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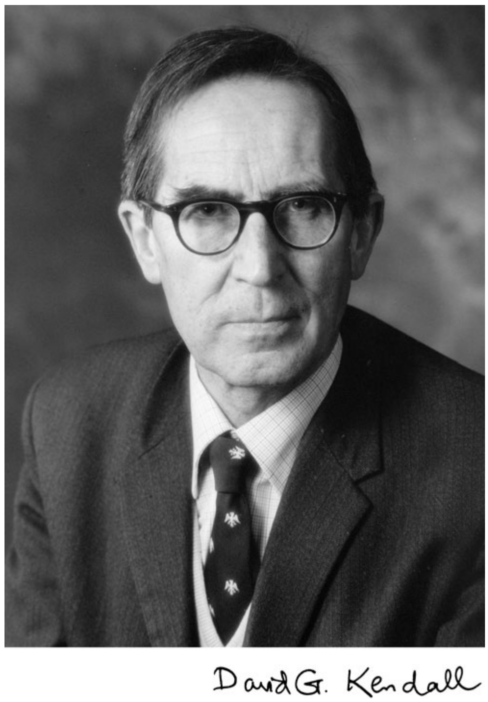
OBITUARY

David George Kendall, FRS, 1918–2007



David Kendall was the father of modern probability theory in Britain, a powerful and scholarly mathematician equally at home in abstract theory and in perceptive applications to diverse fields. Through his own research, his influence on generations of students, and his leadership of the Cambridge Statistical Laboratory from 1962, he inspired the parallel developments of stochastic analysis and applied probability, as well as the statistical analysis of complex structured data.

Ancestry and early life

We know of Kendall's forebears mainly from his own researches, which he summarized in a paper deposited with the Royal Society, and a version of which is in his papers in the Churchill College Archive in Cambridge. His paternal grandfather, Styan Kendall, was born in Whixley, near Knaresborough in North Yorkshire, and there are records of Kendalls living in the Knaresborough area back to the fourteenth century. The name Kendall may indicate that the family originated in the Kent valley in Westmoreland, but there is no reason to doubt a long Yorkshire connection.

The unusual name Styan was the maiden surname of Styan's mother, whose family is still extant in Whixley. At some point Styan Kendall moved to Ripon, also in North Yorkshire, where he married and produced a son, Fritz Ernest Kendall. It is believed in the family that Fritz, hardly a common Yorkshire name, was a tribute to the German Emperor Frederick III, the son-in-law of Queen Victoria, who reigned briefly in 1888 and whom Styan admired. No doubt the admiration did not extend to the next Kaiser, and Fritz Kendall himself preferred to be known as F.E.

If the paternal side came from inland Yorkshire, a more salty flavour entered on the distaff side. David's mother, Emma Taylor, had grown up in the seaside town of Whitby; her father, George Thomas Taylor, had been born in the nearby fishing village of Staithes. George had started life as an ironstone miner, but when his sweetheart, Hannah Mortimer, emigrated with her family to Australia, he became a merchant seaman to join her. They married and eventually returned to England, where George developed interests as a herbalist and magnetic healer. He was also keen on the stars, and transmitted to his grandson an enthusiasm for astronomy.

F.E. was a draper by trade in Ripon, but during World War I he joined the Royal Naval Air Force and was stationed at Felixstowe, the first foray of the family out of Yorkshire. Although his wife joined him there, David was born in Ripon, whither the family returned as the war ended. At the age of eight years he entered Ripon Grammar School, a famous and venerable foundation that offered an excellent education; he described the staff as 'fantastic, dedicated scholars every one of them'. Of particular importance was the senior mathematics master, George Viccars, who detected Kendall's flair for the subject and encouraged him from the age of 13 to press on for himself, reading Viccars's own Cambridge lecture notes and the *Pure mathematics* of G.H. Hardy FRS.

At the same time, the interest in the stars that Kendall owed to his grandfather was strengthened when a family friend offered him a three-inch telescope. The idea that astronomy might be more than a hobby crystallized when he heard the radio lectures on astrophysics given by Sir James Jeans FRS. However, the school advised that the way to serious astronomy lay through mathematics, so that the course was set towards a degree in mathematics. He also received encouragement from a Glasgow professor of botany, F.O. Bower FRS, who had retired to Ripon and who arranged for the young Kendall to visit observatories at Cambridge, London and Greenwich.

In a revealing interview that he gave to Nick Bingham ⟨**2**⟩, Kendall told affectionate stories about his school career, from which it is clear that he gained more than mathematical skill from the broad curriculum offered. It seems likely that he developed there the attention to detail and the refusal to be content with second best, which later became the high scholarly standard that he always set himself. These complemented a keen natural curiosity, which was also to characterize his scientific life.

Viccars, like most of his colleagues at the school, was a Cambridge man, and Kendall sat the scholarship examination at Caius. He won an exhibition, but his family's finances required a full scholarship. Fortunately, Ripon was one of the schools that were eligible for the closed scholarships on the foundation of Lady Elizabeth Hastings at The Queen's College, Oxford. Kendall was successful, and entered Queen's in Michaelmas 1936.

The Queen's College

He used to say that the Oxford award was one of his best pieces of luck, because he joined Queen's at the moment when U.S. Haslam-Jones became the tutorial fellow in mathematics. Haslam-Jones had been a pupil of Hardy's, and although Hardy himself had returned to Cambridge in 1931, his influence still pervaded the pure side of the Oxford curriculum. Kendall encountered a stern regime, in which he was encouraged to read La Vallée Poussin's *Intégrale de Lebesgue* in the original French, and to sit as the sole member of the advanced class on Fourier integrals given by the formidable E.C. Titchmarsh FRS.

If it is surprising that any active young man survived, and even enjoyed, this treatment, it is no surprise that it made Kendall a first-class mathematical analyst. It did not, however, diminish his interest in astronomy, and he contacted H.H. Plaskett FRS and E.A. Milne FRS, to such good effect that he won the Skynner Senior Studentship in Astronomy at Balliol, which he took up for a short time after graduating with first-class honours in 1939. Much later, his interest in astronomy motivated an essay into the theory of comets [**12, 13, 14**][†].

Although Queen's was a college with many men from Yorkshire, Kendall did not make friends easily at first, and he was easy prey for the Buchmanite Oxford Group Movement (later Moral Rearmament). When he realized that this was not the benign organization he had hoped, he managed to tear himself away and make a wider group of friends in and out of the college. He also resolved his religious beliefs into a quietly devout Anglicanism, in which he remained throughout his life.

In his second undergraduate year he wrote his first original paper, which arose from his reading in astrophysics and was published in the *Zeitschrift für Astrophysik* [**1**]. With hindsight, however, this is really a piece of mathematical analysis, indeed, of stochastic analysis, for it concerns the convolution of a Cauchy distribution and a normal distribution. However, probability in those days played no part in a respectable mathematics course, and it took a global conflict to change both British mathematics and D.G. Kendall.

World War II

Counterfactual historians may debate what Kendall might have become had war been avoided in 1939, whether an analyst in the tradition of Hardy and Titchmarsh, or an astrophysicist in the mould, say, of Hoyle or Bondi. The actual outcome was that he was posted to a group of mathematicians working in the Projectile Development Establishment (PDE) at Aberforth near Cardigan in southwest Wales. This was a rich mix, of new graduates like David and of more senior academics brought together by a perceived need for mathematical input into the engineering problems of rocketry. The group was led initially by W.R.J. (later Sir William) Cook (FRS 1962) and later by Louis Rosenhead (FRS 1946).

David was always very discreet about what went on at the PDE, and even in the 1996 Bingham interview he claimed still to be covered by the Official Secrets Act. However, Bingham included two vivid pictures to give a visual impression of what could not be put into words, and Kendall himself published with Kenneth Post two articles [**32, 33**] that give some of the background. It is clear that he entered into the spirit of the deadly game being played, and that he enjoyed both the interesting personalities he encountered and the wonderful scenery of that part of Wales.

He had always been a keen hill walker, and he later became a skilled and intrepid mountaineer. Many people retain fond memories of invigorating mathematical conversations conducted while walking at high speed and preferably on steep gradients, although some, including myself, found it impossible to keep up.

Much of the work of the Cook–Rosenhead group must have involved the mathematics of dynamics, ballistics and sighting, and Kendall had the company of mathematicians such as Robert Rankin, with whom he later wrote joint papers in number theory; but it is more to the point of this story that there was a significant statistical component, and that the group contained two experienced statisticians, Frank Anscombe and the great Maurice Bartlett (FRS 1961).

In his perceptive biographical memoir of Bartlett ⟨**12**⟩, Peter Whittle FRS remarks that 'he had already established his reputation in the period 1932–40 with a series of fundamental

[†]Numbers in this form refer to the bibliography at the end of the text.

advances in the theory and practice of statistical methodology'. The scientific turning point of Kendall's life came when Bartlett and Anscombe were posted to London, leaving a statistical vacuum at the PDE. Rosenhead's solution was to give Kendall a week to stay with Anscombe in London, making himself a statistician by studying the notes of the lectures that Bartlett had given in Cambridge before the war.

It is not surprising that the practical problems of both offence and defence in rocketry should have involved the calculation of errors and statistical variability. What is more of a mystery is the fact that their wartime activities led both Bartlett and Kendall to focus on random phenomena evolving in time, and in particular on models of such phenomena by Markov processes. They obviously remained in contact, and when the war ended their common interest quickly became public and inspired the post-war development in Britain of what came to be called applied probability.

There was, however, one last wartime duty. Kendall was attached to a body called 'T-force', whose job it was to travel through defeated Germany, trying to discover what German scientists had achieved in relation to the problems on which their Allied counterparts had been working. It is clear that this was a frustrating exercise, but he used the opportunity to visit universities and observatories and to contact astronomers and mathematicians who shared his interests. He already knew that he would return to Oxford, having been pre-elected to a tutorial fellowship at Magdalen College, and he was preparing for a career in teaching and research.

Back to Oxford

Kendall took up the Magdalen fellowship on 1 January 1946; he was to remain there until 1962. The college had previously sent its mathematics undergraduates to be taught in other colleges, but its President Sir Henry Tizard FRS thought this inappropriate, and his inside knowledge of the various wartime scientific establishments made it possible for him to appreciate Kendall's abilities. The Waynflete Professor of Pure Mathematics was *ex officio* a (non-teaching) Fellow of Magdalen, and the election of Henry Whitehead FRS to the chair in succession to the venerable A.L. Dixon FRS gave added strength to the mathematical element of the college.

College life proved very agreeable. Fellows under the old statutes were elected for life, and so there were both old and young colleagues, and David found the intellectual atmosphere highly stimulating. His undergraduate tutorial load was light compared with other colleges, six to eight hours a week, and he proved an effective, if demanding, tutor.

In the first years after the war, the students were largely returned servicemen who no doubt needed a firm hand, but later undergraduates fresh from school often found D.G.K. (as he came to be known) rather frightening. This was partly because of the high standards he demanded of them as he did of himself, but partly also due to an unnerving habit of silence. If he had nothing to say, he said nothing.

Most came to see behind the sometimes stony exterior, and to regard David as a real friend. He himself regarded friendship as one of the major virtues, and a recurring theme throughout his life was the way in which teachers, pupils, colleagues, neighbours and chance acquaintances became friends, and the recipients of his quiet kindnesses.

Under the curious Oxford system of the Common University Fund, Kendall was expected to give lectures and classes in mathematics for the University's Faculty of Physical Sciences. The syllabus was very old-fashioned, and he lectured on 'Integral calculus', 'Differential geometry of plane curves' and for many years 'Complex variable'; but he also insinuated probability, aided by Pat Moran (FRS 1975), John Hammersley (FRS 1976) and on the statistical side David Finney (FRS 1955).

Although he had been so greatly influenced by Bartlett, Kendall's approach to probability was very different. To one brought up on the hard analysis of Hardy and Titchmarsh, Bartlett's pragmatic approach lacked conviction. What were these 'random variables' about which probability calculations could be made? What were the limiting processes involved in the celebrated theorems of Abraham de Moivre FRS and Pierre-Simon Laplace FRS? What exactly was the 'law of large numbers'?

He was delighted to discover that these questions had been answered definitively by A.N. Kolmogorov (ForMemRS 1964) in a book ⟨**9**⟩ that was almost unknown in Britain. At last probability calculations could be made within a firm mathematical framework, essentially that of the Lebesgue integral. There was no more need for hand waving, or for shallow tautologies of the kind that asserted that random variables are quantities that vary randomly.

In the wrong hands, this approach could have become sterile and pedantic. Kendall's achievement was to enliven his rigorous analysis by continual reference to real applications, and to show how the Kolmogorov paradigm added power to calculations of real importance.

Marriage and family

Life in Oxford was not all mathematics. David took up bell ringing and Scottish dancing, and he joined the City of Oxford Anti-Aircraft Battery. He took cycle trips with David Finney and pursued his mountaineering interests in Switzerland, Austria and the Pyrenees. And somewhere along the line he met Diana Fletcher, whom he married in 1952. He took her to Princeton (about which more later) and on their return to Oxford they moved into a flat provided by Magdalen.

There the law of large numbers began to take effect. Their eldest son, Wilfrid, now himself a distinguished probabilist, was followed by Bridget, who as Bridget Kendall MBE is an intrepid and perceptive television correspondent. Eventually there were six children, living in a large house on the outskirts of Oxford where they dispensed warm hospitality to generations of students and colleagues.

Diana shared and encouraged David's vivid and eclectic curiosity, and was an essential support to him throughout his life[†]. A happy and lively family brought out the friendly and generous side of his character, to the extent that those who only knew him in later life find it hard to credit tales of his earlier austerity.

When the Kendalls moved to Cambridge with their growing family, they settled into a house off the Trumpington Road from which David could easily walk (and Diana cycle) to town and college. To the end of his life he remained vigorous, and his rapid and long-legged stride made him a distinctive figure around Cambridge.

Applied probability

We return to 1946: Kendall is settled in a tenured job in Oxford, with time to think about mathematics and freedom from wartime restrictions on publication. Bartlett came to stay, just before leaving for North Carolina to give a pioneering course of lectures that eventually became his book ⟨**1**⟩ on stochastic processes. Kendall decided to work on Markov models of the evolution of biological populations, and Bartlett invited him to contribute to a major discussion meeting at the Royal Statistical Society (RSS). The three papers, by Bartlett himself, by Kendall [**2**] and by J.E. Moyal, are contrasting demonstrations of the efficacy of probabilistic calculations in sharpening the questions asked by biologists and physicists. Of course, the models that Kendall studied are crude by modern standards, but they exhibit many of the crucial aspects that later work illuminated.

[†]Note added in proof (6 February 2009): Diana Louise Kendall survived her husband by little more than a year, and died in December 2008.

Much more influential, however, was the sequel, his 1951 paper on the theory of queues [**3**]. In his interview with Bingham he tells us how he first started thinking about queueing problems in the context of the 1948 Berlin airlift. With typical thoroughness, he combed the Oxford libraries for work on queues, and eventually discovered the work of A.K. Erlang (see ⟨**3**⟩) on congestion in telephone systems. He found out, too, that Erlang's work had been developed by teletraffic engineers, notably in Scandinavia and at the Bell Laboratories in the USA, and by Felix Pollaczek and A.I. Khinchin, and he decided to offer the RSS a discussion paper drawing this work together with the aid of modern probability theory.

The first volume of William Feller's *Introduction to probability theory and its applications* ⟨**7**⟩ had just been published, containing a systematic account of Markov processes in discrete time with countably many states. Pollaczek and Khinchin had treated the single server queue with Poisson arrivals and a general service-time distribution by *ad hoc* methods, which Kendall showed to depend on the existence of an embedded Markov chain describing the numbers left in the queue by successive departing customers. He gave a detailed analysis of this chain by Feller's method, deriving all the known results in a unified way.

In a subsequent paper [**4**], Kendall showed that embedded Markov chains exist in other queueing models. This is the paper that introduced his famous notation for such models. The Pollaczek–Khinchin theory applies to M/G/1, and now the same method works for the k-server queue GI/M/k. (It has not gone unnoticed that a k-server queue with deterministic arrivals has the symbol D/G/k.)

The two Kendall papers generated enormous interest, and publicized queueing theory far beyond the teletraffic community. Not only were probabilists encouraged to apply the theory, but also, the budding discipline of OR ('operational research' in the UK; 'operations research' in the USA) made it a major tool. Kendall himself played little part in this explosion, and indeed criticized its wilder excesses, but he applied similar techniques to the theory of dams [**10**], the spread of epidemics [**7**] and with Daryl Daley to the spread of rumours [**16**].

The overall term for these developments is 'applied probability', and as it developed it became too big for the established journals in statistics or in probability theory. In 1963, Joe Gani and colleagues in Australia decided to try to set up a new *Journal of Applied Probability* (*JAP*) dedicated to the new area. They asked D.G.K. to be one of the editors, and he not only agreed but also persuaded the London Mathematical Society (LMS) to give financial support. The venture was a great success; *JAP* and its sister publications by the Applied Probability Trust (APT) are today among the major journals in the discipline worldwide. A more detailed account of Kendall's involvement, and of his work in applied probability, can be found in ⟨**5**⟩.

In the Churchill Archive there is a paper written by D.G.K. in 1975 about the 'usefulness' of his research, which contains a revealing passage about the work on the spread of epidemics:

> Another relevant point is that the 'epidemic' work to which I have referred in fact started and was quite far developed in 1949; it was deliberately suppressed for six years, because it appeared that the medical world was not particularly interested at that time, while the biological warfare people were all too unpleasantly interested. I only released it, six years later, when a change in the attitude of the MRC, (brought about, I suspect, by Sir Edward Collingwood, its Treasurer, who happened also to be a distinguished pure mathematician) made it likely that its positive 'use' might now outweigh its negative 'use'.

(In this passage MRC means the Medical Research Council, whose Treasurer was E.F. Collingwood FRS. He was also Treasurer of the LMS and helped the decision to support APT.)

Princeton, 1952

Perhaps as a result of his queueing paper, Kendall received an invitation to spend the academic year 1952–53 at Princeton University. Statistics was very strong there, with J.W. Tukey and S.S. Wilks, but his main contact was William Feller. He was able to visit the other active probabilists in the USA, including J.L. Doob at Urbana (whose magisterial *Stochastic processes* ⟨**6**⟩ was in the press), Mark Kac and Kai Lai Chung at Cornell. Coming as he was from Britain, where he was virtually the only rigorous probability theorist, it was an eye-opener to experience the rapid progress being made across the Atlantic.

It was Chung who alerted him to recent work on Markov processes in continuous time, to some remarkable and surprising phenomena discovered by Kolmogorov, and the almost mystical explanations by Paul Lévy. This sowed the seed of his future joint work with Reuter, but it was fertilized by Feller, who realized the significance of the theory of one-parameter semigroups being developed by E. Hille, K. Yosida and R.S. Phillips (see ⟨**11**⟩). Up to that time D.G.K. (and indeed most British mathematicians) had been innocent of the abstract approach to linear operators that came to be called 'functional analysis' and was rather frowned on by classical analysts as allegedly avoiding the really difficult questions.

On his return to Oxford he gave a short lecture course on 'Functional analysis', and he also took on David Edwards as a research student. These linked events proved decisive, and established operator theory as a respectable branch of mathematics in Oxford. The episode illustrates two important facts about D.G.K. The first is that he was a superb lecturer, clear and organized, but at the same time able to excite an audience with his own enthusiasm. The second is that, if he felt the need to learn a new branch of the subject, he would put himself down to lecture on it. This he found to concentrate the mind, and very often he had to advance the subject to have it in a suitable form for his audience.

So it came about that David and Diana returned to Britain on the *Queen Mary*, David invigorated mathematically and keen to apply his new tool of operator semigroups to continuous time Markov chains; and he had in mind an ally in this endeavour.

Kendall and Reuter

G.E.H. (Harry) Reuter was the son of Ernst Reuter, who later became the Mayor of Berlin. Harry had come to England before the war and had been befriended by the Cambridge mathematician J.C. Burkill (FRS 1953). He became a mathematician himself, and was teaching at the University of Durham. He had become interested in the equations for birth and death processes from a purely analytical point of view, and was writing a paper with Walter Ledermann on the subject ⟨**10**⟩.

Kendall knew that Reuter had the analytical tools, including operator techniques, to attack the Kolmogorov phenomena, but he also realized that a proper understanding would only come if the probabilistic behaviour of the Markov chain could be elucidated. The two of them complemented one another perfectly, and they also became firm family friends. More immediately, they set themselves the goal of writing a major paper on Markov chains for the 1954 International Congress of Mathematicians in Amsterdam.

They consider a family of random variables $X(t)$ indexed by a real parameter t representing time. The random variables take values in a countable set I representing the possible states of some random system. The Markov property means that the state $X(t)$ contains all the information needed to make probability statements about the system at later times; this fact, combined with weak conditions of continuity and time invariance, defines the theory of continuous-time Markov chains as it was developed by Kolmogorov, Doeblin, Doob and others see ⟨**4**⟩.

Kolmogorov showed that there were always finite transition rates from one state to any other, and that if I is finite, these determine uniquely the joint distributions of the $X(t)$ by means of a family of differential equations. Special cases of these equations had been used by applied probabilists such as Erlang, and they had proved useful even when I is infinite; but Kolmogorov's examples showed that all could go horribly wrong.

The Hille–Yosida semigroup theory provided a way around this. Instead of specifying the transition rates, one calculates an infinitesimal generator, which is a linear operator, possibly unbounded, defined on a dense subspace of the space of absolutely convergent sequences on I. (One can imagine the reaction of a telephone engineer.) On the other hand, Paul Lévy had described possible behaviour of the sample functions $X(\cdot)$ that might account for some of the Kolmogorov excesses.

What Kendall and Reuter did, at Amsterdam [**5**] and in succeeding papers [**6, 8, 9, 11**], was to draw together these two quite different approaches. They calculated the infinitesimal generators of Kolmogorov's examples and described in detail their sample function behaviour. This enabled them to construct even wilder examples and to define accurately the limits of applicability of the Kolmogorov differential equations.

Their joint research student, David Williams (FRS 1984), took up these ideas to discover deep properties of the transition rates corresponding to the different possibilities of sample function behaviour. It turns out that these are best understood by compactifying the countable set I and regarding the process as taking values in the larger space. Many others have contributed to what is now a rich theory, but there is no doubt that Kendall and Reuter took the decisive step.

Moreover, these papers, like all of Kendall's writings, were beautifully composed, precise without being pedantic and immensely readable. Kendall always wrote with clarity and elegance, whether proving deep theorems or describing practical applications.

From Oxford to Cambridge

The history of statistics in Cambridge is a tangled tale, which has been ably chronicled by Peter Whittle in *A realised path* (unpublished but accessible on the Statistical Laboratory's website). Bartlett left for Manchester in 1947, but that same year a Statistical Laboratory was set up under John Wishart, with the remit to teach a postgraduate Diploma in Mathematical Statistics. It recruited excellent staff, including H.E. Daniels (FRS 1980), F.J. Anscombe, D.V. Lindley, D.R. (later Sir David) Cox (FRS 1973) and Whittle himself. But it was not to last. Staff left for different reasons, and by 1961 the Laboratory was a tiny rump, housed in a basement of the University Chemical Laboratories from which Sir Alexander (later Lord) Todd FRS was forever trying to evict them.

Pressure for change came from outside Cambridge, to the extent that the RSS offered to launch an appeal for funds to establish a chair of statistics, to revitalize the Laboratory and the teaching of statistics, especially to mathematics undergraduates. The appeal was successful, and somewhat reluctantly the university advertised a Professorship of Mathematical Statistics in late 1961. The contribution of the RSS was recognized by including the current President, M.G. Kendall (no relation to D.G.K.) of the London School of Economics, and E.S. Pearson (FRS 1966) as electors.

Indiscreet correspondence in the Churchill Archive makes it clear that Kendall and Pearson compared notes before the electoral meeting and found that they had independently concluded that D.G.K. was the strongest candidate. They seem to have had little difficulty in convincing their fellow electors, and they persuaded D.G.K. that he had a calling to missionary work in Cambridge. He took up his post in October 1962.

He found Cambridge mathematics about to enter a period of change. Traditionally the Mathematics Faculty, although the guardian of the oldest Tripos, was disorganized, with its teaching staff scattered among the colleges. There was no equivalent of the Mathematical Institute in Oxford; the university provided offices only for the Lucasian, Plumian, Lowndean, Sadleirian and Rouse Ball Professors. But the applied mathematicians, led by George Batchelor FRS, formed themselves into a Department of Applied Mathematics and Theoretical Physics (DAMTP), and in self defence the remaining mathematicians created a Department of Pure Mathematics and Mathematical Statistics (DPMMS) under Sir William Hodge FRS.

It happened that Cambridge University Press was moving its operations out of the centre of Cambridge, and DAMTP and DPMMS were allocated buildings on the Pitt Press site. The Statistical Laboratory, as part of DPMMS, moved into the old paper warehouse, which was sensibly converted to academic use.

The statisticians in the Laboratory had often been looked down on by more orthodox mathematicians, but Kendall had to be taken seriously. He developed an excellent relationship with Hodge, and with Hodge's successor J.W.S. Cassels (FRS 1963). They encouraged Kendall to appoint new staff, to attract overseas visitors and high-quality research students, and they helped to create a new Churchill Professorship of Mathematics for Operational Research, to which Peter Whittle returned in 1967.

David was *ex officio* Director of the Statistical Laboratory, the first post of leadership responsibility of his career. He was assiduous in carrying out the sometimes mundane tasks of this office, but it also brought out his instinct to be helpful and kindly. He cared about his colleagues, his students and the support staff, and the atmosphere was one of camaraderie but also of serious research and teaching.

This is not the place for the history of the Laboratory under his leadership. For that the reader is referred to Whittle's account and to the series of annual photographs (also on the World Wide Web), which show graphically how it flourished, and how many established and budding statisticians of distinction spent time there.

David remained Director of the Laboratory until 1973, when the rules were changed and he handed over to Peter Whittle. He remained active in the Laboratory up to his retirement as professor in 1985 and for years afterwards. He loyally supported the new Director and took an avuncular interest in the new lines of research that were developing. The D.G.K. of earlier years had mellowed, and he was now called David by all.

On his arrival in Cambridge he had been offered a Professorial Fellowship of the newly established Churchill College. It was attractive as a complete contrast to Magdalen, and David soon became a loyal member of the college. The College Statutes allowed him to become a life fellow after his retirement, and he enjoyed the opportunity to contribute in different ways throughout the rest of his life. He was, for instance, deeply involved with the development of the Churchill Archive, and it is appropriate that the contents of his filing cabinets have come to rest there.

New lines of research

The Statistical Laboratory was an environment quite different from anything Kendall had so far experienced. The teaching of the diploma was built around statistical projects arising from real problems, and many departments in the university brought requests for statistical consultancy. David's intense curiosity was piqued by the rich variety of applications, and it is not surprising that he began to think rather seriously about the analysis of different sorts of statistical data.

This is not to say that he abandoned probability theory, but his contributions to pure theory tended now to be in the form of inspiration to his students. He was always extremely generous with his ideas, insisting that his students take credit for their elaboration of thoughts that had come from him. For instance, he provided several of the key elements of my own theory of regenerative phenomena ⟨**8**⟩. Even more remarkable was his collaboration with Rollo Davidson, the story of which is told in the two volumes [24, 26] that he edited with Ted Harding after Rollo's untimely death.

However, the trademark of D.G.K. in Cambridge was the analysis of statistical data having special structure. The first example was the problem of seriation of archaeological sites. He had learned from the Ashmolean Museum in Oxford that the great Egyptologist Sir Flinders Petrie had classified predynastic sites by the pottery types they contained, recording the results on a large collection of slips of paper that he had intended to try to order manually on the basis that sites with similar pottery were probably close in date. Unfortunately the museum had destroyed these slips after Petrie's death, but it was still a real question whether such a seriation was in principle possible, using modern computers.

His conclusions, in the absence of the data, were necessarily tentative, but the work brought him into contact with the world of archaeology. He worked with Roy Hodson, with whom he organized in 1970 a joint conference of the Royal Society and the Romanian Academy, at Mamaia in Romania, on mathematics in the archaeological and historical sciences. His papers on seriation [15, 17, 18, 19, 21, 22] range from the purely mathematical to the almost earthy.

The seriation problem is one-dimensional, but there are interesting problems in higher dimensions too. A favourite example of his concerned the villages on Otmoor near Oxford, which were being studied in some detail by medieval historians. Kendall got hold of data on intermarriage between villages and showed [23] that the map of Otmoor could be reconstructed from these alone, by postulating that marriages occurred more often between nearby villages.

By far the most fruitful problem, however, was suggested by Simon Broadbent, who had been a student of D.G.K.'s in Oxford. Broadbent had become interested in the alleged phenomenon of Ley lines, according to which it seemed that important historical sites were very nearly collinear. He had collected data and had tried to assess whether these contained statistical evidence for the existence of such lines. He discussed this with Kendall, and they concluded that it was necessary to ask whether, in a completely random array of points, there are likely to be sets of three or more points that are in some sense nearly in a straight line. What, for instance, is the probability that there exists a triplet of points such that the triangle they form has one angle less than, say, $1°$?

Such questions turn out to be very difficult mathematically, as well as leading to significant statistical difficulties. Kendall realized that he had to address fundamental questions of shape. The size and orientation of the random triangle (or a more complex polygon if more than three points are considered) are irrelevant, so that two triangles are equivalent if they have the same angles. The equivalence classes of triangles comprise a structure that is not at all obvious, but it turns out that they can be represented in a natural way on a shape space, which is a curvilinear triangle with a metric and an invariant measure. Thus, data such as those of Broadbent can be exhibited as an array of points in the shape space, with narrow triangles represented by points near one particular side of the curvilinear triangle.

There is in the Churchill Archive a page of manuscript headed 'The (wonderful) world of triangles', with a beautiful freehand drawing of the curved triangle, and elaborate notes explaining its interpretation. This illustrates vividly both his geometrical insight and his artistic ability; among his many talents he was an accomplished amateur painter. It also goes some way in explaining how excited he was with his discoveries, and how he communicated his enthusiasm to a number of collaborators.

His first paper [27] on the subject asks how the shape of a triangle formed by three independent diffusing points itself diffuses on the shape space, and it is very much in the spirit of his former student David Williams. His second is joint with his son Wilfrid [28], and effectively debunks the Ley line theory. But his most extensive collaboration was with Le Huiling (Figure 1), whom he met on a visit to China in 1983 and who eventually published with him the definitive account of shape theory in a book [34] with D. Barden and T.K. Carne.

FIGURE 1. *D.G.K. with Le Huiling at the Bernoulli Congress in Uppsala, 1990.* (*Photographer unknown.*)

David regarded his collaboration with Le as comparable to that of his with Reuter; these were the really fruitful interactions in his mathematical life.

Shape theory in two dimensions is now well understood, but in higher-dimensional spaces there remain challenging problems, for the topologist and differential geometer, and for the probabilist and statistician. Three of David's last papers [**29, 30, 31**] explain how to look at the shapes of tetrahedra in three dimensions, for which the shape space is five-dimensional. One of his regrets is that he did not take up these problems earlier, when he could have consulted his Magdalen colleague Henry Whitehead.

Shape and shape theory (the only book of which D.G.K. is an author rather than an editor) is the coping stone of a remarkable list of publications, broad and deep. After its publication, when David was more than 80 years old (Figure 2), his mental powers began to fail, although his physical vitality never left him. He died peacefully in Cambridge among his devoted family.

Learned societies and journals

David Kendall was a great believer in the importance of the various scientific groupings, formal and informal, that provide encouragement and opportunities for communication and collaboration. He joined the LMS as early as 1940, and served on its council before becoming its president from 1961 to 1963. The LMS awarded him its highest honour, the De Morgan Medal, in 1989.

An important development, loosely linked to the LMS, was the Stochastic Analysis Group (StAG), which he and Reuter founded in 1961 [**25**]. D.G.K. used the phrase 'stochastic analysis' to emphasize that probability is fully a branch of rigorous mathematics, with a broad toolkit of mathematical techniques drawn from analysis, geometry and algebra, serving a wide variety of applications, and (less well understood) providing methods for solving purely mathematical problems. For many years there were StAG meetings at the annual British Mathematical Colloquia, and the Group was much appreciated by young mathematicians interested in probability but disconcerted by the dismissive attitude of some traditional mathematicians.

FIGURE 2. *David George Kendall, at home in Cambridge, Easter 2007, aged 89 years.* (*Photo by Felicity Kendall Hickman.*)

In the end, StAG was the victim of its own success as probability entered the mainstream, but its influence in its time should not be underestimated.

We have already seen that the RSS had an important role in Kendall's early development, and he particularly enjoyed the Ordinary Meetings at which (often extraordinary) papers were read and discussed, the discussions recorded for posterity in the society's journal. Not only were some of his most important papers published in this way, but he joined in the discussions of those of others, often with short but incisive comments. He was twice awarded the Society's Guy Medal, first in Silver for his queueing paper [**3**], and then in Gold in 1981 for his lifetime's work.

The Royal Society was slow to recognize D.G.K. He was not proposed for Fellowship until November 1958, and was elected in 1964. He would have felt it anomalous to be elected before Bartlett, and Bartlett's election had to run the gauntlet of the disapproval of R.A. Fisher FRS. However, once elected, D.G.K. proved an enthusiastic Fellow, serving twice on Council (1967–69 and 1982–83) and being awarded the Sylvester Medal in 1976.

As his international reputation developed, he became more involved in activities in Europe and beyond, and one of his most important contributions lay under the umbrella of the International Statistical Institute (ISI). The ISI was, and is, largely dominated by what used to be called political arithmetic, but in 1958 David's friend Jerzy Neyman (ForMemRS 1979) persuaded it to set up a section that eventually became the International Association for Statistics in the Physical Sciences (IASPS). D.G.K. was closely involved with the IASPS and became its president in 1973.

Most meetings of the IASPS were in Europe, where they competed with an annual European Meeting of Statisticians and with several other meetings on stochastic processes. The time seemed ripe to draw these together. David's first instinct was to defend the organization of which he was President, but more statesmanlike thoughts prevailed and he played a crucial part in setting up a new body, the Bernoulli Society for Mathematical Statistics and Probability. To great acclaim, he became its first president.

We have already seen that D.G.K. was important in establishing the APT, and he was an editor of its journals *JAP* and *AAP*. He also helped Klaus Krickeberg and Leo Schmetterer to set up in 1962 a new journal with a less applied flavour, with the splendid name *Zeitschrift für Wahrscheinlichkeitstheorie und verwandte Gebiete* (now, alas, anglicized as *Probability Theory and Related Fields*). Both of these ventures he supported in the most effective way by publishing his own papers there and by encouraging his students and colleagues to submit good work.

Eastward ho!

D.G.K. was an inveterate traveller, but his travels were biased by a characteristic attitude. He preferred to go to places whose mathematicians would find it difficult to come to him in Britain. Thus, he visited the USA comparatively rarely (although he enjoyed the Berkeley Symposia organized by Neyman) but liked to go, when he could, to the countries under Communist rule whose scientists were not free to travel.

Initially, this led him to Romania, Bulgaria and Poland, and he was helped by the Royal Society's links with the scientific academies in those countries. His first visit was to Romania in September 1968, to a conference in Brasov attended by such distinguished scientists as Paul Lévy, B.V. Gnedenko and E. Sparre Andersen. He returned the next year, but this time he went on to Bulgaria, where he met Liliana Boneva, with whom he was to work on spline transformations [**20**]. In 1970 he helped to organize the Mamaia conference already mentioned, which raised interest in his own work on seriation and map reconstruction [**22, 23**]. His last visit to Romania was in 1994, when he was admitted as an honorary member of the Romanian Academy.

Kendall's links with Poland were connected with his friendship for Neyman, who was admired as a son of Poland even though he had lived for decades in California. In 1974 D.G.K. published an appreciation of Neyman, and for this and other services he was given the title of *Amicus Poloniae*. He again visited Poland in 1981, his zest for scientific collaboration undiminished.

He visited Russia several times and developed a strong family link with Sergei Kapitza, the son of Pyotr Kapitza FRS and a well-known science broadcaster.

Later, as has already been noted, his interest moved to China, and his 1983 visit with David Williams made a lasting impression on him. Towards the end of his life, when he no longer travelled, he enjoyed the company of visitors from China with whom he could exchange memories of his wanderings. In China, as in eastern Europe, his name is revered.

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