Interview with Sir Michael Atiyah Fields Medal 1966 and Abel Prize 2004

Oscar García-Prada (Consejo Superior de Investigaciones Científicas, Madrid, Spain)

Michael Atiyah and his collaborators have changed the face of mathematics in recent decades. In his work, one could single out, among other fundamental works, the index theorem (in collaboration with Isadore Singer) and the study of the geometry of the Yang–Mills equations, with important applications in theoretical physics. His contributions wonderfully illustrate the unity of mathematics and show, in particular, the importance of the interaction between geometry and physics. He is a key actor with tremendous influence on the work of the scientific community devoted to these subjects. Among other prizes, he has been awarded the Fields Medal in 1966, the Copley Medal in 1988 and the Abel Prize in 2004. He was also one of the promoters for the foundation of the European Mathematical Society.

We were with Sir Michael Atiyah in the French city of Brest on 10 July 2014, immersed in a conference on real vector bundles organised by the Centre Henri Lebesgue. This theme has its origins in a seminal paper of Sir Michael from more than 50 years ago.

Sir Michael Atiyah the day of the interview.

Michael, since your work has produced fundamental chapters in the mathematics of the 20th and 21st centuries that are very well known, I think it would be nice if we could speak about the people that you have met in your mathematical career.

Yes, sure. I like talking about people.

Yes, your memories and recollection of some of these people. I'd like to start with your supervisors when you were at university, or even school – I mean your mentors, but especially your supervisors Todd and Hodge. What can you tell us about them?

Yes, well, I went to school in Cairo – it was an English school – and also in Alexandria. I had my teachers there; I had quite a good mathematics teacher (but a bit old-fashioned and not very sophisticated). I mean, I got a good education but nothing special in mathematics. I was always the youngest in the class, by two years actually. I was the small boy in the class. When you're at school and you're two years younger than everybody else, what happened was that I would help the older boys with their homework and in return they would defend me. So, I had powerful friends; they were big but they were not so clever so I would help with their homework and in return I got bodyguards [laughs], which is important if you are small, you know. At school you can get bullied if you're small and everybody's older. So that was very good.

In my last year at school in Egypt, in Alexandria, we had a mathematics teacher who was old-fashioned. He was quite good. In fact, he hadn't trained as a mathematician; he had trained as a chemist but he was a good teacher: very severe and disciplined. I have vague memories of a teacher who had a French education in a very different style. I think he was Greek (his name was Mouzouris). I remember he actually gave me some books on modern analysis that he had studied in France. That was the first time I met such things but it didn't make very much impression on me.

Then, afterwards, I went to school in Manchester in England. There, I went to a very good school. Well, my father asked how to prepare for university; he asked what was the best school for mathematics and everybody said Manchester Grammar School, which was a sort of intellectually elite school – we had a very dedicated maths teacher. He had been in Oxford in 1910 or something – old-fashioned but very inspiring in a way. So, I worked very hard there because we were working on fairly hard examinations to get into Cambridge, which was very competitive. So I worked harder there than at any time in my life, probably.

How old were you then?

Seventeen. I went there at the age of 16/17 and we were all very well trained so we all got scholarships into Cambridge. I arrived in Cambridge with a very good background. Of course, you don't know when you arrive at university how good you are compared to everybody else because everybody is the best person of their school. At the end of the first year, I came top of the university so I realised I was good from that point of view and I had a lot of friends who were very good mathematicians. Many of them became quite famous afterwards, not only in mathematics but in other fields. So it was a good environment. I went to Trinity College, which is famous for Isaac Newton and many other people: Ramanujan, Hardy, Littlewood – so there was a strong mathematical tradition in the college. Eventually, I came back as the Master of the College 50 years later [laughs].

So, I had very good training. The lectures were rather average, with one or two very good lecturers but most rather nondescript, and one or two very bad lecturers. But there were one or two very good lecturers; then I did the courses and I did accelerate. I went to many lectures to progress very fast and published my first paper as a second year undergraduate. I went to some courses by Todd. There was a nice problem in classical geometry; I made a little contribution and he encouraged me to publish it. Just a two-page note, you know, but I was a second year student and it gave me tremendous pride to publish a paper! I am probably more proud of this paper than anything else. So, that was a good start and then, after that, I did my graduate studies. I had to select a supervisor for my PhD. I had been taught as an undergraduate by Todd, who was a good mathematician but very shy. He didn't speak when I would go to see him; he would discuss the problems but then nothing else. So I had to go along with a long list of extra questions to ask him to keep the conversation going.

I decided not to work with him but to work with Hodge, who was much more famous for his work and had an international reputation. I was impressed by him. I thought he would have a bigger vision and he did but he was also a very different person to Todd. He was a very gregarious, extrovert, friendly person. If you met him, you'd think he wasn't a mathematician; he looked like a grocer running a shop. In fact, I discovered afterwards he came from a family that had grocer shops! Grand shops [laughs]! He was the only one who went into mathematics; all the others were doing business in their shops and so on. But he was very affable and very friendly so he had a big influence on me and gave me good direction. So it was a good start to my career and I was lucky to arrive at a good time. I had good fellow students and the mathematical world was just changing after the war. New things were happening in Paris and in Princeton. I used to go to the library every week to see the latest issue of the Comptes Rendus: new papers by Serre, Cartan. And Hodge had contacts in Princeton, I would hear. So I was quite quickly in touch with these movements. This helped me get started and I went to Princeton.

What was the mathematical problem that you tackled in your thesis? Was it Hodge who suggested this problem to you?

Well, I did two quite separate things in my thesis. One, I picked by myself. It was to do with what geometers call ruled surfaces. These are surfaces which are families of lines arising in classical geometry. I got interested in them from one point of view, relating them to vector bundles and sheaf cohomology methods. I used modern methods to start a classification but these were the early days. It became a big industry afterwards. I wrote the first paper on the subject in 1953–54 and wrote it more or less by myself. In my second year of research, Hodge, with whom I had been working, saw how to use modern methods to attack the whole problem he had been interested in in algebraic geometry integrals. So, he gave me the idea to start with, which I developed, and then we wrote a joint paper together on this, which became quite well known. So, I did two quite separate things in my thesis. One was entirely my own work and the other was really in conjunction with my supervisor. By the end of the second year, I had more or less finished.

Where did Hodge come from mathematically?

Well, Hodge was a Scot and Scotland has a very good tradition. He graduated from Edinburgh University, which is actually where I am now. He went from there to Cambridge to finish a degree and so he had a good background in mathematics and physics, which was actually relevant to his work (Hodge theory). Then, in Cambridge, he was in a very strong school of geometry (old-fashioned geometry) and he forged his own way, away from this feeling of ideas. He was very much influenced by Lefschetz, who was revolutionising algebraic geometry by using topology methods. He wasn't present – it was action at a distance; he followed Lefschetz's books and works and eventually he met him. So, by entirely his own choice, he made his name without wanting to and, of course, he was young and went to Princeton. Interestingly enough, when he first met him, Lefschetz refused to believe that he had proved what he had proved. He kept arguing he was wrong and it took Hodge a long time to persuade him he was correct; eventually, he used Lefschetz's ideas in a more complicated way. Lefschetz had a very strong personality and when he was finally persuaded that Hodge was right, he reversed himself and became a strong supporter. From being a strong opponent, he became a strong supporter and he got Hodge a chair; he was a great support. At first, you know, it was all rubbish. Then, after a while: 'Ah! Magnificent!' He was a very colourful personality. I met Lefschetz when I first went to Princeton because I was Hodge's student, and he was very aggressive. By that time, he was doing other things but he looked at my paper with Hodge and he said: "But where's the theory? Come on, tell me." He was sort of aggressive, trying to say there was nothing in the paper of importance. I think it was a matter of style, anyway. We became good friends later on but he was a very strong personality.

Of the people you met after your thesis in Cambridge when you went to Princeton, is there anybody you would like to mention?

Yes, I went to the Institute for Advanced Study. There were a lot of distinguished permanent professors but I arrived just too late to meet people like Hermann Weyl, von Neumann and Einstein. They all died more or less just as I arrived. Besides the permanent professors, they had a large number of brilliant young people that came as post-docs and, because it was just shortly after the war, there was a large backlog of people whose education had been changed by the war – many generations were sort of compressed together. So, there I met Hirzebruch, Serre, Singer, Kodaira, Spencer, Bott – all of these – and I spent a year and a half at Princeton. That was the time when I really met most mathematical talents. I learnt things I'd never heard before, like Lie groups and topology.

They were all in Princeton?

They were all in Princeton Institute, yes, exactly. Kodaira and Spencer were respected professors and the others were all post-docs. We spent a year or two together and some of them had been at Princeton before, so it was a very good meeting place for young people. We learnt a lot from each other. We didn't go to lectures together at university. I'd learnt by myself from the French school of mathematics in France, and while I was at Cambridge, but at Princeton there was personal contact and the influence of people. I'd say I got very friendly with them all. I learnt a tremendous amount in just over a year. It was like reaching adulthood; suddenly I became a sort of professional mathematician. We learnt new ideas; it was one of the top places in the world and there were all sorts of things happening and new advances every week: new theories, characteristic classes, cohomology. It was an ideal time to come in and I made my own contributions.

I got to know Hirzebruch and then, when he came back to Europe, I carried on meeting him and meeting people in Bonn, that sort of thing, so it was very good. It was the ideal time to arrive at Princeton, in that period, and then come back to Europe. Things were happening there too. You know, the war finished in 1945 and I went to Princeton in 1955 (enough time for things to settle down) but many of my colleagues had been not exactly fighting in the war but had been called up. Singer served in the US Navy. Bott was trained and about to enter the war. Hirzebruch was in the German army as a young man and was captured as a prisoner of war by the Americans but only for a few months; he was 17 and he escaped. So I was just on the tail-end. The people who were caught up in the war were older and there for a long time as well. By the time I went to Princeton, it was ten years over. People had recovered and so it was a very good time.

And you came back to Europe after two years, right?

Yes, I had a year and a half and then I came back. I had a job in Cambridge. I came back to a job and I spent a few more years in Cambridge and then I moved to Oxford.

So why don't you say something about your students, both in Cambridge and in Oxford?

Well, in Cambridge I didn't have many students because I left Cambridge young but I had a couple of students I had inherited from Hodge, my supervisor. He had taken on students and by this time he was a very busy man. He didn't have time; his own career had been sort of spoilt by the war. He had become famous when still young before the war and then during the war he'd had to stay in the college and do a lot of administration. By the time the war was over, he was a bit out of touch so he took students but he passed them onto me. So, my first two students were handovers and they were OK. They both did their theses with me. It was good preparation for me; I had to learn how to handle students. It's not so obvious and, of course, you realise after a while that some students teach themselves, some are independent, but many need a lot of help because they come with many different levels of ability. Some are very strong, some are rather weak. So, I had these two students who were with me before I went to Oxford. In Oxford, I was there for very much longer and I gradually got more students over time. When you are young, you wonder why they would want to come and work with you, you see. You have to become a bit older and a bit more famous and then students come. I had a large number of students, altogether around 50 students. Well, it's difficult to count students because the face of a student is not so well defined – or somebody else's student is really, de facto, your student – but between 40 and 50 students over a period, over a lifetime. At a given time, I would have five or six students doing their PhDs with me, two in each year, and so that was good. Then, I went to Princeton as a researcher and had four students there.

You mean that while you were in Oxford, you went to Princeton again?

Yes. I went to Oxford first in 1961 and in 1969 I went to Princeton. So I was in Oxford for eight years and then I went to Princeton for three and a half years and then I came back to Oxford. One advantage at Princeton was that you could invite people to come and work with you, so you had some choice. One person who came with me, originally from Oxford, was George Luzstig, a very young man from Romania; he was a brilliant student. He was my student in Princeton. And I could also invite people as my assistants so I had Nigel Hitchin as my assistant.

He had already been your student in Oxford, right?

He had been my student (or de facto student). He had been officially working for somebody else but he worked as I suggested and I kept in touch with him. So, he was really my student as well. Before that I had Graeme Segal. He had been another student of Hodge for a year.

Hodge sent him to Oxford?

Well, I think he sent himself to Oxford [laughs]. He came to Oxford to work with me. By that time, I was collecting students. In Princeton, I had a few and when I came back to Oxford I got a large number of students because, by this time, I suppose, I was better known. I got many students from Cambridge, many students from abroad, several from India. Ah! Patodi was a very young Indian; he came and worked with me as a de facto student. Then, later on, I had some very brilliant students: Simon Donaldson and so on. It frightens me; I went through a period where I was thinking I'm not getting very good students. I'm not doing very well. Maybe I should stop taking students. I'm no longer sufficiently active. And then something changes and suddenly you find half-a-dozen brilliant students and it's very much, sort of, a chance event. Of course, you learn from your students, the very good students. Donaldson was there. He gave some lectures after a while. I went to his lectures, even when he was just barely doing his PhD. So, yes, you learn quite a lot and with so many students you give them some thesis to work on, you encourage them, you tell them which direction to go, you give them various degrees of help and sometimes they do everything themselves, sometimes you do the work for them and sometimes it's a collaboration. So, it's a very positive experience and I enjoyed that. When I went to Princeton Institute, I didn't really have students; there was no formal university, you know. For Oxford university students, some were local and some would come from outside to do PhDs (specifically with me or some by themselves) and then there were some from countries like Australia (like Graeme Segal), America, India, yes, quite international.

So, you collaborated with some of your students, like Nigel Hitchin.

Yes, I collaborated, usually after they had finished their PhDs, as colleagues, junior colleagues. But, because they had worked with me, they worked in the same area. So it was natural that I continued together on joint papers with Nigel Hitchin and Graeme Segal. Usually, I liked to have my students working in slightly different areas: some in differential geometry, some in algebraic geometry and some in topology – so they weren't all in the same field. So, I would collaborate with them and they would also have their own individual personality and mathematical tastes – they would be different. They would be going in slightly different directions, which is very good. You get to broaden – some more with analysis, some geometry, some more with topology – and that way you learn with these 20-year-old students because they become more expert. Segal became more expert in homotopy theory, Hitchin became more expert in differential geometry… So, it's a way of learning. When you start off, you learn something, but when you're teaching, you don't have much time to go back and study so you have to learn in a different way and one way to learn is through your students, in collaborating with your students.

Perhaps you can say something about the main collaborators you have had throughout your career.

Yes, among my main collaborators (senior collaborators, my age or older) there was Hirzebruch, who was just two years older than me. He seemed older than me; I went into the army and did two years there – he didn't do that. He got promoted very young. He was a professor when I was just finishing my PhD but we were quite close in age really so we collaborated for quite a long time because I used to go to Bonn. Work developed there; it was natural that we should write papers together. Then, the other two people I worked with were Bott and Singer. They were in America, in Harvard and MIT, and I used to meet them in Princeton or I would go to MIT, or they would come to Oxford. We spent a lot of time together. We all wrote papers together. We all had common interests and had different strengths. Hirzebruch was very much close to me in many ways but I learnt from him. He was an expert in characteristic classes and algebraic topology. Bott was more into differential geometry and Lie groups and things like this, and Singer was more from an analytical background and functional analysis and Hilbert space

From left to right: Henrik Pedersen, Nigel Hitchin, Nedda Hitchin, Sir Michael Atiyah, the author of the interview, Graeme Segal, Jacques Hurtubise and Jean-Pierre Bourguignon (10 September 2016 at the celebration of Nigel Hitchin's 70th birthday conference in Oxford).

theory. So, they all had slightly different areas of expertise but they all overlapped and so we had a lot of common interests, which was very good. I was able to write many papers. They were experts – well, not only were they experts but they knew the real experts. Singer had a lot of good friends who were leading figures in differential equations and so on, and Bott knew a lot of people in topology and he knew a lot of people through Bonn, so they all had very wide intellectual networks of contacts and students. Smale and Quillen were students of Bott, so this gives you a good network.

I'm very gregarious. I like to talk, you see [laughs] and I love mathematical discussion. We would get to the blackboard and we would exchange ideas and I like this. It is very stimulating. After we talked, we would think and we would go back and discuss again. So, it's a very social process and so you make good friends too. A working relationship is very intimate in that sense. So, they were my main collaborators. Then I had younger collaborators like Graeme Segal, Nigel Hitchin and, later, younger ones like Frances Kirwan. I wrote quite a few papers with Hitchin and Kirwan. This was a similar relationship just inverted because I was the teacher and they were the students. We had common interests and, again, their interests were paralleled by the interests of someone older. They were quite a new generation with new ideas, so it was a very good network.

You also had very good friends in the physics community, in particular with Witten, right?

That was later, yes. I remember meeting Witten when I went to America in the early 1970s. We had just realised then that there was some overlap between what the physicists were doing and what Singer and I were doing. So, I went and had a meeting with a group of four physicists from MIT – these older people and one young chap sat in the chair and, at the end, after the discussion, I realised he was a really bright guy. He understood much more of the mathematics I was trying to explain – and that was

Ed Witten. He was a junior fellow. After that, I invited him to Oxford for a few weeks; I got to know him well. So, I've known him since he was a young fellow at Harvard and he was always tremendously impressive. I learnt an enormous amount from him and I tried to read almost every paper he wrote. He writes an incredible amount and I think that one of my main contributions was to introduce the mathematical world to the ideas coming out of physics through people like Witten and his collaborators. In the early days, a lot of mathematicians were suspicious of physicists. They said physics was nothing to do with mathematics: 'They don't prove theorems', 'It was a doubtful business.' So, I got a bad reputation for mixing with bad company, you know [laughs]! I think that even with Witten mathematicians were sceptical but they understood he could do things they couldn't do – he opened up many doors and got the Fields medal. So, following his development was really part of my education and, in the end, I became like his graduate student (but this was many years later). I spent a term with him in Caltech and it was a bit like being a graduate student again. I would go and see him in the morning, we would have an hour discussing each problem and then go away and think about it for 23 hours, before coming back. Meanwhile, he would do everything else. I would come back the next day and we would carry on the discussion. I had to work to keep up with him…

You wrote a paper.

Yes, a 100-page paper. I wrote parts of it. He decided we should work on this, probably because it had some relationship with what I had done before. But he had ideas about it. He pushed and he was so good that we would occasionally have arguments about the mathematical side of the results and he would usually be right [laughs] and I would be wrong, yes! It was quite an experience, usually; by this time, I was already getting old, well, advanced in years anyway, but it was like being a student – really exciting. Even now in Edinburgh, among the people I collaborate with there are many physicists, mathematical physicists – physicists of the new generation. I do more and more mathematics in connection with physics.

Going back in time, you also interacted a lot with Roger Penrose.

Yes, well, Roger Penrose was my fellow student. He came as a student from London and started his PhD the same time as me, as a student of Hodge, but he didn't get on very well with Hodge; his interests were different and so, after one year, he switched to Todd.

The reverse thing that you did.

Yes, well, I had been taught by Todd. It was ironic because Todd was doing more algebra and geometry. We lost touch when he finished Cambridge and went elsewhere. So, then he became seriously interested in physics. We met again when he came to Oxford as a professor of mathematical physics, after I came back from Princeton. Then we managed to rebuild our connections. We had this common root in algebraic geometry and he was able to explain to me what he was doing and, after a while, I realised the modern ideas of sheaf theory were really what he needed. I introduced his group to new ideas in physics and that went off very well. I wrote a paper with one of his students, Richard Ward, so that went very well. Interestingly enough, when I was at Princeton at that time, before going back to Oxford, I talked with Freeman Dyson and we discussed Roger Penrose and he said: "Oh! Roger Penrose did some very good things about black holes, which I always admired, but he did some very funny things about twistors. I didn't understand, so maybe, when you go to Oxford, you'll understand what twistors are." And he was right, exactly right [laughs]. That was the connecting link.

It was connected to your common background in algebraic geometry, right?

Of course, we learnt about the Klein representation of lines and Grassmanians. We knew classical geometry well, so it was a good relationship and we got on well. He had a large number of students; he worked with a team of students and he met Hawking when he was a younger man, so I had good links with that group of physicists and I learnt a lot – also through Singer. Both Singer and Bott had degrees outside mathematics originally. Bott trained as an electrical engineer and Singer trained in physics. They got into mathematics after. Singer went into physics and then decided physics wasn't rigorous enough, you know. But Bott was trained as an electrical engineer and got into mathematics through Hermann Weyl, who pushed him in the right direction, in a way. Yes, they came from different backgrounds because, in those days, mathematics wasn't really a profession. Your father didn't think you should do that; you should train in a job, like engineering, that would give you some money [laughs]. To be a mathematician wasn't regarded as an occupation where you could get a job. Of course, it has changed a bit now but in those days it was very much so.

Singer and Bott knew Chern very well. Chern was a very good friend of Yang because he had taught them in Chicago. They were both Chinese so there was a link – Yang, Lee, Jim Simons and Chern, and Singer – and that gave us entry into modern physics at the same time, when things were happening. But it was coincidental. It was very funny. At Princeton, they had this big School of Mathematics and Natural Science, which had originally been one and then had been broken up. The first appointments in Princeton were all big figures: Hermann Weyl, Von Neumann, Gödel – people like Pauli were also there. Ah, but later on, mathematics became a different kind of mathematics; they were rather Bourbaki type, rather pure mathematics and physics. They just drifted away from physics so when I arrived, they were totally divorced; they didn't talk to each other. Dyson could have been a link because he started life as a mathematician and became a physicist, but physicists and mathematicians had, by that time, gone down different paths. They were pursuing different things, it may be said, and mathematicians were not very sympathetic to physics. They thought physics was a messy subject, not really rigorous, and the physicists themselves had similar views about mathematics. Modern mathematics was very abstract, so they really had no link. By the time things had changed and Witten came on the scene, it was totally different. It was more interactive; they had some seminars together but they still kept some distance.

But, if we go back to the 1950s, was it really an accident that physicists were developing Yang–Mills theories and mathematicians were simultaneously developing the theories of bundles, Chern classes, connections and all that? What was the connection?

Well, it's a very interesting story. I mean, the lynchpin would really have been Hermann Weyl. He was the person who introduced gauge theory to physics. He wrote the first paper on how to use gauge theory methods. He was the grand man of mathematics and he was at the institute very early on. But he died in 1955, the year I arrived. Yang–Mills theory was developed, more or less, by that time. I met Mills, who was there as a visitor. One would think that Yang and Hermann Weyl would have spoken while Weyl was still interested in physics.

They overlapped in Princeton but I believe they never had a chance to discuss.

Well, by this time Weyl was a bit older and his interest in physics had been 20 years before. Modern physics had moved in very different directions; he was doing quite different things. New particles had been discovered and he wasn't much into that. But he was the grand old man and if they had just talked to Herman Weyl, he would have told them all about connections and about Lie groups. So, it was just an accident of age and time that he didn't and I really find it mysterious that he and Yang didn't make some contact. So that opportunity was missed. Simultaneously, by the way, one of my contemporaries in Cambridge, Ronald Shaw, wrote his thesis on this. He independently discovered the theory but his superior said it was "not worth publishing" – poor chap, he never published it. But, at that time there were physical objections to the theory, which made it not so popular, so it was dropped. It was some years later when people re-looked at it. They still had to make some use of it, a proper physical use, and then it became popular. But it was probably 15 years later, in the 1970s, that it was taken up again and, in those intervening years, they were chasing different things. They were chasing symmetries, particle representations, classifications… They were doing quite different sorts of things and Yang–Mills theory was left behind. When it resurfaced, that was the time when Singer and I got involved and interested because we were doing mathematics that was related. But Hermann Weyl knew it all, the physics and the mathematics, and he was there before the physicists. But the physicists never emphasised the geometrical side.

But one gets the feeling that there is a missing link that makes it more mysterious, that they were developing similar objects and they took time to realise this.

Well, you see, the story is that Hermann Weyl used gauge theory in order to unify magnetism with Einstein's theory of relativity. When he writes his paper, it was pointed out by Einstein that it was physically nonsense because what Weyl was doing was working with real line bundles where the change of scale took place. Gauge theory was to do with scale and his idea was that if you went round a path in a magnetic field, you would alter the length and scale of things. Einstein said this was nonsense. If that were the case then all hydrogen atoms would not have the same mass because they would have different histories. Despite this, the paper was published; this is what I find interesting. The paper was published because Weyl insisted he was still right, and Einstein's objections appeared as an appendix. So, Weyl knew about it but it was only a few years later, when quantum mechanics appeared, that they reinterpreted the length of a phase. Then, the physical objection disappeared and the theory became standard, a modern standard. By that time, Weyl had left the subject, he had gone off, so he wasn't actually doing that any more. But he knew, of course, that it was all his theory, although the non-abelian version didn't take off until after his death. If he had lived longer, he could have been the main missing link.

Sir Michael Atiyah (right) and the author of the interview the day of the interview.

But it's also interesting that in the mathematics community the non-abelian theory was being developed.

Yes, but that's almost inevitable. The point is that the theory of bundles is an offshoot of Riemannian geometry. That was all developed by Riemann and the Italian geometers – differential geometry, parallel transport. That was for the tangent bundle, for the metric, not for a super-structure of bundles, which is actually easier. The case of a metric is more difficult.

When Einstein presented the theory of relativity, there was a great deal of interest from differential geometers. That gave a big spurt to differential geometry. Parallel transport was all part of general relativity, so this was very natural. What was new was taking vector bundles on top of the space. This was excellent. But the whole notion of parallel transport was familiar to geometers and, shortly after that, Chern and Weil brought it to bundle theory and characteristic classes. In maths, they had been doing this for a long time. They had been doing it ever since Riemann and Betti in differential geometry. Einstein's relativity theory tagged onto differential geometry and Yang-Mills came into it for bundle theory.

This was all part of mathematics. What happened is that Singer and I made links to the Dirac equation, differential equations of the kind familiar to physicists: spin, spinors and so on. That was a new bit of mathematics that hadn't been done before, not seriously. Who knew about it? So, I think mathematics was always there. The physicists had just touched on it here and there and then became seriously interested later. Then Hermann Weyl died. It is an interesting story but, like most things in life, the development of the facts is not what you expect, not what you reap if you do it retrospectively. You'd have done it differently. It's a bit accidental. It depends on the fashions of the time, the people of the time and their personalities. So, it's very interesting, you know. It's not predictable. It's not automatic. It's a bit by chance.

The panorama of theoretical physics has changed enormously after those exciting years, your contributions and those of your collaborators and your school. For example, moduli spaces are now ubiquitous in physics.

Yes, we started off with that and, of course, they came under algebraic geometry, and I knew about those. So, physicists then got seriously interested in string theory and became much more mathematical, and they took over large amounts of mathematics that had been done by everybody else. My students got drawn into Donaldson theory so the interaction increased enormously after that episode in the 1970s and has been enormously influential (and still is). Physics and mathematics are still feeding off each other.

I wanted to ask you about that. How do you feel about things currently? Are there exciting things that you feel are happening?

Yes. As you get older, of course, you get a bit out of touch with what is happening. I get to hear a bit about it indirectly. I read some of the new papers written. There are some developments in Chern–Simons theory. As part of the story that I was interested in, there is knot theory and so on, and I try to follow it to some extent, although less now. The mathematics often gets more sophisticated. There are more abstract things, like derived categories – things that older people don't like. But the interaction is still very close and there's a whole generation of people who are now into both mathematics and physics. It's very hard to distinguish if they are physicists or mathematicians; they are a mixture, hybrids, which means that they have some problems because physicists don't regard them as physicists and mathematicians don't regard them as mathematicians. So, it's difficult for them to get jobs sometimes. I mean, who is going to give you a job if you're neither a fish nor a fowl. But I think this is something that is very healthy and there are some centres where they encourage these hybrid ideas, like string theory. So, there's no question it's still a very active area. Exactly what does it mean to physics? Physics and mathematics have a close relationship but there are differences: physics is looking for a unique solution to the universe while mathematics is exploring all possible universes or possible theories. So we get a lot of ideas. Some of them die in physics because they prefer new ideas, but for mathematicians: they can work on everything so it's a different sort of relationship. You never know with physics.

I have my own ideas. I follow what's going on but I try to be a bit independent. I think there's no point in trying to follow exactly what the young guys are doing. I like to have some thoughts that are a bit more out of the box, so to say, or a bit more original. I play with new ideas that are a bit unorthodox. I am working on some things that are different from what other physicists are currently doing. I mean, nobody knows in physics whether there is a final theory or if we're close to the final theory or whether, in fact, they'll be totally different views in five years time, or whether the series will evolve and there'll be quite radical changes. Some of the ideas at present will be absorbed, some of them will be kicked out, some of them will change but mathematics will benefit from it all, whether it's good physics or bad physics. It has mathematical content and mathematicians have learnt a lot. Mirror symmetries and string dualities are ideas that came from physics. So there's a lot. I think it was Witten's propaganda which said that string theory is a branch of mathematics from the 21st century accidentally discovered in the 20th century. So, it's now coming into its own and it's not quite clear what this is a theory of, but it is bringing new ideas which are transforming mathematics. We're in the middle of a sort of maelstrom of ideas, like swirling winds all around. You don't know what's going to happen. It's hard to predict and you don't want to predict because I always say that if you can predict it's uninteresting. Interesting things are the new developments and if you could predict them they wouldn't be so exciting. So, you have to be prepared for surprises. You have to look for surprises and every now and again there is a surprise.

I'm astonished at how dynamic you are in the conference we're having here. You're still thinking and producing work. Tell me, what is it that occupies your days, nowadays?

Well, unfortunately, at the moment, I'm getting old and my wife is also getting older. She has a lot of problems. I have to spend a lot of time looking after my wife. It happens to us all, in one form or another. So, she occupies 75% of my time. When I come to conferences like this, it's rather a rare event. I get a holiday talking about science. When I'm at home, I just barely survive and I have physics friends I meet once or twice a week to discuss my ideas. For the last year or two, I've been busy writing biographical articles about Hirzebruch. I'm also involved in one for the London Mathematical Society and one for the Royal Society (it's not finished but it has taken up a lot of my time). It was obviously a priority: I had to do it while I am still here.

Outside, I have these crazy ideas that I'm trying to pursue. I talk with younger people because you need young guys to follow it through and so on. And some of them…

well, this year's conference is a bit of an accident because I was into some of these ideas a long time ago and I didn't realise so many people were working on real vector bundles. So, I came in and I found I could follow some but not all of it. And much of it is derived from a paper I wrote 50 years ago. It's a funny experience, you know. I have this experience now. I go to a conference, a big lecture theatre like this. I sit at the top because it's very easy to get in and out. The young guys are at the bottom and they're talking busily about me and my work from 50 years ago. I feel as though I'm living above in the sky, looking down on my past. I'm floating up, closer and closer to heaven. It's a very bizarre experience sometimes. The guys there don't ever know I'm there [laughs]. Also, when you look back on your own work 50 years later, you know, it's a funny experience because you have difficulty following your own papers. When you're a young man, you're very quick and fast. I try to read my own papers and they're quite hard, you know [laughs]. Even though in principal I know them, I've forgotten some of the technicalities and I wouldn't be able to do it now. There are some terrible problems with signs you have to watch out for. So, it's a funny experience looking and it's quite gratifying to find things I did finish years ago that are still alive, you know. Many times things move on and what people do is forgotten, but some of the things I did 50 years ago are still being used and rediscovered or redeveloped by young guys and being pushed in new directions. So, that's very encouraging. I can't say I follow all the stuff but I can see that it's going in a good direction and trying to progress things.

It's been quite nice coming here to this particular event – a small scale event, I mean. I go to other meetings too but I don't have so many chances. I go, of course, to lectures and seminars. I recently went to a festival in Italy. The Italians like festivals, where they have music, poetry and mathematics; it's very nice, sort of a mixed culture. The Italians like this sort of thing. They do a lot of it. Renaissance ideas! I've been to Rome, Milan… The last one was in the south of Naples and I met interesting people. I think it was the one in Rome where I met Boris Spassky – you know, the chess player. We talked about chess and things like that. And then I also met Nash, the mathematician, who got the Noble Prize for Economics. He was there and he was interviewed. I knew him a little bit in Princeton when he was a bit crazy but now he's recovered remarkably well. But, of course, he's an old man – older than me now. [When this interview took place Nash was still alive.]

Did you have a chance to talk?

Yes, He was interviewed, for example, about his life and about the film they made of his life, and I was there. It was interesting but it's a sad case, of course. At least he's recovered from his years of illness. So, you meet interesting people at these events. I met a chap when I stayed at another hotel: Paolo Coelho, the Brazilian writer. He was very famous. He happened to appear on the same stage, in the same performance as me. He didn't care about mathematics. He was a big figure. Yes, so you meet an interesting mixture of people: musicians, poets…

You have recently written a paper on relations between mathematics and beauty, right?

I have a friend of mine who I collaborate with. He's a neuro-physiologist. But he's Lebanese, like me. He's of Lebanese origin so we do Lebanese food together and we meet and, for some time now, we have had discussions. He's interested a lot in art. He's written a book about art and vision, comparing what painters try to achieve with art and what processes happen in the brain. He does scanning and so we got into this question of mathematics. I asked him, you know, when people think about mathematics what happens in their brain. And we wrote about it. So, we had some previous work. The most recent one was about beauty. When mathematicians talk about beauty they know what they mean but is it the same kind of beauty as you see in art and music? Is it the same physiological phenomenon? And, basically, the experiments he did with his team show that, yes, there is a common part of the brain that lights up, whether you're talking about beauty in mathematics or in art or anything else. Of course, other parts of the brain light up depending on the context. There's a common part. So, the abstract notion of beauty is built into the brain and, whether talking about mathematics or painting or music, it is a common experience. So, it's correct to use the word beauty.

So have you experienced this link between mathematics and other art?

Well, we all know what we mean by beauty. We appreciate it through music and art. We also know how to appreciate it in mathematics and I think they are the same but you don't know if this is very objective. Now there is a proof, a scientific proof that it isn't subjective. The notion of beauty is physiologically based on the same kind of experience. So, when we wrote this paper it became immediately famous worldwide. There were articles in the *New York Times*, the *London Times* and one in Madrid. Everyone could understand what it said. So it became instantly quite famous. We originally had difficulty getting it published because for the orthodox guys these sorts of things tend not to be so acceptable. For the general public, of course, it's fascinating.

So, do you think people can get as moved seeing or proving a beautiful theorem as listening or playing a wonderful piece of music?

Yes, absolutely. I mean, obviously they're different but if you compare music and painting, for example, they're not the same; there's a big difference between them but there's a common aspect to the appreciation of art, I think.

But it's more difficult with mathematics, isn't it?

It's more difficult, yes, but that's the whole point. We were unsure if the word was correctly used but as mathematicians we know what we mean by beauty and I think the beauty in mathematics is comparable to the beauty in music. They're not the same but they are comparable, there's no question about it. We know what a really beautiful theorem is [laughs]. It's a subjective feeling but it's true. Now, Hermann Weyl made the following quote: "Most of my life my two objectives were searching for truth and beauty but when in doubt I always chose beauty." Now people think this is ridiculous but why should you be worried about the truth? In fact, I argue this, you see: truth is something you never reach; you find other things while searching for the truth. What you have at any given moment is an approximation of the truth – partial truth. It may even be an illusion. But beauty is subjective, an immediate experience. You see beauty, you know. I'd like to say that beauty is the torch that guides you towards the truth. You can see it. It throws light. It shows you the direction. You follow that and experience has shown that beautiful things lead to true results. So, I think it is a very interesting connection between truth and beauty. I think Hermann Weyl would've agreed with that. People say it was a joke but I'm sure that he meant it.

Talking about beauty, we have dinner very soon.
Yes [laughs].

A light dinner. So I don't want to take up more of your time. Thank you.
Okay. Good. Thank you very much.

I really thank you for this. I enjoyed enormously listening to you.
Yes, I also enjoyed talking about all this.

Thank you very much, Michael.

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Oscar García-Prada is a CSIC research professor at the Instituto de Ciencias Matemáticas— ICMAT. He obtained a DPhil in mathematics at the University of Oxford in 1991 and had postdoctoral appointments at the Institut des Hautes Études Scientific (Paris), the University of California at Berkeley and the University of Paris-Sud, before holding positions at the University Autónoma of Madrid and the École Polytéchnique (Paris). In 2002, he joined the Spanish National Research Council (CSIC). His research interests lie in the interplay of differential and algebraic geometry with differential equations of theoretical physics, specifically in the study of moduli spaces and geometric structures. He participates regularly in public outreach activities on mathematics and their interactions with physics and music, collaborating with the newspaper EL PAIS, Spanish Radio Exterior and Spanish television RTVE.