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Editors' introduction

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E Brian Davies

This special issue of the Journal of Spectral Theory (JST) is devoted to honoring Edward Brian Davies (known to his friends as Brian), mathematical analyst extraordinaire, on the happy occasion of his 80th birthday.

Brian was an undergraduate at Jesus College, Oxford, UK, receiving a Meyricke Scholarship; he was awarded the Junior Mathematics Prize and a first-class degree in Mathematics there in 1965. He remained at Oxford for his graduate studies and received his DPhil (supervisor D. A. Edwards) and the Senior Mathematics Prize in 1968. After visiting Princeton (1968–69) and a Moore Instructorship at MIT (1969–70), he became Lecturer in Mathematics and Fellow at St. John's College, Oxford, UK (1970–81). In 1981, he was appointed to a Chair in Mathematics at King's College London (KCL). He was the Head of the Analysis Group at KCL from 1990 until his retirement in 2010, and the Head of Department for the period of 1990–93.

He was elected a Fellow of the Royal Society in 1995 in recognition of his seminal work in spectral and scattering theory and the dynamics of quantum systems and for his pioneering work on heat kernels of diffusion equations on manifolds. In 1996, he was elected a Fellow of KCL. He was the recipient of the Senior Berwick Prize of the London Mathematical Society (LMS) in 1998 and was the President of the LMS between 2007 and 2009. He was awarded the Pólya Prize of the LMS in 2011.

He was the founding Editor of the LMS Student Texts (1983–90). Together with Ari Laptev, he created the Journal of Spectral Theory published by the European Mathematical Society and became its founding Editor in Chief for the period of 2010–14. In other words, he and Ari laid the foundations for JST and ensured its success from its start in 2010 as demonstrated by its rapid expansion from 460 pages in Volume 1 in 2011 to 1551 pages in Volume 9 in 2019.

After the breakup of the Soviet Union, Brian was instrumental in attracting many excellent analysts from the former Soviet Union to the UK. More broadly, he was among those who inspired and guided the creation of an internationally leading group of pure mathematicians in the UK working in the theory of linear partial differential equations (PDEs) and spectral analysis. The combined strength of the London colleges in the field is now unmatched anywhere in the world, and Brian's influence transcends London and the UK.

In addition to producing over two hundred research papers[1], many of a highly influential character and of lasting importance, he also published five highly successful monographs and two books on the philosophy of mathematics and science. Brian's most recent interests focus on the interactions between ancient history, archaeology, and recent progress in science.

Brian's research fields, very broadly, can be grouped as follows.

- Quantum Theory of Open Systems
  (culminating in a widely quoted 1976 monograph)
- Spectral Theory and Pointwise Bounds of Heat Kernels,
  including the introduction of the notions of ultracontractive
  semigroups and intrinsic ultracontractivity
  (often regarded as Brian's most important achievements)

[1] A list of Brian's publications and a list of his students are appended to this introduction.

– Pseudo-Spectra for various types of Non-Self-Adjoint Operators
(Brian regards these as his most enjoyable research contributions)
– Other miscellaneous results
(including papers on C^*-algebras, Ambarzumian's theorem, etc.)

Brian is a world authority in spectral theory and its ramifications. While his early contributions to the field centered around the spectral theory of self-adjoint operators, one-parameter semigroups, and heat kernel bounds, his later interests revolved around the spectral theory of non-self-adjoint operators and rigorous numerical spectral analysis. In particular, he made major contributions to the rigorous study of the pseudospectra of non-self-adjoint operators.

His standing in the international research community can be gauged to some extent from his MathSciNet data: at the time of this writing, MathSciNet records 7,144 citations by 4,615 authors in 5,168 publications. His Cambridge Tract on "Heat Kernels and Spectral Theory" alone nets a whopping 1,252 citations, exhibiting his enormous talent as an expositor of mathematical writings.

The present volume collects contributions from Brian's colleagues and collaborators resonating with some of his varied scientific interests. They include, in short, topics such as analytic Besov functional calculus for several commuting operators, bound states in bent soft waveguides, Dirac points for twisted bilayer graphene with in-plane magnetic field, an extension of the Liouville theorem for Fourier multipliers to sub-exponentially growing solutions, Hardy inequalities for large Fermionic systems, Sobolev improvements on sharp Rellich inequalities, inequalities à la Pólya for the Aharonov–Bohm eigenvalues of the disk, manifolds whose Weyl spectral asymptotics have small but not tiny remainders, spectral instabilities, spectral inclusion sets, the pseudospectrum of an operator with Bessel-type singularities, computing the spectra and pseudospectra of bounded linear operators, and topological bound states and secular equations for quantum-graph eigenvalues. We thank all authors for their contributions and the referees for their invaluable assistance.

We sincerely hope that reading in this eclectic volume will cause much joy: *A Very Happy Birthday, Brian!*

Curriculum Vitae

E. B. Davies

Biographical data

Full Name: Edward Brian DAVIES

Date of Birth: 13th June 1944

Nationality: British. Born in Cardiff, Wales.

1965 B. A. Mathematics, University of Oxford, First Class.
Junior Mathematical Prize, with special commendation.

1968 M. A. and D. Phil. Mathematics (Functional Analysis), University of Oxford.
Senior Mathematical Prize.

1967–68 Junior Lecturer, Oxford University.

1968–69 Visiting Member, Institute for Advanced Study, Princeton.

1969–70 C.L.E. Moore Instructor, MIT, Cambridge, Mass.

1970–81 Tutorial Fellow, St. John's College, Oxford.

1971–73 C.U.F. Lecturer, Mathematics Department, Oxford University.

Autumn 1972 Visiting Member, Rockefeller University.

1973–1981 University Lecturer, Oxford University.

April 1974 Visiting member, IHES, Bures-sur-Yvette, France.

Spring 1975 Visiting member, Geneva University.

1973–1981 Joint Chief Editor, Quarterly Journal of Mathematics.

1974–1978 Member of Mathematics Faculty Board.

1974–1978 Chairman of the M.Sc. Examiners.

Autumn 1977 Visiting Lecturer, Princeton University.

1973–1978 Organiser of quantum theory seminar.

1979–1981 Senior Dean, St. John's College, Oxford.

Spring 1980 Visiting Professor, Ecole Polytechnique Federale, Lausanne.

From October 1981 Professor of Mathematics. King's College London.

1983–1990 Founding Editor of London Mathematical Society Student Texts.

1984–1987 Member of SERC Mathematics Committee

1986–1990 Member of London Mathematical Society Publications Committee.

September 1986 Visiting Associate, California Institute of Technology.

1987–1990 Member of Academic Staff Committee of International Association of Mathematical Physics.

February–June 1988 Visiting Professor. Cornell University.

May 1990 Visiting Professor, California Institute of Technology

August 90 – July 93 Head of Department of Mathematics, KCL

September 1992 Visiting member, Mittag–Leffler Institute, Stockholm

October 93 – September 94 Leverhulme Trust Senior Research Fellowship

March 1995 Elected a Fellow of the Royal Society.

June 1996 Elected a Fellow of King's College London.

April 1998 Co-organiser of Edinburgh ICMS Instructional Conference.

April 1998 External member of Newcastle University internal review of Mathematics Department.

May 1998 External member of Bristol University internal review of Mathematics Department.

1998–2002 Member of Management Committee of Isaac Newton Institute.

June 1998 Awarded Senior Berwick Prize by London Mathematical Society.

September 98 – December 99 Chair of School Research Committee of KCL.

1998–2000 Royal Society Sectional Committee 1.

1999–2000 Council of London Mathematical Society.

1999–2001 Member of Panel 22 for RAE 2001.

September 1999 Member of Appointment Committee for Chair in Pure Mathematics at University of Sussex.

2000–2002 Member of National Advisory Board of Isaac Newton Institute.

June 2000 Member of Board of Electors for Director of Isaac Newton Institute / Rothschild Professor of University of Cambridge.

2002–2006 One of three principal editors of LMS monographs.

June 2002 Member of appointment committee for Glasgow Pure Mathematics chair.

June 2002 External advisor for Munich Pure Mathematics Chair.

2002–2005 External Examiner University of Nottingham.

2003–2006 Senior Editor for London Mathematical Society Monograph series.

June 2003 External advisor for Newcastle Pure Mathematics chair.

June 2003 Elected an Honorary Fellow of St John's College, Oxford.

September 2003 Analysis Landscape author for International Review of UK Mathematics.

January 2004 Member of Appointments Committee for Mathematics Chair in Cyprus.

February 2005 Member of Appointment Committee for Herchel Smith Chair of Mathematics in Cambridge.

2007–2009 President of the London Mathematical Society.

July 2008 Initiator and organizer of Spectral Theory Minisymposium at European Congress of Mathematics.

August 2007 External Adviser for Warwick Mathematical Physics Chair.

Summer 2008 External Advisor to Panel 20 for RAE 2008.

2008–2012 External Adviser for Wales Institute of Mathematical and Computational Sciences.

8–9 December 2009, 65th Birthday Conference

2010 Elected a Fellow of the Institute of Mathematics and its Applications.

7 May 2010 Gave the German Mathematical Society Gauss lecture in Jena, Germany.

2010 King's College London, King's Award for lifetime achievement.

September 2010 Retired from position at King's College London. Appointed as Emeritus Professor and Honorary Research Fellow.

April 2011 Elected a Fellow of the Learned Society of Wales.

2011 Awarded the Pólya Prize by the London Mathematical Society.

2011–2014 Founding Editor of the Journal of Spectral Theory.

2015 Retired from King's College London.

August 2015 Liver Transplant.

Research papers

1. A generalised theory of convexity. Proc. London Math. Soc. (3) 17 (1967) 644–652.
2. The existence of characters on topological lattices. J. London Math. Soc. 43 (1968) 217–220.
3. The structure and ideal theory of the pre-dual of a Banach lattice Trans. Amer. Math. Soc. 131 (1968) 544–555.
4. On the Banach duals of certain spaces with the Riesz decomposition property. Quart. J. Math. Oxford (2) 18, (1967) 109–111.
5. Tensor products, infinite products and projective limits of Choquet simplexes. With G. Vincent Smith.Math. Scand. 22 (1968) 145–164.
6. The Choquet theory and representation of ordered Banach spaces. Illinois J. Math 13 (1969) 176–187.
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8. The structure of Sigma*-algebras. Quart. J. Math. Oxford (2) 20 (1969) 351–366.
9. Decomposition of traces on separable C^* algebras. Quart. J. Math. Oxford (2), 20 (1969) 97–111.
10. An operational approach to quantum probability. With J. T. Lewis. Commun. Math. Phys. 17, 239–260 (1970).
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12. Quantum stochastic processes. Commun. Math. Phys. 15, 277–306 (1970).
13. Involutary automorphisms of operator algebras. Trans. Amer. Math. Soc. 158 (1971) 115–142.
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16. Quantum stochastic processes III. Commun. Math. Phys. 22, 51–70 (1971).
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19. Hilbert space representations of Lie algebras. Commun. Math. Phys. 23, 159–168 (1971).
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22. Properties of the Green's functions of some Schrödinger operators. J. London Math. Soc. (2) 7 (1973) 483–491.
23. The ideal Boson gas in an external scalar potential. Commun. Math. Phys. 30, 229–247 (1972).
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Some PhD students and PostDocs

David E Evans, DPhil., Oxford 1975

Peter F Palmer, DPhil., Oxford 1977

Greg Singleton, Ph.D., King's College London, awarded 1986

Michael Pang, Ph.D., King's College London, awarded 1988, then postdoc until 1991

Maria Lianantonakis, Ph.D., King's College London, awarded 1990, then postdoc until 1993

Gerassimos Barbatis, Ph.D., King's College London, awarded 1994, then postdoc until 1997

Mark Owen, Ph.D., King's College London, awarded 1997

Owen Nicholas, Ph.D., King's College London, awarded 2000

Narinder Claire, Ph.D., King's College London, awarded 2000

Lyonell Boulton, Ph.D., King's College London, awarded 2001, postdoc March 1–July 31, 2001

Anna Aslanyan, Postdoc at King's College London, June 1997 to December 31, 2000

Paul Redparth, Ph.D., King's College London, awarded 2001

Jiban Nath, Ph.D., King's College London, awarded 2002

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See also in the Mathematical Genealogy Project[3].

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Interview with Brian during an 80th birthday celebration

The following is an edited version of an interview with Brian by Alexander (Sasha) Pushnitski on June 3rd, 2024, during an 80th birthday celebration via Zoom, organized by Sasha and Eugene Shargorodsky. The celebration started with mathematics talks by Nick Trefethen and Rupert Frank, and the interview proceeded as an informal conversation following the talks, with several other mathematicians present.

Alexander Pushnitski: Brian, let me start with the first question. I understand that George Mackey had a major influence on you in Oxford during your PhD. Can you explain how this happened?

Brian Davies: Yes. Probably many of you don't know who he is. He was about 20 years older than me, which would make him around a hundred now. But I've never heard that he's died. He was a Harvard professor. He visited Oxford in the academic year 1966 to 1967 and gave a series of lectures which I was privileged to attend. He gave courses in the first term on unitary representations of locally compact non-abelian groups, in the second term on quantum mechanics, and in the third term on number theory. He had an unusual mode of lecturing: he didn't state theorems precisely and didn't give any ideas on how to prove them. I did actually talk to him because I was assigned as being the right age and subject, to write notes for the first term of his lectures. I asked, "Where could you find the proofs?" He said, "Oh, proofs? One way uses C^*-algebra theory." He'd never mentioned C^*-algebras in the lecture course. But the proofs were apparently to be found in Dixmier's two books on C^*-algebras, which were heavy going. His lecture courses were less heavy because he never mentioned any of the proof techniques. But he got me to realize how much analysis there was beyond what I knew. He set a problem on C^*-algebras whose solution involved a huge step forward for me, but it was not my PhD. In fact, my PhD had nothing to do with him or what he had been talking about. But I owe him a great debt because he was very enthusiastic and obviously believed that graduate students shouldn't be given too much help. You tell them what the problem is, and hope that they might find the solution. You let them sweat over the details. Anyway, he went back to Harvard, after which I saw very little more of him. Does anyone know him? Nick, did you know him?

Nick Trefethen: When I was an undergraduate, he was still there, and I remember we thought of him as a very old man. He died in 2006, according to Wikipedia.

Brian Davies: Oh, did he? I didn't look that up. So, when did you leave Harvard?

Nick Trefethen: I graduated in '77. Maybe he was retired by then. But he was still around. Certainly, somebody we knew of.

Brian Davies: Yes. Well, he made a huge contribution to the theory of locally compact non-abelian groups. His work on induced representations from subgroups to the whole group were instrumental in the development of a detailed analysis of the representation theory of semi-simple Lie groups.

Alexander Pushnitski: Let me come to my second question: Brian, why did you turn to numerical analysis later in your career?

Brian Davies: Well, this is quite an interesting story. I thought about moving to computation as a possible career. I didn't follow it up at that stage because it involved a lot of hard work setting up the rules for multiplying complex numbers together, and things like that, which eventually turned into Matlab. I probably learned how to program a computer in assembly language earlier than any of you, including Nick; this was in 1961.

Nick Trefethen: You beat me by 12 years.

Brian Davies: Well, I had a year between being awarded a place in Oxford and starting the course there in 1962. I had very little to do, and managed to get myself onto a computer programming course at Cardiff University for their new PDP-9 or something like that. I managed to calculate Pi using a program to 2 decimal places. This shows something about how mathematically immature I was! But I was very, very pleased to be getting into it myself. Much later, I was approached by Malcolm Brown, who was a numerical analyst in Cardiff. I've mentioned this earlier today. He was unable to get an eigenvalue program to work, and he asked me for some help. I thought it was strange asking me, and I thought, I don't see how I can do anything except look at a particular, exactly soluble problem, namely the harmonic oscillator with a complex potential, which is exactly soluble, and see what happens in that case. I spent a few weeks translating everything to the case where the potential had a complex coefficient rather than a positive real one. Then I saw what Malcolm's problem was. Not that his programs were faulty, but that the problem itself was numerically unstable for large enough eigenvalues. What he was getting was a result of the instability rather than an error in the program itself. That was about the time when I got in contact with Nick. Did you get in contact with Malcolm Brown via me?

Nick Trefethen: I think that's probably right. I remember we went to one of the conferences in that place in Wales whose name was Gregynog. Right?

Brian Davies: Gregynog.

Nick Trefethen: Okay, you're the Welshman, so I think I met him through you. That's right.

Brian Davies: Yes. He was a very enthusiastic organizer of conferences. He came up with problems he didn't know how to deal with and kept pushing me. I was very pleased because I had been studying heat kernel methods from about 1982 until about 1995. It was time for me to make a change to non-self-adjoint theory. I thought, well, I've got one more subject I can try to make some progress in before I retire. It suited me fine. It wasn't so difficult, but you couldn't guess what the answer was except by doing numerical examples. Once you got the examples, you saw what was happening. That was the point where you could start searching for a proof. Nick somehow managed to understand what the answer was in a few minutes once he was given the problem. I always felt very amazed, almost annoyed but not actually, because I already knew the answer before he told me, but he managed to get the answer within a few minutes by using Matlab, when it had taken me 3 or 4 months.

Nick Trefethen: Healthy competition.

Brian Davies: It was a very enjoyable part of my life. After retirement, I made a decision. Most of you may know, or maybe you don't. I became very ill in the early 2000s and decided to retire completely because I wasn't sure I would survive. I had a liver transplant in 2015, and recovered far more completely than I had expected. I thought, well, it's time to change to another subject. I don't have the concentration any longer to do mathematics, so I must find something simpler to pass my time. I've spent the last 10 years trying to write a book on archaeology, not the archaeology of mathematics, just archaeology. I'm unlike those people who are admired for having an Indian summer after they retired. I actually have retired. I haven't thought of writing a mathematical paper or even reading one for nine years now. My last paper was with Eugene, I think. Yes, with Michael Levitin and finally Eugene.

Eugene Shargorodsky: Going back to numerical analysis, you know, once you said something in a talk, I think it was at UCL, that has become one of my top maths quotes. I absolutely love it. You stated a theorem and said, "Well, here's a theorem. I proved it, but I also checked it numerically, so I know it is correct."

Brian Davies: Nick would really applaud that attitude. It's amazing how convinced you can become of the truth of something by doing some simple numerical examples. You have to develop a feel for it. I believe that Nick Higham, who I got into contact with after Nick [Trefethen], at one stage said he had been approached by this guy, Brian Davies, but didn't know whether he should take what I was writing seriously. He asked Nick [Trefethen]: "does he really *get it*?"

Nick Trefethen: I don't remember that, but it sounds plausible. Can I mention one of my favourite examples of something where, in order to verify the theory, you do numerics? So here's the problem. You have two unit cubes that are attracting each other gravitationally. The question is, what's the gravitational force between two unit cubes? It's incredibly complicated, but after Bengt Fornberg finally solved it exactly, the solution involves 14 terms, each with square roots, and so on. How do you know those 14 terms are correct? Well, of course, you integrate it numerically.

Alexander Pushnitski: Let me ask another question, Brian, if I may. What persuaded you that mathematical entities were invented rather than discovered?

Brian Davies: Yes. This is why I was a bit doubtful when someone today said how much I'd influenced people. My lack of belief in the reality of mathematical entities certainly didn't convince anyone. I picked it up from a book published in my first year of graduate studies by a man called Errett Bishop. He was a well-known and respected complex analyst who got infected by the idea of constructive mathematics as opposed to the mathematics most of us use almost all the time. I was very impressed by this book because he obviously knew what he was writing about. He was trying to impose a rigorous mathematical structure on the idea that there was a difference between saying that there exists a solution (he thought that this was an unsatisfactory type of mathematics) and giving a method of calculating it as accurately as you want. Apparently, he went on a tour of the United States, giving lectures in mathematics departments, saying that having an algorithm for calculating a function gave you better insight into what was happening than just declaring that the solution existed. According to his own account, he persuaded essentially nobody at all except me. I was a first-year graduate student, and I was open to anything that anyone wanted to tell me. It made me a lot more open to seeing the difference between numerical analysis and what we call standard analysis, where the existence of a solution is regarded as a separate question from the method of calculating it. That was one origin of my interest [another being Dixmier's multiple uses of the (non-constructive) axiom of choice when treating von Neumann algebras]. I decided that I would not write any papers that used non-standard constructive methods in any obvious way because I thought that as a young mathematician, this would mean I would be wiped off the map by most people. So, I hedged my bets: when I had a choice between giving the constructive or non-constructive proof of a theorem, I often chose the former. There were several other instances where I could see people doing the same but not referring to the philosophical underpinnings of this sort of constructive mathematics. Eventually, I got involved in philosophy. By this time, I didn't care what people thought about me.

Alexander Pushnitski: How did you obtain your initial results on Gaussian bounds on heat kernels?

Brian Davies: My study of heat kernel bounds proceeded one step at a time. I wasn't trying to prove the heat kernel bound which eventually came into my book. I was trying to understand similar but less detailed results which had been proved in the 1950s and 1960s by several people. I just couldn't understand the papers. I thought, is this because I'm not clever enough to understand what they're doing, or is it because they are not writing it well enough for anyone to understand what they are doing? I thought, can I find a simpler way of proving the results? It took me about a month to produce upper bounds for heat kernels of second-order elliptic operators with various unknown constants in the inequalities. Then I thought, well, I've done it: their approach can be written out in a simpler way than they did. But I felt very dissatisfied with the size of the constants. I spent about three months just plodding along trying to get the constants down to slightly more reasonable values. I spent most of this time with no reason for doing it other than wanting to write out a proof which didn't involve constants of the order of 10^{10^n}. Then I started realizing some strange things were happening because the constants were sort of settling down on their own will. Those of you who know the standard Laplacian on standard Euclidean space, the heat kernel formula is $(4\pi t)^{-n/2}e^{-x^2/4t}$. There's a 4π, an $n/2$, and a 4 in this formula. I found similar numbers were somehow appearing just by me plodding along, trying to get a slight improvement every few days, until in the end, I had the exact formula for the case of the Laplacian. The main change that you needed to make was replace 4 by $4 + \varepsilon$, where ε was an arbitrarily small number. I had no intention whatsoever of proving the inequality which I eventually became best known for. I didn't have any goal except to try to make the estimates simpler. You don't have to have a brilliant long-term goal like Andrew Wiles, that I'm going to prove this theorem whether it kills me or not. It's possible to get results which are worthwhile by just pursuing an argument until it won't go any further. I think we've had an example today in which persistence is a large part of the game. I'm referring to Rupert. This is an extremely impressive piece of work, but you have to be willing to sit down for months or years at a time, just looking for some slight improvement to inequalities in some source which no one else seems to know about. You can get extraordinarily good results by that method sometimes, and sometimes you don't. There's an awful lot of luck in it. Certainly, persistence is the most important thing.

Alexander Pushnitski: Brian, let me ask my last question. How did the breakup of the Soviet Union influence your career?

Brian Davies: Well, yes. I left Oxford in 1981 because it was quite obvious I would never get promoted to a chair in Oxford. At the time, they had a fixed number of chairs, and the only way to get one was by the death or retirement of a holder. Looking around me, the professors were rather young, and I wouldn't have a chance until I was over 60. If I wanted to get a more senior position, I had to leave Oxford, as many other people did. I eventually ended up at King's in 1981, when a dreaded Prime Minister, Margaret Thatcher (1979-90), was trying to impose cuts as strong as she could get away with, particularly to universities, which she didn't like at all – she thought academics were wasting their energy. There was only one new appointment in the

Mathematics Department before 1990. Then the Soviet Union collapsed, and everyone there who was young and energetic and ambitious realized that their best bet in life was to leave and try to find a job somewhere else. I was absolutely delighted at the possibility of advertising posts and getting some of the ablest Soviet mathematicians to come to London. I managed to get one or two with great difficulty. Yuri Safarov was the first. But the person who should be given most of the credit for this is not me, it's Dmitri Vassiliev, who got a position in Sussex. Were you the first Russian to arrive?

Dmitri Vassiliev: Yes, indeed, I was.

Brian Davies: And he knew everyone who was worth knowing in my subject in the Soviet Union. People started flowing in one or two at a time, and it eventually got to the state where what I thought of as my analysis seminar, instead of having six people every week, had about 30. For the dinner after the seminar, we went to a restaurant somewhere. Sometimes it was nearer to Imperial College, sometimes nearer to UCL or King's. There were occasions when I was the only British person at the dinner but I never felt isolated. Everyone learned to speak English remarkably quickly, and I learned to speak Russian remarkably slowly. But it was a wonderful time. Dmitri Vassiliev and Yuri Safarov were the two people who wrote a book together on the Weyl asymptotic formula. They were leading analysts in the whole world, not just in Russia. It made a huge difference to the development of analysis in the UK, absolutely huge. The person you should really thank for making this happen was Dima. Was I involved in your appointment?

Dmitri Vassiliev: I don't know, Brian, maybe indirectly. I know who the members of the interview panel were, but I don't think you were there.

Brian Davies: David Edmunds was very important.

Dmitri Vassiliev: Yes, David Edmunds was crucial in appointing me.

Brian Davies: But you had a greater influence on the future appointment of other people. It was a wonderful time for analysis which had been weak in Oxford and Cambridge for the previous 50 years. Think of Hardy. Nobody can deny that he was a great mathematician, but from about 1960 onwards, neither Oxford nor Cambridge seemed to want to appoint people in the study of partial differential equations, spectral theory, or anything like that, which was regarded as applied mathematics by pure mathematicians and pure mathematics by applied mathematicians. So neither of them had a professor in analysis after 1960.

Dmitri Vassiliev: I think one of the problems was that Titchmarsh didn't leave many students, if I understand correctly.

Brian Davies: Oh, that's certainly true. He was very introverted and didn't build up a school. There were a few people, but nothing like as many as one would have expected. Frederick Atkinson, JLB Cooper, Des Evans, Norrie Everitt, Michael Eastham, and John McLeod were students of his. He was very, very uncommunicative. Probability theory was better, as Terry Lyons knows. The person who might have been a professor of analysis was John Kingman. And his counterpart in Cambridge - what was his name?

Terry Lyons: David Kendall, you mean.

Brian Davies: Yes, David Kendall. They were actually probabilists who used analytic methods. But then a few people got to London and the weekly analysis seminar was growing, more Russians wanted to come here because it was becoming one of the central research areas in PDEs in the country. And so it has remained, I think.

Eugene Shargorodsky: Unfortunately, we have not quite retained the probability half of the seminar. I personally blame Terry because when he left Imperial for Oxford, we never managed to recover, and we could not keep the probability half of the "London Analysis and Probability Seminar" going.

Brian Davies: Yes. Look, I think I'm getting tired, and I need to stop here.

Terry Lyons: It was lovely to see you, Brian. I'm sure lots of people feel the same.

Brian Davies: Let me thank Sasha and Eugene again for organizing this meeting. I've enjoyed it tremendously. I feel very lucky to have had the opportunity of seeing some of my old students and friends. I haven't talked to everyone personally, but maybe some other time. Not a 90th birthday, though, that's not likely. Thank you very much for coming and making this an enjoyable day for me.

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