Journ@l Electronique d'Histoire des Probabilités et de la Statistique

Electronic Journ@l for History of Probability and Statistics

Vol 5, n°1; Juin/June 2009

www.jehps.net

How Paul Lévy saw Jean Ville and Martingales

Laurent MAZLIAK[1]

Résumé

Dans le présent article, nous examinons d'une part la manière dont Paul Lévy dans les années 1930 a fait usage de conditions du type martingales pour ses études de sommes de variables aléatoires dépendantes, et d'autre part l'attitude qu'il a eue envers Jean-André Ville et ses travaux mathématiques.

Abstract

In the present paper, we consider how Paul Lévy used martingale-type conditions for his studies on sums of dependent random variables during the 1930s. In a second part, we study Lévy's troubled relationship with Jean-André Ville and his disdain for Ville's mathematical work.

Keywords and phrases : History of probability theory, martingales, dependent random variables

AMS classification :

Primary : 01A60, 60-03

Secondary : 60G42, 60G44

Introduction

The present paper is a complement to several articles published in this issue of the Electronic Journal for History of Probability and Statistics, devoted to the history of martingales. We will give here some extra information about some actors in probabilistic history (Paul Lévy (1886-1971) and Jean-André Ville (1910-1989) in

[1]Laboratoire de Probabilités et Modèles Aléatoires et Institut de Mathématiques-Histoire des Sciences, Université Paris VI

the first place) and try to explain why they never succeeded in finding a common basis for reflection though their mathematical interest could once have ‘easily’ converged. Ville’s production is studied in several items of the present issue, and it seems natural to expose some details about Lévy’s work. Paul Lévy was one of the major figures on the probabilistic scene of the 20th Century, and his research on limit theorems for sums of dependent variables in the middle of the 1930s had considerable influence on the future martingale theory. However, Lévy was never interested in finding an independent definition for martingales, and the martingale condition always remained a technical condition for him. Added to Lévy’s personal mathematical disdain for Ville for which we will suggest some hints of explanation, this disinterest also explains why Lévy remained away from the birth of martingale theory after World War 2.

The first part of the paper is about Lévy’s important research on subjects connected to the martingale property : how he grew interested in the question, how he dealt with it, what kind of sequences of random variables satisfied the technical condition he introduced. After having briefly recalled the singular path followed by Lévy towards probability after the Great War, we will provide some information on the kind of problems he considered and their origin. In particular, we insist on the important question of the probabilistic study of continuous fractions which, from the very beginning of 20th Century (especially in Borel’s studies) had been a source of inspiration for major developments in probability. We will then describe several works by Lévy in which he introduced martingale-like conditions. More precisely, we propose a detailed presentation of chapter VIII of his seminal book [31], where Lévy collected the results obtained in the 1930s about the extension of limit theorems to dependent variables satisfying a martingale like condition. We see chapter VIII as a kind of survey of the ultimate vision of martingales Lévy kept for the remaining of his life.

The second part of the present article focuses on Lévy’s troubled relationship with Ville and tries to explain his constant misunderstanding of the significance of his work. An unfortunate combination of circumstances, added to a clumsy publication by Ville in 1936, Lévy’s taste for quick and final judgments on people and later the troubled times of the war and the Occupation, widened the gap between the two mathematicians. Lévy never had a real consideration for Ville and this fact is recurrently proved by scornful comments to be found in his correspondence with Maurice Fréchet (1878-1973). We do not know exactly to what extent this disdain had an effect on Ville - but it probably had some. We believe that the description of this complicated situation highlights some aspects of the creation of the fundamental tools of modern probability theory.

1 Lévy and the martingale condition

1.1 Lévy and his growing interest for probability

Before looking more carefully at the main topic of this paper, we want to recall some general information explaining why and how Paul Lévy, who before the Great War had never been interested in probability theory, was suddenly captivated by the subject to the point of becoming the unchallenged major French probabilist of the inter-wars period. We shall only present a sketch of this history here and suggest the interested reader consult other articles where the subject is treated more deeply (see Lévy's comments in his autobiography [33], and secondary litterature : [34], [3], [4], [37]).

The first encounter of Lévy with probabilities as a professional mathematician happened merely by chance. In 1919, Georges Humbert's illness prevented him from reading part of his lectures at the Ecole Polytechnique where he was professor of mathematical analysis. Lévy, who had been a *répétiteur* (lecturer) at the Polytechnique since 1913 (a school where he had been himself an outstanding student 12 years before), was asked to replace Humbert on the spot for some lectures. Among them were three lectures on probability theory. We luckily have the lecture notes on Lévy's first teaching on probability. They were published in 2008 in Volume 3.1 of the Electronic Journal for History of Probability and Statistics, along with the commentaries [4]. A regain of interest for teaching probability at the Polytechnique resulted from the experience of the war where some basic probabilistic techniques had been used at a very large scale. This is in particular the case of the least square method used in ballistics to improve the precision of gun firing.

Lévy's story with probability could have been limited to (rather basic) teaching questions. However, at the same moment, freed at last from the military obligations (during the war, Lévy said he had mainly worked on anti aircraft defense - see [33] pp.54-55), he was resuming his research into potential theory. The prominent figure of the probabilist somehow overshadows today that before becoming a specialist in probability theory, Levy had been a brilliant follower of Volterra and Hadamard's techniques of function of lines for the potential theory of general electric distributions. In 1911, he had defended a brilliant thesis in which he studied Green functions as functions of lines which are solutions of integro-differential equations. The paper [37] explains how after the war Lévy had been asked by Hadamard to prepare the posthumous edition of Gateaux's papers. Young French mathematician Gateaux (1889-1914) had been killed on the Front in October 1914. In the previous months, he had collected material for a thesis (also on potential theory) where he began to construct an original theory of infinite dimension integration. Hadamard's request played a major role in Lévy's evolution, when he realized that a probabilistic framework was well adapted to his problems. A letter written to Fréchet much later (on April 1945) testifies to the technology transfer operated by Lévy during those years between probability and potential analysis.

> As for myself, I learnt the first elements of probability during the spring of 1919 thanks to Carvallo (the director of studies at the Ecole Polytechnique) who asked me to hold three conferences on that topic to the students there. Besides, in three weeks, I succeded in proving new results. And never will I claim for my work in probability a date before 1919. I can even add, and I told M.Borel so, that I had not really seen before 1929 how important were the new problems implied by the theory of denumerable probabilities. But I was prepared by functional calculus to the studies of functions with an infinite number of variables and many of my ideas in functional analysis became without effort ideas which could be applied in probability.

In fact, a first trace of the probabilistic vision can be found in Lévy-Fréchet's correspondence as early as January 1919 (so even before Lévy really became involved in probability...) when Lévy wrote to Fréchet

> For example, I think to limit the oscillations and irregularities of the functions by bounding an integral I such as $\int u'^2(t)dt$, or at least by considering as «**less probable**» the functions for which I would be too large[2].

The new probabilistic oriented mind proved especially spectacular in Lévy's 1922 book [26] on functional analysis, in particular in Chapter VI devoted to the infinite dimensional sphere.

1.2 Genesis of the martingale property

The genesis of a martingale type condition in Lévy's work had already been presented by Crépel in an unpublished and only half-developed note of a seminar given in 1984 in Rennes. The present section closely follows Crépel's chronology. Moreover, it will be interesting for the reader to compare several points we shall develop in this section with the contents of the paper [15] (this issue).

As Crépel mentioned, Soviet mathematician Serguey N. Berstein (1880-1968) had studied several martingale situations during the 1920s and the beginning of the 1930s, though he had not singled out the notion as an autonomous mathematical definition. So one may ask what Lévy exactly knew about these works before he himself considered martingale situations. It is hard to have a definitive answer to such a question but we nevertheless think that S.Bernstein's influence on Lévy at that moment was quite limited. First because it was often repeated by Lévy himself that he was not very fond of reading the works of others. Certainly one must not take such an assertion for granted but in Lévy's case it seems corroborated by converging information. A striking point is that S.Bernstein's name appears only very late in Lévy's correspondence with Fréchet (at least in the letters which were found at the Paris Academy of Science, and published in [3]), contrary

[2] Our emphasis.

to other Soviet scientists such as Andrei N. Kolmogorov (1903-1987) and Aleksandr Y.Khinchin (1894-1959). The first mention of Berstein occured in 1942. Of course, the correspondence is not complete and Berstein may certainly have been quoted before. But in his letter dated 4 November 1942, Lévy explained that he asked Loève to give him a description of S.Bernstein's 1932 talk at the international congress of mathematicians in Zürich, which seems to reveal that he had at most a superficial knowledge of the paper. Crépel says that Lévy had read the paper [6], where the Soviet mathematician obtained limit theorems - in particular central limit theorems - for sequences of dependent random variables satisfying martingale-type conditions. He was besides probably encouraged to read it as it was written in French. And it is true that Lévy wrote at the very beginning of his paper [29] that S.Bernstein's paper was an *important step* in the study of sums of dependent variables. But one must certainly not overestimate the influence of the paper on Lévy. The latter is not referred to before 1935, and maybe Lévy was not acquainted with it at all before someone told him that S.Berntein had dealt with similar questions as himself. Fréchet, who read everything published, often played this role of bibliographical source for Lévy. Our hypothesis is therefore that Lévy had almost not been inspired by S.Bernstein's works when he began to consider martingales.

A first trace of Lévy's observation of the martingale condition in a primitive setting can be found in a paper written by Lévy in 1929 [28] about the decomposition of a real number in continued fractions.

Continued fractions decompositions had been studied by several analysts at the end of the 19th Century. Let us in particular mention the important works by Stieltjès (1856-1894) ([39]). In this study Stieltjès needed to introduce his generalization of Riemann's integral, later extended by Lebesgue (see [22], Epilogue pp.179 and seq). But how did continued fractions enter probability theory ? The probabilistic study of continued fractions began with Swedish astronomer Gylden (1841-1896) who was interested in describing the mean motion of planets around the sun. To approximate this motion represented by a quasi-periodical function, Gylden considered Lagrange's techniques of approximation by continued fractions (this fundamental approximation technique was developed some years later by a student of Hermite, French mathematican Henri Padé (1863-1953), is known today as Padé approximants - see [1]). A smooth (analytical) function f can be represented as

$$f(t) = a_0 + \frac{t^{n_1}}{a_1 + \frac{t_2^n}{a_2 + \dots}}.$$

Gylden was therefore led to study the structure of the decomposition in continued fractions of a real number x to which he devoted three papers dated 1888 (including 2 excerpts from letters to Hermite published by the latter as notes in the CRAS). In one of the papers, Gylden chose a probabilistic approach in which he tried to specify the probability distribution of the quotients a_n for a number x drawn at random from [0,1]. More precisely, Gylden proved that the probability of a value k for a_n is of order $1/k$.

In 1900, Gylden's colleague, Lund astronomer Ander Wiman (1865-1959) considered the problem again in [43][3] , applied to it Borel's new theory of the measure of sets, and obtained the value of the asymptotic probability for $a_n = k$ under the form $\frac{1}{\ln 2} \ln \frac{1+1/k}{1+1/k+1}$. More details on these subjects can be found in [42], pp.29-31.

Unfortunately, we do not know how Emile Borel (1871-1956) got acquainted with Wiman's work. There is no trace of a direct correspondence between Wiman and Borel. Nevertheless, one may suppose that Wiman sent his paper to Borel, maybe through Mittag-Leffler (1846-1927) who had several exchanges with Borel the same year 1900 about the interventions at the Paris International Congress.An interesting possibility may also be another member of Borel's Scandinavian contacts, the Finnish analyst Ernst Lindelöf (1870-1946). On 2 January 1904, Lindelöf wrote to Borel the following line

> One of my compatriots, M.Karl Sundman, a docent in astronomy in our university, has been in Paris for a while and studies astronomy and mathematics. He is a young man with exceptional intelligence and perspicacity who will , probably, make a name in science. Besides, he deserves already great congratulations by having dealt with the edition of Gylden's works which had been left uncompleted. In one word, this young man wish to be a member of the Société Mathématique [de France] and I hope you will accept to be his sponsor.

We have not been able to cross-check Sundman's meeting with Borel. But the young Finn may have been a firsthand informer for Borel about Wiman's works. Anyway, in his first publication devoted to probability in 1905 [7], Borel mentions that to his knowledge, Wiman's work represents the first attempt to apply his measure theory of sets to a probabilistic problem.

Borel always saw the example of continued fractions as a fundamental source of randomness. This example was particularly important in Borel's seminal 1909 publication [8] where he presented the application of denumerable probabilities to the decomposition of real numbers, both in decimal and in continued fractions developments. Borel introduced in [8] the notion of almost sure convergence and a first version of the strong law of large numbers, thus inaugurating a way of proving existence by a probability computation which became a typical feature of the Borelian reasoning. This reasoning was directly inherited from how he had introduced the measure of sets in his thesis 15 years earlier. To prove the existence of an arc of a circle on which a certain series was uniformly convergent, Borel proved that he could choose the center of such an arc in the complement of a set which he had proved to be of measure zero (see [22]). Therefore, from the very beginning of his probabilistic life, Borel used the proof that an event has probability 1 as a

[3] In fact, Wiman was second in line to revise Gylden's papers. He was preceded by another Lund astronomer, Torsten Broden, and Wiman's paper was a criticism and alternative approach to Broden's paper. See [42], p.31

proof of existence. A good example is given in section 13 of the second Chapter of [8], where Borel commented on the proof that almost every real number is *absolutely normal*. Let us recall that a number is said to be *normal* if each figure between 0 and 9 appears with a frequency 1/10 in its decimal decomposition ; it is absolutely normal if the same property is true with the d-basis decomposition (with a frequence $1/d$) for each integer d. Borel wrote

> In the present state of science, the effective determination of an absolutely normal number seems to be the most difficult problem ; it would be interesting to solve it either by building an absolutely normal number, either by proving that, among the numbers which can be effectively defined, none is absolutely normal. However paradoxical may this proposition seem, it is not the least incompatible with the fact that the probability for a number to be absolutely normal is equal to one

This kind of strange existence proof is probably the reason why, as von Plato observes ([42], p.57), the strong law of large numbers and denumerable probabilities seem to have caught mathematicians by surprise and attracted several uncomprehending comments. A vigorous reaction came in 1912 from Felix Bernstein (1878-1956) when he revisited Gylden's approach of the problem of secular perturbations in his article [5] by a systematic use of the 'measure of sets of E.Borel and H.Lebesgue' ([5], p.421)[4]. F.Bernstein contested in his paper the result obtained by Borel in [8] concerning the asymptotical order of the quotients in a continued fraction and thought he had found a contradiction with his own results. F.Bernstein wrote

> For the continued fractions, [Borel] established the following result : if one considers only quotients which have an influence on $\overline{\lim} a_n$, then their growth order is smaller than $\varphi(n)$ with denumerable probability 1 if $\sum \frac{1}{\varphi(n)}$ converges, and larger that $\varphi(n)$ if $\sum \frac{1}{\varphi(n)}$ diverges. The last part of the theorem is contained in the second part of theorem 4[5]. On the contrary, the first part is in contradiction with the result obtained in theorem 4. The reason for this contradiction is of crucial importance and we shall explain it precisely. The following fact is true : *for geometrical probabilities under consideration, the independence of the elementary cases is not realized.*

The basis of the contradiction for F.Bernstein was thus Borel's application of his (Borel-Cantelli) lemma to a non independent case. Several weeks later, Borel replied in a short paper published in the same journal [9]. He emphasized the fact that F.Bernstein's result is in no way contradictory with his own, but admitted that he did not precisely write [8] for the case of dependent variables as the quotients a_n are. Borel proposed thus a new proof. In [9] (p.579), he assumes that

[4]F.Bernstein's interest for secular perturbations had grown from a paper published by Bohl in 1909.

[5]Exposed earlier in [5]

the conditional probability p_n of the n-th event given the preceding ones satisfies $p'_n \leq p_n \leq p''_n$ where the series p'_n and p''_n have the same behavior (convergence or divergence). Borel does not give any hint of how one may obtain the two terms p'_n and p''_n. Moreover he limits the proof (of the conditional Borel(-Cantelli) lemma) to the case when p'_n and p''_n are convergent series, asserting without any comment that the proof would be the same in the divergent case (an unfortunate observation as the result is false in the non independent divergent case !). Nevertheless, one may detect in this proof (where Borel considers the evolution of the conditional means) a first use of a martingale convergence theorem. This is today used as a common tool for obtaining the conditional version of Borel-Cantelli lemma (see for instance [2], p.35). Moreover, it is not by mere chance that at the same moment, Borel revisited Poncaré's card shuffling problem in note [10] and proposed a probabilistic proof of the convergence to the uniform distribution (ergodic theorem) by consideration of the evolution of the means ; this was the first appearance of a probabilistic proof of convergence of a Markov chain, apart from Markov's original proof which remained completely unknown until much later. Besides Borel's note also remained unnoticed, and his proof was rediscovered and extended by Lévy, Hadamard, Hostinský and others at the end of the 1920s (see [14] and [36] on these subjects).

In [9], Borel underlines F.Bernstein's confusion ; for him, F.Bernstein did not understand that in the convergence case, with probability 1, the inequality $a_n \geq \varphi(n)$ stopped being true beyond a rank n *which changed with* ω.

Still more interesting is what Borel wrote in a subsequent part, when he commented on Berstein's axiom on p.419. F.Bernstein indeed explained

> When one relates the values of an experimentally measured quantity to the scale of all the reals, one can exclude in advance from the latter any set of measure 0. One should expect only such consequences of the observed events which are maintained when the observed value is changed to another one within the interval of observation.

Borel wrote ([9], pp.583-584)

> I have often thought about the same kind of considerations and, as M.Bernstein, I am convinced that the theory of measure, and especially of measure zero, is intended to play a major role in the questions of statistical mechanics.

Maybe in F.Bernstein's text Borel found a first formulation of what he called much later (in [12]) the *unique law of randomness* ; for Borel, the significance of probability is related to the events with small probability which are the only ones for which probability has a practical and objective meaning : these events have to be considered as impossible.

As said above, in his 1929 paper [28], Lévy considered continuous fractions. His general problem was to look for properties that the sequence of incomplete quotients had in common with a sequence of independent random variables. On page 190, he wrote

In an unlimited series of experiments giving probabilities $\alpha_1, \alpha_2, \ldots, \alpha_n, \ldots$ to an event A, its frequency during the first n experiments differs from the mean probability

$$\alpha'_n = \frac{\alpha_1 + \ldots + \alpha_n}{n}$$

by a quantity almost surely small for n infinite, that is to say that it converges to zero, except in cases of total probability inferior to any given positive quantity.

It must be observed that this property does not suppose the existence of a limit for α_n : it is besides of little importance that the considered probability be independent or not ; if they form a succession, every probability α_n being estimated at the moment of the experiment on the basis of the previous experiments, the theorem remains clearly true.

As seen, Lévy expressed himself in a rather loose way, proposing rather an assertion than any proof. Only several years later did he feel necessary to provide a complete proof, among a series of papers from 1934-1936 devoted to the studies of limit theorems for sequences (and series) of dependent variables. In the introduction of his paper [30] (pp.11-12), Lévy explains how he interpreted his new considerations on the strong law of large numbers as an extension of the intuition he had had in 1929.

The idea on which this research is based, first mentioned in 1929 about an application to the study of continued fractions, is that most theorems related to sequences of independent random variables may be extended to a sequence of variables in chain

$$u_1, u_2, \ldots, u_n, \ldots$$

if one takes care of introducing, for each of these variables u_n, not its *a priori* probability distribution, but the *a posteriori* distribution on which it depends when $u_1, u_2, \ldots, u_{n-1}$ are given, and which in practice characterizes the conditions of the experience which leads to the determination of u_n. It is well known that, without this precaution, the extension of the simplest asymptotical theorems is impossible ; when these *a posteriori* distributions are introduced, it becomes on the contrary easy.

The simplest application of this observation leads to think that, under slightly restrictive conditions, one obtains a good evaluation of the sum

$$S_n = u_1 + u_2 + \ldots + u_n$$

when each term u_ν is replaced, not by $\mathcal{E}\{u_\nu\}$, but by $\mathcal{E}_{\nu-1}\{u_\nu\}$. One probably will object that the so-obtained approximated value is a random variable, and does not have the practical value of an *a priori*

> evaluation. But in the calculus of probability, at least in a general theory, one cannot hope more than to specify the probable relation between the probability distribution and the result of the experiment, between the cause and the effect ; the obtained assertions could only lead to more precise conclusions in the special cases where one is able to specify how the conditions of each experiment depend on the results of the previous ones. The already mentioned application to the study of continued fractions is sufficient to justify the interest of the method.

In the same paper, in a footnote on page 13, Lévy commented on the loose presentation he provided in 1929.

> If I limited myself to a statement without proof, it was partly not to interrupt a paper devoted to continued fractions by too long a digression, and partly because, being unsure of having read all the published works on the strong law of large numbers, I thought that so simple a result may have been already known ; since then I came to the conclusion that it was a new result, and I do not think that its proof had been published before.

Crépel already mentioned that Lévy's explanation is reliable but insisted that Lévy's lack of precision must also be understood as a proof that at that moment (1929) he had not yet understood that he may formulate an independent property which would guarantee the validity of the theorem.

The martingale condition was formulated in a subsequent paper ([29]), though not at the beginning. [29] is devoted to the extension of the strong law to the case of dependent variables. In Lévy's mind, such an extension was a continuation of the theory of Markov chains.

Lévy's main tool for considering general sequences of random variables was to see them as points in the infinite-dimensional cube $[0,1]^{I\!N}$ equipped with the "Lebesgue" measure. One may recognize there a direct inheritance of Lévy's first probabilistic consideration on the infinite dimensional spaces. In [29], Lévy proves a version of a 0-1 law which is stated in the following way (p.88).

> $P(E)$ and $P_n(E)$ represent respectively the probability of an event E before the determination of the x_ν, and after the determination of $x_1, x_2, \ldots, x_n$ and as a function of these known variables. This event E depends on the indefinite sequence of the x_ν.
>
> **Lemma 1** *If an event E has a probability α, the sequences realizing this event, except in cases of probability zero, also realize the condition* $\lim_{n\to+\infty} P_n(E) = 1$.

In modern terms, one recognizes a particular case of a martingale convergence theorem asserting that if $(\mathcal{F}_n)$ is a filtration such that $\mathcal{F}_n \uparrow \mathcal{F}_\infty$ and z is a random variable, then $E(z/\mathcal{F}_n) \to E(z/\mathcal{F}_\infty)$ a.s. (the theorem is considered here with $z = 1\!\!1_E$).

Crépel quotes Loève's enthusiastic comment in [35]. For Loève, the previous lemma is the first convergence theorem of martingales and *perhaps one of the most beautiful results of probability theory*. Lévy also made comments later on the result (in [33], p.93). He wrote

> This theorem has an important particular case. If α_n is independent of n, and so equal to the *a priori* probability $\alpha = \alpha_0$ of the event E, α is equal to zero or one (otherwise $\alpha_n = \alpha$ could not tend towards one of these possible limits). It is Kolmogorov's theorem of zero-one alternative. It is anterior to my 1934 work, but I did not know it when I wrote this paper, which appeared in 1935.

Lévy's comment is confirmed by what he wrote to Fréchet about the same result in January 1936, when they discussed together Kolmogorov's measure-theoretic proof of the 0-1 law in [23]

> [Kolmogorov's] proof is very simple and correct. One must get rid of the impression that it is a conjuring trick. It uses the following essential notion : the probability of the unlimited sequence of the x_ν cannot be considered well defined unless it appears as the limit (in the sense of convergence in probability) of the probability of a property of the set of the first n variables - which implies the studied property with a probability close to one, if it is realized for very large n. The desired consequence is immediate. My own proof, I think, better highlights these ideas. But one can feel them implicitly in Kolomogorov's.

On Kolmogorov's axiomatic version of probabilities, and in particular his proof of the 0-1 law, and the connection with Lévy's vision, see [38].

The first appearance of an explicit martingale condition is placed later in the paper under the name *Condition ($\mathcal{C}$)*. It is stated on page 93 as

$$(\mathcal{C}) E_{n-1}(u_n) = 0.$$

It is unclear what Lévy had in mind with this letter 'C'. Maybe 'centered', maybe 'convergence', maybe simply 'condition'.

As a main use of condition ($\mathcal{C}$), Lévy proposes the following theorem which can be seen as an extension of Kolmogorov's theorem for the independent case.

Theorem 1 *If the sequence (u_n) satisfies condition ($\mathcal{C}$) and is uniformly bounded by a number U, then $\sum u_n$ and $\sum E_{n-1}(u_n)^2$ have the same nature (convergent or divergent) with probability 1.*

In Hostinský's recension of the paper for the Zentrablatt, the Czech mathematician alluded to this result under the condition that the *probable value of u_n, evaluated when one knows $u_1, u_2, \ldots, u_{n-1}$ in equal to zero.*

What was the genesis of such a condition ? Unfortunately, the years when Lévy formulated it are precisely those when the major gap in Lévy-Fréchet's correspondence is found, between 1931 and 1936 ! However, it is seen that at that time

Lévy was looking for extensions of limit theorems to more general cases than independent sequences. He was therefore led to put a condition on the general term u_n of the series to guarantee the convergence. The condition is stated on this general term and was never seen by Lévy as a property of the sequence of partial sums S_n. Lévy always kept this opinion and never considered a martingale-like property as a property of a sequence of random variables (see below).

1.3 Chapter 8 of the book *Théorie de l'addition des variables aléatoires*

Lévy's most famous book [31] was published in 1937 and was mostly completed during Summer 1936. It played an important role in making several fundamental tools of modern probability theory known (such as Lévy-Khinchin's decomposition formula) and is now considered a classic. We may observe that Lévy himself was probably convinced of the particular importance of the results he had obtained between 1934 and 1936 about the behavior of the sums of random variables. This could explain why he decided so quickly to collect them in a book. It is not impossible that his meeting with Doeblin (Lévy first met him during Spring 1936) influenced him. It is known that Doeblin made great impression on the rather scarcely accessible Lévy (on Doeblin's beginnings in probability see [13] and [36]). And in a letter to Fréchet ([3], 21 December 1936), Lévy mentioned that he prepared for 21-years-old Doeblin a copy of the manuscript.

The eighth chapter of [31] is called *Various questions related to sums of variables in chain*. Lévy himself presents it in a footnote as a collection of questions studied in previous chapters for the case of independent variables and taken again in that chapter but for 'chained' (dependent) variables. The chapter collects the results obtained by Lévy in previous years about the extension of limit theorems to dependent variables and remained probably for him the vision of martingales he accepted. It is therefore interesting to give a more detailed description to understand this ultimate vision. We shall now present a quick survey of Chapter VIII of [31]. Basically, our aim is to emphasize two main ideas, already mentioned above. First for Lévy the (martingale) condition he introduced was nothing but a technical condition on the general term of a series which could allow the extension of the classical limit theorems. Lévy never considered martingales as a property related to the sequence itself. Second, Chapter VIII of the book [31] was probably seen by Lévy as a kind of conclusion to his research in the direction of the series of random variables. And this also may explain why he did not later feel really concerned with the way Ville and Doob began a full theory of martingales.

1.3.1 Representation of a sequence of dependent variables

Lévy begins Chapter VIII by explaining what is for him the *General problem of chained probability* (section 64, page 225). In general, 'chained probability' is a term covering any sequence of (dependent) random variables $X_1, X_2, \ldots, X_n, \ldots$

and Lévy wants to explain how the distribution of the sequence may be constructed. The main tool, Lévy explains, is to obtain a representation of the following kind : $X_n = G_n(Y_1, Y_2, \ldots, Y_n)$ where (Y_n) is a sequence of independent random variables with uniform distribution on [0,1]. The Y_n may be defined as $Y_n = F_n(X_1, X_2, \ldots, X_n)$ where $F_n(X_1, X_2, \ldots, X_{n-1}, z)$ is the distribution function of the conditional distribution of X_n when $X_1, X_2, \ldots, X_{n-1}$ are given.

1.3.2 Markov Chains

In section 65 (p.227), Lévy concentrates on the most important case, Markov chains. After having presented the Chapman-Smoluchowski equations describing the evolution of the transition probabilities, Lévy provides interesting considerations for justifying the importance of the Markovian situation. There are, Lévy writes, situations in Physics where one is not able to know all the parameters defining the state of a system. One has to deal with the 'apparent' parameters and to neglect the 'hidden' parameters. Of that kind are two particularly important situations.

The first one is when the knowledge of the past compensates for the ignorance of the present values of the hidden parameters, and hence allows to predict the future. This is the theory of *hereditary phenomena* developed by Volterra, for whom the analytical tool is given by integro-differential equations. The second one is when only the present value of the (apparent) parameters is known. One then cannot do better than describe the probabilities of the future states (as a simple example, Lévy quotes gambling systems). For this situation, the natural analytical tool is Markov chains for which the Huygens principle (the principle asserting that for given times $t_0 < t_1 < t_2$, one can equivalently determine the situation at time t_2 by looking at the direct evolution from t_0 to t_2 or by looking first at the evolution from t_0 to t_1 and then from t_1 to t_2) is expressed by the Chapman-Smoluchowski equations. Lévy's connection between Volterra's theory and Markov chains is a direct interpretation of the early story of Markov chains at the end of the 1920s, and in particular of Hostinský's considerations. It is indeed probably from his studies on Volterra's integro-differential equations that Hostinský was led to propose a first model of Markov chain with continuous state in 1928 (on Hostinský's beginnings in probability, see in particular [21]). Lévy then develops the classical historical model of cards shuffling proposed by Hadamard for the description of the mixing of two liquids, and subsequently studied by Poincaré, Borel and Hostinský. It has already been mentioned that Lévy had also considered this model in his 1925 book, but without connecting it to a general situation (see [14] and the letters from November 1928 in [3]). Lévy takes advantage of his new book to develop the proof of convergence towards uniform distribution of the cards (ergodic principle) which was only sketched in [27] (Lévy had already written down the proof earlier on Fréchet's request - see Letters 18 and 19 in [3]).

1.3.3 The 'martingale' condition

After this long introduction about Markov chains, Lévy presents section 66 whose title is *extension of Bernoulli theorem and of Chebyshev's method to sums of chained variables*. Lévy begins by looking for conditions under which the variance of the sum S_n of centered random variables is equal to the sum of variances. It suffices, Lévy writes, that $\mathcal{M}'(X_j)$ equals 0 for each $i < j$ where $\mathcal{M}'(X_j)$ is the probable value of X_j when X_i is known (conditional expectation) . This is obviously implied by the more restrictive hypothesis

$$(\mathcal{C}) \qquad \mathcal{M}_{\nu-1}(X_\nu) = 0, \nu = 1, 2, 3, \dots$$

where $\mathcal{M}_i$ is *the probable value calculated as a function of* $X_1, X_2, \dots, X_i$ *supposed given*. And Lévy adds : *This hypothesis will play a major role in the sequel.* If X_n does not satisfy $\mathcal{C}$, one can consider the new sequence $Y_n = X_n - \mathcal{M}_{n-1}(X_n)$. In the same way, writing

$$S_n - \mathcal{M}(S_n) = \sum_1^n (\mathcal{M}_\nu(S_n) - \mathcal{M}_{\nu-1}(S_n)),$$

allows to control the approximation of S_n by $\mathcal{M}(S_n)$ with an error of order $\sqrt{n}$ when the *influence of the ν-th experiment is small on the n-th experiment when $n - \nu$ is large (for instance when $\sum_{h=0}^{p} \mathcal{M}_\nu(X_{\nu+h}) - \mathcal{M}_{\nu-1}(X_{\nu+h})$ is bounded independently of ν and p).*

1.3.4 Consequences of condition $(\mathcal{C})$: Central Limit theorem

Section 67 is devoted to the central limit theorem for sums of dependent variables. The proof is presented as an extension of Lindeberg's method for random variables which are *small with respect to the dispersion of their sum*. Apart from $(\mathcal{C})$, Lévy first introduces two more hypotheses

$$(\mathcal{C}_1) \qquad \mathcal{M}_{\nu-1}(X_\nu^2) = \sigma_\nu^2 = \mathcal{M}(X_\nu^2)$$

$$(\mathcal{C}') \qquad |\, X_\nu \,| < \varepsilon b_n, \text{ where } b_n^2 = \sum_{i=1}^{n} \sigma_\nu^2.$$

Lévy observes that hypothesis $(\mathcal{C}_1)$ implies that the conditional expectation of X_ν^2 is not dependent on $X_1, X_2, \dots, X_{\nu-1}$. Under these hypotheses, Lévy proves that

$$P(\frac{S_n}{b_n} < x) \to \frac{1}{\sqrt{2\pi}} \int_{-\infty}^{x} e^{-u^2/2} du,$$

along the lines of Lindeberg's proof. In a second part of the section (p.242), he proposes to weaken condition $(\mathcal{C}_1)$, and to replace it by the requirement that the probability of divergence of $\sum \sigma_\nu^2$ be positive.

The section 68 is devoted to the general problem of convergence of series with non independent terms. As Lévy stipulates, the *essential hypothesis is that condition* $(\mathcal{C})$ *is satisfied* and the second moments of X_ν are finite. Lévy begins by showing that Kolmogorov's inequality can be extended to that case, which allows him to prove that the series $\sum X_\nu$ and $\mathcal{M}_{\nu-1}(X_\nu^2)$ have the same behaviour. This in particular proves the conditional generalization of the Borel-Cantelli lemma (called by Lévy *the lemma of M.Borel*). Sections 69 to 72 are devoted to the extension of the strong law of large numbers and of the law of the iterated logarithm. These parts are quite technical and we shall not enter into details. Let us only note that Lévy's approach is always the same : extending former results (generally Khinchin's and Kolmogorov's) under condition $(\mathcal{C})$.

2 Lévy versus Ville

The second part of our paper is devoted to the complicated relationship between Lévy and Ville. When one has a look at the *index nominum* of the Lévy-Fréchet correspondence [3], it is surprising to see that Ville's name appears many times in the letters. It is quoted 13 times, first in 1936 (in a letter following the aforementioned letter of December 1936 where Doeblin is mentioned for the first time) and eventually in 1964. However, and quite impressively, when one looks at these quotations one after the other, one can observe that Ville's name is almost always associated with criticisms, being even sometimes rather derogatory remarks. It is well known that Lévy was a scathing person who never hesitated to show disdain for works he considered uninteresting or without originality. But in his letters to Fréchet he recurrently expressed particular negativity towards Ville.

It is interesting to have first a closer look at the last letter in which Ville is quoted. It was written on 28 April 1964, at a moment when Lévy had just conquered a long desired seat at the Paris Academy of Science (at the age of 78) where he succeeded to the almost centenarian Hadamard. The tortuous story of Fréchet and Lévy's elections to the Academy can be followed in details in [3]. As may be imagined, one of the most urgent tasks of a new Academician is to think about future candidates to replace the next dead Immortal and Lévy's letter probably responds to Fréchet's suggestion to take into consideration a possible application from Ville.

> I have never understood Ville's first definition of the collectives ; Loève and Khinchin had told me and written to me they had not understood either. It is in 1950, in Berkeley, that I learnt from Loève that the processes called martingales are those I had considered as early as 1935 ; after your letter, his second definition, p.99, coincides with mine at least by adding constants.
>
> Naturally, I did not use a word that I did not know in 1937 in the 1954 re-edition of my 1937 book ; in order to allow the photographic reproduction, I had only corrected some mistakes and added two notes.

But condition $\mathcal{C}$, introduced p.238, means that the sequence of X_ν is a martingale. This condition appears in the sequel : theorems 67,1 ;67,2 ; 67,3 ;68 ; n.69 1° and 2°. I have therefore sketched a theory, developed afterwards by Doob, and which generalizes the sequences of independent random variables with probable values equal to zero.

As for the theory of collectives, despite all the credits I attribute to von Mises, I have always found it absurd, and I did not hide this from Wald when he presented it in Geneva. I am grateful to Ville for having helped me fighting this theory. But it is not sufficient to place him at the same level as... say Fortet and Dugué, to speak only about the probabilists from the Sorbonne.

From the last sentence, it seems that for Lévy anyone could have been preferable to Ville for the election at the Academy. And the way he insists on quoting all the theorems from Chapter VIII of [31] where the condition $\mathcal{C}$ was used is probably a sign of irritation against what may have seem to him Ville's undue claim of having constructed a new mathematical concept. Lévy's assertion that it was only in 1950 that he learnt about the theory of martingales is probably true (though he was present in Lyon in 1948 and listened to Doob's conference - but maybe the language made difficulties for him to understand it[6]. Lévy had never been a great reader and often selected only papers that were in connection with his present research. However, as the word had been introduced by Ville in the 1930s, his observation also sounds as a renewed proof of disinterest for Ville's contribution. Besides, there is irony in seeing Lévy going astray with the definition of martingale when he mentions that the sequence X_ν is a martingale and not the sequence of the partial sums. We have already observed in the previous section that Lévy had never considered the property otherwise than a technical property on the general term of a series which can allow the extension of limit theorems. The small confusion here is probably related to this fact.

Lévy's first comments on Ville in his correspondence with Fréchet happened in 1936. The name was quoted for the first time on 23 December, but most of the previous letter on December 21 is devoted to demeaning comments on a note by Ville presented to the Academy of Science by Borel on 14 December 1936 [40]. The title of the note is *On the convergence of the median of the first n results of an infinite sequence of independent trials*. It was Ville's third note that year (all presented by Borel) but the two other concerned Ville's studies of collectives. It is not clear why Ville decided to publish this relatively elementary results. That Borel presented it is not so surprising as Borel's opinion on Ville was very positive ; Ville had been a brilliant student at the Ecole Normale Supérieure, and anyway Borel seems never to have been very particular about the notes he transmitted to the Academy. Besides knowing when Ville became closely associated with Borel is an interesting question. Ville claimed later he had been writing up Borel's lectures on games in October 1937 when Fréchet wanted him to go to Geneva ;

[6]On Doob's 1948 conference in Lyon, see Bernard Locker's comments, along with the original text, in this issue.

Doeblin went instead. Perhaps it was during the winter term of 1936-1937 that Borel gave the course and Ville was taking notes. The lectures were published as [11]. But one may ask whether Ville asked Fréchet's opinion about his project. The results Ville obtained could be seen as a consequence of Glivenko-Cantelli's theorem on the uniform convergence of the empirical distribution functions. This theorem had been stated and published in 1933 in an issue of the Italian journal of actuaries (*Giornale italiano degli attuari*, whose director was Cantelli). Besides, the issue in question contained three independent papers with the result, by Cantelli, Glivenko and Kolmogorov (who was surprisingly forgotten when naming the theorem). A striking fact is that the three papers [16], [20] and [24] had the same title 'On the empirical determination of a probability distribution'. In 1936, the result was well known among probabilists and statisticians. Fréchet devoted to the theorem two pages of his volume [19] published the same year. Ville knew Fréchet's book : he mentioned it as a reference for (Kolmogorov) strong law of large numbers at the beginning of [40]. It is very likely that he did not make the connection between his result and Glivenko-Cantelli theorem. Ville had learnt probability with Fréchet at the beginning of the 1930s. It is possible that he failed to realize that there were new topics in [19]. Besides, after two years abroad in Berlin (1933-34) and then in Vienna (1934-35), and, back in Paris, his interests for collectives and game theory in the years 1935-37 had marginalized Ville in the small group dealing with Markov chains around Fréchet at the IHP, where Doeblin became the leader. So, it is not obvious that Fréchet paid much attention to what Ville was doing, and his attempt to support Ville possibly resulted from his conscientiousness about doctorate students, and from a kind of tradition of inter-generational solidarity at the Ecole Normale. Reading Lévy's letter on 21 December 1936, it seems that Fréchet tried to justify Ville's submission to the Academy but Lévy's reaction was rather contemptuous.

> Let me come back to yesterday's talk. It is certain that one can sometimes find important and easy theorems that escaped former scientists and saying that a theorem is easy does not mean condemning it. But, when is under consideration a particular case in a general problem solved a long time ago, except in some difficult particular cases which have been recently studied, I frankly think it would be quite ridiculous to look for a particular case of the classical theorem to build that very case up. (...) Such is the case of the median. (...) The role of the median has been elucidated for a long time ; it is an obvious consequence of Borel and Cantelli's results.

Fréchet immediately answered on December 22, probably trying once again to milden Lévy's opinion. But in a new letter on December 23, extended by a kind of post-scriptum on December 24, Lévy drove a point home. First, he wrote a complete elementary proof of Ville's result (based on the Glivenko-Cantelli theorem about which he referred Fréchet to his own book [19]). Second, he took the opportunity to expose his vision of mathematics and explained how different it was from Fréchet's vision in not so agreeable a tone. In the post-scriptum, he wrote

> In the case under consideration, I see only two fundamental ideas : the uniform convergence, which is well known ; and the strong law of large numbers of Borel Cantelli. Once admitted these two points, all the theorems of Polya Glivenko Cantelli and Ville do not seem to me to overpass what Darmois proposes to his students as an examination test for the *licence*[7].

The subject was closed with this letter, but it certainly convinced Ville not to go forward in that direction, and persuaded Lévy, who liked to have a definite opinion on people (think about the difficult relationship he entertained with Bachelier), that Ville was a dull mathematician. Let us observe moreover that Ville was particularly unlucky with the (unexpected) confrontation with Lévy about the median at the precise moment when the latter was brilliantly making use of it to prove convergence results for sums of random variables.

Though in his 1964 letter (see above) Lévy wrote that he was grateful to Ville for having fought von Mises collectives, there remains some doubts that he really had read Ville's thesis. When he was interviewed by Crépel in 1984 (see [18])

> [Ville said that] Paul Lévy had not read his thesis. 'I don't read' he told Ville. Aside from his aversion to reading other mathematicians, Lévy was displeased that Ville's thesis had been printed by the *Rendiconti del Circolo Matematico di Palermo*. 'You had your thesis printed by the fascists' he objected. 'I didn't have any money', Ville responded.

The last part of Ville's remembrance must not be overinterpreted and, if it is true, it is probably related to the particular situation in 1939 with the outbreak of WW2 . It does not seem that in the 1920s Lévy had harbored an open hostility against Mussolini's regime. Ironically, when he was in semi-clandestinity during the war, he found, with other Jews, a relative security in the Italian occupation zone in France.

Anyway, it is true that he never explicitly mentioned Ville's thesis in his letters to Fréchet. Only in his (late) letter from 28 April 1964 did he write that he had never understood Ville's first definition of collectives - which is in Ville's thesis - but knowing about one definition is not a real proof of having read anything else in the thesis. Therefore, when he wrote that Doob extended *his* theory of martingales, Lévy probably honestly thought that Ville had not substantially modified the notion. However, as we have noticed before, Lévy never considered the martingale property as an intrinsic property of a sequence of random variables. And it is initially in Ville that Doob found his future ideas on martingales.

Ville proposed two definitions of a martingale in his thesis. The first one stipulates that a process with binary outcomes is a non-negative capital process. The second is more mathematical and concerns a sequence of functions $s_n(X_1, \ldots, X_n)$ of a sequence of (dependent) random variables. It is related to Lévy's condition $(\mathcal{C})$ in the following way : the sequence (s_n) is a martingale in Ville's sense if

[7]This means for their graduation.

$s_n(X_1, X_2, \ldots, X_n) - s_{n-1}(X_1, X_2, \ldots, X_{n-1})$ satisfies Lévy's condition. It is probably the second part of Chapter V of Ville's thesis which caught Doob's interest above all. Here Ville generalized his second definition of martingales to continuous time, adopting Kolmogorov's definition of conditional expectation and trying to prove the gambler's ruin inequality in the framework of Doob's 1937 paper on stochastic processes with a continuous parameter. Though Ville failed because he tried to use as probability space the outsize set of all functions of time instead of the topologically suitable set of continuous functions, he gave Doob a fundamental new tool.

It is remarkable that Lévy kept in touch with Ville during the Occupation period, when he lived near Grenoble. Probably, if Lévy had a bad opinion about Ville, the latter had on the contrary a great admiration for Lévy and wished to stay in contact with him. However, he had a second scientific misfortune with Lévy, this time about the recurrence property of Brownian motion. Ville published in 1942 a note to the Comptes-Rendus on the subject ([41]) and was preparing a related paper when he was informed by Fréchet that Lévy had already published some of his results in his great 1940 memoir to the AMS about Brownian motion [32]. Ville decided in 1943 to withdraw his own paper (maybe also because he knew that Lévy could not submit any paper at the time because of Vichy racial laws).

All this did not help Lévy change his opinion on Ville as a poor mathematician, but maybe made him feel some sympathy for the young man. He considered him a serious and capable reader of his papers. In the long letter Lévy wrote to Fréchet on September 27, 1943, Lévy mentioned that he would be happy to learn that Ville would examine his new manuscript about random derivatives. Lévy wrote

> If you had the impression that I had little admiration for his works (and actually they never seem very original to me, he is above all a good pupil) I realize that he is very serious, has a great sense of rigor and deeply knows the questions he deals with. I shall put my complete trust in him.

In fact, Fréchet chose Loève for the work, maybe for safety reasons because he was concerned about Lévy's difficult character. And, after the Liberation, Lévy returned to his former disdain. In the first letter we have (12 March 1945), Lévy again explains to Fréchet that Ville's 1936 note on the medians was not original. However, this time, Lévy had made a mistake, probably because he wrongly remembered Ville's note. A week before, he had copied on a sheet of paper a theorem from [31] (theorem 43.2 which says that if S_n is a sequence of random variables converging in probability to S, then any converging sequence of medians of S_n converges to a median of S) ; Lévy asserted that Ville's result was a direct consequence of this theorem. However, this consequence was only indirect because Ville considered empirical medians, a fact Lévy was besides well aware of in 1936. This was probably what Fréchet had replied to him. At the end of the letter, Fréchet had written with a pencil : *Replied on March 5 that it is a different theorem from Ville's*. Nevertheless, Lévy, made the following not-so-kind comment

> I was amazed when I received your letter. I always made a confusion about Ville's result you mentioned to me in 1936 when it was published. I am sorry about that, but it does not change a lot my opinion on the lack of originality of this note. The strong law of large numbers (...) had been known for a long time (1917 or even 1909). (...) Moreover, in my theorem 43.2, it is of little importance that the distributions be theoretical probability distributions or empirical ones. (...)Taking into account the strong law of large numbers, Ville's result appears therefore as an application of my theorem 43.2. Obviously, I cannot blame Ville for not having known my book at the time when I was correcting the proofs. But my theorem 43,2 has always been, in my opinion, an obvious observation that I have explicitly stated only because I needed it. In the same way, Ville's theorem is for me only an obvious corollary of the strong law of large numbers.

That was still not enough and two years later (on 20 August 1947), the subject came back and Lévy expressed that he was really fed up. He wrote to Fréchet : *Let me frankly tell you that there are details to which I cannot give as much importance as you do*. Later he added

> I sometimes make the mistake of not making clear results which seem obvious to me but are not for others. I have also skipped several priorities which I am not in a position to claim afterwards. In the case under consideration, the only thing I told you is that I had known Ville's result for a long time. But, due to the fact that it is an obvious corollary to Glivenko-Cantelli's result, I did not claim to take any pride in it, or to call it 'my theorem'.

This letter seemed to have completed the discussion, and (if we consider the set of Lévy's letters to Fréchet reasonably complete up to 1965), it was the last time Ville was quoted in the correspondence except the 1964 letter mentioned at the beginning of this section.

The above comments make us conclude that Lévy had only a superficial knowledge of Ville's works, including his thesis. He never set much value to the new approach proposed by Ville and remained convinced that his Chapter VIII of [31] was the ultimate knowledge on 'martingales' before Doob extended it.

Conclusion

As we wrote in the introduction, we do not know exactly how Ville faced Lévy's lack of interest, but it probably played a part in his choice to leave university after WW2 and to begin a career in industry (see Ville's biography by Glenn Shafer in this issue). In fact, this disinterest was only one element among others and Ville had become a perfect outsider in the mathematical community. When he returned from captivity in Germany, Ville mostly turned towards mathematical statistics. It was in particular the theme of his Peccot lectures in 1942-43. Then a professorship of probability in Bordeaux was amazingly offered to Pisot, though Ville was the leading probabilist of the place. A few months later, Malécot was preferred to him for the position in Lyon. Malécot, a typical follower of Darmois' methods in statistics - a recycling of the British methodology (Pearson, Udny Yule) - applied to biology (see [17], [25]) was obviously supported by the latter. A small scandal happened because Malécot was closely related to Lyon (he was Eyraud's son-in-law) and the university had to face an accusation of localism. Joseph Pérès wrote an ambiguous report for the national committee judging the case, supporting Ville but recommending to allow Lyon university to be free for the choice. The choice of Malécot was confirmed.

A possible interpretation of Fréchet's insistance on Ville in the letters with Lévy during and after the war is maybe precisely that he tried to obtain at least a small support from Lévy for Ville who needed to obtain an academic position. Anyway, the support never came and Lévy, after his bad judgment on Ville's note [40] never changed his opinion. In particular, he was not interested in Ville's thesis and did not pay much attention to his introduction of a new category of random processes called martingales. Lévy later claimed that it was only in the 1950s, when he went to USA, that he learnt by chance from Loève that Doob had devised a theory for this kind of processes. Lévy's disinterest was nevertheless not only due to his bad opinion on Ville. A deeper reason was certainly that he was convinced of having presented in [31] (especially, Chapter VIII with its condition $(\mathcal{C})$) a rather complete version of how these processes could be defined and studied. Lévy never had the idea of considering 'martingales' which were not successive sums of random variables , because his basic interest was to study extensions of the law of large numbers and central limit theorem. It is thus true that he was not seduced by Ville, but he was not really seduced by Doob either, though he later admitted that Doob's methods had proven more powerful than his own. Had Lévy studied with more care and attention what Ville had proposed, maybe some martingale techniques would have arrived sooner in France after WW2 and under a different shape. This may be a good subject for an alternate history study.

Références

[1] BAKER, George A. & GRAVES-MORRIS, Peter R. : Padé approximants, Cambridge University Press, 1996

[2] BALDI, Paolo, MAZLIAK, Laurent & PRIOURET, Pierre : Martingales and Markov Chains, Chapman & Hall/CRC, 2002

[3] BARBUT, Marc, LOCKER, Bernard & MAZLIAK, Laurent : *Paul Lévy - Maurice Fréchet : 50 ans de correspondance mathématique*, Hermann, Paris, 2004.

[4] BARBUT, Marc & MAZLIAK, Laurent : Commentary on Lévy's lecture notes to the Ecole Polytechnique (1919), *Electronic Journal for History of Probability and Statistics*, Vol.4, 1, 2008

[5] BERNSTEIN, Felix : Über eine Anwendung der Mengenlehre auf ein aus der Theorie des säkularen Störungen herrührendes Problem, Math.Ann., 71, 417-439, 1912

[6] BERNSTEIN, Serge : Sur l'extension du théorème limite du calcul des probabilités aux sommes de quantités dépendantes, Math.Ann, 97, 1-59, 1927

[7] BOREL, Emile : Remarques sur certaines questions de probabilités, Bull. SMF, 33, 123-128, 1905

[8] BOREL, Emile : Les probabilités dénombrables et leurs applications arithmétiques, Rend. Circ. Palermo **27** , 247–271 (1909)

[9] BOREL, Emile : Sur un problème de probabilités relatif aux fractions continues, Math.Annalen, 72, 578-584, 1912

[10] BOREL, Emile : Sur le battage des cartes, CRAS, 154, 23-25, 1912

[11] BOREL, Emile : Applications aux jeux de hasard (J.Ville, rédacteur). Traité du calcul des probabilités et de ses applications par E.Borel, t.VI, fasc.II, Gauthier-Villars ,1938

[12] BOREL, Emile : Valeur pratique et philosophie des probabilités, Traité du calcul des probabilités et leurs applications (Emile Borel, editor), Gauthier-Villars, 1939

[13] BRU, Bernard : Doeblin's life and work from his correspondence, in *Doeblin and Modern Probability*, H.Cohn (editor), American Mathematical Society, 1-64, 1993

[14] BRU, Bernard : Souvenirs de Bologne, *Jour.Soc.Fr.Stat*, 144, 135-226, 2003

[15] BRU, Bernard & EID, Salah : Jessen's theorem and Lévy's lemma, a correspondence. Jehps, this issue. 2009

[16] CANTELLI, Francesco Paolo : Sulla determinazione empirica delle leggi di probabilità, Giornale Ist.Ital.Attuari, 4, 421-424, 1933

[17] CATELLIER, Rémi & MAZLIAK, Laurent : The emergence of statistics, Preprint. 2009

[18] CREPEL, Pierre : Jean Ville's recollections in 1984 and 1985, concerning his work on martingales, reported by Pierre Crépel. Translation from the French by G.Shafer. Jehps, this issue.

[19] FRECHET, Maurice : Recherches théoriques modernes sur le calcul des probabilités, Livre I. Traité du calcul des probabilités et de ses applications par E.Borel, t.I, fasc.III, Gauthier-Villars ,1937

[20] GLIVENKO, Valerij I. : Sulla determinazione empirica delle leggi di probabilità, Giornale Ist.Ital.Attuari, 4, 92-99, 1933

[21] HAVLOVA, Veronika, MAZLIAK, Laurent & ŠIŠMA, Pavel : Le début des relations mathématiques franco-tchécoslovaques vu à travers la correspondance Hostinský-Fréchet, *Electronic Journal for History of Probability and Statistics*, Vol.1, 1, 2005

[22] HAWKINS, Thomas : Lebesgue's theory, AMS Chelsea Publishing, 1970

[23] KOLMOGOROV, Andrei. N. : Grundbegriffe der Warscheinlichkeitsrechnung, Springer, 1933

[24] KOLMOGOROV, Andrei. N. : Sulla determinazione empirica delle leggi di probabilità, Giornale Ist.Ital.Attuari, 4, 83-91, 1933

[25] LELOUP, Juliette : Les thèses de mathématiques en France dans l'entre-deux-guerres, Thèse d'Université, Université Paris 6, 2009

[26] LEVY, Paul : Leçons d'Analyse Fonctionnelle, Gauthier-Villars, 1922

[27] LEVY, Paul : Calcul des probabilités, Gauthier-Villars, 1925

[28] LEVY, Paul : Sur les lois de probabilité dont dépendent les quotients complets et incomplets d'une fraction continue, Bull.SMF, 57, 178-194, 1929

[29] LEVY, Paul : Propriétés asymptotiques des sommes de variables aléatoires enchaînées, Bull.Sci.Math, 59, 84-96 and 109-128, 1935

[30] LEVY, Paul : La loi forte des grands nombres pour les variables aléatoires enchaînées, J.Math.Pures et Appl., 15, 11-24, 1936

[31] LEVY, Paul : Théorie de l'Addition des Variables aléatoires, Gauthier-Villars, 1937

[32] LEVY, Paul : Le mouvement brownien plan, AMS Journal, 62, 487-550, 1940

[33] LEVY, Paul : Quelques aspects de la pensée d'un mathématicien, Blanchard, 1970

[34] LOCKER, Bernard : Paul Lévy, la période de guerre. Thèse d'Université, Université Paris V, 2001

[35] LOEVE, Michel : Paul Lévy, 1886-1971, Annals Proba., 1,1, 1-18, 1973

[36] MAZLIAK, Laurent : On the exchanges between Wolfgang Doeblin and Bohuslav Hostinský, Revue Hist.Math, 13, 155-180, 2008

[37] MAZLIAK, Laurent : Les fantômes de l'Ecole Normale : Vie et destin de René Gateaux, *in* Catherine Goldstein and Laurent Mazliak (Eds) : Trajectoires de mathématiciens français autour de la Première Guerre Mondiale. To appear, 2009

[38] SHAFER Glenn & VOVK, Vladimir : Kolmogorov's contributions to the foundations of probability, Problems of Information Transmission, 39, 21-31, 2003

[39] STIELTJES Thomas-Joannes : Recherches sur les fractions continues, Ann.Toulouse, 8, J1-J122, 1894

[40] VILLE, Jean-André : Sur la convergence des médianes des n premiers résultats d'une suite infinie d'épreuves indépendantes, CRAS, 203, 1309-1310, 1936

[41] VILLE, Jean-André : Sur un problème de géométrie suggéré par l'étude du mouvement brownien, CRAS, 215, 51-52, 1942

[42] VON PLATO, Jan : *Creating Modern Probability*, Cambridge University Press, 1994

[43] WIMAN, Anders : Über eine Wahrscheinlichkeitsaufgabe bei Kettenbruchentwickelungen, Stockh. Öfv. 57, 829-841 , 1900