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Borel and the St. Petersburg Martingale

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Translated from the French by Maryse Dansereau[ii]

Abstract

This paper examines—by means of the example of the St. Petersburg paradox—how Borel exposited the science of his day. The first part sketches the singular place of popularization in Borel's work. The two parts that follow give a chronological presentation of Borel's contributions to the St. Petersburg paradox, contributions that evolved over a period of more than fifty years. These show how Borel attacked the problem by positioning it in a long—and scientifically very rich—meditation on the paradox of martingales, those systems of play that purport to make a gambler tossing a coin rich. Borel gave an original solution to this problem, anticipating the fundamental equality of the nascent mathematical theory of martingales. The paradoxical role played by Félix Le Dantec in the development of Borel's thought on these themes is highlighted. An appendix recasts Borel's martingales in modern terms.

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[ii]Editors' note: In consultation with the authors, we have silently corrected some minor errors that appeared in the original, and we have slightly updated the bibliography.

1 Borelian popularization

Émile Borel (1871–1956) is well known for his measure and his measurable sets, for the so-called Borel-Lebesgue (1894) property, perhaps also for the order, regular growth and zeros of integer functions (1896–1900), for divergent series (1896–1901), for his contributions to approximation theory (1905) or for his theory of monogenic functions (1894–1912), and of course for denumerable probabilities (1909). But it is sometimes thought that he abandoned mathematical research starting in 1914 to devote himself completely to his political and administrative career (he was general secretary of the Government, minister, deputy, member of the National Assembly's most important committees, director of the Henri Poincaré Institute, etc.) and to editing popular and pedagogical works, especially his huge 18-fascicle *Traité du calcul des probabilités et de ses applications*[iii] (1924–1939). In fact, Borel continued to publish mathematical work throughout his university career at the Sorbonne, where he was professor from 1904 to 1940, and after his retirement. Freed from most of his national and local mandates and from his university responsibilities after the second World War, Borel published from 1946 to 1953 almost forty articles or books, some astonishingly original.

Questions of style

Borel had, it is true, a unique failing. He found ingenious ways of concealing his most interesting ideas in publications where no one expects to find them and in forms so peculiar that one at first does not recognize them and may even wonder whether he is fully aware of their potential. Consider for example his remarkable classification of sets of zero measure, begun in 1911 and then picked up and developed only in an elementary book *Théorie des ensembles* [1949a] in the series "L'éducation par la science" that he edited at Albin Michel, or else his mathematical solution of the St. Petersburg problem, neatly concealed, as we will see, in a book in the "Que sais-je?" series[iv] entitled *Probabilité et certitude* [Borel 1950]. Even though it is outside our topic, we could observe the same thing for most of his important contributions, his measure theory for example, which he never allows to be defined precisely, or his theory of denumerable probabilities, which some people, here and there, still claim was never given a proper foundation. But at the beginning of the XXth century, this was only a scholarly habit favoring the exposition of ideas, syntax, and the esthetic of beautiful language in the writing of great mathematical texts, over logical and mathematical rigor when reasoning flowed so clearly that rigor was not needed, and over intermediate

[iii]Editors' note: The title can be translated as "Treatise on the Probability Calculus and its Applications". Each of the 18 installments or fascicles is actually a substantial book. Some were written by Borel, alone or in collaboration; some were written by others.

[iv]Editors' note: "Que sais-je?" means "What do I know?". The series, consisting of pocket-sized surveys designed for undergraduate students, began in 1941.

arguments the reader was supposed to "see easily". Gradually it became instead a deliberate, systematic, and fixed attitude. Borel wants to keep the nature of the objects he manipulates only vaguely defined, and he wants to give great latitude of interpretation and generality to statements that specify this nature and even to proofs of these statements, even as postwar mathematics is becoming purer, more axiomatic and everywhere regimented in order to be better protected from lazy mistakes that overlook the real complexity of mathematical objects and from logical traps where they lose their Greek soul.

Yet in his prime of his youth, Borel had triumphed in the large and small olympiads of his time (see [Lebesgue 1991], [Guiraldenq 1999]). For example, in one academic competition, the Vaillant award, he forced himself to classify all displacements with spherical trajectories whose fundamental equations have 17 terms. So we must grant that he had at least certain logical and combinatorial capabilities. Why does he not permit himself to use them when he suddenly has one of those ideas that can change things deeply, for example the idea of Borel measure, the unique countably additive extension of length for intervals, which can measure more and more exactly all the Borelian sets that the Borelian theory of functions needs? Why does he mention the idea briefly without bothering about rigor and generality, so that we never know what he is talking about or who he is talking to? Why are his works on probability, so profound, presented in such a manner as to make it possible for people to claim (as some, hardly the least able, do) that he never once stated or demonstrated in a clear and indisputable way the least identifiable mathematical result? The answer generally given is that Borel's ideas are too far in advance of his technical possibilities and that, if he is content with a demonstration so unsatisfactory for today's mathematicians, it is because he cannot provide a better one. Borel himself tended to encourage this type of analysis, by responding when he was questioned that more details could certainly be given, but that it might take too long, and would in any case be too tedious for him to look into, and that he had better things to do. He also willingly admitted that he had abandoned higher mathematics after the war of 1914 (even after 1905), no longer feeling he had the "force of mind"[v] to really devote himself to it [Marbo 1968]. Lebesgue amicably reproached Borel for this sort of "prideful modesty" when Borel wrote to him in 1909 that he was "profoundly disgusted" with mathematics as a career [Lebesgue 1991]. But this does not get to the bottom of the question. Borel, a brilliant and visionary mathematician, saw himself very early as a missionary; the science that at first, like Hermite and Poincaré, he had cultivated for itself, for its beauty, its moral rigor, its austere grandeur, does not deserve our devotion unless it is put into the service of man and society.

As far as society is concerned, it is quite well known that Borelian math-

[v]Editors' note: Here and elsewhere, the translation reproduces the quotation marks in the original even though the words being quoted have been translated.

ematics does or should have a practical value. Starting in 1906, Borel develops this theme in his lectures, in his editorial activity as founding director of *Revue du mois*, and in his engagement in developing the applications of probability: statistical physics, biology, engineering sciences, actuarial science, etc. We will not come back to these applications here. For them, all that really counts is the "formulas", which must be established as quickly as possible, without paying too much attention to higher mathematics, of which only techniques of calculation are used. But as derivation of such formulas sometimes requires the creation of original methods, mathematics has benefited throughout its history from the unexpected contributions of its "applications" (think of Newtonian mechanics, of heat theory or simply of probability). And Borel does not seem to have ever questioned the "beneficial role" of this "practical" mathematics, intimately linked to the highest and purest Science, but engaged in action. On the contrary, he magnifies it constantly, following his friend Jean Perrin whom he likes to quote: "*The marvelous Adventure that has engaged humanity for scarcely more than a generation, which no doubt announces the advent of a new Civilization, could not follow its ever more hurtling rhythm, were it not for the ever accelerating progress of Science*" [Borel 1932, p. 99].

Exposit, invent

The value of science for the individual is less obvious. From the nature of the matter, it must be a matter of individual happiness. What other meaning could it have? Borel, having experienced it himself, adheres to the Cartesian and Socratic theory of happiness through clarity of ideas, the serene harmony born from luminous explanation. Only science, and only science at the highest level possible at a given time, can really contribute to respond to every man's natural demands for the absolute and the truth, and it falls to the most advanced scientists to undertake this task of exposition, whose nobleness is obvious but whose difficulty seems insurmountable. How to explain to a reasonably cultivated public, or simply to a single reader, the idea of measure for example, or any other mathematical idea, so it will appear with as much force and clarity as for the one who first conceived it? Borel convinced himself rather quickly (rightly or wrongly, that is another debate) that the needed logical rigor, general axioms, and associated techniques of proof brought nothing to that purpose.

The mathematical reader could "easily" reconstitute the rigorous logic (if he is able to, otherwise, too bad for him); as for the ordinary reader, it would be completely impenetrable for him and hence without the least value. But is it possible, with only minimal mathematical technique, to elevate the reader towards the scientist's happiness, to present to him not only a metaphorical or sentimental version of great scientific ideas, but an approach as close as possible to the concepts, in their integrity and original intuition? For this, it would be necessary to accumulate, in all possible ways, sketches of demonstrations, paths of new developments, explicit computations when possible, or even to present completely new ideas authorized by this curious method and unimaginable without it, and especially to give clear and lucid explanations, inventing if needed new explanations to show that human reason can conquer anything if we leave it free and do not trap ourselves in words.

Borel begins by applying this method of exposition to his mathematical works. For example, he explains to the readers of the series *l'Avenir de la science*,[vi] edited by Jean Rostand, the Borel(-Lebesgue) covering lemma, whose biblical simplicity hides its real profundity, and how to understand that the rational points of a ruler can have measure zero even though they are packed infinitely close to each other. It is enough to imagine removing successively a quantity of matter more and more infinitesimal around each of them, like a precision instrument maker tracing lines finer and finer as he refines the subdivisions in order for the ruler to stay legible. And this manufacturing process allows the mathematician to escape from the circles to which the theory of analytical functions, and much else, was until then confined [Borel 1946, pp. 183–191]. But Borel could not limit himself to Borel's work; starting in 1906, he presents, in his manner and in the very way he understands them, the great scientific themes of his time, molecular theories, relativity, probability calculus, genetics, economics, psychology, etc. Seen from this point of view, "popularization" is no longer clearly marked off from scientific creation. For Borel, it becomes a genre that is authentically philosophical and scientific, requiring from him an effort of mathematical imagination and a constantly renewed lucidity, a clear understanding of his own thought (the task is no longer to convince his peers, but to convince man in general and himself in particular), and requiring from his reader in return intelligent discipleship and unfailing vigilance, so discrete are the allusions and so rapid the insights that are the only links between what we see and do not yet see, between what we do not understand and what we will understand some day.

Fortunately, the scientist's two temporal missions, "invention" in the service of Humanity and "exposition" of Science, are indissolubly linked together. The duty of popularization, incumbent on the real scientist, does not merely serve the flowering of the individual. It also has an eminent social role, that of raising the cultural level of the "average person", and thus renewing the "elites", attracting towards Science a larger and larger fraction of the Nation's "intelligences", so that the happiness the individual derives from a clear and lucid contemplation of Science contributes to society's happiness by constantly putting new generations of scientists at its service. As Borel wrote: "*In order that the elite necessary to scientific progress endure, it must not live apart from the mass of intelligent people; the general elevation of the level of culture must allow it to enter into direct or indirect contact with the average classes and with manual laborers. Isolation would be fatal to*

[vi]Editors' note: The Future of Science.

the aristocracy of the mind; yet we must maintain and strengthen this aristocracy if we do not want civilization to disappear." Borel 1931, p. 767]. "*It would not be too ambitious*", Borel adds, "*to think that an Anatole France or Pierre Loti could reach a million readers, while a Renan, Taine or Henri Poincaré could reach more than one hundred thousand* [in a France with twenty million inhabitants]". The general elevation of the cultural level of middle classes would "necessarily affect" the "peasantry and working class", thereby permitting avoidance of "*the greatest danger which could threaten humanity. The danger would be that increasingly perfected techniques resulting from the progress of science would fall into the hands of men incapable of understanding them completely, who would use them only as a routine. Perhaps this was what happened to some insects, bees or termites. The day when the average person's scientific culture is notably behind industrial development, true scientific culture would risk complete disappearance... Science would stop progressing and humanity would freeze in a mechanism without a future, soon followed by an inevitable decline*" [Borel 1931, p. 768].

So it may be interesting to discuss quickly here this Borelian project of raising the real scientific culture of the average person. We do this by examining a single example, among the more modest: the St. Petersburg paradox. This problem is remarkable in that it defies the computations and theories of mathematicians as well as common sense and the ability of gamblers. Though it had been discussed continually since the beginning of the XVIIIth century, it still had not found a really satisfactory mathematical solution, nor, for that matter, a practical solution above all suspicion. We can easily imagine that for Borel, who intended to establish the practical and philosophical value of the probability calculus as definitively and broadly as possible, this was an enigma that reason needed to clear up. We will see that Borel takes on this type of paradox starting with his very first course on the probability calculus at the Sorbonne, in the first semester of 1908–1909, and that he provisionally concludes his reflection only in his last "Que sais-je?" on probabilities, published in 1950, after having shown in 1939 that this famous "St. Petersburg paradox" could be seen as a minor avatar of a yet more fundamental paradox, the paradox of martingales that make a fair game infinitely advantageous for one of the players, and that this paradox is susceptible of a mathematical explanation, an explanation that he was the first to give at the time. So in this very case, Borel reaches the ideal of popularization, which consists of giving to the middle-class reader, and to the manual laborer as well, in lay terms, the beginnings of a mathematical theory of prime importance even before the scientific aristocracy gets hold of it. This history of the St. Petersburg martingale, which we briefly relate here, should allow us to penetrate the Borelian universe for a moment and in this way shed a bit of light on the whole of a work that is exceptional in so many respects.

It might also illustrate a very modest thesis, according to which popularization can be the occasion of mathematical creation, and Borel, more than anyone else, understood and practiced it quite well. Unlike Laplace, for whom the exposition of great scientific theories often reduces to transcription into lay terms of his most impenetrable mathematical results, leaving to the stunned reader the job of understanding what is going on, having himself neither the time nor the desire to explain (a rereading of *l'Essai philosophique sur les probabilités* will make the point), Borel sees in popularization the opportunity to go even further in his effort towards intellectual sincerity and in his determination to understand and to make understood beyond all doubt or loss of certainty, leaving it to others to transcribe into mathematical language his most daring popularized advances, which sometimes cross without warning the scientific frontiers of his time. At this point it is not very surprising that this extreme Borelian popularization did not meet the desired success. Cournot, whose *Exposition de la théorie des chances* remains forever a summit in this literary genre for a large middle-brow public, suffered the same disappointments a century earlier, as mathematicians found no theorems in it, while others could not make heads or tails of it.

Parallel readings

There is another thesis, equally rather well known, which we would like to argue incidentally in the course of our history: the thesis according to which scientists of a new century, including Borel but also Bachelier and many others, looked at classical problems of the probability calculus from a new perspective, formed from the "graphical recordings", the "sinuous paths",[vii] and the "consequential paths"[viii] of the science of their times, physiology, physics, economy, etc., which led them to see and ask other questions, such as Borel's theorem on normal numbers or the game of heads or tails is the most celebrated, but which also touch on those multiple "passages", "recurrences", "crossings", "oscillations", "extremes", "gaps", "stoppings", "returns",..., which constitute the rhythm of the random course of a gambler's good and bad luck. The theory of Markov processes, even that of martingales, can only be understood through this perspective. Hence Borel's martingales, as elementary and apparently anecdotal as they are, naturally take their place in this slow movement that changes the course of a discipline, the probability calculus, destined to occupy one of the dominant positions in XXth century science.

It is not the result of blind chance that these two theses meet here. Any

[vii] Editors' note: courbes sinueuses.

[viii] Editors' note: Play on words. Poincaré introduced the notion of the consequent of a point M_0 on a curve in relation to a (two-dimensional) differential equation. The consequent of M_0 is the next point M_1 of the curve reached by an integral curve passing through M_0. See Chapter V, "Théorie des conséquents", of "Mémoire sur les courbes définies par une équation différentielle", Journal de Math., 7, 1881, 375–422. Poincaré later used the notion to describe trajectories in celestial mechanics (Méthodes nouvelles de la mécanique céleste, Gauthier-Villard, 1899).

lucid exposition of a scientific theory requires an original outlook, and every change of view brings discovery, at the same time as it stimulates exposition. Borel's scientific intuition is embodied in many ways that are mixed and confounded: specialized publications, scientific treatises, and textbooks and popularizations. Sometimes the latter are more erudite than the former, more durable no doubt, depending less on contingent fashions and thus displaying more the genius proper to their time and their author.

This second reading assumes a certain familiarity with probability theory. We touch here on one of the shortcomings of the Borelian enterprise of scientific exposition. How can we present questions irreducibly specialized to non-specialists? Borel, for his part, proceeds when the moment comes, when he really has no choice, with furtive and mysterious allusions, for whomever may understand! We have adopted a middle way. In an appendix, we re-establish in modern language, accessible to many, the basics of Borel's martingales. This part is aimed at facilitating the reading of what comes before and can be read independently of the rest. On the other hand, for everything touching on the emergence of the mathematical theory of martingales, about which we must say a word in order to locate Borel's atypical position, we limit ourselves to succinct and sometimes enigmatic indications. A complete book would have been needed to deal properly with such a topic.

We give a particular role to Félix le Dantec, a scientist very much in fashion in France before the first World War but rather unknown today. Le Dantec was a neo-Lamarckian biologist of the beginning of the century, who defended brilliantly and zestfully transformism and the evolution of species in a most often hostile climate, but at the same time vigorously opposed not only Weismann and de Vries's neo-Darwinisms but also remerging Mendelian genetics [Bateson 1902], [Morange 2000] — the theories that were going to dominate an important part of modern biology. To understand Le Dantec's positions, we would have to develop his theses extensively, always pertinent in spite of their decidedly marginal character, and situate them in the history of biology at the beginning of the twentieth century. But is this the place? We limit ourselves to a few notes, certainly very inadequate. The reader can nevertheless consult some of the references we give, for example the beautiful book by François Jacob [1970], which unfortunately mentions neither Le Dantec nor even Rosny Aîné! In general, we postpone to notes at the end of the article everything that seems useful to mention to give our discussion a little texture or less platitude, and to open leads that may deserve to be followed up.

2 From the illusion of returns to equilibrium to the St. Petersburg paradox

The St. Petersburg problem, as it is called, appears for the first time in correspondence between Nicolas Bernoulli and Pierre Rémond de Montmort in 1713, correspondence Montmort reproduced in his *Essay d'analyse sur les jeux de hazard* [Montmort 1713]. Its name comes from Daniel Bernoulli's very famous article on the subject, published in the *Mémoires de l'Académie de Saint-Pétersbourg* [Bernoulli 1738], the object of innumerable past, present and future commentaries.

Let us recall what it is about in the version given by Borel [1939, pp. 60–61]: Peter and Paul play heads or tails. Peter pays Paul a stake A on the following conditions. If he wins on the first toss, Paul pays him 2 francs; the mathematical expectation of this case is therefore twice $\frac{1}{2}$, namely 1 franc. If he wins only on the second toss, Paul pays him 4 francs; the mathematical expectation is again 1 franc. And so on: if he wins only on the nth toss after losing all the preceding tosses, Paul pays him 2^n francs, for a mathematical expectation again of 1 franc. "*As n can take successively every integer value from unity to a number as large as desired, the total mathematical expectation and hence the value of A is infinite.*" The paradox lies in the fact that "*no one would agree to play this game with a stake of a thousand francs*" (or even a hundred francs or even fifty): common sense defies mathematics and Borel. How can we answer?

Borel seems not to have alluded directly to this problem in his first lectures on probabilities at the Sorbonne, nor in his popular books or articles written before 1939. He probably thinks that this paradoxical intervention of infinity is without practical interest and might divert a naive reader from what is essential. Or perhaps he has nothing sufficiently new to say on this question, pondered and pondered again by the best minds since the beginning of the XVIIIth century[1].[ix] In his 1908 probability course, Borel devotes a section to "Remarks on some paradoxes" [Borel 1909b, §9], mainly because it gives him the opportunity to respond to some negative remarks by Félix Le Dantec, who just expressed very serious doubts about the probability calculus in the *Revue du mois*, founded by Borel in 1905. The "paradox of returns to equilibrium" considered by Borel in 1908 in response to Le Dantec apparently has no direct relation to the St. Petersburg problem, but Borel indicates himself in the philosophical essay that closes his great *Traité* [Borel 1939] that the one is not so far from the other. The 1949 solution that we now present concerns in fact the paradox of 1908 as well as the St. Petersburg paradox, so that it would be impossible to understand Borel's martingales without first presenting the Borel-Le Dantec controversy, which began in 1905 and never stopped preoccupying our author, right up to his last publications.

[ix] Editors' note: The authors' notes are at the end of the article, before the bibliography.

Borel and Le Dantec, engaged scientists

To begin, recall briefly the position of the two protagonists of this curious history at the beginning of the XXth century.

Son of a Protestant pastor from Montauban, Émile Borel, after brilliant successes at school, defended a surprisingly original thesis in analysis in 1894. Quickly appointed assistant professor[x] at the École normale supérieure by Darboux, he schooled a whole generation of mathematicians in the set-theoretic approach to the theory of functions [Borel 1898]. He gave the first three Peccot courses at the College of France from 1899 to 1901, and in the same year he married Marguerite Appell, becoming the son-in-law of the new dean of the Paris Faculty of Sciences and nephew by marriage of Joseph Bertrand, Hermite, and Picard. When the École normale supérieure was reattached to the Sorbonne in 1904, he was named adjunct professor, and in 1909 he became professor in a chair of function theory created for him. This faultless trajectory seemed not to satisfy him, for stating in 1905 he devoted himself in parallel to various undertakings to exposit and disseminate recent science, particularly the kinetic theories whose difficulties and paradoxes he tried to reduce. Because of these difficulties, these theories had long been rejected with contempt, especially by the Parisian school of mathematical physics, even though they were visibly destined to play a central role in the new physics of the infinitely small at the beginning of the XXth century, with its corpuscles of all kinds: ions, electrons, etc. Borel was the first to make the simple observation that at the atomic level, the idea of "determinate initial conditions" is "pure abstract fiction": any hypothetical determination would be instantly modified (by the impromptu motion of an atom on Sirius for example) and this change, first imperceptible, would soon affect the resulting motion in an extravagant way [Borel, 1906b, 1913]. The only scientific defense against this indetermination of initial conditions is an explicit and well implemented probability calculus, and from this time on Borel made himself the principal propagandist of such a calculus in France.

The *Revue du mois* served as a rallying-point and platform for the young scientists of the new century: Perrin, Langevin, Pierre and Marie Curie, but also Painlevé, Tannery, Drach, Caullery, Bernard, Duclaux etc. Poincaré himself, the greatest of them all, whose hostility to kinetic theories and skepticism towards the probability calculus are well enough known, soon gave the *Revue* a resounding article [Poincaré 1907], in which he finally admits that certain physical phenomena at an appropriate scale can only be "fortuitous", and that in fact only a probability calculus can deal with this, once its results no long depend on the details of the initial conditions, which is the case every time the probabilistic ergodic principle is applied, for example when cards are shuffled so that the final distribution is completely independent of the arbitrary initial conditions, except in cases of obvious cheating that do not occur in nature. It is hard to deny that Poincaré had been partly influenced

[x]Editors' note: maître de conférences.

on this subject by the reflections of Borel, whose scientific work was then taking on a new dimension. From then on, Borel would spare no effort in trying to convince his contemporaries, learned aristocracy, middle classes, manual laborers, etc., that "*the mathematical answer for many practical questions is a* probability coefficient... *A probability coefficient constitutes a completely clear answer, corresponding to an absolutely tangible reality... If the notion of* statistical truth *became familiar to everyone who talks or writes on questions where the statistical truth is the only truth, many sophisms and paradoxes would be avoided*" [Borel 1907b, p. 698], [Borel 1914, p. 137].

As for Félix Le Dantec, (1869-1917), he is the son of a Voltairian and Breton doctor who gave him an exclusively scientific education to preserve him from any metaphysical temptation. Admitted to the École normale supérieure in 1885, Félix Le Dantec was won over by transformism and decided to devote himself completely to natural science, as did many normalians of the end of the century, Noël Bernard and Charles Pérez for example. Le Dantec distinguished himself very early not only by his original scientific works, but especially by his brilliant and thought provoking works of scientific and philosophical popularization. He participated in the Pavia mission in Indochina, and in 1899, he was teaching biology at the Sorbonne,[xi] which he would continue doing until his death in 1917, without ever being permanently appointed[2] . Eloquent advocate of the inheritance of acquired characters, "inscribed in the chemical patrimony", Le Dantec declares himself, in opposition to Weismann [1892] and de Vries [1909], in favor of a Lamarckian transformism. The evolution of species is ruled by Lamarckian complexification of individuals under the "authority of circumstances", transmitted through heredity (see also notes 3 and 10). According to Borel, he was "*one of the most distinguished minds of our time, well known through his scientific and philosophical publications, and whose mathematical education was very serious*" [Borel, 1909b, p. 18].

Chance knows no law

In an article published in 1907, Le Dantec [1907b] undertook to examine the notion of chance, which Darwin and his successors seem to consider the principal motor of Evolution. For Félix Le Dantec, chance has meaning only relative to an individual. It is the "totality of the elements of [the external environment]" that are not direct consequences of his "vital functioning" and "against which his intelligence is helpless". Thus considered, chance could not be the motor of Evolution, or of anything. Not only does chance explain nothing, it does not follow any law and therefore cannot be the object of a calculation, other than a calculation *a posteriori*. In fact, *some* games (games of chance for those who play) do satisfy a law of large numbers when they are suitably organized, for example as many tails as heads in the long

[xi]Editors' note: As chargé de cours.

run for a coin that is well balanced and thrown honestly. These "games of chance" are then subject, no more or less than any other phenomenon, to the experimental method: for a given game, we cannot conclude anything, but globally, "the law of large numbers is verified", and this alleged "law" is the experimental sign of the existence of a real law at a higher level (the famous constant causes of Laplace [1814]).

For example (and one senses that all the rest derives from this example), Darwinian chance applies to the smallest elements, but it is Lamarckism that explains the coordination and the individual's adaptation; "*law at the higher level (Lamarckism), the phenomenon seems to be ruled by chance at the lower level (Darwinism). Here the mystery of the law of large numbers comes to light, it is because all the elements at the lower level are endowed with elementary life that their union gives life to the being at the higher level; it is not with chance alone that we create a law*"[3] [Le Dantec 1907b, p. 285]. The rest, according to Le Dantec, is bad metaphysics (and for him all metaphysics is bad), in particular, "*the probability of an isolated toss is a conception with no meaning*" [Le Dantec 1907b, p. 270]. It is possible of course, to evaluate it *a posteriori* after many observations, if we are in a case of application of the law of large numbers, but its determination, in any case not very precise, is of little interest. Inverting the process and bringing forward the probability of a single case to establish a law of large numbers is irrational. Especially since the calculation itself is very imprecise and says nothing that a sensible observer does not already know. It took some cheek for Le Dantec (and for Borel) to publish such an article in the *Revue du mois*, one of whose stated goals was, as we have mentioned, to promote the practical and scientific value of the probability calculus [Borel 1906b, 1907b, etc.]. Borel must respond. He does so in his first course on the probability calculus at the Sorbonne, published in 1909 by Hermann [1909b].

Borel's first response, the paradox of returns to equilibrium

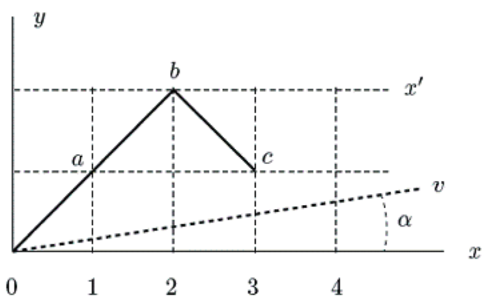
Precisely in order to make the point that common sense gets lost in the presence of chance if it does not rely on calculation, Borel, inspired by Bertrand [1888, chap. VI] and especially by Le Dantec [1907b,c], considers the indefinitely repeated returns to equilibrium in the symmetric game of heads or tails, which could in principle allow a patient player to wait for a sure profit after a return to zero (according to Borel's lemma, which would appear shortly [1909a], see the appendix and note 17) and to become infinitely rich once he repeated this operation sufficiently often; and this applies equally to both players. The biologist is lost facing the practical absurdity of this reasoning, the geometer on the other hand can show that returns to equilibrium preceding the player's profit, in the long run (indispensable for significant profits from this "martingale"), require so long a wait that the expected fortune is illusory. Borel nevertheless does not really make his assertions precise with calculations. He merely refers the reader to an earlier section of his course, which deals with a simpler question: a player wins 1 franc for every return to equilibrium, calculate his mathematical expectation for the first n tosses. The calculation is easy and leads to the approximate result $1.128\sqrt{n}$. Hence if a player pays 20 francs to play a series of a thousand tosses, he will certainly be at an advantage, but if he pays two thousand times 20 francs, to play two million tosses, he will lose a lot for sure, the expectation for two million tosses being about 1600 francs. Borel explains this curious phenomenon in the following way: the first series of a thousand tosses each will probably have relatively regular returns to zero when put end to end, but sooner or later the series of tosses will produce "exceptional departures" that prevent rapid returns to equilibrium and ruin the player's wait[4].

As ingenious and sensible as they are, Borel's arguments remain fairly imprecise numerically. He nevertheless concludes that common sense reasoning is inane as soon as a problem is a bit complex, and that it is then absolutely necessary to turn to the probability calculus. He adds: "*In my opinion, there is a very great scientific and social interest in the fundamental principles of the probability calculus being accepted without restriction by as many people as possible*" [Borel, 1919b, pp. 16–17], maybe even by Le Dantec, who did not, however, seem to want to be counted among them.

In fact, Le Dantec responded right away with an article published in the *Revue philosophique* [Le Dantec 1910] and reprinted in a chapter with an unequivocal title—"The alleged laws of chances and Bernoulli's stratagems"—in one of his numerous books, *Le chaos et l'harmonie universelle* [Le Dantec 1911a]. Le Dantec is one of those resolutely contrary scientists, capable of the best and the worst, but never caught off guard, especially in polemics, where they excel.[5] His argumentation is in any case remarkable. He proposes to prove without calculation, using the sole hypothesis that "the game of heads or tails obeys no law" (minimal form of von Mises's irregularity axiom [1919,1931]), solely by the power of "commonsense reasoning", all the alleged results of the probability calculus, especially the Borelian hair-splitting about returns to zero in heads or tails and Bernoulli's law of large numbers, thus showing this calculus to be only a "mathematical verbalism" without any more content than you put into it in the beginning. This verbalism doubles as a scientific hoax, moreover, when it pretends to talk about the probability of an isolated event and thus leads people to believe that chance can be controlled by a number. It is imperative to ban this verbiage from science and to keep only what is needed by actuaries and for the counting by combinations that is sometimes very useful. All the rest is only illusion or "stratagem". "Simple mathematical analysis" cannot lead to a physical law, especially since there is no law at all here, chance not knowing any law in principle. Le Dantec here rediscovers a theme that recurs, as everyone knows, in Auguste Comte [Coumet 2003], a theme to which Borel had responded in advance by separating the science of probabilities from the common trunk of mathematics, in order to give it a refereeing role over opinions and laws, a role it would have lost by integrating itself too intimately in the abstract science of numbers. For Borel, as we know, the object of probability theory is to "*be able to predict with an almost absolute certainty, humanly absolute as it were, certain events whose probability is such that it cannot be distinguished from certainty*" [Borel 1914, no 8].

Le Dantec's path and common sense

Let us briefly point out the most original part of the "common sense" Le Dantec marshaled against Borel's (unperformed) "calculations". Le Dantec had the ingenious idea of representing graphically, on graph paper, the sequence of tosses of heads or tails using a broken line starting at the origin, going up one step if the toss results in a tail and otherwise going down one step. If we are not mistaken, this is one of the first appearances in the long and rich history of the game of heads or tails of this type of representation, so constantly used nowadays but then just introduced in the experimental sciences[6] (see also notes 8 and 9).



Le Dantec's and Borel's path ([Le Dantec 1910, p. 341] *and* [Borel 1914, p. 43])

Le Dantec then comments on this "sinuous" path with commonsense remarks rather resembling Bachelier's considerations [1900] on the stock exchange (whose sinuous paths had appeared in the specialized literature since the Second Empire) and the reasoning later offered by Doeblin, Kolmogorov, Lévy, Doob, and all contemporary probabilists on the trajectories of a random walk. His study leads him to a first conclusion (which in fact contains all the others, according to him, and which is now called the recurrence property, well identified since the beginning of the XIXth century by Ampère and Laplace, who had themselves demonstrated it). For any number N given in advance, no matter how large, there will come a moment when the ordinate of the path will equal N. This follows from two commonsense reasons: assuming, to fix ideas, that N is positive, it is certain that the sinuous path will not always remain below the x axis, otherwise it would follow the law of always remaining negative, in contradiction to the principle that "the game follows no law". So the sinuous path will eventually go up one step above the x axis. Once this point is reached, the path continues, forgetting where it started. Indeed, we are certain that the game, free from all constraint, makes at every moment a clean sweep of the past. But for this new game and its new origin, the height N is diminished by one unit, and we repeat the reasoning with $N-1$ in the place of N, and so on. "The theorem is proven", concludes Le Dantec, who, without realizing it, has slipped from commonsense reasoning to formal mathematics, contrary to Borel, who sometimes contents himself with an accumulation of commonsense arguments while pretending to do calculations.

It is easy, in fact, to put most of Le Dantec's arguments in mathematical form. The exception is the most astonishing one of all, by which he claims to prove the (strong) law of large numbers for the symmetric game of heads or tails using only qualitative commonsense reasoning about returns to the origin. For Le Dantec, indeed, it follows from the general shape of his sinuous path that the ratio of the ordinate to the abscissa ($S(n)/n$) obviously tends to zero. The (mathematical) argument he gives is obviously erroneous, as Borel points out, but the statement is remarkable, for Borel had just then established this strong law in his article of 1909, which Le Dantec surely had not read. For Le Dantec, the law of large numbers, or what some people call by that name, is merely a commonsense consequence of the general principle that chance knows no law, so that Bernoulli's alleged demonstration of the (weak) law of large numbers is nothing but a useless and perfectly dishonest "stratagem". In the same way, the so-called paradox of returns to equilibrium that Borel uses to argue against him exists only in the individual Borelian consciousness (which is, for that matter, only one of many properties of the Borelian (bio)chemical constituents evolving in contact with the environment): there are moments when the sinuous path favors Peter and others when it favors Paul, and returns to equilibrium indefinitely into the future. Who could take exception to that? (Polemic ability that always responds to the aspect of the question where no one sees any interest or sense). And Le Dantec concludes: "*questions of probability differ from ordinary mathematical problems, in that commonsense reasoning must always be used alongside analytical development*" [Le Dantec 1910, p. 356]. So much so that Bernoulli and Borel mislead the public by pretending to calculate where only common sense can play a role. If people were properly informed, they "*would not suffer the scientific anguish of believing that chance follows laws, and they would not be tempted to use martingales at Monte-Carlo*" [Le Dantec 1910, p. 360].

Borel's second response, returns to equilibrium again

Facing this frontal attack against the scientific, practical and philosophical value of the probability calculus, Borel is forced to counter-attack in an article of the *Revue du mois* [Borel 1911], which becomes the main part of chapter II of his 1914 *Le Hasard*. Borel is a rather awkward polemicist[7], but incontestably (even if it is sometimes contested), he is a mathematician of exceptional depth, which allows him to grasp immediately the strengths and weaknesses of the commonsense mathematical argumentation of his old classmate. First he puts Le Dantec into the "Pantheon of inattentive scientists", by clarifying for him that the paradox in Borel's paradox of returns to equilibrium is not that both players may win as much as they want at different moments, for there is nothing absurd here even for a mathematician. In fact, as Borel wrote in his course, the paradox is that both players can theoretically become infinitely rich with time, while the game is and always remains fair, and the response to this paradox is too subtle for the biologist's common sense to grasp. The sinuous path Le Dantec introduced for the game of heads or tails, whose mathematical value Borel visibly appreciates, is actually more complicated than Le Dantec seems to imagine. The very long periods of sojourn above and below the x axis (the large excursions) are not as rare as one might think (without calculation).

This time, Borel does not evade the question but treats it in a more convincing way. He proposes to show that in the course of a very large number of consecutive tosses, say a million, the probability that Le Dantec's path does not cross the x axis is of the order of 1/1000, which is far from negligible from the very long-run viewpoint we are taking. To this end, Borel calls his reader's attention to the analogy between this question and Bertrand's ballot problem [1887a], noticed by Désiré André [1887] and treated again by Bertrand [1887b], [1888] (see *e.g.* [Feller 1950, chap. 3] for a statement of the ballot problem). According to the ballot formula, if the path is at m at the nth toss, the probability that it never touched the horizontal axis on the previous tosses is $|m|/n$. So the probability that the path stays on the same side of the x axis during the first n tosses is $n^{-1}E(|S_n|)$, where S_n is the ordinate of Le Dantec's path at toss n. But, Borel adds, $E(|S_n|)$ is of order $\sqrt{n}$, which proves the announced result. Indeed, Borel could have observed that it had been known since Moivre (1730) that $E(|S_{2n}|) = 2nP\{S_{2n} = 0\}$, this remarkable identity appearing in Bertrand's course as well Poincaré's (on this topic, see [Stigler 1986] and [Diaconis, Zabell 1991]). It follows that the required probability, when we consider an even number of tosses $2n$, is[8] approximately $1/\sqrt{\pi n}$.

One wonders, as we did at the beginning of this article, for whom Borel really wrote his works of popularization, so rich in ingenious insights and enigmatic bends. Has Borel fabricated for himself a virtual reader endowed with a superior intelligence, constantly questioning him, pushing him back to his last defenses, for whom he reserves his sharpest arrows (in short a kind of Le Dantec who must be convinced)? This would explain Borel writing mathematics like novels with secret meanings,[xii] for which he was much criticized by the rising generation between the two wars (and surely would have been criticized even more by the following generations if they had read

[xii] Editors' note: romans à clef.

him in the original text).

Moreover, Borel adds, returns to equilibrium over the long run can be very far apart for the very reason invoked by Le Dantec: indeed, Le Dantec's path probably begins with small oscillations around the x axis, but as soon as it reaches a considerable height, say N, which it necessarily reaches eventually, it starts anew, making small oscillations around the horizontal line of height N, as if it thought it was back at the origin; this slows down its return to the x axis considerably, and these successive slowdowns from axis to axis only grow with time. The small oscillations at the beginning thus explain the later very long oscillations, when the path has had the time to reach a bit of height.[9]

Borel went no further in 1914, but he must have thought to himself that the story was not over. One of the questions raised by Borel's paradox remained unanswered mathematically. How, indeed, can we make a clear and distinct mathematical statement out of the principle of the impossibility of a gambling system, postulated for such a long time by moralists and calculating mathematicians and now for a while also by commonsense biologists? How can we do this at least for the game of heads or tails, here precisely where there are theoretical possibilities of infinite wealth? Aren't arguments about excessive waiting times as vague and ordinary as the reasoning by Le Dantec that Borel judged to be sentimental? Doesn't Borel's paradox turn on him, showing yet more clearly the accuracy of Le Dantec's heretical thesis, namely that the common sense of biologists (and gamblers) is a better guide on questions of chance than mathematicians' calculations? We understand why Borel did not want to prolong the polemic, interrupted by the war, but we can easily imagine that he often had to struggle against the phantom-epiphenomenon of Le Dantec, who died prematurely in 1917.[10] How can he rid himself of this laughing jumping jack, always jumping back out of his box? Feeling unable to equal his adversary's verve, Borel finally calls his friend Paul Valéry to his rescue. In a section of his first "Que sais-je?", entitled *Les probabilités et le bon sens*, Borel uses a long citation from *Regards sur le monde actuel* to reduce to very little the "common sense" that is being talked up so much and, at the same time, Le Dantec's ironic and inappropriate remarks: *this common sense is a very local intuition, deriving from experiences that are neither precise nor scientific, mixing logic with analogies too loose to be universal* [Borel 1943, pp. 15–16].[11]

It is only after 25 years, in the final fascicle of Borel's *Traité* [1939, pp. 48–50], that Borel takes up anew the illusion of return to equilibrium. The fight for probability calculus had then been won, and no one would any longer dare to maintain seriously, as Le Dantec had done, that Bernoulli's stratagem or the law of large numbers are merely mathematical verbalisms, unneeded by science. Borel can step back and examine the question in depth. He starts by recalling the mathematical statement in his 1909 course: return to equilibrium is (almost) certain, but the mean value of the time needed for such a return is infinite, and he adds: "*this result, seemingly paradoxical, is not* *without analogy to what mathematicians of the XVIIIth century called the St. Petersburg paradox*" [Borel 1939, p. 50] (see also [Bertrand 1888, §86]). Borel had reflected a lot on the role of infinity in mathematics in the meantime, and he is no longer satisfied with his response to Le Dantec. In both cases, returns to equilibrium followed by a gain, and the St. Petersburg game, wealth is guaranteed and potentially infinite. How then to make mathematically precise how the principle of the impossibility of infinite wealth is combined with games of chance within the classical mathematical theory of probability (modernized by Borel and Kolmogorov)? Where is the true mathematical coherence in Borel's paradox on returns to equilibrium, or in the St. Petersburg paradox, which Borel now sees as an "analogy"? Pretending to answer the question by changing the rules of calculation, as Daniel Bernoulli [1738] had proposed for the Petersburg game, or establishing a recipe to be used by prudent and mortal gamblers, is this not to doubt human reason and the explanatory power of mathematics? Borel cannot escape; he must answer.

3 More paradoxical than the St. Petersburg paradox: The St. Petersburg martingale

Borel's first work on the St. Petersburg paradox, as we mentioned earlier, appears in the last fascicle of his *Traité*, entitled *Valeur pratique et philosophie des probabilités* [1939, pp. 60–69]. In a chapter entitled "Reflections about some errors and paradoxes", Borel devoted two sections to this famous problem. Even though infinity enters into the problem's initial formulation, his most interesting remarks about the problem, as he emphasizes at the outset, concern what happens when we "limit ourselves to the finite". We will see what he says about this and where his originality lies.

For Borel, as for many other authors in earlier centuries (see note 1 for references), the explanation of the paradox begins by noting that we must evidently limit ourselves to possible and not astronomically impossible gains and not take into consideration eventualities of hypercosmically small probability. All the difficulty lies in finding a good equilibrium between these two constraints, in order to find a value for A acceptable to both players; and we can rely on Borel's common sense to carry out this task intelligently: "*Our conclusion is thus that, if Peter's mathematical expectation is the sum of an unlimited series whose terms are equal to unity, only the first terms of this series are effectively negotiable and the values of the following terms quickly become absolutely null, since they represent the absolutely illusory expectation of winning a sum so big that it could not be paid*" [Borel 1950, p. 96]. Cournot [1843] said nothing different, and the mathematician is left with his hunger unsatisfied (Borel too no doubt, and what do we say about Le Dantec?).

This is not the most interesting point; indeed, in a second section, Borel proposes to define a "St. Petersburg game" that would be mathematically "fair". How does Borel do this? We will see that he constructs a simple martingale in the heads or tails game that allows Peter to obtain Petersburgian gains with the same probabilities [Borel 1939, §35]. To begin, let us ask ourselves about this idea of a martingale (or fair game), which Borel puts to work for the first time here. It is obvious that it comes directly from Jean Ville's doctoral thesis [1939], whose defense Borel chaired, and which he agreed to publish in his new series of *Monographies des probabilités*.

The impossibility of a gambling system and Ville's theory of martingales

When he graduated from the École normale supérieure in 1933, Jean Ville (1910–1989) benefited from a scholarship for research in Berlin and then in Vienna, where in 1934 he participated in the discussion of the axiomatics of von Mises's collectives [1919] in Karl Menger's seminar. Von Mises, as we know, proposed to finally rid the probability calculus, whose physical applications were growing, of any reference to the notion of the "probability of an isolated event". Not only was this concept meaningless according to Le Dantec; it also offended considerably the empiricism of the new positivists of the Vienna and Berlin circles frequented by von Mises. His initial idea is particularly seductive; it suffices to take as the basic mathematical object, not as before an event that we probabilize with the help of the Holy Ghost, but an infinite sequence of events, governed by axioms that make such a sequence a mathematical copy of those sequences of chance events of which statistical physics and demographic theory provide innumerable examples. According to von Mises, these new objects of thought (it was legitimate to think, according the dogmas of the time), "collectives", must satisfy two axioms that were recognized, by the end of the twenties, as logically irreconcilable in their primitive form (see for example [Plato 1994]). Menger's seminar took this interesting problem as a theme, and soon Feller, Wald, and others re-established von Mises's theory on solid grounds, which did not keep it from being rather neglected at the end of the 1930s in favor of a competing axiomatic, that of Kolmogorov, who, without quite saying so, axiomatizes the classical calculus of probabilities of isolated events (saying, all the while, that these probabilities have a physical meaning only as limits of frequencies). Kolmogorov's axiomatic would in fact show itself to be mathematically richer, for as Borel had already implicitly shown in 1909, it allows the statement of theorems about what is "almost sure", theorems that von Mises's theory cannot obtain without useless contortions. Ville's thesis demonstrated precisely this in a particularly lucid way and thus gave great pleasure to Borel, who was not easy to impress. Secondarily, Borel saw in the thesis a new opportunity to argue against those who pretend to limit exaggeratedly the natural abilities of human reasoning. We will not go into details on this point, which would detour us too far, but it is in this context that Ville undertook his "critical study of the concept of a collective".

Ville started with a deep examination of the second axiom in von Mises's theory, the axiom of irregularity or of the impossibility of a gambling system (*e.g.* [Mises 1931, p. 4]). Neither the statement nor the status of this axiom is obvious. The principle of the impossibility of a gambling system had been clearly identified by many authors in previous centuries, notably Buffon, Ampère and Cournot, and certainly Félix Le Dantec: in a fair game, it is impossible to get rich for certain by adopting a gambling system, you are even certain to go bankrupt if you are not careful. How do we make this commonsense statement into a mathematical theorem or axiom? Ampère [1802] seems to have been the first to have made it into a theorem of classical probability theory in the simplest form imaginable: in a fair game of heads or tails, starting with any initial fortune, a gambler is sure to be ruined with probability one (and Ampère, in his discussion, implicitly admits that a gambling system would do nothing to change this, as do Laplace and Cournot after him, and many others still). It is from this result (restated by Laplace in 1811) that Joseph Bertrand constructed his chapter on the gambler's ruin, inspiring not only Borel's martingale of returns to equilibrium [1909b] but also the works of Bachelier. Richard von Mises preferred to make it an axiom in his theory of collectives [Mises 1919]: in a collective, any selection made with knowledge only of the past does not change the fundamental frequency. In other words, a gambler in the Misesian theory who chooses his entries into the game taking previous results into account will not change his probability of winning or losing. This axiom would soon become a theorem of Kolmogorov's (modernized classical) theory, thanks to Doob [1936].

Ville's idea was to make precise and extend the notion of a gambling system, which is too restrictive in von Mises's work, to take gamblers' practice (idealized or not) into account. In the case of the equiprobable alternative, heads or tails, 1 or 0, the most general gambling system boils down to one of the players, say Peter, adopting two sequences of positive functions with a sum less than or equal to 1, $\lambda_n(x_1, x_2, \ldots, x_n)$ and $\mu_n(x_1, x_2, \ldots, x_n)$, which define his bets on the following toss, $n+1$, after he has observed the sequence of the previous n results, so that if $s_n(x_1, x_2, \ldots, x_n)$ represents his capital after the nth toss, Peter bets $\lambda_n s_n$ on $x_{n+1} = 1$ and $\mu_n s_n$ on $x_{n+1} = 0$. We then obviously have

$$s_n(x_1, x_2, \ldots, x_n) = \frac{1}{2} s_{n+1}(x_1, x_2, \ldots, x_n, 1) + \frac{1}{2} s_{n+1}(x_1, x_2, \ldots, x_n, 0).$$

Conversely, this property of being the mean conditional on the results of the first n tosses, called the martingale property, binding together the sequence of positive values for Peter's capital, makes it possible to define two sequences of bets satisfying the previously stated conditions and hence a gambling system, or martingale, that is possible for our gambler.[xiii] Richard von Mises

[xiii] Editors' note: In other words, if the displayed equation holds for a sequence of positive

had limited his gambling systems to choosing arbitrarily, having seen the previous results, which tosses the gambler would bet on. Ville now authorized the gambler to change the amount bet as he wants, within the limits set by his current capital and his knowledge of the game, as gamblers do in practice. Within this more precise framework, we now only need to develop the theory of martingales and to respond within it to the paradoxes evoked above.

Ville begins by defining the general notion of a (positive) martingale adapted to an arbitrary sequence (X_n) of random variables, by the now classical martingale property (*e.g.* [Neveu 1972]), the conditional expectation being "defined" "in the sense indicated by M. Paul Lévy" (rather loosely, that is to say [Lévy 1937], but this poses no mathematical problem).[xiv] Ville [1939, p. 100] then shows that every positive martingale with expected value equal to 1 satisfies, for every λ greater than 1, a (maximal) inequality that he calls the inequality of the gambler's ruin. As the gambler's capital remains bounded, he can only go bankrupt: the principle of the impossibility of a gambling system becomes a general inequality on positive martingales

$$\Pr\{\sup s_n(X_1, X_2, \ldots, X_n) \geq \lambda\} \leq \frac{1}{\lambda}.$$

Ville's proof is remarkably simple and elegant. It consists of forming the sequence σ_n, equal to s_n if $\sigma_{n-1} \leq \lambda$ and to σ_{n-1} otherwise—*i.e.*, the initial martingale kept constant as soon as it exceeds λ (in contemporary notation, $\sigma_n = s(T \wedge n)$, if $T = \inf\{n; s_n > \lambda\}$). Ville shows that the sequence σ_n is a new martingale with expected value one; the inequality then follows easily. This type of reasoning "by stopping" is used by all sound authors, it has become a classic of the theory. Ville had extended the inequality of the gambler's ruin to the continuous case already in 1938 [Ville 1938a, 1939]. On the other hand, neither Ville nor Borel (nor Lévy) seem to have realized that Ville's positive martingales converge almost surely (but generally not in mean), a result due entirely to Doob [1940, 1953], as is the study of the convergence of martingales of arbitrary sign, for which the condition of equi-integrability is essential (on all these questions, see [Crépel 1984a]).

In fact, the "martingale property" had already been introduced explicitly by Lévy in 1934 (under the enigmatic name "condition C") in order to extend the central limit theorem and then the law of the logarithm iterated to dependent variables, in line with early work by Serge Bernstein [1926] and himself ([Lévy 1934, 1935, 1936a, 1937] and [Crépel 1984a]). There is no doubt that Ville was influenced by Lévy's work, at least after the event; he

functions of the form $s_n(x_1, x_2, \ldots, x_n)$, then there exist sequences of positive functions $\lambda_n(x_1, x_2, \ldots, x_n)$ and $\mu_n(x_1, x_2, \ldots, x_n)$ such that the bets $\lambda_n s_n$ on $x_{n+1} = 1$ and $\mu_n s_n$ on $x_{n+1} = 0$ produce $s_n(x_1, x_2, \ldots, x_n)$ as the capital, no matter how the sequence $x_1, x_2, \ldots$ comes out.

[xiv]Editors' note: Ville gives the definition discussed here in Chapter V of his thesis and book. In the preceding chapter, he uses the definition of martingale the authors of this article explain in the appendix below.

said as much in the introduction to his thesis.[xv] In any case, Ville was the first to really identify the central role of martingales in probability theory and to point to many interesting applications, as he himself explains very clearly at the beginning of Chapter V of his thesis: "*The foundations of all the sciences will always remain controversial. Whatever we think of the usefulness of martingales for clarifying, as we have tried to do, the difficulties in defining irregularity, we propose to show here that this notion can also be used for precise mathematical purposes. In this chapter, we will review certain classical questions; we will associate a hypothetical fair game with each problem, and we will study the corresponding mathematical expectation. At bottom, our remarks boil down to dealing with the problem of the gambler's ruin in the cases studied, but from a new point of view: the probability of ruin will be evaluated not for its own sake, but to establish certain propositions that bound deviations*" [Ville 1939, p. 78].

In 1938, Ville presented his results to the "Borel Seminar", which had actually been initiated by Ville and Doeblin [Crépel 1984b], and as he indicated, "*the discussion that followed has really been beneficial to me*" [Ville 1939, p. 2].[xvi] The benefit was doubtlessly shared, because starting the next year, Borel clarified the St. Petersburg paradox by associating with it a fair game in Ville's sense–*i.e.*, by constructing "a very simple martingale" in the symmetric game of heads or tails that gives Peter the Petersburg gains with the same probabilities, the stakes at each toss being the same for Peter and Paul and the players being perfectly interchangeable, thus restoring to the initial problem its lost symmetry and at the same time giving it added strangeness.

The St. Petersburg martingale

To avoid repetitions, we will present here the second Borelian version of this really simple martingale, the one from 1949–1950; the version before World War II differing only by a factor 2. So Paul and Peter play heads or tails, Peter winning if the coin falls tails. The stake on the first toss is 2, on the second 6, on the third 16, and so on, $(n+1)2^{n-1}$ on the nth toss. If Peter decides to stop playing as soon as he wins a toss, say toss n, his gain will obviously be the same as in the classical Petersburg game, 2^n, and the probability of this happening is indeed $1/2^n$; his cumulative loss, if he loses this nth toss along with all the previous ones, will be $n \cdot 2^n$. So if n is large enough, this loss will exceed Peter's total capital, and the game

[xv]Editors' note: In this introduction, Ville wrote "I have used certain results of Mr. Paul Lévy, who read part of the manuscript on this occasion. His observations have been very valuable to me." As the authors have already noted, Ville acknowledged in his thesis his use of Lévy's definition of conditional probability. He did not acknowledge having noticed or used Lévy's condition C.

[xvi]Editors' note: This sentence (and also the acknowledgment of Lévy's influence) appears in the introduction to the thesis, which is not in the version published by Borel.

will necessarily stop. The game is fair, mathematically and in practice, but cannot continue beyond Peter's or Paul's bankruptcy. This is the hidden face of the game that guarantees Peter a win every time, if he does not go bankrupt first. This remark, as Borel emphasizes, applies just as much to the classical martingale: double the stake in case of loss (see the appendix for details).

Ville's inequality is not very useful for these two martingales that oscillate freely on the whole real line, as Borel no doubt realized. In any case, the 1939 text stops there; Borel suggests that the two players should agree to limit the number of tosses, without exactly saying what would then happen and which of the two players, in this case, would have the advantage. The same remarks apply to §59 of his 1941 book *Le jeu, la chance et les théories scientifiques modernes*, which considers the same problem. As he often did, Borel only recopied what he had written earlier while adapting it to the public concerned (what public is concerned?). In this latter book, however, Borel devotes a section to "martingales" [Borel 1941, §31, pp. 93–97], in which he returns to the apparent paradox of a fair game that is advantageous for sure, and brings bankruptcy almost certainly if one is not careful. The reason is simple: "*martingales cannot have the effect of changing the conditions of the game,..., but they can change a lot the limits of risk* [Ibid., p. 93], going so far as to bankrupt those who play, their capital being too limited to tolerate for long such large deviations.

It is a pity that neither Borel nor Ville pursued their reflections on the mathematical and practical theory of martingales. The war broke out in September 1939 and stopped everything.[12] Borel, who had to leave Paris after his incarceration by the Germans in November 1941,[xvii] came back to the St. Petersburg martingale only ten years later, in three notes to the Academy of Sciences [Borel 1949b,c,d], quickly printed in the last edition of his *Éléments* [Borel 1909b/1950, note 10] and in his last "Que sais-je?" on probability [Borel 1950, final note]. It is hard to know why. At the time, Borel was publishing a series of notes on the classification of sets of measure zero, detailed in his *Éléments de théorie des ensembles*, one of the volumes of the new series he was editing at Albin Michel, the *Bibliothèque d'éducation par la science.* His *Éléments de la théorie des probabilités* having been out of print for a long time, Borel decided to give this series the job of reprinting the work, and this was done in 1950. It is possible that he wanted to add an original contribution to it and that he chosen the St. Petersburg problem. It is also possible, although quite unlikely, that Borel had been informed (by Ville or Fréchet) of the brilliant presentation by Doob [1949] at the colloquium on probability theory organized by the Centre National de la Recherche Scientifique (CNRS) at Lyon at the beginning of the summer of 1948.[13] During this presentation, Doob explained in front of Fréchet and

[xvii] Editors' note: See "Why did the Germans arrest and release Emile Borel in 1941?", by Laurent Mazliak and Glenn Shafer, arXiv:0811.1321.

Lévy his theory of martingales and two spectacular applications, the strong law of large numbers for independent and identically distributed variables and also the almost-sure Laplace-Bienaymé-Bernstein-von Mises theorem. (Recall that it is also Doob who showed in 1934 that Kolmogorov's strong law of large number is an elementary consequence of Birkhoff and Khinchin's ergodic theorem.)

Borel's martingale is fair if we bound the time

Summarized to an extreme, Borel's original contribution to the St. Petersburg problem in 1949–1950 consists in noting that his 1939 martingale possesses the following curious properties. If we write $X(n)$ for Peter's (algebraic) cumulative gain after the nth toss, $X(n)$ has expected value zero (the game is mathematically fair). Peter's gain is constantly negative until a tail happens. Only at that time, which we denote by T, does it become positive and equal to 2^T. Peter then quits the game with his gain. Borel shows that $E(T) = 2$, that $E(X(T))$ is infinite, and yet that if n is a fixed integer, we still have

$$E[X(\min(T, n))] = 0 \qquad (*)$$

(see the appendix), so that if Peter decides to avoid the risk of bankruptcy by not continuing beyond n tosses, the stopped game is still fair. It only becomes infinitely favorable if he lets it take its natural course, "virtually" infinite (and virtually infinitely risky for Peter as well as for Paul).

The principle of the impossibility of a gambling system now translates into the equalities (*). A martingale stopped at a "bounded" (stopping) time has a constant expected value: nothing can ever change the mathematical expected value if we bound the time (and besides, conversely, the equalities (*) are equivalent to the "martingale property", as we know). On the other hand, a "finite" but virtually infinite stopping time can arbitrarily change the value of the gambler's expectation, even if the likely value of this time is finite (equal to 2 here). A special theory must then be developed for these stopping times, which, in any case, is of concern only to the mathematician. Finally a satisfactory mathematical answer to all the martingale paradoxes and, at the same time, a reasonable mathematical statement of the principle of the impossibility of gambling systems, the equalities (*)!

Was Borel intoxicated by it for a moment? There is no longer a paradox for a finite distance fixed in advance; the game is and remains fair; the paradox simply results from considering a virtual infinity to which people have, in any case, only a virtual and paradoxical access. Borel explains clearly that the St. Petersburg paradox is a softened version of the real paradox of martingales: a fair game system susceptible of lasting indefinitely can become infinitely advantageous for the one who controls the stakes and the stopping of the game. And according to Borel, this paradox is even more paradoxical than the preceding one, because it amounts to saying that Peter can obtain the same advantages as in the St. Petersburg game, without giving a single cent to Paul in exchange [Borel 1950, p. 133] and without any possible complaint from the latter. Borel, who has seen many other paradoxes, does not let himself be defeated by this one. He concludes: "*we see that introducing infinity in a virtual form is enough to make no longer exact the principle according to which a game is fair if it is composed of a finite number of rounds each of which is fair* [*Ibid.*, p. 132]. Not only does "realized infinity", that prerogative of advanced mathematics, make an abstract discipline out of this beautiful science; but even unrealized virtual infinity is enough to lead the mathematician beyond the realities of our world (see also [Borel 1946]). The scholastic doctors would no doubt have liked to debate Borel's virtual infinity, which they would have located somewhere between the potential infinite of the peripatetic theory and the syncategorical infinity of the Paris School, but we do not know any more recent and less virtual commentary on this interesting Borelian contribution to the theory of the infinite.

No doubt people will object that this all does not go very far, that Borel's equalities (*) are already implicit in the proof of Ville's inequality of the gambler's ruin, which we mentioned above, and that in any case an average probability student knows (more or less) how to stop a martingale properly. True, but if he knows it, this is precisely because the theory of martingales and stopping times was constructed during the fifties and sixties by mathematicians directly or indirectly confronted with the paradox of Borel's martingales (see [Neveu 1972]). Who, in 1949, knew the St. Petersburg martingale and its virtually paradoxical behavior? One of us (Kai Lai Chung) conducted a quick inquiry with Doob (beginning of April 1999), from whence it emerged that the latter, the founder of modern martingale theory, never thought to establish a formula resembling (*) before his fundamental book of 1953, where it appears as a corollary of a general result. Doob testifies that he never knew (*) before this date.[14]

Borel could certainly have insisted more on the generality of his result, treating the 1908 paradox of returns to equilibrium in the same way, for example; he did not do this explicitly but he quickly suggested it in passing. He could also have proposed a general theory of equalities of the type (*), he did not do this either. Borel had the genius of brilliant ideas and at the same time that of hiding them under a bushel. One must never do too much; one risks encouraging mediocre mathematicians by explaining to them what is obvious at too great a length, and one risks discouraging manual laborers by drowning them in symbols. In fact, Borel did so little that nobody seems to have noticed that his 1950 "Que sais-je?" contributed an original mathematical solution to the St. Petersburg paradox and at the same time demonstrated for a particular case a remarkable identity of the (not yet invented) theory of stopped martingales, thus assuring the theory's rational coherence and representative value. Borel also does seems not to have noticed that all the inequalities (*) result from the single general theorem that the sequence $\{X(\min(T,n))\}$ is a new martingale, as Ville already did in 1938 in a (fundamental) particular case, and as everyone would do after Doob [1953]. But Borel resolutely does not care about the general theory of martingales that will soon become, it is well known, one of the great probabilistic theories of the sixties, with innumerable applications.

The important thing is elsewhere for Borel; he wants once more, and this time for the last time, to respond to Le Dantec: the probability calculus finally overcomes common sense, caught in the traps of martingales. Not only can the real scientist reduce common sense to the probability calculus as Laplace suggested, he can replace failing common sense by a probability calculus able, much more than Le Dantec's treatises, to deliver man finally from his anguishes and to satisfy his natural desire for certitude. This is the meaning of the 1950 “Que sais-je?”: *Probabilité et certitude*. The probability calculus leads to “absolute certainty”, not merely to the commonsense certainties of Laplace and Condorcet.[15]

The St. Petersburg problem

We still need to deal with the practical problem (should we play the Petersburg game, and at what price?), but on this point (which at bottom hardly interests him), Borel [1950], [1958] contents himself with the usual generalities, which we will not discuss, to which he adds, as we have already emphasized, the fundamental remark that a martingale, as seducing as it may be, presents the disturbing peculiarity that it exaggerates the deviations significantly even when we stop it at a fixed time. As Condorcet very appropriately observed, already in response to the St. Petersburg paradox, a game's being (mathematically) fair is not enough to make it neutral for the two players. One must also take account of the lesser or greater variability of the possible outcomes and the risks which these pose for the two players facing each other ([Condorcet 1994, see also Cournot 1843, §62]). To see this, it is simplest to calculate the distribution of Peter's gains $X(\min(T, n))$ for moderate values of n, say from 1 to 10. This is easy for the St. Petersburg martingale, it allows us to convince ourselves that Paul has a lot to lose playing this fair game with Peter.

We reproduce here a table calculated by James A. Given at to our request for the case of the martingale of returns to equilibrium. The table speaks for itself and does not particularly encourage Peter to play this fair game with Paul. Here T_1 designates as before the time of the first tail and $X(t)$ Peter's (cumulative) gain at time t (starting at zero) for a fair game of heads or tails. The rows in the table give the distributions of $X(\min(T_1, t))$, for t from 1 to 10. In the row for $t = 5$, for example, we read in the last column $P\{X(\min(T_1, t)) = 1\} = \frac{22}{32}$.

$t \backslash X$	−10	−9	−8	−7	−6	−5	−4	−3	−2	−1	0	1
1										$\frac{1}{2}$		$\frac{1}{2}$
2									$\frac{1}{4}$		$\frac{1}{4}$	$\frac{2}{4}$
3								$\frac{1}{8}$		$\frac{2}{8}$		$\frac{5}{8}$
4							$\frac{1}{16}$		$\frac{3}{16}$		$\frac{2}{16}$	$\frac{10}{16}$
5						$\frac{1}{32}$		$\frac{4}{32}$		$\frac{5}{32}$		$\frac{22}{32}$
6					$\frac{1}{64}$		$\frac{5}{64}$		$\frac{9}{64}$		$\frac{5}{64}$	$\frac{44}{64}$
7				$\frac{1}{128}$		$\frac{6}{128}$		$\frac{14}{128}$		$\frac{14}{128}$		$\frac{93}{128}$
8			$\frac{1}{256}$		$\frac{7}{256}$		$\frac{20}{256}$		$\frac{28}{256}$		$\frac{14}{256}$	$\frac{186}{256}$
9		$\frac{1}{512}$		$\frac{8}{512}$		$\frac{27}{512}$		$\frac{48}{512}$		$\frac{42}{512}$		$\frac{386}{512}$
10	$\frac{1}{1024}$		$\frac{9}{1024}$		$\frac{35}{1024}$		$\frac{75}{1024}$		$\frac{90}{1024}$		$\frac{42}{1024}$	$\frac{772}{1024}$

To be complete, let us mention that Borel was never interested in Daniel Bernoulli's theory of moral expectation [1738], the object of so much passion and so many comments in the years after the war. It is not exaggerated to think, as S. M. Stigler has written, that the St. Petersburg problem and the prisoner's dilemma have nourished a large part of the debate about many modern economic theories. Borel merely suggests that the notion of moral expectation is interesting from a psychological viewpoint but is now "abandoned" [Borel 1950, p. 93], at a time when Fréchet [1947] had just saluted its use by Buffon. This is a curious historical point, which it would be interesting to develop more, but which is relevant to a completely different subject (see *e.g.* [Jorland 1987], [Dutka 1988] and their references). Borel's solution, as we have said, is mathematical (and pedagogical), with very limited practical value. Once more, his goal is elsewhere. He does not want to encourage or discourage Peter's playing the St. Petersburg martingale or any other martingale, but to show how well executed mathematics can solve any paradox about this subject, contrary to the insinuations of certain negative thinkers. Borel seems not have been directly interested, moreover, in the economic theories of the years 1945–55. Recall, nonetheless, that he had been first to develop, starting in 1920, a theory of games of strategy that notably anticipated von Neumann and Morgenstern's game theory. In particular, his solution to the classical game of paper, rock, and scissors is marvelously clear. See [Borel 1938], written up by Ville, and [Borel 1941] for a general idea of this work, which we will not discuss here (see also [Fréchet 1959], [Guilbaud 1961], and [Dell'Aglio 1995]).[16]

In guise of a conclusion

But our history does not stop there, and does not stop at all: the St. Petersburg paradox will never cease to defy the human mind, and Borel knows it. A little before his death, he starts to write a history of energy that could be entitled *De Prométhée aux Curies.*[xviii] The manuscript, unfinished, seems to have been lost. But we have two sections reproduced in 1958 in the *Annales de l'Institut Henri Poincaré* [Borel 1958], which deal, as might be expected, with quantum theory and the St. Petersburg problem. Borel deals with his martingale one more time and underlines the great analogy he finds between the quantum hypothesis and one of the classical solutions of the St. Petersburg problem. Borel recalls, indeed, that the theorem of equipartition of energy in kinetics, otherwise so well confirmed, would require, if we want to apply it to black-body radiation, that we attribute an unbounded energy to the highest frequencies. Planck's solution consists in postulating that energy with frequency ν can propagate only by quanta of size $h\nu$, so that if this product exceeds the total available energy, it cannot be present and can be ignored, just like the very improbable events that affect the St. Petersburg game, whose realization is impossible. Borel concludes: *This total available energy, admittedly finite even if it has a high value, here plays the role played in the St. Petersburg paradox by the total fortune of the two players, about which we have no precise information, but which must admittedly have a value that is determined and hence finite* [Borel 1958, p. 5]. We can easily imagine that Borel would have liked to define grains of probability that could permit man to escape from the hell of paradoxes, and that he brought his last Petersburgian dreams to the Protestant square of the old cemetery at St. Affrique, where he rests in peace. *We finally throw earth on the coffin, and this is forever.*

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Appendix: Borel's martingales

Peter and Paul play the game of heads or tails. Peter wins when a tail comes up and loses otherwise. Let $\{Y_n\}$ denote the sequence of random variables taking the values $+1$ or -1 depending on whether the result of each successive toss is a tail or a head. A "Borel martingale" is formed by giving a sequence $\{b_n\}$ of nonnegative numbers and forming the sequence

$$X(n) = \sum_{k=1}^{n} b_k Y_k, \quad n \in \mathbb{N}.$$

[xviii] Editors' note: From Prometheus to the Curies.

Here b_n is the sum bet by Peter on the nth toss; for example, the St. Petersburg martingale corresponds to $b_n = (n+1)2^{n-1}$, so that $b_n - \sum_{k=1}^{n-1} b_k = 2^n$.

In all cases, $\{X(n)\}$ represents Peter's sequence of cumulative winnings during the game. It is a centered martingale in the sense of Ville and Doob. Peter's expectation of gain is zero on each round, and knowledge of the outcomes up to the nth round does not change at all what our player can hope for on the next round. The paradox is that if the game is stopped at a time depending only on rounds already played, what has been called a stopping time since Doob, Peter's expectation of gain may be given an arbitrary value, finally even infinite.

In the example of the St. Petersburg martingale, the stopping time is simply the moment a tail first appears—that is, the first time Peter wins. Again let T denote this time. We obviously have

$$P\{T = n\} = 2^{-n}, \quad E(T) = 2, \quad E[X(T)] = \sum_{n=1}^{\infty} \left(b_n - \sum_{k=1}^{n-1} b_k \right) 2^{-n},$$

the latter quantity being equal to an infinity of 1s in the case considered.

In general, the game would remain fair for a Borel martingale, stopped at the time T of the first tail, if the series with terms $b_n 2^n$ converged, in which case we would have

$$E[X(T)] = \sum_{n=1}^{\infty} \left(b_n - \sum_{k=1}^{n-1} b_k \right) 2^{-n} = \sum_{n=1}^{\infty} b_n 2^{-n} - \sum_{n=1}^{\infty} 2^{-n} \sum_{k=1}^{n-1} b_k = 0.$$

Borel now fixes a time n and undertakes to calculate the expected value $E(X(\min(T,n))$ as we have explained. This expected value is composed of two parts, one positive and equal to $\sum_{k=1}^{n} (b_k - \sum_{j=1}^{k-1} b_j)2^{-k}$, the other negative and equal to $(\sum_{k=1}^{n} b_k)2^{-n}$. The two cancel out, as we can see by developing the double sum of the positive part, or simply by noting, as Borel did [1950, final note], that in the Petersburg case the two sides are equal at n and supposing, no doubt, that this is enough to carry conviction in general. (This is Poincaré's principle of sufficient reason: if a particular relatively ordinary case is verified, this is sufficient evidence for the generality of the property outside a set of measure zero.)

As a second example of his formula, Borel gives the best known martingale, sometimes called d'Alembert's martingale, in which Peter doubles his bet at each toss until a tail appears. This martingale corresponds to the choice $b_n = 2^{n-1}$, so that $b_n - \sum_{k=1}^{n-1} b_k = 1$, from which it follows that $X(T) = 1$; the game thus becomes favorable to Peter, but if he stops it at a time n fixed in advance, it becomes mathematically fair again, $E(X(\min(T,n)) = 0$.

Borel seems to suggest that the same equality is verified by his 1908 martingale, where Peter stops at the first tail after a return to equilibrium. According to Le Dantec [1910, pp. 354–355], this martingale is known to gamblers as "making" Charlemagne. It is certainly a Borel martingale. This time, Peter does not change his bet, which remains constant (say one ruble for each n): Peter's cumulative gain $X(n)$ at the nth toss is then equal to the sum of the variables Y over the n first tosses. For the martingale to become advantageous to Peter, it is not enough to stop at the time T of the first tail; in general, he must wait longer. Borel chooses to stop the game after the first tail after a return to equilibrium.

So set $T_1 = \inf\{n; X(n) = 1\}$. For a very long time, it has been known that T_1 is finite with probability 1; this is also a consequence of Borel's lemma as stated earlier.[17] Here Borel would interject, quite on the mark, that T_1, though almost certainly finite, is "virtually infinite" because it cannot be bounded by a fixed quantity, no matter how large. By exploiting the ballot argument already presented above, we easily obtain, following Bertrand [1887b, 1888], Borel, Bachelier and many others (*e.g.* [Feller 1950])

$$P\{T_1 = 2n+1\} = \frac{1}{2n+1} C_{2n+1}^{n} \frac{1}{2^{2n+1}}$$

for every integer n, and consequently $E(T_1) = \infty$, which makes Peter's average waiting time for a gain pretty long. We evidently have $X(T_1) = 1$, (and if Peter repeatedly starts again playing the same way, he will have an infinite gain in the end). Nevertheless, Borel seems to indicate, in his last works of 1949–1950, that for each fixed n we still have $E(X(\min(T_1, n)) = 0$. Of course, this is exactly right and in fact very easy to show directly—in the following way, for example: fix n, and try to evaluate $E(X(\min(T_1, n))$. This expected value is composed of two parts, a positive part from stopping before $2n+1$, equal to $P\{T_1 \leq 2n+1\}$, and a negative part from the contrary case, equal to

$$\begin{aligned} E[X(2n+1)1_{\{T_1>2n+1\}}] &= -E\left[X(2n+1)1_{\{T_1\leq 2n+1\}}\right] \\ &= \left[\sum_{k=1}^{n} E\left[(1+Y_{2k+2}+\ldots+Y_{2n+1})1_{\{T_1=2k+1\}}\right]\right] \\ &= -P\{T_1 \leq 2n+1\}. \end{aligned}$$

Yet Borel wrote down neither this reasoning nor any other. Might he have half-heartedly tried to replace it with a combinatorial calculation, which does not work out straightforwardly, and then concluded that the effort had become "fastidious"?

Notes

1. See *e.g.* [Cournot 1843], [Todhunter 1865], [Samuelson 1977], [Jorland 1987], [Dutka 1988], [Hald 1990, 1998] or [Condorcet 1994] for references and developments. For some time, S. Csôrgô and G. Simons have been announcing a whole book dedicated to the St. Petersburg paradox. The paradox has generated an immense literature over a period of three hundred years, and it is hard to imagine this literature coming to an end.

2. Together with her future husband, Raïssa Maritain followed Le Dantec's lectures in the new premises of the Paris Faculty of Sciences at the beginning of the century; for them, "*he was the most brilliant, the most engaging of our professors*", a "*good, generous, loyal man*" with whom they had a relationship of intimate friendship. His theory of consciousness as an epiphenomenon and his "*convinced, absolute and calm*" atheism did not prevent Jacques and Raïssa Maritain being seduced by the intuitionism of Bergson, whose lectures at the College of France were having a considerable success at the time. Soon afterwards, they were converted to Catholicism by Léon Bloy and adhered to an integral Thomism diametrically opposed to the philosophy of Le Dantec, who united with Borel (for once) against those who divert the youth from "modern Science" and true intuition, and polemicized against Bergson for some time. (On this subject, see [Maritain 1941, pp. 75–77 ff], [Maritain 1910], [Borel 1907a, to appear], etc.)

3. What interested Le Dantec above all was the "phenomenon of life", which he intended to deal with as a physicist, like Fourier dealt with heat. The evolution of species must therefore be explained from the inside by the properties of the living and not from the outside by the chance of circumstances. He writes, for example: "*Darwin never asked himself about the cause of variations of living beings. For that he would have had to ask what life is... He attributed the variations to chance, and their conservation or destruction to environmental factors external to the living being itself. This belief in the possibility of an explanation of coordination by chance is related to belief in the* laws of chance*; it is common to many eminent minds, among naturalists as well as among mathematicians...*" [Le Dantec 1909, p. 268]. The principle of "natural selection" merely assures that the survivors survive and the others disappear, it is nothing more than an explanation *a posteriori* or a tautology. Here Le Dantec comes back to one of the classical criticisms of Darwinism (*e.g.* [Schiller 1979, chap. XII]). He writes: "*We could say that the principle of natural selection explains that* at every moment things are as they are and not otherwise, *and that this was true at every moment in the history of the world...; there is the whole principle of the great English evolutionist*" [Le Dantec 1909, p. 269].

The neo-Darwinists were treated no better by Le Dantec. Weismann only exaggerates the Master's thinking, by attributing to sexual reproduction, to the mixing of "germinal cells", the principal responsibility for the variation of species. Bringing in invisible corpuscles that would be separately responsible for an individual's characters is metaphysical obscurantism, if not an assimilation of heredity to a "microbial" infection and a denial of the unity of the living being; all the more so because Weismann, more coherent than Darwin, deduces from it the impossibility of inheritance of acquired characters, which leads to the absolute impossibility of all transformism. With regard to the new theory in vogue, Hugo de Vries's sudden and risky mutations [Le Dantec 1909], it can explain at most "ornamental" variations, without selective value of any sort, which do not affect "mechanisms of life"; such a theory of mutations distorts the true Lamarckian philosophical transformism [1909]. Le Dantec thinks that chance intervenes without discernment, without law, at " lower scale", but according to him, the global adaptation of living organisms to their environment depends on an internal "Mechanics" that Lamarck [1809] had already identified well enough: living conditions, acquired habits, use and non-use, progressively modify the chemical patrimony that the individuals transmit to their descendants. The fundamental equation of this mechanics of life, which describe the Lamarck-Le Dantec complexification process, is given below, in note 10.

Le Dantec's position with respect to Darwinism is curiously close to Cournot's [1872,1875], although opposite on every point. Like Le Dantec, Cournot was interested first of all in the "phenomenon of life", which according to him, requires the intervention of a specific "vital" force. Cournot thought that the selection of advantageous random variations probably explains certain simple adaptations of a species to its environment, but cannot account for the complexity of the finalized functional organs of a living creature. Nature would have to constantly produce infinitely numerous deviations from existing species in order for selection from random circumstances to be sufficient to explain even an elephant's trunk. (What selective advantage would there be in a trunk a bit longer than an ordinary snout but too short to reach the ground? An elephant would have to see a sudden growth of its trunk to its present size in order for it to provide a substantial advantage in the struggle for survival, and this violates not only the theory of gradual selection advanced by Darwin but also the surprising stability of species, whose variability is tightly governed by standard ranges. Or else we would have to imagine that nature herself selects elephants in the same way as a breeder, to obtain races of elephant with longer and longer trunks until they reach a sufficient size; but then this is no longer random selection, instead a subscription to the thesis of an intelligent nature that Darwin in fact disputes. Or else we would have to conjecture a coordinated evolution an elephant's size and snout, and therefore numerous intermediary species that are in fact not to be found according to Cuvier, etc.)

Chance alone cannot explain the coordination of parts towards a goal, vision for an eye, hearing for an ear, etc., just as it cannot write the Iliad letter by letter, blindly. And even if chance were to compose an eye or the Iliad, it would take so many billions and billions of millennia that there would not remain enough time to write the Odyssey or the tragedies of Racine, or to complete a living being by giving it a functional ear and nose as we know them (a commonsense statement that deserves refutation or confirmation by a calculation, easy in the case of the Iliad [Borel 1914], but not that clear in the case of the eye, see *e.g.* [Kimura 1983]). Cournot then asks why excavations carried out randomly to construct railways, quarries, and mines did not manage to reveal the "innumerable intermediaries" which should, according to Darwin, explain the current species, but instead provided the observer of extinct species only remains very distinct from each other; chance could not have produced such a selection unless the ancient fauna and flora were already like that.

The mechanisms Darwin imagined to justify the thesis of the evolution of species were thus insufficient, although the thesis itself, in Cournot's eyes, was completely plausible and undoubtedly constituted one of the fundamental scientific contributions of the XIXth century. The theory of philosophical probabilities that Cournot extracted from the calculus of chances thus permitted a "truly scientific" critique of the Darwinian theory (for this topic, see the fundamental works of T. Martin [1996]) and to demonstrate its weaknesses, which were obvious for Le Dantec. But for the latter, it is because chance is not subject to any law that it cannot explain the evolution of species, whereas for Cournot, it is precisely because chance is subject to laws that he can dispute Darwinian reasoning and thus test Cournotian philosophical criticism on an interesting example. As for Le Dantec's as well as Cournot's proposals to account for the transformation of species, they are at least as debatable as those given by Darwin, to whom Cournot, as well as Le Dantec, nevertheless gives great credit for having "*gathered together the various conditions that permit us, if not to scientifically solve* [the question of the genesis of organic types], *at least to attack it methodically*" [Cournot 1875, p. 98].

There are many other French reactions to Darwinism, contemporary to the work of Le Dantec, see [Giard 1904], [Le Dantec 1899, 1909], [Delage, Goldsmith 1909], [Labbé 1929], [Conry 1974], etc. The questions related to the mechanisms of evolution, quite complicated, are moreover far from being resolved. The classical essay of Jacques Monod [1970], for example, can be consulted to justify the thesis according to which, "*Chance alone is the source of all novelty, of all creation in the biosphere. Pure chance, chance alone, absolute but blind liberty, at the very origin of the prodigious edifice of evolution*". To answer the objection of the improbability of producing randomly a living organism of any complexity, Monod takes up for himself one of d'Alembert's most famous paralogisms: "*The a priori probability that a particular event happens out of all the possible events in the Universe is close to zero. Nevertheless, the Universe exists. Particular events must happen, whose probability (before the event) was infinitesimal* [Monod 1970, p. 184]. Borel, Le Dantec, and Cournot would have smiled at such candor. But Le Dantec might have approved the following extract from the same work, in which Monod deals with Evolution [1970, chap. 7, pp. 162–163]: "*The theory of selection has been too often understood or presented as treating only the conditions of the* external environment *as agents of selection. This is a* *completely erroneous conception. For the external conditions are in no way independent of the teleonomical performances characteristic of the species... It is obvious that the part played by teleonomical performances in the orientation of selection becomes more and more important with the organism's level of organization and hence* autonomy *with respect to the environment. So much so that this part can probably be considered decisive among superior organisms, whose survival and reproduction depend mainly on their behavior.*"

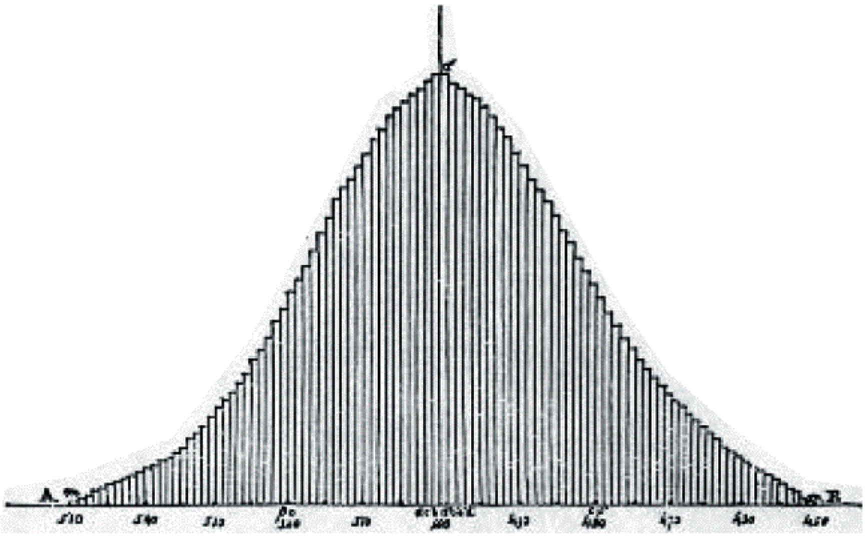
For modern probabilistic theories of molecular evolution and a more detailed discussion of the evolution issue, see Kimura's synthesis [1983]. For up-to-date information on the theory of evolution, see [Devillers, Chaline 1989], which proposes an alternative explanation of Lamarck's giraffe neck and hence of Cournot's elephant trunk. On the uses of "chance" in biology at the end of the XIXth century, see Charles Lenay's beautiful thesis [1989]. See also [Gayon 1992], [Pichot 1993], etc.; the literature on these topics is too rich and abundant to all be cited.

4. People are often content to say, here as in the case of the St. Petersburg game that all possible calculations lead to paradoxical results because the expected waiting time until return to equilibrium is infinite, *e.g.* [Richard-Foy 1910]. Borel looks deeper, as does Feller later [1950/1968, p. 314 and chap. 3].

5. The polemic between Jules Tannery and Le Dantec on the epiphenomenon of consciousness in the *Revue du mois* [Tannery 1906], [Le Dantec 1906] is interesting to read and helps us understand why Borel took a stand against the Le Dantec biophilosophy, which leaves nothing to Man, not even the probability calculus. Borel nevertheless published two of his books and many of his articles or columns, in particular his polemic with Bergson in 1907, which had (good) Borelian sense. But Le Dantec was hard to incorporate into Borel's system or into any system other than his own. After 1911, Le Dantec, no longer published in Borel's series at Alcan or in the *Revue du mois*. The review nevertheless published an excellent critique of one of his last books [Le Dantec 1913].

Let us mention that Le Dantec's letters to Borel, which cover the period from the end of November 1905 to December 1911 and deal with various important points of the polemic presented here, are now accessible in the archives of the Academy of Sciences, Fonds Borel, M176 to 178, RM 185 to 187. We do not know the location of Borel's answers or Dantec's general correspondence, which would certainly merit study.

6. The graphical representation of temporal phenomena had then become customary in experimental science, especially after the deployment of the graphical recorders of Jules Marey (1830–1904), see *e.g.* [Braun 1992]. So



Quetelet's path [1846, p. 103] *for the game of heads or tails* [Stigler 1986, p. 209]. *Distribution of the number of white balls observed in a draw of 999 balls "at the same time" from an urn containing white and black balls in equal and infinite number.*

reasoning about "trajectories" in the game of heads or tails might derive, in part, from the recording of biological processes by physiologists of the end of the XIXth century and from Félix Le Dantec's common sense. Neither Bertrand, nor Poincaré, nor Borel in 1909 traced Le Dantec's path, which could give weight to the hypothesis ventured here. "Curves of possibilities" and then "curves of frequencies" had been used long before in the probability calculus, particularly by Laplace, Cournot and Quetelet, but this way of representing the probabilities or frequencies of various possible results is completely foreign to Le Dantec's path, and even opposed to it. Darwin against Lamarck: group portraits by Quetelet, individual portraits by Le Dantec. Quetelet's curves, empirical or theoretical, like the one sketched below, for example, represent sets of n rounds of heads or tails that are simultaneous or without a sense of temporal order. They thus display the mathematicians' laws of chance, such as Bernoulli's theorem (weak law of large numbers), De Moivre's theorem, or even, if we imagine n tending towards infinity, the almost sure theorems that describe the convergence of the empirical distribution to the theoretical distribution. Le Dantec's path, which for its part deals with the individual history of one game of heads or tails lasting a long time, shows other laws of chance of the mathematicians (such as Le Dantec's law of chance, although he denies it): Borel's strong law of large numbers, Le Dantec's recurrence law, the law of returns to the origin, etc., which Quetelet's curve does not allow us to see, because it drowns individual histories in the mass and prevents their expressing themselves in the long run.

On the other hand, we find Le Dantec's empirical paths, representing the evolution of a quantity across time, in growing numbers all through the

XIXth century, in the literature on finance, physics, biology, statistics, etc., and in Quetelet's work in particular (see [Droesbeke, Tassi 1990]). This was in fact the goal of Marey's recorders, which Le Dantec knew well and which he seems to have adapted to the (mathematical) game of heads or tails. Here is one of those natural ideas, obvious afterwards, but which change everything: the proof is that Le Dantec demonstrates the recurrence property better than Ampère, Laplace, and Borel together, simply by looking at the path. In the same way, Bachelier's thesis [1900] gets its main originality from the consideration of paths of stock market prices; for him, they represent the gains of a gambler who would play heads or tails continuously in time. They thus become Le Dantec's paths (before the letter) traced on paper with infinitely small squares, and all formulas of the classical game of heads or tails then find very simple continuous analogs, even simpler than those from which they come. See also notes 8 and 9.

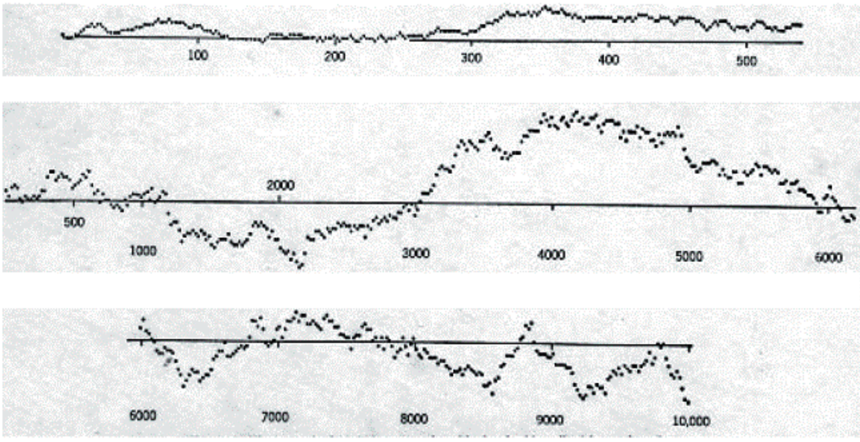
7. Borel's wife, the novelist Camille Marbo, was adept at polemics. It is well known that she wrote for him some of his most skillful answers to Lebesgue when their debates were most intense [Borel 1919], [Lebesgue 1991].

8. This detail, which Borel did not think useful to present, allows us to measure the originality of his method retrospectively. William Feller (1906–1970), one of the great representatives of modern probability theory, whose "volume I" [Feller 1950/1968] is usually considered, with good reason, as a summit of the probabilistic literature of the XXth century, completely rewrote chapter III for the third edition in 1968. He started from a *main lemma*, whose significance for the study of the fluctuations in the game of heads or tails, he says in a note, is "recent" (in 1968). Now this *main lemma* is precisely Borel's equality of 1911–1914:

$$P\{S_1 \neq 0, S_2 \neq 0, ..., S_{2n} \neq 0\} = \frac{E(|S_{2n}|)}{2n} = P\{S_{2n} = 0\} \approx \frac{1}{\sqrt{\pi n}}.$$

Feller's proof of the *main lemma* is rather less elegant than the one we just presented following Borel, but this is a matter of taste. From this *main lemma* follows in particular the arcsine law for the game of heads or tails, which Bachelier [1915, 1925] had already computed well enough in the case of the fluctuations of an interest rate, reasoning like Borel about Le Dantec's path seen from afar. Moreover, Louis Bachelier was very likely inspired by the Borel-Le Dantec polemic on the paradoxical periodicities of the game of heads or tails, published in 1914 in *Le Hasard*, which would prove that this book, unique and so little read, had at least one reader. Let us recall that the arcsine law was demonstrated for Brownian motion by Lévy [1939] and directly for the game of heads or tails by Chung and Feller [1949]. See [Feller 1950/1968] for other references.

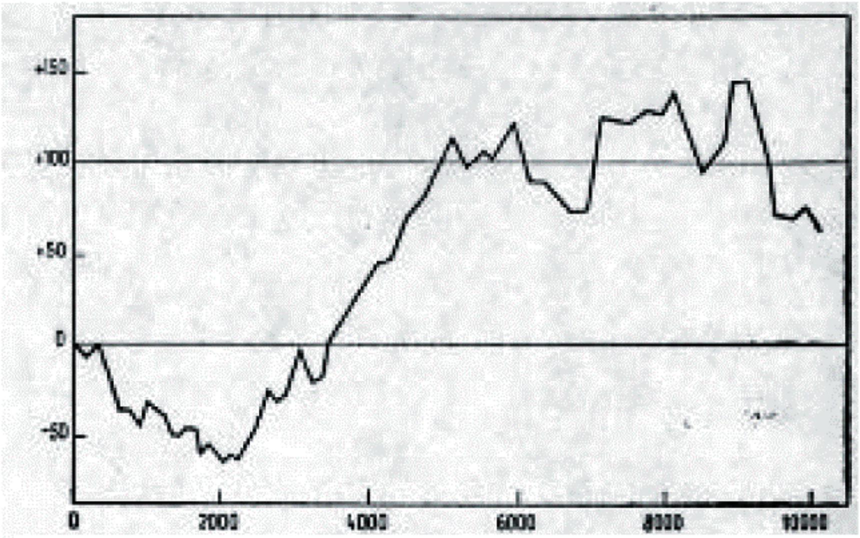
Feller who, according to certain sources, was not far from thinking of Borel what Borel thought of Le Dantec, might have profited from a more attentive



Feller's path [1950/1968, p. 87]. *Computer simulation of 10000 rounds of heads or tails. The first path represents the first 550 games, the next two lines represent the 10000 games, the horizontal scale being compressed by a factor of 10 without changing the vertical scale.*

reading of *Le Hasard*. Let us note nevertheless that, without knowing it, Feller reproduced Monsieur Le Dantec's sinuous path in his volume I and commented on it in a completely Borelian way [Feller 1950/1968, p. 87]. We heartily recommend that beginning readers (if there are any) consult pages 46–53 of *Le Hasard* if they want to understand why the arcsine law has that strange U form.

9. Borel's brother-in-law Jacques Duclaux (1877–1978), professor of general biology at the College of France, conducted interesting experiments in the game of heads or tails. In one of his popular books [1959], he traces Le Dantec's sinuous curve along 10240 tosses, which he claims he obtained in six weeks, one hour a day. Jacques Duclaux thus beat by a short head the record established by the South-African statistician John Kerrich, who used the leisure of a long captivity in Denmark during the war to conduct probabilistic experiments. Kerrich's results were analyzed in a book published in Copenhagen in 1946; he analyzes 10000 tosses of a coin and 5000 draws from an urn containing two red balls and two green balls. Feller [1968, p. 87] uses, for his part, a computer simulation of 10000 tosses of heads or tails (see the preceding note). Buffon's experiments, which we will recall later in note 17, are known, perhaps also those of Quetelet (note 5), and Westergaard and Weldon [Hald 1998], [Stigler 1999]. Less known in France are the statements of the Genoese lottery of Prague and Brno, analyzed over 133 years by Czuber [1889, 1902], etc. But none of these scientists traced the sinuous path of the game of heads or tails. This seems to belong to Le Dantec (unless his title is disputed).



Duclaux's path [1959, p. 75] *(10240 rounds of heads or tails). A gambler who wants to get rich using the martingale* "leave the game at the first gain after a return to equilibrium and start again indefinitely" *must definitely have patience and not fear overdrafts at the bank.*

10. If we believe the *Tribune de Lausanne* of June 16, 1917: "*Le Dantec was one of the most representative men of contemporary atheism and materialism, a bigot of negation, an apostle of nothingness...*", but he also was a prophetic mathematician without knowing it, and according to those close to him a sensitive being and attentive friend, who stoically stood up to the torments of a cruel illness from which he died on June 6, 1917. Le Dantec spent many long stays at the Sanatorium Mangini d'Hauteville in Bugey; he wrote there in particular *Le Conflit* [Le Dantec 1901], which had many editions. A volunteer in the army's health services during the war, he did not withstand the exhaustion from his battle for the "sacred rights" of the human person ridiculed by Austro-German barbarism [Le Dantec 1917b]. (Recall that the botanist Noël Bernard (1874–1911) also died of tuberculosis; see for example [Lebesgue 1991].)

Le Dantec did not hesitate to attribute self-consciousness to aphids and considered thought as a property of "raw matter", anticipating the most extreme theses of contemporary cognitivists. For this alone he is worth remembering.

For details about the life and work of Félix Le Dantec, see [Moreau 1917], [Pérez 1917], [Lenoir 1919], [Sageret 1924], [Reinach 1926] or [Bonnet 1930]. Charles Pérez (1873–1952), normalian in the class of 1894 and one of the intimates of Le Dantec (and of Lebesgue and Noël Bernard, of whom he was a classmate), professor at the Paris Faculty of Sciences, wrote Le Dantec's obituary in the annual of the École normale supérieure in 1918, which we have used.

There does not seem to be any recent book on Le Dantec's philosophical and scientific work. Let us only point out that his whole "biological philosophy" can be summarized in a law of evolution of the type $A_{n+1} = A_n + A_n \cdot B_n$, where A_n denotes the state of an individual at time n and B_n the action of the environment at the same time, so that for Le Dantec [1909, p. 37]: "*it is obvious that A_n, namely the body of the individual, at a given time, was not foreseen in the egg, but is the result of an evolution, a history... We can say: an individual is a history.*"

The same equation controls as well the evolution of species and gives form to the "fundamental biogenetic law" according to which ontogeny and phylogeny are images of one another (the development of an individual from the embryo to the adult reproduces the development of the species; notice that this same fundamental law, attributed to various authors, is considered by Dedebant and Machado as a biological metaphor of the ergodic principle of mechanics [Dedebant, Machado, 1963]). Le Dantec's equation applies in particular to the genetic patrimony of an individual (or of a species), reducing the fixed Mendelian inheritance to its proper allotment (the Mendelian characters specify form and color but do not affect the vital mechanisms: the Mendelian peas are certainly smooth or wrinkled, but their inherited patrimony evolves with time according to Le Dantec and Lamarck's laws). This very move completed the marginalization Le Dantec's extreme transformism at the beginning of this century of the "genome", fixed, independent of the environment, and sovereign. As for Le Dantec, it is clear enough that his equation of evolution is stochastic, the action of the environment on the individual not being subject to any law, so that without wanting to, Le Dantec also anticipated Bernstein, von Mises and Hostinsky's stochastic non-hereditary schemas. He was, in spite of himself, a pioneer of the Markovian studies of which he surely would have thought the worst.

On the philosophical front, Sageret [1924] and especially Le Dantec [1907a] himself suggest that Bergson only "poetizes", for ladies and metaphysicians, Le Dantec's biomathematical philosophy in *Evolution créatrice* [Bergson 1907a]; the polemic that ensues in Borel's journal is rather astonishing (see [Bergson 1907b] and [Callens 1997]). We could also reread *Jean Barois* to bathe ourselves in the atmosphere of that time. Barois is in fact a true disciple of Le Dantec, who is even cited in the lesson on transformism the hero presents in front of the director of the Collège Venceslas: Lamarck's transformism reworked by Le Dantec is the definitive scientific truth, opposed to the holy fathers' perfidious relativism. And Luce's death is a troubling anticipation of Le Dantec's [Martin du Gard 1913]: "*The last act is bloody, no matter how pleasant the comedy overall.*"

11. Let us nevertheless note that according to the testimony of L. J. Savage, who knows what he is talking about, Borel was the first to have proposed a defense of "personal probabilities", in his critique of the treatise by Keynes [1921] (see [Borel 1924]). We rather imagine that Borel's personal probabilities are tied in one way or the other to his reflections on the personal chance of Le Dantec, who, under this hypothesis would also become, without having wanted it, one of the pioneers of the modern subjective theories.

12. Jean Ville, professor at the Nantes lycée, was mobilized in the Artillery. Taken prisoner in June 1940, he was incarcerated at the Oflag XVIIA at Edelbach, in Austria, until autumn 1941.[xix] For one year he was responsible for the course on probability and, in collaboration with Frédéric Roger, for the course in differential and integral calculus at the Oflag's "study center", directed by Jean Leray (1906–1998), one of the century's great French mathematicians [Leray 2000]. Edelbach's study center is no doubt the most famous university in captivity of the second world war. It was competitive with many French universities of the time, and its history is not written. Two future members of the Academy of Sciences, professors at the College of France, taught there for five years, Leray in mathematics and Étienne Wolff in natural sciences. Both made beautiful discoveries while they were there (French national archives AJ/16/5826).

Liberated at the beginning of classes in 1941, Ville went back to his job in Nantes; he taught statistical correlation as the winner of the Peccot prize at the College of France in 1942 [Ville 1955, p. 10]. He soon moved to the University of Poitiers, and in 1943 to the University of Lyon. Frédéric Roger, a brilliant student of Fréchet and Denjoy, was liberated and sent to the Berlin Academy and then to a German university, with Christian Pauc (causing them a lot of trouble at the Liberation, Fréchet Archives carton 11). Ville and Roger were replaced at the Edelbach center by Camille Lebossé (1905–1995) and Corentin Hémery (1909–1992), both ex-students of the École normale supérieure of Saint-Cloud and mathematics professors at the Lycée Pasteur. Through their works, published by Fernand Nathan, Lebossé and Hémery educated whole generations of students at the secondary level, until "modern mathematics" came, for a time, to compete with them (it is not impossible that Leray's well known hostility to the introduction of modernism in mathematical teaching at the secondary level came in part from his long acquaintance with Lebossé and Hémery at Edelbach). The Oflag at Edelbach is also known for the number and length of tunnels dug for collective escapes; in particular, it was the location of the "great escape" of 143 prisoners at the same time, but we leave our subject.

Ville then seems to have dropped his interest in martingales, perhaps after reading Doob's first article on the subject [1940], which seemed to wrap up magisterially and definitively the theory Ville had so brilliantly initiated, and probably also because the applications Ville had in mind were related to a topic Paul Lévy had been studying at the same time unbeknownst to him: the geometry of vectorial Brownian motion. When, back from captivity, Ville

[xix]Editors' note: Documents made available in the French national archives after this was written show that he was released in June 1941.

published a first note on this subject [Ville 1942], Fréchet, probably alerted by Lévy, with whom he corresponded very regularly, called to his attention an article by Lévy [1940] in the *American Journal of Mathematics.* In a letter addressed to Fréchet dated June 4, 1943 (carton 5 of the Fréchet archives at the Academy of Sciences), Ville acknowledged very lucidly that he had tackled the same problems as Lévy without knowing it; Lévy being ahead of him on many important points, he withdrew his own work. (This is really a pity; in his note of July 1942, Ville demonstrates, through a martingale method, that a Brownian motion starting at zero in three dimensions almost surely does not return to zero, and thus that the double points on the time axis form a set of measure zero. He asks the same question for the case of two dimensions, suggesting that the answer is positive, as Lévy showed independently in his own way ([1940] and [1948, p. 257]).

We may also note that Lévy always adopted a very reserved attitude towards Ville; in a letter to Fréchet he maintained that Ville had never been more than a student without great originality (on all these questions, see the very interesting thesis defended by Bernard Locker in 2000).[xx] He was wrong, as he also was in most of his scientific judgements concerning the work of mathematicians of his time; as he himself very willingly admitted [Lévy 1970], he read them very little and rather badly. We can also imagine a thousand other reasons for Ville's giving up martingale theory, one of the most promising of the following half-century: the conditions of life and work under the Occupation, and other private or public interests of which we know almost nothing.

13. In 1947, following an academic disagreement, Ville left the University of Lyon[xxi] (where he was replaced by Max Eger) for a research engineering position at the Alsacienne de Construction Mécanique (which will become Alcatel); he then takes an interest in the transmission of information and telecommunications; his presentation at the Lyon Colloquium presents some of his results, all very far from the ideas in his thesis [Ville 1948, 1949]. Moreover, Ville, not on the best terms with the Lyon faculty, seems not to have taken an active part in the colloquium, to which he had been invited early on. According to Fréchet [Lyon 1949, p. 47], all the planned lectures, and hence Ville's, were in fact presented. It is possible that Ville was physically present only on the day of his talk, and that he did not attend Doob's lecture (we may mention that only C. R. Rao, who attended the colloquium without giving a lecture [Lyon 1949, p. 26], dared to speak up after Doob's lecture). It seems to be established in any case that Ville and Doob did not meet in Lyon (personal communication from Ville to P. Crépel [1984b]). Doob,

[xx]Editors' note: [Locker 2000]. See also *Paul Lévy, Maurice Fréchet. 50 ans de correspondance mathématique*, edited with notes and commentary by Marc Barbut, Bernard Locker, and Laurent Mazliak, Paris, Hermann, 2004.

[xxi]Editors' note: On March 4, 1947, Ville was granted a leave of absence by the Ministry of Education, effective October 16, 1946.

for his part, did not recall ever meeting Ville, in Lyon or elsewhere (personal communication from Doob to K. L. Chung). Ville or Fréchet might nevertheless have talked with Borel again during the editing of the proceedings about these questions from before the Debacle.[xxii] We lack any positive evidence on this topic and will not say anything more about it.

In 1956, Jean Ville was named professor in econometrics in the Paris Faculty of Sciences, where he completed his career while continuing his activities as scientific consultant at Alcatel.[xxiii] For more information on the participation of French mathematicians in the telecommunication industry after the second world war, see J. Segal's brilliant thesis [1998].

14. In a letter dated April 6, 1999, which constitutes a first draft of this article, one of us (K. L. Chung) concludes: "*Tout le monde, from d'Alembert, Buffon,..., onward to Bertrand, Poincaré, Czuber, Coolidge (Doob's teacher in Harvard who wrote a textbook on probability in which he discusses Petersburg at length). All these people considered limiting the number of bets to a fixed . BUT PERSONNE never thought of conjecturing (*)!!! They computed all kinds of probabilities under various conditions of limiting the number REALISTICALLY, but never had the audacity of testing a few cases of (*). (Did I not send to you a computer printout for up to* 10*?) This is* most curious *and worthy of a HISTORICAL ÉNONCÉ. For martingale it is a large watershed, missed by Ville and Doob!*" In another letter written a bit earlier, the same remark: "*As far as I recall, nobody ever tried to compute the expectations for a stopped game. The reason is obvious: nobody converted the Petersburg game into a martingale as Borel did (1938/9). For this reason it is better to take up a simpler game: that of equitable coin-tossing... If return to* 0 *is certain, then after each return there is* $\frac{1}{2}$ *probability of winning* 1 *(sou), and therefore by Borel 's lemma (no need of Cantelli) it is certain that the gambler will win* 1*. Many words were wasted on how long it takes and how much he can suffer to lose (before win!). Tout le monde talked this kind of rot. Nobody thought of computing ((min( )))! Nor did Borel himself for this "game". His Petersburg martingale is unfortunately too complicated, for perhaps people like Dantec, and "before its time" for Buffon et al.*"[xxiv]

15. Borel will even go further in 1953 in his last "Que sais-je?", *Les nombres premiers*, where he undertakes to show how the probability calculus sometimes allow us to reduce to certainties the most profound conjectures on the "formidable and sacred mysteries of numbers", a romantic utopia long present in Borel's work [1929, 1952], which no one had to take seriously, but which allows us to get to the bottom of the Borelian enterprise of popularization. Hermite's analytic theory, Hilbert's algebraic theory, and their imitators, fascinating as they are, are accessible only to very small cenacles, whereas the contemplation of numbers is and should be an essential element of Humanity's culture. The probability calculus allows the impossible popularization of the highest science, that of numbers, so there is that much more need to rid it of the paradoxes and illusions that centuries of ignorance have burdened it with, for want of sufficient belief in the universality of human reason. On this point as on others, Borel was hardly heard by his contemporaries, for a few years later his "Que sais-je?" on prime numbers was rewritten in strictly proper algebraic language [Itard 1969]. Let us add that the latest version of the same "Que sais-je?" ([Mendès-France, Tenenbaum 1997]) is a little less distant from Borelian ideas, which have virtual timelessness in their favor, although it has become difficult to get any of the books where they are presented; the Borelian "Que sais-je?" in particular are unobtainable.

16. We can easily understand why Borel makes no precise reference to the various empirical and theoretical "laws of large numbers" that scientists, from Buffon and Condorcet to Feller, have proposed to try to clarify the St. Petersburg paradox. The problem is to study the likely asymptotic behavior of Peter's average gains in the Petersburg game restarted over and over indefinitely after each success. Buffon [1777] made "a child" (about whom he tells us nothing) play 2048 rounds of Petersburg for a total gain of 10057 crowns, namely five crowns per game, conceivably a price to resolve the paradox (Augustus de Morgan repeated the same experiment with many friends [Morgan 1872]; see concerning this subject [Stigler 1999], [Jorland 1986], and [Dutka 1988], who, for his part makes a computer play the game 22528 times, for average of 7.34 dollars). Such averages are nevertheless deceptive, mathematically as well as practically, especially for Paul who can suddenly suffer an enormous loss with no immediate compensation, the Petersburg game looking to him more like Russian roulette than a fair game (always the exaggerated amplitude of the deviations!). The first "weak law of large numbers" for Petersburg variables (with infinite expected values) seems to have been due to Feller [1937, 1950/1968], who showed that the sum of Peter's gains $S(n)$ during n rounds of Petersburg is equivalent in probability to $n\ log_2 n$, but this type of result, made precise in an appropriate way [Martin-Löf 1985], does not get to the bottom of the practical and theoretical problem, because Peter still has a substantial asymptotic advantage over Paul, for

$$e^{-1} \leq \liminf \frac{S(n)}{n \log_2 n} \leq \limsup \frac{S(n)}{n \log_2 n} = \infty \quad \text{almost certainly,}$$

[Aaronson 1978], so that there is nothing to hope for from a strong law of large numbers [Chow, Robbins 1961].

In fact, it is very likely that Borel was unaware of the results of Feller and modern mathematical probabilists, whom he never cites anywhere. Even had he known about these results, he certainly would have not used them to resolve the paradox. As this paradox results from considering an actual or virtual infinity abstractly, it would not have been appropriate to resolve it using the virtues of this same infinity of the mathematicians, even if, extraordinarily, they had provided a reasonable solution of the St. Petersburg paradox, which moreover is not precisely the case.

17. This could even be one of the motivations for Borel's lemma on " denumerable probabilities" ([Borel 1909a], written in 1908; on this topic see [Lebesgue 1991]): If $(A(n))$ is a sequence of independent events with respective probabilities $p(n)$, the necessary and sufficient condition for an infinity (and hence at least one) of these events to happen with probability one is that the series with general term $p(n)$ diverge (see *e.g.* [Feller 1950/1968, chap. 8]). To illustrate, we can apply this result to Borel's martingale of 1908. Suppose it has been shown that Le Dantec's path goes through zero again with probability one (see below). After such a return to equilibrium, the game becomes identical with the original game and independent of the past. It is then (almost) certain to return to equilibrium a second time and so on: there is almost surely an infinity of periods bounded between two zeros, and these are independent of each other. But as there is one chance out of two that an excursion between two returns to equilibrium starts with a tail, we have a case where Borel's lemma can be applied with $p(n) = \frac{1}{2}$, and consequently, an infinity (and hence at least one) of the sequences starts with a tail with probability one (see note 14). You will have noted the celerity of this last deduction; when it can be applied, Borel's lemma is remarkably efficient. It was only in 1936 that such applications were made in the study of Markov chains with denumerable states, independently by Kolmogorov and Doeblin, who derived theorems sometimes more powerful than those obtained by the spectral theory of operators!

If we thus imagine that Borel launched his "deep study of the game of heads or tails" and "saw" his lemma on denumerable probabilities in reaction to the writings of Le Dantec [1907b,c], then the biology lecturer at the Sorbonne would be directly responsible for Borel's lemma and consequently for the strong law of large numbers and then for denumerable probabilities and, hence for modern probability theory. He would have contributed in the most determinate way possible to the development of a theory founded completely on a conception, the probability of an event, which has no meaning (but has played one of the leading roles in XXth century science).

To convince oneself that Le Dantec's path returns to zero with probability one, we could of course evoke Le Dantec's principle, recalled above: "chance knows no law", from which it follows that the path could not stay indefinitely on one side of the horizontal. If it is first negative, it must go up until it becomes positive; bad luck is a transitory state, we just have to wait. Nevertheless, this argument has the drawback of neglecting the cases when all waiting is in vain, for example when one of the players, say Paul, always or almost always wins, preventing Peter from ever getting back to equilibrium. This is, moreover, one of the points Borel emphasized. Le Dantec's principle implies returns to the origin are necessary, whereas they are only almost sure in the classical probability calculus where Borel stands. Le Dantec's implicit axiomatic is that of sequences of rounds (Le Dantec's collectives) that do not know any law, that of Borel is based on sets, and the cases of non returns to equilibrium have probability zero.

Staying in the Borelian framework (made explicit by Kolmogorov in 1933), it is very easy to show that returns to the origin have probability one. We can use for example Bertrand's and Adelman's classical method of successive doublings, which shows that with probability one a return to equilibrium will be followed by a gain for Peter. Indeed, de Moivre's theorem ($S(n)$ is asymptotically normal with zero mean and variance n) requires Le Dantec's path to go outside any horizontal band fixed in advance, no matter how large. It then suffices to reason as follows. The path starts at 0. It necessarily goes outside the horizontal band with ordinates $[-1,+1]$ (after the first toss, in fact),[xxv] and it has one chance in two of going up and reaching 1 from 0. Suppose it does not do that and instead goes down. It then has one chance in two of exiting upwardly from the band $[-3,+1]$, of width 4, as it lies in the middle of it, and hence of cutting the x axis and going to 1. Suppose again that it goes down. It still has one chance in two of exiting upwardly from the conveniently doubled band $[-7,+1]$, and hence of cutting the x axis and going to 1, and so on. It cannot continue to systematically go downwards out of the horizontal bands $[-(2^n-1),+1]$. So with probability one, it finally exits upwardly, cutting the horizontal axis and going to 1. We can also obtain this result using other even more classical methods, for example those given by Feller [1950, chap. 3 and 13] or by Borel, Bertrand, etc.

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