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A Conversation with David Kendall

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Abstract. David George Kendall was born on 15 January 1918 in Ripon, Yorkshire, and was educated at Ripon Grammar School. He studied Mathematics at Queen's College, Oxford (1936–1939), where he held a Hastings Scholarship. He was taught by Haslam-Jones and much influenced by Milne and Titchmarsh. His early interests were in astronomy. He spent the war years at the Projectile Development Establishment working on rockets under Cook, Rosenhead and Knight, together with Anscombe, Bartlett, Moran, Rankin, Slater and others. In 1946 he reentered academic life as a Fellow of Magdalen College, Oxford, where he was a colleague of Henry Whitehead. He became the first Professor of Mathematical Statistics in the University of Cambridge (1962) and continues to work there in retirement. He has been a Fellow of Churchill College, Cambridge since 1962. He was one of those who joined with Jerzy Neyman in the campaign for the establishment of the Bernoulli Society, and he became its first President (1975). His honors and awards include the Guy Medal in Silver of the Royal Statistical Society (RSS) (1955), Fellow of the Royal Society (FRS) (1964, Council 1967–1969, 1982–1983), President of the London Mathematical Society (1972–1974), Weldon Memorial Prize (1974), Sylvester Medal of the Royal Society (1976), Honorary D.de l'U. (Paris René Descartes, 1976), D.Sc. (Oxford, 1977), Senior Whitehead Prize of the London Mathematical Society (1980), RSS Guy Medal in Gold (1981), President of the Mathematics and Physics Section of the British Association (1982), Milne Lecturer (1983), Honorary Fellow of Queen's College Oxford (1985), Hotelling Lecturer (1985), Honorary D.Sc. (Bath, 1986), Sc.D. (Cambridge, 1988), De Morgan Medal of the London Mathematical Society (1989), Member Academia Europaea (1991), Honorary Member of the Romanian Academy (1992). He was joint editor of *Mathematics in the Archaeological and Historical Sciences* (1971), *Stochastic Analysis* (1973), *Stochastic Geometry* (1974) and *Analytic and Geometric Stochastics* (1986). He has collaborated extensively with G. E. H. Reuter on stochastic analysis and with Huiling Le on stochastic geometry.

David Kendall married Diana Fletcher in 1952; they have six children. The eldest, Wilfrid S. Kendall, has collaborated with his father and is a Professor of Statistics at the University of Warwick.

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The following conversation took place in the Statistical Laboratory, University of Cambridge, on 11 January 1994. The transcript below is a lightly edited version; additions are added in square brackets. Works of Kendall are referred to by number, in square brackets; works of others, by name and year in parentheses.

1. EARLY LIFE

Bingham: You were born in 1918 in Ripon, Yorkshire; your family origins were in Whixley and Staithes, I believe.

Kendall: That's right.

Bingham: I know you're very interested in family history, and have traced your ancestry back quite some way.

Kendall: I've explored most branches of my family back to about 1700 or so. But then they become rather difficult to trace, because they weren't very important people . . .

Bingham: I'd like to return to your interest in genealogy later, but perhaps we should talk about your schooldays first. You were educated at Ripon Grammar School. At what stage of your school career did you begin to think of going to university to read mathematics specifically?

Kendall: I wouldn't put it quite that way. What I wanted to do was to become an astronomer, and mathematics was a prerequisite, obviously, for that. But for a very long time—until I was about 21, I suppose—astronomy was the ultimate target.

Bingham: Could I press you on how far back your interest in astronomy goes?

Kendall: I used to talk to my maternal grandfather, who was a deep-sea sailor and knew his stars; I think it probably started there.

Bingham: I can remember vividly being shown the sky at night by my own father, learning stars by name, seeing shooting-stars, that sort of thing, and becoming absolutely fascinated. I suppose that I remained below the threshold, whereas you clearly went above it.

Kendall: Well, I think the event that switched me from astronomy as entertainment to astronomy that might become my profession happened in the early thirties, when Sir James Jeans gave a popular series of radio lectures on astrophysics, which probably most people switched off, but I listened right through, and there it became evident that one would have to learn mathematics. And I had a Charles Lett's Schoolboys' Diary, which had a collection of formulae—calculus and trigonometry, but with little explanation. I managed to teach myself both these subjects, although for a long time I had great trouble because there was no very clear explanation. I got confused between sines and cosines; that obviously wasn't explained in a way that I could follow. At that time school work was rather boring; when I'd finished what I was doing I would fish out my Schoolboys' Diary and try to understand another trigonometrical formula, and one day when I was doing this one of my teachers, George Viccars, came up and saw what I was doing. He was actually the senior maths master in the school. From then on life was very different, because he went straight home and got together his Cambridge Part I lecture notes and handed them over to me!

Bingham: Wonderful.

Kendall: And I started reading Hardy's *Pure Mathematics*.

Bingham: Do you remember how old you were at the time?

Kendall: About 13 I suppose.

Bingham: What did you think of Hardy's *Pure Mathematics*? What do you think of it now, looking back on it?

Kendall: Well, I think in a way it's a mischievous book, because I should think most people would be utterly baffled by the first chapter. The curious thing was that it was the first chapter that I loved—the Dedekind sections and so on. The remainder of the book—how to integrate complicated products of trigonometric functions—I found, as I should, very dull.

Bingham: Yes! Thank you. Presumably you studied geometry and the like; did you learn your Euclid from Euclid, or from . . .

Kendall: Durell. I used to work very hard at Durell. In that book *Modern Geometry* there are some ferociously difficult problems—like, for instance, Feuerbach's theorem . . .

Bingham: Good heavens—that's for grown-ups! That's not for schoolboys . . .

Kendall: Well, yes, I certainly couldn't prove it now . . .

Bingham: No, no indeed, nor could I, David. Presumably you did the work for your first public examination—School Certificate—and then went into the Sixth Form to prepare for university.

Kendall: I had trouble with the School Certificate, because I only got what was technically called a weak pass in French, and later on this was a big obstacle to me.

Bingham: No one would know it, as you've published in French.

Kendall: It's rather interesting, because you see there was a delightful old man who had been Regius Professor of Botany in Glasgow, F. O. Bower FRS, who retired to Ripon, which was his birthplace. He was the typical figure of the Victorian scientist, with a beard and so forth, like Father Christmas, and he used to stop and talk to me in the town whenever he saw me, because he knew what sort of interests I had. And one day he said to me "Now I know you're working very hard at your mathematics, and that is as it should be. But you must take care with your French. Do work hard at your French. I worked hard

at my French, and in due course they gave me an honorary degree." I took this to heart, and history repeated itself . . . [laughter]

Bingham: Wonderful, wonderful!

Kendall: This had profound consequences, because Bower knew lots of people. He wrote to an old friend of his, equally old but in those days still holding his chair, the Professor of Astrophysics in Cambridge, whom I never actually met, but he put me in touch with the firm Adam Hilgers, who made and still make spectroscopes. One summer, when we were holidaying in Felixstowe, my mother—who was very keen that I should develop these interests—organized at great expense that we should go, first of all, to Cambridge—which was a long way from Felixstowe—to see Professor Newall, who, however, wasn't there. Nevertheless, we were received by the chief technician in the Solar Physics Observatory and shown everything. That of course was a tremendous thrill for me. After that we went to London to visit Adam Hilgers and met the top man there, Frank Twyman FRS, who arranged for me to see all their projects. The last expedition we made was to the Royal Observatory in Greenwich. My mother had written to say that I might want to make my career in astronomy. What happened was that we were received with a lot of tourists and shown the longitude line, various telescopes and so forth. Then the man who was taking us round—the uniformed Office of Works bloke—said "Well now, you're the *boy*, aren't you. Mr. Edney wants to see you; he's the Secretary to the Observatory." So I saw this Mr. Edney, who said, "Yes, we've had a letter about you. I suppose you realize that astronomy is not a profession where there are any jobs." I said that I was hoping when I was older to get a job, say in this Observatory or some other, and were there *really* no prospects? And he said, "No, absolutely none whatsoever. The only people we take on have been to Cambridge, have degrees in mathematics, and so forth." So I said that this was exactly what I wanted to do. "Oh well, then of course there might be some chance."

Bingham: The strongest possible motivation . . .

Kendall: Or an attempt to put me off—but it didn't work!

Bingham: Yes, that's lovely. Your time in the Sixth Form, David: did you do the two years, and then the third year—or part of it—as a Scholarhip candidate, as was done in my day?

Kendall: As far as I can remember, I had four years in the Sixth Form. There was a great deal of misinformation in those days. For example, there was a girl at Ripon, remotely related to us, much older than I, who had a State Scholarship. She had all her fees paid, and so my mother said, 'Well, that's obviously the thing you've got to do. You must work hard: you mustn't just *pass* all these examinations, you've got to do really well in every one of them, and then perhaps you'll get a State Scholarship." So that was now my target, and I stayed at school until I got one—only to discover that it would only be for a nominal amount. It was a terrible blow to us, and it did lead to a wasted year, essentially.

Bingham: I'm so sorry.

Kendall: Still, maybe it was a year that was well spent in other ways.

Bingham: Consolidation.

Kendall: Learning languages, for instance—French . . .

Bingham: Yes! French!

Kendall: I'd like to enlarge on that a bit. Ripon Grammar School was a very small school in those days, it had just about 200 boys—only boys, though it's now happily mixed—and the staff was fantastic. The Headmaster was a very good judge of people, and he collected mostly Cambridge staff because he was a Cambridge man himself. Many of them became very close friends of mine. Just the other day one of the staff came to see us, now in his eighties, and we had a wonderful time remembering the old days. But they were all marvellous people; they not only knew their subjects really thoroughly, and were in love with their subjects, but they spent a huge amount of time taking boys out on expeditions. One of them introduced me to brass-rubbing—you know, things that enlarged one's horizons.

Bingham: You've always had notably wide horizons, David.

Kendall: Well [laughs], if that's so, it's because of those splendid people.

2. OXFORD

Bingham: You then went up to Queen's College, Oxford. Was there any particular reason for the choice of Oxford and Queen's?

Kendall: There's a story about that. My mathematics teacher George Viccars had been at Caius, and it seemed to me the most obvious thing in the world that I should go to Cambridge and to Caius in particular, because what other College could be better? I competed for a Scholarship, but I didn't come high enough in the list to get anything better than an Exhibition, and from a financial point of view it just had to be a full scholarship. This was found very satisfactory by my school, because they wanted me to go to Oxford. The reason was that Ripon was one of the 20-odd schools in Yorkshire and Westmoreland who had access to the valuable Hastings

Scholarships at Queen's. So they said, "Never mind about Cambridge, Oxford's the place for you; start working towards that." This got me into difficulties again, because to get a Scholarship at Oxford was one thing, but you also had to pass what was called Responsions. At Cambridge just Latin sufficed, but at Oxford it had to be Latin plus a modern language. I had done badly in French in the School Certificate Examination, and so was now given a huge dose of extra French at my school, and a further dose of extra French from a splendid woman who taught the girls at the Girls' High School. They managed between them to get me through, and that's how I got into Oxford.

Bingham: Now you went up to Queen's, as a student of mathematics, intending to specialize somewhere along the line in astronomy. You were taught I believe by Haslam-Jones. Was he the only Mathematics Fellow at Queen's then?

Kendall: Yes, he was, apart from A. E. H. Love, who was Sedleian Professor of Natural Philosophy, and author of *Elasticity*. Haslam-Jones arrived when I did. His predecessor was still under the old Statutes, as a Tutor for life, but had just then decided to retire because he'd done some arithmetic and discovered that a hundred of his pupils had got First Classes, and that was obviously a good time to go. Delightful old man—and there was another special thing about him: he was an old boy of Ripon Grammar School, which may well have played some part in my getting an award.

Bingham: How amazing! What was his name?

Kendall: C. H. Thompson

Bingham: Ex-Ripon, and a hundred Firsts. Absolutely wonderful.

Kendall: Haslam-Jones was a Queen's man himself and had been brought up by Thompson. He was determined to change the mold, however, and he did it in a drastic way. Thompson was very old, and old-fashioned in some ways, but still made all his pupils read Goursat, and he made them read it in French, because it was considered that the American translation was too inaccurate. But Haslam-Jones went one better; he started me straight away in my first term reading de la Vallée Poussin's *Intégrale de Lebesgue*, and pushed me very hard in that direction—which of course has colored my outlook very much. He was a most remarkable teacher.

Bingham: Forward-looking, at the time?

Kendall: Remarkably so. (By the way, HJ gave me one of his own offprints which cited a man called Kolmogorov!)

Bingham: And all this *in your first term*? There is a school of thought that strongly believes one should kick off with the Lebesgue integral, but most of us don't have the courage of those convictions, and are saddled with the Riemann integral . . .

Kendall: Which is harder.

Bingham: Indeed. Horrible too.

Kendall: I'd like to tell you a bit more about what happened when I arrived. I had by now good friends at Adam Hilgers. The Managing Director Frank Twyman wrote to me from time to time and gave me much good advice, but what was crucial was that he wrote to H. H. Plaskett, who was the Professor of Astronomy in Oxford, and as a result I was invited in my first week to go to the Astronomy Colloquium, which I did. And there at the Colloquium were many people who were to become lifelong friends. One of these was Professor E. A. Milne. Milne was a good man at breaking the ice with young people and talked to me for a long time. He told me to come to tea at his house in Northmoor Road any time I liked; I used to go there and chat with his small children—a couple of girls, one of whom I met again recently, much older of course! It was an interesting reunion. Milne and Plaskett really fathered me for a long time. It was only later that I got to know Titchmarsh; I suppose I first encountered him giving fairly straightforward lectures on complex analysis, which I knew pretty thoroughly anyway. Later on I went to his Advanced Classes on Fourier integrals and on prime numbers, and also to Milne's Advanced Class on his version of relativity—kinematic relativity—and I met another lot of very interesting people there.

Bingham: One gets the impression that Titchmarsh wasn't such an outgoing man.

Kendall: He wasn't outgoing, but he was very generous with his time. It was the form for Professors to give Advanced Classes. Titchmarsh had one Advanced Class—which was really a lecture—on Fourier integrals, and another on prime number theory. I went to the number theory class, and to my dismay found I was the only person there. But Titchmarsh wasn't at all bothered, he stood before the blackboard—he might have been lecturing to 500 people—and lectured just to me.

Bingham: Just last week I was reading Mark Kac's account of a similar experience he had with Steinhaus. So to be the only person in the audience can have its advantages!

How did your interests evolve during your time in Oxford? Did you stay the full three years, and only three years?

Kendall: I had the full three years. The first term was difficult, because I became involved with what was then called the Oxford Group Movement, later called Moral Rearmament. That was all right, but the way it worked in Oxford was that they had what

they called a Team Meeting every day from five to six p.m., and lots of other activities besides. My work began to fall away because of this double load—a second university education alongside the other one—and it came to a crisis when it was clear that my work was suffering. My parents talked to me, as they clearly had to do, and I gave up that association. As a result I was very short of friends, because most of my friends had been people involved in the Oxford Group. However, I had several good friends in Queen's who kept me going, but the next two or three terms were pretty miserable.

Bingham: Do pardon my ignorance, but I believe the Oxford Group Movement was born out of the Anglo-Catholic movement in Oxford in the nineteenth century, is that right?

Kendall: No, that was a different Oxford Movement! The Oxford Group Movement were people who were often called Buchmanites, after Frank Buchman, who was their "apostle," and they went in for total sway over their acolytes in every part of their lives, which was pretty tough.

Bingham: Was this proselytizing?

Kendall: Oh, yes!

Bingham: Go out and convert the heathen?

Kendall: Yes.

Bingham: I see. I'm very glad that you had the sense to give that up, David. The proselytizing has left no trace on you; you've turned away from that?

Kendall: Well . . . I think that under psychoanalysis it might become clear that I am still deeply involved with it—in the background! Proselytizing is one thing; caring in a very deep and important way for one's pupils is another, but nevertheless there is a sort of . . .

Bingham: Yes. Now I know you're a devout Anglican, and you've passed this on, certainly to Wilfrid. Did you get your Anglicanism directly from your parents?

Kendall: No, I think in a sense they got it from me. They were both of them brought up in the Methodist tradition. My maternal grandfather [the former sailor—Kendall's paternal grandfather died before his father was born] was a devout local Methodist preacher—though he later became a Spiritualist, which is another story. There are lots of things I could tell you about my grandfather. He was a magnetic healer and a practicing herbalist. He would do extraordinary things. There's a famous story which I remember because I was old enough to be told what was going on. There was a man who had an inexplicable pain in his shoulder-blade. No doctor could deal with it. My grandfather wrote to him, enclosing a handkerchief, and said, "Pin this inside your shirt, just over the place where the pain is, wear it for a month and then send it back to me. I will then pin it over my shoulder-blade, and after a month I will tell you what to do." When this was done, he wrote to say, "I think you ought to drink an infusion of such-and-such herbs"; quite soon the man was cured. That kind of thing. And of course when that happened (I was very small, about six) I thought *all* grandfathers were like this! Later on I realized that he was a very unusual man [laughter]. He was a wonderful and warm person too.

Where were we? The first year had been got over. After that, I was secure and happy with a number of good friends. The closest of these was Kenneth Thornhill, a Sheffield boy. We met when we were both competing for Hastings Scholarships, and we became lifelong friends. I worked with him during the war, and I still see him. He became a Deputy Chief Scientific Officer in the Civil Service, and now in retirement he's writing most interesting papers on relativity theory.

Bingham: Wonderful.

Kendall: Another close friend was Malcolm Robins, another Queen's man. He is a physicist; in our third year we shared digs together. I learned a lot of physics in that way; really, all the physics I know I learned from Mac. He went to Farnborough and eventually became the Chief Administrator of the space program in the UK. He now has a CBE [Companion of the British Empire]—a very distinguished career! Then there was Arthur Spencer, another Yorkshireman, a classicist and later a lawyer. We're godparents to each other's children and meet regularly. A lot of Queen's people were Yorkshiremen.

Bingham: Yes. You graduated in 1939. Did the Oxford system at that time have any equivalent of the Cambridge Wrangler list?

Kendall: No, you simply got a First or not. If you did get a First, perhaps even if you did not, you might get a letter "f" or whatever after your name—highly distinguished in the special paper, the theory of functions, in my case. And now I was in a real quandary, because I was deeply in love with analysis and yet I also wanted to become a professional astronomer—and of course this was a nonsense. I simply could not decide which way to go. In the end I was advised to compete for the Senior Studentship in Astronomy at Balliol, which had just become vacant. There is only one of these, and one only had an opportunity to compete for it if someone else had just used up his three years. I applied, and was awarded it. So there I was, committed to becoming an astronomer but still unhappy about it unless I could combine it with pure mathematics in some way. In fact, I had already done that. Talking

to Milne and Plaskett and reading the astronomical literature I came across a rather interesting integral, which is actually the convolution of a Gaussian distribution and a Cauchy distribution, for which there is no closed form. I produced a series by means of which these could be computed, and it was published in the *Zeitschrift für Astrophysik* at the end of my second undergraduate year [1]. That was a good thing to have done, of course. It impressed people awarding research scholarships, and it pushed me towards astronomy, although it was really a piece of analysis. I was still torn between the two subjects and couldn't see how the conflict would be resolved, but Hitler resolved it for me.

3. THE WAR

Bingham: Hitler resolved it for you! The Second World War began in September 1939, when you were presumably about to take up your Skynner Studentship at Balliol. What happened then?

Kendall: We all went before a Board, which told us what we had to do. They told me to carry on with astronomy until I was needed. When I was needed turned out to be March 1940, when I was told to report to the Projectile Development Establishment.

Bingham: "PDE" means "partial differential equations" in ordinary mathematics, and to exrocketeers like yourself it means "Projectile Development Establishment."

Kendall: I used to tell my parents it meant "PLEASE DON'T ENQUIRE"! [Laughter]

Bingham: That's lovely! At PDE, you worked I believe with Frank Anscombe, Maurice Bartlett, Pat Moran, Robert Rankin and Noel Slater [13], under Cook and later Rosenhead. Perhaps you could fill us in on PDE, your time there and your contact with these people.

Kendall: I can only tell you about PDE in a very general sense because I am still covered by the Official Secrets Act. The Establishment was in West Wales, and it was a very beautiful part of Wales, so that was extremely agreeable. As a Home Guard one did one's exercises on the mountains, and that was fun. PDE was concerned with rocket weapons of all sorts. There was a firing establishment, and also a house in the country where the administration and the mathematicians worked. Initially it was impossible, if you were a mathematician, to be allowed to go and even *watch* the firing trials. I remember—and I'm very proud of this—in the first week I'd done some calculations—minor calculations but necessary ones—for a particular weapon for the Navy, and I learned that towards the end of that week there was a firing trial, to which my senior officer was going, so I said, can't I come with you? He was a professional, and of course he said "No, it's out of the question. We can't have you watching firing trials!" Having been trained by people like Plaskett who maintained that however theoretical you are, you must rub your nose against the telescope, I was not going to stand for this.

Now the Head of the section was the famous Sir William Cook (Penney and Macklen, 1988). So I wrote—I must have written to Cook, I think—and anyway, Cook said "Yes of course, let him go!" This permanently changed the pattern, because after that, whenever there was an experimental firing, the people working on it, if they wished, could go and watch it, and it was important that they should, because if the projectile turned round backwards and shot up the observers, your head would be on the block, wouldn't it? That was a nice thing to have done.

Most of the time, in the early days, one was simply doing rather long repetitive calculations, using the Brunsviga machine if you could get your hands on it and a meter-long slide-rule if you couldn't, and this could be very boring. I got into the way of keeping my interests alive by thinking about various aspects of the behavior of these weapons, and how these might or might not have military significance. No one considered this an appropriate way to spend my working time, so I used to go into the establishment every Sunday and do the work there. And in due course, my report was approved, and sent to the Ordnance Board! A little bit after that, I was sent to the Ordnance Board myself to talk to Egon Pearson and Bernard Welch, who were the statisticians there, and I said, "I think my former professor E. A. Milne is here, isn't he?" "Oh yes, he's a full Member of the Board; his office is the third door down the corridor." So I went along, and chatted to Milne a bit, and he said, "What have you been doing?", and I said that to relieve the tedium of ordinary ballistic calculations I had written a report on such-and-such, which he might have seen. "Indeed I have!", he said, "It was exactly what we needed!"

Bingham: Yes. I had imagined that it was Milne's influence that led you in this direction, as Milne had been in antiaircraft work in World War One and in the Ordnance Board in World War Two (McCrea, 1950/51).

Kendall: Milne had been in the Navy in the First World War, and he urged me to go into the Navy too, during that ambiguous period between finishing my undergraduate career and getting fixed up with something else. I tried to become an Instructor Lieutenant RN. There were several vacancies, and I put in for one. I remember it as a humili-

ating experience because there was a tremendous form—much longer than an income tax form—with a narrow little band about an inch wide in which to write up one's academic career, and huge boxes for sporting achievements in which I could write absolutely nothing! So not surprisingly, they didn't take me on. Later in Wales I joined the Home Guard, and I quickly became a Sergeant, and every now and again I used to say to the man who ran the Home Guard, I can't go on being a Sergeant, because my official work's terribly pressing, so will you please reduce me to the ranks, and then a few months later they'd put me up to Sergeant again [laughter].

Bingham: Lovely, lovely! By the way, you're not alone in having been turned down as an Instructor Lieutenant; George Barnard was, too (De Groot, 1988). I believe that Louis Rosenhead was your boss . . .

Kendall: Rosenhead succeeded Cook as my boss; Cook was rightly bound for much greater responsibilities, though he was still within the same organization. Rosenhead was an especially kindly man. I loved him dearly. I'm still in touch with his widow, and I know his two sons; eventually he became a close friend.

Bingham: Rumor has it that Louis Rosenhead said to you, "PDE needs a statistician. I'll give you a week to learn the subject." Is this apocryphal?

Kendall: It is not apocryphal. It happened when Maurice Bartlett and Frank Anscombe, who had been the statisticians at PDE, were needed in London, where Cook now was. So we were without a statistician and those were the words, roughly, that Rosenhead used. "I'll give you a week to learn the subject. Go up to London, and stay in Anscombe's lodgings. You'll help him during the day with whatever he's been doing, and at night he will teach you statistics." What Frank did was to take me through Bartlett's Cambridge lecture course [for background here, see Olkin (1989)]. And that's the only tuition I've ever had in statistics.

Bingham: So after a week you went back to PDE as their statistician, did you?

Kendall: Well, the statistics we had to do there was not at first at all demanding. One had to know how to do a 2×2 contingency table test and how to do an analysis of variance. But from time to time there were also elaborate probability calculations, and these I was happy with.

Bingham: In Robert Rankin's monograph on rockets (Rankin, 1949), he gives some background about the general need for statistics with rocket work as opposed to shells, because of the uncertainties induced by irregularities in the gas flow, or asymmetries of design, during the "burn" period. Hence questions of dispersion would obviously be very relevant to a weapon's performance and usefulness. But why the emphasis on rockets, rather than, say, conventional artillery, at this stage of the war?

Kendall: The great need after Dunkirk was to develop some substitute for all the guns and predictors [devices for directing antiaircraft fire] we had left in France, after the evacuation from Dunkirk [the British Expeditionary Force had to abandon all its heavy equipment in the retreat to Dunkirk and the evacuation of a third of a million men from the Dunkirk beaches in the famous armada of small boats]. We had practically no 3.7-inch or 4.5-inch antiaircraft guns, and no predictors, and there had to be *some* substitute for them. The obvious thing for the time being was to use rockets. But there was no fire-control system for rockets: that had still to be devised. Duncan Sandys, as a Major, and Kenneth Post, as his Battery Captain, had been in the Norway Campaign. The brief Sandys and Post were given was to remedy this state of affairs, and they had an Experimental Battery which was located in a field just next to our firing site. Rosenhead fixed up a new Section with Colin Knight in charge, Gordon Sparrow as his number two and myself as the dogsbody [assistant]. We liaised with the Battery. They did a remarkable job. For example, they built a predictor with Meccano [a brand of construction toy beloved by generations of British schoolboys] , and in due course it went into action—a Battery controlled by Meccano! They invited me to go and spend some time with them, and have the fun of watching this Heath-Robinson predictor working [Heath-Robinson: a proverbial inventor of unlikely gadgetry] . . . the guns firing . . . we thought we'd shot a plane down but it was disputed by the authorities. Wonderful, very exciting. They were both people I got to know: I knew Sandys, and worked for him again later, but he was . . . rather a grand man; I got to know Kenneth Post personally, and he's still a very good friend. They both come into the story again later, when they asked for Rosenhead, Anscombe and myself to join them during the Flying Bomb [the V1] Blitz. That, however, was more of an Intelligence operation, but statistics still played a role.

The time I spent with Duncan Sandys and Kenneth Post was an experience that taught me a lot. I don't quite know how I stand with regard to the Official Secrets Act about that, and I did think, actually, that this might be a good time to write and ask them . . . I have got, somewhere, a list of the ti-

tles of all my wartime reports. Some of them are probably still on the restricted list, . . .

Bingham: It would be very nice for posterity to be able to look at this interesting period in as much detail as is appropriate. What amazed me, when I began preparing for this interview, is how many fascinating things there are on the published record. The first thing I did was to consult the various obituaries. For instance: Lord Penney's on Sir William Cook—that was a fund of information, including mentioning specific weapons, like the 3-inch unrotated projectile, as a stand-in for the 3.7-inch antiaircraft gun, which you mentioned just now, Duncan Sandys's role as commander of the first regiment using this weapon, near Cardiff, which you mentioned just now; Bill McCrea's obituary of Arthur Milne, which was fascinating about both world wars; Trevor Stuart's on Louis Rosenhead; and Chris Heyde's obituary of Pat Moran, which I think you had a hand in—absolutely fascinating. Robert Rankin's monograph on rocketry—I haven't gone into the mathematics, but what I immediately picked up was the names of people who are household names as mathematicians. J. L. Kelley of *General Topology* was a rocketeer; so was E. J. McShane of integration (Kelley, McShane and Reno, 1953), and J. Barkley Rosser of Rosser's sieve in number theory (Rosser, Newton and Gross, 1947), . . .

Kendall: It amazed me that they published that (Rankin, 1949) immediately after the war, and the only explanation I can think of is that they wanted to get the British work on the record, so that the Americans shouldn't claim that they did it all! Because it could quite reasonably have been put under the secret blanket and kept forever. I suppose if one looks back *now*, it's all very old hat, naturally—but at the time, it certainly wasn't. In that world Rankin's name will always be a household word, like Littlewood's.

Bingham: Yes indeed. One is also aware of the enormous use that the Red Army made of rocketry in the Second World War—Katyusha rockets are a familiar sight on television screens. Perhaps less familiar is the use of massed batteries of 5-inch rockets during D-Day, and rocket-firing aircraft during the Battle of Normandy.

Presumably the emphasis in everything to do with rockets changed dramatically when manned spaceflights started, and at that time the Kalman filter was becoming available. The emphasis changed to actual control of rockets in flight, whereas in the early days everything had to be done before the rocket was fired.

Kendall: Yes, and their production had to be done by inexperienced people, very often working without the proper tools and in an atmosphere of nightly bombing . . .—that you could get anything done at all was a miracle. The people who made the rocket projectors for the original 3-inch rockets were bedstead manufacturers, I believe. The sights had to be set with nail files. I remember Churchill was coming to see an experimental firing of these things, and we went along to check the sights—and, fortunately, somebody had a nail file!

Bingham: Is there anything more you'd like to say, or shall we pass to the end of the war and your time with T-Force?

Kendall: Oh yes, T-Force. Well, it was obviously desirable when the Germans packed up that as many people as possible should go roaming around seeing what they could find in the files and talking to people in the various scientific establishments the Germans had set up. You only had to mention that it might be interesting to talk to so-and-so and it was at once fixed up. You were put into khaki, technically a Captain but without badges—which was a rather unsatisfactory situation—and whizzed off. There was a particular person I wanted to talk to, who had been doing work similar to something we were interested in. This meant a long and complicated journey to Lübeck right on the boundary of the Russian Zone—which was a difficult journey because often there were no bridges over the rivers—only to be told "Oh well" (it was a woman scientist), "she's gone to the French Zone"—and of course, we couldn't reach her there. That was that.

However, I still had a fortnight to fill in and I decided to use it profitably. I went to look at a number of places that were interesting. One was a giant ballistic proving ground built in a forest so that you couldn't see it from the air—and who should be running it but Kenneth Thornhill, and another friend Ernest Hicks. And I went to Göttingen and looked at the observatory there, and the telescopes they'd built during the war. I got a lot of offprints from the Director, Professor ten Bruggencate, and took them back to the Royal Astronomical Society. He also gave me a picture of Gauss on his death-bed, which of course I treasure very much. But on the whole it was rather a free ride. Nothing much was achieved, but it was all extremely interesting.

Bingham: I'm sure it was. That's the luck of the draw. You know about Harry's experiences, of course . . . [Harry Reuter and John Todd preserved the Mathematisches Forschungsinstitut für Mathematik at Oberwohlfach for mathematics].

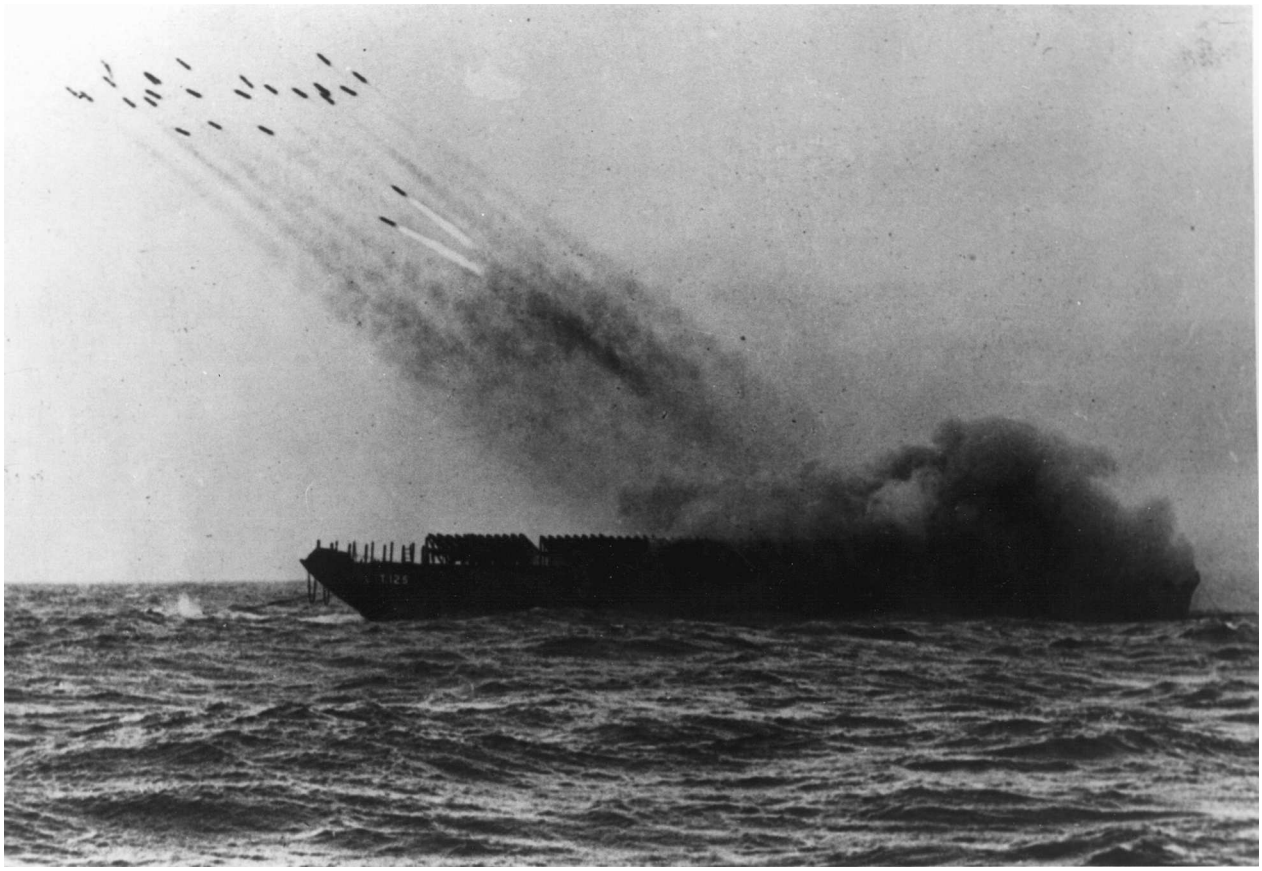

FIG. 1. *An LCT(R) [Landing Craft Tank (Rockets)] firing 5-inch rockets in training for D-Day. Picture, Imperial War Museum.*

4. OXFORD AGAIN

Bingham: Shall we pass now to your reentry into academic life: 1946, Special Election Fellowship in Mathematics, Magdalen College, Oxford.

Kendall: Well, that came about in an interesting way. One blessing we had in PDE was that, whatever else we didn't have, we got *Nature*, and we used to pore over each number, as the only window onto the scientific world available to us. One day about the middle of 1945 there was an advertisement for a Tutorial Fellowship in Mathematics at Jesus College and I applied and got an interview. Edward Thompson got the job. However, while I was there I went to see Haslam-Jones, who said, "Can you stay another day?" I duly went the next day, and HJ said, "This is Dr. Sutton of Magdalen College." He said, "We've decided we ought to do something about Mathematics. There hasn't been a Mathematics Tutor at Magdalen College for 25 years, and we think that it's time, perhaps, that we appointed one." (Of course, they had somebody teaching for them, from another College.) So I said I would be a candidate, and later on went up to be interviewed. I was told that as Sir Henry Tizard—the President—was a great big mogul in war science; I should apply to my Head of Station to get permission to send him a list of my various written reports during the war. And on the strength of these I got the job.

Bingham: Now you stayed at Magdalen for 16 years, from 1946 to 1962. Could you tell us, perhaps, about your time at Oxford?

Kendall: It was wonderful going back to Oxford, and it was particularly so going to Magdalen because, at that stage, the Fellows appointed for life under the old Statutes were still around, and they liked nothing better than to seize upon a new Fellow and tell him what it was like in the old days. One got a lot of ancient history—people, events and so on—from these dear old grandpas. The College was quite strikingly scientifically oriented. There were many remarkable people there, like Robert Robinson, Leslie Sutton, David Whitteridge and John Young, and later on Henry Whitehead.

Bingham: But how was it possible for a scientifically oriented College not to have had a Mathematics Tutor for 25 years?

Kendall: From remarks made to me I think it had something to do with the previous incum-

FIG. 2. *Rocket-firing Typhoons at the Falaise Gap, Normandy, 1944. Painting by Frank Wooton, Imperial War Museum.*

bent, 25 years before. Apparently he spent much of his time fishing for salmon in Norway. Perhaps they thought all mathematicians did that! Anyway, they had good arrangements with first-class teachers in other Colleges. This may be an old wives' tale, but that's what people told me. "We hope you don't fish"—they didn't actually say that, but . . . [laughter]

Bingham: A lovely Oxford story. I think posterity will treasure that! You mention Henry Whitehead as coming to Magdalen later on [Whitehead was elected Waynflete Professor of Mathematics in 1947]. One gets the impression that Whitehead was very much an extrovert and a mover.

Kendall: Whitehead, who had lectured to me as an undergraduate, was at that time a Fellow of Balliol. The Waynflete Chair of Mathematics was then and is now tied to Magdalen. And when that dear old man A. L. Dixon voluntarily retired, Whitehead got the job and became a close friend and, moreover, gave me tremendous support in an environment where probability and statistics were regarded as somewhat disreputable subjects. Whitehead would have none of that. He'd seen what probabilists could do at Bletchley [Park] (Hilton, 1988; Hinsley and Stripp, 1993). He never failed to take any opportunity that might help me in any way. A backer, in the very best sense of the word.

Bingham: Thank you. It might be invidious to name names, but I'm saddened, and rather surprised, that a negative view of probability and statistics should have survived in Oxford for so long.

Kendall: Just to follow that point up, because it *is* odd, it is clear that Hardy *was* interested in probability. It was not so long since Hardy was there. [Hardy resigned the Savilian Professorship of Geometry at Oxford in 1931 to move to Cambridge. He was succeeded by his pupil Titchmarsh, who held the chair until his death in 1963.]

Bingham: Hardy I believe was responsible for persuading Harald Cramér to write his Cambridge Tract. One of the things which has always fascinated me is the gaps in the Hardy–Littlewood school, which has deeply influenced me. They were very clearly not brought up—as you were—on Lebesgue integration. They knew it, but one gets the impression that it didn't come naturally to them.

Kendall: I can complement that. A nice thing that happened to me in my second year as an undergraduate was to get a small but adequate scholar-

ship from the British Association to go to its annual meeting, which was held that year in Cambridge. In the book display there was a new Cambridge Tract by Cramér on random variables and probability distributions (Cramér, 1937) which I thought looked very interesting. I sent a postcard to Haslam-Jones to say there's this book by Cramér, on probability, do you think it might be good? He sent me a postcard back: "I can't say anything about probability, but Cramér is all right" [laughter]. So I bought the book.

Bingham: On the strength of Cramér's number theory, I presume.

Kendall: Yes. I told Cramér about this, many years later; he was rather amused.

Bingham: Yes! Do any other names stand out as people who influenced you during your Oxford years, David?

Kendall: Well of course I was still in touch with Milne and Plaskett. I dropped away rather from astronomy, but I went back to the subject towards the end of my Oxford time partly because of John Hammersley. John was there for much longer of course—he's still there now, indeed. Pat Moran and I ran a joint seminar for a very long time.

Bingham: In probability or statistics or both?

Kendall: In probability theory. Towards the end of my Oxford period John Hammersley got in touch with astronomers over interesting probabilistic problems concerned with comets, focussed on Ray Lyttleton. He talked to me a lot about this, and we wrote one or two things, so this brought me back into astronomy for a bit.

Bingham: You dedicated your comets paper in the Berkeley Symposium to the memory of Milne [7, 8; see also papers there by Lyttleton and Hammersley].

Kendall: Yes, Milne had just died.

Bingham: I was fascinated to see that in your work there on comets you were using Spitzerian methods—there was a random walk on configuration space or energy space, as I recall. Wonderful stuff.

Kendall: Yes, it was great fun.

Bingham: What about the teaching side of the Oxford job? What was the teaching load like?

Kendall: It was quite light. Technically, I had to do at least six hours a week; I eventually did up to eight hours, perhaps. The norm was 14, or 20, even.

Bingham: What about lectures?

Kendall: Usually two lectures a week and a class, something like that.

Bingham: What kind of thing did you lecture on?

Kendall: Well, I gave the standard course on complex variables for many years . . .

Bingham: A wonderful experience, which I have had too.

Kendall: I did integration, with the first year. I also gave a course on probability. Later on I gave classes on functional analysis and on axiomatic set theory.

Bingham: If we can talk about some of your early published work in the Oxford years, your joint work with Robert Rankin stands out. Did you become mathematically involved with Robert during the war?

Kendall: Yes. I think those joint papers with Rankin all started as wartime conversations. But what spare time topics one was interested in during the war depended on what books were around. One of us had a copy of Watson's *Bessel Functions*, which was much in demand, and somebody else had Harold Hilton's book on groups. What books there were was quite random, really, but if there were any, you read them.

5. POPULATION PROCESSES

Bingham: Your work with Maurice Bartlett: one gets the impression that Maurice influenced you greatly. He was one of the senior statisticians in Britain . . .

Kendall: Well, Bartlett had influenced me during the war to a very great degree. But the real push came later. He came to stay with me in Magdalen just before he went to North Carolina to give what was to be a famous course of lectures on stochastic processes. He sent me his notes on these, and my work on birth and death processes emerged from my reading of them. Shortly after that Bartlett was invited to give a paper to the Royal Statistical Society, and by this time he was in touch with Moyal, who essentially belonged to the French school of "physics combined with stochastic processes." Arley's thesis was another influence around this time. Eventually we had an enormously long RSS meeting, in which Bartlett gave a talk on biological processes, I spoke on birth and death processes [2] and Moyal spoke on stochastic processes and quantum theory. It was a bit tough on the audience, my word! There was an elderly Fellow of the RSS, who gave the vote of thanks and said, "This seems to be the revelation of a new organon." I never discovered what an organon was [laughter], but it's supposed to be rather splendid . . . It was, of course, a turning point—for the Society, at any rate—to become aware of stochastic processes as not just a theoretical topic but as a whole new world of activity.

FIG. 3. *Gauss on his death bed. Given to me by Professor Bruggencate in 1945.*

Bingham: Those three papers are still worth reading today. They must have been very influential.

6. QUEUES

Kendall: The next thing was queueing theory, wasn't it?

Bingham: You mention that your first interest in birth and death processes arose from reading Maurice Bartlett's notes. Were these the notes that eventually turned into his book on stochastic processes?

Kendall: Essentially, yes.

Bingham: And Anscombe's influence?

Kendall: Well, I can't remember doing anything with Frank—except military things—until the business with queues came up. This arose because of the blockade of Berlin. There was a lot of writing in the newspapers about how Berlin had to be fed, and all the resources of the Air Force went into taking them bread and milk and so forth. But it was a very difficult scheduling operation, there weren't enough landing places, queues were occurring—and this seized my imagination. I wrote to Frank about it. Frank rattled back with a very quick instant solution of what is in fact a classical queueing problem of the Erlang type, and I decided to go into the matter in more detail.

That was quite a story in itself, because the source of information in Oxford is the Radcliffe Science Library. Above ground, there were wings for mathematics, physics, chemistry and so on, and one knew one's way about. If you wanted to know about queues, you wouldn't find anything useful there at all. However, there was an immense basement containing all the books no one would be likely to read. Among these were Post Office journals, engineering technology journals and so on. I spent a long time there combing through them—often fruitlessly—looking for references to queueing, and eventually found the work of Erlang. I wrote at once to the people at Copenhagen and got an invitation to go and give a lecture there. I met Arne Jensen, who is still a good friend, and eventually I had correspondence with Conny Palm in Sweden, who unfortunately died very young but who had invented the concept of a regeneration point. Soon it became clear that there was a good story to be written up, and my RSS paper came out . As you know, they gave me

FIG. 4. *D. G. Kendall and D. J. Finney with a Danish friend, during a cycling tour in Sweden and Norway, 1947.*

a silver medal for that—perhaps the only medal to be awarded for inventing a terminology and a notation.

Bingham: I do indeed. I think the queueing theory paper stands out . . .

Kendall: The interesting thing about that paper is that everybody uses the terminology created in it but no one has read the paper itself.

Bingham: I've read the paper, David! It influenced me deeply when I was young. Is it your favorite, would you say?

Kendall: I'm not sure; I have a lot of favorite papers, but certainly I was fond of that one.

Bingham: I must come back at you about that. I'd like you to tell us about your favorite few later on.

Now you married Diana Fletcher, formerly of the Womens' Royal Naval Service, in 1952. I had the pleasure of talking to Diana earlier today; there is nothing that changes life like a good marriage, . . .

Kendall: The Queen's Ball—the Sexcentenary Ball—was a major factor in this event.

Bingham: Was it? Would you like to elaborate?

Kendall: No, but I took her to the Ball . . .—it was very successful!

Bingham: Wonderful . . .

Kendall: Oh, we've missed out something, haven't we? We've missed out my little military career. Can we interpolate—it fits in here quite well, really. Almost as soon as I went to Magdalen the contrast, between the wartime life that I'd been involved in and this extraordinary, almost mediaeval life, struck me very sharply, and I thought, well, I wonder what effect this will have on me.

Then I read in the *Oxford Mail* that the City of Oxford Heavy Anti-Aircraft Battery was being reformed and that volunteers were welcome. I bicycled up to the Drill Hall at the top of Banbury Road and was received by the sergeant in charge, who later became a good friend of mine, and he said "Well, if you'd like to join us you'd be most welcome—you'd actually be our first recruit." He swore me in there and then on the Bible. And I stayed with them for six years, starting as a Gunner, then becoming a Lance-Bombardier at the first annual camp, then progressed up to be Sergeant for quite a long time—they put me in charge of the radar—then eventually went on one of these weekends where they find out whether you swear like a trooper [laughter]; if you don't, they make you an officer, and I wound up as a two-pip Lieutenant. At which point, in their wisdom, the authorities dissolved the Battery. So that was that. But I was first in and last out. It was a most enjoyable experience.

Bingham: One of the things I look for is reference to Old Gunners in the mathematical literature, and there are quite a number of them. Littlewood was one famous example; John Hammersley, your former Oxford colleague, was another . . .

Kendall: There was a delightful occasion I shall never forget when we were at a Firing Camp in Norfolk. I was Number One on the predictor at that stage, but I said, look, it can't make much difference, why can't I do a day on the guns, because I ought to find out what it's like. So they said all right, you can be the fuse setter—but make sure you set it well, because we're going to be inspected today by a lot of boffins from the Ministry of Supply. I enjoyed that episode!

Bingham: I'm sure you would [laughter], seeing it from both ends. Lovely.

7. USA, 1952

Bingham: You visited the USA in 1952, I believe.

Kendall: Oh yes. That was an invitation from Wilks and Feller, for me to go to Princeton. They

FIG. 5. *D. G. Kendall and three companions on the Tête Blanche, Switzerland, 1950.*

had got some money to fund a visiting lectureship for an academic year. They offered it to me, and subsequently my successor was Frank Anscombe. They gave us a tremendous welcome; incidentally, it just displays the generosity of Americans. They had fixed it up for me to go alone, and then I went and got married, and all their plans were thrown into disarray. But it didn't faze them one little bit. There was a little hut on an encampment for retired exservicemen, and they went through their attics, found what spare furniture they had and lent us this and that—we hadn't a bed, however. We made friends with a student who was an Ensign, US Navy, who for some reason had two double beds and couldn't get them both into his hut so he lent us one of the beds, and so we managed.

We joined the Princeton Outing Club, an undergraduate affair. In January we went with them into the Adirondacks—snow something like six feet deep—crossing frozen rivers—fantastic. Wonderful, but I wouldn't want to do it again!

Bingham: What was Feller like, as a man? . . .

Kendall: Well, you'll get all sorts of different answers to that question. One would have to say first of all that he had a heart of gold. Second, he was difficult . . .

Bingham: Difficult?

Kendall: Yes . . . one had to be careful about attributions, and things like that.

Bingham: *He* wasn't! Feller hardly attributed anything.

Kendall: Well, exactly, but he liked to be attributed *to*. He was a bit of a martinet, with a heart of gold. He was wonderfully kind to us, as was Clara his wife. Equally kind were the Tukeys and the Wilkses. One of the things that helped me most was that Alan James was there and had been there the preceding year when Feller had given a lecture course on semigroups. Feller wasn't going to give this course again; the only course I got out of him was the straightforward probability course for beginners, which was interesting and full of extraordinary philosophical remarks and so on. But Alan James had made careful notes on the semigroup course and I learned semigroups—learned functional analysis, indeed—from that, helped by Hille's book, which irritated Feller considerably. If he saw anybody walking across the campus with

FIG. 6. *480 HAA Regt RA (TA) at Cleeve Camp, Bude, 1948: Lance-Bombardier D. G. Kendall is sixth from the left in the back row.*

Hille's *Semigroups* under his arm, he would shout "DISLOYAL!" [laughter]

Bingham: Extraordinary—what didn't he like about it?

Kendall: He thought his approach was better—but he never wrote it up, of course. Hille's book is a perfectly good one, except that it doesn't provide you with the mechanisms you need to discuss the probabilistic theory of semigroups, and there are no positive cones in it.

Bingham: No, no . . . Hille–Phillips is fuller.

Kendall: Yes, but it's too full, of course, really, and here again the order-theoretic stuff isn't there.

Bingham: Yes, and it is rather forbiddingly thick, isn't it.

Kendall: I enjoyed those books, both of them. Later on, when Harry Reuter and I got together, Phillips published an abstract of a long paper that was to become essentially his part of Hille and Phillips (Phillips, 1955), which just consisted of assertions, and one of the tasks Harry and I set ourselves was to go through the assertions and prove the lot. Then we wrote to Phillips and sent him our proofs—he was delighted!

But we're jumping ahead, aren't we: how did we get onto that? Oh, going to America. Well, the other thing about going to America was, we did a lot of travelling; we were generously given lots of invitations, and one of these was to Urbana where we met Doob. That was most satisfying. Another, which had even more interesting consequences, was a trip to see Chung and Kac at Cornell, and of course they both became close friends. Chung said "Have you seen this little note by Kolmogorov in a Russian journal on two very peculiar Markov processes?" Now Diana had some Russian, because while she was in the WRNS stationed at Winchester a lot of wounded Russians had been brought over to a hospital there. She had been learning Russian from a friend of hers, and volunteered to talk to them in such Russian as she knew. Because of this we had a Russian dictionary with us, and Diana knew enough to help me translate the Kolmogorov paper—though with some difficulty. And that of course opened the floodgates, and all the work on semigroups and so forth followed from that.

Bingham: Wonderful. Still in America, you met Neyman, I believe.

Kendall: Yes, I met Neyman at a conference. My job in Princeton finished at the end of the summer term, and he urged us to come to Berkeley for the summer, which we did, and he asked me to give the

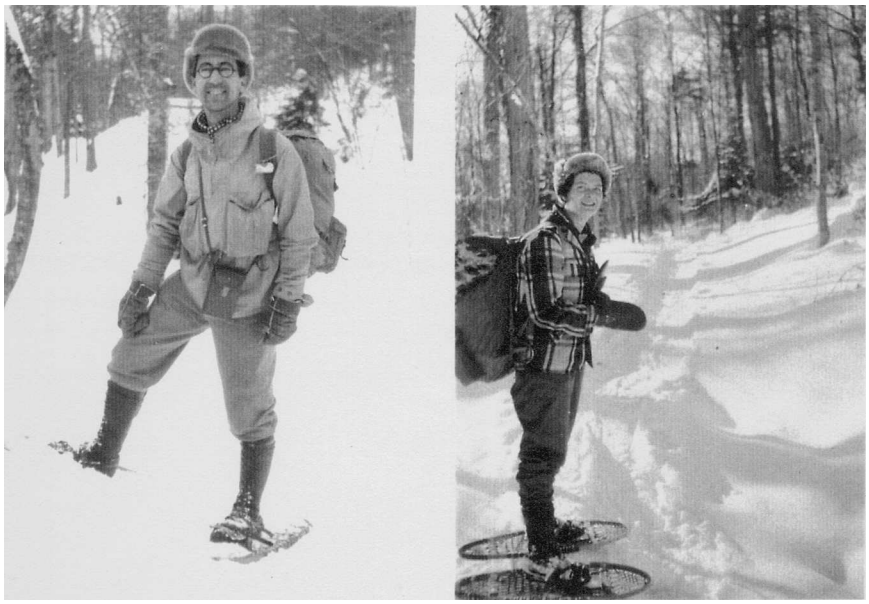

FIG. 7. *D. G. Kendall and Diana Kendall snowshoeing in the Adirondacks, January 1953.*

most elementary possible course on statistics and probability. He saw to it that we had lots of free time to see the Sierras, mountain-climbing, that sort of thing. In fact we climbed Half Dome (the easy way).

Bingham: Was Berkeley the leading department in probability and statistics in the US at that time, would you say?

Kendall: A case could be made out for Cornell, which had Kac, and Chung, and Hunt—and some time earlier, Feller also—so it had a tremendous tradition. But Berkeley was a lively place. What happened was that every day people brought their lunch—what they called a bag lunch—and Neyman and Elizabeth Scott would arrive with two enormous sacks full of *extra* lunch, which they handed out, and we all sat round a table and chatted about general things, philosophy, religion, anything you like. Fascinating people like Loève were there; he told an extraordinary story. He said "When I finished my thesis the form was that you then took it to the professor, who considered it, and later said whether it was suitable to present as evidence"—to get, whatever it is, *maître de conférence* and so on. So Loève took his thesis to Fréchet, who said, "Come back in 18 months, and I will give you an opinion." Loève said, "But next week I'm going into a concentration camp!"

Bingham: Did Loève actually go into a concentration camp?

Kendall: I suppose he did—maybe it wasn't all that concentrated [as he came out]...

8. MARKOV CHAINS AND COLLABORATION WITH HARRY REUTER

Bingham: Now you returned to Oxford in 1953. I understand that this was the year you met Harry Reuter.

Kendall: Yes. What happened was that we came back on the *Queen Mary*—we went out on the *Mauritania* and back on the *Queen Mary*, a nice double—and at Southampton, where she docked, Diana's sister was waiting. Diana went off with her, and I went to Durham where there was a British Mathematical Colloquium. And I was upset because I discovered I had a hole in the seat of my trousers, and I couldn't bear the thought of meeting the son of the *Burgomeister* of Berlin in such a dishevelled condition. So crossing the bridge in Durham and going up that little winding street I found a mens' outfitters, and went in and bought another pair of trousers, and put them on. And in these, decently clad, I met Harry Reuter.

Bingham: Very nice. I don't suppose Harry would have turned a hair about your trousers...

Kendall: No, no, of course not [laughter].

Bingham: Well that was the beginning of a beautiful friendship, both mathematically and personally, which I've heard you describe as by far one of the two most important mathematical contacts of your life. What amazes me is the speed with which you and Harry went from meeting at the BMC in 1953 to contributing to the Proceedings of the Inter-

FIG. 8. *D. G. Kendall and Diana Kendall translating Kolmogorov* (*United States*, *1953*).

national Congress of Mathematicians at Amsterdam in 1954.

Kendall: Well that was partly Chung. You see, I told you that Chung directed us to these papers of Kolmogorov, in which he simply presents two examples of what we called in those days (but wouldn't now) *pathological* Markov chains, and what Harry and I decided to do was to investigate these from every possible point of view that we could, *including the sample-function point of view*—which of course had not been touched. Then when I got the invitation to give a lecture at Amsterdam I suggested it should be a joint lecture. So that's how it came about. It was rather fortunate, because there was no space limitation; we were able to write the thing up in full [4].

Bingham: Did you have the key results before you got the invitation, or was the actual problem cracked against the clock, as it were?

Kendall: Oh no, it wasn't cracked against the clock, it was a program. Everything we did was part of a program. When we'd completed something, we looked around for some conference to present it at. This turned out very happily.

Bingham: It certainly did, yes; it's wonderful stuff.

Kendall: Especially as Kolmogorov was there—one of the few conferences he came to.

Bingham: Yes indeed—talking on KAM theory, as I recall [Kolmogorov–Arnold–Moser theory; see Moffatt (1990), pages 71–73]. You've written in your appreciations of Kolmogorov about your meeting with Kolmogorov, in the Zoo [17, 18], and your lunch together. Shall we talk more of Kolmogorov later, and carry on with the Markovian side now? You mentioned functional analysis; Harry was an analyst through and through. I get the impression that Harry passed on his functional analysis viewpoint to you—though this may have fallen on fertile ground, as you had this Hille–Yosida background from America.

Kendall: Yes . . . we were perplexed by the Hille book, and of course the second version, with Phillips, is better, but it's still a difficult book, and we realized that a lot of the functional analysis wasn't there anyway. I suppose we spent about half of our time during those early years searching out papers on functional analysis that seemed fundamental, and absolutely gutting them—I mean, really digesting them thoroughly, one after another, until we couldn't find anything more. No doubt there was more, but we read everything that we could, we analyzed it carefully and we explained it to one another whenever we got the chance to meet.

Bingham: Karlin and McGregor were working their way towards spectral theory from their own viewpoint. Was there any contact between Kendall–Reuter and Karlin–McGregor?

Kendall: I was invited to go to Stanford, and met Karlin there. We then compared notes on the various approaches to these things.

Bingham: The pathological examples that you looked at in such detail—the Feller–McKean chain came around that time (Feller and McKean, 1956), then there was your own work on pathology [5, 6], giving a Feller–McKean–like example done your own way, and I think there was another example by David Blackwell around the same time (Blackwell, 1958) . . .

Kendall: Yes, David Blackwell's paper was about a process where all the q_i's were infinite, which surprised us.

Bingham: This was a totally unstable chain, as in your second paper. When did words like boundary theory and ideas like compactification of the state-space begin to impinge?

Kendall: They didn't impinge on what we did. That emerged more from Feller's later work. Of course, we became interested in boundaries eventually.

Bingham: There's a shrewd passage in the preface to the second edition of Chung's book, where he laments the divorce between the two branches of Markovian theory, the chain theory and the process theory, and he says something to the effect that surely future progress must hinge on bringing them together. And eventually they have been brought much more closely together, through Ray–Knight compactification, boundary theory and the like. The name that springs to mind here, apart from your own and Harry Reuter's, is David Williams. You and Harry shared paternity of David Williams.

Kendall: Yes. You know the David Williams story, don't you—perhaps we should have it for the record. We got a family car eventually, and I was sent off in this car to find a suitable holiday place [in 1958]. And I said, there's nowhere better than Wales; I went off to the Gower, to try to find a place. We found a super place—almost actually on the beach, run by delightful people in a lovely spot called Horton. We went there on a Saturday, with I suppose at that stage just two babies, I can't remember now, and I went to church on the Sunday in the little village church and talked afterwards to the dear old parson, who became a great friend. "Oh, you're from Oxford," he said, "We've got a boy here who's got a scholarship to Jesus College, Oxford—he's going to do Mathematics" [spirited attempt at a Gower accent]. "He's been playing the organ this morning"—and this was David Williams. And David took me crabbing. And in due course he came up to Oxford, and we entertained him a few times, and in due course he became my research student [in 1961, moving to Durham under Reuter, 1962–1964].

Bingham: And John Kingman?

Kendall: I inherited John Kingman from Peter Whittle when I moved to Cambridge.

Bingham: Were they your first pupils?

Kendall: Oh no, I had lots of pupils before that. Alan Mayne was an early one; Gordon Foster, who became a professor at Trinity College, Dublin; Bill Waugh, who went to the Royal Military College of Science at Shrivenham; David Edwards, who did a very long stint of National Service and soon knew far more functional analysis than I did. Simon Broadbent was one of the last of my Oxford students and we'll come back to him in a moment. I also like to count Mike Westmacott, who later did wonders on Everest—he's now President of the Alpine Club.

9. CAMBRIDGE

Bingham: Passing now to your time in Cambridge, you were appointed Professor of Mathematical Statistics here in Cambridge in 1962, the first holder of that chair, and began in the cellars of the Chemistry Lab, I believe, followed by a move to the old Press site in Mill Lane, where we are sitting now. Peter Whittle circulated the history of the Lab quite recently, *A Realised Path*, which is full of interesting details about the early history of the Lab: Maurice Bartlett's influence, John Wishart's influence and so on. But turning to your arrival here . . .

Kendall: Well, if you look at the photographs in the corridor—and I'm sure you've often done so—you'll find that the number of people each year shrinks and shrinks and shrinks, until the year in which I arrived when it was damned nearly zero. And then there was a tremendous explosion—the fact that there was a Chair made all the difference, really. Since then it's grown and grown. As you say, we lived in the basement of the Chemistry Lab, and I have benefited greatly from this fact because in physical chemistry there is one of the best scientific photographers in the world, who became a friend of mine. He always says, "Remember, whenever you've got any diagrams you need for your papers, I'll do them for you," and he does, with superlative skill. We actually wrote a little paper together, ourselves, he and I, on how to make good lantern slides, and it was published by the Royal Society. Eric Smith—one of the greatest slide-makers in the world. So that's a bonus inherited from our time in the cellars of the Chemistry Lab, and then we got this place.

Of course, it was a very important decision, to agree that there should be a Department of Pure Mathematics. There was already a Department of Applied Mathematics. Some people didn't like the idea of having a Department to come to, instead of comfortably working in their homes or their Colleges. It was a tremendously important, positive decision, and Bill Hodge pushed it through. And a second important decision was, should the Statistical Laboratory be part of the Department, or should it be a Department of Statistics, and there were people who felt that the latter would be the best thing, because they felt that it would be more distinguished—in a narrow sense—but I was emphatically of the opinion that we ought to be one big group, to balance the big group in Applied Mathematics. And I'm sure this was right. All the way through, we've had lots of Directors at the top; they've always been pure mathematicians, and

they've always given us 150 percent of their time and their influence.

Bingham: There's a story that when you were appointed Harold Davenport said he'd been a bit worried about who they might get, but "Eeh, they've appointed a statistician who knows about lattice points"!

Kendall: That's right, yes. He told me that himself [laughter].

Bingham: I believe it's also the case that after your arrival statistics and probability were declared respectable, and the Colleges began to open their doors to the staff of the Stats Lab.

Kendall: Yes.

Bingham: It reads strangely now, that this hadn't happened earlier. Henry Daniels . . . (Whittle, 1993).

Kendall: Well, you see Bartlett didn't have a Fellowship. It's extraordinary.

Bingham: Yes it is. And John Wishart?

Kendall: I can't remember, but certainly it was a terrible situation. Dennis Lindley didn't have a Fellowship. Frank Anscombe didn't have a Fellowship.

Bingham: Can you wonder that statisticians are human enough to resent mathematicians sometimes.

Kendall: Well, yes, but you know, what you should say is not how beastly mathematicians are, but how beastly people are when they're well dug in and don't like change. But I think there has been a revolution in thinking. We now have a lecturer in the Statistical Laboratory who's an authority on chaos, for instance. That's a big step. It would be easy to say, that isn't statistics, but it would also be easy to say it isn't deterministic mathematics either.

Bingham: Quite. Indeed, I think the Lab did well to get Colin Sparrow in here before the links between statistics and chaos became fashionable. There was an RSS meeting on this just a year or two ago, lots of lovely stuff there [RSS Meeting on Chaos, *J. Roy. Statist. Soc. Ser. B* **54** (1992) 301–474].

You became a Fellow of Churchill College—not a Founder Fellow?

Kendall: Not a Founder Fellow—the College had been in existence for a year before I came—but I was its first Professor.

Bingham: And you retain your association with Churchill.

Kendall: Yes, they're most generous about life tenure, and I'm extremely grateful for that.

Bingham: Now I'm not sure when you made your first visit to the USSR, but I believe you went to Tbilisi, and met Kolmogorov again, Gnedenko, Dynkin, Ambartzumian, . . .

Kendall: Oh yes, there were lots of people I met then: Yaglom, for instance, and especially Ambartzumian. I was anxious to see more of the Caucasus. Ambartzumian said, "That's not a problem, half the people running this hotel are Armenians, we'll fix it up for you—but don't tell anybody!" However, Dynkin got to know that we were planning to make this—actually quite illegal—trip by car to the Caucasus on the Sunday, in the middle of the conference, and Dynkin was upset, because he said, "We don't mind you going to see the Caucasus, but there is a dinner that evening, and it's absolutely essential that you should be there because *the Minister is coming*. You see, we have to show him these foreign statisticians we've invited." And we argued and argued over the telephone about this, and in the end Dynkin, practically on his knees by now, said, "If you come to the dinner on Sunday, we will take you to the Caucasus on Monday." And they did, bless them.

We went right up to Kazbek. We didn't of course climb Kazbek, which is a horrifically difficult and dangerous mountain, but we walked a considerable way up it—a magnificent experience.

10. ARCHAEOLOGY, SERIATION, EPIDEMICS, . . .

Kendall: We've got to my involvement with archaeology now; that started in my last year in Oxford. The story is a curious one. There was a man called Flinders Petrie, who'd done a great deal of fundamental work on predynastic Egyptian pottery. He made a classification of pottery types, and there was then in existence, in the form of a great mass of paper slips, the equivalent of a contingency table showing which varieties were found in which graves. This contingency table was the basis for a predynastic Egyptian pottery chronology.

I was approached by one of the staff of the Ashmolean Museum in Oxford, who said that they'd got interested in this again, and that it was unfortunate to have to confess that Petrie's slips of paper had all been burned in a spring-cleaning in a fit of enthusiasm many years earlier—*but*, could I do some calculations to say whether the sort of inferences they could have made (had the slips still been available) would have been statistically sound.

This was a silly question, really, but interesting, and it started me thinking about contingency tables with a temporal factor, and this seriation problem became a preoccupation of mine for many years. I wrote my first paper on that in 1963, which was just after I arrived in Cambridge.

The other thing I was very busy with at that time was epidemics. A rather nice thing happened. There was a small conference in London, on mathematics and computer science in biology, and I had just started being interested in epidemics. I wrote a little paper on the growth rate of epidemics which adapts what we would now call reaction–diffusion theory to the epidemic context, and that was given as a lecture in Cambridge, and then published in the Proceedings of this conference by HMSO, and that is why my paper [9] (which is one of my favorites) never appeared in *Mathematical Reviews*. So few people know of it.

Bingham: That's the trouble with working from *Mathematical Reviews*, which was my database when I was preparing for this interview!

Kendall: Immediately after that Daryl Daley and I started to work on stochastic rumors, which is a rather similar problem but not quite the same thing. The next thing that happened was "Branching processes since 1873" [10] [London Mathematical Society Centenary Address, 1966, continued as Kendall's Presidential Address to the London Mathematical Society, 1975]. This was an historical paper, trying to find out more about H. W. Watson and trying to understand the background to his rather inconclusive but still very pioneering work, and I went into it in great depth. I wanted to find out more about all the people concerned, I wanted to find out more about Watson's Alpine career, and this made me interested in other great alpinists, including the Rev. Charles Hudson, who was killed on the Matterhorn. We wound up with the grandson of Charles Hudson coming to spend the night in our home.

After this there came the Loutraki Conference in 1966, and Harry and I went to that together. I gave a lecture on "Renewal sequences and their arithmetic," which is the origin of Delphic semigroups and all that. We had a lot of free time, and one weekend Harry and I made a trip into the Peloponnese. We wanted to go to the Megaspelion Monastery, which is up in the mountains, and beyond that to the source of the Styx—a very ambitious project. There was attached to the conference a travel agent, to facilitate comings and goings, so we got hold of him and asked for train and bus times to enable us to carry this out. He provided us with a complete list of schedules. We then set off, only to be told that there was a general strike, and the railways were not running. However, we got by bus to the foot of the mountain railway, and then discovered that the strike applied also that. Now there is a tremendously deep gorge, several hundred feet deep, with the railway zigzagging across it, like this [gestures]—just tracks and sleepers, with this appalling drop underneath, lasting for about five miles, up into the mountains. We went up this thing—but it was worse than it sounds, because sometimes where the zig met the zag you would just reposition your rucksack (and not look down too much) and readjust your direction, but also sometimes the railway would go into a tunnel and it would go up for a while through the cliff before it came out. Well, coming out of one tunnel we saw a snake . . . we didn't like the subsequent tunnels much [laughs], but we did eventually reach the monastery. And the day was really ended then. You know what a good monastery is like, marvellous hospitality, we spent the night there, it's a famous and very dramatic monastery, and we explained what we wanted to do next.

Everybody said this is crazy, the mountain is dangerous . . . we did make a try to reach the source of the Styx, but we got lost—we took a wrong turning in the forest somewhere. So that wasn't successful. But there were a lot of lovely things I remember about that conference. I can't remember any of the lectures, but I do remember climbing up to the Corycian Cave by myself, which is a long climb, and takes you up to a high point where you can see Parnassus in profile. The Corycian Cave is very dramatic, a stalagmite cave with lots of ancient legends about it, and it was while going up to it that I began to work out the theory of Delphic semigroups—which is why they were so called.

Bingham: Yes, indeed. Well, that was going strong when I was in Cambridge—that's what was in the air.

Kendall: You came over from Oxford, after I did . . .

Bingham: Yes, in 1966—till 1969.

Before we talk about Delphic semigroups, could I ask when it was that you and others—John Kingman particularly—became so determined to extract as much juice as it was possible to extract from *just looking at whether or not you're in a particular state*? When did that key insight—that it was whether or not you were there, rather than the Markov property as such, that was a good thing to go for—the birth of regenerative phenomena . . .

Kendall: Well, I suspect—I can't remember now—it came as a result of a careful comparison of the work on renewal sequences, which was Loutraki, and then transferring that to the continuous-time case, a sort of continuous-time renewal theory. [For further background, see Kingman (1966).]

Around then, I had a paper in a Hungarian journal, so maybe we ought to talk about Rényi. I got to know Rényi at the International Congress of Math-

ematicians in Edinburgh in 1958, and later he came to give a talk to a society of which I was Secretary when I was in Oxford. And then pretty soon after I was in Cambridge we picked up the friendship again. I got to know him very well, and his family. His wife I knew; we got to know his daughter even better, because she still comes to Cambridge about every three years, so we see her regularly.

Bingham: Rényi has left a deep mark on me. I've never forgotten his visits to Cambridge, and what a wonderful lecturer he was.

Kendall: Wonderful lecturer! But impractical in many ways. I remember when I was in Budapest most of the time was spent working, or talking to people, but then I had to give a lecture. There was a big audience, and I was just getting my notes ready to start when his secretary came in. "Professor Rényi needs to see you immediately," so I had to say to the audience, "I'm sorry—some hitch," and went out to his office. There was Erdös, and a lot of coffee cups set out, and he said "Look! Erdös is here! Sit down, have some coffee, let us talk!" So I said, "I'm just about to give a lecture." "Oh, they will wait!", he said [laughter]. They had to wait . . .

Bingham: There speaks a man from the Eastern European tradition.

Kendall: When the Rényis came to Cambridge there was a Ball at Churchill. We were going, and we invited Rényi and his daughter to go. We explained that it was customary to wear tails; it isn't now of course, but it was then. We explained that one wrote to Moss Bros., sending one's size and so on. And I remember, before we left for the Ball, saying to Diana, "What will happen—how will he know what fits into what, how to tie the tie, what does he know about cuff-links—I should have been there, to help him into it!" However, we arrived, and there was Rényi, faultlessly dressed—he had the old Austro-Hungarian Empire look about him, it was splendid.

Bingham: Wonderful! Your interest in information theory presumably comes from Rényi?

Kendall: I'm not sure about that.

Bingham: If it predates Rényi, it must have been one of your links with him.

Kendall: Yes. I think it predates him. Because, for instance, I met Shannon.

Bingham: Did you indeed? Could we talk about Shannon?

Kendall: Well I can't tell you much about Shannon, except that he had a tape of musical sounds, which gave one the impression of climbing a musical staircase but which was, in fact, periodic.

Bingham: Amazing. Escher-like.

Kendall: Yes—it made a great impression.

I have already told you about archaeology, and I went on with that after I came to Cambridge. Morris Walker had a friend, Roy Hodson—they knew each other because at one stage they had both lived in Madingley Hall—and Morris Walker introduced me to Roy Hodson. We quickly got onto the right wavelength, because Roy during the war was in the Gunners, and had some contact with Mortimer Wheeler. And I said, "Well, if you were a Gunner, that's enough—mathematics is a doddle if you are a Gunner; why don't you come to the Lab for a year and see if we can do something together?" And that was tremendously successful. I think it was helpful to him too, to have this contact, which we've kept up. He's now a Professor in London [at the Institute of Archaeology]—just down the road from Cambridge.

He handed over to me the draft of his book, on the Iron Age cemetery at Münsingen [Switzerland, near Berne], and said, "Well if you're interested, you've done this extraordinary work for the Oxford people telling them how they should analyze material they've already thrown away, so now you can have a look at my thesis and tell me how I should have handled that—only this time you will have the details." And so I then set to work on a systematic study on how to handle the seriation problem in the presence of noise, and that kept me busy for quite a long time.

About then I went to Romania for the first time—1968 that was. Of course I talked on archaeology while I was there, and there were some interesting features of that. I had given a short talk on archaeological seriation at the conference in Brasov. When that was over we were taken by car back to Bucharest, and by this time I was pretty tired and went straight up to my hotel bedroom and went to sleep. In the middle of this the telephone rang. It was Professor Moisil, who had been present at the meeting and heard my lecture, and he said, "My brother is a Professor of Archaeology; I think you ought to meet him. Perhaps we should do something about mathematical archaeology here." What emerged from this was the Royal Society/Romanian Academy Conference of 1970, which was quite influential. A little bit disturbed by a cholera scare—I remember one of the American speakers was just getting ready to give his lecture. He went down with tremendous diarrhoea and sickness, and everyone thought the cholera had arrived, but actually it was just ordinary diarrhoea and sickness, so we all escaped. And this gave rise to a long involvement with Romania; I've been back there many times, and I'm going back there again this summer.

One of the things that happened during my first visit to Romania was that I met Liliana Boneva,

who was there representing Bulgaria. And she said, "I know you like it here, and of course it is a beautiful country, but so is Bulgaria; you must come there too!" And accordingly, after going back, I had another opportunity (with the Royal Society paying) to make an official exchange visit to Bulgaria. We started working together, on what turned out to be splines in the end [12]. And she came to Cambridge for a year when that joint paper was published, with Ivan Stefanov as the third author.

Bingham: Yes, I remember that.

11. RECONSTRUCTING MAPS; ROLLO DAVIDSON; GEOMETRY

Kendall: Other things that cropped up during that period were general questions about "constructing maps from odd bits of information" [15]. The most spectacular example of this was the region north of Oxford called Otmoor. There was a large interdisciplinary group studying Otmoor in enormous depth: they had recorded all the physical and psychological features of the population that they could. They also had good records of marriages, births and deaths and so forth. It occurred to me that it would be fun to take the extensive marriage data and observe when a person from one village married a person from another—which incidentally was not often, because it was a marsh, and at one time it was really difficult to get from one village to another. So the question was whether this weak linkage through occasional marriages would enable one to draw a map of the area. And the answer is, "yes"! So that was quite a coup.

Bingham: Your interest in genealogy again! Could I ask you one thing about Rényi, by the way, before we pass on? You've been interested in exchangeability, and I was looking at your paper on exchangeability just the other night, and it mentions work by Rényi and Révész. Did you become interested in exchangeability through Rényi, and if not, do you remember where you first became interested—were the Bayesians already enthusiastic because of de Finetti's theorem? . . .

Kendall: It was definitely nothing to do with Bayesians. I wrote the exchangeability paper in Geoff Watson's department at Johns Hopkins in Baltimore. Just before this I went to a meeting in London and stayed in Imperial College. I ricked my spine carrying my suitcases up and down those narrow spiral staircases, and by the time I arrived in Baltimore I was in dreadful pain and couldn't think what to do. And Geoff said, "What a lucky thing! We got the hospital here to underwrite your stay"—I was lecturing in stochastic processes in biology, I think—"and they'll give you free treatment." And they did! And that relieved the pain to some extent—but it was a long time before I got over it. However, it passed away.

We're into the Rollo era now, aren't we. Rollo [Rollo Davidson, 1944–1970] shocked his Trinity teachers by saying he didn't want to do Part III [of the Mathematical Tripos], he wanted to do the Diploma [in Mathematical Statistics]—typical independence of mind—and so I got to know him at an early stage, but not closely, because he was shy. He was my research student, but rather in the way John Kingman was my research student. The only contact between us was that every Monday morning John would come in and say "I've finished the problem you gave me last week, and I've nothing else on my plate; please give me something else!"—and I had to think of another mad thing to try. Rollo was a bit like that, but more self-propelled.

There was a great debate about what he should do. He'd been offered a job—I suppose it was at Cheltenham [GCHQ, the Government Communications Headquarters], and Rollo planned to take it up. His parents were horrified. They arranged a dinner party at Trinity, to which they came, with Rollo, Rollo's Moral Tutor and myself. They evidently thought that I would do my best to dissuade him, but I took the view then—and I think I would still do this, though I may have mellowed—that if people really wanted to do something, then they should do it. The Davidson parents were a bit cross with me about this, but they generously forgave me in the end. Anyway Rollo decided to stay.

Bingham: I never heard Rollo mention the GCHQ business.

Kendall: I think, you know, it was the romantic aura of GCHQ that tempted him a bit, perhaps. But alas, Rollo wasn't with us for long. And then there was the business of the two books, which Ted Harding and I organized together. [Rollo Davidson was tragically killed in a climbing accident in the Alps in 1970; the books *Stochastic Analysis* and *Stochastic Geometry* (Wiley, 1973 and 1974) are memorial volumes to him.] That took quite a long time.

Bingham: Yes, it must have done; you did a very thorough job on that.

Kendall: And that gave me an opportunity to write about random sets. People sometimes think I'm talking about random sex [laughter] . . .

Bingham: The geometrical strand to your thinking has been part of your life for quite a long time now . . .

Kendall: That came in quite suddenly, when Henry Whitehead died [in 1960]. Henry Whitehead went to Princeton—he loved Princeton—and he just

collapsed in the street and died. Diana and I were on holiday in the Yorkshire moors at the time when we got this news, and I was deeply upset about it. Because, you see, I'd lived beside Henry Whitehead in Magdalen for quite a long time; we were good friends. He'd been a constant backer for me; he was upset that at that time so few of College Lecturers took any real interest in research, and he encouraged me as much as he could to keep doing this. And perhaps if he hadn't been there I'd have been pushed into becoming a full program tutor, but I think Henry must have protected me from that.

But then suddenly he was gone, and I realized that I'd had a window open to geometry, that I'd just never bothered to walk through. And this made me think I must learn geometry nevertheless, so that I could perhaps understand some things in it, and I deliberately set about that; I had a sabbatical year coming and took it, and devoted it entirely to trying to learn geometry.

Bingham: Which year was that, David?

Kendall: [Pause] There's some confusion in my mind now, because there were a lot of things going on at about the same time. I spent a lot of time working on the rather remarkable range of archives that exists for the village of Whixley, where my paternal ancestors came from, and came to know the professionals in that field, saw what was yet another problem where interesting questions of statistical inference arose, and I started getting involved in that. I was also getting involved, through John Hammersley, in the business of Alexander Thom's quantum, and I wanted very much to find what I would consider an acceptable way to analyze that kind of data.

Bingham: This is the megalithic yard?

Kendall: Yes. All those things began to emerge around the middle 1970s, and took up a lot of time.

Bingham: The geometrical side can be traced back further. Chris Heyde, in his obituary of Pat Moran, says that Moran's interest in geometric probability arose from a question on fragmentation of shells that you gave to him.

Kendall: Ah well, that's not quite right, because when Pat Moran arrived at PDE he was already deeply interested in geometrical probability. And he taught me a lot. In particular, he published a paper (Moran, 1944) generalizing some results of a Polish writer on the connection between mean cross-section and total surface area. I think the theorem must be something like this: if you have—I don't think it has to be smooth—a convex body and you photograph it along the normals of an icosahedron—20 views—and then average the areas that you see, and multiply by $4/\pi$ or something, then you get the surface area with a small error. And I suggested to Pat that we should do this to shell fragments. Of course, it doesn't matter that shell fragments aren't convex, because what you would be measuring would be characteristics of the convex hulls of the shell fragments. And he invented an ingenious little device, which was published in *Nature*, where you make an icosahedron as a little cage, and put the shell fragment in the middle, and just keep on turning the icosahedron over—you have to do this very carefully, to make sure you don't do one face twice—and for each face, or pair of faces, you get a cross-section . . . very beautiful; lovely stuff.

Bingham: Before we pass on, David: we've mentioned your interest in history, a couple of times. This is something that comes over very strongly in your later writings. How far back in your life does an interest in history in general, and in history of mathematics in particular, go?

Kendall: Well, the history of mathematics for me has been rather incidental. History—in the sense of real history—started with my enquiries into my own family history, and that started with one of my Magdalen pupils, who was a fanatical genealogist and told me everything you need to know to find the basic things—to set up a family tree for three or four generations, anyway—and I found that my father's family came from a little village called Whixley, about 20 miles from Ripon, and went there.

I arrived unannounced and knocked on the door of the vicarage. The vicar then said, "Oh do come in—what can I do for you?" And I said that I was interested in finding out about my family, and wanted to look at the registers, and he said, "I'm just going out, but I'll leave you in the hands of my young son Giles. He's very capable; he'll cook you lunch; you can have my study, and I'll leave all the registers out for you." Wonderfully kind. And he became a great friend. I went back to Whixley again and again, and got to know lots of people there. I also came to know the Professor of Historical Geography at Leeds, who was also interested in Whixley, and through him I got all sorts of maps and plans. I was able to persuade the aerial photography people at Cambridge to fly over Whixley and get me some photographs; I tried to integrate information from photographs, information from hearsay, information from parish registers, and then later—still more interesting—information from mediaeval documents, which are very rich for Whixley, and made a connected corpus of historical facts about the place, which I then put together in a long Royal Society paper called "The recovery of structure from fragmentary information" [15]. This paper I consider to be my best. It's not my favorite paper—my favorite

paper is the paper on epidemics which [laughter] never appeared in *Mathematical Reviews* [9]. But I think this one is my best paper, and certainly the most original.

Bingham: Thank you very much indeed. It's always fascinating to hear a person say what *he* regards as his best work, and then look at the world at large . . .

Kendall: Very different perceptions.

Bingham: *Very* different perceptions! I think the mathematician in the street would say, "Some problems in the theory of queues (with discussion)"! [laughter] That must be your most influential paper, at any rate.

12. SPLINES

Bingham: Could you tell us more about your work on splines, David [12].

Kendall: Liliana Boneva and I spent a whole year working on splines. We considered splines on the line, and splines on the circle. And then, with some of the data we wanted to analyze, the splines were clearly living on a compact interval—we didn't satisfactorily deal with that. What we did was to say, when the spline gets to one of the ends of the interval, let us suppose it to have a horizontal tangent. But later, when I met Grace Wabha, I realized that this was rather feeble. She's considered this problem in great detail and has a technique using ordinates near to the ends of the interval—so that what you ought to do about the boundary conditions is to have an inclined tangent. What we did was really limited to splines on the whole line or the circle—our techniques then agree with everyone else's—but when it comes to splines on the half-line or the segment there are now more professional approaches.

Bingham: Do splines stem entirely from I. J. Schoenberg?

Kendall: I don't know. The origin of the word—and this takes one back to wartime days—is that if you wanted to calculate some quantity that is very laborious to calculate for many different values of something or other, you calculate it for a few points and then draw a smooth curve through it using what used to be called French curves. But what engineers do is to use a flexible blade with weights, called dogs, so that you can bend the blade and the dogs stay where you put them. The dogs go on the observations, and the splines are got from the form the blade takes in between. That's the technology, which one tries to imitate when one does splines theoretically.

13. BIRD NAVIGATION

Bingham: The other thing that's always fascinated me is your work on bird navigation [14].

Kendall: Ah yes, well, that was a work of piety. One of my great friends in Oxford was David Lack. He was Director of the Institute of Ornithology on the opposite side of the High from Magdalen, so it was always fun to go over and chat to him. Anyway, he'd been in Operations Research during the war, so we had a lot in common. When he died [in 1973] I thought it would be nice if I could write something about birds in some way, and the mathematics of bird navigation ought to be an interesting topic. There was a book by Matthews on bird navigation (Matthews, 1966), which is well written, and it contains extraordinary information. They did some experiments with the Manx Sheerwater on Skokholm, an island near St. Davids—not very far, of course, from our rocket-firing establishment—in one experiment they put a numbered band round the leg of a nesting bird and then sent it by air to the Eastern coast of the US. There it was released, and the time noted and the information posted back. The nest was regularly checked until the bird returned, in 12 days, actually before the letter reporting its release reached the island!

Bingham: Wonderful.

Kendall: Pole-seeking Brownian motion came into that. That was a nice topic in itself. It interested Harry, and he wrote a short note [14, page 410] about diffusion with a drift.

Bingham: How long did the bird project take you?

Kendall: Well, it was a long paper—much too long, I think now. I suppose it took about a year. Byron Morgan played an important role, because when you start writing down the differential equations that tell you what's going on, you get a nasty second-order equation with very awkward coefficients. But Byron Morgan for some other purpose had done a lot of work with the Bateman project books, and he was able to say you can fit this into the scheme and get an explicit solution.

Bingham: It slightly surprised me that it was someone of Byron's generation who said to you, confluent hypergeometric functions. I would have thought that someone of your vintage, who'd been brought up on Whittaker and Watson, . . .

Kendall: Oh yes—but then, you can live a long time and still be finding things out about confluent hypergeometic functions! And indeed they have them around still, with more parameters now, don't they?

Bingham: Indeed. The essence of what you say in the bird navigation paper, as I recall, is that there is a circle of confusion, and selective pressure has driven birds to be able to navigate to accuracy within the circle of confusion, but once they're inside and can see the target there is no biological bonus in being able to navigate any more accurately. Now in the 20 years in which the ornithological world has had your paper, has that broad idea been confirmed ornithologically—is that now mainstream thinking, do you know?

Kendall: I simply don't know. I don't have any ornithologists among my contacts now, so I haven't any information. [Professor G. V. T. Matthews has kindly carried out a literature search, and it seems that Kendall's work has not subsequently been cited in the ornithological literature.]

Bingham: Before leaving this topic, I must mention David Williams' lovely plug for the time-substitution method ([14], pages 414–415), which I think must have been instrumental in convincing you that Itô calculus was something one had to learn!

Kendall: Yes indeed!

14. SHAPE AND COLLABORATION WITH HUILING LE

Bingham: I think that disposes of everything I wanted to ask about before we begin on shape, which is clearly the last major theme.

Kendall: Shape for me started with the death of Henry Whitehead. I remember saying to Diana then what a huge opportunity I'd missed in not learning from him directly about topology, differential topology and so on—what a fool I was, not to have taken advantage of that—and this turned me towards geometry. I had a second sabbatical year coming up. I decided to devote it to trying to learn some geometry, which I attempted to do systematically. And after that came this idea of trying to build a theory of shape.

Bingham: Do you remember what sources you consulted, during your learning process?

Kendall: The main thing was to learn classical topology, homology theory and so on. And I went to lectures . . . which I found difficult.

Bingham: Algebraic topology has long been a British specialty—that is Henry Whitehead's doing, is it?

Kendall: Henry Whitehead and Max Newman.

Bingham: And Max Newman, yes. Simon Broadbent is credited with having launched you on the shape phase of your life; is that right?

Kendall: Oh well, in a way it is—you mean this business about alignments in two-dimensional random sets of points, the paper Wilfrid and I wrote together [16]? It develops further a theory for the analysis of data collected by my friend and former pupil Simon Broadbent. What he did was to go and survey what are called the Old Stones of Land's End—52 of them—which was quite a big task, and that has become a classic data set. And what we attempted to do in that paper—with some success, I think—was to construct a systematic approach to such problems. Recently Huiling Le and I have constructed an even better approach. Now Huiling Le, Keith Carne, Dennis Barden and I are writing a book on the subject.

Bingham: How's that coming along?

Kendall: Well, I've written quite a lot of my section. It's slow work, but it's making positive progress.

Bingham: I'm not sure that this is the right place to raise it, but in case I forget, I keep coming across references to a perhaps prototype book of yours called *Markov Methods*, based on a course you taught in the seventies—do you intend to publish this, David?

Kendall: I don't even know where the notes are!

Bingham: Hm—I hope Frank Kelly does—he cites you. The word processor has meant that anyone can turn a long-neglected file of notes into an attractive book in a few weeks.

Kendall: Yes—I'm sure I don't have it on disk. That precedes my computer phase.

Bingham: That's another thing I wanted to ask you about, your computer phase: what's your history as a computer?

Kendall: The first computer exercise I undertook was when a friend of Violet Cane's was here—Jessie McWilliams, who had worked on multidimensional scaling and so knew the inventors of multidimensional scaling, including Joe Kruskal. She was in Cambridge for a year, maybe less, and she knew I was interested in this program, and what she generously did was write a simplified version of it. And then a little after that, I got from Joe Kruskal a complete print-out of his very long program, and read it through completely and then wrote a suitably simplified version of that.

Bingham: That was the MDSCAL program that was used to produce horseshoe-shaped plots for Petrie's data?

Kendall: It produced horseshoe plots, yes, because I didn't realize at that time the importance of using primary rather than secondary scaling. The display down the corridor is a horseshoe plot. That's because it used secondary scaling, but if you run

it changing from secondary to primary scaling you get a more nearly linear plot, demonstrating the seriation much more clearly. Of course, this display is out of date now, but it's historically interesting; it's been shown in the British Museum.

Bingham: It's certainly historically interesting! That's how the plots were, when I used to listen to you lecturing about it. That was your great enthusiasm, when I was here.

And you must have used computing for your bird navigation studies. When did you start using things like TeX?

Kendall: I don't know—but I remember it was very painless; you just start doing it.

Bingham: Yes, I was surprised at how painless it was for me. Coming back to shape: when you learned Itô calculus, David, during your bird navigation period, that must have stood you in very good stead when you started to bring a Brownian component into your shape studies [16].

How far back in time does your association with Huiling Le go?

Kendall: It began when I went to China. Harry and I were invited to China, and then it was agreed that Harry couldn't possibly go (on health grounds), so David Williams came instead. And then, happily, Sheila Williams was told she could come too, provided she also gave talks on everyday life and such things—and she was very necessary, I may say, in keeping us both sane. Especially when it came to our intolerance of the food.

We went first to Guangzhou, which in Western language is Canton, and spent a week there, which we enjoyed greatly. The Department in Guangzhou is run by Liang Zhi-shun, who was brought up in a missionary school and is still very keen on English songs. We had a party just before we left, and they said, "You must sing us some English songs." David could manage the music, but none of us knew the words of more than the first verse of anything. But Liang Zhi-shun knew them all.

We went from there to Changsha and Xiangtan, where our original contact Hou Zhen-ting was working—at the Railway Institute, but now he is the Head of one of the universities there—and that was a very exciting visit because we had said that we very much wanted to see some of the surrounding country.

I particularly wanted to see some of the mountainous country, and he said, "From Changsha, only a hundred miles south of here there is a very fine mountain, and we'll *get permission* to climb it." So in due course there arrived passports for us to ascend the mountain *on a particular named day*. Well, the particular named day turned out to be the monsoon. So we said, oh well, that's off. "No," they said, "here it says, on your passport—you *shall* climb it, on such-and-such a date!" So of course, we had to go. And we went. About half the climbing was done in the truck, the rest on foot. It's quite a high mountain; it's one of the five sacred mountains. And when we got to the top there was a little Chinese temple, actually sitting just below the summit. We went inside, and there were some monks; we signed the visitors' book and so on. Then Hou Zhen-ting said, "Follow me," and behind the altar at the back end of the church, you can go out through a door and up a spiral staircase, and then you come out on a splendid rock which is *the top*. It was raining cats and dogs—absolute mist in all directions. And Hou said, "Look, face this way, ... bit more, ... no, back a little, ... now, ...—now, looking in *that* direction, I cannot tell you on a nice day what a wonderful view there is!" [laughter]

Bingham: Lovely story ...

Kendall: And then from there we went to Xi'an, which we greatly enjoyed. We got to know these two girls, of whom Huiling Le was one. I remembered her well—Huiling had been at Xiangtan, where we gave our longest lecture course. She was quite visible from the podium as the most seriously interested person, really hanging on one's lips. So after a bit I went up to her and chatted a bit and said, "What are you working on?" She said "Fuzzy sets." And I said, hmm ... [laughter]. But she was very willing to change to shape theory, she told me.

Bingham: You saved her from a fate worse than death!

Kendall: Well, that was the first meeting; we hoped she'd come here, and eventually she did. Of course, as you know she's now a Lecturer at Nottingham. I've just come back from spending a couple of days with her there. We've worked together now for nearly 10 years—another marvellous collaboration, exactly like that with Harry Reuter.

Bingham: Your blockbuster with her has just been published, in the last issue of *The Annals of Statistics* [19].

Kendall: Well, writing contributions for our joint shape-theory book is not my whole-time occupation, but very nearly so. And the last thing we've done in that area has been to produce a series of papers called "How to look at a five-dimensional space." We have a technique for looking at objects in the first nontrivial shape-space, which is five-dimensional—and you really can get a good impression of what's happening in it.

Bingham: What kind of data would that apply to?

FIG. 9. *Berkeley, 1953*: *S. Bochner on extreme left, facing D. G. Kendall, Diana Kendall, Henry Daniels and Erich Lehmann; Florence Baker in far back.*

Kendall: Well, it gives a setting in which you can plot observations, in which you can plot distributions, in which you can plot geodesics, and in each case you can interpret what you see. It's as near to seeing the real thing as you can get. It's quite impressive, when you see the pictures.

Bingham: Thank you. Well, I look forward to seeing that.

15. AND NOW...

Bingham: One of the general things that I think must be said about you, David, is the extent to which you serve as the effective beginning of the probability tradition in this country. Whenever I go to a meeting of the Committee of Professors of Statistics I look around me, and I see your old pupils everywhere I look, and more generally former Stats Lab pupils. You now have mathematical sons, and the odd daughter, scattered quite thickly. And, particularly through your two tremendously distinguished pupils, David Williams and John Kingman, you have mathematical grandsons wherever you care to look—including one particular person who is not only your physical son, but your mathematical grandson, and also your brother, because you have collaborated—that really must be quite unusual. That must also be a source of great pride to you. I believe it's the case that you have a mathematical great-great-grandson here in this very building.

Kendall: It wouldn't surprise me—who is it?

Bingham: Gareth Roberts studied under Saul Jacka, who studied under Martin Barlow, who studied under David Williams, who studied under David Kendall.

Kendall: That's a pretty good line, isn't it!

Bingham: It is, yes! One has to make an effort to dig any further back in British probability. The main influence in British probability before you that I'm aware of is Littlewood–Offord, still extant in the person of Cyril Offord, who lives in retirement in North Oxford. Also Sir Harry Pitt—incidentally a link with Norbert Wiener.

Kendall: But you have to remember P. J. Daniell of Sheffield. Daniell wrote his major papers in the US—in the South, I think. Who taught him?... Sheffield doesn't have a portrait. When he went to Sheffield he apparently gave up probability and started working on the design of blast furnaces.

FIG. 10. *Harry Reuter, Eileen Reuter and D. G. Kendall with Wilfrid Kendall standing behind, 1983.*

FIG. 11. *Huiling Le and D. G. Kendall at the Bernoulli Society Congress in Uppsala, 1990.*

Bingham: One other thing I wanted to ask you about is the Australian connection. As you are to British probability, Pat Moran was to Australian probability, and it's been a wonderful enrichment of the Statistical Laboratory, where we sit, to have a steady stream of wonderful Australians passing through it. Does that all stem from Pat Moran?

Kendall: I was wondering who the first Australian Stats Lab member was—Daryl Daley was at any rate an early one.

Pat Moran and I moved into the subject simultaneously, by the way. Besicovitch had a great influence on Pat.

Bingham: Besicovitch had a great influence on everyone who came his way—an honorary probabilist, almost. He influenced James Taylor, for instance, and me through him.

Kendall: Besicovitch once gave us a talk, when we were in the basement of the Chemistry Lab. We invited him to give a talk on Russian probabilists, and particularly Markov. He did it, though halfway through he said his legs had gone, and he had to sit down. He told us this story, about Markov. Markov's father didn't wish his son to be a mathematician. You know this story?

Bingham: I know this story from Peter Lee, but please tell it.

Kendall: He does it very well, doesn't he? [laughter] Well, he said, you shouldn't study mathematics, you should study history, history [spirited impersonation of Besicovitch's Russian accent—his English remained famously execrable, despite half a century in Cambridge]. "History very interesting subject. Historian find archive. He take down archive. He blow dust off archive (pff! pff!). He *copy* archive. He replace archive. His work is done!" [laughter] You know, Kolmogorov started in that area, in ethnographic history. When he was an undergraduate he was interested in finding out how a region called the Upper Pinega was settled—it was previously uninhabited. I mentioned this in the obituary [17], and they're now publishing those papers.

Bingham: Tremendous!

Are there any other specifics, David, or would you like to start summing up. Life, death and what we're here for, why do we do it, what does it all amount to and what does one tell the aspiring young?

Kendall: I can only answer the last question, even though I do come from a Non-Conformist family! But what you tell the aspiring young is, I think, *try to work on difficult or unusual problems*. Perhaps everyone's given them up because they haven't seen the right way to do them. You learn more by failing at a difficult problem than by succeeding at an easy one. And maybe it will teach you what you still need to learn . . .—but these are merely pious remarks.

Bingham: All right, let me part comment and part probe a little. One of the striking things about your life's work—apart from its sheer volume and its sheer quality—is how you have followed your own star wherever it has taken you; indeed, a succession of different stars have taken you in very different directions. Which is wonderful. But no man can serve two masters; the main stream of the subject pursues its own path, and one wanders away from it at some cost. I, like most active professionals, have stayed nearer to the main stream. You haven't chosen to do that. Was this deliberate policy, was it the accident of the various stimuli that have cropped up at various points in this talk? I once heard a man, who's no fool, say that your choice of topics had smacked a little of, I think his phrase was, studied eccentricity. Now, that's unfair—but you know what is meant by that.

Kendall: [laughter] Yes, I know what is meant. But you look at the targets that haven't been shot at much yet. If you find there's nothing to be done there you can always give it up. But you usually find something really interesting. You also meet a lot of new people, if it's an applied subject. Tomorrow, for instance—I don't think anything will come of this—but tomorrow, I'm supposed to go to the Hamilton Kerr Institute, which is an out-station of the Fitzwilliam [Museum, Cambridge] and works on the technology of preserving works of art. There's a clever young man there who is persuaded that a lot more can be learned than has yet been learned from the crazing of paintings—the crack system. This must be comparable to other phenomena that have been looked at in a probabilistic way, so he wants a probabilist to go and look at these pictures.

Bingham: Tessellations, mosaics, ...

Kendall: It's a bit worse than that, because there's more irregularity. I had a long talk about it with Wilfrid, when he came to see us last, and he said—and I thought he might be right—that the best thing you can do is start by looking at the longest crack. Then you look at the longest cracks that come off from that. Then you look at the longest cracks that come off from those, and in this way you impose at least a tree structure. But what you do when you've got the tree structure I don't know, and I wouldn't like to do the donkey work, either. It would be difficult to automate, I would have thought. But at least it would give you a description of the crack pattern, and might suggest something else on the way. [The latest information, however, makes it clear that the real problem is even more complex, so new ideas are needed.]

Bingham: That's a lovely prospect, and there I think we'd better leave it as a dream for the future.

Kendall: If I can't make anything of it, I think I shall just bore a hole and drop it into Frank Kelly's office, which is just underneath this one!

Bingham: Well, I think I've subjected you to all that it's fair to ask one person to endure in one sitting or two. It's been a great pleasure and a great privilege to talk to you, David, and I'm sure all the readers of *Statistical Science* will be very grateful to you for everything you've told us.

Kendall: When it comes out in the year two thousand and something!

Bingham: David Kendall, thank you very much indeed.

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