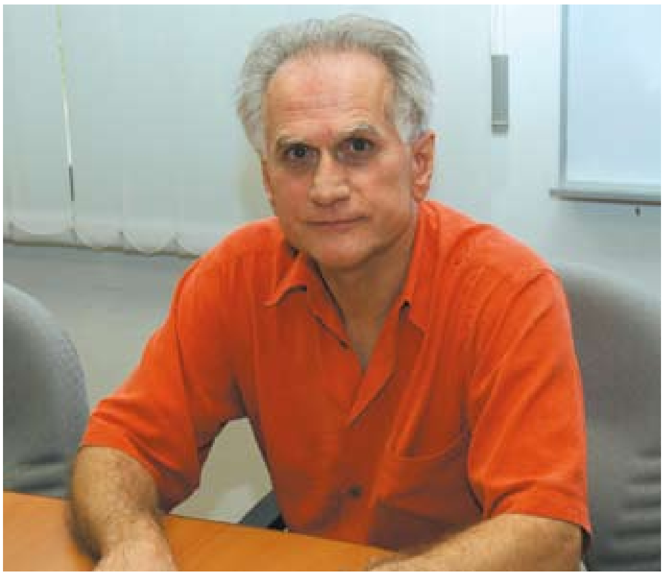
Béla Bollobás: Graphs Extremal and Random >>>



Béla Bollobás

Interview of Béla Bollobás by Y.K. Leong (matlyk@nus.edu.sg)

> *As long as a branch of science offers an abundance of problems, so long is it alive; a lack of problems foreshadows extinction or the cessation of independent development.*
> - David Hilbert (1862 – 1943)
> International Congress of Mathematicians, Paris, 1900

Béla Bollobás is well-known for a wide range of significant contributions to graph theory, combinatorics and functional analysis. His recent work on applications of random graph techniques to percolation theory is a ground-breaking contribution to the theoretical basis of a newly emerging field motivated by physical phenomena and first explored by computer simulation.

He may be regarded as a leading exponent of the Hungarian school of graph theory, having paved the way for the current widespread applications of random graphs in numerous areas in applied mathematics, physics and engineering. In addition to more than 350 research papers, he has written 10 books and edited 9 volumes. He is also well known for his mathematical exposition and for championing the cause of the combinatorial approach in mathematics. His two books *Extremal Graph Theory* and *Random Graphs*, published in 1978 and 1985 respectively, were the first books to systematically present coherent theories of early results in those areas. His latest book *Percolation* is written jointly with Oliver Riordan. Bollobás's personal and mathematical connections with his mentor, the prolific and consummate problem-solver Paul Erdős (1913 – 1996) and with his intellectual mainspring Trinity College in Cambridge are the stuff of legends of contemporary mathematics.

A Fellow of Trinity College since 1970, Bollobás has a long and distinguished career at the Department of Pure Mathematics and Statistics in Cambridge University from 1971 to 1996; from 1982 to 1994 he paid long visits to Louisiana State University at Baton Rouge. In 1996, he accepted the Jabie Hardin Chair of Excellence in Combinatorics at the Department of Mathematics of the University of Memphis, Tennessee, while keeping his Fellowship at Trinity College. Since 2005, he has been a Senior Research Fellow of Trinity College. He is also a foreign member of the Hungarian Academy of Science. He has held visiting appointments in various countries throughout the world and has been invited to give lectures at major conferences and scientific meetings. He has supervised over forty PhD students, some of whom have gone on to distinguished careers, notably Tim Gowers, 1998 Fields Medalist and Rouse Ball Professor of Mathematics at Cambridge University. Bollobás excelled not only in mathematics but also in sports: he represented Oxford University in the pentathlon, and Cambridge University in fencing.

Bollobás's connections with NUS date back to 1994 when he was visiting professor from June to August. During his second visit from May-June 2006 for the Institute's program *Random Graphs and Large-scale Real-world Networks*, of which he is chair, Y.K. Leong interviewed him on behalf of *Imprints* on 17 May 2006. The following is an edited and enhanced version of the transcript of the interview, in which he traces his mathematical journey from a closed Hungarian communist system to an eclectic academic environment in Cambridge and speaks passionately about his personal mission in spreading the philosophy of combinatorics within mathematics, his reminiscences giving us glimpses of the richness of modern mathematical traditions.

Imprints: You did your first doctorate in Hungary. Who was your supervisor then?

Béla Bollobás: I should be able to answer this question very easily, but I cannot, since in the Hungary of the 1960s we didn't have well-defined supervisors: we would join a group of mathematicians, attend the right seminars, talk to the right people, and work on our dissertations on problems we picked up. The group I joined was that of László Fejes Tóth, who worked on discrete geometry and had written the famous book on the subject, so I wrote my dissertation on packings, coverings, and tilings. However, my real supervisor was Erdős. I had got to know him when I was 14 or so, and from then on he gave me lots of mathematical problems; over the years he kindly stayed in touch with me and inspired me. Of course, he was not in Hungary all that much, but even when he wasn't there, he wrote letters

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with problems and so he was my real supervisor from the very beginning.

I: Was your education a typically traditional Hungarian one?

B: Yes. I always went to school, didn't stay at home and was not home-schooled like Erdős, for example. But I did have lots and lots of private tutors, not for school work, but for extra-curricular activities. I grew up 5 – 10 years after the war, and in the communist Hungary of the day many people who had played prominent roles before the war lost not only their livelihoods but even their homes: they were sent into "exile". So I was taught at home by a former general, a count, a baroness and a former judge. They were excellent people, but in those days, they were deemed to be nobodies. So with my education I was exceptionally lucky: I couldn't have had better people to tutor me. The judge was not allowed to remain in the judiciary, so he took up teaching. The general was pretty famous – he was the head of Hungarian fencing. Fencing was actually popular in Hungary for many years. This great-uncle of mine had the wonderful idea of setting up a Fencing Academy in the army, so that able recruits had a chance of being trained to be coaches, rather than go on mindless drills. Within a few years Hungary produced more coaches than the rest of the world put together. Before the war in Central Europe fencing was much more important than it is now: for doctors, judges, lawyers and civil servants fencing played a role somewhat similar to that of golf today. The three countries that were great in fencing were France, Italy and Hungary.

I: Were you spotted by Erdős?

B: In some sense, yes. In Hungary there are many competitions; in fact, the idea of having mathematical competitions at all was born in Hungary. When I was 14, I won the national competition, and, as luck would have it, Erdős just returned to Hungary for a week or so: he sent word to me that I should go and meet him. I met him in a fancy hotel in Budapest, on a hill-top. We had lunch and it was amazing that he was willing to talk to a 14-year-old boy. He was 45 but to me he looked ancient. Throughout his life he was extremely good to youngsters. His favorite was Louis Pósa, whom he got to know when Pósa was 10 or 11. Erdős was very disappointed when, after a good start to his career, Pósa didn't continue in mathematical research but chose to nurture very talented teenagers.

I: It seems that Hungary has produced a disproportionately large number of mathematicians.

B: That is certainly true. I'm pretty sure it's due to two things. Firstly, in Hungary we had a journal for secondary school pupils. It's a monthly based on attractive and challenging problems. Readers are invited to send in their solutions which are then checked, marked, and the best of which get published. That made a huge difference. The other reason is that there are annual mathematical competitions: three-hour long exams testing your ingenuity on a handful of problems. I believe that the existence of the journal was even more important than the annual competitions, since the competitions in the journal went on throughout the year. All the time, you have problems that you wanted to solve – elegantly. The judges gave you bonus marks if you gave several solutions or you generalized a problem or you sharpened the bounds, which generated much research. Practically everybody I can think of went through this system – Marcel Riesz, Alfréd Haar, Eugene Wigner, von Neumann, Pólya, Szegő, von Kármán. But Erdős was never good at those competitions; von Neumann and von Kármán were very good at them. Wigner and von Neumann were in the same school, and Wigner considered von Neumann to be the only genius he had ever met, although he had known Einstein as well.

I: What made you go to Cambridge to do a second doctorate after your fist one in Hungary?

B: Hungary was a very closed-in country. You were not allowed to travel outside, and going abroad was always a tremendous feat. From an early age, I felt claustrophobic. At the beginning of my university studies, I asked Erdős whether I could go and study abroad. I knew that he was allowed to live abroad and came back to Hungary for only short periods. He spent a lot of time in Israel and even had a job there. I asked him whether I could go to Israel for a semester or even a year to study mathematics. Then he said, "Why Israel? You are not even Jewish. Why not Cambridge? I have a very good friend who had just gone there to work with Davenport and maybe he can help you." Of course, going to Cambridge was beyond my wildest dreams. So Erdős wrote a good recommendation to Harold Davenport to try to get me into Cambridge. By then I had a joint paper with Erdős which I wrote when I was still at high school. But we needed permission from the communist authorities. That took ages and ages, and was very humiliating, but eventually I did get the permission and was allowed to go to Cambridge for a year. That was in the middle of my undergraduate studies. After a year in Cambridge I returned to Hungary but very soon I had a scholarship to go Cambridge to do a PhD. I applied to the authorities for permission to go there, but I was refused. Next, I had a scholarship to Paris but was again refused permission to leave the country.

I: You went to Moscow?

B: Yes. After I had got my degree, I spent a year in Moscow

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to work with Israel Moiseievich Gelfand. My year there was a wonderful mathematical experience. After Moscow, but not quite immediately, the communist authorities allowed me to go to Oxford on a scholarship (from Oxford, of course, not Hungary). By then, I said to myself, "If I ever manage to leave Hungary, I won't return." So when I arrived in Oxford, I decided to take up my old scholarship to Cambridge rather than return to Hungary. That way I didn't have to apply for anything because it had been sitting there for years. But then within a year, I got a fellowship from Trinity College, which was better than getting a PhD. There was no pressure on me whatsoever to submit for another PhD. But I thought that as the College had given me a scholarship to do a PhD, it was my duty to get one.

I: I notice that your PhD in Cambridge was done with Adams, who was a topologist.

B: Yes, Adams was my official supervisor but in reality I worked by myself, getting my problems from the Functional Analysis Seminar. When I was in Moscow, Gelfand said that it would be very good to work with Michael Atiyah or Frank Adams, the great topologists. However, when I was in Oxford, Atiyah was on his way to the Institute for Advanced Study, and when I arrived in Cambridge, Adams was still in Manchester although on his way back to Cambridge. By the time Adams arrived a year later, I already had a fellowship at Trinity College. Nevertheless, Adams remained my official supervisor; in fact, I learned a fair amount of algebraic topology from him and I did work on some of his questions. During my first year in Cambridge I joined the functional analysis seminar, where I found several beautiful problems, some of which I solved, so my Cambridge PhD thesis was on Banach algebras.

I: Was your interest in graph theory shaped by your early years in Hungary?

B: It was certainly due to Erdős. If he hadn't been there to give me lots of attractive problems, I'm sure I would have ended up doing either number theory with Turán or probability theory with Rényi.

I: You have published several books on graph theory, including *Extremal Graph Theory* in 1978 and *Random Graphs* in 1985. What made you write these books?

B: It really goes back to the picture I had of graph theory, not only picture but reality. For some peculiar reason, in the early 1970s or later, graph theory came in two flavors; one was done in Western Europe and America, and the other in the East, mostly Hungary by Paul Erdős, Tibor Gallai, Gabriel Dirac and others. In the west, they didn't do any extremal graph theory. On the other hand, in Hungary, graph theory was almost exclusively extremal graph theory. I very much wanted to show that extremal graph theory was a pretty serious subject and not only a collection of random problems that Erdős thought up and popularized. The usual charge against graph theory is, "Ah, it is made up of ad hoc problems that have nothing to do with each other. What's the point?" To some extent, at the very beginning, this is true, but slowly, slowly all these results do gel into a single theory, so my aim was to show that there is such a theory – extremal graph theory. I started to write this book very soon after I arrived in Cambridge but it took me ages to finish. I had to take a sabbatical to find enough time to finish it.

The theory of random graphs was founded by Erdős and his good friend Alfréd Rényi in the late 1950s and early 1960s. At the beginning, they wrote several joint papers but the whole theory didn't take off. People didn't jump on it and said "How exciting! Let's try to continue it." The climate started to change in the 1970s. In particular, Erdős came to visit me in Cambridge for a term as a Visiting Fellow Commoner in Trinity College – perhaps the longest period he spent in one place for many decades, since he never stayed anywhere for more than a week or so. He suggested that we work on random graph problems. I got interested and from then on, I was doing random graphs. I had this urge to showcase the classical theory together with lots of new developments and show that it is not only a beautiful subject but also very important. Really, random graphs became more and more active in those days. Once you write a book, parts of it became outdated almost immediately. It was the first serious book on random graphs just as the book on extremal graph theory was the first book on the subject. They are on different aspects of graph theory but they are closely connected.

I: How do you see the future of combinatorics, especially random graphs?

B: Hilbert, I think, said that a subject is alive only if it has an abundance of problems. It is exactly this that makes combinatorics very much alive. I have no doubt that combinatorics will be around in a hundred years from now. It will be a completely different subject but it will still flourish simply because it still has many, many problems. The same applies to random graphs. In fact, the field of random graphs has connections with statistical physics, percolation theory and even computer science. It's very strange that just at about the same time that random graphs were founded, Broadbent and Hammersley founded percolation theory. These two subjects are all about random subgraphs of certain graphs. They should be about the same – okay, one is finite and the other is mostly infinite and lattice-like, but still, they have about the same questions. For many, many years there were no interactions between the two subjects, none

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whatsoever. Now this is changing quite a bit. Quite a few combinatorialists are doing percolation-type problems.

I: Are random graphs applied to biology?

B: Yes. In the last 10 years or so many new spaces of random graphs have been defined in the hope of modeling phenomena in various areas, including biology. People have realized that large-scale real-world networks resemble random graphs. You can't really say that they have this structure or that structure. But this random graph is very different from the classical Erdős-Rényi model of a random graph. It has different characteristics: for example, the degree distribution may follow a power law, unlike in the classical case. One of the main advocates of using new models of random graphs is László Barabási, who also proposed several interesting models.

I: Was the power law discovered empirically?

B: Yes, it was observed that several graphs seem to obey a power law, but there were no proofs that they really do. Physicists and experimentalists have a very different attitude from that of mathematicians: much of the time they are not very interested in rigorous proofs. For a mathematician it is rather annoying that proving even the basic results about these new models can be pretty tough. Oliver Riordan and I have done a fair amount of rigorous work on properties of power law graphs.

I: You mentioned that there is an abundance of problems in combinatorics. It seems that combinatorial problems are very easy to formulate but very hard to solve.

B: For me, the difference between combinatorics and the rest of mathematics is that in combinatorics we are terribly keen to solve one particular problem by whatever means we can find. So if you can point us in the direction of a tool that may be used to attack a problem, we shall be delighted and grateful, and we'll try to use your tool. However, if there are no tools in sight then we don't give up but we'll try to use whatever we have access to: bare hands, ingenuity, and even the kitchen sink. Nevertheless, it is a big mistake to believe that in combinatorics we are against using tools – not at all. We much prefer to get help from "mainstream" mathematics rather than use "combinatorial" methods only, but this help is rarely forthcoming. However, I am happy to say that the landscape is changing.

When Erdős and Rényi started the theory of random graphs, they had to make do with basic probabilistic results concerning sieves and moments, but combinatorics changed the landscape of probability theory considerably. In order to answer questions in probabilistic combinatorics, results of a different flavor had to be proved in probability theory: results concerning sharp thresholds, isoperimetric inequalities, rapidly mixing random walks, and so on.

There are many other tools as well: algebraic, analytical, and even topological. For example, Borsuk's theorem has been used to prove several beautiful results in combinatorics. The achievement is not in applying such a theorem, after all, every schoolboy knows the theorem, but in discovering that it can be applied, and how it can be applied.

A totally ignorant and unfair way of judging a result in combinatorics is to ask the author: "What have you used to prove your theorem?" Then, upon being told that such and such a theorem was used, comes the retort: "Oh, that's very easy. I could have done it". What nonsense. Yes, of course it's easy once you are told what to do. The achievement is in finding the tool that can crack the problem after a series of clever manipulations that make the problem amenable to the application of the tool.

I: Could the difficulty of combinatorial problems be due to the discreteness of the objects?

B: Not really. Frequently, it is fairly easy to change a discrete problem into a continuous one but more often than not this change does not bring us any closer to a solution. The trouble with the combinatorial problems is that they do not fit into the existing mathematical theories. They are not about functions, topological spaces, groups or operators. More often than not, we simply do not have the machinery to attack our problems. This is certainly not the situation in other branches of mathematics. In fact, it may happen that first a wonderful machine is built and then the search starts for a worth-while problem that this machine can be applied to. This attitude is totally foreign to combinatorics. In combinatorics we have our problem which at the beginning looks like a Chinese box: there seems to be no way in, there is no indication as to how to start it. Here's the problem: we want to solve it and we don't care in what way we solve it.

I: So you are almost starting from nothing or from the bare minimum . . .

B: To some extent, yes, but of course, these problems are also built on top of each other. Once a problem gets solved, another one arises, and the theory does build upwards as well, not only sideways. A problem I certainly love and I'm sure is very deep is the problem of conformal invariance in percolation theory. I also love the related problems about the existence of various critical exponents. I have no doubt that these beautiful problems are so hard that they'll be around for many, many years. The original problems are

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combinatorial although they can also be considered to be problems in analysis or probability theory. I'd be surprised if we didn't need totally novel ideas to solve them.

I: Going the other way, are there any problems in more traditional areas of mathematics that can be solved by combinatorial methods?

B: Oh yes. It is frequently the case that once you have applied all the tools at your disposal, at the end you have to solve an essentially combinatorial problem in the traditional sense: you have to argue from the bits of information you have better than anybody else.

I: I think that the perception of combinatorics has changed considerably.

B: I hope that it is changing, for it should certainly change. Combinatorics is becoming a more "serious" subject, closer to the traditional branches of mathematics – there's no doubt about this. Combinatorics has many really hard questions, like number theory, algebraic topology and algebraic geometry.

I: Is there a single result or discovery of yours that has given you the greatest satisfaction?

B: I wonder how many people can say "Yes" to such a question. There are quite a few results that made me very happy at the time, but not one that I would trade for the rest. Let me tell you about some of my favorite results. Not surprisingly, people often like results they proved when they were young. Thus, I rather like a certain lemma of mine that I proved when I was an undergraduate. It is still one of the very few proper exact extremal results about hypergraphs. (Hypergraphs tend to be nastier than graphs, so this may not be so surprising.) Also, it can be applied in lots and lots of ways. It can be proved very easily: some years after I discovered it, Gyula Katona gave a ridiculously easy and very beautiful proof. But still, I am happy that I found it when I was an undergraduate.

Also, in the early 70s, I wrote a paper with Erdős in which we greatly improved a 30-year-old fundamental result of his, the so-called Erdős-Stone theorem. This theorem says that if a graph G on n vertices has εn^2 more (so, really, very few more) edges than the number guaranteeing a complete subgraph on r vertices, then suddenly it has a complete r-partite graph with t vertices in each class, i.e., r disjoint classes of t vertices, with an edge joining every pair of vertices belonging to different classes. (A little more precisely, we take $r \geq 2$ and $\varepsilon > 0$ fixed, and let $n \to \infty$.) This is very much a "phase transition" type result: once the number of edges increases beyond the point at which a "very thin" complete r-partite graph can be guaranteed, we can guarantee a rather "thick" (t-thick) complete r-partite graph as well. The question is all about the largest t one can guarantee. Erdős and Stone proved that the largest t one can guarantee is at least the $(r-1)$th iterated logarithm of n, the order of the graph. Erdős conjectured in numerous papers that the correct bound is precisely this iterated logarithm. To our great surprise, in the early 70s, almost thirty years after the publication of the Erdős-Stone theorem, we proved that the bound is log n, much larger than we imagined.

Another result I do like very much is about the scaling window in the phase transition of a random graph. Let us take a set of n vertices and add to it edges one by one, at random, with the uniform distribution, so that at "time" t we have t edges. The question we are interested in is "What does this random graph look like at various times?" (Here and elsewhere, all assertions are claimed to hold "with high probability", i.e., with probability tending to 1.) We are mostly interested in one of the crudest properties of our random graph: the number of vertices in the largest connected component. The greatest discovery of Erdős and Rényi was that at time $n/2$ there is a sudden *phase transition* in the sense that if the number of edges is a little less than $n/2$ then there is no large component, in fact, every component has at most order log n vertices; however, if the number of edges is $cn/2$ for some constant $c > 1$, then suddenly there is a *giant component*, a component of order n, in fact, a component with about $\alpha(c)n$ vertices, where $\alpha(c) > 0$. So the size jumps from order log n to order n.

Although at first sight this is a sharp result, it is far from so. Let us look at the point of phase transition through a magnifying lens. What magnification should our lens have to enable us to see the continuous emergence of the giant component? More formally, let us look at our process at time $t = n/2+s$. For what values of s is the largest component much larger than the second? Here are two rather different scenarios consistent with the theorem above. (1) If $s > n/\log n$ then with high probability the maximal component is at least 10^{10} times as large as the second, while for $s < n/(2\log n)$ this is false. (2) If $s > n^{1/2}$ then with high probability the maximal component is at least 10^{10} times as large as the second, while for $s < n^{1/2}/\log n$ this is false. Now, in the first case we would say that the *window* of the phase transition is about $n/\log n$, while in the second the window is about $n^{1/2}$.

About a quarter century after Erdős and Rényi proved their famous result, I proved that the size of the window is, in fact, $n^{2/3}$. Furthermore, if s is substantially larger than $n^{2/3}$, say, $s \geq n^{2/3} \log n$, but is still $o(n)$, then the largest component has about $4s$ vertices, and all other components are *much* smaller. This was the very first rigorous result about the size of a nontrivial window. All this is, of course, very close to

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percolation.

Let me finish with two more results. First, a lovely little theorem I proved with Andrew Thomason, which really should have been proved 150 years ago by Steiner or another geometer. Take any d-dimensional body of volume one. In that case, I can give you a box (a rectangular parallelepiped), also of volume one, so that no matter on which plane you project your body and the box, the projection of the box has at most as big a volume as the projection of the body. Note that we are talking about projections into $2^d - 2$ nontrivial subspaces: d subspaces of dimension 1, $d(d-1)/2$ subspaces of dimension 2, and so on. It is a little surprising that there is a body that in this sense minimizes all these projections.

And the last. Very recently, Oliver Riordan (one of the co-organizers of this program) and I proved that the critical probability of random Voronoi percolation in the plane is one-half. Of course, everybody who knows a little about percolation would have sworn that this critical probability must be $1/2$ and nothing else, but proving it was a very different matter. There is a strong similarity with the events in the 1960s and 70s, when everybody in percolation theory knew that the critical probability of bond percolation on the square lattice was $1/2$, but nobody could prove it; eventually, after a ten-year gap, Harry Kesten found a proof. The question concerning Voronoi percolation turned out to be much more complicated than that about the square lattice; my paper with Oliver will be published soon. Actually, our hope was that it would be the first step towards proving conformal invariance for random Voronoi percolation. The trouble is that even the "preliminary step" of showing that the critical probability is $1/2$ was much more difficult than we had bargained for, so we haven't yet managed to make much progress with conformal invariance.

I: It seems that you are a counter-example to the belief that good results can only be obtained before the age of forty.

B: Maybe, maybe, but, of course, the belief that a mathematician is dead after the age of forty is very much the figment of G.H. Hardy's imagination. Hardy loved to say that only young man can do real mathematics when, in fact, he himself was a very strong counter-example to that. Hardy after 40 was much, much better than Hardy before 40.

I: You have quite a few research students. Do you like teaching them?

B: I love to have good students. One of the many reasons why I love to be in Cambridge is that Cambridge has by far the best research students in Britain. I have had over 40 research students, many of them extremely good. It would be wrong to list them because whomever I wouldn't mention would be right to feel slighted. But let me just say that four of my students are professors in Cambridge. One of them is a Fields Medalist – Tim Gowers. His is the only name that I consider legal to mention because he's the only one to have got a Fields Medal.

I: Who are the people who influenced you most?

B: Paul Erdős is clearly the man who influenced my mathematical career the most. He was at almost every conference that I attended for 25 years. And one of my jobs at these conferences was to look after him. I really enjoyed his company very much. I would not have imagined how much I would miss him: I am really surprised that even a decade later I miss him very much.

When I was at Cambridge as an undergraduate, I got to know the great physicist Paul Dirac and his wife very well; I became very much part of their family. Mrs Dirac was from Hungary: she was a sister of Eugene Wigner, the Nobel-prize-winning physicist. It was wonderful to be around the Diracs. Mrs Dirac was the best hostess I have ever seen: she was very well read, had a great appreciation of art, loved antiques of all kind, and was extremely skilful to move the conversation to interesting, unconventional topics. Paul Dirac was an absolutely "free man", the free man *par excellence*, free in the sense that he was free of convention, and didn't have any baggage to carry, as he didn't want to prove himself, and did not mind what people thought about him. He was very polite and considerate, but he could say quietly his own opinion which was often different from that of other people's.

I: He was well-known for not saying too much, wasn't he?

B: That's true, but he did talk quite a lot when he was among friends. He talked to me quite a lot; I could never complain that he didn't. He is someone I have always respected tremendously. Unfortunately, precisely when we moved to Cambridge from Oxford in 1969, he retired to live in Tallahassee, Florida. It was a great blow to us because the Diracs were the people we knew most intimately in Cambridge. From then on, we always went to visit them in Tallahassee and stayed there for a week or even a month. People in Cambridge could never understand what Dirac could be doing in Florida, how he could "put up" with Florida after Cambridge. However, Dirac loved to be in Tallahassee and often told me that he should have moved there much earlier.

When I arrived in Cambridge for good, to become a fellow of Trinity College, I was surprised that J.E. Littlewood was still alive, as to me he was quite legendary. I was amazed

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that he was still around in the college. It was mostly through my wife, Gabriella, that I got to know him very well, and I am very lucky that I did. Gabriella, who is a sculptor, made several busts of him; one of these is now in the Combination Room of Trinity College. Littlewood had the reputation of being totally unapproachable, but by the time I got to Cambridge, he had mellowed much. Unfortunately, most of his former students and colleagues still respected him too much and were also a little afraid of him, so they very rarely visited him. He came to have dinner with us a lot; many times. When we had people for dinner, we asked him as well; his presence lent a weight to the evening as everybody was honored to be at dinner with Littlewood.

I: How old was he then?

B: He was 85 when we got to know him, and died at 92. He loved mathematics and had many stories about his friends, including Hardy, Russell and Wittgenstein. While sitting in the Combination Room, sipping claret, he would start his story with "Before the war..." Whenever somebody would ask "Which one?" the answer was always "The first". That was really wonderful.

When he died, I became his literary executive and inherited all his letters and papers; many of these papers originally had come from Hardy. I edited a collection of his stories, *Littlewood's Miscellany*, which is a delightful book, about twice as long as its predecessor *[A Mathematician's Miscellany]* and has many more stories. Of course, the stories were not new, but he remembered them after he had published that book. The extended version was published only after he died.

I: What do you think about Erdős's idea of the "ideal proof from the Book"?

B: Not very much. Actually that was really a joke of his – I talked about this with him many times. He was interested in proving good results; he did not set out to find the proof from the Book, as has been said about him many times. Of course, he was particularly pleased to find beautiful proofs of *simple results*. He always said, "Look, such gems, such really simple, beautiful proofs can only be found in the Book." You don't expect the Riemann Hypothesis to have a proof from the Book that one can give in 5 minutes. Of course not. You would expect an infinitely more complicated proof. So he always used "The Book" as a joke to enliven his lectures; it should not be taken seriously.

I: You have positions at Memphis and Cambridge. Isn't that a strange combination?

B: I must admit that it is. Everybody thinks it is. Actually I love both places very much. Cambridge is our true home: that's where we have been for close to forty years, and that is where our real house is – I'm sure that eventually we shall live only in Cambridge, with occasional trips to Budapest. But we also love to be in Memphis.

When I say that I love Memphis, people tend to be puzzled, but they don't know what they are talking about. In the first instance, we went to Memphis because my wife got absolutely fed up with Cambridge, finding it claustrophobic, and Erdős suggested that I go to Memphis, which he had visited many times, often several times a year. In Memphis I have a really wonderful job – no lecturing, no administration, a great assistant to look after me, funds to invite visitors, funds to travel, very clever and kind colleagues, an excellent gym, and so on. Although I do not have to lecture, I always give a graduate course on a topic I hope to write a book on. I view Memphis as a mathematical training camp, where the first thing to do is mathematics, and there is no second. Erdős had very good friends at Memphis – Ralph Faudree, Dick Schelp, Cecil Rousseau, Chip Ordman – mathematicians who helped him a lot: they are still in Memphis and now they are my friends as well; since my arrival they have been joined by several other excellent people like Paul Balister, Vladimir Nikiforov and Jenő Lehel.

On the other hand, when I say that I love Cambridge, nobody is surprised: "Of course, Cambridge is great." And Cambridge is great. I don't know whether you have been to any of the Cambridge colleges. For me one of the best aspects of my own college, Trinity, is that academics from different disciplines mix: we have outstanding people from all kinds of different subjects at our fairly informal lunches and rather formal dinners. You may find yourself sitting next to a physicist and an economist, and opposite a historian and a physiologist. These are wonderful occasions: you can talk about a great variety of topics to real experts in those fields. Also, it is flattering to be in a place where so many excellent people work. Of course, many a first-time visitor misses this aspect of a college entirely since with him the conversation tends to be shallow: "How long are you staying in Cambridge?", "Have you been here before?", "Where do you come from?", and so on. Thus, Vladimir Arnold got it completely wrong when he imagined that this kind of conversation goes on all the time. This couldn't be further from the truth.

I: Do you have a special position in Memphis? Was it created for you?

B: I'm the first occupant of a rather special chair, the Jabie Hardin Chair in Combinatorics. This chair was not created for me, but Erdős persuaded me that I should accept it, and my colleagues in Memphis were kind enough to be happy about it.

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I: Do you travel a lot?

B: Yes, I do: too much. I'm sure the urge to travel goes back to my childhood. In Hungary I grew up feeling imprisoned, and I was always longing to travel, especially to the South. I still find the South very romantic.

I: Erdős traveled a lot too.

B: Yes, Erdős traveled an awful lot. He traveled in a different way, he traveled alone, and almost always went for rather short periods. I frequently go for several months, and then I take lots of people with me, mostly my students and former students from Cambridge and Memphis. I feel that I have to take my current students with me if I want to take care of them: it would be very unfair to leave them at home.

I: I understand that you have taught our present Prime Minister Lee Hsien Loong.

B: I certainly taught him more than anybody else in Cambridge. I can truthfully say that he was an exceptionally good student. I'm not sure that this is really known in Singapore. "Because he's now the Prime Minister," people may say, "oh, you would say he was good." No, he was truly outstanding: he was head and shoulders above the rest of the students. He was not only the first, but the gap between him and the man who came second was huge.

I: I believe he did double honors in mathematics and computer science.

B: I think that he did computer science (after mathematics) mostly because his father didn't want him to stay in pure mathematics. Loong was not only hardworking, conscientious and professional, but he was also very inventive. All the signs indicated that he would have been a world-class research mathematician. I'm sure his father never realized how exceptional Loong was. He thought Loong was very good. No, Loong was much better than that. When I tried to tell Lee Kuan Yew, "Look, your son is phenomenally good: you should encourage him to do mathematics," then he implied that that was impossible, since as a top-flight professional mathematician Loong would leave Singapore for Princeton, Harvard or Cambridge, and that would send the wrong signal to the people in Singapore. And I have to agree that this was a very good point indeed. Now I am even more impressed by Lee Hsien Loong than I was all those years ago, and I am very proud that I taught him; he seems to be doing very well. I have come round to thinking that it was indeed good for him to go into politics; he can certainly make an awful lot of difference.

I: Do you have any books in the pipeline?

B: I have two books coming out for the International Congress in August. One of them is a collection of problems – lots of beautiful problems, exactly what we discussed over coffee in Memphis with Paul Balister and others. It will be published by Cambridge University Press and is called *The Art of Mathematics* with the subtitle *Coffee Time in Memphis*. The other one is a book I wrote jointly with Oliver Riordan: its title is just *Percolation* – short and punchy.