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A Conversation with Jim Pitman

David Aldous

Abstract. Jim Pitman was born in June 1949, received a Ph.D. in 1974 from the University of Sheffield with advisor Terry Speed, and since 1979 has been in the U.C. Berkeley Statistics department. He is known for research on many topics within probability, in particular for a long-running collaboration with Marc Yor on distributional properties of Brownian motion, and for his influential lecture notes *Combinatorial Stochastic Processes*. The following conversation took place at his home in December 2017 and February 2018.

Key words and phrases: Mathematical probability, Markov chain, Brownian motion, combinatorial stochastic processes.

1. EARLY LIFE

Aldous: Readers may know that you are the son of the famous statistician E. J. G. Pitman, who was Professor of Mathematics at the University of Tasmania, and that you grew up there. Tell us about your early life and influences from your father.

Pitman: It was a rather protected and idyllic life, in an academic family where one was expected to go to university eventually. I was always interested in science, as a way of understanding the world, as opposed to religion or arts or literature, etc., and went through phases of being interested in particular sciences, starting at an early age with Geology, collecting rocks. Around 14 or 15, I was fascinated by Chemistry—my sister Mary did Chemistry at university. I read all her books, and messed around in an improvised lab at home. It is amazing that I didn't actually blow anything up.

Aldous: Illustrating a dramatic difference in the experience of childhood over our lifetimes that sounds insanely dangerous nowadays.

Pitman: In my high school senior year, I lost interest in Chemistry in favor of Physics. We had a very good Physics teacher named Vernon Osborne. The parts I liked most were Newton's Laws and electrostatics and the amazing exact things you could do with mathematical calculations. Here, there was encouragement from my father, who was very interested in mechanics and had a practical understanding of getting things done in the physical world.

I was not primarily interested in mathematics until quite late in my school career. My father's influence was that he taught me how to *think* about mathematics. He had an appreciation of elegance and style in mathematics, and infected me with that idea. And I have always been drawn to mathematics with some sort of beauty or elegance. I learned from my father the importance of good notation. A result in good notation becomes a formula that can be used again more easily. Then good notation takes a life of its own, with simple things like $\mathbf{E}[X]$ for the mean of a random variable rather than having to write a sum or integral each time.

Two influential years for me were spent abroad with my father and family, while he was at Stanford in 1956 and Berkeley and Johns Hopkins in 1963. So, I spent those two separate years in American schools. That was almost like a holiday for me, because nobody knew I had been Dux[1] so I was just one of the kids without special expectations of me. And naturally it broadened my outlook and made it easier to come to America later in life.

My father was quite an authority figure in the house, and a little intimidating. I did nothing sporting with him, but I picked up sports' basics from my mother, who had been a tennis and (field) hockey player at school. At school, I played cricket and soccer in the backyard but was never good enough to be on the school teams.

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[1] best in the class (*Australian*)

2. COLLEGE YEARS

Aldous: What about your college years?

Pitman: I was an undergraduate at the Australian National University in Canberra, from 1968–71. I was on a scholarship based on exam results; those scholarships were designed to "seed" ANU as it was relatively new and intended to be elite. We all noticed that, in contrast to having been rather easily the best in our schools, at college there were other people just as good as you, so it was a very competitive environment and it spurred us all to do better than we otherwise might have.

Aldous: Was it like the British system, where you would do only Mathematics?

Pitman: No, we were doing Science degrees; the first year was Pure Mathematics, Applied Mathematics, Physics and Chemistry. In the second year, I dropped Chemistry, in the third year I did Mathematics and Statistics and in the fourth year I specialized in Statistics and wrote an Honors' thesis. By the end of the first year, I had figured out that I liked this Mathematics business much more than the Physics or Chemistry business! My peers at that time were Michael Cowling and Richard Hartley, both of whom have gone on to academic careers. Richard is the more interesting of us, doing a Ph.D. in Mathematics but then turning to computer vision, whereas Michael and I pursued a straighter path to mathematics careers.

Aldous: How did you get into Probability in particular?

Pitman: I had decided that I liked analysis much more than algebra, and there was a limited amount of analysis offered, In the third year, we did Edwards' books [13, 12] on Fourier analysis, which was quite advanced—I can't imagine modern U.S. undergraduates getting to that level—but somehow we were coaxed there. Then there wasn't much pure mathematics left in the fourth year that interested me, but I had noticed that probability and stochastic processes had a lot of analysis in them, so I specialized in them during the fourth year. My undergraduate thesis advisor was Chris Heyde, and I worked with him on stuff related to weak convergence that Ward Whitt and others had been doing around limit theorems for queueing theory. Heyde also got me interested in classical fluctuation theory and renewal theory involving stable(α) distributions.

Aldous: Any other teachers who readers might know of?

Pitman: I did a genetics course with Eugene Seneta and we went through Warren Ewens' book [15]. The funny thing is that I rather hated the subject then, and I don't quite remember why, even though my interests came back to that later with the Ewens' sampling formula and related things.

Aldous: You were a graduate student in Sheffield, U.K. At that time, Sheffield was something of a center for applied probability, with Joe Gani and the Applied Probability journals. Your advisor was Terry Speed, who readers will know from his later work in bioinformatics. Tell us about your time in Sheffield—what you worked on and who you interacted with during that period.

Pitman: There was a tradition of Australians going overseas for graduate school, either in the U.S. or the U.K. For my scholarship, I had to rank three places in the U.K., but only one of (Oxford, Cambridge, London) was allowed. My first choice was London, to work with Harry Reuter, who was active in Markov chains at that time, and my second choice was Sheffield, because of the active group there. Joe Gani had strong connections to Australia. My first advisor at Sheffield was Bob Loynes, who wanted me to work on some problem about conditional inference in Statistics, but I wasn't particularly interested in that, and instead preferred Probability. So I basically drifted for a term, making no progress—I can't even remember what the problem was, just that I didn't like it! I spent a lot of time in the computer room simulating percolation-type processes.

Aldous: This was early 1970s, so did you have to use punch cards?

Pitman: No, we used BASIC on a Wang minicomputer. Anyway, somehow I got talking with Terry Speed, who at the time was working with Elja Arjas on Wiener–Hopf factorizations for Markov additive processes, a matrix analog of classical Wiener–Hopf factorizations for sums of independent random variables. I didn't like their transform methods so much, but they had come up with an identity which was really some kind of stopping time identity, a matrix generalization of Wald's identity. I wanted to understand that in a simpler way and that led me to look at various generalizations of Wald's identity in the context of Markov chains. So I was generally playing around with stopping times and Markov chains. One of the things that came out of it was that I rediscovered Doeblin's coupling proof of Markov chain convergence. I was quite excited about this. No-one in England I spoke to at that time—including David Kendall and John Kingman who were experts—knew this proof.

Aldous: In retrospect, it was already in Breiman's book.

Pitman: Yes, in his undergraduate book [7], but not the graduate book [6]. Anyway, my Ph.D. thesis had the title *Stopping Time Identities and Limit Theorems for Markov Chains* and consisted of results for Markov chain occupation time identities (from Wald-type identities) and rates of convergence. In those days, that was enough for a Ph.D. thesis. (*Laughter.*) Looking back it seems like a baby thing nowadays.[2]

Sheffield had this very active environment; Gani encouraged visitors and Terry looked after them. Some I remember in particular were Parthasarathy, who came and gave a brief course on quantum mechanics. I recall long walks on the Moors with discussions of unitary operators. Another visitor was Debabrata Basu, a "foundations of statistics" person. He gave a course which teased us mercilessly with paradoxes involving MLEs. At that time, Terry was interested in concepts around sufficiency, so I picked up on some of those ideas. A later influence was David Williams, and I learned quite a bit of general Markov process theory, struggling with Dynkin's book [11] which was very difficult and Itô–McKean [21] which was also pretty difficult. I was particularly inspired by one lecture that David Williams gave in Manchester about his new results about path decompositions for Brownian motion [40] and connections with Bessel(3) and squares of Bessel processes and Ray–Knight theorems. I thought these are wonderful things and well worth understanding; that thought stayed with me for quite a long time. In 1973, the *Stochastic Processes and their Applications* meeting was in Sheffield, and I remember spending most of my time there at the blackboard talking with David Williams and Martin Jacobsen about splitting times [22], a hot topic back then: random times (more general than stopping times) at which you could say that past and future of a process had some conditional independence property. David understood that Itô's excursion theory was relevant, and I picked up that idea and ran with it.

I was determined to find some more comprehensible explanation of some of David's theorems involving Bessel(3), and that led to what is now called Pitman's 2M-B theorem [35]—I was lucky to find this succinct formulation.

[2] Pitman's results on occupation time identities [36] were later rediscovered and developed in the context of mixing times [25].

3. EARLY CAREER

Pitman: Interaction with Martin Jacobsen led to me taking a brief post-doc in Copenhagen in Spring 1974. Then I spent the two years 1974–76 at Berkeley, attracted by David Freedman. I loved David's books on Markov chains [16]. In fact, by the time I arrived, David's interests had moved on, mostly to Statistics but also to fancy forms of de Finetti's theorem,[3] which I wasn't so interested in then.

Aldous: We were together in Cambridge for 1976–78, you as Lecturer and me as grad student/post-doc. At the risk of romanticizing the past, I myself view that as a kind of Golden Age for Cambridge probability/statistics, with people like Frank Kelly, Andrew Barbour, Brian Ripley, Bernie Silverman and Adrian Baddeley all being grad students within a few years. Do you have any particular memories of Cambridge?

Pitman: Well I remember the doubles tennis, sometimes on grass courts at Churchill College, with you and Andrew Barbour—and we were always looking for a fourth. We tried to play even in the winter, on hard courts.

Aldous: And Andrew was clearly the best.

Pitman: Yes, Andrew dominated. David Kendall was one of the people who attracted me to Cambridge. I was influenced by his teaching. He had lecture notes for courses I was teaching, in particular the undergraduate "Markov methods" course he had devised, which was a very Kendalesque course with lots of generating functions and queues[4] and branching processes. He was a bit of a father figure and a bit intimidating. He gave an idiosyncratic Part III[5] course on stochastic calculus which I found very stimulating. He insisted on only using Brownian motion as the integrator, rather than the French tradition of martingales, which makes it more difficult. Mike Steele has persisted with this approach in his book [38]. The following year I agreed to give a Part III course on stochastic integration, and I chose to try to learn as I went along from Meyer's *Séminaire 10* [26]. This was "jumping in the deep end" but there was a lot of support and interest and much discussion of how best to prove the Doob–Meyer decomposition theorem.

As for research, I was mostly working with Cindy Greenwood, who visited on a sabbatical, writing papers

[3] See, for example, [9, 10].

[4] It was Kendall who invented the M/M/1 style notation for queues.

[5] Similar to a U.S. first-year graduate.

on splitting at the maximum for Lévy processes [20] and an elementary construction of local time for nested arrays [19]. And you and I interacted quite a lot.

Aldous: We had an obscure paper [1] on Hewitt–Savage 0–1 law for independent nonidentically distributed sequences, and an even more obscure one on a discrete particle system [2]. Though my subsequent interest in Markov chains was strongly influenced by you.

Pitman: I didn't like the winters in Cambridge. It was very cold. I used to bike to my lectures, and there was one day when I arrived and my fingers were so cold that I couldn't grip the chalk!

Aldous: (*Laughter.*) Stories to tell one's children about how tough life was when we were their age.

Pitman: So there wasn't much outdoor recreation in the winter—I played a lot of squash.

4. ON TO BERKELEY

Aldous: You came to Berkeley in 1978 as Assistant Professor, and have stayed ever since ...

Pitman: My early influences at Berkeley were from Lester Dubins and David Freedman, who was a kind of mentor (although the word maybe hadn't been invented then). We had pretty much weekly lunches, and there was a culture—those two and David Blackwell and others—a culture of telling each other mathematical stories, explaining what you were thinking about. You were expected to be able to explain on the back of a napkin what your thoughts were.

Aldous: That reminds me: tell us about the origins of the phrase *Chinese Restaurant Process*. I believe it first appeared in print in my exchangeability survey [4], but I carefully attribute it to Dubins and Pitman.

Pitman: I remember very clearly how that came about. I knew this way of constructing random permutations by inserting new elements into the cycles, and

FIG. 1. *Jim Pitman and David Blackwell*, *circa* 1980.

FIG. 2. *Chuck Stone*, *Jim Pitman Lester Dubins and David Siegmund*, 1980*s*.

once you understood this you could read off things like the Logan–Shepp description of the asymptotic sizes of cycles, and so on. But I was struggling to find a good metaphor. In a conversation with Lester at our usual coffee shop, somehow we came up with the idea that the cycles should be circular tables where you can have any number of items, so we thought of customers in a restaurant, and the familiar instance of specifically circular tables was Chinese restaurants.

Around that time, Persi Diaconis and I gave a course in Berkeley on random partitions, and connections with Bayesian inference and Dirichlet priors and so on. That was the start of my interest in those fields.

Aldous: You had a long-term collaboration with Mark Yor, from the early 1980s to the 2000s, with many papers on what one might call structural and distributional properties of Brownian-type processes. Tell us how this started, what are your favorite results there, what was it like working with Marc?

Pitman: What kept us together was a shared interest in the Brownian world, especially the circle of results around David Williams' path decompositions, the Ray–Knight theorems, local times and so on. We were developing the heritage of Lévy in the sense of deep properties of distributions embedded in the Brownian path. Distributions were central—we didn't care so much about a.s. limit theorems or capacities or Hausdorff dimensions or Erdős–Taylor-style analysis. I visited Paris a couple of times in the late 1970s and early 1980s. Our first paper [32] concerned infinitely divisible laws and things related to the skew-product decomposition of Brownian motion and Bessel processes and last passage times. Then we were especially taken with the idea that the squares of Bessel processes of dimensions 0, 2 and 4 are somehow just "sitting there" in the

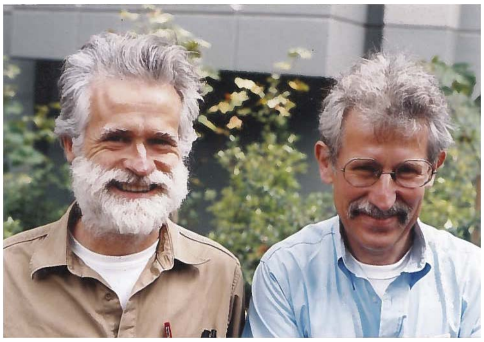

FIG. 3. *Jim Pitman and Marc Yor, circa* 2005.

local times of suitably stopped Brownian motion. So what about other dimensions—where are their Bessel-squared processes? How can we understand the "additivity in dimension" structure? Those questions led to the paper [33] on decomposition of Bessel bridges where we build up the Lévy–Khintchine representation of Bessel-squared processes and show the deep relation with local time. In these early years, we had somewhat complementary backgrounds; I provided the expertise in Markovian excursion theory and Marc was the master of martingale calculus. There was some friendly competition across the Atlantic about how one should best calculate the laws of Brownian functionals, and this "I can do it better" competition turned out to be productive. Then we got drawn into studying windings of planar Brownian motion. Spitzer's theorem gave the limit law for number of windings around a singe point, but it was much less obvious what was going on with several points. The analysis was quite interesting, though easy to make mistakes, and eventually we figured out the "several points" case, which turned out to be a beautiful combination of ideas from excursion theory and stochastic calculus [34].

Then we got interested in arc sine laws and related things to do with occupation times for Brownian motion. That was the start of a very productive period, late 1980s to early 1990s, also together with my student Mihael Perman. We had a suite of results [29] related to lengths of excursions of Brownian motion and how you could analyze occupation times by thinking about excursions being assigned by a coin-tossing process. That involved some interesting nonobvious connections with models for random discrete distributions, especially the ones that came out of the Ewens' sampling formula and the normalized gamma processes used by Ferguson for Bayesian inference. I knew of these things from general Berkeley culture going back to Blackwell and was determined to draw these threads together. There was Kingman's very general theory of random discrete distributions and his theory of partition structures, but it needed more work to bring the Brownian excursions under the same cover. That proved to be a very fruitful circle of ideas—the result is now called a Pitman–Yor process, basically scattering random atoms generated by some kind of Brownian process, to give a model amenable to Bayesian inference.

Aldous: So this is the theory surrounding your two-parameter Poisson–Dirichlet distribution, and these discrete Bayesian models have become widely used in the modern Machine Learning era. Younger readers may associate you most with the phrase Combinatorial Stochastic Processes, the title of your 2002 St. Flour notes [31] and subject of many of your papers. Tell us about CSP.

Pitman: This grew out of the topics above and also incorporated many ideas from your work on limits of random combinatorial objects. You and I had worked together on asymptotics of random mappings of finite sets [5], influenced by your work on continuum random trees. The CSP notes were an attempt to make a kind of umbrella under which you could start with some basic combinatorial contexts and end up with some quite deep mathematics of stochastic processes.

Aldous: And it was the first book-length treatment of material that was previously scattered amongst papers.

Pitman: There was the 1967 book [39] of Takács with almost the same title but a much narrower scope—centered on the ballot theorem.

Aldous: We both had an interest, and some joint papers, around 1995–2002 in stochastic coalescence. Your memory of how we got started on this topic may be different from mine, so I'm interested to hear it.

Pitman: We were both interested in random trees for various reasons. I got interested in trees embedded in Brownian motion through the work of Knight and Walsh on downcrossings in the Ray–Knight theorem. Neveu understood rather deeply that these embedded trees represented genealogy of continuous-state branching processes. I spent sabbaticals in Paris in the late 1980s and interacted with Le Gall—his exposition is in [23]—and I ended up writing two papers [27, 28] with Neveu on this subject. This was at the same time that you were developing the continuum random tree from a totally different perspective; we had some interaction but the dots were not well connected before the CSP notes.

Aldous: Yes, Le Gall's picture [24] of the tree embedded into Brownian excursion, without explicit reference to downcrossings, was (to me) the final piece in the circle of equivalent descriptions of the continuum random tree, clearly somehow related to your work but I never formulated that carefully.

Pitman: The next step in the story is when a post-doc in Astronomy, Ravi Sheth, knocked on my door. He had seen one of my papers on trees, and was interested in models of gravitational clustering and had come across the Borel–Tanner distribution for total progeny in a Poisson–Galton–Watson (PGW) process. This led to a joint paper in an astronomy journal [37] and got me thinking about the connection between the PGW process and combinatorial random trees, because I had learned from you that a PGW tree conditioned on total size n and with randomly permuted labels was just the uniform random tree on n labeled vertices—the random Cayley tree

Aldous: This was implicitly known from older work of Meir and Moon, but expressed there in the language of combinatorics and generating functions.

Pitman: This seemed a strange thing to do from a probability viewpoint. Ravi was interested in the idea of cutting up these trees—fragmentation—and the opposite idea of joining up trees—which we now call coalescence. This got me thinking about the circle of ideas around Cayley's enumeration of trees and the Lagrange inversion formula, and I came up with a proof [30] of Cayley's theorem via randomly cutting up trees rather than via Prüfer codes. I always felt there had to be a better way of proving Cayley's theorem! This led to the nonobvious result that the process of randomly cutting up the random Cayley tree was, in reversed time, the discrete additive coalescent, that is, two trees merge at rate proportional to the sum of their sizes. Taking limits of that "combinatorial" story, we get the continuous additive coalescent as a time reversal of the fragmentation-by-random-cutting process associated with the continuum random tree—our joint paper [3].

Aldous: We should also mention your related work with Steve Evans [14].

Pitman: Generally, I was interested in the duality between coalescence and fragmentation, which is not easy to analyze. There are beautiful special cases ......

Aldous: ... but no useful general theory.

Pitman: Anyway, does this agree with your recollections?

Aldous: Well it's amazing how many different sources there were. At some point, I found the 80 years of work in physical chemistry around the deterministic model for coalescence, the Smoluchowski coagulation equation and the then-more-recent and not very sophisticated statistical physics literature around the discrete stochastic coalescent, called the Marcus–Lushnikov process. And of course we knew the Kingman coalescent.

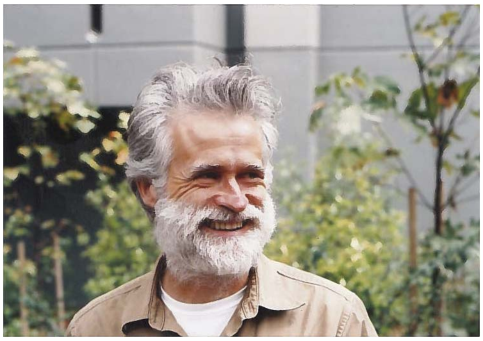

FIG. 4. *Jim Pitman*, *circa* 2010.

Pitman: Giving the St. Flour lectures was a very pleasant experience, and I have fond memories of food and wine and conversations. It was the start of a long collaboration with Sasha Gnedin.[6] In general, conferences are a place to talk with people one-on-one ...

Aldous: *networking*, before that word became popular.

Pitman: And I always found the value comes from conversations at the blackboard rather than from listening to talks. Earlier in my career, I enjoyed the old *Seminar in Stochastic Processes* series[7] with Erhan Çinlar and Kai Lai Chung in good form.

5. MATHEMATICS, ASIDE FROM RESEARCH

Aldous: You were Editor of Annals of Probability for 1994–96, and I remember us and Steve Evans having weekly meetings to discuss which papers to summarily reject. But I'm trying to remember, were we mailing paper copies or e-mailing PDFs?

Pitman: We were mailing hard copies. But I wrote my own management system, to email Associate Editors to tell them their assignments and to get email reports and to keep track of everything. Indeed I passed this on to Varadhan, the next Editor. But the system was very clunky—this was before Python—and it was written with UNIX tools: AWK, C shell and sed. I also had a secretary to manage files and mailing.

Aldous: You developed an interest in open access journals, a Global Digital Mathematics Library, bibliographies within mathematics, and other "mathematics

[6] See, for example, [17, 18].

[7] See, for example, [8].

exposition/publishing" matters. I imagine many readers are only vaguely aware of such things, so—as well as your own activities—what is the overall state of affairs here, and what hopes have been realized or dashed or still under construction?

Pitman: Well the state of the world has certainly changed quite a bit over the last decade or so. There's greater interest in and support for Open Access efforts in publishing, coming indeed at government level especially in the U.K. This is all encouraging. I made some efforts in pushing toward open access, especially when I had some influence in the IMS. We did manage to launch some new open access journals, Probability Surveys with you and Statistics Surveys. And we brought EJP (Electronic J. Probability) and its sister ECP, started by Chris Burdzy and others, into the fold of recognized IMS journals and eventually onto Project Euclid. It had been an effort to get those journals started.

Aldous: And they have now become mainstream. Recall there was a hope, amongst us young radicals, that we could drive expensive commercial publishers out of the academic journal business ...

Pitman: This has not happened, and it's just not going to happen. It's a very difficult problem, and I think the publishers are here to stay, with substantial control over what can be done with the literature. An interesting aspect is what can be done about data mining the literature, that is thinking of the literature as a corpus of theorems to be organized and mined. knowledge management facilities to assist mathematicians and statisticians. Of course, one could try this for all sciences, but mathematics has some special attributes which hopefully could be organized in a better way. For instance, the dominance of open source code in academic work is very encouraging. Most researchers don't think much about how one might automate knowledge extraction. That's a very interesting direction, and it's obvious that the big commercial publishers are doing that as much as they can. You go to Elsevier and look at a paper and see "readers who liked this paper might also like ...," and that's often helpful. I'd like to see IMS doing more in that direction. I have been on various committees with names like Global Digital Mathematics Library. That concept is maybe a bit of a pipe dream. Maybe more feasible would be to have some distributed system of open databases, and there are some exemplars out there, in very structured environments. One I like is the NIST digital library of mathematical functions, and another is the On-Line Encyclopedia of Integer Sequences founded by Sloane. There's potential for doing the same sort of thing with theorems. We'd like to be able to enter a theorem statement into an interface and have the oracle respond with the places similar theorems have appeared. Potentially, we could do this soon, and I would like to see such resources be as open as possible. I'm currently on a committee which has founded a nonprofit organization IMKT (International Mathematical Knowledge Trust)—an umbrella organization intended to encourage open mathematics knowledge systems of whatever kind. We don't have the resources to build the ubersystem but we encourage people to build parts of the system and to talk to each other.

On the bibliographic side, I was interested in the compilation and maintenance of bibliographies, but it's hard to do that better than MathSciNet and Zentralblatt already do. I wasn't able to convince enough people to engage in my proposed activities. We did start the online "collected works" at *Celebratio Mathematica*, part of Robion Kirby's MSP open access publishing outfit.

Other things ... post your papers on arXiv. Sooner or later we will have some serious data mining, and preparing for when that day comes you should make the data as open as it can be, so it's not just the commercial publishers that can provide mathematical information services.

Aldous: You were IMS President in 2007. When we were young, scholarly societies were a kind of social network, based around useful publishing and conference-organizing, and young academics would join and stay rather automatically. But this seems to have changed. Your thoughts on the role and future of societies is general, and IMS in particular?

Pitman: I think there's an important role of the kind I've just mentioned, to ensure the knowledge base in a way that's under the profession's control. But you need a business model. IMS is unusual—it's the minimal size for a functioning organization, with one full-time person who is coordinating everything. There aren't many such in mathematics. There are the big national societies, but they don't have more special-interest aspects. So IMS is in the position to do something different, but it's vulnerable because of size. And IMS needs a business model because the tradition in mathematics is not to pay page charges.

Aldous: I do pay, for IMS journals, out of grant money.

Pitman: So I don't know what to say—I'm happy to encourage people to become members and engage with the governance of the society.

FIG. 5. *Attendees*[9] *at the* 2014 *UCSD meeting Combinatorial Stochastic Processes in celebration of Jim Pitman's work.*

Aldous: It's odd that there are two societies for probabilists: IMS and Bernoulli.

Pitman: But this is like Math Reviews and Zentralblatt, basically a U.S. versus Europe thing.

Aldous: What are your general thoughts about the evolution of Probability and its connections over your career? Initially, it was (in many countries) quite closely tied to Statistics but the connection has weakened—our colleague Bin Yu decries the lack of participation of probabilists at the IMS Annual Meetings, for instance. Conversely, more pure mathematicians nowadays get involved with probability, and the majority of students in our measure-theoretic graduate course are from EECS.[8]

Pitman: Well, Probability is not going to go away. There's a continuing need for probabilistic concepts to deal with ever more complex models for inference in Machine Learning, etc. To know what these Machine Learning algorithms are doing helps to know a lot about algorithms and also a lot about probability. Now the discipline of Probability has become very specialized, in the sense of mathematically deep analysis of very specific sophisticated models, rather than broad general theories. It's not clear to me if there will be many more advances in "pure probability" in the sense of general purpose tools like stochastic calculus or martingale theory. Perhaps Talagrand-style concentration inequalities are the most recent example of a new general purpose tool. But when you look at current textbook treatments of martingales or CLTs, etc., there is not much variation in treatment—things have stabilized, and it's hard to imagine those changing much.

Aldous: Well, people are studying classes of processes—SLE and KPZ and Tracy–Widom random matrices and rough paths ...

Pitman: Yes, these exemplify very fascinating mathematical models but are extremely specialized and refined in their definition and analysis. To even understand the definitions, you need more than a first graduate course in probability. These kind of models will continue to attract attention—quantum gravity and random maps, etc. are seductive for mathematicians because of deep connections with other mathematics. So there's a different skill set needed for this research, compared to the broader but less deep skill set when we started out.

Of course, new problems will continue to come from outside. And training in Ph.D. level probability and stochastic processes allows one to quite easily move into realms like Finance or Machine Learning. Several of my recent students could have continued to an academic research career but choose to go to Credit Suisse or Google or ...

I've been lucky to have had a number of very talented students over the years. Among those who stand out in terms of continuing to research careers: Chris Burdzy,

[8]Electrical Engineering and Computer Science.

[9]List of names available at https://www.stat.berkeley.edu/~aldous/Pitman_Conference.

Tom Mountford and Jason Schweinsberg. I remember Tom Mountford used to walk around the department with a copy of Itô and McKean always in his hand. I think he must have read it from cover to cover.

Probability will continue to be stimulated and refreshed by applications. The subject will endure, but not in ways we can easily anticipate.

Aldous: You are married to Ani Adhikari, who teaches Data Science and Statistics here at Berkeley, and have a college-aged son, Ian; with this background, any chance of a third generation of statistical Pitmen?

Pitman: He's at the University of Chicago and there is some danger of him becoming a statistician. In fact, he's doing a double major in Economics and Mathematics, so if you have to pick a subject halfway between those two ...

Aldous: You wrote a well-known introductory post-calculus textbook *Probability*, which I'm sure involved a lot of work. Any thought on textbooks?

Pitman: Yes, writing one is a lot of work! It went through many versions before the final form. Maybe the most interesting thing is that Ani is developing a version of that course (our STAT 140) that seriously involves computation. She has shown that you can teach students probabilistic ideas using simulation; they can learn to write code to do interesting things, much more easily than via analysis, and that students get to a better understanding of probabilistic ideas that way. And writing code that works involves understanding of the math structure of probability, so it helps students understand theory.

Aldous: On the subject of teaching, I still mostly use blackboard and chalk, except for my "Probability in the Real World" course where I need to show data, and occasional advanced topics courses to be recycled as summer school type courses. What about you?

Pitman: I like chalk. I think it helps to see mathematics being created, so to speak, as the professional thinks of it, in a fresh way, rather than a stale recital. It's not easy to replicate that with slides.

6. OUTSIDE MATHEMATICS

Aldous: I know you are an avid cricketer—tell us more about your life outside mathematics.

Pitman: In earlier years, I had more numerous outside activities—mountaineering, skiing, tennis. Around the mid-1980s, I got to know the people in the local Marin Cricket Club and began playing there. Quite a long time ago! So I was fairly late in developing my cricket skills, but over the last 30 years I have played on a fairly regular basis. I'm an all-rounder, but mainly a bowler, a right arm leg spinner, in the tradition of Richie Benaud, my childhood hero. A social thing, but it certainly keeps your brain and body active. Last summer the club toured in England, so we got to play in some beautiful cricket grounds. I'm happy to have found a sport I can still play at my age without embarrassment.[10] Age and experience versus youth and ability!

Aldous: Other interests?

Pitman: I like listening to music and going to concerts, mostly classical—Beethoven and Bach, that early classical era. And my son, Ian, is a chorister, so I've been going to many choir concerts.

Also I enjoy gardening, orchids in particular.

REFERENCES

[1] ALDOUS, D. and PITMAN, J. (1979). On the zero-one law for exchangeable events. *Ann. Probab.* **7** 704–723. MR0537216

[2] ALDOUS, D. and PITMAN, J. (1983). The asymptotic speed and shape of a particle system. In *Probability, Statistics and Analysis. London Mathematical Society Lecture Note Series* **79** 1–23. Cambridge Univ. Press, Cambridge. MR0696018

[3] ALDOUS, D. and PITMAN, J. (1998). The standard additive coalescent. *Ann. Probab.* **26** 1703–1726. MR1675063

[4] ALDOUS, D. J. (1985). Exchangeability and related topics. In *École D'été de Probabilités de Saint-Flour, XIII—*1983. *Lecture Notes in Math.* **1117** 1–198. Springer, Berlin. MR0883646

[5] ALDOUS, D. J. and PITMAN, J. (1994). Brownian bridge asymptotics for random mappings. *Random Structures Algorithms* **5** 487–512. MR1293075

[6] BREIMAN, L. (1968). *Probability*. Addison-Wesley, Reading. MR0229267

[7] BREIMAN, L. (1969). *Probability and Stochastic Processes with a View Toward Applications*. Houghton Miffin Co, Boston, MA. MR0254942

[8] ÇINLAR, E., CHUNG, K. L. and GETOOR, R. K., eds. (1983) In *Seminar on Stochastic Processes*, 1982. *Progress in Probability and Statistics* **5**. Birkhäuser, Boston, MA. MR0733662

[9] DIACONIS, P. and FREEDMAN, D. (1980). de Finetti's theorem for Markov chains. *Ann. Probab.* **8** 115–130. MR0556418

[10] DIACONIS, P. and FREEDMAN, D. (1980). de Finetti's generalizations of exchangeability. In *Studies in Inductive Logic and Probability, Vol. II* 233–249. Univ. California Press, Berkeley, CA. MR0587993

[11] DYNKIN, E. B. (1961). *Theory of Markov Processes*. Prentice-Hall, Englewood Cliffs, NJ. MR0131900

[12] EDWARDS, R. E. (1967). *Fourier Series: A Modern Introduction. Vol. I.* Holt, Rinehard & Winston, New York. MR0216227

[10] **Ani:** More fit in the nets than the 20-year-olds he plays with!

[13] Edwards, R. E. (1967). *Fourier Series*: *A Modern Introduction*. *Vol*. *II*. Holt, Rinehart & Winston, New York. MR0222538

[14] Evans, S. N. and Pitman, J. (1998). Construction of Markovian coalescents. *Ann*. *Inst*. *Henri Poincaré Probab*. *Stat*. **34** 339–383. MR1625867

[15] Ewens, W. J. (1969). *Population Genetics*. Methuen, London. MR0270768

[16] Freedman, D. (1971). *Markov Chains*. Holden-Day, San Francisco, CA. MR0292176

[17] Gnedin, A. and Pitman, J. (2005). Exchangeable Gibbs partitions and Stirling triangles. *Zap*. *Nauchn*. *Sem*. *S*.-*Peterburg*. *Otdel*. *Mat*. *Inst*. *Steklov*. (*POMI*) **325** 83–102, 244–245. MR2160320

[18] Gnedin, A. and Pitman, J. (2005). Regenerative composition structures. *Ann*. *Probab*. **33** 445–479. MR2122798

[19] Greenwood, P. and Pitman, J. (1980). Construction of local time and Poisson point processes from nested arrays. *J*. *Lond*. *Math*. *Soc*. (2) **22** 182–192. MR0579823

[20] Greenwood, P. and Pitman, J. (1980). Fluctuation identities for Lévy processes and splitting at the maximum. *Adv*. *in Appl*. *Probab*. **12** 893–902. MR0588409

[21] Itô, K. and McKean, H. P. Jr. (1965). *Diffusion Processes and Their Sample Paths*. *Die Grundlehren der Mathematischen Wissenschaften*, *Band* 125. Academic Press, New York. MR0199891

[22] Jacobsen, M. (1974). Splitting times for Markov processes and a generalised Markov property for diffusions. *Z*. *Wahrsch*. *Verw*. *Gebiete* **30** 27–43. MR0375477

[23] Le Gall, J.-F. (1989). Marches aléatoires, mouvement brownien et processus de branchement. In *Séminaire de Probabilités*, *XXIII*. *Lecture Notes in Math*. **1372** 258–274. Springer, Berlin. MR1022916

[24] Le Gall, J.-F. (1993). The uniform random tree in a Brownian excursion. *Probab*. *Theory Related Fields* **96** 369–383. MR1231930

[25] Lovász, L. and Winkler, P. (1998). Mixing times. In *Microsurveys in Discrete Probability* (*Princeton*, *NJ*, 1997). *DIMACS Ser*. *Discrete Math*. *Theoret*. *Comput*. *Sci*. **41** 85–133. Amer. Math. Soc., Providence, RI. MR1630411

[26] Meyer, P. A. (ed.) (1976). *Séminaire de Probabilités*, *X*. *Lecture Notes in Mathematics*, *Vol*. 511. Springer, Berlin. MR0431319

[27] Neveu, J. and Pitman, J. (1989). Renewal property of the extrema and tree property of the excursion of a one-dimensional Brownian motion. In *Séminaire de Probabilités*, *XXIII*. *Lecture Notes in Math*. **1372** 239–247. Springer, Berlin. MR1022914

[28] Neveu, J. and Pitman, J. W. (1989). The branching process in a Brownian excursion. In *Séminaire de Probabilités*, *XXIII*. *Lecture Notes in Math*. **1372** 248–257. Springer, Berlin. MR1022915

[29] Perman, M., Pitman, J. and Yor, M. (1992). Size-biased sampling of Poisson point processes and excursions. *Probab*. *Theory Related Fields* **92** 21–39. MR1156448

[30] Pitman, J. (1999). Coalescent random forests. *J*. *Combin*. *Theory Ser*. *A* **85** 165–193. MR1673928

[31] Pitman, J. (2006). *Combinatorial Stochastic Processes*. *Lecture Notes in Math*. **1875**. Springer, Berlin. MR2245368

[32] Pitman, J. and Yor, M. (1981). Bessel processes and infinitely divisible laws. In *Stochastic Integrals* (*Proc*. *Sympos*., *Univ*. *Durham*, *Durham*, 1980). *Lecture Notes in Math*. **851** 285–370. Springer, Berlin. MR0620995

[33] Pitman, J. and Yor, M. (1982). A decomposition of Bessel bridges. *Z*. *Wahrsch*. *Verw*. *Gebiete* **59** 425–457. MR0656509

[34] Pitman, J. and Yor, M. (1986). Asymptotic laws of planar Brownian motion. *Ann*. *Probab*. **14** 733–779. MR0841582

[35] Pitman, J. W. (1975). One-dimensional Brownian motion and the three-dimensional Bessel process. *Adv*. *in Appl*. *Probab*. **7** 511–526. MR0375485

[36] Pitman, J. W. (1977). Occupation measures for Markov chains. *Adv*. *in Appl*. *Probab*. **9** 69–86. MR0433600

[37] Sheth, R. K. and Pitman, J. (1997). Coagulation and branching process models of gravitational clustering. *Mon*. *Not*. *R*. *Astron*. *Soc*. **289** 66–82.

[38] Steele, J. M. (2001). *Stochastic Calculus and Financial Applications*. *Applications of Mathematics* (*New York*) **45**. Springer, New York. MR1783083

[39] Takács, L. (1967). *Combinatorial Methods in the Theory of Stochastic Processes*. Wiley, New York. MR0217858

[40] Williams, D. (1974). Path decomposition and continuity of local time for one-dimensional diffusions. I. *Proc*. *Lond*. *Math*. *Soc*. (3) **28** 738–768. MR0350881