“It took a global conflict” — the Second World War and Probability in British Mathematics

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Abstract
In the twentieth century probability became a “respectable” branch of mathematics. This paper describes how in Britain the transformation came after the Second World War and was due largely to David Kendall and Maurice Bartlett who met and worked together in the war and afterwards worked on stochastic processes. Their interests later diverged and, while Bartlett stayed in applied probability, Kendall took an increasingly pure line.

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Probability played no part in a respectable mathematics course, and it took a
global conflict to change both British mathematics and D. G. Kendall.

Kingman “Obituary: David George Kendall”

Introduction

In the twentieth century probability is said to have become a “respectable” or “bona fide”
branch of mathematics, the transformation occurring at different times in different
countries.\footnote{Kingman (2009: 125) and Kac (1982: 62) writing of Britain and Poland respectively.}
In Britain it came after the Second World War with research on stochastic
processes by Maurice Stevenson Bartlett (1910-2002; FRS 1961) and David George
Kendall (1918-2007; FRS 1964).\footnote{Autobiographical details are scattered through their writings but, for Bartlett in his own words, see Bartlett (1982) and Olkin (1989) and, for Kendall, Bingham (1996). There are memoirs by Whittle (2004) and Kingman (2009) and I have used the Bartlett Papers at the Royal Society (filed as MSB) and the papers of the Projectile Development Establishment at the National Archives.}
They also contributed as teachers, especially Kendall
who was the “effective beginning of the probability tradition in this country”—his pupils
and his pupils’ pupils are “everywhere” reported Bingham (1996: 185).

Bartlett and Kendall had full careers—extending beyond retirement in 1975 and ‘85—
but I concentrate on the years of setting-up, 1940-55. War brought together Bartlett, an
established mathematical statistician, and Kendall, a novice mathematician; afterwards
they worked on processes in the field now called “applied probability”; in the 50s
Kendall’s research took a pure turn, combining Markov processes and functional
analysis, while Bartlett consolidated his position in the applied field with *An Introduction to Stochastic Processes: with Special Reference to Methods and Applications* (1955).

When Bartlett and Kendall started there were two distinct approaches to stochastic processes, as Feller (1949: 404) explained:

> In the mathematical literature stochastic processes are usually treated in a formal and general way which does not clearly show the practical meaning and applicability. On the contrary, practical problems leading to stochastic processes are usually treated by special methods and under various disguises so that the connection with the general theory does not become apparent.

Treating practical problems by special methods was a British speciality, becoming the *methods and applications* of Bartlett’s book. Kendall liked stories and two from his (1990: 34; 35) appreciation of Kolmogorov illustrate the vigour and isolation of the British contribution: “Feller used to say that if Kolmogorov had not written his 1931 paper, the whole of stochastic diffusion theory would eventually have been pieced together starting with the ideas in Fisher’s *Genetical Theory of Natural Selection* (1930)” and “I once asked Sydney Chapman [physicist author of (1928)] about ‘Chapman-Kolmogorov’ and was surprised to find that he did not know of that

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3 There was much British work on probability with no part in the present story, including books by Keynes, Burnside and Jeffreys and—quickly forgotten—contributions to Continental probability from Edward Linfoot on the law of large numbers and Alan Turing on the central limit theorem. For Linfoot and Turing see Aldrich (2009a) while Aldrich (2008, -5 and -9b) discuss Keynes, Jeffreys and Burnside and the probability tunes they danced to. Aldrich (2007: Section 11) sketches the different British probability traditions.
terminology.” The attitude of pre-war British statisticians is caught in a remark Doob (1996: 586) attributes to Egon Pearson: “Probability is so linked with statistics that, although it is possible to teach the two separately, such a project would be just a tour de force.”

The account below describes the first years of two lives in probability and how they were shaped by ‘the times’—manpower planning in war-time Britain, the flight of Jews from persecution in Continental Europe and the shift of intellectual weight from Europe to the United States. Wartime planning brought together Kendall and Bartlett and Nazi persecution led Jo Moyal, Bartlett’s guide to Continental probability, to Britain and Willy Feller to America. In the course of a few years the world map was redrawn. In 1937 Maurice Fréchet organised a conference on probability theory in Geneva—recalled by Harald Cramér (1976). Cramér and Feller came from Sweden, Hugo Steinhaus from Poland and the Pole, Jerzy Neyman, from Britain; no Soviets attended. Nobody came from the United States and yet in 1946 Bartlett was there discussing stochastic processes with Cramér, Feller, Mark Kac (a student of Steinhaus) and Niels Arley, Danish physicist but Stockholm probabilist by adoption. Kendall spent 1952/3 with Feller in Princeton and saw Kolmogorov in Amsterdam in 1954; in 1956 Bartlett visited Moscow.

1 Initial conditions

I begin in the last years of peace with Bartlett and Kendall, their circumstances, and Britain’s place on the probability map. Practical problems in time series correlation

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4 For the allocation of scientific manpower see Barnard and Plackett (1985) and Aldrich (2018).

For the flight of Jewish mathematicians see Siegmund-Schultze (2009a).
(1935) and genetics (1937) led Bartlett (unknowingly) to stochastic processes. Maurice Bartlett was born to the purple of the English school of mathematical statistics for Jack Wishart (1898-1956), his teacher at Cambridge, had worked for Karl Pearson (1858-1936: FRS 1896), founder of the school, and for Ronald Fisher (1890-1962: FRS 1929) his successor as its leading figure. The origins of the school were in biology—the leading journal was called *Biometrika*—and its original focus was on heredity and evolution. Those fields’ practical problems led Fisher to make various contributions to stochastic processes and Pearson studied the “random walk”—his term—in connection with malaria-carrying mosquitoes. Members of the school, beginning with Udny Yule in the 1890s, joined the Royal Statistical Society (RSS) and applied biometric techniques to the ‘state-istic’ topics of population, disease and economics; in the 30s the RSS formed an Industrial and Agricultural Research Section for agricultural experiments and industrial quality control. The newcomers of the 1930s were less involved with biology though Bartlett did some work with/for the geneticist J. B. S. Haldane: Bartlett told Olkin (158)

5 Aldrich (2016) sketches the English school and Aldrich (2010a: §6) its state in the 1930s.

6 When stochastic processes ‘arrived’ Fisher was not impressed, writing in 1954: “I am not myself an enthusiast for stochastic processes, the statistical importance of which seems to be rather limited, though they certainly have their uses. My impression is that they have been elaborated for their own sweet sakes. They have also developed a rather troublesome jargon of their own. I wish some of their enthusiasts would read back into the history of the last one hundred and fifty years so as to show the relationship of what they are doing to that of (say) Laplace.” Some enthusiasts did read back for that relationship: see e.g. Bartlett (1955) and Kendall (1975, 88). Bartlett (1965: 407) comments on Fisher’s “surprising” neglect of the subject.
how Haldane “had some matrix and characteristic-root problems to be solved, and he asked me to help with them.”

In 1938, after spells as a lecturer in statistics in Egon Pearson’s department at University College—Karl’s son had taken it over—and as a statistician at an agricultural research station, Bartlett returned to Cambridge as a lecturer in mathematics, teaching the advanced course in statistics and supervising research students. He was more open to new mathematical ideas and to ideas from abroad than Fisher as his embrace of stochastic processes would show. An early indication came when he reviewed Cramér’s *Random Variables and Probability Distributions*: he (1938: 207) found its approach through measure alien but worthwhile—“from the point of view of mathematical analysis it is a fairly natural and logical one.” Bartlett even envisaged an application for the book’s central limit theory—to the asymptotic properties of Fisher’s maximum likelihood. In July 1939 Bartlett attended a conference in Geneva on “the applications of calculus of probability.” On his return he (1940: 4-5) he told the RSS that the conference “would probably, if held in this country or America, have been called a conference on mathematical statistics.” The British route to Continental probability would be through mathematical statistics, not pure mathematics, physics, or even mathematical biology.

Nineteenth century statistics had been a matter of national traditions mediated by the International Statistical Institute but the rise of mathematical statistics made the subject truly international. By the 1930s English mathematical statistics had a following among

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7 For Bartlett’s review of Cramér see Aldrich (2010: 11-14).
American and French mathematicians, some with a deep interest in probability. The word “following” is critical: it was understood that the canonical works were written in English by Englishmen. Fisher’s French contacts were at the Institut Henri Poincaré, in particular Georges Darmois and Daniel Dugué; they had taken to Fisher’s statistics unlike Fréchet who followed his own line and Paul Lévy a probabilist with no interest in statistics. The IHP had been founded by Émile Borel to promote what Siegmund-Schultze (2009b) calls “stochastic studies” and, measured by footfall, was the world headquarters of probability. By contrast, Soviet probability was isolated: after the Luzin affair of 1936 Soviet mathematicians were discouraged from publishing or travelling abroad and writing in western languages; there were no foreign visitors. Three accessible Soviet works would be central to later developments. Kolmogorov’s Grundbegriffe (1933), the work that gave probability a measure theory basis, was noticed in Britain but seemingly not the key papers on stochastic processes—his (1931) on Markov processes and the associated partial differential equations for the transition probabilities or Khinchin’s (1934) on the basic properties of continuous-time stationary processes—although they appeared in the leading journal Mathematische Annalen.

The most productive connection for English mathematical statistics was with the United States where Harold Hotelling was Fisher’s friend among mathematicians. Hotelling

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8 Establishing rigorously the claims Fisher made for maximum likelihood became something of an international challenge; see Stigler (2007: 609).


drew Joseph Doob (1910-2004) and Abraham Wald (1902-50) into probability and the English brand of mathematical statistics: Fisher himself had no direct connections with Hotelling’s protégés and did not care for their work. Wald stayed in statistics—see Section 3 below—but Doob moved away, becoming the first native international probabilist in the English-speaking world. In 1939 the *Annals of Mathematical Statistics*, founded in 1933, was reconfigured with an editorial team that included Samuel Wilks, Hotelling and Neyman—all of whom had spent time in Britain and whose research reflected British preoccupations. Fisher was on the advisory committee of the ‘new’ journal and he and Bartlett published in it. The new *Annals* was not, however, a copy of *Biometrika* for it had no biological ancestry and mathematics was part of its identity. The separation due to war made the journals more distinct and the *Annals* became a journal of probability theory as much as of statistical theory: in 1941 Feller was the first probabilist to publish in it and by 1945 he was on the editorial board. The pre-war westward movement was not only of ideas: Fisher and Bartlett contemplated moving though in the event Neyman went to Berkeley and William Cochran to Iowa. Scholarships attracted younger Britons to learn, but not about statistics: at Princeton

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11 When Doob started doing international probability (around 1936) the only English language contributions to the field were Wiener’s. For Doob’s career see Snell (1997).

12 For its founding and early history see Stigler (1996).

13 Pitman (1982: 116) recalled having a paper rejected by *Biometrika* for being “too mathematical.” In Britain the more mathematical statistics papers appeared in mathematics journals like the *Proceedings of the Cambridge Philosophical Society*. 
Henry Whitehead learnt topology, Frank Smithies functional analysis and Alan Turing and George Barnard mathematical logic.

While Britain was ‘headquarters’ for mathematical statistics, probability-as-pure-mathematics scarcely existed. It arrived with Cramér’s *Random Variables* and, while, nobody in Britain could have written it, the commission came from the country’s most influential pure mathematician, the analyst G. H. Hardy.\textsuperscript{14} Hardy collaborated with J. E. Littlewood and their partnership had extensions in America—to Wiener at MIT—and Continental Europe—Pólya at the ETH and Cramér in Stockholm; all contributors to international probability. Some Britons did international probability: Harry Pitt (1914-2005: FRS 1957) worked with Wiener and returned with an interest in probability and Cyril Offord (1906-2000: FRS 1952) worked with Littlewood on series with random coefficients.\textsuperscript{15} However, the impact of Hardy-Littlewood on British probability was less through these Cambridge mathematicians than through David Kendall, a product of its Oxford branch

In 1939 David Kendall was an Oxford mathematics graduate about to begin research in astronomy.\textsuperscript{16} Like Bartlett, he wanted to apply mathematics, though a different

\textsuperscript{14} See Cramér (1976: 528). Linfoot and Turing (mentioned in note 4 above) were transients in probability-as-pure mathematics: Turing was a self-taught original while Linfoot, through his teacher, Besicovitch might count as a separated member of the Russian school. Besicovitch ceased probability research when he came to Britain.

\textsuperscript{15} For general background, see Aldrich (2010) and Bingham (2015); for Offord, see Hayman (2005); for Pitt, see Bingham and Hayman (2010: 544-5).

\textsuperscript{16} These paragraphs are based on Bartlett (1982: 42-3), Olkin (1989: passim) and Bingham (1996: 162; 168-9).
mathematics and a different application. There was no statistics or probability and his mathematics was Hardy-Littlewood classical analysis taught in Oxford by Edward Titchmarsh, the professor of pure mathematics, and U. S. Haslam-Jones, Kendall’s tutor.17 Bartlett had studied in Cambridge, the home of Hardy-Littlewood but, given a choice, had taken either traditional applied mathematics—the theoretical physics of Eddington (relativity), Fowler (statistical mechanics) or Dirac (quantum mechanics)—or the new applied mathematics of statistics.

Bartlett and Kendall published as students—Bartlett in the Proceedings of the Cambridge Philosophical Society, the house journal of Cambridge mathematics/mathematical physics and then an important journal for mathematical statistics. His contribution to Wishart and Bartlett (1932) was the use of the characteristic function as a tool in obtaining distributions instead of the geometric method that Wishart learnt from Fisher. Curiously Kendall’s first article used similar means to attack a distributional problem—one of interest to astronomers and treated as a problem in analysis.18 Kendall (1938) appeared in the Zeitschrift für Astrophysik in which Arthur Milne, his professor of applied mathematics, had published. Kendall used the Fourier integral technique he learnt from Titchmarsh who contributed an inequality to the paper. Titchmarsh’s very different

17 For Titchmarsh and Haslam-Jones, see Cartwright (1964) and Titchmarsh (1963). Kendall recalled that Haslam-Jones “started me straight away in my first term reading de la Vallée Poussin’s Intégrale de Lebesgue.” Kendall recalled Haslam-Jones’s opinion of Cramér’s Random Variables: “I can’t say anything about probability but Cramér is all right.”

18 The problem and existing solutions to it are described by Mitchell and Zemansky (1934: 99; 321). For the probabilistic formulation see Nason (2006).
attitude to applications was shaped by Hardy: “I worked on the theory of Fourier integrals under [Hardy’s] guidance for a good many years before I discovered that this theory has applications in applied mathematics, if the solution of certain differential equations can be called applied.”

2 Rules of war and recruitment to the PDE

Bartlett was a university lecturer and Kendall a student when war broke out. New arrangements for the use of scientific manpower in case of war gave them a different war from Titchmarsh or Milne. From school Titchmarsh (born 1899) went to army signals: “if my superior officers supposed that my special abilities would be particularly useful in this sphere of action, they were mistaken.” Milne (born 1896) spent the war in military science, for a while dressed as a naval officer. He was a second year Cambridge undergraduate considered unfit for military service when he joined “Hill’s brigands”—an ad-hoc group working on anti-aircraft guns under the soldier-scientist A. V. Hill. Later in the war, to meet the increasing demand for soldiers, conscription was introduced and some of those previously considered unfit were called up. Research groups could retain personnel by having them put in the forces and then seconded to the group—so to keep Milne, Hill conjured a naval commission for him. The war found Milne’s abilities useful

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19 Quoted by Cartwright (1964: 545; 555).

20 Aldrich (2018) compares the direction of scientific manpower in the two wars.

21 For Titchmarsh’s war see Cartwright (1964: 544).

22 See van der Kloot (2011) and Barrow-Green (2014: 89ff) for Hill’s brigands and McCrea (1950-1) for Milne generally.
and he (1945: 66) got something from it—a “schooling in research” from comrades Hill and Ralph Fowler:

to behold the way they set about a new problem, often on a new terrain and with new material, to see the way they made inferences and came to conclusions and to sound judgement and to take part in it all was a better training than most Universities can offer to aspirants in science.

The training of the Second War provided new careers but Milne, like most First War scientists, resumed his pre-war plans; the exceptions were in the aviation-led sciences of aerodynamics and meteorology.

The rules of 1939 recognised the value of scientific manpower and imposed a selective form of conscription. University lecturers were not called up, though many volunteered for war work. Existing science and medical students were allowed to finish their studies before being allocated war work. When the war began Kendall was about to take up an astronomy scholarship and he was told to wait for assignment. Bartlett joined the Ministry of Supply’s Projectile Development Establishment (PDE): it is not known how he was recruited though more understandably his research student Frank Anscombe (1918-2001) went as his assistant. The projectiles were rockets and research into their

23 Even those who stayed often did war work. Percy Daniell at Sheffield and Pitt at Aberdeen (until 1942) both worked to make Wiener’s work on prediction accessible; for Daniell, see Aldrich (2007: Section 10) and for Pitt, Bingham and Hayman (2010: 544-5) and Barnard and Plackett (1985: 45).

24 Milne, who returned to war science, encouraged Kendall to join the navy but Kendall quickly found he lacked the right stuff as he recalled amusingly to Bingham (1996: 164-5). Bartlett had some involvement with Milne in the war—see MSB2/20.
military use had begun in 1935 under the military scientist Alwyn Crow. The work was well-resourced for it had the support of Winston Churchill, as First Lord of the Admiralty in 1939 and as Prime Minister from 1940.\(^\text{25}\)

Kendall was assigned to the PDE in May 1940: he (1991: 374) recalled the interview in a note on Pat Moran who was being interviewed with other Cambridge graduates.\(^\text{26}\)

The PDE employed career civil servants like Crow and William Crook and temporaries at different stages of their careers: from Professor Louis Rosenhead, to new PhDs like Gerald Whitrow and Noel Slater, to unfinished PhDs like Anscombe, to fresh graduates like Kendall, Moran, and Robert Rankin. There was much coming and going between the different sites and teams of specialists: although Bartlett and Kendall spent the entire war in the same organisation, they only worked together near the end.

3 At war

Until recently little had been published about what Bartlett and Kendall did at the PDE or indeed what the PDE did. Air-defence rockets figure in Churchill’s (1949: ch. 19) history of the “wizard war”, rockets generally were celebrated by Crow (1946) and Rankin (1949) summarised the mathematics of rocket ballistics—as Hill’s brigands, Fowler et al (1921), had done for the weapons of the previous war. Information has since accumulated

\(^{25}\) See Edgerton (2011: 108-112). The rockets were not used in their original anti-aircraft role but were very effective as airborne ground attack weapons—see Price (2009).

\(^{26}\) Kendall (1991: 374) felt intimidated by their knowledge, not only of mathematics. He recalled “an argument about how one should describe the noise of a rocket firing, Moran said wisely, ‘it’s exactly like the tearing of silk underwear.’ This made a great impression.”
as participants have related their experience, relevant documents have been de-classified
and historians have begun examining them.27

Bartlett published little about his time at the PDE—his fullest report is one long
sentence (1982: 46):

A considerable part of my time was spent in assessing the effectiveness of
rocket weapons for various defensive and offensive purposes, and my particular
statistical expertise was very relevant, both in analysing trials and in theoretical
calculations, although all the work was naturally concerned with immediate and
urgent applied research and development.

The report in Barnard and Plackett (1985: 48), which presumably came from Bartlett
and/or Kendall, mentioned “other problems” concerning “the optimal distribution of shell
fragment sizes, and the mean area of projection of shell fragments.” Bartlett’s reticence
may have been because the work was secret, tedious and/or scientifically unproductive—
much of it perhaps all three. Statistical design and analysis—albeit of agricultural trials—
had been one of Bartlett’s pre-war subjects but he did not return to it after the war or
publish anything arising from his war work on experiments.

The younger—and presumably more impressionable and adventurous—Kendall put
more on record about life at the remote site in Wales, whether talking to Bingham or
remembering friends, like Slater and Moran. Kendall was posted to the group of
mathematicians and did different things in his five years: “in the early days, one was
simply doing rather long repetitive calculations.” These were done for Captain Kenneth

27 For participants see Bingham (1996: 161-6), Kendall and Post (1996 and -7), Churchhouse
Post who recalled his “secret weapon”—“a ‘computer’ of great power and versatility […] called ‘David Kendall’ aged 23.”

What Kingman (2009: 1137) calls “the scientific turning point of Kendall’s life” came when he was instructed to become a statistician—one was needed because Bartlett and Anscombe had been moved. Rosenhead, Kendall’s boss, told him:

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.

Anscombe took Kendall through Bartlett’s lecture notes and Kendall returned a statistician. Other mathematicians of that generation had the same metamorphosis but it usually involved taking a university course. Kendall’s course—short, intense and sophisticated—compared favourably with that given to Dennis Lindley (1923-2013) and others at Cambridge as part of the short wartime degree. As to his duties, Kendall told Bingham (165): “One had to know how to do a 2x2 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.” Later in the war—no precise dates are available—Bartlett moved back to Wales and it was then that he and Kendall worked together with Kendall taking Anscombe’s place as Bartlett’s assistant.

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28 Quoted from Bingham (1996: 164) and Kendall and Post (1997: 135). Post’s commendation is the only specific information on Kendall’s mathematical activities in Kendall and Post’s (1996 and -7) otherwise informative articles.

29 Cf. Bingham (165).

30 That training is described in Aldrich (2018).
The accounts on which I have drawn appeared decades after the events and are vague on dates and details. I have found little contemporary personal material but official records of the PDE survive in the National Archives. Before considering these once-classified documents, Bartlett and Kendall’s wartime publications may be noted. From Bartlett came a trickle of papers on pre-war themes up to 1941, then nothing and in 1945 a paper on a new and surprising theme (discussed in the next Section). Kendall published a second piece of Titchmarshian analysis (1942) though this time with no application in view: though the material had no obvious connection with his war work, the article appeared by permission of his employer, the Ministry of Supply.\footnote{The MoS imprimatur appears on Bartlett (1946a and –c) and Bartlett and Kendall (1946), publications which originated as PDE documents. These post-war publications appear in Barnard and Plackett’s (1985: 53-55) list of “publications arising from war work”—some items in the list appeared during the war, like Wald’s Annals papers in America.}

The PDE produced many reports and notes and I have probably missed some relevant ones: the documents were not necessarily signed though sometimes the author can be identified—thus the “Theoretical note on sequential analysis” of November 1944 resembles a first draft of Bartlett (1946a) while “Statistical analysis of variance-heterogeneity by means of logarithmic transformation” from the same month is an ancestor of Bartlett and Kendall (1946). The published version (1946: 137) acknowledges Anscombe “with whom these problems were discussed at the outset of this investigation”—underlining Kendall’s position as Anscombe’s successor. Bartlett worked on transformations before the war, see Bartlett (1936), and the Bartlett-Kendall research
continued in the same vein but sequential analysis was a new topic whose development came out of the war.

A report signed by Kendall dated April 1942 develops a “number of approximate formulas for the chances of damage to aircraft from A.A. rockets” and fits the bill of “elaborate probability calculations” though the expertise displayed belonged to applied mathematics rather than probability. Another Kendall production from April 1942 is more interesting for the present story: an annotated translation of Khinchin (1934) as the “Correlation theory of stationary stochastic processes.” This was the first embrace of Continental probability on the part of Kendall—or of Bartlett. Kendall’s annotations show him as more than somebody who could understand mathematical German though they reflect an interest in analysis rather than in probability; the notes also had some input from Anscombe. For Bartlett the translation was a significant event: he (1978: 149) recalled, “I first became familiar with Khintchine’s important 1934 paper […] from an English translation prepared, if I recall rightly, by D. G. Kendall some time during the war.” It is likely that the translation was done at Bartlett’s behest—see the next section—but the vagueness of the recollection suggests that he and Kendall were not much involved with each other at the time. The translation had no impact on work at the PDE

32 Kendall’s report mentions the parallel work by Egon Pearson—described in Pearson (1963). Bartlett (1981: 5) recalled how Pearson “was concerned with studying the effectiveness of patterns of shell fragmentation against aircraft, and with analysing trials set up to measure such fragmentation patterns. When I became concerned with somewhat analogous investigations for anti-aircraft rockets, I recall some degree of professional discussion with Egon during this period.”
which seems to have found no use for time series analysis and Bartlett’s post-war paper on such analysis (1946b) bore no Ministry imprimatur.

British war science took very seriously Wiener’s work on fire control and prediction though Bartlett appears not to have been aware of it. The other significant American development was sequential analysis, the second keystone in Bartlett’s earliest work on stochastic processes and one that was linked to work at the PDE. Theory and methods for sequential analysis were developed in the United States by Wald and by Barnard and his co-workers in the Ministry of Supply unit, SR17. Bartlett’s “Theoretical note on sequential analysis” was precisely that, linking the technique to probability theory and to Fisher’s inference theory. Wald had mentioned the relationship between cumulative sums and the random walk and Bartlett (1946d: 22) recalled how he “was rather fortunate to read Wald’s original report at about the same time as he was reading a review by Chandrasekhar on random walk theory.” The names Chandrasekhar, Wiener and Wald illustrate how scientific communication with the United States had replaced communication with Continental Europe. However a channel existed through which past Continental work would influence Bartlett.

4 Away from war—Cambridge and Moyal

University life continued on a reduced scale with some colleagues continuing their research and teaching the shortened wartime degree course: in Cambridge statistics, with

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33 See fn. 26 above. Bartlett (1946b) does not mention Wiener’s wartime work.

34 For Wald’s work see Wallis (1980) and for Barnard’s see Barnard (1946) and Aldrich (2018). Bartlett and Wald corresponded in 1944.
Bartlett and Wishart absent, was covered by Oscar Irwin, an evacuee from London. Bartlett and Wishart maintained an intellectual presence, evidenced by a three-way correspondence in 1943 between them and F. P. White, the Secretary of the Faculty Board of Mathematics, on the shape of mathematics and statistics in post-war Cambridge: see Whittle (2002: Section 2) and Section 5 below. Bartlett became more visibly present in September 1944 when he sent the *CPS Proceedings* a paper on negative probabilities in quantum mechanics. The subject and references to Dirac (1942) and Eddington (1943) place the work in Cambridge rather than at the PDE.

“My own acquaintance with negative probabilities first arose in connexion with unpublished work by Mr J. E. Moyal” states Bartlett (1945: 71). Later he (1982: 47) recalled long being “puzzled by the anomalous way that probability had slipped into the new wave mechanics” and how he had met Moyal after learning from Irwin of their common interest in such questions. Moyal had a mission to reform quantum mechanics to which Bartlett contributed acting as intermediary to the Cambridge mathematical physicists and producing ideas—eventually there was a joint publication, Bartlett and Moyal (1949). Moyal’s importance in the present probability story is not, however, because he involved Bartlett in the quantum theory project but because he taught Bartlett about continental probability and stimulated him to work on stochastic processes. For well over a decade Moyal and his unpublished work would be part of Bartlett’s life.

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35 J. O Irwin (1892-1982) was one of the group—with Bartlett, Wishart, Pearson and Maurice Kendall—with a plan, aborted by war, to write an advanced textbook. Wartime teaching in Cambridge is described by Aldrich (2018).

36 Preserved in MSB/2/114.
José Enrique Moyal (1910-1998) was variously an engineer, a physicist and a probabilist. Of Sephardic Jewish origin, Jo or Joe was born in Palestine in the Ottoman era but had a British education after Palestine came under British control. In 1927 he enrolled at Cambridge but withdrew owing to the cost and spent most of the next decade in France. In 1938-40 he studied theoretical physics at the IHP and mathematical statistics at Institut de Statistique of the University of Paris and then worked on fluid mechanics with the meteorologists Georges Dedebant and Philippe Wehrlé. In June 1940 Moyal fled to Britain and made his way to Cambridge, seeing Dirac in December and attending Harold Jeffreys’s lectures on probability. Moyal was sent to work for De Havilland Aircraft and he later went on to the Metalastic Company which used rubber as an engineering material. Anne Moyal (2006: 46) writes that Moyal and Bartlett first met “sometime during 1940-1”—in December 1941 Moyal became a Fellow of the RSS, proposed and seconded by Bartlett and Anscombe “from personal knowledge.”

When Moyal died Bartlett (1999: 273) remembered “his wide knowledge of the European literature on stochastic processes, much of which was unfamiliar to me as well as to many of my English statistical colleagues.” Half-a-century before Bartlett (1946b: 31) announced his epiphany, “This new conception of continuous random or stochastic processes is still unfamiliar to many people; I owe my personal realization of its importance to Mr. J. E. Moyal, and to his own fundamental contributions to its

37 The information in this paragraph comes from the obituaries by Bartlett (1999) and Gani (1998) and the life by Ann Moyal (2006). (The reform of quantum mechanics and Moyal’s attempts to convince Dirac of its necessity are treated in ch. 3-4 & Appendices II-III.)

38 I owe this information to Janet Foster, the RSS archivist.
development (13).” Reference (13) was Moyal’s “Theory of random functions” which was “not yet published.”

The negative probabilities paper betrays no “European” influence and the extent of what Bartlett learnt from Moyal is unclear though it seems to have included Khinchin (1934) on stationary processes—see the previous Section—and Slutsky (1928) on stochastic differentiation. Moyal’s own publications reveal little of what he had to offer. Stochastic differentiation is used in his Paris papers, Dedebant, Moyal and Wehrlé (1940a and -b), which treat fluid mechanics as statistical mechanics, but his JRSS article (1942) has no processes and is a piece of distribution theory. Moyal’s engineering work generated a few articles—e.g. Zdanowich and Moyal (1945)—but these involved no probabilistic analysis. Moyal may well have introduced Bartlett to Kolmogorov’s work but the Markov process paper (1931) had no immediate effect on Bartlett.

Through the war years Moyal kept working on probability and quantum mechanics. He produced a big manuscript that Bartlett tried to get published in the Proceedings of the Royal Society of Edinburgh and in Cambridge. Bartlett (1982: 47) quotes from a letter of June 1944 in which Moyal reported on the state of discussions with Fowler, Dirac and Jeffreys on how best to publish his work:

What [Fowler] finally suggested was that the parts of my paper not dealing with quantum theory should appear in the form of a book. Professor Hardy and the Cambridge Press have now accepted it for publication as a monograph of about 200 pages.

Moyal’s paper/monograph was presumably the “Theory of random functions” or an ancestor of it; such a book on stochastic processes would be have been a companion to
Cramér’s *Random Variables*. In the same letter Moyal encouraged Bartlett to change his mind and collaborate on the book.³⁹ In 1946 Bartlett agreed but as the years passed he decided to go it alone—see Section 9 below. Moyal’s 60 page “Stochastic processes and statistical physics” (1949) is the only indication of what the Cambridge mathematicians saw in 1944 and what Bartlett was still expecting a decade later.

The Moyal channel from Europe to Bartlett and thence to English statistics was important but there were other channels. Another pre-war book from Stockholm brought Europe closer: *A Study in the Analysis of Stationary Time Series* (1938) by Cramér’s student, Herman Wold. It was reviewed enthusiastically in the *JRSS* by Neyman (1939) but he was now in California. The book seems to have made little impression on Bartlett who told Olkin (157) that Wold “must have sent me a copy, but I can’t remember whether I studied it particularly before the war.” Others, however, were studying it—see the next Section.

Kendall lacked the connections to support a rich life outside the PDE: he had been a graduate student when he left Oxford though his 1942 publication would have entailed some communication with Oxford and Titchmarsh. Inside the PDE there was some scope for outside interests: he told Bingham (1996: 169) how his post-war collaboration on group theory with Robert Rankin—see Kendall and Rankin (1947)—originated in wartime conversations.

The PDE existed for war—something that Bartlett and Kendall wanted to put behind them: Bartlett told Olkin (1989:157) how he turned down a post-war position at the Ordnance Board. In their published recollections Bartlett emphasised what he learnt

³⁹ MSB/2/23 for Bartlett’s wartime correspondence with Moyal.
professionally (mainly) outside the PDE, Kendall emphasised the friendships he made inside. Yet the years at the PDE were about war and in a personal communication Nick Bingham tells me, “While the tape-recorder was switched off during my conversation with [Kendall] for *Statistical Science*, he told me how the two of them had sweated blood to have the rocketry ready for D-Day—both the massed salvos fired at the beach defences from landing craft, and the rocket-firing Typhoons that were so deadly against panzers.”

5 Re-starting and starting

The war over, Bartlett returned to Cambridge and statistics but he, the university and the statistical scene had all changed. There was the course on stochastic processes he had proposed to Wishart and White in 1943; it covered time series and the random walk applied to sequential analysis—the subjects of Bartlett (1946a and –b). The students were different: Whittle (2002) relates how Lindley “asked Bartlett to prove consistency of the maximum likelihood estimate in class in 1946.” Lindley, a wartime graduate who worked at SR17, was a statistician with the sensibility of a pure mathematician and a new animal in British statistics. Referring to a somewhat later period, Lindley told Smith (1995: 308) of his ambition “to make statistics a respectable branch of mathematics.” Norman Bailey (1923-2007) who attended Bartlett’s stochastic processes lectures in their second run after his return from America told Whittle (2002), “the evolutionary process component was nothing short of inspiring and had considerable importance for a lot of

The Cambridge Mathematics Faculty responded to the growing importance of statistics by creating a second lectureship—to which Henry Daniels (another of Wishart’s pre-war students) was appointed—and advancing the creation of a Statistical Laboratory. But it did not promote Bartlett and he left in 1947 to be the first Professor of Mathematical Statistics in Manchester’s Department of Mathematics. However the teaching of stochastic processes course had taken root in Cambridge and was part of the new Diploma in Statistics course that started in 1947/8. In the next few years there would be contributions to applied stochastic processes from Cambridge figures like Bailey, Lindley, David Cox and Walter Smith.

The national statistical scene was also changing. The more technical work had once appeared under the auspices of the RSS’s Industrial and Agricultural Research Section but in 1945 “the Research Section (without any taint of application) was formed” recorded Tippett (1972: 550). The new Section—like its journal, reconstituted in 1946 as *JRSSB*—took papers that would once have been deemed too mathematical. The mathematical level of *Biometrika* rose too so that the *Proceedings of the CPS* was not needed for the more mathematical statistical papers: it would be a second home after *JRSSB* for stochastic processes. In the United States there were more statisticians with

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41 Aldrich (2018: Sections 6-7) describes the experience of the Lindley-Bailey generation.

42 Bartlett also applied for the new Readership in Statistics at Oxford; see MSB2/1.

43 See Cambridge University Reporter and Whittle (2005). Stochastic processes were in the air: they have a section of their own in the 1946 issue of the RSS bibliography “Recent Advances in Mathematical Statistics” compiled by Florence Rigg, late of SR17.

44 For the Laboratory in these years see Whittle (2002: Section 3).
backgrounds in pure mathematics and the mathematical level of the *Annals of Mathematical Statistics* rose too—its papers looked very different from those in *JRSSB*, *Biometrika* or the *PCPS*.

The raised mathematical level of the RSS was evident in the Section’s symposium on autocorrelation in time series held in January 1946. The papers presented were by Bartlett, the Air Force scientists, Cunningham and Hynd, and the textile statistician G. A. Foster. Foster’s applications paper fitted the old Industrial and Agricultural mould as did Cunningham and Hynd’s but the latter paper, like Bartlett’s was on the new mathematical level.

Bartlett (1946b: 27) reported results on the sampling errors of correlations but a wider purpose was to “link up the work of the ‘English school’” with “the important mathematical work developed of recent years on the auto-correlation theory of continuous time-series.” Bartlett’s (31ff.) summary of that theory emphasised the autocorrelation function for a continuous time stochastic process and, while declaring that “the general theory mainly originated with the ‘Russian school’ of Khintchine, Kolmogoroff, Slutsky and others,” he singled out Khinchin’s contribution to understanding its structure.

The symposium spoke to the RSS’s tradition in time series, especially economic and medical. The English school had moved on since Bartlett last contributed to its literature in 1935 and its current state was represented by the work of Maurice George Kendall (1907-83), one of the symposium discussants. MGK, no relation to David, was then Statistician to the British Chamber of Shipping. “In wartime this must have been a taxing job,” wrote Stuart (1984: 121), and yet MGK managed to produce the first volume of the
Advanced Theory and to embark on a study of time series.\textsuperscript{45} That interest came out of his pre-war work at the Ministry of Agriculture and his collaboration with Yule on revising the \textit{Introduction to the Theory of Statistics}—as Yule and Kendall (1937). His first paper “The effect of the elimination of trend on oscillation in time-series” (1941) drew on the pre-war American literature on business cycles with an injection of ideas from Yule (1921).\textsuperscript{46} Kendall subsequently (1943 -44 and 45) focussed on the theory and applications to agriculture of the autoregressive scheme from Yule’s (1927) study of sun-spots, as elaborated in Wold’s \textit{Stationary Time Series}. Kendall had none of Bartlett’s evangelism for a larger movement behind time series analysis.

David Kendall had no position to return to—no job, like Bartlett, or degree to finish, like Lindley or Bailey. He applied for jobs in Oxford and “got the job” of tutorial fellow in mathematics at Magdalen College on the strength of his war-time reports, he told Bingham (1996: 168).\textsuperscript{47} At that moment the Titchmarshian analyst and member of the

\textsuperscript{45} Mills (2012: ch. 2 & 3) reviews Kendall’s work in time series in relation to Yule’s.

\textsuperscript{46} See Morgan (1990: chapter 2).

\textsuperscript{47} Testimonials must also have helped. In the Bartlett papers (MSB/2/11) is an undated and unaddressed document headed Confidential: “I have known Mr. D. G. Kendall since we both joined the research staff of P.D.E. (Ministry of Supply) in the beginning of 1940. For the last year or so he has been my chief assistant in the statistical section at P.D.E. in Wales; and this closer contact has strengthened my opinion of Kendall’s marked ability and promise in the field of mathematics and mathematical statistics. I have been very much impressed not only by his (secret) official work […] but also by his keen interest in general mathematical and physical research. When I returned to Cambridge in March of this year Kendall was left in charge of the
London Mathematical Society (LMS) looked more like a mathematical statistician: he had joined the RSS in 1944—proposed and seconded by Bartlett and Anscombe—and was working on Bartlett’s problems: besides a joint paper, Bartlett and Kendall (1946), there was a short piece, Kendall (1946) on Fisher’s “Problem of the Nile” a follow-up to Bartlett’s 1940 paper on quasi-sufficiency. Kendall started in Oxford in December 1945 and in February was telling Bartlett how the teaching was demanding and there was “practically no time” for research. Nevertheless he had been working on problems in geometric probability, spurious periodicity in time series and statistical theory—“I have a hankering to compile my own synthesis of existing work on Statistical Estimation.” The statistical moment passed and only one subsequent paper in our period—Kendall (1948b)—has statistical matter.

Moyal began an academic career as a lecturer in the Department of Mathematical Physics at Queen’s University Belfast before joining Bartlett in Manchester in 1948. For several years Bartlett, Moyal and Kendall were an invisible college, in touch, as acknowledgements in papers reveal, but seldom appearing or publishing together: there were a few joint papers—Bartlett and Moyal (1949), Bartlett and Kendall (1951) and Kendall and Moyal (1957)—and the three contributed to the 1949 symposium on stochastic processes that Bartlett organised. Ten years of seniority made Bartlett the natural leader and most of their work was written for his journals, JRSSB, Biometrika and the Annals of Mathematical Statistics.

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statistical section, but I hope he will soon (e.g. through a research fellowship) have the opportunity of fulfilling his high promise in the research sphere at Oxford.”
6 To America with stochastic processes

In 1946 Bartlett spent the fall quarter at the University of North Carolina at Chapel Hill where Hotelling was creating a statistics department. There was something old, something new about this visit to America. The old was a job offer to a British statistician and the new was the emergence of the United States as a centre for probability and stochastic processes. In the US Bartlett saw Cramér, Feller, Kac and Arley; Cramér and Arley were visiting but Feller and Kac had settled.

The printed version of Bartlett’s course on stochastic processes (1947a) has a prefatory note dated January 1947 and errata list dated October. The standpoint was statistical: the lectures were directed to “statistical students” who were asked to think of the subject as “the statistics of ‘change’” in contrast to “the ‘static’ distributional problems with which we have hitherto been mainly preoccupied.” Following the General Introduction are two substantive parts: Part II (pp. 13-56) on “Evolutionary processes of the Markoff chain type and their application to population growth, industrial renewal theory, and allied problems;” Part III (pp. 57-93) on “Stationary Processes and their bearing on the analysis of time-series.” Sequential analysis and random walks were not covered as these were treated in other courses.

Stationary processes had been in Bartlett’s sights since 1942 but “evolutionary processes” was a new topic seemingly unconnected to any war work. His treatment was based on the treatise by Fréchet (1937-8) and on Annals papers by Lotka and Feller: Fréchet’s book was in Moyal’s domain but the other items were not. One of the Markov chain examples (20-22) was from Bartlett’s past—a genetic scheme he had worked on with Haldane. Part II was largely a survey but there was a novelty in the treatment of
Markoff chains with continuous time (sections 11-13, pp. 43-55) where it was found useful “to make frequent use of operational methods in conjunction with the probability and moment-generating functions.” Later Bartlett (1982: 55-6) brought out the connection between this work and a 1937 genetics paper involving what he now called a discrete time stochastic process. The finale, section 13 (54-6), developed the thought, “It is evident that the operational method used in the preceding sections may be generalised.” We look at this work in the next Section.

Kendall had an interest in time series but had not shared in Bartlett’s wartime awakening to stochastic processes through random walks cum sequential analysis. He told Bingham (1996: 168-9) that “the real push came later” again from a set of Bartlett notes: “my work on birth and death processes emerged from my reading of them.” Writing to Bartlett in March 1947 he mentioned a statistics paper that needed revision: “but am now so pleasantly involved in processes of one kind or another that it will be very hard to pitchfork myself back into those ways of thought.” By 1946-7 the mould was set—for Bartlett certainly and for Kendall seemingly. In the following 5 years Kendall made excursions into group theory and central limit theory but his main effort was on stochastic processes, particularly birth and death processes. Bartlett meanwhile juggled processes with old interests in multivariate analysis, including factor analysis, and in statistical inference. The latter intersected with stochastic processes in the form of inference for Markov chains and for time series: see e.g. (1950a and b)—material later incorporated in the last chapters of the Introduction. Such work would be Bartlett’s special preserve but the following sections will put this aside to concentrate on what
would make the turning points in our story of respectability—the birth and death process and the general theory of continuous time discrete state space Markov processes.

7 A new ordinary—Feller

“I was interested by then [1946] in population processes starting from Feller’s work” Bartlett recalled in 1987—Olkin (1989: 161). William Feller (1906-1970) was the second outsider—after Moyal—to affect the course of British efforts. He also contributed to Kendall’s emergence as a pure probabilist. Speaking for himself and his collaborator Harry Reuter, Kendall (1988: 3) attested “how much we both owe to Will Feller for his generous encouragement and appreciation.” Bartlett (1983) and Kendall (1990) recognised Kolmogorov’s greatness but it was admiration from afar.

In 1987 Feller’s Introduction to Probability Theory and Its Applications had been so prominent for so long that starting from Feller must have seemed unremarkably ordinary. Yet Feller hailed from deepest Europe where he worked on the central limit theorem and helped in the preparation of Cramér’s book. In Stockholm, however, Feller developed another side. Cramér was professor of actuarial science and probabilists there, including Feller, involved themselves with applications: thus in his book on sickness and accidents Lundberg told that Feller had “animated my interest in this work.” Such involvement, which the Introduction emphasised, rested on a determination that theory inform the treatment of practical problems. In America Feller’s first articles in the Annals of Mathematical Statistics were on applications though he continued to work on the theory of Markov processes: a 1940 paper in the Transactions of the American Mathematical Society.

For overviews of Feller’s career see Rosenblatt (2007) and Schilling and Woyczynski (2015).
Society followed his 1936 paper on existence and uniqueness of Markov processes which in turn built on Kolmogorov’s work. This strand was not picked up in Britain until the 1950s—see Section 8 below.

Among the references in Bartlett’s notes (1947a: 93-5) are two papers by Feller—“On the integral equation of renewal theory” (1941) and “Die Grundlagen der Volterraschen Theorie des Kampfes ums Dasein in wahrscheinlichkeitstheoretischer Behandlung” (1939). The latter, Feller’s last paper in German, was what Bartlett started from—at least in the sense that the birth and death process originated there.\textsuperscript{49} Kendall (1947a: 134) later described it as “a most important and (in this country) little known paper” and I have not been able to discover how Bartlett got onto it.\textsuperscript{50} It appeared in a Dutch biology journal but it was not invisible: it was mentioned in Feller’s 1940 theory paper, listed in Hartley’s “Recent advances in mathematical statistics” (1940: 537) and abstracted—not too fully—for Mathematical Reviews by Doob.\textsuperscript{51} It may have been known to Moyal. It is likelier that Bartlett did not start from Feller’s article but from something at one remove: Arley’s On the Theory of Stochastic Processes and their Application to the Theory of Cosmic Radiation (1943) was the key reference for Bartlett’s discussion of population problems and it referred to Feller’s paper.

\textsuperscript{49} Baake and Walkobinger (2015: Section 2) review the paper’s contents and place among Feller’s biological works.

\textsuperscript{50} Kendall (1948: 14n) was “greatly indebted to Prof. Feller for the gift of a reprint of this paper.”

\textsuperscript{51} MR was part of the post-war ordinary. Its first executive editor was Feller and its summaries served the probability community well. Today it shows historians how new work was received.
Bartlett and Kendall soon had papers for the *Annals*: Bartlett wrote up his method (at Moyal’s suggestion) and Kendall treated a generalised birth and death process. The papers were linked: Kendall (1948: 1) explained how his method “starts from M. S. Bartlett’s idea of replacing the differential-difference equations for the distribution of the population size by a partial differential equation for its generating function.” In August 1947 Bartlett’s paper was rejected because the referee judged it a “large mass of undigested & indigestible material quite unsuitable for publication” with “no new results”—the main result had been published by Palm. Bartlett was angry and felt badly treated. He told Kendall (whose submission would be accepted):

> It is possible that Feller is the referee as the letter also refers to the solution being given by Feller in a lecture in January 1946, and if so maybe (with perhaps some justification) he feels I did not take sufficient trouble to check up on his and Arley’s recent work; but why on earth he or Arley did not tell me this when I raised the subject at Princeton I don’t know.

It seems that Bartlett’s personal experience with probabilists in the United States was not productive: Kendall fared better—see Section 9 below.

Bartlett’s notes were not published—or covered by *MR*—but they became part of the literature, especially in Britain; his first regular publication (on evolutionary processes) waited until 1949. Kendall published more, starting with a review essay (1947a) of two Stockholm books, Lundberg (1940) and Arley (1943). Kendall “warmly recommended”

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52 The letters are in MSB/2/27. Bartlett (1982: 48; 56-8) suggests that the material is “perhaps of some historical interest.” Whittle (2004: 20) alluded to the incident when he criticised “referees, who demanded rigour when insight was the issue.”
them: besides summarising them, he gave an overview of Markov processes, a survey of birth and death processes and their applications to physics and actuarial science and a preview of his own *Annals* paper; he mentions the theoretical contributions of Kolmogorov (1931) and Feller (1936, -40) but does not examine them in detail. In “On some modes of population growth leading to R. A. Fisher’s logarithmic series distribution” Kendall (1948b) synthesised a number of biological contributions.

In 1949 the RSS’s Research Section held a symposium on stochastic processes, organised by Bartlett with Moyal speaking on “stochastic processes and statistical physics,” Kendall on “stochastic models of population growth” and Bartlett on “some evolutionary stochastic processes.” Doob summarised them in *Mathematical Reviews*: 53

Expository papers, with valuable bibliographies and historical remarks. The mathematics is mostly formal in nature, and detailed proofs are omitted. The authors derive the results from their own points of view, frequently giving new insight into the problems considered. Of particular interest, for example, is the systematic operational method used by Moyal and Bartlett of deriving differential equations for probability distributions in stochastic processes (Fokker-Planck equations and others). Moyal’s paper stresses detailed applications to statistical physics. Bartlett’s stresses random walks and multiplicative (birth) processes from a general point of view. Kendall is interested in the details of birth and death processes from the point of view of constructing models for various applications. He obtains new results on the

53 See Whittle (1951) for another contemporary view.
stochastic fluctuations of the age distributions of a population, assuming a continuous time parameter.

The praise of the “systematic operational method” is perhaps surprising. Bartlett’s section on the method, headed *A general differential equation*, has two historical footnotes (220): one acknowledged Palm’s priority, “My own use of this method […] was developed at first in ignorance of Palm’s work, to which I regret that no reference was made,” while the other staked a claim of his own, “The wide applicability of this equation, to discrete or continuous $X$, especially if formulated in terms of the characteristic function, was noted in 1946 in a letter to J. E. Moyal.”

The RSS symposium brought together generations and interests and marked the arrival of stochastic processes in the statistical community. Irwin reckoned it a “great honour” to be presiding and shared reminiscences of past figures and of Bartlett and Moyal in earlier days. The contributions of the old Pearsonians—Greenwood, Soper and Yule—were recognised: Kendall (1949: 242) referred to Greenwood and Yule (1920) on accidents and Bartlett (1949: 225-7) discussed the deterministic modelling of epidemics in Soper (1929). Major Greenwood (1880-1949: FRS 1928), senior epidemiologist, past President of the Society, Yule’s co-author and Soper’s one-time colleague, declared (1949: 271), “It has been a pleasure to be present at what, I think, is an epoch-making occasion, the revelation of a new organon.”

Among the discussants were wartime entrants from mathematics—Barnard, Lindley, Quenouille, Good and Armitage. Lindley, who was introducing measure-based probability to the Cambridge curriculum, complained of lack

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54 For Greenwood and Soper see Aldrich (2010: Section 4).

55 See Aldrich (2018) for the wartime generation.
of rigor, “I am still not clear, however, what a random function is, and doubt whether any completely satisfactory definition is available.”

In the Introduction I described 1940-55 as a period of setting up. In fact, there were two set-ups and two sub-periods: by 1950 Bartlett’s wartime vision of bringing to statisticians a dynamic probability theory for understanding the statistics of change was largely realised; Kendall was involved in this development but in 1950-55 he moved onto a new project.

8 Pure mathematics and applied mathematics

The symposium celebrated an established collaboration but some of Kendall’s (266-7) remarks suggest he was restless and on the look-out for a pure-mathematical approach:

[I] found particularly interesting the account which [Moyal] gives of the pure-mathematical approach to stochastic process theory. In my own work on population processes I have adopted quite deliberately the point of view of the applied mathematician, because the whole object of these preliminary skirmishings has been a rapid exploration of the territory.

Looking beyond the exploratory stage, there was an equation in Bartlett’s paper which excited me very much […] because it suggested the word group, which has such a high emotional content for contemporary mathematicians. Actually it is not in general a group but a semigroup which is relevant. […] one may expect a semi-group to be associated with every stochastic process of the Markoff type. This is so, and some of the details have been worked out in Hille’s recent book on Functional Analysis and Semi-Groups.
Kendall was a mathematician addressing statisticians but the message would have challenged a mathematical audience for the functional analysis techniques of the American analyst Einar Hille were new to Britain, as was the literature on Markov processes by Kolmogorov, Doob and Feller that Hille (1948: ch. 21.3ff.) worked from.\footnote{Hille’s work on semigroups is reviewed by Yosida (1981): semigroups were as much a Japanese as an American speciality.}

There was no immediate change in Kendall’s research but he kept such work in view, as is evident from a paper (1951) by his research student Gordon Foster (1921-2010) on discrete-time enumerable state space Markov processes which mentioned Yosida and Kakutani’s (1941) operator-theoretic treatment of Markov processes and papers by Doob. Kendall awaited further stimulus, re-tooling and a collaborator but first a new process had caught his attention.

“The Theory of Queues has a special appeal for the mathematician interested in stochastic processes because it provides a simple example which is both (i) stationary and (ii) (in general) \textit{not} Markovian,” so Kendall (1951: 151) informed the Research Section. The theory also had numerous applications and Kendall (171) recalled “a very stimulating three-cornered correspondence [with former PDE workers Anscombe and Jack Howlett] which first started my interest in these problems some four years ago.” In the discussion (180-1) Howlett described how he had been interested in the arrival of taxis at Euston railway station and Kendall told Bingham (170-1) how his and Anscombe’s interest originated in the scheduling of flights during the Berlin airlift of 1947. As he had done with the birth-and-death process Kendall immersed himself in the
literature—e.g. with a review (1951) of Erlang’s work—and a paper (1953) followed for the *Annals*.

In Manchester stochastic processes flourished—Cox (1988: xxiii) recalls making the “arduous weekly trek” from Leeds for the Bartlett-Moyal seminar. Two local pure mathematicians became involved: an algebraist, Walter Ledermann (1911-2009), and an analyst, Harry Reuter (1921-92). Both had come from Germany, Ledermann in 1934 as a formed Berlin algebraist and Reuter in 1935 as a schoolboy. In the war Ledermann taught airmen navigation, while Reuter served in the Royal Naval Scientific Service after graduating from Cambridge in 1941. None of the sources describes Reuter’s wartime duties and there is no stamp of war in his early published work—on integral transforms and differential equations. Reuter and Ledermann did not only do pure mathematics: Ledermann, who had once given matrix algebra support to the psychologist Godfrey Thompson, contributed an appendix to Bartlett (1951) on factor analysis; Reuter collaborated with the theoretical physicist Ernst Sondheimer who, having derived an integro-differential equation, found that “my mathematics was not up to solving [it].”

The first paper from Manchester’s pure mathematicians was Ledermann’s “On the asymptotic probability distribution for certain Markoff processes.” It thanks (1950: 581) Bartlett for “drawing his attention to the problem” and Bartlett, Moyal and Kendall for advice and “many valuable suggestions.” Ledermann treated a continuous time process with a finite number of states which was, he (1950: 582) explained, “only a very special

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57 Ledermann and Reuter arrived in Manchester in 1946. For a time I. J. Good (1916-2009) was a colleague. For Ledermann, see Gaines and Laffey (1985) and for Reuter, Kendall, Bingham and Sondheimer (1995).
instance of a stochastic process, and it is to be expected that some of my conclusions follow from the general and far-reaching results that various writers have obtained in this important branch of probability theory.” His probability references were Fréchet and Doeblin; the other references were for algebra. Fréchet was within the orbit of Bartlett’s notes but the particular set-up had not been treated.

In 1952 Ledermann and Reuter submitted papers on stochastic processes to the *CPS Proceedings* and the *Philosophical Transactions*. In the latter, “Spectral theory for the differential equations of simple birth and death processes,” Ledermann and Reuter (1954: 367) recorded a debt to Bartlett “who not only first stimulated our interest in this problem but has always been ready to give his valuable advice and suggestions throughout the progress of this work.” The main reference, however, was Feller’s new book and its chapter 17 on “the simplest time-dependent stochastic processes.”

The *CPSP* paper Reuter and Ledermann (1953) “On the differential equations for the transition probabilities of Markov processes with enumerably many states” had no personal acknowledgements though it was communicated by Bartlett. The paper referred to the Symposium papers and its final section was on birth-and-death processes but the main focus was on the “well-known” Kolmogorov equations. These had not been studied in Britain and their fame was likely due to Feller’s *Introduction*. Reuter and Ledermann’s starting point was not Kolmogorov (1931) but Feller (1940). They (1953: 248) explained the relationship between his work and theirs:

> It is [...] to be expected that less powerful methods might suffice to treat the special case of enumerable systems, and we have in fact found that a rather
elementary derivation of Feller’s results can be given if full use is made of simple facts about finite systems of linear differential equations.

British pure mathematicians were treating the same issues as foreign ones but their work, as yet, was derivative.

9 America again and international probability

Kendall spent the 1952/3 academic year in Princeton, an experience as formative as the wartime meeting with Moyal was for Bartlett. Feller and Wilks, a past-editor of the *Annals*, invited him. The *Annals* still took Bartlett’s articles on statistical topics but after the 1947 debacle his articles on stochastic processes appeared elsewhere. The journal had taken to Kendall’s work: it was publishing a second paper—on queues (1953)—and Foster also published there. Kendall had a good relationship with Feller even before they met. The “most important” 1939 paper had inspired a string of pieces, most of them reviewed by Feller for *MR*, and the two were in contact: Kendall’s 1948 *Annals* paper was mentioned in Feller’s *Introduction* (1950: 374, fn.) but so too was his “forthcoming” *Series B* paper. Kendall reviewed Feller’s book for the *JRSS*, welcoming (1951: 229) it as “an extremely important book, which will exercise a profound influence on the development of the subject.”

As a visitor, Kendall had less standing than Bartlett when he went to North Carolina but more than the graduate students who went to Princeton before the war. Like the latter,

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58 For this time at Princeton see Bingham (171-4), Kendall, Bingham and Sondheimer (1995:178) and Kingman (129). Kendall (1982: 385) remembers being cast as a “small-sample expert”—“That’s all right”, said Sam Wilks, “stochastic process theory is about samples of size one.”
he went to learn and he found, besides ideas, a community—including Feller, Doob, Kac
and Kai Lai Chung—without counterpart in Britain. His year saw the publication of
Doob’s *Stochastic Processes* (1953). A book (v) that “made no attempt to sugar-coat” the
fact that “probability is simply a branch of measure theory” predictably divided British
statisticians. Bartlett’s contemporary F. N. David (1954: 285) explained that the book had
nothing for the statistician but conceded, “it is well that there are probabilists such as
Prof. Doob stressing the formal mathematical arguments in what is, after all, the root and
branch of our work.” Kendall’s contemporary Lindley (1953: 456) wrote a long, studied,
positive review: “Here then, is a book which every person interested in stochastic
processes and with the necessary mathematical ability will find indispensable.” *MR*
hailed it as a “valuable book” which “cannot fail to exercise a decisive influence on the
development of its subject.” The assessment was unsurprising but it came from Kendall,
hitherto a reviewer of applied works for *MR*. The review registered a change in what he
wanted to do in probability and what the probability community thought him capable of.

While Lindley read in Doob’s book, Kendall read around it and extended it. He noticed
how Doob had only registered “some topics receiving much attention at the moment
(semi-group methods, Lévy’s analysis of the possible pathologies, and Feller’s necessary
and sufficient condition for the occurrence of an infinity of transitions in a finite time).”
Kendall also remarked how, “Just after the book went to press, the author’s theorems on
the limiting behaviour of the transition-functions near $t = 0$ were sharpened by A. N.
Kolmogorov.” Kolmogorov’s note, which also introduced two pathological examples, was reviewed for *MR* by Doob though Chung brought it to Kendall’s attention.\(^{59}\)

Kendall’s first post-Doob piece was submitted to the *TAMS* in May 1953: “Some analytical properties of continuous stationary Markov transition functions.” The project and terms of reference (1955: 529) were unlike anything in his past work:

A systematic treatment of Markov processes with Euclidean state-spaces has recently been presented by Doob [1953], the restriction on nature of the state-space being associated with the very illuminating probabilistic method which he uses throughout. At about the same time a new step was taken by Kolmogorov [1951] who established for countable state-spaces the existence and finiteness of the derivative of the transition-function \(p_{ij}(t)\) at \(t = 0^+\) when \(i \neq j\). In this paper some of Doob’s and Kolmogorov’s results are combined and shown to be valid (when suitably formulated) for an arbitrary state-space.

Kendall (540) thanked Doob for “a number of helpful suggestions.”

In Princeton Kendall picked up the semigroup research he had contemplated in 1949 though getting into it would take longer than the week learning statistics in war-time London. Kendall had notes of Feller’s lectures on semigroups from the year before and, from them and using Hille’s book, he “learned semigroups—learned functional analysis indeed.” Kendall was also following the work in Manchester of Reuter and Ledermann. Ledermann wrote to him about their results and when Kendall showed the letter to Feller he “was aghast, and explained to me that he had himself tried the same idea and worked

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\(^{59}\) As Kendall recounted on several occasions: see Kendall, Bingham and Sondheimer (1995: 178) and Bingham (1996: 173).
very hard at it, but had abandoned the project when he became convinced that it simply would not work.”

On his return to Britain in September 1953 Kendall met Reuter for the first time and they would produce a number of joint and solo pieces founded on the belief that “the formulation in terms of semigroups should make possible a completely satisfactory theory of the most general Markov process” (Kendall and Reuter (1956: 381). Where once Haldane contracted out problems to Bartlett or Sondheimer to Reuter, Kendall and Reuter worked together. Their backgrounds were different: Reuter had more experience with functional analysis going back to the teaching of Smithies in post-war Cambridge but Kendall had more probability experience and had been preparing for several years. They took the task of learning the new subject very seriously. Kingman (129) recalls Kendall’s lecturing on a subject as a way of learning it—so he put on a short course on functional analysis. Kendall told Bingham (175), “I suppose we spent about half our time during those early years searching out papers on functional analysis that seemed fundamental and [...] really digesting them thoroughly one after another, until we couldn’t find anything more.” Kendall also recalled to Bingham (173) their working though the review by Phillips (1955) of developments since Hille (1948): the review “just consisted of assertions” and “one of the tasks Harry and I set ourselves was to go through the assertions and prove the lot.”

Information on the Kendall-Reuter collaboration is from Bingham (171-4), Kendall, Bingham and Sondheimer (1995: 178-9) and Kingman (130).

They did not just do this for the exercise—they (1954: 387) wanted to use the assertions which would go into the second edition of Hille (1948)—Hille and Phillips (1957).
Kendall and Reuter had a paper ready for the International Congress of Mathematicians held in Amsterdam in September 1954: it (1956: 382) had an acknowledgment, “Feller’s work on denumerable Markov processes is as yet unpublished, but the authors of this paper are particularly indebted to it.” Although the Cold War was still very cold, this was the first ICM to be attended by Soviet mathematicians since 1932 and Kolmogorov was present. The “pathological processes” of Kendall and Reuter’s “Some pathological Markov processes with a denumerable infinity of states and the associated semigroups of operators on $l$” were Kolmogorov’s two examples which they (1956: 382) dealt with as follows:

We propose to “re-manufacture” his examples via the theory of semigroups, and we hope that the detailed analytical study from this point of view will be of value in guiding the general theory towards completeness by showing how such pathologies in behaviour [...] appear when transferred to the semigroup formulation.

The paper also incorporated the possible pathologies described by Lévy.

The Amsterdam paper put its authors in the forefront of probability research and confirmed them as members of the international probability community. I now turn to Bartlett whose work I have been neglecting he had been consolidating rather than changing—his change came in the decade before, at around the age Kendall was now. The great expression of consolidation, the Introduction to Stochastic Processes: with Special Reference to Methods and Applications (1955), was about five times the size of the 1946 notes, an expansion reflecting both the growth of the literature and of Bartlett’s

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62 Kendall (1990: 34) fondly recalls the conference.
knowledge of it. Yet its scope was limited for Bartlett (xi) envisaged a companion Moyal volume on the “basic mathematical theory” and possibly another on “stochastic processes in physics.” The theory volume was real enough for references to it to appear in Bartlett’s text, as “M”. Bartlett had no wish to be M himself and his book was quite unlike Doob’s: Doob’s (1953) definition of a stochastic process (on p. 46) is supported by a chapter on probability and a supplement on measure theory while Bartlett was treating his first process, the random walk, by p. 15.

Bartlett (xiii) made “grateful acknowledgments” to Kendall and Moyal “with whose work in recent years I am fortunate enough to have been in close contact” but Kendall’s newest, post-Princeton, work did not fit into a book written for the “applied mathematician and statistician” for whom the theory of stochastic processes “may be regarded as the ‘dynamic’ part of statistical theory, with a multiplicity of applications.” For Bartlett statistical theory encompassed the probability model, its properties and the relevant statistical inference procedures: accordingly, the book opens with the theory of the main models and closes with the associated inference procedures after an interlude on prediction and communication theory; see the MR review for a fuller synopsis.

For “methods and applications” Bartlett looked to British authors: from the past, Fisher, Yule, McKendrick, etc., from the present, Bailey, Lindley, Quenouille, etc. He also collected his own and Kendall’s contributions. Bartlett (1955: xiii) recognised the “important theoretical contributions” of “American, French, Russian and Swedish writers” and singled out Kolmogorov. The 1946 notes had not treated Kolmogorov’s forward and backward equations but they were now central.
The *Introduction* was to the taste of British statisticians: insider Bailey (1955: 485) had “no doubt that [it] is a substantial contribution to the development of the subject” while spectator David (1955: 539) thought it would remedy a situation in which “many statisticians” still solve problems in an ad hoc fashion despite “a great deal of elegant mathematics” having been developed. In *MR* Theodore Harris gave it a qualified welcome but the reviews of the *Annals* statisticians, Kiefer (1956), Savage (1956) and Wolfowitz (1956) were less welcoming, even hostile. After observing the book’s faults Wolfowitz (124) was led to reflect on two mathematical cultures:

The fact that the reviewer was often familiar with the subject matter often did not seem to help much; even some definitions were difficult to understand. Several distinguished probabilists have reported the same experience to the reviewer. It seems reasonable to conclude that this is not a book mathematicians can profitably use to obtain an understanding of the theory of stochastic processes. Perhaps applied mathematicians and statisticians, to whom the book is addressed, will think differently. The reviewer has often noted divergences of opinion between the two groups as to what is clear and easy to understand. 63

Bartlett was invited to visit the Soviet Union, an invitation he put down to his book which was being translated. Being different was, presumably, his appeal to the Soviets.

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63 Cox (1988: xxiii) defended Bartlett’s way of addressing matters “in a very direct and powerful way without a miasma of regularity conditions or formulations of distracting generality[…] For some readers the papers would no doubt have been easier to follow if written in a different mathematical style, but it seems to me that their difficulty stems primarily from the high density of ideas rather than from any lack of mathematical precision.”
The publication of the *Introduction* and of the first papers by Kendall and Reuter ends the present story but I will mention some later developments bearing on its theme: the references in note 2 above consider Kendall and Bartlett’s later careers. Kendall found platforms from which to project, what was in Britain, a new take on probability. In 1962 he left Oxford for Cambridge to become the first Professor of Statistics and Head of the Statistics Laboratory, positions Bartlett might have once seemed natural for. Jobs were created and students flowed. Kendall was active in the LMS and in 1961 he and Reuter formed the Stochastic Analysis Group with the aims of making a home for probabilists and creating more effective contact between the LMS and the RSS. In 1963 Kendall was President of the LMS and he involved the Society in establishing the *Journal of Applied Probability*; see Gani & Spier (1965). These initiatives plus his publications show how Kendall promoted the new take, supported the old and worked to bring the two together.

Modern probability entered the British undergraduate syllabus in the 1960s: textbooks by Pitt (1963) and Kingman and Taylor (1966) treated probability with measure and integration. Stochastic processes à la Bartlett were presented by Bailey (1964) and Cox and Miller (1965); work in similar vein appeared in the series of Methuen’s monographs on applied probability and statistics of which Bartlett was the general editor. Stochastic processes à la Kendall-Reuter waited until the 80s to be presented in undergraduate textbooks, see e.g. Grimmett and Stirzaker (1982).

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64 The Manchester personnel also dispersed: Moyal to ANU in 1957, Reuter to Durham in ‘59, Bartlett to University College in ‘60 (succeeding Egon Pearson) and Ledermann to Sussex in ‘62.
10 Global conflict, respectability and two cultures

I began with Kingman’s (2009: 125) remark, “probability played no part in a respectable mathematics course and it took a global conflict to change both British mathematics and D. G. Kendall.” I end by reflecting on how the global conflict effected the change and on how the change played in two mathematical cultures.

The global conflict put established British statisticians into war work and pressed unformed mathematicians into statistics. So far, so ordinary—see Aldrich (2018)—but Bartlett was the only established statistician to re-make himself as a specialist in stochastic processes and Kendall the only impressed statistician to become a pure probabilist. Behind these unique outcomes were the circumstances and personal relations detailed above. The central relation was occasioned by war and originated with Bartlett, in effect, turning Kendall into a statistician. Kendall, however, did not make statistics his career but instead followed his mentor into applied stochastic processes before adding pure probability to his portfolio. The other relationship occasioned by war—between Bartlett and Moyal—was more a distraction from, than part of, the business of war. The global conflict produced these life-changing encounters but it also changed the international distribution of intellectual power so that the post-war ordinary was US centred and modern probability entered Britain with an American not a French accent.

Kingman’s respectability was the respectability of a branch of pure mathematics and the older probabilists needed such recognition: Feller remarked to Fréchet in 1946 on the difference between France and America, “you in France have an atmosphere more friendly to probability and luckily meet less prejudice. Here good mathematicians declare
openly that probability is not mathematics.”\textsuperscript{65} A reference to mechanics in Feller’s Introduction (1950: 6) suggests that the issue of really being mathematics affected all of applied mathematics: of course, the name does not settle it—domestic animals are animals but glass animals are not.\textsuperscript{66} For Doob (1996) the 20\textsuperscript{th} century saw the placing of probability in analysis, the development of “rigor in mathematical probability” and the “mathematizing of probability”—they amounted to the same. Statistician contemporaries of the older probabilists had their own vision of mathematising—the recognition of mathematical statistics as a branch of applied mathematics: with probability an adjunct to mathematical statistics, its respectability was automatic.\textsuperscript{67} This respectability was achieved in the 1930s and 40s when Cambridge and Manchester—mathematical centres and arbiters of respectability—gave Wishart, Bartlett (and their statistician successors) positions as lecturers and professors alongside traditional applied mathematicians.

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