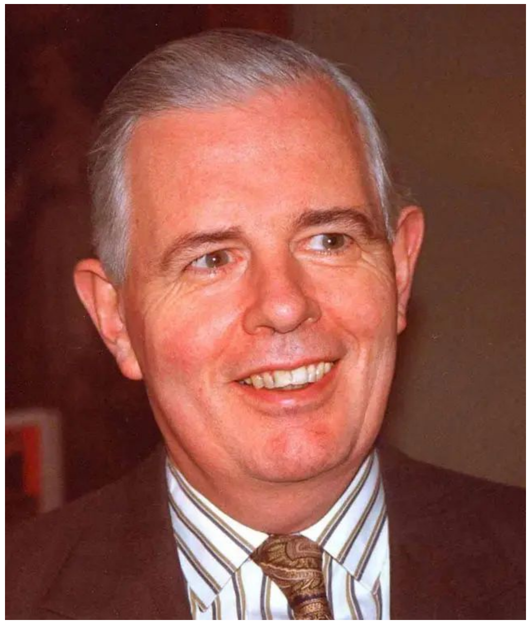
Interview with Sir John Kingman

Ana Isabel Mendes and Martin Raussen

Sir John Kingman, Fellow of the Royal Society, was the 4th president of the European Mathematical Society during the years 2003–2006. From his career as a professor of probability theory and statistics to holding top positions as a science administrator and leader, his professional journey continued until his retirement in 2006. Now, at age 86, he does not do contemporary mathematics any longer, but he still likes to think about and solve non-trivial puzzles of a mathematical flavour.

In January 2025, Ana Mendes and Martin Raussen had the opportunity to ask Sir John about his life within and around mathematics. Among many other things, we learned that mathematics is a fuzzy and messy subject, nevertheless connected, and quite often very useful!



Sir John Kingman.

A mathematical education

Ana Isabel Mendes/Martin Raussen: *To start this conversation, we would like to ask how you became a mathematician and a statistician. Can you please tell us what and perhaps who stimulated your interest in the first place?*

Sir John Kingman: Well, I went to an ordinary state grammar school in the north of London. In general, the teaching was good, and I enjoyed most of the subjects. I didn't enjoy subjects like art or woodwork or experimental science where I needed to use my hands because I wasn't very good at that. I famously dropped the apparatus that made hydrogen sulphide, which made me rather unpopular. But I found that I could do mathematics, and I enjoyed doing mathematics. As time went on, I found I enjoyed it even more. And of course you know, when you can do something, you are never bored.

So, I quite naturally thought, I should try and study mathematics at university. I was fortunate to get into Cambridge. Well, in Cambridge you choose a college, and my college did not have much in the way of mathematics. The person in charge was a very old-fashioned applied mathematician. He wouldn't have us use vectors or anything modern like that. Everything had to be written out. I can still write out Stokes's theorem in coordinates. And of course, xyz-coordinates, not $x_1x_2x_3$. But I was very fortunate, because in my second year I was taught by a young mathematician who was still making his way, called M. F. Atiyah. That, as you can imagine, was quite an experience. I learned a great deal from Michael in that year.

But for reasons I'll explain if you'd like, I developed an interest in probability. In Cambridge, probability was neither pure mathematics nor applied mathematics. And there was a strict division between pure and applied, so it fell through the middle. But I was again fortunate because they fixed it up, so I could have, I think, four one-hour sessions with Dennis Lindley. He was one of the most brilliant statistical teachers that I have ever come across, and that was also a formative experience.

[AM/MR]: *Studying in the late 1950s must have been very different from what it was later on and how it is nowadays. Can you tell us a little more?*

JK: It was certainly different. Thinking about Cambridge, it was very old-fashioned. The way it was organised made it very difficult to bring in any new ideas. For instance, they never talked about probability theory. We always talked about random variables. One day I asked: *"What is a random variable?" "It's something that varies randomly"*. Oh, but what is it? And there was no answer.

It was also very strictly divided between pure maths and applied maths, and you had exactly half of one and half of the other. You went to lectures, some of the lectures were good and some were awful. Some of the lecturers were brilliant mathematicians, world-class, and some were completely unknown. And the correlation between those two classifications was not great: I'm not saying that the great mathematicians were the great lecturers or vice versa. In fact, the two of the most distinguished mathematicians, I will not give you their names, but you would certainly know them, they were dreadful lecturers. And there were other people who had not done any mathematics since their PhD, but who were very good at putting it across.

And then you had supervision. Normally, two students were meeting a senior person. During my first year, we had this old-fashioned applied mathematician, so we did very old-fashioned applied mathematics. But then in my second year, Atiyah took up supervision, and wow, I used to come out just wiping my brow. It was a quick fire! I remember one day I asked him a question about compound matrices. And I got 20 minutes of exterior algebra, which I of course never had met before. It was a wonderful experience, and I was very lucky to have it.

In the third year, you could choose what you wanted to do. And you just went to the lectures you felt like going to, and you could mix and match. So, I went to lectures on probability and statistics, and I went to lectures on genetics, which later had an effect. But I also went to lectures about fluid dynamics, differential geometry and general relativity, all sorts of things. And that was rather good because the lecturers were typically talking about what really enthused them. But some of them were not enthusiastic at all.

Perhaps the highlight of that third year was going to lectures on quantum theory by Dirac. He would, of course, talk about the things that he had invented himself. They were rather boring lectures; he read sections from his book on the subject. But one Saturday morning he decided to tell us how he discovered the spin on the electron. By pure mathematics, essentially by non-commutative algebra. And because it was non-commutative, something had to be spinning, and there was only one electron; the electron had a spin of $\frac{1}{2}$. That was rather remarkable, because what Dirac was saying was that if the creator had not given the electron a spin of $\frac{1}{2}$, he wouldn't have been doing as good a job as Dirac would have done.

[AM/MR]: *You must have studied very intensively. How was social life as a student?*

JK: I was what we call a swot or a nerd these days. That is to say, I spent my time reading and doing all the exam papers, and so on. I probably wasted many of the opportunities of social life.

I had quite a lot of friends. I went to concerts and there was a lot of music, things like that. But I spent an awful lot of time doing mathematics. I slightly regret that there was not so much else, but I was that sort of person.

[AM/MR]: *How did you then get into probability and into mathematical research?*

JK: The reason why I became interested in probability came from a quite different source. We had these long summer vacations, and I thought I'd better try and earn some money because we weren't very well off. I signed up with the post office engineering research station which was nearby.

A friend of my father worked there. I didn't realise that he was actually a very great man. He was Tommy Flowers who had been at Bletchley during the war. He was the person who built the Colossus computer, which was really the first electronic computer. He was a very quiet chap. It is famous that at Bletchley, the mathematicians and the theorists imagined what one could do with a machine that could do this and that, but no one had ever built such a machine. But Tommy Flowers, who was a telephone engineer said, *oh yes, we do this when we do telephone exchanges. I can do it*, and he did!

Anyway, he arranged for me to have this job, and I was given work which involved learning a bit about queueing theory. Queuing theory, as you know, originated in the Copenhagen telephone company with the work of Erlang, and it was very much a part of the telephone engineering world. I found it very interesting, and I wanted to try and do research in that direction.

Working as a university mathematician

[AM/MR]: *You obtained your first academic appointment, I believe, as a reader and then as a professor at the newly founded University of Sussex in the South of England. How was that experience in contrast with the traditional universities?*

JK: In fact, that wasn't my first job. I started doing research under the applied probabilist Peter Whittle. He was marvellous, but after a year, he said: *You had better go and work under David Kendall*. Kendall was the person who introduced the Kolmogorov type of probability to the UK. We had quite a tradition of statistics in England with people like Bartlett, Daniels, Lindley, and so on. But it was very much applied probability, and they really hadn't heard about the mathematical basis established by Kolmogorov before Kendall came along.

In my second year at work, I went to Oxford where Kendall was working, and I spent a year there. During that year, Kendall was appointed to a professorship in Cambridge. Cambridge had never had a professor of statistics, and they decided that they wanted one that was mathematical. Hence, they went for the most mathematical person they could find in the subject, and that was Kendall. One day when I went to see him in Oxford, he said: "*I'm going to Cambridge*." And he said that he needed an assistant lecturer to do the work. I didn't apply, I was just appointed, that happened in those days, and I went back with Kendall to Cambridge. Indeed, I had to do the work! I had to give 1st year lectures and so on, which was quite an education!

But then I was offered a job at Sussex. Well, that was an attractive offer because the lady I was courting had also been offered, quite independently, a job at Sussex. We both accepted these offers and settled into married life in Brighton.

Sussex, as you were saying, was a new university. The difference was that in Cambridge, you do what you did last year, unless you fight very hard for a change. But in Sussex everything was new, so everything was being thought through from first principles. There wasn't any last year. You must decide yourself. They tried all sorts of things. Some of them worked and some of them didn't. I had quite an interesting time because I was the first statistician they appointed. One of the things they decided was that all the students of social science, economics and sociology, and so on, had to pass a compulsory course in statistics. And I was told that I was to teach this course. That was very different from teaching Cambridge mathematical undergraduates. One of the students had found the course very difficult because she had been away from school when they were taught decimals. She couldn't do decimals, and that made statistics a little bit tricky!

[AM/MR]: *In 1969, you moved from the University of Sussex to Oxford University, which at that time hardly had highbrow expertise in statistics. How could you and statistics develop and thrive in Oxford at the time?*

JK: Oxford University did have a thing called the Institute of Economics and Statistics. It hosted a lot of economists, and John Hammersley, who was a mathematician who had managed to get a job there, so he didn't have to teach any undergraduates and just could get on with doing mathematics. There was also a department of biomathematics which was by then headed by Maurice Bartlett, a renowned statistician. He was developing it as a sort of statistics department with a biological flavour. But there was no statistics in the mathematics faculty, and they did not want to introduce statistics because it was being done by Maurice Bartlett's department.

But they thought they might need some probability because they had lost David Kendall. Well, Kendall never had a university job, he had a college job at Oxford. Anyway, they set up a chair in probability and I didn't know that. I hadn't seen an advertisement or anything, and I just got a letter from the Vice Chancellor saying we have elected you to the professorship. And they just assumed that I would immediately pack up and go to Oxford, which I did! I had happy years there trying to introduce probability and then some statistics, and I tried to bring together the different statisticians in the university. Maurice Bartlett was very helpful, and a very good colleague, and we did in fact make a certain amount of progress. Later, they set up a proper department of statistics, but that was after my time.

Heavy traffic and the coalescent

[AM/MR]: *I hope it's correct to say that your scientific work centred around random processes, random walks, queuing theory, and then also mathematical genetics. You are, among many other things, particularly known, at least to Wikipedia, for Kingman's formula in queuing theory published when you were only 21 years old. Moreover, for the Kingman coalescent in mathematical genetics from the early 1980. Can you please explain what these are all about?*

JK: I find it one of the ironies with mathematics that people are not necessarily known for their best achievements. The things in mathematics which I am proud to have done are not the ones you mention; neither of them contains particularly deep mathematics. The heavy traffic came out of my work in the post office. Actually, for the telephone engineers we were talking about, 'queues in heavy traffic' was a telephone engineering term. They were interested because, if you have too many people coming into a queue, it becomes unstable, and the queue gets longer and longer. But when you're just below that difficult point, the actual assumptions that you make are not very relevant. There are robust approximations which are very easy to handle.

I don't think I invented that formula. I think it was pretty well-known to people in the telephone engineering world. What I did was to popularise it among the mathematicians, the applied probabilists who were working on traffic theory. They had not been interested in approximation. If you look at the classical work, including Kendall's two papers in 1951 and 1952, they were all about exact solutions under very strong assumptions. But the people in the practical world were interested in approximate results under very loose assumptions. Of course, things don't come in as Poisson processes, there are all sorts of irregularities which are difficult to describe, so you want robust results.

And then I published a theorem that you could prove. But I don't think it was very new. I think it encouraged people to look at robust approximations, and from the point of view of the operational research people, who were using queuing theory, that's exactly what they wanted. They didn't want very long formulae with lots of Laplace transforms and things in it. They wanted some nice simple things that gave them some insight into what was actually going on. So much about the heavy traffic approximation, the work that I have done as an undergraduate.

The coalescent was quite different. I have written about the way the coalescent came about.[1] As I already mentioned, I had been to this course of lectures on genetics in my third year in Cambridge. The lecturer had inherited the notes from R. A. Fisher, the great statistician, who had given that course for many years. When he retired, he handed his lecture notes, which he had used year after year, to this other lecturer who just leafed through and read us bits of Fisher's work. Well, one of them was a rather nice inequality that Fisher had conjectured. He had proved it for a locus with two alleles and there was increasing interest in situations with more than two alleles. Could you extend the result to a general case? And people had proved that you could. But the proofs were very long and involved, and it was clear that we were going to be asked to reproduce the proof in the exam. The lecturer dropped a heavy hint that one of the questions would be proving this inequality. This is why I thought I had better find a proof that I could remember, which I was able to do.

But that brought me into contact with the people working in mathematical genetics, particularly in Australia. In 1963, I visited Australia and met Moran, Ewens and Watterson, and we had lots of fruitful discussions. We went on over the years corresponding about the problem. Genetics was developing in a very interesting way. In classical genetics, you talk about genes, but you don't know what they are. They were simply abstract entities that could exist in different forms, and their patterns of inheritance could be studied. But of course, with the discoveries by Crick and Watson, people knew what genes actually were, these strings and double spirals and things like that. That raised quite new mathematical problems that people were grappling with. But I think there is a connection to what I felt when I was looking at queuing theory. What was needed was approximation and robust results. We didn't want to have to make very strong assumptions; they are not realistic!

I had produced some elaborate formulae that were pretty useless. But then I realised that, if you work *backwards* in time, then it can actually get much simpler. Particularly if there is not a strong selection operating, as is often the case at molecular level. You just look back.

Suppose you and I trace back our ancestry in the maternal line. We start with our mothers and then their mothers and so on. Maybe we find that once there was somewhere up there a Viking maiden who is an ancestor to both of us, Martin. Even for all three of us, we could still do this. You'd have to go further back. If you think about it mathematically, you've got the coalescent. It's not a deep thing at all, a straightforward Markov chain. The important thing is it just happened to be the tool that the people in genomics wanted. That's marvellous! I am very pleased that this simple idea has proved to be very useful. But it's not hard maths.

[1] J. F. C. Kingman, Origins of the coalescent: 1974–1982. *Genetics* **156**, 1461–1463 (2000)

Difficult mathematical results

[AM/MR]: *That allows us to ask you what piece of mathematical work you actually are most proud of.*

JK: I am afraid it is very old-fashioned. When I worked with Kendall, I got very interested in Markov processes in continuous time with only countably many states. That was very fashionable at the time. There is a marvellous book by my friend Kai Lai Chung, all about it, and there were all sorts of difficult problems. Kolmogorov had asked David Kendall, they met at a conference, if he could characterise the functions of time that can arise as transition probabilities. They are continuously differentiable, they satisfy various inequalities and so on. He said, could you actually characterise them? Kendall then, rather cruelly, passed the problem on to me. I was able to solve it, but that took me a decade, and that I am very proud of.

After I ceased to be a full-time mathematician, I went on doing some research rather as a hobby. I stayed in that field and over the years I have been able, from time to time, to prove some theorems which I like. But they are of very little interest because the subject has moved on. Young people now have different interests, and that is fair enough, that is as it should be. But it still gives me great pleasure that I was able to crack some, I think, quite difficult problems. But they will not get into Wikipedia.

[AM/MR]: *But they are published in the research literature, obviously.*

Applications

[AM/MR]: *Are you aware of other applications of your work outside mathematics?*

JK: Well, some of the results in queuing theory have been of interest in Operational Research and I think the coalescent has some other applications. I'm slowing down because I know that some of the applications are in the secret world, in cryptography and so on. There is for instance an interesting connection with large prime divisors of large numbers. That is apparently of interest to cryptographers, and that all links with the coalescent and a distribution which I have found and named after Poisson and Dirichlet, now called $PD(\theta)$. I know that $PD(\theta)$ is one of the things people talk about over coffee in those secret places. But I am not in that world, so I don't really know. I do not think that there are many more applications of the work I have done. But who knows?

I did get involved during the Covid epidemic because I got very cross. I don't know whether it was like that where you are, but certainly in the UK, there was an awful lot of talk about the *R*-number. You know the *R*-number in models of an epidemic? It is essentially the average number of people you give the disease to. And if it is greater than 1, the epidemic will grow, and if it is less, it will die out. The government actually published these numbers every week, they published *R*-numbers for the United Kingdom and other local *R*-numbers for regions of the UK.

That was bad science, for a number of reasons, of which the strongest was that it ignored the heterogeneity of the epidemic. Even within a single country, the disease affected different regions, different age groups, different social classes, very differently. This means that R is really a matrix, with rows and columns corresponding to different subgroups of the population. The critical parameter is the largest eigenvalue of this matrix. Unfortunately, few politicians are familiar with the Perron–Frobenius theorem, and they want simpler tools. The local R-numbers are essentially row sums, which do not determine the spectral radius. This is, I think, now understood by many epidemiologists, and I hope that, when we next have a pandemic, we will not hear much about the R-number. Much more useful is the exponential rate of increase of the epidemic.

Mathematics is a mess, but this mess is connected!

[AM/MR]: *Can we ask you about the tensions between applied mathematicians and statisticians and pure mathematicians, from a historical perspective, but also how do you perceive the development nowadays?*

JK: You ask me about the unity of mathematics? I think that this is not a very useful concept because it suggests that mathematics is a nice neat compact discipline. But mathematics is really a great sprawling organism, that finds its way into all sorts of corner of life. It's a sort of fractal, since if you look at a bit of it in more detail it is still messy. No one can define mathematics. Maths is what mathematicians do, and mathematicians are people who do maths. But if you regard maths as a complicated topological space, the important thing is that it is a *connected* space.

Alright, there are pure mathematicians and applied mathematicians, and there are statisticians and there are computer scientists and so on. But when you have a practical problem which looks as though mathematics might help, someone will try and do it, and then they will realise that they do not really have the mathematics to do it. They go and talk to someone who knows a bit more mathematics and says: Yes, I know I could do some calculations on that. And then this person realises that she or he does not quite understand some of the bits and pieces and asks someone else, and this someone else is not really interested in the application but finds the mathematics interesting. And then someone else comes along and constructs some vast abstract theory, and a new branch of mathematics suddenly starts to appear. And then someone spots a link with other applications, and we realise that some of the problems have already been solved.

That is why I strongly disagree with universities that divide between pure and applied mathematics or send the statisticians off into a corner somewhere. When I was at Stanford in the 1960s, I was in the mathematics department. The statisticians were in Sequoia Hall, a few yards away. If you were seen sneaking off to a seminar in Sequoia Hall, you were told in the coffee room that you were letting the side down. That is not my idea of maths! I was very shocked when I went back to Cambridge in 2001 to find that the divide between pure and applied mathematics, which I had seen in the 1960s, was still there. I had assumed that when the old men died off, common sense would prevail. But the trouble was the old men had successors, and they were just as keen to keep the two apart. That is ridiculous.

I am sorry, I am getting passionate, but I do feel passionate. Kai Lai Chung says in one of his prefaces that mathematicians have more interest in building fire stations than in putting out fires. Well, some mathematicians are! They have an abstract theory of fire stations which is formulated within category theory or something. But some other mathematicians like holding the hose, squirting water on the flames. And the important thing is that they keep talking to one another!

[AM/MR]: *In Portugal, for example, this is still a problem. Often, they don't speak with each other. But it begins changing.*

A second career: science administration and leadership

[AM/MR]: *In parallel with your scientific work, you moved on to an amazing series of occupations in science, administration and leadership. You headed the British Science and Engineering Research Council, you were the vice-chancellor of the University of Bristol for 16 years before you chaired the fairly new Isaac Newton Institute in Cambridge. You have also been president of the Royal Statistical Society, the London Mathematical Society, the European Mathematical Society, the British Statistics Commission, and we could move on mentioning top positions in private companies, and others.*

What did propel you into science administration and leadership? Did you enjoy this work as much as or perhaps even more than your work in the mathematical arena? Could you combine both a bit?

JK: It happened incrementally. I was a harmless professor of mathematics in Oxford doing what professors of mathematics do, when I was asked to sit on the mathematics committee of what was then the Science Research Council. That council gave grants to mathem-

Wills Memorial Building, Bristol University.

Isaac Newton Institute, Cambridge.

aticians and spent its time trying to persuade the real scientists, the experimentalists, that a little bit of money spent on mathematics would be a good investment.

Historically, very little grant money went into mathematics. The person who changed that was Christopher Zeeman. He had been chairman of this maths committee, and he made a real nuisance of himself. On one occasion, at the committee when people were arguing for lots of money to be spent on producing neutrons, he proposed the abolition of the neutron! Anyway, we were trying to build up grant income for mathematicians, so that we could go to conferences and publish, have decent libraries and even a work computer. We are talking now about what happened in the 1970s, when computers were coming in.

I was made chairman of this committee after a couple of years, and I sat on the next committee up, that was called the Science Board. And then I was made chairman of the Science Board. That meant I sat on the Council itself, which was bringing engineering into its title. And then I was asked to be chairman. Well, the previous jobs had all been, of course, part-time, and I was just doing them along with my ordinary work in Oxford. But the chairmanship of the Council itself, which meant that you were in charge of the whole thing, of spending several million pounds a year, was a full-time job. From then on, I took to doing mathematics really as a hobby, as an aid to sanity. When there was a little time, I could think about mathematics, and I was able to publish a little. But basically, I had given up being a full-time mathematician.

I did enjoy it; it was very hard work. It was very stimulating, and it brought me into contact with an enormous variety of scientific and engineering activity. I learned an awful lot of all sorts of physics, chemistry and biology and engineering, and I came to admire the wonderful things that were being done in some of these areas.

Then I had assumed that I would just go back to Oxford. But I was asked to be vice-chancellor of Bristol, which was the university where my father and my brother had started. Of course, that was not a very easy job either. Altogether, I had 20 years in which I was an amateur mathematician. Then, in 2001, I was asked to be director of the Isaac Newton Institute. I thought, that is fine, I can go back to mathematics.

But what I discovered was that the subject had moved on so much. Perhaps I should have worked harder to keep up with all the new developments, but I had not. I could please myself by working on the old-fashioned problems. But I really could not contribute to the way the subject would develop in the meantime, and that's good! I'm very much in favour of that. Subjects that stay the same, stagnate, and mathematics does not stagnate. But there were times when I felt like someone on a railway journey who decides to get off at an intermediate station to stretch his legs and then realises that the train has gone off without him and there is not much that can be done. I really have not been a full-time mathematician since 1981, when I went to the Research Council.

[AM/MR]: *You have already mentioned that not every brilliant mathematician is a good academic teacher as well. In the same spirit, not every brilliant mathematician is good as a science leader and promoter. From your perspective, which properties, which qualifications are essential as head of a scientific organisation or as a science politician?*

JK: That is a very difficult question. I have not thought about it thoroughly and in general. I am just considering and watching what has happened to the Research Council and to the University of Bristol since I left. My successors have been very different people with very different qualities. I think it is actually quite difficult to list the qualities, to give a job specification, as it were, because different people bring different things to these jobs.

Obviously, a depth of vision and ability to appreciate. A broader point of view and the ability to put that across to the people who provide the money. When I was at the Research Council, I spent a lot of time talking to Mrs. Thatcher, the Prime Minister, and other influential people, trying to get them to put more support into science, and you have got to be able to do that. You have got to be organised to be able to chair a meeting and that sort of thing. But when you put all those qualities together, you already have something. The people who really make an impact have something which is quite difficult to define, and it is different for different people.

Also, often the job makes the person! You challenge someone, and they say, well, I would never do that. I just say, put their mind to it, and they may discover qualities in themselves that they did not suspect. Of course, it goes the other way, too. There are even historical examples for that: Someone who everyone thought would make a marvellous Roman emperor until he became emperor, he was not marvellous at all.

You asked about enjoying it. I found that I have enjoyed more or less each phase of my career. They have been very different. But I have never wished I was back in the previous job. I always found that the job I was doing was interesting and enjoyable even if sometimes a bit hairy.

The European Mathematical Society

[AM/MR]: *You were elected president of the EMS when it was still fairly new, 12 years after its foundation. What or perhaps who brought you into the EMS? What were the most important items on your agenda at the time?*

JK: I had not had anything to do with the EMS before. I think I was an individual member, but I had not taken part in any duty. It was quite a surprise when I was asked to be president. I think it was partly because I was about to take over the directorship of the Isaac Newton Institute. And of course, the research institutes are an important aspect to the EMS. The society had formed a sort of umbrella, called ERCOM (European Research Centres on Mathematics), for those institutes, like IHÉS in Paris, Oberwolfach, and so on. I was obviously very honoured to be asked.

But I knew very little about what was going on, so I investigated. I learned about the way it had been founded as the result of a considerable argument between the French and the British, with the French wanting a society of individual members and the British thinking more of a federation of national societies. I think it is one of the strengths of the EMS that it managed to be both of them. It gives it a very strong structure, relying on both individual members and corporate members. It means, of course, that it has

EMS Executive Committee meeting, Prague 2004.
(Photograph by David Salinger)

to add value to both aspects of the mathematical world in Europe. It should not be in a competition with the mathematical societies in the countries, or indeed with the International Mathematical Union, which does a very good job. But in between those two levels, the global and the national, there is room for a European dimension.

There have proved to be many useful things that the EMS could do. I have always thought that complementarity is the important thing. What can we deal with at the European level, because there are things which are specifically European.

When the European Research Council was set up, for instance, the EMS lobbied very hard to have a mathematician on it, and we got Pavel Exner, who was then the vice-president, on to the council, and later the inimitable Jean-Pierre Bourguignon took it over; that was splendid.

It was a great experience to work with colleagues from all over Europe on the Executive Committee, and I am most grateful to people like Helge Holden and Tuulikki Mäkeläinen. We found plenty to do, and I believe we did it well.

[AM/MR]: *How did you tackle the tensions between applied and pure mathematicians within the EMS?*

JK: I was lucky, because after the presidencies of Fritz Hirzebruch and then Jean-Pierre Bourguignon, Rolf Jeltsch came in as very much of an applied mathematician, and he really strengthened that aspect of the EMS. I tried to do the same on the statistical front.

Mathematics throughout the world

[AM/MR]: *When you were president of the EMS, the UK was still a solid part of the European Union. What effect has Brexit had on science cooperation in Europe?*

JK: I haven't been involved in the administration of any of this since Brexit, so my information is second hand. Scientists have tried very hard to find ways to maintain the collaboration that was set up under the European Union and with variable success. Of course, not all international collaboration is based on the European Union. CERN, for example, was an international organisation long before any of these. You can do that in a narrow area like in particle physics or some of the astronomy, you can set up a close collaboration. There is so much involved in building telescopes in the Canaries or in Hawaii. These international collaborations don't need something like the European Union. There are just a number of countries that have agreed to collaborate. And when scientists get the opportunity to have some wonderful new equipment, they are willing to work across the national boundaries.

[AM/MR]: *Do you have any clues about how the new US government will influence scientific cooperation more broadly?*

JK: I think we have to wait and see. Anyone who tries to predict what President Trump is going to do next week has my admiration [laughs]. But I think it's worth stressing that whatever the politicians decide, science is an international activity. You saw that when Europe was divided by the Iron Curtain. Despite all the obstacles, people managed to continue to collaborate somehow. For example, the way that Hungarian and Polish mathematicians maintained contact with Western Europe and other countries. They've got to find ways of following their passion, and if that means establishing international links, they find ways of doing it. They don't necessarily wait for the government to catch up.

Mathematical practice – earlier and now

[AM/MR]: *The way mathematicians work on a research topic, and how they publish, has changed a lot while you were active. Would you comment, please?*

JK: That's one of the things which has changed enormously over my career. When I was a young mathematician, we went to conferences from time to time, and we wrote letters and sent them out. If you published a paper, you got 50 offprints that you sent off to anyone who you thought might be interested. If you thought that someone was doing interesting work, you wrote them a letter and asked them for offprints. You might or might not get them! It was very sedate and rather slow work.

And now, mathematics, like the rest of science, moves incredibly fast. If you're working on a particular problem, if you have a contact via e-mail or on social media or whatever is your taste, with people working on it in different parts of the world, if you have an idea, you send it down to your friends in Japan or America or whatever, and in a couple of hours later, someone comes up with a counter example.

That means that the subject is moving very fast. It also means that it's quite difficult for young people too, because you've got to get into these informal means of communication. I don't quite know how that works now. I think some young mathematicians find it quite difficult unless they have some strong links, for instance through a department or a senior collaborator. A present-day Ramanujan might have a hard time.

Preparing young mathematicians

[AM/MR]: *There are many indications, for example in the European PISA records, that we are not preparing our students in a very satisfactory way for their professional life in modern society. What should we do to prepare them better?*

JK: I think it has always been like that. When I think about the old-fashioned mathematics course I learned from Cambridge, I wasn't being prepared for the modern world. You weren't even allowed to learn functional analysis in those days. That was regarded as very unsound mathematics.

And we're never really preparing our young people. They have to find their own way, and they do. The nice thing about mathematics is that we are all being challenged by young people who come up. Perhaps you have been working on a problem all your career, and then some young woman or some young man comes along and proves the theorem!

I have always liked the story behind elliptic functions. People had been working on elliptic integrals for years and years. Then, Jacobi came along and said, *Let's look at them backwards*. It is as if we had invented trigonometric functions by studying the indefinite integral $\int \frac{1}{\sqrt{1-x^2}}\,\mathrm{d}x$, and then someone comes along and says, *But you should take the inverse function!* And then trigonometry, sines and cosines, and all sorts of clever things like Fourier series arise…

Elegant mathematics

[AM/MR]: *Are there other particularly elegant or beautiful mathematical concepts or ideas that still fascinate you?*

JK: A lot of mathematics is incredibly exciting, even some very old mathematics. I still like Euclid's proof that there are infinitely many prime numbers. I knew a very distinguished biologist who didn't believe you could prove that, and he thought that there were only finitely many prime numbers, and he went to his grave believing that!

And you know, in probability there are wonderful things. I like even simple things like the central limit theorem or the law of the iterated logarithm. Why is it $\log\log n$? It is very odd, but easy when you understand it.

Very often, something that looks ugly, when looked up at in the right way becomes beautiful and elegant. I am still very cross about my proof of the subadditive ergodic theorem. I gave the first proof of that theorem, but it's the most awful proof. I am ashamed to look back at the paper in which I published that proof. But since then, people have come along, and given much more natural elegant proofs, and I can now look at their proofs and admire what they've done. Einstein once said, "*leave elegance to the tailor*." But that was a physicist's remark. For a mathematician, elegance is actually very important, because it is a sign that you are going with the grain of the subject. You are looking at the thing in the right way. If it is all very ugly and very hard, you are probably not looking at it in the right way.

I gave an example much earlier, going with the problem about determinants with Michael Atiyah, and being told about the exterior algebra. That was a case in point. I had been reading in a book about determinants and a theorem. It was incredibly difficult to prove just because the notation was so bad. Michael just drew some arrows and a commutative diagram, and push, push, and there you are! Because he was looking at it in the right way!

That, of course, is part of teaching mathematics. You try to teach people to think the way a mathematician would think. When I was at school, I had a very good and experienced maths teacher. One day, when I produced a very elaborate piece of algebra, he said to me: "*Kingman, let me give you a word of advice. Mathematicians are lazy.*" What he meant was: "*You don't fill pages of algebra if you can do it in a much slicker way. And the slicker way will lead to better understanding. Your pages of algebra don't help me to understand it, even if they were a valid proof.*"

Speculations about current and future mathematics

[AM/MR]: *In which mathematical areas do you observe or expect the most exciting developments, right now, and in the years to come?*

JK: That we can't say, can we? Because the exciting thing is that next week someone will get up and say, *"Look, let's do it this way"* and suddenly the whole area will take off.

That's what makes it such fun. We're not just doing things the way our fathers and grandfathers did it. The nice thing about mathematics is that young people can do this, and we have to take them seriously.

For instance, my wife was a historian. To be a professional historian, you have to spend years in archives and so on. And nowadays, doing interviews and all sorts of things. Gathering evidence, you can't suddenly say "*This is the truth about Napoleon*." You have to do lots and lots of work and go over the evidence. And then when you are about 55, you write a great book showing that Napoleon was actually a quite nice man after all.

Whatever your theory is within mathematics, a young person can come along and say "*Well, I just proved the Goldbach Conjecture*." Unless you've made a mistake, quite possible. Even Andrew Wiles has made a mistake. But unless you've made a mistake, they have to give you the prize, because you've done it.

And everyone, all these grand professors in the grand universities, suddenly have to say "*Yes, she has done it, or he's done it*. *We didn't think of it that way, because we've been working in a particular way".* When you have done that for many years, it's very difficult suddenly to say: "*Oh no, we'll do it in a different way*." You need a fresh person coming on board.

Max Planck said that science progresses from funeral to funeral. It's not that the old men change their mind, it's that they die, and new people come with new ideas. You don't have to kill them off; you got to bring in the new ideas!

I don't know what areas are going to be particularly fruitful. I can see applications like artificial intelligence, for instance. Something struck me: When software people started producing browsers or search machine engines, at some account they had to use matrix algebra. That was a surprise. Undoubtedly, artificial intelligence will require new maths, perhaps some novel infinite-dimensional geometry?

[AM/MR]: *Now that artificial intelligence is on everybody's mind, it will certainly have an impact on how mathematics will be done in the future and new types of applications will arise.*

JK: Yes, it's amazing to think that AI might produce a proof of the Riemann hypothesis. But it raises questions, doesn't it? I remember when people started serious work on the four-colour problem, they realised that they needed computers to look at all the different cases. And for a long time, no human being had actually worked through the entire proof. So, we were trusting computers. Well, that is a sort of artificial intelligence. If you have got a computer proof that you really can't work through yourself line by line, you are relying on a sort of artificial intelligence. Examples like this made people think more deeply about the nature of mathematical proof. We never give a proof which is in formal symbolic logic. Bertrand Russell tried to do it, but he couldn't get very far, just well enough to get to quite elementary mathematics. Over the centuries we have worked out all sorts of shortcuts. If you read a mathematical paper, it has a proof. But that proof is really a rhetorical proof. We like to call it a proof when it is convincing the reader that all possibilities have been thought through and that the result must be true. It is not that there is a series of rigorous steps with every little bit filled in. A mathematician would often say: *It is easily seen that*. You know, that's rhetoric. That's not logic.

[AM/MR]: *But by now quite a lot of at least semi-elementary mathematics has been transformed into logic, into LEAN software, for example. Apparently even quite complicated proofs have been checked in the meanwhile. It's quite amazing.*

JK: Yes. But another thought. Mathematicians used to produce beautiful asymptotic expansions and other devices to derive accurate numerical results. Then computers made these redundant by churning out numbers to high orders of accuracy. Technology changed the nature of mathematical practice. This will happen again.

Young and experienced mathematicians

[AM/MR]: *A question of a different type at the end. When we last saw each other many years ago, I remember having asked you what you wanted to do after you retired. You answered at that time, tongue in cheek, as little as possible. I don't know if you remember. Have you really retired completely from mathematics and statistics? Or are you at heart still busy?*

JK: I'd stick to my original answer. It was complicated, because I retired in 2006. My wife and I had bought a house, this house in Bristol, where I still live, and we set up a retired life. We travelled. We had social activities. But then in 2010, she had a very bad stroke. For the next seven and a half years, I was really looking after her, and that was my major activity. I couldn't do mathematics. If I first started thinking about a mathematical problem, then we wouldn't get anything for lunch. I really had to push mathematics away. She died in 2018, and then I had to decide what kind of life to lead moving forward.

I didn't think that there was really any chance of my catching up with serious mathematics. I enjoy doing puzzles and things like that. And sometimes, you know some of the puzzles that you read in the newspaper actually have a mathematical structure. It is quite interesting to look at them. Not from the point of view of just filling in the numbers, but thinking what an algorithm would look like, that sort of thing. I have never published anything about that.

Just occasionally I return to real maths. My friend Persi Diaconis, famous for instance for proving that a pack of cards must be shuffled seven times, came to lunch in Bristol. He set me a problem in advance, about large permutations. I thought I'd solved it, but it turned out that I had misunderstood and solved a different problem. Still, it was a not quite trivial result, and it gained an honourable mention in one of Persi's papers.

I spend a lot of my time reading, listening to music, I go out, but only in Bristol, visiting friends. I have a son and a daughter and three very interesting teenage granddaughters.

I greatly admire mathematicians who can go on into their 80s and 90s, producing non-trivial mathematics. I remember when I was, many years ago, an editor of the Journal of the London Mathematical Society, we got papers submitted by Littlewood and Besicovitch; they were both very old. I had to find referees, and the only thing I could think of doing was sending Littlewood's paper to Besicovitch and Besicovitch's paper to Littlewood. We received some interesting comments. Some mathematicians just go on and on and on, and I greatly admire that.

[AM/MR]: *In a way, you have already commented that there is some truth in Hardy's dictum that mathematics is a young person's game.*

JK: I think it's true in the sense that there are sorts of handbrake turns when mathematics takes a quite new direction, and this is almost always caused by a young person, and that has always been the case. Think of Galois or Abel attacking the old question of solving equations by radicals. They brought a completely new dimension to the field, introducing ideas like group theory and Galois theory. But older mathematicians develop a low cunning which can often complement the ideas of the young.

I always like it when someone writes to say they have found a mistake in my 1971 paper.

[AM/MR]: *Really?*

JK: Yes.

[AM/MR]: *Thank you very much for an interesting conversation covering many insights and many years!*

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