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ARTÍCULO

MANDELBROT, FAMA AND THE EMERGENCE OF ECONOPHYSICS

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It is argued that Mandelbrot's stable Lévy-Pareto distributions were not accepted into the emerging field of financial economics due to their incompatibility with the analytical techniques and properties of equilibrium economics, and to the absence —both in physics and in economics— of analytical solutions to the infinite variance associated with those distributions. Whilst physicists made stable Lévy distributions plausible, creating Econophysics in the meantime, economists just forgot about them, suggesting their strong bias towards desirable properties and against established facts.

Keywords: Efficient markets hypothesis, Mandelbrot, Fama, stable Lévy-Pareto distributions, Gaussian distribution, Kuhn, Lakatos, methodology.
JEL: B30, B40, G14.

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Se argumenta que las distribuciones Lévy-Pareto estables de Mandelbrot no se aceptaron en el campo emergente de la economía financiera debido a su incompatibilidad con las técnicas y propiedades analíticas de la economía de equilibrio, y a la ausencia —tanto en la física como en la economía— de soluciones analíticas para la varianza infinita asociada con aquellas distribuciones. Si bien los físicos hicieron posibles las distribuciones Lévy estables, creando entretanto la econofísica, los economistas simplemente las olvidaron, sugiriendo su fuerte sesgo hacia las propiedades deseables y en contra de los hechos establecidos.

Palabras clave: hipótesis de mercados eficientes, Mandelbrot, Fama, distribuciones Lévy-Pareto estables, distribución Gaussiana, Kuhn, Lakatos, metodología.
JEL: B30, B40, G14.

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On explique que les distributions Lévy-Pareto stables de Mandelbrot n'ont pas été acceptées dans le domaine émergent de l'économie financière étant donné leur incompatibilité avec les techniques et les propriétés analytiques de l'économie d'équilibre, et l'absence —tant dans la physique que dans l'économie— de solutions analytiques pour la variance infinie associée à ces distributions. Bien que les physiciens aient réussi à rendre les distributions Lévy stables, créant ainsi l'éconophysique, les économistes les ont tout simplement oubliées, suggérant leur forte inclination envers les propriétés désirables et contre les faits établis.

Mots-clés : hypothèse de marchés efficients, Mandelbrot, Fama, distributions Lévy-Pareto stables, distribution Gaussienne, Kuhn, Lakatos, méthodologie.
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Argumenta-se que as distribuições Lévy-Pareto estáveis de Mandelbrot não foram aceitas no campo emergente da economia financeira devido à sua incompatibilidade com as técnicas e propriedades analíticas da economia de equilíbrio, e à ausência, tanto na física quanto na economia, de soluções analíticas para a variância infinita associada àquelas distribuições. Embora os físicos tornaram possíveis as distribuições Lévy estáveis, criando, entretanto, a econofísica, os economistas simplesmente as esqueceram, sugerindo sua forte tendência para as propriedades desejáveis e contra dois fatos estabelecidos.

Palavras-chave: Hipóteses de mercados eficientes, Mandelbrot, Fama, distribuições Lévy-Pareto estáveis, distribuição Gaussiana, Kuhn, Lakatos, metodologia.
JEL: B30, B40, G14.

INTRODUCTION

Back in the early 1960s economists were a kinder bunch. Top economic practitioners used to invite scientists from other disciplines to engage in conversation, in order to learn from them about economics. Exchange with practitioners from other disciplines was not unheard of, for a significant and very influential fraction of the profession was composed of physicists and mathematicians who had arrived in America fleeing from Nazism and the war in Europe. Hendrik Houthakker was one of those economists and Benoit Mandelbrot was a wandering mathematician working at the time on Pareto's income distribution law. Houthakker invited Mandelbrot to give a talk at the Harvard economics department on his insights on Pareto's income distribution law. The invitation unleashed a complex episode of scientific cooperation whose effects can still be felt in the current debate over the ongoing global financial crisis (Mandelbrot & Hudson, 2004).

This article is an attempt to understand the choices made at that time by financial economists as they were working at the expanding and uncertain frontier of economic knowledge. It does not pretend to be a general theory of how economists choose between alternative theories. It is just a study of a reasonably well documented intellectual relationship between Eugene F. Fama, one of the founding fathers of equilibrium financial economics, the economists that made up the MIT "random crowd"[1], and the late polymath Benoit Mandelbrot. At the time, Fama was a very young economist and Mandelbrot an established mathematician with an interest in the distributions of price variations in speculative markets. Economists were kind to Mandelbrot during the early 1960s: they invited him to seminars, granted him visiting professorships, listened to him and tried to learn from his ideas.

The encounter would be but another anecdote, lost in the sea of episodes that constitute academic life, were it not for the fact that Fama and the "random crowd" were working on the foundations of what was destined to become the central research program of financial economics, and Mandelbrot was on his way towards the discovery of fractal geometry, via his work on the statistical distributions of real phenomena**.**

Unknown to them were the high stakes involved in their decisions. Fama was developing the foundations of his influential efficient market hypothesis, and Mandelbrot was working across the frontiers of economics, physics, geometry, statistics and mathematics, trying to unravel the underlying patterns hidden in statistical distributions associated with natural and social phenomena. One of those patterns is fat-tailed distribution: the emergence of an abnormal number of large observations outside the central section of the distribution curve. This pattern contrasts with its Gaussian or normal counterparts, in which most data falls into the central section of the distribution with only small observations at the tails. When

[1] A set of economists and statisticians working on finance economics during the early 1960s. The name was coined by Paul Samuelson (Fox, 2009).

they involve large observations, clustered in time, fat-tailed distributions can have powerful implications for the basic equilibrium properties of financial markets.[2]

As a matter of fact, Mandelbrot had uncovered the existence of a family of stable statistical distributions —first discovered by the Italian economist Pareto (1909) and the French mathematician Paul Lévy and largely ignored by economists[3]— that matched surprisingly well with the fat-tailed distributions found in the variations of real stock market prices. This was exciting news for the empirically minded Fama, who happened to have at his disposal a personal database of 30 stock market records, inherited from a former professor of his with a knack for speculative markets.

A rapid collaboration ensued (Mandelbrot, 1982, 2012; Mandelbrot & Hudson, 2004): Mandelbrot became one of Fama's dissertation advisors, and the young economist started what would be one of the first empirical tests of Mandelbrot's hypothesis on the fat-tailed distribution of price variations. Though Mandelbrot's hypothesis went beyond stock market prices to include the prices of most commodities, and would eventually extend to foreign exchange rates, Fama's exercise was limited to the 30 stock market prices of his original database. The providential arrival of computers to American campuses contributed to the speed with which Fama was able to work through the data. The final results were not in dispute: Mandelbrot's hypothesis was vindicated by Fama's empirical exercise (Fama, 2007).

But what followed is indeed open to inquiry. After having upheld Mandelbrot's hypothesis, Fama went back to work on what would become the efficient market hypothesis. The intriguing question is why he never integrated his recently discovered empirical results on fat-tailed distributions of financial stocks price variations into his fledging theoretical work. The question becomes even more intriguing if the important role PhD dissertations and dissertation advisors play in the future careers of students' is taken into account. Newly minted PhDs are very unlikely to give up on their dissertation results, especially if they are closely related to their ongoing research work and in particular in such a short period of time. Why did Fama decline to use his own empirical results when developing his theory? At issue are the role played both by empirical evidence and by deeply ingrained heuristics in the decisions taken by economists working at the frontiers of their discipline.

I will argue, in agreement with Lakatos' research programs hypothesis, that neither refutations nor anomalies played a crucial role in Fama's choices at that time. However, in contradistinction to the tenets advanced by Lakatos, Fama's research program did not anticipate, but circumvent and ignore, the potential anomalies and

[2] Mandelbrot's encounters with economists in the early 1960s extended beyond Fama and the random crowd: Michael Mitzenmacher reported on the "amusing" discussion Mandelbrot had with Herbert Simon on the latter's use of preferential attachment assumptions in the treatment of Zipf distributions (Mitzenmacher, 2004, p. 235).

[3] Some economists did not fail to note that these distributions were leptokurtic (Cootner, 1962; Kendall, 1953) and leptokurticity has become with time a stylized fact in financial economics, but its analytical and statistical consequences were never fully taken up.

refutations implicit in his own empirical work. As a matter of fact, Fama believed Mandelbrot's and his own empirical findings to be facts well beyond the reach of current statistical tools and techniques, and without any intuitive economic explanation. At the conclusion of his 1963 article on Mandelbrot and the stable Paretian hypothesis, Fama explicitly stated the need "to develop more adequate statistical tools for dealing with the stable Paretian distributions" (Fama, 1963, p. 429).

For a young economist in a hurry to establish analytical and empirical foundations for the emerging discipline of financial economics, the prospect of investing time and position in trying to come up with the statistical techniques and economic concepts that might have spanned the gap between finance economics and fat-tailed distributions with infinite variances did not appear to represent a reasonable option. From a purely pragmatic point of view, it looked indeed to be a very long and costly detour from the path towards a new financial economics based on equilibrium (Fama, 1976).

Since there had been no truly unprecedented scientific achievement before the appearance of the conundrum of infinite variance, it is difficult to interpret Fama's decisions from a "failure of articulation" perspective (Kuhn, 1962/2012, 2000). His failure to produce the statistical tools and tests required to incorporate Pareto-Lévy distributions into financial economics was not the result of his personal failure as a scientist, but of a structural mismatch between the statistical heuristics of equilibrium economics and the unsolved statistical and analytical problems associated with those distributions that best fitted the fat-tails; fat tails, that is, that were already a stylized fact of the observed distributions of stock price variations.

My contention is that the reasons Fama did not invest his research time in creating and applying the statistical tests and tools required to incorporate Pareto-Lévy distributions into financial economics involve *analytical convenience and incompatibility* with the statistical techniques available both to economists and physicists at the time. From the perspective of equilibrium economics, Pareto-Lévy stable distributions were a dead end. But this situation was not restricted to financial economics. It was not plausible in physics either to take at face value real world variables with infinite variances. As a matter of fact it took physicists almost 30 years to produce results that dealt effectively with the infinite variance difficulties faced by Fama in the early 1960s (Mantegna & Stanley, 1994; Schinckus, 2013). For their part, financial economists never attempted to incorporate fat-tailed distributions into their canon.

Practitioners of any discipline will always tend to choose those propositions, ideas, or facts that are fully translatable into the basic language of the current paradigm and are compatible with the desirable properties of its principal results. By the same token they will reject all propositions that are clearly incompatible with their main paradigmatic commitments and beliefs – and that are very unlikely to be workable, given the concepts and techniques currently at hand.

The reaction of some financial economists to Mandelbrot's proposal made explicit the implicit commitments and beliefs underlying their decisions within a technically restrained context. It is easy to detect the magnitude of the perceived threat in Cootner's oft-quoted reaction to Mandelbrot's proposals:

> Mandelbrot, like Prime Minister Churchill before him, promises us not utopia but blood, sweat, toil and tears. If he is right, almost all of our statistical tools are obsolete –least squares, spectral analysis, workable maximum likelihood solutions, all our established sample theory, closed distribution functions. Almost without exception, past econometric work is meaningless. Surely, before consigning centuries of work to the ash pile, we should like to have some assurance that all our work is truly useless (Cootner, 1964, p. 337).

Though it is not at all clear to which centuries of work Cootner was referring, it may be inferred that he was thinking of all the statistical and economic theory that had been accumulated by the neoclassical approach to finance and econometrics. The reference to "centuries of work" going to the ash pile suggests not only a very free use of hyperbole as a rhetorical device, but also how terrifying was the arrival of foreign statistical and analytical tools for experienced financial economists. The kindness of economists was thus not without limits: it was strongly bounded by the combined constraints of incompatibility and by deep-seated scientific values that privileged theoretical elegance over empirical content.

Mandelbrot himself lost interest in Lévy stable distributions and moved on to the task of creating the fractal geometry of nature. In fact, physicists tried to overcome the difficulties associated with the infinite variance of Lévy-Pareto stable distributions by coming up with truncated Lévy processes or flights, which allowed them "to use these processes to statistically characterize turbulence phenomena without the problem of infinite variance" (Jovanovic & Schinckus, 2013, p. 327).

Mantegna and Stanley (1994) introduced truncated Lévy flights —that combined Mandelbrot's stable Lévy distribution with an extremely slow convergence to a Gaussian distribution— in order to account for stochastic processes with big jumps but finite variance, thus making them plausible both in the physical world and in the world of finance. Truncated Lévy flights became the statistical and theoretical foundation for explaining the existence of fat-tailed distributions in stock market price variations, and contributed to the emergence of the new transdisciplinary field known later as Econophysics.

The divergent paths taken by fat tails and stable Lévy distributions in physics and in economics (Jovanovic & Schinckus, 2013; Rickles, 2007; Schinckus, 2013; Stanley *et al*., 2002) suggest divergent styles and methodological rules with respect to the treatment of facts and statistical tools. Whereas economists readily gave up on the enigma and all but completely forgot about the Lévy stable distributions, physicists, not without initial infighting about their relevance, went on to produce the conditions that rendered Lévy stable distributions tractable as a predictive tool for variations in stock market prices.

That the path taken by neoclassical financial economists at the time was neither exclusive nor inevitable may easily be confirmed by following the way physicists approached the infinite variance associated with stable Lévy distributions. Not only did they successfully introduce truncation techniques into their analytical tool-kit, making stable Lévy distributions plausible for physics (Schinckus, 2013), but they also went on to create a whole new research field, related to economics but living beyond its boundaries as a scientific discipline: Econophysics. This new field provided as strong a set of counterfactual evidence as it is possible to find against the inevitability hypothesis that governs the decisions of financial economists.

Facing similar scientific challenges and having to take decisions at equivalent bifurcation nodes, economists and physicists chose opposite paths: whereas economists clung to the properties associated with equilibrium economics as it applied to financial issues, introducing —at best— alternative models that preserve the Gaussian distribution while allowing large variations by combining Gaussian and Poisson distributions, physicists took seriously the intellectual challenge posed by Mandelbrot and succeeded in reconciling established facts and statistical distributions other than the Gaussian. The outcomes were also highly divergent: financial economics grew into a very elegant mathematical construction with indisputable practical consequences, but it was a system that displayed severely limited agreement with the stylized facts of financial markets. Econophysics, on the other hand, exhibits a better record of agreement between observed facts and theory, but is lacking in the kind of theoretical foundations and statistical inference preferred by economists.

THE SITUATION

Benoit Mandelbrot was no stranger to the economists working on speculative markets at the beginning of the 1960s. There are well-known anecdotes of his encounters with Hendrik Houthakker at Harvard in 1961 (Gleick, 1988), and with Paul Cootner (Fox, 2009) during the editing process of Cootner's influential 1964 book on random markets. Moreover, he (Mandelbrot, 1966) and Paul Samuelson (Samuelson, 1965) published —just a few months' apart— articles characterizing speculative price variations as sub-martingales.

All those encounters, and many more to come, involved in one way or another the joint issues of market dynamics and the proper statistical tools needed to deal with them. So close was Mandelbrot to the emerging community of economists working on market dynamics that he was awarded a visiting professorship at Harvard for the academic year 1962-1963 and even wrote an informal introduction for economists to the new statistical methods available for dealing with empirical distributions. This piece was published in the already influential *Journal of Political Economy,* published by the University of Chicago. It was in this congenial ambiance that Benoit Mandelbrot ran into Eugene F. Fama (or was it the other way around?) at some moment during the very early 1960s (Mandelbrot and Hudson, 2004). According to Mandelbrot (2012), Fama, at the time a graduate student at Chicago Graduate School of Business, visited him often[4].

Mandelbrot's narration of events dated his fateful encounter with Hendrik Houthakker to early in May 1961. At the time, Houthakker was working on price dynamics and, as he was familiar with Mandelbrot's work on Pareto's income distribution law, had invited him to speak at a seminar at the Harvard Economics Department. In a note written in 1982 Mandelbrot recollected:

> Early in 1961, while on my way to a seminar, I stepped into the office of my host, a Harvard economist [Houthakker]. On his blackboard, I noticed a diagram nearly identical to one I was about to draw. His diagram referred to a topic of which I knew nothing: records of the price of cotton. My host had given up his attempt to model this phenomenon, and *he challenged me to take over* (Mandelbrot, 1982, p. 20, my emphasis).

The issuing of challenges such as this was not –and unfortunately never became— a common practice in economics. Mandelbrot took Houthakker's seriously however and in a few weeks he had succeeded:

> [By] introducing a radically new hypothesis, I preserved the random walk hypothesis (...) I also preserved the efficient market hypothesis (...). The third basis of the usual model is the hypothesis that price changes follow a Gaussian distribution. These entire hypotheses, due to Louis Bachelier, were first faced seriously in 1960. The resulting theory, claiming that price (or its logarithm) follows a Brownian motion, would be mathematically convenient, but it badly fails to fit the data (*ibid*.).

Here, precisely, was the crux of the matter: even preserving the random walk hypothesis and the efficient market hypothesis the data on price changes did not follow the Gaussian distribution —they showed large variations and fat tails— and thus this aspect of the emerging theory was at fault. Houthakker had run into similar data when researching the dynamics of future prices. He had also found very large, and unexpected, deviations, in the distribution of daily price changes. In his words:

> The distribution of day-to-day changes in the logarithms of prices does not conform to the normal curve. It is not significantly skew, but highly leptokurtic (that is, there are more very large and more very small deviations than in a normal distribution with the same mean and variance). (...) It complicates the application of the available methods of time-series analysis, which are none too satisfactory even for the normal case (Houthakker, 1961, p. 168).

[4] Fama even introduced Mandelbrot to his Chicago adviser, Merton Miller, who later on convinced his colleagues to hire the peripatetic scientist.

More in line with Mandelbrot's findings Houthakker had found that:

> Very large deviations, in fact, seem to come in bunches. The leptokurticity mentioned above may be related to the changing variance (*ibid*.).

The fact that large deviations seemed to come in bunches suggested an underlying statistical regularity. Houthakker gave it serious thought, suspecting that:

> [A] nonlinear stochastic process is at work, the detection of which requires different techniques of analysis (*ibid*., 169).

But nothing came of this line of research and in the closing section of his 1961 piece, Houthakker shifted gears towards a more Walrasian discussion of the problem, without finding a convincing solution. It is at this point that Mandelbrot's non-economist perspective entered the economic scene –for a while at least. Responding positively to the gauntlet thrown down by his host at Harvard, Mandelbrot came through with a new model for the long-tailed distribution conundrum:

> My model replaces the customary Gaussian hypothesis with a more general one, while allowing the population variance of the price changes to be infinite. The model is time-variant, but it creates endless configurations; it accounts for all the data, including both the seemingly nonstationary features, and the seemingly nonrandom large excursions (Mandelbrot, 1982, p. 20, *op. cit*.).

Why was the customary Gaussian hypothesis so important for Mandelbrot's research and for standard economic theory? A few words about the Gaussian distribution will help both to establish an answer and to understand its importance and the ambitions associated with Mandelbrot's response to Houthakker's challenge. The normal or Gaussian distribution is a family of continuous probability distributions, extremely useful for describing real-valued random variables as clustering around a central mean value. Its analytical properties make it unique among all available distributions. First, it is very tractable in analytical terms, allowing for the derivation of explicit and relatively straightforward mathematical results. Second, it has a natural and strong relationship with one of the most important foundations of modern probability theory: the central limit theorem. This theorem states that, under some mild conditions, the sum of a large number of independent observations from the same distribution has an approximate normal or Gaussian distribution. This result can be extended to any large number of systems always preserving the expected outcome: a normal or Gaussian distribution.

From a purely methodological perspective the Gaussian distribution was a major plus: it guaranteed parsimony, analytical tractability, convergence to a mean price and strong probabilistic results. Moreover the Gaussian was crucial in the development of a very important piece in the economists' tool-kit: statistical inference. Thus it came as no surprise that economists had bet on it heavily. Unfortunately

—and this was Mandelbrot's strongest point— *it did not fit well with the data* for speculative price variations. The distribution of stock market price variations did not fall as rapidly as predicted by a Gaussian distribution, large variations seemed to cluster together at short intervals, and fat or long tails emerged not as a fluke, but in a systemic way. Mandelbrot had observed this type of pattern in data sets from other disciplines, and had been working on a general hypothesis that would accommodate the ubiquitous presence of phenomena whose statistical distributions seemed to be non-Gaussian. The huge amount of data that Houthakker had collected on historic cotton prices proved to be a bonus for Mandelbrot's intuition.

The hypothesis advanced by Mandelbrot was that speculative price changes follow a stable Lévy-Pareto distribution. It was Mandelbrot who disclosed this connection to the world. Though celebrated in France, Lévy was barely known in the US, and his work on probability theory was not seen as related to the research the Italian economist Pareto (1909) had carried out on income distribution. It was indeed a very improbable connection, as improbable perhaps as the tortuous path taken to this point by Mandelbrot: from his earliest dealings with his uncle Szolem (a mathematics professor) to George K. Zipf, and from the latter to Lévy and Pareto via Houthakker's research on cotton prices and his invitation to give a talk at Harvard. Central to Mandelbrot's alternative model was Lévy's demonstration:

> (…) that the tails of all non-Gaussian stable laws follow an asymptotic form of the law of Pareto (Mandelbrot, 1963b, p. 398).

Applying Lévy's hypothesis, and assuming that —in contradistinction to conventional heuristics— the population moments were infinite, Mandelbrot was able to show that the distribution of cotton price changes followed a stable Pareto distribution. Moreover, he conjectured that since 1816 only scale variations had affected the process underlying cotton price changes in the US. But his application of the hypothesis was not limited to a single product: many other commodities, securities and some interest rates were also covered by the Pareto distribution.

In passing, Mandelbrot reviewed Cootner's (1962) recently introduced theory on stock price changes critically. He found it attractive, but felt again that it was in serious *disagreement with the data*. In what will become a pattern of serious divergence between Mandelbrot and the economists of the "random crowd", both parties valued, in almost opposing ways, the weight of empirical evidence in the choice of theories. While Mandelbrot, following the physics tradition, held agreement with empirical evidence to be a methodological priority, economists tend to undervalue the impact of findings and to develop ways to underplay their importance –mostly through *ad-hoc* procedures. In the same paper, Mandelbrot unveiled some of these maneuvers. His observation is a very early insight into a sequence of *ad-hoc* procedures that would have an important role in the future of financial economics and econometrics:

> One very common approach is to note that, a posteriori, large prices changes are usually traceable to well-determined causes that should be eliminated before one attempts a stochastic model of the remainder. Such preliminary censorship obviously brings any distribution closer to the Gaussian. This is, for example, what happens when one restricts himself to the study of "quiet periods" of price change (Mandelbrot, 1963b, p. 403).

This way of "massaging" or smoothing the data (McCauley, 2004) in order to make them compatible with current analytical tools, and desirable equilibrium properties, is at the core of the equilibrium economics' uneasy relationship with reality and established facts[5].

Mandelbrot's paper got an initially mixed reception from economists, which became altogether hostile as the full implications of his findings began to be confronted by some of the big players in the field. Writing many years later, Mandelbrot himself seemed not to have expected a different, or friendlier, reaction from the "random and financial crowd":

> The results were clear and irrefutable. Far from being well-behaved and normal as the standard theory then predicted, cotton prices jumped wildly around. Their variance, rather than holding steady, as expected, gyrated a hundred-fold and never settled down to a constant value. In the world of finance theory it was a bombshell (Mandelbrot & Hudson, 2004, p. 95).

And a bombshell it certainly was! Perhaps the most significant and telling reaction was Paul Cootner's paragraph, quoted above. Cootner was the editor of one the most influential books in the field, *The Random Character of Stock Prices*, and only second to Samuelson in terms of rhetorical display within the "random crowd". As editor of the book he reacted in the form of a five-page introductory note to Mandelbrot's article, sending a major signal of alert to the whole field. His appraisal of the dangers implicit in Mandelbrot's ideas traced the early lines of a deep divide between Mandelbrot's perspective and the tools preferred by economists.

The problem, according to Cootner, was not confined to modern finance: it would eventually affect all work based upon the econometric methods everyone held dear at the time. No doubt Cootner was acting as the established professor too willing to defend his intellectual capital (Mehrling, 2005) —and, in passing, the social and intellectual capital held by all practitioners of economics. But there was more to his position than this. If all econometric and statistical tools turned out to be useless, what would happen to foundations that underpinned the work of all those engaged in financial economics and in economics in general?

[5] Over the last two decades econometricians and economists, from both the European and Anglo-Saxon traditions (Clements & Hendry, 2008; Hendry, 2009, 2011; Hoover, Johansen & Juselius, 2008; Juselius, 2009, 2009a; Spanos, 1995, 2009) have progressively developed a very solid criticism of the economics mainstream's disregard for established facts, so frequent in the so-called "theory first" approach to econometrics.

FAMA'S DECISIONS AND THE EMERGENCE OF FINANCIAL ECONOMICS

Enter Eugene Fama, an intense[6] young Ph. D. student at the University of Chicago, with a BA from Tufts University and a keen interest in stock market prices. According to Mandelbrot's version of events, Fama contacted him at IBM and at Harvard, "by telephone, mail, and repeated visits" (Mandelbrot & Hudson, 2004, p. 55). Not long afterwards Mandelbrot became his thesis advisor[7]. Although Fama's dissertation was on Mandelbrot's views on market dynamics, the men also discussed the implications of Louis Bachelier's ideas other than the model of independent increments.

> And, in subsequent years, Fama elaborated them into what is called the Efficient Markets Hypothesis. It is the intellectual bedrock on which orthodox financial theory today sits (Mandelbrot & Hudson, 2004).

Mandelbrot's narration downplays —perhaps deliberately— the surprise involved in Fama's drift towards financial orthodoxy, while at the same time giving away the outcome of the whole story in an extremely detached tone —as if it were a non-important consequence of a deeper stream of events. Or: the unavoidable consequence of a process beyond his understanding.

However, in his memoir Mandelbrot (2012) gives a more revealing version of that intellectual exchange:

> This was the same Fama who, in 1964, submitted a thesis subtitled "A Test of Mandelbrot's Stable Paretian Hypothesis". He believed that successive price changes were statistically independent. I had to convince him that I have never claimed independence and that he was in fact testing a much weaker hypothesis. (…) Fama conceded, corrected his early assertions, replaced his mysterious label "martingale" with "efficient market", and built his career on becoming its champion (Mandelbrot, 2012, p. 226).

In his 2007 autobiographical note, Fama (2007) asserts that it was Merton Miller —the creator, together with Franco Modigliani, of the modern theory of corporate finance and an influential leader of the Chicago revolution in Economics— who advised him to pursue the empirical study of fat-tailed distributions of stock price variations as a dissertation topic. Fama followed his advice and very soon he

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6 In his narrative of the emergence, consolidation and fall of financial economics Fox (2009) chose this adjective to describe Fama's behavior in the early 1960s.

7 This fact is disputed by Fama himself. In an interview with coauthor Richard Roll, Fama and Roll teased about the fact that Mandelbrot claimed to be his thesis advisor. After the laughs, Fama went on to say that: 'Anyway two thirds of my thesis was based on his work? (Mandelbrot's). But right away he retold the story (Fama, 2007) about going to Merton Miller with 5 thesis topics, and Miller advising him to choose the one on long-tailed distributions. Of course, Miller was one of the founding fathers of the Chicago school of finance. Mandelbrot was the quintessential outsider.

was working with Mandelbrot. He was a fast worker, and by 1963 he had already published an offshoot of his unpublished Ph.D. thesis in Chicago's *Journal of Business*. By following the behavior of 30 stocks in the New York Stock Exchange Fama was able to uphold Mandelbrot's hypothesis that stock price changes follow a stable Pareto distribution.

At stake was one of the most important foundations of the emerging theory of speculative markets: That prices behave like a random walk. Two assumptions were crucial to the theory: that price changes were independent random variables, and that they followed some kind of probability distribution (Fama, 1963). Fama's wording of the second assumption suggests he had a very open position concerning the exact form of that distribution. Instead of choosing the Gaussian distribution as the natural choice, or as the hypothesis to be maintained, Fama took a different path: he focused his work on the empirical potential of Mandelbrot's hypothesis. He was fully conscious of the theoretical and statistical implications involved in taking such an uncommon path. In the introduction to this 1963 paper he announced:

> We shall see later that, if Mandelbrot's hypothesis is upheld, it will *radically revise* our thinking concerning both the *nature* of speculative markets and the *proper statistical tools* to be used when dealing with speculative prices (*ibid*., my emphasis).

Standard econometric methods did not escape the devastating consequences of upholding Mandelbrot's hypothesis either. Fama suspected that they would have become almost useless had the stable Paretian distributions for stock-market price changes survived strong empirical testing:

> Moreover, other statistical concepts, such as least squares regression, which are based on the assumption of finite variance are also either inappropriate or considerably weakened (*ibid*., p. 421).

Most of the article can be read as a serious effort to uphold Mandelbrot's hypothesis, both statistically and economically. Fama exhaustively checked the stability of Pareto distributions against the influx of new bits of information into speculative markets. He found the stable Pareto distribution to be in better agreement with the data than the Gaussian. At this point it seemed that Mandelbrot's hypothesis was compatible with the way in which new information reflected changes "(i)n the underlying economic conditions that determine equilibrium prices in speculative markets" (*ibid*., p. 426).

The main implication was that in speculative markets, prices go through larger, and more abrupt, changes than they would if they followed a Gaussian distribution. This image of financial markets displaying wild, volatile, behavior was not compatible with the ideal of perfection attributed to financial markets by the emerging "random crowd". Volatility was not only a theoretical or statistical problem; it also had serious practical implications. Here are, in Fama's own words, the unsavory practical consequences of accepting volatility as a permanent feature of financial markets:

> The fact that there are a large number of abrupt changes in a stable Paretian market means, of course, that such a market is inherently more risky for the speculator or investor than a Gaussian market. The variability of a given expected yield is higher in a stable Paretian market than in a Gaussian market, and the probability of large losses is greater (*ibid*., p. 427).

In the context unveiled by Mandelbrot, Houthakker's "stop-loss" orders strategy would become useless as speculators could not protect themselves from large losses by means of automatically selling their positions when losses hit some predetermined bottom line. For Fama himself, strategies devised for smooth markets would thus become useless in a world of wild markets:

> In a Gaussian market if the price change across a long period of time is very large, chances are the total change will be the result of a large number of very small changes. In a market that is stable Paretian (…) however, a large price change across a long interval will more than likely be *the result of a few very large changes that took place during smaller subintervals* (*ibid*., my emphasis).

These practical consequences were incompatible with the underlying ideal of perfection and efficiency normally associated with speculative markets, and with competitive markets in general. It seemed that the empirical findings, extensively confirmed by Fama's testing of Mandelbrot's hypothesis, did *not fit* into the theoretical image of speculative markets envisioned by Fama. At this point, I presume, Fama was at a major theoretical and methodological crossroads. On the one hand, he had confirmed that the stable Paretian hypothesis was more consistent with the data than the Gaussian one but, on the other hand, his empirical findings were in serious contradiction with the image of perfect and efficient financial markets induced by the hard core of equilibrium economics. But that type of impasse was, and is, not unheard of in the often circuitous development of any new theory. It is the type of tension that could have been overcome by classical Kuhnian tenacity or by the rise to dominance of one of the scientific visions competing for paradigmatic status in the budding new field of financial economics. Light at the end of the tunnel would eventually materialize in the guise of new mathematical devices and more general theories.

Unfortunately, the conventional Kuhnian exit was not to succeed in this situation. Contrary to Kuhn's (1962/2012) maxim according to which scientists only try to solve easy enigmas, the riddle of the speculative markets' long-tailed distributions turned out to be a non-enigma. But of course easy enigmas can only occur in a normal science situation, which was not what Fama, Mandelbrot or any member of the entire set of characters involved in this story faced. Their situation was closer to the opening of a new field of research via speciation or evolution: the branching out of a new field from mainstream economics.

Two features made it particularly difficult to succeed under such circumstances. First, at that moment no theoretical and statistical solution to the infinite variance problem associated with stable Lévy distributions existed, either in physics or in economics. Second, neither Mandelbrot's statistical approach nor heuristics were compatible with the analytical and econometric tools valued by economists working in the emerging field of financial economics. Following Kuhn again, only a huge and unprecedented scientific discovery could have propelled Mandelbrot's hypothesis to assume the status of a foundational proposition. But these circumstances did not exist at the time and would only emerge some decades later as a result of the work of a different kind of scientist: the (Econo) physicists who applied statistical physics and complexity to the field of finance.

To his credit, Fama tried to reconcile both sides of the analytical gap, but he failed in his attempt. My conjecture is that he did not see —given the analytical tools available to him and given his scientific beliefs— a feasible theoretical path that would help him solve the riddle.[8] Instead, he might have caught a glimpse of a difficult and costly path to nowhere. Whilst his interest in finding a statistical solution to the infinite variance enigma vanished with time, his empirical work on the efficient market hypothesis grew at an accelerated pace until it gobbled up most of his creative efforts. This can be checked against the sequence of articles that started with the very sympathetic "Mandelbrot and the Stable Paretian Hypothesis" (Fama, 1963)[9], continued with Fama [1965a, 1965b], and closed with the extremely influential "Efficient Capital Markets: A Review of Theory and Empirical Work" (Fama, 1970), after which he appears never again to have considered the Pareto-Lévy distributions to be a relevant issue for financial economics.

Fama is not to be blamed for this: he was a young economist, launching a career in the University of Chicago at a moment in which equilibrium economics was rapidly extending its hard core into new territories. Moreover, before him was the possibility of giving strong equilibrium foundations to stock market dynamics. And even without being able to foresee the glory that awaited him, it was more natural to lay down strong equilibrium foundations for speculative prices variations than to try a

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[8] Incidentally, he was not the only one facing such a situation. Following different paths and strategies Houthakker and Cootner attempted, unsuccessfully, to produce theories of speculative markets that featured both market efficiency and some form of volatility.

[9] How sympathetic Fama was towards Pareto-Lévy distributions in 1965 can easily be verified in the following passage:

> At the moment, the most impressive single piece of evidence is a direct test of the infinite variance hypothesis for the case of cotton prices. Mandelbrot (37, Fig. 2 and pp. 404-71) computed the sample second moments of the first differences of the logs of cotton prices for increasing sample sizes of from 1 to 1,300 observations. He found that the sample moment does not settle down to any limiting value but rather continues to vary in absolutely erratic fashion, precisely as would be expected under his hypothesis (Fama, 1965b, p. 44).

path that not only promised tears and blood, but was also beset with problems that had no foreseeable technical solution in the immediate or even intermediate future.

Second, though Mandelbrot had come up with a model that satisfactorily solved the long-tailed distribution riddle, from the perspective of economic behavior, and from the viewpoint of the budding discipline of financial economics, he did not possess a theory. Worse still, it was very likely that a general solution —if indeed there one were to exist— was not to be found in the world of rational economic agents and efficient markets, but in uncharted territories where statistical tools and solutions went beyond the frontiers of economics and introduced real referents extremely at odds with the current paradigm.[10] Almost two decades later, Mandelbrot himself recognized this: "Nevertheless, the progress of this new economics is slow, due to inherent mathematical difficulties and to my failure to push its development" (Mandelbrot, 1982, p. 20).

Facing such an extremely difficult situation, Fama rhetorically chose the empirical way out. Although all direct tests on 'unprocessed and unsmoothed price data' (Fama, 1963, p. 428) had upheld Mandelbrot's hypothesis, in order to accept it as a general model for speculative prices, "[t]he basis of testing must be broadened to include other speculative prices" (*ibid*.).

His empirical strategy was a way to postpone the decision about Mandelbrot's hypothesis. In order to justify his rhetorical turn, Fama now introduced the "intuitive" dimension, absent up to this point in his argumentation:

> The stable Paretian hypothesis has far-reaching implications. The nature of the hypothesis is such, however, that its acceptability must ultimately depend on its empirical content rather than on its intuitive appeal. The empirical evidence, up to this point, *has* tended to support the hypothesis, but the number of series tested has not been large enough to warrant the conclusion that further tests are unnecessary (*ibid*. Fama's emphasis).

Here it is crucial to understand what Fama had in mind when dealing with empirical evidence. Some years later, with much more statistical evidence on his side, which allowed him to pass more solid judgment on the empirical relevance of the Lévy stable distributions, Fama established once again that his own empirical work, Blume's statistical analysis of common stocks, and research by Roll on U.S. Government Treasury Bills confirmed beyond doubt that Lévy stable distributions fitted the empirical data better than did Gaussian distributions (Fama, 1970, p. 399).

However, his concept of empirical evidence was not consistent with the views of physicists concerning the best agreement between theory and observable data. Fama's vision was more akin to a search for better statistical techniques that would permit sharper hypothesis testing. In fact, for financial economists statistical

[10] As a matter of fact, it took some decades for econophysicists to come up with plausible ways of applying Lévy stable distributions to financial data (Schinckus, 2013).

tests were crucial for the scientific development of their field (Schinckus, 2011). Following the econometric tradition, Fama and fellow financial economists privileged the identification of major, or long-term, trends in financial prices variations and returns. Whatever happened in the tails was not crucial for explaining the major trends in financial price variations. Those were minor events that could be discarded, or smoothed-out, without losing the big picture that theory is trying to apprehend.

For a scientist working in the physics tradition, like Mandelbrot, extreme values are decisive, particularly when their magnitude might account for up to 40% of total variation, and when big changes might accumulate over time, to create extreme events. The physics tradition considers that ignoring extreme values in order to gain in statistical testing precision is poor empirical work and bad scientific heuristics.

This sharp divergence in the meaning and practice of empirical work explains why Fama (1970) saw the growing sophistication of statistical work on distributions, in the context of the efficient market hypothesis, as the most important contribution of Mandelbrot's work to financial economics. It is of course ironic that Mandelbrot's contribution was appreciated precisely for stimulating the type of statistical work he himself judged to be innocuous or inconsequential for understanding processes in the real world.

UNIFYING THE FIELD

But all this was happening when financial economics was an emerging field, still in its infancy, trying to find a place within the expanding domain of equilibrium economics. It is in such a context that Fama's decisions must be understood. A simple Kuhnian reading of that situation would not do, since practitioners of financial economics did not yet have a paradigm that provided them with the exemplary models, technical solutions and statistical techniques that would make their work less prone to risk; nor were they in a situation of crisis, or facing a technical debacle or catastrophe.

As a matter of fact the situation was just the opposite: since everything was still up for grabs, different ideas, tools, techniques and visions were available to practitioners. At the center of everything was the search for a scientific explanation of the behavior of speculative prices. This should be in mathematical form and strongly rooted in statistical theory. At the same time, and this was a crucial point, it should have economic content – meaning a "natural" interpretation in terms of equilibrium and its desirable properties.

Now consider the situation of a promising young economist like Fama, working in Chicago and having in his hands the possibility of laying strong mathematical and statistical foundations for the behavior of stock prices. This was not a crisis situation, but neither did it belong to normal, textbook, science. It was an ambiguous situation in which this young economist could, at the same time, work with the polymath Benoit Mandelbrot on statistical techniques far removed from the economists' toolbox, choose Merton Miller as his thesis advisor, and pursue fundamental work along the path provided by the equilibrium economics framework preferred by Chicago economists. Here is Fama's own recollection of those days:

> After two years of graduate school, I started talking to Merton Miller, Lester Telser, and Benoît Mandelbrot—a frequent visitor at the University of Chicago. They were thrashing around the idea of what prices would look like if markets worked properly. That was my path into research on market efficiency and equilibrium risk–return models (Litterman, 2012, p. 15).

It was this last line of research that eventually gained the upper hand. With the benefit of hindsight it seems easy to state today that there was no other way out. But it did not have to be that way. For the inevitability of the so-called efficient markets hypothesis can only be accepted if one is able to come up with a believable story of the way in which, in the 1960s, economists chose and developed theories in the field of financial economics. When Fama began looking around for empirical, theoretical or intuitive factors in order to justify his decisions, he seems to have followed Friedman's (1953) basic methodological rules: always choose simple, fruitful, and empirically sound hypotheses.

Fama's efficient markets hypothesis was, no doubt, simple: just a string of a few words[11] that condensed the incredible power of markets to produce order even in such speculative venues as stock markets. It would indeed become fruitful in time. Perhaps thousands of papers[12] have been written under its spell. Its wording has been invoked by economists hundreds of thousands of times in order to demonstrate that their statements are rooted in the scientific superiority of the propositions of financial economics rather than the musings of chartists, amateurs and disoriented mathematicians. Thousands of econometric tests have been performed in order to confirm its inner truth.

In a recent interview (Clement, 2007), Fama said that the true contribution of his hypothesis was to establish that the only way to make sense of the idea that prices reflect all available information was through market equilibrium. In other words, without market equilibrium foundations it was impossible to test if indeed prices did condense all the information available to market agents.

Fama's methodological point is important. He is stating that there is no way to perform tests on the power of prices to reflect all information available without a market equilibrium framework, effectively reversing the original sequence of events

[11] Here is Fama's own wording of the efficient market hypothesis:

> The basic wording of it is very simple. It says prices reflect all available information. The conundrum is how to determine whether prices reflect all available information, and you can't do that without a model of market equilibrium. What I added to the story was just pointing out that you need a model of market equilibrium in order to carry out the tests of market efficiency (Fama, 2007).

[12] Justin Fox, with journalistic facility, coined it as "the best proposition of economics" (Fox, 2009, p. 89).

in the understanding of stock-prices variations: in the beginning, empirical work in this area "preceded the development of theory" (Fama, 1970, p. 383), now theory preceded empirical testing of the efficiency proposition. Of course Fama's statement cannot be taken literally. The precedence of empirical work over theoretical developments is a fair description of the willingness of economists to accept the possibility of non-normal stable distributions for price variations in the early 1960s, but it is not strictly equivalent to the total absence of a theoretical bias underlying the work of the early financial economists.

In any case, this key reversal of the original situation reflects in fact the unifying force of Fama's proposition: what had previously been a budding field subject to different, contradictory and centrifugal influences became, as a consequence of Fama's proposition, a *unified field*, ready to develop along a paradigmatic path.

THE RELUCTANCE OF ECONOMISTS

How, then, should Fama's decisions be interpreted? Are there two Eugene F. Famas: the world-acclaimed founding father of modern finance theory and the enthusiastic collaborator and student of Benoit Mandelbrot in the early days of fat-tailed distributions of stock market price changes? It is straightforward to document conclusively the existence of both. But Fama's relationship with Mandelbrot's hypothesis and the emergence of modern finance is not a story that follows the plotlines of a tale of everyday schizophrenia. On the contrary, it is the story of a sequence of decisions taken in the midst of a highly creative period in which everything related to the foundations of the emerging modern approach to finance was up for grabs: heuristics, statistical tools, assumptions, language.

This was fertile terrain indeed for the judicious application of Lakatos' programs of scientific research theory (Lakatos, 1970). The classical Lakatosian themes concerning the futility of empirical evidence, the non-consequentiality of empirical anomalies and refutations, and the irrelevance of competition between individual theories and empirical evidence seemed extremely apposite to the episode under scrutiny. However, in contradistinction to Lakatos, I contend that Fama and his colleagues did not anticipate the principal anomalies and refutations affecting their research program on finance, but simply circumvented them by advancing interpretations that reduced them to mere empirical findings without any theoretical consequence, and by taking the path that gave priority to statistical testing.

Moreover, they ran an inverted test of agreement between data and theory. Instead of testing the extent of agreement between a set of theoretical hypotheses and the data at hand, they did just the opposite: they checked how compatible the newly observed set of distributions was with the ruling theory. They found, of course, that there was no compatibility between Mandelbrot's stable Pareto-Lévy distribution of financial price variations and the tenets of the rising field of financial economics. Mandelbrot himself captured the irony of the whole situation in the following exchange, which starts with his recall of a statement and rhetorical question from his interlocutors and proceeds to refer to his reaction in the first person plural:

> 'Your models look fine, but how do you relate them to economic theory?' In moments of irritation, we are quoted as responding, 'There is, as yet, no explanation of these findings; in fact, no explanation could reasonably be expected to come from existing economic theory. After all this theory has been growing for well over a century and has yet to predict anything' (Mandelbrot, 1989, p. 12).

Underlying Mandelbrot's exasperation with the economists' quips is the natural rhetorical reaction against what a scientist —coming from applied mathematics and the natural sciences— would surely regard as a bizarre methodological procedure: how can science proceed without looking for some type of agreement between theory and reality? What can the role of empirical evidence and reality be in a discipline with such a high disregard for the degree of compatibility of empirical regularities with its own assumptions and beliefs? What role is left, if any, for ontology and reality in the construction of economic knowledge?[13]

This takes us to the problem of how practicing economists take up or reject opportunities in their scientific work. In a context of rapid intellectual growth, anybody would guess that new and exciting empirical regularities, and the statistical methods that come with them, would be welcome as paths towards promising research, and as opportunities for future research and growth of knowledge. In all fairness, this, or something akin to it, is what happened at first. But the initial phase did not last long. Within a couple of years, curiosity and interest vanished, to be replaced by a harsh protectionist reaction. This was no minor decision, since both the opportunity to discover other dimensions of market dynamics and to initiate research into new ways of penetrating to the heart of market dynamics were openly rejected. From an ontological point of view, Mandelbrot's possible alternative world was not accepted into the set of worlds associated with equilibrium economics.

The best evidence with respect to Fama's appraisal of the empirical implications of Mandelbrot's hypothesis, and of its standing vis-à-vis the finance research program, can be found in his classic 1970 article on efficient capital markets (Fama, 1970). There, in a passage already referred to above, Fama directly addresses the question of the empirical soundness of Mandelbrot's hypothesis. The results were heavily favorable to Mandelbrot's findings:

> Drawing on these finding and some empirical work of his own, Mandelbrot [28] then suggested that these departures from normality could be explai-

[13] As noted in footnote 7, above, the situation has become so scandalous that a growing number of economists and econometricians (*e. g*., Hendry, 2009, 2011; Hoover *et al*., 2008; Juselius, 2009, 2009a) are openly voicing their criticism of the current status of reality and ontology in mainstream economics.

> ned by a more general form of the Bachelier model. In particular, if one does not assume that distributions of price changes from transaction to transaction necessarily have finite variances, then the limiting distributions for price changes over longer differencing intervals could be any member of the stable class, which includes the normal as a special case. (...) After extensive testing, Fama concludes that non-normal stable distributions are a better description of distributions of daily returns (...). This conclusion is also supported by the empirical work of Blume on common stocks, and it has been extended to U.S. Government Treasury Bills by Roll (Fama, 1970, p. 399, all citations are Fama's).

But a significant turn of the screw was waiting in the next paragraph. There Fama, the empirical economist, became Fama the theoretical economist, speaking from the point of view of his fellow practitioners:

> Economists have however been reluctant to accept these results, primarily because of *the wealth of statistical techniques available to deal with normal variables and the relative paucity of such techniques for non-normal stable variables* (*ibid*., pp. 399-400, my emphasis).

The key word, of course, is "reluctant". Why were these kind, open-minded, and empirically motivated economists so reluctant to accept Mandelbrot's findings and techniques? Reluctance about Mandelbrot's model and techniques was not a personal trait: it was a group reaction towards his eccentric perspective. The key reason was analytical and econometric convenience worded as 'the wealth of statistical techniques available... and the relative paucity of such techniques for non-normal stable variables'. Why bother then with distributions for which there was no 'wealth of statistical techniques available'?

Yes, all the evidence supported Mandelbrot's findings, but alas, economists were reluctant to integrate them into their practice because there were not as many available statistical techniques and tests to deal with non-Gaussian distributions as there were to deal with Gaussian ones, and the road ahead looked uncertain, bumpy and dangerous. The unavailability of statistical tests that would uphold the soundness of Mandelbrot's Lévy stable distributions was at the heart of the economists' reluctance to invest their best efforts in the theoretical development of his suggestion. Moreover, there was no way of coming up with a solution, since Mandelbrot's view of empirical evidence was radically at odds with the vision and practice of financial economists. Only scientists working within a similar tradition —physicists working on the dynamics of financial markets— could come up with a plausible solution to that problem.

Samuelson's advice to his fellow financial economists about the consequences of not taking Lévy distributions seriously when evaluating empirical time series can be interpreted as illustrating the chasm that separated physicists and financial economists with respect to the meaning of empirical evidence. In a 1973 review article on the mathematics of speculative prices, Samuelson recommended "some reservation":

> Since any member of the Lévy-Pareto stable-additive class satisfies (4.2), and since all the members of this class that lack finite second moments are non-Gaussian, such demonstration is invalid. The recent works of Mandelbrot [61]-[64] and Fama [25]-[31] suggest that the non-Gaussian Lévy distributions, with the so-called kurtosis between the 2 of the Gaussian distribution and the 1 of Cauchy distributions, must be taken seriously in evaluating empirical time series. Thus, *when we supply Bachelier with the regularity conditions, such as a finite second moment, to make his deduction valid, we must do so as a temporary loan and with some reservation* (Samuelson, 1973, p. 7, my emphasis).

But these cautionary words were never to be listened to by modern finance practitioners. Following a classical *ad-hoc* strategy, finance economists went on to build up an elegant and complete theory, in the spirit of efficient markets and mathematical perfection, that managed progressively to incorporate long-tailed distributions and volatility into its domains by means of increasingly costly correcting techniques –the most recent, popular and effective of these being the Arch and Garch estimations (Engle, 2004).

The eventual solution to Mandelbrot's mathematical difficulties involved the creation of a new scientific community and a research program grew out of physics. The plausibility of stable Lévy distribution was successfully demonstrated by physicists who applied the tools of statistical physics and condensed matter to the behavior of financial prices (Schinckus, 2013). But before overcoming the difficulties associated with the stable Lévy distributions, physicists working on financial market dynamics had first to believe that it was important to find a solution to the conundrum left by Mandelbrot: was it possible to preserve the power of the stable Lévy distributions to fit the fat-tails of stock market price variations, without having implausible infinite variances?

The emergence of this riddle as a valid scientific problem for a number of physicists, and its solution within the context of a totally new, and transdisciplinary, scientific community, reflects the full distance separating economists and physicists with regard to their choice of scientific problems and statistical tools. That distance became a deep divide separating physicists and economists with respect to the status of empirical data and reality (Schinckus, 2011). Whereas economists privilege the elegance and simplicity of axioms and propositions and the central role of statistical tests, physicists look for the best agreement available between theoretical predictions and empirical data. For Schinckus it is possible to reach a methodological resolution of the huge methodological divide separating financial economics and Econophysics:

> While financial economics can be seen as an empirical field, Econophysics is better described as an empiricist discipline (Schinckus, 2011, p. 152).

The new transdisciplinary field of Econophysics is reality's way of filling the gap *outside* the discipline of economics. As Econophysics is situated outside economics, and has its own journals, academic programs and rules of engagement, its impact on financial economics is still marginal and inconsequential (Jovanovic & Schinckus, 2013; Schinckus, 2010).

The entire episode described in this article can thus be interpreted as an odd instance of the emergence of new disciplines through speciation and evolution as Kuhn (1962/2012) suggested could occur a long time ago. But the tale has an original twist to it: the new development did not occur within the boundaries of the original discipline, but within the uncertain territory of a new and uncharted, transdisciplinary field.

CONCLUSION

Though empirical testing invariably vindicated Mandelbrot's hypothesis, economists decided against allowing it entry to the foundations of modern finance due to the analytical and statistical incompatibility between Mandelbrot's model and standard statistical techniques in economics. Divergent visions with respect to the role of statistical testing and empirical evidence meant that undisputed facts about leptokurticity and fat-tailed distributions could not be incorporated into the corpus of financial economics. When practitioners judge new paths as uncertain, far too costly or highly eccentric, methodological conservatism has the upper hand and brave new models and tools are not accepted into the canon.

But methodological conservatism is neither a personal trait nor necessarily the pernicious effect of a hidden ideology. It is the outcome of the complex interplay between the survival of successful research programs and the expectations of practitioners facing hard decisions at the frontiers of economic knowledge. In this particular episode the failure to integrate Lévy distributions into the corpus of emerging financial economics was related to the inherent mathematical and ontological difficulties of its application to any field of knowledge, including physics. Only in the early 1990s did a solution emerge within the burgeoning transdisciplinary field of Econophysics, in the form of truncated Lévy flights or distributions (Jovanovic & Schinckus, 2013; Mantegna & Stanley, 1994; Stanley *et al.,* 2002).

Physicists and economists parted ways with respect to methodological rules on the integration of new facts and statistical tools into their scientific activity. The emergence of Econophysics as a transdisciplinary field, and the rise to dominance of the efficient markets hypothesis, reflect the way in which divergent ideas and statistical methods concerning financial economics have grown from a single distant originating episode.

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