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Bachelier and his times: A conversation with Bernard Bru

Murad S. Taqqu

Boston University, Department of Mathematics, 111 Cummington Street, Boston, MA 02215-2411, USA (e-mail: murad@math.bu.edu)

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.

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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

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youth. What do we know about it?

B.B. : Not much. Bachelier was born at 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 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's what was called a "perpetual bond", and the success of the original offering lead 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, 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 physicist-mathematician. There were two important areas in mathematics at the end of the 19th century: mathematical physics, ie. 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 transfered to a chair in celestial mechanics.

M.T. : Bachelier then 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. : It was 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]. After the thesis, it was necessary to find a university position, which 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?

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[1] equivalent to a Bachelor/Masters 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.

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, mister, 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 win 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. : He was 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, 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 later. 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 – they renewed their interest 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. : Bachelier's thesis was considered a thesis on probability. Is that how he saw it?

B.B. : No. It was a thesis on mathematical physics, 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 would 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 one's we know today.) He uses the increments of Brownian motion to model "absolute" price changes, whereas today, one prefers to use

[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 is described in [54]. 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 [50] in 1905.

them to model “relative” price changes (see Samuelson [105, 106, 107][5]).

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 [104], 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.

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[5] The idea of modeling the logarithm of prices by independent and normally distributed random variables was also suggested by Osborne [88] 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 [90] which are his lecture notes at the University of California at Berkeley. In his 1959 article [88], Osborne does not mention Bachelier but, following a letter by A. G. Laurent [75] in the same volume, Osborne provided a reply [89], where he quotes Bachelier. He starts [89] by indicating that after the publication of his 1959 article [88], 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 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

Borel answered, “You are probably under the impression that our relationships with people who attend our courses are similar here as 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?

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

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’s 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)*[10]. If

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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 [55] for the story of his life.

[8] See [104], p. 66.

[9] The full text, translated into English, by Selime Baftiri-Balazoski and Ulrich Hausmann, can be found in [43].

[10] 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.

you look at Poincaré's course on probability, you will see that he is skeptical with regard to its applications.

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 [98, 99]. You are adding n oscillations together. The simplest version of this is coin tossing. One of Bachelier's demonstrations (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. : The reflection principle is 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 passed his theses, 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. Was he then not the first to see trajectories since the reflection principle is based on them?

B.B. : The argument in Désiré André involves combinatorial symmetry. There is neither time nor trajectories, but obviously, he is 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

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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.* [Tranlation by M.T.].

distorted. In coin tossing, we see the trajectories rise and fall. During that period, that 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 [94]). 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, that 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's series", "the Bertrand's 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 [98], 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 [96] who was in Switzerland, in Zürich, who in 1930 made known in Paris Rayleigh's results. Pólya was one who 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 at the end of the 1920s. 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 under 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 if one looks at the manuscripts that are in the Archives of the Academy of Science. The formulas are calligraphed as though they were works of art (and the demonstrations 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 view, but he never did what he, himself, had done.

M.T. : The asymptotic point of view that's the point of view of the Gaussian limit. The hyperasymptotic view is the point of view of trajectory limits, 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 [70] in 1931[11] and Khinchine [69] in 1933[12] 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 didn't have descendants and bequeathed their bonds to the university. The first research grants date from 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 [103] in 1863 (see [66]). Forty years before Bachelier, he saw that the square-root law applied, that the standard deviation is expressed in terms of square-root of time. It's a book on the philosophy of the Exchange that is quite rare; I don't know whether it exists in the USA. I know only of one copy, at the *Bibliothèque Nationale*.

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[11] See below.

[12] This is what Khinchine [69] 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 [70, 71] and de Finetti [45, 44] constitute one the most beautiful chapters dealing with probability theory ... [Translated from the German. The reference numbers are ours.]

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...). But he verified the square-root 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 Quetelet and perhaps Cournot[13]. We still know nothing of this Regnault, who would have been the Kepler of the Exchange as Bachelier would have been the 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 in 1873 in reference to the square root law (see [48]). The stockbrokers took Regnault's book into account and if you look at the finance courses of the end of the 19th century, they refer to square-root law.

M.T. : Then 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[14]. One could at the same time buy and sell the same product in different ways. There is a graphic means of representing this. Bachelier's first observations are based on these diagrams.

M.T. : All of that applies only to bonds?

B.B. : Yes.

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

[13] Adolphe Quetelet (1796-1874) was influenced by Laplace and Fourier. He used the normal curve in settings different from that of the error law [97]. Antoine Augustin Cournot (1801-1877) wrote [42] but also [41], where he discusses supply and demand functions.

[14] 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 a French society *l'Agence centrale de l'union financière* and his books on the stockmarket [76, 78] date from that period. He was well acquainted with the economic life of the time and his diagrams are quite clever (see [65b]). These diagrams were rediscovered independently by Léon Pochet [93], a graduate from the *École Polytechnique*, but Lefèvre complains and claims priority [77]. 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. He died circa 1885.

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 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 recommended 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 obviously 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* [40].

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. It was in 1905 that Borel published his first works on probability [30].

M.T. : Was he younger than Bachelier?

B.B. : No, they were about the same age. Borel 1871, Bachelier 1870. Borel surely was very interested in probability, but not in Bachelier. Borel was brought to report on Bachelier’s requests for grants. He always made 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. There was no more family money. Borel had a prominent position on the Council of the Faculty of Sciences. Each time that Bachelier submitted a request, Borel made a favorable report. These were small sums of money that allowed Bachelier to live very modestly for a year. I believe he received 2000 francs four times. This was in gold francs, but it was a very small sum. So Bachelier, beginning in 1906-1907, obtained small grants three or four times like that. He then had a small income. It was then that he must have written his enormous treatise on probability, published at the author's 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, which is titled, "On continuous probability" (cf. [7]). It's an extraordinary article. He had two major accomplishments, his thesis and this.

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

B.B. : De Montessus[15] [46] 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[16] [25] on financial transactions. And there is also a popularizing book on the stock market by Gherardt [57a], where Regnault and Bacherlier are quoted. 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 was it that Émile Borel had so much power to award grants? He must have been very young as well?

B.B. : Borel defended his doctorate in 1894 at the age of 23. He was exceptional. He was appointed to the Sorbonne at 25, which was without precedent 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.

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, for Borel, who was a brilliant

[15] 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.

[16] 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 [46] did not have much success, the one by Barriol [25] was used by generations of students in finance and insurance.

thinker, did not interest him. 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[17]. 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. You understand what "elitist" means within the French context? It means that Bachelier had no importance.

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[18]. He also wrote another book [15] which appeared in 1914, entitled "Game, Chance and Randomness", 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 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 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: 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[19], a

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[17] Francis Perrin (1901-1992), the son of the Nobel prize laureate Jean Perrin, did not receive a 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.

[18] Borel taught a probability course [32] twice in 1908 and 1909 and it is likely that it is this 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 held since 1908 to the chair in probability and mathematical physics, then held by Boussinesq.

[19] Maurice Gevrey (1884-1957) was an important mathematician working on parabolic partial differential equations, following Hadamard [61]. The existence and uniqueness theorem of Markov processes in Feller [52] is based on the theory of Hadamard and Gevrey.

specialist in partial differential equations. It was he who was to make 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. In 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 a probability $1/2$ each. Therefore the variance of its position is $\mathrm{Var}(\sum \pm v dt) = (v\, dt)^2 t/dt$, of the order of dt. Since dt is infinitesimal, there is no motion. 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 $\mathrm{Var}(\sum \pm v dt) = \mathrm{Var}(\sum \pm v_0 \sqrt{dt}) = (v_0^2 dt)t/dt = v_0^2 t$, 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?

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 made a devastating report. But since he was not competent in probability, he sent it to Paul Lévy[20]. Lévy, at that time (1926), had just published an important work on probability (cf. [79]). 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

[20] 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.

students who formed a type of caste. Obviously, Gevrey wanted nothing to do with Bachelier. Gevrey sent 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 was just pointed out by Jean Perrin in the context of the motion of molecules. It's indeed in 1913 that Jean Perrin published "The Atoms" (cf. [92]), 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 this type of article to the *Annales de l'École Normale Supérieure*, in spite of the 'mistake" revealed by Lévy and Gevrey, which is finally nothing but a daring mechanics 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[21].

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 his book [82] of 1948 on Brownian motion[22] and in his 1970 book of memoirs [83]. In that second book, 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[23]. It seems that

[21] 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. [**?**] contains a number of excerpts from this letter.

[22] Here are the footnotes in [82] (second edition) about Bachelier, which mention:

-page 15 footnote (1): the priority of Bachelier on Wiener about Brownian motion.

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

-page 193 footnote (4): the priority of Bachelier on 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.

[23] Lévy [83] writes (p. 97):

The linear Brownian motion function $X(t)$ *is often called the* function of Wiener. *It is indeed 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* 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[24]. 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 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[25]". 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 $\max_{0\leq\tau\leq t} X(t)$.[26]

In this matter with Gevrey, Lévy did not bother to understand what Bachelier wanted to say, that once and for all, Brownian motion existed, since the time of his thesis where the normalizations were made 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 $\pm v_0/\sqrt{dt}$ that represents for Lévy as for Bachelier a different kind of speed which "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 [68], quoting Bachelier's texts [12, 15] but only in the context of statistical frequency and Laplace's rule of succession. He had also reviewed Bachelier's text *Calcul des Probabilités* [12] in 1912. Bachelier's work on finance is not mentionned.

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

by his initial mistake. It is Kolmogorov who quoted Bachelier in his 1931 article ... and I recognized then the injustice of my initial conclusion. [Tranlation by M.T.].

[24] 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 on Bachelier in [86], pages 392-394.

[25] Bachelier's case.

[26] Another excerpt from this letter will be quoted below.

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[27]. Since Cerf had graduated from the 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 [60], independently of Finetti. He did some very interesting studies on stochastic algorithms applied to the adjustments that must be done when shooting big guns [59]. 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 recopy their notes at 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,

[27] 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). It is René Lagrange who got the position in Dijon in 1927 after Cerf was appointed in Strasbourg.

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 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 [73]), 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 knows[27a] what he knows 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)[27b]. The letter to Langevin is found in Einstein's correspondence.

M.T. : Did Kolmogorov[28] read Bachelier?

B.B. : Yes. It is Bachelier's article [7] and its extension to the multidimensional case [10] that prompted Kolmogorov toward the end of the 20s to develop his theory, the analytical theory of the Markov processes [70, 72]. This is what Kolmogorov wrote in 1931 ([72], Volume 2, p. 63)[29]:

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

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[27a] 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 full of knowledge but only a seeker).

[27b] 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.

[28] 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 [109].)

[29] 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 [85] 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.

$t_1, t_2, \ldots, t_n, \ldots$ *which form a discrete series. As far as I know, Bachelier*[30] *was the first to make a systematic study of schemes in which the probability* $P(t_0, x, t, \mathscr{E})$ *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.*

M.T. : At the time, Kolmogorov therefore knew Bachelier's work better than did other mathematicians[31].

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 [70] 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 [62, 63], 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[32] 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 at Gauthier-Villars [21, 22, 23] which are revisions of his pre-war works, but most importantly, in 1941, he published an article [24]

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[30] 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.

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

[32] Fréchet archives at the *Académie des Sciences*, Institut de France, quai Conti.

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 by way of the Polish school, in particular Marcinkiewicz who was in Paris in 1938. He rediscovered all of Bachelier's results which he had never really seen earlier[33]. Lévy had become enthralled with Brownian motion. The book on stochastic processes [82] that he undertook to write was not published until 1948. Lévy was Jewish, and therefore forbidden from publishing 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[34]. 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[35] 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 1906 in a math journal. Can you verify whether that is accurate or have your students verify

[33] This is what Paul Lévy writes in his book of memoirs [83], 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 that there may be in this book some of the results of my [later] paper. Being busy by other work, I have never checked this. [Tranlation by M.T.]

[34] Archives Lévy at the interuniversity mathematics library, Universités Paris VI et VII, Paris.

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

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've 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 [91] 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 to him and apologized[36]:

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[37]. That must have been Bachelier's great satisfaction, read by someone, and by the best!

Epilogue

Kiyosi Itô, in Japan, was also influenced by Bachelier, more so than by Wiener[38], and in the United States, Bachelier was read by probabilists such as Paul Erdös, Mark Kac, William Feller and Kai Lai Chung[39]. But it seems that it is Paul Samuelson[40] who introduced Bachelier to economists in the 50s. This is how it happened[41]:

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

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[36] Contination of the letter dated January 25, 1964 from Lévy to Mandelbrot [86].

[37] 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.

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

[39] See Erdös and Kac [51], Chung [39], and Feller [53] 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 the Markov type is based largely on Bachelier's ideas. See in particular L. Bachelier Calcul des Probabilités*, Paris, 1912.*

Doob [47], 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.

[40] Paul Samuelson received the Nobel prize in Economics in 1970.

[41] As told to M.T. by Paul Samuelson on August 14, 2000. See also [108] for a somewhat similar account. The date 1957, indicated in [108], is probably a little late because Savage's postcard must have been sent no later than 1956, the year of Richard Kruizenga's thesis at MIT. (Kruizenga, who was Samuelson's student, quotes Bachelier in his thesis.)

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 [53]. 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[42].

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

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Dates

1700–1800	
Pierre Simon, marquis de Laplace	1749–1827
Robert Brown	1773–1858
Adolphe Quetelet	1796–1874
1800–1850	
Antoine Augustin Cournot	1801–1877
Joseph Bertrand	1822–1900
Henri Lefèvre	1827– ?
Émile Dormoy	1829–1891
Désiré André	1840–1917
John William Strutt Rayleigh (Lord)	1842–1919
Joseph Boussinesq	1842–1922
Ludwig Eduard Boltzmann	1844–1906
1850–1875	
Henri Poincaré	1854–1912
Paul Appell	1855–1930
Émile Picard	1856–1941
Jacques Hadamard	1865–1963

[42] 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 [58], who used it instead of the Pareto distribution, to model income. The article Armatte [4] provides many references about that. See also Aitchson and Brown [1], Osborne [89] and Cootner [40].

1850–1875 (continued)	
Louis Bachelier	1870–1946
Robert de Montessus	1870–1937
Jean Baptiste Perrin	1870–1942
Émile Borel	1871–1956
Paul Langevin	1872–1946
Alfred Barriol	1873–1959
René Baire	1874–1932
1875–1900	
Maurice René Fréchet	1878–1973
Albert Einstein	1879–1955
Jules Haag	1882–1953
John Maynard Keynes	1883–1946
Bohuslav Hostinský	1884–1951
Maurice Gevrey	1884–1957
Paul Lévy	1886–1971
George Pólya	1887–1985
Georges Cerf	1888–1979
Alexander Yakovlevich Khinchin	1894–1959
Norbert Wiener	1894–1964
1900–1925	
Francis Perrin	1901–1992
Andrei Nikolaevich Kolmogorov	1903–1987
William Feller	1906–1970
Stanislaw Ulam	1909–1984
Paul Erdös	1913–1996
Marc Kac	1914–1984
Kiyoshi Itô	1915–
Paul A. Samuelson	1915–
Kai Lai Chung	1917–
Benoit B. Mandelbrot	1924–

Remarks on the bibliography

Louis Bachelier's books are [5, 12, 15, 21, 22, 23]. His articles are [6, 7, 8, 9, 10, 11, 13, 14, 16, 17, 18, 19, 20, 24]. The English translation of his thesis [5] can be found in [40]. The best available biography of Louis Bachelier is by Courtault et. al. [43]; 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 [86]. 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 [65a]). The Paris financial market of the second empire is described in Pierre Dupont–Ferrier's book [49]. 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 [40], the articles of Christian Walter [111, 112] on the financial aspects of Bachelier's work, and Jean–Pierre Kahane's article [67] on the mathematical origins of Brownian motion.

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