The Construction of the Canonical History of Financial economics

Franck JOVANOVIC*

ABSTRACT

This article takes place in the revision of the history of financial economics. The major argument is that the history of financial economics nowadays known was built to defend theoretical viewpoints, and therefore, to convince the scientific community to adopt these theories. More precisely, this "history" was built thanks to the creation of the first canon of texts during the 1960s; and therefore it constitutes a canonical reading of the past of financial economics –what I call here a canonical history of a discipline. This revision leads to break with this canonical reading that has been imposed and provides new results. Indeed, breaking with the canonical reading, it is possible to conciliate the history of financial economics with historical data.

To demonstrate that, this article examines in particular the link between the introduction of financial economics into the scientific field during the 1960s and the construction of a canonical history of that discipline. This article analyzes, first, the structure, the aim, and the positioning of the discipline during this decade, second, how and why this canonical history was elaborated. Finally, this article shows how the canonical history of financial economics was elaborated to support theoretical viewpoints.

INTRODUCTION

The history of financial economics has been studied for about ten years. Traditionally, this history is described as linear progress. However, Jovanovic (2002) explains that this presentation differs from the historical movement of this discipline. Moreover, Jovanovic (2003a) shows that such

* Département des Sciences économiques, Université du Québec à Montréal (UQAM), 315 St. Catherine St. East, Montreal H3C 3P8, Canada. E-mails: jovanovic.franck@uqam.ca.

a presentation is inconsistent with much historical data[1] and that these data therefore renders that history falsified. This article deals with that problem. It has two major goals.

The first goal aims to explain the origin of the deficiency within the history of financial economics that we have known. This article suggests that this inadequacy between the historical presentation and the historical data is derived mainly as it was once practiced from the existence of a canonical history of financial economics, which is not in accordance with the past of this discipline. This article highlights this point by analyzing how and why this canonical history was elaborated. A canonical history can be defined as a history created from the canon of texts[2] –*i.e.* from a selection of texts– without other historical data. In other words, such history classifies selected theoretical texts thanks to a chronology, but the links it makes are neither historical assumptions nor historical results; they are inventions on the past. This article shows that canonical history of financial economics was created during the 1960s to support theoretical viewpoints, which emerged with the introduction of the discipline into the scientific field.

The second goal is to analyze the introduction of financial economics[3] into the scientific field during the 1960s, and therefore its acceptation as a scientific discipline. This point has never been studied: the canonical history of financial economics and the traditional presentation that followed do not analyze this introduction, nor evoke it at all. Indeed, these presentations suppose that financial economics has existed since the first theoretical or empirical works in this field were published. Therefore, many questions remind unanswered with theses presentations. This article focuses on the 1960s, a crucial decade in the construction of this discipline: although works in financial economics had existence since the mid-19th century, this discipline was only brought into the scientific field during the 1960s. According to Bourdieu who introduced this concept, a scientific field is "a structured field of forces, and also a field of struggles to conserve or transform this field of forces" (2004, 33). Through their relationships, individuals create the very space (i.e. the structure of the field), which determines them, thanks to the state of the forces that are exerted on scientific production and on the scientists' practices. The scientific field is constituted by all scientific disciplines, which are sub-fields. Each scientific discipline imposes its own rules, behaviors, methods,

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[1] In accordance with the historical approach, I distinguish historical data from historical facts. Historical data are the primary supports, which come from archeology, testimonies or archives. Historians build historical facts when they elaborate the history by using historical data.
[2] A canon is a privileged set of texts whose interpretation and reinterpretation define the background of a discipline.
[3] It seems important to differentiate financial economics –identified as the discipline–, mathematical finance –identified as mathematics applied to finance– and financial econometrics –econometrics applied to financial data. The article will return to these differences.

etc. to distinguish itself from other scientific disciplines and from approaches recognized as non-scientific. Relying on Gingras, Bourdieu (2004, 50) identifies two steps within the development of a scientific field: "first, the emergence of a research practice, in other words, agents whose practice is based more on research than on teaching, and the institutionalization of research in universities through the creation of conditions conducive to the production of knowledge and the long-term reproduction of the group; and, secondly, the constitution of a group recognized as socially distinct and a social identity, either disciplinary, through the creation of scientific associations, or professional, with the creation of a corporation –the scientists provide themselves with official representatives to give them social visibility and defend their interest".

The scientific field concept helps to better understand financial economics and its creation. In order to create a new scientific sub-field, individuals who try to make recognize that new sub-field have to fight against the structure of the existing scientific field to impose their new results, concepts, hypotheses, theories, etc. In other words, when it is created, this new scientific sub-field cannot be independent from the existing structure: it is created with and against the existing field of forces, results, etc. This article will focus on one particular point that shows the integration of financial economics into the scientific field: the theoretical explanation. Financial economics became a scientific sub-field in consequence of the theoretical explanations given to empirical and statistical results accumulated during several decades. Indeed, following Bourdieu (1975, 96), "we have to distinguish the [author] who has discovered the unknown phenomenon from the one who made it a new scientific *fact* integrating it in a theoretical construction" of a scientific discipline, which accordingly places it within the scientific field. For instance, during the 1960s, the random character of stock market prices became a scientific fact about 100 years after its discovery by Jules Regnault in 1863[4]. It is also true for portfolio selection model, developed by Markowitz (1952) and Roy (1952) that also became a scientific fact during the 1960s by receiving a theoretical explanation. More generally, it is consistently during the 1960s that many discoveries by financial economists became scientific facts. It is also during that decade that a canonical history of this discipline emerged to support these new theoretical explanations.

The integration of financial economics into the scientific field was made possible by the synthesis of results. These results belong to three analytical components that were developed successively: financial econometrics, modern theory of probability and economic equilibrium.

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[4] Jovanovic (2006), Jovanovic and Le Gall (2001) analyze Regnault's work. A biography of this author is available in Jovanovic (2006).

Efficient market theory, CAPM and Modigliani-Miller theorems played a key role in this synthesis, and therefore in the rise of the new scientific discipline. They established links between, on the one hand, empirical and mathematical results in finance, and on the other hand, economic equilibrium. These links led to the creation of theoretical explanations for empirical results, explanations that were the last step in the categorization of financial economics as a science. This article focuses on efficient market theory. However, results presented here can be generalized for all subjects analyzed by financial economics for CAPM or Modigliani and Miller's article (Jovanovic 2007).
The format of this article is as follows.

Part 1 examines the incorporation of this new discipline into the scientific field during the 1960s, in particular its position toward the existing approach and economics. This first part shows that the organization of the financial economics as a scientific discipline was only made during the 1960s.

Part 2 analyses the internal organization of the new discipline during the 1960s to show how this discipline was structured as sub-field. It explains that two theoretical viewpoints existed and that they led to the creation of two canonical histories to support them.

1. THE CREATION OF FINANCIAL ECONOMICS: THE INTEGRATION OF THEORETICAL HYPOTHESIS AND EMPIRICAL RESULTS INTO THE SCIENTIFIC FIELD

Before the 1960s, works in financial economics were very marginal in the scientific field. Milton Friedman's reaction against Harry Markowitz's Ph.D. thesis gives a good illustration. This thesis, defended in 1952, deals with the theory of portfolio selection. It is one of the first Anglo-Saxon works in what it is now called financial economics that was not exclusively empirical. In the defense, Friedman declared: "Harry, I don't see anything wrong with the math here, but I have a problem. This isn't a dissertation in economics, and we can't give you a Ph.D. in economics for a dissertation that's not economics. It's not math, it's not economics, it's not even business administration" (in Bernstein 1992, 60).

While Friedman's reaction could be considered inappropriate or excessive, given the importance of Markowitz's work today, it is understandable in the light of Markowitz's contribution in his 1952 article. This article consisted on using mathematical properties of random variables to show that shares diversification from a portfolio could reduce the variability of returns: the expected value of a weighted sum is the weighted sum of the expected values, while the variance of a weighted sum is not the weighted sum of the variances. Markowitz did not give any theoretical demonstration of his mathematical result; he just operated a financial window-dressing of some mathematical properties[5]. Therefore, Friedman's reaction is a good signal about the situation of financial economics before the 1960s, and more specifically before Modigliani and Miller's contribution in 1958: the few existing works did not constitute either an academic or a scientific discipline yet; there were applied mathematics and empirical investigations without theoretical contribution that took place in a scientific or an academic discipline that already existed. This situation changed with the creation and the organization of a new community during the 1960s.

This first part shows that financial economics became a science during the 1960s by analyzing two points: first, the construction of a new scientific community, which had two main groups of research; second, the integration of this community into the scientific field thanks to economics.

1.1. THE RISE OF A NEW SCIENTIFIC COMMUNITY

The creation of a community with two main groups of research

At the beginning of the 1960s, a new generation of economists started their graduate studies and contributed to the creation of financial economics[6]. The studies about financial markets or corporate finance were not new; the novelty came from the manner of study them. One major feature was the access to new mathematical tools from modern theory of probability. Since the 1950s, the modern theory of probability was progressively used to study financial markets and corporate finance. At the beginning, academics exploited the proprieties of random variables. Then they used stochastic processes, which are analyzed by the modern theory of probability. Before the 1960s, hardly any economist and financier used these processes, because they were not well understood and they were not greatly diffused. Effectively, the modern theory of probability, which mainly comes from Kolmogorov's work, was truly accepted in the 1950s by the new generation of mathematicians – Mazliak (2003), Chaumont *et al.* (2004). Even during the 1960s, few economists or financiers used them. For instance, Samuelson (1965a, 1965b), who was the first with Mandelbrot (1966) to substitute the martingale model[7] for the random walk model/Brownian Motion to represent stock

[5] Obviously, this situation completely changed with the publication of Markowitz's book in 1959, which gives theoretical explanation to his model.

[6] For instance, John Bauer, Eugene Fama, Benjamin King, Arnold Larson, Sydney Levine, Jacob Michaelson, Arnold Moore, William Sharpe, William Steiger, Alan Wright.

[7] A sequence of random variables P_t adapted to $(\Phi_n ; 0 \leq n \leq N)$ is called a martingale if $E(P_{t+1}/\Phi_0, \Phi_1, ..., \Phi_t) = P_t$. This means that the better estimation of the security's price at the time t+1, P_{t+1} with the available information at the time t, Φ_t, that we can do at the time t is the security's price at the time t, P_t. Thus the expected profit, y, between two periods, considering the available information at the time t, Φ_t, is zero $E(y_{t+1} / \Phi_t) = 0$.

price variations, needed the help of a mathematician to make his mathematical demonstration (Samuelson 1965b).

With the use of stochastic processes, the mathematical treatment of the random character of stock market variations has taken on a large importance since the 1960s. Most of the academics who innovatively studied financial markets with modern theory of probability during the 1906s and the 1970s were at the University of Chicago or at MIT.

At the University of Chicago, research was made at the Graduate School of Business where Harry Roberts worked with James Lorie and Lawrence Fisher. In 1960, the latter two professors started an ambitious 4-year program of research on security prices[8]. They created the *Center for Research in Security Prices* (CRSP), which had an important group of Ph.D. students –such as Eugene Fama, Benjamin King and Arnold Moore. Merton Miller joined them one year later, in 1961[9].

At the same time, MIT opened a new area of research on this topic with Sidney Alexander, Paul Cootner, Dick Eckaus, Hendrik Houthakker (visiting professor), Ed Kuhn, Paul Samuelson, and several students, including Walter Barney, John Bauer, Sidney Levine, William Steiger and Richard Kruizenga. During the 1960s, Cootner supervised more than 20 theses in financial economics and became an essential figure of the development of this discipline at MIT.

The training of the new entrants: the creation of scientific journals, seminars and publication of textbooks

The creation of a new scientific community requires that its new members share common tools, references and problems. This was precisely the role of textbooks, seminars and scientific journals. Those in financial economics were developed from the beginning of the 1960s with the arrival of this new generation of students. Concerning periodicals, the two journals that had published articles in finance, the *Journal of finance* and the *Journal of Business*, changed their editorial policy during the 1960s. Both started publishing articles based on modern theory of probability and on modeling –see Bernstein (1992, 41-4 and 129). These two reviews published several special issues to reinforce the new orientation and results. In 1966, *Journal of Business* published a special issue on "Recent quantitative and formal research on the stock market". In addition of these two journals, other scientific journals specializing in financial economics were created, such as the *Journal of Financial and Quantitative Analysis*, created in 1965. In 1968, this journal published a special issue about the application of the random walk model to stock prices.

[8] Lorie was precisely recruited in 1951 at Chicago to revitalize the Graduate School of Business.
[9] Fisher Black, whose successor was Myron Scholes, also managed this center. Both elaborated the option pricing model, which was published in 1973.

It was also during the 1960s that textbooks and collected articles started to be published[10]. These publications also helped to define and stabilize a culture shared by the members of this new community. Given that collected articles are published before textbooks, these two kinds of publications provide an indication about the evolution of the discipline. Indeed, the diversification of the subjects analyzed and the publication of textbooks are good indicators to identify the period when a new scientific discipline is finally accepted. During the 1960s, following the publication of Markowitz's book in 1959, the publications of collected articles focused on portfolio selection. It was only at the end of the 1960s that textbooks on this subject were published. During the second part of this decade, there was a diversification of subjects, which started to structure the discipline. In addition to portfolio selection, subjects dealt with the nature of stock price movements, the investment returns, the market efficiency and the CAPM –*Capital Asset Pricing Model*. However, textbooks on these subjects only started to be published during the 1970s.

Among these new collected articles, there is *The Random Character of Stock Market Prices* edited in 1964 by Paul Cootner. This book constitutes the first anthology of articles that analyze random stock price movements. It has an important place in the history of financial economics for three reasons. First, it contributed enormously to the diffusion of the random walk model and its interpretation. Second, it sketched a research program for the future that was largely followed. Third, this book provided the first presentation of historical data relative to financial economics.

The differentiation from existing approaches

The third feature deals with the definition of a new field of investigation. To fill a new domain of research and to justify the usefulness of their new approach, academics had to adopt a strategy to differentiate from previous approaches. Articles that dealt with stock price variations provide a good illustration of this kind of opposition. New academics chose to open several debates between their new approach, mainly based on mathematical models and tools –in particular the random walk model–, and previous approaches that studied stock prices changes, in particular Chartism and business cycles –as those defended at the NBER. These debates generally took place in specialized journals and newspapers, such as *Business Week* or the *Financial Analysts Journal*[11], in which academics

[10] For instance, Cootner (1964), Fredrikson (1965), Wu and Zakon (1965), Fredrikson (1971) or Lorie and Brealey (1972) published collected articles, while Moore (1968), Mao (1969) or Jean (1970) published textbooks.

[11] See for example the debate between Levy (1967, 1968) and Jensen (1967). See also Granger (1970), Hoffland (1967), Shelton (1967), Van Horne and Parker (1968) or Wallich (1968).

popularized their results. To justify the new approach, academics used a Manichean presentation of each approach. For instance, Cootner introduced his book by saying that:

> “these academic studies have proven to be more skeptical about the folklore of the market place than those of the professional practitioners. To several of the authors represented in this volume the "patterns" described by some market analysis are mere superstitions. […] it is hard to find a practitioner, no matter how sophisticated, who does not believe that by looking at the past history of prices one can learn something about their prospective behavior, while it is almost as difficult to find an academician who believes that such a backward look is of any substantial value. The essays in this book are exclusively of the academic type” (1964, 1-2).

As other defenders of the random walk model and the new ideas, Fama (1965b, 59) or Archer (1968, 31-2) presented their results as a “challenge” to practitioners, who had to justify the usefulness of their practice. Finally, Hoffland provided a good summary of the situation:

> “Folklore is a body of knowledge incorporating the superstitions, beliefs and practices of the unsophisticated portion of a society […]. Folklore is distinguished from scientific knowledge by its lack of rigor […]. The Dow theory is often used as an example of a crudely formulated stock market "theory"” (1967, 85).

As we see, the most important argument was the scientific claim: new academics argued that their approach was based on scientific criteria, while Chartism would be based on folklore and would have no scientific foundation. Financial economics was supposed to drill above and beyond previous folkloric practices, and the random walk model was presented as the uniquely available scientific analysis of the character of stock price changes. The vocabulary used was intentionally clear-cut to convince the reader: “scientific”, “folklore” or “challenge”. In addition, academics chose to call the new discipline *modern* financial theory to insist on its novelty. Chartists and professionals were not the only targets. Many authors used the publication of textbooks as opportunity to express their dissatisfaction with the traditional approach. However, after the 1960s, once financial economics was permanently embedded into the scientific field, these debates disappeared: they lost their significance, because the scientific community and many financiers permanently recognized and adopted financial economics.

1.2. THE LINKS WITH ECONOMICS THROUGH THE CONSTRUCTION OF EFFICIENT MARKET THEORY

These new seminars and publications contributed to the creation of a truly homogenous community, which shared common problems, tools and language, scientific journals and courses in universities. The use of the theory of modern probability, in particular through the conception of uncertainty, offered new perspectives on already existing problems. At that time, however, such developments were technical and any theoretical explanation did not exist. In other words, during this period the modern theory of probability provided new tools that social sciences could exploit, but, obviously, this is not enough to build a new discipline: a model does not contain causalities *per se*, because the choice between endogenous variables and exogenous variables comes from theoretical frameworks. Indeed, a theory gives causalities that allow defining the structure of the model. These new tools from modern theory of probability cannot provide an explanation to the empirical environment. Therefore, theoretical frameworks are necessary to introduce financial economics into the scientific field. More precisely, it was necessary to link the new approach with an existing science or with the *criteria of conventional acceptance*[12] of that time. The use of the contemporaneous scientific method –the tests and the hypothetico-deductive method– already constituted an important link. However, during the 1960s, the crucial step for the creation of the financial economics was the construction of theoretical explanations based on concepts from economics.

The lack of theoretical explanation before the 1960s

Before the 1960s, no theory was explaining the new results in portfolio selection or in the random character of stock market prices. This crucial point illustrates what kept financial economics from becoming a scientific discipline. This absence characterizes all existing works written during that transitional period –see Jovanovic (2007).

Neither Cowles (1933), Working (1934) nor Kendall (1953), who are the first Anglo-Saxons to analyze the random character of stock prices, created any theory to explain this phenomena. Indeed, the enthusiasm for the new econometric practices developed since the 1930s clouded the research for theoretical explanations of the random character of stock prices. Theoreticians pointed out the absence of theoretical explanation during the 1950s. This is particularly striking after the Koopmans-Vining debate at the end the 1940s, which set NBER against Cowles Commission over

[12] I consider that the acceptance of a theory or a theoretical model does not only depend on empirical validation; there are also the criteria of conventional acceptance. The conventional acceptance concerns the conventions –postulates, beliefs, etc.– that a theory (or a model) must respect in order to be accepted as a scientific result of a discipline.

the lack of theoretical explanation and the necessity to link measurement with theory[13]. For instance, when Maurice Kendall published his statistical study on random price fluctuations in 1953, this study was accepted with interest even as its economic contribution was harshly criticized. The most important critique was the absence of links with economic theories or concepts (Houthakker 1953, 32, Prais 1953, 29)[14].

This evolution in economics had a direct influence on the two main defenders of the random character of prices at that time, Working (1956, 1958, 1961) and Roberts (1959), who also consistently highlighted the absence of theoretical explanation and the weakness of the statistical results. For instance, Roberts (1959, 9) pointed out that the independence of the variations –one of the two aspects of the random walk model with the distribution function– was still not established. Working (1956, 1436) underlined that there was no true verification of the random character of stock price variations and added that it was impossible to reject the chartist analysis with certainty. These remarks were very important because, at the end of the 1950s, there were very few authors who had published on this subject.

The random walk caught up with economics

This lack of theoretical explanation was filled during the 1960s when empirical and mathematical results were linked with economic equilibrium. Although they recognized the absence of theoretical explanation for the random character of stock price fluctuations, Working and Roberts were also the first to make links with the economic theories in order to give theoretical foundations to empirical observations. Roberts (1959, 7) used the "arbitrage proof" argument, which is an extension of the economic law of one price in perfect capital market: the forces of competition will ensure that any given commodity will be sold at the same price. Franco Modigliani and Merton Miller, one of Roberts' colleagues in Chicago, popularized this argument in their article published in 1958[15]. This argument forged a first link between economics and financial results: their demonstrations are an implication of equilibrium in a perfect capital market, which provides a direct link with economic equilibrium. Roberts used it for the same aim: create a link between economic concepts and random walk model. On the other hand, Working (1956) established an explicit link between the unpredictable arrival of information and the random character of stock price changes. Another

[13] The Koopmans-Vining debate influenced the whole works of the 1950s. On this controversy, see Mirowski (1989).
[14] See also the remarks of Champernowne, Bartlett, Rao or Jevons in the discussion that follows Kendall's article.
[15] Although these authors were not the first to apply arbitrage proof in finance (Rubinstein 2003), their article led to its popularity for two reasons: 1) their article is one of the first to use modern theory of probability to analyze a financial problem; 2) these authors had a strong academic anchorage (MIT and University of Chicago).

important step was the identification of a link between financial econometric results and the equilibrium from economics made by Cowles (1960, 914-5). By making a first reference to a competitive market, this article constitutes the beginning of a connection with the standard economic theory that progressively led to elaborate the efficient market theory. And, two years later, Cootner presented the idea of the efficient market theory, although he did not use that expression (1962, 25). Indeed, he suggested linking the random walk model, information, and the economic equilibrium.

Several of his students used and diffused his suggestion. It also interested researchers at the University of Chicago Graduate School of Business, most notably a young graduate student, Eugene Fama. In his Ph.D. thesis, defended in 1964 and published the next year in the *Journal of Business*, Fama synthesized empirical work and gave his first formulation of the efficient market theory. He formulated the definition of the efficient market that is generally used in his 1970 article: “a market in which prices always "fully reflect" available information is called "efficient"” (1970, 383). According to efficient market theory, if the model of equilibrium does not use all available information to evaluate the value of the security, it will be possible to make an arbitrage. Thus, in an efficient market, the equalization between the price and the equilibrium value means that all available information is included in the price. Consequently, it is not possible to use past information to predict the future changes of the prices: present and future prices are independent from the past prices[16]. For this reason, in an efficient market, stock price changes must be as random as the arrival of new information. In other words, according to this theory, the random walk model can simulate the dynamic evolution of equilibrium prices in a competitive market. As a result of this link with economic equilibrium, the efficient market hypothesis, as well as portfolio selection and arbitrage proof (Jovanovic 2007), allowed the introduction of financial economics into the scientific field.

2. THE INTERNAL ORGANISATION OF THE NEW DISCIPLINE

The first part has examined the integration of financial economics into the scientific field. This second part will analyze the internal organization –the positions and relationships in this sub-field– of the new discipline. We have seen that, during the 1960s, several theoretical links were made between the random walk model and economic concepts and theories. Although these links could give a theoretical explanation to the random character of stock prices, this explanation was not unique. This second part shows that two theoretical viewpoints coexisted: the first contended that financial markets are perfect markets and that stock prices follow a pure random walk, and the second that financial markets are not perfect and that stock prices do not follow a pure random walk. Roughly, the University of Chicago Graduate School of Business defended the first position, while MIT defended the second. These two viewpoints structured the new sub-field. They also directly influenced empirical investigations conducted during the 1960s and they led to the creation of two canonical histories of financial economics.

2.1. THE DEFENSE OF TWO THEORETICAL VIEWPOINTS

Two theoretical viewpoints

As I explained above, Cootner (1962) was one of the first to sketch the efficient market theory. However, after his suggestion to link the random movements of stock prices with the idea of perfect stock markets, Cootner (1962, 25-6) added:

> "Where else can the economist find that ideal of his –the perfect market? Here is a place to take a stand, if there is such place. Unfortunately, it is not the right place. The stock market is not a random walk. A growing number of investigators have begun to suspect it, and I think I have enough evidence to demonstrate the nature of the imperfections. On the other hand, I do not believe that the market is grossly imperfect. In fact, I do not know why the process I see going on in the market is not worthy of the name perfection too. It strays from "perfection" only to the extent that it defines the Frank Knight – Milton Friedman assumption of profitless speculation. Even more interesting, perhaps, is that my model is perfectly compatible with much of what I interpret Wall Street chart reading to be all about".

Cootner's position gives a clear illustration of the MIT position: stock markets are not perfect. Moreover, these theoreticians defended Keynesian considerations, in particular the Keynesian "normal backwardation" of commodity prices[17], which is not completely relevant to the markets' perfection. In 1960, Cootner answered Lester Telser, a young scholar of the University of Chicago, about the possibility of a perfect stock market. Telser expressed Knightian doubts on Keynes' normal backwardation. Telser used the no arbitrage opportunity to suggest that there would be no difference between the price of future and the spot price expected upon expiration of that contract,

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[16] This is a property of the random walk model, which is a Markov process.

[17] The normal backwardation is a fee paid by a seller of a security to the buyer for the privilege of deferring delivery. It implies that a risk premium exists so that futures prices fall short of the expected future spot price.

and that futures would give an unbiased estimate of such expected prices (Cootner 1960, 397). In this case, stock markets would be perfect, a position that Cootner criticized[18].

This position against the idea that stock markets are perfect also influenced Cootner's conception of the random character of stock prices. He suggested that prices behave as a constrained random walk within reflecting barriers. He assumed that "When prices neared the barrier there would be a tendency for some negative autocorrelation, since movements to the barrier would be more likely to be followed by movements in the opposite direction" (1962, 28). The following year, his constrained random-walk model was validated empirically by one of his students, William Steiger. Cootner's model was supported by Samuelson who explained, "I believe Paul Cootner was right to doubt that the speculative price of a stock, bond, grain futures, grain physical, machine tool, or acre of perpetual land would wander endlessly (like $\sqrt{T}$) away from its rendezvous with its equilibrium dictated by economic law. His reflecting barriers were a valiant attempt to evade the unacceptable" (1982, 114)[19]. MIT's position on the imperfection of the stock markets was also shared by Working. Starting at the end of the 1950s, Working began to develop a theoretical explanation of the more or less random character of stock prices. He arranged short-time movements of stock price into two categories (1956, 1431). The first one includes movements related to pertinent new market information. This category is divided into three subcategories, according to whether the price movement was: a) exactly appropriate, in size and direction, to the new information that induced it; b) larger than the new information warranted; or c) smaller than the new information warranted. The second category is divided into two subcategories, according to whether the movement was: a) unrelated to any pertinent economic information; or b) corrective, tending to eliminate existing price maladjustment. In another article, Working added:

> "["perfect market"] assumption would impair the usefulness of our model because it would eliminate the differences of opinion that are the source of much trading in a real market. To provide for differences of opinion, it is necessary only to specify that the traders are human rather than superhuman in their mental capacity" (1958, 192).

Finally, for Working as well as for MIT, stock markets are imperfect.

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[18] Samuelson also justified the introduction of the martingale model with the normal backwardation: he criticised the most restrictive properties of the random walk model, which precisely cannot explain "the alleged Keynes-Hicks-Houthakker-Cootner pattern of "normal backwardation,"" or "the Cootner pattern" (Samuelson 1965a, 41).

[19] Note that the increase of prices according to the $\sqrt{T}$, where T is the time, is a consequence of the independence of the stochastic process. Samuelson (1982, 108-110) gives a mathematical interpretation of Cootner's model.

These positions are strongly different from those defended at the University of Chicago Graduate School of Business. One of the starting points of the CRSP was the defense of the random walk model, and Fama was "the most energetic and prolific randomist" (Lorie 1965, 17). As we saw above, they claimed that stock markets are perfect, and characterized them as efficient. To demonstrate this, they considered that financial markets are perfect and that they reflect the competitive market of economics with rational agents. Fama gave the first theoretical demonstration in his thesis. He considers two kinds of agents: on one hand, "sophisticated traders", who are the only fundamentalists and chartists able to determine the intrinsic value of securities; on other hand, other participants who do not have this skill and who are responsible for the "noise" on stock markets. "Thus these two types of sophisticated traders [fundamentalists and chartists] can be roughly thought of as superior intrinsic-value analysts and superior chart readers. We further assume that, although there are sometimes discrepancies between actual prices and intrinsic values, sophisticated traders in general feel that actual prices usually tend to move toward intrinsic values" (1965a, 37-8). "Sophisticated traders", due to their skills, make a better evaluation of the intrinsic values than other agents do. The major feature here is the existence of a homogeneous group who shares the same intrinsic values –this group can be for instance institutional funds. To have an equalization between intrinsic values and prices, Fama adds another hypothesis: it is necessary that the financial resources of the "sophisticated traders" are superior to those of other agents (Fama 1965a, 40). Due to the fact that the "sophisticated traders" have the majority of financial resources and share the same intrinsic values, their actions lead both to prices equaling the intrinsic values they share as well as to any consistent profit being erased. Therefore, according to Fama, no bubbles can rise, because "sophisticated traders" "may cause these "bubbles" to burst before they have a chance to really get under way" (1965a, 38). In the same way, these traders should guarantee independence in the arrival of new information (1965a, 39). Finally, "the stock market *may* conform to the independence assumption of the random walk model even though the processes generating noise and new information are themselves dependant" (1965a, 39)[20]. Therefore, no trader can make a profit.

These two theoretical viewpoints guided the intensification of research in financial econometrics

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[20] Fama dropped his distinction between sophisticated traders and other traders in his 1970 article.

Since the 1960s, empirical research gained the support of computers[21], which intensified investigations on empirical data. However, contrary to what the traditional history generally presents, empirical results obtained during this period were not homogeneous: empirical investigations on stock price variations completely depended on the two theoretical viewpoints identified above.

At MIT, academics aimed to identify correlations in price series or possibilities to anticipate price variations. They found a first support to their conviction that the stock markets are not perfect with the initial research on the random character of stock price movements. The presence of trends and correlations in stock price movements had been noted by Cowles who identified a bias that can give the possibility to predict futures stock price fluctuations. Following a remark made by Working (1960)[22] about these "pretended trends", Cowles took once again to the calculus of his 1960 article: he obtained the same results (Cowles [1960], 914). Kendall also found significant correlations, but he minimized them. Alexander wrote of Kendall's conclusion that:

> "Kendall felt impelled to draw the moral that it is dangerous to generalize even from fairly extensive sets of data. For, from the behavior of wheat prices and the stock prices, one might have concluded that speculative markets do not generate autocorrelated price changes […]. Alas, Kendall drew the wrong moral. The appropriate one is that if you find a single exception, look for an error" (Alexander 1961, 9).

Working from these first results, MIT's academics also identified systematic movements in price variations. Houthakker analyzed stop orders and showed that "prices changes are not purely random but follow certain longer run trends" (1961, 166). These trends could be exploited to make profits[23]. In his 1957 article, he had already shown that "large speculators show definite evidence of forecasting skill, both in the long and in the short run" (1957, 151). In addition, Alexander (1961) reexamined the main results that validate the random walk model. He concluded that trends exist. In 1962, Cootner published an extensive study of weekly data on 45 stocks. Essentially, he reported that prices appeared to move randomly when studied at one-week intervals. Nevertheless, he also found evidence of trends in the same data at 14-week intervals[24].

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21 The first computers were created in the 1930s, but entered the academic research laboratories in the 1960s –see Metropolis et *al.* (1980).

22 Cowles and Jones (1937) obtained statistical correlations for monthly or weekly averages of daily stock prices. Working (1960) explains that the use of averages can introduce correlations not present in the original series.

23 Houthakker also showed that the normal law is not verified because the distribution of time-series is much more leptokurtic (1961, 168).

24 Weintraub (1963), Mellon (1964) and Niederhoffer (1965) obtain similar results.

At Chicago, academics worked towards a different goal. The CRSP wanted to validate the random walk model at any price. It was "the *idée fixe* of the school" (Niederhoffer 1997, 271). As Niederhoffer explains, they were

> "a team of four of the most respected graduate students in finance [who] had joined forces with two professors, now considered venerable enough to have won or to have been considered for a Nobel prize, but at that time feisty as Hades and insecure as a kid on his first date. This elite group was studying the possible impact of volume on stock price movements, a subject I had researched. […] I could see this Group of Six gathered together on a stairway landing, examining some computer output. Their voices wafted up to me, echoing off the stone walls of the building. One of the students was pointing to some output while querying the professors, "Well, what if we really do find something? We'll be up the creek. It won't be consistent with the random walk model." The younger professor replied, "Don't worry, we'll cross that bridge in the unlikely event we come to it" (1997, 270).

This group testified the random character of stock market prices by different ways. Moore (1962) was one of the first to examine the serial correlation between successive price variations. His results suggested that the previous price changes could not be used to predict future changes. Fama (1965a) studied the daily proportionate price changes of the 30 industrial stocks in the Dow Jones. He used several tests and his results supported the random walk hypothesis. In addition to these tests, academics from the CRSP systematically tried to provide counter-tests against results that identified significant price correlations. For instance, following Alexander's 1961 results, Fama and Blume (1966) demonstrated that filter schemes cannot, in general, provide returns larger than a naive policy of baying and holding stocks.

Finally, the community in financial economics organized itself through two groups according to two theoretical viewpoints. Each group found empirical results to validate their theoretical positions. Now, I will analyze how each group elaborated its own canonical history.

2.2. The two Canonical Histories of the Financial Economics

The inauguration of financial economics as a science and the organization of research were also made through a particular manner of presenting the history of the discipline. This manner of presentation is former with the construction of the cannon of theoretical articles: it is due to the fact that the creation of the canon led to select references and particular founding fathers. The canon not only exaggerates the importance of a few great men or contributions but simultaneously excludes or discredits the non-canonical. It can also link the selected contributions with a particular historical presentation and perspective. During the 1960s, the creation of a canon of theoretical contributions became the basis of a canonical history. I will show however that, during this decade, there was not one canonical history of the financial economics but two: each theoretical approach –perfect markets *versus* imperfect markets– created its own unique version to support a particular theoretical viewpoint.

I must however confess that the terminology is not completely satisfactory. The question is: do these theoreticians provide a historical work? In other words, is the term "history" appropriated? It is important to clarify that. During the 1960s, when canons were elaborated in financial economics, the creation of the canonical history of this discipline was not a goal, but a consequence of the way that the canons were made. Therefore, according to the method used, they did not create a historical work. As I explained in the introduction, such canonical history classifies selected theoretical texts thanks to a chronology, but the links it makes are neither historical assumptions nor historical results; they are inventions on the past. For these reasons, I chose to call this presentation of the past a "canonical history".

The selection among references

The diffusion of the modern theory of probability and the stochastic processes led Anglo-Saxon economists to discover Bachelier's work on stock prices. A mathematician, Leonard Jimmie Savage[25], who worked at the University of Chicago, found Bacheliers' work at the end of the 1950s, and, "around 1960, Haloid (now Xerox) machines in universities across the United States were busy running off copies of Bachelier's 60-year-old dissertation" (Hagin*, et al.* 1973, 61). This discovery led to the acknowledgement of a common founding father for financial economics. However, Bachelier was the only reference shared by the two main groups of research; indeed, the defense of different theoretical viewpoint led each group to select among references.

It can be observed that Cootner (1964) gave a large place to MIT's thesis. However, Cootner's book was the first publication of collected articles on the random character of stock prices at a time when few authors dealt with this subject. Therefore, the lack of references is not so visible. In opposition, this lack is distinctly observable in the authors from Chicago. When we compare Fama's articles and the book edited by Cootner, we note that Fama did not mention several references, in particular any thesis defended at MIT. More significantly, Fama's 1970 article does not

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[25] He discovered Bachelier's book, *Le jeu, la chance et le hasard*, around 1957. It is interesting to notice that the book is precisely classified in mathematics. Moreover, as Osborne explained, "Bacheliers' thesis was "rather inaccessible (it is available in the Library of Congress rare book room)" (1959b, 808).

mention any other references published after Cootner's book: apart from some articles reprinted in Cootner (1964), he only referenced articles published by his colleagues at the University of Chicago. For instance, there is no mention of works developed at University of California, *e.g.* Kassouf (1968), or of the special issue of the *Journal of Financial and Quantitative Analysis* on the random walk model and published in 1968. The same tendency can be found in Lorie and Hamilton (1973). In a similar fashion, Lorie and Brealey explained in their preface:

> "We felt a need for a book of readings that included the most important articles in the development of the modern theory of portfolio management. After selecting the thirty-seven articles in this book, we were surprised to find that almost half of them were in some way associated with the Center for Research in Security Price" (1972, viii)[26].

More precisely, while the references from works done in University of Chicago were exhaustive, very few works from outside of this University were referenced.

Moreover, to stay in harmony with their theoretical viewpoints, Fama, Lorie or Hamilton voluntarily ignored results and articles that contradict the acceptance of the perfect market's idea – random walk model and market efficiency. For instance, in his article, Fama did not quote several results developed at MIT; neither the article of Theil and Leenders (1965), Niederhoffer (1965), Shelton (1967) nor the patterns identified by Cowles (1944) and Cowles and Jones (1937) were considered. LeRoy also explained that

> "Fama's interpretation of Victor Niederhoffer and M. F. M. Osborne's (1966) evidence on runs –successive price changes of the same signs– is difficult to square with the fair game interpretation. Niederhoffer and Osborne found that reversals –pairs of successive price changes of opposite sign– occurred two to three times as frequently as continuations. Such systematic patterns are inconsistent with the fair game model. Despite this, Fama concluded and emphasized that such patterns, even though statistically significant, do no imply market inefficiency (p. 398)" (1989, 1595).

To be precise, academics of the University of Chicago cited articles that contradicted the random character of stock price movement only when they had been published articles that obtained opposite results with the same kind of tests. In the same way, Fama, Lorie and Hamilton did not

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[26] Let me precise that, for the authors, "modern theory of portfolio management" concerns the whole literature in financial economics of that time. Therefore, their book contains three parts: "The Behaviour of the Market" (with a section on market efficiency), "Portfolio Management" and the "Valuation of Securities".

mention the attempts of Working and Cootner to give theoretical foundations to the random walk model.

The choice to not reference works that did not share the same theoretical viewpoint had a crucial influence on the construction of the financial economics' canonical history.

The canonical history provided by MIT's theoretical viewpoint

Cootner's book, *The Random Character of Stock Market Prices*, is not only the first anthology of articles that analyzed random stock price movements; it is also the first to provide historical data of financial economics. Cootner organized this data according to the theoretical position defended at MIT: the imperfection of the stock exchange. His presentation consisted of justifying the search for correlations or predictability of stock price movements. It suggested a history in three steps: "Origins and justification of the random walk theory", "Refinement and empirical testing", "The random walk hypothesis reexamined".

For each step, Cootner discusses the place of the papers included, and their significance, in the historical development of the subject. He writes that:

> "It is clear, however, that until fairly recently, the study of [speculative] prices was the province of the speculator, rather than the academician. To be sure, Louis Bachelier (1900) pioneered in this study over 60 years ago, but his work, about which we will soon learn more, stands as an isolated event" (1964, 1).
>
> "While Roberts' paper gives, at some length, a justification of the assumption that stock prices should have independent increments, Bachelier had, some 59 years earlier, developed an elaborate mathematical theory of speculative prices based on that proposition and tested it […]. So outstanding is his work that we can say that the study of speculative prices has its moment of glory at its moment of conception […]. It also marked the beginning of the theory of stochastic processes, a beginning which went unrecognized for decades" (1964, 3).
>
> "If Bachelier was before his time in the development of probability he was even farther ahead in the analysis of stock market prices. It was not until 1934 that H. Working revived the idea of random walk in commodity prices, and it was more than twenty years later before Kruizenga (1956), stimulated by some work of Samuelson, used methods similar to Bachelier's to evaluate put and call options" (1964, 5).
>
> "After Cowles' work in the 1930's, there was little or nothing published along these lines until Kendall's 1953 paper […]. The publication in 1959 of both Roberts and Osborne papers marked the beginning of the sharp recent increase in interest in this subject, by bringing it to

the attention of the American academic audience for the first time since Cowles' articles in the thirties" (1964, 81-2).

According to Cootner, the history of financial economics starts in 1900 with Louis Bachelier –I will come back to this error in the epilogue. He is credited with having suggested for the first time the modeling of stock prices by a random. Then, Working, Cowles and Kendall tested the random walk hypothesis between 1933 and 1956. However, it was the articles of Roberts and Osborne published in 1959 that gave a large interest to these problems. The first statistical study was due to Moore in 1962. Cootner summarized in this way the two first steps of the financial economics' history. The following step, which occurred during the 1960s, was the re-examination of the random walk hypothesis. Cootner used several articles to point out three kinds of problems, the normal distribution, the independence of stock prices and the stationary of the process:

"Most of the work that follows the Moore paper stresses, in one way or another, the deviation of stock prices from the Einstein-Wiener process. The Alexander-Larson-Cootner-Steiger papers all question the independence hypotheses. The Fama-Mandelbrot material questions the assumption of Gaussian increments. The Osborne paper [1962] examines the stationary of the process. In raising these questions it was necessary to invent subtle new tests, or to apply more esoteric probability distribution theory" (Cootner 1964, 189).

This presentation in three steps gave a justification to works developed at MIT, which aims to identify correlations in security prices: the random walk model had been a roughly formed first hypothesis that had to be improved.

Cootner's introductions also gave the first historical elements of the presentation of the past as well as the first canonical history. The elements of these introductions are reprinted in many other books and articles and had a great influence during the 1960s. It can be evaluated through the reviews of Cootner's book that were published in the most important American journals between 1965 and 1968[27]. These reviews used Cootner's introductions extensively and, therefore, contributed to diffuse the historical data that became popular.

The canonical history provided by the Chicago's viewpoint

Academics of the University of Chicago Graduate School of Business used historical data from Cootner's book, but they elaborated another canonical history, which is mainly found in Fama (1965a), Fama (1970) and Lorie and Hamilton (1973). The goal of this latter history was to defend

[27] For instance, see Granger (1965), King (1965), Markowitz (1965), Weiss (1966), Beals (1966) or Rosett (1968).

the random walk model. More precisely, this canonical history was built to impress the triumph of this model. Therefore, it described a linear history that was focused on tests. Fama's articles provide a good illustration. Fama wants to build a testable theory although his definition of efficient market "has no empirically testable implications" (1970, 384).

Therefore, in his three articles (1965a, 1965b, 1970), he kept the same structure: the first part deals with theoretical implications of the random walk model and its links with EMH, the second part presents empirical results which validate the random walk model. This sequence –theory then empirical results– is nowadays very familiar. It constitutes the hypothetico-deductive method, the scientific method defended in economics since the middle of the 20th century. Then, Fama linked the historical data with a methodological perspective: he used the historical data that he did not discard to suggest that efficient market hypothesis would be successfully validated. Thus, the canonical history that arises from this maneuver gave the following presentation: the origin of the random walk model/efficient theory; the first generation of empirical tests; then, the second generation of empirical tests that also constituted a future program of research.

In his thesis, Fama gave a short presentation of the history of the random walk model in financial economics:

> "the first complete development of a theory of random walks in security prices is due to Bachelier [1900], whose original work first appeared around the turn of the century. Unfortunately his work did not receive much attention from economists, and in fact his model was independently derived by Osborne [1963] over fifty years later. The Bachelier-Osborne model begins by assuming that price changes from transaction to transaction in an individual security are independent, identically distributed random variables" (1965a, 41).

This would be the origin of the random walk model, which Fama called the Bachelier-Osborne model. Then, Fama pointed out that the first tests were not satisfactory:

> "Although Osborne attempted to give an empirical justification for his theory, most of his data were cross-sectional and could not provide an adequate test. Moore and Kendall, however, have provided empirical evidence in support of the Gaussian hypothesis"(1965a, 42).

In his 1970 article, he added:

> "Kendall's conclusion had in fact been suggested earlier by Working [1934], though his suggestion lacked the force provided by Kendall's empirical results" (1970, 390).

In 1970, Fama introduced three forms of efficiency: the weak form, the semi-strong form and the strong form. Following this presentation, Fama, Lorie and Hamilton classified works according to the form of efficiency they tested. However, they only dealt with results that validate the random walk model.

We can notice that this presentation seems close to Cootner's book, which was the main source used by Fama. However, there are two important differences with Cootner: on one hand, the place given to efficiency and, on the other hand, that given to the tests. The methodological perspective adopted in this canonical history suggested a linear history that strengthens the idea of the random character of stock prices' triumph: each time a result invalidated this hypothesis, authors from Chicago provided a counter-example against this result. This methodological perspective led to create continuities between authors or empirical investigations but these continuities are ad-hoc constructions and they are not compatible with historical data. For instance, the term "Bachelier-Osborne model" suggested continuity that does not exist because Osborne did not Bachelier's work when he published his article. We have exactly the same thing with the empirical tests and investigations presented in the first part.

EPILOGUE

An observation must be made: if we analyze ideas defended in textbooks and scientific journals, we must admit that since the 1970s the efficient market theory has been the most diffused, and can be considered as having won the theoretical battle. The assimilation of random walk model and market efficiency, although theoretically contestable[28], constitutes a good illustration. It is not the goal of the present article to explain either the reasons for this victory or the theoretical consequences of it. However, with the triumph of the efficient market theory, the canonical history of the University of Chicago Graduate School of Business has been largely diffused in scientific journals and in textbooks. It has been presented or extended by Bernstein (1992), Walter (1996, 2002), Merton (1998), Scholes (1998), Dimson and Mussavian (1999, 2000), Whelan *et al.* (2002), etc. It is however important to recall two points.

First, a large majority of contemporary tests validate the random character of stock price movements or returns[29]. However, what was the situation during the 1960s and before? Houthakker and Williamson (1996) tested the random character of stock prices during three periods: January

[28] See Jovanovic (2003b).
[29] It must keep in mind that the random character of stock prices does not automatically validate efficient market theory.

1969 to June 1975; July 1975 to June 1982; July 1982 to June 1992[30]. They showed that "the stock market, as measured by the S&P500, did not follow a random walk during the first two periods but did so in the third period. In other words, the market became more efficient over years" (Houthakker*, et al.* 1996, 136). They added that "it is somewhat ironic that in the 1970s, when the EMH [Efficient Market Hypothesis] gained widespread acceptance, there were significant departures from a random walk. Fortunately for financial theory –and for the functioning of our capital markets– the market now conforms closely to a random walk. Reality has caught up with theory" (Houthakker*, et al.* 1996, 136). Therefore, empirical tests were not the main element that led to the adoption of the efficient market theory and the random character of stock market price. This element has an important implication: during the 1960s, at least, MIT's academics gave a better description of these movements; however, the history defended by this institution place was not kept.

Second, considering the number of publications and the content of textbooks, we note that efficient market theory triumphed. The triumph of the efficient market theory, and with it the wide acceptance of the canonical history defended by the CRSP, has led to forgot many authors and contributions that do not enter into the framework of this canonical history –for instance, Jules Regnault or Irving Fisher[31]. The canonical histories analyzed here also led to obscure the fact that Anglo-Saxon mathematicians knew Bachelier's mathematical works since the 1910s, although they did not use them often[32]. The *Calcul des probabilités*, published in 1912, gave to Bachelier international recognition in the mathematicians' community, and some Anglo-Saxons mathematicians applied Bachelier's suggestions to answer financial problems. For instance, in December 1922 at the Mathematical Association of America, Arne Fisher used Bachelier's formulas to resolve financial problems (Cairns 1923, 97).

[30] These three periods come from fundamental change in transaction costs, which can constrain informed investors to use their information: in 1975, fixed commissions were abolished; in 1982, stock index futures were introduced, which was tantamount to a further reduction in transaction costs. However, as Houthakker and Williamson (1996, 136) clarified, while these changes in transaction costs are consistent with the evolution of the efficiency, this link does not prove causality.

[31] On Fisher's financial work, see Dimand (2004).

[32] We can note that Samuelson had known of Bachelier's work at the end of the 1930's (Taqqu 2001, 26). However, before the Second World War, the American school of mathematics did not exist. Therefore, the use of Bachelier's work was not necessarily shared by groups of research. On the diffusion of Bachelier's work, see Jovanovic (2004) and Taqqu (2001).

CONCLUSION

This article has shown that financial economics became a scientific discipline during the 1960s thanks to the creation of a new community of researchers and to the links of empirical and mathematical anterior results with economical concepts and theories. However, the fight of two theoretical viewpoints marked the integration of financial economics into the scientific field. This article has demonstrated that to defend their theoretical viewpoints, each group created his own representation of the past of their discipline, *i.e.* their canonical history. The representations of the past of the discipline were a weapon into the theoretical fights.

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