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A Conversation with David J. Aldous

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Abstract. David John Aldous was born in Exeter U.K. on July 13, 1952. He received a B.A. and Ph.D. in Mathematics in 1973 and 1977, respectively from Cambridge. After spending two years as a research fellow at St. John's College, Cambridge, he joined the Department of Statistics at the University of California, Berkeley in 1979 where he spent the rest of his academic career until retiring in 2018. He is known for seminal contributions on many topics within probability including weak convergence and tightness, exchangeability, Markov chain mixing times, Poisson clumping heuristic and limit theory for large discrete random structures including random trees, stochastic coagulation and fragmentation systems, models of complex networks and interacting particle systems on such structures. For his contributions to the field, he has received numerous honors and awards including the Rollo Davidson prize in 1980, the inaugural Loeve prize in Probability in 1993, and the Brouwer medal in 2021, and being elected as an IMS fellow in 1985, Fellow of the Royal Society in 1994, Fellow of the American Academy of Arts and Sciences in 2004, elected to the National Academy of Sciences (foreign associate) in 2010, ICM plenary speaker in 2010 and AMS fellow in 2012.

Key words and phrases: Exchangeability, Markov chain mixing times, scaling limits, local weak convergence, random graphs, network models.

Owing to the COVID pandemic, these conversations took place via Zoom on May 12th, May 25th and September 23rd, 2021, with David at his home in Redmond, Washington and Shankar at his home in Carrboro, North Carolina.

1. INTRODUCTION

Shankar: Thanks for taking the time to chat. This is quite early for you! I know you retired from Berkeley three years ago and moved up to Redmond, Washington. You are currently in Redmond?

David: I have become a morning person, or at least we have dogs who wake me up at 6:00. Of course being a coffee addict goes with being a morning person. So it all works out. We are near Redmond, up on the outskirts of greater Seattle. We're in this community that was carved out of forests, so we still have lots of forests. And if you're a wild animal, you can walk to the mountains, through forests, just having to go across the occasional road or river. I like to say I have become a *Gentleman of Leisure* (Shankar cackles hysterically) which sounds more positive than *retired*. So when someone asks me to do something, I tell them I am happy to interrupt my idyllic life as a gentleman of leisure (laughs)!

Shankar: Have they started opening up in Washington or is it still ....

David: We are in a large 55 plus community, so at the start of the pandemic we feared the worst, but in fact very few people here have been seriously affected. We have a clubhouse with a small bistro and meeting rooms, and many social clubs and activities. Rather like a college campus. Obviously the social activities were shut down during the depths of the pandemic but are now opening up. As are restaurants and stores.

Shankar: What have you been up to during the last few months?

David: Not much is the answer! I have this quiet life. I spend a few hours a day on professional activities. I exercise every day: two days a week I play volleyball, two days a week I go on a long walk with a walking buddy, two days a week I go to the gym.

Shankar: Amazing!

David: I like some structure to life, or otherwise you feel like you're wasting your time. If you have nothing else to do, maybe you start obsessing about meals for the day. So exercise is something to focus on, rather than having your life focused on eating. At the start of the pandemic, I went to the AAA office and said "once upon a time you had paper maps ...." So (Aldous produces

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and unfolds a very large paper map of Greater Seattle), I started walking on all the trails in our region, and marking them on the map.

Shankar: Amazing! OK I guess we can get started, we don't have to be super formal ....

2. EARLY LIFE

Shankar: Can you tell us about your early life and family?

David: Okay, so I grew up in Exeter in the southwest of England. That's a town of a hundred thousand, has a university, basically a very quiet, peaceful place. Middle class family, one older brother, a stay at home mom, this was the 50s. One memorable thing is that at age eight, I almost died from rheumatic fever. Caught by the family doctor and saved by penicillin. Luckily no after effects. Back then they were obsessed with not getting reinfected. So I was effectively quarantined, I was stuck in bed at home for several months after the illness. As you can imagine, for an eight year old boy who feels well, this is very annoying. No one would have thought of putting a television in an eight year old's room back then. So part of that time I spent amusing myself with pencil and paper doing some math for fun. I was always tall and I was always good at math. But no big deal—children are very matter-of-fact about these things.

At age eleven you go to the equivalent of high school. This was 1963. I went to a pretty good school. In particular, the math teachers had adopted drafts of what Americans would call *new math*, which has a bad reputation now, but it was being tried out in some number of volunteer schools, which meant that the teachers were enthusiastic about it. There's this general effect that any new scheme will work if it's run by enthusiastic people. So this was less Euclidean geometry etc and in the most advanced courses you had more things like basic group theory, a little linear programming and so on. So the fact that the teachers were actually interested in math rather than just grinding through a job helped.

Shankar: Do you remember anything else about your time at school? I know you are really into Volleyball but did you grow up playing sports, in particular what we all believe, is the greatest sport ever invented: cricket?

David: The school had 60 students per year so around 400 students total. It was unusual in that a quarter of them were boarders and the rest (like my brother and myself) were day students; we wore stupid uniforms which we didn't like. The school had been around for almost 400 years. Boys only. You had to play sports, which included rugby, which was tough if you weren't into physical sports. I was tall so that helped a bit, but I wasn't particularly keen on rugby. So rugby in autumn, field hockey in winter and cricket in the spring. They finally relented and let us do tennis instead. One of the odd rules was you weren't allowed to have a soccer ball on the premises—a remnant of class prejudice.[1]

Shankar: In our earlier conversations, you had mentioned a road trip around the US before you started college. What motivated this?

David: There is a story here. I could have gone to Cambridge just after my 17th birthday, but decided I would rather take what is now called a gap year. It wasn't particularly common in my time. I got on an exchange program to a prep school (which in America just means a boarding high school) back in the eastern United States. So that was a change, I spent a year in a boarding school in a new culture. At the end of the school year, I decided to hitch-hike across the US. The program organization first warned us against doing this, and then said if you want to do this we'll match you up with somebody else. So me and another British guy (who I hadn't met before or since) spent six weeks hitchhiking around the United States without any plan. Out to San Francisco (saw the original production of *Hair*—this was 1970) and down to L.A. Spent my 18th birthday day at Disneyland, perhaps common for someone living in California but odd for a British hitchhiker. One memorable story was we got robbed at gunpoint by a driver in the middle of the Arizona desert—actually more surrealistic than scary. Amongst possible career choices, robbing hitchhikers seems least wise. Mostly we were picked up by friendly young people in Volkswagen Beetles. Quite often they'd just invite us to spend the night and sleep on the sofa. This was 1970, so youth solidarity.

Shankar: I guess it would be less common for parents to allow their kids to hitchhike in this day and age ....

David: Right, but my parents were 5000 miles away! Along that theme, back around ages 9–13, I was into the ultimate nerd thing at that time, trainspotting. It was a stereotypically British thing, looking at locomotives and

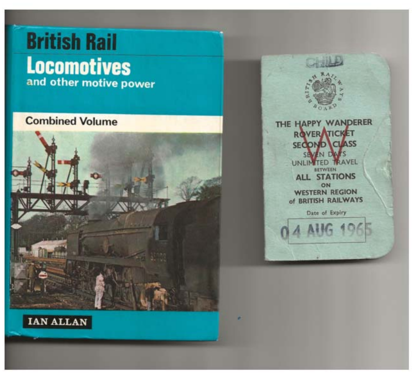

FIG. 1. 1965: *The trainspotter's bible, and freedom of the Western Region.*

[1] "Soccer is a gentleman's game played by hooligans, and rugby is a hooligans' game played by gentlemen"—attrib. Winston Churchill.

writing down numbers. You can get a book (gets up and shows SB a book, Figure 1), this is from 1965 or so, every trainspotter had this book and you went through it and marked the locomotives you had seen (David opens a page with many such underlines). If I had any sense of shame or embarrassment I would keep this quiet (laughs). When I started, the mainline trains were still steam. There was this very quick transition, over a period of about five years, they basically got rid of all the steam locomotives and replaced them with the diesels. I had a lot more independence than kids do now. There was a bus that went past my house and ended at the local main line station, so at age 10 I could pay my five old pence to take the bus and hang out there on Saturdays. Later I just rode the bike. When I turned 13, I bought a rail pass for a quarter of the country, to spend a week trainspotting. I came home at night but was free to go as far as possible within a day. Mom was a little worried, but I was allowed because I had always behaved responsibly. Back then, people perceived life as safe.

Shankar: Interesting! I think Peter Hall was also a trainspotter ....

David: Oh I didn't know that. Nowadays we would regard it as a manifestation of a touch of Asperger's; we hadn't heard of Asperger's then.

3. COLLEGE YEARS

Shankar: You entered Cambridge University in 1970. How did you end up at Cambridge?

David: Most universities accepted applicants on the basis of the "A level" national exams at the end of high school. Cambridge required also their own written exam, different by subject. I think there were three math papers and one general sort of English, "are you a human being not just a mathematician" paper. These were graded on a one through five scale, I got mostly fours and the only part I got five on was the precis section of the English paper where you are given a 600 hundred word article and you need to cut it down to 200 words. I often say that compressing things to their essence is my only recognizable skill here! They let me in on the basis of exams—before I went on the International Math Olympiad in Romania; that was the first time I had flown.[2]

Another rather British aspect was that, whereas my family had no connection with Cambridge, my school had a longstanding relationship with St. John's College (within the University) which was highly regarded in mathematics, so it was natural for me to enter that particular college.

Shankar: How was life at Cambridge when you joined?

[2]David won a silver medal at the IMO in 1969 and a special prize.

FIG. 2. 1972: *Aldous punting on the River Cam.*

David: This was the early 1970s and Cambridge was modernizing but there were traces of the earlier ethos in which you would work in the mornings, play manly sports in the afternoon and drink and discuss philosophy in the evening. This was the old ethos that some people would aspire to (Shankar cackles hysterically). It was fashionable to claim not to be working hard, even if you were. But I feel that we were genuinely much less stressed than subsequent generations of students, if only from the unconscious belief that by getting into Cambridge we had already "won," and that few of us worried about future careers. There was time and energy to be social (Figure 2), and I still keep in touch with a group of (mostly nonmathematical) friends who have subsequently led interesting lives.

St. John's College was one of the larger colleges so there were maybe 12 people per year doing math in my college, 200 throughout all colleges. You had lectures for six days a week but only in the mornings, for eight weeks, what Americans would call a quarter. The first year probability course in my year was taught in an uninspiring way by a famous number theorist. But in the second year we had an interesting course on Markov Chains by David Kendall, who of course was one of the eminent persons in probability, a key figure from 1950s queuing theory and broader Markov theory. This course was more lively with funny examples, three person duels etc and I found that entertaining. In the final year, there was the measure theoretic probability course, at the level of Chung's textbook (Chung, 1968). I also sat in for a while an advanced part III course, but stopped when it came to the abstract definition of conditional expectation. Being an intuitive person I said I don't understand this, what is going on with this definition? I always told this story when I was teaching the Berkeley first year graduate course: this is where I found things difficult!

Shankar: That is interesting! I started reading your St. Flour lecture notes on exchangeability, only after leaving Berkeley, but many of the arguments in those notes seem to be out of the twilight zone. In particular, I was always amazed with your mastery with abstract objects like regular conditional distributions.

David: Well my thesis work needed them so I used them. But my experience then replicated that of David Freedman,[3] so I stopped.

Shankar: In our past conversations, you had intimated that, your experience as a visiting student to Yale after the last year of undergraduate studies, was particularly influential in your eventual career. Can you tell us a little more?

David: The typical British system was a three year undergraduate program and the three year Ph.D. In math that was too short. And so Cambridge had this intermediate year, confusingly called Part III, even though it would be the fourth year of your time at Cambridge. It was essentially like a Masters program where you take a bunch of more advanced courses. I tried the same trick as my gap year and went on a completely different exchange program (Henry Fellowship) for that year at Yale. In those pre-internet times one had no idea who was going to be there, but by chance it worked out marvelously well and I found three memorable courses. A probability topics course taught by the famous analyst Kakutani, where students read and talked on research papers: I remember talking about a paper by Root (Root, 1969) on Brownian motion stopped at hitting a boundary, which was one of the first research papers I read. Ward Whitt in particular taught a course out of Billingsley's weak convergence book (Billingsley, 1968) which I really got into as it was a right combination of analysis and probability. Leo Breiman, who had taken time out of academia for private consulting, was visiting and he taught out of his book (Breiman, 1968) which was one of the most interesting books on probability at that time and was inspiring.

4. PH.D. AND EARLY CAREER

Shankar: Was this when you decided to do a Ph.D. in probability?

David: Doing a Ph.D. seemed the default option: it would have required effort to think of doing anything else! Also I had done two summer intern positions, at an actuarial group and a civil service group, and in both cases thought, with the arrogance of youth, that my bosses were complete idiots. Happily for me, everywhere there is some exam to take before proceeding to a Ph.D. program, but at Cambridge this was in May, at Yale this was in September, so I was in neither place at exam time. Cambridge took me back despite this, things were more informal then. But this explains some gaps in my knowledge, because I never had to study for the exam.

Shankar: You started working, in 1974, with Ben Garling and later, with Geoff Eagleson. Tell us a little about your experiences as a graduate student.

David: Ben Garling was also in St. John's College so I had had lots of interactions with him as an undergraduate student. He was an analyst but had gotten into probability on Banach spaces. For instance, do the IID laws of large numbers or central limit theorems hold in a given Banach space? Well, for some Banach spaces it always does while for others it doesn't. So there is a characterization just using geometry of Banach spaces. Ben was interested in this so I got started reading about Banach spaces.

Shankar: Was this what your thesis work was about?

David: Indirectly. There was a conjecture by Haskell Rosenthal, about embeddings of l^p spaces into L^1, which was essentially about existence of subsequences of random variables with a certain property. At the same time, John Kingman had gotten interested in Chatterji's subsequence principle, that for any sequence of random variables you can find a subsequence satisfying asymptotic laws analogous to the IID world. Kingman had realized this was about exchangeability, that essentially from any sequence you could extract a subsequence that was "asymptotically exchangeable" in some sense, which then by de Finetti had all these properties. So these two different things (Banach space embeddings and subsequence principles) involved the same ideas, although superficially in quite different ways.

I first learnt about Kingman's work by reading the Cambridge physical notice board which had the printed schedule of the seminar talks at Oxford. He was giving a talk on what I was working on, I better go! Oxford and Cambridge are eighty miles apart as the crow flies, but it has always taken three hours however you do it. I took the three hour bus ride and stayed overnight with Richard Haydon. Anyway, there is a natural conjecture that "asymptotically exchangeable" means you can couple things in a strong way, but after thinking about it earlier, I had realized there was a simple counter example. So John gives the lecture and ends up with this strong conjecture which I know is false. He was the most famous probabilist in Britain and I could have gone up to him and said I know this is wrong and start scribbling on the blackboard. But I was a little too shy (hard to imagine me being shy and retiring), so I said nothing then but carefully wrote out the example by hand, and asked Richard to check it and pass it on to Kingman. John was very gracious and wrote back saying thank

[3] The quote from Freedman (2012): "When I started writing, I believed in regular conditional distributions. To me they're natural and intuitive objects, and the first draft was full of them. I told it like it was, and if the details were a little hard to supply, that was the reader's problem. Eventually I got tired of writing a book intelligible only to me. And I came to believe that in most proofs, the main point is estimating a probability number: the fewer complicated intermediaries, the better. So I switched to computing integrals by Fubini."

you, and we stayed in occasional contact. Undoubtedly he was influential in my getting the job at Berkeley.

Anyway in those days exchangeability was this obscure topic within probability. Some textbooks would have de Finetti's theorem, but they did not do anything with it, an old curiosity accompanied by some phrase saying there are these people called Bayesian statisticians who care about this. My work involving exchangeability on the subsequence principle and on the Banach space problem were published as Aldous (1977) and Aldous (1981a), but, by virtue of closing off the topics, have faded from anyone's notice.

Shankar: Seeing pictures, and reading about the history of the Cambridge statistical lab during your time there, some of the most well known researchers of our time seem to have gone through it while you were a student and an eventual postdoc—Frank Kelly, Brian Ripley, Bernie Silverman, Andrew Barbour, Jim Pitman .... Were there people around that time who were influential in your development?

David: There were lots of subsequently famous people at the same time. I describe it as a golden age for probability and statistics at Cambridge. The first four people above were recent Ph.D. students at Cambridge, and this was amongst only four students per year in probability and statistics! Of course I have talked with and collaborated with Jim Pitman throughout my career—more later. But here's a story you can't find in print.

In terms of research, the most important little seed came from a statistics graduate student called Kevin Donnelly (not to be confused with Peter Donnelly). Kevin was doing statistical genetics, back then before molecular data came along it was a classical applied probability topic. He was looking at questions related to identity by descent of genes of different individuals in a population. Toy models of these involve random walks on the vertices of a hypercube, asking for the probability that two random walks on a low dimensional hypercube meet within a few steps. That is a finite calculation which you can do by hand, exploiting symmetry. But then being a mathematician you start thinking what if the dimension of the hypercube goes to infinity? If you have a stationary process, under some sort of asymptotic independence, when you look at rare events like hitting times, they have to look like a Poisson process, which means that the first hitting times have approximately exponential distribution. This one example became a starting point for my work in two different areas. First, you realize that when you want to quantify asymptotic independence, you need to quantify some notion of *mixing times* of Markov chains. Second, the argument above became a canonical use of the *Poisson clumping heuristic*.

All just from one conversation with a fellow graduate student! This became one of the pieces of advice I gave to incoming graduate students at Berkeley: talk to your fellow students! In Cambridge we were physically close. In the small lounge there was a designated morning coffee time (instant coffee), and a tea lady brewing tea and cookies for the afternoon tea time, so you saw most people most days. This would be true then in most places, but I fear that this sense of closeness has largely gone away nowadays.

Shankar: What about the faculty? Did Hardy's description of working for four hours in the morning, and then playing cricket in the afternoon carry over?

David: No no, faculty were busy but not unduly busy. Nowadays everyone seems stressed by our electronic toys. Back then, faculty were busy but seemed happily busy rather than being overwhelmed with stuff. The eight-week terms were intense but from the faculty point of view you were engaged with students intensely but only for half the year. We had a social group that played tennis in summer but with English summer that didn't happen every day.

Incidentally, it took me many years to discover that I was an academic great-grandchild of G.H. Hardy—Ben Garling's advisor was Frank Smithies, whose advisor was Hardy. I commemorate by giving my graduating Ph.D. students a party and a copy of *A Mathematician's Apology*, and some vintage port.

Shankar: Your early work was relatively "abstract," dealing with properties of subsequences of Banach space valued random variables. Then came your groundbreaking paper on stopping times and tightness (Aldous, 1978) which also seemed to be undertaken during your Ph.D. Can you tell us a little bit about how you started working on this?

David: A big topic in the 1970s was the Dellacherie–Meyer style of analyzing continuous-time processes via martingales. I had learned weak convergence from Billingsley (Billingsley, 1968). Of course the basic example was random walks converging to Brownian motion; then generalizations to mixing processes but Billingsley (Billingsley, 1968) had only a cursory reference to martingales. He established tightness from the basic definition of compactness, which seemed rather clunky. Conceptually, the condition for tightness in Skorokhod space $D[0,\infty)$ is that you don't have two jumps close together. Then you see the idea that, for for a sequence of martingales $\{X_n(\cdot)\}_{n\geq 1}$, if this sequence was not tight in $D[0,\infty)$, then there would be a stopping time (after a jump) with the property that you can partially predict an imminent jump, and that just leads to the condition in the paper.

I also thought hard about connections between weak convergence and the "filtration" setup in Dellacherie–Meyer. This led to a long manuscript *Weak convergence and the general theory of processes* formalizing weak convergence of processes regarded as pairs $(X_n(t), \mathcal{F}_n(t), 0 \leq t < \infty)$. This was in fact the second half of my thesis, but never published. I interacted a little with David Kreps in Cambridge in the late 1970s, when he was doing the "martingales and arbitrage in continuous time" work (Kreps, 1981) that became foundational theory in modern mathematical finance. I joke that I "narrowly escaped" getting into finance at that time.

Shankar: I have seen you quote this amazing phrase of David Blackwell a few times "Basically, I'm not interested in doing research and I never have been ... I'm interested in understanding, which is quite a different thing. And often to understand something you have to work it out yourself because no one else has done it." ...

David: I just thought of it as our job is to find interesting problems and solve them. I was trying to find general principles, there were these general ideas around exchangeability, or stopping times and tightness or mixing times while avoiding abstractions for the sake of abstractions.

Shankar: Soon after this, you started working on Exchangeability, in particular proving what is now known, as the Aldous–Hoover representation for partially exchangeable arrays. Can you tell us about how you got interested in this?

David: That was an interesting example of several groups of people for different reasons thinking about the same question in the late 1970s, and an example of "why wasn't this done a long time before?" *This* meaning analogs of de Finetti's theorem, characterizing the structures with given invariance-of-distribution properties. Phil Dawid, a Bayesian statistician, had a version of this for matrices (Dawid, 1981). Sometime in the last few weeks I heard this phrase "a matrix is an array with benefits," so matrices have additional structure suggesting stronger exchangeability-like conditions involving symmetry by multiplication by matrices, which tie things down quite narrowly. So then I thought, take away the benefits, what if you just had an array? It doesn't take you long to guess a general way of constructing such arrays and then convincing yourself this has to be the only way. It turned out that a bunch of other people were thinking about this including Kingman and especially Kallenberg, the one person who had been thinking systematically about exchangeability in the 1970s studying, for instance, processes with exchangeable increments. Then there were also Persi Diaconis and David Freedman, with Persi being interested in Bayesianism, and finally I saw the preprint by Hoover! So there was this whole bunch of people who pretty much independently had started thinking about the same topic. Hoover did it first in his technical report via logic, which none of us understood. I had a complicated proof and typed it up and sent it to John Kingman. He then simplified it by embedding the array in the plane, which made everything conceptually much simpler. That became the published version (Aldous, 1981b). Kallenberg then tidied up the uniqueness part, which is rather subtle. So I always call this the Hoover–Aldous–Kallenberg theory. FYI the cleanest version of the proof I have seen is in Tim Austin's lecture notes (Austin, 2012).

Shankar: And all this led to your St. Flour lecture notes on Exchangeability (Aldous, 1985)?

David: In Berkeley, after the first year graduate courses there were topics courses where faculty could teach anything they wanted. Every two years or so throughout my career I taught that type of course—it was one of the most rewarding aspects of Berkeley. When there is some new topic you want to think about, you have to read up on it. I was always a bit bad at reading what had been done, so the only way you learn new stuff is signing up to give a course on it. Most of my research topics are ones where once I realized I was interested in this I said I was gonna give a course on this and maybe write a survey paper. St. Flour was arranged a year or two in advance, so I first taught the course at Berkeley and then wrote up the notes. Anyway the organizers were pleasantly surprised when I showed up and said here is my writeup, take it away. I volunteered to give my first talk in my excruciatingly bad French, after which the organizers begged me "no don't ruin our language any more."

Shankar: And subsequent developments?

David: Well, for 15 years the subject rather died, except for further work by Kallenberg. I recall writing referee comments along the lines of "alas no one seems to care now, but symmetry is a central concept in mathematics and so these ideas will resurface sometime." And then the characterization result was rediscovered in the special case of graphons as dense graph limits in the mid 2000s (Diaconis and Janson, 2008, Lovász and Szegedy, 2006). And also these exchangeability-like models became popular in Machine Learning via Bayesian nonparametrics. The upshot is that, because people who use exchangeability feel obliged to cite something, the St. Flour lectures are my most cited publication, being cited today in papers with titles like *Hippocampal remapping as hidden state inference* which I know absolutely nothing about.

Back to my own work, I had previously studied exchangeability in the contexts of the subsequence principle and Banach space embedding, where the work was technically hard and "closed down" a small topic. In contrast, Hoover–Aldous–Kallenberg "opened up" a topic. Perhaps unconsciously learning a lesson, the later works I am known for are contributions toward "opening up" a new topic, rather than solving some capstone conjecture in an established topic.

5. THE ROARING 80S AND 90S

Shankar: In 1977, you started a three year postdoc position in Cambridge, but then moved to Berkeley in 1979.

David: So if you have a three year postdoc, then in year two you can apply for one or two jobs that you really like.

I applied to Berkeley and Vancouver because I knew people in both places; Jim Pitman had just moved to Berkeley from Cambridge, and Cindy Greenwood from Vancouver had just spent a year visiting Cambridge. So Berkeley and Vancouver got together and paid to visit both places on the same trip. I gave two talks in Vancouver and they didn't offer me a job and one talk in Berkeley and they did. So not only have I had only one job in my life but also only one *job offer* in my life, probably strange by today's standards. Berkeley had a world-famous reputation with the faculty then, so I took the job. It was just one thing leading to another without any conscious planning.

Shankar: I think this is important, for junior researchers to hear, as it is often not possible to plan every aspect of one's career or future, and I keep telling them most things are not in our control. Can you tell us about the atmosphere amongst faculty and graduate students?

David: When I arrived in 1979, there was a fairly regular group having their sandwich lunches in the coffee room. In particular, there were three spots reserved (by custom) for Jerzy Neyman, Lucien Le Cam and Betty Scott. Woe betide anyone who sat in one of those chairs! There was a certain deference to the old people! Betty Scott in particular was a den mother. Another part of my speech for incoming graduate students way back was "don't be upset if Betty Scott treats you as a five year old, she treats all the faculty like five year olds" (Shankar cackles uncontrollably). I remember being in the coffee room and leaning back on my chair and being firmly told not to do that.

Shankar: How was the graduate student life in Berkeley when you got there?

David: The 1970s were still a laid back time to be a graduate student. Berkeley nominally had zero tuition and there was money around and there was still the 1960s feeling that one could be a graduate student indefinitely if one didn't mind not having much money. There was no hurry to get out and be an adult. I occasionally thought of saying "by the time he was your age, Alexander the Great had conquered the known world, and died."

Shankar: Berkeley at that time had some of the great names (David Freedman, Jim Pitman, David Blackwell etc). What was the atmosphere like?

David: Well it still had a "small department" feeling, and a large cohort of well-known mid-career faculty as well as the famous older ones. There was a Tuesday "Department colloquium" which everyone attended, and a Wednesday "Neyman seminar" which Jerzy presided over in his avuncular authoritarian style. I can't convey atmosphere very well, but the "Conversation with Jim Pitman" article has some. I do remember thinking of David Freedman as Mycroft Holmes,[4] to be bothered only to solve the most intractable problems.

[4]Sherlock's smarter older brother.

Shankar: It seems like around this time, you started publishing papers on what is now referred to as Mixing time of Markov chains. You described one motivation earlier .... Can you tell us a bit more and also about the magnum opus (Aldous and Fill, 2002)?

David: Well, as with exchangeable arrays, this is another case where it's incredible that people hadn't thought systematically about a topic earlier. Part of the reason was surely that, in mainstream mid-1970s mathematical probability research, finite Markov chains were regarded as a dead subject. When at Cambridge I mentioned thinking about mixing times, the comment by someone was "you are brave to do that," which is the British way of saying you are a damned fool to do that. But in the late 1970s, several people independently starting thinking in different contexts. For me, the earliest motivation was the exponential approximation for hitting times. Then the observation of the cut-off phenomenon for random walk on the hypercube. Then the use of coupling times. Then the analysis of mixing time for the riffle shuffle, getting the correct first order asymptotics $\frac{3}{2}\log_2 n$ (justifiably overshadowed by the later famous "7 shuffles suffice" work of Bayer and Diaconis (1992)). These were recorded in Aldous (1983). Of course others were thinking of other contexts, such as Markov Chain Monte Carlo methods for sampling and other algorithmic methods, and the iconic (Diaconis and Shahshahani, 1981) analysis of shuffling by random transpositions.

As with exchangeability, I realized that studying finite but large state Markov chains was an interesting topic, but it wasn't clear what in the classical literature—for instance the matrix-based treatment by Kemeny and Snell (1960)—was relevant. So I planned to teach a course and write up notes for myself and the course, and that is what grew into the notorious unfinished manuscript (Aldous and Fill, 2002). Of course the field itself is now huge, with the excellent introductory account of Levin and Peres (2017). And our unfinished work, posted online for many years, has become my second most cited work. I repeated the course several times over the years, including once at Stanford.

Shankar: Another great name in this area is Persi Diaconis. You have of course, written a number of fundamental papers (Aldous and Diaconis, 1986, Aldous and Diaconis, 1987) with him, and have often mentioned his influence in some of your other work, including the so called Aldous–Broder algorithm (Aldous, 1990). Can you tell us a little bit about his influence, in founding the field (of mixing time of Markov Chains), and generally your own relationship and friendship with him?

David: A couple of months after arriving in Berkeley in 1979 there was a Berkeley–Stanford joint colloquium. Those were a much bigger deal back then, lots of people went from one place to another and you had a party, maybe at someone's home. And Persi came up to me and said Hi! I had previously corresponded with him about exchangeability—I don't remember if I had thought about the riffle shuffle before or after talking to Persi.

I already mentioned a bit about how the mixing time field got started, that by the end of the 70s a bunch of different people were thinking about it for different reasons. Persi's paper on random transpositions (Diaconis and Shahshahani, 1981) using group representation theory was probably the first time pure mathematicians recognized this as "serious math." Persi continued working with random walks on groups—see the delightful short monograph (Diaconis, 1988)—as well as the "strong stationary time" methodology appearing in the joint papers you mentioned. You can read his own account in Aldous (2013a).

Regarding our relationship, the way I say it is that Persi is one of the two or three people I have regularly worked with, whereas I am one of the 57 people he works with (laughs)! In network science terminology, Persi is a hub. We have not written so many joint papers but we have talked regularly forever and he has certainly been a great influence. There is a notion of the right distance between the way you think and your collaborators, you don't want to talk to a clone of yourself but communicating with someone who thinks or is interested in completely different things is also hard. So Persi and I were somehow the right distance apart. A good instance was our work on the length L_n of the longest increasing subsequence of a uniform random permutation. A well known paper (Hammersley, 1972) showed, via subadditivity and a construction using a two-dimensional Poisson point process, that $n^{-1/2}L_n \to c$ for some constant c, and it was later proved by combinatorial/analytic methods that $c = 2$. But is there a simple explanation of 2? It turns out that one can re-interpret part of Hammersley's construction by taking one axis as space and the other as time, and now one sees an interacting particle process on the line. In this representation one can quickly see (heuristically) that $L_n \approx \ell(n^{1/2}, n^{1/2})$ where ℓ satisfies the PDE $\frac{\partial \ell}{\partial t} = 1/\frac{\partial \ell}{\partial x}$ which has solution $\ell(x,t) = 2\sqrt{tx}$. The constant has to be 2! This was formalized in our paper (Aldous and Diaconis, 1995). Separately, Persi had his *patience sorting* algorithm for L_n and we had some analysis which was interesting but maybe not so substantial, so we set it aside. Later the seminal[5] paper (Baik, Deift and Johansson, 1999) appeared, which gave us an opportunity to fold our discussion of it into a short survey paper (Aldous and Diaconis, 1999). A rare instance of procrastination paying off!

Shankar: So now we're up to the mid-1980s. What was going on then?

FIG. 3. 1986 *wedding*: *Persi Diaconis* (*best man*), *David Aldous*, *Katy Edwards*, *Martha Edwards* (*maid of honor*).

David: Well, I got married to the delightful Katy Edwards, and Figure 3 includes a photo of Persi that's rather different from those you can find on Google Images.

Shankar: Amazing! I did not know this. In terms of research, it seemed like this era, was bookended by your book on Poisson Clumping Heuristic (Aldous, 1989). The genesis for this, was the same as what started you thinking about rapidly mixing Markov chains .... How did you go about writing this book?

David: Yes, a basic example is hitting times of rare sets for a Markov process. The insight is that many superficially different contexts involve locally dependent rare events, and so the very familiar Poisson approximation can be replaced by compound Poisson approximation. The book is essentially just a hundred back-of-the-envelope calculations and a little language to go with it. So the actual *writing* was easy, in that I just collected all the examples I could think of. Because there weren't any theorems it gave me a reputation for eccentricity in the theorem-proof world which is fine by me! The rigorous Barbour, Holst and Janson (1992) came out round the same time, and fit in well with Aldous (1989). For the rest of my career, maybe once a year at a meeting someone would come up to me and would say you don't know me but I have used stuff from Aldous (1989). As with previous projects, on Google Scholar one can see citations from distant fields, such as *Ternary gradients to reduce communication in distributed deep learning*. I taught this once as a Berkeley topics course, and also at Cornell on a sabbatical, but frankly it isn't a great topic for an actual lecture course.

Shankar: The 90s were the start of your work on limits (scaling limits, local weak convergence) of random discrete structures, including the well known continuum random tree trilogy (Aldous, 1991a, Aldous, 1991b, Aldous, 1993). How did this start?

David: I had never taken a course or read through a book on combinatorics. Some time in the mid 1980s I was in a conference with Mike Steele who told me about

[5]As an early occurrence of the Tracy–Widom universality class.

Prüfer code. This is a bijection between sequences and unrooted trees on n labelled vertices which therefore allow you to construct a uniform random tree from $n-2$ throws of an n-faced die. But this doesn't allow one easily to see properties of such random trees. By a strange chain of lateral thinking there is another construction method which is less efficient but is much easier to visualize (Aldous, 1990). And it's intuitively obvious, if you know the Birthday Problem story, that using this construction and drawing edges of length $1/\sqrt{n}$, there should be some $n \to \infty$ limit structure. This led to a first construction (Aldous, 1991a) of what we now call the Brownian CRT. Next one can connect this to more general tree models using Galton–Watson trees (Aldous, 1993). The final piece was Le Gall's idea (Le Gall, 1993) of seeing that the limit object could also be coded through Brownian excursion, and the overview paper (Aldous, 1991b) put it all together. At the time I wondered if anybody would ever actually care about this, it is so abstruse (laughs). Around 1990 I gave a talk in a conference in Durham U.K. which was the first time I had spoken about this and I didn't get any reaction at the time. As a history-of-technology aside, the talk was written in SliTeX, basically LaTeX formatted to fit a page, to be printed out and then copied onto a plastic transparency sheet, after I had hand-drawn figures. You can see the slides (without figures) at the link Durham 1990 slides.

Anyway the original chain of thought started with what is now called the Aldous–Broder algorithm, actually arising from our two separate conversations with Persi Diaconis. This uses random walk on a graph to construct a uniform random tree in the graph. In Aldous (1990) I was not thinking about this as an algorithm but as a way to prove things about uniform spanning trees via random walks on the corresponding graph. Applying this algorithm to an i.i.d. Uniform$[1, 2, \dots, n]$ sequence, that is, random walk on the complete graph, which one might think would be some trivial case, is what actually leads to my original construction method for the uniform random tree.

Shankar: As an aside, it seems that much of your career, you have developed fundamental theory, that has had ramifications in an array of fields. Perhaps in slight contrast, in this era, you solved one of the most well known problems in probabilistic combinatorial optimization, establishing that the random assignment problem has a limiting value (Aldous, 1992), whilst eventually also establishing the conjectured $\zeta(2)$ limit (Aldous, 2001). I am cheating of course, since part of the resolution of this, leaned on the general theory of local weak convergence (Aldous and Steele, 2004). Can you tell us how all this transpired?

David: This also goes back to Mike Steele explaining the problem to me. The central idea is an analog of the fact that critical Erdős–Rényi random graphs look locally like critical Poisson Galton–Watson branching processes. The present context involves bipartite random matchings where the local limit is the Poisson Weighted Infinite Tree (PWIT) and you realize intuitively that the limiting value is the average weight-per-edge of the optimal matching on the infinite tree. The issue was to make sense of that. The formalization in the first paper (Aldous, 1992) is horrible and so people have not taken up this way of doing it. The second paper (Aldous, 2001) was making sense of the conjectured $\zeta(2)$ limit in Mezard and Parisi's work (Mézard and Parisi, 1987) which I just could not understand. It all reduces to a three line heuristic but it takes a while to say what the three lines are! This is another conceptual point for students: any theorem whose conclusion is a formula, there is almost always a three line argument for where that number comes from. Some authors are good at explaining these heuristics in the introduction although others aren't.

Local weak convergence is now appropriately known as Benjamini–Schramm convergence as they had the first interesting theorem on this, concerning the recurrence of random walks on limits of planar graphs (Benjamini and Schramm, 2011). Our version was slightly different as we had it for weighted graphs as opposed to unweighted graphs where one can allow infinite degrees as long as it is locally finite as measured by the distance induced by the edge lengths. I sometimes now say "Networks are graphs with benefits" with the benefits being weights on edges and that should be the default definition of *network*. There should be an introductory textbook on networks starting at this level of generality, which takes for granted very basic graph theory and linear algebra and develops a first course in networks from this. With a view towards applications, by starting with a long list of specific questions about specific real world networks, to motivate the book. When we are writing books we think about methodologies as opposed to real world questions we are trying to answer.

Shankar: The 90s ended with your work on coalescence and fragmentation processes, including what I consider a paper full of hidden gems, your paper relating critical random graphs to stochastic processes like Brownian motion. How did this work start?

David: Part of the continuum random tree work was that you could code trees by walks, an idea which goes back to Harris. It was same idea for critical Erdős–Rényi random graphs, where components should look essentially like critical random trees. So I started thinking about exploration processes of the critical Erdős–Rényi random graph and literally it just worked. Normally you guess a theorem and then think about how to prove it, here it was the other way around, you start off with the proof. Basically here is the exploration process; what does it converge to? Scale like a random walk and it converges to

Brownian motion with parabolic drift. The end! There was zero effort in writing down the basic theorem and the issue was could you calculate anything with the limit. Previous papers like Janson et al. (1993) knew of the critical scaling window and that sizes of components scaled like $n^{2/3}$ in this window. But these came out of combinatorial calculations without the "probability" observation that there was only one way to get a nontrivial scaling limit. The resulting connections to the multiplicative coalescent led to a good thesis project for Vlada Limic (Aldous and Limic, 1998).

Shankar: Soon after this you started working on the probabilistic foundation of coalescent processes. Can you tell us a little about this arc of your research?

David: There were three different inputs. I knew about the Kingman coalescent which is a very natural way of looking at neutral evolution in population genetics. The finite system starts with n objects and each object wants to merge with any other object at rate one, and then clusters merge with any other cluster at rate one. Since the total time for the system to go from n clusters to one cluster is uniformly bounded for any n, a natural guess is that a limit for this system exists as $n \to \infty$ and Kingman gave a description of the limit. At some point I came across statistical physics literature on coagulation and fragmentation of molecules, where for example clusters of size x and y merge into a single cluster of size $x + y$ at a rate governed by some kernel $\kappa(\cdot, \cdot)$. This field goes back to famous work of Smoluchowski (1916). They were looking at deterministic $n \to \infty$ limits for relative cluster sizes, and one gets infinite collections of differential equations. Separately there was a small literature on stochastic versions of these models. As an aside, that literature suddenly seemed to stop in 1984 when the DLA growth model was introduced, which gave fractal growth and rapidly generated a huge literature. Anyway, for the stochastic model they had explicit solutions for the three most concrete functions of two variables: $\kappa(x, y) \equiv 1, x + y$ and $x \cdot y$ and in no other context. I look at this and say well $\kappa \equiv 1$ is just the Kingman's coalescent and $\kappa(x, y) = x \cdot y$ is just the mergers of components in the Erdős–Rényi random graph processes, which in the limit becomes the multiplicative coalescent process (Aldous, 1997). So I decided to think both about the general case and the specific cases. The additive coalescent which I worked on with Jim Pitman (Aldous and Pitman, 1998) was less apparent than the other cases. It turned out that if you started with a random n-vertex tree and started cutting edges, this process in reverse time (where you have subtrees joining up) is exactly the additive coalescent.

So this was enough material for the survey paper (Aldous, 1999). The topic was maybe too specialized for an entire Berkeley course but I did give 4 lectures at the Fourth Brazilian School of Probability in 2000.

Shankar: There were a number of collaborations during this period with Jim Pitman, many of these described beautifully in his St. Flour lecture notes (Pitman, 2006). Can you tell us a little bit about your collaborations with him over the years, both in research, and in initiatives like the Probability Surveys journal?

David: Well over our career Jim and I have 13 joint papers, as well as a lot of parallel works on the general topic of continuous limits of discrete structures. The "standard additive coalescent" paper (Aldous and Pitman, 1998) mentioned above is perhaps the most elegant. We also had three papers with Grégory Miermont (visiting as a post-doc) extending the continuum tree framework from trees to random mappings. This line of work (Aldous, Miermont and Pitman, 2005) has not been much continued subsequently, but we were impressed by Grégory's formidable technical skills and so were not surprised by his subsequent stellar career. Regarding Probability Surveys, this arose from Jim's advocacy of open access publication, and I volunteered to be first Editor. It's a little disappointing that it now seems to attract only 6 or 7 papers a year, and I don't know whether this reflects a dearth of surveys being written, or availability of other venues.

Shankar: Is it true that you spent months holed up in the physics library at Berkeley when writing your survey paper on stochastic coalescence (Aldous, 1999)?

David: Well it seemed like that! Before MathSciNet and Google Scholar it was necessary to use the library. Instead of Google Scholar there was the printed Science Citation Index. There was a very long survey paper (Drake, 1972) where one could see earlier results and then use the SCI to find subsequent papers on the topic. But outside mathematics, typically citations *do not include the title of the paper* so locating the few "stochastic model" papers amongst the mass of ODE papers was a real chore.

6. THE NEW MILLENNIUM

Shankar: The 2000s witnessed another switch in your research interests, to the world of "complex networks." In particular, you seem to be motivated by thinking about, general principles that can apply, to a wide array of network models, as opposed to specific case by case analysis of networks.

David: In retrospect my activities focus changed around 2000, though not from any deliberate plan. Previous research was "merely theorem-proof mathematics" motivated by intellectual curiosity. Around 2000 I started the "probability in the real world" (which I'll talk about later) project for teaching, which then encouraged me to consider models more closely relating to easily-obtainable current data.

On the research side, networks have been a recurring theme, though with no major "success" in the strong sense of contributing to opening up a new field. Ironically, what's most widely known is a failure. I claimed to have a proof of what is now called the Aldous–Lyons conjecture: that every unimodular random graph is a local weak limit of finite graphs. So the proof collapsed, but inspired Russ Lyons to write the extensive and authoritative paper (Aldous and Lyons, 2007) to which I made no actual contribution. Incidentally, everyone except me thinks the conjecture is false.

I should mention your own thesis work (Aldous and Bhamidi, 2010) on flows through the "mean-field" network. I was thinking of a range of such "flow" problems but didn't get far, on my side. On the other hand this topic did lead to your own extensive work, with Remco van der Hofstad and others, on locally tree-like random graphs.

There were several different papers on random trees motivated by phylogeny, but let me mention just one, the thesis work of Lea Popovic (Aldous and Popovic, 2005). One can take the critical binary branching model for species, started at a past time with the (improper) uniform prior on $(-\infty, 0]$ but then conditioned on n species at present time 0. It turns out that this has a nice description in reversed time which enables explicit calculations.

My own main focus has been on toy models for spatial networks, meaning those embedded in two-dimensional space such as physical transport and distribution networks. A Google Scholar search on "spatial networks" reveals the long survey paper (Barthélemy, 2011), by the statistical physicist Marc Barthélémy who also has a subsequent monograph (Barthelemy, 2018). There's a lot of math but almost no actual theorems! So I have spent a lot of time hunting for theorems, but have only come up with some fragments of theory. In considering optimal networks on random points (e.g. an inter-city road network) choosing the appropriate optimality criterion is rather subtle—see Aldous and Kendall (2008) and Aldous and Shun (2010). Similarly, finding optimality criteria for subway networks which qualitatively match real data proved to be very difficult—see Aldous and Barthelemy (2019).

A different approach, more in tune with modern mathematical probability, starts from the observation that the average distance between two U.S. cites is about 18% longer than straight-line distance, and this average is true over a wide range of distances. This is a weak kind of "self-similarity," and motivated study of strict "exact self-similarity of a network" models, which are necessarily in the continuum—there is a defined route between a.a. pairs of points in the plane. There is a class of such models (as Shankar noted in his question) though apparently no specific fundamental example. This approach is outlined in Aldous and Ganesan (2013); I haven't been able to make further progress but it is one of several open topics that I hope others will study in future, as a counterpart to the study of geodesics in first passage percolation.

I gave a topics course on spatial networks in 2013, but my knowledge was too fragmentary to write a survey paper.

Shankar: Where do you think probabilists can be most beneficial in the general world of "network science"?

David: I've been heard to say sarcastically that since 2000 "everyone and their dog" has devised a random network model and claimed some relevance to data—including myself in Aldous (2004). There has been excessive attention to studying basic statistics of a random network—degree distribution, diameter, etc. Real world networks are typically designed to do something specific, so one should try to study processes over networks. So the research direction I advocate is to start with some toy model of a process, but then try to study its qualitative behavior on general networks, rather than quantitative behavior on specific network models. This has been done to some extent with the well-known examples from what probabilists call *interacting particle systems*—voter model, contact process, etc. I have recent work for instance, Aldous (2016a) which basically says: take an edge-weighted graph, set up bond percolation (each edge appears at a random exponential time with mean given by the edge weight). Then there is a critical time for the emergence of a giant component under almost no assumptions on the topology. And there's an analogous result for for first passage percolation in Aldous (2016b). Surely there are other toy models to study.

Shankar: In 2017, you chartered the USS Potomac for a cruise around the San Francisco bay. Frances (my wife), and I were super honored to be invited, and we had a great time during the entire excursion. This was where, I first officially heard you saying you were planning to retire from Berkeley. Can you tell us a little bit about how you came to this decision?

David: *Jump before you're pushed!* As I've mentioned several times, I have enjoyed periodically engaging in new research topics to the extent of signing up to teach a course, thereby forcing myself to read systematically about the topic. The last time I gave such a new course was in 2013, and realized that I was running out of enthusiasm for serious research, so it was time to make way for younger people. Separately I had agreed with Katy, who had lived in the Bay Area all her life, that we would move to the Seattle area while still young enough to enjoy an active life.

7. PROBABILITY IN THE REAL WORLD

Shankar: Since we are talking about special topics courses, you also have "Probability in the real world" seminar course for advanced undergraduate students, that seems to cover a wide array of interesting topics . . . .

David: Berkeley had this course STAT 157 listed as a junior/senior seminar course in statistics where you could teach anything you liked. Sometime around 2000 I was passing the then-chair in the hallway and they said you have never taught one of these courses, so why don't you do one? And I said, without any forethought, let me teach one on probability in the real world, without ever seriously thinking about it! I guess a year in advance you can say anything. So just like before (joining Berkeley etc), nothing was planned, it just happened, just unpredictability in real life. I slowly realized that in our internet era there is so much data that is gettable, so why teach any idea that you can't illustrate with interesting recent data? Get away from the stereotypical examples of games of chance, opinion polls etc. So the plan for the course was 20 lectures on 20 different probability topics, each anchored by some explicit recent data. Much harder to do than I originally imagined! You can see what I actually did on my web site.[6] Students had to do course projects. For the projects, I had some suggested ones but said I preferred if they came up with their own project. They devised some amazing projects, especially from those interested in sports like baseball. There is enormous amount of data in baseball—for every hit, the angle and velocity off the bat—and students with access to this sort of data did wonderful things.

Shankar: This led to supervising lots of undergraduate research projects as well . . . .

David: Yes, I taught the course every three years or so, but in other semesters I was available to supervise undergrad projects, but again preferably projects devised by the student. It was fairly low key, but once it got going there were maybe five or six students per semester, and I had a special office hour for them to drop in. Part of my goal was to show them how hard it is to do novel research with real data. The best were students from computer science and I had them writing code leading to several joint papers where they did all the coding. For many years my joke was if you can't simulate it in ten lines of Fortran I learnt in 1969 then it is too hard to understand anyway! Though six years ago I finally decided to learn basic Python. Back to undergraduate students. Berkeley had a work–study program, and normally these students might have some boring clerical job but Weijian Han just knocked on my door and said can you give me something more interesting to do. He was the only one with the initiative to do this, and he was extremely helpful.

The course led to a number of other things. On the research side it got me into considering prediction markets and prediction tournaments, since these and gambling odds are the main accessible contexts where the data is actually probabilities. I participated in IARPA sponsored prediction tournaments for several years. It's a remarkable insight that, even for unknown probabilities of different real-world future events, one can tell who is relatively better at estimating probabilities, in the long run—see Aldous (2021). And from prediction markets, in which prices reflecting consensus probability should (according to theory) behave as martingales, one can test the hypothesis that they actually do behave like martingales, in particular via what I call the *serious candidate principle* (Aldous, 2013b) for maximal perceived probabilities of the different contestants in an election or other contest

In a different direction, queueing theory is a classical topic in applied probability, but I could never find data for students to compare with textbook models, until thinking of a "long waiting line at airport security" model. If you are the 170th person in line, then even if people at the front are processed in fixed time, the "wave of motion" reaches you less often and you move forwards several positions when the wave reaches you. There's a simple model, which turns out to be analyzable via my favorite method, which is "a picture that explains everything (intuitively)" (Aldous, 2017), and which fits data reasonably well. One can code peoples trajectories in a clever way, so that the graphic looks like coalescing random walks, except that the space/time axes are switched. This suggests there must be an $n^{1/2}$ scaling for times/distances of moves by the n'th person in line, which turns out to be true but requires substantial work to prove.

Shankar: You have an extensive set of reviews on nontechnical books in probability. Why did you start this? Aside from the valuable service to the community, how do you organize your own mental picture of this "space"?

David: As usual it was one thing leading to another. I had started collecting nontechnical book on the "probability in the real world" theme in order to find topics to lecture on. A student's main responsibility was to do a course project. But students procrastinate and so I had to get them to do something right near the start. So I had the students give a six minute talk to the class in the fourth week, on something vaguely related to the course and the real world that they found interesting. That is hard to do so I suggested students could look for a topic by reading one of these books, and indeed I started wheeling these books to class in a library transport cart for them to browse. Student talks when I started in 2000 were blackboard and chalk but rapidly transitioned over the next years to Powerpoint. The book reviews were initially 3-line reviews for the students. Eventually I thought I might as well post them on Amazon as well as my home page.

Related to book reviews and undergraduate research projects, I was recently reading a really good book, *The Rules of Contagion* (Kucharski, 2020) by a British epidemiologist, which came to press just as the pandemic was starting last year. It does not have mathematics but it is as serious as you can get about epidemiology without doing the math. The material in the book suggests a

[6] https://www.stat.berkeley.edu/~aldous/157/index.html

host of possible student projects, such as contact tracing under a budget constraint. By a weird coincidence, the first monograph I had read as an undergraduate (for an essay contest) was the foundational account of the mathematics of epidemics (Bailey, 1957). Anyway Kucharski (2020) has a nice history of epidemiology, and memorably wrote that Bailey (1957) was a complete disaster for the subject because it did the math in detail but did not emphasize fundamental concepts such as the reproduction number R_0. Of course the real world is complicated and it is hard to say precisely what R_0 is, mathematically, so it was apparently ignored in our community until being emphasized by the ecology community in the 1980s. The moral being that the mathematician's instinct, to not say anything if it can't be said precisely, can be counter-productive. This story motivated me to write an extended book review for mathematicians, which will appear in *AMS Notices*.

8. YOUR APPROACH TO LIFE AS A RESEARCH MATHEMATICIAN

Shankar: Your career has been interspersed with relatively major directional changes, where you made fundamental contributions in a given area, and then essentially completely switched gears to a new area, e.g. from exchangeability, to mixing time of Markov chains, then random discrete structures, then coalescent theory etc. I remember, as a graduate student, giving you this book abut the Nobel prize winning physicist Chandrashekhar (Wali, 1991) whose own journey reminded me of your career. Was this intentional?

David: There was never a plan to work on a topic for five years and then switch, but the plan was to find some not-yet-active topic, engage some research projects, teach a course on the subject and write a survey paper and then move on. It's about keeping your eyes open to lots of other possibilities. I have been heard to mutter that "one's intellectual horizons should extend beyond the narrow confines of the math library!" If you pay attention only to theorems then you think too narrowly.

Shankar: Is there general advice you would give, to beginning graduate students and/or junior faculty, about finding research projects etc?

David: There are generally four ways to find research problems other than "next step" (i.e., following a previous paper) problems. A first way is to look at sophisticated nonrigorous math done by (for instance) physicists, in our case statistical physicists, and try to make it rigorous. In general this is hard to do, but rewarding if you succeed. My one success of this kind was the proof of the Mezard–Parisi solution of the random assignment problem. A second way is find an applied area which is using less sophisticated math, and think about different techniques that could be used and different questions to ask. For me this was the stochastic coalescence topic from physical chemistry where I could make connections with stochastic processes. The third way, the "blue sky" or "external DLA" method, is to start with a problem that looks far outside what's feasible for anyone to prove in the foreseeable future. Then work inwards, following some version of Polya's quote *if you can't solve a problem, then there is an easier problem you also can't solve*. Maybe you eventually find a problem you can solve. Admittedly this usually doesn't get you anywhere; though curiously often, some old thought becomes useful in apparently unrelated problems you study later. The fourth way is "keep your eyes open and look around."

Next you need a firm list of "projects in progress," meaning you have a goal and have gotten started and written something down, and you guess a $>50\%$ chance it will eventually become a paper. You should have at least *one* but (when young) not *seven* such projects. What proportion of time should you be focusing on some "in progress" project? Certainly at least 50%, but I assert that young people typically spend too high a proportion. In your unfocussed time you are thinking of possible new projects via the ways above, reading widely, listening and talking to people etc. The simplest way to avoid over-focusing is to set a mental deadline: if you spend 10 hours on a project without progress, then take a break and switch to unfocussed activity.

Somewhat idealistically, I assert that your goal should be to publish one good paper a year. *Good* meaning of lasting interest in its own right or opening the door to a new topic. The only excuse for writing more is that you can't tell this in advance. This assertion needs qualification in several ways. First, I am talking only about theory. Different criteria apply to real world analyses. Second, the deplorable inflation in expectations for publication volume before tenure means you are de facto forced to write more before tenure.

Shankar: Can you tell us any rules you followed, in deciding on which of your solved problems you actually sent for publication, as well as the choice of journals? For example, I heard you say, if you could solve a problem in a week, you would type out the solution, but not send it for publication.

David: I was modest in the sense that I would send my work only to places appropriate for the quality of the paper—for example, only my best to *Annals of Probability* or PTRF. Over my career there has been a huge increase in the number of probability papers and to a smaller extent the number of journals, so there's a lot of choice. You want the journal editor to be happy to see a paper from you. Inevitably lots of papers are on the borderline and those are the papers that are hard on everyone. So if your paper is borderline for a journal then don't send it. Otherwise you're just adding work for everyone concerned. Regarding quickly-solved problems, it's a matter of judgment. If the solution would equally easily be found by an expert in the given area, then don't publish, whereas if you have some less familiar method then do publish.

Shankar: Operationally, were there general principles or rules you followed? For example, I remember you regularly spending time away from a computer, in a cafe working ....

David: I was slow to adopt a computer at home, and didn't get a laptop until 2011 when I started using slides for the probability in the real world course. So I was away from cellphone or email when I was away from the office. Also I'm a coffee addict, sometimes spending thirty minutes with a latte and free associating with little problems. Almost none of which get anywhere but do the math! If you do this twice a week, that is probably three thousand little problems in a lifetime you have thought about. It is surprising how often the "wasted time" thoughts come back to you as possible lines of enquiry for some current project.

My work has mostly involved going sideways as opposed to deep, considering something off on the side which people have not thought of and could combine different fields. It is amazing how things like the continuum random tree had not been thought of earlier, being so analogous to rescaling random walks to Brownian motion.

I reluctantly got a cellphone in 2018 when we moved to Seattle and we were Airbnb hobos for a few months. I am hugely inconsistent. I am a minimalist in the sense that I say I will only buy a new thing if it allows me to get rid of two old things, so from that point of view I should love a cellphone. But I hate it! I am conscientious about answering emails which means people expect you to do it, but then with cellphones people expect you to be available all the time. I joke that on some distant planet the aliens are proud of their latest invention in making humans stupid, first no one can do arithmetic in their head, then no one can spell, and then no one can get between two points using a map ....

Shankar: Over your research career, what do you think is the biggest change, in say research culture or expectations in our field? I don't know why, but this question reminds me of seeing this picture or coffee mug, in your office in Berkeley.

FIG. 4. *From Flickr by Nelleke Poorthuis. No changes made.*

David: I wasn't an empire builder in the sense of wanting a large group of students around me. Theorem-proof math used to be a very individual thing where you had a student and their advisor, but now there are big groups around famous researchers. When I started on the pure maths side, grads often didn't send out preprints till they had completed their thesis, and you would apply for a job based on your thesis and a letter from your advisor. Nowadays even for getting a postdoc you need to have papers, technical reports etc and things seem to have become much more stressful for junior researchers ... unless you are in data science where everyone is knocking on your door, meaning it's hard to get students to finish their thesis before they are whisked off to Silicon Valley. I once checked *Annals of Statistics* regarding papers in statistics over the last 30 years and it seems like they are now generally three times longer than before with three times as many authors. Academic life seems to have gotten much more busy and less social in the physical sense, with less time spent in activities like department tea etc. Especially as a junior researcher you are expected to write many more papers.

Shankar: Sitting atop the metaphorical mountain top, as you gaze at the expanse of research on probability below, what is your take on where this field is "going"?

David: I have not followed all the active deep topics, SLE, KPZ etc. Lots of smart young folks doing it and I have no intention of competing with them. These are very interesting mathematically, with deep structure and having distant connections to applications and this is evidenced by the number of recent Fields medals for probabilists. It is great that probability is viewed as much more central in pure mathematics, compared to when I started my career. However probability has moved away from real statistical or experimental data. My colleague Bin Yu has often commented sadly that the probabilists are not really engaging with mathematical statistics as they did 40 years ago.

Shankar: You have received (and continue to receive) many awards in your career, yet you also don't seem to take yourself too seriously. I have heard you sharing referee reports of rejections you have received, or contrasting your own (professional) life, to some of your friends, who have worked on things like the Good Friday Agreement[7] ....

[7]The major development in the Northern Ireland peace process of the 1990s.

David: Right, well it's just mathematics (laughs)! Hopefully nobody's life depends on your theorem being right or not. I am very conscious of having led a very privileged life. Being good in math high school meant one could go on to some more or less pleasant career doing math, and so there was a path before me and I had only to choose whether to walk the path or not. Most people don't even have a path before them and come out of high school without a definite option. And we get paid to do things we basically like doing!

Shankar: You have done a lot of service to the community, ranging from editorial and reviewing work, for instance over 200 reviews on MathSciNet of papers in our area. Were there any general principles you followed? Things you think are getting lost in the wayside in the modern world?

David: Well again this just happens—you first get asked to referee technical papers in your field, and then to become an Associate Editor, and everyone wishing to be considered a serious researcher should do this. One thing which may sadly be getting lost is professional societies, which to young people may not seem to offer much. I don't have any general principles, just think of some way to contribute to the profession.

Shankar: I hope you are ok with me ending this section with an amazing quote of yours: "*Aim to do the most substantial work that no one else is both able and willing to do*."

9. OTHER ACTIVITIES AND INTERESTS

Shankar: Can you tell us a little about your interests, outside of math? For example, science fiction? An inside source (whose identity I need to protect), told me that you are also addicted to video games.[8] How true is this, and if so, how do you still manage to be productive?

David: Well I already mentioned volleyball. When someone new joins the team, the first time I do a dive and roll I lie there dramatically and they think "the old guy has had a heart attack"! Evil fun indeed!

I have always been interested in science fiction books, the "serious" ones that seek to portray a plausible alternative society. In the spirit of *Brave New World* and *1984*, though in fact the amazingly prescient classic of that spirit is the 1909 *The Machine Stops* envisaging a world dominated by (Edwardian analogs of) Amazon and Facebook which have replaced physical contact between people. So I like Neal Stephenson's *Snow Crash* or Bruce Sterling's *Distraction*. Or books based on an actual idea, such as Iain M. Banks *The Culture* series, based on the idea "how should a rather utopian society deal with its less utopian neighbors?" Or genuinely imaginative like Adrian Tchaikovsky's *Portia labiata* civilization.

I have read *The Economist* for many years, presenting the most rational analysis of the world I have encountered, and in recent years nicely overlapping my (for fun) participation in geopolitical forecasting. I like hiking, though not very strenuously nowadays. I claim to have been born 125 years too late because I really wanted to be a Victorian gentleman scholar, so my home study is vaguely traditional, and of course a gentleman needs a wine cellar (of sorts).

Regarding games, I tell people to look up 4X games on Wikipedia. These are distant descendants of board games, like Risk or chess, but with many more dimensions. Chess has one dimension in the sense that you are given a bunch of pieces with different given capabilities. Now you can make that more complicated, with pieces becoming available as the game goes on and that you can customize in various ways. But that is just one dimension. In 4X games there are many other facets that are partly given and partly under your control and which interact with each other in complicated ways. And these games have imperfect information—you can't see much of what opponents are doing. The particular game I am doing now is Stellaris. An excuse for doing this playing is to combat cognitive decline—you have to keep track and remember all kinds of things! Anyway it keeps me out of trouble.

And Katy and I play word games and watch old movies and murder mysteries . . . .

10. CONCLUSION

Shankar: What do you think the future holds for DJA? I remember, you had a bucket list, and one of the things you already checked off was hiking the Milford track in New Zealand . . . and there was something you did when you went back to England.

David: I am too unimaginative to have an interesting bucket list. I have lots of "lightweight" research projects including projects with undergraduates, which continued even during COVID through Zoom. Some of the nontechnical discussions on my web site are being posted more

FIG. 5. 2005 *Loeve Prize ceremony*: *Oded Schramm*, *Wendelin Werner*, *David Aldous*.

[8]DA's son when I talked to him in DA's retirement cruise.

FIG. 6. 2012 *Columbia Conference*: *Former students Lea Popovic*, *Shankar Bhamidi*, *Jose Palacios*.

FIG. 7. 2019 *Huntsman Senior Games*: *Volleyball team*, *Aldous back center*.

visibly on medium.com. Without any planning I have gotten to the right level of busyness and engagement. That is, I don't wake up thinking oh I don't have anything to do today, but also I don't worry that there are things I *need* to do today, other than Zoom calls I have promised.

The England thing is a secret I will take to the grave, being an ultimate nerd activity which would cause me to be teased for the rest of my life if anyone knew.

Shankar: Thank you, Professor Aldous!

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