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Jessen's Theorem and Lévy's Lemma: A Correspondence

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Contents

1 Introduction

Jessen's theorem and Lévy's lemma, both of which date from 1934, are the earliest general formulations known to us of the martingale convergence theorem. (1). The theorem treats the almost sure convergence of regular martingales and is usually stated as follows: let (F_n) be a sequence of sub σ - fields of a probability space (Ω, A, P) increasing or decreasing towards a σ - field F, and let X, be an integrable random variable, then $E(X/F_n) \to E(X/F)$ a. s. and in L^1. It is recognised that the theorem had ancestors in two different frameworks: in Lebesgue's theory of integration there was his proof (1903) of the theorem about differentiation almost everywhere (2); in Borel's theory of denumerable probabilities there was his statement and proof by probabilistic arguments (1909) of the almost sure convergence of frequencies in the game of heads or tails—the first version of the strong law of law numbers (3). There were two complementary visions of the world, sometimes closely linked, sometimes resolutely antagonistic and thus in the image of their authors. These, we will see, would be the inspirations for Jessen and Lévy in the 1930s.

It happened that Lévy, who read little and badly, read (part of) Jessen's article [1934a], and presented it to Hadamard's Seminar in the spring of 1935. Lévy realised that his own results obtained by completely different methods resembled those of Jessen and a singular correspondence ensued, a kind of dialogue of the deaf between two mathematicians who conceived of mathematics in entirely different ways, who wrote it in languages without visible contact and yet sometimes understood better than they admitted. This exchange is perhaps one of the possible origins of the formulation and proof of the modern theorem, stated above and found in all treatises of probability theory since the beginning of the 1960s. The statement, written in the language of sequences of sub-fields of probability theory, appears for the first time in this form, in a famous 1946 article by Sparre Andersen and Jessen, which Doob included and developed in his great treatise of 1953 (4), and which one may say, without too much exaggeration, closed the quiet and surely forgotten conversation between Jessen and Lévy. As for the proof of this theorem given today, it is not really different from the extremely simple one that Lévy proposed to Jessen, in the case of increasing filtrations and of set indexes. It consists in supposing that the variable X is F-measurable and can thus be approximated in L^1 by a sequence of (simple) variables X_n, each F_n- measurable. The convergence in L^1 result then follows easily from the following inequalities, where we have (following Lévy) E^n for the conditional expectation operator given F_n :

$$\left|E^n(X)-X\right| = \left|E^n(X)-X_n+X_n-X\right| \le \left|E^n(X)-X_n\right| + \left|X_n-X\right|$$

$$\left\|E^n(X)-X\right\|_1 \le 2\left\|X-X_n\right\|_1 \to 0$$

For almost sure convergence, it is enough to consider an unspecified bounded stopping time, σ, and to write the same inequalities to show convergence in L^1 of $E^{\sigma}(X)$ towards X, along the filter of bounded stopping times and thus convergence a.s. (5). This theorem, whose proof has been reduced to its simplest form, now stands in the timeless and "pasteurised" ranks of university courses, but it took more than half a century to find its place true. The theorem contained nearly all the almost sure or almost everywhere results of the time; Birkhoff's ergodic theorem was one of the few exceptions. So it may not be without interest to recall the confused debates the result generated in 1935 when it had hardly emerged from the ocean of ignored, misunderstood or mislaid theorems.

It is well known that Lévy, who worked alone, liked to correspond. Moreover there is hardly any difference in style between his letters and his publications, long monologues delivered in a single breath as though he were reporting a film that was rolling in front of him without ever stopping. This is why one can learn much from reading his correspondence about the mathematical universe he inhabited. To be convinced, consider his letters to Fréchet preserved in the Archives of the Académie des Sciences, and issued in a remarkable volume published in 2004 (6).

Lévy's files were destroyed during the war but Jessen kept all his letters and they were deposited, after its death in the Archives of the Institute for Mathematical Science at the University of Copenhagen (7). Christian Berg, professor at the University of Copenhagen and former student of Jessen, has very kindly provided us with very readable copies of all of Lévy's letters and the drafts of Jessen's replies. We are infinitely grateful to him. This correspondence is published here with short notes after a quick introduction to the two protagonists. In 1934/5 Børge Jessen, born in 1907 and a student of Harald Bohr, was professor of geometry at the Polytechnic School of Copenhagen and already making a name among analysts. Paul Lévy, born in 1886 a student of Hadamard and Borel and professor of analysis at the Paris École Polytechnique, had been developing after his own fashion the modern theory of probability for the previous fifteen years. We also publish some later exchanges (from 1948-9) between Jessen and J. L. Doob and between Jessen and J. Dieudonné. These letters also come from the Copenhagen Archives and again we are indebted to Professor Berg for his help.

We also thank Glenn Shafer and Niels Keiding who put us in touch with Christian Berg and are thus the originators of this small contribution.

Christian Berg and Glenn Shafer have also very kindly provided us with copies of Jessen's Danish articles from 1929 to 1947 and these have helped us to reconstruct the genesis of the article of 1934. They have also read our manuscript and made very interesting suggestions which we have incorporated in this final version. We are infinitely grateful to them. Any novelty in this paper is entirely due to them.

We also give our warm thanks to John Aldrich who has undertaken the English translation of this paper and who has suggested numerous improvements.

2. Jessen's Theorem

The life and the work of Børge Jessen are described in Christian Berg's fine article reprinted in this issue of the *JEHPS*. Here we recall only some points needed for understanding what follows. We have mentioned that Jessen, in the course of his university studies of mathematics in Copenhagen, embarked on research under the direction of Harald Bohr (8). Harald, the younger brother of Niels Bohr, was born in 1887 and was already the author of important mathematical works—some with Edmund Landau—on classical analysis and the analytical theory of numbers. He worked on the Riemann zeta function, Dirichlet series and most especially on the theory of almost periodic functions of real or complex variables which have their origin in Dirichlet series. Bohr was the real creator of the theory of almost periodic functions, [1923a, b], [1924-1926]. The fundamental theorem of the theory states that an almost periodic function *f* has a countable number of proper frequencies. If *M* denotes the mean taken on increasingly large intervals, $a(\lambda)=M\{f(x)e^{-i\lambda x}\}=0$, except for a countable number of values of λ. The function *f* has a generalised Fourier series, $\sum a(\lambda)e^{i\lambda x}$, which satisfies the Parseval equation of classical Fourier analysis : $M\left\{\left|f(x)\right|^2\right\}=\sum\left|a(\lambda)\right|^2$. This remarkable result, which was immediately re-derived and extended in various directions by the great analysts of the day, made Bohr's name (9). It would also be the subject of Jessen's "magister" thesis which marked the end of his schooling.

2.1 Magister thesis 1929

Bohr's almost periodic functions arise as natural extensions of ordinary periodic functions which have only one frequency and the quasi periodic functions of Bohl and Esclangon which have a finite number. By extending the results of these last authors to the case of a countable number of frequencies, Bohr showed, [1923b], [1924/26], that if f is an almost periodic function in his sense, it can be written

$$f(x) = F(x, x, \ldots, x, \ldots)$$

where $F(x_1, x_2, \ldots, x_n, \ldots)$ is a function in an infinite number of variables, periodic in each variable, or it can be uniformly approximated by such functions. It follows that Bohr's almost periodic functions are the only functions that can be approximated uniformly by generalized trigonometric sums. Thus almost periodic functions may be seen as the restriction on the diagonal of the periodic functions on a torus in infinitely many dimensions, an infinite annulus, to which an appropriate Fourier theory might apply. But the generalised Fourier series of almost periodic functions no more converge in general than the Fourier series of ordinary periodic functions (10). To obtain a satisfactory theory, it would be necessary to do for Bohr's theory what Lebesgue [1905a], [1906] had done for Fourier's and it is easy to imagine experienced analysts having this idea and dismissing it. Lebesgue measure in itself did not extend to infinite dimensions and the Daniell integral and the Gateaux means were not known. So Bohr may well not have pushed Jessen towards this apparent dead end. The first six chapters of Jessen's magister's thesis describe the recent, explosive developments in the theory but Jessen added (in extremis?) a seventh independent chapter, entitled "On functions of infinitely many variables." This contains the first version of "Jessen's theorem."

Jessen was 21 and his education was over. He had already published some elegant short articles and Bohr thought enough of him to involve him on his own work on the zeta function, which introduced him to functions in infinitely many variables (11). Jessen was brilliant and naïve and able to pursue a new idea, unencumbered by too much knowledge and prejudice.

To produce a Fourier theory for functions $f(x)$ defined on the torus Q_ω in infinitely many dimensions, where the variable $x = (x_1, x_2, \ldots, x_k, \ldots)$ is an infinite sequence of real numbers modulo 1, it is necessary to begin by defining the integral of such functions. In the spring of 1929 Jessen knew the Lebesgue integral for one or of a finite number of variables (12) but he was completely unaware of the Daniell integral, as he told Lévy later—see below. So he goes off in the first direction that offers itself, as though he said to himself at the start, the simplest procedure is to integrate successively the function f on the basis of the first coordinate and then continue with the other coordinates. Thus he considers the sequence of "Lebesgue integrals"

$$\int dx_n \ldots \int dx_2 \int f(x_1, x_2, \ldots)\, dx_1$$

in which f is a function defined on Q_ω, integrable in the sense of a theory yet to be born. If the sequence converges, in a sense to be made precise, it can only be to the desired integral. It is also a first informal formulation of the theorem of the downward martingales, but we should not anticipate.

It was now a matter of formalising the idea. Jessen read and studied the fundamental article of F. Riesz [1910], with Pal or on his own. The Riesz article presented the theory of L^p spaces and of L^2 in particular, which, by the Riesz-Fischer theorem of 1907, is isomorphic to Hilbert's space l^2. To begin with, Riesz treats the case of functions of a real variable and in a final paragraph, p. 496-497 he extends the whole theory to the case of functions of n variables, using what he calls a "principe de transfert" (13), which establishes (without proof, considered unnecessary) an (almost) bijective and measure preserving correspondence between any bounded interval on the real line and an *n*-dimensional cube of the same measure, thus permitting the automatic transfer of all real theorems to the vector case.

Jessen thought to extend this principle to infinite dimensions. His plan was to define a set function on Q_ω, which coincides with Lebesgue measure on the cylinder sets whose base depends only on a finite number of coordinates and to build a bijective correspondence (up to a set of measure zero) between the unit circle, the torus in one dimension, and the torus of infinite dimension, which preserves the (pseudo) measure thus constructed and transfers to it all the properties of Lebesgue measure, in particular countable additivity. The result is a theory of integration with all the properties of the Lebesgue integral. In his magister's thesis as in [1929b], [1930] and [1934a], Jessen sets off from the natural measure of the generalized intervals of Q_ω : $a_i \le x_i \le b_i$, for a finite number of indices *i*, and by using the compactness of Q_ω and the Borel (- Lebesgue) covering lemma, he constructs a set functon on Q_ω by the process of outer and inner measures (14). This set function is not yet a true measure, but it becomes so (and can consequently be extended in the manner of Carathéodory) by transfer, after Jessen had established a continuous correspondence between the intervals of the circle and the generalized intervals of Q_ω, by extending the curve of Hilbert, [1891], to infinite dimensions. Finally and still following Carathéodory, Jessen constructs the integral on Q_ω for "summable" functions which he writes $\int_{Q_\omega} f(x)dw_\omega$.

It remains to establish a link between this integral and the process of successive integrations defined above and, if possible, to produce a Fourier-Lebesgue theory for functions on the infinite torus along with some applications. What Jessen had outlined in his magister's thesis was developed brilliantly in successive memoirs until [1934a]. But first it may be appropriate to put Jessen's theory and his principle of transfer into a somewhat broader context.

There was nothing astonishing in this discovery from the spring of 1929, viz. the direct transfer from infinite dimension to one dimension, or at least not with hindsight. This had been Borel's starting point when he [1909] considered infinite plays of heads or tails: with any infinite sequence of heads and tails can be associated an expansion in base 2 of a number in the interval [0, 1], and the dyadic subintervals of this interval correspond to sequences for which the first outcomes are fixed. This rather informal correspondence was made precise and extended by Steinhaus in 1923 in as rigorous a fashion as could be desired (15), and applied to the study of series of terms of which the signs are drawn from an urn "which always contains as many plus signs as minus signs". It was Steinhaus again who (a little after Jessen or even a little before) extended Borel's transfer principle to the space Q_ω put in quasi-bijective measure preserving mapping with [0,1], in a study of series with terms drawn at random from the unit circle (16):

$$\sum_{k=1}^{\infty} a_k e^{2\pi i x_k}$$

It suffices to develop each of the x_k in [0,1[in base 2 (for instance), then to reconstruct, starting from these expansions written one below another in an array, a unique *x* from [0,1[, moving zigzag in the table thus formed. This could be in one of the traditional ways but the choice has no importance, provided that it rewrites on a line the initial two-dimensional board. One thus obtains a generalised "Peano curve" almost bijective correspondence, which obviously preserves measure, or rather transforms Lebesgue measure into the product probability which governs the drawing of the terms from the urn. Indeed, in both cases, linear order or plane table, one deals with the same sequence of independent Bernoulli random variables of the same law, except for a rearrangement which leaves the law invariant. The measure on Q_ω is nothing other than that which governs a countable sequence of plays of heads or tails, i.e. (if one wants to avoid admitting chance), by appealing again to Borel's principle of transfer, Lebesgue measure on the unit interval (which is, above suspicion). So that the measurable sets of the interval [0, 1] and those of the space Q_ω which correspond by transfer have the same measure.

Thus the principle of transfer that Jessen developed in an elegant and rigorous way in his own framework should not surprise us. There are earlier, later and contemporaneous versions, as is common in such cases. Moreover, we will see that Lévy asserted his own paternity of the principle, which he called the "principe de correspondance", having used it since 1924 and perhaps before, in an unpublished course given at the Collège de France in 1919, (and certainly well before the time of his childish walks to the garden of Luxembourg). Priority in the matter of this principle is one of the themes in the correspondence published here and gives it its title. We will see that Lévy seemed to consider this a subordinate point, though he kept returning to it. This was evidently not so for young Jessen who would undoubtedly have liked to have had his priority in the principle recognised for he had discovered it on his own. However, more than any else and without the least visible sign of probabilistic intuition or reasoning, Jessen was guided by the theory of the Lebesgue integral and the principle of transfer which form the basis for his theory.

We should return to Jessen's magister thesis. §5 of the last chapter contains an outline of a theory of the differentation of set functions associated with the integral on Q_ω, following a net of increasingly fine generalised intervals. Jessen transfers directly the method of La Vallée Poussin (17). More precisely, if f is integrable on Q_ω, we have following Jessen, $F(E)=\int_E f(x)dw_\omega$. F is an additive set function on Q_ω, which is established by transfer on the unit circle, with notations which go from oneself: $\int_E f(x)dw_\omega = \int_e \varphi(t)dt$.

We define on Q_ω a net of generalised intervals $\overline{I_n}$, corresponding to a net of intervals on the unit circle, and form the associated simple functions: $\Delta_n(x)=\dfrac{F(i_n)}{w_\omega(i_n)}$ if x is in i_n, an element of the net. By transfer of the Lebesgue-La Vallée Poussin theorem on differentiation, it follows that $\Delta_n(x)$ converge almost everywhere to $f(x)$.

In the next section, Jessen transfers Fubini's theorem to infinite dimension, in the obvious way that one imagines, by collecting the coordinates of Q_ω, in a finite number of packets $\left(Q_\omega^{(1)},Q_\omega^{(2)},....,Q_\omega^{(n)}\right)$, and by writing the corresponding Fubini theorem (18).

Having defined for Q_ω endowed with the Jessen measure w_ω, convergence in measure (19) and convergence in L^2, which is thus to be complete by transfer, Jessen comes to the statement and proof of Jessen's theorem.

We have reached §8. Jessen defines an integrable function f on Q_ω. By the theorem of Fubini-Jessen, applied to the decomposition of Q_ω in two blocks, the block Q_n of the first n coordinates and the block $Q_{n,\omega}$ formed of the infinity of those following, while noting $w_{n,\omega}$ the Jessen measure on $Q_{n,\omega}$, the integral

$$\int_{Q_{n,\omega}} f(x_1,x_2,....,x_n,x_{n+1},....)dw_{n,\omega}$$

is an integrable function on Q_n, provided with Lebesgue measure.

Jessen states that this sequence of integrals converges in measure towards f, (p. 50). This is the first known version of the theorem of (increasing) martingales in a general framework, or at least it can be made as general as one wants.

Jessen's proof is interesting though still a little awkward. It anticipates the first part of the modern proof, as we recalled it in the introduction, although Jessen had not yet seen convergence in mean. The idea is to approach f by a sequence of functions depending only on the n first coordinates and go forward by such a sequence of increases in measure. Jessen obviously hesitates over the nature and the general information of such an approximation. For this he uses the functions $\Delta_n(x)$ defined in § 5. These functions, by construction, depend only on a finite number of coordinates and converge almost everywhere to *f*. For want of anything better and pressed by time, Jessen was satisfied to exploit convergence in measure of the sequence $\Delta_n(x)$ and treat first of all the case of a bounded function *f* and then introduce truncation. The proof is correct but unnecessarily complicated and restrictive.

The next section proves the proposition on which he is going to build his theory of integration. This is the first known form of the theorem of downward martingales but it is still about convergence in measure:

The sequence of integrals $\int dx_n \ldots\ldots \int dx_2 \int f(x_1, x_2, \ldots\ldots) dx_1$ converges in measure towards the integral $\int_{Q_\omega} f(x) dw_\omega$.

Again Jessen truncates and uses the inequalities established in the previous section. The last section treats the Fourier theory of functions on the infinite torus. Jessen establishes a Parseval equation (p. 54) and a Riesz-Fischer theorem for this framework.

In broad outline, the entire theory of [1934a] is already present in chapter 7 of Jessen's magister thesis, which can truly be called masterly. Bohr was certainly very impressed.

2.2 Doctoral thesis 1930

After such an achievement and with the support of Bohr with his well-known academic clout, one can imagine that Jessen was propelled at once to the firmament of new mathematical stars, at least in Copenhagen. He was invited to make a presentation to the seventh Congress of Scandinavian Mathematicians, held in Oslo from 19 to 22 August 1929 (20). Jessen presented his theory of integration in German and in a particularly clear way, a very nice exposition with a reproduction of Hilbert's space-filling curve as it appeared in the original article of 1891, which shows at first glance that measure is preserved, the curve preserving throughout construction a perfect symmetry between the two axes. Jessen did not state his two theorems which he undoubtedly considered marginal, but announced a Fourier theory for functions with a countable infinity of periods. The transactions of the congress were published in 1930, so that at the proof stage Jessen could add a reference to Daniell of whom he had been informed meanwhile. Lévy read this paper in 1934 and we will see that reading it played a very important part in the emergence of his theory of measure as it is expounded in his great treatise [1937].

Christian Berg tells us elsewhere in this issue that in 1929 Jessen obtained a grant from the Carlsberg Foundation to travel in Europe, first to Szeged, where he met F. Riesz, one of the principal inspirers of his thesis, then to Göttingen, from where Hilbert reigned over universal mathematics, and to Paris to "see" Lebesgue, though he hardly saw him at all. But Bohr soon recalled him to Copenhagen. A position of docent (university lecturer) in mathematics was about to become vacant at the Royal Veterinary School of Copenhagen. Considering the scarcity of positions and the good health of their occupants, this was an exceptional opportunity that was not likely to be repeated soon and which could not be allowed to escape. (21)

Jessen's habilitation became an urgent matter. For Bohr there was no doubt that the final chapter of the magister's thesis was already a doctoral thesis. It was enough to improve the presentation and to print the whole in Copenhagen. The university's approval was needed and this was obtained on March 25, 1930, signed by J.F. Steffensen, professor of actuarial sciences. The thesis was submitted before the deadline and Jessen was appointed docent. He was 22 years old.

Jessen's doctoral thesis is entitled "Contribution to the theory of the integration of the functions of an infinity of variables" [1930]. It is in three parts. The first treats integration of functions of n variables, by transferring the Lebesgue integral in one dimension. This part has no new results but serves as a preparation for the next part, which treats functions in an infinite number of variables and which includes all the results from chapter 7 of the magister's thesis as described above. The last part develops some applications, in particular to the theory of almost periodic functions.

We will make a brief examination of the second part which has a new version of Jessen's theorem. This part begins with the construction of the integral on the infinite torus following the magister's thesis. Things only start to change in §13. There Jessen, pp. 37-38, proves a theorem which he says is due to F. Riesz, which affirms that the quotients defined above converge "strongly" towards f, in the sense of convergence in L^1. The proof is very simple. If f is bounded by a constant, the sequence is bounded by the same constant and the result follows from the dominated convergence theorem. If not, it is sufficient to truncate f and to take the limit of the truncated versions. (22)

Thus by sections 15 and 16, Jessen can prove his theorems for convergence in L^1 in much the way we did in the Introduction, for the increasing case; the approximating functions which depend on only a finite number of coordinates are precisely the quotients $\Delta_n(x)$. Thus in the spring of 1930 Jessen has the statement and proof of the martingale theorem in a form which will hardly be improved upon, except for the framework (23). The intervention of Riesz was no doubt crucial but the idea was Jessen's.

The third part of the thesis of 1930 is extremely interesting, but to do it justice would take us too far from our subject. Jessen presents in detail his Fourier theory for functions defined on the infinite torus and proposes various applications which he would develop more fully in his article [1934a]. Among the applications were Weyl's equi-distribution theorem [1916], of which he could have learnt in Göttingen, almost periodic (random) functions of a complex variable, which are represented in the form of series $f(s,x)=\sum_{k=1}^{\infty} a_k e^{2\pi i x_k} e^{\lambda_k s}$, where s is the complex variable, and $x=(x_1,x_2,\ldots,x_k,\ldots)$ is a "parameter" formed of an infinite sequence of real numbers modulo one, and the Riemann zeta function, in particular his work with Bohr which was the initial motivation of the theory. (24).

2.3 The *Acta* article 1934

In 1934 Jessen married and he needed to prepare his courses for the veterinary school. These changes may have slowed down his work but it did not stop it. However, Jessen does not seem to have returned to his theory of integration. Was he discouraged by discovering Daniell's earlier work and Steinhaus's concurrent work on almost the same subject (but without Jessen's theorem)? It was still there in his fine presentation to the International Congress of Mathematics of Zurich in 1932, where Jessen gives the results of the third part of his thesis, which now completed by taking account of the papers by Paley and Zygmund [1930/1932]. Naturally Jessen referred to his work on the theory of integration on the infinite torus, (with a geometrical representation) but without emphasising or even mentioning Jessen's theorem. He must have thought, not without reason, that the time was not yet ripe—or worse still, that it had no interest.

For the academic year 1933-1934 Jessen was a Rockefeller fellow in Cambridge, England, with Hardy, and at the newly-established Institute for Advanced Study in Princeton. Under the leadership of J. von Neumann and H. Weyl the Institute was stealing Göttingen's mathematical supremacy as it sank under Hitlerism. Thus Jessen had the opportunity to mix with some of the leading analysts of the day, von Neumann, Hardy, Weyl, Wiener, Daniell, Bochner, Besicovitch, etc as well as with brilliant young people from all over the world. In Princeton there was an atmosphere of great mathematical euphoria and intense activity. No doubt Jessen wanted to give an account of his direct way of tackling the problems and the principle of transfer, in which he hardly any more believed, which had led him to propositions that were unsuspected by both analysts and probabilists (25), who saw things differently. It would be interesting to emphasize them in a work of synthesis written in English and presenting all the theory and applications as a self-contained coherent whole. Besides he had improved his theorem and found unexpected applications. This was the rationale for his article, "The theory of integration in a space of an infinite number of dimensions", printed on July 6, 1934 in volume 63 of *Acta Mathematica*. This contains the definitive version of "Jessen's theorem" and we need to examine it.

Jessen began by remarking that in the previous fifteen years the theory of integration in infinite dimensions had been considered by several authors who exploited in various ways a principle of direct extension in the space under consideration (26). Jessen, for his part, intended to remain faithful to the theory developed in his thesis, based on a "transferring principle" which allowed him to go from the interval [0, 1 [, where the Lebesgue integral is available, to the torus space in infinite dimensions, the correspondence automatically transferring any theorem in one dimension to a theorem in infinite dimension, and conversely. Jessen uses, as in his thesis, a procedure of increasingly fine successive partitions of space, in networks of generalized intervals which are put in correspondence with networks of intervals from [0,1 [, so that the construction of the Lebesgue integral passes to infinite dimension by simple translation. Jessen remarks (§ 9) that this integral enjoys the Lebesgue property of differentation: on following a sequence of increasingly fine dissections of space, the derivative of a primitive gives again the function almost everywhere. As we saw, this is an immediate application of the principle of transfer but this result is the basis for almost all the others (and it is also a martingale theorem, as we have already remarked).

All the preceding results had appeared in Danish in Jessen's two theses, but § 11, with the title "An Important Lemma", contains a new result which has no natural analogue in finite dimensions. It will be debated at length in the correspondence presented below. The lemma states that a measurable function defined on Q_ω which takes the same value on two points which differ only by a finite number of coordinates, is constant almost everywhere. Today we would notice that, if this function were the indicator of a set, the important lemma is only a special case of the 0-1 law of Kolmogorov [1928, 1933], but in 1934 Jessen was not aware of this. He refers only to a related result by Steinhaus [1930]. His proof uses the differentiation theorem of § 9, for suitable dissections. It seems that it was this lemma that first attracted Lévy's attention, who at once re-obtained it by a "direct" probabilistic method. Lévy did not go back to Kolmogorov either although he cited him with Khinchin in [1931c]. Lévy had not really read him, as he acknowledged in his *Souvenirs*, [1970], p. 87. We will return to this point.

§§ 12, 13 and 14 treat variations on "Fubini's theorem" in infinite dimensions. We go to the statement of Fubini's theorem itself, which had already appeared in the magister thesis. The following paragraphs contain at last the almost everywhere version of "Jessen's theorem." (27)

In § 13 Jessen states:

$$\int_{Q_\omega} f(x)dw_\omega = \lim_{n\to\infty}\int dx_n \ldots\ldots \int dx_2 \int f(x_1,x_2,\ldots\ldots)dx_1 \text{ for almost every } x \text{ in } Q_\omega.$$

Jessen's proof is inspired by one in Kolmogorov [1928-1930]. The argument uses measure theory, a subject whose richness our author seems to have appreciated during his stay in Princeton.

§ 14, "Representation of a Function as the Limit of an Integral," would be the main topic in Jessen's correspondence with Lévy presented below. It is the counterpart to the preceding result: integrating backwards from infinity, one recovers the function almost everywhere, or

$$f(x) = \lim_{n\to\infty} \int_{Q_{n,\omega}} f(x)dw_{n,\omega} \text{ for almost every } x \text{ in } Q_\omega.$$

Jessen's proof is rather complicated. It follows Riesz's proof of a result in Fourier analysis in Q_ω which can be seen as a special case of a result proved in Jessen's § 18, (28). It relies essentially on the differentiation theorem of § 9. As we will see, Lévy tried to persuade Jessen that there is a direct Borelian proof; this was inspired in part by a new proof that Jessen put to Lévy in one of the letters presented below.

§ 15 shows that the two theorems mentioned hold for "strong convergence", that is for convergence in L^p if f is in L^p, $p \geq 1$. In § 16, Jessen establishes the maximal inequalities for martingales in L^p (known in their definitive form as Doob's inequalities) which Jessen describes as analogues of the "well known maximal theorem" of Hardy-Littlewood [1930].

The later sections of the paper are given over to applications. These sections contain great riches but we cannot consider them here. (29)

Thus in 1934 Jessen had a general theory of martingales in a particular framework, the space Q_ω and its partition into nets, with no mention of probability, random variables or conditional expectations. (30)

2.4 A probabilistic interlude 1934-5

In Princeton Jessen met Aurel Wintner (31). Two more different mathematicians can hardly be imagined. Jessen was elegant, reserved, rigorous, scrupulous, Wintner was impassioned, a compulsive eater, overflowing with projects and works in progress, all done with great noise. Wintner was always interested in celestial mechanics and accordingly in almost periodic functions. For some time he had been studying the limiting laws of series of independent random variables of the type considered by Steinhaus, Jessen and others, what was called at the time the problem of infinite convolutions, on the line or in a finite-dimensional space. Wintner had obtained interesting results on the subject (32), which was one that Jessen had also come near to either alone or with Bohr. Sometime in 1934, probably in the spring, Jessen and Wintner decided to pool their experience and write an article. "Distribution functions and the Riemann zeta function" was published in 1935 in the *Transactions of the A. M. S.* having been received by the journal on July 9, 1934 and presented to the Society on April 20, 1935. The authors proposed to treat the problem of infinite convolutions by the method of Fourier transforms, which they tell us was first applied by Lévy in his book of 1925. This is inaccurate (33) but at least it indicates that the theory of the Fourier transform or Laplace-Fourier-Poisson-Cauchy… transform which had long been considered suspect by mathematicians was now, after re-examination by the new analysis, well-established. We will not examine the main part of the paper but only the final two very short sections where the theory of infinite convolutions is considered from the point of view of the (probabilistic) theory of sums of independent random variables of Khinchin, Kolmogorov and Lévy (34). There may seem nothing surprising in this but in 1934 it was different and our authors wanted to show with complete clarity the relationship between the two theories (of infinite convolutions and of sums of independent variables) in a new way, while placing themselves within the framework (more analytical) of the theory of integration in infinite dimensions. Suppose we follow them for a moment.

§ 15 gives a brief account of the theory of measure and integration in general product spaces. The authors tell us that the theory was presented in great detail by Jessen [1934a] in the particular case of the infinite torus and that the general case will be published by Jessen "in a forthcoming paper". The framework is a theory of abstract measure, a set Q, a "Borel field" of Q, a positive and countably additive set-function m, with total mass 1. Given a countable family of such spaces, q_1, q_2, .., q_n, .., each supplied with a probability measure, μ_n , there exists on the product space Q = (q_1, q_2, .., q_n, ..,) a product measure with natural properties (35) and one has the theorems of Jessen which are given without demonstration, in particular the "important lemma" of § 11, stated this time for set indices: If a measurable set of the product space contains all points differing from one another in only a finite number of coordinates, its measure is 0 or 1 (36). Naturally there are theorems from § 13 and § 14 of Jessen [1934a]; one theorem (in self-explanatory notation) states that:

If f is an integrable function defined on Q, and if one puts $t=(t_1,....,t_n,t_{n+1},....)=(t_n,t_{n,\omega})$ for an arbitrary point of Q, then,

$$f_n(t)=\int_{Q_{n,\omega}} f(t_n,t_{n,\omega})m_{n,\omega}(dt_{n,\omega})\to f(t) \text{ for almost all } t \text{ in } Q.$$

In this general version of Jessen's martingale theorem, the concept of conditional expectation does not appear. We remain in the case of product spaces and Fubin's theorem makes the passage possible.

Jessen and Wintner did not actually need such a general formulation since in the following section, §16, they consider a sequence of independent random variables $x_1(\tau_1),....,x_n(\tau_n),....$with values in R^k, which they define as measurable functions on the abstract spaces $q_1,....,q_n,....$ provided with as many probability measures. Kolmogorov, Lévy and others undoubtedly (among them Jessen and Wintner) knew that such variables can be defined on Q_ω (or the unit interval provided with Lebesgue measure), so that the theory of Jessen [1934a] is amply sufficient, but evidently our authors stick to their generality; they are unaware of the axiomatics of Kolmogorov and it is to best to be careful.

A fundamental theorem of the theory of the sums of independent variables states that, for such sums, there is equivalence between convergence in law (the convergence of infinite convolutions), convergence in probability (convergence "in measure") and almost sure convergence (almost everywhere). Jessen and Wintner proceed as follows. Equivalence between convergence in law and convergence in probability is easy, it is enough to work in the sense of Cauchy. The only real difficulty is to show that convergence of probability entails almost sure convergence. Let us put with our authors $s(t)=x_1(\tau_1)+....+x_n(\tau_n)+....$ (in probability) and $f(t)=e^{is(t)y}$, where y is fixed. f is a function bounded in absolute value and so integrable in Q, to which one can apply the theorem of Jessen stated above. The integral taken starting from n, $f_n(t)=e^{ix_1(\tau_1)y}....e^{ix_n(\tau_n)y}a_n(y)$ where, for all y, the sequence of constants $a_n\to 1$, when $n\to\infty$. Whence it follows that, for all y, $e^{is(t)y}=e^{ix_1(\tau_1)y}e^{ix_2(\tau_2)y}......$, for almost all t and finally that the series $x_1(\tau_1)+x_2(\tau_2)+....$ converges towards $s(t)$ for almost all t.

This is, to our knowledge, the first probabilistic application of the martingale theorem. (37). It dates from 1934 and was published in 1935 in the journal that in 1940 published Doob's first article on the theory of martingales, all without Doob or Jessen noticing.

2.5 After 1934

What was the fate of the theorems of 1934 in Jessen's later work? To treat the question adequately would take us too far from our subject but here are some indications.

We have just seen that, in the spring of 1934, Jessen envisaged a complete memoir where his theorem would be presented in the greatest possible generality. He told Lévy so in one of his letters. However, the project seems to have been repeatedly put back. Jessen was appointed professor at the Polytechnic School of Copenhagen where his father-in-law P.O.

Pedersen (38) was director. He had to prepare his courses which he did with meticulous care. Less was at stake academically and he was no longer in Princeton with its atmosphere of high-speed mathematics. Jessen could pause and hope that by investing the necessary time he would obtain still more powerful theorems in a yet more general framework, for example treating the case of an arbitrary set of indices or working in spaces that are no longer products of measure spaces. The matter was not simple, as he must have realised rather quickly, and difficulties appeared at every turn and they accumulated. The new theory of abstract measure being born here or there conceals under the simplicity and generality of its concepts and statements awful traps and the majority of papers go wrong in one way or another. So it was necessary to begin by imposing some order on this proliferation before trying to place his results in their natural abstract framework. So between 1934 and 1947 Jessen published a series of memoirs in Danish in the journal he edited, *Mat. Tidsskrift*. These were so many chapters of a treatise on abstract measure theory and Jessen assembled them in a volume he published in 1947. One suspects that an English translation was planned but that the the project was abandoned because of other pressures. The exposition was remarkably clear and as good as the works that appeared in the fifties and which it inspired on a number of points.

Of this collection of articles, we will attend only to the fourth, published in 1939, which touched directly on Jessen's theorem, and which was undoubtedly a provisional version of the forthcoming article promised in Jessen, Wintner [1935] § 15. The article treated product spaces. It showed the existence of a product measure for an arbitrary family of probability spaces (n° 4.4). The demonstration is done by extending the natural set function defined on the cylinder sets. There is no more of the principle of transfer and the proofs of the article [1934a] must be modified. However in N° 4.7, Jessen shows Jessen's theorem for the decreasing case for set functions and, in n° 4.8, he succeeds in proving the theorem for the increasing case, such as it is stated above according to [Jessen-Wintner 1935], § 16. We see below that Jessen had a first version of this proof when he wrote to Lévy. It is reproduced (in English) in [Andersen Jessen, 1946], n° 26 and may be consulted. (39).

The tragic situation of the world between 1939 and 1945 explains the long silence that follow but perhaps Jessen was still trying (unsuccessfully) to improve his theorems. In the absence of convincing documents, we can at least imagine the issues involved. It would be necessary, in one way or another, to leave countable product spaces, while working within the framework adapted to the families of dependent random variables, with abstract values and indexed by sets of filter indices (increasing or decreasing) as general as possible. Already Lévy, as we will see, in his treatise of 1937, relaxed the assumption of independence (though staying with a countable family of real variables). This potential development raises two delicate questions. The first, posed by many around 1935, is whether the Daniell-Kolmogorov theorem can be placed within an abstract framework, or, if, with every compatible family of abstract measures defined on the system of finite cylinders of an unspecified product of measurable spaces, one can associate a measure of which they are the marginals. The second question is peculiar to Jessen: how can his theorem be extended to this new framework, by supposing that the set of indices is filter towards infinity without being completely ordered. It was found that the answers to both these questions is no (40) and that one cannot thus obtain in complete generality abstract generalizations of Jessen's "Fubini's theorems". Things had to be viewed differently.

The situation appears to have developed truly only after the war. In a famous article written in collaboration with Erik Sparre Andersen (41), [1946], clarified and extended in [1948b], Jessen could state his two theorems in a satisfactorily general framework, that of the modern theory recalled in the Introduction. One obtains an absolutely general result by forgetting the product structure and Fubini's theorem and using the general concept of conditional expectation of Kolmogorov. One suspects that this very simple idea came from the young Sparre Andersen, although the proofs are almost identical to those of 1939 and these, we will see, Jessen wrote in part for Lévy in 1935.

At all events, Jessen could at last write, [1946], n° 1:

> The present paper deals with two theorems on integrals in an abstract set. The first theorem (that of § 14 of [1934a]) is a generalization of the well-known theorem of differentiation on a net, the net being replaced by an increasing sequence of σ - fields. The second limit theorem (that of § 13) is a sort of counterpart of the first, the sequence of σ - fields being now decreasing. The proofs follow the lines of the proof of the theorem on differentiation on a net.
>
> In case of integrals in an infinite product set the theorems lead to known result, when for the nth σ - field of the sequence we take either the system of measurable sets depending on the n first coordinates only, or the system of measurable sets depending on all except the n first coordinates.
>
> If the abstract theory of integration is interpreted as probability theory, our theorems lead to two theorems concerning conditional mean values.

We now turn to our second protagonist.

3 Lévy's Lemma

The life and the work of Paul Lévy are fairly familiar and we do not need to rehearse them (42). Lévy's interest in probability theory went back to 1919, when he went to teach at the École Polytechnique, [Lévy 1919] and [Barbut, Mazliak 2008a]. His first works were mainly concerned with the theory of stable laws, where only convergence in law is involved. Essentially Lévy worked within the framework of a finite-dimensional space, provided with a positive additive set function of unit mass obeying Lebesgue's theory, which he transferred if necessary to the unit cube endowed with Lebesgue measure. Infinite dimensions as such were not involved, nor what Borel called "denumerable probabilities", i.e. probabilities for events depending on a countable infinity of trials. Thus for ten years, until around 1929, Lévy seemed to restrict himself to the "point of view of Bernoulli", in the expression of the day. How can we explain this restriction and then the sudden change towards denumerable probabilities which would be lead on without pause to one of the great achievements of 20th century probability, including the lemma we are about to consider?

3.1 Before 1930

In the absence of convincing answers to these questions, we can at least make some suggestions. To begin with, note that between 1925 and 1929 Lévy produced no important publications on probability theory, a subject he seems to have abandoned completely after producing his first probability book [1925b] which was itself based on earlier memoirs [1924]. No denumerable probabilities, no finite probabilities, nothing! Lévy published a few papers in analysis, on the Riesz-Fischer theorem, divergent series, entire series, the Riemann zeta function, doubtless all based on presentations to the Hadamard seminar in which he took an active part. His contribution to the Congress of mathematicians at Bologna in September 1928 was on "functions of regular growth and iteration of fractional order", an esoteric theme in his work, that Lévy has described as idealist (43). And then suddenly in 1929, as noted above, began an almost uninterrupted flow of contributions of the highest rank, all related in one way or another to denumerable probabilities, including—besides much else—the 1935 article containing Lévy's lemma.

One can suggest at least two hypotheses to explain Lévy's return to probability. First there was Fréchet's arrival in Paris at the end of 1928, when he was appointed professor at the Institut Henri Poincaré, on the recommendation of Borel who planned to create a major probability centre in Paris. At the beginning of 1929, Borel organised, with Fréchet's help, general lectures on probability theory and theoretical physics to be given by the principal proponents of these theories in Europe. Lévy, whose only mathematical contacts had been with speakers at the Hadamard seminar and the SMF who were not much concerned with probability theory, now had in Fréchet an interlocutor who knew all the probability literature and maintained relations with most of the analysts of the time. (44).

Secondly, there was the Congress of Bologna in September 1928, where the principal "probabilists" of the day met for the first (and last) time and where Lévy, who was registered in the analysis section, discovered that probability theory was not only a subject taught at the École Polytechnique but a flourishing field. Also, after Bologna nobody could be unaware of the theory of denumerable probabilities for it was at the centre of a famous academic incident. Cantelli and Slutsky (and others in support) disputed the paternity of the outstanding result of the theory of denumerable probabilities, the strong law of large numbers for the game of heads or tails. Cantelli argued, with some energy and not without reason, that Borel's proof was fundamentally incomplete and that he was the author of the first truly probabilistic proof of this new type of law of large numbers [1917], while Slutsky believed, not without reason, that it was all in Borel's original article of 1909 and moreover had since been repeated by a number of authors (45). Did Cantelli's claims clash with Lévy's patriotic feelings? Did they make him read Borel's article more closely? Or did he suddenly realise that there was an immense field of which he had been unaware and where he could revive his rather dormant mathematical work? At all events, we have to recognise that at the end of 1928, Lévy launched himself into denumerable probabilities. We believe that these factors together are enough to explain Lévy's return to probability around 1929.

Lévy's withdrawal in 1925, and, most of all, his not knowing about denumerable probabilities for ten years and having only the assistance of the point of view of Bernoulli, is more difficult to discuss, even hypothetically. One might invoke Lévy's peculiarly cyclical disposition, with its alternating periods of intense creative activity and times when nothing interested him and he just performed the strict duties of his functions (46). But that does not explain his lack of interest in denumerable probabilities during his active period after the war. Indeed everything seemed to predispose him to be interested in the probability of events where the mean of functions depends on an infinite number of variables, and that from the time of his first work in probability in 1919-1920. Consider what he writes in his *Souvenirs mathématiques*, p. 55: "In January 1918, I had been in a hospital bed for more than two months when I suddenly reconsidered the subject of functional analysis. In my first work I had not thought of extending the concept of the integral to spaces of infinitely many dimensions. Suddenly it appeared to me possible to tackle the problem on the basis of the concept of the mean on a sphere in the space of square summable functions. Such a function can be approached by a step function, with the number n of distinct values increasing indefinitely. The desired mean can then be defined as the limit of the mean on a sphere in n dimensional space." Shortly afterwards, Lévy communicated his "remarques" to Hadamard who told him that Gateaux, who was killed in combat at the beginning of October 1914, left manuscripts on the subject, exploiting substantially the same idea: the mean of a function of infinitely many variables is obtained by taking the limit of the mean of this function on a sphere of increasingly large dimension and radius (47). It is known that this idea, common to Gateaux and Lévy, most probably came from a course that Borel gave in the Sorbonne, during the winter 1912-1913, on the metrical geometry of spaces of very large dimensions—of the order of Avogadro's number. Lévy says so explicitly in many places. It is a natural and powerful idea insofar as, when the dimension, n, is immense, the volume of a sphere of radius R is negligible compared to the volume of a sphere with very slightly higher radius

($R^n = o\left(\left(R+\varepsilon\right)^n\right)$), so that in a sphere of very high dimension, the volume is essentially on the periphery and the calculation of the averages in such a sphere is limited to a calculation on its surface, which simplifies the matter greatly. For example, one can deduce from this remark that, necessarily, for a fixed value of *t*, an asymptotic mean of a functional Φ in L^2, of the form $\Phi(x) = \varphi(x(t))$, is equal to $M(\Phi) = \int \varphi(\xi)\frac{1}{\sqrt{2\pi}}e^{-\frac{1}{2}\xi^2}d\xi$, and more generally, for fixed values $t_1, t_2, \ldots t_p$, if $\Phi(x) = \varphi\left(x(t_1), x(t_2), \ldots, x(t_p)\right)$, then

$$M(\Phi) = \int \varphi\left(\xi_1, \xi_2, \ldots, \xi_p\right)\frac{1}{\sqrt{(2\pi)^p}}e^{-\frac{1}{2}(\xi_1^2+\xi_2^2+\ldots+\xi_p^2)}d\xi_1 d\xi_2 \ldots d\xi_p$$

so that, in the calculation of the Gateaux means, everything happens as though the variables *x(t)* were independent and of the same reduced centered normal law (48).

However, this theory of mean values is not directly applicable to probability theory. The Gateaux means exist in general but are not expectations corresponding to a probability measure, which, if it existed, would make independent all values of the function which one would draw according to this law, which is obviously not possible (one does not draw with the fate continuously, or there does not exist measurement of probability beyond the power of continuous [Lévy 1925a]). On the other hand Wiener, having read Gateaux and Lévy on the topic and discussed it with the latter (49), saw at once the part which it could play in the construction of mathematical Brownian motion, a subject which had occupied him for several months and which had resisted him, in spite of the remarkable work of Daniell [1919]. Daniell had provided a general method for constructing an integral in infinite dimensional spaces but he did not provide a means for calculating laws or the expectations of functionals that were slightly complicated, such as the maximum, nor for showing that the functions drawn at random from such measures are continuous, nondifferentiable, Lipschitzian,…, exactly what Wiener wanted to do for his Brownian motion. Wiener, while reading Gateaux-Lévy, understood very quickly that to apply this theory to Brownian motion, it sufficed to treat not the values of the function at fixed times as independent variables but their changes. In Einstein's theory, indeed, it is not the *x(t)* that are independent but the *dx(t)*, so that a function *x* must not be represented by coordinates *x(t)*, but by coordinates *dx(t)*. This is the guiding idea of Wiener's first paper on "Differential-space", and is actually the only original idea on this point, his calculations merely reproducing those of Borel, Gateaux and Lévy, by replacing $x(t_1), x(t_2), \ldots, x(t_n)$, by $x(t_1), x(t_2) - x(t_1), \ldots, x(t_n) - x(t_{n-1})$, uniformly distributed on the suitable sphere, and in going to the limit on dimension *n* and the radius, time and space, in a coordinated way. The asymptotic result is still Gaussian, but now the distributions of *x(t)* have variances proportional to *t*, as envisaged in the physical theory of Einstein that Wiener knew well, or in Bachelier's theory of speculation of which he was quite unaware (50). One can thus calculate the average of any simple functional defined on the space of *x*, and it comes from a probability measure, "Wiener measure", as Wiener showed at the end of his article, by interpreting this asymptotic mean as a Daniell integral, after having shown, in various ways (without the Daniell integral), the characteristic properties of this new object, which resembled no other.

This idea which appears so simple today did not occurr to Lévy, it seems, and it was one of the missed oppurtunities that "gave him most regret" (51). All that is to say that Lévy knew, before 1925, the integral of Daniell and the measure of Wiener (at least the name), (52), and one understands a little better that rather long part of his correspondence with Jessen where he tries to persuade the latter that he had known for a long time about integration in infinite dimensions, the principle of transfer and a thousand other things as well, even if he did nothing but see them in the distance (or in the fog according to what one wishes to grant him), too far from the face of this research, thus missing several opportunities, that others, more perspicacious, seized. First there was Wiener with Einstein's Brownian motion, then Steinhaus and Wiener again with their series with random signs or random terms, followed quickly by Khinchin and Kolmogorov who will create the theory of series of independent variables before even Lévy realised that they could be the subject of a theory (53). Finally if Lévy did nothing or almost nothing with his theories of integration with an infinite number of variables, it is undoubtedly that the functional fields that interested him did not lend themselves to it and that the probability fields that lent themselves did not interest him. Here at least is a simple explanation that will do to begin with (54).

From 1919 to 1929 Lévy thus worked mainly on analysis in infinite dimension, the functional calculus of the school of Hadamard, and on probability theory in finite dimensions, an elaborate form of the theory of the errors to the programme of the École Polytechnique. Here Lévy was the first in France, after Laplace, Fourier, Poisson, Cauchy and Poincaré, to develop the method of characteristic functions and its applications to convergence in law, far from the work of Borel who would hasten to tell him that probability theory did not require such analytical sophistication which served for nothing (55). The theory of probability must have a "practical value", or apply to physical sciences or to authentic mathematics—analysis or the theory of numbers.

Was Lévy acting out of pique, from a preoccupation with reciprocity or from loyalty to Hadamard? In any case, he did not seem to be interested in practical probabilities or in Borel's denumerable probabilities in the twenties. Borel, on his side, was certainly never interested in the stable laws of Lévy, however useful in times of crisis (56).

Things changed, it has been said, after 1928. Lévy seems to have realised that there is a difference between the weak and the strong laws of laws numbers. He admitted this to Fréchet (57): "It may be that before 1928 I confused the point of view of Bernoulli and that of the strong law of large numbers. But since 1929 and in any case since 1930 I can tell you that, except for an always possible lapse, I did not …". Lévy was undoubtedly thinking of his first partly denumerable articles [1929] and [1930b, c]. The first is on the metric theory of continued fractions: a number between 0 and 1 is chosen at random, what are the limiting laws of its quotients complete and incomplete when it is developed as a continued fraction? This was a topic that Lévy returned to on several occasions and which he used to test his increasingly elaborate methods (e.g. [1936c]). This subject had already been tackled from the viewpoint of denumerable probabilities by Borel in his famous article, [1909], which Lévy quotes (58). However, Lévy still mainly works from the point of view of Bernoulli; he treats convergence in law and denumerable probabilities are not involved. But the end of the article is concerned with the frequencies of different incomplete quotients of a number taken at random randomly and tries to establish a strong law of large numbers for variables which are not independent. To this end, Lévy uses (for the first time it seems) one of the basic principles of his theory of dependent variables: one goes from the case of independent variables to that of dependent variables by replacing prior with posterior probability. (59)

The interesting point in this paper is elsewhere and it may help us to bring to a close this very short introduction to Lévy's denumerable silence, at least provisionally. Lévy states, p. 190-191: "Finally there is an essential point whose interest was long unperceived and which has been highlighted by M. Cantelli and Mlle Mezzanotte: it is not enough to consider each value of n independently of the others to show that the difference between the frequency and the average probability is almost surely lower than a function of n tending towards zero, but also and more especially to consider the whole of the experiments to show that this difference almost surely tends towards zero, i.e. becomes and *stays* almost surely lower than any positive number given." One should not be content to show convergence of probability (or a convergence in mean square) but almost sure convergence; this was the "essential point" which was “a long time unperceived” (especially by Lévy), that Mlle Mezzanotte and M. Cantelli highlighted (60). Lévy came to it late but for him it finally opened the door to denumerable probabilities—around 1930 and not earlier.

From here it is enough to follow the list of Lévy’s articles until inevitably we reach Lévy’s lemma. As remarked earlier, the article written in 1930 [1929], was already treating dependent events. Lévy would stick to this path until 1935. We will notice one or two stages.

3.2 Lévy’s denumerable probabilities

One of Lévy’s first “denumerable” notes, [1930c], goes over again Borel’s disputed proof of the law of large numbers and tries to make it precise.

Recall that Borel’s lemma, in its 1909 version, is stated as follows: there is a countable infinity of successive trials, assumed independent, and the probability of a successful outcome for the *n-th* trial is p_n. The probability that the favourable cases are produced infinitely often is zero or one, according to whether the series of the p_n converges or diverges. Thus it treats a 0-1 law and it is in this form that Lévy understood it, as he would understand Lévy’s lemma four or five years later. There is nothing surprising in laws of this type since it had been known since 1918, that in infinite dimension, non-trivial volumes have a natural tendency to be null or infinite. The completely innovative and brilliant idea of Borel, which for a long time remained unnoticed by Lévy was, we repeat, to regard the event “the successful outcome occurs an infinite number of times” as worthy of interest. The entire theory of denumerable probability is there, and, around 1930, Lévy finally understood it. So much so, that from then on Lévy regarded Borel as his master, officially on a par with Hadamard, but secretly more and more preferring him.

As is well known, the two applications that Borel gave of his theory, to decimal fractions and to continued fractions, used the lemma in cases where the successive trials are not independent. When the observation was made orally, Borel replied that it had no importance in the cases considered (61). In 1911 however, Felix Bernstein wrote in Hilbert’s journal, *Math. Annalen*, and openly challenging Borel’s results on continued fractions which, according to him, contradicted his own, but brought out the undeniable fact that Borel, to show these results, had applied his lemma to the case of dependent trials. Borel answered at once in the same journal (62) that Bernstein’s results did not contradict his own since they were identical except for the notations, and that its lemma remained valid in cases of dependent tests such as those which he considered. It was enough that the conditional probability p_n of the n–th trial taking account of the results of earlier trials satisfy the inequalities:

$$p'_n \leq p_n \leq p''_n$$

where p'_n and p''_n are sequences of constants to which one can apply the lemma in the form: the probability that the successful outcome occurs an infinite number of times is 0 or 1 according as the two series $\sum p'_n$ and $\sum p''_n$ are convergent or divergent, (63). Borel would say no more and would never take the trouble to write down the complete proofs for his memoir of 1909. Let him understand who can understand!

Lévy only became involved after Bologna. In the *Souvenirs* (64) he tells us that only then did he read Borel’s article [1909]. To prove his law of large numbers, Borel had applied his lemma, valid for independent events, to events related to numbers X_n of heads from n throws, which are not independent. Lévy reports that he was stopped for “several months” by this difficulty and that he finally overcame it, by considering, in the sequence of all the parts, those subsequences sufficiently distant from one another that the results cumulated are independent or almost so, and that is enough to justify the demonstration of Borel (65). Lévy was thus led naturally to the law of the iterated logarithm, which he published in [1930c], not knowing that Khinchin had published it six years before in the same journal [1924a].

This was the starting point of Lévy's search for a 0-1 law for the case of dependent trials. He seems to have given up this investigation temporarily, in 1930-1931, to resume only in 1934, after having published one of his showpieces, the generalisation of Wiener's differential space to the case of processes with the most general independent increments, the *dx(t)* remain independent, but they are no longer necessarily drawn at random on the sphere (66). After this bravura performance, which in a certain way—Lévy's—completed the theory of sums of independent variables, finite or infinitely small, Lévy decided to interest himself in series of dependent variables. The first thing to do was obviously to establish a 0-1 law, similar to that in Kolmogorov's theory, itself a generalisation of Borel's 0-1 law for indicators. In the case of centered, independent variables the series of variances plays the part of the series of probabilities in Borel, and makes it possible to determine almost sure convergence or divergence of the series of variables. This was the object of the note [1934c] presented on October 1, 1934 (67).

It is very clear to Lévy, that it was necessary, in the dependent case, to replace the conditions on expectations and variances of the independent case, by similar conditions relating to means and variances conditioned on the past. If one considers, following Lévy, u_n a series of dependent variables, and $E_{n-1}(.)$, the conditional expectation knowing the past up to n-1, the condition of centering of the independent case is written

$$E_{n-1}(u_n) = 0$$

so that u_n is a martingale difference and $S_n = \sum_1^n u_\nu$ is a martingale of Ville-Doob, avant la lettre. The condition on the variances of the independent case becomes a condition on the conditional variances $\mu_n^2 = E_{n-1}(u_n^2)$, and theorem I of the note [1934c] is a 0-1 law for square integrable martingales, the Borel-Kolmogorov condition on the series becoming a condition on the series of the μ_n^2. For this one wold conclude that Lévy was the father—not the pioneer or the prophet—of the theory of martingales. He handles the concept by analogy with Kolmogorov's theory without seeing its central role in the theory of processes and without wanting to make a theory of it (68).

The article developing the note [1934c] under the same title [1935b], was submitted in September 1934 and appeared only in May 1935 but its results go back to 1934, the year when Lévy obtained so many new results that he could only publish them only over one or two years. See in particular his [1935e], written in November 1934, which presents, from another point of view, the theory of connected variables in the sense of Lévy (i.e. Lévy's martingales). Finally he brought everything together in hiss great work written in 1936, [1937], a unique classic in the field of probability theory—finite or denumerable—of the last 300 years.

The article [1935b] has a preliminary part which has no obvious relationship with the note entitlled §1 "denumerable probabilities and the theory of measure" which is a direct result, as we will see, of reading the first piece by Jessen [1929]. In §1 Lévy specifies in as detailed a way as possible, and for the first time, the theoretical framework in which he works, when he uses denumerable probabilities for series of independent variables. This section is included and extended in section 39 of his 1937 treatise. It is thus within an explicit mathematical framework and in this §1 that Lévy's lemma is first stated along with with a very simple and perfectly acceptable proof, p. 86-88:

There is given a sequence of independent random variables $x_1, x_2, ..., x_n, ...$ each with the same uniform distribution on [0,1], (defined in the sense of Jessen-Lévy) and an event E

which "depends" on this sequence, $P(E)$ and $P_n(E)$ denote respectively the probability of E before the determination of the x_ν and after the determination of $x_1, x_2, ..., x_n$ and according to the values of these assumed known variables. One has then:

Lemma I: If an event E has probability α, the sequences realising this event, except in cases of null probability, satisfy also the condition

$$\lim_{n\to\infty} P_n(E) = 1$$

The work of 1937, § 41, sets Lemma I within the most general framework of Lévy's theory of denumerable probabilities:

Taking a "sequence X_n of variables independent or not", defined on the interval [0,1] provided with uniform measure, so that the variables appear truly like measurable functions definite on the unit interval. Lévy showed in § 39, that it is always possible, in the case independent of [1935b] as in the dependent case.

A property E of the sequence X_n is given. That is, states Lévy, the set of reals in [0,1] for which this property holds. Expressing its probability as $\Pr.\{E\}$, that is to say, states Lévy, the measure of this set, and $\Pr_n\{E\}$ its conditional probability given $X_1, X_2, ..., X_n$ assumed known, a concept that Lévy defined in § 23, (according to [1936d]). One has then:

Theorem 41. - Except in cases of which the probability is null, if $\Pr.\{E\}$ is determined, $\Pr_n\{E\}$ tends, for n infinite, towards one, if the sequence $X_1, X_2, ..., X_n, ...$, satisfies the property E, and towards zero, in the contrary case.

What in current language one writes, as we recalled in the Introduction, in an obvious notation:

$$E^n(1_E) \to 1_E \text{ a.s}$$

And this 0-1 law contains all known 0-1 laws, in particular that which Lévy attributes to Jessen in [1935b], p. 89, note 1, and which, in [1937], p. 130, he restores in Kolmogorov: If $\Pr_n\{E\} = \Pr.\{E\}$ for an infinity of n, $\Pr.\{E\}$ can only take the values 0 or 1. It is the case when E is an asymptotic event relative to a sequence $X_1, X_2, ..., X_n, ...$ of independent variables, for example if E is the event of Borel: "the case is favorable an infinity of times", and the lemma of Borel joined thus finally the lemma of Lévy.

To summarise. In the spring 1934, before Jessen and Lévy began their epistolary relationship, the first saw his theorem as an extension of the Fubini-Lebesgue theorem of 1907-1910, and the second saw his lemma as an extension of Borel's lemma of 1909. It is time to consider their letters.

4. A correspondence begins

In the spring of 1934 the brand new Institute of Mathematics was inaugurated in Copenhagen under the direction of Harald Bohr. It was built with a donation from the Carlsberg Foundation on the model of the Institute of Theoretical Physics, which the Rockefeller Foundation had financed for Niels Bohr, of the Institute Henri Poincaré in Paris and the Institute of Mathematics at Göttingen (69). There were funds for visitors and Harald Bohr could invite foreign lecturers of reputation to speak in Copenhagen. Among them was Paul Lévy, who seems to have had good relations with Bohr. Lévy was always more esteemed abroad than in Paris and there was nothing surprising about this invitation which came about in the beginning of April 1934.

Lévy tells us that he presented his brand new theory of integrals whose elements are independent random variables before the Mathematical Society of Denmark on April 9, 1934 (70). One can imagine that the audience was rather surprised by Lévy's performance but Harald Bohr, a man with a cool head and perfect manners, could at least grasp that it was a matter of integrating functions with an infinity of variables and indicate to his guest that his student Børge Jessen, then in Princeton, had done much work on the subject. In any case it can be shown that Bohr gave Lévy Jessen's articles on the subject in German and in Danish, since Lévy quotes [Jessen 1929b] in his article [1935b], p. 86, note 1 and that paragraph 1 of this article seems a kind of criticism or commentary on Jessen's theory. When Jessen returned to Copenhagen in September 1934, he wrote to Lévy, undoubtedly on Bohr's advice. We have not found this letter but it must have accompanied part of his great article in *Acta Mathematica* [1934a], which he had just received. This letter is the starting point of the correspondence which is published in Section 5.

Notes to the Introduction Sections 1-4

(1) [Jessen, 1934a], §§13 and 14, and [Lévy, 1935b], pp. 88-89, included in [Lévy, 1935e], pp. 6-7 [Lévy ,1936a and b], and [Lévy, 1937], n° 41. (1)

(2) [Lebesgue, 1903], included in [Lebesgue, 1904], pp. 124-125, [1906], p. 13, developed in [Lebesgue, 1910] and from 1914 incorporated in all the major European treatises of analysis. The theorems on differentiation almost everywhere of Lebesgue, La Vallée Poussin, Denjoy, etc. are the first known statements of the theorem on increasing martingales. The theorems predate the term "almost everywhere" which was introduced by Lebesgue [1904] and then adopted generally (e. g. Lebesgue [1904], second edition, 1928, page 179, note 1). See also [Kahane, 1988]. (2)

(3) [Borel, 1909] and [Kolmogorov, 1933] for a definitive version. Borel's theorem was recognised officially within the framework of the theory of decreasing martingales by Doob [1948]. The term "almost sure" was not yet standard in the thirties, when the usual expression was "convergence with probability one." Lévy seems to have been one of the first to have adopted "presque sûrement" after 1930, though Fréchet in his courses at the IHP favoured "presque certainement", without quite imposing it [1936], p. 225. Of course the terminology of probability, like the concepts themselves, remained somewhat fluid until the fifties. (3)

(4) [Andersen, Jessen, 1946, 1948b] and [Doob, 1953]. The Andersen-Jessen formulation and proofs are reproduced in [Hewitt, Stromberg, 1965], chapter V, § 20 - 22. For the Moscow school, see Kolmogorov [1950] and Bogachev [2006], vol. II, p. 469. (4)

(5) There are simplified proofs of the martingale theorem in [Doob, 1961], [Chow, 1962], [Meyer, 1966],…, and of course [Lévy, 1935b]. The demonstration given here follows [Edgar, Sucheston, 1976] and is close in spirit to Lévy's proof, which did not isolate the concept of stopping time but nevertheless used it implicitly to great effect.

[Halmos, 1950], p. 213, theorem B gives Lévy's statement and original proof, expressed in slightly pasteurised terms and less esoteric than in the inimitable original to which one will want to turn to taste the salt and the bitterness of Lévy composition. Lévy's proof is the first "direct" demonstration of the almost sure theorem and Jessen ended up by acknowledging this, tacitly at least.

We have borrowed the adjective "pasteurised" from G. Choquet's very beautiful foreword to the Lebesgue-Borel correspondence edited by Dugac [2004], page 5: "Mathematical activity cannot be reduced to the pasteurised theorems which sleep in the journals of libraries; their genesis, which would reveal the operation of creative thought, seldom appears in printe printed statements. The mass of those millions of theorems resembles those coral masses which increase each day, but are exhausted as soon as the living corals that secrete them die. "

Pasteurisation is necessary to ensure diffusion across great surfaces, but it tends to sterilise the life of which the historian must give an account. Norms from Brussels or Geneva change nothing, a history of pasteurised mathematics misses the point and can be at best only pasteurised history of mathematics. To recover the life and the creative thought, it is necessary to look elsewhere and letters are an invaluable resource. This is why Choquet himself fought against winds and tides to have the letters of Lebesgue published, for without them one can hardly grasp the genesis of one of the most fertile theories of the 20th century, a theory of raw milk which could survive pasteurisation only in a faded form, exhausted and tasteless. We should add that Choquet was a rarity amongst Paris mathematicians in welcomng martingale theory into his seminar on potential theory at the end of the fifties and in encouraging the work of Meyer, Courrège, Dellacherie and so many others. (5)

(6) [Barbut, Locker, Mazliak, 2004], a superb and fundamental work referred to as BLM. (6)

(7) The Jessen archives are described on the site http://www.math.ku.dk/arkivet/jessen/bjpapers.htm. It was from this well constructed site that we learnt of the existence of the Jessen-Lévy correspondence. We are very grateful to the authors of the site, particularly K. Ramskov and S. Elkjær, and also to Jesper Lützen and the Committee of the Archives of the Institute of Mathematics of the University of Copenhagen. (7)

(8) On Harald Bohr, see the *DSB*, [Jessen 1951], [Ramskov 1995]. Bohr was first director of the Institute of Mathematics of the University of Copenhagen which was founded in 1934. He was a very important figure, not only in the development of Danish mathematics, but also in the beginning of what is now called "the internationalisation of mathematics". In the early 1930s Harald and his brother Niels were influential advisers to the Rockefeller Foundation. Jessen's theorem, in its way, was a concrete expression of this new way of doing of mathematics, based as it was on contacts with the principal schools of the old and the new world. On this subject there is an excellent book by R. Siegmund-Schultze [2001].

Biographers of H. Bohr never fail to recall that he was a member of the Danish football team at the London Olympic Games of 1908, which beat the French team 17-1 in the semi-finals, after beating France 9-0 in the first round. There were five teams in the tournament and in the final Denmark lost to Great Britain 2-0. (8)

(9) [Jessen 1949] gives references for the proofs of Bochner, Riesz, La Vallée Poussin, Weyl, and Wiener, as well as proposing another. The literature on this subject beginning in 1925 and continuing for the next ten years is very important. The bibliography below contains only a small sample of titles but among them is a paper by Ellen Pedersen, Jessen's future wife. We might at least notice the almost periodic functions of Stepanoff [1926], the periodic pseudo functions of Paley and Wiener [1934],… In the thirties, von Neumann and Weyl showed the links between the Bohr functions and the theory of group representations. See Weil [1940], ch. VII, for an account of this theory. After the war, Bohr's almost periodic functions were extended to distributions [Schwartz 1950], ch. VIII § 9, etc. (9)

(10) For this subject see Pier [1990, 1994] and [Kahane 2004] who also provide references. (10)

(11) Jessen makes this precise in [1934a], p. 252: "The present author was led to the theory in connection with some investigations by Bohr concerning the distribution of the values of the Riemann zeta-function, which were carried out in collaboration with

the author.” We will not detail these investigations for Jessen did not return to them in this form in [1934a].

Functions in an infinite number of variables have a long prehistory, which includes the theory of infinite determinants of Poincaré [1886] (for which see Riesz [1913]), but their history really begins in 1906 with Hilbert's theory of integral equations, [1912], in which he introduced and used the Hilbert space l^2; see also Hilbert [1909], Fréchet [1909] and Dieudonné [1981]. However the idea of using functions in an infinite number of variables to study the Riemann zeta function and almost periodic functions appears to be Bohr's. He was followed by Jessen who must have exceeded all his mentor's hopes. (11)

(12) Jessen knew the Lebesgue theory from the treatise of Carathéodory [1918/1927] of which he had made a thorough study, as Christian Berg tells us (this issue), and also, it seems, from Julius Pal, a Hungarian mathematician established in Copenhagen, who had taught Jessen. Pal worked with the Hungarian School and in particular with F. Riesz and was very familiar with the new functional analysis of Riesz where the Lebesgue integral played a central role. On this point there is an interesting article, Filep and Elkjær [2000]. (12)

(13) “Übertragungsprinzip” which Jessen translates as “Overførelsesprincip” in his magister's thesis [1929a], p. 44, and and as “Transferring principle” in his [1934a] §7.

The memoir by Riesz [1910] is the only reference Jessen gives in his magister's thesis. However, by the end of 1929, and undoubtedly after his visit to Riesz, he knew that Lebesgue and La Vallée Poussin had used such a principle around the same time. Thus in his doctoral thesis [1930] p. 19, Jessen cites Lebesgue [1910] p. 402ff., who used the Hilbert curve to transfer his theorems on differentiation and La Vallée Poussin [1911] who did substantially the same. When he corrected the proofs of his Oslo talk, during 1930, Jessen added the same references in a note; cf [1929b], p. 134, note 1.

However questions of attribution and dating are never easy, especially when they concern a principle like this which imposes itself naturally. Already in a note [1907a] F. Riesz had used his principle of transfer to go from functions of a single variable to functions of two variables. In a note [1899] Lebesgue had used a “principe de transfert” to extend Baire's theorem for functions of one real variable to the case of two variables. For this purpose he used the Peano ([1890]) space-filling curve, still without seeing—he had no use for it—that this curve preserves the measure that Borel had just defined in his course [1898]. Lebesgue returns to the method, extending and improving it, in his famous memoir [1905b], pp. 193-201, where measure does not intrude either. But from the first edition of his *Intégration*, [1904], pp. 116-117, Lebesgue used the Peano curve to construct the plane measure he needed for the “geometrical definition” of his integral (see [1903-1905] and the second edition of [1904], p. 137ff.). This method is included in the second edition of 1928 of the same work, p. 44, where Lebesgue adds in note 2, that his method, “different from that of MM. Peano and Hilbert, can be used for spaces with an unspecified number of dimensions and even for spaces with a countable infinity of dimensions”. This note written in 1926 could be a version of Jessen's generalised principle of transfer, and as such would be neither the first nor the last, but it could be also the pat of a tired old cat wanting to push away that funny bird, Lévy, when he presented related ideas to the Hadamard seminar of 1924. The question comes up in the correspondence published below. This second hypothesis would also lend some credence to Lévy's priority claim which we will be discussing below: perhaps Lebesgue recognised in Lévy's talk precisely what Lévy would assert ten years later, but Lebesgue (like Lévy) did not take the trouble to write down. Borel became a mandarin of the radical Republic and Baire was left to die on the shore of Lake Léman. They were no longer there to play mathematics with Lebesgue; Lévy was a bad player, like Lebesgue.

On the history of the transfer principle there is the very nice article by Riesz [1949]; see in particular p. 37-38. At the beginning of the 40s the principle was put into an abstract framework as the "isomorphism theorem" by Halmos and von Neumann [1942] and independently by Rokhlin [1949] around 1940. Halmos [1950] n° 41 has a statement and there are more references in Bogachev [2006] vol. II, p. 549. The latter is a remarkably scholarly work with very interesting historical comments and a bibliography of over 2000 titles. (13)

(14) See Lebesgue [1904] and Carathéodory [1918]. Jessen follows them closely. Halmos [1950] chapter II, etc may also be consulted. (14)

(15) [Steinhaus 1923] uses the axiomatic set-up of Sierpinski [1919]. This article had a very important role in the development of the mathematical theory of probability, especially in Moscow where it ws followed by Khinchin [1924] and then by Kolmogorov, [1925], [1928] etc. (15)

(16) [Steinhaus, 1930b and c], included and extended in [Kaczmarz, Steinhaus 1935], chapter 4, n° 7, p. 134-139, which gives the construction of the correspondence, and in Steinhaus [1936] and [1938]. Jessen quotes Steinhaus [1930c] in his thesis of 1930, p. 29 notes, and in [1934a]. In his letters to Lévy we find Jessen recognising the independent priority of Steinhaus, then that of Lévy (more arguable) and that of others, known or unknown, (Denjoy, Wiener, Cantelli, Mazurkiewicz,…)

We may note that Steinhaus [1936] proposed another correspondence based on the generalised Peano curve and indicated that the earlier construction of Jessen [1929b], based on the generalised Hilbert curve was not so well adapted.

From his construction [1930b] Steinhaus deduced that *in general* entire series whose coefficients have arguments chosen at random have a singularity at all points on their circle of convergence. This result gave a precise sense to the prophetic statements of Fabry [1896], p. 398-399, and the enigmatic ones of Borel [1896], [1897]. This work of Steinhaus was developed soon after by Paley and Zygmund [1930-1932], followed by many authors including Jessen [1934a]. See Kahane [1963, 1985] and, for recent references and developments, [Marcus, Pisier 1981], [Kahane, Lemarié-Rieusset, 1998],…. (16)

(17) Jessen quotes [La Vallée Poussin 1915, 1916] and uses his terminology and method of "derived in nets" ([1916], chapter IV). The theorem of La Vallée Poussin, like Lebesgue's, is a theorem of increasing martingales, the nets of intervals of La Vallée Poussin being of filtrations of finite type, and the $\Delta_n(x)$ the conditional expectations according to these filtrations. In 1946 Jessen recognizsed this. (17)

(18) The first "Fubini's theorem" for the Lebesgue integral in the plane is in Lebesgue's thesis [1902], n° 37-40 and it treats the case of bounded measurable functions. In 1910 Lebesgue returned to the subject but meanwhile several authors had stated and proved Fubini's theorem in a more or less complete way, in particular Beppo Levi in 1906, Hobson and Fubini in 1907, Tonelli in 1909. There is a detailed

study of this complex development in T. Hawkins [1970]. Hawkins finally attributes to Tonelli the first complete demonstration of the theorem stated by Fubini, for integrable functions of two variables. The modern formulation of Fubini's theorem is due to La Vallée Poussin [1914, 1916]. Fubini is credited only for his extension of the theorem to nonmeasurable functions, which is also partly in Lebesgue's thesis, [1902] n° 40, as La Vallée Poussin points out in [1916], p. 53, note 1. (18)

(19) In a note Jessen indicates that he is following Riesz [1909], who was, in effect, the first to name "convergence in measure", although the concept had already been used, without being named, by Borel and Lebesgue in 1903. See Bogachev [2006], vol. 1, p. 426 for references. The relationships between convergence in measure and the other modes of convergence of the theory of the functions—convergence almost everywhere, convergence in mean,… —were published several times by various authors. They are laid out in Fréchet [1921b], who was one of the first to show that convergence in measure "corresponds" to (without being identified with) convergence in the sense of the theory of probability of Bernoulli, Moivre and Laplace, the Laplacian double approximation of "très probablement très proche." See Slutsky [1928a], Fréchet [1930] and [Cantelli 1935]. The two concepts fused in the axiomatic framework of Kolmogorov [1933] and also in that of Jessen-Steinhaus-Lévy [1930/1937], but for a long time they were considered distinct. On this subject see [Doob, 1994]. For Jessen, as for Doob, the problem did not arise: it had no meaning. (19)

(20) In the list of Oslo lecturers is another young Danish mathematician, Georg Rasch, (1901-1980). Rasch was slightly older than Jessen and had submitted his magister's thesis in 1925 and was due to submit his doctoral thesis in the coming months. He was appreciated and supported by Nørlund, a professor at the University of Copenhagen from 1922 in a chair specially created for him. There were very few mathematical positions in Danish universities but if one became vacant Rasch had a reasonable chance of success. However Rasch's mathematical career was destroyed at a stroke in the spring 1930 when Jessen, with Bohr's backing, submitted his doctoral thesis—and what a thesis! Undiscouraged, Rasch turned to the new Anglo-Saxon statistics of Fisher, but also of Neyman, Pearson and others. This was an unknown discipline in Denmark which remained attached to the continental school of statistics and actuarial science. Like Jessen and at the same time, Rasch obtained a Rockefeller scholarship but in his case it was to study with Fisher in England. When he returned he trained in the new methods the leading Danish statisticians of the next generation, among whom Anders Hald, an important statistician and a remarkable historian of statistics, became a friend. Rasch was eventually appointed university professor of statistics in Copenhagen, but only in 1962 and only in the Faculty of Social Sciences. It seems a just return that Rasch's posthumous fame stands a good deal higher than Jessen's. Only scholars know Jessen's theorem while Rasch's models are cited, applied and extended every day. On the life and work of Georg Rasch there is an interesting thesis by L.W. Olsen [2003]. (20)

(21) Bohr's appointment to the University of Copenhagen in 1930 had set off a chain of movements. His old position at the Polytechnic School went to A. F. Andersen who was docent at the Royal Veterinary and Agricultural School and Jessen was at once appointed to replace him there. Aksel Frederik Andersen (1891-1972), an analyst of Bohr's school, was very interested in mathematics teaching in both school and

university. In particular he took part in revising the great treatise of analysis by Bohr and Mollerup. He retired in 1960. (21)

(22) The theorem on differentiation in L^1 was not given by Lebesgue, who did not use convergence in mean, but it was known to Riesz from 1910 and undoubtedly earlier; convergence a.e. had been proved (Lebesgue) and convergence in L^1 had been defined (Fischer, Fréchet, Riesz, Schmidt). However, this result does not appear explicitly in the few works that Jessen had read, and he only learnt of the role of "strong" convergence at the time of his stay in Szeged; on this see note 27 below. (22)

(23) As we saw in the introduction, the form of the simple functions matters little, but Jessen does not know yet, it seems, that the functions depending only on a finite number of coordinates are dense in L^1, by construction, an argument from measure theory that Lévy grasped and used at once for his "lemma", but which appeared nowhere else in the current mathematical literature. So Jessen, without realising it, proved the martingale theorem in L^1 twice, first in the form of the theorem on differentiation, following Riesz, and then in the form of Jessen's theorem which he deduces from it. From the time of Cournot at least, it has been known that this complication is characteristic of creative mathematics where theorems are discovered by stray wandering in the forest and proved using whatever arguments are available. It is work of a different kind to produce proofs that are simple, clear and well-organised; these seldom come first. In this art too Jessen became a master, to the point perhaps where he forgot the thick jungle in which were hidden the new theorems, true or false. (23)

(24) These themes are developed in the article [1932b] and the last part of [1934a]. They were taken again by Hunt [1955] and are still the object of research; note 16 above gives references.

Jessen [1932b] was inspired in particular by a very fine article of Jensen [1899]. It was rumoured that Jensen had proved Riemann's conjecture and on Jensen's death, Rasch was charged with seeing what could be found in the many papers he left. The interesting details of this affair are related by Olsen [2003]. It may be recalled that J.L.W.V. Jensen (1859-1925) was chief engineer at the Copenhagen Telephone Company, where A.K. Erlang also worked. For the latter see Brockmeyer and Al [1948]. (24)

(25) In Princeton Jessen discovered the new theory of probability, in particular the work of Kolmogorov on series of independent variables which includes his generalised Fourier series with variable coefficients on the circle (without being drawn randomly they vary however freely). Jessen also had occasion to meet Wiener. According to the *Bulletin of the Amer. Math. Soc*, (March 1934), p. 177-178, one of the sessions (on December 27) of the Society's meeting at MIT, December 26-29 1933, was a "Symposium of invited papers on the topic of probability". The invited contributions were: E. Hopf, MIT, "Remarks on causality and probability", F. Bernstein, Columbia University, "Foundations of probability in the natural sciences", G.E. Uhlenbeck, University of Michigan, "The probability of position in has canonical together", and N. Wiener, MIT, "The Brownian motion". A fifth lecture was planned, "Some analytical problems relating to probability", to be given by Dr. B. Jessen, Institute for Advanced Study, but that it did not take place "on account of illness." These lectures (including Jessen's) were published in the *Journal of Maths.*

and Phys., 24,1 (1935), p. 1-35. Jessen's lecture (p. 24-27) is a short summary of his 1934 article. Thus Jessen was fully aware that the theory of measure in infinite dimensional spaces was related to the new theory of denumerable probabilities. He had cited Kolmogorov's *Grundbegriffe* in his 1934 article but it is clear that he had not had time to study it in detail, as we will see. (25)

(26) See [Daniell 1919] who uses the extension of the integral, in the manner of W.H. Young and F. Riesz, [Carathéodory, 1918], [Feller-Tornier 1932], [Kolmogorov 1933], who use the extension of the measure. (26)

(27) In a note to § 15 (page 278), Jessen indicates that he had originally proved his two "Fubini heorems" for convergence in measure and he adds: "It was pointed out to me by Prof F. Riesz that the (well-known) argument used above would give the same theorems for the more convenient concept of strong convergence. Finally it was Prof Danielll who suggested to me that the theorems should be true for convergence almost everywhere. " This suggests that Jessen put this finishing touch at the time of his stay in England and at the United States in 1933-1934, with generous help from Daniell, who would therefore have been the first to have had the idea of of the almost sure version of Jessen's martingale theorem. On the very remarkable personality of Daniell, see a recent nice article [Aldrich 2007], which is very complete. (27)

(28) Jessen writes in a note p. 285, in connection with the result of Riesz: "A proof of the theorem by means of the differentiation theorem of § 9 was given by Prof F. Riesz and communicated to me by Dr. Kalmár. It was this proof that suggested to me the proof of the theorem in § 14. I note from a letter from Prof Zygmund that a proof on similar lines was given by Paley." Jessen had certainly met Laszlo Kalmár, at the time of his stay in Göttingen, where Kalmár, a student of Fejér and Riesz, was discovering his vocation in mathematical logic. We do not know where Paley proved his "theorem on martingales", but it is a theorem he must have known at least implicitly, given his admiration for Borel's "denumerable probabilities." Jessen probably never met Paley. He was already dead when Jessen arrived in Cambridge

Symmetrically, F. Riesz recommended Jessen's memoir [1934a] "for a detailed exposition [of the principle of transfer using the method of nets] (for the case of an infinity of variables) and for bibliographical indications." (Riesz [1936], p. 193, note (5)), a faint recommendation which could have only half-pleased him. (28)

(29) These applications principally concern Fourier theory in Q_ω, random Fourier series, developments taking off from the articles of Paley, Wiener and Zygmund, but also orthogonal systems (e.g. Rademacher [1922], [Kaczmarz, Steinhaus, 1935]), functions almost periodic analytical random, Dirichlet series (Carlson [1933]),…. For these subjects, see the references given above, notes 16 and 24. On the other hand, Jessen [1934a] contains no overtly "probabilistic" applications, unlike the article he published a little later with Wintner [1935] which is described below. (29)

(30) It is not only a matter of difference in language; Jessen's martingale theory knows nothing of the concept of stopping time and of the stopping theorem, keystones of the probabilistic theory of Ville and Doob, but also of Lévy. On this subject see [Bretagnolle 1987], p. 241, who gives a very intelligent modern reading of [Lévy, 1935b]

We know from [Andersen-Jessen, 1948b] that Jessen only noticed Doob's first article after the publication of the 1946 article. Moreover Doob [1940] does not yet contain the stopping theorem. Doob, for his part, seems to have had access to Jessen's work only in 1948. As for the book by Ville [1939], Jessen wrote a very short review for the *Mat. Tidsskrift* apparently without noticing any connection with his own work; this was not the case with Doob who immediately understood the interest and the originality of Ville's thesis.

There was a difference of a philosophical or poetic nature. Jessen did not adhere to (or only very slightly and with evident discomfort) the philosophy of chance that Lévy inherited from his masters Bertrand, Poincaré and Borel (for this subject see *JEHPS*, December 2006). Jessen does not draw at random the terms of his series, which are parameters but this deprives him of probabilistic intuition and the mathematical concepts linked to it. One can do probability theory without chance as one can do mechanics without force, but, Cournot would add, what is gained in logical clarity is lost in richness of reasoning. (30)

(31) The *DSB* has an article on Aurel Wintner (1903-1958). Wintner is credited with 437 articles and 9 books. No doubt some of these hundreds of articles had only a passing interest (Doeblin is more acid in his notebooks) but they testify to his astonishing publishing activity and some of them are first class, for instance those written with P. Hartmann, and undoubtedly those we are considering here. We do not know when Jessen and Wintner met. Wintner spent part of the academic year 1929-1930, on a postdoctoral scholarship at the Copenhagen Observatory which was then directed by E. Strömberg. He certainly met Harald Bohr and may have been present at Jessen's doctoral defence in the spring of 1930. To learn more, it would be necessary to analyze the voluminous Jessen-Wintner correspondence, which we were not able to consult, There are letters in the Jessen papers at the Institute of Mathematics of Copenhagen and in the Wintner papers at the Mr. S. Eisenhower Library of Johns Hopkins University. According to the library's site (http://ead.library.jhu.edu/ms281.xml#id2620966). There is, in addition, Lévy-Wintner and Doob-Wintner correspondence and it may be interesting to go through these letters. (31)

(32) The literature on this subject was very important in the thirties and subsequently: see the works of Kahane in the bibliography although these give only a sample of the work on infinite convolutions around 1935. There is a large bibliography in Jessen, Wintner [1935], of which we have transcribed only one part. (32)

(33) The Fourier transform was introduced by Laplace in 1810 precisely for the purpose of evaluating the asymptotic laws of the sums of independent random variables. For this topic, see the great work of Hald [1998]. (33)

(34) Jessen and Wintner cite Khinchin, Kolmogorov [1925], Kolmogorov [1928/1930] and Lévy [1931c], works with which they were only superficially acquainted. (34)

(35) The existence theorem for an infinite product measure in an abstract framework was stated for the first time, with an incomplete proof, in [Łomnicki-Ulam, 1934]. Von Neumann gave a complete proof in 1934 in his course of Princeton, although this was only published in 1950. It is likely that Jessen, who was in Princeton from

September 1933 to July 1934, followed von Neumann's course. In any case, he published his own proof in 1939, [Jessen, 1934-1947, 4]. Other authors gave proofs around the same time, in particular Doob [1938] (the validity of which Jessen challenged) and Kakutani [1943]. However, Jessen's proof, translated into English in [Andersen-Jessen, 1946], n° 23-24, is very simple and is valid for a general set of indices. It served as model for the authors of the treatises of the fifties, in particular Halmos [1950], n° 58, pages 157-158, and Loève [1954], chapter I, section 4.2. For a history and for references.refer to [Andersen Jessen, 1946], p. 22, note 1 and [Andersen, Jessen 1948a], n° 3. (35)

(36) Jessen and Wintner do not refer to Kolmogorov at this point. Thus when they were correcting the proofs in February 1935 neither knew Kolmogorov's 0-1 law, nor moreover the *Grundbegriffe*, which they do not quote. We will see that Jessen belatedly quotes this law in his correspondence with Lévy. Dunford, Tamarkin [1941] may be consulted for other abstract versions of the 0-1 law and of Jessen's Fubini's theorems. (36)

(37) On the other hand, as is well known, the concept of martingale is as old as the theory of probability, under the generic and polysemous name of "fair game". The method of martingales, which associates a fair game with any unfair game, is in Pascal and especially in Moivre. Starting from an unfair game of heads or tails the latter constructed a martingale (exponential) which is very similar to the martingale f_n of Jessen and Wintner. Naturally Moivre did not use Jessen's theorem of which he could have had no conception, but the stopping theorem of Borel-Doob, with against direction, lacking the justification which would only truly appear in the 1950s, around three centuries after calculations began. For these matters, see the treatises of Moivre [1718-1756] and Bertrand [1888] and also the thesis of S. Eid [2008] which gives all the references.

Jessen and Wintner, [1935], § 16, add two applications of the (abstract) 0-1 law of Jessen. The first shows that the probability of convergence of a series of independent variables is 0 or 1, a well-known result to "probabilists." The second, on the other hand, is original: it is the Jessen-Wintner law of pure types which states that infinite convolutions of probability laws are pure, i.e., they are discrete, singular or absolutely continuous and not mixtures of the three. There is an account in Breiman [1968], chapter 3, § 5. (37)

(38) Peder Oluf Pedersen (1874-1941), Danish engineer and physicist, specialised in electrical engineering. He was director of the Polytechnic School from 1922 to his death in 1941. (38)

(39) There is a very clear exposition of the work of Sparre Andersen and Jessen in the treatise of Hewitt and Stromberg [1965], chapter VI, § 22. The article [1946] reproduces Jessen's fourth article of 1939. The article [1948b] re-expresses everything in the framework of set functions which is natural and simplifies things considerably. It also regulates a minor dispute which seems to have arisen between Doob and Jessen, who had just realised that they had treated the same theorem without knowing it (Lévy being except category, and being especially used, when it is quoted, to fold back the claims exaggerated of the other). See the account in Doob [1953], pp. 630-632, but also that of Moy [1953]. Shu-Teh Chen Moy (1920-1969) is

an interesting mathematician in more than one way and his article on Jessen-Doob is quite clear. The theorems of Doob, Jessen and Lévy are equivalent. (39)

(40) With regard to the first question, all the attempts to extend the Daniell-Kolmogorov [1933, p. 24-30] theorem to an abstract framework proved to contain errors (in particular those of [Doob, 1938, p. 90-93, 96-97], [Halmos 1941, p. 390] and [Andersen 1944]). The first counter-examples to such an extension are due to [Andersen-Jessen, 1948a] and [Dieudonné, 1948]. The case of dependent variables with values in an abstract set cannot be treated in general like that of real variables, and this impossibility is related to the non-existence of conditional probabilities, or of "disintegrations" in an abstract framework. For a current view of these questions, with interesting historical notes, see [Dudley, 2002] and [Bogachev 2006] vol II, chapter 10.

Christian Berg has very kindly given us a copy of the Doob-Jessen and Jessen-Dieudonné correspondence bearing on the simultaneous publication of their counter-examples to the abstract Daniell-Kolmogorov theorem. We reproduce in an appendix these correspondences from the Archives of the Institute of Mathematics of Copenhagen, which gives new precise details on this important subject.

In the very nice historical note in the last of the *Integration* volumes, N. Bourbaki [1969], remarks, p. 121, note 12: "It seems that it is the absence of a satisfactory theory of disintegrations which marks the limit of the theory of "abstract" measure. This difficulty reappears in an insistent way in probability theory in connection with conditional probabilities." What is not false in the abstract, but erroneous in is undoubtedly lived and the history of the known as calculation, which particularly seems to have suffered from this insistent difficulty in second half of the 20th century, and hardly in 1969.

The second question is more subtle. Jessen's theorem passes without difficulty with to the case of decreasing filter sets (see for example Hewitt-Stromberg, op cit. note 38 above). The increasing filter case was put in jeopardy by Dieudonné [1950], a serious difficulty found in the theory of the filter martingales and the differentiation theory of the 50s and 60s; see Krickeberg [1956] and Krickeberg-Pauc [1963]. One can however obtain relatively general Jessen theorems in the increasing case; for this see the beautiful article of Dorothy Maharam [1958]. (40)

(41) Erik Sparre Andersen (1919-2003) studied mathematics at the University of Copenhagen, where he worked chiefly with B. Jessen. In 1945 he became an actuary, a career in which he continued for a long time; thus his [1957] is a classic of the actuarial literature. In parallel he published mathematical work which was always very original. From 1948, after a stay at Cornell University with Feller, he worked on fluctuations of sums of independent random variables (an actuarial topic). For the time his results were truly astonishing and his combinatorial methods revived the theory in the United States; see e.g. [Sparre Andersen, 1949, 1953-1954], [Feller 1950,1966] and [Spitzer, 1964]. In 1958 Sparre Andersen was appointed professor of mathematics at the University of Aarhus and then in 1966 at Copenhagen.

One can assume that, during his stay at Cornell in 1948-1949, Sparre Andersen had occasion to present Jessen's martingale theory and conversely to take note of Doob's work. For a moment a collaboration between Doob and Jessen was contemplated (cf Andersen, Jessen [1948a] p. 5) but it did not happen. How could it? The theory of such processes was the great affair of Doob's life, as it was for Lévy, but it did not interest Jessen. (41)

(42) See, in particular, his scientific autobiography [Lévy, 1970], the recollections of his son-in-law Laurent Schwartz [1997], the thesis of Bernard Locker [2001] and also [Barbut, Locker, Mazliak, 2004]. This volume of annotated correspondence is invaluable for the proper understanding of the story and it would be good to have it to hand. It is referred to here as BLM with page number. (42)

(43) [1935a], p. 61, where Lévy wrote: "This work plays a particular part in my mathematical work, for, not only have I deliberately used idealist reasoning there, but I have there admitted without demonstration the compatibility of a certain number of axioms, which appeared to me to be essential for intuitive reasons, and consequently essentially subjective. " Some apply these adjective to all of Lévy's work, especially to his probability work, which is clearly exaggerated, as one can see from the example of Lévy's lemma. See [Bretagnolle 1987] and especially Locker [2001] and [2009]. Lévy reconsiders this article in his late correspondence with Fréchet; see *BLM*, pp. 300-301. (43)

(44) See BLM. We note that one of the first probability lecturers of the great seminar of the IHP, in March 1929, was Lévy, himself, [1930d], who excused himself for having so little to say, coming after "the remarkable lectures of M. Pólya", [Pólya, 1930]. (44)

(45) Slutsky had written in a note to the *CRAS*, of August 13, 1928, only a few days before Bologna, [1928a, p. 371, note (1)] : "Similar considerations apply to all the cases subject to the strong law of law numbers (the happy expression of M. Khinchin, [1928]), which, after being established for the Bernouilli case by M. Borel, has been studied for the last years by M. Cantelli, M. Khinchin, M. Steinhaus and by the author of this Note [1925]." The remark displeased Cantelli. On the affair, see [Cantelli, 1916-1938], [Benzi, 1988, 1995], [Seneta, 1992], [Regazzini 1987, 2005]. As is well-known, Borel [1909] gave two versions of his theorem, which to him, as to other scientists of the time, were distinct yet related. According to the analytical version, normal numbers (and even absolutely normal numbers) are of measure one on the unit interval. According to the probabilistic version, with probability one the frequency of heads in the game of heads and tails converges to one-half. His proofs, it has often been said, were, at the very least, cavalier, but Borel never condescended to change them. The analytical version of "Borel's theorem" was at once proved convincingly by a great number of mathematicians, including Lebesgue (from 1909—see [1991] and [1917]), Faber (in 1910), Hausdorff, Hardy-Littlewood, Rademacher, Sierpinski etc. The probabilistic version, which was harder to put into a recognised mathematical framework, was stated and proved independently by Cantelli (for the first time in 1917 in his own framework), and in the framework of Borel or their own, by Pólya, Steinhaus, Khinchin, Mazurkiewicz, Slutsky… . Thus by 1928, Borel's theorem was a classic known to all (except Lévy, it seems) and the theory of denumerable probabilities was coming to be recognised as a separate theory, the most evident sign of which was the paternity dispute in Bologna.

It would be very interesting to look in more detail at the motivations of the scientists who, mostly unaware of each other, set about proving the strong law of law numbers for Bernoulli variables at the beginning of the twenties, but, alas, it would take too long. For example, the proof of György Pólya [1921b] was motivated by a problem of the philosopher of science Hans Reichenbach, communicated to him by the Göttingen theoretical physicist, Paul Hertz, and which he discussed with the pacifist engineer Swiss Pierre Cérésole. All this was very far from the concerns of Italian actuaries and the spirit of Borelian mathematics.

On probability theory at the Congress of Bologna, see [Krengel 1994] and, as a last resort, an unreadable article in the *Journal de la Société Française de Statistique,* 144, 1-2, (2003), p. 135-226. Khinchin was present in Bologna, but it seems that he and Lévy did not meet, or at least Lévy recalled no meeting in his *Souvenirs* [1970], p. 108. In any case, what would they have had to say in 1928? Apparently nothing, for they did not know that in the thirties they would be competing on all the open topics of the theory of sums of independent variables, the domains of attraction in particular, before Gnedenko in Moscow and of course Doeblin at Givet overtook them. Without ever seeing one another, Lévy and Khinchin ended up publicly opposed, in the heat of the Cold War, on the philosophical and ideological foundation of the concept of probability, [Lévy, 1956]. We do not know what became of the Lévy-Khinchin correspondence which was surely very interesting between 1930 and 1940. (45)

(46) In October 1943 Lévy admitted as much to Maurice Fréchet, who was not in any way similarly afflicted. See BLM p. 207-208, where Lévy states that his periods of incapacity and apathy sometimes lasted for a year. This may explain the quiet years 1932-1933, between the two impassioned and overflowing periods, 1930-1931 and 1934-1935, but it cannot explain his being ignorant of denumerable probabilities for ten years. Lévy seems to have come to have come to terms with his moe or less extended absences by saying that they allowed his brain to be refreshed and reconstituted, but he did not acknowledge them readily, except as an excuse. For example, in his *Souvenirs*, Lévy refers to periods of nervous breakdown in 1921 which stopped him giving his book of 1922 the necessary breadth and elegance [1970], p. 58.

There is also the phenomenon of "dryness" experienced by a number of mathematicians, among them the greatest. The phenomenon is usually hidden (especially in research reports bound for academic authorities or for historians). It is difficult to understand the phenomenon properly, as it is to understand the opposite phenomenon, of exuberant creativity. There is not much literature on this topic, though the *Dieudonné* of P. Dugac [1995], p. 17 should be noticed. However in the case of Lévy, it seems likely that this temporary dryness was due essentially to the blocking of denumerable probabilities, which locked his intuition which was otherwise bubbling with impatience. One may speculate that the barrier came from Hadamard and his seminar and that to overcome it, the father had to be killed, but surely that would be an exaggeration. At all events, one may affirm that Mlle Mezzanotte released the richest part of Lévy' genius; see below note 59. (46)

(47) Hadamard asked Lévy to edit the posthumous works of Gateaux, in particular [1919a, b]. For the remarkable life and work of Gateaux, see the very complete and enthralling article by L. Mazliak [2007]. (47)

(48) These calculations were already in part in Borel's course of 1912-1913, taken down by R. Deltheil and published in the form of a book [1914], chapter V. They are detailed in the paper by Gateaux [1919] that Lévy published and reproduced in the lectures on functional analysis that Lévy gave at the Collège de France in 1919, [1922, 1951], p. 228-234. (48)

(49) [Wiener 1923], p. 132 says so after writing, "Now, integration in infinitely dimension is a relatively little-studied problem. Apart from certain tenative investigations of Fréchet and E.H. Moore, practically all that has been done on it is due to Gateaux, Lévy, Daniell and the author of this paper. Of these investigations, perhaps the most complete are those begun by Gateaux and carried out by Lévy in his *Leçons d' Analysise Fonctionelle*."

Wiener often travelled to Europe. He visited Lévy on several occasions in the 1920s, but it seems that the conversations on the Gateaux means occurred during the summer of 1922, at the time of Wiener's visit to Pougues-les-Eaux, where the Lévy family was on holiday. The *Souvenirs* of Lévy, [1970], p. 85-86 and the memoirs of Wiener [1956] agree on the point and so Pougues-les-Eaux may be regarded as the cradle of the mathematical theory of Brownian motion. (49)

(50) [Bachelier 1900], [Einstein 1905]. Wiener quotes [Perrin 1910] on the very irregular character of the observed Brownian motion—continuous but seemingly without a tangent (like the Breton coasts where he spent his holidays). See also Perrin [1913]. (50)

(51) [Lévy 1970], p. 98. (51)

(52) In 1934, in his astonishing memoir on processes with general independent increments, Lévy constructs his processes (Lévy processes) by interpolation and states that, at the end of this construction, "the probability appears as a Daniell integral." The method of interpolation is preferable, according to him (and one cannot fault him), to the "differential" method of Wiener which consists in dividing the time interval into n small intervals and considering the law of the differences of the values of the process on these intervals and then letting n tend to infinity. This method (of Gateaux-Wiener) lends itself well, by a passage to a suitable limit, to the calculation of probabilities and "probable values" of the functionals given by a simple analytical expression, [1934b], p. 344. Lévy adds in a note: "In his paper on differential space, without introducing this concept from the beginning as we do here, Wiener clearly showed that the average of a uniformly bounded continuous functional is a Daniell integral." This revenge on Wiener is also a revenge on Lévy, since the memoir of 1934 makes it possible to show how the stable laws of Lévy-1920 are interpreted naturally within the framework of the denumerable probabilities of Lévy-1934. (52)

(53) [Khinchin, Kolmogorov, 1925], [Kolmogorov 1928-1929]. Lévy took up this topic rather tardily in [1931c] using his own methods, based in particular on the concepts of dispersion and of concentration which he introduced in this memoir, p. 124-125, and which belong to the basic tools of his treatise of 1937 and of all his probability work. Lévy tackled the question of series of independent variables only in [1930a] and in [1930b], chapter II. (53)

(54) This explanation of Lévy's silence was suggested by Lévy himself. In a curious history of the concept of measure inserted in [1936b], p. 168, Lévy points out that measure as a tool existed long before the concept was isolated, adding in note (1), "One can make the same observation in connection with the appearance of the infinitesimal calculus. The tool existed in Archimedes, but he could not have had the idea that it was interesting to study unspecified curves and surfaces. The work of

Leibniz and Newton was made possible only by the progress of the concept of a function." For a theory to emerge, it is necessary not only that the tools have been fashioned, but there is something to which they can be applied or at least there is the idea that it would be interesting to apply them here rather than there. The Daniell integral or the Gateaux means, possibly re-examined by Lévy, were not enough to make Lévy interested in denumerable probabilities, he needed to believe that the theory was interesting and the belief was obviously lacking. To build a theory, tools are needed, but above all a sun is needed to illuminate it. Lévy's remark applies equally to the history of martingale theory, the subject of this issue of the *JEHPS*. (54)

(55) Lévy answers Borel in the introduction of his book of 1925. One could undoubtedly add to the objective reasons of the lapse of memory of the countable probabilities some subjective reasons, but it would not be very interesting. The brain of Lévy refused to pass to the countable probabilities and hardly saw any substantial difference between convergence in law and almost sure convergence. In this way one can partly explain his not particularly judicious rejection of Bachelier's work in the twenties. See on this point, Courtault and Al [2002]. (55)

(56) See especially the beautiful book by M. Barbut, [Barbut, 2007]. (56)

(57) Letters of January 8, 1937, BLM, p. 168. (57)

(58) This article [1929] was announced in a note presented to the CRAS on March 10, 1930. It was thus written in 1930. It establishes a celebrated formula given by Gauss to Laplace without proof. Kuzmin [1928-1932] proved it at the Bologna conference of 1928, without Lévy realising, or without him recalling. But it is easy to speculate that it was from a conversation in Bologna, that Lévy became aware of the problem of Gauss, although he says in his *Souvenirs*, pp. 88-89, that the idea came to him "one day" without any warning. (58)

(59) This principle can be found already in Borel [1912a] and Bernstein [1926b] but the strong laws of large numbers for dependent variables were not yet known in 1930. Lévy is clearly improvising or rather he had not properly read or had misunderstood the article of Mlle Mezzanotte; see the next note. He recognsed it in [1937] n° 69, page 252, where he writes in a note, in connection with the article of 1929: "I had been satisfied, with an application an application in mind, to state this theorem which I believed could be regarded as known." This was not the case and Lévy was ahead of the theory of denumerable probabilities without realising it. The strong law of large numbers that Lévy "applied" to continued fractions in 1929 was proved by Lévy in [1935c], [1936a] and included in [1937] in the section cited.

On the other hand, by 1930 several weak laws of large numbers for dependent variables were known. The earliest date from the work of Markov beginning in 1907 and developed after the 1914-18 war by Serge Bernstein and then absorbed in the new theory of "Markov chains" (also consecrated at Bologna), which would lead to the Markovian strong laws of large numbers in 1936 with celebrated contributions from Kolmogorov and Doeblin. (59)

(60) On Francesco Paolo Cantelli, (1875-1966), there is important work by E. Regazzini, and M. Benzi. Cantelli is truly the first modern "probabilist", and he proclaimed himself as such. He had little doubt that he had converted Lévy to

denumerable probabilities. Mlle Anna Mezzanotte published, between 1928 and 1938, some interesting actuarial and probabilistic work, in particular [1928] which seems to be the main source for Lévy. There exists, to our knowledge, no biography of Mlle Mezzanotte.

Lévy adored Italy and often spent his holidays there, until 1938 when the fascist statute on the Italian Jews was promulgated. Did he meet Mlle Mezzanotte in Bologna or elsewhere, or did Cantelli send him her 1928 paper? We do not know, but this text, of which Lévy read at least the first two or three pages, indicates very clearly, [Mezzanotte 1928] p. 333, that convergence in probability ("nel senso del calcolo delle probabilità") does not imply convergence with probability one. This made a strong impression on Lévy, who evidently believed the opposite. We are very grateful to E. Regazzini for obtaining a copy for us of Mlle Mezzanotte's remarkable piece. She was thus one of the inspirers of Lévy, and not one of the least. In his *Souvenirs*, Lévy tells a different story, without much conviction. He recognised that he had not done much before 1929, adding, p. 85: "I believe, without being able to affirm it, that it is thanks to Noaillon that I started to think of the various modes of convergence of probability theory. Up to then, I used convergence in the sense of probability theory for the condition, $\sum E\left(X_n^2\right) < \infty$, on a series of independent random variables X_n and I considered it obvious that this involves almost sure convergence." The second part of this quotation is certainly correct, but the first is hardly so, like everything that relates to that silent period Lévy no longer comprehends and which in any case he regrets. He tries to mask this as, for example, when he maintains that he was interested in the phenomena of contagion after hearing Pólya's lectures in 1927 or 1928, at the Institute Henri Poincaré (ibid p. 88), which was then under construction. Pólya gave his lectures to the IHP in March 1929, i.e. probably after Lévy's return to probability, [Pólya, 1930]. Noaillon is thus a decoy to hide something else. We will stick to the version of 1930, which does justice to the incomparable Mlle Mezzanotte. But it would be a mistake to remove Noaillon from the list of masters of Lévy, great and small. Paul Noaillon was indeed an important scientist in Paris between 1920 and 1940, one of faithful of the Hadamard seminar, like Lévy and all the others. Who was he exactly? We know only a little about him and that little is rather doubtful. He was born in Lyon on December 20, 1875. His father, Alexandre Noaillon (1843-1916), was an engineer from Arts and Métiers, a chemist and photographer who was appointed director of a chemistry laboratory in Angleur near Liège about 1885 and settled in Belgium. Thus young Paul had his basic eduation in Liege. A Doctor of Science, Paul Noaillon seems to have worked in Belgium, probably in hydrodynamics but we do not know in which branch. His publications were in Belgian journals and were often of a great quality, in particular his memoir [1912] which is still cited in the literature. It appears that in the 1914-18 war he was in the engineering service of the Artillery. Certainly after the war, he was "attaché au Service technique de l'Artillerie", place Saint Thomas d' Aquin. He had links to Hadamard who thought highly of him, as did Picard. Noaillon participated in the great Hadamard seminar at the Collège de France where he sat with Lévy. His work was always original and it touched on various questions of analysis, in particular harmonic functions, and the equations of hydrodynamics, a subject that fascinated Hadamard. He married in Paris in 1923 and was clearly settled there. In 1929 he received the Academy's prix Francoeur (Hadamard was rapporteur) and he went on publishing until his death in Paris on December 20, 1940. We have not found in his work anything on convergence in the sense of probability theory or of denumerable probabilities. On the other hand, Noaillon handled with ease the integral of his contemporary Lebesgue, convergence almost everywhere, convergence in measure, convergence in mean square, and the Riesz- Fischer theorem. On this subject see [Lévy 1925d] which derived from a communication of Noaillon to the Hadamard seminar and re-proved the Fischer-Riesz theorem of 1907. (This is the result from Fischer [1907, p. 1023] that L^2 is complete. The analogous theorem for L^p is proved in [Riesz, 1909] and especially [1910], which gives all the references). Lévy recounts this history in [1970], p. 84-85, without bringing in denumerable probabilities.

The intervention of Mlle Mezzanotte thus seems to have been decisive, and we will say no more. (60)

(61) This was actually Borel's answer to Lebesgue when he made that objection in correspondence. See [Lebesgue 1991], p. 166-167, letter of September 23, 1909, where Borel wrote in the margin: "regarding the matter of independence; its role here is not essential; it could be otherwise. (61)

(62) The article of F. Bernstein [1911] is of great interest. Bernstein, following Bohl and numerous other scientists, was interested in the problem of the mean movement posed by Lagrange,. The analytical version of Lagrange's problem was treated in numerous works in the twenties and thirties, in particular those by Bohr, Weyl, Wintner, etc. A complete solution was given by Jessen in 1938. See C. Berg, this volume, and Jessen, Tornehave [1945] for a complete history of the problem in the first half of the 20th century.

Historically the problem of the mean movement comes from a very famous memoir by Lagrange [1783] which shows that to first order, the positions of N planets of a given planetary system can be written as trigonometrical polynomials of the type $z(t)=\sum_{j=1}^{N} c_j e^{i\lambda_j t}$. It is then a matter of showing that the average duration of revolution (the mean movement) of this model system neither accelerates nor decelerates in the very long term, a problem which depends on the nature of the values of the λ_j. Lagrange states that if those are real and distinct, necessarily the average mean angular velocity of the argument of z remains bounded and he shows this for various special cases. The analytical problem of Lagrange is to show that it is always the case if the λ_j are real and distinct. The mechanical problem of Lagrange consists in knowing if the values of λ_j corresponding to planets belonging toof the solar system are in fact real and unequal, (Lagrange, [1783] section 3). This problem was immediately taken up and developed by Laplace in his pieces on Jupiter and Saturn *Œuvres* XI) and in *la Mécanique céleste* (livre VI). The λ_j are the eigenvalues of a certain symmetric matrix. Laplace establishes that they are real, anticipating the general results of Cauchy and Sylvester, but his proof that they are necessarily distinct, which would prove the secular constancy of the mean movements in all cases, is fallacious. There is a very clear account of the results of Laplace and Lagrange on this topic in chapter 26 of volume I of the treatise of Tisserand [1889].

The difficulty of the mechanical problem of Lagrange is that there are likely to be exceptions depending on the initial positions and the masses of the planets. The delicate question of the nature and of extent of these exceptions was one motivation for the introduction into celestial mechanics, at the beginning of the 20th century, of metric methods, "geometrical probabilities", "sets of measure zero" in the work of Gylden, Poincaré, Bohl, Borel, Bernstein,…. The superb thesis of Anne Robadey [2005] has references and comments. (62)

(63) In 1912, Borel still did not see that the case of convergence of his lemma does not require the independence of the trials. The fact that almost surely there is, in this case, only a finite number of successful outcomes, follows from a very simple inequality of Boole, as Cantelli showed in 1917 (the second very elegant idea of Cantelli was to consider the fourth moments). On the other hand, in the case of divergence, the new lemma of Borel is almost optimal. Evidently it was Lévy who gave this its definitive form, the series of conditional probabilities not needing to diverge uniformly while being undervalued by a divergent series constants, but only to diverge almost surely (i.e. most probably almost uniformly). This is lemma II of his memoir [1935b], p. 91. One can even suppose only that the series of conditional probabilities diverges for an entirely unfavourable past; see Neveu [1964], p. 121, proposition IV-4-4, for a precise statement and a high-speed proof of Borel's lemma thus generalised, a proof so fast and so elegant that one cannot understand how scientists of the first rank did not see it immediately and argued over it for twenty years. But this is a frequent observation in the history of mathematics, elegance only comes after the battle, which generally proceeds in a thick fog, with some breaks sometimes in the distances and very exceptionally a "flash in the night". Is this a general phenomenon? Does the beautiful only come after the truth, which only comes after the good? Is the scheme circular? (63)

(64) Lévy, [1970], p. 90-92, where he dates his return to the infinite play of heads or tails to 1929. On page 22 of the same book Lévy indicates that he could have read the paper of Borel [1909] "around 1922" and he states that he had known all about for a long time (since 1902). This seems doubtful. The reasoning he gives on page 23 to show the recurrence of the play of heads or tails, which he tells us is equivalent to his work of 1902 (he was then about fifteen years old), does not depend on Borel's lemmas nor on his law of large numbers but on a well-known method of Bertrand [1888], the method of successive doubling. If we followed Lévy on this point, we would have to make Bertrand rather than Lévy the true father of denumerable probabilities, a case strengthened because Bertrand was a proven source for Borel too. (See *JEHPS*, December 2006). (64)

(65) Lévy makes the reasoning precise in [1931a], theorem II, which developed his note [1930c]. He show that if a sequence of constants c_n is given, and if one records X_n the number of heads in n tosses, the probability P that the inequality $X_n > c_n$ holds inifinitely often can be only 0 or 1. So that the lemma of Borel applies to the theorem of Borel, in spite of the non-independence of the events $X_n > c_n$. This type of 0-1 law was extended after 1935 and especially after the Second World War in several directions, in particular to sequences of exchangeable variables by Hewitt and Savage [1955], (treated in [Breiman, 1968], chapter 3, § 7), also to the study of recurrence in random walks, for example [Hsu, Chung, 1946] and [Chung, 2000], and to Markov chains of Markov by Kolmogorov, Doeblin, etc

In [1935b], p. 89, note 1, Lévy observed that theorem II of [1931a] is a simple consequence of Lévy's lemma, which covers all possible 0-1 laws. For this subject see BLM, p. 156, note 111.

We may recall incidentally that there exist complete demonstrations of Borel's theorem which follow Borel's "indications", for example Fréchet [1936], Uspensky [1937], ..., Chung [2000]. But that hardly matters, in view of the not easily contested fact that Borel posed the right question in the right way at the right time and that is all that really counts, according to Cantor's thesis III. Cantor was Borel's only master (before he moved away from him and from free mathematics), [Cantor, *Œuvres*], p. 31, and [Décaillot, 2008], p. 120, note 143. (65)

(66) Lévy [1934a, b], included in [1937], chapter VII. See the fine study by J. Bretagnolle [1987]. It is well known that one of the starting points for K. Itô's exceptional work was his attempt to set down completely Lévy's resoning, e.g. [Itô 1998]. (66)

(67) In [1935b], p. 84, note 1, Lévy states that his results were presented to the SMF on May 23, 1934, which is easily checked by referring to the *Bulletin de la SMF*, 62 (1934), Vie de la Société, p. 42-43, "L'addition de variables aléatoires enchaînées et la loi de Gauss". In the space of a month Lévy presented to the SMF, his theory of "integrals whose elements are independent random variables", ibid, meeting of April 25, 1934, p. 39-41, and his theory of sums of connected variables on May 23. We may add that on November 28 of the same year, Lévy communicates to the Society a paper "sur la loi de Gauss (condition nécessaire et suffisante pour son application à la somme d'un grand nombre de variables indépendantes, extension au cas de variables enchaînées) ", ibid, p. 48, in which Lévy stated the definitive theorem on the tendency towards Gaussianity of sums of independent variables. This theorem is sometimes called the Feller-Lévy theorem because Feller published it independently at the same time. See Feller [1935], Lévy, [1935a], p. 37-41, [1935d], [1970], p. 107-108, and the beautiful synthesis by L. Le Cam [1986]. On the history of the central limit theorem since Moivre and Laplace there is the superb book of A. Hald [1998], which is very complete although it stops in 1930.

We may also recall that it was in the same meeting of November 28, 1934, that Lévy stated his conjecture about the Gaussian law: if X and Y are independent random variables whose sum is Gaussian, then they are themselves Gaussian, a conjecture whose consequences he describes in [1935d], p. 381-388. Almost immediately Cramér showed that Lévy's conjecture was correct, [1936a, B], and this led him to write his memorable treatise [1937]. On this subject see Lévy's *Souvenirs* [1970], p. 111-112. (67)

(68) Following Lévy's own principle (above note 34) one might say that Lévy was no more (and no less) the founder of the theory of martingales than Archimedes of differential and integral calculus. One might consider, on the other hand, that Doob took preexisting martingale techniques and made them part of an entire theory. What changed for ever was the way of seeing and of treating the theory of the processes. On this subject see [Yor 2007].

We may add that Doob did not find his theory of martingales in Lévy [1935b, c, d, e], or in [Jessen 1934a] but in the book by Ville [1939], where the concept is treated in its own right and not by analogy with the theory of Kolmogorov. Of course, in his 1940 article Doob quotes [Lévy 1937] but he does not cite Jessen, whom he seems to have read and used only after 1948. In his later writings, on the other hand, Doob was very accurate about the original contributions of Jessen [1934a] and of Lévy [1935b, 1937]; e.g. Doob [1984] p. 807 even details the differences in framework between Lévy 1935 and Lévy 1937. Some other very good, more recent works, e.g. [Kallenberg 2002], p. 574, also bring this out. (68)

(69) The book by R. Siegmund-Schultze [2001] has all the details on these foundations. See also [Schøtt, 1980]. (69)

(70) [Lévy 1934b], p. 337, note 1 tells us that Lévy had given the same lecture at the Hadamard seminar, on March 16 of the same year; his first note on this theory was presented on February 26, [1934a]. (70)

5. The Jessen- Lévy correspondence

The letters presented in this postscript come from the Jessen Archive at the Institute of Mathematics at Copenhagen; see the Introduction above. The Lévy series appears to be complete. Nothing of Jessen's first letter survives but copies, or drafts, of his later letters exist.

Calendar of correspondence

0. Jessen to Lévy. This letter has not survived and its date is unknown.
1. September 27, 1934. Lévy replies to Jessen.
2. April 4, 1935. Lévy writes, enclosing a note, "Demonstration of a theorem of M. Jessen on the basis of my lemma I."
3. Undated but probably April 8 1935. Jessen replies to Lévy's letter of April 4. There are two draft versions of a long letter.
4. April 24, 1935. Lévy replies to Jessen's letter of April 8 and his sending Daniell papers.
5. May 3, 1935. Lévy writes continuing his letter of April 24.
6. August 11 1935. Jessen replies to Lévy's letter of May 3.
7. August 23, 1935. Lévy replies to Jessen's letter of August 11 and the correspondence ends.
8. July 14 1947. Bohr and Jessen write to Lévy.

1. Lévy to Jessen
Paris, 38 Avenue Théophile Gautier (16^{o}), 27 September 1934.

Dear Sir,

Thank you for your friendly letter. As soon as I have finished writing up some results that I have had already for several months, I will not fail to look into those works of yours that you have pointed out to me. Unfortunately I am a little discouraged by the difficulty of researching $\zeta(s)$; I have not obtained any important result on this question (1). I was happier with functions of infinitely many variables.

Professor Bohr spoke to me about your work, and what he said interested me very much. To tell you the truth, your theory of measure and that of H. Steinhaus have been familiar to me for a long time, perhaps since 1920 (2). For me they are elementary concepts that I specify as I need them. But in the use you have made of them, you have gone far beyond what I knew. I informed M. Denjoy of your communication to the Oslo Congress (3). He has just re-discovered the theory of measure (Note of June 6, 1933 to the Académie des Sciences) (4) and I have cited it in an article which I have just finished drafting and which will appear in the Bulletin des Sciences Mathématiques (5). It is a pity that I do not know Danish and cannot read the more elaborate report that M. Bohr has given me (6).

I am presenting a note to the Academy summarising my new memoir (7). It supplements, and corrects the summary I gave to the Société Mathématique de France on May 23, 1934, and which I have sent to M. Bohr and to M. Lublin (8). I also point out that, in my paper in Studia Mathematica, theorems XI and XII are true, not for $\sum x_n$, but for $\sum(x_n - a_n)$, a_n being, for each n, a suitably given constant which can be the term of a semi-convergent series (9). I noticed this error only while returning from Denmark, so that it is not corrected on the copies I left at the Copenhagen Institute.

I would like to ask you to remember me to all your colleagues I saw in Copenhagen, M.M. Norlund, Bohr, Steffensen, Petersen, Bonnessen, Mollerup,… (10) but they are too many and I cannot really ask. I have excellent memories of the days I spent in Copenhagen, and regret not having met you there. But I hope to see you one day in Paris.

In the meantime trust in my devoted feelings.

P. Levy

2. Lévy to Jessen.
Paris, 4 April 1935.

Dear Sir

I think of interesting you by sending the proofs of a memoir which soon will appear and which is related to your own work. It was written last summer and sent to the editor of the Bulletin des Sciences Mathématiques before you sent me your article from Acta Mathematica. I could only indicate one of the common points in a note added afterwards (1). For a few days now I have been occupied in studying your article more completely. I have to give an account of it in the presence of M. Hadamard and I see that the common points between your ideas and mine are even more numerous than I had thought (2).

My lemma I is fundamentally the same as the theorem in your § 14 (Representation of function as limit of an integral). Only I establish it directly (3). Your "important lemma" of § 11 is then a special case of mine; and your theorem of § 13 is obtained readily enough from my lemma I.

Also I think that you are unaware of a lecture I gave to M. Hadamard's seminar in January 1924; it appeared in the Revue de Métaphysique et de Morale, and I reproduced it in my Calcul des Probabilités (p. 325-345) (4). Re-reading it recently, I found that, as well as an error on page 330 (l. 12 to 19), which M. Steinhaus pointed out to me (5), there is something unfortunate on p. 332 (l. 5), (6). In spite of this, I introduced at that time the ideas that M. Steinhaus and you have developed and clarified, without you suspecting that some were already in my article of 1924—and even in a course I gave in 1919 (7). What I call a partition corresponds to what you call "construction of nets" (8). I indicate it for abstract sets, and then (p. 334, l. 6 to 13) I indicate the means of realising it for the cube in an infinite number of dimensions (9); it is what you have done. As for your "transferring principle", I was not very explicit in the article, but when I wrote p. 332 (towards the bottom) that one can carry out the image of the partition on a segment of right-hand side, it was of this principle that I was thinking (10). Indeed a partition is not simply an unspecified subdivision of the relevant set, but a subdivision where each cell has a weight and which leads to one definition of the probability (or, if you prefer, of measure). I recognise that I should have been more explicit. Perhaps I was at the seminar; after 11 years I am no longer sure (11). I am sure that I knew this result which seemed so obvious to me that it was enough to indicate it by a word. On the other hand, is only very recently, in particular after reading the paper by Steinhaus (Studia t. II) and your communication to the Oslo congress of 1929 (12), that I saw how the principle could be used for denumerable probabilities.

In any case it clearly follows from my article that the principles of the theory of measure in any set are those of M. Lebesgue.

Of course the very brief indications in my article are not always sufficient, and your very complete study remained necessary. Besides it taught me many things that I did not know (in particular § 3; before reading it I had not thought that it of interest to specify whether one considered open or closed intervals; and I had never studied the representation of the measure in Q_ω by the symbol of an integral of infinite order (13); and I do not speak of the application to Fourier series which I have only just begun to study).

Excuse this slightly long letter. Like Steinhaus, who, however, knew my 1924 article and seemed in no doubt what it contained, I think it cannot always be very clear, and that it is of interest if I indicate to you explicitly the points which are connected to your work.

Believe, dear Sir, with my most cordial feelings.

P. Levy

Please recall me to the memory of M. Harald Bohr.

Note attached: (14)

Proof of a theorem of M. Jessen (Acta, vol 63, p. 273) on the basis of my lemma I (Bull. Sc Maths. 1935)

Let *f(x)* be measurable in Q_ω. We can assume $0 < f(x) < 1$ [Otherwise we can take $g(x) = \frac{1}{2} + \frac{1}{\pi} Arctgf(x)$].

We apply lemma I, using E to denote the inequality $\frac{h}{2^p} \le f(x) < \frac{h+1}{2^p}$. For every $\varepsilon_p > 0$, there is an N_p such that if E holds and except in the case of a probability $< \frac{1}{2^p}\varepsilon_p$, one has for every $n > N_p$,

$$P_n\left\{\frac{h}{2^p} \le f(x) < \frac{h+1}{2^p}\right\} > 1 - \varepsilon,$$

and consequently

$$\frac{h-1}{2^p} < f_n(x) < \frac{h+2}{2^p}$$

on putting

$$f_n(x) = E_n\{f(x)\} = \int_{n,\omega} f(x) dw_{n,\omega}$$

and finally

$$|f(x) - f_n(x)| < \frac{\varepsilon}{2^p}$$

Applying this result for $h = 0,1,...,2^p - 1$, one sees that the previous inequality holds, except in the case of probability $< \varepsilon_p$, for all $n > N_p$.

Put $p = 1,2,...$; $\varepsilon_p = \frac{\varepsilon}{2^p}$. We see that, except in the case of probability $< \sum \varepsilon_p = \varepsilon$, we have

$$|f(x) - f_n(x)| < \frac{\varepsilon}{2^p}, \text{ for every } p, \text{ and } n > N_p.$$

There is convergence almost everywhere of $f_n(x)$ to $f(x)$. QED.

Corollary. From

$$|f(x) - f_n(x)| < \frac{\varepsilon}{2^p} \text{ for } n > N_p, \text{ except for a set of measure } < \varepsilon_p,$$

$$|f(x) - f_n(x)| < 1 \text{ always,}$$

We conclude

$$\left|\int_{Q_\omega} f(x) dw - \int_{Q_\omega} f_n(x) dw_n\right| \le \int_{Q_\omega} |f(x) - f_n(x)| dw < \frac{2}{2^p} + \varepsilon_p, \text{ for } n > N_p.$$

As $\dfrac{2}{2^p}+\varepsilon_p$ is arbitrarily small, we have the theorem from § 13 of M. Jessen.

4/4/35
P. Lévy

3. Jessen to Lévy.
Undated draft but probably from April 8, 1935.
We have very slightly corrected the orthography of this draft. Jessen would certainly have corrected it himself at the time of sending.

Dear Professor Lévy. (1)
I have your letter of April 4 and the proofs of your memoir, which is to appear in Bulletin des Sciences Mathématiques, and I am very grateful for both.

From your letter I learn that the notion of measure in infinitely many dimensions as well as the transferring principle is indicated already in your paper from 1924 and reproduced in your Calcul des Probabilités and that your ideas on this subject partly go as far back as 1919. I am sorry not to have known this, as I have done my best to give the complete references in my memoir in Acta mathematica. It seems that the notion of measures in infinitely many dimensions has had the rather curious fate to be discovered and rediscovered at least five times. The priority clearly belongs to Daniell who has given a complete treatment of the notion and not only indications already in 1919 using his theory of general integrals (cf. references in my memoir). That Daniell has been quite clear about what he did is seen not only from these but also from other papers of his from this period ; it might interest you that he indicates a subdivision of the infinite-dimensional cube in Bulletin of the American Math. Soc. 26 (1919-20) p. 448 and explicitly points out the importance of the problem for the calculus of probability in The Rice Institute Pamphlet 8 (1921) p. 60-61, (2). Wiener did not rediscover the theory as he knew Daniell's papers, but he made several applications of the theory (cf. the references in my memoir) ; he emphasized more than Daniell the usefulness of the subdivision, see for instance his paper in Proceedings of the London Math. Soc. (2) 22 (1924) p. 454-467, as far as I remember, the Transferring principle does not occur explicitly in his early papers where he always need Daniell's integrals, but he told me that it was quite familiar to him and in later papers he uses it (3), in order that he may work with Lebesgue integrals instead of Daniell integrals; see for instance his memoir in Acta mathematica 55 (1930) § 13. As I now learn you have had similar ideas without knowing that the problem was already treated by Daniell. The same has been the case with Steinhaus and myself, who founded the theory independently of each other and at the same time; neither of us knew Daniell's work. Finally I learned from you that Denjoy had recently rediscovered the theory. (4)

In my memoir in Acta mathematica I did not go too much into the history of the subject which is complicated by the fact that the ideas in question have developed gradually, so that it is now hard to say who has the priority in each case. I hope, however, that the first sentences in § 1 have made it clear, that my program was "to study in greater details than has been done before" the theory in question.
Page 2 : (5)

That is : Q_ω is the product of an infinite number of abstracts spaces *C1, C2*, … in each of which a measure has been defined, so that the measure of the space itself is 1.

I am writing on a paper in which I intend to develop the theory for abstract spaces. (Cf the references in my memoir at the top of p. 251)

In this paper I shall make due reference to your paper from 1924 and to your book. (6)

Your remark in your letter that my theorem from §§ 13 and 14 will follow from your lemma I interested me very much as I have tried hard to find simple proofs for these theorems. I do not think however that the proofs which you sent me are sufficient to give my theorems in full generality, for the following reasons :

1°. Is it sufficient to prove the theorems for bounded functions ? I do not think that you can deduce them for an arbitrary integrable *f(x)* from their validity for $g(x)=\frac{1}{2}+\frac{1}{\pi}Arctgf(x)$.

2°. Is it really possible to deduce the theorem of § 13 from that of § 14 ? This is as far as I can see what you wish to do. As it stands your proof is not valid, since the function

$$f_{n,\omega}=\int_{Q_n} f(x)dw_n$$

does not appear at all. (The term $\int_{Q_\omega} f(x)dw_\omega-\int_{Q_n} f_n(x)dw_n$ on the left in your estimation is simply $A-A=0$). (7) It might be possible to argue as follows (and this, I believe, is what you have in mind)

$$h_n(x)=f(x)-f_n(x)\to 0 \quad p.\ p.$$

This implies

$$\int_{Q_n} h_n(x)dw_n=f_{n,\omega}(x)-A\to 0 \quad p.\ p.$$

The theorem « $h_n(x)\to 0$ $p.\ p.$ implies $\int_{Q_n}|h_n(x)|dw_n\to 0$ » is actually true for bounded functions but I do not know how to prove it [?] just my theorem of § 13. I do not [?] is true for integrable functions. (8)

In the case of abstract spaces the proofs of the theorem of §§ 11, 13 and 14 must be rearranged (cf my memoir footnote 2) on page 251), the reason being that the notion of a net can be applied only in special cases of abstract spaces. I intend first to give a new and direct proof of the theorem of § 14 ; the lemma of § 13 (which is of course only the 0- and 1- law of the calculus of probability in a general form) (9) follows then from this theorem, and the proof of the theorem of § 13 may then be left unaltered. The theorem of § 14 I prove by generalizing F. Riesz's proof of the differentiation theorem for monotone functions as follows : (10)

Let *f(x)* be integrable in Q_ω and $f_n(x)=\int_{Q_{n,\omega}} f(x)dw_{n,\omega}$. In order to prove that $f_n(x)\to f(x)$ $p.\ p.$, I first prove that $lim\, f_n(x)$ exists $p.\ p.$. It is sufficient to consider the case where $f(x)\geq 0$. Put $\varphi(x)=liminf\, f_n(x)$, $\psi(x)=limsup\, f_n(x)$. It is sufficient to prove that if $0<\alpha<\beta<\infty$ then the set $D_{\alpha\beta}=[\varphi(x)<\alpha,\psi(x)<\beta]$ is a null-set. For $0\leq m<n$ let

$$A_{mn}=[f_{m+1}(x)>\alpha,\ldots,f_{n-1}(x)>\alpha,f_n(x)\leq\alpha]$$
$$B_{mn}=[f_{m+1}(x)<\beta,\ldots,f_{n-1}(x)<\beta,f_n(x)\geq\beta]$$

A_{mn} and B_{mn} are cylinders with basis in Q_n. We shall make use repeatedly of the remark that if *C* is a cylinder with base in Q_n then $\int_C f(x)dw_\omega=\int_C f_n(x)dw_\omega$ so that if $f_n(x)\leq\alpha$ or $\geq\beta$ in *C* we have $\int_C f(x)dw_\omega\leq\alpha m(C)$ or $\geq\beta m(C)$ respectively. Suppose now that $0\leq m<n<\beta$

and that C is a cylinder with base in Q_m (if $m = 0$ we take $C = Q_\omega$) – We consider the set $CA_{mn}B_{np}$ which is a cylinder with base in Q_p. Hence since $f_p(x) \geq \beta$ in this set we have $\beta m(CA_{mn}B_{np}) \leq \int\limits_{CA_{mn}B_{np}} f(x)dw_\omega$. Summing this for all $p > n$ for fixed m, n we get $\beta m \sum\limits_p CA_{mn}B_{np} \leq \int\limits_{\sum\limits_p CA_{mn}B_{np}} f(x)dw_\omega \leq \int\limits_{CA_{mn}} f(x)dw_\omega \leq \alpha m CA_{mn}$ since CA_{mn} is a cylinder with base in Q_n in which $f_n(x) \leq \alpha$. Summing now for all $n > m$ we get $\beta m \sum\limits_{np} CA_{mn}B_{np} \leq \alpha m \sum\limits_n CA_{mn} \leq \alpha m C$. We now take first $m = 0$ and $C = Q_\omega$; observing that $D_{\alpha\beta} \subseteq \sum\limits_{np} A_{on}B_{np}$ we get . Next we take first $C = A_{on}B_{np}$ for a fixed n and p ; then we get $m \sum\limits_{qr} A_{on}B_{np}A_{pq}B_{qr} \leq \frac{\alpha}{\beta} m A_{on}B_{np}$ the indices q and r being restricted by $p < q < r$. Summing afterwards over n and p and observing that $D_{\alpha\beta} \subseteq \sum\limits_{npqr} A_{on}B_{np}A_{pq}B_{qr}$ we get $mD_{\alpha\beta} \leq m \sum\limits_{npqr} A_{on}B_{np}A_{pq}B_{qr} \leq \frac{\alpha}{\beta} m \sum\limits_{np} A_{on}B_{np} \leq \left(\frac{\alpha}{\beta}\right)^2$. Proceeding in this manner we get $mD_{\alpha\beta} \leq \left(\frac{\alpha}{\beta}\right)^n$ for every n, hence $mD_{\alpha\beta} = 0$.

Summing afterwards over n and p and observing that $D_{\alpha\beta} \subseteq \sum\limits_{npqr} A_{on}B_{np}A_{pq}B_{qr}$ we get $mD_{\alpha\beta} \leq m \sum\limits_{npqr} A_{on}B_{np}A_{pq}B_{qr} \leq \frac{\alpha}{\beta} m \sum\limits_{np} A_{on}B_{np} \leq \left(\frac{\alpha}{\beta}\right)^2$. Proceeding in this manner we get $mD_{\alpha\beta} \leq \left(\frac{\alpha}{\beta}\right)^n$ for every n, hence $mD_{\alpha\beta} = 0$.

Page 3 :

It remains to prove that $\lim f_n(x) = f(x)$ *p. p.*. This may be proved as follows. From the definition of measure one readily deduces the following approximation theorem : If *f(x)* is integrable in Q_ω and $\varepsilon > 0$ is given then there exists an $m = m(\varepsilon)$ and an integrable function *g(x)* depending only on $x_1, \ldots, x_m$ so that $\int\limits_{Q_\omega} |f(x) - g(x)| dw_\omega < \varepsilon$. This implies $\int\limits_{Q_\omega} |f_n(x) - g_n(x)| dw_\omega < \varepsilon$ for all *n*. Now $g_n(x) = g(x)$ for $n \geq m$. Hence $\int\limits_{Q_\omega} |f_n(x) - g(x)| dw_\omega < \varepsilon$ for $n \geq m$ and consequently $\int\limits_{Q_\omega} |f(x) - f_n(x)| dw_\omega < 2\varepsilon$ for $n \geq m$. Hence $\int\limits_{Q_\omega} |f(x) - f_n(x)| dw_\omega \to 0$ as $n \to \infty$ and this, together with the existence of $\lim f_n(x)$ *p. p.* proves that $\lim f_n(x) = f(x)$ *p. p.*

This is the simplest proof I know of the theorem of § 14. For bounded function and more generally for functions of the class L^p ($p > 1$) it is possible to give very short proofs of the theorems of §§ 13 and 14 just mentionned and the majorisation theorem of § 16, but in this way I could not prove the theorem for arbitrary integrable functions. (11)

Excuse me this long digression ; I thought it might interest you to know this other proof.

With kind regards also from Prof. Bohr

Sincerely yours

Børge Jessen

Written over the calculations of page 2 in a blacker ink :

I hope that your memoir in Bulletin des Sciences mathématiques will have appeared when I finish my paper so that there will be nothing to prevent me from using these new proofs (of course with due reference to your work ; my paper will at any rate be mainly expository). (12)

Extract of another more confused draft, probably a first attempt at the preceding letter. This draft consists of two sheets and it is not obvious which comes first. The order we propose is not in the least certain.

"First sheet". The beginning is truncated, but it is easy to imagine its character; Jessen speaks about the statement of lemma I of Levy:

« ristic functions, is true, but I do not think that you can prove the theorem for unbounded functions in this way (the validity of the theorem for $g(x)=\frac{1}{2}+\frac{1}{\pi}Arctgf(x)$ does not imply its validity for *f(x)* itself). It is, however, possible, through a rearrangement of the proof to make it work for arbitrary integrable functions. The proof runs then as follows :

Let *f(x)* be integrable in Q_ω and $f_n(x)=\int\limits_{Q_{n,\omega}} f(x)dw_{n,\omega}$. The theorem states that $f_n(x)\to f(x)$ almost everywhere. For a given $\varepsilon>0$, let *g(x)* be chosen as a function depending only on a finite number of the variables $x_1,\dots,x_m, m=m(\varepsilon)$ and such that

$$\int\limits_{Q_\omega} |f(x)-g(x)|dw_\omega \leq \varepsilon^2 .$$

Put $|f(x)-g(x)|=h(x)$, then $|f_n(x)-g_n(x)|\leq h_n(x)$ for any *n*. For $n\geq m$ we have $g_n(x)=g(x)$. Now by (16.4) and (16.5) if *E* denotes the set of points in Q_ω where bound Sup $h_n(x)>\varepsilon$ we have

$$\varepsilon mE \leq \int\limits_E h(x)dw_\omega \leq \varepsilon^2$$

hence $mE\leq\varepsilon$. It follows that the measure of the set of points where $|f(x)-f_n(x)|\geq 2\varepsilon$ for some $n\geq m$ is at most 2ε which implies that $f_n(x)\to f(x)$ almost everywhere. »

The end is crossed out and the document does not continue.

"Second sheet". Here there is writing in ink from top to bottom over calculations written in pencil with the two texts running in opposite directions. The over-writing is legible but it is impossible to decipher the calculation which may well supplement the reasoning outlined above.

In a similar way the theorem of § 13 follows from (13.2) and (13.3). I do not think that it is possible as you intend to do to deduce the theorem of § 13 immediately from the theorem of § 14 not even for bounded functions. (The proof in your letter seems at least to be defective since the term $\int_{Q_\omega} f(x)dw_\omega - \int_{Q_n} f_n(x)dw_n$ on the left in your estimation is = 0 ; and I do not see how this can be repaired.)

The above proof was familiar to me in a somewhat less general shape, namely when *f(x)* belongs to L^p for a $p > 1$ in which case I need (16.1) instead of the more elementary relations (16.4) and (16.5).

I am indebted to you for calling my attention to the problem once more.

4. Lévy to Jessen
Hennequeville, 24 April 1935. (1)

My dear Colleague,

I have received your letter of April 8 and your memoirs and thank you.

Naturally what you have said about Daniell's priority interests me greatly. I knew of the existence of his work but at that time I read English with difficulty. Though I have made progress, I still do not read it easily. I read a summary of some of Daniell's results at the beginning of a paper by Wiener, and thought I knew his most important results. I learn only from your letter that he knew well before me the principle of correspondence. (2)

So I no longer have any reason to ask that my paper of 1924 be mentioned in the history of this principle. That does not much surprise me. I believe me well to recall that if I more explicitly did not indicate this principle of correspondence, it is that it appeared probable to me that a so simple principle was to be known. It is only by finding it rediscovered by you and Steinhaus that I regretted not having expressed it more explicitly.

I agree with what you say about my lemma I. Actually the argument by which I thought of deducing your § 13 and 14 from it was not correct. I had noticed that your § 14 contained my lemma I as a special case; but, as usual because I read English very slowly, I had to approve the printing of my paper before I had read enough of yours and for that reason I quoted only a part. I apologise from that. (3)

M. Bohr had already given me your thesis. But I had not succeeded in understanding it. It is only now by bringing it closer to your memoir written in English that I see that it already contained several important results of this memoir.

As I have two copies now, I am thinking of giving one to the Library of the Institut Henri Poincaré.

I will send you my paper as soon as I have copies but you will have without doubt been able to see it earlier in the Bulletin des Sciences Mathématiques. I wrote, also in 1934, another paper which will appear in the Journal de Mathématiques where I give the necessary and sufficient condition for the sum of a large number of independent random variables to tend to Gaussianity. It was known already that this condition was sufficient; the new result is that it is necessary. (4)

I add finally that I write a Note summarising my work. The impression was started before I received your letter. As I did for my Memoir, I am going to add some Notes at the bottom of the page mentioning the new priorities that have lately come to my knowledge, i.e. this time those of Daniell. (5)

Believe, dear Sir, with my devoted feelings.

P. Levy

5. Jessen to Lévy
Copenhagen, Phistersvej, 24, August 11, 1935.

Dear Professor Levy,
I am sorry to have been so long in answering your letter of May 3 and thanking you for kindly sending me your « Notice sur les travaux ».

Regarding the question of priority for the transferring principle for the infinite dimensional cube it is so, that, as far as I know the literature, it occurs first in your paper from 1924-1925 and later by Steinhaus and myself in 1929-1930. But it is difficult to separate the transferring principle from the underlying construction of a net in the infinite dimensional cube, and this construction occurs already in a paper of Daniell from 1919-1920 (*Bulletin of the American Mathematical Society*, vol. 26, p. 448 below) and is reproduced by Wiener in 1920-1921 (Annals of Mathematics, 2. Series, vol. 22, pp. 66-72, Example 3). These authors had no reason to use the transferring principle, since they had the general Daniell integral, which is much more satisfactory. It is only a pity that the general integral was not a larger success, and that therefore later writers (including myself) have preferred by means of the transferring principle to reduce everything to ordinary Lebesgue integrals. (1)

I do not think that you estimate Daniell's papers sufficiently since you can say that you do not see, what he has added of essential to the ideas of Frechet. There is the following essential difference: Frechet (in his paper in *Bulletin de la Societe math. de France* 1915) starts from a completely additive set function, so that what he has generalized is the definition of the Lebesgue integral when the Lebesgue measure is already known. This he has done in a very elegant way. What Daniell has done is to generalize the definition (due to Young) of the Lebesgue integral based on the properties of the Riemann integral, that is on an object, which is much more elementary that the Lebesgue measure. Personnally I prefer the definition of the integral based on a measure to a direct definition (though the latter have also their great importance) ; to Daniell's work corresponds in the theory of measure a generalization of the definition of the Lebesgue measure based on the properties of the Jordan measure. Of this important problem (which is treated e. g. in Kolmogorov's «Grundbegriffe der Wahrscheinlichkeitsrechnung ») I find nothing in Frechet's paper. (2)

With kind regards, I am
Very sincerely,

6. Lévy to Jessen
Paris, 3 May 1935.

My dear Colleague,

I write to you, after having looked at the memoirs of Daniell, and having read again your letter. I realised that there had been a misunderstanding. I do not know why I had believed that you were telling me about the priority of Daniell in the principle of correspondence; I see that you speak only of the measure in space with infinitely many dimensions.

However, for measure in abstract sets, the priority belongs to Fréchet (Bulletin de la Société Mathématique de France e, 1915, p. 248-265). He did not study in particular the case of space to infinitely many dimensions. He is not less that which gave the first the essential components of this theory. (1)

I did not know his work which was published during the war. Like Daniell, I found these results again in 1919. I heard of Daniell for the first time in 1922 from N. Wiener who indicated his main results to me, and it was not, I believe, until 1924 or 1925 that I realised Fréchet's priority, so that you can find in my work the incorrect expression of the Daniell integral. (2)

As for the principle of correspondence, I think again that the question presents itself much I thought originally; first the imprecise indication in my paper of 1924-1925; then your work and those of Steinhaus.

Believe, my dear Collègue, with my devoted feelings.

P. Levy

P.S. – The March 1935 number of the Bulletin des Sciences Mathématiques, containing the beginning of my Memoir, appeared. My Memoir starts at p. 84. I counted 28 pages, I think thus that will make p. 84 to 111.

–I noticed that Daniell cited Fréchet well in one of his first papers. I admit to not clearly seeing what he added of essence to the ideas of Fréchet, except some precise details for a space with infinitely many dimensions.

P.L.

7. Lévy to Jessen
S. Christina, 23 August 1935. (1)

My dear Colleague,

I received your letter of August 11 a few days ago. I am not able to re-examine Daniell's work here but I am persuaded that you are right.

I know very well that I have the fault of being so absorbed in my own works that it is always very hard for me to read those of others completely. I knew vaguely those of Daniell, and when you spoke to me again of them, being too occupied, I read them too much quickly, especially wanting to see whether I found there the statement of the principle of correspondence and if I could maintain what I had written in my note, which at the time I had to approve for printing. Also I was not surprised by your answer.

Besides I expressed my opinion badly when I wrote to you. I should have written, "I do not yet see well what Daniell added to Fréchet's paper. Your letter helps me to see better now and I thank you.

I had already quoted besides Daniell, whom I knew a little thanks to N. Wiener; see my fascicule 5 du Mémorial des Sciences Mathématiques. I then found Fréchet's 1915 Memoir which I had not known or which I had forgotten and it seemed to me that I was not the only one who had forgotten to quote it.

Cordial wishes to you.

P. Levy

8. Jessen to Lévy, Copenhagen, July 14, 1947.

Dear Professor Levy,

First of all we wish to thank you and Mrs Levy heartily for the most agreeable evening spent with you and your family in Paris (1), and for all your kindness during the interesting days in Nancy, on which we look back with great pleasure (2). We also thank you very much for your kind letter. It would have been such a great pleasure to us all to have seen you in Copenhagen already this autumn. However, we are very sorry to say that, as you also felt yourself, in these difficult times it does not seem possible to obtain a sufficient grant to cover the expenses of your contemplated stay here. In earlier times this would have been easy, but at present the funds are rather hard up and have already disposed of their means for the nearest future. We hope, however, that within long it will be possible to make arrangements for your coming here to give some lectures which would be a great pleasure to all the Danish mathematicians.

As you may have heard, Mr. Schwartz has been invited to visit Copenhagen in September to give some lectures on his extraordinary theory of distributions (3). That this invitation has been possible is due to the interest of this theory also among all the applied mathematicians, which has made a grant available that otherwise would have not been obtainable for mathematical lectures.

With kind regards to yourself and your family.

Yours sincerely

HB (Bohr) BJ (Jessen)

Notes on the Jessen-Lévy correspondence Section 5

Notes to letter 1: Lévy 27 September 1934

(1) Lévy had discussed the Riemann zeta function at the Zurich Congress [1932] and it is possible that Jessen mentioned it in that lost first letter. Jessen had also discussed the topic in Zurich [1932] and worked on it with Bohr from 1928-1929, [Bohr, Jessen 1930 - 1932]. See also Lévy [1970], p. 41 and [1930b], p. 143. (1)

(2) An allusion to his lectures at the Collège de France in 1919, included in [Lévy, 1922, 1925a, b, c]. (2)

(3) [Jessen, 1929b]. (3)

(4) [Denjoy, 1933]. Denjoy, whose lectures followed those given by Cantelli at the IHP in 1933 [Cantelli 1935], used the principle of correspondence to construct what he called “variables pondérées multipliables” (infinite sequences of independent random variables) and which the Polish School called “fonctions indépendantes”, e. g. [Kac, 1936], [Marcinkiewicz, 1938]). (4)

(5) [Lévy, 1935b]. From this letter, it is safe to conclude that Lévy’s paper on dependent variables was written after his visit to Copenhagen in April 1934 and before the end of the 1934 summer holidays. As noted above, Lévy added to the proofs a note, p. 89, where he indicated that he had since been informed of Jessen’s article in *Acta Math.*. In his *Notice* [1935a], p. 84, note 1, Lévy also states that he had submitted his paper [1935b] in September 1934. See the next letter and BLM, p. 156, note 111. (5)

(6) [Jessen, 1930]. It is therefore clear that § 1 of [1935b] (or § 39 of [1937]), that is to say, the “foundations” of Lévy’s theory of denumerable probabilities, came from a close reading of [Jessen, 1929b] and memories, real or imagined, of his first works on integration in infinitely many dimensions. (6)

(7) [Lévy, 1934c] presented on October 1, five days after this letter. This note corrects the statement of the central limit theorem for martingales, given in his communication to the SMF, which we will not examine here. We may recall, however, that the first general central theorem limit for connected variables is due to Bernstein [1926b], work in which Lévy had some part. However, Lévy’s theorem exploited a novel idea, a random change of time; Lévy imagines that the *n*th part whose result is X_n, assumed to have zero mean conditional on the past, has a random duration equal to the conditional variance $\sigma_n^2 = E^{n-1}(X_n^2)$. If one cumulates the winnings of n left by relating not to the square root time n (or with $\sqrt{\sum_1^n E\left(X_k^2\right)}$), but to the new time equal to the sum of n random durations thus defined, one obtains asymptotic normality, e. g. [1937], p. 242.

This remarkable idea in every way, though it conforms to the transformation of prior probabilities into posterior probabilities, was taken up by W. Doeblin in the framework of continuous time martingales with continuous trajectories. Locally such martingales behave like the martingales of Lévy at infinity, so that by changing the time in the natural way and by replacing the sum by an integral, one obtains Brownian motion. This theorem of Doeblin [1940b], p. 1068, lemma IX, which is the first known statement of the theorem of Dubins-

Schwarz [1965], enables him to solve in masterly fashion the problem of Bernstein-Kolmogrov, i.e. the search for probabilistic solutions of Kolmogorov's equation. On this topic there is M. Yor's account of [Doeblin, 1940b], p. 1033-1035. However, Lévy did not think of treating the theory of continuous martingales, which can be found in Ville [1939], Doob [1940], and Doeblin [1940b], without him noticing. (7)

(8) Mogens Lublin was a young Danish mathematician and contemporary of Jessen's. He submitted his magister's thesis, written under the direction of N. Nielsen and N. E. Nørlund, in 1930. In the years 1930-1940 Lublin published actuarial works and he had a brilliant career as an actuary in Copenhagen; he died in 1972. (8)

(9) [Lévy, 1931c Π, § 15, p. 148-149. (9)

(10) Niels Erik Nørlund (1885-1981) was a very well known mathematician. He was a professor at the University of Lund and then at the University of Copenhagen. For 55 years he was editor of *Acta Mathematica*. His sister Margrethe Nørlund married Niels Bohr in 1912. See [Gårding, 1998].

Johan Frederik Steffensen (1873-1961) was an important Danish statistician, a professor of actuarial science at the University of Copenhagen and correspondent of Fréchet.

Richard Petersen (1894-1968), a pupil of Bohr, was assistant in mathematics at the University of Copenhagen, then professor at the city's Polytechnic School. This institution, founded in 1829, on the model of the Paris École Polytechnique, is now called the Technical University of Denmark; thanks to Christian Berg for this information. Richard Petersen pioneered the use of computers in Denmark.

Tommy Bonnesen (1873-1935) was professor of descriptive geometry at the Polytechnic School of Copenhagen, which followed the programme of Monge like its Parisian model. Bonnesen worked in particular on the isoperimetric inequalities, convex bodies, etc. When he died in 1935 Jessen succeeded him—he had to learn the geometry of engineers and stone masons.

Johannes Mollerup (1872-1937) was professor of analysis at the Polytechnic School of Copenhagen. C. Berg provides these details: "Concerning Mollerup, I mention that Bohr and Mollerup initiated the writing (around 1915) of a 4 volume treatise of mathematical analysis for the Polytechnical School (I suppose inspired by Jordan's Cours d'analyse which Bohr had studied himself). It became a legend for Danish engineers, used in many new editions up to around 1970 and just called Bohr-Mollerup. There is a famous theorem called the Bohr-Mollerup theorem, namely the characterization of the Gamma function as the only log-convex function satisfying the functional equation and being normalized to 1 at 1. It appeared in a slightly disguised version in the 1922 edition of Bohr-Mollerup and was later made known by Artin in his small book about Gamma (with due credit to Bohr and Mollerup). Bohr and Mollerup never published the result in a journal."

On the Danish mathematical community in the 1930s, see [Ramskov, 2000b] and [Schøtt, 1980]. The number of mathematical positions in Denmark was much reduced and the number of pure mathematicians his been put at ten. Thus in 1930 there were only three professors of pure mathematics, N. E. Nørlund, H. Bohr and Johannes Hjelmslev (1873-1950); Jessen would eventually succeed Hjelmslev. (10)

Notes to letter 2: Lévy April 4 1935

(1) See above note (5) to the preceding letter. (1)

(2) This passage shows that in April 1935, when he was preparing his presentation to the Hadamard seminar, Lévy did not realise the similarity between the theorem in § 14 of Jessen and his own lemma I, though he had received Jessen's memoir in September 1934. (2)

(3) Lévy added in the margin at this point: [see the sheet attached to this letter]. (3)

(4) [Lévy, 1925a, b]. Lévy's page references are to his book [1925b] and not to the original article in the *Révue de Métaphysique et de Morale*. (4)

(5) In [1925b], p. 330 Lévy maintained the possibility of a countably infinite (though not invariant and accordingly "very arbitrary") extension of Lebesgue measure to all the subsets of the interval [0,1]. On this point there are some very interesting observations in BLM, p. 153, note 103.

Lebesgue posed the problem of extending Lebesgue measure to all the subsets of the real line in his first Peccot course at the Collège de France in 1903, [1904], p. 102. In the following year Vitali [1905] showed that an invariant extension is impossible if one accepts the axiom of the choice. Vitali's nonmeasurable sets, so contrary to the geometrical intuition of Lebesgue, led Lebesgue to contest and then to reject the axiom of the choice in mathematics. To save intuition, the axioms must submit or be dismissed. This was not the view of Lévy who tolerated the axiom of choice and the transfinite axiom within the much broader limits of his own intuition ([1937] § 39, p. 124 - 125). He would reconsider the problem of measure in [Lévy, 1961].

In 1914 Hausdorff [1914], p. 469 posed the problem of additive invariant extensions of Lebesgue measure and concluded that it was impossible in spaces of three or more dimensions. Banach [1923] proved the converse, that such an extension (additive and invariant) exists for the line and the plane. But in 1924, when Lévy wrote his note on "the laws of probability in abstract sets", the problem of countably additive noninvariant extensions of Lebesgue measure on the line was open and Lévy thought it self-evident that such extensions exist using the "method of M. Zermelo". In 1929 however, Banach and Kuratowski, using the continuum hypthesis, showed that it was not possible (also Ulam [1930]). Steinhaus had to inform Lévy about this result in the course of their correspondence in 1930-1931. At the same time Steinhaus probably also communicated to Lévy his principle of correspondence [1930b], without the latter noticing; we do not know the fate of the Lévy-Steinhaus correspondence. Lévy returned to the point in [1937], n° 9. In its *Souvenirs*, [1970], p. 67, Lévy recalls his very great surprise at learning the negative result of Banach-Kuratowski. He adds: "It was very necessary that I consider the evidence; my intuition had misled me and I am still sometimes astonished that my intuitive idea is false; I remain tempted by the same error." To know why Lévy's intuition was comfortable with the axiom of choice, but not with the continuum hypothesis, one would need to know much more. To go further it would be necessary to return to Cantor and his debates with the Paris school, but that would take us too far from our subject. On this matter there are some interesting recent works, [Décaillot, 2008] and [Graham, Kantor, 2009], and, naturally, [Guilbaud 2008].

We recall incidentally that at the time of his visit to Paris May 1935, at the invitation of the Rockefeller Foundation and the IHP [1937], Bruno de Finetti gave a lecture at the SMF [1936], on null probabilities, which reconsidered these issues and proposed an axiomatic additive theory of probability. On the long and rich history of the problem of measure, see the important work of J.P. Pier [1986, 1990]. (5)

(6) Lévy gives a characterisation of summability in an abstract set which was valid only for bounded functions, an error he acknowledged in [1936b], p. 157, note 2. (6)

(7) This claim appears in all, or nearly all, of Lévy's writings from 1935, for instance in [1936b], p. 157-158, 169, [1937], foreword, page XII, BLM p. 163. Steinhaus also reports the claim in a note in his communication to the Colloque de Genève, [1938] p. 65, note 14: "M. Paul Lévy has informed me in private correspondence that the solution of the problem of measure on the [infinite] cube had come to him in the course of general considerations that the reader will find in a note at the end of his Calcul des Probabilités, without his judging it necessary to go into details. This Note of 1925 was followed by two articles by M. Paul Lévy, [1931c, d]…" Steinhaus adds: "My article [1923] appears to have escaped M. Lévy's attention."

In note II of the second edition of Lévy's treatise of 1937, written when he was correcting the proofs and therefore around 1954, Lévy writes finally, p. 370, note 1: "I had hoped to bring the mean in the sense of Gateaux closer to the concept of integral in the sense of Fréchet. It was an attempt likely to fail. However this very difficult problem had led me not to insist enough on simpler questions which seemed to me trivial." This may explain the obstinacy which made Lévy assert things he had not written down but which he knew or would certainly have known if he had gone further in this direction. The Gateaux means are not Fréchet integrals (conforming to a probability measure) and everything else is trivial. (7)

(8) Lévy introduced the concept of partition in his [1925b], p. 331 indicating that the concept came from Norbert Wiener. Lévy developed the idea in [1935a], § 9, [1936b] chapter I, § 3, and in [1937] chapter II, § 10, but forgot about the correspondence with Jessen and his correspondent's article [1934a]. These were most probably the direct cause of these later developments given that the note of 1925 had a different objective and the contents of the Peccot course of 1919 are unknown.

Jessen introduced the concept of "nets" in [1934a], § 6, entitled "The Construction of Nets", and in his theses [1929a] and [1930], following La Vallée Poussin. (8)

(9) As Christian Berg points out to us, Jessen underlined this last sentence member and added in the margin, in Danish: "This is done by Wiener: Ann. of Maths. 22 (1920-21) p. 66-72. Daniell: Bull. Amer. Math. Soc. 26 (1919-20) p. 448 below. Also Rice Inst.Pamphlet 8 (1921) p. 60-61. "

Lévy, [1925b], p. 334, lines 6 to 13 writing, in connection with the construction of the partitions of the cube Q_ω: "A partition of this can for example be obtained in the following manner: at the *n*-th stage, the interval of variation of each of the *n* coordinates $a_1, a_2, ..., a_n$ will be divided into 2^n equal intervals, making in all 2^{n^3} partial volumes; but these volumes are small in *n* directions only, and large in all others. For *n* infinite, the coordinates being fixed the ones after the others, one arrives at this result that each *e* contains one point and only one; and, however, a uniformly continuous functional will not in general be summable. "

At this point in the Note, Lévy's goal is "*to define a law of partition such that any uniformly continuous functional is summable*", p. 333, § VIII, first paragraph. Lévy uses substantially the same "construction of nets" as Jessen but he uses it to show that a theory of strong integration is impossible in Q_ω (what is perfectly correct), while Jessen uses it to construct a weak integral in Q_ω, having concluded that it is not necessary to consider strong integration (which would integrate all uniformly continuous functionals) for the applications he envisages. This is also true for applications in probability, as Lévy ends up realising, though not before 1934, as he acknowledged later.

Lévy is thus at once right and wrong in this matter. That, at least, is our conclusion without claiming it is right or definitive. The whole business is singularly obscure for Lévy reconstructed the entire history in his new texts [1935a], [1936b], [1937], reinterpreting his earlier work, [1925], and thoughts from 1919—and even from 1918 in the military hospital—in the light of what he had learned since.

Lévy is not the only mathematician to reconstruct in this way, far from it. For him it is neither a trick nor mischievousness. He acts in good faith in truth and in error, which makes him sympathetic and frightening at the same time. Usually historians are known to be careful not to reject testimonies, even the most erroneous. They busy themselves in subjecting these accounts to a benevolent and determined external criticism, to try to reach the truth through the error, without ever getting there. Let us hope that we do not get lost following them. For the historian of mathematics, the task is even harder; mathematical understanding is always an act of creation, even if what is created was already created very well by others, before or afterwards, independently or not, so that attributions, with rare exceptions, remain random and partial, and the reasons presented are multiple, fugitive and misleading. (9)

(10) Again Lévy is right and wrong. He uses a principle of transfer, not to establish a correspondence between the measure on Q_ω (of which he seems only to have an implicit idea) with Lebesgue measure on [0,1], but with a very different aim: to show that one cannot define a probability law (distinguishing the points) in a set with a power exceeding that of the continuum (Note § VII), a result found by Ulam [1930] and taken up again by Finetti [1936], p. 280. (10)

(11) Lévy makes the same reproach in [1935a], p. 29, note 1, [1936b], p. 158, and [1937], n° 10, p. 21, note 1. He should have stated at this point that the correspondence (which he sees and which he constructs very well) preserves measure. He even adds in a note to [1936b], p. 158, that he certainly spoke about it at the time of his "lecture of 1924", a presentation to the Hadamard seminar in January 1924 which was published in [1925a] and included as the final note of [1925b]. One may doubt this "memory of Lévy" for nowhere in the Note does he refer to a "measure" in Q_ω, a measure which appears to him now (in 1935) as so obvious that he believes he remembers it, a Socratic recollection as was his way. In the lecture Lévy may have mentioned that the correspondence preserves measure at the same time as he defined it. There is a basis for this position in note 2 of page 44 of the second edition of Lebesgue's *Intégration*, and Lebesgue was probably present at the seminar in 1924 (see on this subject note 12 of the presentation). But between these two hypotheses and others that could be conceived, it is best not to take sides and to leave the question to the more learned.

At all events, Lévy readily recognises in his note [1935a], p. 29, note 1, that it was Jessen's memoir which re-awoke (or awoke) him to the principle of correspondence, and its "applications to the theory of denumerable probabilities." So, without hesitation, we may place Jessen in the pantheon of masters of Lévy, beside Hadamard, Borel, Wiener, Fréchet, Cantelli, Mlle Mezzanotte, Steinhaus, Khinchin, Marcinkiewicz, etc, all of whom aroused the mind of Lévy. That pantheon admits a hierarchy of pantheons. The tutelary deities are Borel and Hadamard but they are also awakeners. Then there is the unattainable paradise where sit those, like Doeblin or Kolmogorov, who shoot so quickly that Lévy's brain does not have time to awake to find the parade. For Kolmogorov, see Chaumont, Mazliak, Yor [2004] and Shafer, Vovk [2005]. On Doeblin, see [Doeblin 2000b], in which Doeblin shows, among many other things, that a continuous martingale is a changed Brownian movement of time. This was in 1940, twenty or thirty years before the leading specialists of the time realised that this was an important property (above note 7). The relations between Doeblin and Lévy are almost oedipal, like those between Wolfgang and Alfred Doeblin. For this subject see the beautiful book of M. Petit [2003]. We may recall that W. Doeblin's first publication was joint with Lévy and in it Doeblin showed "easily" a conjecture of Lévy. (11)

(12) [Steinhaus, 1930b], [Jessen 1929b, 1930]. (12)

(13) [Jessen 1934a], § 9 and especially § 13 and 14 which with the Jessen theorems are the finest part of the paper. Here perhaps is the origin of Lévy's "awakening." Obviously Jessen's integral on Q_ω is not an integral in the sense of Cauchy for the continuous functions are generally not integrable, but, as it is in "correspondence" with the Lebesgue integral, it acquires enough properties from it to be applicable to the theory of denumerable probabilities, in particular the Fubini property. This is also at the heart of Lévy's lemma but Lévy did not realise this until he had read Jessen. (13)

(14) As the reader will appreciate, Lévy's proof is perfectly correct and very simple but it assumes that the function f is bounded, without indicating correctly how to go to the case of an unbounded integrable function. On the other hand, the proof of the corollary is quite invalid, as Jessen recognised immediately.

Fundamentally the transition from Lévy's lemma to Jessen's theorem is natural and quite obvious as Doob grasped at once [1940], [1953]. The explanation is given by Lévy in his article [1936b], which can be seen as a supplementary letter to Jessen. Indeed Lévy p. 177-178 notices that, by transfer, his lemma is nothing other than Lebesgue's density theorem, while Jessen's theorem corresponds to Lebesgue's differentiation theorem. Moreover he states that Lévy's lemma can be proved in this way, although the proof is more complicated than his "direct" method ([1936b], p. 178). Actually it is known that Lebesgue first obtained his density theorem from the theorem on differentiation, [1905], and then proved the differentiation theorem from the density theorem in [1910], § 33; this second method is that recommended by La Vallée Poussin in his contemporary work. There is no reason to surprised that the same situation is found in the new framework. (14)

Notes to letter 3: Jessen April 8 (?) 1935

(1) At the top of this letter Jessen wrote in pencil, "Sendt I noget anden Form," which Christian Berg has kindly deciphered and translated as, "Sent in a somewhat different form." (1)

(2) J. Aldrich informs us that the pamphlet by Daniell that Jessen mentions to Lévy was not known in the literature. It is an extremely interesting review and would deserve detailed study. The pages 60 and 61 quoted by Jessen show explicitly how the measure that Jessen constructed in 1929 on Q_ω can be defined starting from a Daniell integral, adding: "This type of integral might possibly be useful in connection with probability of sets of functions defined by means of Fourier constants or by the coefficients of a series expression." This prophetic sentence seems to anticipate, or at least forecast, the works of Steinhaus of 1924-1930 and of Paley, Wiener, Zygmund of 1930 on random Fourier series. For more information, see J. Aldrich [2007], to which we owe the essence of this note and whom we thank very warmly. (2)

(3) Jessen met Wiener at the end of 1933. In his talk at the AMS conference in December 1933, to which Jessen also contributed, Wiener constructed his measure by using the principle of correspondence [Wiener 1935], also [Wiener, Zygmund 1933]. This principle is also the basis for his great paper [1930] quoted by Jessen and of his work with Paley in 1930; presumably he learned it from Steinhaus or from Paley. As already briefly indicated, the

Daniell integral is not useful for calculations, it is only there to ensure their coherence. To calculate it is to better use the Lebesgue integral by transfer, or the Gateaux averages, or the approximation by the game of heads or tails, or the changes of variables and suitable symmetries, etc In the bibliography will be found the precise references of the memoirs quoted by Jessen. (3)

(4) [Denjoy 1933]. (4)

(5) Evidently there is a piece missing here in which Jessen told Lévy of his work in progress on the extension of his theory to an abstract framework. (5)

(6) We have not found any reference by Jessen to [Lévy 1925a, b]. [Lévy 1937] is cited in [Andersen, Jessen 1946]. (6)

(7) Jessen's objections are well founded. (7)

(8) This part is difficult to decipher. Jessen wrote over the original text in blacker ink obscuring the original formulas. (8)

(9) This bracket, which appears in the margin of the text and which was thus added afterwards, seems to be by itself, but it does not appear in the initial article [1934a], nor in [Jessen Wintner 1935]. § 11 mentions only an application of the "important lemma" to a result of Steinhaus [1930c]. Between the publication of his article and this letter of April 1935, Jessen learned of the existence of 0-1 law of probability theory, perhaps by reading Kolmogorov's *Grundbegriffe* more attentatively.

For his part, as we have noted, Lévy initially saw Jessen's important lemma as a consequence of the lemma I of [1935b], without stating that it followed from Kolmogorov's 0-1 law. Moreover, a short note by L. Schwartz [1936], which undoubtedly resulted from a conversation between Schwartz and Lévy at the time of a Sunday meal in the spring of 1935, also attributed to Jessen [1934a] the 0-1 law in the probabilistic formulation of Kolmogorov, of whom Schwartz was obviously unaware. The mathematical conversations between Lévy and his future son-in-law are discussed by [Schwartz 1997], p. 93-95. Laurent Schwartz did not continue in this line although he took it up again at the end of the sixties; see e. g. [Schwartz 1980] and [Yor 2003].

On the other hand, Lévy devoted a paragraph of his survey article [1936b], p. 179, to the "Lemma of MM. Kolmogorov et Jessen", (0-1 law), quoting the *Grundbegriffe* of 1933 in a note. Thus Lévy finally took note of Kolmogorov's text between May and December 1935; perhaps it was at the time of Jessen's letter of August 11, 1935 although we cannot be sure. See also a letter to Fréchet of January 29, 1936 in BLM, p. 155-156 and its very enlightening note 111. The entire book is indispensable for anyone study the work of Lévy.

We see that in [1936b], p. 179, Lévy raised the question of how the 0-1 law of the theory of denumerable probabilities could be transferred to the interval [0,1] endowed with Lebesgue measure, by using the principle of correspondence. In a note he writes, "It is necessary to note that, taking into account the principle of linear representation indicated in § 4, this lemma is to be brought closer to a theorem on linear sets established in 1916 by M. Burstin, which is a corollary of Lebesgue's theorem, as the lemma of M. Jessen is a corollary of our theorem of § 10 (the lemma of Lévy)".

We do not know how Lévy learnt of Burstin's result. Did they meet in Bologna? Celestyn Leonovitch Burstin was born on January 28, 1888 in Ternopol in Ukraine. He studied at the University of Vienna where he published interesting work on analysis, in particular [1916] which Lévy cited, and on Rriemannian geometry; for his work on the latter see Gromov, Rokhlin [1970]. Unable to find a position in Vienna, being Jewish and a member of the Communist party, he emigrated to Bielorussia, where he was a professor at Minsk and a member of the Academy of Science of Bielorussia. During the Stalin purges, he was arrested at the end of 1937 and died in prison on October1 1938. See also Borodin, Bugai [1987]. For the purges in Bielorussia there is the collection published by the Academy of Science of Minsk [1992], and also [Marakou, 2003-2005]. These two last references were provided by J. - M. Kantor, whom we thank most warmly. (9)

(10) The new proof of the theorem of § 14 that Jessen gave to Lévy, where one senses the influence of Lévy's note, is the first direct proof of Jessen's theorem. It makes no appeal to the theorem on differentiation or to the principle of transfer. It is so close to the analogous theorem of [Jessen 1934-1947, 4] and [Andersen, Jessen, 1946] that that one cannot see what prevented Jessen from publishing the result ten years earlier, if it was not the lack of the abstract framework he considered appropriate. Lévy also lacked a framework with the result that the correspondence was not a complete success. However the framework was almost clear in Lévy's [1937] version of the lemma, but Jessen did not see this and neither did Doob in his first article of 1940. Lévy never saw it though he was persuaded of the contrary. Simple mathematical ideas are always the most hidden, as Laplace, who could calculate everything, complained. (10)

(11) Jessen follows the same reasoning as Lévy and he cannot be reproached. To drown Lévy's proof of the bounded case, as short as it is elegant, in a fog of vague comments would be to devalue it: the bounded case is treated very easily, all that is well-known for me for a long time,… (11)

(12) This refers to the memoir announced in Jessen-Wintner [1935] which was only published in 1939 in the series of memoirs [1934-1947]. Lévy's name does not appear there although his book [1937] is in the general bibliography published in the last memoir of 1947, n° 10, and in [Andersen Jessen 1946]. (12)

Notes to letter 4: Lévy April 24 1935

(1) Hennequeville is a district of Trouville in Calvados where Lévy was probably spending the Easter holidays. In 1935 Easter was on April 21. (1)

(2) Lévy did not understand or had not yet read Daniell. He thought that Daniell had used the principle of correspondence to construct his integral. He reconsiders the point in the next letter. (2)

(3) [Lévy 1935b], p. 89 note 1, which only cites part of Jessen's memoir, § 11 "An important lemma". Lévy corrects this in his Notice [1935a], p. 43-44, where he writes, "This theorem [Lévy's lemma] was obtained independently of me by M. Jessen, or at least it appears to be a special case of a theorem of M. Jessen, who in addition indicates a more special case and one of great importance." The reference is to Jessen's "important lemma", i.e. Kolmogorov's 0-1 law in Jessen's framework. We have already commented on the note on page 45 where Lévy states that the memoir [1935b] "was sent to the editors of the *Bulletin des Sciences Mathématiques* in September 1934". Again in [1936b], p. 179, Lévy states that M. Jessen had established "several theorems on integration in Q_ω which include and exceed" Lévy's lemma. Lévy did not include similar statements in his [1937] or later writings. (3)

(4) [Lévy, 1935d]. (4)

(5) [Lévy, 1935a], p. 28, note 1. (5)

Notes to letter 5: Lévy May 3 1935

(1) [Fréchet 1915] constructed a theory of integration associated with an abstract measure, on the model of the Radon-Stieltjès-Lebesgue integral, but he did not construct a (non-trivial) measure in a space of infinitely many dimensions, contrary to what Lévy says and would go on saying. In his reply Jessen insists very precisely on this point but without much effect. In Lévy's mind measure in infinite dimensions existed and so obviously that there was no need for an explicit construction, which a posteriori was perfectly obvious. For Jessen on the contrary, what mattered was the construction of the measure in all its rigour and complexity, and he wished to date it and attribute it as accurately as possible. He attributed it to Daniell in the first place, but also to Jessen who rediscovered it only a few years later, to Steinhaus and others, and perhaps even to Lévy, though Jessen is too polite to write exactly what he thinks on this matter). A dialogue without issue. (1)

(2) Lévy does not quote Daniell in his note of 1924-1925, but in [1925c] (see his next letter). He then cites him regularly from 1934, in particular in the big paper [1934b], in the *Notice* [1935a], p. 28, note 1, then in [1936b], § 7, p. 167, [1937], n° 10, p. 17-18, note 2, where Lévy again merges his own work from 1918-1919 with that of Daniell 1918-1919, and with the earlier work of Fréchet 1915, which, however, does not address itself to the integral of Daniell. Is the reader, even a relatively well-disposed one, convinced? And Lévy himself, why does he keep going back? (2)

Notes to letter 6: Jessen August 11 1935.

(1) Without diminishing in any way Daniell's fundamental contribution which gave the foundations of Kolmogorov their generality and the measures of Wiener and Lévy their first mathematical existence, one can undoubtedly make the opposite case, following [Steinhaus 1938]. The fact that the Daniell integral was little known in continental Europe was also an opportunity. Instead of the Daniell integral, the analysts (Danish and Polish especially) created and applied the principle of correspondence, finding theorems that could not be found using the Daniell integral. There were, for example, the theorem in Jessen's § 14 on the reflection in infinite dimensions of the differentiation theorem of one dimension, or, in the other direction, there was the theorem, which apparently Lebesgue did not see but which Borel nicely anticipated ([1912b], chapter II), viz. that Riemann sums of an integrable function in the sense of Lebesgue converges to the Lebesgue integral of this function almost everywhere for increasingly fine partitions of the interval of definition; this was a reflection in one dimension of the theorem in Jessen's § 13, as Jessen showed in [1934b]. And, as is well-known, the theorem of Borel-Jessen is a way of seeing the strong law of large numbers, as Doob would make clear in 1948, [Locker 2009], him which places from the start in the axiomatic one of Kolmogorov. (1)

(2) Jessen's comments are to the point. Fréchet's article does contain a theorem on the extension of a measure on an algebra to the generated σ-algebra, independently of the theorem of Carathéodory [1918-1927]; see e. g. Bogachev [2006], vol. 1, p. 419. Thus one might follow Lévy here and consider that Fréchet could have had measure in infinite dimensional space, if only he had thought of looking for it. Bogachev has further comments. (2)

Notes to letter 7: Lévy August 23 1935

(1) The Lévy family spent their holidays in San Cristina in the Dolomites. Bernard Locker has very kindly given us information on this subject: "Denise Lévy-Piron often spoke to me about the pleasure her father took in spending time in the mountains, even telling me that "my father was an exceptional mountaineer", which I doubted, attributing the adjective "exceptional" to filial piety… San Cristina is in the valley of Val Gardena in Italy…. and I know from M. and Mme Piron that Lévy adored the mountain and Italy…. ". (1)

Notes to letter 8: Jessen July 14 1947

(1) Lévy liked to entertain foreign mathematicians passing through Paris. See BLM, p. 12, where K.L. Chung recalls a dinner at avenue Theophilus-Gautier, at the end of which Lévy served Port-Salut cheese. In those days the cheese was made by the monks of the abbey of Port-Salut and is unrelated to the cheese now sold under that name. We do not know which members of the Lévy family were present at the Danish dinner. (1)

(2) The reference is to the conference on harmonic analysis, chaired by S. Mandelbrojt, and held in Nancy on 15 June 22, 1947. This prestigious conference was financed by CNRS and the Rockefeller Foundation. It brought together the great names in a field going through great changes. Among the invited lecturers was, of course, Lévy who presented a paper on the harmonic analysis of stationary random functions, which contained a very beautiful result of Blanc-Lapierre and Fortet ([1946, 1953]). Whether Lévy was remembering or rediscovering this could not have pleased the two involved; on this subject see [Brissaud 2002], p. 31). For their part, Bohr and Jessen presented a paper on almost periodic functions [1947]. The conference proceedings were published by the CNRS in 1949. For the post-war CNRS-Rockefeller conferences see Locker [2009]. (2)

(3) At the Nancy conference L. Schwartz described Fourier analysis for distributions which greatly impressed Harald Bohr. Bohr would present Schwartz's work to the International Congress of Mathematics in Cambridge USA in 1950, and he would share in its international fame. In his memoirs L. Schwartz [1997], chapter VIII, p. 309 describes his visit to Copenhagen in October 1947 at the invitation of H. Bohr. (3)

Appendix: Jessen's theorem in Jessen's theses

As explained in Section 2, Jessen's two theses are in Danish and relatively unavailable. The master's thesis [1929a] is a manuscript in the archives of the Mathematics Institute of Copenhagen. The dissertation [1930] was printed in Copenhagen but not widely distributed. There are copies in the Bibliothèque Nationale Française and in several libraries in the United States, but not in the Library of Congress.

We are grateful to Sigurd Elkjær, who has provided the following English translation of the main passages concerning Jessen's theorem in the two theses. In the passages from the master's thesis, Jessen states and proves his theorem for convergence in measure. In the dissertation, he states and proves the same theorem for strong convergence in L^1. The notation Jessen uses is explained in Section 2.

1. From the master's thesis (Magisterkonferens)

Chapter 7 of the thesis is entitled *On functions of infinitely many variables*. Following is a translation of Sections 1, 8, and 9 of this chapter.

1. Introduction.

Functions which depend on a sequence of variables have only been scantily investigated in the literature (in spite of the fact that their significance has been strongly emphasized). In what follows, an attempt is made to show the validity of the Lebesgue theory for functions of infinitely many variables. It turns out that this is possible, even with all essential theorems preserved. However, problems are encountered which do not have their counterpart in functions of finitely many variables, for example which meaning should be attributed to an infinitely multiple integral

$$\ldots\int_0^1 dx_n \ldots \int_0^1 dx_2 \int_0^1 f(x_1, x_2, \ldots, x_n, \ldots) dx_1$$

This investigation has been carried through to the two theorems, which appear to me to be the most beautiful in the theory of real functions, which are the theorem on splitting of a multidimensional integral into simple integrals, and the Riesz-Fischer theorem.

8. Representation of a function as the limit of an integral.

Let $f(x) = f(x_1, x_2, x_3, \ldots..)$ be a summable function in Q_ω. For each value of *n*, we look at the integral over the unit cube $Q_{n,\omega}$ determined by the coordinates $x_{n+1}, x_{n+2}, \ldots$

(2) $$\int_{Q_{n,\omega}} f(x_1, x_2, \ldots., x_n, x_{n+1}, \ldots.) dw_{n,\omega} \quad .$$

According to the Fubini theorem, this is a summable function of $x_1, x_2, \ldots, x_n$, which is defined for almost all points in the *n*-dimensional unit cube Q_n. If, for each value of *n*, we regard it as a function of *x* which is constant in $x_{n+1}, x_{n+2}, \ldots$, then we can say: All the functions (2) are defined except in a fixed set of measure zero (that is, independent of *n*) in Q_ω. We will show that:

The integral (2) converges in measure for $n \to \infty$ *toward* $f(x)$.

Let us first assume that $f(x)$ is bounded, e.g. $|f(x)| < M$. Let $\eta > 0$ be given. We consider the function $\Delta_n(x)$, which were introduced earlier. They converge, for $n \to \infty$, almost everywhere toward $f(x)$, and we determine (according to the theorem of Egoroff) *N* sufficiently large, so that the set of points in Q_ω, for which $f(x)$ and

$\Delta_n(x)$ deviate more than η from each other, at most has the measure η^2. For each value of n, which is greater than N, we have

(3) $$\int_{Q_{n,\omega}} \Delta_N(x_1,x_2,....,x_n,x_{n+1},x_{n+2},....)dw_{n,\omega} = \Delta_N(x) \quad ,$$

since $\Delta_N(x)$ is constant in the variables $x_{N+1},x_{N+2},....$ For such an n, we now consider the set of points e_n in Q_n, for which the function $|f(x)-\Delta_N(x)|$, regarded as a function of $x_{n+1},x_{n+2},...$, either is not summable or, alternatively, is summable and in a set, the measure of which is greater than η, is itself greater than η. This set e_n is measurable, and its measure is (according to the Fubini theorem), at most η. For each point of Q_n outside e_n we have, according to (3),

$$\left|\Delta_N(x) - \int_{Q_{n,\omega}} f(x_1,x_2,....,x_n,x_{n+1},....)dw_{n,\omega}\right| \le 2M\eta + \eta$$

In all of Q_ω except for a set, the measure of which is at most $\eta+\eta^2$, we have accordingly

$$\left|f(x) - \int_{Q_{n,\omega}} f(x_1,x_2,....,x_n,x_{n+1},....)dw_{n,\omega}\right| \le 2M\eta + 2\eta$$

This finishes the proof.

Next, we consider the case where $f(x)$ is not bounded. We write, for $M>0$ $f(x)=f_M(x)+r_M(x)$, letting

$$f_M(x) = \begin{cases} M \\ f(x) \\ -M \end{cases} \text{when} \begin{cases} f(x) > M \\ |f(x)| \le M \\ f(x) < -M \end{cases} .$$

If $\eta>0$ is given, we choose M sufficiently large, so that, firstly, $f(x)=f_M(x)$ except in a set, the measure of which is less than η. And secondly, the integral

(4) $$\int_{Q_\omega} |r_M(x)| dw_\omega$$

is less than η^2. The splitting of $f(x)$ also divides the integral (2) into two parts:

(5) $$\int_{Q_{n,\omega}} f dw_{n,\omega} = \int_{Q_{n,\omega}} f_M dw_{n,\omega} + \int_{Q_{n,\omega}} r_M dw_{n,\omega}$$

Here, we have

$$\left|\int_{Q_{n,\omega}} r_M dw_{n,\omega}\right| \le \int_{Q_{n,\omega}} |r_M| dw_{n,\omega}$$

Because the integral of the last (almost everywhere in Q_n defined) function over Q_n according to the theorem of Fubini is equal to (4), and so is less than η^2, the second term in (5) can at most in a set, the measure of which is less than η, be numerically greater than η.

Now, after the proof above, let N be chosen sufficiently large, so that, for every $n>N$, the first term of (5) at most in a set with the measure η deviates more than η from . Then, (2) will at most in a set with the measure 3η deviate more than 2η from $f(x)$. Thus, the theorem is proved.

9. Infinitely multiple integrals.

We consider, for each value of n, the integral over the unit cube Q_n determined by the coordinates $x_1,x_2,...,x_n$

(6) $$\int_{Q_n} f(x_1,x_2,...,x_n,x_{n+1},x_{n+2},...)dw_n$$

.

The integral (6) represents a function which is constant in $x_1, x_2, \ldots, x_n$, and which is defined almost everywhere in Q_ω. The preceding theorem makes it possible for us to give a very simple proof of the following theorem (*) :

The integral (6) converges in measure for $n \to \infty$ *toward the integral over* Q_ω

(7) $$\int_{Q_\omega} f(x) dw_\omega \quad .$$

Let us first assume that $f(x)$ is bounded : $|f(x)| < M$, and let η>0 be arbitrarily given. We choose *N* sufficiently large, so that for every *n*>*N*, and everywhere in Q_n except in a set, the measure of which is smaller than η^2, the integral (2) deviates at most η from $f(x)$. Then $f(x)$, conceived as a function of $x_1, x_2, \ldots, x_n$ in a set in $Q_{n,\omega}$, the measure of which is greater than η, deviates more than η from (2) at most in a set in , the measure of which is smaller than η. The integral (7) deviates more than $2M\eta+\eta$ from (6) in a set, the measure of which is at most η. This is seen by integrating over Q_n, since the integral (2), like $f(x)$ itself, is numerically smaller than *M*. This was what had to be shown.

If $f(x)$ is unbounded, the splitting $f(x) = f_M(x) + r_M(x)$ is again performed, and again we choose *M* sufficiently large, so that the integral (4) becomes less than η^2, and subsequently we choose *N* sufficiently large, so that, for *n*>*N*, the integral

$$\int_{Q_n} f_M(x_1, x_2, \ldots, x_n, x_{n+1}, x_{n+2}, \ldots) dw_n$$

deviates more than η from

$$\int_{Q_\omega} f(x) dw_\omega$$

at most in a set with the measure η. Then (6) will deviate more than $2\eta+\eta^2$ from (7) in a set, the measure of which is at most 2η, and the theorem is again saved.

The theorem which we have deduced has a counterpart in the theorem in the preceding section. There, we integrated over fewer and fewer variables, and we obtained the function as the limiting value; here, we have integrated over more and more variables, and we obtained its integral as the limiting value.

Another formulation of the last theorem is the following:

When $f(x)$ *is summable over* Q_ω*, we have*

$$\int_{Q_\omega} f(x) dw_\omega = \ldots \int_0^1 dx_n \ldots \int_0^1 dx_2 \int_0^1 f(x_1, x_2, \ldots, x_n, \ldots) dx_1$$

where the symbol on the right denotes the constant, toward which

$$\int_0^1 dx_n \ldots \int_0^1 dx_2 \int_0^1 f(x_1, x_2, \ldots, x_n, \ldots) dx_1$$

converges in measure for $n \to \infty$.

All of the last integrals exist except in a fixed null set in Q_ω.

By means of the concept which we have introduced, namely the infinitely multiple integral, the theorem in the preceding section can be formulated in this way:

The integral

$$\ldots \int_0^1 dx_{n+1} \int_0^1 f(x_1, x_2, \ldots, x_n, x_{n+1}, x_{n+2}, \ldots) dx_{n+1}$$

converges in measure for $n \to \infty$ *toward* $f(x)$.

(*) By returning once again to the functions $\Delta_n(x)$, we can obtain another simple proof.

2. From the dissertation

Following is a translation of Sections 15 and 16 of the dissertation of 1930.

15. Infinitely multiple integrals.

We are now in a position to carry out the mentioned *extension of the* Fubini *theorem* to functions of infinitely many variables. Let $f(x)$ be a function which is summable in Q_ω. We will show that, with a suitable definition of the expression on the right-hand side of the equation, the relation

$$\int_{Q_\omega} f(x)dw_\omega = \dots\int_0^1 dx_3 \int_0^1 dx_2 \int_0^1 f(x_1,x_2,x_3,\dots)dx_1$$

holds.

Let N be a positive integer; we apply the result from the previous section, letting

$$x' = (x_1,x_2,\dots,x_N)$$

$$x'' = (x_{N+1},x_{N+2},\dots)$$

If we designate by Q_N and $Q_{N,\omega}$ respectively, the unit cube in the space determined by the coordinates $x_1,x_2,\dots,x_N$ and $x_{N+1},x_{N+2},\dots$ respectively, we get

$$\int_{Q_\omega} f(x)dw_\omega = \int_{Q_{N,\omega}} dw_{N,\omega}\int_{Q_N} f(x_1,x_2,\dots,x_N,x_{N+1},x_{N+2},\dots)dw_N$$

In other words: the function

(8) $$\int_{Q_N} f(x_1,x_2,\dots,x_N,x_{N+1},x_{N+2},\dots)dw_N$$

defined almost everywhere in $Q_{N,\omega}$, is summable over $Q_{N,\omega}$ with the integral

$$A = \int_{Q_\omega} f(x)dw_\omega$$

For the sake of what follows, it is more convenient instead of (8) to consider the function

$$f_{N,\omega}(x)$$

which is defined almost everywhere in Q_ω, and which at each point $x = (x_1,x_2,\dots,x_N,x_{N+1},x_{N+2},\dots)$ is equal to the value of (8) at the corresponding point $(x_{N+1},x_{N+2},\dots)$ of $Q_{N,\omega}$. This function, which is constant in the variables $x_1,x_2,\dots,x_N$, is obviously summable over Q_ω, and its integral is equal to the integral of (8) over $Q_{N,\omega}$, that is, A. We will show, that, for $N\to\infty$, the function $f_{N,\omega}(x)$ converges strongly toward the integral A, in other words that

$$\int_{Q_\omega} |f_{N,\omega}(x)-A|dw_\omega \to 0 \quad \text{for} \quad N\to\infty$$

The proof is extremely simple. Let $\varepsilon>0$ be given; we have to show that for every N, which is larger than a certain value,

(9) $$\int_{Q_\omega} |f_{N,\omega}(x)-A|dw_\omega < \varepsilon$$

.

For this purpose, we consider the functions $\Delta_N(x)$, which were introduced in §12, and which according to §13 for $N\to\infty$ converge strongly toward $f(x)$, and we determine an N_0 sufficiently large, so that

(10) $$\int_{Q_\omega} |f(x)-\Delta_{N_0}(x)|dw_\omega < \varepsilon$$

.

Then, for $N\ge N_0$, the relation (9) holds; because from (10) it appears for each N as a consequence of §14, that

$$\int_{Q_{N,\omega}} dw_{N,\omega}\int_{Q_N} |f(x)-\Delta_{N_0}(x)|dw_N < \varepsilon$$

that is, furthermore,

$$\int_{Q_{N,\omega}} dw_{N,\omega} \left| \int_{Q_N} \left(f(x) - \Delta_{N_0}(x) \right) dw_N \right| < \varepsilon$$

but $\Delta_{N_0}(x)$ being constant in the variables $x_{N_0+1}, x_{N_0+2}, \ldots$ and having its integral over Q_ω equal to A, for $N \geq N_0$ this is simply the inequality (9).

If we apply the usual Fubini theorem to the integral (8), the result we have obtained can be formulated in this way:

If $f(x)$ is a function which is summable in Q_ω, we have

$$\int_{Q_\omega} f(x) dw_\omega = \ldots \int_0^1 dx_3 \int_0^1 dx_2 \int_0^1 f(x_1, x_2, x_3, \ldots)$$

where the expression on the right-hand side of the equation denotes that constant, toward which the integral

$$\int_0^1 dx_N \ldots \int_0^1 dx_2 \int_0^1 f(x_1, x_2, \ldots, x_N, x_{N+1}, x_{N+2}, \ldots) dx_1$$

conceived as a function of x which is constant in the variables $x_1, x_2, \ldots, x_N$, converges strongly for $N \to \infty$.

The symbol which we have introduced is referred to as an *infinitely multiple integral*.

16. Representation of a function as the limit value of an integral.

The theorem, which was proved in the preceding section, has a natural counterpart, which will be proved by a quite similar consideration.

Let $f(x)$ be summable in Q_ω, and let again *N* be a positive integer. Setting

$$x' = (x_{N+1}, x_{N+2}, \ldots)$$
$$x'' = (x_1, x_2, \ldots, x_N)$$

then, from the result from §14, the relation

$$\int_{Q_\omega} f(x) dw_\omega = \int_{Q_N} dw_N \int_{Q_{N,\omega}} f(x_1, x_2, \ldots, x_N, x_{N+1}, x_{N+2}, \ldots) dw_{N,\omega}$$

then follows. The function

$$\text{(11)} \qquad \int_{Q_{N,\omega}} f(x_1, x_2, \ldots, x_N, x_{N+1}, x_{N+2}, \ldots) dw_{N,\omega}$$

defined almost everywhere in Q_N, is therefore summable over Q_N. From this it follows, that the function

$$f_N(x) = f_N(x_1, x_2, \ldots, x_N, x_{N+1}, x_{N+2}, \ldots)$$

which is defined almost everywhere in Q_ω, and which at every point *x* is equal to the value of (11) at the corresponding point $(x_1, x_2, \ldots, x_N)$ in Q_N, and therefore is constant in the variables $x_{N+1}, x_{N+2}, \ldots$, is summable over Q_ω. We will show, *that the function $f_N(x)$ converges strongly toward $f(x)$ for $N \to \infty$*, that is, for every $\varepsilon > 0$ and for all *N* from a certain step

$$\text{(12)} \qquad \int_{Q_\omega} \left| f(x) - f_N(x) \right| dw_\omega < \varepsilon$$

.

With this purpose, we will proceed in a way similar to that above, and choose a number N_0 sufficiently large, so that the relation

$$\text{(13)} \qquad \int_{Q_\omega} \left| f(x) - \Delta_{N_0}(x) \right| dw_\omega < \frac{\varepsilon}{2}$$

is satisfied. Then, according to §14, for every *N*

$$\int_{Q_N} dw_N \int_{Q_{N,\omega}} \left| f(x) - \Delta_{N_0}(x) \right| dw_{N,\omega} < \frac{\varepsilon}{2}$$

and so, furthermore

$$\int_{Q_N} dw_N \left| \int_{Q_{N,\omega}} \left(f(x) - \Delta_{N_0}(x)\right) dw_{N,\omega} \right| < \frac{\varepsilon}{2}$$

Because $\Delta_{N_0}(x)$ is constant in the variables $x_{N_0+1}, x_{N_0+2}, \ldots$, then for $N \geq N_0$, this is simply the inequality

$$\int_{Q_\omega} \left| f_N(x) - \Delta_{N_0}(x) \right| dw_\omega < \frac{\varepsilon}{2}$$

and this, in combination with (13), shows that (12) is correct for all $N \geq N_0$.

In the theory of the Fourier series, we shall make an important application of the theorem which we have proved. After the infinitely multiple integral is defined, it can be formulated as follows:

If $f(x)$ is an arbitrary function which is summable in Q_ω, then, for $N \to \infty$, the integral

$$\ldots \int_0^1 dx_{N+2} \int_0^1 f(x_1, x_2, \ldots, x_N, x_{N+1}, x_{N+2}, \ldots) dx_{N+1}$$

regarded as a function of x which is constant in the variables $x_{N+1}, x_{N+2}, \ldots$, converges strongly toward the function $f(x)$.

6 Postscript : Doob-Jessen and Dieudonné-Jessen correspondence

These are from the Jessen Archive at the Institute of Mathematics at Copenhagen; see the Introduction above. Written in the course of a year, from the spring of 1948 to the spring of 1949, they are presented in chronological order. They mainly concern abstract generalisations of the Daniell-Kolmogorov theorem and of the disintegration theorem advanced by Doob in 1938. In 1948 Dieudonné and Sparre Andersen and Jessen showed independently that these generalisations were not possible. The details are given in letter 2 and in notes 34 and 39.

1. Jessen to Doob. May 11, 1948.

Dear Professor Doob

As you will have noticed Mr Sparre Andersen and I have raised doubt as to the validity of the proof of your abstract generalization of the theorem on the introduction of measures in a real Cartesian space of an infinite number of dimensions. The problem had interested us very much, and actually Sparre Andersen had given a proof of an even more general theorem (1). This proof we had discovered to be wrong before noticing that the theorem was in your paper from 1938. On reading your proof we found, however, that it used an argument, which we had also attempted to apply but could not carry through, and your proof therefore seemed incomplete. I owe you an apology for not having written to you before publishing our paper (2), my only excuse is that when the paper was written the mail service to America had not yet been opened.

When I write to you now, it is because we believe to have found a counter-example of the theorem, and this we would like to show you before publishing it.

…. (3)

I would very grateful to hear your opinion of this example.

I remain very sincerely yours

Børge Jessen

2. Doob to Jessen, May 17, 1948

Dear Professor Jessen :

Thank you for your letter, although I can hardly say its news was welcome. I had already realized that the theorem following the one to which you give a couterexample was false, but I had not realized that the first was false (4). These two theorems are very closely related, and counterexamples to the two are of essentially the same type.

Perhaps you have seen Kakutani's proof in the case of independent finite dimensional measures the extension to the infinite dimensional case is correct. (5) His proof is quite simple, of the same type as the treatment of two dimensional measure by Hopf in the latter's Ergodentheorie in the Ergebnisse series. Kakutani's proof can be used word for word in the general case, with the following hypothesis, which saves the theorem for the purposes of probability.

Let $P(E)$ be a probability measure in x_1 space.

For $n>1$ let $P(x_1,\dots,x_{n-1};E)$ be for fixed $x_1,\dots,x_{n-1}$ a probability measure in x_n sets *E*. Then these probability measures can be used to define finite dimensional measures in the usual way ; $P(E)$ is the x_1 probability measure and the other function is the conditional probability of x_n sets if the preceding x_j's are known. Under the hypothesis that there are such conditional probabilities to define the finite dimensional measures, it follows that the extension to infinitely many dimensions can be accomplished following Kakutani in the independent case, in which case the conditional measures do not actually depend on the conditioning variables. Conversely, if finite dimensional measures are given, they determine conditional probabilities as described above in very general cases but not always (my second theorem is also false). Your example is also a counterexample to the second theorem. (6)

I think that this way of looking at it is helpful ; to find out when the extension to infinitely many dimension is possible, one may find out when the finite dimensional measures are determined by conditional probability measures. This is true for example if the coordinate spaces are themselves finite dimensional Borel sets, as can be seen by the principle of my proof ; the essential property is that the given field can be mapped on a Borel sets, of course the conditions I describe are not necessary, but I suspect that they are pretty close to it. (7)

Do you intend to visit this country in the next few years? Our work has many common points and it would be interesting to discuss these and other matters in detail. (8)

Sincerely,

Doob

3. Jessen to Doob, May 29, 1948

Dear Professor Doob

Thank you for your letter. That a counter example to your first theorem would imply that conditional probability fields need not exist, I knew, having realized in the course of my attempts to prove this theorem, that the proof succeeds along the same lines as the proof in case of product measures when conditional probability fields exist. Kakutani's proof for product measures I do not know, since we have not received the Japanese journals from the war. But it can hardly be simpler than my proof which was announced in Wintner's and my paper in Trans. Amer. Math. Soc. 88 (1935) (§15) and which appeared (in danish) in Mat. Tidsskr. B 1939. This proof (which I believe to be the proof that has been published) is reproduced in Sparre Andersen's and my article in Dansk Vid. Selsk. Mat.-fys. Medd. 22, n° 19 (1948) (§ 23).

The introduction of a measure in an infinite product by means of conditional probability fields I have not worked out in detail. It hardly seemed worth while as long as the validity of your first theorem was undecided. Now it seems to me that it should be done. What would you think if we joined in a little article giving this result which, as you mention, is sufficient for the probability applications. Further results might possibly be included. Sparre Andersen and I might then in our article put in some words to the effect that according to our example the introduction of measure in infinite products intended to cover the case of dependent variables must be done in a different manner and that you and I would treat this question in a forthcoming paper (9). I expect to spend the major part of 1949 (from february) in America and hope very much to see you. We might write the paper then or perhaps we might do it by correspondence, though that, of course, is not so convenient.

Sparre Andersen will be in America this winter spending most of his time with prof. Feller at Cornell, who, as you may know, is a good friend of the Copenhagen mathematicians (10).

Sincerely yours

Børge Jessen

4. Doob to Jessen, June 4, 1948.

Dear Professor Jessen :

I think it would be a good idea to write a joint paper clearing up this whole subject. I first heard about my error through Halmos who sent me a counter example to my second wrong theorem that had been sent him by Dieudonné. My errors were rather unfortunate, among other reasons because the second theorem was used very essentially in papers by Halmos, Kakutani and Ambrose (11). I have just been trying to read your proof of the existence of measure in infinitely many dimensions in the independence case, and as far as I can understand the language it seem to be the same as that of Kakutani which I entionned to you. Of course yours is much earlier (12). I have a vague recollection that von Neumann may have also prove the theorem in a course of lectures at Princeton, and that it appeared in a mimeographed edition of his lectures, but we do not have the volume in our library (13).

I do not think that there is any hurry in our publication. We might as well wait until you are here in this country. Perhaps you will be able to visit Urbana for a while. Please write me your plans when they are definite. I shall be in Europe to attend the Lyon conference on probability and statistics, but shall return immediately after it, leaving July 9, from Cherbourg (14).

Our work has had many points of contact. The war has confused my records, and I do not know which of my reprints I have sent you. Have I sent you the one Amer. Math. soc. Trans. 1940 in which I derive theorems which are essentially yours in Kgl. Danske Vi. Sels. 1946 ? (15) Of course our terminologies and points of view are quite different. I do everything from the point of view of functions, you from the point of view of set functions (16). The theorems involved are very important in probability theory, and I am going to discuss various applications at Lyon.

If Andersen will be at Cornell, I shall see him. Feller and I visit each other frequently (17).

Best wishes,

Doob

5. Jessen to Dieudonné, June 17, 1948 (18)

Dear Professor Dieudonné.

Together with Mr. Sparre Andersen I have recently found an example showing that the Daniell-Kolmogoroff theorem (Kolmogoroff : Grundbegriffe der Wahrscheinlichkeitsrechnung, p. 27) on the introduction of measure in an infinite Cartesian product by means of consistent measures in the finite sub-products cannot be extended to abstract sets in the case where the coordinates are dependent (in the probability sense). The example will be published in Danske Vid. Selsk. Mat.-fys.Medd. (19)

Professor Doob, who like Sparre Andersen has attempted to prove the extension, and whom I have communicated our example, has informed me, that you have given a counterexample of the related theorem about the existence of conditional probability measures. Naturally, our example is also a counterexample of this theorem, since the extension of the Daniell-Kolmogoroff theorem to abstract sets may be carried through in the same manner as for product measures when conditional probability measures are supposed to exist.

I would be very grateful if you would let me know whether your example has been published in order that we may then quote it. If it has not we shall restrict ourselves to mention that you have given such an example.

I allow myself to send you (under separate cover) some of my papers relating of functions of infinitely many variables.

Please give my kind regards to your colleagues. I regret not to have met you when I was at Nancy last year. (20)

Believe me, very sincerely yours

Børge Jessen

6. Dieudonné to Jessen
Nancy, June 28, 1948.

Dear Professor Jessen

I have received your letter of June 17 and your reprints on Integration, for which I thank you most heartily. The example Professor Doob refers to was found by me last september, while working on Prof Halmos's paper « The decomposition of measures » (21). My paper is due to appear in the next few weeks in the « Annales de Grenoble » under the title « Sur le théorème de Lebesgue-Nikodym (III) », p. 25-53 ; the example which interests you is given p. 42. As soon as I have reprints of this paper, I shall have great pleasure in sending one to you, together with some of my older papers on Integration and Banach spaces.

Hoping to have the pleasure of meeting you some day, I am

Very sincerely yours

J. Dieudonné, 2, Rue de la Craffe, Nancy.

7. Jessen to Dieudonné, September 13, 1948

Dear Professor Dieudonné,

Thank you very much for your kind letter and for the reprints which I received some time ago.

The paper of Sparre Andersen and myself has now appeared and I send you enclosed a copy. As yours, our example is based on non-measurable sets. In order to disprove the existence of conditional probability measures it is, of course, sufficient to work in a product of two sets. An example of this type we found long ago, but it was not until recently that we noticed that by the same idea the extension of the Daniell-Kolmogoroff theorem to abstract sets may be disproved.

With best regards, I am

Very sincerely yours

Børge Jessen

8. Jessen to Doob, September 13, 1948

Dear Professor Doob,

Thank you very much for your letter of June 4. I have postponed the answer until I could send you the paper of Sparre Andersen and myself containing our example. I thank you for the references to Dieudonné (whose paper has just appeared) and von Neumann.

I am looking very much to write a joint paper with you on our common results. I except to come to the United States about middle of January and intend to spend the first months at eastern universities. In the second quarter (from March 28 to June 18) I will be in Chicago lecturing, and in the fall (from the middle of september) in Princeton at the Institute of Advanced Study. It will be a great pleasure for me to come in Urbana for a while either before or after the visit in Chicago. Please write me when it would be most convenient for you. (If it is not too hot in June it would perhaps be more convenient to arrange your collaboration after June 18 when I shall not have the lectures to think of.)

Your paper from Trans. Amer. Math. Soc. 1940 I had not received from you, and Sparre Andersen and I were not aware of it when writing our first paper. Your results are, as ours, closely related to my old results on integrals in infinitely many dimensions, though the connection is not so apparent in your exposition. You will notice that in our paper there is a little unsymmetry between the two limit theorems, the first dealing with set-functions which may have a singular part, whereas in the second the set-function is supposed to be continuous with respect to the measure considered. In a note, which will appear in the Danske Vid. Selsk. Mat.-fys. Medd., we give easy generalisations of the two theorems which are completely symmetrical. Here we use the opportunity to quote your paper from 1940. Actually the generalization makes the proofs more conspicuous.

With best wishes, Sincerely yours

Børge Jessen

9. Jessen to Doob, May 17, 1949 (22)

Dear Doob :

Just a few lines to thank you and your wife for all your hospitality during my stay in Urbana. I enjoyed very much being with you and talking with you. On the matters we discussed I have had time to think them (?)

I had my promize not to read english detectivestories cancelled for a night and read one of Steve's novels (23). Please tell him that it was most exciting as was also Gene Autry (24), whom I did not know before. It might interest Steve and Peter that we (or rather Hochschild) found a live turtle at one of the creeks in Turkey Run. (25)

10. Jessen to Doob, June 23, 1949

Dear Doob :

I took longer time than I had expected to get the car, but now it is all in order, and with Mr Calderon from Argentina as chief pilot (26) I expect to leave for Urbana tomorrow about noon (top speed 30 m/h). If the car does not make trouble we shall continue on Monday for New York to meet my wife. As passenger we bring Mr Nachbin from Brazil (27), who is also living here. I am ashamed to make the visit such an invasion. I think I did not like to ask one and not the other. He will return to Chicago by train probably on Sunday. If somebody would put him up (I think his main interest is general topology) it would be most welcome but he is prepared to stay in an hotel.

Please do not be unhappy about the proposed collaboration (28). It has been a great pleasure to discuss the subject with you, if we do not arrive to anything worth while a publication there is the possibility to leave the subject to some … (29)

Notes on the Doob-Jessen and Dieudonné-Jessen correspondence

Section 6

(1) Andersen [1944] and Doob [1938].

J.L. Doob (1910-2004), professor at the University of Illinois, Urbana-Champaign, from 1935, was one of the most fertile analysts of the 20th century and clearly the central figure of the modern theory of martingales, a name he borrowed from Ville. Doob comes into the present story rather late, in 1948, and then only on the margins. Jessen's theorem plays little part in the correspondence presented here (and Lévy's lemma one at all) for the letters mainly concern the theorems that Doob had believed he had proved in 1938. These were so natural, so necessarily true, that for ten years they were accepted, especially in Princeton, without anybody thinking of questioning them. Nevertheless the correspondence does have a place in the history of martingale theory since it allows us to date more precisely when Doob read Jessen's theorem in its new probabilistic version ([Sparre Andersen, Jessen, 1946]) and which he finally adopted in 1953. Jessen's first letter sounds the death-knell for the Daniell-Kolmogoroff-Doob theorem. It was a bitter pill for a mathematician of Doob's strength but it may have occasioned his return to his own theory of martingales [1940]. It is not easy to identifiy the moment when Doob truly recognised the importance of this class of random variables which he had considered in 1940 following Ville. It was definitely not in 1940 and must have been before 1953, which leaves some margin. Already by the time of his Lyon paper [1948] one senses that something had changed. The concept has a name and that is not a misleading sign. Doob's student, Laurie Snell, submitted his thesis on "martingale systems" in 1951. Was Doob's reading Jessen cause or consequence of the emergence of the theory? We do not know but it is incontestable that, from the spring of 1948, the concept begins to exist in its own right and that Doob was the first to discover it and in part to invent it. It is always difficult to determine the date and circumstances of the birth of a theory. For example, Laurent Schwartz tells us in his memoirs, [1997], p. 223, that the theory of the distributions was born "suddenly in only one night" in November 1944 on the ground floor of 11 rue Monticelli in the 14th arrondissement of Paris, where he was living at the time. But he admits that he is unable to understand what triggered this discovery, which would change dramatically his career as a mathematician, nor, moreover, who were "his precursors and its personal antecedents", which leaves the field free for historians to find him his place (Lützen [1982], Kantor [2004]). In the case of Doob and the theory of martingales, we can say that the invention occurred in the spring of 1948 suddenly some share at his place or in the surrounding countryside and that among his personal antecedents and his precursors were Jessen, Lévy, Ville, Doob and a few tens of others. Of course, for Doob, the Jessen- Lévy-Doob theorem was only a part of the theory of martingales, which draws some of its richness, and not the least part, to stopping properties. A suitably stopped martingale remains a martingale. These properties may have their origins in Doob's conversations with Feller and Chung about heads and tails and in the abundant work of Ville, Doeblin, Lévy and of all the inventors of probability theory over three centuries. That is in the nature of things. The universe is not obliged to be beautiful, however it is beautiful. (1)

(2) The reference is to [Andersen Jessen 1946] which was submitted on October 5, 1945 (before the postal servicebetween America and Denmark was restored) and printed on April 1, 1946. About the existence of measure in a countable product of probabilised sets, the paper writes (p. 22, n° 24, note 1): “An analogous theorem on arbitrary measures in product sets has been given by Doob, but his proof seems incomplete (it is not seen how the sets $\tilde{\Lambda}_n$ on p. 92 are chosen). The proof by Sparre Andersen of a more general theorem is incomplete … ”

It is possible that Doob objected to this critical note and that Jessen wanted to be apologise, but we have found no proof of this. We should recall that Doob was not sparing in writing notes of this kind (for example Doob [1938] notes pages 91 and 135) and that Doeblin (which was at least as tough as Doob) complained to him about it; cf. his correspondence with Doob in [Cohn, 1993].

However, the essence of the letter is concerned with the counterexample discovered in the spring of 1948 by Sparre Andersen and Jessen, which left in no doubt the irreparable inaccuracy of Doob’s theorem. (2)

(3) The passage omitted reproduces word for word the description of the counter-example as it appears in n° 4 of the paper [Andersen Jessen, 1948a]. The paper was submitted on June 28, 1948 and, therefore, after Doob’s reply. (3)

(4) The “first theorem” in question here and in the following letter is theorem 1.1, p. 90, of Doob [1938] which extends to a general abstract framework the Daniell-Kolmogorov theorem for the real case or for the abstract case for product probabilities (the case of independent variables). The “following theorem” (or the “second theorem” lower down) is theorem 3.1, page 96 of the same paper is a theorem of abstract disintegration (or existence of regular conditional probability) is equally erroneous. Jessen’s counterexample also works for this theorem which Doob already knows is false from an example by Dieudonné that Halmos had communicated to him (see below letter 4). However, until Jessen’s first letter arrived Doob thought that his Daniell theorem was correct. (4)

(5) Kakutani [1943, I], which has a very simple treatment of the independent case. S. Kakutani (1911-2004) was at the Princeton IAS between 1940 and 1942 and it was there that he learned the general theorem from Doob. (5)

(6) The statement suggested by Doob is a version of the very general result, without topological assumptions, which was published in 1949 by C. Ionescu Tulcea. Neveu [1964], V-1, has a presentation of this theorem which is adequate for the general theory of Markov chains, and of which Jessen said in the following letter that he had become convinced “in the course of my attempts to prove this theorem [the theorem of Daniell-Doob]”. (6)

(7) The search for the minimal topological assumptions ensuring the validity of Doob’s disintegration theorem produced an abundance of literature in the fifties and subsequently. See the very many references in Bogachev [2006] vol. II, p. 462. (7)

(8) From this last sentence it can be concluded that by May 17, 1948 Doob had read [Andersen Jessen 1946] and undoubtedly also Jessen’s articles from the thirties and had understood that they contained a satisfactory version of his first theory of [1940], what he still was not calling the theory of martingales. (8)

(9) There is a note in Sparre Andersen Jessen [1948a] n° 3 final paragraph. The project of a joint Doob-Jessen paper did not materialise (see letter 10 below). Obviously this consolation prize was not much motivation for Doob, and Jessen rather quickly realised that it was very interesting for him beyond expressing his sympathy for a colleague he had put into difficulty.

In any event, Doob preferred to work on his own at home and published very little with others. In his interesting conversation with Snell [1997] he says: "I corresponded with many mathematicians but never had detailed interplay with any but Kai Lai Chung and P.- A. Meyer in probability and Brelot in potential theory. My instincts were to work alone and even to collect enough books and reprints so that I could do all my work at home. " (9)

(10) W. Feller (1906-1970) fled Nazi Germany in 1933 for Denmark and Sweden, before emigrating in the United States in 1939. See C. Berg in this issue for Feller's links with Danish mathematicians. (10)

(11) Paul Halmos (1916-2006) was Doob's first doctoral student in Urbana and he was awarded his PhD in 1938. His article [1941] was based on the theorem of disintegration of Doob; it is referred to below in the Dieudonné correspondence. See the autobiography of Halmos [1978] and Burkholder [2005].
Warren Ambrose (1914-1996) completed his PhD under Doob's direction in 1939. He used Doob's theorem in [1939], [1940].
For Kakutani, see note 5 above. He used Doob's theorem in [1943, II].
The brilliant careers of these three mathematicians do not seem to have especially suffered from this unfortunate mistake. (11)

(12) Jessen's proof probably went back to 1934-1935. It was published in Danish in 1939 and English in 1946. (12)

(13) Von Neumann [1934b]. (13)

(14) Doob [1948]. The Lyon conference was held from June 28 to July 3. See [Locker 2009], in the present issue. (14)

(15) Doob [1940], Andersen, Jessen [1946]. It is clear that by this time Doob had made the connection between his theory and Jessen's. However, Jessen is not quoted in Doob's address at Lyon, where only Ville's name appears, [Doob, 1948], p. 23. (15)

(16) Jessen reconsiders this point in Andersen, Jessen [1948b], n° 1. Jessen states that in 1946 he was unaware of Doob's work [1940] and that he preferred to adopt the view-point of set functions, if only for the convenience of the exposition. However the article [1948b] is explicit that the results are also valid for point function, thus making the two theorems perfectly symmetrical so that they include the results of Doob 1940. This article was received by the journal on August 16, 1948 and published on October 23. It is discussed in letter 8 below. (16)

(17) Doob and Feller met for the first time at the meeting of the AMS of Darmouth in 1940 (Snell [1997]). He was, according to Doob "the first mathematical probabilist I

ever met." Doob and Feller had very different visions of mathematics. To be convinced, simply compare [Doob 1953] and [Feller 1950]. Doob did not like calculations and looked for the most general possible results and concepts. Feller loved only formulas and the rare and precious flowers that only appear as a result of complicated calculations and meticulous specialised investigations. That did not prevent them from getting together to try to convince American mathematicians that the theory of probability was a branch of mathematics like any other, contrary to general believef. See also Doob [1941], [1972], [1994]. (17)

(18) There is no shortage of literature on Jean Dieudonné (1906-1992), cofounder and principal writer of Bourbaki; see in particular Dugac [1995]. In 1948 Dieudonné was a professor at the Faculty of Science in Nancy, having spent the previous academic year, from May 1946 to December 1947, in Brazil, in Rio de Janeiro and São Paulo where he visited his friend and teacher André Weil who was professor there. Dieudonné would be with Weil in Chicago in the fifties before being appointed to the Institut des Hautes Études Scientifiques in 1959 and then in 1964 to the new University of Nice. (18)

(19) Sparre Andersen, Jessen [1948a]. (19)

(20) This is a reference to the Nancy Conference of June 1947 (see above Jessen's letter to Lévy of 13 September 1948). At the time Dieudonné was in Brazil. (20)

(21) Halmos [1941]. The counterexemple of Dieudonné [1948] p. 42 shows that the fundamental theorem of Halmos (theorem 1, p. 390) is not true in general. The argument is correct but it is based on an erroneous result of Doob [1938], theorem 3, p. 96, as Dieudonné points out (p. 42 notes 1). (21)

(22) Letters 9 and 10 are dated from Chicago. These are incomplete drafts and were doubtless altered at the time of sending. From these letters, it seems that the Jessen-Doob meeting took place in Urbana in the first fortnight of May 1949, without much scientific result and that the two men met again at the end of June 1949, after Jessen had given his course in Chicago. (22)

(23) Steve is Doob's oldest son. Burkholder [2005] describes Doob's life in Urbana. (23)

(24) Gene Autry (1907-1998), "the singing cowboy", was a very famous actor and American singer. According to Wikipedia, he is the author of an interesting "Cowboy code" whose first rule is: "must never shoot first, hit a smaller man, and take unfair advantage." (24)

(25) Turkey Run is a national park in Indiana.
Gerhard Hochschid was born in Berlin in 1915. An important algebraist, he was a student of Chevalley at Princeton. In 1949 he was professor at Urbana, before being appointed to Berkeley. The draft ends in a badly written, crossed out sentence: "Please give my kind regards to the Cairns, Landen?." Stewart S. Cairns (1904-1982), a student of Marston Morse at Harvard, was mathematics professor at Urbana from 1948 to 1972. (25)

(26) Alberto Calderón (1920-1998), a mathematician of Argentinian origin, was one of the most important analysts of the 20th century. Spotted by A. Zygmund at a conference in Buenos Aires in 1949, he followed Zygmund to the USA where he spent his career. In Chicago, where he was appointed in 1959, he and Zygmund established an important school of analysis. See Calderón [2001] for an outline of his work and his influence. (26)

(27) Leopoldo Nachbin (1922-1993), a brilliant Brazilian mathematician, was a student of Dieudonné and Weil when they taught in São Paulo. He was a professor in Rio de Janeiro and then in Rochester. In 1949 he was a Guggenheim foundation fellow. See Barroso [1986]. (27)

(28) Here there is a fragment of a crossed out phrase: "If we decide to drop the matter I…" (28)

(29) Thus ended the collaboration of Doob and Jessen. (29)

Bibliography

Many of the articles listed below are available on open access web sites. *Le Bulletin des sciences mathématiques*, *les Comptes rendus de l'Académie des sciences* and *le Journal de Liouville* are accessible through *Gallica*. *Le Bulletin de la SMF*, *les Annales de l'IHP*, etc. can be found on the site *Numdam*. The German periodicals are accessible on the sites of the libraries of the University of Göttingen and of the Berlin Academy. For the Polish journals go to the site, *Kolekcja Matematyczna*.

ACADEMIA NAUK BELARUS

[1992] *Repression during the Stalinist* Period, (in Russian), Minsk, 1992. http://www.ihst.ru/projects/sohist/repress/belan.pdf

ALDRICH (J.)

[2007] "But you have to remember P. J. Daniell of Sheffield", *Journal Electronique d'Histoire des Probabilités et de la Statistique,* 3, 2, (2007).

AMBROSE (W.)

[1939*] Some properties of measurable stochastic processes,* PhD, University of Illinois, 1939.

[1940] On measurable stochastic processes, *Trans. Amer. Math. Soc.,* 47 (1940), pp. 66-79.

SPARRE ANDERSEN (E.)

[1944] Indhold og Maal i Produktmaengder, *Mat. Tidsskrift,* B (1944), pp. 19-23.

[1949] On the number of positive sums of random variables, *Skand. Aktuarietidskrift,* 32 (1949), pp. 27-36.

[1953-1954] On the fluctuations of sums of random variables, *Math. Scand.,* 1 (1953), pp. 263-285, 2 (1954), pp. 195-223.

[1957] On the collective theory of risk in case of contagion between the claims, *Transactions XVth International Congress of Actuaries, New York,* 1957, II, pp. 219-229.

SPARRE ANDERSEN (E.), JESSEN (B.)

[1946] Some limits theorems on integrals in an abstract set, *D. Kgl. Danske Vidensk Selskab Mat. -fys. Medd.*, 22, 14 (1946). here

[1948a] On the introduction of measures in infinite product sets, *D. Kgl. Danske Vidensk Selskab Mat. -fys. Medd.,* 25, 4 (1948). here

[1948b] Some limit theorems of set-functions, *D. Kgl. Danske Vidensk Selskab Mat. -fys. Medd.,* 25, 5 (1948). here

ARTIN (E.)

[1931] *Einführung in die Theorie der Gammafunktion,* Leipzig, Teubner, 1931, trad. anglaise, *The Gamma Function,* New York, Holt, Rinehart, Winston, 1964.

BACHELIER (L.)

[1900] *Théorie de la spéculation*, thèse sci. math. Paris, *Ann. E.N.S.*, 17 (1900), pp. 21-86, Reprinted Paris, Jacques Gabay, 1995.

[1912] *Calcul des probabilités*, tome 1, Paris, Gauthier-Villars, 1912, Reprinted Paris, Jacques Gabay, 1992.

BAIRE (R.)
[*Œuvres*] *Œuvres scientifiques,* Paris, Gauthier-Villars, 1990.
[1898] Sur les fonctions discontinues développables en séries de fonctions continues, *C. R. Acad. Sci. Paris,* 126 (1898), pp. 884-887.
[1899] *Sur les fonctions de variables réelles,* thèse sci. math Paris, et *Ann. Mat. pura ed applicada,* (3) 3 (1899), pp. 1-123.

BANACH (S.)
[*Œuvres*] *Œuvres,* Warszawa, PWN - Editions scientifiques de Pologne, 1967-1979.
[1923] Sur le problème de la mesure, *Fund. Math.,* 4 (1923), pp. 7-33.
[1932] *Théorie des opérations linéaires,* Warszawa, Ed. sci. Pologne, 1932, reprint New York, Chelsea, 1955.

BANACH (S.), KURATOWSKI (C.)
[1929] Sur une généralisation du problème de la mesure, *Fund. Math.,* 14 (1929), pp. 127-131.

BARBUT (M.)
[2007] *La mesure des inégalités. Ambiguïtés et paradoxes,* Genève, Droz, 2007.

BARBUT (M.), LOCKER (B.), MAZLIAK (L.)
[2004] *Paul Lévy, Maurice Fréchet. 50 ans de correspondance mathématique, édition, notes et commentaires,* Paris, Hermann, 2004.

BARBUT (M.), MAZLIAK (L.)
[2008a] Commentary on Lévy's lecture notes to the Ecole Polytechnique, *Journal Electronique d'Histoire des Probabilités et de la Statistique,* 4/1, (2008).
[2008b] On a letter of Paul Lévy to Maurice Dumas, *Journal Electronique d'Histoire des Probabilités et de la Statistique,* 4/1, (2008).

BARROSO (J. A.) ed.
[1986] *Aspects of mathematics and its applications,* dedic. Leopoldo Nachbin, Amsterdam, Noth-Holland, 1986.

BENZI (M.)
[1988] Un probabilista neoclassico : Francesco Paolo Cantelli, *Historia Mathematica,* 15 (1) (1988), pp. 53-72.
[1995] Uncertainties and controversies : the debate about logic and probability in Italy in the early twentieth century (en italien), *Historia Mathematica,* 22 (1) (1995), pp. 43-63.

BERG (C.)
[1989] Introduction to the almost periodic functions of Bohr, *The Harald Bohr Centenary* (Copenhagen 1987), *D. Kgl. Danske Vidensk Selskab Mat. -fys. Medd.,* 42 (1989), pp. 125-142.
[1994] Børge Jessen, *Årbog 1994,* Københavns Universitet, 1994, pp. 809-813 (en danois), version anglaise, *Journal Electronique d'Histoire des Probabilités et de la Statistique*, this issue.

BERNSTEIN (F.)

[1912a] Über eine Anwendung der Mengenlehre auf ein aus der Theorie der säkularen Störungen herrührendes Problem, *Math. Ann.,* 71 (1911), pp. 417-439.

[1912b] Über geometrische Wahrscheinlichkeit und über das Axiom der beschränkten Arithmetisierbarkeit der Beobachtungen, *Math. Ann.,* 72 (1912), pp. 585-587.

BERNSTEIN (S.)

[*Works*] *Oeuvres complète* (in Russian), 4 vol., Moscou, Nauka, 1952-1964.

[1904] *Sur la nature analytique des solutions des équations aux dérivées partielles du second ordre,* thèse sci. math. Paris, Leipzig, Teubner, 1904 et *Math. Ann.,* 59 (1904), pp. 20-76.

[1917] Essai de fondement axiomatique de la théorie des probabilités, (en russe), *Comptes rendus de la Soc. Math. Kharkov,* 15 (1917), pp. 209-274, *Oeuvres* IV, pp. 10-60, French translation by M. Louanchi, to appear.

[1922] Sur le théorème limite du calcul des probabilités *Math. Ann.,* 85 (1922), pp. 237-241.

[1926a] *Leçons sur les propriétés extrémales et la meilleure approximation des fonctions analytiques d'une variable réelle,* professées à la Sorbonne, (*Collection de Monographies sur la Théorie des fonctions,* publiées sous la direction d'Émile Borel), Paris, Gauthier-Villars, 1926.

[1926b] Sur l'extension du théorème limite du calcul des probabilités aux sommes de quantités dépendantes, *Math. Ann.*, 97 (1926), pp. 1-59.

[1927a] État actuel de la théorie des probabilités et de ses applications, *Travaux du 1^er^ Congrès pan-russe des mathématiciens, Moscou 27 avril-4 mai 1927,* pp. 50-63.

[1927b] *Théorie des probabilités* (in Russian), Moscou Leningrad : Gostehizdat, 1927, 4ième éd., 1946.

[1927c] Sur les sommes de quantités dépendantes, *Izvestiya Akad. Nauk SSSR,* (6), 20 (1927), pp. 1459-1478, [*Oeuvres*] IV, pp. 177-196.

[1928] Sur les sommes de quantités dépendantes, *Doklad. Akad. Nauk SSSR,* (1928), pp. 55-60.

[1931] Revue de [Kolmogorov 1931], *Zentralblat f. Math.* 1 (1931), pp. 149.

[1932] Sur les liaisons entre les variables aléatoires, *Actes Congrès Int. Math. Zurich,* I (1932), pp. 288-309.

[1940] Nouvelles applications des grandeurs aléatoires presqu'indépendantes, *Izvestiya Akad. Nauk SSSR,* 4 (1940), pp. 137-150.

[1941] Sur les sommes de grandeurs aléatoires liées de classes (A,N) et (B,N), *Doklad. Akad. Nauk SSSR,* 32 (1941), pp. 303-307.

BERTRAND (J.)

[1888] *Calcul des probabilités,* Paris, Gauthier-Villars, 1888, deuxième éd. ibid. 1907, reprinted, New York, Chelsea, 1972, reprinted Paris, Jacques Gabay, 1997.

BESICOVITCH (A. S.)

[1932] *Almost periodic functions,* Cambridge, Cambridge University Press, 1932.

BINGHAM (N. H.)

[2000] Measure into probability from Lebesgue to Kolmogorov, *Biometrika,* 87 (2000), pp. 145-156.

[2005] Doob : a half-century on, *J. Appl. Probab.,* 42 (2005), pp. 257-266.

BIRKHOFF (G. D.)

Collected Mathematical Papers, 3 vols, New York, AMS, 1950.

[1931] Proof of the ergodic theorem, *Proc. Nat. Acad. Sci. U.S.A.,* 17 (1931), pp. 656-660.

[1942] What is the ergodic theorem ?, *Amer. Math. Monthly,* 49 (1942), pp. 222-226.

BLANC-LAPIERRE (A.), FORTET (R.)

[1946] Sur la structure des fonctions aléatoires strictement stationnaires à spectre totalement discontinu, *C. R. Acad. Sci. Paris,* 222 (1946), pp. 1155-1157.

[1953] *Théorie des fonctions aléatoires. Applications à divers phénomènes de fluctuation,* avec un chapitre sur la mécanique des fluides par J. Kampé de Fériet, (*Collection d'ouvrages de mathématiques à l'usage des physiciens,* publiée par G. Darmois et A. Lichnérowicz), Paris, Masson 1953.

BOCHNER (S.)

[1925] Sur les fonctions presque périodiques de Bohr, *C. R. Acad. Sci. Paris,* 180 (1925), pp. 1156-1158.

[1927] Beitrage zur Theorie der fastperiodischen Funktionen, *Math. Ann.,* 96 (1927), pp. 119-147, 383-409.

[1932] *Vorlesungen über Fouriersche Integrale,* Leipzig, Akad. Verlagsges., 1932.

BOCHNER (S.), von NEUMANN (J.)

[1935] Almost periodic functions in groups, *Trans. Amer. Math. Soc.,* 37 (1935), pp. 21-50.

BOGACHEV (V. I.)

[2006] *Measure Theory,* 2 vol., New York, Springer, 2006.

BOHL (P.)

[1893] *Ueber die Darstellung von Funktionen einer Variablen durch trigonometrische Reihen mit mehreren einer Variablen durch proportionalen Argumenten …,* Inaug.-Dis., Dorpat, Druck von C. Mattiesen, 1893.

[1909] Über ein in der Theorie der säkularen Störungen vorkommendes Problem, *J. reine angew. Math.,* 135 (1909), pp. 189-283.

BOHR (H.)

[*Œuvres*] *Collected mathematical works,* København, Dansk Mat. Forening, 1952.

[1914] Sur la fonction zêta de Riemann, *Bull. Soc. Math. France, Comptes rendus des séances,* (1914), pp. 52-66.

[1923a] Sur les fonctions presque périodiques, *C. R. Acad. Sci. Paris,* 177 (1923), pp. 737-739.

[1923b] Sur l'approximation des fonctions presque périodiques par des sommes trigonométriques, *C. R. Acad. Sci. Paris,* 177 (1923), pp. 1090-1092.

[1925] Sur les fonctions périodiques d'une variable complexe, *C. R. Acad. Sci. Paris,* 180 (1925), pp. 645-647.

[1924-1926] Zur Theorie der fastperiodischen Funktionen, I, *Acta Math.,* 45 (1924), pp. 29-127, II, *ibid.,* 46 (1925), pp. 101-214, III, *ibid.,* 47 (1926), pp. 237-281.

[1932a] *Fastperiodische Funktionen,* Berlin, Springer, 1932.

[1932b] Fastperiodische Funktionen einer komplexen Veränderlichen, *Vortrag gehalten auf dem Internationalen MathematikerKongress in Zürich,* September 1932.

BOHR (H.), JESSEN (B.)

[1930-1932] Über die Werteverteilung des Riemannschen Zetafunktion I, II, *Acta Math.,* 54 (1930), pp. 1-35, 58 (1932), pp. 1-55.

[1947] Mean motions and almost periodic functions, *Colloques internationaux du CNRS, 15, Analyse harmonique, Nancy 15-22 juin 1947,* Paris, Gauthier-Villars, 1949, pp. 75-84.

BOHR (H.), LANDAU (E.)

[1914] Sur les zéros de la fonction zêta de Riemann, *C. R. Acad. Sci. Paris,* 158 (1914), pp. 106-110.

BOHR (H.), MOLLERUP (J.)

[1920-1923] *Laerebog i matematisk Analyse,* 4 vol., København, Gjellerup, 1920-1923.

BOREL (É.)

[*Oeuvres*] *Oeuvres d'Émile Borel,* 4 vol., Paris, CNRS, 1972.

[1894] *Sur quelques points de la théorie des fonctions*, thèse sci. math., Paris, Gauthier-Villars, 1894, *Ann. Sci. Ec. Norm. Sup.,* 12 (1895), pp. 9-55. *Œuvres* 1, pp. 239-285.

[1896] Sur les séries de Taylor, *C. R. Acad. Sci. Paris,* 123 (1896), pp. 1051-1052.

[1897] Sur les séries de Taylor, *Acta Math.,* 21 (1897), pp. 243-247.

[1898] *Leçons sur la Théorie des fonctions,* Paris, Gauthier-Villars, 1898, deuxième édition, (Collection de Monographies sur la Théorie des fonctions , publiées sous la direction de M. Émile Borel), ibid., 1914, troisième édition, ibid., 1928, réimpression Paris, Jacques Gabay, 1997.

[1905] Remarques sur certaines questions de probabilités, *Bull. de la Soc. Math. Fr.,* 33, (1905), pp. 123-128. *Oeuvres* 4, pp. 985-990.

[1909] Les probabilités dénombrables et leurs applications arithmétiques, *Rend. Circ. Mat. Palermo*, 27, (1909), pp. 247-271. *Oeuvres* 2, pp. 1055-1079.

[1912a] Sur un problème de probabilités relatives aux fractions continues, *Math. Ann.,* 72, 4, (1912), pp. 578-584. Reproduit dans [Borel 1898/1914], note V, § 4. *Oeuvres* 2, pp. 1085-1091.

[1912b] Le calcul des intégrales définies, *J. Math. Pures Appl.,* (6) 8 (1912), pp. 159-210, *Oeuvres* 2, pp. 827-878.

[1914b] *Introduction géométrique à quelques théories physiques,* Paris, Gauthier-Villars, 1914.

[1939] *Valeur pratique et philosophie des probabilités,* (fascicule 3, tome 4 du *Traité du Calcul des probabilités et de ses applications*, par Émile Borel), Paris, Gauthier-Villars, 1939, reprint Paris, Jacques Gabay, 1991.

BORODIN (A. I.), BUGAI (A. S.)

[1987] *Dictionnaire biographique des mathématiciens,* (en russe), Kiev, 1987.

BOURBAKI (N.)

[1969] *Intégration* chapitre IX, Paris, Hermann, 1969.

[1984] *Eléments d'histoire des mathématiques,* Paris, Hermann, 1984.

BREIMAN (L.)
[1968] *Probability,* Reading, Addison-Wesley, 1968.

BRETAGNOLLE (J.)
[1987] Notes de lecture sur l'œuvre de Paul Lévy, *Annales de l'I.H.P., section B,* 23, S2 (1987), pp. 239-243.

BRISSAUD (M.)
[2002] *Ecrits sur les processus aléatoires, mélanges en hommage à Robert Fortet,* Paris, Hermes, Lavoisier, 2002.

BROCKMEYER (E.), HALSTRØM (H. L.), JENSEN (A.)
[1948] *The Life and Works of A. K. Erlang,* Copenhagen, The Copenhagen Telephone Company, 1948.

BROWDER (F. E.)
[1975] The relation of functional analysis to concrete analysis in 20th century mathematics, *Historia Math.,* 2 (1975), pp. 577-590.

BURKHOLDER (D.), PROTTER (P.) et al.
[2005] Joseph Leo Doob, 1910-2004, *Stochastic Processes and their Applications,* 115 (2005), pp. 1061-1072.

BURSTIN (C.)
[1916] Über eine spezielle Klasse reeler periodischer Funktionen, *Monat. f. Math. und Phys.,* 27, 1 (1916).

CALDERON (A.) et al.
[2001] *Harmonic Analysis and Partial Differential Equations : Essays in Honor of Alberto PP. Calderón,* University of Chicago Press, 2001.

CANTELLI (P.)
[*Oeuvres*] *Alcune Memorie Matematiche : Onoranze a Francesco Paolo Cantelli,* Milano, A. Giuffrè, 1958.
[1916] La tendenza ad un limite nel senso del Calcolo delle probabilità, *Rend. Circ. Mat. Palermo,* 41 (1916), pp. 191-201.
[1917] Sulle probabilità comme limite della frequenza, *Rend. Acc. Lincei,* 26 (1917), pp. 39-45.
[1932] Sui confini della probabilità, *Actes Congrès Int. Math. Bologne, 3-10 septembre 1928* , 6 (1932), pp. 47-59.
[1935] Considérations sur la convergence dans le Calcul des probabilités, *Ann. Inst. H. Poincaré,* 5 (1935), pp. 3-50.
[1938] Sur la définition des variables éventuelles, *Colloque consacré à la théorie des probabilités et présidé par M. Maurice Fréchet, Genève 11 au 16 octobre 1937*, deuxième partie, *Les Fondements du calcul des probabilités* , Paris, Hermann (Actualités Sci. Ind. 735), 1938, pp. 5.

CANTOR (G.)
[*Works*] *Gesammelte Abhandlungen,* Berlin, Springer, 1932.

CARATHEODORY (C.)
[*Works*] *Gesammelte matematische Schriften,* München, Bayer. Akad. Wiss., 1954-1957.
[1918] *Vorlesungen über reelle Funktionen,* Leipzig, Teubner, 1918, 2d ed., ibid., 1927, reprint New York, Chelsea, 1948, 3d ed., 1968, Amer. Math. Soc., 2004.

CARLSON (F.)
[1933] Contributions à la théorie des séries de Dirichlet, *Arkiv f. Mat.,* 23A , 19 (1933), pp. 1-8.

CASTELNUOVO (G.)
[*Œuvres*] *Opere matematice,* Roma, Academia nazionale dei Lincei, 2002-2007.
[1919] *Calcolo della probabilità* , Milano, Dante Alighieri, 1919, seconda edizione riveduta, 2 vols, Bologna, Zanichelli, 1925-1928.
[1937] *La probabilité dans les différentes branches de la science,* Paris, Hermann, (Actualités Scientifiques et Industrielles 463), 1937.

CHATTERJI (S. D.)
[1973] *Les martingales et leurs applications analytiques,* Lecture Notes in Math. 307, Berlin, Springer, 1973.

CHAUMONT (L.), MAZKIAK (L.) , YOR (M.)
[2004] Quelques aspects de l'œuvre probabiliste, in E. Charpentier, A. Lesne, N. Nikolski, *L'héritage de Kolmogorov en mathématiques,* Paris, Belin, 2004, pp. 55-79.

CHENG (F.)
[2008] *Cinq méditations sur la beauté,* nouvelle éd., Paris, Albin Michel, 2008.

CHOQUET (G.)
[1974] *Notice sur les travaux scientifiques,* http://www.academie-sciences.fr/membres/discours_pdf/notice_Choquet.pdf
[2004] Préface de *Henri Lebesgue. Lettres à Emile Borel,* Paris, Vuibert, 2004, pp. 5-14.

CHOW (Y. S.)
[1960] Martingales in a σ-finite measure space, indexed by directed sets, *Trans. Amer. Math. Soc.,* 97 (1960), pp. 254-285.
[1962] Convergence theorems of martingales, *Z. Warscheinlichkeitstheorie,* 1 (1962-63), pp. 340-346.

CHUNG (K. L.)
[*Œuvres*] *Selected Works,* River Edge, World Scientific Press, 2008.
[1954] Review : *Stochastic processes* by J. L. Doob, *Bull. Amer. Math. Soc.,* 60 (1954), pp. 190-201.
[1967] *Markov Chains with stationnary transition probabilities,* Berlin, Springer, 1967.

[1995] *Green, Brown and Probability*, Singapore, World Scientific, 1995.
[1998] Probability and Doob, *Amer. Math. Monthly,* 105 (1998), pp. 28-35.
[2000] *A Course in Probability Theory,* revised ed, New York, Academic Press, 2000.

CHUNG (K. L.), HSU (PP. L.)
[1946] Sur un théorème de probabilités dénombrables, *C. R. Acad. Sci. Paris,* 233 (1946), pp. 467-469.

COHN (H.) ed.
[1993] Doeblin and modern probability, *Contemporary Mathematics,* 149 (1993).

COLLOQUE D'ANALYSE HARMONIQUE
[1949] *Colloque d'Analyse harmonique [Nancy 1947],* Paris, CNRS (Coll. intern. du CNRS n° 15), 1949.

CONGRÈS de BOLOGNE
[1928] *Argomenti delle Comunicazioni,* Bologna : Zanichelli, 1928.
[1929] *Atti del Congresso Internazionale dei Matematici, Bologna,* tomo I, Bologne : Zanichelli, 1929.
[1932] *Atti del Congresso Internazionale dei Matematici, Bologna,* tomo VI, Bologne : Zanichelli, 1932.

COURNOT (A.)
[*Oeuvres*] *Œuvres complètes,* Paris, J. Vrin, 1973-2009.

COURTAULT (J. M.), KABANOV (Y.) (dir.)
[2002] *Louis Bachelier. Aux origines de la finance mathématique,* Besançon, Presses Universitaires Franc-Comtoises, 2002.

CRAMÉR (Harald)
[*Works*] *Collected Works,* 2 vol., Berlin, Springer-Verlag, 1994.
[1934] Sul un teorema relativo alla legge uniforme dei grande numeri, *Giorn. Ist. Ital. Attuari,* 5 (1934), pp. 1-13.
[1936a] Sur une propriété de la loi de Gauss, *C. R. Acad. Sci. Paris,* 202 (1936), pp. 615-616.
[1936b] Ueber eine Eigenschaft der normalen Verteilungsfunktion, *Math. Zeitschrift,* 41 (1936), pp. 405-414.
[1937] *Random variables and probability distributions,* Cambridge, Cambridge Tracts in Mathematics 36, 1937
[1976] Half a century with probability theory : some personal recollections, *Ann. Prob.,* 4 (1976), pp. 509-546.

CRÉPEL (Pierre)
[1984a] Histoire de la théorie des martingales, *Séminaire de probabilités,* Rennes I, 1984.
[1984b] *Conversation avec Jean Ville*, Langon, 1984 .
[1980-2008] *Recherches érudites et mathématiques sur Bachelier, Gateaux et quelques autres,* chez l'auteur et divers savants.

DANIELL (PP. J.)

[1918] A general form of integral, *Ann. of Math.,* (2) 19 (1918), pp. 279-294.

[1918-1919] Integrals in an infinite number of dimensions, *Ann. of Math.,* 20 (1919), pp. 281-288.

[1919-1920] Functions of limited variation in an infinite number of dimensions, *Ann. of Math.,* 21 (1919-1920), pp. 30-38.

[1920] Stieltjes derivatives, *Bull. Amer. Math. Soc.* (1920), pp. 444-448.

[1921] The Integral and its Generalizations, *The Rice Institute Pamphlet,* 8 (1921), pp. 34-62.

DECAILLOT (A.-M.)

[2008] *Cantor et la France,* Paris, Kimé, 2008.

DENJOY (A.)

[*Œuvres*] *Articles et mémoires,* Paris, Gauthier-Villars, 1955. *Un demi siècle (1907-1956) de notes communiquées aux Académies,* Paris, Gauthier-Villars, 1957.

[1933] Sur les variables pondérées multipliables de M. Cantelli, *C. R. Acad. Sci. Paris,* 196 (1933), pp. 1712-1714.

[1980] Lettres à Paul Lévy, éditées par PP. Dugac, *Cahiers du Séminaire d'Histoire des Mathématiques,* 1 (1980), pp. 51-67.

DIEUDONNÉ (J.)

[*Oeuvres*] *Choix d'oeuvres mathématiques,* 2 vol., Paris, Hermann, 1981-1984.

[1948] Sur le théorème de Lebesgue-Nikodym (III), *Ann. Univ. Grenoble,* 23 (1948), pp. 25-53.

[1950] Sur un théorème de Jessen, *Fund. Math.,* 37 (1950), pp. 242-248.

[1981] *History of Functional Analysis,* Amsterdam, North Holland, 1981.

DOEBLIN (W.)

[1936a] Sur les chaînes discrètes de Markoff, *C. R. Acad. Sci. Paris*, 202 (1936) pp. 24-26.

[1936b] Sur les chaînes de Markoff, *C. R. Acad. Sci. Paris*, 202 (1936) pp. 1210-1211.

[1937a] Le cas discontinu des probabilités en chaîne, *Publ. Fac.Sci. Univ. Masaryk,* 236, 1937.

[1938a] Exposé de la théorie des chaînes simples constantes de Markoff à un nombre fini d'états, *Rev. Math. de l'Union Interbalkanique,* 2 (1938), pp. 77-105.

[1938b] *Sur les propriétés asymptotiques de mouvements régis par certains types de chaînes simples*, thèse sci. math. Paris, Bucarest, Imprimerie Centrale, 1938, et *Bull. Soc. Math. Roumaine Sci.,* 39 (1937), (1) pp. 57-115, (2) pp. 3-61.

[1940a] Éléments d'une théorie générale des chaînes simples constantes de Markoff, *Ann. Sci. École Norm. sup.,* 63 (1940), pp. 317-350.

[1940b] Sur l'équation de Kolmogoroff, *pli cacheté,* accepté par l'Académie des Sciences le 26 février 1940, ouvert le 18 mai 2000, publié dans *C. R. Acad. Sci. Paris,* 331 (2000).

DOEBLIN (W.), LEVY (P.)

[1936] Sur les sommes de variables aléatoires indépendantes à dispersions bornées inférieurement, *C. R. Acad. Sci. Paris*, 202 (1936), pp. 2027-2029.

DOOB (J. L.)

[1936] Note on probability, *Ann. of Math.,* 37 (1936), pp. 363-367.
[1937] Stochastic processes depending on a continuous parameter, *Trans. Amer. Math. Soc.,* 42 (1937), pp. 107-140.
[1938] Stochastic processes with an integral-valued parameter, *Trans. Amer. Math. Soc.,* 44 (1938), pp. 87-150.
[1940] Regularity properties of certain families of chance variables, *Trans. Amer. Math. Soc.,* 47 (1940), pp. 455-486.
[1941] Probability as Measure, *Ann. Math. Stat.,* 12 (1941), pp. 206-214.
[1948] Application of the theory of martingales, *Actes du Colloque International Le Calcul des Probabilités et ses applications (Lyon, 28 juin au 3 juillet 1948),* Paris, CNRS, 1949, pp. 23-27.
[1953] *Stochastic Processes,* New-York, Wiley, 1953, 1990.
[1961] Notes on martingale theory, *Proc. Fourth Berkeley Symposium on Math. Stat. and Prob.,* 2, pp. 95-102 , Berkeley, Univ. California Press, 1961.
[1972] William Feller and twentieth century probability, *Proc. Sixth Berkeley Symposium on Math. Stat. and Prob.,* 2, pp. xv-xx , Berkeley, Univ. California Press, 1972.
[1984] *Classical Potential Theory and its Probabilistic Counterpart,* Basel, Birkhäuser, 1984.
[1994] The Development of Rigor in Mathematical Probability, (1900-1950), *Development of Mathematics 1900-1950,* ed. J.PP. Pier, Basel, Birkhäuser, 1994, pp. 157-169.

DUBINS (L.), SCHWARZ (G.)
[1965] On continuous martingales, *Proc. Nat. Acad. Sci. USA,* 53 (1965), pp. 913-916.

DUDLEY [R. M.]
[2002] *Real Analysis and Probability,* Cambridge University Press, 2002.

DUGAC (P.)
[1995] *Jean Dieudonné mathématicien complet,* Paris, Jacques Gabay, 1995.
[2003] *Histoire de l'Analyse,* Paris, Vuibert, 2003.

DUNFORD (N.), SCHWARTZ (J. T.)
[1958] *Linear operators. Part I : General Theory,* New York, Interscience publishers, 1958.

DUNFORD (N.), TAMARKIN (J. D.)
[1941] A principle of Jessen and general Fubini theorems, *Duke Math. J.,* 8 (1941), pp. 743-749.

DUPORTET (M.)
[1947] Publications de M. Fréchet de 1902 à 1947 classées par ordre chronologique, *Répertoire permanent des intellectuels,* 12 (1947). (Cote BNF Per Z 512).

EDGAR (G. A.), SUCHESTON (L.)
[1976] Amarts : A class of asymptotics martingales, I, II, *J. Multivariate Anal.* 6 (1976), pp. 196-221, 572-591.
[1992] *Stopping times and directed processes,* Cambridge, Cambridge University Press, 1992.

EID (S.)

[2008] *Martingales et géométrie borélienne des probabilités,* thèse univ. Paris Diderot, 2008.

EINSTEIN (A.)

[*Œuvres*] *Œuvres choisies,* 6 vol, Paris, Seuil-CNRS, 1999.

[1905] Über die von der molekularkinetischen Theorie der Wärme gefordete Bewegung von in ruhenden Flüssigkeiten suspendierten Teilchen, *Annalen der Physik,* 17 (1905), pp. 549-560.

ERDÖS (P.)

[1940] On the smoothness properties of a family of Bernoulli convolutions, *Amer. J. Math.,* 62 (1940), pp. 180-186.

ESCLANGON (E.)

[1904] *Les fonctions quasi-périodiques,* thèse sci math., Paris, Gauthier-Villars, 1904.

FAVARD (J.)

[1933] *Leçons sur les fonctions presque-périodiques,* Paris, Gauthier-Villars, 1933.

FABER (G.)

[1910] Über stetige Funktionen II, *Math. Ann.,* 69 (1910), pp. 372-443.

FABRY (E.)

[1896] Sur les points singuliers d'une fonction donnée par son développement en série et l'impossibilité du prolongement analytique dans des cas très généraux, *Ann. Sci. ENS,* (3) 13 (1896), pp. 367-399.

FATOU (P.)

[1906] Séries trigonométriques et séries de Taylor, *Acta Math.,* 30 (1906), pp. 335-400.

FELLER (W.)

[1935] Ueber den zentralen Grenzwersatz der Warscheinlichkeitsrechnung, *Math. Zeitschrift,* 40 (1935), pp. 521-559.

[1938] Sur les axiomatiques du calcul des probabilités et leurs relations avec les expériences, *Colloque consacré à la théorie des probabilités et présidé par M. Maurice Fréchet, Genève 11 au 16 octobre 1937*, deuxième partie, *Les Fondements du calcul des probabilités,* Paris, Hermann, (Actualités Sci. Ind. 735), 1938, pp. 7-21.

[1950] *An Introduction to Probability Theory and its Applications,* Vol 1, New York, Wiley, 1950. Third ed., ibid., 1968.

[1966]] *An Introduction to Probability Theory and its Applications,* Vol 2, New York, Wiley, 1966. 2d ed., ibid., 1971.

FELLER (W.), TORNIER (E.)

[1932] Mass- und Inhaltstheorie des Baireschen Nullraumes, *Math. Ann.,* 107 (1932), pp. 165-187.

FILEP (L.), ELKJÆR (S.)

[2000] Pál Gyula – Julius Pal (1881-1946), the Hungarian-Danish Mathematician, *Acta Math. Acad. Paedagogicae Nyíregyháziensis,* 16 (2000), pp. 89-94. http://www.emis.de/journals/AMAPN/vol16/amapn16_9.pdf

FINETTI (B. de)

[*Œuvres*] *Scritti (1926-1930),* Padova, CEDAM, 1981.

[1929a] Sulle funzioni a incremento aleatorio, *Rend. Accad. Naz. Lincei,* X 6 3-4 (1929), pp. 163-168.

[1929b] Sulla possibiltà di valore eccezionali per una legge di incrementi aleatori, *Rend. Accad. Naz. Lincei,* X 6 7-8 (1929), pp. 325-329.

[1929c] Integrazione delle funzioni a incremento aleatorio, *Rend. Accad. Naz. Lincei,* X 6 11 (1929), pp. 548-553.

[1930a] Funzione caratteristica di un fenomeno aleatorio, *Mem. Accad. Naz. Lincei,* IV, 5 (1930), pp. 86-133.

[1930b] La funzioni caratteristiche di legge istantanea, *Mem. Accad. Naz. Lincei,* XII
6 37-8 (1930), pp. 278-282.

[1936] Les probabilités nulles, *Bull. Sci. Math.,* (2) 60 (1936), pp. 275-288.

[1937] La prévision : ses lois logiques, ses sources subjectives, *Ann. Inst. H. Poincaré,* 7 (1937), pp. 1-68.

FISCHER (E.)

[1907] Sur la convergence en moyenne, *C. R. Acad. Sci. Paris,* 144 (1907), pp. 1022-1024.

FRÉCHET (M.)

[1906] Sur quelques points du calcul fonctionnel, thèse sci. math. Paris, *Rend. Circ. Mat. Palermo,* 22 (1906) pp. 1-74.

[1907] Sur les ensembles de fonctions et les opérations linéaires, *C. R. Acad. Sci. Paris,* 144 (1907), pp. 1414-1416.

[1909] Les fonctions d'une infinité de variables, *Comptes rendus du Congrès des Sociétés savantes en 1909,* Paris, Imprimerie nationale, 1910, pp. 44-47.

[1915] Sur l'intégrale d'une fonctionnelle étendue à un espace abstrait, *Bull. Soc.*
Math. *France,* 43 (1915), pp. 248-265.

[1921a] Remarque sur les probabilités continues, *Bull. Sci. Math.,* 45 (1921), pp. 87-88.

[1921b] Sur divers modes de convergence d'une suite de fonctions d'une variable, *Bull. Calcutta Math. Soc.,* 11 (1921), pp. 187-206.

[1923/1924] Des familles et des fonctions additives d'ensembles abstraits, *Fund. Math.,* 4 (1923), pp. 329-365, 5 (1924), pp. 206-251.

[1930] Sur la convergence en probabilité, *Metron,* 8 (1930), pp. 1-50.

[1933] *Notice sur les travaux scientifiques de M. Maurice Fréchet,* Paris, Hermann, 1933, *Supplément pour la période 1934-1951*, Paris, Sennac, 1951.

[1935] Généralisations du théorème des probabilités totales, *Fund. Math.,* 25 (1935), pp. 379-387.

[1936] *Recherches théoriques modernes sur le calcul des probabilités.* Premier livre. *Généralités sur les Probabilités. variables aléatoires,* avec une Note de M. Paul Lévy, (fascicule 3, tome I du *Traité du calcul des probabilités et de ses applications*, par Émile Borel), Paris, Gauthier-Villars, 1936, deuxième édition revue et augmentée, ibid., 1950.

[1938] Exposé et discussion de quelques recherches récentes sur les fondements du calcul des probabilités, *Colloque consacré à la théorie des probabilités et présidé par M. Maurice Fréchet, Genève 11 au 16 octobre 1937*, première partie, *Conférences d'Introduction,* Paris, Hermann (Actualités Sci. Ind. 734), 1938, pp. 19-23, deuxième partie, *Les Fondements du calcul des probabilités* , Paris, Hermann (Actualités Sci. Ind. 735), 1938, pp. 23-55.

[1949] Avant-propos, *Actes du Colloque International de Calcul des Probabilités (Lyon 1948),* Paris, CNRS, 1949, pp. 5.

[1963] Notice nécrologique sur Jacques Hadamard, prononcée devant l'Académie des sciences en la séance du 23 décembre 1963, *Institut,* 34 (1963).

[1965] *La vie et l'oeuvre d'Émile Borel,* Genève, Monographie de *L'Enseignement mathématique,* 1965.

FUBINI (G.)

[*Œuvres*] *Opere scelte,* Roma, Ed. Cremonese, 1957-1962.

[1907] Sugli integrali multipli, *Rend. R. Accad. Nazionale dei Lincei*, (5), 16 (1907), pp. 608-614, *Œuvres* 2, pp. 243-249.

GÅRDING (Lars)

[1998] *Mathematics in Sweden,* History of Mathematics vol. 13, Amer. Math. Soc., London Math. Soc., 1998.

GATEAUX (R.)

[1919a] Sur la notion d'intégrale dans le domaine fonctionnel et sur la théorie du potentiel, publié par PP. Lévy d'après un manuscrit de juin 1914, *Bull. Soc.Math. France,* 47 (1919), pp. 47-70.

[1919b] Fonctions d'une infinité de variables indépendantes, publié par PP. Lévy d'après deux manuscrits datés de mars 1914, *Bull. Soc. Math. France,* 47 (1919), pp. 70-96.

GRAHAM (L.), KANTOR (J.-M.)

[2009] *Naming Infinity : A True Story of Religious Mysticism and Mathematical Creativity,* Cambridge, Belknap Press, 2009.

GROMOV (M.), ROKHLIN (V.)

[1970] Embeddings and immersions in Riemannian geometry, *Russian Math. Surveys,* 25, 5 (1970), pp. 1-57.

GUILBAUD (G. Th.)

[2008] *Lumen in Manibus : Essai sur la connaissance intermittente,* inédit, St Germain-en-Laye, 2008.

HADAMARD (J.)

[*Oeuvres*] *Oeuvres de J. Hadamard,* 4 vol., Paris, CNRS, 1968.

[1903] Sur les opérations fonctionnelles, *C. R. Acad. Sci. Paris,* 136 (1903), pp. 351-353, *Oeuvres,* I, pp. 405-408.

[1922] Les axiomes du calcul des probabilités, *Revue de Métaphysique et de Morale,* 39 (1922), *Oeuvres,* IV, pp. 2161-2165.

[1929] Le développement et le rôle scientifique du calcul fonctionnel, *Comptes rendus du Congrès international des mathématiciens de Bologne, 3-10 septembre 1928,* 1 (1929), pp. 143-161.

HAHN (H.)

[*Works*] *Gesammelte Abhandlungen,* 2 vol., Vienna, Spriger-Verlag, 1995-1996.

[1932] *Reelle Funktionen : Punktfunktionen,* Leipzig, Akad. Verlagsgesellschaft, 1932.

[1933] Über die Multiplikation total-additiver Mengenfunktionen, *Ann. Scuola Norm. Super. Pisa* (2) 2 (1933), pp. 429-452.

HALD (A.)

[1990] *A History of Probability and Statistics and Their Applications before 1750,* New York, Wiley, 1990.

[1998] *A History of Mathematical Stratistics from 1750 to 1930,* New York, Wiley, 1998.

HALMOS (PP. R.)

[1941] The decomposition of measures, *Duke Math. Journal,* 8 (1941), pp. 386-392.

[1950] *Measure Theory,* New York, Van Nostrand, 1950.

[1978] *I want to be a mathematician, an automathography,* New York, Springer, 1978.

HALMOS (PP. R.), von NEUMANN (J.)

[1942] Operators methods in classical mechanics, II, *Ann. Math.* 43 (1942), pp. 332-350.

HARDY (G. H.), LITTLEWOOD (J. E.)

[*Works*] *Collected papers of G. H. Hardy, including papers with J. E. Littlewwod and others,* Oxford, Clarendon Press, 1966-1979.

[1914] Some problems of Diophantine approximation, *Acta math.,* 37 (1914), pp. 155-239.

[1930] A maximal theorem with function-theoretic applications, *Acta math.*, 54 (1930), pp. 81-116.

HAUSDORFF (F.)

[*Works*] *Gesammelte Werke,* 9 vol., New York, Springer, 2001-2009.

[1914] *Grundzüge der Mengenlehre,* Leipzig, Veit, 1914, 2d ed., ibid., 1927, reprint, New York, Chelsea, 1965. [*Oeuvres*], vol. 2, 2002.

HAVILAND (E. K.)

[1933] On statistical methods in the theory of almost periodic functions. *Proc. Nat. Acad. Sci,* 19 (1933), pp. 549-555.

[1934] On distribution functions and their Laplace-Fourier transforms. *Proc. Nat. Acad. Sci,* 20 (1934), pp. 50-57.

HAWKINS (T.)

[1970] *Lebesgue's Theory of Integration : Its Origins and Development,* Madison, University of Winconsin Press, 1970, reprint AMS Bookstore, 2001.

HENNEQUIN (PP. L.), TORTRAT (A.)

[1965] *Théorie des probabilités et quelques applications,* Paris, Masson, 1965.

HEWITT (E.), SAVAGE (L. J.)

[1955] Symmetric measures on Cartesian products, *Trans. Amer. Math. Soc.,* 80 (1955), pp. 470-501.

HEWITT (E.), STROMBERG (K.)

[1965] *Real and Abstract Analysis,* New York, Springer-Verlag, 1965, 1969.

HEYDE (C. C.), SENETA (E.), CRÉPEL (P.), FIENBERG (S. E.), GANI (J.)

[2001] *Statisticians of the Centuries,* New York, Springer, 2001.

HILBERT (D.)

[*Oeuvres*] *Gesammelte Abhandlungen,* 3 vol., Berlin, Springer, 1932-1935, reprint, New York, Chelsea, 1965.

[1891] Ueber die stetige Abbildung einer Linie auf ein Flächenstück, *Math. Annalen,* 38 (1891), pp. 459-460.

[1909] Wesen und Ziele einer Analysis der unendlichen unabhängigen Variablen, *Rend. Circ. Mat. Palermo,* 27, (1909), pp. 59-74.

[1912] *Grundzüge einer allgemeinen Theorie des linearen Integralgleichungen,* Leipzig, Teubner, 1912.

HOBSON (E. W.)

[1906] On Absolutely Convergent Improper Double Integrals, *Proc. London Math. Soc.,* (2) 4 (1906), pp. 136-159.

[1907] On Repeated Integrals, *Proc. London Math. Soc.,* (2) 5 (1907), pp. 325-334

[1910] On Some Fundamental Properties of Lebesgue Integrals in a Two-Dimensional Domain, *Proc. London Math. Soc.,* (2) 8 (1910), pp. 22-39.

[1926-1927] *The theory of functions of a real variable and the theory of Fourier's series,* 2 vol, third ed., Cambridge University Press, 1926-1927, reprint, New York, Dover, 1957.

HOPF (E.)

[1937a] On causality, statistics and probability, *Journal of Math. Phys.,* 13 (1937), pp. 51-102.

[1937b] *Ergodentheorie,* Berlin, Springer-Verlag, 1937.

HUNT (G. A.)

[1951] Random Fourier Transforms, *Trans. Amer. Math. Soc.* 71 (1951), pp. 38-69.

[1963] *Les martingales,* Orsay, Cours polycopié, 1963, reproduced in *Martingales et processus de Markov,* Paris, Dunod, 1966.

IONESCU TULCEA (C.)

[1949] Mesures dans les espaces produit, *Atti Acad. Naz. Lincei Rend. Cl Sci. Fis. Mat. Nat.,* (8) 7 (1949), pp. 208-211.

ITÔ (K.)

[*Œuvres*] *Selected papers,* New York, Springer-Verlag, 1987.

[1998] *My Sixty Years in Studies of Probability Theory,* acceptance speech of the Kyoto Prize in Basic Sciences, 1998.

JENSEN (J. L. W. V.)

[1899] Sur un nouvel et important théorème de la théorie des fonctions, *Acta Math.,* 22 (1899), pp. 359-364.

[1906] Sur les fonctions convexes et les inégalités entre les valeurs moyennes, *Acta Math.,* 30 (1906), pp. 175-193.

JESSEN (B.)

[1929a] *Hovedsætningerne indenfor de næstenperiodiske Funktioners Teori og deres indbyrdes Sammenhæng,* Magisterkonferens i Matematik, København, 23/4 - 21/5 1929.

[1929b] Über eine Lebesguesche Integrationstheorie für Funktionen unendlich vieler Veränderlichen, *Comptes rendus du septième Congrès des Mathématiciens scandinaves tenu à Oslo,* 19-22 août 1929, Oslo, A. W. Broggers Boktrykkeri, 1930, pp. 127-138.

[1930] *Bidrag til Integralteorien for Funktioner af uendelig mange Variable,* Afhandling for den filosofiske Doktorgrad, København, G. E. C. Gads Forlag, 1930.

[1932a] Eine Integrationstheorie für Funktionen unendlich vieler Veränderlichen, mit Anwendung auf das Werteveilungsproblem für fastperiodische Funktionen, insbesondere für die Riemannsche Zetafunktion, *Verhandlungen des Internationalen Mathematikerkongresses, Zürich,* September 1932, vol. 2, pp. 135-136, et *Mat. Tidsskr.* B (1932), pp. 59-65.

[1932b] Über die Nullstellen einer analytischen fastperiodischen Funktion. Eine Verallgemeinerung der Jensenschen Formel, *Math. Ann.,* 108 (1932), pp. 485-516.

[1934a] The theory of integration in a space of an infinite number of dimensions, *Acta Math.,* 63 (1934), pp. 249-323.

[1934b] On the approximation of Lebesgue integrals by Riemann sums, *Ann. Math.,* 35 (1934), pp. 248-251.

[1934-1947] Abstrakt Maal- og Integralteori 1-10, *Mat. Tidsskr.* B (1934), pp. 73-84, (1935), pp. 60-74, (1938), pp. 13-26, (1939), pp. 7-21, (1942), pp. 43-53, (1944), pp. 28-34, 35-37, (1947), pp. 1-20, 21-26, 27-36, rassemblés sous le même titre en un volume, København, Matematisk Forening, 1947.

[1938] Abstrakte Mass- und Integraltheorie, *Neuvième Congrès des Mathématiciens Scandinaves, Helsingfors du 23 au 26 août 1938,* Helsingfors, Mercators Tryckeri, 1939, pp. 121-134.

[1949] On the Proofs of the Fundamental Theorem on Almost Periodic Functions, *D. Kgl. Danske Vidensk Selskab Mat. -fys. Medd.,* 25, 8 (1949).

[1951] Harald Bohr, 22 April 1887-22 January 1951, *Acta Math.,* 86 (1951), pp. I-XXIII.

JESSEN (B.), TORNEHAVE (H.)

[1945] Mean motions and zeros of almost periodic functions, *Acta Math.,* 77 (1945), pp. 137-279.

JESSEN (B.), WINTNER (A.)

[1935] Distribution functions and the Riemann Zeta function, *Trans. Amer. Math. Soc.* 38 (1935), pp. 48-88.

KAC (M.)

[*Œuvres*] *Selected Papers,* Cambridge, MIT Press, 1979.

[1936] Sur les fonctions indépendantes (I), *Studia Math.,* 6 (1936), pp. 46-58.

[1959] *Statistical independence in probability theory, analysis and number theory,* Carus Math. Monograph, 12, New York, Wiley, 1959.

[1985] *Enigmas of chance, an autobiography,* New York, Harper and Row, 1985.

KAC (M.), ROTA (G.-C.), SCHWARZ (J. T.)

[1986] *Discrete thoughts. Essays on mathematics, science and philosophy,* Boston, Birkhäuser, 1986.

KACZMARZ (S.), STEINHAUS (H.)

[1935] *Theorie der Orthogonalreihen,* Warszawa-Lwów, Monografje Matematyczne tom VI, 1935, reprint New York, Chelsea, 1951.

KAHANE (J.-P.)

[1963] *Séries de Fourier aléatoires,* Montréal, Presses de l'Université de Montréal, 1963, 2e éd. 1966.

[1971] Sur la distribution de certaines séries aléatoires, *Mém. Soc. Math. France,* 25 (1971), pp. 119-122.

[1985] *Some random series of functions,* second ed., Cambridge, Cambridge University Press, 1985.

[1988] Une théorie de Denjoy des martingales dyadiques, *Enseign. Math.,* (2) 34 (1988), pp. 255-268.

[2001] Naissance et postérité de l'intégrale de Lebesgue, *Gazette des mathématiciens,* 89 (2001), pp. 5-20.

[2004] La jeunesse d'Andrei Nikolaevich et les séries de Fourier, in E. Charpentier, A. Lesne, N. Nikolski, *L'héritage de Kolmogorov en mathématiques,* Paris, Belin, 2004, pp. 17-29.

KAHANE (J.-P.), LEMARIE-RIEUSSET (G.)

[1998] *Séries de Fourier et ondelettes*, Paris, Cassini, 1998.

KAKUTANI (S.)

[*Works*] *Selected Papers,* 2 vol, Boston, Birkhäuser, 1986.

[1943] Notes on infinite product measure spaces I, II. *Proc. Impp. Acad. Tokyo,* 19 (1943), pp. 148-151, 184-188.

KALLENBERG (O.)

[2002] *Foundations of Modern Probability,* second ed., New York, Springer, 2002.

KAMPEN (E. R. van)

[1936] Almost periodic functions and compact groups, *Ann. Math.,* 37 (1936), pp. 78-91.

[1940] Infinite product measures and infinite convolutions, *Amer. J. Math.,* 62 (1940), pp. 417-448.

KANTOR (J.-M.)

[2004] Mathématiques d'Est en Ouest. Théorie et pratique : l'exemple des distributions, *Gazette des mathématiciens,* 100 (2004), pp. 33-43, trad. anglaise, *Math. Intelligencer,* 26 (2004), pp. 39-50.

KENDALL (D. G.)

[1990] Obituary : Andrei Nikolaevich Kolmogorov (1903-1987), organized by D. G. Kendall, *Bull. London Math. Soc.,* 22 (1990), pp. 31-100.

KHINCHIN (A. Ya.)

[*Works*] *Izbrannye trudy po teorii chisel,* Moskva, MTSNMO, 2006.

[1924a] Sur un théorème général relatif aux probabilités dénombrables, *C. R. Acad. Sci. Paris,* 178 (1924), pp. 617-619.

[1924b] Ueber einen Satz der Wahrscheinlichkeitsrechnung, *Fund. Math.,* 6 (1924), pp. 9-20.

[1928] Sur la loi forte des grands nombres, *C. R. Acad. Sci. Paris,* 186 (1928), pp. 285-287.

[1929] Über einen neuen Grenzwertsatz der Wahrscheinlichkeitsrechnung, *Math. Ann.,* 101 (1929), pp. 745-752.

[1932] Zur Birkhoffs Lösung des Ergodenproblems *Math. Ann.*, 107 (1933), pp. 485-488.

[1933] *Asymptotische Gesetze der Warsheinlichkeitsrechnung*, Berlin, Springer, (Ergebnisse der Mathematik und ihrer Grenzgebiete 4) 1933.

[1937] Déduction nouvelle d'une formule de M. Paul Lévy, *Bull. Univ. Moscou,* A, 1 (1937), pp. 1-5.

[1952] La méthode des fonctions arbitraires et la lutte contre l'idéalisme dans la théorie des probabilités, (en russe), *Les questions philosophiques de la physique actuelle,* Acad. Sci. URSS, 1952 ; traduction française, *Questions scientifiques,* tome V, Paris, La nouvelle critique,1954, pp. 9-24.

KHINCHIN (A. Ya.), KOLMOGOROV (A. N.)

[1925] Über Konvergenz von Reihen, deren Glieder durch den Zufall bestimmt werden, *Recueil Math. de Moscou,* 32 (1925), pp. 668-677.

KOLMOGOROV (A. N.)

[*Works*] *Selected Works,* 3 vols, Dordrecht : Kluwer Acad. Pub., 1991-1993.

[1928-1929] Über die Summen durch den Zufall bestimmter unabhängiger Grössen, *Math. Ann.,* 99 (1928), pp. 309-319, 102 (1929), pp. 484-488.

[1930] Sur la loi forte des grands nombres, *C. R. Acad. Sci. Paris,* 191 (1930), pp. 910-911.

[1931] Über die analytischen Methoden in der Wahrscheinlichkeitsrechnung, *Math. Ann.,* 104 (1931), pp. 415-458. Traduction anglaise *Oeuvres* II, pp. 62-108.

[1933] *Grundbegriffe der Wahrscheinlichkeitsrechnung,* Berlin, Springer (Ergebnisse der Mathematik und ihrer Grenzgebiete 3), 1933. Traduction anglaise, *Foundations of the Theory of Probability,* New York, Chelsea, 1950.

[1935] On some modern trends in probability theory (en russe), *Proc. 2nd All-Union Math. Congress (Leningrad 24-30 June 1934),* pp. 349-358.

[1936a] Zur Theorie der Markoffschen Ketten, *Math. Ann.,* 112 (1936), pp. 155-160.

[1936b] Anfangsgründe der Theorie der Markoffschen Ketten mit unendlich vielen möglichen Zuständen, *Mat. Sbornik,* 1 (1936), pp. 607-610. Traduction française par J. Barbot, Institut des Actuaires français, 1949.

[1937] Chaînes de Markoff à états dénombrables, (en russe), *Bull. Univ. Moscou,* I, 3 (1937). Traduction anglaise *Oeuvres* II, pp. 193-208.

[1950] A theorem on convergence of conditional expectations, *First Congress of the Hungarian Mathematicians,* Budapest, Akadémiai Kiado, 1950, pp. 367-376, (en Russe).

KÖTHE (G.)

[1989] Banach und die Lemberger Schule der Funktionsanalyse, *Math. Semesterber.,* 36 (1989), pp. 145-158.

KRENGEL (U.)

[1990] Wahrscheinlichkeitstheorie, *Ein Jahrundert Mathematik 1890-1990, DMV-Festschrift,* ed. G. Fischer, F. Hirzebruch, W. Scharlau und W. Töring, Brauschweig, Vieweg, 1990, pp. 457-489.

[1994] History of Probability and Statistics at the International Congresses of Mathematicians, ICM Zürich, 1994.

KRICKEBERG (K.)

[1956] Convergence of martingales with a directed index set, *Trans. Amer. Math. Soc.,* 83 (1956), pp. 313-337.

[1965] *Probability Theory,* (translated from the German 1963), Reading, Addison-Wesley, 1965.

KRICKEBERG (K.), PAUC (C.)

[1963] Martingales et dérivations, *Bull. Soc. Math. Fr.,* 91 (1963), pp. 455-543.

KURATOWSKI (K.)

[*Works*] *Selected Papers,* Warszawa, PWN, 1988.

[1980] *A half century of Polish Mathematics : remembrances and reflections,* New York, Pergamon, 1980.

KUZMIN (R.)

[1932] Sur un problème de Gauss, *Comptes rendus du Congrès international des mathématiciens de Bologne septembre 1928*, 6 (1932), pp. 83-89.

LAGRANGE (J. L.)

[*Oeuvres*] *Oeuvres de Lagrange,* 14 vols., Paris, Gauthier-Villars, 1867-1892.

[1783] Théorie des variations séculaires des planètes. Première partie contenant les principes pour déterminer ces variations, *Nouv. Mém. Acad. Berlin, pour l'année 1781*, (1783), pp. 199-276, *Œuvres* 5, pp. 125-207.

LAPLACE (PP. S.)

[*Oeuvres*] *Oeuvres de Laplace,* 14 vols., Paris, Gauthier-Villars, 1878-1912.

[1799-1825] *Traité de mécanique céleste,* [*Oeuvres*] I-V.

[1812-1820] *Théorie analytique des probabilités,* Paris, Ve Courcier, 1812, troisième éd. revue et augmentée, ibid., 1820. [*Oeuvres*] vol VII.

[1814-1825] *Essai philosophique sur les probabilités*, Paris, Bachelier, 1814-1825. Rééd. Paris, Bourgois, 1986.

LA VALLÉE POUSSIN (C. de)

[*Œuvres*] *Œuvres scientifiques,* 3 vol., Académie Royale de Belgique, Circolo Matematico di Palermo, 2000-2004.

[1909] *Cours d'analyse infinitésimale,* 2e éd., Volume I, Louvain, A. Uystpruyst-Dieudonné, 1909.

[1911] Sur la transformation d'une intégrale multiple en une intégrale simple, *Ann. Soc. Sci. Bruxelles,* 35 (1911), pp. 189-190.

[1914] *Cours d'analyse infinitésimale,* 2e éd., Volume II, Louvain, épreuves détruites dans l'incendie de Louvain. 12e éd., Paris, Gauthier-Villars, 1957, rééd. Paris, Jacques Gabay, 2003.

[1915] Sur l'intégrale de Lebesgue, *Trans. Amer. Math. Soc.* 16 (1915), pp. 435-501.

[1916] *Intégrales de Lebesgue, fonctions d'ensemble, classes de Baire,* leçons professées au Collège de France, (Collection de Monographies sur la Théorie des fonctions, publiées sous la direction d'Émile Borel), Paris, Gauthier-Villars, 1916.

[1927] Sur les fonctions presque périodiques de H. Bohr, *Ann. Soc. Sci. Bruxelles,* 47 (1927), pp. 141-158.

LEBESGUE (H.)

[*Oeuvres*] *Oeuvres Complètes,* 5 vol., Genève, L'Enseignement Mathématique, 1972.

[1899] Sur les fonctions de plusieurs variables, *C. R. Acad. Sci. Paris,* 128 (1899), pp. 811-813.

[1901] Sur une généralisation de l'intégrale définie, *C. R. Acad. Sci. Paris,* 132 (1901), pp. 1025-1028, *Œuvres* I, pp. 197-199.

[1902] *Intégrale, Longueur, Aire,* thèse sci. math., Paris, 1902 et *Ann. Mat. pura ed applicada,* (3) 7 (1902), pp. 231-331.

[1903] Sur l'existence des dérivées, *C. R. Acad. Sci. Paris,* 136 (1903), pp. 659-661.

[1903-1905] Sur le problème des aires, *Bulletin de la S. M. F.,* 31 (1903), pp. 197-203, correction, *ibid.,* 33 (1905), pp. 273-274.

[1904] *Leçons sur l'intégration et la recherche des fonctions primitives,* (*Collection de Monographies sur la Théorie des fonctions* , publiées sous la sous la direction de M. Émile Borel), Paris, Gauthier-Villars, 1904, second edition revised and augmented, ibid., 1928, new edition, ibid., 1950, reprinted Paris, Jacques Gabay, 1989.

[1905a] Recherches sur la convergence des séries de Fourier, *Math. Ann.,* 61 (1905), pp. 251-260, *Œuvres* III, pp. 181-210.

[1905b] Sur les fonctions représentables analytiquement, *J. Math. Pures Appl.,* (6), 1 (1905), pp. 139-216.

[1906] *Leçons sur les séries trigonométriques,* (*Collection de Monographies sur la Théorie des fonctions* , publiées sous la sous la direction de M. Émile Borel), Paris, Gauthier-Villars, 1906.

[1910] Sur l'intégration des fonctions discontinues, *Ann. Sci. ENS,* (3) 27 (1910), pp. 361-450, *Œuvres* II, pp. 185-274.

[1917] Sur certaines démonstrations d'existence, *Bull. Soc. Math. Fr.,* 45 (1917), pp. 132-144.

[1991] Correspondance avec Émile Borel, éd. PP. Dugac, *Cahiers du Séminaire d'histoire des mathématiques*, 12 (1991), morceaux choisis dans *Les* *lendemains de l'intégrale. Lettres à Emile Borel,* préface de G. Choquet, Paris, Vuibert, 2004.

LE CAM (L.)

[1986] The central limit theorem around 1935, (with discussion), *Stat. Sci.,* 1 (1986), pp. 78-96.

LEHTO (O.)

[1998] *Mathematics without borders. A history of the International Mathematical Union*, New York, Springer, 1998.

LEVI (B.)

[1906a] Ricerche sulle funzioni derivate, *Atti R. Acad. Lincei, Rendiconti,* XV 1 (1906), pp. 423-438, 551-558, 674-684, XV 2 (1906), pp. 358-368.

[1906b] Sul principio di Dirichlet, *Rend. Circ. Mat. Palermo,* 22 (1906), pp. 293-359.

LÉVY (P.)

[*Oeuvres*] *Oeuvres complètes,* 6 vols, Paris, Gauthier-Villars, 1980.

[1919/2008] Cours de calcul des probabilités de Paul Lévy à l'Ecole Polytechnique (1919), *jehps,* 4/1 (2008).

[1922] *Leçons d'analyse fonctionnelle,* Paris, Gauthier-Villars, 1922, seconde édition revue, augmentée et publiée sous le nouveau titre, *Problèmes concrets d'analyse fonctionnelle,* Paris, Gauthier-Villars, 1951.

[1924] Théorie des erreurs. La loi de Gauss et les lois exceptionnelles, *Bulletin de la S. M. F.,* 52 (1924), pp. 49-85.

[1925a] Les lois de probabilité dans les ensembles abstraits, *Revue de Métaphysique et de Morale,* (1925), pp. 149-175.

[1925b] *Calcul des probabilités*, Paris, Gauthier-Villars, 1925.

[1925c] *Analyse fonctionnelle,* Mémorial des sciences mathématiques, n° 5, 1925.

[1925d] Sur le théorème de M. M. Fischer et Fr. Riesz sur la convergence en moyenne, *Bull. Sci. Math.* (2) 49 (1925), pp. 341-352, 374-380.

[1928] Fonctions à croissance régulière et itération d'ordre fractionnaire, *Atti del Congresso Internatiozionale dei Matematici, Bologna, 1928,* Bologne, Zanichelli, 1932, et *Annali di Matematica*, 2 (1928), pp. 277-282.

[1929] Sur les lois de probabilité dont dépendent les quotients complets et incomplets d'une fraction continue, *Bulletin de la S. M. F.,* 57 (1929), pp. 178-194.

[1930a] Sur quelques inégalités relatives aux fonctions entières, *C. R. Acad. Sci. Paris,* 190 (1930), pp. 419-421.

[1930b] Sur la croissance des fonctions entières, *Bulletin de la S. M. F.,* 58 (1930), pp. 29-59, 127-149.

[1930c] Sur la loi forte des grands nombres, *C. R. Acad. Sci. Paris,* 191 (1930), pp. 983-984.

[1930d] Le théorème fondamental de la théorie des erreurs, *Ann. Inst. H. Poincaré,* 1 (1930), pp. 163-175.

[1931a] Sur un théorème de M. Khintchine, *Bull. Sci. Math.* 55 (1931), pp. 145-160.

[1931b] Sulla legge forte dei grandi numeri, *Giorn. D. Ist. Ital. D. Attuari,* 2 (1931), pp. 1-21.

[1931c] Sur les séries dont les termes sont des variables éventuelles indépendantes, *Studia Math.,* 3 (1931), pp. 119-155.

[1931d] Sur quelques questions de calcul des prebabilités, *Prace Mat.-Fiz.,* 39 (1931), pp. 19-28.

[1932] Sur les méthodes de M. Norbert Wiener et la fonction $\zeta(s)$, *Vortrag gehalten auf dem Internationalen MathematikerKongress in Zürich,* September 1932.

[1934a] Généralisation de l'espace différentiel de M. Wiener, *C. R. Acad. Sci. Paris,* 198 (1934), pp. 786-788.

[1934b] Sur les intégrales dont les éléments sont des variables aléatoires indépendantes, *Ann. R. Sc. Norm. Supp. Pisa,* (2) 3 (1934), pp. 337-366, Observations sur le mémoire précédent, *ibid.,* 4 (1935), pp. 217-218.

[1934c] Propriétés asymptotiques des sommes de variables aléatoires enchaînées, *C. R. Acad. Sci. Paris,* 199 (1934), pp. 627-629.

[1935a] *Notice sur les travaux scientifiques de M. Paul Lévy,* Paris, Hermann, 1935.

[1935b] Propriétés asymptotiques des sommes de variables aléatoires enchaînées, *Bull. Sci. Math.,* (2) 59 (1935), pp. 84-96 et 109-128.

[1935c] La loi forte des grands nombres pour les variables enchaînées, *C. R. Acad. Sci. Paris,* 201 (1935), pp. 493-495, 800.

[1935d] Propriétés asymptotiques des sommes de variables aléatoires indépendantes ou enchaînées, *J. Math. Pures Appl.,* 14 (1935), pp. 347-402.

[1935e] Sur la sommabilité des séries aléatoires divergentes, *Bulletin de la S. M. F.,* 63 (1935), pp. 1-35.

[1936a] La loi forte des grands nombres pour les variables enchaînées, *J. Math. Pures Appl.,* 15 (1936), pp. 11-24.

[1936b] Sur quelques points de la théorie des probabilités dénombrables, *Ann. Inst. H. Poincaré,* 6 (1936), pp. 153-184.

[1936c] Sur le développement en fraction continue d'un nombre choisi au hasard, *Compositio Mathematica,* 3 (1936), pp. 286-303.

[1936d] Sur la notion de probabilité conditionnelle, *Bull. Sci. Math.*, (2) 60 (1936), pp. 66-71.

[1937] *Théorie de l'addition des variables aléatoires*, (Fascicule I de la Collection des monographies des probabilités , publiée sous la direction de M. Émile Borel), Paris, Gauthier-Villars, 1937, 2e éd., augmentée de deux notes, ibid., 1954, réimpression, Paris, Jacques Gabay, 2003. (Nous donnons les pages de l'édition 1954).

[1947] L'analyse harmonique des fonctions aléatoires stationnaires, *Actes du Colloque d'Analyse harmonique de Nancy 1947,* CNRS, 1949, pp. 117-120.

[1948] *Processus stochastiques et mouvement brownien*, suivi d'une note de M. Loève, (Fascicule VI de la Collection des monographies des probabilités , publiée sous la direction de M. Émile Borel), Paris, Gauthier-Villars, 1948, réimpression Paris, Jacques Gabay, 1992.

[1956] Compte-rendu critique de [Khinchin 1952], *Bull. Sci. Math.,* (1956), pp. 175-180.

[1961] An extension of the Lebesgue measure of linear sets, *Proceedings of the Fourth Berkeley Symposium on Math. Stat. and Prob.,* June-July 1960, Berkeley, Univ. of California Press, 1961, vol II, pp. 273-287.

[1967a] Les mathématiques, *Troisième centenaire de l'Académie des sciences, 1666-1966,* Paris, Gauthier-Villars, 1967, pp. 143-212.

[1967b] Jacques Hadamard, sa vie et son œuvre. Calcul fonctionnel et questions diverses, *La vie et l'oeuvre de Jacques Hadamard,* (Monographie n° 16 de l'Enseignement mathématique), Genève, Kundig, 1967.

[1970] *Quelques aspects de la pensée d'un mathématicien,* Paris, Blanchard, 1970.

LOCKER (B.)

[2001] *Paul Lévy : « La période de guerre ». Intégrales stochastiques et mouvement brownien,* thèse Univ. Paris 5, 2001.

[2009] Le Colloque de Lyon, *Journal Electronique d'Histoire des Probabilités et de la Statistique,* this issue.

LOÈVE (M.)

[1941] *Étude asymptotique des sommes de variables aléatoires liées*, thèse sci. math. Paris, Gauthier-Villars, 1946 et *Journal Maths pures et app.,* 9ième série, XXIV, 4, (1945), pp. 249-318. La première partie de la thèse de Loève a été publiée en 1942 aux *Annales de Lyon*.

[1953] *Theory of Probability*, New-York, Wiley, 1953, 1960, 1963, 4ième éd. New York, Springer, 1978, traduit en espagnol et en russe.

ŁOMNICKI (A.)

[1923] Nouveaux fondements du calcul des probabilités, *Fund. Math.,* 4 (1923), pp. 34-71.

ŁOMNICKI (Z.), ULAM (S.)

[1934] Sur la théorie de la mesure dans les espaces combinatoires et son application au calcul des probabilités I. Variables indépendantes, *Fund. Math.,* 23 (1934), pp. 237-278.

LOUANCHI (M.)

[1997] Aperçu sur la vie de S. N. Bernstein, *Matapli,* 52 (1997), pp. 21-30.

LÜTZEN (J.)

[1982] *The prehistory of the theory of distributions,* Berlin, Springer-Verlag, 1982.

MAHARAM (D.)

[1958] On two Theorems of Jessen, *Proc. Amer. Math. Soc.,* 9 (1958), pp. 995-999.

MARAKOU (L.)

[2003-2005] *Repressed writers, scientists, educators, public and cultural figures of Belarus,* (in Russian), 4 vol., Smolensk, Minsk, 2003-2005.

MARCINKIEWICZ (J.)

[*Works*] *Collected papers,* Warszawa, Państwowe Wydawnictwo Naukowe, 1964.

[1938] Sur les fonctions indépendantes (I), (II), *Fund. Math.,* 30 (1938), pp. 202-214, 349-364.

MARCUS (M. B.), PISIER (G.)

[1981] *Random Fourier series with applications to harmonic analysis,* Ann. of Math. Studies, Princeton University Press, 1981.

MARKOV (A. A.)

[*Oeuvres*] *Izbrannie Trudy,* Leningrad, A.N.S.S.S.R., 1951.

[1907] Extension de la loi des grands nombres aux événements dépendants les uns des autres, (en russe), *Bull. Soc. phys.-math. Kazan,* 2ème série, 15 (1906), pp. 135-156. *Oeuvres* pp. 339-361.
[1910] Recherches sur un cas remarquable d'épreuves dépendantes, *Acta math.,* 33 (1910), pp. 87-104.
[1912] *Wahrscheinlichkeitsrechnung. Nach der zweiten Auflage der russischen Werkes übers. von H. Liebmann,* Leipzig 1912, 4ième éd. (en russe), Moscou, 1924.

MAZLIAK (L.)
[2007] Les fantômes de l'Ecole normale. Vie, mort et destin de René Gateaux, *Séminaire d'histoire du calcul des probabilités de l'EHESS,* 2007, et http://www.proba.jussieu.fr/~mazliak/GateauxFr.pdf.

MAZURKIEWICZ (S.)
[*Œuvres*] *Travaux de topologie et ses applications,* Warszawa, PWN, 1969.
[1922] O pewnej nowej formie uogolnienia twierdzenia Bernouilli'ego. (Sur une nouvelle généralisation du théorème de Bernoulli), *Wiadomosci Aktuarialne,* 1 (1922), pp. 3-8, (en polonais, résumé français).
[1939] Sur les espaces de variables aléatoires, édité par E. Marczewski, *Fund. Math.,* 36 (1949), pp. 288-302.

MEDVEDEV (F. A.)
[1976] *L'école française de théorie des fonctions et des ensembles à la frontière des 19^e et 20^e siècles,* (en Russe), Moscou, Nauka, 1976.

METIVIER (M.)
[1963] *Limites projectives de mesures, martingales, applications,* Thèse Sc. math. Rennes, 1963.

MEYER (PP. A.)
[1966] *Probabilités et potentiel,* Paris, Hermann, 1966, édition refondue avec C. DELLACHERIE, 4 vol., Paris, Hermann, 1975-1992.
[2000] Les Processus Stochastiques de 1950 à Nos Jours, [Pier 2000], pp. 813-848.

MEZZANOTTE (A.)
[1928] Estensione di un teorema sulla oscillazione delle frequenze alla probabilità, alle medie di variabili casuali più generali, *Rend. Circ. Mat. Palermo,* (1) 52 (1928), pp. 331-344.
[1936] Intorno ad una questione di probabilità, *Giorn. D. Ist. Ital. D. Attuari,* 3 (1936)

MOIVRE (A. de)
[1718-1756] *The Doctrine of Chances : or, A Method of Calculating the Probability of Events in Play,* London, W. Pearson, 1718, 3rd ed. London, Millar, 1756, reprint. New York, Chelsea, 1967.

MONTEL (P.)
[1958] La mathématique méditerranéenne, *Bulletin de la S. M. F.,* 86 (1958), pp. 257-270.

MOORE (E. H.)

[1917] A question, *Amer. Math. Monthly,* 24 (1917), pp. 31, 333.

MOY (S.-T. C.)

[1953] Measure extensions and the Martingale convergence theorem, *Proc. Amer. Math. Soc.,* 4 (1953), pp. 902-907.

NEUMANN (J. von)

[*Works*] *Collected Works,* A. H. Taub éd., 6 vols, Oxford, Pergamon Press, 1961-1963.

[1932a] Einige Sätze über messbare Abbildungen, *Ann. Math.,* 33 (1932), pp. 574-586.

[1932b] Zur Operatorenmethode in der klassischen Mechanik, *Ann. Math.,* 33 (1932), pp. 587-642.

[1934a] Almost periodic functions in a group I, *Trans. Amer. Math. Soc.,* 36 (1934), pp. 445-492.

[1934b] *Functional Operators,* vol I, *Measures and Integrals*, Notes on lectures given at the I. A. S., Princeton (1933-1934), *Ann. of Math. Studies,* Princeton Univ. Press, 1950.

NEVEU (J.)

[1964] *Bases mathématiques du calcul des probabilités,* Paris, Masson, 1964.

[1965] Relations entre la théorie des martingales et la théorie ergodique, *Ann. Inst. Fourier,* 15 (1965), pp. 31-42.

[1972] *Martingales à temps discret,* Paris, Masson, 1972.

NIKISHIN (E. M.)

[1975] Dirichlet Series with independent exponents and some of their applications, *Math USSR Sb.,* 25, 1 (1975), pp. 1-36.

NIKODYM (O.)

[1930] Sur une généralisation des intégrales de Radon, *Fund. Math.,* 15 (1930), pp. 131-179.

NOAILLON (P.)

[1912] *Développements asymptotiques dans les équations différentielles linéaires à paramètre variable,* Mém. Soc. Roy. Sci. Liège, 9, 10 (1912), et Bruxelles, Hayez, 1912.

[1926] Sur la détermination sans ambiguïté de la solution du problème de Dirichlet, pour les fonctions sommables, *C. R. Acad. Sci. Paris,* 182 (1926), pp. 1371-1373.

[1927] Point isolé non critique d'une fonction harmonique, *C. R. Acad. Sci. Paris,* 184 (1927), pp. 360-362.

[1929] Ebauche d'une nouvelle théorie de la résistance des fluides, *C. R. Acad. Sci. Paris,* 188 (1929), pp. 441-443.

[1935] Brusque détente dans un gaz, *C. R. Acad. Sci. Paris,* 201 (1935), pp. 258-260.

OLSEN (L. W.)

[2003] *Essays on Georg Rasch and his contributions to statistics,* PhD thesis, Institute of Economics, University of Copenhagen, http://www.rasch.org/olsen.pdf.

OXTOBY (J.), ULAM (S.)

[1941] Measure preserving homeomorphisms and metrical transitivity, *Ann. Math.* 42 (1941), pp. 874-920.

PALEY (R.E.A.C.), WIENER (N.), ZYGMUND (A.)

[1933] Note on random functions, *Math. Zeit.,* 37 (1933), pp. 647-668.

PALEY (R.E.A.C.), WIENER (N.)

[1934] Fourier Transforms in the Complex Domain, *Amer. Math. Soc. Colloquium Pub.,* 19 (1934).

PALEY (R. E. A. C.), ZYGMUND (A.)

[1930-1932] On some series of functions (1), (2), (3), *Proc. Camb. Phil. Soc.,* 26 (1930), pp. 337-357, 458-474, 28 (1932), pp. 190-205.

PEANO (G.)

[*Works*] *Opere scelte,* Roma, Cremonese, 1957-1962.

[1890] Sur une courbe, qui remplit toute une aire plane, *Math. Ann*., 36 (1890), pp. 157-160.

PEDERSEN (E.)

[1928] Über einige besondere Klassen von fastperiodischen Funktionen, *D. Kgl. Danske Vidensk Selskab Mat. -fys. Medd.,* 8, 6 (1928).

PERRIN (J.)

[1910] *Brownian Motion and Molecular Reality,* London, Taylor and Francis, 1910, translation by F. Soddy from *Annales de Chimie et de Physique,* 18 (1909), pp. 1-114.

[1913] *Les Atomes,* Paris, Alcan (Nouvelle collection scientifique, directeur Émile Borel), 1913.

PETIT (M.)

[2003] *L'équation de Kolmogoroff. Vie et mort de Wolfgang Doeblin, un génie dans la tourmente,* Paris, Ramsay, 2003, 2e éd. revue, Paris, Gallimard (Folio 4240), 2005.

PICARD (E.)

[1901] *Traité d'Analyse,* vol. I, 2e éd., Paris, Gauthier-Villars, 1901, 3e éd., ibid., 1925.

PIER (J.-P.)

[1986] Mesures invariantes de Lebesgue à nos jours, *Historia mathematica,* 13 (1986), pp. 229-240.

[1990] *L'analyse harmonique : son développement historique,* Paris, Masson, 1990.

[1994] (ed.) *Developments of Mathematics 1900-1950,* Basel, Birkhäuser, 1994.

[1996] *Histoire de l'intégration : vingt-cinq siècles de mathématiques,* Paris, Masson, 1996.

[2000] (ed.) *Developments of Mathematics 1950-2000,* Basel, Birkhäuser, 2000.

POINCARÉ (H.)

[*Oeuvres*] *Oeuvres de Henri Poincaré,* 11 vols, Paris, Gauthier-Villars, 1916-1956.

[1886] Sur les déterminants d'ordre infini, *Bulletin de la S. M. F.,* 14 (1886), pp. 77-90.

[1896] *Calcul des probabilités,* leçons professées pendant le second semestre 1893-1894, rédigées par A. Quiquet, ancien élève de l'École normale supérieure, Paris, Gauthier-Villars, 1896.

[1912] *Calcul des probabilités*, second edition revised and augmented by the author, Paris, Gauthier-Villars, 1912, new impression 1923, reprint Paris, Jacques Gabay, 1987.

[1913] *Dernières pensées,* Paris, Flammarion, 1913.

PÓLYA (G.)

[*Oeuvres*] *Collected papers,* 3 vol., Cambridge, MIT Press, 1984.

[1920] Über den zentralen Grenzwertsatz der Wahrscheinlichkeitsrechnung und das Momentproblem, *Math. Zeitschrift,* 8 (1920), pp. 171-181.

[1921a] Über eine Aufgabe der Wahrscheinlichkeitsrechnung betreffend die Irrfhart im Strassennetz, *Math. Ann.,* 84 (1921), pp. 149-160.

[1921b] Eine Ergänzung zu dem Bernoullischen Satz der Wahrscheinlichkeitsrechnung, *Nachrichten von der Gesellschaft der Wissenschaften zu Göttingen, Mathematisch-Physikalische Klasse,* (1921), pp. 223-228, Berichtigung, *ibid.,* (1923).

[1930] Sur quelques points de la théorie des probabilités, *Ann. I.H.P.*, 1 (1930), pp. 117-161.

RADEMACHER (H.)

[*Works*] *Collected papers,* Cambridge, MIT Press, 1974.

[1918] Zu dem Borelschen Satz über die asymptotische Verteilung der Ziffen in Dezimalbrüchen, *Math. Zeitschrift,* 2 (1918) pp. 306-311, 3 (1919), pp. 317.

[1922] Einige Sätze über Reihen von allgemeinen Orthogonalfunktionen, *Math. Ann.,* 87 (1922), pp. 112-138.

RADON (J.)

[1913] Theorie und Anwendung der absolut additiven Mengenfunktionen, *Wiener Sitzungsberichte,* 122 (1913), pp. 1295-1438.

RAMSKOV (K.)

[1995] *Matematikeren Harald Bohr,* Licentiatafhandling, Institut for de eksakte videnskabers historie, Aarhus Universitet, 1995.

[2000a] Sources for Danish Mathematics, *Historia Mathematica,* 27 (2000), pp. 164-170.

[2000b] The Danish Mathematical Society through 125 Years, *Historia Mathematica,* 27 (2000), pp. 223-242.

RASCH (G.)

[1929] Om Systemer af lineære Differentialligninger, *Comptes rendus du septième Congrès des Mathématiciens scandinaves tenu à Oslo,* 19-22 août 1929, Oslo, A. W. Broggers Boktrykkeri, 1930, pp. 117-126.

[1960] *Probabilistic Models for Some Intelligence and Attainment Tests,* København, Danmarks pædagogiske Institut, 1960, expanded edition, Chicago, University of Chicago Press, 1980.

REGAZZINI (E.)
[1987] Probability theory in Italy between the two world wars. A brief historical review, *Metron,* 45 (3-4) (1987), pp. 5-42.
[2005] Probability and statistics in Italy during the First World War I : Cantelli and the laws of large numbers, *Journal Electronique d'Histoire des Probabilités et de la Statistique*, 1 (1) (2005).

RIESZ (F.)
[*Oeuvres*] *Oeuvres Complètes,* 2 vol., Paris, Gauthier-Villars, 1960.
[1906] Sur les ensembles de fonctions, *C. R. Acad. Sci. Paris,* 143 (1906), pp. 738-741.
[1907a] Sur les systèmes orthogonaux de fonctions et l'équation de Fredholm, *C. R. Acad. Sci. Paris,* 144 (1907), pp. 734-736.
[1907b] Sur une espèce de Géométrie analytique des systèmes de fonctions sommables, *C. R. Acad. Sci. Paris,* 144 (1907), pp. 1409-1411.
[1909] Sur les suites de fonctions mesurables, C. *R. Acad. Sci. Paris,* 148 (1909), pp. 1303-1305.
[1910] Untersuchungen über Systeme integrierbarer Funktionen, *Math. Ann.*, 69 (1910), pp. 449-497.
[1913] *Les systèmes d'équations linéaires à une infinité d'inconnues,* (Collection de Monographies sur la Théorie des fonctions, publiées sous la direction d'Émile Borel), Paris, Gauthier-Villars, 1913, nouveau tirage, 1952.
[1918] Über lineare Funktionalgleichungen, *Acta Math.*, 41 (1918), pp. 71-98.
[1932] Sur l'existence de la dérivée des fonctions monotones et sur quelques problèmes qui s'y rattachent, *Acta Sci. Math. Szeged,* 5 (1932), pp. 208-221.
[1934] Eine Bemerkung über den Eindeutigkeitssatz der Theorie der fastperiodischen Funktionen, *Mat. Tidsskrift* B (1934), pp. 11-13.
[1936] Sur l'intégrale de Lebesgue comme opération inverse de la dérivation, *Ann. R. Sc. Norm. Supp. Pisa,* (2) 5 (1936), pp. 191-212.
[1949] L'évolution de la notion d'intégrale depuis Lebesgue, *Ann. Inst. Fourier,* 1 (1949), pp. 29-42.

RIESZ (F.), SZ-NAGY (B.)
[1952] *Leçons d'Analyse Fonctionnelle,* Budapest, Académie des sciences de Hongrie, 1952, 3ième éd., 1955, réimpression Paris, Jacques Gabay, 1990.

ROBADEY (A.)
[2005] *Différentes modalités de travail sur le général dans les recherches de Poincaré sur les système dynamiques,* thèse univ. Paris7, 2005.

ROKHLIN (V. A.)
[1949] On the fundamental ideas of measure theory, *Mat. Sbornik,* 25 (1949), pp. 107-150, English transl. *Amer. Math. Soc. Transl.* 1952, n° 71, reissued in *AMS Translations* (1) 10 (1962), pp. 1-54.

ROSENTHAL (A.)

[1923-1927] Neuere Untersuchungen über Funktionen reeller Veränderlichen. Nach den Referaten von L. Zoretti, PP. Montel und M. Fréchet bearbeiten von A. Rosenthal, *Encyklopädie der Math. Wiss.,* t. III, H 2, pp. 851-1187, Leipzig, Teubner, 1923-1927.

ROUANET (M.)
[1999] *L'infini de π,* Castelnau-le-Lez, Climats, 1999.

SAKS (S.)
[1933] *Théorie de l'intégrale,* Warszawa, Monografje Matematiczne, Tom II, 1933, 2e éd., *Theory of the integral*, New York, Stechert, 1937, reprint New York, Dover, 1964.

SCHMIDT (E.)
[1906] Sur la puissance des systèmes orthogonaux des fonctions continues, *C. R. Acad. Sci. Paris,* 143 (1906), pp. 955-957.

SCHØTT (T.)
[1980] Fundamental Research in a Small Country : Mathematics in Denmark 1928-1977, *Minerva,* 18 (1980), pp. 243-283.

SCHWARTZ (L.)
[1936] Sur une question de calcul des probabilités, *Bull. Sci. Math.,* (2) 60 (1936), 1ère partie, pp. 101-102.
[1947] Théorie des distributions et transformations de Fourier, *Actes du Colloque d'Analyse harmonique de Nancy 1947,* CNRS, 1949.
[1950] *Théorie des distributions,* 2 vol., Paris, Hermann, 1950, nouvelle éd. entièrement corrigée, refondue et augmentée, ibid., 1966.
[1980] *Semi-martingales sur des variétés, et martingales conformes sur des variétés analytiques complexes,* Lecture Notes in Math., Springer Verlag, 1980.
[1997] *Un mathématicien aux prises avec le siècle,* Paris, Odile Jacob, 1997.
[2002] *Petit manuel de garde à vue et de mise en examen,* Paris, Arlea, 2002.

SENETA (E.)
[1982] Bernstein, Sergei Natanovich, *Encyclopedia of Statistical Sciences,* Kotz, Johnson eds, vol I, New York, Wiley, 1982, pp. 221-223, reprint [Johnson Kotz, 1997], pp. 223-224.
[1984] The central limit problem and least squares in pre-revolutionary Russia, *Math. Scientist,* 9 (1984), pp. 37-77.
[1992] On the history of the strong law of large numbers and Boole's inequality, *Historia Mathematica,* 19 (1992), pp. 24-39.

SHAFER (G.), VOVK (V.)
[2005] The origins and legacy of Kolmogorov's *Grundbegriffe,* http://www.probabilityandfinance.com/articles/04.pdf . Abridged version : The sources of Kolmogorov's Grundbegriffe, *Statistical Science,* 21 (2006), pp. 70-98.

SIEGMUND-SCHULTZE (R.)

[2001] *Rockefeller and the Internationalization of Mathematics Between the Two World Wars,* Basel, Birkhäuser, 2001.
[2006] Probability in 1919/20 : the von Mises-Pólya Controversy, *Arch. Hist. Exact Sci.,* 60 (2006), pp. 431-515.

SIERPINSKI (W.)
[*Oeuvres*] *Oeuvres choisies,* 2 vol., Warszawa, PWN-Éditions Scientifiques de Pologne, 1974-1975.
[1917] Démonstration élémentaire du théorème de M. Borel sur les nombres absolument normaux et détermination effective d'un tel nombre, *Bull. Soc. Math. Fr.,* 45 (1917), pp. 125-132.
[1919] Sur une définition axiomatique des ensembles mesurables (L), *Bull. Acad. Sci. Cracovie,* (1919), pp. 173-178.

SLUTSKY (E. E.)
[*Oeuvres*] *Izbrannie Trudy,* Moscou, Izd AN SSSR, 1960.
[1925] Ueber stochastische Asymptoten und Grenzwerte, *Metron,* 5, 3 (1925), pp. 1-91.
[1926] Über zufällige zyklische Anordnung paarweise gleicher Elemente, *Zeitschrift f. angew. Math. Mech.,* 6 (1926), pp. 150-159.
[1927] The summation of random causes as the source of cyclic process, (en russe), *Problems of Economic Condition,* 3, 1 (1927), version anglaise révisée, *Econometrica,* 5 (1937), pp. 105-146.
[1928a] Sur un critérium de la convergence stochastique des ensembles de variables éventuelles, *C. R. Acad. Sci. Paris,* 187 (1928), pp. 370-373.
[1928b] Sur les fonctions éventuelles continues, intégrables et dérivables dans le sens stochastique, *C. R. Acad. Sci. Paris,* 187 (1928), pp 878-880.
[1928c] Sur les fonctions éventuelles compactes, *Actes Congrès Int. Math. Bologne 3-10 septembre 1928,* 6 (1932), pp. 111-115.
[1937] Qualche proposizione relativa alla teoria degli funzioni aleatorie, *Giorn. d. Ist. Ital. d. Attuari,* VIII, 2 (1937), pp. 183-199.
[1938] Sur les fonctions aléatoires presque périodiques et sur la décomposition des fonctions aléatoires stationnaires en composantes, *Colloque consacré à la théorie des probabilités et présidé par M. Maurice Fréchet, Genève 11 au 16 octobre 1937*, cinquième partie, Paris, Hermann (Actualités Sci. Ind. 738), 1938, pp. 33-55.

SNELL (J. L.)
[1951] *Applications of Martingale system theorems,* PhD, University of Illinois, 1951.
[1997] A conversation with Joe Doob, *Statistical Science,* 12, 4 (1997), pp. 301-311, http://www.dartmouth.edu/~chance/Doob/conversation.html.
[2005] Obituary : Joseph Leonard Doob, *J. Appl. Probab.,* 42, 1 (2005), pp. 247-256.

SPITZER (F.)
[1964] *Principles of Random Walks,* Princeton, Van Nostrand, 1964.

STEINHAUS (H.)
[*Œuvres*] *Selected Papers,* Warszawa, PWN-Polish Scientific Publishers, 1985.
[1923] Les probabilités dénombrables et leurs rapports avec la théorie de la mesure, *Fund. Math.,* 4 (1923), pp. 286-310.

[1929] Anwendungen der Funktionanalysis auf einige Fragen der reellen Funktionentheorie, *Studia Math.,* 1 (1929), pp. 51-81.

[1930a] Sur la portée pratique et théorique de quelques théorèmes sur la mesure des ensembles de droites, *Comptes-rendus du premier Congrès des mathématiciens des pays slaves, Warszawa 1929,* Warszawa 1930, pp. 348-354.

[1930b] Sur la probabilité de la convergence des séries, *Studia Math.,* 2 (1930), pp. 21-39.

[1930c] Über die Wahrscheinlichkeit dafür, dass des Konvergenzkreiss einer Potenzreihe ihre natürlich Grenze ist, *Math. Zeitschrift,* 31 (1930), pp. 408-416.

[1936] La courbe de Peano et les fonctions indépendantes, C. *R. Acad. Sci. Paris,* 202 (1936), pp. 1961-1963.

[1938] La théorie et les applications des fonctions indépendantes au sens stochastique, *Colloque consacré à la théorie des probabilités et présidé par M. Maurice Fréchet, Genève 11 au 16 octobre 1937*, cinquième partie, (Actualités Sci. Ind.738), Paris, Hermann, 1938, pp. 58-73.

STEPANOFF (W.)

[1926] Über einige Verallgemeinerungen der fast periodischen Funktionen, *Math. Ann.,* 95 (1926), pp. 473-498.

TISSERAND (F.-F.)

[1889] *Traité de mécanique céleste,* tome 1, Perturbation des planètes d'après la méthode de la variation des constantes arbitraires, Paris, Gauthier-Villars, 1889, reproduction Paris, Jacques Gabay, 1890.

TONELLI (L.)

[*Works*] *Opere scelte,* Roma, Cremonese, 1960.

[1909] Sull'integrazione per parti, *Rend. Accad. Nazionale Lincei,* (5) 18 (1909), pp. 246-253.

TORNEHAVE (H.)

[1989] Harald Bohr : Professor and Head of Department, *The Harald Bohr Centenary* (Copenhagen 1987), *D. Kgl. Danske Vidensk Selskab Mat. -fys. Medd.,* 42 (1989), pp. 9-14.

ULAM (S. M.)

[1930] Zur Masstheorie in der allgemeinen Mengenlehre, *Fund. Math.,* 16 (1930), pp. 140-150.

[1932] Zum Massbegriffe in Produkträumen, Verhandlungen des internationalen Mathematiker-Kongresses Zürich 1932, ii, pp. 118-119.

[1976] *Adventures of a mathematician,* New York, Scribner, 1976.

USPENSKY (J.V.)

[1937] *Introduction to Mathematical Probability,* New York, McGraw-Hill, 1937.

VILLE (J.)

[1939] *Étude critique de la notion de collectif*, thèse sci. math. Paris, (fascicule III de la Collection de monographies des probabilités, publiée sous la direction de M. Émile Borel), Paris, Gauthier-Villars, 1939.

VITALI (G.)
[*Œuvres*] *Opere,* Bologna, Ed. Cremonese, 1984.
[1905] Sul problema della misura dei gruppi di punti di une retta, Bologna, 1905, *Œuvres,* pp. 231-235.

WEIL (A.)
[*Oeuvres*] *Oeuvres Scientifiques - Collected Papers,* New York, Springer-Verlag, 3 vols, 1980.
[1940a] *L'intégration dans les groupes topologiques,* Paris, Hermann (Actualités Sci. Ind. 869), 1940, 2ième édition, (Actualités Sci. Ind. 1145), 1965.
[1940b] Calcul des probabilités, méthode axiomatique, intégration, *Revue Scientifique,* (1940), pp. 201-208.
[1991] *Souvenirs d'apprentissage,* Bâle, Birkhäuser, 1991.

WEYL (H.)
[*Oeuvres*] *Gesammelte Abhandlungen*, Berlin, Springer-Verlag, 1968.
[1909] Über die Konvergenz von Reihen, die nach Orthogonalfunktionen fortschreiten, *Math. Ann.,* 67 (1909), pp. 225-245.
[1916] Über die Gleiverteilung von Zahlen mod. Eins, *Math. Ann.,* 77 (1916), pp. 313-352.
[1927] Integralgleichungen und fastperiodischen Funktionen, *Math. Ann.,* 97 (1927), pp. 338-356.
[1934] Harmonics on homogeneous manifolds, *Ann. Math.,* 35 (1934), pp. 486-499.
[1938-1939] Mean motion I, II, *Amer. J. Math.* 60 (1938), pp. 889-896, 61 (1939), pp. 143-148.
[1952] *Symmetry,* Princeton University Press, 1952, 1983

WIENER (N.)
[*Works*] *Collected Works with Commentaries,* 4 vol., Cambridge, MIT Press, 1979-1985.
[1921a] The average of an analytical functional, *Proc. Nat. Acad. Sci.,* 7 (1921), pp. 253-260.
[1921b] The average of an analytical functional and the brownian movement, *Proc. Nat. Acad. Sci.,* 7 (1921), pp. 294-298.
[1921c] The mean of a functional of arbitrary elements, *Ann. of Math.,* 22 (1922), pp. 66-72.
[1923] Differential-Space, *J. of Math. Phys.,* (1923), p.131-174, *Oeuvres* I, pp. 455-498.
[1924] Un problème de probabilités dénombrables, *Bull. Soc. Math. France,* 11 (1924), pp. 569-578.
[1930] Generalized harmonic analysis, *Acta Math.*, 55 (1930), pp. 117-258.
[1935] Random functions, *J. of Math. Phys.,* 24, 1, (1935), pp. 17-23.
[1956] *I Am a Mathematician. The Later Life of a Prodigy,* Garden City, New York, Doubleday & Company, 1956.

WIENER (N.), ZYGMUND (A.)
[1933] Notes on random functions, *Math. Zeitschrift,* 37 (1933), pp. 647-668.

WINTNER (A.)

[1932] On the asymptotic repartition of the values of real almost periodic functions, *Amer. J. Math.,* 54 (1932), pp. 339-345.

[1934] On analytic convolutions of Bernoulli distributions, *Amer. J. Math.,* 56 (1934), pp. 659-663.

[1947] *The Fourier Transforms of Probability Distributions,* Lectures, Baltimore, The Johns Hopkins University, 1947.

YOR (M.)

[2003] Deux maîtres ès probabilités, *Gazette des mathématiciens,* Edition spéciale Laurent Schwartz (1915-2002), (2003).

[2007] Joseph Leo Doob (27 février 1910-7 juin 2004), *Gazette des mathématiciens,* 114 (2007), pp. 33-39.

YOUNG (L.-C.)

[1981] *Mathematicians and their times, History of mathematics and mathematics of history,* Amsterdam, North-Holland, 1981.

YOUNG (W. H.)

[*Œuvres*] *Selected Papers of G. C. Young and W. H. Young,* Lausanne, Presses polytechniques et universitaires romandes, 2000.

[1904] The general theory of integration, *Proc. Royal Soc. London,* 73 (1904), pp. 445-449.

[1905] On the general theory of integration, *London Phil. Trans.,* A 204 (1905), pp. 221-252.

ZDRAVKOVSKA (S.), DUREN (PP. L.)

[1993] *Golden Years of Moscow Mathematics,* History of Mathematics, Vol. 6, Amer. Math. Soc., London Math. Soc., 1993.