looking at this. Again, even mathematicians needed a computer (actually several computers) to do this. The trouble was that Ellingsruud and Stromme did not agree with our result for the number of cubics. They came up with a very different number.

X: It was more striking because we were not just predicting that number, we produced a generating function for the numbers of each degree.

P: The community divided. The algebraic geometers said; Ellingsruud and Stromme are good guys and we understand their method in principle, even though the calculation is complicated. They must be right, and we've got to support them. The physicists were lining up and saying, look, if you read the quintic paper, you see how it's done, and this is the answer. It took a year to resolve.
And it turned out to be some elementary thing that was wrong in one of their original computer codes. Once that was corrected, the answers agreed.

G: Can you remember when you first found out that algebraic geometers agreed?

X: Yes, there was an e-mail from Herb Clemens!

P: The email simply said: *Physics wins.*

It was a different world [at the time]. For example, some mathematicians wouldn't even talk to you. And if they did talk to you, they'd say, come to my office and they'd close the door because they didn't want it to be seen that they were talking to a physicist. Some people thought it might impact their tenure prospects. It was a disreputable thing to do.

X: It wasn't talking, it was collaborating!

P: Well, conversations could turn into collaborations.

G: That's extraordinary. I was totally unaware of that.

X: It was only some people.

G: It must have shifted massively in the 90s, because when I grew up, mathematically, to talk to a physicist was a very positive thing.

P: It changed, certainly attitudes changed. Prior to that, mathematicians had always gone first. Einstein had a clear idea of what he wanted to develop with general relativity, and he was fortunate that mathematicians had gone before and there was already a formalism for discussing geometry. People had thought carefully about what geometry was, and Einstein was able to learn and to implement these ideas.

This was an instance where, because we have this model of the world in physics, we could see things that, at the time, were not appreciated in algebraic geometry.

G: Surely that was also the case with path integrals?

P: But path integrals weren't respectable.

G: So this was the first time that it was respectable for a mathematician?

P: Well, it wasn't necessarily the first time. Atiyah and Singer and others have done important work in gauge theories, understanding things that physicists were telling them. But there was a feeling that this involved path integrals...and the path integral was a black art that was infinite and couldn't be defined in mathematics, and that physicists were talking about things that they didn't understand, which is in part true. The world has changed.

G: I think there's two things going on. One is that lots of mathematics is very conservative. That can be nice, it means that mathematicians are anti-hype. But also I think that there's a degree of intimidation. When I speak to some physicist, I just have no idea what they're talking about, so it's really difficult for me to even start a conversation.

X: That is a language issue, at least in part.

G: When was Witten's Fields medal? [Witten is a leading theoretical physicist known for his impact on pure mathematics]

X: 1990.

G: I think this is widely seen as a testament to increased dialogue. If you imagine a graph throughout the centuries of mathematics and physics interaction, how would you characterize that over the last three decades?

X: I think it's extremely rich, I think that the interaction is fantastic.

G: Do you think it's stronger or weaker than it was 20 years ago?

X: It's difficult to say. For example, among the people we know well, which includes geometers, there were people like Nigel Hitchen who had been pushing for this interaction for a long time. This interest in physics has come from mathematicians and vice versa. In our field, it's so mathematical that that that we tend to interact quite a lot with mathematicians.

P: It's definitely much more now.

X: But we're biased by our own research area. What we do is very mathematical. But with string theory, the overlap with mathematics is huge.

G: I remember when I was a graduate student and there's that paper of Kontsevich where he counts rational curves in the projective plane. I just thought this was one of the most beautiful things I've ever seen in my life. If I can ask you both, what are you proudest of?

X: There are some different stages in my life that I'm proudest of...I think the moduli paper. That was the one that had the ideas. Of course, the quintic paper brought this to life. I am also very proud of the work on number theory, the research that are happening now.

G: I'm interested in what role homological mirror symmetry plays for you? For us mathematicians, we love it.

X: We never talk about it, for some reason. I always wondered whether we should have some sort of small learning group of mathematicians to discuss whether we have something to say about it.

G: Just one more technical question. Can you tell me about Batyrev's reflexive polytopes?

X: That we use all the time!

P: At the time, we were in Switzerland. I was at CERN and Xenia was doing a postdoc at Neuchatel. Batyrev came and gave a seminar at CERN to the theory group. He's a great guy, but he's a hard-core algebraic geometer. He was talking about these polytypes, and I think it's fair to say that no-one in the audience understood what was going on. Seeing that this was, in fact, incredibly useful changed the entire direction of our work for years.

Sheldon Katz explained this to us in terms that we could understand, and then we wrote a paper.

X: At the time, we were trying to understand what Batyrev did.

P: At the beginning, the language was a considerable barrier and an important role was played by people like Sheldon Katz and Dave Morrison, who can translate from one language to the other.

X: That has changed. There are more people like that now. At Oxford, we get students with two degrees, math and physics. They may go one way or another eventually, but they learn both.

G: So, Philip, what result are you proudest of?

P: I guess the quintic paper, or the work around that. The establishment of mirror symmetry. The quintic, it wasn't easy, but it was easier than some of the things that came afterwards. The more complicated examples forced a better understanding and forced you to clarify issues.

The work on the on the mirror manifolds was much more intricate. There was this drama when people said, what you say is very interesting, but it doesn't work. It really makes you think; I have an argument, but do I really believe and understand it?

G: What do you think about the current disaffection with string theory.

X: On one hand, this theory has produced an enormous number of results in mathematics. It has been very fruitful. Some criticism has come from people who say, well, predict something.
You can either throw it away, as a physical theory and continue using it as a ground for new ideas. Or you can continue working on it and see whether you eventually can say something about the world. I think another criticism is due to the amount of money that is going into this research.

G: It is a very important question, where money goes.

X: It may be that string theory is not the "final" theory. But still, you cannot take away the enormous number of new ideas that have been produced.

P: I would go further. It's clear that that that people say that string theory is very valuable for mathematics, but it doesn't tell us much about physics. That's an exaggeration. I don't think that could be true. String theory clearly is consistent at many levels, it's a very deep theory. Where it affects mathematics, it's been shown to be correct.

I don't think that you can have a theory that's as rich and not have it apply to the physical world in some ways. It includes gravity, it includes quantum theory. It's definitely based in physical theories and it solves problems that were previously insoluble by putting together general relativity and quantum theory.

G: One could make a kind of devil's advocate argument that any mathematical theory which is true has some impact on physics just because physics reflects the mathematical world.

P: This has a big impact on physics. We may not be formulating string theory in the right way. I'm sure that's true, but eventually we will crack this. It's slower than many expected, and people are anxious: what will we see next at CERN? I would like to know that as much as anybody. At the moment, we don't have a hard prediction, but we'll get there, eventually.

Experiment has been very important in developing these theories.

G: It's somehow a really beautiful dance, the experiment-theory dance. Many people criticize string theory because it's in some sense an experiment, but that's also been the case historically with general relativity.

P: You have a theory, and it may tell you about things that you can't measure yet, but nevertheless, you have a theory: you must explore the consequences of the theory. Even if it's not practical to measure all the consequences, you can think about it.

G: Thank you very much. I enjoyed this conversation very much.

After the interview, Philip kindly provided further detail on two incredible discoveries:

1. The story of the appearance of Calabi-Yau manifolds in string theory:

P: In the Fall of 1984, there was a semester long program on Kaluza-Klein theory at SBITP, in Santa Barbara, in which Xenia (then a graduate student) and I were participating. I had slowly convinced myself that one needed a Ricci flat metric on the compact manifold for things to make sense. This, however, was beset by problems in Kaluza - Klein theory. It is a motivating feature of such theories that the geometrical symmetries of the compact manifold lead to gauge theories in spacetime. So if the compact manifold is S^n, for example, there is a gauge group *SO(n+1)* in spacetime.

The problem (oversimplifying slightly) is that one easily sees that if the compact manifold is Ricci-flat then there cannot be any Killing vectors, so there cannot be a corresponding gauge group. During that Summer, Mike Green and John Schwartz had, by taking their cue from string theory, established that theories involving both gravity and gauge fields could be consistent, at the quantum level, only if the gauge group is *SO(32)* or $E_8 \times E_8$. So one has to give up on Kaluza-Klein theory or at least admit that the gauge group cannot come directly from the geometry, but rather has to enter as a separate entity.

Andy Strominger, who at the time held a five-year position at IAS, visited for a month towards the end of the program. One weekend Xenia, Andy and I, together with others, who included Chris Pope, went for a hike up to Gibraltar reservoir. We stopped for lunch and I was explaining why the Ricci flat condition was critical. At that time it was a "folk theorem" among physicists that the only compact Ricci-flat manifolds were tori, K3 surfaces and products of these. A group around Stephen Hawking at Cambridge had been interested in performing sums over Ricci flat manifolds and this was a "fact" that was relevant to that project. Andy, however knew Yau from the IAS and was aware of his proof of the existence of "manifolds with SU(3) holonomy'". Suddenly the scenario was much more interesting. Andy and I spoke to Gary Horowitz, who was on the faculty at UCSB but had previously been Yau's postdoc. Gary telephoned Yau and checked that these manifolds indeed existed. It is curious that, at the time, only a handful of examples were known, basically enough that it was known that Yau had proved the existence of a non-empty class of manifolds.

Andy, Gary and I were excited about this, and began writing this up. While we were engaged in this, Andy's time in Santa Barbara came to an end and he returned to the IAS. At a reception Andy talked to Ed Witten who had similar ideas and was either writing, or was about to write, a paper of his own. We agreed to write a joint paper, and this turned out to be a very good decision. At that stage Gary and I were on the west coast, and Andy and Ed were at the IAS and we would fax copies of the paper back and forth. There was quite a bit of grinding of the gears to get the sections of the paper to fit together into a coherent whole. However the process, though painful, was well worthwhile; I certainly learnt a lot from this.

The semester and programme at ITP were coming to an end so there was pressure to finish the paper and submit, which we did. Xenia and I had married while at the program, so our lives were changed in that regard also. As we were packing to return to Austin, John Schwarz invited me to give a talk in Caltech. He was very excited about our work, and I was invited to speak about it during their lunch-time seminar. A little before the seminar, we ran into Feynman in the corridor. John very excitedly told Feynman how our work was a major step in understanding string theory, and might lead to concrete predictions. Feynman listened and then said: "What you say is very interesting, but 12 pm is when I eat lunch."

2. The story of Batyrev's construction entering string theory:

P: Forgive me if I take a step back from this and provide some history as I see it. In 1990 Monika Lynker, Rolf Schimmrigk and I had written a paper listing some 6000 polynomials which constructed a class of Calabi-Yau manifolds as hypersurfaces in weighted projective spaces. The list of these spaces showed a striking symmetry: in the overwhelming majority of cases, where a manifold occurred with an Euler number χ there was a "mirror manifold'' in the list with the Euler number $-\chi$. We knew that we did not have a complete listing so we were not troubled by the relatively rare occurrences of cases for which the list did not contain a mirror.

As an aside: the manuscript of the paper contained a table listing all 6000 cases, which ran to 30 pages. Paul Ginsparg, who ran the ArXiv, would not allow us to post the paper, until we removed the table, because this would entail an unconscionable waste of storage on something that was perhaps of interest to mathematicians, but of no interest in theoretical physics.

In retrospect, it is surprising how durable the process of compactifying string theory on a manifold has proved to be. As long as this process continues to be useful then we will have to continue to study and describe the manifolds that are relevant, and this study is called geometry.

Allow me to return to the subject of the hypersurfaces in weighted projective spaces. Two teams, Albrecht Klemm and Rolf Schimmrigk, and Max Kreuzer and Harald Skarke, had set out to compile a complete list of these. As luck would have it both teams announced a complete list on the same day. Klemm and Schimmrigk had a list of 7555 spaces and Kreuzer and Skarke had a list of 7554. Only these numbers were exchanged between the two teams. By the end of the day Kreuzer and Skarke had found the single, very degenerate, configuration that they were missing. Curiously, with the new lists, the proportion of spaces that did not have a mirror had increased to about 10%. Kreuzer and Skarke even put out a paper with the provocative title "No Mirror Symmetry in Landau—Ginzburg Spectra".

Now, at this time, we were in Switzerland, Xenia had a postdoc at Neuchâtel and I had a visiting position at CERN. Victor Batyrev came one week to give a seminar to the Theory Division at CERN. Victor was very enthusiastic and did what he could to explain his ideas about mirror symmetry and dual reflexive polyhedra, but I think it is fair to say that no one in the audience understood the seminar. Together with Sheldon Katz, Xenia and I were trying to understand the Klemm—Schimmrigk and Kreuzer—Skarke lists and, in particular, how to understand the difficult cases that did not seem to give rise to mirror pairs of manifolds. Fortunately Sheldon Katz was able to explain Victor Batyrev's ideas to us. The formalism to do with the reflexive polyhedra turned out to be key to resolving the difficult cases. So we were able, in the end to show that, properly understood, each of the 7555 "Landau—Ginzburg theories'' led to a pair of reflexive polyhedra and so a mirror pair of manifolds.

Kreuzer and Skarke were immediately convinced and they embarked on what we regard as a triumph of modern mathematical physics, which was the construction of a list of all four-dimensional reflexive polyhedra, which are close to 500 million in number. Sadly, Max Kreuzer died in 2010. It is fair to say that this construction of the list of all four-dimensional reflexive polyhedra is his most famous work.