Preface

"La probabilité qui est une abstraction rayonne comme un petit soleil"
Le Jeu, la Chance et le Hasard by Louis Bachelier.

Even though it has only recently emerged as a scientific field of its own, mathematical finance came to existence in 1900, with the doctoral dissertation of the French mathematician Louis Bachelier entitled "Théorie de la Spéculation".

Researchers in finance will find there the first definition of options (it seems that options on tulips were traded in the Netherlands in the seventeenth century but it is not possible to trace any document which contains the precise description of the contract) as well as the first pricing model ever formalized in finance. Assuming that the underlying stock price dynamics are driven by a random walk, Bachelier *derives* from it the price of the option. At the same time, Bachelier supports his novel theoretical analysis with a sophisticated study of the French capital markets which were at the turn of the century a significant trading place and the main one worldwide for perpetual bonds.

Bachelier's work remained mostly unknown to financial economists until it was rediscovered in the 1950s by two brilliant minds. Jimmie Savage was sending postcards to several theorists in the field asking whether any of them "knew of a French guy named Bachelier who had written a little book on speculation". The answer was a definite yes on the part of Samuelson who had heard of him in the late 1930s from the Polish-American mathematician Stan Ulam, and had also kept in mind a footnote reference to Bachelier in Volume I of *Probability Theory and its Applications* published by Feller in 1950. In this footnote, Feller states that "Credit for discovering the connections between random walks and diffusions is due principally to L. Bachelier. Kolmogorov's theory of stochastic processes of the Markov type is based largely on Bachelier's ideas".

Hence, when Paul Cootner decided to produce a 1960s anthology of finance memoirs, Paul Samuelson urged him to commission an English translation of Bachelier; he also gave in 1964 a vibrant tribute to Bachelier's work: "So outstanding is his work that we can say that the study of speculative prices has its moment of glory at its moment of conception". Accordingly, Paul Samuelson decides to adopt Bachelier's model while correcting for the possibly negative values implied by Gaussian distributions and incompatible with the limited-liability feature of common stocks. And he "pragmatically replaces the Absolute Gaussians by log-normal probabilities".

The stochastic differential equation which became the key assumption in the Black-Scholes-Merton and many other pricing formulas first appeared in the 1965 paper by Paul Samuelson *Rational Theory of Warrant Pricing.* To report a modest anecdote of mine, while I was driving Professor Samuelson back to his hotel on June 29, after the magnificent inaugural ceremony in the Amphithéâtre Marguerite de Navarre at the Collège de France, marked by the talks given by Paul Samuelson, Henry McKean and Robert Merton to an audience comprising an impressive number of great names in mathematics and economics, I mentioned to him that many people were unaware that he was responsible for the fundamental equation. Professor Samuelson replied: 'Yes, I had the equation but "they" got the formula...' As we know, the number of times the equation and the formula have been restated and used in the last 35 years is beyond counting.

The 1965 paper of Paul Samuelson has another remarkable trait: it contains the unique (to my knowledge) piece of work Henry McKean ever dedicated to finance: in the Appendix, the explicit solution of the American option problem is provided when the underlying stock follows a geometric Brownian motion and the maturity is infinite. As of today, the exact solution for a finite maturity in the same setting has not yet been obtained. However, we understand better why the problem was "easier" for an infinite maturity (or for a maturity which would be an exponential time independent of the Brownian motion) thanks to recent pieces of work on functionals of Brownian motion, by Marc Yor in particular.

Coming to mathematicians, they recognize in Bachelier's pioneering work the development of the properties of Brownian motion that Brown had started to exhibit; the first expression of the Markov property which was only made fully explicit in 1905 (and which remains today a key assumption in most of the reference models in finance); and the introduction of the beautiful concept of trajectories at a time when the classical probabilistic representation was a sequence of heads and tails in coin tossing. Even years later, the great Kolmogorov was more interested in the analytical objects attached to stochastic processes than in their trajectories. Louis Bachelier paved the way to the work by Wolfgang Doeblin in the late thirties and to the profound study of Brownian excursions by Paul Lévy. Returning to finance, trajectories of diffusions, jump-diffusions or pure jump processes have become familiar tools, indispensable for the trader placing his orders in commodity or equity markets on the basis of charts, for the fundamental analyst relating stock price changes to earning announcements or news arrival and for the risk-manager simulating trajectories to compute the Value at Risk or the economic capital attached to a position or a portfolio.

The first World Congress of the Bachelier Finance Society, taking place in the home country of Louis Bachelier one hundred years after the defence of his PhD dissertation, and during the year 2000 which had been declared World Mathematical Year by the International Mathematical Union, had to be an exceptional manifestation and indeed it was. Paul Samuelson crossed the ocean to talk to us about "Finance Theory Within One Lifetime", a tale that by definition, he only was in the position to recount. Robert Merton who often honoured our country with his presence, was this time in the company of his PhD adviser and current peer in the exclusive group of Nobel Laureates. Henry McKean covered nine blackboards of the amphitheatre with his elegant formulas and figures. S.R.S. Varadhan gave one of his brilliant talks and yet, more research needs to be done to analyze the applications of large deviations to finance. Albert Shiryaev, Hans Föllmer and David Heath, experts in – among other topics – potential theory and stochastic processes, proved that they had also fully captured the major subtleties in financial economics. Last but not least, Steve Ross and Eduardo Schwartz were representatives of the field of mathematical finance via excellence in financial economics from arbitrage to interest rate models, from information theory to option pricing.

As shown (only partially) by this volume, the quality of the audience was as impressive as the list of Invited Speakers; the streets between the Collège de France, Ecole Normale Supérieure and Institut Henri Poincaré were humming for four days with animated discussions and we all left with the emotion of having been part of a unique scientific event.

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Hélyette Geman
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Bachelier and His Times: A Conversation with Bernard Bru*†‡

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Abstract. Louis Bachelier defended his thesis "Theory of Speculation" in 1900. He used Brownian motion as a model for stock exchange performance. This conversation with Bernard Bru illustrates the scientific climate of his times and the conditions under which Bachelier made his discoveries. It indicates that Bachelier was indeed the right person at the right time. He was involved with the Paris stock exchange, was self-taught but also took courses in probability and on the theory of heat. Not being a part of the "scientific establishment," he had the opportunity to develop an area that was not of interest to the mathematicians of the period. He was the first to apply the trajectories of Brownian motion, and his theories prefigure modern mathematical finance. What follows is an edited and expanded version of the original conversation with Bernard Bru.

Bernard Bru is the author, most recently, of *Borel, Lévy, Neyman, Pearson et les autres* [38]. He is a professor at the University of Paris V where he teaches mathematics and statistics. With Marc Barbut and Ernest Coumet, he founded the seminars on the history of Probability at the EHESS (École des Hautes Études en Sciences Sociales), which bring together researchers in mathematics, philosophy and the humanities.

M.T. : It took nearly a century for the importance of Louis Bachelier's contributions to be recognized. Even today, he is an enigmatic figure. Little is known about his life and the conditions under which he worked. Let's begin with his youth. What do we know about it?

B.B. : Not much. Bachelier was born in Le Havre to a well-to-do family on March 11, 1870. His father, Alphonse Bachelier, was a wine dealer at Le Havre and his mother Cécile Fort-Meu, was a banker's daughter. But he lost his parents in 1889 and was then forced to abandon his studies in order to earn his livelihood. He may have entered the family business, but he seems

* This article first appeared in *Finance and Stochastics* [119]. This is a slightly expanded version. It appears in French in [120].

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to have left Le Havre for Paris after his military service around 1892 and to have worked in some capacity at the Paris Stock Exchange. We know that he registered at the Sorbonne in 1892 and his thesis “Theory of Speculation” [5] of 1900 shows that he knew the financial techniques of the end of the 19th century perfectly.

M.T. : How important was the Paris Stock Exchange at that time?

B.B. : The Paris Stock Exchange, had become by 1850, the world market for the *rentes*, which are perpetual government bonds. They are fixed-return securities. When the government wished to contract a loan, it went through the Paris Exchange. The bond’s stability was guaranteed by the state and the value of the gold franc. There was hardly any inflation until 1914. The rate ranged between 3 and 5%. The securities had a nominal value, in general 100 francs, but once a bond was issued, its price fluctuated. The sums that went through Paris were absolutely enormous. Among the French, the bonds remained in families through generations. A wealthy Frenchman was a “rentier”, a person of independent means, who lived on the products of his bonds.

M.T. : I thought that a “rentier” is someone who lives off his land holdings.

B.B. : That’s also true but an important part, that which was liquid because easy to transfer, came from financial bonds. It all began with “the emigrants’ billion” (*le milliard des émigrés*). During the French Revolution, the nobility left and their holdings were sold as national property. When they returned in 1815, it was necessary to make restitution. The French state took a loan of a billion francs at the time, which was a considerable sum. The state paid the interest on it but never repaid the capital. It is what was called a “perpetual bond”, and the success of the original offering led to subsequent new issues. In 1900 the nominal capital of this public debt was some 26 billion francs (on a France’s annual budget of 4 billion). The international loans (from Russia, Germany, etc.) brought the total to 70 billion gold francs. All of the commercial houses had part of their funds invested in bonds. The state guaranteed that every year interest would be paid to the holders at fixed rates. This continued until the war of 1914, when the franc collapsed.

M.T. : Could the bonds be sold?

B.B. : They were sold for cash or as forward contracts or options, through stockbrokers. There was an official market on the exchange and a parallel market. It’s quite complicated, but it required a large workforce, for there were no phones, so there were assistants who carried out the transactions. Many of the financial products we know today existed then. There were many ways to sell bonds. If you read Bachelier’s thesis, he explains the workings of the system briefly.

M.T. : Why did people sell their perpetual bonds?

B.B. : For purposes of transfer or for speculation. It was, however, a speculation that was tolerated since it was not particularly risky. The bonds prices fluctuated markedly only during the great French political crises of 1830, 1848, and 1870.

M.T. : Was there fear of default?

B.B. : Yes. Considerable fortunes were then made and lost. These extreme fluctuations were not addressed by Bachelier in his thesis, he was merely concerned with the ordinary day-by-day fluctuations.

M.T. : Where did Bachelier work?

B.B. : I've searched, but I've been unable to locate the firm where Bachelier worked. It remains a mystery. But what is indisputable is that he loved science. As soon as he was able to set aside some funds, he returned to his studies. He earned his degree in mathematics at the Sorbonne in 1895 where he studied under professors such as Paul Appell, Émile Picard and Joseph Boussinesq, a mathematical physicist. There were two important areas in mathematics at the end of the 19th century: mathematical physics (that is, mechanics) and geometry. Those were the things one studied at that time. He therefore learned the theory of heat (diffusion equation) with Boussinesq [35], and also, he had Henri Poincaré. It was prior to Poincaré's change of chair.

M.T. : At the Sorbonne?

B.B. : Yes, where Poincaré occupied the chair in mathematical physics and probability between 1886 and 1896. Poincaré then transferred to a chair in celestial mechanics.

M.T. : So Bachelier almost missed studying under Poincaré?

B.B. : He would no doubt have followed his courses on celestial mechanics, since Poincaré was idolized at the time. Poincaré's courses were difficult to follow; they were also very innovative and without exams. The math degree[1] required taking exams in mechanics, differential and integral calculus, and astronomy. Bachelier finally succeeded in passing these. He also took Poincaré's exam in mathematical physics in 1897[2]. So Bachelier and Poincaré did meet.

M.T. : Was it an oral exam?

B.B. : Yes. It was probably there that Bachelier got the idea of continuing his studies. At the time, it was an honor, since the next degree was the thesis[3].

[1] equivalent to a Bachelor/Master of Arts.

[2] This course had been offered since 1834, but there were no exams because the course used to be elective. Bachelier was the first to pass the examination after the rules changed.

[3] In fact, there were two theses, an original one and a second one, which is an oral examination and whose purpose is to test the breadth and teaching abilities of the candidate. Bachelier's second thesis was about Boussinesq's work on fluid mechanics. The subject involved the motion of a sphere in a liquid.

After the thesis, it was necessary to find a university position, and these were rare. At the universities in the provinces, there were probably about fifty positions in mathematics. There were two at each university. To teach at a university required a thesis, but that was not enough, for there were almost no positions.

M.T. : The subject of Bachelier's thesis was out of the ordinary.

B.B. : In fact, it was exceptional. On the other hand, Bachelier was the right man at the right time, first because of his experience in the stock exchange. Secondly, he knew the theory of heat (this was the height of classical mathematical physics). Third, he was introduced to probability by Poincaré and he also had the probability lecture notes [27] of Joseph Bertrand, which served him well. If you look at Bertrand's chapter on gambling losses, you will see that it was useful to Bachelier. But the idea of following trajectories is attributable to Bachelier alone. It's what he observed at the Stock Exchange.

M.T. : Bachelier does seem to have been the right man at the right time.

B.B. : He was undoubtedly the only one who could have done it. Even Poincaré couldn't have done it. It had to happen in Paris, the center of speculation in bonds. It required a mathematical background, but not too extensive, since the mathematics of the time was not about that: it was about the theory of functions, especially functions of complex variables. The thesis of Émile Borel, that of Jacques Hadamard, were on the theory of functions. Bachelier was incapable of reading that. Moreover, Bachelier's thesis did not receive the distinction that he needed to open the doors of the university. It required getting the grade "very honorable". He only received the grade "honorable".

M.T. : Were there two possible grades?

B.B. : There was "adjourn", which indicated that the thesis was not worthy of being considered. And there were three grades: "passable", which was never given; "honorable", which meant "that's very good, sir, so long", and the "very honorable" grade, which offered the possibility of a university career, although not automatically.

M.T. : Why do you believe that he received only the grade "honorable"?

B.B. : It was a subject that was utterly esoteric compared to the subjects that were dealt with during that period, generally the theses of mechanics, which is to say partial differential equations. The big theses of the era were theses on the theory of functions (Borel, Baire, Lebesgue). Therefore, it was not an acceptable thesis topic. If we look, moreover, at the grades Bachelier earned in his degree exams, which are preserved in the national archives, they were very mediocre. He had a written exam in analysis, mechanics and astronomy. He had a great deal of difficulty. He tried many times before finally succeeding, and when he did succeed, it was just barely. He was last or next-to-last. That was still very good, since there were relatively few successes. The exams were difficult, and he was self-taught.

M.T. : Why?

B.B. : He did not go to a *lycée* following his *baccalauréat.* He had to take a job right away. The baccalaureate was the exam that opened the doors of the university. But in fact, all of the students followed two years of "special mathematics" in a *lycée* in order to gain entrance to the great scientific schools (such as the École Polytechnique or the École Normale Supérieure). The fundamentals of science were acquired at the *lycée* level. Bachelier must have studied on his own, which explains his difficulties on examinations. Thus Bachelier never had a chance to obtain a university chair. In the end, the quality of his thesis, the fact that it was appreciated by Poincaré, the greatest French intellect of the time, did not change the fact that Bachelier lacked the "necessary" distinction.

M.T. : Was he already working?

B.B. : He was working and studying at the same time. He occasionally took courses and also examinations. He was employed, I don't know where, perhaps in a commercial firm. Since his thesis was not enough for him to gain employment at a university, it is likely that he continued to work.

M.T. : Were there any errors in his thesis?

B.B. : No, absolutely not, there were no errors. The thesis was written rather in the language of a physicist. Fundamentally, this was not the problem. At that time, Poincaré would have pointed out a true error, had there been one. Poincaré's way of reasoning was similar: he left the details aside, he assumed them justified and didn't dwell on them. Bourbaki came only later. As for the question of "errors", that was something else. It came after the war of 1914. The thesis was in 1900. He was not awarded a position because he was not "distinguished" enough. What's more, Probability did not start to gain recognition in France until the 1930's. This was also the case in Germany.

M.T. : Who were the great probabilists in 1900?

B.B. : There were none. Probability as a mathematical discipline dates from after 1925. There was a Laplace period until 1830, then it's the crossing of the desert – mathematicians took no interest in those things – their interest was rekindled only much later. Let's take Paris, for example. Bachelier's thesis was 1900. We'd have to wait another twenty years for Deltheil, Francis Perrin and especially the end of the 30's with Dugué, Doeblin, Ville, Malécot, Fortet, Loève.

M.T. : Was Bachelier's thesis considered a probability thesis?

B.B. : No. It was a mathematical physics thesis, but since it was not physics, it was about the Stock Exchange, it was not a recognized subject.

M.T. : Wasn't there some notion of Brownian motion at the time?

B.B. : Bachelier doesn't refer to it at all. He learned of this much later, for there were to be many popularized publications on the subject. But in 1900, zero. The translation of Boltzmann[4] [28] in France was done in 1902 and 1905. And Boussinesq was a mathematician doing mechanics and hydrodynamics. For him, mathematical physics was differential equations.

M.T. : Why did Bachelier introduce Brownian motion?

B.B. : To price options. (The options considered by Bachelier were somewhat different from the ones we know today.) He uses the increments of Brownian motion to model "absolute" price changes, whereas today, one prefers to use them to model "relative" price changes (see Samuelson [113–115][5]).

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[4] Brownian motion is named after Robert Brown [36], the Scottish botanist who noticed in 1827 that grains of pollen suspended in water had a rapid oscillatory motion when viewed under a microscope. The original experiment and its re-enactment are described in [55]. The kinetic theory of matter, which relates temperature to the average kinetic energy, was developed later in the century, in particular by Ludwig Boltzmann, and it is the basis of Einstein's explanation of Brownian motion [51] in 1905.

[5] The idea of modeling the logarithm of prices by independent and normally distributed random variables was also suggested by Osborne [96] in 1959. Osborne was a physicist working at the Naval Research Laboratory in Washington, D.C. At the time, he knew apparently of neither Bachelier nor Samuelson (see also [2] and [26]) He later wrote an interesting book [98] which are his lecture notes at the University of California at Berkeley. In his 1959 article [96], Osborne does not mention Bachelier but, following a letter by A. G. Laurent [82] in the same volume, Osborne provided a reply [97], where he quotes Bachelier. He starts [97] by indicating that after the publication of his 1959 article [96], many people drew his attention to earlier references, and then he gives the following nice summary of Bachelier's thesis (the reference numbers in the text below are ours):

> *I believe the pioneer work on randomness in economic time series, and yet most modern in viewpoint, is that of Bachelier [5] also described in less mathematical detail in reference [15]. As reference [5] is rather inaccessible (it is available in the Library of Congress rare book room), it might be well to summarize it here. In it Bachelier proceeds, by quite elegant mathematical methods, directly from the assumption that the expected gain (in francs) at any instant on the Bourse is zero, to a normal distribution of price changes, with dispersion increasing as the square root of the time, in accordance with the Fourier equation of heat diffusion. The theory is applied to speculation on rente, an interest-bearing obligation which appeared to be the principle vehicle of speculation at the time, but no attempt was made to analyze the variation of prices into components except for the market discounting of future coupons, or interest payments. The theory was fitted to observations on rente for the years 1894-98. There is a considerable quantitative discussion of the expectations from the use of options (puts and calls). He also remarked*

M.T. : Is it Poincaré who wrote the report on the thesis?

B.B. : Yes, that's how it was done at that time. There were three people in the jury but only one reported. The other two members of the jury were Appell and Boussinesq. They probably read nothing, as opposed to Poincaré, who read everything. When there was a thesis that no one wanted to read, on any subject, applied physics, experimental physics, it was directed to Poincaré. I've seen some Poincaré reports on some incredible works. He had an unbelievably quick intelligence.

M.T. : Is that why he was asked to report on Bachelier's thesis?

B.B. : Perhaps. But it's also because he knew Bachelier.

M.T. : Bachelier had indeed taken his course. But in those courses, did one speak to the professor?

B.B. : Never. It was unthinkable to question a professor. Even after the course. In the biography of Jerzy Neyman[6] by Constance Reid [112], Neyman recounts that, when he was a Rockefeller fellow in Paris, he followed Borel's course in probability[7]. He once approached Borel to ask him some questions. Borel answered, "You are probably under the impression that our relationships with people who attend our courses are similar here to what they are elsewhere. I am sorry. This is not the case. Yes, it would be a pleasure to talk to you, but it would be more convenient if you would come this summer to Brittany where I will be vacationing"[8]. This was in 1926. Neyman was at the still young age of 32.

M.T. : Where did you find Poincaré's thesis report?

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that the theory was equally applicable to other types of speculation, in stock, commodities, and merchandise. To him is due credit for major priority on this problem.

[6] This is the Neyman (1894-1981) of the celebrated Neyman-Pearson Lemma in hypotheses testing.

[7] Émile Borel (1871-1956) founded the French school of the theory of functions (Baire, Lebesgue, Denjoy). In his 1898 book [29], he introduces his measure as the unique countably additive extension of the length of intervals; it became the basis of modern measure and integration theory. Borel sets are now named after him. Starting in 1905, Borel focused on probability and its applications and developed properties related to the notion of almost sure convergence. See [56] for the story of his life.

[8] See [112], p. 66.

B.B. : At the National Archives[9], where things remain for eternity. Here's the beginning of the report[10]:

> *The subject chosen by Mr. Bachelier is somewhat removed from those which are normally dealt with by our applicants. His thesis is entitled "Theory of Speculation" and focuses on the application of probability to the stock market. First, one may fear that the author had exaggerated the applicability of probability as is often done. Fortunately, this is not the case. In his introduction and further in the paragraph entitled "Probability in Stock Exchange Operations", he strives to set limits within which one can legitimately apply this type of reasoning. He does not exaggerate the range of his results, and I do not think that he is deceived by his formulas.*

M.T. : Poincaré does not seem convinced of the applicability of probability to the stock market.

B.B. : It must be said that Poincaré was very doubtful that probability could be applied to anything in real life. He took a different view in 1906 after the articles of Émile Borel. But prior to this, there was the Dreyfus Affair.

M.T. : What is the connection between Poincaré and the Dreyfus Affair?

B.B. : Dreyfus was accused of dissimulating his writings in a compromising document. The question was then to determine whether this document was written in a natural way, or whether it was constrained writing, in other words, "forged," a typical problem in hypotheses testing. Poincaré was called by the defense to testify in writing on the actual value of the probabilistic argument. Poincaré began by saying that the expert witness for the prosecution, Alphonse Bertillon, had committed "colossal" computational errors and that, in any case, probability could not be applied to the human sciences (*sciences morales)*[11]. If you look at Poincaré's course on probability, you will see that he is skeptical with regard to its applications.

[9] The original document of Poincaré's thesis report is held at the *Registre des thèses de la Faculté des Sciences de Paris*, at the *Archives nationales*, 11 rue des Quatre-Fils, 75003 Paris, classification AJ/16/5537. It is dated March, 29, 1900, the day of the defense.

[10] The full text, translated into English, by Selime Baftiri-Balazoski and Ulrich Hausmann, can be found in [44]. The French text of the report is given below, as well as the short defense report, signed by Paul Appell.

[11] The transcript appeared in the newspaper *Le Figaro* on September 4, 1899. Poincaré's letter, concerning Bertillon's way of reasoning, was addressed to Painlevé who was a defense witness. Painlevé read it in court. Here is what Poincaré writes around the end of his letter: *None of this is scientific and I do not understand why you are worried. I do not know whether the defendant will be found guilty, but if he is, it will be on the basis of other proofs. It is not possible that such arguments make any impression on people who are unbiased and have a solid mathematical education.* [Translation by M.T.].

M.T. : What especially interested Poincaré in Bachelier's thesis?

B.B. : It's the connection to the heat equation. Yet this connection was already commented upon by Rayleigh in England. Rayleigh (1842-1919) was a great physicist, the successor of Maxwell at Cambridge and a specialist in random vibrations. He received the Nobel Prize in 1904. Rayleigh had made the connection between the problem of random phase and the heat equation [106,107]. You are adding n oscillations together. The simplest version of this is coin tossing. One of Bachelier's proofs (he had a number of different arguments) is a bit like that. On the other hand, what Rayleigh did not see at all, and what Bachelier saw, and Poincaré understood and appreciated, was the exploitation of symmetries, the reflection principle, which leads to the law of the maximum. It's something that probably comes from Bertrand [27]. Poincaré was undoubtedly the only one capable of quickly understanding the relevance of Bachelier's method to the operations of the Stock Exchange because, as of 1890, he had introduced in celestial mechanics a method, called the *chemins conséquents*, which involves trajectories.

M.T. : Is the reflection principle attributable to Bertrand?

B.B. : For coin tossing, yes. The purely combinatorial aspect of the reflection principle is due to Désiré André, a student of Bertrand. Désiré André was a mathematician, professor in a parisian *lycée* . He had written his thesis, but was never able to obtain a position at the University of Paris. He did some very fine work in combinatorics (1870-1880). The reflection principle in gambling losses can already be found in Bertrand [27], but especially in Émile Borel. But the continuous time version is not obvious. Evidently, Bachelier obtained it in a heuristic fashion, but this is nonetheless remarkable.

M.T. : Désiré André discovered the reflection principle. Wasn't he then the first to see trajectories since the reflection principle is based on them?

B.B. : The argument in Désiré André involves combinatorial symmetry but not time or trajectory, but he is obviously not far away. Trajectories are implicit in the work of almost all the classical probabilists, but they do not take the ultimate step of making them explicit. Things would have been different, had they done so. For them, these are combinatorial formulas. Today our view is distorted. In coin tossing, we see the trajectories rise and fall. At that time, this was not the case.

M.T. : Bachelier learned probability in Poincaré's course. Do the lecture notes still exist?

B.B. : Yes, they do (see reference [102]). There are two editions, the first is from 1896, the second from 1912, the year of Poincaré's death. The 1912 edition is very interesting. The one of 1896, which Bachelier must have read, is less so. Bachelier referred primarily to Bertrand's book [27], which appeared in 1888. Bertrand is a controversial figure. He gave us "the Bertrand series", "the Bertrand curves", etc. He died in 1900, the year of Bachelier's thesis. He was professor of mathematical physics at the Collège de France. He taught a course on probability all his life, for he was jointly professor at the École Polytechnique, and his book is very brilliant.

M.T. : Did Poincaré know of Rayleigh's results?

B.B. : Not at all. Rayleigh's works on random vibrations began in 1880 and ended the year of his death in 1919. (The second edition of his book [106], dated 1894, contains many results on the subject.) Rayleigh's articles were published in English journals, which were not read in France. At that time, the French did not read English. French physics then was in a state of slumber. It's Pólya [104], then in Zürich, Switzerland, who in 1930 made Rayleigh's results known in Paris. Pólya read widely. He became interested in geometric probability in 1917, and in road networks during the 20s.

M.T. : But I suppose that after Einstein, one made the connection with what Rayleigh did.

B.B. : These were different fields. Their synthesis occurred when probability was being revived in the 1930s. One then realized that all this was somewhat similar but belonging to different scientific cultures.

M.T. : After his thesis, did Bachelier want to do something else?

B.B. : No, not at all. When he discovered diffusion, it was a revelation, a fascination that never left him. These were ideas that had been around since Laplace (1749-1827). Laplace went from differential equations to partial derivatives. He had no problem with that. It was only analysis with a combinatorial perspective. Bachelier was of a physical mind set, very concrete. He could see the stock fluctuations. They were right before his eyes. And that changed his point of view. He was in an original, unique position. Rayleigh did not have this vision. He saw vibrations. Bachelier saw trajectories. From that moment on, Bachelier committed all his energies to the subject, as far as we can determine. This can be seen by looking at the manuscripts that are in the Archives of the Academy of Science. The formulas are calligraphed as though they were works of art (while the proofs are slapdashed). He was never to cease until his death in 1946. As soon as he defended his thesis, he published an article [6] in 1901, where he revised all of the classic results on games with his technique of approximation by a diffusion (as it is now called). He corrected Bertrand's book in large part, and he completely rewrote everything while adopting as he said, a "hyperasymptotic" view. For according to Bachelier, Laplace clearly saw the asymptotic approach, but never did what he, Bachelier, had done.

M.T. : The asymptotic approach deals with the Gaussian limit. The hyperasymptotic one concerns limits of trajectories, which is continuity perceived from a distance.

B.B. : He did it in a very clumsy manner, for he wasn't a true mathematician. But Kolmogorov [76] in 1931[12] and Khinchine [75] in 1933[13]and the post-war probabilists understood the richness of the approximation-diffusion point of view.

M.T. : But these techniques did not exist at the time of Bachelier.

B.B. : No, but there is a freshness in the point of view and enthusiasm. He therefore continued to work, and he tried to obtain some grants. There were some research grants in France during that period, an invention attributable to the bond holders. A few among them had no descendants and bequeathed their bonds to the university. The first research grants date back to 1902. Before that, they did not exist. That's why research in France was strictly marginal. It was only at the Université de Paris that research was done, and even there not that much.

M.T. : Did Bachelier have any forerunners at the Exchange?

B.B. : There was Jules Regnault who published a book [111] in 1863 (see [70]). Forty years before Bachelier, he saw that the square-root law applied, namely that the mean deviation[14] is expressed in terms of square-root of time. It's a book on the philosophy of the Exchange that is quite rare. I know only of one copy, at the *Bibliothèque Nationale*[15].

M.T. : To find that law without an available mathematical structure means that it must have been observed empirically.

B.B. : The reason that Regnault gave is curious (the radius of a circle where time corresponds to the surface...)[16]. But he verified the square-root

[12] See below.

[13] This is what Khinchine [75] writes (page 8):

> *This new approach differs from the former, in that it involves a direct search for the distribution function of the continuous limiting process. As a consequence, the solution appears as a proper distribution law (and not, as before, as a limit of distribution laws). Bachelier [5,12] was the first to take this new approach, albeit with mathematically inadequate means. The recent extensive development and generalisation of this approach by Kolmogoroff [76,77] and de Finetti [46,45] constitute one the most beautiful chapters dealing with probability theory ...*

[Translated from the German. The reference numbers are ours.]

[14] L'*écart moyen* in French. Regnault does not provide a formal definition but the term seems to refer to the average of the absolute deviations of prices between two time periods. It was translated incorrectly as "standard deviation" in [119].

[15] There is also one copy at the *Library of Congress* in Washigton D.C. The card catalogue indicates that Jules Regnault died in 1866.

[16] Excerpts are given below.

law on stock prices. How he found it, I don't know. Regnault is obviously not someone who studied advanced mathematics. I tried to see whether he got his *baccalauréat*, but I could not find this. No doubt he studied alone, probably the works of Quetelet and perhaps Cournot[17]. We still know nothing of this Regnault, who would have been the Kepler of the Exchange just as Bachelier would have been its Newton (relatively speaking).

M.T. : Who published Regnault's book – the Exchange?

B.B. : There is a gigantic body of literature on the Exchange. But these are not interesting books ("How to Make a Fortune", etc.). There's Regnault's book which is unique, and which we know about. Émile Dormoy, an important French actuary, quotes it[18] in 1873 in reference to the square root law (see [49]). The stockbrokers took Regnault's book into account and if you look at the finance courses of the end of the 19th century, they do refer to the square-root law.

M.T. : So Bachelier must have been familiar with that law.

B.B. : Certainly – in the same way that Bachelier knew Lefèvre's diagrams, which represent the concrete operations of the Exchange[19]. One could buy and sell the same product at the same time in different ways. There is

[17] Adolphe Quetelet (1796-1874) was influenced by Laplace and Fourier. He used the normal curve in settings different from that of the error law [105]. Antoine Augustin Cournot (1801-1877) wrote [43] but also [42], where he discusses supply and demand functions.

[18] Dormoy writes ([49], page 53):

> *In order to get an idea of the real premium on each transaction, one must estimate the mean deviation of prices in a given time interval. But following the observations made and summarized a long time ago by Mr. Jules Regnault in his book titled Philosophie de la Bourse, the 30 day mean deviation is 1.55 francs for the rente. For time intervals that are either longer or shorter than a month, the mean deviation of prices is proportional to the square root of the number of days.*

[Translation by M.T.].

[19] Henri Lefèvre was born in Châteaudun in 1827. He obtained a university degree in the natural sciences in 1848. Not finding a teaching position, he worked as an economics correspondent for several newspapers. He later became the chief editor of *El eco hispano-americo*, a newspaper with focus on South America. Lefèvre in 1869, was one of the founders of *l'Agence centrale de l'Union financière* and his books on the stockmarket [83,85] date from that period. He was well acquainted with the economic life of the time and his diagrams are quite clever (see [69]). These diagrams were rediscovered independently by Léon Pochet [101], a graduate from the *École Polytechnique*, but Lefèvre complains and claims priority [84]. Lefèvre then became a full member of the society of actuaries and worked at the *Union*, one of the most important insurance companies in Paris.

a graphic means of representing this. Bachelier's first observations are based on these diagrams.

M.T. : Does all of this apply only to bonds?

B.B. : Yes.

M.T. : Bonds must then have been issued on a regular basis?

B.B. : For example, the Germans financed the war of 1870 by issuing loans in Paris and the French paid "reparations" to the Germans after the war by a loan of five billion francs underwritten at the Paris Exchange. The large networks of railroads were financed by loans underwritten in Paris, etc.

M.T. : Where did Bachelier publish?

B.B. : Until 1912 Bachelier published his works thanks to the support of Poincaré, for it was necessary that someone recommend them to the *Annales de l'École Normale Supérieure* or to the *Journal de Mathématiques Pures et Appliquées*. These were important journals. But Bachelier's articles were not read. And though Poincaré in the end clearly did not read them, he encouraged him.

M.T. : Was Bachelier's thesis published?

B.B. : It was published in the *Annales de l'École Normale Supérieure* [5] in 1900.

M.T. : It was also translated into English and reprinted in 1964 in the book, *The Random Character of Stock Market Prices* [41].

B.B. : What is curious is that Émile Borel, who was a prominent mathematician and who was part of the establishment, never took an interest in Bachelier. His interest was in statistical physics, in conjunction with the theory of kinetics and the paradox of irreversibility. Borel published his first works on probability [30] in 1905.

M.T. : Was he younger than Bachelier?

B.B. : No, they were about the same age. Borel born in 1871, Bachelier in 1870. Borel surely was very interested in probability, but not in Bachelier. Borel occasionally had to report on Bachelier's requests for grants. He always wrote favorable reports, for Bachelier had little money, but without ever taking any interest in his works (as far as I know).

M.T. : But Bachelier worked at the Exchange?

B.B. : Perhaps, but he must have made a very modest salary. Borel had a prominent position on the Council of the Faculty of Sciences. Each time that Bachelier submitted a request, Borel wrote a favorable report. These were small sums of money. I believe he received 2000 francs four times. This was in gold francs, but it was a small sum. So Bachelier, beginning in 1906-1907, obtained small grants three or four times like that. It was then that he must have written his enormous treatise on probability, published at his own expense [12]. But, in that book, he only went over his articles.

M.T. : He wrote an article on diffusions after his thesis. Was it interesting?

B.B. : Yes, it's an article published in 1906 entitled "On continuous probability" (cf. [7]). It's an extraordinary article. He had two major accomplishments, his thesis and this.

M.T. : Was Bachelier rather isolated before the First World War?

B.B. : De Montessus[20] [47] published a book in 1908 on probability and its applications, which contains a chapter on finance based on Bachelier's thesis. Bachelier's arguments can also be found in the 1908 book of André Barriol[21] [25] on financial transactions. And there is also a popularizing book on the stock market by Gherardt [60], where Regnault and Bachelier are quoted[22]. But yes, Bachelier was essentially isolated. In those years he remained in Paris. He seemed to have no interactions with anyone.

M.T. : But how is it that Émile Borel had so much power to award grants? Wasn't he also very young?

B.B. : Borel defended his doctorate in 1894 at the age of 23. He was exceptional. He was appointed to the Sorbonne at 25, something unprecedented, since most appointments to the Sorbonne took place after one turned fifty. Borel was first in everything. He married the daughter of Paul Appell, dean of the Faculté des Sciences de Paris.

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20 Robert de Montessus (1870-1937) was professor at the *Faculté Catholique des Sciences* of Lille and at the *Office National Météorologique*. In 1905 he wrote a thesis on continuous algebraic functions, which was awarded the "Grand Prix des Sciences Mathématiques" in 1906.

21 Alfred Barriol (1873-1959) graduated from the *École Polytechnique* in 1892 and became an economist and actuary. He was the first professor of finance at the *Institut de Statistique* of the University of Paris and financial advisor to several french governments. Whereas the book of de Montessus [47] did not have much success, the one by Barriol [25] was used by generations of students in finance and insurance.

22 Maurice Gherardt did not belong to a scientific organization. He wrote books entitled *Vers la fortune par les courses, guide pratique du parieur aux courses de chevaux...exposé théorique et pratique d'une méthode rationnelle et inédite de paris par mises égales permettant de gagner 4000frs par an avec 500frs de capital* (Paris: Amat, 1906); *La vie facile par le jeu à la roulette et au trente-et quarante* (Paris: Amat, 1908); *Le gain mathématique à la Bourse; la spéculation de bourse considérée comme un jeu de pur hasard, théorie mathématique de la probabilité en matière de cours, écarts et équilibres, conjectures alternantes, tableaux et graphiques à l'usage des spéculateurs, exposé théorique d'une méthode de spéculation assurant un bénefice considérable et continuel* (Paris: Amat, 1910), which is [60].

M.T. : Appell of polynomial fame?

B.B. : Yes. Appell was an important mathematician. Borel wrote extensively, but he doesn't seem to have paid attention to Bachelier. Borel took a great interest in Probability. In 1912 (cf. [33]), he wrote that he wanted to dedicate all of his energy to the development of applications of probability, and he succeeded. He viewed probability as a general philosophy, an approach to understanding the sciences, in particular, physics. But Bachelier's appeared to him to have little importance, because this business of the Stock Exchange was not too serious. And this business of hyperasymptotic diffusion, just did not interest Borel who was a brilliant thinker. He undoubtedly judged it pointless, since Stirling's formula sufficed for games. But Borel directed Francis Perrin's thesis on Brownian motion and its applications to physics[23]. It's a remarkable thesis published in 1928. Borel is somewhat paradoxical. He was a powerful mathematician and a founder of the modern theory of functions. On the other hand, Borel was very elitist. Do you understand what "elitist" means within the French context? It means that Bachelier was unimportant.

M.T. : Why did Bachelier write a book?

B.B. : It was his lecture notes [12]. Bachelier was allowed to teach an open but unpaid course on probability at the University of Paris from 1909 until 1914[24]. He also wrote another book which appeared in 1914, entitled *Game, Chance and Randomness* [15], which proved very popular. In any case, the war in 1914 stopped all these scientific activities.

M.T. : Was he drafted?

B.B. : Yes, he served through the entire war and was promoted to lieutenant. In a manner of speaking he had a "good war". The war killed many young mathematicians. This presented new career opportunities for Bachelier. From 1919, Bachelier was lecturing at the universities of Besançon (1919-1922), Dijon (1922-1925) and Rennes (1925-1927). The position of *chargé de cours* (lecturer) was without tenure but it was paid and relatively stable. The

[23] Francis Perrin (1901-1992), the son of the Nobel prize laureate Jean Perrin, did not receive the usual schooling. Together with the children of Marie Curie and those of Paul Langevin, he was tutored privately by the best scientists of the time. Émile Borel taught him Mathematics (Borel was a close friend of his father since their days at the École Normale Supérieure). After his theses, one in Mathematics, the other in Physics, Francis Perrin became a professor at the Sorbonne and then at the Collège de France. As high commissioner of atomic energy, he played a major role in designing the French nuclear policy of the 50s and 60s.

[24] Borel taught a probability course [32] twice in 1908 and 1909 and it is likely that this is the course that Bachelier took over. After the First World War, in 1919, Borel taught the course again after transferring from the chair in function theory that he had held since 1908 to the chair in probability and mathematical physics, then held by Boussinesq.

lecturer replaces a professor who is away or whose position is temporarily vacant.

M.T. : Did Bachelier apply for a permanent position?

B.B. : René Baire's chair in differential calculus in Dijon became available in 1926 and Bachelier applied for it, at the age of 56. In the provincial universities, there were two chairs in mathematics: a differential calculus chair and a mechanics chair. Those were the two required courses for the degree. The mechanics chair in Dijon was occupied by a well known mathematician, Maurice Gevrey[25], a specialist in partial differential equations. He was to write a report on Bachelier. He must have gone over Bachelier's writings very quickly since it was not his own theory and it looked strange. Bachelier, in fact, often took shortcuts, not paying much attention to questions of normalization and of convergence.

M.T. : This was undoubtedly a matter of simplification.

B.B. : Yes, indeed. Reading Bachelier, one occasionally gets the impression that he considers that Brownian motion is differentiable though it is not. Gevrey had the 1913 article published in the *Annales de l'École Normale Supérieure* [13], where Bachelier asks the following: "A geometric point M is moving at a speed v whose velocity is constant but where direction keeps varying randomly. The position of M is projected on the three rectangular axes centered at its initial position. What is the probability that at time t, the point M will have given coordinates x, y, z ?". The answer is that the point M moves according to Bachelier's Brownian motion, but this is not possible if the speed is constant and finite, as Bachelier seems to suppose. Indeed, if we place ourselves in dimension 1, the speed of Bachelier's point M is at every instant either $+v$ or $-v$, with probability 1/2 each. Its position at time t is $\sum \pm vdt$. Therefore the mean of its position is 0 and the variance of its position is $\mathrm{Var}(\sum \pm vdt) = (v\,dt)^2 t/dt$, of the order of dt. Since dt is infinitesimal, the variance is negligeable and there is no motion. The point M can never leave its original position. In order that there be motion, one must normalize v by $1/\sqrt{dt}$, and therefore give to M an infinite speed, which will allow it to move. Normalizing v by $1/\sqrt{dt}$ means setting $v = v_0/\sqrt{dt}$, where $0 < v_0 < \infty$, and thus replacing the increments vdt by $(v_0/\sqrt{dt})dt = v_0\sqrt{dt}$. This gives $\mathrm{Var}(\sum \pm vdt) = \mathrm{Var}(\sum \pm v_0\sqrt{dt}) = (v_0^2 dt)t/dt = v_0^2 t$, a finite and non-zero quantity. That's what Bachelier had done in his thesis, within the context of coin tossing, but he did not reproduce this reasoning in 1913.

M.T. : But did Gevrey know that?

[25] Maurice Gevrey (1884-1957) was an important mathematician working on parabolic partial differential equations, following Hadamard [64]. The existence and uniqueness theorem of Markov processes in Feller [53] is based on the theory of Hadamard and Gevrey. His collected works can be found in [59].

B.B. : No, he had no idea, but he must have read this page and gone through the roof. For Bachelier, it was his usual way of talking.

M.T. : It was a true misfortune then.

B.B. : It fell to the wrong referee. He wrote a devastating report. But since he was not competent in probability, he sent it to Paul Lévy[26]. Lévy, at that time (1926), had just published an important work on probability (cf. [86]). Gevrey knew him very well, for they were both students of Jacques Hadamard. Hadamard was professor at the Collège de France and was surrounded by many brilliant students who formed a type of caste. Obviously, Gevrey wanted nothing to do with Bachelier. Gevrey sent Lévy the incriminating page asking him (I'm paraphrasing) "What do you think of this?" Lévy answered, "You're right, it doesn't work," having read nothing but this famous page. One can imagine that Bachelier's goal in his 1913 article was to show that his modeling of stock market performance is equally applicable to the Brownian motions whose importance had just been pointed out by Jean Perrin in the context of the motion of molecules. Indeed, in 1913, Jean Perrin published "The Atoms" (cf. [100]), aimed at a popular audience, in which he talks about his experience with Brownian motion. One could just as well imagine that this is also why Poincaré, who had read Bachelier's thesis, recommended an article of this type to the *Annales de l'École Normale Supérieure*, in spite of the "mistake" revealed by Lévy and Gevrey. This "mistake" is ultimately nothing but an audacious metaphor to Bachelier's 1900 thesis *The Theory of Speculation*. Obviously, Lévy never knew anything about that.

M.T. : Did Bachelier learn about Lévy's intervention?

B.B. : Yes, he was very upset. He circulated a letter accusing Lévy of having blocked his career and of not knowing his work[27].

M.T. : Do we have Lévy's text?

B.B. : I never saw the Lévy–Gevrey letter. I don't know whether it still exists. On the other hand, what we do have of Lévy are two or three sentences in his books, in that of 1948 on Brownian motion [89][28] and in his 1970 book

[26] Together with Kolmogorov and Émile Borel, Paul Lévy (1886-1971) is one of the most important probabilists of the first half of the twentieth century. He received his doctorate in 1912 (Picard, Poincaré, and Hadamard were on the committee). Paul Lévy contributed not only to probability theory, but also to functional analysis. He was professor at the École Polytechnique from 1920 until his retirement in 1959.

[27] Several copies of this letter were found by Ms. Nocton, the head of library at the *Institut Henri Poincaré* in Paris. The article Courtault et. al. [44] contains a number of excerpts from this letter.

[28] Here are the footnotes in [89] (second edition) about Bachelier, which mention:
-page 15 footnote (1): the priority of Bachelier over Wiener about Brownian

of memoirs [90]. In the latter, Lévy says he is sorry that he ignored Bachelier's work because of an error in the construction of Brownian motion, but he does not tell us what the error is, and for good reasons[29]. It seems that it is a late value judgement. Hence, a few cryptic notes on Bachelier which in summary state that "I erred, but Bachelier did too". There is also a letter that Lévy wrote to Benoit Mandelbrot[30]. This is what Lévy writes, about Bachelier:

> *I first heard of him a few years after the publication of my Calcul des Probabilités, that is, in 1928, give or take a year. He was a candidate for a professorship at the University of Dijon. Gevrey, who was teaching there, came to ask my opinion of a work Bachelier published in 1913 ... Gevrey was scandalized by this error. I agreed with him and confirmed it in a letter which he read to his colleagues in Dijon. Bachelier was blackballed. He found out the part I had played and asked for an explanation, which I gave him and which did not convince him*

motion.

-page 72 footnote (4): the priority of Bachelier over Kolmogorov about the relation between Brownian motion and the heat equation.

-page 193 footnote (4): the priority of Bachelier over Lévy about the law of the maximum, the joint law of the maximum and Brownian motion, and the joint law of the maximum, the minimum and Brownian motion.

[29] Lévy [90] writes (p. 97):

> *The linear Brownian motion function $X(t)$ is often called the function of Wiener. Indeed, it is N. Wiener who, in a celebrated 1923 article, gave the first rigorous definition of $X(t)$. But it would not be right not to remember that there were forerunners, in particular the French Louis Bachelier and the important physicist Albert Einstein. If the work of Bachelier, which appeared in 1900, has not attracted attention, it is because, on one hand, not everything was interesting (this is even more true for his large book "Calcul des Probabilités," published in 1912), and because on the other hand, his definition was at first incorrect. He did not get a coherent body of results about the function $X(t)$. In particular, in relation to the probability law of the maximum of $X(t)$ in an interval $(0, T)$ and also in relation to the fact that the probability density $u(t, x)$ of $X(t)$ is a solution of the heat equation. This latter result was rediscovered in 1905 by Einstein who, evidently, did not know about Bachelier's priority. I myself did not think it useful to continue reading his [Bachelier's] paper, astonished as I was by his initial mistake. It is Kolmogorov who quoted Bachelier in his 1931 article ... and I recognized then the injustice of my initial conclusion.*

[Translation by M.T.].

[30] Letter dated January 25, 1964 from Paul Lévy to Benoit Mandelbrot, in which he recounts the Gevrey incident. Mandelbrot includes excerpts of this letter in a very interesting biographical sketch of Bachelier in [93], pages 392-394. According to Mandelbrot (private communication), the original copy of this letter may be lost.

of his error. I shall say no more of the immediate consequences of this incident.
I had forgotten it when in 1931, reading Kolmogorov's fundamental paper, I came to "der Bacheliers Fall"[31]*. I looked up Bachelier's works, and saw that this error, which is repeated everywhere, does not prevent him from obtaining results that would have been correct if only, instead of* $v =$ *constant, he had written* $v = c\tau^{-1/2}$*, and that, prior to Einstein and prior to Wiener, he happens to have seen some important properties of the so-called Wiener or Wiener–Lévy function, namely, the diffusion equation and the distribution of* $\max_{0\leq\tau\leq t} X(t)$.[32]

In this matter with Gevrey, Lévy did not bother to understand what Bachelier wanted to say, namely that once and for all, Brownian motion existed since the time of his thesis where the normalizations were included and the convergences established. The irony of the story is that, while Lévy would publish his beautiful works on Brownian motion beginning in 1938, the same mathematicians (starting with Hadamard) would much mock this $\pm v_0/\sqrt{dt}$ which represents for Lévy as for Bachelier a different kind of speed that "varies constantly in a random way".

M.T. : The British economist John Maynard Keynes seems to have quoted Bachelier.

B.B. : He did so in 1921 in his book on probability [74], quoting Bachelier's texts [12,15] but only in the context of statistical frequency and Laplace's rule of succession[33]. Bachelier's work on finance is not mentioned.

[31] Der Fall Bacheliers (Bachelier's case).
[32] Another excerpt from this letter will be quoted below.
[33] Keynes [73] had reviewed Bachelier's text *Calcul des Probabilités* [12] in 1912. He writes:

> *M. Bachelier's volume is large, and makes large claims. His 500 quarto pages are to be followed by further volumes, in which he will treat of the history and of the philosophy of probability. His work, in the words of the preface, is written with the object, not only of expounding the whole of ascertained knowledge on the calculus of probabilities, but also of setting forth new methods and new results which represent from some points of view une transformation complète de ce calcul. On what he has accomplished it is not very easy to pass judgment. The author is evidently of much ability and perseverance, and of great mathematical ingenuity; and a good many of his results are undoubtedly novel. Yet, on the whole, I am inclined to doubt their value, and, in some important cases, their validity. His artificial hypotheses certainly make these results out of touch to a quite extraordinary degree with most important problems, and they can be capable of few applications. I do not make this judgment with complete confidence, for the book shows qualities of no negligible order. Those who wish to sample his methods may be recommended to read chapter ix, on what he terms Probabilités connexes, as a fair specimen of his original work.*

M.T. : Did Bachelier teach in a *lycée*?

B.B. : No, he did not have the necessary diplomas. You had to pass the "aggregation", the competitive examination for *lycée* teachers. He taught only at the university.

M.T. : I've also heard it said that Bachelier made errors while teaching.

B.B. : Yes, it's a rumor that's circulating but I do not know on what it is based. A brilliant candidate, Georges Cerf, obtained the Dijon chair. But after one year, Cerf left for the University of Strasbourg, which was, after Paris, the most famous university in France[34]. Since Cerf had graduated from the

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Keynes notes at the beginning of his review:

> *There never has been a systematic treatise on the mathematical theory of probability published in England, and it is now nearly fifty years since the last substantial volume to deal with this subject from any point of view (Venn's Logic of Chance, 1st edit., 1866) was brought forth here. But a year seldom passes abroad without new books about probability, and the year 1912 has been specially fertile.*

He then reviews four books, Poincaré [102], Bachelier [12], Carvallo [39] and Markov [95]. This is what he writes about (the second edition) of Poincaré's text:

> *Poincaré's Calcul des Probabilités originally appeared in 1896 as a reprint of lectures. This new edition includes the whole of the earlier edition, but is now rearranged in chapters according to the subjects treated, in place of the former awkward arrangement into lectures of equal length...*
> *The mathematics remain brilliant and the philosophy superficial – a combination, especially in the parts dealing with geometrical probability, which makes it often suggestive and often provoking. On the whole there is not a great deal in the book which cannot be found, substantially, elsewhere. Poincaré had to lecture on probability, and this is what without giving any very profound attention to the subject, he found to say. This new edition must have been almost the last material to leave his hands before his lamented death. The immense field of Henri Poincaré's achievements had made him one of the greatest mathematicians in Europe, and it must always be a matter of regret to statisticians that modern statistical methods, with their almost equal dependence on mathematics and on philosophy and logic, had not found their way to France in time to receive illumination from his brilliant and speculative intellect. This book has no reference to any of the researches, either German or English, which seek by the union of probability and statistics to forge a new weapon of scientific investigation.*

[34] Baire had been very sick and was often replaced by lecturers. Cerf had taught previously many times in Dijon, in particular from 1919 to 1922 (Bachelier did so later, from 1922 to 1925). René Lagrange got the position in Dijon in 1927 after Cerf was appointed in Strasbourg.

École Normale Supérieure (he was *normalien*) and was a specialist on partial differential equations, Gevrey's choice was obvious. Bachelier had no chance.

M.T. : What then happened to Bachelier?

B.B. : Fortunately, Bachelier was saved. He had been lecturer at Besançon and when a position became available in 1927, he obtained it. At Besançon there was a very innovative mathematician who is unfortunately no longer well known, Jules Haag. Haag was at Besançon because he headed the school of chronometry (Besançon is close to Switzerland). In probability, Haag has introduced among other things the notion of an exchangeable sequence [63], independently of Finetti. He did some very interesting studies on stochastic algorithms applied to the adjustments that must be done when shooting big guns [62]. The fact remains that he welcomed Bachelier. So the story that Bachelier taught poorly or that he made errors in his teaching, may not be fair. If that story were true, Haag would not have recommended him at Besançon.

M.T. : Where does it come from?

B.B. : I don't know. I know that it's something that had been said about him, but there is contradictory testimony, and in particular at Besançon, where he remained for almost fifteen years teaching analysis. It was probably not a very advanced course, but he must have given it in a very conscientious manner. He undoubtedly found teaching difficult. He was not capable of writing a calculation to the end without notes. In France, we do not like people who copy their notes onto the blackboard.

M.T. : Is this still the case?

B.B. : Yes, but a bit less today because students are less docile than in the past. A course for which there are no prepared notes rapidly becomes a vague and empty discourse with occasional incomprehensible flashes. Borel and Hadamard, contemporaries of Bachelier, brilliant representatives of the French mathematical elite, had reputations in the 20s and 30s of never ending a calculation nor a proof. Students always appreciate a calculation that is well done without notes, but they do not tolerate calculations that come up short. The attitude to lecturing on mathematical subjects at French universities has therefore evolved. There are innumerable anecdotes on the subject. One of the best that I know occurred in the 30s at the time when Einstein decided to leave Berlin. All the great countries offered him a position in their most prestigious universities. In France, on the recommendation of Langevin (the author in 1908 of the stochastic differential equation of Brownian motion [80]), the government decided to create a new chair for Einstein at the Collège de France, the most prominent institution of learning in the country. To Langevin, who was a professor at the Collège de France, and who invited him to accept, Einstein replied that they were doing him a great honor, but his scientific culture was so reduced that his lectures would be a laughing stock.

Any ordinary student would know what he knew[35], and he felt like a gypsy who cannot read music and is asked to become first violinist in a symphonic orchestra. Einstein preferred Princeton where he didn't have to teach (with or without notes)[36]. The letter to Langevin is found in Einstein's correspondence.

M.T. : Did Kolmogorov[37] read Bachelier?

B.B. : Yes. It was Bachelier's article [7] and its extension to the multi-dimensional case [10] that prompted Kolmogorov toward the end of the 20s to develop his theory, the analytical theory of the Markov processes [76,78]. This is what Kolmogorov wrote in 1931 ([78], Volume 2, p. 63)[38]:

> *In probability theory one usually considers only schemes according to which any changes of the states of a system are only possible at certain moments $t_1, t_2, \ldots, t_n, \ldots$ which form a discrete series. As far as I know, Bachelier*[39] *was the first to make a systematic study of schemes in which the probability $P(t_0, x, t, \mathcal{E})$ varies continuously with time t. We will return to the cases studied by Bachelier in §16 and in the Conclusion. Here we note only that Bachelier's constructions are by no means mathematically rigorous.*

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[35] He writes: *Ich bin eben kein Könner und kein Wisser sondern nur ein Sucher* (In fact, I am neither a man of action nor a man of knowledge but only a seeker).

[36] Ironically, a few years later, the situation was reversed. Langevin was arrested in October 1940 by the Gestapo and Einstein then wrote to the American Ambassador William C. Bullitt at the Department of State asking him to offer refuge to Langevin in the U.S.A.

[37] Andrei Nikolaevich Kolmogorov (1903-1987) was one of the greatest mathematicians of the twentieth century. He made fundamental contributions to many areas of pure and applied mathematics, such as trigonometric series, set theory, approximation theory, logic, topology, mechanics, ergodic theory, turbulence, population dynamics, mathematical statistics, information theory, the theory of algorithms and, naturally, probability theory. He is particularly well-known for setting the axioms of probability, for the development of limit theorems of independent random variables and for the analytic theory of Markov processes. Kolmogorov was also very interested in the application of mathematics to the social sciences and linguistics and also in the history and pedagogy of mathematics. (See the overview article [117].)

[38] One of the major contributions of Kolmogorov in his 1931 article is to make rigorous the passage from discrete to continuous schemes. He does that by extending to this setting Lindeberg's method [92] for proving the Central Limit Theorem. In this way the "hyperasymptotic" theory of Bachelier becomes rigorous. One can then derive the parabolic differential equations of Kolmogorov from the difference equations which hold when time is discrete.

[39] I. 'Théorie de la spéculation', *Ann. École Norm. Supér.* **17** (1900), 21; II. 'Les probabilités à plusieurs variables', *Ann. École Norm. Supér.* **27** (1910), 339; III. *Calcul des probabilités*, Paris, 1912.

M.T. : Thus, at the time, Kolmogorov knew Bachelier's work better than did other mathematicians[40].

B.B. : There are two important sources for Kolmogorov, Bachelier and Hostinský. Bachelier is a known source; Hostinský, much less so. Hostinský was a Czech mathematician who revived the theory of Markov chains. Markov chains as done by Markov, were meant to generalize the classical probability results to situations where there was no independence. But the development of the physical aspect of chains is due in large part to Hostinský in the last years of the 20s. To understand Kolmogorov's article [76] of 1931, where we find Kolmogorov's equation, we must refer to the two sources, Bachelier and Hostinksý. The conditions of the ergodic theorem are found in Hostinský [65,66], and the idea of continuity in probability under the condition stated by Chapman-Kolmogorov is found in Bachelier [7]. Bachelier considers a case that is not quite general, for he supposes homogeneity.

M.T. : What did Hostinský think of Bachelier?

B.B. : Not much. Hostinský wrote to Fréchet[41] that it was not worth reading Bachelier because there were too many mistakes. In fact, the mathematicians of the 30s who read Bachelier felt that his proofs are not rigorous and they are right, because he uses the language of a physicist who shows the way and provides formulas. But again, there is a difference between using that language and making mistakes. Bachelier's arguments and formulas are correct and often display extreme originality and mathematical richness.

M.T. : What did Bachelier do at Besançon?

B.B. : Bachelier published practically nothing. Obviously he must have been preparing his courses. He was at Besançon between 1927 until his retirement in 1937. He began publishing again once he left Besançon. He published three books at his own expense with Gauthier-Villars [21–23] which are revisions of his pre-war works, but most importantly, in 1941, he published an article [24] at the *Comptes Rendus* that was extremely innovative. It's that paper that Paul Lévy read.

M.T. : How did this happen?

B.B. : Lévy began to take an interest in Brownian motion toward the end of the 1930s through the Polish school, in particular through Marcinkiewicz who was in Paris in 1938. He rediscovered all of Bachelier's results which he had never really seen earlier[42]. Lévy had become enthralled with Brownian

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40 Kolmogorov told Albert Shiryaev that he has been very influenced by Bachelier (private communication from Shiryaev) [M.T.].

41 Fréchet archives at the *Académie des Sciences*, Institut de France, quai Conti.

42 Paul Lévy writes in his book of memoirs [90], p. 123:

> *I learned only after the 1939-1945 war that L. Bachelier had published a new book on Brownian motion just before the war. I do not exclude the possibility*

motion. The book on stochastic processes [89] that he undertook to write was not published until 1948. Lévy was Jewish, and therefore forbidden to publish during the war.

M.T. : Where was Lévy during the Second World War?

B.B. : He went to Lyon since he was professor at the École Polytechnique. The École Polytechnique had relocated to Lyon, a "free zone" under Pétain. There were racist laws. But since he was professor at a military school, he was able to continue teaching for a while. After the American landing in North Africa in 1942, the Germans invaded the free zone. The first large raid on Jews in Paris occurred in July 1942. Lévy hid under an assumed name in Grenoble, and then in Mâcon.

M.T. : Bachelier's paper was 1941.

B.B. : It was while Lévy was still at Lyon. Bachelier, who had retired to Brittany with one of his sisters, must have sent him a reprint. An annotated copy exists in the Lévy archives[43]. Lévy wrote in the margin of that copy that he had written to Bachelier and that Bachelier had told him about additional properties that he knew about. One also finds in the margin comments by Lévy about the obvious enthusiasm that Bachelier has for mathematical research (this was 1942 or thereabouts). The results in this paper of Bachelier, annotated by Lévy, are about excursions of Brownian motion and they were beyond Lévy's latest results. Here is also an excerpt of a letter from Lévy to Fréchet[44] dated September 27, 1943:

> *Concerning priority, I recently had a correspondence with Bachelier, who told me that he had published the equation attributed to Chapman in a math journal in 1906. Can you verify whether that is accurate or have your students verify it? He also gave me some indication about Brownian motion on the surface of a sphere, which would have been studied by Perrin, and I have asked Loève to verify it.*

This excerpt shows that until 1942 or 43, Lévy really knew neither Bachelier's articles from the beginning of the century, not even the thesis [99] of Francis Perrin of 1928. Lévy, who was at that time doing detailed studies of Brownian motion, at last recognized the originality of Bachelier's results. He also wrote

> *that there may be in this book some of the results of my [later] paper. Being busy with other work, I have never checked this.*

[Translation by M.T.]

43 Archives Lévy at the interuniversity mathematics library, Universités Paris VI et VII, Paris.

44 Box 2 of the Fréchet archives at the *Académie des Sciences*, Institut de France, quai Conti, Paris.

to him and apologized[45]:

> *We became reconciled. I had written him that I regretted that an impression, produced by a single initial error, should have kept me from going on with my reading of a work in which there were so many interesting ideas. He replied with a long letter in which he expressed great enthusiasm for research.*

Bachelier, who died in 1946 at the age of 76, thus corresponded with Lévy just before his death[46]. That must have been Bachelier's great satisfaction, to be read by someone, and by the best!

Epilogue

Kiyosi Itô, in Japan, was also influenced by Bachelier, more so than by Wiener[47], and in the United States, Bachelier was read by probabilists such as Paul Erdös, Mark Kac, William Feller and Kai Lai Chung[48] in the forties. But it seems that it is Paul Samuelson[49] who introduced Bachelier to economists in the 50s. This is how it happened[50]:

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45 Contination of the letter dated January 25, 1964 from Lévy to Mandelbrot [93].

46 Louis Bachelier died on April 28, 1946 in Saint-Servan-sur-Mer, near Saint Malo in Brittany. He is buried in the Bachelier family's plot in Sanvic, Normandy, near Le Havre.

47 Personal communication from the economist Robert C. Merton. Itô told this to Merton during the 1994 Wiener symposium at MIT.

48 See Erdös and Kac [52], Chung [40], and Feller [54] who writes (in a footnote, p. 323):

> *Credit for discovering the connection between random walks and diffusion is due principally to L. Bachelier (1870-   ). His work is frequently of a heuristic nature, but he derived many new results. Kolmogorov's theory of stochastic processes of Markov type is based largely on Bachelier's ideas. See in particular L. Bachelier Calcul des Probabilités, Paris, 1912.*

Doob [48], in his article on Kolmogorov, also writes positively about Bachelier:

> *Bachelier, in papers from 1900 on, derived properties of the Brownian motion process from asymptotic Bernoulli trial properties. His Brownian motion process was necessarily not precisely defined, but his application of the André reflection principle becomes valid for the Brownian motion process as an application of the strong Markov property. His valuable results were repeatedly rediscovered by later researchers.*

49 Paul Samuelson received the Nobel prize in Economics in 1970.

50 As told to M.T. by Paul Samuelson on August 14, 2000. See also [116] for a somewhat similar account. The date 1957, indicated in [116], is probably a little late

Around 1955, Leonard Jimmie Savage, who had discovered Bachelier's 1914 publication in the Chicago or Yale library sent half a dozen "blue ditto" postcards to colleagues, asking "does any one of you know him?" Paul Samuelson was one of the recipients. Samuelson, however, had already heard of Bachelier. First from Stanislaw Ulam, between 1937 and 1940, who then belonged like him to the Society of Fellows at Harvard University. Ulam was a gambler by instinct. He was a topologist who later popularized Monte Carlo methods and worked on the atom bomb at Los Alamos. Samuelson also knew of Bachelier from Feller [54]. But prompted by Savage's postcard, Samuelson looked for and found Bachelier's 1900 thesis at the MIT library. Soon after, in ditto manuscripts and informal talks, Samuelson suggested using geometric Brownian motion as a model for stocks[51].

Today, a full century after his thesis, Bachelier is rightly viewed as the father of mathematical finance.

Acknowledgments: I thank Bernard Bru for having received me so graciously at his home, for granting me this interview and for his continuing valuable input. This meeting was suggested by Jean-Pierre Kahane and Christian Gilain. I am equally grateful to Franck Jovanovic for a careful reading and to Corine Astier, Catriona Byrne, Marie-France Bru, Vladas Pipiras, Jean-Francis Ravoux, Rachelle Taqqu, Gérard Vichniac and Nader Yeganefar for their assistance in formatting the original French version of this paper. And I thank Marie Isenburg for doing the English translation.

because Savage's postcard must have been sent no later than 1956, the year of Richard Kruizenga's thesis [79] at MIT (Kruizenga, who was Samuelson's student, quotes Bachelier in his thesis).

[51] The lognormal model was used in several contexts in economics. It was fashionable in Paris in the thirties and forties because of the economist Robert Gibrat [61], who used it instead of the Pareto distribution, to model income. The article Armatte [4] provides many references about that. See also Aitchison and Brown [1], Osborne [97] and Cootner [41].

Dates

1700 – 1800		
Pierre Simon, marquis de Laplace	1749 – 1827	(78 years)
Robert Brown	1773 – 1858	(85 years)
Adolphe Quetelet	1796 – 1874	(78 years)
1800 – 1850		
Antoine Augustin Cournot	1801 – 1877	(76 years)
Joseph Bertrand	1822 – 1900	(78 years)
Henri Lefèvre	1827 – ?	
Émile Dormoy	1829 – 1891	(62 years)
Désiré André	1840 – 1917	(77 years)
John William Strutt Rayleigh (Lord)	1842 – 1919	(77 years)
Joseph Boussinesq	1842 – 1922	(80 years)
Ludwig Eduard Boltzmann	1844 – 1906	(62 years)
1850 – 1875		
Henri Poincaré	1854 – 1912	(58 years)
Paul Appell	1855 – 1930	(75 years)
Émile Picard	1856 – 1941	(85 years)
Jacques Hadamard	1865 – 1963	(98 years)
Louis Bachelier	1870 – 1946	(76 years)
Robert de Montessus	1870 – 1937	(67 years)
Jean Batiste Perrin	1870 – 1942	(72 years)
Émile Borel	1871 – 1956	(85 years)
Paul Langevin	1872 – 1946	(74 years)
Alfred Barriol	1873 – 1959	(86 years)
René Baire	1874 – 1932	(58 years)
1875 – 1900		
Maurice René Fréchet	1878 – 1973	(95 years)
Albert Einstein	1879 – 1955	(76 years)
Jules Haag	1882 – 1953	(71 years)
John Maynard Keynes	1883 – 1946	(63 years)
Bohuslav Hostinský	1884 – 1951	(67 years)
Maurice Gevrey	1884 – 1957	(73 years)
Paul Lévy	1886 – 1971	(85 years)
George Pólya	1887 – 1985	(98 years)
Georges Cerf	1888 – 1979	(91 years)
Alexander Yakovlevich Khinchine	1894 – 1959	(65 years)
Norbert Wiener	1894 – 1964	(70 years)
1900 – 1925		
Francis Perrin	1901 – 1992	(91 years)
Andrei Nikolaevich Kolmogorov	1903 – 1987	(84 years)
William Feller	1906 – 1970	(64 years)
Stanislaw Ulam	1909 – 1984	(75 years)
Paul Erdös	1913 – 1996	(83 years)
Marc Kac	1914 – 1984	(70 years)
Kiyoshi Itô	1915 –	
Paul A. Samuelson	1915 –	
Kai Lai Chung	1917 –	
Benoit B. Mandelbrot	1924 –	

Regnault's 1863 law on the square root of time[52]

After much thought, we realized that it is not possible to find a relation between stock market gains and losses. It is... with respect to time ... that we found a relation...

In decreasing the time periods to 5 days, 3 days, 2 days ... the mean deviations decrease steadily.

Consequently, the deviations are smaller for shorter time intervals and larger for longer time intervals.

Finally, if one tries to find how these different deviations are related to the different times in which they occur, one notices that as the period decreases by half, the deviation decreases not by half but, roughly, in the proportion 1:1.41; for a period which is three times shorter, the deviation decreases in the proportion 1:1.73, for a time period which is four times shorter, the ratio is 1:2.

There exists therefore a mathematical law which regulates the variations and the mean deviation of stock market prices, and this law, which seems never to have been noticed, is given here for the first time:

THE PRICE DEVIATION IS DIRECTLY PROPORTIONAL TO THE SQUARE ROOT OF TIME.[53]

Hence the investor who wants to sell after the deviation doubles, that is with a difference twice as large between the buy and sell price must wait four times longer, if he wants to sell with triple deviations, [he must wait] nine times longer, and so forth. One multiplies the time by the square of the deviations.

One who leaves only one day between [his buying and] selling, would sell with a deviation which is smaller by one half than one who sells every four days, three times smaller than one who sells every nine days, etc..., dividing the deviations by the square root of time.

Quite a large number of transactions is required, however, in order to make these ratios clearly apparent, and they become strictly correct when the number of transactions is exceedingly great.

Let us understand the reason for this remakable law:

The security varies but is always looking for its real price or an absolute price, which one can represent as the center of a circle whose radius represents the deviation, which may be anywhere on the surface. Time is equal to the surface and the points on the circumference represent extreme deviations. As

52 Regnault [111], pages 49-52 (text provided by Franck Jovanovic). Translated by M.T.

53 Capitalized in the original text.

it varies, the security moves either away from or closer to the center, and the basic notions of geometry teach us that the radii or deviations are proportional to the square root of the area, that is of time.

Why is it that the reciprocal law holds when dealing with either gravity or the oscillations of a pendulum, where [in one case] the space traveled or [in the second case] the deviation of the oscillations is proportional to the square of time? It is only because these falling bodies go from the circumference to the center, whereas the stock price in its greatest deviations, is pushed away from the center towards the circumference.

How astonishing and admirable are the ways of Providence, what thoughts come to our mind when observing the marvelous order which presides over the most minute details of the most hidden events! What! The changes in stock market prices are subject to fixed mathematical laws! Events produced by the passing fancy of men, the most unpredictable shocks of the political world, of clever financial schemes, the outcome of a vast number of unrelated events, all this combines and randomness becomes a word without meaning! And now worldly princes, learn and be humble, you who in your pride, dream to hold in your hands the destiny of nations, kings of finance who have at your disposal the wealth and credit of governments, you are but frail and docile instruments in the hands of the One who brings all causes and effects together in harmony and who, as the Bible says, has measured, weighed and parcelled out everything in perfect order.

Man bustles but God leads.

Regnault writes further:

The price of the "Rente," while fluctuating capriciously, remains influenced in final instance by constant causes. The most important one, clearly defined and whose existence is without doubt, is the interest rate. This cause, so feeble in appearance, finally dominates all others. The accidental causes [will] have totally disappeared and, however powerful their effects, however strange and irregular they appear, they always end up after a while cancelling almost completely, revealing the influence of constant and regular causes, however weak the effect [of these regular causes] is[54].

The causes for a drop [in price] are fewer than those for a rise [in price] but, while they are few in number, they make this up by their strength; so that by multiplying number by strength one would obtain a constant value.[55].

The price variations obey two distinct laws. The first is that the deviations are proportional to the square root of time ... The second is that the value [of the stock] whatever its deviation, is constantly attracted towards its average price as the square of its distance [to that price][56].

[54] [111], page 154.
[55] [111], p. 161.
[56] [111], p. 187.

Report on Bachelier's thesis (March 29, 1900)[57]

Le sujet choisi par M. Bachelier s'éloigne un peu de ceux qui sont habituellement traités par nos candidats; sa thèse est intitulée Théorie de la Spéculation et a pour object l'application du Calcul des Probabilités aux Opérations de Bourse. On pourrait craindre d'abord que l'auteur ne se soit fait illusion sur la portée du Calcul des Probabilités, comme on l'a fait trop souvent. Il n'en est rien heureusement; dans son introduction et plus loin dans le paragraphe intitulé "La probabilité dans les Opérations de Bourse", il s'efforce de fixer les limites dans lesquelles on peut avoir légitimement recours à ce genre de Calcul; il n'éxagère donc pas la portée de ses résultats et je ne crois pas qu'il soit dupe de ses formules.

Qu'a-t-on donc légitimement le droit d'affirmer en pareille matière? Il est clair d'abord que les cours relatifs aux diverses sortes d'opérations doivent obéir à certaines lois; ainsi on pourrait imaginer des combinaisons de cours telles que l'on puisse jouer à coup sûr; l'auteur en cite des exemples; il est évident que de pareilles combinaisons ne se produisent jamais, ou que si elles se produisaient elles ne sauraient se maintenir. L'acheteur croit la hausse probable, sans quoi il n'achèterait pas, mais s'il achète, c'est que quelqu'un lui vend; et ce vendeur croit évidemment la baisse probable; d'où il résulte que le marché pris dans son ensemble considère comme nulle l'espérance mathématique de toute opération et de toute combinaison d'opérations.

Quelles sont les conséquences mathématiques d'un pareil principe? Si l'on suppose que les écarts ne sont pas très grands, on peut admettre que la probabilité d'un écart donné par rapport au cours coté ne dépend pas de la valeur absolue de ce cours; dans ces conditions le principe de l'espérance mathématique suffit pour déterminer la loi des probabilités; on retombe sur la célèbre loi des erreurs de Gauss.

Comme cette loi a été l'objet de démonstrations nombreuses qui pour la plupart sont de simples paralogismes, il convient d'être circonspect et d'examiner cette démonstration de près; ou du moins il est nécessaire d'énoncer d'une manière précise les hypothèses que l'on fait. Ici l'hypothèse que l'on a à faire c'est, comme je viens de le dire, que la probabilité d'un écart donné à partir du cours actuel est indépendante de la valeur absolue de ce cours. L'hypothèse peut être admise, pourvu que les écarts ne soient pas trop grands. L'auteur l'énonce nettement, sans y insister peut-être autant qu'il conviendrait. Il suffit pourtant qu'il l'ait énoncée explicitement pour que ses raisonnements soient corrects.

La manière dont M. Bachelier tire la loi de Gauss est fort originale et d'autant plus intéressante que son raisonnement pourrait s'étendre avec quelques changements à la théorie même des erreurs. Il le développe dans un chapitre dont le titre peut d'abord sembler étrange, car il l'intitule "Rayonnement de

[57] *Registre des thèses de la Faculté des Sciences de Paris*, at the *Archives nationales*, 11 rue des Quatre-Fils, 75003 Paris, classification AJ/16/5537.

la Probabilité." C'est en effet à une comparaison avec la théorie analytique de la propagation de la chaleur que l'auteur a eu recours. Un peu de réflexion montre que l'analogie est réelle et la comparaison légitime. Les raisonnements de Fourier sont applicables presque sans changement à ce problème si différent de celui pour lequel ils ont été créés.

On peut regretter que M. Bachelier n'ait pas développé davantage cette partie de sa thèse. Il aurait pu entrer dans le détail de l'Analyse de Fourier. Il en a dit assez cependant pour justifier la loi de Gauss et faire entrevoir les cas où elle cesserait d'être légitime.

La loi de Gauss étant établie, on peut en déduire assez aisément certaines conséquences susceptibles d'une vérification expérimentale. Telle est par exemple la relation entre la valeur d'une prime et l'écart avec le ferme. On ne doit pas s'attendre à une vérification très exacte. Le principe de l'espérance mathématique s'impose en ce sens que, s'il était violé, il y aurait toujours des gens qui auraient intérêt à jouer de façon à le rétablir et qu'ils finiraient par s'en apercevoir. Mais ils ne s'en apercevront que si l'écart est considérable. La vérification ne peut donc être que grossière. L'auteur de la thèse donne des statistiques où elle se fait d'une façon très satisfaisante.

M. Bachelier examine ensuite un problème qui au premier abord semble devoir donner lieu à des calculs très compliqués. Quelle est la probabilité pour que tel cours soit atteint avant telle date? En écrivant l'équation du problème, on est conduit à une intégrale multiple où on voit autant de signes $\int$ *superposés qu'il y a de jours avant la date fixée. Cette équation semble d'abord inabordable. L'auteur la résout par un raisonnement court, simple et élégant; il en fait d'ailleurs remarquer l'analogie avec le raisonnement connu de M. André au sujet du problème du dépouillement d'un scrutin. Mais cette analogie n'est pas assez étroite pour diminuer en quoi que ce soit l'originalité de cet ingénieux artifice. Pour d'autres problèmes analogues, l'auteur s'en sert également avec succès.*

En résumé, nous sommes d'avis qu'il y a lieu d'autoriser M. Bachelier à faire imprimer sa thèse et à la soutenir.

Signed: *Appell, Poincaré, J. Boussinesq*

Here is the thesis defense report:

Dans la soutenance de sa premiere thèse, M. Bachelier a fait preuve d'intelligence mathématique et de pénétration. Il a ajouté des résultat intéressants à ceux que contient la thèse imprimée, notamment une application de la méthode des images.

Dans la $2^{ième}$ *thèse, il a montré qu'il possédait à fond les travaux de M. Boussinesq sur le mouvement d'une sphère dans un fluide indéfini.*

La Faculté lui a conféré le grade de Docteur avec mention honorable.

Signed: *Le président P. Appell*

Remarks on the bibliography

Louis Bachelier's books are [5,12,15,21–23]. His articles are [6–11,13,14,16–20,24]. The English translation of his thesis [5] can be found in [41]. The best available biography of Louis Bachelier is by Courtault et. al. [44]; we have made use of it here. (Jean-Michel Courtault and Youri Kabanov organized an exhibit on Bachelier at the University of Besançon.) See also the biographical sketch in Mandelbrot [93]. The complicated relations between Émile Borel and Paul Lévy are detailed in Bru [38]. Jules Regnault's book is analyzed in a thesis by Franck Jovanovic, Université de Paris 1 (see also [71]). The Paris financial market of the second empire is described in Pierre Dupont-Ferrier's book [50]. A study on Bachelier's mathematical works that is quite complete and very interesting is now being done by Laurent Carraro of l'École des Mines of Saint-Etienne. Finally, we mention Paul Cootner's introduction [41], the articles of Christian Walter [121,122] on the financial aspects of Bachelier's work, and Jean-Pierre Kahane's article [72] on the mathematical origins of Brownian motion.

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