A Moment of the Probabilistic Experience

The Theory of Stochastic Processes and Their Role in the Financial Markets[i]

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Prisme N° 17

February 2010

English translation and updated version

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[i] This article is based on the seminar "The Autonomization of Probability as a Science: The Experience of a Probabilist", delivered by Nicole El Karoui at the Cournot Centre, with Michel Armatte as commentator. The seminar was transcribed by Delphine Chapuis. The original French text was translated into English by Richard Crabtree. Special thanks to Elizabeth Purdom for her contribution to this version.
A film of the seminar is available at www.centrecournot.org.

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Contents

Probabilists are often interested in the history of their discipline, and more rarely by the fundamental questions that they could ask about the facts they model. Works like *Augustin Cournot: Modelling Economics*, and especially the chapter by Glenn Shafer,[1] throw light on some of my experience in the domain of probability over the last 40 years, which began in 1968, at the end of the first year of my Ph.D. They have prompted me to present my own point of view here.

I had the good fortune to participate in an extraordinary moment in the development of probability, more precisely the theory of stochastic processes. This was an unforgettable period for me. At the time, I had the feeling that I was witnessing science – probability theory – in the making. Subsequently, (rather by chance, it must be said) I switched over to the side of probability users, about 20 years ago, by focusing my research on one particular sector of finance. In the present text, I shall try to explain what interested me in the approach and in this aspect of finance on which I am still working today. To begin with, my account will be that of a pure probability theorist, and then that of an applied probabilist.

Random processes in the 1970s and 1980s

How probability theory developed in France at the end of the 1960s

Here I will discuss what framed and accompanied my research into stochastic processes. First, what is a probabilist? It is someone who does not ask (too many) questions about probability, someone who just finds him or herself doing it. Most of my colleagues, myself included, have always used probability as a means of expressing and solving questions relating to uncertainty. A probabilist is also a theorist who has happened upon the *magic* world of probability, where rigour is introduced into the world of chance. This is a feeling that I have often shared with my colleagues.

According to a survey we carried out amongst ourselves, most of those doing research in probability never had lessons on finite probabilities, nor on counting techniques. This was lucky for us, because we would never have embarked on probability afterwards! We were more motivated by the modelling possibilities. For continuous time probability, there were two leading research centres. (I won't describe the whole range of people who have worked on probability, simply my experience, which corresponds to a great moment in France, which was first in the field at that time). I say "leading" because research in probability was also developing in other places, but the most important sites were the *Laboratoire de Probabilités* at the University of Paris-VI and the *Centre de Mathématiques* at the University Louis-Pasteur in Strasbourg. Recently, the concentration of probabilists in France has diversified to places such as the *École Normale Supérieure*, Grenoble, Orsay, Rennes and Toulouse.

In 1968, when I was starting my thesis, the *Laboratoire de Probabilités* was situated at the Institut Henri Poincaré. The whole history of this institute is of interest to us. It was created by the Rockefeller Foundation with government support and a donation from the Rothschild family in 1928, to promote "the calculus of probability and mathematical physics on the one hand, and theoretical physics on the other". There was therefore a special mention for probability theory in this context. Since that time, probability research has intensified and been structured, notably by the creation of a research unit of the French National Centre for Scientific Research – the CNRS – in 1960 and the *Laboratoire de Probabilités* in 1969. The latter is probably one of the most important laboratories in the world in terms of the concentration of probabilists: there are currently nearly 80 permanent researchers and research lecturers in the rue du Chevaleret.

The other pole around which a large part of the research into stochastic processes developed was located in Strasbourg, around Paul-André Meyer, research director at the CNRS. The activity was very intense and continues to this day. It was notably expressed through publication of the journal "*Séminaire de probabilités*".[2] This journal allowed the researchers to publish new results quickly. Meyer controlled the content and quality of the journal, revising and rewriting the articles he found interesting enough to publish. This publishing policy had a very strong international impact and played a large role in establishing the reputation of the French school, despite the fact that the articles were for a long time written in French. The seminar was launched in 1966 and continues to this day, but over the last few years English has been the dominant language. Incidentally, the journal has recently been digitized in a most remarkable manner. Now, if one looks for articles from the seminar using a search engine, they are often among the most cited on the list. Consequently, these publications have had an even stronger impact. This digitalization is quite rare in the field of scientific publishing. In the current French context of "research promotion" by the *Agence d'Évaluation de la Recherche et de l'Enseignement Supérieur* (the French National Higher Education and Research Evaluation Agency), this would no longer be possible, because the *Séminaire*'s articles are counted only as proceedings, even though most of them contain original results.

In 1970, the French school was the pioneer in the domain of stochastic processes, alongside the North-American school, which had been particularly active in the 1950s, and whose Berkeley symposia (1950, 1955, 1960), which brought together the best in the field, periodically took stock of the latest theoretical advances. The Russian and Japanese schools were also very active, but exchanges with mathematicians from the Soviet Union were very difficult for a long time, and only started during the 1960s with Japan. Many scientific articles were published in local journals, in their language of origin (and probability in Japanese is not easy!). One had to wait at least two years after publication for their translation.

I prefer to speak of "probability theorist", rather than mathematician. In 1968–70, the "pure" mathematics community did not consider probability as mathematics, and the male students at the *École Normale Supérieure* were not allowed to study probability (a restriction that did not apply to the female students!). I was a research assistant at Orsay, and when I defended my doctoral thesis in 1971, the jury said to me: "Ah, but isn't it counting?" At the end of the 1970s, probability was still being condemned as a sub-discipline by mathematicians such as Jean-Alexandre Dieudonné. The 1980s saw an explosion of probability applications to Analysis, notably via Malliavin calculus. Probability then became solidly established in mathematics. In 2006, Wendelin Werner won the first Fields Medal to be awarded to a probabilist in the history of the discipline. Probability was thereafter consecrated as a branch of both pure and applied mathematics, although in France, the applied side has remained poorly developed.

What a probabilist is not, at least in the French school

By definition, probabilists do not bother with applications or data. They "do" theory, pure and unadulterated. They are as contemptuous of statisticians as pure mathematicians are of them. This is surprising, because probability, above all continuous-time probability, has always been driven by real-world phenomena, as we shall see. In our community, the phenomena worthy of consideration have traditionally been drawn from physics. Today, biology has in turn acquired the status of noble domain, but not the social sciences, in the broadest sense of the term.

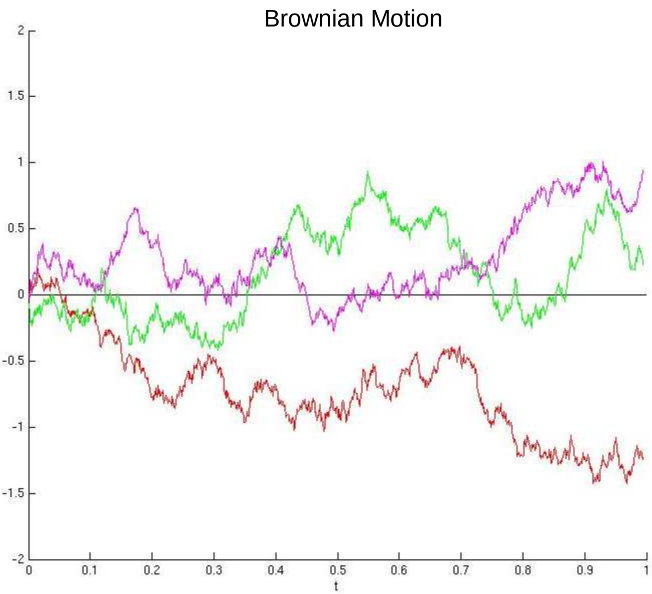
Probabilists are not interested in the quantitative properties of models, which in any case they do not test with the data. They are more interested in the qualitative properties, which are often deduced from complex asymptotic results. In this sense, they are standard mathematical analysts. Probabilists do not investigate applied questions, but theoretical ones. During the period 1970–80, the theoretical questions were of such a magnitude that they could keep quite a few people busy. This continues, but now over an incredibly wide range of topics. I moved from the *Laboratoire de Probabilités* to the *École Polytechnique* about 10 years ago, and I am sure that already about two-thirds of the current research themes at the laboratory today would baffle me. The tremendous growth of probability has been perceptible in many fields of application, both in domains close to mathematics, such as computing and image processing, and in close companions such as physics. More recently, it has burst into the field of biology, leading to a completely new approach to the study of life.[3] The Cournot Centre's seminar, *The Probabilism Sessions*, clearly testifies to this situation and revives Augustin Cournot's questions within a multidisciplinary context.

How should I sum up my experience of theoretical research into stochastic processes during the 1970s and 1980s? A stochastic process, especially for us, is a process indexed by what is assumed to be real time, which is qualified as continuous, as opposed to discrete time (finite or countable). One typical example is Brownian motion, but many other processes have been studied. The first description of Brownian motion was made by the Scottish botanist Robert Brown at the end of the 1820s.[4] Something I found most spectacular, particularly in relation to my future activity, was the mathematics doctoral thesis by Louis Bachelier, entitled *Theory of Speculation* [5] and defended at the Sorbonne in 1900 before a jury chaired by Henri Poincaré. Contrary to what has sometimes been said, Bachelier did not introduce Brownian motion to model prices; he did not speak of historical data. His purpose was to define an abstract axiomatic system of contract prices that could guarantee future operations, like the purchase of a share. He showed that it is necessary to consider that share prices behave like Brownian motion in order for transactions to take place. This was very similar to the current approach, because he only tested his model *a posteriori* on contract prices.

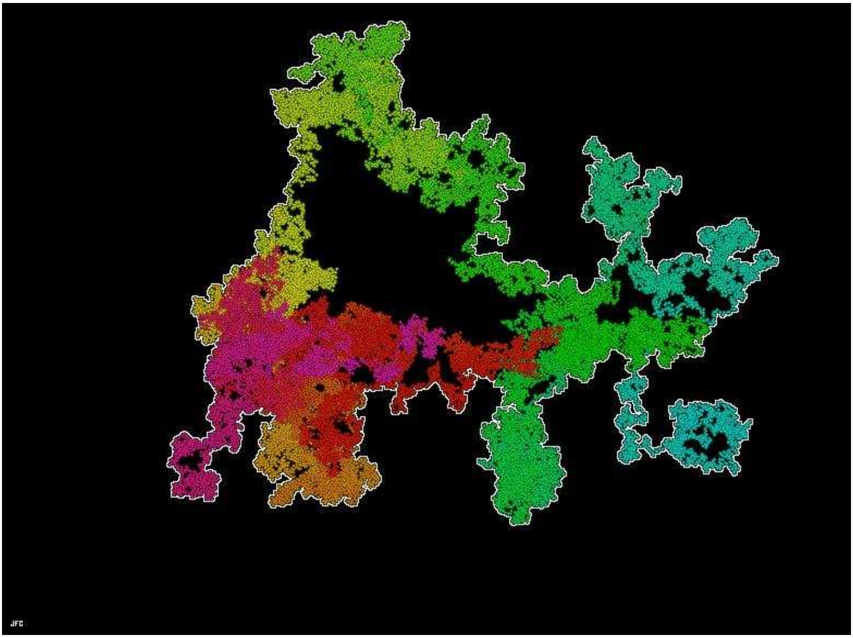
Very much in keeping with these (experimental) observations, Albert Einstein's[6] description in 1905 of the movement of particles scattering under the impact of atoms was no longer a solution of the type "the particle will be at such-and-such a location at time t", but only "the probability that the particle will be located in a little ball around this position at time t has such-and-such a value". To this very real physical phenomenon, he gave a probabilistic solution. Norbert Wiener, in 1923, then introduced Brownian motion as a form of noise in signal transmission.[7] With the exception of Bachelier, the introduction of stochastic processes derived essentially from the physics of the pre-First-World-War period. The modelling of these processes had numerous beginnings. The decisive turning points were achieved by Andrei Kolmogorov in 1933, with the introduction of what is called the probability space,[8] and then by Paul Lévy.[9] Paul Lévy, a particularly active probabilist, had extraordinary intuitions about the behaviour of random phenomena over time. The French school of stochastic processes was built in the lineage of his work. During the 1960s and 1970s, it was often said that many probabilists spent their time finding rigorous proofs for results that had already been expounded by Lévy. It is true that the idea of "proof" was not the same in the 1930s; it became more formalized after the Second World War.

Brownian motion revisited

In the diagram below, I have plotted trajectories of what is called "Brownian motion". What is a "trajectory"? It is the evolution of a phenomenon over time, of which we observe the movement. Here, the three trajectories represent the possible evolutions of a Brownian motion, meaning that the characteristics of the object we simulate are exactly the same in each case. This shows that the intuition of the "individual" behaviour of a trajectory is non-trivial.



Brownian motion is a completely erratic movement, which changes fast. Slope changes are very substantial and above all very frequent. Then, there is a link between movements over time and movements in space: that is one of the characteristics of this phenomenon. This gives us intuition about the phenomena that we are trying to model. Jean-François Colonna, from whom I borrowed the following image, is a specialist in digital modelling at the *Centre de Mathématiques Appliquées* at the *École Polytechnique*. He has greatly studied fractal representation, of which this is a particular illustration. It is the graph of a Brownian motion in two dimensions, when it scatters across the plane: the different shades represent the evolutions over time of some of these phenomena.



The autonomy of continuous-time processes

The seminar on which this text is based was focused on the gradual autonomization of the probability theory of continuous-time processes. To make them autonomous, it was necessary first of all to find a conceptual framework within which to study these objects. The solution was curves (functions), and no longer points, variables or vectors. The object was thus considered as a curve. A curve is much more complex than a sequence, for reasons that I must emphasize a little. So the first step was to acquire the concepts and tools that make it possible to think about these objects.

Kolmogorov was the first to introduce an acceptable formalism to tackle these questions. This formalism is a cause of suffering for students learning probability for the first time. They wonder why this famous probability space is introduced – Ω for the sample space; F for the set of events and P for the probability measure – since for most of these students the set F is not going to change. They thus logically deduce that the exercise is fairly pointless. At the Institut Henri Poincaré, I found the proceedings of the probability seminar from just after the war, as well as those for 1950 and 1953 (it started during the 1930s). I found amusing the way these accounts always started with: "Mr. So-and-so justified his introduction of the space $(\Omega, F, P)\ldots$", whereas this idea is now one of the basic elements in the teaching of probability. This was an extremely important step in modelling.

The stakes of formalism

The concept of information structure, present when phenomena unfold over time, was gradually introduced. Phenomena started being viewed as a curve gradually growing over time, meaning that there is useable information (possibility of learning) about the evolution of the phenomenon over time. Consequently, if we know what has occurred up until time t, we can better predict what will happen afterwards.

When the phenomenon studied is "Markovian"[10] – meaning that the knowledge of a value at a given moment is sufficient to sum up, from a future standpoint, all the past information – this contribution is important, but not decisive, because intuition is often sufficient for reaching conclusions (but not for proving). In the 1960s, it became apparent that there was a need to depart from this context, because many random functions, such as martingales, do not satisfy this property. New tools were developed, as well as a better formalism. It was, however, more and more abstract and difficult to handle and teach.

One of our strategic tools, which became, in a way, the instrument of emancipation from other mathematical theories, was the concept of "martingale". With this concept, the probabilist Joseph Doob helped to seal the demise of Cournot's principle,[11] as Glenn Shafer explains so well in his chapter of *Augustin Cournot: Modelling Economics*.[12] It was no longer necessary to repeat a random experiment many times and then use frequencies to estimate probabilities, as well as fluctuations around the numbers found. The same kind of results could be obtained with random processes, of which the only required properties are the following: given the information structure up to time t, the best estimation of what will happen at time $t+h$ (best in a sense not defined here, meaning more or less the average) is the value observed at t. That is the definition of what is called a "martingale". It refers to games of chance, but mathematically these are processes that can be found in a large number of situations in various fields.

A number of elements reappear, in line with the universal results of classical probability, centred on two main theorems that every probabilist uses with their own applications in mind. These theorems are also meaningful when the motivation is data processing, and the discussion of Cournot is again particularly interesting and relevant.

The first of these theorems is the law of large numbers: if we repeat a random experiment a sufficiently large number of times and average the results obtained, we get an idea of the uncertainty, of the quantity we are studying and its mean value. Fluctuations around the mean are governed by the universal principle – Gauss's law and some of its extensions. These are universal laws, and their existence means that even if we do not know the law of the phenomenon, we can still say a number of things about its behaviour thanks to these asymptotic results. In the case of martingales, Doob proved that limits still exist, and that when the limit is a constant, one always obtains results of Gaussian fluctuations, making it possible to control the speed of convergence.

Other universal results exist on which reflection has advanced considerably over the years; they involve extreme behaviour, also known as rare events, meaning events that have a low probability of occurring. These questions bring Cournot's principle back to the centre of the debate, since this principle proposes that rare events should be considered as not happening in practice. Doob proved that in martingale theory, one always has control over rare events, linked to large deviations, insofar as one can control the probability that the absolute maximum exceeds a given threshold ε by a quantity proportional to the variance of the terminal value of the martingale controlled by $1/\varepsilon$. In financial markets, these rare events have been closely studied since the work of Benoît Mandelbrot in the 1960s and many others subsequently. The financial crisis has revived interest in these questions.

Brownian motion appears in a whole host of very different situations: biology, physics, financial markets, signal processing, and so on. A Brownian motion is something that reproduces itself over time with a certain stationarity: that is the essential hypothesis. It has some independence structure, and it is therefore often a representation of the noise that can appear over time. As this movement is the sum of these increments, we can apply the law of large numbers and theorems of fluctuation to it. Unsurprisingly, Gaussian distributions appear once again. This characterizes Brownian motion. That is not completely accurate, because stationary independent increments can be something other than Brownian motion. There is a sort of control of the speed of convergence that says whether we are tending more towards a continuous or a jump phenomenon.

According to the property of stationarity, this noise is centred and its variance is proportional to time. Because of its Gaussian nature, we can therefore know the probability that the Brownian motion will be in state y at time t if it started at zero, thanks to the Gaussian density of variance t, the graph of which is the famous bell curve. This Gaussian density has the remarkable property of satisfying a second-order partial derivative equation (which we can verify by hand), known as the heat equation. The presence of terms involving second derivatives (of the second order) is not surprising, because in these highly fluctuating phenomena, even over very small intervals of length *dt*, the second-order moment is in *dt*. By contrast, in usual differential phenomena, it is the first-order moment that has this property.

In the 1970s and 1980s, continuous-time phenomena, including Brownian ones, had already been studied for a long time. They were first modelled by Wiener for telecommunications, then by Kolmogorov in the 1930s. Lévy described a huge number of things about the processes named after him, analogous to Brownian motion but with "jumps". Very subtle and quite complicated things had therefore been studied. And then, from the 1940s, a whole activity became established, notably with the Russian and Japanese schools.

Up until the 1960s, great use was made of the links between Markov processes and the theory of semi-groups to study these phenomena. The developments of these theories are old, and reference to better-constituted mathematical theories made it possible to advance further in the understanding of these phenomena, particularly in order to obtain asymptotic results. As the theory referred to was determinist, the quantities studied were of the type mean value of certain variables depending on the process studied.

From the 1950s, based on the theory of martingales, the whole probabilist reflection on processes sought to promote what we call trajectories and their study. What was the interest of this approach? What sort of operations can one carry out on trajectories? Many trajectories are intuitive, others less so: stopping or restarting trajectories at a well-chosen time, rejoining trajectories, shifting them in time.… Greater freedom was acquired, which was also infinitely more fertile. The aim was therefore to leave the deterministic world that allowed analysis and calculation – potential theory, semi-groups, and so on – in order to focus on trajectories. There was nothing trivial about this, because it is much more complicated to consider the randomness of a curve than to consider a discrete phenomenon that repeats itself over time.

Stochastic differential calculus

Integral and differential (deterministic) calculus has indelibly marked the modelling of deterministic phenomena. It was therefore only natural that these ideas should be introduced into the random world, even if at first glance the trajectories do not appear to be regular enough: they change slope all the time and are therefore fundamentally not differentiable. And yet this is what started to happen very quickly in the 1940s, around Itô Kiyoshi[13] on the one hand and Doob on the other. We saw the emergence of the concept of (stochastic) integration in relation to Brownian motion and more generally in relation to martingales. In the 1950s, what was known as "stochastic differential calculus" led to the development of an exact differential calculus, known as Itô's formula. Itô's formula explains how to analyse a function of Brownian motion, of the martingale type, solely in terms of stochastic and classical integrals. This opened the way to an infinite number of possibilities. Curiously, it constitutes the foundation of all the financial strategies applied to derivatives that emerged from the 1970s on. I should also mention the very special role that Ito played in stochastic processes theory. His mathematical contributions were fundamental, and his social role no less significant: the mathematical school he established in Kyoto had a tremendous influence.

At the heart of the debate over Cournot's principle lies the question of the relative importance attached to events of small probability *compared to* those of zero probability. In the additive representation of probability – when one adds together the probabilities of a finite number of separate events and passes to the limit (this is the countable view) – there is always a phenomenon of stability in the background concerning events of zero probability. The union of a countable number of events of zero probability does itself have zero probability. All the great theorems that we have seen appear only exist because of the accepted hypothesis that some phenomena occur with an almost zero probability: it is here that the concepts of "convergence", "almost-sure", and so on, are defined.

When we move from the discrete to the continuous, this no longer works at all. We need only look as far as the interval (0,1) to understand why: evaluated with the Lebesgue measure, which associates its length to an interval, each point has a measure of zero, but when you bring them all together, that makes something with a mass of 1. There is obviously a serious technical problem here. One of the major contributions of the Strasbourg school was to explain how to solve these difficulties by using the deep mathematics of non-linear probability, called "capacities in potential theory". This issue was of crucial importance. It was central to the theory that was developed during the period 1965–75, notably around Meyer and Claude Dellacherie in Strasbourg.[14] Of course, the way had been paved for this development by Doob and numerous other researchers throughout the world. This theory is known as the general theory of processes: we know how to treat negligible sets, how to advance with the analysis of the regularity of trajectories, and we now have the tools to solve the many new theoretical problems that arise.

Hence, we can now deal with phenomena that are more and more complicated and abstract. The general theory of processes has reached such a level of abstraction and complexity that it is almost impossible to teach at the introductory Ph.D. level. The community of young probabilists is gradually losing touch with the foundations of this theory. It is amusing to see that the problems encountered in financial mathematics raise similar questions to those that motivated the development of the general theory of processes. It is being rediscovered, in a way, 30 years later. The knowledge acquired by science is never established once and for all.

Simulation

Beyond these theoretical aspects, I must mention something that has been fundamental to both theorists and practitioners of probability: Monte Carlo simulations. They have been reinvigorated through their enormous potential for applications, owing to the power of modern-day computing. What does "simulation" mean? Above, there is an illustration of a Brownian trajectory. In 1970, I had never seen such a trajectory. In the best of cases, I could play "heads or tails" and analyse the results, but it was not very dense. Now, the trajectories illustrated above are simulated, meaning that one has drawn random numbers and then played a very compact, very dense "heads or tails" game, giving the result shown above.

This digital simulation has completely changed our view of a number of probabilistic phenomena in terms of both theory and application: being able to simulate the highly complex phenomena studied does truly change the landscape. The theorist's next questions concern the value and the qualities of simulations. Much of classical probability theory is re-applied to these simulated objects to try to test their quality, to measure errors and speed of convergence.

Currently, this type of practice is widely used in the financial industry, for example. Much of risk calculus is almost exclusively performed now using Monte Carlo methods, corresponding to problems of large dimensions for solving partial differential equations. More surprisingly, these tools can even be used to tackle optimization problems. To calculate the mean value of a quantity associated with a stochastic process, the order of magnitude of the simulations needed to obtain a small confidence interval – that is, for the results to be credible – varies between a hundred thousand and a million. Twenty years ago, it was impossible to perform a million simulations, at least on a personal computer. Now it is possible to do all this and much more besides, especially since these simulation methods (drawing at random with a great deal of iteration), can easily be parallelized. Powerful computations can then be done in parallel.

It is possible not only to compute, but also to simulate trajectories, as I have done, and to see, for example, a Brownian trajectory that decreases for a long time, when one would expect to see it rise. Thus, counter-intuitive phenomena become observable in simulations. This probabilistic digital experimentation makes it possible to take a different look at phenomena, affecting how theorists reason. But do not worry. In the *Laboratoire des Probabilités*, which is the heartland of pure theoretical probability, this is only penetrating very slowly.

To what extent I have become an "applied" probabilist

The sphere of application in derivatives

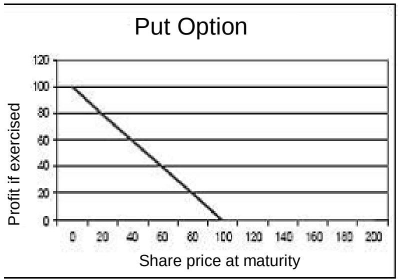
The subject of this text is not in itself the application of the theory of stochastic processes to financial risks. I would simply like to explain why the theorist that I am became interested in this specific application associated with the financial markets. It is an experience that I find quite innovative, concerning a particular, limited sector: the financial risk market and what are called derivatives.

I am thus talking about a very specific kind of risk. It involves internal risk linked to the functioning of the market and not to that of the behaviour of the operators, whether they are securitizing, fabricating, circulating or selling their products. The questions I have tried to answer involve, for example, price formation and the principles of hedging. Financial mathematicians, as such, do not exist: rather there are mathematicians who have found applications in finance.

What are "derivatives"? What are they derived from? Some are traded in stock markets, exchange rate markets, interest rate markets, and so on, where the prices are easy to read. These prices can present pronounced trends and enormous fluctuations. The aim is not to examine the microeconomic reasons underlying the formation of these prices. To enable economic agents to operate in such a fluctuating world, one proposes financial products that give some stability to the operations that these agents will be led to perform in the future, for either operational or managerial reasons. These tools are either *futures contracts*, which are "promise-of-sale" type contracts, or guaranteed maximum (or minimum) price contracts. These are called derivatives, the latter type being "optional contracts", or options.

These contracts have existed for a long time, but in the United States, in the 1970s, organized markets were created to promote these products by making the transactions more reliable and by circulating information about contract prices. This brought about high liquidity and the posting of prices. Bank margins were reduced and large losses were reported. In this competitive context, business disappears if risk is not managed efficiently. Business prospered, however, and activity in this type of product has reached extraordinary levels, even if the effective trading is 10 to 50 times lower in nominal rates. The price of these products depends on the style of the underlying on which they are written. There are, nonetheless, common characteristics in the management of these contracts, stemming from the principles that I shall now describe.

I will approach this subject from the point of view of the bank, as the seller of optional contracts. The bank assumes specific risks of the contract in lieu of its customers, and so it must therefore manage those risks. In the case illustrated here, the contract is a put option: in three months, or six months, you can sell some shares, or dollars, at a price guaranteed in the contract. At present, this may be particularly worthwhile, because the market is forecasting a fall in the dollar and there is a guaranteed minimum price for the sale of your product. The graph corresponds to the analysis made by Bachelier: Bachelier's motive was to study the behaviour of these prices in the future. The question was first one of price, and second one of risk analysis. Bachelier did not approach this second aspect. And yet, in the organized markets, the price is fixed by supply and demand, and the management of risk is the fundamental issue.



New problems, new concepts

In 1973, the markets started to get organized and contract prices became public. In France, this happened in 1987 (these contracts had previously been settled by mutual agreement). The high liquidity meant that these markets were no longer profitable enough, according to the banks, which were perceived as the supermarkets of finance, providing easily accessible, simple and standardized products.

The important thing for the seller is to reduce risk; the seller is in exactly the same position as an insurer covering a risk in the future. But in the case of insurance, things happen completely differently. Insurers try to acquire a large number of customers with the same type of risk, which is known as diversification by numbers. In finance, agents have no control over the evolution of prices. Their trajectories are very similar to those of Brownian motion, with trends that are sometimes very pronounced, particularly downwards. In reality, we see that this is not the case. Rather than estimating the potential losses deduced from a statistical analysis of prices and behaviours, a completely different strategy is applied, known as the strategy of hedging portfolios. Thus, the risk one insures against is held to be of a very particular type, associated with an underlying that is traded on the market. Rather than estimating potential losses, one seeks to reduce them by means of a dynamic strategy over time.

In the United States, there was considerable awareness of portfolios in the 1970s, as the great theories on portfolio management and efficient frontier had been introduced in the 1950s by Harry Markowitz. *A priori*, the idea of portfolios had no particular reason to appear in the world of derivatives. I believe that this represented a very important break in the way that problems of risk were approached, introducing the *intertemporal* dimension by means of portfolios. One cannot diversify by numbers, as there is only one customer.

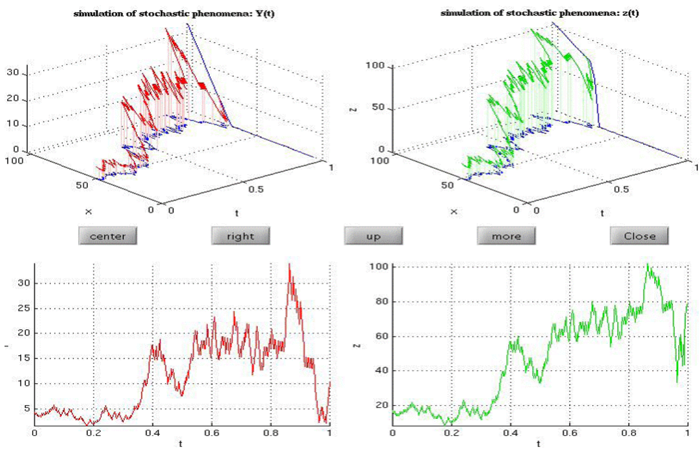
Time is used as an element of hedging. A dynamic management is thus established, which adapts to the day-to-day evolution of the market over the duration of the contract. Thus, a kind of average view of potential losses over the management horizon has therefore been completely abandoned. One thinks over short time periods, adapting one's strategies according to past behaviour: this is a differential spirit, analogous to that which has been developed in stochastic calculus. One has a trajectory, which one does not know; it reveals itself over the course of time, and one can buy or sell over the course of time of the underlying. The value of the portfolio corresponds to what is called, in the domain of stochastic processes, a stochastic integral. The idea is to use the information that one has gathered over time in order to adapt as best as one can to the market for whatever one has insured, so as to reduce the overall risks of one's position.

The problem is that of a random target: one has an objective, for example to pay something at the end; one can trade a certain number of securities according to the evolution of the market, and one uses this strategy to get as close as possible to the target. In the logic, and even in the mathematics behind this, it is not radically different from the problems, for example, that NASA faced in the 1960s when trying to send a spacecraft to the moon: it had to adjust the spacecraft's course as and when it gathered new information. The evolution over time matters. Information becomes an extremely important parameter, and this dynamic portfolio becomes the key to risk coverage in the world of options. One must therefore adapt from day to day, obtain the maximum amount of information, and adapt one's strategy to get as close as possible to the target and reduce the final risk.

One will charge a price for one's contract that one will invest in this strategy. In many cases, as I have mentioned, the price is not the main issue, except in the case of very strange products: most of the time one can find the price in the market based on supply and demand. The contract price is also posted every day in the market, and there is additional daily information that one will be able to use. So in fact, one adds the daily market information to what is already known, and one tries to use that to conclude.

Admittedly, the idea is interesting, but then what? The theory appears sound, but the practical question remains the volume of dollars that one should buy from day to day. The model appears at the end as a means to quantifying. It is no longer an object in itself, as it is in statistical forecasting, portfolio management, profit forecasting, and so on. Here, it is the model that quantifies the strategy between two calibration dates. For this to work, it is clear that one must adopt a differential point of view: the dates must be close together. So in the derivatives market, a trader is expected to re-examine his or her risk exposure every evening. And that is the maximum time scale: it may be several times a day.

Things appear as in the following diagram. The line on the x-t plane in the top right chart represents the simulation of a price. Obviously, one can simulate right to the end, so one has produced information, the accuracy of which remains to be seen. There is a formula, called the Black–Scholes formula, which tells one how much stock one should buy to hedge oneself. The line in the top left chart (also represented two-dimensionally in the bottom left chart) shows the evolution of the product over time. The target is the value of the put option



mentioned above, that is, the price at which one has guaranteed that someone will be able to sell the stock at a decent price. If the trajectory arrives above the guaranteed price, one will lose money,because one will be selling at a loss. If the trajectory arrives below the guaranteed price, the customer buys or sells his or her put option at the market price and one loses nothing.

One cannot afford to base one's business on the reasoning that "well, I have a one-in-two chance that the guaranteed price will not be reached, so everything should be fine for me". That is not the issue. On the right are shown the amounts that should be invested in the risky asset to reduce the final risk. On the left is the value of the hedging portfolio, which is also the price at intermediate dates of the product one has guaranteed. As we can see in the simulations, one arrives exactly at the point of impact, the target price. This is the ideal world: one has succeeded in hedging just about anything.

The simulations were performed using a given model, and the hedging is deduced from the model. In the market, of course, one does not know the true model. The issue is therefore how to identify the underlying model and bring it to the surface. Recall what we have said so far: we have information about prices and their evolution, but the aim is not to try to predict how they will fluctuate in the future. The aim is rather to determine how to hedge oneself between today and tomorrow, because one will learn the new prices tomorrow. This is what agents in the market do. On the basis of market prices, they identify parameters, manage with those parameters, which are implicit parameters, and then implement a hedging strategy until tomorrow, when they start all over again.

So the message of Fischer Black and Myron Scholes is not how to determine prices, but how to cover the risks generated by a financial product, which is radically different from the messages of portfolio management and forecasting. One of the characteristics of these products hedged by the dynamic strategy is that they are very insensitive to the real market trend, as Bachelier had shown in 1900. For example, one could find put options in the early autumn of 2009, despite the fact that the general trend of the market had taken a strong downturn. The person selling, by adapting to the market, could considerably reduce the effect of the trend. This seller is, on the other hand, exposed to the magnitude of the fluctuations. Ultimately, the derivatives market, excluding pure speculation, involves managing the risk of fluctuations, and therefore of what we might call second-order risk.

What really interested me was this conceptual break in the view of risk and the estimation of future risks introduced by Black, Scholes and Merton in this context. The ideas were new to the markets at that time. Agents in the market would not have been able to do this back then, not because of a lack of technical skill, but because they were too involved in the market. This discontinuity appears each time that there is a technical breakthrough or a new concept introduced, which always comes from the outside, and most often from scientists.

Conclusion

The implementation of these new ideas had a considerable impact on the world of derivatives, effectively reducing the specific risk of each contract. The financial crisis casts doubt on this observation, even if the most problematical products, such as credit derivatives, are those for which the risk analysis was the most superficial. With the new ideas came new risks, notably the model risk that is structurally related to this type of activity: *a priori* several models exist that all satisfy the constraints of calibration. In mathematics, we would say that we have an ill-posed inverse problem. As these models generate different hedging strategies and different prices, in practice one chooses the simplest model and makes provision for the maximum possible shortfall.

So the problem of risk-taking arises very quickly, as it is whenever one succeeds in offsetting risks. It is tempting to take more risks, but they then become harder to manage, especially since we are dealing with second-order risk. In the markets, one might say, for example, once one has hedged the product: "OK, our residual risk is the volatility… so how about making products on volatility?" This has led to a mushrooming of products, which are often very complex and of less financial interest. Another problem is induced by the underlying day-to-day management of these markets, which makes it possible to sell long-maturity products. The traders' horizon is still the working day, and indeed, on the floor, people are insensitive to what may happen during the night-time. This day-to-day concern can eclipse the future risks of the product, because the traders rarely look six months, a year, five or 10 years ahead, all the more so since their bonuses also bring the horizons down to a maximum of one year. In this world, very short-term risks are not all that poorly managed, and traders' behaviour is not all that unreasonable. In the trading room, however, little account is taken of the total aggregation of risks and the medium- to long-term consequences. Traders have an objective to reach every day, and they do. If, however, by carefully watching their step, they manage not to slip and fall flat on their faces, they are able to remain oblivious to the fact that the slope they are navigating is slippery and above all very steep.

As this works on the whole, the temptation is to transfer a lot more risks to the market: securitization and subprimes are examples, but they elude the analysis presented above, because their principle of risk management is diversification by numbers, rather than intertemporal. The risk is divided up between a large number of investors who only bear a small fraction each unless the defaults suddenly concentrate.

These products become instruments of pure speculation, and even if they are hedged from day to day, they are not sheltered from large movements. This was the case in particular, from 2003 on, for the explosion in credit derivatives with which the activity started. New activities usually generate large margins, because the profitable information is not widely available, and in that case, as we have seen, the margins exceeded anything we could have imagined.

The media hype surrounding the incredible performances of the financial system in the years 2005, 2006 and 2007 never evoked the fact that it should anticipate the cost of a crisis that was bound to happen. During speculative bubbles, those who are winning think they will always continue to win, at every level. Probabilities show that, on the contrary, extra vigilance is called for.

Comments by Michel Armatte

My career as a researcher has taken place in another branch of the science of chance, although I began my Ph.D. in mathematical statistics in the same year as Nicole El Karoui (1968). As you said, a statistician is not a probabilist, and a probabilist is a very particular type of mathematician. Consequently, my comments may come from a different perspective. As a statistician, the profession I have followed can be summed up as the treatment of large data bases, either in a descriptive and multivariate manner, using the tools of data analysis, or with the help of probabilistic schemes, which are processes uniquely concerned with temporal data. The research into the sciences that I conduct at the Koyré Centre provides a second point of view, in particular the study of models and their transformation during the post-Second-World-War period, in domains as varied as macroeconomics, finance, transport and climate change studies. Finally, it is from a third point of view, that of a historian of statistics and probability, that my comments will be made.

First, let us return to the history of probability. In Paris, the seminar on the history of statistics and probability has played host to a large number of researchers who have made varied studies of the earliest periods. They have studied, for example, the very beginnings of probability calculus, sometimes even before the famous correspondence between Blaise Pascal and Pierre de Fermat, which constitutes the mythological origin of a "geometry of chance"

(Pascal's expression), and which explores all the origins of probability. As you did not go so far back in time, I will just mention that one can first associate probability with the question of the "probable opinion" that was the subject of numerous theological debates, such as the controversy between Jesuits and Jansenists that lies at the heart of Pascal's *Provincial Letters*. By applying this view to our present-day financial economy, I believe that we could recover the idea of expertise, of the authoritative opinion, leading to subjective evaluations and probabilities associated with the tradition of "the probability of testimony and judgement". Are there not still decisions being made on the trading floor without calculation, but rather based on the strength of authorized appraisals by people with intimate knowledge of the markets?

A second path that could be explored consists in following the emergence of expected value in the idea of a fair price, or more generally in random contracts, that spread in the seventeenth century in the fields of trade, law and insurance. Our seminar at the EHESS was founded by a mathematician, Georges-Théodule Guilbaud, and a philosopher, Ernest Coumet, author of a fine article entitled, "*La théorie du hasard est-elle née par hasard?* " ("Was the theory of chance born by chance?"). Both of them did a lot of work on random contracts. Is the price of a financial asset a form of random contract in the same way as the simple insurance contract to which you referred, where the premium represents the equivalent of a lottery for which one seeks to be deterministic as well as to reduce the risks through mutualization? You have shown that this is a valid interpretation for some financial products, but is no longer true in the case of options. Here, the underlying principle is no longer the insurance-style approach of risk reduction through mutualization, but that of non-arbitrage by a hedging portfolio. The latter is "equivalent" at every moment to the intertemporal lottery of the option, which follows the moving target of its fundamental value, allowing the bank to organize its risk management option by option rather than globally by compensation. It is interesting that you are able to confirm this essential difference.

A third meaning of probability derives from the sciences of observation and the theory of errors. This is a vast source of theorization, of construction of probability: one perceives that error can be modelled by the concept of random variable – quite slowly, however, because it is not easy to identify error with chance. One must differentiate between systematic error and accidental error – that which remains when everything else has been explained. And here, the multiple causes that can come into play create the marvellous "law of chance", the law of Moivre, Laplace and Gauss, which provides the easiest justification for using means and least squares to extract the "true" value from observations. The notion that this "normal" law also governs stock market prices is the subject of controversy today, reviving Mandelbrot's criticism from the 1960s, which disputed the idea that asset prices follow normal laws and Brownian processes.

This whole historical background is of little interest to probabilists and financiers. Their only desire is to ignore it. For them, this is the prehistory of probability, in which the latter is not properly defined. The foundations of probability laid down by Christiaan Huygens and Pierre-Simon Laplace, for example, were based on the ratio of the number of favourable cases to all possible cases. It is totally circular, because it depends on the assumption that these cases are equiprobable. The same circularity can be found in Jacob Bernoulli's law of large numbers, where the word "probability" appears twice, and it is therefore difficult to see how it can serve to define a probability. The break from these unstable foundations did indeed come with Kolmogorov, the measure theory and the concept of Borel set, and above all an axiomatic definition that does away with the questions of meaning and usage.

Consequently, my question is not how to establish the correct axiomatic foundation for probability measures, but what do we get rid of when we do it? What remains of interest from all the debates of the past about the interpretations of probability? As a probabilist who "does not bother with applications", or with interpretations, probably, you propose little in the way of an answer in the above text, because as a theorist, you are only concerned with the formal properties of your objects – stochastic processes defined in one way or another – and not with their semantics or pragmatics. Nevertheless, I am sure that you have an idea about these things, if only because you have taught them, or subsequently applied them to finance, which has constituted the second part of your career. In your current domain, what does it mean to "interpret a probability"? What is the nature of the chance that lies behind a probability? Is it epistemic, that is, linked to our ignorance of the causes as Laplace thought? Is it ontic, in other words, intrinsic to physical, biological or economic phenomena as James Clark Maxwell and Charles Darwin thought in 1859? Is it neither one nor the other? Is it a logical property as John Meynard Keynes believed? Is the probability itself, as the measure of risk, objective as Augustin Cournot held it to be? Is it subjective? Or is this debate about the type of chance involved of no interest to us, because what counts is to be able to calculate with operational, operative or even performative models, as some say. Finally, whatever the nature of the chance, the important thing is to master it.

2. My second observation is about the "autonomization of probabilities". When something gains autonomy, it is liberated from some kind of domination. From exactly what

have probabilities been liberated? Mathematics? As you explained very well, it is true that probability calculus was for a long time looked down upon by the mathematicians of the nineteenth and even the first half of the twentieth centuries. That calculus did not have very firm foundations, and above all, it was the subject of inappropriate applications: the confidence that one could attach to testimony and judgement, for example. During the 1950s and 1960s, the period of our youth, this rejection was heightened, and you are right that the Bourbakist environment played a large part in this, as is now attested by the work of historians. We can find traces of how the Bourbakists hindered the development of probability in the *Journal Electronique d'Histoire des Probabilités et de la Statistique*.[15] So one can speak of the autonomization of probability calculus with regard to mathematical analysis, which had previously contained it, for example, in most bibliographical classifications.

This autonomization of probability calculus could also be, in your expression, autonomization with regard to the fields of application that had long been confused with this speciality. You tend to define this domain as one of pure science with its own logic of development, but that can also be applied to some other domains, as you have done with finance during the second half of your career. Nevertheless, this domain of stochastic processes has its own autonomy, and in particular its applications have little return effect on this field of pure mathematics. According to you, it would be impossible for someone on the trading floor to invent what mathematicians have invented. I would qualify this somewhat, because in my view, the separation between the concepts of pure science and application seem incorrect for those observing what happens in financial markets. Mathematicians affirm that much of mathematics is driven by real problems, taken from outside their domain. The science studies that we practice at my research centre show that the academic science of the twentieth century cannot be dissociated from the industrial or managerial operations that it favoured. Today, there are only the technosciences, which are forms of engineering. Finance is typically a branch of economic activity that has become an industry that incorporates as much mathematics as the design of a bridge. Do you think that the autonomization of the mathematics of processes is still real in relation to fields of application such as finance? Is this field of continuous stochastic processes really independent from the progress of finance and the role that the latter has played in the economic regime that we have known since 1990? Could we not say, on the contrary, that this field has benefitted from financial innovation, to which it owes its problems, solutions and software tools?

Lastly, this autonomy could be professional and claimed, on the contrary, by the financiers, a small number of whom have asserted the possibility of still being able to carry out their profession without any need for mathematicians. Other, more numerous financial market specialists have called for total autonomy from the science of economics, of which they no longer wish to be a branch. According to Nicolas Bouleau, finance was initially a branch of economic theory, but the extraordinary fertility of mathematical innovations (and most especially Itô's integral) has led to the emergence of a new engineering developed by bankers and mathematicians. It was totally independent of economics until a number of awards from the Nobel Memorial Prize in Economic Sciences allowed for the reintegration of these innovations into the field of economics.[16] In universities, some finance tracks have been emancipated from both mathematics and economics, to become management or independent courses. Today, this claim for the autonomy of the financial system can be seen in the strong distinction often made between financial crisis and economic crisis. So here is my second question: what do you think of these complex relations between the mathematics of processes, economics and finance? What kind of autonomization of stochastic calculus do you have in mind?

3. My third series of comments concerns the question of the relation between the mathematics of continuous processes and the reality of today's markets. To what extent do the models really reflect the reality of these markets? Or does this reality exist independently of the models, because it is the models that largely shape market behaviour, between realism and constructivism? In more practical terms, I am referring to the Pygmalion effect and the property of self-fulfilling prophecy of most economic and financial models. Everybody believes that the true price is the one given by the model and acts in such a way that the real world conforms to the model. Transactions on options markets depend on agents' decisions, but those decisions are made with the help of the same management and decision models. Portfolio management models, and even more so options pricing models, are not simply models that give a representation of the market. They are normative and performative. They indicate what should be done on the market and construct this reality of the market. So can they still be controlled? This is a question that frightens the general public, who feel, reasonably, that we are entering a world bereft of human intervention. And this provides a culprit to blame for the blunders of finance: the models were to blame for the crises such as those of 1987 and 1998; today, the fault lies with securitization. You said something slightly worrying: what happens on a day-to-day basis can be controlled, but it is impossible to have a broader, more "macro" perspective, because the vision becomes muddled. There is therefore a sort of disquieting paradox: we build models to control chance (the *Taming of Chance*, to

borrow the title of Ian Hacking's work,[17] or even Bouleau's "abolition of chance" for the revolutionary management of options), but in doing so we cause an inflation of risks. In a way, we are creating the possibility that low-probability risks become huge when their domino effect starts working on the global system. As, in addition, we were mistaken about the "true" laws of chance, which have much fatter tails than we had imagined, it is extremely alarming to find ourselves in such a situation. Do you consider models in this way, not only as the producers of the market, but also as sources of major risk? More broadly, is there not a problem of insufficient transparency in financial practices, causing people to see the alliance of mathematicians and bankers as a fool's game? Bouleau speaks of knowledge in the public domain for mathematics, but privatization of the professional knowledge of operators and traders, given the high stakes involved.

4. My fourth point concerns models and simulation. In the simulation of complex systems, "complex" can simply mean, as it does for mathematicians, a system that is not linear. To simplify, this means a system like that of the Earth or the climate in which there are many agents – human and non-human – and mixtures of phenomena of very different scales, which have different physics and therefore different theories. The computer model is no longer the reflection of a single theory, but an object that integrates and links together all these heterogeneous bits of knowledge. In laboratories, people work on these global models that unify these items of knowledge of very diverse epistemological status, because it is impossible to do otherwise for these complex systems. In these domains, simulation models are used for evaluation and collective decision making, for example, with regard to policies for stabilizing and regulating the world economy or climate. That is not the status of the probabilistic models of finance that you presented. They are still within the paradigm of a financial model based on a precise mathematical theory of the process. Second, your models do not serve to simulate the unknown futures that will be our common environment so much as to calculate prices and risks at time *t*, therefore serving more directly to help individuals make decisions. Your models do not serve for the collective policy decision of market regulation. They guide the investor and the speculator. Whence my two questions about the history of simulation: first, does the financial system possess some kind of complexity, in the two senses of the word I have just mentioned, or is it ultimately less complex than the geophysical and human systems that we can study? The climate is an example: it involves not only the atmosphere, but also the oceans, the fauna and flora, and so on. Furthermore, you seemed to suggest that in the banks, the work never stops; simulation takes place during the

weekends and nights, and the rest of the time "live", with the listing of prices triggering purchases and sales. In the laboratories, on the other hand, researchers are more reserved and slightly afraid of embarking on this new dimension of research that simulation represents. We can see the same thing in climate research, where some researchers are reluctant to base their activity on numerical simulations. They are very attached to the fundamental physical processes. They want to study, for example, the role of clouds from a physical and chemical point of view..., and they find that simulation takes up too much of their time to the detriment of fundamental research. How do you feel about this? Does simulation continue to arouse doubt and reticence in laboratories of probability?

Reply from Nicole El Karoui

The aim of the emancipation of probability theory was not to separate from mathematics, but to allow its instrument to give the most appropriate answers by itself, without having to go through intermediaries that were always very restrictive. To a large extent, the power of the theory of martingales made this possible, and consequently enabled probabilistic reflection and modelling to be applied in a wide variety of fields. In fact, this emancipation was a singular moment, which led to other back-and-forth movements. I am not speaking of applications, because these continuous-time processes originate in Einstein's Brownian motion, that is, the general observation. Depending on the period, problems can be extremely complicated, and the conceptual tools needed to solve them lacking. Once a first step has been made, one explores new fields, and again problems arise for which the tools are inadequate. Thus, more theoretical periods alternate with more applied periods. The period in which I participated was very theoretical, and necessary. The fruits of that period were diversified, even in terms of the fields of application. The subjects of reflection in terms of application, for example in the *Laboratoire de probabilités*, have indeed diversified.

It often occurs in mathematics that there is a period where, internally, the theory sees the emergence of tools that are more efficient than those that could be imported from outside. The danger, as we have seen, is that the theory reaches a sort of maximum point of what is acceptable to the younger generation, the environment, and so on. Often, one sets aside a certain number of things and starts anew in other directions. In the case of financial markets, I find extraordinary the fact that the work of Black, Scholes and Merton and the field of application – such as the way of using time instead of diversification by numbers – would not have been possible if all the concepts and reasoning of stochastic differential calculus (which is, in a way, not at all normal from a mathematical point of view) had not existed beforehand. At some moment, one uses old concepts (well, dating from the 1940s, 1950s and 1960s, so they were not that old in 1973), because the environment allows it. This question also concerns our funding agencies, which makes one think that directions should be given when there is mathematics to be done or applied research to be funded. Nobody could have imagined that Itô's stochastic calculus would be the engine of risk management in the financial markets 30 years later. The existence of these tools has made a good number of things possible. I have a lot of belief in the back-and-forth movement, and in that, this period was a singular experience.

In probabilistic activity, derivatives are maths-dependent. It is the only activity I can think of that would not exist if it did not have this quantitative accompaniment. I say mathematical but that is a slight exaggeration, because it is structural. This is quite rare, because we are not in the domain of forecasting or management where a good tool improves the performance. Here, there is no activity if there is no hedging. Indeed, this has become statutory. Behind the debates on the definition, usage and interpretation of probabilities lie the questions concerning chance, and in particular, the meaning of « zero probability ». I have always found these questions fascinating. When we dared to name the value zero, how did we approach the subject? The moment of extraction of the reality of "zero" is one of the great breakthroughs in the domain. It was not a new discovery, but rather the first time that anyone dared to name it. By naming something, it becomes obvious in one's mind. The theory of processes is thus very closely linked to the experimental side of the physical sciences. To answer Michel's third question, I think that the same kind of formalism could make it possible to theoretically take into account all these aspects. It is the way in which we use that formalism that will reveal the point of view anticipated.

Derivatives markets are trying to do without probability. In the day-to-day management, one seeks to attach the least possible importance to the identification of the model, because this model is chosen one day and then modified the next. The vision is adaptive; it is an approach of permanent adjustment.

So the model *per se* no longer has a fundamental role. Consequently, the question of the definition of the model becomes secondary. I am being slightly provocative here, because that is not quite true. The question that is not secondary for me – here we return to a much more fundamental debate – and which I cannot answer, is the question of negligible sets, or events of very low probability. In derivatives markets (the only one that I know very well), there are scenarios, notably of stress and all sorts of other things. There is not always a very clear understanding of what is being set aside as negligible. I think that ultimately, a probabilistic model in this universe is first a set of events that one decides are impossible; this is a sort of consensus. If one writes the equations of what I presented above, the problem becomes completely algebraic. There is therefore no need for probability; it comes into play *a posteriori* for the calculus. What needs to be defined is “that which is impossible”. And in the complexity with which one is confronted, that is not trivial. Likewise, at the level of simulation methods, one has in each case a perception of what one is prepared to consider or accept as impossible. So we return to the foundation of all the questions about the use of probability, where probability serves to define negligible sets, which in turn define the probability itself. That question is not at all clear to me.

As regards simulations, I just mentioned the heartland of probability theory, the *Laboratoire de probabilités*, in which people are, nevertheless, getting interested in numerical probabilities. It really is a sector in full expansion, calling for permanent reflection to define what one means by “simulate”. This creates complex theoretical problems, and the light thrown by models or by practice gives resources for outlining certain quantities. What analysis can one have of that which one sets aside as negligible? Can one partly measure it? In any case, it is important to be vigilant, because it is easy to make big mistakes: rare phenomena can have a substantial impact.

Scientists continue to reflect intensely about the problem of large data sets, their simulation, their asymptotics and their consequences in terms of risks. For me, theory and practice are complementary in understanding the scale of that which one cannot analyse or the belief that one can have.

Notes

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