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“But you have to remember P. J. Daniell of Sheffield”

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Abstract: P. J. Daniell is a mysterious figure who appears at several turns in the history of mathematics in the 20th century, in the fields of integration, stochastic processes, statistics, control engineering and even in the history of English mathematical education. The main focus of this paper is on Daniell's work in relation to the development of probability in the twentieth century. But as it seems that no survey of his work and life has been attempted for 60 years I try to consider all his contributions and place them in an appropriate historical context.

Résumé: P. J. Daniell est un personnage mystérieux qui apparaît à plusieurs moments clés de l'histoire des mathématiques du 20ème siècle, dans le domaine de l'intégration, des processus stochastiques, des statistiques, de la commande optimale et même dans l'histoire de l'éducation mathématique en Angleterre. Ce papier se concentre sur le travail de Daniell en relation avec le développement des probabilités au vingtième siècle. Comme aucune description de sa vie et de son œuvre n'a semble-t-il été réalisée depuis 60 ans, nous essayons de dresser un tableau de l'ensemble de ses contributions et de les placer dans un contexte historique approprié.

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P.J.Daniell

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1 Introduction

In a conversation with David Kendall, the doyen of British probabilists, Bingham (1996, p. 185) raised the question of the origins of a British probability tradition. After some names came up, Kendall remarked:

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

These remarks convey a feeling that Daniell should have been the source of a tradition and explain why he was not but mainly they reveal uncertainty about the man and his circumstances. The mystery is especially teasing when the name is so familiar through eponyms—the “Daniell integral,” the “Daniell window” and the “Daniell-Kolmogorov extension theorem.”

There should be no mystery. Percy John Daniell (1889-1946) went to a famous school and to Trinity College Cambridge. Before he wrote anything, he had a place in the history of mathematical education in England, as the last Senior Wrangler before Cambridge abolished the order of merit of its mathematics graduates. He became a Vice-President of the London Mathematical Society and an elege by C. A. Stewart (1947) appeared in its *Journal*. For statisticians he was discovered—or perhaps re-discovered—when Stigler (1973) brought to light a paper on robust estimation and mentioned in passing one on stochastic processes.

My aim is to dispel as much of the mystery as I can and to explain Daniell’s peculiar position in the British probability and statistics tradition(s). His work in

probability and statistics did not have a central place in any of what seem to have been his several careers. All the careers are registered by Stewart, a colleague at Sheffield for more than twenty years, but naturally the emphasis is on the pure mathematician and the papers he published on analysis between 1918 and -28. As this is the first account of Daniell in sixty years, I have tried to indicate what has come to light in the intervening years pertaining to all the careers. There is much to report for Daniell was part of some big stories. There is no ‘new’ data in the form of personal papers; an Appendix describes the few letters I have found. Of course much of what Stewart took for granted has to be explained today and new perspectives have opened up, including that of a British probability tradition. Stewart missed a few of Daniell’s efforts, which is not surprising when they were so dispersed. *If* Sheffield had a portrait, it might hang in any of the departments of Pure Mathematics, Applied Mathematics, Automatic Control & Systems Engineering, Physics or Statistics!

2 King Edward’s, Trinity and Liverpool

Percy Daniell was born in Valparaiso, Chile on the 9th of January 1889. His parents William and Florence were from Birmingham and the family settled there on its return to England in 1895. In the 1901 census their ages are given as 44 and 37 and William’s occupation as an export merchant’s buyer—presumably William had had business in Chile. The couple were religiously minded and involved themselves in the local Baptist church; Percy’s younger brother, Eric, later became minister there. In January 1900 Percy went to King Edward’s School, Birmingham, “one of the great schools of England,” according to its historian, Hutton (1952, p. xiii). The school was especially well-regarded for its mathematics teaching and here the great figure was Rawdon Levett who made mathematics at the school his life-work: Levett was the “great schoolmaster” celebrated by Mayo & Godfrey (1923). Although Levett retired in 1903—half-way through Daniell’s time at the school—the tradition was maintained under his successor Charles Davison. The school excelled at getting its pupils into Cambridge University although Mayo & Godfrey (1923, p. 329) stress that “systematic preparation for scholarships [Levett] eschewed and derided. We imbibed from him a contempt for every kind of cram and commercialism in learning.” Many of Levett’s pupils did well at Cambridge, becoming “wranglers” as mathematics students who achieved a first class degree were called. The wranglers were listed in order of their examination marks: Senior Wrangler, Second Wrangler, Third Wrangler, and so on down. The ranking of students generated a ranking of their colleges and even of their schools and schoolmasters. Hutton (p. 162) notes that, while Levett produced three Second Wranglers, his successor Davison produced two Senior Wranglers, Daniell and A.

W. Ibbotson. Warwick (2003, pp. 356-7) places Levett and his school in a system in which Cambridge standards passed to the schools that fed the university with its students with past wranglers (Levett was 11th Wrangler in 1865) preparing future ones. Stewart (1947, p. 75) reports that Percy Daniell was “an outstanding pupil of the school, not only as a scholar but also as a prefect and a member of the school Rugby XV.” Percy won a major scholarship to Trinity College Cambridge to read mathematics; his Cambridge career would be even more exemplary.

“The study of Higher Mathematics in the British Empire is now practically concentrated at Cambridge” opined the *Times* newspaper in 1906 (quoted by Howson (1982, p. 144)) and at Cambridge it was concentrated at Trinity College. When Daniell went up in 1907 the fellowship included the mathematicians Forsyth, Whitehead, Herman, Barnes and Hardy and the physicists G. H. Darwin, Eddington, Rayleigh, J. J. Thomson and N. R. Campbell. A few months before Daniell went up a reform of the mathematics teaching had been decided upon and Daniell belonged to the last generation of undergraduates who went through on the old regulations; Hassé (1951, pp. 155-7) recounts the heated reform debate. The old system with its many ramifications has been analysed by Warwick (2003) and several of those who experienced it have left recollections, those of Hassé (1951) and Littlewood (1953/86) being particularly pertinent.

Two features of the system stand out—the competition and the syllabus. Classics at Oxford had overtaken mathematics at Cambridge as the ideal preparation for a career in public life but, as Oxford did not grade its runners, the middle-class public still followed the Cambridge event. Private coaches pushed the most promising students to the highest honours and Daniell coached with the best of them, R. A. Herman: see Warwick (pp. 282-3). Herman “had a genius for teaching” recalled Neville (1928). J. E. Littlewood, who was Senior Wrangler in 1905, also coached with Herman: he recognised the excellence of the teaching but he (1953/86, pp. 83-6) remembered too the gruelling preparation, how to be in the running for Senior Wrangler, “one had to spend two-thirds of the time practising how to solve difficult problems against time.” Littlewood’s reflection on Part I was that “I wasted my time, except for rare interludes” but “the game we were playing came easily to me, and I even felt a satisfaction of a sort in successful craftsmanship.” The syllabus was notoriously old-fashioned and Hassé (p. 154) emphasises one particularly serious gap—“what we now know as analysis.” Nevertheless he thought, “The *real* mathematician ... will survive the effects of any teaching and of any syllabus.”

The naming of the *last* Senior Wrangler in June 1909 attracted special attention: “Killing an Academic Tradition” was the headline in the *Literary Digest*. The *Times* report, reproduced in Warwick (2003, p. 203) describes the scene in the Senate Room—“a wild shout went up from Trinity men” when the name Daniell

was pronounced—and has biographical sketches of the top three wranglers. They were Daniell, E. H. Neville, and Louis Mordell with William Berwick and C. G. Darwin among those coming fourth. Mordell and Darwin had the most illustrious careers, the first a number theorist who replaced Hardy as Sadleirian professor and the second a mathematical physicist who became head of the National Physical Laboratory. Someone who would have known of Daniell was Ronald Fisher who went up in 1909 but, as Stigler (1973) notes, he was two years behind and went to a different college; most probably they did not know each other.

After his day as the most famous mathematician in England Daniell could have left with a degree, like Ibbotson and J. M. Keynes (1905, 11 w.) who prepared for the Civil Service examinations, or continued to Part II of the Mathematics Tripos like Neville, Mordell, Berwick and Darwin. Instead he switched to what may be called the *other* physics programme, the Natural Sciences Tripos. With its great reputation for applied mathematics the Mathematics Tripos was once *the* preparation for the theoretical physicist; in the period 1850-1910 wranglers held around 40 per cent of all the physics professorships in the United Kingdom, according to Wilson (1982, p. 365). By Daniell's time the Natural Sciences Tripos had outgrown its early reputation as the course for the weaker students. The Cambridge physicists headed by J. J. Thomson of Trinity and the Cavendish Laboratory were highly regarded—outside Britain they were probably more highly regarded than the Cambridge mathematicians. Whatever Daniell's reasons, the choice worked out well for in 1911 he was placed in the First Class of the Natural Sciences Tripos; here the ordering was strictly alphabetical.

Fifty years earlier a college fellowship would have come automatically and fifty years later the best graduates who wanted an academic career were doing PhDs. Of Daniell's cohort, Neville got a Trinity fellowship, Mordell wrote an (unsuccessful) fellowship dissertation for St. John's, Berwick went to Bristol as an assistant lecturer in mathematics and Darwin to Manchester as a lecturer in mathematical physics. Daniell went to Liverpool as an assistant lecturer in mathematics. The Darwin and Daniell appointments show how there was no sharp division between physics and mathematics. Daniell, for all his Senior Wrangler prowess, did not have much of a mathematical training.

The University of Liverpool was one of the civic universities created at the end of the 19th century: established as a university college in 1881, it became a university in 1903. In the 80s Forsyth and then Herman had briefly occupied the mathematics chair and so there were ties to Trinity. The university's historian—Kelly (1981, pp. 106-7)—says little about mathematics beyond registering the massive immobility of F. S. Carey (1863-1934) who occupied the chair from 1886 to 1923 and the “immense teaching task” facing a small number of teachers. One of those was W. H. Young (1863-1942). Young, Britain's leading analyst, had a part-

time appointment from 1906 to -14; Hardy (1942) and Grattan-Guinness (1972) describe his work and improbable career, while his contributions to integration are discussed by Hawkins (1979) and Pesin (1970). A spell at Liverpool was almost a rite of passage for high wranglers: among those who passed through were Ebenezer Cunningham (1 w. 1902), Harry Bateman (1 w. 1903), James Mercer (equal 1 w. 1905), Hassé (7 w. 1905) and H. W. Turnbull (2 w. 1907). On his watch, 1911-12, Daniell rounded off his Cambridge career by winning the Rayleigh Prize. His contemporaries were prize-winners too: Neville and Mordell were first and second Smith's prizemen in the same year while Berwick and Darwin had submitted the year before; Barrow-Green (1999) explains the history and significance of these prizes. Revised versions of the prize essays were often published but Daniell's "Diffraction of light for the case of a hole in a plane of perfectly reflecting screen" appears not to have been.

3 Göttingen through Texas

There was nothing surprising in the progressions King Edward's, Trinity, Liverpool or Scholar, Senior Wrangler, Rayleigh prizeman but there was in Daniell's next step, to a new university in Houston, the Rice Institute (renamed Rice University in 1960). A move to America was not unknown in Cambridge for shortly before Bateman had gone to Bryn Mawr where Charlotte Scott was professor and then to Johns Hopkins where Frank Morley was based; Scott and Morley were both earlier migrants.

The Rice president, Edgar Odell Lovett, had vast ambitions for the Institute and from August 1908 to May 1909 he visited institutions of higher learning all over the world discussing the shape of the new university and taking steps towards recruiting faculty. "Lovett wanted faculty members who were found through his endeavours instead of those who found him" writes Meiners (1982, p. 44) in her history of Rice. Lovett found applied mathematicians and physicists through J. J. Thomson. For the professor of physics Thomson recommended one of his first research students, H. A. Wilson (1874-1964), fellow of Trinity, professor at London and a well-established experimental physicist; see Thon (1965). For the assistant professor of applied mathematics Thomson recommended Daniell. There appears to be only one very short letter in the Rice archives from Thomson to Lovett regarding the appointment: "Dear Dr. Lovett: I have no reason to think there is any Jewish strain in Daniel [sic]. Yours very truly, J. J. Thomson." Anti-Semitism was a fact of academic life in the United States and in Britain; Synnott (1986) surveys the situation in the US. For Norbert Wiener, who was a Jew, it was a very significant fact; see his autobiography (1953, *passim*) and the remarks following letter (2) in the Appendix below.

Daniell did not take up his post immediately for he was given a travelling fellowship of \$1000 to study in Germany for a year. Evidently Lovett judged him and another English assistant professor, the Oxford biologist Julian Huxley not quite ready; certainly the other assistant professor of mathematics, Griffith Evans, was much better prepared—see §4 below. Huxley (1970, pp. 96-7) recalls going to Heidelberg “to polish up my comparative biology” and on to Munich. Huxley attended the opening of the Institute in November 1912, an event marked by an elaborate international convocation of scholars: “If there was one thing President Lovett was good at, it was organizing a show with as many notables as possible” recalled Huxley (p. 94). The mathematical notables were Volterra and Borel. Poincaré was invited but he died earlier in the year and Volterra spoke about his life and work; see Volterra (1917). Daniell missed the opening but he was there when Volterra re-visited Rice in 1919.

From July 1912 to October 1913 Daniell was in Göttingen. There was much going on that would be relevant to interests of the later Daniell, including the beginnings of functional analysis—see §4 and §6 below—but in 1912 he was there to study “under Born and Hilbert” (Stewart (p. 75))—that is, theoretical physics and more theoretical physics. Max Born (1882–1970) was a newly appointed docent in physics, while David Hilbert (1862-1943) the world-famous mathematician was absorbed in the problem of providing the proper mathematical foundation for physical theory. Corry (2004) has reviewed Hilbert’s activities and in his book he (p. 451) lists the courses Hilbert gave on physics and related subjects during the time of Daniell’s stay: the molecular theory of matter, partial differential equations, mathematical foundations of physics, foundations of mathematics (and the axiomatisation of physics) and electron theory. Daniell’s stay had one tangible product, an article with Ludwig Föppl (1887-1976). Föppl was a physicist who had just completed a thesis under Hilbert on the “Stable arrangement of electrons in the atom.” In their “Kinematics of the Born rigid body” Föppl and Daniell (1913, p. 519) state that their problem was raised in Hilbert’s course on electron theory. Ludwig’s father, August, figures in histories of relativity as a possible influence on Einstein; see Miller (1981, pp. 150-4).

Daniell was around one of the most important and best documented developments in twentieth century physics. Miller (1981) covers the entire history of the special theory while Corry (ch. 4) describes the Göttingen contribution of Minkowski and Born: Born tells his own story in his autobiography (1978). Born (1910) attempted to apply the principle of relativity to the analysis of a rigid body and his effort was widely discussed—see also Pauli (1921/58) and Maltese & Orlando (1996). When Daniell arrived the peak of the controversy had passed and the Föppl-Daniell paper did not find a place in the literature. It was unearthed recently by Walter and he (1996 pp. 74-8; 1999, p. 159) has given a short account

of the paper. Walter emphasises the similarity with the independent work of Borel (1913).

Daniell was not the first Cambridge physicist to work on the principle of relativity: three of his predecessors at Liverpool—Cunningham, Bateman and Hassé—preceded him there as well. Warwick (1992/3 and 2004) has compared their work—all graduates of the Mathematics Tripos—with the work of graduates from the Natural Sciences Tripos. He argues that both groups failed, in different ways, to get inside the theory as it was understood in Germany. Daniell (from the Natural Sciences Tripos) seems to have been the only Cambridge physicist who did special relativity from the inside. However, this Cambridge physicist never reported back in person; he published a paper in English from Rice—see §5 below—and then left the field.

4 The Rice Institute and Griffith Evans

How long Daniell intended to stay at Rice is not known. Stewart does not describe Daniell’s ambitions and domestic plans although he reports one significant development, that in 1914 Daniell married Nancy Hartshorne, also of Birmingham; they would have two sons and two daughters. On the 18th of August Percy and Nancy left Liverpool to cross the Atlantic; two weeks before Britain had declared war on Germany.

The Rice Institute was like nothing in a European’s experience. That was the theme of Huxley’s “Texas and Academe” (1918) written on his return to England. Houston with its population of one hundred thousand was “not much more than an overgrown commercial village seventeen hundred miles away from the American metropolis.” (p. 55). Huxley marvelled that an institution dedicated to pure research would be established in such a place: indeed “It is hard for an Englishman to realise that the civilisation of the whole of the area west of the Mississippi ... is to all intents and purposes the product of the fifty short years since the civil war.” (p. 63)

The university Daniell joined was very small with an entering class of seventy-seven students and a faculty of ten. President Lovett had been head of mathematics at Princeton and he retained the position at Rice. But running the university left him no time for mathematics and the working mathematicians were Daniell and Evans. Griffith Conrad Evans (1887-1973) was Daniell’s most important colleague and it is a pity that there is nothing beyond their publications from which to form a view of their relationship. In his autobiographical notes—they cover 40 years in 3 pages—Evans (1969, p. 10) dispatched the first years at Rice with the remark that he was assistant professor of pure mathematics and Daniell assistant professor of applied mathematics, “sufficient reason of course for Evans to work in

applied mathematics and for Daniell to invent the Daniell integral.” The symmetry is nice though Evans was never so very pure or so very applied as Daniell.

Evans had written a thesis on integral equations at Harvard under the supervision of Maxime Bôcher, “Volterra’s integral equation of the second kind with discontinuous kernel” published as Evans (1910 and -12). Birkhoff & Kreyszig (1984, p. 278) describe how Fredholm’s papers of 1900-3 had the effect of moving integral equations “into the centre of interest of contemporary mathematics.” There was activity in Harvard and in the main European locations, Göttingen (Hilbert), Rome (Volterra) and Paris (Hadamard). “It was my good fortune to study under Professor Volterra from 1910 to 1912” recalled Evans (1958, p. 1); he also got the job at Rice on Volterra’s recommendation. Volterra had very broad interests and his influence on Evans went far beyond functionals and integral equations; this is reflected in Evans’s (1959) sketch of the master and it is a theme Weintraub (2002) develops. There was also a link between Rome and Paris. Volterra lectured in Paris and René Gateaux studied with Volterra in Rome; see Mazliak (2007). Evans was soon a rising star of American mathematics and in 1916 he was invited to give the American Mathematical Society Colloquium Lectures, his topic was *Functionals and their Applications*. In the same year he was made a full professor. In 1933 Evans moved to Berkeley as chairman of the mathematics department to become a great figure in the story of Berkeley mathematics. Morrey (1983) describes his life and career and Weintraub (2002) his work in mathematical economics. Neither author mentions Daniell or reports on how the Rice department functioned.

All Rice students took mathematics in their first year and so there was a lot of teaching. The entry standards were low compared to the English civic universities—not to speak of Cambridge—but at the other end of the scale there were research students. The mathematics departments of the English civic universities did not expect advanced students to come to them. Indeed they often sent their best graduates to Cambridge to begin all over again: Cambridge, like the Mother Country in the British Empire, was the location of all excellence. By 1918 Rice mathematics had its first PhD: Hubert Bray with a thesis on “A Green’s Theorem in Terms of Lebesgue Integrals.” Bray’s early research tied in to that of Evans and, to some extent, of Daniell. Bray spent his career at Rice, becoming a full professor in 1938.

5 The applied mathematician 1915

The first solo publications of the Rice assistant professor of applied mathematics appeared in 1915 in the *Philosophical Magazine* the British journal for applied mathematics/physics. These productions harnessed Cambridge and Göttingen in

a way that should have gratified President Lovett.

“The rotation of elastic bodies and the principle of relativity” was on the same theme as the paper with Föppl. It (1915, p. 754) begins

It is well known that the rotation of a “rigid” body about a stationary axis is inconsistent with the Principle of Relativity. In fact the circle along which any part moves is contracted while the radius is unaltered. It has been suggested that a rotating circular disk might buckle, but it could no longer be regarded as rigid. Herglotz, in a paper on the mechanics of deformable bodies has shown a method by which problems relating to elastic bodies may be solved.

The problem was certainly “well known” for between 1908 and -10 the *Philosophical Magazine* had published a string of papers on the subject; these are discussed by Walter (1996) and Warwick (2003, p. 428). Walter (1996, p. 73) discusses Daniell’s paper. By showing how the method of Herglotz (1911) could be combined with the theory of elasticity as presented by Love (1906) it brought together German and English physics in a surprising way.

The other contribution to the *Philosophical Magazine*, a two-part paper “The coefficient of end-correction”, was not connected to relativity. It was a piece of vintage Cambridge applied mathematics quite independent of recent discoveries. The object, as Daniell (1915, p. 137) relates, is to improve upon an estimate made by Rayleigh (1894):

If an electrical current passes through a long cylindrical tube of conducting material, and then out into a large hemispherical volume of the same, the total resistance is proportional to the total length of the tube plus a certain multiple of the radius. This multiple is the coefficient of end-correction which we require to find. Rayleigh, in his ‘Theory of Sound,’ found first that $.0785 < \text{this coefficient } k < .0845$. In the appendix he showed further that $k < .8242$, and he supposed that its true value did not differ greatly from this.

While the rotation paper was a piece of speculative physical theory that did not lead anywhere, the end-correction paper was a contribution to a standard problem and it entered the literature: see Selamet, Ji & Kach (2001) for a recent survey.

By 1915 Daniell must have thought it time he stopped working on the rigid body in relativity theory but he stopped doing applied mathematics altogether. Apart from some book reviews in 1923, he did not publish in applied mathematics for a decade and he only returned to the 1915 level of intensity in the Second World War when he worked on a different kind of applied mathematics. Daniell

continued to teach and to read in the field: the *BAMS* (January-February 1922, p. 76) announces a course on “Theory of radiation, electrons, and gravitation.” Presumably he talked to his colleagues in physics although I have only noticed one reference to an interaction with Wilson, the other Trinity man: Wilson (1922, p. 8) thanks Daniell for evaluating an integral.

In 1916-17 Daniell published on two new topics, integral equations and logic. The 1916 note on integral equations belonged to the world of Evans: it drew on the Italian literature and the publication was communicated by Volterra. It seems likely that Evans put Daniell onto the problem but the note is no pointer to the later division of labour in which Daniell supplied the deep analysis on which Evans’s work could rest—the note contains *no* deep analysis. Daniell’s paper on “The modular difference of classes” (1917) went even further from applied mathematics. Stewart (1947, p. 77) knew from Sheffield of Daniell’s enthusiasm for symbolic logic and the foundations of mathematics and how he “was skilled in the technique of the *Principia Mathematica*.” That skill was developed at Rice; a second article (1924) on mathematical logic was Daniell’s last American publication. Incidentally the *Principia* of Whitehead and Russell (1910-13) was not the only Trinity work read in Texas. There is one Daniell letter in the Trinity Library: dated 1919 and addressed to Sir James Frazer, it takes issue with a numerological argument in Frazer’s *Golden Bough*. The letter is signed “P. J. Daniell, (Former Scholar of Trinity).”

In 1918 there was a new Daniell, not an applied mathematician or a mathematical logician but an analyst publishing in American journals and citing American research. The new man and his writings appear in Kellogg’s (1921) census of American mathematics. The change was productive for the new papers had immediate impact and his reputation is still based upon them.

6 The “major papers”—1918-19

For Kendall the “major papers” were those on the “Daniell integral” and on the “Daniell-Kolmogorov extension theorem”, standard topics in modern texts on analysis and probability like Dudley (2002). “A general form of integral” (1918a) and “Integrals in an infinite number of dimensions” (1919c) are the papers usually cited but they are only two out of the ten or so on related themes that Daniell published in 1918-20. The results of the major papers were applied almost at once by Wiener in his work on Brownian motion—see §8 below—and on potential theory—see Wiener (1923)—but the papers themselves were contributions to the ‘pure’ theory of integration. The war disrupted communications between the American and European mathematical communities but Daniell’s work is recognised in Lebesgue’s (1926, p. 63) review of the development of the notion of an integral. Five of

Daniell's papers are referred to in the survey by Saks (1937) and his approach to integration is discussed in the histories by Pesin (1970), Bourbaki (1984) and Pier (2001). Kendall's view of the major papers as contributions to probability depended on the change in perspective brought about by Kolmogorov (1933); see §§8 and 11. Daniell produced a paper on probability but that *never* became known to probabilists; it is discussed in §8 below.

Daniell's 1918 paper and the pieces it cites—the earliest from 1912—came after the heroic period of the development of the Lebesgue integral, for which see Hawkins (1970). Daniell (1918a, p. 279) indicated the direction of recent research:

The idea of an integral has been extended by Radon, Young, Riesz and others so as to include integration with respect to a function of bounded variation. These theories are based on the fundamental properties of sets of points in a space of a finite number of dimensions.

For Daniell this was a new field with new people—apart from Young his Liverpool colleague. Although Daniell often refers to Young, Young never refers to him and there are no traces of any communication between them. Daniell also cites the Americans E. H. Moore of Chicago and T. H. Hildebrandt, one of his students; Moore was one of the “towering figures” of American mathematics—see Zitarelli (2001).

Daniell (1918a, p. 279) outlines his contribution and its very broad scope

In this paper a theory is developed which is independent of the nature of the elements. They may be points in a space of a denumerable number of dimensions or curves in general or classes of events so far as the theory is concerned.

Daniell presented an abstract theory but did not illustrate it for the cases he mentioned. In 1919 he produced integrals in for points in infinite dimensional space but the promise of applications to probability, implied in the phrase “classes of events,” was not made good either in this paper, or in any of his other writings. The referee has suggested that, as Daniell did not mention probability, there was no such promise and it is possible that Daniell had nothing specific in mind and only wanted to emphasise the generality of the approach.

Daniell provides no further orientation. His (1918a, p. 280) comment on Fréchet (1915), that it “does not discuss existence theorems so completely,” seems curiously pedantic when modern commentators—cf. Pesin (1970, p. 172) and Shafer and Vovk (2006, p. 79)—find their approaches completely different: Fréchet begins with an additive set function on subsets of an abstract set E while Daniell begins with an “integral” on E —a linear operator on some class of real-valued functions on

E. The construction is the basis of what is taught today as the “Daniell integral” or the “Daniell-Stone integral”—this name acknowledging Stone’s (1948) development of the original notion. Daniell (1918a, pp. 280-1) gives “a few instances of the theory” showing how existing integrals fit in: the Riemann integral is extended to the Lebesgue integral, the Stieltjes integral is extended to the Radon integral and so on. The only integral of “a really new kind” is a one-dimensional Stieltjes integral where the integrating function is not of bounded variation.

In 1919 Daniell published two papers on integrals in an infinite number of dimensions. They aimed to deliver on the claim in his (1918a) as he explained in the first paper (1919c, p. 281):

In it a method was given whereby integrals could be defined for functions of general elements (p) which could theoretically be of any character. The author was then unable to give a definite example in which the elements (p) were points in a denumerably infinite number of dimensions.

The paper presents two examples where this can be done.

Doob (1989, p. 819) summarises the contributions of the two papers: in the first Daniell defined “product probability” measures in R^T for T countably infinite “corresponding to the probability context of independent trials, not necessarily with a common distribution” and in the second “general probability measures.” But this was not how Daniell expressed himself for as Doob says his papers are “not probabilistically oriented.” Shafer and Vovk (2006, pp. 88-9) make the same point more emphatically in a comparison of Daniell (1919d) and Kolmogorov (1933):

Daniell’s and Kolmogorov’s theorems seem almost identical when they are assessed as mathematical discoveries, but they differed in context and purpose. Daniell was not thinking about probability, whereas the slightly different theorem formulated by Kolmogorov was about probability.

See §9 below for some further history of this result. The (1918a, -19c and -19d) are, by common estimation, the “major papers” but there were more. Thus Dudley (2002, p. 184) refers to Daniell (1920b) for the Radon-Nikodym theorem, associated usually only with Radon (1913) and Nikodym (1930).

How and when did the new Daniell come about? There is a clue to “when” in a remark in Daniell (1918b, p. 353), “Since this paper was first written, a paper by Young [1916] has appeared.” But in the circumstances of the war such dates cannot be taken at face value. Was Evans somehow involved in Daniell’s change of scientific personality? Evans had not been involved with the development of

measure theory or with any of the personnel; his links were to Volterra and, by extension, to Hadamard—see Evans (1914). However there is a section on the “linear functional” in his colloquium lectures *Functionals and their Applications* (given in 1916, published in -18) which shows that he had been studying some of the same papers as Daniell. Evans (1918, p. 66) also cites a 1917 paper on the Stieltjes integral by Daniell although I have found no other trace of this. It is clear that Evans and Daniell made a move in the same direction at the around same time; who moved first and whether the move was concerted is unknown. Daniell certainly made the more prolonged study of the new field.

In those days it was unusual for authors of article to acknowledge the general assistance given by others. When Daniell referred to Evans and Bray it was for what they had published: in “Integral products and probability” Daniell (1921c, p. 143) refers to Evans (1916) and in “Further properties of the general integral” he (1920a, p. 218) refers to Bray (1919) whose theorem he was generalising. However when Evans wrote a long review article, *Fundamental Points of Potential Theory*, for *The Rice Institute Pamphlet* series he (1920, pp. 254-5) said something about how he interacted with Daniell and Bray:

The author ... takes the opportunity of acknowledging how much he has benefitted by the exchange of ideas with his colleagues, especially with P. J. Daniell, whose studies on a general form of integral are now available. The theory has been worked out here for two dimensions only, but much of the material is obviously independent of the number of dimensions. For the working out of the rest the author is counting on the help of his colleague H. E. Bray, who has already published a study of Green’s theorem in terms of Lebesgue integrals.

In the body of the paper there are references to the work of Daniell and Bray and throughout there is a strong impression of a Rice team effort. In the division of labour Daniell worked on the most abstract problems.

Daniell’s contribution to the *Pamphlet* series was a review of his work on integration, *The Integral and its Generalizations*. Starting from the “college textbook” notion of a definite integral Daniell (1921b) examines two notions akin to the integral, the moment of a mechanical structure and the statistical average. He identifies the common characteristics of these notions behind their superficial differences: in the mechanical cases “we have the familiar space and time as a background” but “statistical averages are obtained for many kinds of entities, numerically measurable by necessity, but entities dependent on objects or qualities which are not always numerical in their essence.” With this motivation Daniell recapitulates the arguments of the 1918 paper finishing off with 10 applications of the idea, including Wiener’s first (pre-Brownian motion) paper. The applications

are all in pure mathematics and Daniell does not return to moments or to statistical averages. There is no formalisation of expectation or hint of the probability via expectation approach advocated by Whittle (1970) and other modern writers.

For the first four of Daniell's years in America Britain was at war. Some of his compatriots, including Huxley, went home. In his obituary Stewart says nothing about the war and there is no point in speculating about Daniell's feelings over what was a catastrophe for his generation. In April 1917 the United States declared war on Germany and for the next two years life at Rice was disrupted: Bray joined the infantry and, after being discharged for defective vision, served as "ballistic computer" at the Ordnance Proving Ground, Aberdeen Maryland; Evans was in the army from February 1918 until June 1919, much of the time in Italy where he managed to see Volterra; Wilson did research for the US Navy. Britain introduced conscription in 1916 and Daniell would have been called up. The United States introduced conscription on entering the war. Daniell was registered with the US Draft Board but his only contribution to the war as recorded in the King Edward's commemorative volume—Heath (1920)—was lecturing flyers on aerodynamics in California in October 1918.

7 "Observations weighted according to order"

In 1920 and -21 Daniell published two papers in the *American Journal of Mathematics*. The papers had a common fate in that Daniell did not follow them up and they were not known until Stigler (1973) discovered them fifty years later. The *American Journal* was the oldest American journal, founded by an earlier English migrant J. J. Sylvester and edited in those days by another, Frank Morley. In 1920 there was no journal known for publishing work on the combination of observations (or on probability) but the *American Journal* proved a poor choice as Daniell's papers were the only ones of their kind it published in the 20s. In 1932 it published a paper on a related theme—by A. T. Craig—but this did not refer to Daniell.

The first of the unnoticed papers, "Observations weighted according to order" (1920c), is possibly Daniell's most striking paper both for what it contains and for seeming to come out of nowhere. (The 1921 paper on "dynamic probability" is discussed in the next section.) The papers on integration came out of nowhere only in the sense that nothing in Daniell's previous work seemed to prepare for them, they belonged to a literature with a past and a future. Nothing in his previous work prepared for the "Observations" but nothing in the literature did either and the paper had no sequel: as Stigler (1973, p. 876) says, "It could in fact be claimed that Daniell was at least thirty years ahead of its time, for it took that long for his major results to be rediscovered."

“Observations” (1920c, p. 222) begins with the matter of comparing different estimators of “norm” and “deviation”:

When a series of measurements of some quantity are made, two particular quantities require to be calculated expressing respectively the norm and the deviation. For the norm the mean or the median is used while there are three measures of dispersion, the standard or root-mean-square deviation, the mean numerical deviation and the quartile deviation. The question is as to which of these are the more accurate under a general law.

The problem comes from the theory of errors but Daniell’s language reflects wider reading. “Standard deviation” and “quartile” come from biometrics/statistics and Daniell must have had some acquaintance with its literature, an inference supported by the appearance of the Pearson curves later in the paper. The paper has only one explicit reference, to Poincaré’s (1912) book on probability.

The passage in Poincaré (1912, p. 211) treats the practice of “discarding” one or several “extreme measures” when calculating the mean or median to produce what Daniell calls a “discard-average” (a trimmed average). Daniell does not take anything from Poincaré’s technical discussion and he (p. 222) mentions the passage only to motivate a spectacular extension of the mean-median contest:

Besides such a discard-average we might invent others in which weights might be assigned to the measures according to their order. In fact the ordinary average or mean, the median, the discard-average, the numerical deviation (from the median, which makes it a minimum), and the quartile deviation can all be regarded as calculated by a process in which the measures are multiplied by factors which are function of order. It is the general purpose of this paper to obtain a formula for the mean square deviation of any such expression. The formula may be used to measure the relative accuracies of all such expressions.

In the terminology Fisher was introducing elsewhere *the formula* would give the (large-sample) “variance” of all such “statistics.” Having obtained a formula (p. 228) for the variance of a linear function of order statistics, Daniell went on to consider the “most accurate weighting.” This corresponded to Fisher’s “efficiency.” Daniell ends by computing the relative accuracies of the different statistics for the normal distribution and the two extreme Pearson symmetric forms with short tails and the long tails (Student’s distribution).

Stigler (1973, pp. 876-7) reviews the paper’s argument and matches its results to results in the modern literature. Daniell’s results did not depend on recent

advances in mathematical or statistical technique and Stigler finds related results on order statistics in Gauss, Laplace and Sheppard. There is nothing to indicate that Daniell knew these earlier writings and it would be surprising if he did. On the other hand, there were things he knew and knew to ignore, especially on the inference side—in Poincaré, inverse probability and, in Pearson, the method of moments. While Fisher (1922) can be interpreted as the last stage in a struggle to escape from “inverse probability” and from what he had been taught as an undergraduate—see Aldrich (1997, 2006a)—no such ghosts are lurking in Daniell (1920c).

Daniell’s contribution should have fitted rather nicely into the American literature for discarding observations was seen—in England at least—as something of an American speciality with Benjamin Peirce and Chauvenet the standard references. Daniell does not even allude to this old work and he does not refer to any contemporary American work. Naturally there was work but it is largely forgotten for, with the exception of Sewall Wright’s writings on path analysis, only two papers from the 20s enjoy any modern reputation, Daniell (1920c) and Working & Hotelling (1929)—the latter a very creative response to Fisher’s ideas on regression; Aldrich (2007) has some comments on Hotelling and the American scene. Neyman (1976), Hunter (1996) and Stigler (1996a and -b) review the personnel of American statistics but less for their research than for creating the Institute of Mathematical Statistics and the *Annals of Mathematical Statistics* which would be so important later. The authors most visible in the American mathematical journals of the 20s were E. L. Dodd, H. L. Rietz and J. E. Coolidge, the last two of whom produced textbooks. Like American mathematicians in general they were more influenced by Continental European work than their English contemporaries. Daniell does not refer to them but Dodd (1922) refers to him; this was the only reference to Daniell (1920c) that Stigler could find before the 1960s and I have not found any more.

Edward Lewis Dodd (1875-1943) was at the University of Texas at Austin. In those days Austin was not easily accessible from Houston and there is no reason to suppose that Dodd and Daniell ever met. Dodd was the most consistently productive American contributor to the field which became mathematical statistics—he was a charter member of the IMS. Many of his publications, beginning with his (1912 and -14) dealt with the problem of the mean. It should have been good to have been noticed by Dodd for, as well as being the top specialist, he had connections—in particular with Henry Lewis Rietz (1875-1943) of Iowa whose PhD students would include A. T. Craig and S. S. Wilks. As a piece of writing Dodd (1922) is very different from Daniell (1920c): the wide reading is on show and so is the author’s use of that reading. At least one of the ideas in the paper should have interested Daniell, combining the distribution function of von Mises (1921)

with the Stieltjes integral; for the latter Dodd went not to Rice but to Chicago–Hildebrandt and Bliss. Daniell’s paper was caught in Dodd’s (1922, p. 152) net but all that was extracted were the numerical values for the relative accuracies of the mean and median for the normal distribution and the two Pearson curves. Dodd did not mention that there was a general theory which the values illustrated. Dodd’s paper, which was read, hardly encouraged its readers, including Craig (1932), to look back at Daniell’s work.

Isolation from active statistical research and the fact that Daniell wrote only once on the topic are the factors Stigler (1973, p. 877) uses to explain the obscurity the paper has enjoyed. A decade after the publication of Daniell’s paper the coming generation of American statisticians seemed to think that active statistical research was something done abroad. Today Dodd and Rietz are remembered as the teachers of Wilks who was one of the forces behind the reconfiguration of American statistics in the 1940s but Wilks, like Hotelling and Neyman the other leaders of that movement, learnt about modern statistics in England.

8 Daniell and Wiener 1921-22

For fifty years “Integral products and probability” went entirely unnoticed but the paper was neither out of nowhere nor decades ahead of its time; it came out of Daniell’s previous work and Wiener was doing related work. The relationship between Daniell and Norbert Wiener (1894-1964) is fascinating: the influence was always one-way but the direction changed. Wiener very visibly used the Daniell integral in some of his most important early work; less visibly—and twenty years later—Daniell translated Wiener for British engineers; see below §10. There is a brief account of how Wiener used Daniell’s ideas in Bourbaki (1984, pp. 239-242) and a more extensive one in Shafer & Vovk (2005 and -6); Shafer and Vovk investigate the work of Daniell and Wiener as part of the background to Kolmogorov’s *Grundbegriffe*. but they conclude that Kolmogorov was not influenced by it. Daniell makes fleeting appearances in the various lives of Wiener–Levinson (1966), Masani (1990), Segal (1992) and perhaps most authoritatively in Wiener’s two volumes of autobiography (1953, -56).

In his survey of the integral Daniell (1921b) Wiener’s first paper appears as the last of 10 illustrations. Daniell (p. 61) writes:

Recently N. Wiener has investigated the preliminary problem of weighting in general integrals and in his example (d) defines an integral in a space of continuous functions. Wiener proves that every bounded continuous functional is summable in accordance with his definition of an integral. Further papers are to published soon.

Daniell did not use anything from Wiener (1920) and he never wrote about the further papers. He saw them because he was one of Wiener's referees for jobs in England and the British Empire—see letters (1) and (2) in the Appendix—and Wiener kept Daniell informed of his research. In the post-script to the first letter (February 1922) Daniell wrote, “Thanks for reprints which interest me considerably.” That seems to be the only record of Daniell's reaction to the Brownian motion papers.

This odd relationship began about 1920 but before Wiener ever encountered the Daniell integral the two men had in common places, people and ideas. Wiener started in mathematical logic and after his PhD at Harvard he spent 1913-14 at Trinity and in Göttingen. He studied logic with Russell and analysis with Hardy, followed by differential equations with Hilbert, group theory with Landau and philosophy with Husserl; see Masani (1990, p. 59). Wiener knew Daniell's Rice colleagues: Bray from Tufts and the Aberdeen Proving Ground where they had been undergraduates and then computers together and Evans from Harvard where the fifteen year-old post-graduate had attended his vector analysis class as an “informal listener”; see Wiener (1953, p. 257) and Evans (1969, p. 10). Huxley (1970, p. 100) even recalls Wiener visiting Evans in Rice but I think he was confusing Wiener with W. J. Sidis, another prodigy in Evans's class; Sidis is the subject of a long discourse in *Ex-Prodigy* (1953, pp. 131-8). Wiener knew the Rice subjects—integral equations, functionals and integration—for at the end of the war he read them up in “Volterra's *Théorie des équations intégrales*, a similarly title book by Fréchet ... and Lebesgue's book on the theory of integration.” “For the first time I began to have a really good understanding of modern mathematics” he (1953, p. 265) recalled.

In 1919 Wiener was appointed an instructor in mathematics at the Massachusetts Institute of Technology. Although he had been doing mathematics for some years, this was the beginning of his career as a mathematician. Wiener started publishing in several areas but the five articles he wrote in 1920-22 applying the Daniell integral to function space were his most concentrated effort and represent his first great success. Wiener (1953, -56), Levinson (1966), Masani (1990) and Segal (1992) all describe the genesis of the work. The accounts are based on what Wiener remembered thirty years later and there are discrepancies both between them and between them and what Wiener wrote at the time. Putting the accounts together, it seems that I. A. Barnett put Wiener onto Daniell's work when Wiener asked for advice on a research area in analysis. Barnett was a bit more established in mathematics than Wiener—his Chicago thesis was on “Differential Equations with a Continuous Infinitude of Variables”—and perhaps he knew Daniell's work because he had too had published in the *Annals of Mathematics*. Wiener's accounts (1953, p. 274: 1956, p. 35) differ on how exactly Barnett de-

scribed the possibilities of the new field but it would be most surprising if it were in terms of generalizing the concept of probability so that occurrences were “something of the nature of path curves in space” as Wiener recalled in 1956. There was no probability in Wiener’s first paper or in the papers it cites. Wiener refers to Lévy (1919) and Gateaux (1919 and -19) although he (1920, p. 67n.) states that he had not read Gateaux when he first drafted the paper. Lévy (1886-1971) and Gateaux (1889-1914) were at the Paris end of the Paris-Rome functionals axis; see above §4. Like Wiener, Gateaux wrote on “the mean of a functional.”

Wiener’s first paper (1920) is dated November 15, 1919 and, like the major papers, it appeared in the *Annals*. After noting that the construction in Daniell (1918a & -19b) “leaves the mode of establishing integration over the original restricted set in general undetermined” Wiener (p. 66) described a general method for setting up a Daniell integral when the elements of the underlying space are functions; this is much more ambitious than the examples in Daniell (1918a). Brownian motion appeared a year later in the two papers Wiener presented at the December 1920 meeting of the AMS—published as 1921c & -d. In between came the International Mathematical Congress in Strasbourg which Wiener (1956, pp. 44-70) remembered as the great event of 1920.

The Strasbourg Congress was a victors’ conference as the location and the exclusion of mathematicians from the defeated Central Power underlined. For Wiener it was an opportunity to establish contacts with French mathematicians and he arranged to spend the weeks before the conference working with Maurice Fréchet (1878-1973) the Frenchman who he thought had most to teach him; Fréchet’s work figured in Daniell’s 1918 and -19 papers. Wiener waited a long time for Fréchet to become free but the time was not wasted. In August he wrote to his sister Constance (quoted by Segal (1992, p. 397)):

I find that I am making a little headway with my problem—integration in function space—and in a way that may have practical application. I define the measure of an interval in it in a way that hitches up with probability theory as it is defined in statistical mechanics, and I have been living in hopes that the Lebesgue integral which I may get from it will be good for something. At any rate, when I meet Fréchet, I shall have peach of a problem to work on.

Wiener (1956, pp. 36-7) and Masani (1990, p. 79) say that the first “practical application” Wiener considered was turbulence following the work of G. I. Taylor the Cambridge applied mathematician. However it is not clear how Wiener could have known about Taylor (1922) for, while the paper had been written, it was not yet published. It certainly played a role in some of Wiener’s later work, especially his (1930), and Wiener may have been casting forward its influence. It is possible that Brownian motion was the first application.

Strasbourg was a small conference with 200 delegates of whom 11 were from the United States. Daniell was there with his wife; presumably they took advantage of the conference to make their first trip home since before the war. Young was there too and in the same session as Daniell. Wiener's published recollections do not mention any meeting with Daniell but it would be extraordinary if they had not met. They had common interests and common friends and 18 months later Daniell was writing references for Wiener when he applied for jobs; see the Appendix. However their conference papers did not treat the subject of greatest common interest, the Daniell integral. Daniell's paper was on "Stieltjes-Volterra products" and Wiener's two were on bilinear operations (1921a) and normed spaces (1921b).

The stimulus for the paper on "Stieltjes-Volterra products" may have come from Volterra's recent visit to Rice but the topic fitted into the larger project of reconstructing the standard applications of the integral around the new integration theory. Referring to Volterra (1913), Daniell (1921a, p. 444) writes that "Volterra has defined an integral composition which possesses some of the properties of an algebraic product." Daniell adds that "For many purposes in mathematical physics it is an advantage to use Stieltjes integrals in place of ordinary integrals and this suggests the following type of integral product which we may call the S-V (Stieltjes-Volterra) product":

$$\alpha \cdot \beta(s, t) = \int_{-\infty}^{+\infty} \beta(u, t) d_u \alpha(s, u).$$

Daniell's main business was the investigation of the algebraic properties of this product.

Daniell applied the theory to summable series (pp. 135-6) with reference to Hardy & Riesz (1915). Perhaps Daniell chose this application to show the relevance of his work to what was being done in England with an eye to returning. He (p. 134) also drew attention to an equation of special importance which "occurs in connection with probability and other parts of mathematical physics" which is an interesting turn of phrase from somebody associated with the 'purification' of probability. The equation is

$$\alpha_u \cdot \alpha_r(s) = \alpha_{u+r}(s).$$

Daniell does not consider the applications of this equation but Dieudonné (1981, p. 229) remarks how the Fourier transform of a bounded Stieltjes measure became a "favorite tool of probabilists." A similar point is made in Bourbaki (1984, p. 235) where the note is described as "rarely consulted."

Whether Wiener and Daniell talked probability in Strasbourg—whether they talked at all—is unknown but the following year both published papers on probability: Wiener's two papers on Brownian motion appeared in the *Proceedings of*

the National Academy, Daniell's "Integral products and probability" in the *American Journal*. Coming upon Daniell's paper 50 years later an evidently surprised Stigler (1973, p. 877) reported that

[Daniell] presents one of the earliest treatments of continuous time Markov processes, including the Chapman-Kolmogorov equation (ten years before Kolmogorov) and a short treatment of the Wiener process (two years before Wiener).

Perhaps the Wiener date should be brought forward from the culminating "differential space" paper of 1923 for Wiener may have been on the way as early as the summer of 1920.

The 1921 papers of Daniell and Wiener are strange to contemplate. They are both applications papers and both derive from Daniell (1918a) but how different they are! The "Brownian Movement" gave Wiener (1921c and -21d) an application for his ideas on integration in function space. His papers are contributions to mathematical physics with references to Einstein and Perrin; the background is sketched by Plato (1994, pp. 124-132) and Bourbaki (1984, pp. 239-242). The papers look like the work of a mathematical physicist and like nothing Wiener had written before. 1921 brought a new Wiener rather as 1918 had brought a new Daniell; perhaps Wiener's move into applied mathematics was connected with his new job at the "Tech". Daniell's "Integral products and probability" was also a demonstration of the applicability of his ideas—in this case the Stieltjes-Volterra product. Daniell was a mathematical physicist but his paper was decidedly not about physics.

Daniell (1921c, p. 143) describes the scope of his paper as follows:

In many problems arising in statistical biology and statistical economics time enters as an indispensable factor. It is the chief aim of this paper to provide a form of analysis suitable for such problems.... The first step in the analysis is a search for some standard formula on which may be built a more complex and general theory. It is found that, if certain natural assumptions are made, a functional equation is satisfied which is expressed in terms of a Stieltjes integral product. ... The Stieltjes integral product itself forms a second nucleus for our paper ...

The mathematics in this paper was very clearly out of somewhere—out of the Strasbourg paper and the literature behind it: Daniell (1921c) refers to Volterra (1913), Evans (1916) and Hildebrandt (1917). For results on integration he refers to his own (1918a and -b) and for probability methods he refers again to Poincaré (1912) and to Pearson (1895).

The idea of applying this machinery to “problems arising in statistical biology and statistical economics” was new but the problems seem to be out of somewhere. Alas, Daniell does not say where. His only clear reference is to “Gibbs’ Statistical Mechanics” which he mentions only to distinguish from his new “Dynamic Probability”; the reference may also have been intended to distinguish his work from what (he thought) Wiener was doing. There are no references for those “many problems” or enough detail to identify them. References come to mind, e.g. Bachelier (1900) to match the allusion to the behaviour of prices–Daniell (1921c, p. 144)–or Pearson (1905) and Pearson & Blakeman (1906) to match the notion of time-variation of position. These references may have been in Daniell’s mind but they were not in the paper. Another possibility is that Daniell was influenced by conversations with Volterra who was interested in both biology and economics.

Daniell finishes by saying, “The author hopes to obtain some interesting results by an extension of the analysis to two or more dimensions.” However he did not follow up the paper. Maybe it is not surprising that the paper fell dead-born from the press: it seems to belong to that category of applied mathematics in which unconvincing applications are found for not very interesting mathematics. “Observations” is a much better production and one can speculate whether it would have fared better if Daniell had sent it to *Biometrika*. “Integral products and probability” might have established itself in a branch of the applied literature if Daniell had refocussed on one of the “problems” and done it properly; cf. Chapman (1928).

Why did Daniell write the unnoticed papers? There may be a clue in the list of Rice courses for the academic year 1921-22 that appeared in the *BAMS* (January-February 1922 p. 76). One of Daniell’s courses is “Statistical economics.” There is no information about what the course contained–or whether it was ever given. One possibility is that it contained applications of “dynamic probability” and another is that it considered the statistical analysis of economic data, what was later called econometrics; at the time the connection between modelling a random process and statistical analysis was not appreciated in economics. Complementing Daniell’s course was one by Evans, “Theoretical economics, mathematical treatment.” Evans was interested in mathematical economics and stayed interested, producing several articles–among them (1922, -25)–and a book (1930). Bray was inspired to write a paper (1922) and Evans (1925, p. 108) reports that Bray did some statistical work inspired by Irving Fisher’s distributed lag analysis. In the early 20s mathematical economics was certainly in the Texas air. At the end of the last section I mentioned how from the late 20s Americans looked to England for modern statistics. An aspect of this was the recruitment of statisticians from England. The most significant relocation, of Neyman from London to Berkeley, was engineered by Evans after he gave up on R. A. Fisher–see Reid (1982, pp.

141-8). It now seems ironical that in the 30s Evans should have sought to bring outsiders to Berkeley when in the 20s a Rice colleague had done such outstanding work.

Shafer and Vovk (2006) raise a number of questions about Daniell's non-contribution to the development of probability. They (p. 80) argue that Wiener was "in a better position than Daniell himself to appreciate and advertise their [the Daniell articles] remarkable potential for probability Having studied philosophy as well as mathematics, Wiener was well aware of the intellectual significance of Brownian motion and of Einstein's mathematical model for it." This may be true but I can see no evidence for it in what Wiener wrote at the time. Indeed I suspect that Shafer and Vovk are bringing forward to the 1920s the Wiener of the 1940s, of *Cybernetics*: there is no sign of any philosophical concern with probability in the writings on logic and philosophy of science that Wiener published in his early years. It seems likely that Daniell with his German physics background and papers on special relativity would have known about Einstein's other work of 1905. On the other hand, when Borel (1912/15, p. 181) discoursed in Rice on "Molecular theories and mathematics" and described Perrin's likening of the path of the particle undergoing Brownian motion to a continuous curve possessing no tangent he did not mention any German work. I do not think that there was anything obvious, natural or inevitable about the development of Wiener's theory of Brownian motion.

Shafer and Vovk (2006, p. 88) raise a similar question about Daniell and Wiener on the one hand and Kolmogorov on the other: neither Daniell nor Wiener "undertook to make probability into a conceptually independent branch of mathematics by establishing a general method for representing it measure-theoretically." This is very evidently true. I cannot see any evidence that Daniell thought there was any problem with probability; the only probability reference he gave was Poincaré (1912) and there is no sign that he was acquainted with any of the literature concerned with deep problems described by Shafer and Vovk or Plato. Daniell applied his fundamental reasoning not to the foundations of probability but to higher level concepts such as the characteristic function. The idea of the "integral products" paper was to introduce into non-physical science a form of probabilistic reasoning as rigorous as that used in physical science. Wiener was not familiar with modern work in probability either but the relationship he established with Lévy gave him an entry into modern French probability.

What influence did Daniell's papers have on the development of probability? The invisible Daniell (1921c) obviously had none. The other papers had an influence only through Wiener. Their indirect influence was limited for, as Plato (1994, p. 134) notes, Wiener's theory "did not succeed in having influence before the 1930s." Wiener's practice in referring to the major papers was to emphasise

Daniell (1918a) but to only mention Daniell (1919c). Lévy (1925a and -b) cites Wiener's papers and along with them Daniell (1918a) but not the (1919c). Of course, Lévy and Wiener formed an association, a successor to the old pre-war association between America and Europe based on functionals between Evans and Volterra. In 1922 Wiener again visited France. In "Differential-space" he (1923, p. 132) writes, "The present paper owes its inception to a conversation which the author had with Professor Lévy in regard to the relation which the two systems of integration in infinitely many dimensions—that of Lévy and that of the author—bear to one another." Daniell has no place in the "creating modern probability" studies which concentrate only on Kolmogorov (1933) and his sources—Plato (1994) or Bingham (2000)—for Kolmogorov does not mention Daniell, or Wiener for that matter. Shafer and Vovk (2006, p. 88) describe how Kolmogorov (and Jessen) produced essentially Daniell's (1919c) result and discuss how Kolmogorov came to know of Daniell's work. Daniell (1919c) is noted in the first American publication to register Kolmogorov's work; Dobb (1934, p.760) compares Daniell's argument with Kolmogorov's. Doob also discussed Daniell's work in his retrospective piece (1990). Daniell's very belated entry into the British probability tradition is considered in §11 below.

9 Return to England: Sheffield

After the hectic years 1918-21 Daniell published nothing in 1922 and only some book reviews in 1923. From the previous silent period, 1916-17, had come the new career as an analyst but this period seemed to mark the beginning of the end of Daniell's career as a creative mathematician. At Rice Daniell had a productive relationship with Evans and Bray and he was made a full professor in 1920. Yet when Wiener applied for a chair in London in 1922 (letter (1) in the Appendix below) Daniell told him that he "nearly put in for the job myself." Daniell did his best to discourage Wiener with a description of the role of a professor in England, as a "very active & sometimes autocratic head of a department," and a warning, "you would be rather loaded down with work and would most likely get stuck in a rut." Daniell was probably recalling the situation at Liverpool but the warnings seemed to pre-figure his own professorial future in England. Yet, knowing the drawbacks, Daniell moved to the University of Sheffield as Town Trust Professor of Mathematics in 1923.

Ten years before Daniell had left Britain as a mathematical physicist and now he was back as an analyst. He must have been something of an outsider for his publications were nearly all in American journals and there was little local interest in his brand of analysis; that, at least, seems to be the lesson from Young's career (see Grattan-Guinness (1972) and Hardy (1942)). Even so, in 1922 he was awarded

a Cambridge ScD, a recognition of achievement “more or less equivalent to being proposed for the Royal Society” in Harold Jeffreys’s estimation. Of course Daniell’s contemporaries had moved on too and Daniell stood somewhere between Darwin who was elected FRS in 1922 and appointed to the chair of Natural Philosophy at Edinburgh in 1924 and Berwick who became professor at Bangor in 1926.

The University of Sheffield presented itself very differently from the Rice Institute, or even Wiener’s “Tech.” The entry in the Empire Universities Handbook of 1924 states:

The city being an important centre of steel, electro-plate, glass, and other manufacturing industries, and in the heart of an extensive coal-mining area, students of Engineering, Metallurgy, Mining, Fuel Technology and Glass technology have exceptional opportunities.

Sheffield was the most important centre for steel manufacturing and the symbol of the British steel industry—Mathers (2005) calls her centenary history of the university *Steel City Scholars*. The university was a more technical, smaller and poorer version of the University of Liverpool. The immediate outlook was not good: Mathers (2005, p. 85) relates a warning from 1921, “the Vice-chancellor of Leeds ... reportedly said he would not lay ‘heavy odds’ on the survival of the universities of Leeds or Sheffield.”

Daniell replaced the retiring professor, A. H. Leahy, appointed in 1892 in the days of the university college-like Carey of Liverpool. The staff of the mathematics department comprised a professor, a lecturer (already C. A. Stewart), two assistant lecturers and a lecturer in engineering mathematics. The Sheffield department was about half the size of the Rice and Liverpool departments. The students mainly came from outside mathematics: the number of students graduating each year in mathematics in the period 1923-40 fluctuated between 0 and 6. There was no call for courses on topics like “statistical economics” or “integral equations.” Although British universities began awarding PhDs in the 1920s, there were few in mathematics outside Cambridge and Oxford and none were awarded in Sheffield until after the Second World War. Daniell’s situation was no different from that of most mathematicians at the civic universities and fundamentally no different from Burnside’s who had taught applied mathematics to naval cadets at the Royal Naval College while he worked on group theory.

Daniell had one American publication in the pipeline—his (1924)—and he took some time to establish himself in Britain. He resumed publishing in the integral/derivative field but now instead of the torrent of publications of 1918-21 there were only the (1926a), (1928a) and (1928b). The 1928 papers were the last from the integration programme he had embarked on a decade earlier. Daniell was now sending his papers to British journals and he began to play a role in the London

Mathematical Society, the national society for mathematicians. He served on the Council between 1927 and 1932 and was a Vice-President from 1929-31. Daniell's willingness to take on jobs and his conscientiousness in carrying them out are characteristics that Stewart emphasises in his life. Daniell was in contact with other mathematicians and must have talked to them but there is little trace of these interactions. I have found evidence of interaction in one paper by Littlewood who (1931, p. 164) writes, "Professor P. J. Daniell recently asked me if I could find an example of a function of two variables of bounded variation according to a certain definition of Fréchet, but not according to the usual definition." On the next page Littlewood acknowledges that in writing the paper he had "profited by a discussion" with Daniell.

In Sheffield Daniell formed no productive relationships with his mathematical colleagues like those he had with Evans and Bray. There was some staff turn-over and by 1939 the mathematics department had grown a little: it had a staff of 6, one more than in 1924. Daniell appointed some excellent people, especially towards the end of his tenure. The best known was Richard Rado (1906-1989) appointed in 1936. Rado came from Berlin and Göttingen via Cambridge. His interest was in number theory and he was a long-term collaborator with Erdős. Leon Mirsky, an algebraist, was appointed in 1942. There is no evidence of interaction between Daniell and these younger colleagues: he is not mentioned in the memoirs of Rado by Rogers (1991) and of Mirsky by Burkill et al. (1986).

While Daniell did not literally work on the "design of blast furnaces", he certainly did some Sheffield-centred research. Stewart (p. 78) says that he assisted on the "heat-conduction problems arising in the manufacture of steel." There are no publications that can be linked to this work but there were two connected with the safety of mines. Sheffield was the home of the Safety in Mines Research Board headed by R. V. Wheeler, Professor of Fuel Technology; Chapman, (1955, pp. 305-7) and Mathers (pp. 118-9) describe the activities of the Board which was an important part of the university. Daniell contributed to a discussion in a mining engineering journal in 1926 and in 1930 he published a substantial paper on the velocity with which flames are propagated. In this partnership with Wheeler Daniell seemed to play the physicist to Wheeler's chemist. Daniell was trained as a physicist but he had never published anything like this before. The 1915 study of end-correction was the closest but this was more applied mathematics than physics. There is an assessment of the flame research and a comment on its lack of impact in *Combustion and Flame* (1958, p. 203):

The important work of P. J. Daniell in 1930, resulting in the first solution for the energy balance equation in flames, including heat losses but not diffusion, is noted in this chapter. This work has largely been ignored but some of the predictions arising from it have been found to

agree with experimental results of recent years.

Stewart (1947, p. 76) knew Daniell as “a prodigious reader of scientific journals [who] was conversant with the latest developments in Physics, Chemistry and Biology as well as those in most branches of pure mathematics.” There is a string of publications, (1926, -27, -29), best described as miscellaneous. They are single-shot reactive works in which Daniell makes an interesting observation on somebody else’s work without following it up or participating any further in the other’s research programme. Daniell was probably the same prodigious reader in his Rice years but the implication Stewart drew did not apply to the younger man and his activities in Texas, “As a consequence of this dispersion of interest, he seldom gave his undivided attention to the systematic development of particular lines of research.” The *American Journal* papers also reflected dispersion of interest but that did not stop them being sustained pieces of work.

In 1928 Wiener was again wanting a reference from Daniell. Replying to Wiener—see letter (2) in the Appendix—Daniell mentioned Wiener’s research and lamented “It’s quite time I did some work myself but a Chair in England involves a great deal of business which is done in America by the office.” Stewart (p. 79) records Daniell’s role in the life of the university and his university offices are listed by Chapman (1955, *passim*): Dean of the Faculty of Science 1934-7 and Member of Council 1934-7 and 1944-6. Stewart (p. 79) describes how his “interests extended beyond the university, however, in many directions—to the training of teachers, for example, to the Mathematical Association, and to the School Certificate Examination.”

Stewart does not offer any explanation of why Daniell stopped publishing in the 1930s and presumably stopped doing his own research

Much of his time and energy was expended in advising and assisting research workers in many fields and it was only on rare occasions that he troubled to make a permanent record of his own contributions to the problems involved.

I have not been able to find many instances of acknowledgments in the writings of others but there is an interesting example in Bradford (1932). An instance of his leaving a permanent record of his own was his paper (1929a) on correlation coefficients. This paper, published in the *British Journal of Psychology*, treated “Boundary conditions for correlation coefficients” and was a follow-up to a paper (1928) by his colleague, the psychologist J. Ridley Thompson. Thompson belonged to the camp of Geoffrey Thomson, Spearman’s great rival and critic in the field of factor analysis. However, neither Daniell’s paper nor Thompson’s become part of the literature. They are not mentioned in the papers by Thomson and Ledermann

that appeared in the first volume of *Psychometrika* in 1936. Daniell seems to have perfected the knack of not being noticed.

Daniell was also a great reader of books and in England, as in America, he wrote reviews; these inevitably covered a wide range of subjects. He reviewed several books on calculus and introductory analysis and a persistent theme in his reviews was the importance of a “conscience” in presenting the basic concepts and the need to escape from the “rubbish” and “poison” contained in the old works. There are no new ideas in these reviews but there are some interesting reflections and reactions and I will retail three of these. In his review of two works on set theory by Fraenkel Daniell (1929c, p. 581) described his own attitude to the foundations of mathematics:

At the present moment Mathematics is almost as insecure in its foundations as is Physics. There are several schools of opinion, typified by Russell, Brouwer and Hilbert respectively. All of these deserve a sympathetic hearing but none quite satisfy us.

The reviewer’s personal opinion is that the mathematicians too often attempt to attain a security which no other sciences except Physics have possessed and which even that science has now been compelled to give up. Mathematics is a doing, not a knowing. Let us remember the fate of the great reptiles and put our trust, not in armour but in the most agile and persistent intelligence.)

Daniell appears as an interested and well-informed but ultimately sceptical spectator on the debate on foundations.

Daniell reviewed one book on probability, Erhard Tornier’s (1936) curious book on probability theory and general integration theory. Tornier was a follower of von Mises in probability and a leading advocate of “Nordic mathematics.” Shafer and Vovk (2006, *passim*) and Plato (1994, pp. 193-4) discuss his work. Daniell (1937b, p. 67) begins with a warning:

Let no one buy this book hoping to obtain an account of Probability as it is usually understood. More than half the book is devoted to an abstract theory of the measure of sets of a general character and of the corresponding integrals.

Doob (1937, p. 317) began *his* review with the observation, “In the last few years the theory of probability has been more and more influenced by the modern theories of measure.” It is clear that Daniell was not aware of this development. More remarkably Tornier does not refer to any of this literature although his book is an attack on the identification of probability with Lebesgue measure.

Doob does not mention Kolmogorov by name but his review can be read as a defence of Kolmogorov against Tornier, of Lebesgue measure against Jordan content. Daniell does not get into these issues and indeed his objection to Tornier's treatment of probability proper seems as applicable to Kolmogorov's *Grundbegriffe*:

Professor Tornier has apparently never heard of J. M. Keynes or other critics of fundamental notions. He writes glibly of obtaining probability in some cases "näherungsweise", though he does not say how this can be done without a circular use of Bayes' formula or else by the often disproved limit theories, such as Venn's. Professor Tornier merely assumes that the probability is a number satisfying the postulates for a general mass.

It appears from the review that Daniell either did not know about contemporary Continental work on probability or did not take it seriously.

Finally, I will quote some remarks on different approaches to integration in Daniell's (1938, p. 198) review of Kestelman's 'measure first' approach:

I prefer to follow W. H. Young in regarding the Lebesgue integral as designed specifically to ensure that for bounded sequences

$$\lim \int f_n dx = \int \lim(f_n) dx.$$

The measure of a set is then simply the integral of the characteristic function of the set. However, it must be admitted that general practice and public opinion are against the reviewer.

Daniell adds, "It is always difficult in mathematics to decide which is the cart and which is the horse."

Stewart (p. 75) reports that shortly before the war "the state of [Daniell's] health caused some anxiety, but he recovered, though not it would seem completely." In 1940 Daniell started publishing again in analysis but not in the abstract style of the major papers but in the concrete style of works like Bromwich (1908) which were part of his youth.

10 The Second World War, fire control and death

The Second World War brought more teaching and a new research mission to mathematics at Sheffield. Student numbers in science and technology increased

as the state encouraged recruitment into these subjects and in 1942 the summer vacation was turned over to teaching; Mathers (pp. 130-4) describes the effects on Sheffield. The paragraph in Chapman (1955, p. 495) on the war work of the mathematics department describes how the design of fire control systems requires the solution of “very complex mathematical problems” and how “it was one of the functions of the Department of Mathematics to contribute to the solution of these.” This was Daniell’s work and it took him into different areas of mathematics and new professional networks.

Daniell’s activities fall under two headings. He acted as a kind of intelligence officer where his habit of voracious reading must have been very useful. He discovered works by Bode, Nyquist and Wiener and translated them so that British engineers could understand them—see Daniell (1943a, 1944b, 1945b). Bennett (1993, p. 136) reports that the local engineers did not know about frequency response methods and learnt about the Nyquist diagram from Daniell. Apart from this role in intelligence, Daniell was the mathematician partner in several research projects. Daniell’s two main associates were Arnold Tustin (1899-1994), an engineer employed in industry, and Arthur Porter (b. 1910), a physicist just starting his career. By the end of the war there was a distinct group within the larger engineering community interested in the study of “servo-mechanisms.” Tustin and Porter went on to become academic engineers, pioneers of “automatic control” as the study came to be called. There is a valuable account of wartime activity in Britain, based on once-secret documents and on interviews with the leading figures in Bennett’s *History of Control Engineering 1930-1955* (1993). Daniell appears at several points in the book.

Arnold Tustin (1899-1994) was an engineer at the Sheffield works of Metropolitan-Vickers a big engineering company which had just begun to be interested in the control of naval guns on ships; see Bissell (1992) for Tustin’s recollections. Tustin and Daniell developed a way of handling nonlinear control problems based on Daniell (1945); it is described by Bennett (1993, p. 154). When Tustin wrote up the method he (1947, p. 143) acknowledged Daniell’s contribution:

The general method is based on unpublished work of the late Professor P. J. Daniell, who provided an analytical treatment of the effect of backlash of which the present paper is essentially an interpretation in geometrical terms.

The publication being in Tustin’s name, the method came to be associated with him alone. The particular form of quasi-linearisation was developed independently by several engineers; see Gelb & Vander Velde (1968) for a survey.

Arthur Porter was a physicist, who as a student in the early 1930s had worked with Douglas Hartree on a version of Vannevar Bush’s differential analyser. Just

before the war Porter spent time with Bush at MIT. During the war Porter was a civilian employee at different defence research establishments; for his recollections see Porter (2004). Porter was an important figure in the “Servo Panel” set up to exchange information between all the agencies concerned with control problems; it operated like an academic society with meetings of a hundred or so at which papers were read. Porter has told me that Daniell attended “religiously” and that he presented at least one paper. In his notes on the history of the panel Porter (1965, p. 331) describes Daniell’s activities:

Early in 1942, the late Prof. P. J. Daniell, Professor of Mathematics at Sheffield University, was asked to study the problem of filtering radar information, with special reference to the automatic-tracking problem.

Daniell’s subsequent contributions to servo theory, although not widely known because his reports and memoranda were security classified, were of high significance. Indeed, it is probable that Daniell was the first man in Europe to ‘translate’ Norbert Wiener’s work on the interpolation and extrapolation of stationary time series, which in turn formed the mathematical basis of Wiener’s ‘cybernetics’. Daniell’s interpretation of the early Wiener papers on control theory are refreshingly elegant and make a noteworthy contribution to the evolution of control-systems engineering in Britain. During the period 1941-43, Daniell collaborated with J. G. L. Michel and myself in optimising networks for radar-tracking systems—the Manchester differential analyser was used in these studies.

Porter states that the security classified papers were issued “with strictly limited (50 copies) circulation.”

Wiener’s wartime activities in the field of fire control are described in his (1956, pp. 240ff) memoirs; see also Bennett (1993 ch. 7 and -94). The most important product of Wiener’s work was a classified report published as a book in 1949, *Extrapolation, Interpolation and Smoothing of Stationary Time Series*. Daniell produced a “digest” of this work, Wiener (1942/1949). I have not been able to see a copy of this work but Bennett (1994, p. 180) did and quotes from it: “any future theory of statistical fluctuations and of prediction problems will certainly build on the fundamental ideas in this manual.” The task of writing the manual does not appear to have involved Daniell in any direct contact with Wiener. From his side Wiener later (1949, p. 16) recalled how “the ideas of prediction theory and the statistical approach to communications engineering [were] familiar to a large part of the statisticians and communications engineers of the United States and Great Britain” and how Daniell was among the authors of expository papers.

Bennett (1993, p. 190) recounts how at the end of the war there was an explosion in servo publications. Daniell was commissioned to write a book on control theory to be called *The Theory of Closed-Cycle Systems*, according to Porter (1950, p. 147) In the event Daniell died before he could finish it and only two expository papers (1943 and -44a) were ever published.

Stewart (1947, p. 75) describes Daniell's last illness and death:

The strain of the war years became evident during the summer of 1945 when he was attacked by serious heart trouble. He recovered to some extent and decided to undertake the work of the session 1945-1946, but there seems little doubt that his life would have been prolonged if he had made a different decision. He continued with his many activities in a spirit of great fortitude and determination, but early in May, 1946, he collapsed at his home and died a few weeks later without fully recovering consciousness.

11 Daniell in the British probability tradition(s)

There is one last contribution from Daniell to consider, a product of his work on fire control commemorated in the eponym, the “Daniell window.” That will lead into a discussion of Daniell in the British probability tradition and a last look at Kendall's injunction, “But you have to remember P. J. Daniell.”

The servo panel and the lives of Tustin and Porter illustrate how the war created new networks and redirected careers. The mobilisation of statisticians had similar effects as established statisticians were directed to war work and beginning mathematicians—among them David Kendall—were diverted into statistics. Daniell came into contact with two established statisticians, Henry Daniels (1912-2000) of the Ministry of Aircraft Production and—through him—with Maurice Bartlett (1910-2002) of the Ministry of Supply. In his peace-time job Daniels had used order statistics—see his (1945)—but he probably knew nothing of Daniell (1920). It was through Bartlett and time series analysis that Daniell first became a name in British probability; for Daniels see Whittle (1993) and, for Bartlett, see Bartlett (1982), Olkin (1989) and Whittle (2004).

The statisticians' society, the Royal Statistical Society, had a tradition in time series analysis though its best days were in the 1920s when Yule was still active. But interest in time series analysis was picking up and a symposium on “auto-correlation in time series” was the first great post-war occasion of the society's Research Methods Section. Among those who presented papers in January 1946 were Bartlett and two wartime colleagues of Daniels, the physicists Cunningham and Hynd. In his paper Bartlett (1946, p. 29) referred to an unpublished note

by Daniell on “Sampling errors of the lag-covariance of fluctuating series” from which he quoted a “useful result.” (Characteristically Daniell commented, “This he kindly attributes to me, but I imagine it is well known.”) Daniell contributed to the discussion, though necessarily in writing for he was already ill. His message began, “My absence from this symposium is a grief to me. The subject is very important and interesting, and I send the following notes.”

Unlike his 1920 excursion into statistics, this one had some impact and some background is necessary to see why. In 1937 Cramér’s *Random Variables and Probability Distributions* had appeared and the society’s traditional apathy, if not antipathy, towards Continental work on probability was beginning to change. Although Bartlett (1938, p. 207) found Cramér’s approach, which derived from Kolmogorov, difficult, he admitted that “from the point of view of mathematical analysis it is a fairly natural and logical one.” Maurice Kendall (1907-1983), another important figure in the revival of British time series analysis, used Cramér’s book when he wrote his *Advanced Theory of Statistics* (1943). In 1938 came *Stationary Time Series* (1938) by Cramér’s student Herman Wold; this made known the work on stochastic processes of the Russian school and of Khinchin, in particular. Comparing Bartlett’s 1946 paper and his earlier piece on the “time-correlation problem” (1935) the Continental influence is clear. Bartlett knew of Wold and he learnt more about Continental work from a wartime colleague, J. E. Moyal, who had studied in France—see Bartlett (1998). Bartlett had become so interested in probability that he was projecting a book; this finally appeared in 1955 as the *Introduction to Stochastic Processes: with Special Reference to Methods and Applications*. The book had a very British slant which made it an entirely different work from Doob’s *Stochastic Processes* (1953). It recognised the “important theoretical contributions” made by “American, French, Russian and Swedish writers” (p. xiii) but its heart was in the “methods and applications” for which it looked to Fisher on genetics, Yule on time series, McKendrick on epidemics, ..., and Daniell on estimating spectra.

Daniell (1946, p. 88) begins his three pages of notes with what is implicitly a claim for his own lineage

The work done in America has been based on a fundamental study by N. Wiener of integrals in an infinite number of dimensions, corresponding to the values of the fluctuating quantity at various instants. The work is not behind that of the Russian school in time or importance.

Wiener’s classified research was not known to the statisticians and, while his earlier generalised harmonic analysis (1930) was known—cf. Wold (1938, pp. 16-17) and Bartlett (1946, p. 32)—it was not influential. At this time British statisticians (and engineers) were unaware of Kolmogorov’s work on prediction and Khinchin

personified the Russian school: Kolmogorov's work is summarised by D. G. Kendall (1990, pp. 36-7).

The first part of the notes advertise the power of Laplace transform methods as Daniell reconsiders some of the results obtained in the papers under discussion. The last two pages treat the problem of estimation, both for autocorrelations and for the "spectral intensity." This function figured both in the approach of Khinchin and Wold and in the approach of Wiener. Daniell's principal originality was in broaching the problem of estimating the function and in indicating the difficulties of the task. He considered the properties of the periodogram as an estimate of the spectral intensity; he showed that it is not a consistent estimate "the variance of [the estimate of the intensity] is always greater than the square of its theoretical value" however great the sample size. His proposal was to average over neighbouring values of the frequency. Later his solution acquired the name "Daniell window" for by then, of course, there were other ways of "smoothing the periodogram" as Bartlett (1950) described it. The eponym does not do justice to Daniell's contribution: he framed the problem and laid down the requirements for a solution—to be followed by Bartlett among others. See Priestley (1981, pp. 440-2) for a textbook presentation.

This time the leading local expert understood the significance of Daniell's contribution and promoted it and so Daniell (1946) had a different fate from that of the much more substantial Daniell (1920c)—see §7 above. The 'unnoticed papers' were never canonised although they are known to historians through Stigler (1973) and are a 'small type' presence in at least one scholarly textbook—see Dudley (2002, p. 149). While Daniell made a single very effective 'visit' to the statisticians, the London Mathematical Society was *his* society and the community it served his community. It took a long time for the "major papers" to be canonised in British mathematical probability—years after Daniell's death and decades after they were written. To understand the canonisation process it is necessary to pick up the British sequel to the events summarised at the end of §8 above.

The major papers could be considered for inclusion in the British probability story only when the view of probability as a branch of analysis took hold. In Daniell's time the British mathematical community recognised probability only as applied mathematics; matters, of course, were different across the Channel. The tradition went back to Maxwell's work on the theory of gases; there is some discussion of the state of this tradition in the 1920s in Aldrich (2006). Examples of probability in applied mathematics that appeared in the LMS *Proceedings* ranged from Taylor (1922) on turbulence to Fisher (1929) on the moments of sampling distributions. Fisher, the leader of the biometry-statistics school that came out of applied mathematics, loathed "academic" probability yet produced work on stochastic processes that impressed Kolmogorov and Feller. In the following

decade there was some movement towards probability on the part of Hardy and Littlewood the leading pure mathematicians. Von Plato (1994, pp. 46-60) emphasises the interplay between probability and number theory in the work of Borel and Khinchin but the English number theorists came to probability much later. Hardy never quite came to probability: Diaconis (2002) prefaces his account of the relationship between Hardy's research and probabilistic number theory with the remark that Hardy had "a genuine antipathy" towards probability. Yet Hardy encouraged Cramér, another number theorist, to write the 1937 tract and after the war he wanted Moyal to write one on stochastic processes (see Bartlett (1999, p. 273)). Littlewood was more of a participant, collaborating with Cyril Offord (1906-2000) on random algebraic equations; see Littlewood & Offord (1938). Offord continued to work on probability after the war; see Hayman (2002). All the time there was a second route to probability through Wiener, the overseas member of the Hardy-Littlewood school. "The mathematician who was to have greatest influence on me in later years" was the description Wiener (1953, p. 183) used when he recalled his first meeting with Hardy. But Hardy showed little interest in Wiener's probability work: as Segal (1992, p. 394) notes, "With Hardy's support, Wiener was to become better known for his work on Tauberian theorems than for his earlier and probably more innovative work on Brownian motion." However two mathematicians trained by Hardy and Littlewood went to work with Wiener and became involved with probability. The first was the prodigy Raymond Paley (1907-1933) whose work with Wiener—written up after Paley's death by Wiener as Paley and Wiener (1934)—had significant probability content. The second was Harry Pitt (1914-2005) who worked with Wiener on Fourier-Stieltjes transforms—see Pitt & Wiener (1938)—but who turned to probability after the war.

'Pure' probability only really became established after the Second World War. Bingham (1996, p. 185) makes a strong case for identifying the "effective beginning of the probability tradition in this country" with David Kendall (1918-2007): one consideration is that Kendall's pupils and his pupils' pupils are "everywhere." Herman's pupils and his pupils' pupils had been everywhere but Kendall's pupils were PhDs doing probability. As noted above, the war took Kendall into statistics and applied probability; Bartlett (again) was the important link with the statistical community. Bartlett and Kendall contributed to the RSS symposium on stochastic processes (1949) with Kendall talking on stochastic models of population growth. Kendall gradually moved from applied probability to pure probability so successfully that by 1961 he was acceptable to the Cambridge mathematicians as the first professor of Statistics! One of his pupils, J. F. C. Kingman, became president of both the LMS and the RSS. Perhaps Daniell's time to be considered for inclusion in the British probability tradition came in 1968 when the "Daniell integral" and the "Daniell-Kolmogorov extension theorem" appeared in Kingman and Taylor's

Introduction to Measure and Probability. Daniell is the only 20th century British mathematician to do figure in the book but he may as well have been a contemporary of Bayes for all the personal connection there was: he was known through books, books from abroad, like Bourbaki (195?), Doob (1953) or Loomis (1953). Doob's book was the first to bring together measure and integration and the work of Kolmogorov and Wiener. It provided the landscape which gave meaning to the question, how does Daniell fit in?

In the mid-90s when David Kendall was reminiscing "P. J. Daniell of Sheffield" had a double ring for Sheffield had also become a name in probability. In 1955 Daniell's Department of Mathematics was divided into departments of Pure Mathematics, Applied Mathematics and Statistics. Statistics took off in 1965 with the appointment of Joe Gani, an applied probabilist, as professor; see Mathers (p. 216) and Heyde (1995, pp. 221-3). Gani built up the largest probability and statistics group in Britain and founded 2 new probability journals. The new formation, however, had no links to Daniell and neither had the Department of Automatic Control & Systems Engineering which was created in 1968 from the Department of Applied Mechanics. This too became a leading centre of its kind in the country. Just as the major papers were not probability so Daniell's Sheffield was not the modern Sheffield.

12 The real Daniell?

Daniell has turned out to be much more complex than the man who gave up probability to design blast furnaces. Daniell went through a number of surprising transformations and seemed in the middle of another—to control theorist and time series statistician—when he died. This long search for Daniell has taken us through his different careers and presented the intellectual communities he belonged to—or quite often did not quite belong to. Something has been learnt but some mysteries remain.

It is possible to answer Kendall's question, who taught him? At King Edward's school Daniell was taught by Charles Davison, at Cambridge he coached with Robert Herman. Coaching with Herman put him in the company of many of the most distinguished British mathematicians of the period. He only did Part 1 in mathematics but at the time Cambridge offered nothing of any use to the author of the major papers. But Kendall probably meant, who started him in research? Whether anyone guided him in the research for the Rayleigh Prize is unknown—probably not, given that he was in Liverpool. Lovett may have thought that a year in Germany would train Daniell in research as well as fill gaps in his knowledge; it turned out so for Daniell teamed up with the 'post-doc' Ludwig Föppl, with Hilbert and Born in the background. This was training, of course, for a career in theoretical

physics, not in pure mathematics. Looking at his relativity papers there is no sign that he had acquired any useful analysis. The ideal teacher of the future analyst would have been W. H. Young who happened to be in Liverpool at the same time as Daniell. However that was some years before Daniell needed him. Daniell followed Young in that he paid him more attention than French and German authors did but whether he followed Young in any more significant sense is not known. Another possible teacher was his pure mathematician colleague, Griffith Evans. Most likely he was his own teacher. Hassé's remark quoted earlier, "The *real* mathematician ... will survive the effects of any teaching and of any syllabus," may express a general truth but it had particular application to the mathematicians of Daniell's generation—real mathematics was something they taught themselves.

In Daniell's case a pertinent question is, who did he work with? Daniell published only one joint paper—with Föppl—and yet involvement with others seemed essential to his productivity. One great difference between his productive and unproductive years was the presence of interested others—Evans and Bray in the 10s and Tustin and Porter in the 40s. Even in his inactive period Daniell was "advising and assisting research workers in many fields." Daniell seems not to have developed any productive relationships with mathematicians at a distance, although the absence of personal papers makes it difficult to be sure about this. There were the relationships that did not quite happen: the most obvious one is the one with Wiener. But there is no personal data to help explain why.

The transformations were big and remain mysteries. Why did Daniell change from theoretical physics to pure mathematics, why did his research virtually stop in 1930 and then pick up again in 1940? Stewart does not go into causes and reasons. It is frustrating to know so little of Daniell when we know so much of his most famous other—Wiener. Wiener's confessions (1953 and -56) cover not only actions and motives but even ways of thinking. His (1956, pp. 167-70) account of working with the pure mathematician Raymond Paley contains some nice reflections on pure and applied mathematics:

I saw as my habit, a physical and even an engineering application, and my sense of this often determined the images I formed and the tools by which I sought to solve my problems...

One interesting problem which we attacked together was that of the conditions restricting the Fourier transform of a function vanishing on the half line. This is a sound mathematical problem on its own merits, and Paley attacked it with vigor, but what helped me and did not help Paley was that it is essentially a problem in electrical engineering.

Daniell has left us no corresponding self-analysis but Stewart (1947, p. 77) has some observations on what motivated his work in pure mathematics:

In spite of the very theoretical character of all this work, there was always behind it the background of physical ideas.... In his generalisation of Green's theorem ... the concept of a boundary as the boundary of a set, measurable Borel, and the use of functions of limited variation representing mass or electrical charge provided for him a true representation of the physical reality.

However it seems that his pure mathematics work was about enabling applied mathematics by establishing certain abstract constructs: when he did applied mathematics he generally did not use his pure mathematics: unlike Wiener who took Lebesgue integration to engineering.

Daniell's inner life remains a mystery but how he appeared to others is not. Stewart (p. 79) recalls:

Daniell impressed all who came into contact with him by his great integrity of character and his sincerity of purpose. ... He disliked publicity and his tastes were simple. He delighted in good music, in books, in friendly discussion, in country walks and in the quiet pleasures of a happy family life.

In October 2006 I talked to Arthur Porter, now retired in North Carolina, and over the phone he told me how he had "very fond memories" of Daniell and among the adjectives he used to describe Daniell were "charming, delightful, low key, modest."

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13 Daniell's letters

Daniell's papers appear not to have survived and I have found only six letters—all from Daniell and none to him. The letters have little scientific content but at least we hear Daniell's voice in a less formal setting than in his published papers. The letters also give some insight into his character. The most interesting letters are to Wiener and I have reproduced two of the three letters in the MIT library. Those letters are about jobs while the other letters reproduced are about meetings: to Mordell in 1944 about a missed meeting and to Porter in 1945 about a planned meeting. The sixth letter is the one mentioned in §5 above, to Sir James Frazer about a passage in the *Golden Bough*.

(1) This is the earliest of the three letters from Daniell to Wiener in the MIT archive.

Feb. 9 [1922]

Dear Wiener

You will be wondering whether I received your letter about the London chair. I wrote to them my opinion of your suitability for the position and it was partly praise and partly otherwise. That is to say I think highly of your promise as a mathematician but—and, I naturally expect you to disagree with me on this—I feel that you have not yet attained as established a position & have not had as much experience as they try to get for such a position. A professor in England is not merely a man of professorial rank & ability—he is a very active & sometimes autocratic head of a department.

To tell the truth I think it very improbable that they would choose an American unless they had some very special reason for doing so—and I doubt it would be good for you if your application were successful. You would be rather loaded down with work and would most likely get stuck in a rut.

I nearly put in for the job myself & doubted if I had any chance but I didn't apply because I feel I owe Rice Institute something for the leave they gave me. Besides that, H. A. Wilson was once professor of physics in London & has a low opinion of it as fossilized and full of red tape & conservative politics. It may be quite different now.

It was no use to give you any pointers about applying because I am as much in the dark as you are. The various universities in England are all different in their ways of looking at such things. In this case just

a straightforward application would be best—of course Hardy’s recommendation if you managed to get that would have considerable weight. I’m not forgetting the fellow but Evans wants to know why Mass. Tech. doesn’t want him. Anyhow Lovett is away at present & I feel it would be better to bring up the matter next term when there is not so much excuse for postponing a decision.

Yours sincerely

P. J. Daniell

Thanks for reprints which interest me considerably.

For the “leave” see my note on letter (3). In March Daniell wrote to Wiener about “the fellow” from Mass. Tech. wanting a job at Rice. He seems to have been Alfred J. Maria who did indeed spend some time at Rice

(2) This is the third and last of the three letters from Daniell to Wiener in the MIT archive. In 1928 Wiener, now an assistant professor, had applied for the chair of Mathematics Pure and Mixed at the University of Melbourne. Again he used Daniell as a referee. Masani (1990, p. 94) describes how Wiener’s application was backed by strong supporting letters but still no offer came.

Aug 15th 1928

Dear Wiener

Altho’ I have put the Sheffield address we are away in the country in Derbyshire about 20 miles out, and having a very pleasant time.

I have written to the Melbourne people about you & at the same time about my senior assistant who is applying. I don’t know of course what kind of man they are looking for. These colonial posts often go in the end to ‘favourite sons’ in spite of the London committee so I shouldn’t count on it much if I were you.

I must congratulate you on your family. You don’t say if the ‘family’ is a son or a daughter.

I’ve noted the work you are doing on the quantum theory & on Almost Periodic Functions. It’s quite time I did some work myself but a Chair in England involves a great deal of business which is done in America by the office.

Yours sincerely

P. J. Daniell

I presume you are not going to Bologna. Certainly I am not but I wish I could have seen you when you were in this hemisphere.

A “favourite son” did get the chair: Tom Cherry was a Melbourne graduate and son of one of the professors; he was also a fellow of Trinity and the first Cambridge PhD in mathematics. Terry Speed, who was a student at Melbourne at the end of Cherry’s career, tells me that Cherry “was a formidable mathematician and teacher, though of course not as good a researcher as Wiener.” Speed is also sure that the decision in 1928 was not just a case of Tom Cherry being a local man, but also of Wiener being a Jew. In 1929 Wiener was promoted to an associate professorship and in 1932 to a full chair.

(3) This letter is in the Mordell Papers in St. John’s College Library. At the time of writing, Hadamard was in England and Mordell (third wrangler after Daniell) was President of the London Mathematical Society.

[Nov. 3 1944]

Dear Mordell

I should have come to hear Hadamard on the 16th if I could possibly have done so but I have to be at a joint JMB meeting in Manchester. If an opportunity should arise I should be very glad if you could convey my good wishes to Hadamard. he may faintly recall a great kindness he did me by giving me the right to use the Institute Library in Paris when I was on the staff of the Rice Institute Houston Texas & had a leave of absence. I remember his great interest in the wild flowers of Texas.

Yours sincerely

P. J. Daniell

The JMB (Joint Matriculation Board) was the local school examinations board and as, Stewart says (see §9), Daniell took his work for it very seriously. It is possible that Daniell was away from Rice for longer than attendance at the Strasbourg congress required and that the “leave of absence” refers to this prolonged stay. The letter is striking for the courtesy and consideration it shows.

(4) This letter is in the Public Records Office in the Servo Panel file. On this occasion Daniell is writing to Arthur Porter to organise Porter’s visit to Sheffield. “Metro-Vick” was Arnold Tustin’s employer.

[Jan. 1945?]

Dear Dr. Porter

On Thursday Feb 8 I have to be in Manchester although if this is to be the only possible occasion for our long discussion I could be absent from that meeting (Matric. Board). Otherwise except on Saturdays, I have lectures up to 11.30. If Mr. Tustin is to join us it might be better to meet at Metro-Vick since our refectory is very crowded and the lunch at Metro-Vicks is better, However we can get lunch here.

As far as I can see at present any day of that week except Thursday would do from 11.30 on into the afternoon as long as may be necessary. On Saturday I have no lectures.

The same applies the following week except that on Thurs Feb 15 I simply have to be in Manchester and cannot absent myself.

As for the 'Smoothing and Filtering' Technical Group no doubt Tustin will explain what is in our minds. To me it looks a very big problem and I may need advice from many people. Tustin is very stimulating and helpful to me and knows a lot of the practical side but if the group is to consider matters at all fundamentally I think there will arise such a question as the following:- disturbances other than those indicating true position must of course be smoothed out. The target must be followed but (1) are erratic but true accelerations of target to be counted as disturbances, and if so, why? (2) in what sort of way does the lack of certainty in prediction come in. Is it mainly due to the target appearing anywhere in this field and moving in any direction? If so my preliminary suggests that the time during which targets are to followed , on the average, is an essential element.

If all this relevant to the work of the group the experiences of many people such as your brother may be very useful. If I have misunderstood the object to be achieved there still remains this important matter to be discussed by some one because only in that way can on estimate the weights (statistical) to be attached to following versus smoothing.

As to backlash and Coulomb friction there are many openings suggested by the simpler approximations which I have been developing with Tustin's guidance.

Yours Sincerely

P. J. Daniell

This letter is striking for the modesty Daniell shows when referring to Tustin and the help Tustin gave him. Porter's wartime experience are described vividly in his autobiography(2004). Alas it contains nothing about Daniell.

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This list is based on that in Stewart (1947) with additions indicated by an asterisk. The classified wartime items to which Stewart had no access come—with two exceptions—from Bennett (1993). I have not been able to see copies of all the items. Stewart did not include book reviews and the references to those in the *Mathematical Gazette* were supplied by Stephen Stigler. Stigler also told me about the 1916 paper. The items from the *Rice Institute Pamphlet* series are available on the website of the Fondren Library, Rice University.

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