PROBABILITY UNFOLDING, 1965-2015

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Abstract.
We give a personal (and we hope, not too idiosyncratic) view of how our subject of Probability Theory has developed during the last half-century, and the author in tandem with it.

1. Introduction.
One of the nice things about Probability Theory is that it is still a young subject. Of course it has ancient roots in the real world, as chance is all around us, and draws on the older fields of Analysis on the one hand and Statistics on the other. We take the conventional view that the modern era begins in 1933 with Kolmogorov's path-breaking *Grundbegriffe*, and agree with Williams' view of probability pre-Kolmogorov as 'a shambles' (Williams (2001), 23)[1]. The first third of the last century was an interesting transitional period, as Measure Theory, the natural machinery with which to do Probability, already existed. For my thoughts on this period, see [72][2], [104]; for the origins of the *Grundbegriffe*, see e.g. Shafer & Vovk (2006).

Regarding the history of mathematics in general, we have among a wealth of sources the two collections edited by Pier (1994, 2000), covering 1900-50 and 1950-2000. These contain, by Doob and Meyer respectively (Doob (1994), Meyer (2000)), fine accounts of the development of probability; Meyer (2000) ends with his 12-page selection of a chronological list of key publications during the century.

Regarding half-centuries, we celebrated last year the half-centennial of the *Journal of Applied Probability JAP*, and the Applied Probability Trust APT, founded in 1964 by Joe Gani (15.12.1924 - 12.4.2016), under whose auspices this volume will appear (Søren Asmussen, one of the speakers at the conference, *Limit theorems in probability*, Imperial College, 23-26 March 2015,

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[1]David Williams is my personal mathematical hero.
[2]Numbers in square brackets refer to my papers, in the order on my CV.

whose proceedings form this volume, was Editor-in-Chief of JAP and AAP until 2015, and continkues as an APT Trustee)[3]. Gani was very conscious of the human side of our subject, and how it evolves; witness the two APT volumes Gani (1982, 1986) (statistics in 1982, probability in 1986), which are full of good things. My personal favourites include two autobiographical pieces by my old friend Peter Whittle, one in Gani (1986), one in his own Festschrift (Kelly (1994)), the very interesting account by John Kingman (Kingman (2010)) of his view of British probability 1957-67 in his Festschrift (Bingham & Goldie (2010)), and the fine account by Cramér (1976) of his experiences 1920-70.

I decided to include an autobiographical piece here for two reasons. First, I have a long-standing professional and personal interest in the history of mathematics in general and probability in particular (witness my pieces on Kolmogorov [46,47], Rényi [55], Reuter [56, 59], Takács [58], Kendall [63], Greenwood [91], Marcinkiewicz [154], Gnedenko [123] and Norberg [133], among others), and if not here and now, where and when?[4]. Secondly, I have always loved hearing stories of the illustrious dead, and thought that I should pass some on.

2. Early years: 1965-69.
What I find surprising looking back on my own emergence as a probabilist is that I survived the (to me) stultifying effect of a first exposure as a Sixth Former to a surfeit of problems about coloured balls and urns. I then had the good fortune to be taught by a probabilist, John Hammersley (1920-2004), at Trinity College, Oxford (1963-6); during the last year I fell in love with probability [102]. So I could regard my serious exposure to probability as dating from 1965, although this was really only confirmed during my time as a research student at Churchill College, Cambridge (1966-9) under David Kendall (1918-2007).[5] My love of limit theorems had two specific triggers: realising (from seeing J. S. (Jack) de Wet (1913-95), of Balliol, a teacher of legendary ability, prove the Weierstrass approximation theorem from the weak law of large numbers) that if one knew some probability one

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[3]Since 1.1.2016, the Editor in Chief has been Peter Glynn.

[4]If a man can't stick an autobiographical piece in his own Festschrift, whose Festschrift can he stick an autobiographical piece in? – to paraphrase Gilbert and Sullivan (*Ruddigore*).

[5]For my impressions of this time, see *Recollections of the Statistical Laboratory, 1966-69*, on my homepage (under Reminiscences).

could sometimes do analysis better than analysts who didn't,[6] and Kendall dropping a paper on my desk half-way through my time at Cambridge (by Dwass and Karlin, which led to my early work on the Darling-Kac theorem).

I loved the Stats Lab, and was very impressed by the seminar programme, which I never willingly missed. Apart from David Kendall, and David Williams, the main influence on me was Rollo Davidson (1944-70), my office-mate at one time and 'mathematical elder brother' (he was one year above me, also under Kendall).[7] I met Charles Goldie there; he left to follow Kingman to Sussex; our collaboration followed much later.[8]

One of our most distinguished regular visitors was the great Hungarian probabilist Alfred Rényi (1921-1970). His talks were always superb. I recall in [55] his talk of 28.5.1969 as 'what I regarded then and regard now as the best mathematical talk I have ever heard'. John Lamperti's year-long visit was a delight. I was extremely lucky in getting prior access to Pat Billingsley's now classic book (Billingsley (1968)), just when I needed it for my growing interest in limit theorems. I met Mark Kac, a wonderful man (I recommend his introduction to his Selected Works, and his superb autobiography (Kac (1979, 1985)). I fondly remember having lunch with him and his wife Kitty, with David Kendall in Churchill.

The developments of the time that I remember best were the books – Meyer, Loève, Feller volume 2, Breiman, McKean (my first exposure to stochastic integration) – and the excitement generated by the Kunita-Watanabe inequalities (Kunita & Watanabe (1967)). This was recognised immediately as opening up much of probability theory, including stochastic integration, to the power of Hilbert-space methods. Also in 1967 came Volume I of the *Séminaire des Probabilités* edited by Meyer. The work of the Strasbourg (and later, Paris) school, and the *théorie générale des processus*, was obviously too important to ignore, but seemed to a young man with a thesis still to write too potentially all-consuming to commit to. That would have to wait. Meanwhile, I was intrigued by the well-known treatment of Karamata's

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[6]I was lucky enough to be tutored by Jack de Wet, as well as lectured to by him. I credit him with turning me into an analyst, just as I credit John Hammersley and David Kendall for turning me into a probabilist.

[7]Following Rollo's tragic death in a climbing accident in the Alps, the Rollo Davidson Trust was set up by David Kendall to commemorate his life and work. I served as a Trustee for many years, and as Chairman.

[8]Charles Goldie's personal appearance has changed incredibly little over the 47 years or so I have known him: he seems almost ageless.

regular variation in Feller's book, which was destined to have important consequences for me.

3. Westfield: 1969-74.

The 1960s was the first great period of post-war university expansion. I did not realise then how lucky I was, in being able to get an academic job (in 1969, aged 24) while still finishing my PhD. I was offered two, both in London, and chose Westfield College, then in Hampstead, largely because of the probabilist James (S. J.) Taylor. This former ladies' college of the University of London (together with Bedford, Chelsea and Queen Elizabeth Colleges) sank under the influence of the first round of Government cuts in 1983-4, only Royal Holloway of the former ladies' colleges surviving.[9]

The University of London Probability Seminar was co-organised by Harry (G. E. H.) Reuter (1921-92) at Imperial College and James Taylor at Westfield. I loved this, and attended it assiduously. [10] This wonderful institution played a crucial role in my mathematical development; I owe it a great deal.[11] I would often be driven to Imperial by James, but if I went alone I would use the tube and the tunnel from South Kensington tube station. This struck me as a vision of Dante's Inferno at first – I found London overwhelmingly big at first, after the beautiful mediaeval cities – York, Oxford and Cambridge – that I knew. But now I find the tunnel pleasantly nostalgic, as it reminds me of the University of London Probability Seminar.

One wonderful thing about Westfield then was the stream of visitors that James Taylor attracted. As I recall, Don (D. L.) Burkholder (1927-2013) was there in my first year, working with Dick Gundy on martingales, and Mike (M. B.) Marcus in my second year, working on Gaussian processes. Jim (J. G.) Wendel also visited.

Westfield hosted the London Probability Seminar less than Imperial, but we did get some good speakers there. One was Kai-Lai Chung (1917-2009), a

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[9]Clive Kilmister (Kilmister (1986)) wrote an account of this, including 'departmental obituaries' of the closing Mathematics Departments.

[10]I co-organised it with Harry, and later with other Imperial colleagues, then with colleagues at Queen Mary, 1976-99, from my return from the US to my leaving the University of London.

[11]It also played a crucial role in my personal life: it is what kept me in London, where I met my wife Cecilie in 1978. Being a country boy from the North, I nearly defected there in 1971. I was talked out of this by the fatherly advice of Harry Reuter and (independently) Fred Piper, my Westfield colleague and later Best Man at my wedding.

fine probabilist and author; he was a difficult man, but was always very nice to me. He spoke at Westfield c. 1971, on the state of play in Markov processes. He began: "We've been going — too fast too fast; we've been proving — too many theorems too many theorems; now it's time for a period of — retrenchment retrenchment" – an unforgettable piece of theatre.

Kolmogorov is to probability as Gauss is to mathematics and Fisher is to statistics. I heard him once, at the International Congress of Mathematicians in Nice in 1970. He was speaking on information theory, in French. Kolmogorov seems to have been blessed with the full measure of almost every gift, except that of speaking clearly in public. His voice was rather high-pitched; as the sentence progressed, he would get more and more excited; the pitch would rise, and would fall off the top end of his register before he got to the crux of the sentence. It was wonderful as theatre, but [123, §9] not particularly successful as an exercise in communication.

The Mathematics Department at Westfield was small (it varied between 12 and 16 people when I was there). One great advantage of this, which I did not foresee and barely noticed at the time, is that it gave me real versatility on the teaching side. I am a probabilist; I was regarded as thus a de facto analyst, so I taught probability and analysis indiscriminately.[12] On my return from the US (below), I was asked to teach statistics, which I agreed to do. The upshot is that I have a range covering all three fields (augmented later by history of mathematics and mathematical finance), and so great teaching flexibility. I did not plan this, but I have found it very useful.

My first five years in Westfield (before my visits to the US, 1974-6) saw me adjusting to life in London, learning to teach, beginning my research career publishing (my first dozen or so papers, the first eight based on my thesis), and beginning to collaborate – with Ron Doney (another speaker), and Jef Teugels.

David Williams' wonderful career has been much influenced by two men who much influenced mine, David Kendall and Harry Reuter. David is seven years my senior; after a year in Stanford with Chung and three in Durham with Reuter, he was in Cambridge when I was there, as a Fellow of Clare; it was already clear that he is a force of nature. He then went to Swansea for 16 years; rumour had it that he was systematically working through Itô and McKean, a famously formidable book, in great detail. He is a probabilist's

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[12]A number of my probabilist friends have wept into their beer with me, grumbling that they are *not allowed* to teach analysis.

probabilist, and has been so at least since his big paper (Williams (1974)), where he brought path properties in general and path decomposition in particular into centre stage in probability. Results of this type are now known as *Williams decompositions*.

4. The USA: 1974-76.

In 1974 I took a year's leave to become a Visiting Assistant Professor in the Math. Department at the University of Michigan, Ann Arbor, visiting Jim Wendel. Jim had been a fireball of activity in his early research, but family commitments (he and June had six children) had slowed down his output by that time. I found him scholarly and friendly, but my main mathematical stimulus in the U of M (or A^2 as it was often called) lay on the analysis side. This was superb. I recall Allen Shields (1927-89) in functional analysis, Fred Gehring (1925-2012) in complex analysis, and Peter Duren in Hardy spaces, a lovely blend of the two[13]. I was immediately put on the lecture circuit, and flew all over the mid-West giving seminars. This was tremendous fun. In particular, I spoke at Minnesota (Pruitt, Jain, Fristedt), Illinois (Doob, Burkholder, Knight, Phillip, Stout), Northwestern (Marcus, Pinsky, Gugu – Alexandra Ionescu-Tulcea, now Bellow), Cornell (Kesten, Spitzer) and Wisconsin (Ney, Askey, Wainger, Chover). One of my favourite talks at that time was *Fluctuation theory in continuous time*, later [15] for long my most cited paper (until 1996, when it was largely subsumed into Ch. VI of Bertoin's masterly book on Lévy processes, Bertoin (1996)). I was charmed by the warmth and friendliness of my reception by my American hosts. I loved the parties and the cameraderie, and developed a taste for bourbon, which I still have.

In 1975 I was delighted to be offered a year-long post as Visiting Assistant Professor in the Math. Department at the University of Illinois, Champaign-Urbana, visiting Don Burkholder (Westfield kindly agreeing to release me for a further year). This was Doob's last year before retiring, and the probability group at the U of I was at its splendid best. Again, I was on the lecture circuit (Pat Billingsley at Chicago, Burgess Davis at Purdue, Lajos Takács at Case Western, Cindy Greenwood (speaking here) at UBC). I had a wonderful time, mathematically and socially.

Analysis was also important at the U of I: functional analysis (Haskell Rosenthal, J. Jerry Uhl (1940-2010)), and analytic number theory (Paul

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[13]which I have recently employed systematically in my work on time series and prediction

Bateman, Harold Diamond). Not only did Jerry Uhl tell me about the links between the martingale convergence and Radon-Nikodym theorems in the geometry of Banach spaces, he enriched my social life by introducing me to his weekly drinking sessions, where beer was served in pitchers. I also met Gilles Pisier there – an established and rising star, but technically still a research student, under Laurent Schwartz.

I was systematically preparing the ground for writing what became my book with Charles Goldie and Jef Teugels on regular variation. I was becoming more aware of martingales, thanks to Burkholder and U of I, and more confident with Hardy spaces, thanks to Duren's book (one of the few I had at that time, living out of a suitcase as a bird of passage). The other influence I recall was reading Dellacherie's books Dellacherie (1972a, 1972b), and starting to realise how important analytic sets were – but more of that anon.

Joe Doob's retirement conference in 1976 was excellent. David Williams was there, and I consulted him about possible directions to go in. He replied, in a fatherly way, that I should just carry on and do my own thing. This strikes me as wise and obvious now, but struck me as wise and wonderfully insightful then.

I had the delightful experience of a canoeing holiday in the Ozarks (Missouri) with Joe Doob, Frank Knight and Paul Potter (a non-mathematician). I will never forget white-water canoeing and shooting rapids (and being reproached by Joe for not shouting out warnings loudly enough), for seeing water-snakes swimming beside us, and for bivouacing in the open air, with whip-poor-wills calling[14] as we fell asleep, and hoar-frost on our pillows when we woke.

5. Westfield again: 1976-86.

All good things come to an end, and (after a hard struggle to resist a chair in the US) I returned home to London (aged 31) – where to my surprise and annoyance I found I had to readjust to life in the UK. I found myself co-organising the London Probability Seminar, succeeding James Taylor, who had left for Liverpool. Another pleasant development was that Charles Goldie, after a quiet period, had written a long and important paper in my absence. I determined to keep him at it; one thing led to another; he began

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[14]The name is onomatopoeic: the birds have a call which sounds just like this. It is quite unforgettable, and still sends shivers down my spine when I think about it.

to visit me regularly at Westfield, and our research collaboration began quite naturally, in the late 70s (first papers 1982). I was successful in applying for a grant for Paul Embrechts (another speaker at the conference), a pupil of Jef Teugels (PhD 1978), to visit me at Westfield 1978-9; this led to papers of Paul with Charles, and to an ongoing friendship, mathematical and personal. Meanwhile, at UCL in July 1978 C. A. (Ambrose) Rogers organised an LMS Instructional Conference on Analytic Sets. During this, it became apparent to me that analytic sets held the key to the important structural questions in regular variation. I made an attempt, not at that time successful, to enlist the help of Adam (A. J.) Ostaszewski here. So our collaboration, which began to come to fruition from 2006 on, can be traced back to then.

I had returned to London partly for family reasons (my mother), partly the prospect of eventual promotion. On my return, I was told that my Achilles heel was lack of administrative experience. I took the most academically interesting of the major admin jobs, Departmental Supervisor – in charge of the curriculum, plus student registration (card index, in those pre-computer days). It was time-consuming, but I rather enjoyed being departmental curriculum-wallah. I did it for three years, and was promoted Reader in 1980 – while on my honeymoon. James followed in 1982.

No sooner had I committed myself to family life and fatherhood than the Good Ship Westfield began to sink beneath me. We did not know it until the middle of the academic year, but our 1983 intake was our last. The Department split: we had a choice between Royal Holloway College (University of London, but in Egham, Surrey) and Queen Mary College (Mile End Road). I found the choice difficult, but eventually chose Royal Holloway (staying in Hampstead till 1986 to see out our last intake – I lived, and still do, in N. London).

During this time, my first book [BGT], on regular variation with Charles and Jef, was being finalised. It appeared in 1987, the same year as the related book Resnick (1987) by Sid Resnick, another speaker. Cambridge University Press was still using hot metal (these were pre-TeX days for us as well as for CUP). The proof-reading was a nightmare. Misprints had to be corrected manually; the physical intervention necessary was liable to introduce new errors, so the iterations were improvements only overall, and certainly not in detail. We came through it, though not unscathed.

During my pre-US years, my friend, contemporary and later co-author John Hawkes (1944-2001) and I seemed the youngest figures in British probability (Geoffrey Grimmett, also a speaker, took his DPhil in 1974, as I left).

On and soon after my return in 1976, I found myself surrounded by the most extraordinarily talented group, whom I dubbed The Bunch. Their names read like a role of honour of British probability and related fields: David Aldous (back from Berkeley, and another speaker), Frank Kelly in OR, followed by Martin Barlow, Chris Rogers and Wilfrid Kendall (another speaker), with Terry Lyons (another speaker) soon after, not to mention Peter Green and Bernard Silverman in statistics (6 FRSs there alone).[15] I cannot resist mentioning here that I taught Ed Perkins (FRS, Martin's colleague at UBC) at the U of I, 1975-6.

David Williams wrote his first book, Volume 1 of what later became (with L. C. G. (Chris) Rogers) Rogers and Williams Volumes 1 and 2, in 1979; I thought it admirable, but remember finding it hard. Then in 1980 he co-organised the LMS Durham Symposium on Stochastic Integrals, a crucial event for me, and I think for all of those who attended it. The conference proceedings (Williams (1981)) begin with a 55-page survey, *To begin at the beginning*, which I have always thought of as the Epistle of St. David to the Anglo-Saxons. We take stochastic integration for granted nowadays, but (although I had read Meyer's exposition (Meyer (1976)) a great deal of missionary work remained to be done in the UK. The 1980 Durham Symposium was a wonderful step in this direction. It was also where I met the splendid Marc Yor (1949-2014). I remember his seeking me out and consulting me about Bessel functions, on which he regarded me as an expert on the strength of my early work on probability on spheres and the like. As I had tended to regard French probability as both formidably powerful and formidably abstract, I was struck that here was a French probabilist who could handle the most abstract theory, *and calculate.*

Harry Kesten had just proved his theorem that the critical probability for bond percolation on the square lattice is $\frac{1}{2}$; he spoke on this in Durham. For background, see Grimmett (1999).

The previous year saw an LMS Durham Conference on complex analysis, which was full of good things for probabilists. For example, Burkholder spoke on Brownian motion and Hardy spaces, Doob on Brownian motion and classical potential theory, and Burgess Davis on rearrangements. There I met Jaap Korevaar, whose distribution-theoretic proof of the Wiener Tauberian theorem I had long admired. He greeted me by saying "Tauberian Bing-

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[15]Those who follow football will be struck by the analogy with The Bunch at Manchester United, who made the career of Sir Alex Ferguson.

ham". I replied "Tauberian Korevaar". We both roared with laughter, and have been firm friends ever since (I spoke at his 80th birthday celebration in Amsterdam in 2003, and was able to make some probabilistic input to his magisterial book on Tauberian theorems, Korevaar (2004)). The link between probability and analysis (particularly complex analysis) was developed further in the excellent Durrett (1984), and is an area that Wilfrid Kendall has made very much his own.

6. London: 1986-99

My new college became Royal Holloway and Bedford New College (RHBNC), until it reverted to its former name Royal Holloway, or RHC. I became one of three probabilists: David (J. D.) Knowles and I from Westfield joined David Mannion, another pupil of David Kendall and a specialist in stochastic geometry and shape theory. After I had learned a new set of ropes, I found myself comfortably placed: I became a Professor in 1985, just after turning 40 and just before Ruth arrived. I was on the point of leaving in 1988, but was induced to stay, partly by being excused becoming Head of Department. The journey was awkward (a 26-mile drive, so one had to rise at crack of dawn to beat the London rush hour, or leave after 9:30; I left work at 7:30 in the evening). I found myself as a lone wolf on the research side – nothing new to me; I have always been self-propelled, and moved between working alone and with a well-chosen collaborator depending on the specifics of the project. But I knew that the departmental priorities – cryptography, and their kind of theoretical physics – would never allow me to build up anything like a group there.

I was lucky in my visitors at RHC. I succeeded in getting grants for two valued colleagues, Albert Shiryaev and Cindy Greenwood, to visit me simultaneously, in 1988. This was after their joint book of 1985 on contiguity. The visit leaves happy memories: of international football matches on my back lawn, and of Albert's special relationship with my daughter Ruth – Rufina Nikolaevna.

In 1990 I spent a semester at Iowa State University (with my wife and then two children), visiting Krishna Athreya. We had a learning seminar on Persi Diaconis' lecture notes on group representations ((Diaconis (1991): the cut-off phenomenon – "Seven shuffles suffice"). This linked with the algebraic side of my interests, going back to my thesis (probability on groups, symmetric spaces, hypergroups etc.; see e.g. [5], Bloom & Heyer (1994, §3.4.23)), and led to Bruce Dunham's thesis topic. For the analytic side of Diaconis'

interests, see e.g. Diaconis (2002), and my recent sequel to it, [126].

In 1995, Martin Barlow and I co-organised the LMS Durham Symposium on Stochastic Analysis. There were six keynote speakers: Aldous (the continuum random tree), Kesten (diffusion-limited aggregation) and Sznitman (Brownian motion with Poisson obstacles), included in [BB], plus Dawson, Meyer and Varadhan.

What led me to leave RHC was the death in office as Professor of Statistics at Birkbeck College (in Bloomsbury – the 'University of London night school') of my old friend Philip Holgate (1934-93). The then Master of the College moved to close down the Mathematics Department. I was at that time Chairman of the Board of Studies in Mathematics of the University of London, and was appealed to by the appalled maths staff. I got so involved that, when Philip's chair was eventually advertised, to carry two new posts with it, I applied, and accepted the chair when offered. I knew from knowing Philip, and from my dealings with his colleagues after his death, that this was a risky move. But I realised that the safety play of staying put and retiring as a singleton from Royal Holloway would leave me feeling that I had allowed life to pass me by, and wondering wistfully what I might have been able to make of the two new posts.

So I moved to Birkbeck in 1995, aged 50, as Professor of Statistics rather than of Mathematics. The two new posts dwindled to one, in a way that still makes my blood boil. But what a one: that was Rüdiger Kiesel (below), my friend and co-author. But Rüdiger left, for a Readership at the LSE. Meanwhile, I had long had good links with the Maths Department at Brunel University, in West London (almost on my way to Royal Holloway). Eventually, a combination of carrot at Brunel and stick at Birkbeck led me to leave the University of London, which I loved, at the end of 1999, after 30 years.[16]

I have always valued my German connections in mathematics, going back to meeting Hans Föllmer and Rolf Trautner, both at conferences in UK (Rolf at the Lancaster BMC, 1978). I became a regular visitor to Ulm, which was my 'home from home' for many years. Rolf's colleague and former pupil Uli Stadtmüller (another speaker) became a collaborator; I examined the Habilitationsschrift of his pupil Rüdiger Kiesel. This led on to the Birkbeck connection, and to [BK]. My valued German links also include those with Claudia Klüppelberg and Thomas Mikosch; how nice to have all three au-

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[16]Like father, like son: my father, R. L. Bingham, taught French for 30 years at Nunthorpe School, York, 1933-63.

thors of Embrechts, Klüppelberg & Mikosch (1997) as speakers.

Starting with [24] in 1981, my interest in limit theorems had begun to move from weak to strong convergence. I had always been fascinated by Tauberian theory (I remember falling in love with the proof of the Prime Number Theorem by Wiener Tauberian theory in Widder's book, while at Cambridge). During the 80s, I became deeply involved in the interface between probability theory and summability theory (of which Tauberian theory forms part), with particular reference to strong laws of large numbers (I have recently returned to this; see [124], [130]). I worked on this with Makoto Maejima [35], the first of my Japanese contacts, while he and Paul Embrechts were visiting me at Westfield. This also led to my work with Gérald Tenenbaum [37], through whom I have my Erdős number of two; this taught me a lot of analytic (as well as probabilistic) number theory, which I now teach. I returned to an old love, branching processes; I had worked on these with Ron Doney, and alone, in the 70s; now thanks to questions from Martin Barlow and Ed Perkins, I worked on it again, alone and with John Biggins. Inspired by seeing Kingman's regenerative phenomena (before the name) in Kac's book (Kac (1959, III.21-28)), I worked on Einstein-Smoluchowski theory and number fluctuations, with Bruce Dunham (my PhD student at RHC) and Susan Pitts of Cambridge. Akihiko Inoue (then of U. Hokkaido, Sapporo, now of Hiroshima) visited me at RHC to work on regular variation. Although I had hoped that [BGT] was thorough enough to enable me to move on from regular variation honourably, my first working session with Akihiko convinced me otherwise; we produced a slew of papers on this in the 90s, before moving on to Szegő theory later.

When I moved to Birkbeck, my predecessor as Departmental Chairman, Andris Abakuks, kindly drafted the teaching schedule, putting me down for a topics course for the MSc in Applied Statistics. That particular cohort of students were largely City practitioners (in the days when they still wore striped suits), and he conveyed a request to me from the class – to teach them mathematical finance. There was a theory; they didn't know it; their competitors did; they wanted to learn it. From the Black-Scholes formula of 1973 on, there had been a steady flow of ideas between probability theory and mathematical finance (I first heard about this from my friend and contemporary Mark Davis at the London Probability Seminar in the 70s, and then again in the Harrison-Pliska work of 1981, making the martingale link explicit). I had always had plenty else to do, and felt a fastidious disdain for

the area.[17] But, 'Or what man is there of you, whom if his son ask bread, will he give him a stone?' (Matthew vii.9). I gulped, said yes after a brief pause, and thus committed, learned the stuff (thanks to Paul Embrechts for guiding my first steps, during my visit to ETH, 1995). I promptly fell in love with it: it is such interesting mathematics. With my then new colleague Rüdiger Kiesel, this became the subject of my second book, [BK], and has remained an important part of my mathematical life – despite the disapproval of two of the best men and best mathematicians I have ever known, David Williams and Marc Yor.

The late, great Paul-André Meyer (1934-2003) often used to say that stochastic integration could have been created with mathematical finance in mind – but it wasn't.

The subject continued in excellent academic health: witness e.g. Meyer's selection Meyer (2000, p.848), and such books as Ethier and Kurtz in 1986, Jacod and Shiryaev in 1987, Revuz and Yor in 1991, Nualart in 1995, Bertoin (1996) and Kallenberg in 1997, the rough-path theory of Terry Lyons in 1998, and the development of super-processes, random trees and the like, in the hands of Le Gall (another speaker), Dawson, Etheridge, and others.

7. On my travels: 2000-06.

I started work at Brunel on 1.1.2000 (the post was conditional on my being in place in time for the March Research Assessment Exercise). I was very happy there at departmental level. At university level, things changed while I was there, and I ended by wishing fervently for less and better management, further away. I worked on various things, including mathematical finance, and taught an undergraduate named Bujar Gashi (an Albanian refugee from Kosovo), of whom more anon. I have happy memories of the running, along the Grand Union Canal.

I visited Japan in 2001, and again in 2002 (Makoto Maejima at Keio, Akihiko Inoue at Hokkaido, Yuji Kasahara at Ochanomizu). I loved Japan, and was delighted to get to know most of the grand old men of Japanese probability – Watanabe, Ikeda, Kunita and others, though not alas Itô, who was already ill – and Tokyo, Sapporo and Kyoto (each lovely in its own way; I fell in love with Kyoto).

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[17]After I got thoroughly involved, one of my friends told me that Marc Yor had asked him "What's Nick Bingham doing working on mathematical finance when he's such a good socialist?"

I visited Peter Hall, Daryl Daley and Joe Gani at ANU in 2003. I have very happy memories of my time there – including runs round the lake discussing Kant's categorical imperative with a fellow-visitor, the American philosopher Jason Stanley.

Again through a combination of carrot and stick, I moved in 2003 to Sheffield. This was the only Department of Probability and Statistics in the country, a range tailor-made for me, I thought; it would be nice to be in a Russell Group university, immune from the pressures of trying to haul itself up by its own bootstraps to move some way in that direction; it was in my native Yorkshire, indeed, in a beautiful part on the edge of the Peak District. I commuted (less stressful than the drive to RHC; I always worked on the train). I taught an MSc student named John Fry, later my collaborator (with Kiesel) on [110] and on my third book [BF] on regression. I found writing it great fun – though I wonder whether I as a probabilist would have had the courage to write a book on statistics without David Williams' example to follow (Williams (2001)). I acquired as a valued colleague my old friend Dave Applebaum (another speaker). Sheffield is famously hilly, so the running was strenuous – I was glad of my background on Hampstead Heath in my early London days, and many years of running from my parents' home in Snowdonia.

Since starting to age as an athlete, I have become interested in the statistics of aging in distance running, which combines nicely with the relationship between times over different distances [127]. Keeping a racing log provided a ready-made data set, which John Fry and I made good use of in [BF].

I took early retirement from Sheffield in 2006, aged 61, for family reasons.[18]

8. Imperial: 2006-15+.

At my drinks party on leaving Sheffield, I was asked by my friends John Greenlees and Vic Snaith of Pure Maths what I would do. I replied that I would be a gentleman scholar and house-husband (I can still remember Vic chortling as he replied "Oh yes – you'd be good at that"). But I was asked by Mark Davis and David Hand to come to Imperial as a Senior Research Investigator, which I gladly did. I have a little office high up in the Hux-

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[18]Cec ended her 12-year career break in 1998, when Tom was 5. The danger was that Granny looking after Tom might turn into Tom looking after Granny. I was determined that Cec should not put my career before hers for a second time. Something had to go.

ley Building, and use of the excellent Imperial resources. Requests to teach followed. It would be churlish to decline, and it's in my blood: both my parents taught (my father French, my mother English); my wife Cec is a fellow-academic; our daughter Ruth is a teacher. In teaching, research and every other way, I am having the time of my life at Imperial; I love it.

I looked up Adam Ostaszewski at LSE when I was newly back in London, and asked him again to look at the questions on regular variation that I had asked him in UCL in 1978. This time I was armed with [BGT], and urged him to read the first few pages. This worked; he took off like a rocket, and we have never looked back. I am a Visiting Professor at LSE also, with a home base there – unusual for a retiree to have two bases in central London! We have twenty-odd papers together, with around ten by him alone (or with Harry Miller), and a book to write.

In addition to his work with me – which has developed from specific questions about regular variation (which we have answered) to a wide-ranging study of the interplay between category and measure (after Oxtoby's book Oxtoby (1980), but with the emphasis reversed) – he has another line of work, with Miles Gietzmann, on disclosure (good news is trumpeted, bad news buried – what inference can be drawn from what is announced, or from no announcement?), the subject of his talk at the conference.

I visited Akihiko Inoue three times more (Hokkaido 2007, Hiroshima 2010, 11) after retiring, and worked with him and his colleague and former pupil Yuji Kasahara [115], [119], [132]. I became fascinated with Szegő theory and orthogonal polynomials on the unit circle (OPUC [116]), and their matrix analogues (MOPUC [117]), and applications to multivariate time series, particularly financial ones [120]. The passage from one to many dimensions led me naturally (and perhaps belatedly) to probability in infinitely many dimensions, the area of Dave Applebaum, Markus Riedle and many others, and to my current work on prediction theory with my colleague Badr Missaoui [123] and my research student Pierre Blacque-Florentin. This has links with filtering and control, the area of my colleague Dan Crisan, another speaker.

My recent work with Adam ([121], [128]) has taken me back to an old interest of mine in the 80s, moving averages. I was surprised and impressed to learn from Bujar Gashi, now my colleague at Liverpool (where I lecture on mathematical finance) that there was more to be said here (I had thought that Charles and I exhausted the area in [43] in 1988). Not at all: this has rekindled my interest in the field, and led to our joint paper [124], and [130].

Thanks to the work of Cindy Greenwood and Jim Pitman in 1980, Ron

Doney, Jean Bertoin, Andreas Kyprianou (another speaker) and others, the fluctuation theory of Lévy processes, an old love of mine from [15] in 1975, has enjoyed a renaissance, e.g. in actuarial mathematics; see e.g. Kyprianou (2013, 2014).

I have always had a profound respect for the Russian (formerly Soviet) school of probability, and for the French school. For the first, I had the honour of being invited in 2012 by Albert Shiryaev to speak at the B. V. Gnedenko Centenary Conference at Moscow State University on the world-wide influence of his work [122]. For the second, I had the honour to speak on the worldwide influence of the work of Paul Lévy, at the dedication of the Salle Paul Lévy in the University of Paris VI (Jussieu); it was there that I met Marc Yor for the last time. It is a great pleasure to have both Jean Jacod and Jean-Francois Le Gall speaking here.

Particularly since the work of Gelfand and Smith in 1990, the Gibbs sampler and Markov chain Monte Carlo have enjoyed explosive growth. The area is a lovely illustration of what probability theory, particularly limit theorems, has to offer statistics, by way of results, and vice versa, by way of problems. It has been a pleasure to watch Gareth Roberts' distinguished contributions to this field over the years. See the contributions by him and my colleague Alex Mijatović in this volume; also that by Bálint Tóth on scaling limits (see [126] for links with regular variation) and long-memory in models from physics, in both of which I have long been interested.

9. To be continued.

Probability in general, and British probability in particular, are now so well established that we do not need to try to gaze into the crystal ball on their behalf – a lost endeavour anyway, as the future of any scientific area that is genuinely alive is unpredictable even in principle.

I am now older than anyone who taught me, but not than a number who have influenced me. My old friend Cyril Offord (1906-2000) was publishing good mathematics into his 90s; (Sir) David Cox, now also into his 90s, continues as an ornament to the statistical scene in Oxford as he has done for decades. My former office-mate at Imperial, John Nelder (1924-2010), 20 years my senior, had a standing joke with me that he would show me how to continue active in later life, and did so. Nearer to myself in age, my old friend Albert Shiryaev (1934-) shows no sign of fading gracefully away. I propose to take my cue from such illustrious examples, and do my best in turn to set an example to the young, who are our future, of how to grow

older gracefully. As Cindy Greenwood says of growing older, more things get to seem familiar. Granted good health and a continuing love for what we do, mathematics, like a good marriage, can get better with time. I hope that those now young will lead by example in their turn, and be saying similar things to the young in fifty years time. You can tell the odd story about me if you like.

I close by expressing my deep gratitude to my past teachers, to my collaborators past and present, to the speakers and participants at this meeting, and to the editors and organisers, for all their good work, and for making the conference, and this volume, happen.

10. Postscript: on revient toujours au premier amour.

In 1965-6, I had two special subjects at Oxford: statistics/probability (above), and numerical analysis. What I loved about the second was Gaussian quadrature and orthogonal polynomials; the latter led (via David Kendall and delphic semigroups) to half my thesis (*Limit theorems and semigroups in probability theory*), to five of the eight papers that emerged from it, to a lifelong love of orthogonal polynomials and a lifelong interest in probability on algebraic structures, on which my mentor has been my old friend Herbert Heyer. I had the pleasure of seeing my old work on random walk on spheres [5] emerge as the Bingham hypergroup (Bloom and Heyer (1994), 3.4.23). But I always felt a sense of loss at the separation between the two strands in my mathematical life made visible by the two halves of my thesis. So I have found it very emotionally satisfying to bring them together, and this has now happened twice. The first was in [6], on Tauberian theorems for Hankel transforms. The Hankel transforms were from John Kingman's work on random walks with spherical symmetry (Kingman (1963)). I found a domain-of-attraction condition in terms of the transform, couldn't translate it into a condition on the distribution in my thesis, but did so in [6], by 'bare-hands analysis'. I spoke on this at the BMC in Kent in 1972; my old friend Milne (J. M.) Anderson (1938 - 2015) pointed out to me in a fatherly way that one should be able to do this by Wiener Tauberian theory; I saw that he was right, went away and did it ([20], [31]), and have loved Tauberian theorems ever since. The second is much more recent: a unification of my old interest in probability on spheres with my new interest in prediction theory for stationary processes ([115], [116], [117], [119], [120], [123]), through the work of Hösel and my old friend Rupert Lasser (Hösel and Lasser (2003)) – via the Bingham hypergroup. This is just in time for the work of my new

research student Tasmin Symons, under the Mathematics of Planet Earth
initiative. How all things come together.

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Afterword: On the analysis-probability interface

– not for publication.

I add here some thoughts on the analysis-probability interface, and on my own development as an analyst, at the request of my friend, collaborator and former undergraduate pupil Bujar Gashi, who teases me by saying that I am really an analyst, and so really a fraud as a probabilist, and should have the honesty to admit it, etc.

Analysis and Probability.

Grown-up probability is measure-theoretic (despite which, to my ongoing surprise and regret, far fewer undergraduates study measure theory than study probability, at least in the UK). Measure theory is a 20th century development, while probability has old (though, surprisingly, not ancient) roots. One probabilist can judge another in several ways – how analytical, how statistical, etc. To quote from the author's review (Bingham (2002)) of one of Stroock's books (Stroock (1999)): 'As he remarks in his preface '...I am not a *dyed-in-the-wool* probabilist (i.e., what Donsker would have called a true *coin-tosser*)' '.[19] One pleasure the book affords is the chance to place oneself on this probability/analysis scale (I consider myself fairly analytical as probabilists go, but less so than Stroock, to give one personal view).

The tensions resulting from the disparity between the vast number of people who need to use probability, as randomness is all around us, and those willing and able to master the mathematics necessary to do it properly, have been with us for a long time, and are well addressed in the Preface to Doob's classic book (Doob (1953); cf. [84]). We note that the treatment of measure theory in Doob (1953) is in a Supplement (p. 599-622); almost the only standard work on the subject then available was by Halmos (1950), his pupil. Doob returned to the subject in later life and wrote his own book on measure theory, Doob (1994).[20]

Apart from measure theory, the area of mathematics most obviously relevant to probability is functional analysis. The connection goes back at least as far as the work of Robert Fortet and Édith Mourier in the 1950s; see

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[19]It is one of my regrets that I never met Donsker, of the Erdős-Kac-Donsker invariance principle, etc. – though I am very glad to have known both Erdős and Kac.

[20]Doob maintained – no doubt for dramatic effect – that he only wrote the book to justify his purchase of a computer. He never forgot the effort involved in typing himself all seven versions of Doob (1953).

e.g. Fortet (1956a,b).[21] Probability on Banach spaces is now a great and growing field (Ledoux and Talagrand (1999)), in which I have long been interested. Infinite-dimensional probability in general is a field I have come to more recently [123]. The functional-analytic and measure-theoretic aspects come together in, e.g., the important area of empiricals, which is part of non-parametric statistics (van der Vaart and Wellner (1986)).

Complex analysis has deep links with probability, going back at least to Lévy's work on conformal invariance of Brownian motion (Lévy (1948); Durrett (1984, 5.1). Both have deep links with potential theory; see e.g. Doob (1984) for the first (which grew out of Doob's interests pre-probability), and Meyer (1966) for the second (and its five-volume re-working, with Dellacherie and Maisonneuve).

Complex analysis and functional analysis interact most obviously in the area of Hardy spaces (Duren (1970)). These have many applications to probability; see e.g. Burkholder (1980) for links with Brownian motion, [116] for links with time series and the prediction theory of stationary processes.

The links between Fourier analysis and probability arise most obviously in the characteristic function and its role in turning convolution into multiplication. More generally, one can see this theme in much of the work of the great and versatile Norbert Wiener, in random Fourier series (Kahane (1985, Ch. 5)), and in harmonic analysis on locally compact groups (Heyer (1977)).

All this calls to mind the preface of Chung (1968), where he grumbles that '...many still use probability as a front for certain types of analysis such as combinatorial, Fourier, functional and whatnot.'

Analysis and me.
To Hardy, an analyst was a mathematician habitually seen in the company of the real or complex number systems. These – $\mathbb{R}$ and $\mathbb{C}$ – are the simplest environments in which one can take *limits*; limits are the core of *calculus*; calculus is our most important weapon, in mathematics and in science more generally. It thus came as something of a shock to me as an Oxford freshman to be told that in order to do all this properly, we had to start again from scratch, and that, contrary to our (or at least my) fond imaginings, we did *not* know about the real number system, let alone limits. Thus began my

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[21]I had the pleasure of meeting Édith Mourier in Paris once. I was surrounded by good English speakers, and reluctant to inflict on them my then rusty French. She simply looked me kindly but firmly in the eye, spoke to me slowly and with impeccable clarity, and gave me no option but to reply in French – which was fine.

undergraduate exposure to analysis (the texts used then were Apostol (1957) and Titchmarsh (1939)). My first love at that time was geometry and matrix algebra; I found freshman analysis both hard and pointless. The turning point was meeting complex analysis in the second year; this was obviously both genuinely new (rather than being a prelude to a re-working of school calculus) and very powerful. I worked on my weak suit, analysis, and found that it became my strong suit. I knew that analysis was so vast that one needed a particular focus; I wanted to do something of practical value; this led to my choice of special subjects in my final year, statistics/probability and numerical analysis. The second is the source of my lifelong love of orthogonal polynomials (via Gaussian quadrature – §10). By the time I moved on to measure theory and functional analysis I was ready for them – and motivated. By then, I knew that I wanted to be a probabilist, and that this was the weaponry that was needed.

I remember my Oxford tutor John Hammersley telling me that the way to get into a new area was to read a small number of well-chosen books, but as I recall, it was during my Cambridge years as a research student that I realised that I liked books. I read Hardy, and Titchmarsh, and Wiener. I read Lévy in French (and was amused by the informality of the style: 'alors, ces probabilités sont assez petites . . . '), and Bourbaki in whichever language was to hand. I did German (and Russian) for scientific translation; I remember that Bochner (1948) was the first book I read in German, and that it was only the foreword that I found hard. I read Widder (1941) on the Laplace transform, where I fell in love with the Wiener Tauberian theorem on seeing it used to prove the Prime Number Theorem; this was reinforced later when I read Hardy (1949, XII).

When I began to publish in my London years, I realised that the unity in my work was that it was all probability or probabilistically motivated analysis, but that nevertheless the two were better published separately. Thus my random walk on spheres paper [5] appeared separately from the special-function theory on ultraspherical (Gegenbauer) polynomials [7], etc. A string of papers involving special functions (Jacobi series, Hankel transforms etc.) emerged from the algebraic side of my thesis; the motivation is probabilistic, but the mathematical context is Lie theory, representation theory, manifolds etc. My life-long involvement with regular variation grew out of my love of limit theorems and Feller Volume II, and the fact that there was then no text-book treatment of this area, which was too important to do without one. My resulting life-long love of Tauberian theory is also touched on above.

Tauberian theory forms a part of the much larger field of summability theory. My favourite theorem – the strong law of large numbers – concerns the almost sure aspects of a summability method, the Cesàro method C_1. Extensions involve other summability methods, and I wrote a string of papers on this in the early 1980s. Apart from that, summability theory did not much appeal to me – until one summer, when I had finished this string of papers, and thought I would complement it with a gap (or 'high-indices') theorem (see Levinson (1940), Hardy (1949, 7.13), [125, §3]). I couldn't do it. I tried again the following summer – and still couldn't do it. The summer after I found out why: the result I was trying to prove is false. The proof of this is a 'sliding-hump' argument from summability theory (Meyer-König and Zeller (1958, 1960); [125, §3]); this cured me of my reservations about the field. I have returned to the area recently, with Gashi [124].

Although I am no number-theorist, I have always loved analytic number theory ([37], with Gérald Tenenbaum[22], [75], [76]; [125, §2]), and the writings of Landau. When I read Landau's *Ergebnisse* (Landau and Gaier (1986)), I had a sense of déja vu: it reminded me powerfully of Titchmarsh. The part of summability theory most relevant to analytic number theory is Riesz (or typical) means (used in the analytic continuation of Dirichlet series). It has been a pleasure recently to link this with the Beurling moving averages encountered in my work in the 80s with Goldie [30, 43], and in my recent work with Ostaszewski [121, 132] and Gashi [124] (Bingham (2016)). And of course, probabilistic number theory is an area I find irresistibly attractive. Primes play a game of chance (Kac's dictum); It's obvious that the primes are randomly distributed – it's just that we don't know the rules yet (Vaughan's dictum); linking these ((Cecilie) Bingham's dictum): Primes play a game of chance – we just don't know the rules yet [125 §2].

It was rather a shock to me when it emerged in my work with Ostaszewski that some results in regular variation *disaggregate* when one attempts to generalise them: the results that emerge depend on the axioms of set theory that one uses [103]. I realised that my previous attitude – the common one (use Zermelo-Fraenkel (ZF); augment it with the Axiom of Choice (AC) to get ZFC when needed; anything else is best left to mathematical logicians and model theorists) – is too naive: by weakening AC, or using an alternative, one can make *all sets measurable*, or *all sets have the Baire property*, etc . Indeed, my Hardyesque feeling of comfort with $\mathbb{R}$ and $\mathbb{C}$, solidly based (as

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[22]to whom I owe my Erdős number of 2

I thought) on constructing $\mathbb{R}$ both ways as a student (Dedekind cuts and Cantor's completion via Cauchy sequences) is thereby itself revealed as too naive, witness such books as Bartoszynski and Judah (1995) and Bukovsky (2011). I am fortunate to have in Adam Ostaszewski a collaborator who grew up with such things. But this was already clear in 1978 (§5).

It was also rather a shock to me when it emerged in my work with Gashi that in the area that Goldie and I thought we had closed in the 80s there is still much to say [124; 135, 136]. The moral is that there is always more to say, thanks to the inexhaustible richness of mathematics.

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