An Interview

with Jean-Pierre Bourguignon

by **Carlos Florentino** e **Jorge Milhazes Freitas**

Jean-Pierre Bourguignon holds an engineering degree from École Polytechnique and a PhD in mathematical sciences from the University Paris VII. A differential geometer by training, he has since pursued his interest in the mathematical aspects of theoretical physics. He is the President of the European Research Council as of 1 January 2014. He was the Director of the Institut des Hautes Études Scientifiques (IHÉS) from 1994 till 2013 and president of the European Mathematical Society from 1995 to 1998.

Jean-Pierre Bourguignon visited Portugal in July 2016, when he delivered an opening speech for the ENCONTRO CIÊNCIA '16. This was the perfect occasion to make an interview, which flowed as an enriching and pleasant conversation, full of personal insights and experiences, with a mathematician who also occupied several high profile positions.

Jean-Pierre Bourguignon was invited to give three Pedro Nunes Lectures, which were delivered at the Universities of Aveiro, Porto and Lisbon, in October 2016. The Pedro Nunes Lectures is an initiative of CIM, which aims at bringing outstanding mathematicians to Portugal in order to encourage the interest in Mathematics and, in particular, in research in Mathematics.

How did you become a Mathematician? And how did you get interested in Differential Geometry?

Well, it's kind of an unusual story. When I was in secondary school, I was very much interested in Literature and Philosophy. So, this is really what I thought I could be involved in although I had good grades in Mathematics. I was in a small *Lycée*, and had the same Math teacher for five years, which is unusual. He was teaching very efficiently, and using the best students to help the others. So, very early on, I was asked to explain mathematics to others, and now I am sure this played a critical role in my being comfortable with Mathematics.

Then, in the last year of secondary school in France, I moved to a much bigger *Lycée*. Here, I had a very challenging teacher, known to be a remarkable mathematician. All of a sudden, I was confronted with somebody who was saying something which I perceived as interesting and important but that I could not understand. Just to show you to what extent I suffered, being used to having good grades in Math, my first grade with this teacher was 0.5 out of 20. It wasn't the worst grade, as some people had 0.25 and others zero. Actually, he was teaching some form of Linear Algebra, without it being supposed to be taught! So, I started studying Mathematics by myself. Fortunately, at the same time, I had a Physics teacher who convinced me that I was not that bad, and I was successful in Physics. Slowly, I also recovered in Mathematics.

Then, I decided to go on studying Science and went to the *Classes Préparatoires* where I realized that, because I had been thinking by myself, I could be among the bests in a class where some students already had received some prizes. From that on, I thought that maybe I could be able to do Mathematics as a profession. A year after, when preparing for the competition to enter the *Grandes Écoles*, I had another good mathematician as teacher, but his grading method was very peculiar. He would grade according to what he thought you could do. So, if he was expecting something great from you, then you could have terrible grades, and next to you there could be someone from whom he had no such expectation, who would end up having better grades than you. So, even though I was understanding better than others, I was lost, and it wasn't clear I would be able to do Mathematics.

I successfully passed the entrance competition to *École Polytechnique* where the Mathematics courses were very solid — Gustave Choquet was one of my teachers — but I realised others, e.g. at École Normale Supérieure, were learning much more Mathematics than me. At this time, the teachers I had in Mechanics were very disappointing and confusing and, with a small group of students, we organized some kind of *pirate courses* to replace teachers. So, I learned a lot, and read all possible books I could find on Mechanics: Arnold, Truesdell, Sedov, etc.

So, at the end of my two years as student at *École Polytechnique* and one year of *Diplôme d'Études Approfondies* in Mathematics studying *Sheaf Theory*, I decided to try and study Mechanics for a PhD.

I already had a clear idea of what I wanted to do: I wanted to solve the Euler equations of the motion of fluids using a technique introduced by Vladimir Arnold based on the search for geodesics of the group of diffeomorphisms. However, at the time, most of the teachers in Mechanics in Paris were quite senior people, and when I approached them, they all basically told me the same thing "no, you are not going to do what you want, you are going to do what we tell you to do". And actually, shortly afterwards, Claude Godbillon, with whom I had spoken about my project, just forwarded to me the just published work by David Ebin and Jerrold Marsden in which they solved the Euler equations using precisely the method I had in mind. So, I was again lost, and went back to the subject closest to Mechanics: Differential Geometry.

With this in mind, I approached Marcel Berger, and started to work on a PhD with him. Then, I could convince him to invite David Ebin to France to give a course, so that I could learn more systematically Global Analysis at a moment when it was not considered so important. Earlier, I had received from Choquet a good training in Analysis. Therefore, I could combine Analysis and Geometry, and started working on non-linear partial differential equations (PDEs) with a reasonably solid background. Accept my apologies for such a long story. As you have now read, my idea of doing research in Mathematics did not come so straightforwardly.

Do you have a favorite mathematician that has particularly inspired you?}

Besides Berger of course, who was very generous with his time to share his broad knowledge about Geometry with me, a person who had quite some influence on me was Shiing Shen Chern.

Very interestingly, in 1972, I was invited to Stony Brook by Jim Simons He had attended one of the lectures I gave in Berger's seminar in June 1972, in Paris. The next day, I had on my desk a fax from Stony Brook, offering me an Assistant Professor position there. At the time, I did not hold a PhD; I did not even bother to get a *Thèse de Troisième Cycle*, as I had already a position at the CNRS on the basis of a small article written while at the École polytechnique. After an intense discussion with my family we decided to take the chance and go.

I spent the year 1972–1973 in Stony Brook, which was then really the *Mecca* of Differential Geometry, with 14 mathematicians in this field in the Mathematics Department. Can you imagine? There was of course Simons himself, John Millson was a student there, James Ax, John Thorpe, Leonard Charlap, Jeff Cheeger, Detlef Gromoll, Wolfgang Meyer, Shing Tung Yau and several others. Actually this was a fantastic opportunity to become very close to Yau, who was enjoying his first position (at age 23) after having been a student of Chern. Being in Stony Brook was an unbelievable chance.

During the summer of 1973, I was invited by Robert Osserman to Stanford, and spent the whole summer there. While I was at Stanford, I got a phone call from Chern saying that he would like to have lunch with me, in Berkeley. At the time, I was 26, did not have a PhD, and here is Chern calling me to have lunch. I was just amazed! Actually, I was told later that he was doing that with a number of young people. Nevertheless, being called by Chern was something special. We had a very interesting discussion, he wanted to know what I was doing. By then, in France doing Differential Geometry was more or less proving that you were not a real mathematician. If you were one, you would be doing Algebraic Geometry or Number Theory, Differential Geometry was viewed by a number of people as a secondary subject considered technical. When I came back from the US, I thought that maybe what I was doing was not so silly. After all, Chern and a lot of other people were interested in it. In this way, and a few other ways later on, S.S. Chern had a lot of influence on my career.

Moreover, in September 1973 there was a Summer Institute of the American Mathematical Society (AMS) on Global Analysis, which actually was a turning point of the whole theory. At the time, working with Yau, we were trying to disprove the so-called Calabi conjecture, a major conjecture in Kähler Geometry, and we published a paper on it, showing that at least quotients of K3 surfaces dit not admit a metric with SU_2-holonomy. During that summer, Yau thought that he had disproved the conjecture.I attended the lecture he gave there to Calabi and Chern, but actually there was a gap. Finally, two years later he proved that the conjecture was *true*.

So, this visit to the US changed my perspective on my own work a lot, I was exposed to fantastic mathematicians, and I improved substantially my knowledge and practice of English.

Another important point is that I was blocked for defending my *Thèse d'État* because somebody had announced the result I was trying to prove, namely a stratification of the space of Riemannian metrics, in a Physics journal. And he never replied to any of my letters, neither to those of Berger, asking if he had proved it or not. Finally, in September 1973, he was also attending that conference and I could ask him directly: "Do you have a full proof?" and he said: "I am writing for physicists, so why should I have the full proof?" But he agreed to have Berger speak to him, and, some half a year later back in France, I could defend my *Thèse d'État*.

Among your many results and achievements, is there one that you are particularly proud of?

Well, there is one which I am proud of and with which goes a somewhat crazy story. In 1979 Blaine Lawson was in Paris, with Marie-Louise Michelsohn, his wife, visiting IHÉS, and I was meeting them regularly. At the time, the topic of Gauge Theory had become popular and much in demand from physicists. As I had studied physics quite seriously at École polytechnique, in particular quantum physics, I had an advantage over other mathematicians. I think I understood quite solidly what was behind Gauge theory and quantum effects. So, at some point, I was asked by physicists to give a course on the differential geometric background needed to develop Gauge theory. One day, before starting the course, I came to Lawson and showed him the outline of the course, and in passing, I mentioned to him what I knew about one of the conjectures that physicists were very much looking into, concerning the critical points of the Yang-Mills functional on the four sphere S^4. He looked at me and asked "What can you exactly do?" After I explained what I could prove, he said "I think I know how to do the missing half!". So, just by talking, we had the proof of a nice theorem. The heart of the matter is that I had understood how to use ideas of Jim Simons to go from 5 dimensions to 4 dimensions, but I was stuck at one point. In 5 dimensions the Yang-Mills functional is non-degenerate, but in 4 dimensions it is degenerate. I did not know how to get rid fully of the degeneracies, but Blaine did. We could very quickly publish an announcement in the Proceedings of the National Academy of Sciences. We decided to do it jointly with Jim Simons because we knew that he had just decided to quit mathematics. At this time he was not famous nor a rich person. The full article with Lawson was published in Communications in Mathematical Physics and it's one of my best papers.

There is another one which I like very much, but remains partly a mystery to me. It's about proving that a metric on a 4-dimensional manifold whose curvature is harmonic, is actually an Einstein metric, i.e. one for which the Ricci curvature is a constant multiple of the metric. The way to prove the result is, I think, very peculiar, because it uses the fact that, if you apply a certain identity called a Weitzenböck formula to the curvature tensor that you need to view as a harmonic vector-valued 2-form, it satisfies a generalized Laplace equation from which you can derive a peculiar pointwise algebraic commutativity property. From this property you can get information on the integrand of the signature of your 4-manifold Hence, under the topological condition that the signature is non-zero, harmonicity of the curvature — a third order condition on the metric — implies that the metric is Einstein, which is a second order condition.

I like this theorem very much because it brings together non-trivial facts about PDEs and Topology, but still the reason why it works remains mysterious to me.

Scientifically speaking, do you have any particular unfulfilled goal that you still would like to accomplish?

Oh, many. Well, the first one is the first problem suggested to me by Berger, I worked on it several times in my career: namely to decide whether $S^2 \times S^2$ admits metrics of strictly positive sectional curvature. This is still an unsolved problem. One can ask the same question for products of spheres in all dimensions. My guess is that the situation for $S^2 \times S^2$ may not be the same as the one for $S^3 \times S^3$. I tried many things and many people tried also, since it is a question which can be formulated in easy terms. My first publication actually was to show that, in fact, there is no such metric in the vicinity of the standard product metric of $S^2 \times S^2$. It's far from the final solution of the problem, but at least it shows that the problem is non-trivial.

It is widely acknowledged that Physics has had a long tradition of providing important challenges for mathematics research in particular for geometry, such as General relativity and Quantum Mechanics.

What physical theory do you think will have an analogous impact and provide the next big challenge for geometric research in this century? String Theory? Supersymmetry?

It's a complicated question. This influence has already happened to an extent people would have never believed. String Theory (ST) had an impact in particular towards Algebraic Geometry. For example, Kontsevich has a totally new way of thinking about Geometry using categories of higher order, which is certainly inspired by the challenges posed by ST. One theory which I personally spent quite some time on is supergravity (SG). Of course, it is not clear whether physicists are so interested in this theory anymore, but what I find really interesting is the way it combines classical DG with the study of connections with torsion. In SG there is, besides the usual structure, a 3-form. The geometry of such objects has been investigated recently by Nigel Hitchin and some people around him. And I think there is more to be said, in particular in connection with geometries with special holonomy (G_2 in 7 dimensions and $Spin_7$ in 8 dimensions).

Another area in which I was involved is the fantastic progress in the theory of systems of non-linear PDEs which came from the study of the Einstein equations. Actually, I taught General Relativity for 15 years at École polytechnique. Since the work of Demetrios Christodoulou and Sergiu Klainerman, as well as others who followed, we now have an understanding of the kind of regularity which is needed to guarantee the existence of solutions to the Einstein equations. I think this is a domain in which fantastic progress has happened thanks to both geometric ideas and sophisticated physics and mathematics. For me it is one of the most amazing achievements of the last 20 years.

This is a speculation now: do you think sometime soon Quantum Field Theory (QFT) will be placed in a rigorous mathematical basis?

There are some versions of QFT which are rigorous, but these are not the ones that physicists find the

most relevant. We always face the dilemma: on the one hand one can make the theory rigorous; on the other hand, one is not touching what the physicists consider to be the heart of the matter. Probably, we are missing some new mathematical concepts and background, and I wouldn't be surprised if one has to look at it from a very different perspective. In some recent approaches by people like Kontsevich using a new geometry involving higher categorical structures, the level of abstraction and the sophistication of the algebraic machinery seems to completely kill the geometry behind it. Not for him of course.

Another question that is talked a lot about is Alain Connes' programme of non-commutative geometry (NCG). It's a point of view providing very interesting approaches to theoretical physics. His belief, and there is evidence to support it, is that the Standard Model (SM) of particle physics has an internal structure which is much more meaningful than usually assumed. For many physicists, the SM is something where various pieces fit in a quite *ad hoc* way as the values of some coefficients in the SM were obtained through measurements. But for Connes, using NCG, these constants are really built into it for geometric reasons. So far, physicists are looking at this with some kind of a smile, as experiments should tell you which values are correct. As you know, the mass of the Higgs boson is not the one supersymmetry was predicting, and at some point, Connes thought that the LHC [Large Hadron Collider, CERN, Geneva] had proved one of his predictions to be wrong. But now, his latest conclusion is that he had made a mistake in one of his estimates and now, after correcting it, he gets a value for the mass of the Higgs particle compatible with experiments.

I think Connes'geometric approach is extremely interesting, in particular because it allows to put on an equal footing discrete and continuous spaces. This plays an important role in physical theories, which may have to deal with discrete or continuous objects, but also in Number Theory.

Throughout your career you have assumed several high profile administration positions, such as president of the Société Mathématique de France, president of the European Mathematical Society, director of the Institut des Hautes Études Scientifiques and president of the European Research Council. Portugal has been going through a severe financial and social crisis, which meant that only very few positions for mathematicians have been opened in the past years. Nonetheless, the PhD programmes in Mathematics have grown and have become quite successful. Given your experience, do you have any advice for these young researchers who have just finished (or are about to finish) their PhD, in terms of career opportunities?

Well, this is a big question. Maybe I should remind you that I was among the people who reviewed Portuguese Mathematics during the nineties. This was an extremely interesting exercise. At that time, and it has nothing to do with the quality of people, a number of researchers there were really looking at narrow and sometimes bizarre problems. And so, since the landscape was dominated by senior people doing at times somewhat routine research, these evaluations brought up a broader perspective that some younger people were able to take up when there were not even proposing them spontaneouly themselves.

So, for young people in order to be ambitious and develop research at the highest level, to have a clear idea of what their career path can be is critical, I even mentioned it in my speech this morning [Ciência 2016 — Encontro com a Ciência e Tecnologia em Portugal, 4–6 Julho, Lisboa]. It is fundamental that policy makers understand that, to have leading researchers, at some point one has to offer them a decent career perspective. This is exactly what happened in France in the early 1990s and this led to the generation of Jean-Christophe Yoccoz and Pierre-Louis Lions.

But still, there is one point that I would like to make here, namely that the possibilities for mathematicians to be employed are much broader now than they used to be, for several reasons. First of all, the interfaces of Mathematics with a number of other disciplines developed fantastically in the last thirty years: There are new interfaces with Biology and Medicine, for example, touching many areas of Mathematics, not just Statistics; but also with Social Sciences or Humanities there are many possibilities of involving mathematicians. I think it is quite important for the next generation of mathematicians to be exposed to several fields. Of course, in the end, people do what they feel is interesting. But, at some point, teachers must understand that you can become a mathematician in many more ways than one used to. You have to let students choose what is most appealing for them, but it would be a mistake to say that to do Mathematics you have to do Algebra, Geometry, Analysis, and so on. It's very important to expose students to various possibilities.

I'm not sure you know the figures, but for France, today one Mathematics Ph.D. out of two takes a job outside academia. In the early 1990s around 90% would stay in academia. So, this has broadened the perspective for students in Mathematics. There are people working in many different environments. Also many companies now want to have mathematicians as members of their teams. I often give the example of Veolia, a company doing transportation, garbage collection and many kinds of things, which employs many engineers. Talking to the head of research 4 or 5 years ago, he told me that, at that moment, 8% of the engineers had a strong mathematics background, and the objective was, by 2025, that 20\% of all engineers should have a broad mathematics background. This means that the number of people with very sophisticated mathematics knowledge employed in companies will grow considerably in the years to come.

Another point that I would like to make, is that three European countries have now studied what is the impact of advanced Mathematics in their economies: The UK, Netherlands and France. The conclusion was that the impact of advanced Mathematics was much bigger than people ever thought. In the case of France, the figure is 15% of all GDP is directly related to advanced Mathematics. And the number of jobs induced by this use is above 2 millions. The report can be found on the website of the Société Mathématique de France. It shows that mathematicians have been, in some sense, collectively underestimating their impact on Society, and that there are now many more ways of being a professional mathematician than before. But of course, it depends a little on how each country is dealing with this issue.

In Portugal, there are few purely research permanent positions, in contrast to the French CNRS. What do you think about this? Would you have any advice for the Portuguese government with respect to this issue?

I was an employee of CNRS for 44 years, and it is clear to me that I owe my career to this organisation. But when one considers the overall organization of the academic personnel involved in Mathematics in France, one finds that 85% are holding positions at higher education institutions and that only 15% are employed by the CNRS. Of course, given the size of France, the number of mathematicians employed by the CNRS exceeds 400. In a number of cases, the researchers from CNRS still teach somewhat, but of course less than if they were holding a regular teaching position.

So, the right thing to do is to make sure that, in a given country, there are enough positions to give a relief from teaching to a significant number of people. It should be possible, for example, that for 5 years, someone takes a relief from teaching in order to pursue research more intensely.

Actually, in France, one thing that was organized with this in mind was the *Institut Universitaire de France* (IUF). A national selection done both at the junior and at the senior levels, allows people to be relieved of one third of their teaching duties and get some extra support to do research in their own home institutions. Being a member of the IUF is seen as a very distinguished position with a positive impact on both the recipient researcher and his or her university.

This structure works quite well. Hence, I think this is another way of funding research personnel which is less expensive than having permanent research positions. It also helped to recognize that, for some people, the teaching load was too heavy to achieve excellence in research.

You are definitely a person who travelled the world. How do you see Portugal in terms of its scientific development?

Since I have been president of ERC, I lost a little bit contact with what different countries have been doing from a strict mathematical point of view. But since the middle of the nineties, I would say the transformation has been quite positive. Nowadays, many more people are exposed to international competition and all in all Portuguese mathematicians have been very successful, in particular young ones.

I understand that the recent years have been tough, as I heard from several Portuguese colleagues. But I think that taking a longer perspective, Portugal has really gone through a long transition. Actually, I think n the first years the efforts on the side of the Portuguese government were really important, with a significant increase in the number of funded research projects. I would like to stress that that these projects were evaluated by international panels. This was a smart move particularly in a small country like Portugal, where most people know each other very well, maybe too well. So, globally, I would say that the evolution has been very positive. I am not saying this to be nice. You may have noticed that I tend to be blunt.

Here, I must mention the very positive, in my opinion, influence José Mariano Gago had in this respect. We became friends and we exchanged on a regular basis on European issues. With Philippe Busquin, he played a critical role in the establishment of the ERC. He left us much too early.

As mentioned before, you have been the president of the European Mathematical Society (EMS). How do you see the importance for Europe to have a Mathematical Society?

It took a long time for the EMS to develop. Actually, you may not be aware since you are too young to have witnessed how slow the process was. Part of the problem was a remake of the traditional disagreement between the British and the French about the level of integration of the European process. Fortunately, there were the Germans to bring us together. I am serious about that. Two models were indeed competing: a British one where the EMS should be a society of societies with no individual members, and another one, supported by the French, according to which the EMS should be a much more integrated structure with individual members. The compromise was to have both, which is actually the current situation in the EMS, showing the compromise found was a good one.

I remember, in particular, the controversial foundational meeting in 1990 in Madralin. It was a not so gentle fight. Fortunately, the person who had been chosen to become the first president of the EMS, Friedrich Hirzebruch, imposed a mixed view which was accepted by Sir Michael Atiyah, who had been chairing the European Mathematical Council, which in some sense has been the matrix for the EMS later on. The key decisive step taken by Friedrich Hirzebruch was to ask Sir Michael Atiyah whether he would agree to become the member number one of the EMS, which of course would mean that he was accepting the compromise. He agreed.

But why should there be a European Mathematical Society? There are actually several obvious reasons. At the time, the European Commission was developing its framework programmes and mathematicians were unable to be present enough in this process. The only way was by having a lobbying power with a European flag in Brussels. So, the EMS played a role there and was able to force the presence of some meaningful programmes for mathematicians in the agenda.

Another important reason for me has been the need to enhance the development of the bibliographic databasis Zentralblatt Math (ZM) to avoid the monopoly of MathSciNet, property and one of the main providers of resources of the AMS. Attempts to get the two databases to cooperate had failed. It was of paramount importance that the European mathematical community could get organized to stand behind ZM and press for its modernization and presence worldwide. The EMS soon was a dynamic partner of the FachInformationZentrum Karlsruhe, the Heidelberg Akademie der Wissenschaft and Springer in ZM.

After this complicated start, I was completely surprised when Friedrich Hirzebruch invited me to be his successor. I could hardly believe that. But my relation with Hirzebruch was one of great respect. I greatly admired his efficiency and appreciated very much his efforts, for example, to develop the Max Planck Institute for Mathematics. I truly believed in the importance of the EMS and accepted the challenge.

It turned out to be a fantastic experience. I was very lucky with the excellent team who worked directly with me. We continued the work initiated during the previous presidence. I remember vividly for example the creation of an active website and the first years of the Journal of the European Mathematical Society. Other important achievements were attained in direction of applied mathematicians, as we managed to create contacts and start some studies between Mathematics and Industry.

In the past few years, the Mathematical and the general scientific community have been overwhelmed with the use of bibliometric data to assess and evaluate individuals and institutions. This has been happening in job and grant applications, individual evaluations at reputable universities, research institutions' evaluations by funding agencies, and so on. On the other hand, we have the San Francisco Declaration on Research Assessment signed by many important scientists and scientific organizations. Do you have a personal opinion on this matter that you would like to share with us?

This has become an important issue. I am fighting the use of bibliometrics to evaluate people in a very explicit way. With ERC panel members, I have been insisting that they do not to use that. Of course the temptation to use this information varies much from one discipline to the other. Disciplines where this is more or less routine are Biology and Biomedical Sciences but for Physics and Mathematics, for example, I have not seen any of the panels making real use of this.

Of course figures at certain levels can be useful to obtain a global picture. For example, at the ERC we sometimes use the 10% or 1% more cited papers figures globally for Europe or at the level of nations. But the idea of evaluating and funding individuals or teams based on bibliometrics is inappropriate, and there are several arguments against it. The first argument is that people have different publication and citation habits across different disciplines and subjects. Even within Mathematics, for example, the geometers do not quote and cite in the same way as analysts do. People doing applications have even more different patterns. A second argument is the fact that most of these data are using citations from the last three years, when the average age of citation of a mathematical paper is between eight and nine years, so using this type of citation information does not actually make any sense. Of course this varies a lot with disciplines because in some other fields a paper with more than three years of age has basically no value for quotation. This is of course not the case for Mathematics.

The other argument why I insist not to use this at the ERC is the following: the objective of the ERC is to fund ambitious projects with bright new ideas, and looking at passed data does not give much of a clue about the value of the project. Hence, I have been very explicit about this and, although I experience some resistance from biologists, the position of the ERC Scientific Council on this is very clear. We highlight the necessity of evaluating the potential of a good idea as the most important thing.

This does not mean that bibliometric data have no value. It just means that they have no place in the evaluation of individuals and can be used for the evaluation of research teams when properly aggregated at a large enough scale.

Research in Mathematics has a dual mode: fundamental research and applied research. Often they are closely connected and one stimulates the other. However, in certain fields or subjects, applications occur (if they occur) only after a very long time gap. In a society eager for technological advances, the pressure for financing almost exclusively applied research is overwhelming. Do you have any advice for people working in fundamental research on how they should proceed to have access to funding?

There are several sides to your question. First of all, at the level of the ERC we insist that we are dealing with frontier research. We do not want to discriminate between fundamental or basic or pure and applied or technological research. The truth is that, if you look at the ERC portfolio (and this was not decided a priori), 85% is pure or basic research and 15% is applied or technological research. But this can change over time.

The second comment is that people who decide policies are very often under pressure by politicians. For politicians the key issue is to have short term results. The reason is that the next election is tomorrow. In some countries like China, they do not care so much about short term results because the government has longer periods, like 20, 30 years, in mind. Therefore they initiate programmes like the new 5 year plan with a considerable focus on fundamental research because they want to build a community able and eager to develop new technologies in the future. We, as scientists and this is especially true for mathematicians, have to teach politicians how research really works. Research does not work as well when you tell people what to do. Actually, this should not be called research, this is development. When you do research, it is very difficult to anticipate what is going to come out

since you are dealing with the unknown. This does not mean you should not make specific efforts in some particular areas. The best response we can give to politicians is that they should adopt a balanced strategy. Clearly there can be top down priorities on topics like energy, climate change, etc. But at the same time, there should be a very significant percentage of research left at the initiatives of researchers using a bottom up approach. Then you have to make the case for numerous initiatives of researchers which turn out to be relevant for politicians. One such example is the recent use of perovskite minerals to build batteries which are much cheaper and have a very promising efficiency output when compared with other batteries. It came from a totally bottom up approach. I made this point to the Vice-President of the European Commission in charge of the energy portfolio, Maroš Šefšovič. The people who came up with this technology were not told to do that. This discovery just came from their own team dynamics.

Moreover, there are more short circuits coming from research projects not led by any a priori request but which can suddenly become a big story. The example I like to quote is the case of *CRISPR Cas9*, a new gene editing technique, which is actually used by bacteria for millions of years and was studied, in the 1980's, by Japanese researchers, who could not really understand the process at the time. Then, recently, the work was picked up by Jennifer Doudna and Emmanuelle Charpentier who paved the way for the discovery of this very promising new gene editing technique. In fact, to give an example of the impact of this breakthrough, at the ERC, last year, we had less than fifty projects using this technique and, this year, we have several hundreds. So, this is something that was spotted at a given time but not understood. Then, much later, someone managed to understand it so well that it became a new wide spread technique with a very promising and challenging future. This is a fantastic example of something which is most likely to have an enormous impact in several areas both from the economical and from the health point of view. And the key point is that all this happened just because people wanted to understand better something that looked mysterious. This is the perfect example to show that one cannot only rely on top down strategies but that bottom up is badly needed. So the key is to look for the right balance between the two.

Given the fact that research in Mathematics is most of the time less expensive when compared to other types of research, demanding intensive lab work, what do you think about the idea of reducing the huge amount of money for a single ERC grant and make them available to a larger number of people?

First of all, if you allow me, I am always surprised with this question because ERC allows people to ask for the amount of money they find appropriate to achieve their project. Recently a 200 000 Euro grant has been given for five years, which I think is not such a big grant. At no moment does the ERC press people to ask for large amounts of money. Most of the time it is the institution that presses the researcher to ask for more money, probably because of the 25% overhead it receives for each grant. So it really depends on the the people applying for the grants. It is true that the researcher can use this money to pay typically half of his or her salary, in line with the time dedicated to the project. If the institution is fair, then it should use that money to improve the support around the grantee, so that more people benefit from it. That is for example what the CNRS in France is doing: if the researcher decides to take half of his or her salary from the grant, then a large part of that money is distributed around him or her. I personally think it is a good way of lifting the spirit of all the people around the grantee.

The difficulty with giving very small grants is the fact that the administrative burden needed for putting in place a two million Euro grant is almost the same as the one for a 200.000 Euro. So, of course, by multiplying the number of grants by ten, for the agency, it would mean a huge increase in administrative costs. At the moment, the Executive Agency in charge of the ERC is managing about 5.000 grants, and there are only 90 people to do that. Another issue here is also to determine the European added value of distributing small grants. The national or even local levels are almost surely the right one to do that. This points to the fact that the support to research has to be thought in systemic terms: different means for distributing support should be in place and enough money be given in a recurrent way with decisions taken as close as possible to the researchers. I do not see any reason why the support to research has to be given only through projects. In order to develop completely new ideas, researchers need to be able to do it in a spontaneous and totally non bureaucratic way. Unfortunately, in a number of countries the balance has gone too much in the direction of supporting competitive projects. Not enough money has been left for recurrent support. This is, for me, a major mistake with, potentially, a very negative long term impact.

Concerning the ERC, another thing you need to keep in mind is that, in the end, the people who determine the amount of money granted are the panel members.

In fact, at the ERC, there is something sometimes referred to as the "Bourguignon policy", which goes back to the time I was chairing the first panel distributing starting grants in Mathematics, because I insisted that the budget should be checked thoroughly. Already then, people tended to ask for an amount of money which was not related with the real needs of the project. So the mathematics panel I was in charge of did cut the budgets of some projects, because we felt the request made was not based on actual scientific needs. To conclude people should ask the amount of money they really need for thesatisfactory development of their project.