When Physics Became Undisciplined: An Essay on Econophysics

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Abstract

In the 1990s, physicists started looking beyond their disciplinary boundaries by using their methods to study various problems usually thrown up by financial economics. This dissertation deals with this extension of physics outside its disciplinary borders. It seeks to determine what sort of discipline econophysics is in relation to physics and to economics, how its emergence was made possible, and what sort of knowledge it produces. Using a variety of evidence including bibliometric analysis Chapter 1 explores the field's disciplinary identity as a branch of physics even though its intellectual heart is better seen as the re-emergence of a 1960s research programme initiated in economics. Chapter 2 is historical: it identifies the key role played by the Santa Fe Institute and its pioneering complexity research in the shaping of methodological horizons of econophysics. These are in turn investigated in Chapter 3, which argues that there are in fact three methodological strands: statistical econophysics, bottom-up agent-based econophysics, and top-down agent-based econophysics. Viewed from a Lakatosian perspective they all share a conceptual hard-core but articulate the protective belt in distinctly different ways. The last and final chapter is devoted to the way econophysicists produce and justify their knowledge. It shows that econophysics operates by proposing empirically adequate analogies between physical and other systems in exactly the ways emphasised by Pierre Duhem. The contrast between such use of analogy in econophysics and modeling practices implemented by financial economics explains why econophysics remains so controversial to economists.

Preface

This dissertation is the result of my own work and includes nothing which is the outcome of work done in collaboration except as declared in the Preface and specified in the text.

Section II in the Chapter 1 is broadly based on a paper I published in the *Journal of the History of Economic Thought*:

- Gingras, Yves, and Christophe Schinckus. 2012. "Institutionalization of Econophysics in the shadow of physics." Journal of the History of Economic Thought 34 (1):109 - 130

Section III in the Chapter 1 is adapted from Chapter 2 of the book by Jovanovic and Schinckus (2017).

- Jovanovic, Franck, and Christophe Schinckus. 2017. *Econophysics and Financial Economics: An Emerging Dialogue*, New York: Oxford University Press.

It is not substantially the same as any that I have submitted, or, is being concurrently submitted for a degree or diploma or other qualification at the University of Cambridge or any other University or similar institution except as declared in the Preface and specified in the text. I further state that no substantial part of my dissertation has already been submitted, or, is being concurrently submitted for any such degree, diploma or other qualification at the University of Cambridge or any other University or similar institution except as declared in the Preface and specified in the text

Acknowledgment

I would like to express my special appreciation and thanks to my advisor Dr Anna Alexandrova who was a tremendous mentor for me. I really thank her for always encouraging my research and for being so patient with me. This dissertation could not have been done without her.

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General introduction

Scientific practices are sometimes messy. Physics is considered to be a branch of science dealing with the nature and properties of matter and energy. However, such a perception has recently been challenged. For the last three decades, physicists have been moving beyond the boundaries of their discipline, using their methods to study various problems instigated by social sciences. This movement was initiated in the 1970s, when certain physicists began publishing articles devoted to the study of social phenomena. While some authors extended what is called "catastrophe theory" to social sciences, others created a new field labelled "sociophysics".[1] In the 1990s physicists turned their attention to economics, particularly financial economics, giving rise to econophysics. Mantegna and Stanley (1999, p.2) defined this new field as "a quantitative approach using ideas, models, conceptual and computational methods of physics". Although the name suggests interdisciplinary research, its foundations are in fact still ill-defined. There is no clear description of the methodological and scientific scope of the field, and its definition remains wide. In this thesis econophysics will be studied through different lenses to clarify its current situation and identify its scientific/methodological foundations. This task is quite difficult, simply because the methods come from physics while the studied phenomena are usually investigated by economists. Furthermore, the advent of econophysics echoes several methodological debates that have appeared in the history of economics and finance. Econophysics generates various questions: is econophysics an extension of financial economics or rather an additional step in the naturalization of modelling in economics? What are the origins of econophysics? How is this new field evolving in the current literature? What is the scientific justification for such an extension of physics in finance? These are, roughly speaking, the questions that structure this research, since they will be investigated individually in the chapters of this dissertation. The literature devoted to econophysics is scattered, and this in-

[1] The number of physicists publishing papers devoted to the analysis of social phenomena and the number of themes studied are increasing nowadays, examples being the formation of social groups (Weidlich, 1971), social mimetism (Callen et al., 1974), industrial strikes (Galam, 1982; Galam et al., 1982), democratic structures (Galam, 1986) and elections (Ferreira et al., 2008).

between situation echoes a number of debates that deserve specific philosophical attention. The aim of this thesis is to instigate this philosophical analysis further.

Econophysics was created outside financial economics by statistical physicists, who study economic phenomena, more specifically financial markets. Indeed, despite this thematic diversification of the literature, the vast majority of the works published by econophysicists have dealt with the dynamics of financial markets.[2] In other words, there are several ways to do econophysics but this dissertation only investigates the one focused on financial markets. Over the past two decades, econophysics has carved out a place in the scientific analysis of financial markets, providing new theoretical models, methods and results. The framework that econophysicists have developed describes the evolution of financial markets in a way that is very different from the current standard approach in finance. Today, although less visible than financial economics, econophysics influences financial markets by proposing new ways of dealing with financial data and therefore with financial management (Jovanovic and Schinckus, 2017).

In contrast to economists who use statistical tests, econophysicists are driven by a more phenomenological method in which visual tests are used to identify the probability distribution that fits the observations. Interestingly, what defines recognition in one community is often without interest in another one, and most econophysicists are unaware that such visual tests are considered to be unscientific in financial economics. Furthermore, the econophysics literature has largely remained silent on the crucial issues of the validation by existing tests of statistical distributions and their use. However, financial economists have developed econometric models, the validation of which required a significant statistical analysis. Such a methodological approach is very different from the one implemented by econophysicists, explaining why the two communities do not interact. This lack of dialogue can be traced to three main causes. The first is reciprocal ignorance, strengthened by some differences in disciplinary language. The second cause is

[2] Mantegna and Stanley (1999) and Jovanovic and Schinckus (2017) explained that this specific situation is mainly due to the computerization of the financial sphere, which literally offered billions of data (therefore facilitating the identification of macro-patterns). See Jovanovic and Schinckus (2017) or Schinckus (2017) for further information.

rooted in the way in which each discipline deals with its own scientific knowledge. The third reason is the lack of a framework that could allow comparisons between the results provided by the models developed in the two disciplines.

Few philosophers of science work on econophysics - one can mention Weatherall (2013), Thebault et al. (2017) or Jhun et al. (2017) – however, these studies either deal with a very specific topic (Thebault et al., 20017 and Jhun et al. 2017) or with a biographic analysis of physicists working in finance (Weatherall, 2013). Rickles (2007, 2008) proposed a disciplinary analysis of econophysics and he paved the way for a deeper philosophical investigation of the field as a new issue in philosophy of science. This thesis aims at following this way and it seeks to make a contribution to the history and philosophy of science by clarifying the methodology and the epistemology of this field in comparison with financial economics. This is a deliberate choice of the author, who is also an economist and therefore cannot discuss the development of econophysics without situating this field in the existing knowledge of financial dynamics. This is a particular feature of this research, which offers a conceptual analysis to gain a better understanding of the disciplinary barriers that currently limit the dialogue between econophysics and financial economics.

The second task of this dissertation is to investigate the distinctive philosophical problems of this very new field. Such contemporary history of a moving, hybrid area of knowledge is challenging but interesting for a philosopher of science. Precisely, the emergence of this in-between field forces us to confront the nature of disciplinarity, methodological disunity, the justification for the emergence of new knowledge, complexity and the nature of asymptotic reasoning. In this thesis, I will discuss the emergence of econophysics by situating this field in the existing knowledge (financial economics) dealing with the phenomena studied by econophysicists.

In terms of methods, econophysics will be analysed from four different perspectives. The first chapter will investigate the disciplinary nature of econophysics using a bibliometric analysis to understand the complex situation of econophysics in the scientific landscape. The second chapter will propose a historical analysis of the context that favoured the emergence of econophysics. Such an approach will clarify the key role played by the Santa Fe Institute (SFI) in the development of econophysics. Precisely, I will present the major computational approaches developed by the SFI members in their works on complexity to emphasize how today econophysics can be seen as a field dealing with agent-modelling (i.e. computerized simulation of systems composed by a large number of interacting components) and statistical patterns (i.e. identification of macro-laws in data). The third chapter will show that the contemporary evolution of econophysics is best understood as progressive methodological diversification. Through an analysis inspired by Imre Lakatos's vision of science, this chapter shows how this fragmented evolution of econophysics actually corresponds to the coherent evolution of the research programme. Finally the fourth chapter will study the modelling practices implemented by econophysicists. By mobilizing the notion of Duhemian analogy, this last chapter will clarify how econophysicists justify their approach and how this contrasts with the modelling practices implemented by economists. Finally, in the light of my analysis, I will conclude by discussing the potential future of econophysics. The following schema sketches out the story to come:

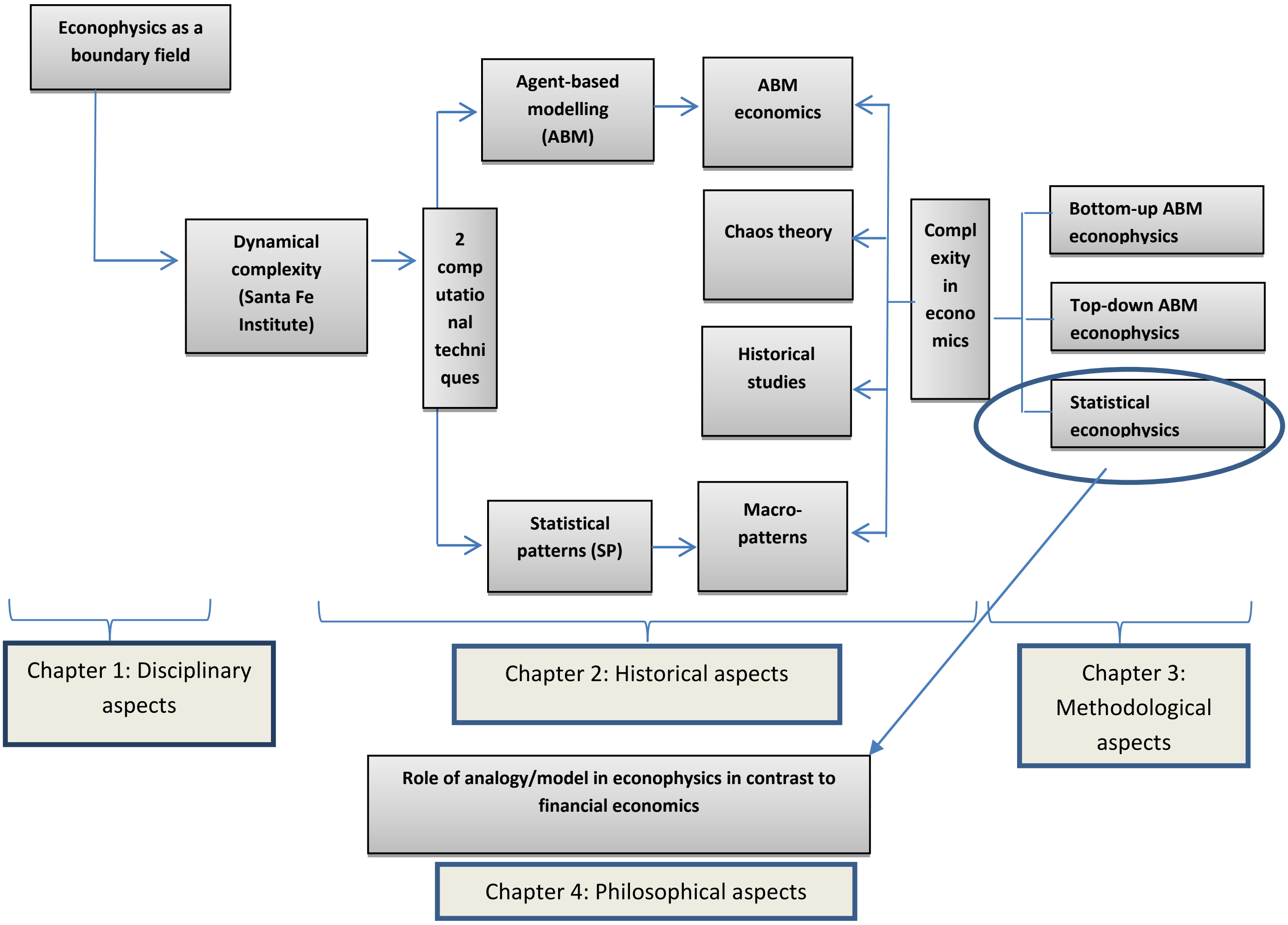

Figure 1: Research process developed in this PhD thesis

Chapter 1: When physicists became undisciplined: The case of econophysics

I. Introduction

Econophysics is a new hybrid discipline (its name resulting from the contraction of "economics" and "physics") finding its methodological origins in statistical physics. The term econophysics was coined in 1996 by Eugene Stanley[3] to describe a specific way of thinking about economic and financial systems by using physical concepts such as statistical regularities, scaling laws, self-organization and emergence. Although econophysics focuses on economic phenomena, there are many methodological and conceptual dissimilarities between the theoretical framework used by economists and the one used by econophysicists. Indeed, these communities employ very different scientific practices and their epistemological foundations are opposed. From this perspective, econophysics is sometimes presented as an autonomous, emerging field that has its own annual conferences[4], its own code (89.65*Gh*) in the "Physics and Astrophysics Classification Scheme" (*PACS* under the code) and its own academic education and PhD[5] (Gingras and Schinckus, 2012). Other works (Jovanovic and Schinckus, 2013 or Mandelbrot et al., 2004) emphasize the historical connections between this field and financial economics by tracing the roots of the former in the emergence of the latter (in the 1960s); and more precisely, in the papers written by Benoît Mandelbrot (1963a, 1966) in financial economics. All these works are enhanced by controversial writings of econophysicists who often tend to exaggerate their contribution to economics and

[3] Eugene Stanley (born in 1941) is an American physicist and professor at Boston University who is well known for his works on statistical physics and on interdisciplinary studies in physics. He coined the name "econophysics", the discipline of which he is said to be the father. He is also the author (with Rosario Mantegna) of the first textbook on econophysics, which was published in 1999 by Cambridge University Press. Eugene Stanley has also been the editor of *Physica A,* a journal that was originally dedicated to condensed-matter physics and which appears to be today the first journal in econophysics.

[4] See <PhysicsWorld.com>.

[5] New PhD programs in econophysics have recently appeared. See <phys.uh.edu/research/econophysics/index.php> and R. Kutner and D. Grech (2008).

finance by claiming they deal with new concepts (such as invariance or power law (see McCauley, 2006)) or stable Lévy processes (Rosser, 2008), which would not be studied in economics[6]. In the light of these debates, the disciplinary nature of econophysics is very difficult to identify and therefore, this chapter aims at further discussion of this dimension. Beyond this discussion, another objective of this chapter is also to introduce econophysics by situating it in a disciplinary space. Is econophysics a subfield of physics or a recent extension of previous works in financial economics? This is the question at the centre of this first chapter.

In Section II, I will use bibliometrics in order to examine the disciplinary space of econophysics, which clearly shows that it can be seen a sub-field of physics, since the vast majority of articles are published in physics journals. Moreover, all conditions related to the perpetuation of knowledge are also controlled by physicists. Therefore, from an institutional perspective, econophysics can be considered as a "unidisciplinary field" (i.e. related to only one scientific discipline).

Section III will propose a detailed historical inquiry showing that this "unidisciplinary" dimension is not totally justified. In particular, many historical similarities (in terms of concepts and practices) will be emphasized between financial economics and econophysics. This historical inquiry is the cornerstone of this first chapter because it calls into question the unidisciplinary dimension of econophysics through the exploration of:

- How this field could be considered as the re-emergence of an old research programme introduced in the 1960s and,
- How actors involved in the emergence of econophysics and that of financial economics adopted the same strategy in order to justify the development in their field.

The re-emergence of this old research programme in the 1990s was favoured, on the one hand by the evolution of knowledge in statistics, and on the other hand by

[6] Since Pareto (1897), stable Lévy processes are well-known by economists (see Gabaix, 1999 for further information).

the increasing computerization of financial data[7]. Interestingly, this re-emergence arose in a different discipline (physics) from where the original programme appeared (financial economics). This specific situation will give me the opportunity to see how the advent of econophysics echoed a dead-end situation that financial economists faced some decades ago.

In the 1960s, financial economics was an emerging field based on a Brownian (and then Gaussian) characterization of uncertainty in which statistical parameters, such as mean and variances, become key concepts. The theoretical foundations of finance were laid by Harry Markowitz[8] (1952) in his portfolio theory, which formally defined the relationship between mean and variance: the first is associated with the expected return, while the latter usually described the financial risk[9]. Although this mean-variance analysis provided an operational framework for financial management, the following sections will review the collection of empirical evidence that shows the occurrence of extreme values that cannot be explained within a Gaussian framework. These empirical contradictions led financial economists to improve their description of empirical data.

The first statistical alternative that financial economists studied is what is called stable Lévy processes. However, despite the existence of empirical evidence that confirms the descriptive power of these processes, their implementation in financial economics generated a problematic issue for three reasons: there is a methodological difficulty in identifying stable Lévy distribution, there is an absence of consensus about the parameterization of stable Lévy processes and, last but not least, the mathematical properties of these processes generate an infinite variance. Many researchers (Godfrey et al., 1964; Officer, 1972) considered the infinite-variance hypothesis unacceptable because it is meaningless within the financial

[7] I will explain in detail the factors that favoured the re-emergence of this research programme in the second chapter of this PhD.
[8] Harry Markowitz (born in 1927) is an economist who won the Nobel Memorial Prize winner in 1990. He is often presented as the founder of financial economics because of his pioneering portfolio theory that was developed in 1952. By offering a statistical translation of risk, return and interaction between securities and diversification, his works laid the foundation for the emergence of today's finance.
[9] This relationship is still the cornerstone of contemporary finance (Bernstein, 1992).

economics framework. In other words, the properties of stable Lévy processes seemed, at that time, to be in opposition with the disciplinary knowledge that ruled finance, which, in the 1960s and 1970s was an emerging field. At that time, theoreticians were focused on the development of a statistical framework[10] that was in line with pre-existing, codified knowledge, which would therefore avoid creating potential puzzles (a potential infinite risk) that could discredit their emerging field.

Surprisingly, econophysics emerged three decades later as the result of studying financial phenomena through stable Lévy processes. Simply said, the evolution of statistics used in physics[11] led physicists to develop analytical solutions for having a finite variance for stable Lévy processes. Afterwards, they decided to apply their solutions to finance. From that perspective, the development of econophysics in the 1990s can be seen as the re-emergence of an old research programme that financial economists had abandoned in the 1970s because at that time it generated puzzles that challenged the codified knowledge in the emerging discipline of finance.

In this PhD I will study the historical, theoretical and contextual reasons for why the field of econophysics emerged in physics and not in economics. Moreover, historical similarities paved the way to an investigation of the disciplinary nature of econophysics, which I discuss in more detail in the last section of this chapter.

As previously mentioned, econophysics is often presented as “an approach somewhere in between economics and physics” (Rosser, 2008). What is the disciplinary nature of econophysics? Is it a new sub-field of physics or perhaps a new interdisciplinary field? The answer is unclear, and these questions still generate many debates in the literature (Sinha et al., 2010). This chapter aims to clarify the situation of econophysics in relation to this issue.

[10] This will be detailed later in this chapter.
[11] As I will explain, this evolution is specific to physics and has no link with the first attempt made by financial economists in the 1960s to describe financial leptokurticity.

II. Disciplinary perspectives on econophysics

As a hybrid field, econophysics involves two areas of knowledge, physics and economics. The disciplinary nature of econophysics therefore appears as a challenging issue. This section further analyses the institutional space in which econophysics emerged by identifying the most important journals that publish papers dedicated to econophysics and by presenting the increasing institutionalization of this new field.

II.1. The Position of Econophysics in the Disciplinary Space[12]

Given that econophysics is based on different fundamental assumptions than mainstream economics, an analysis of the publication venues can give us a good idea of the position of this new field in the space of scientific disciplines. Econophysics is an "outsider" to the discipline of economics, which is well known to have a strong tendency to refer essentially to itself[13]. In this context, one can expect econophysicists to have difficulty publishing their results in the major economics journals.

In order to reconstruct the subfield of econophysics, I started with the group of the most influential authors in econophysics and tracked their papers in the literature using the Web of Science database of Thomson-Reuters (The sample is composed of: Eugene Stanley, Rosario Mantegna, Joseph McCauley, Jean-Pierre Bouchaud, Mauro Gallegati, Benoît Mandelbrot, Didier Sornette, Thomas Lux, Bikas Chakrabarti and Doyne Farmer)[14]. These key authors are often presented as the fathers of econophysics simply because they contributed significantly to its early definition and development. Because of their influential and seminal works, these scholars are actually the most quoted authors in econophysics. Having the 10 highest quoted

[12] This section is broadly based on a paper I published in the *Journal of the History of Economic Thought* (see Gingras and Schinckus, 2012).
[13] Pieters and Baumgartner (2002).
[14] We could have added other names but the objective of this research is to identify the main bibliographic trends in econophysics. Moreover, given the usual practice of citations, we would retrieve other important authors through the analysis of the cited references in these papers as well as in the papers citing those source papers.

fathers of econophysics as a sample sounds an acceptable approach to define bibliometrically the core of econophysics.

I thus identified a group of 242 source papers covering the domain of econophysics and the papers that cite them over the period 1980–2008 to analyse the emergence and early evolution of the field. I started the empirical analysis in the 1980 when the first works combining economics and physics have been published (mainly by the Santa Fe Institute as I will detail it in the second chapter). The objective of this study was to identify the disciplinary origin of econophysics. With this purpose, I fixed 2008 as an endpoint for two reasons: 1) as detailed in Schinckus (2016) econophysics literature gradually became very diversified after 2009 making the identification of the original disciplinary core difficult; and, 2) starting from 2007 econophysicists created new journals in economics (I will discuss this point later in this chapter) leading to a situation that makes difficult a disciplinary analysis.

Starting with these key authors and their papers as the population of analysis, I then identified 1,817 other papers that cited the source articles. The core papers being central to econophysics, I estimated that papers citing them would in all probability also be discussing econophysics. Analyzing all the cited authors in those papers show that indeed, all the usual figures associated with econophysics are well cited[15].

As shown in Table 1, more than 70 percent of the key papers that have been published since 1996 appear in physics journals, while only 21.6% found their place in either economics or finance journals. For the previous period (1980–1995) there were very few papers written by the source authors. They were mainly written by economists and were not really based on a physics approach[16].

[15] I found that the core of the econophysics is essentially composed of five authors: Mantegna, Bouchaud, Mandelbrot, Sornette and Lux, who are by far the most cited authors in our 1,817 papers. All the others are also cited, albeit on a lesser scale.

[16] These papers were mainly written by Thomas Lux and Mauro Gallegati and dealt with macroeconomics (Lux, 1992a, 1992b; Gallegati, 1990, 1994) or history of economics (Gallegati and Dardi, 1992). Let us remember that economists who are interested in econophysics (Lux or Gallegati for example) do not write only papers about econophysics. They have been trained as economists and thus they also write papers about economics (mainly macroeconomics). This shows that the papers about extreme value in finance written by Mandelbrot and Fama in the sixties (I will deal with these papers in the next section) are not the only connection between economics and econophysics. There are also contemporary economists who make connections between these two fields.

The data shown in Table 1 point to a specific trend: papers promoting a physics approach to economics did not find a place in the mainstream of the discipline and moved in the shadow of physics. A reliable measure of rejected submissions is difficult to obtain; however, I will explain that the main actors of econophysics did try to publish in those mainstream economics journals but without much success. This situation is not simply the effect of self-exclusion of econophysicists, but it also reflects a resistance on the part of mainstream economists.

Discipline	**1980–1995**	**%**	**1996–2008**	**%**	**Total**	**%**
Physics	**8**	**32.0%**	**153**	**70.5%**	**161**	**66.5%**
Economics & Finance	**13**	**52.0%**	**47**	**21.6%**	**60**	**24.2%**
Economics	13	52.0%	35	16.1%	48	19.8%
Finance	0	0.0%	12	5.5%	12	5.0%
Mathematics	**0**	**0.0%**	**9**	**4.1%**	**9**	**3.7%**
Other fields	**1**	**16.0%**	**3**	**3.8%**	**4**	**5%**
Total	**25**	**100%**	**217**	**100%**	**242**	**100%**

Table 1: Disciplines in which the source papers have been published (*Web of Science*)

Table 2 shows that one single physics journal, *Physica A,* which is devoted to “statistical mechanics and its applications”, published by far the largest number of econophysics papers, publishing 41% of the total number of papers of the second period (1996–2008). It has thus become the leading journal of this new field, the second being another physics journal, the *European Physical Journal B,* which is devoted to Condensed Matter and Complex Systems.

In Table 2, we see that only 4% of the key papers were published in *Physica A* between 1980 and 1996, when a majority of the papers were still published in economics journals. Taken together, Tables 1 and 2 suggest that the resistance to the ideas of econophysics was such that after 1995, the promoters of econophysics created their own niche outside of economics and finance in order to publish their results. This is consistent with Whitley’s observation that “research which ignores current priorities and approaches and challenges current standards and ideals is unlikely to be published in academic journals of the discipline" (Whitley, 1986, p. 192).

Journals	1980–1995	%	1996–2008	%	Total	%
PHYSICA A	1	4.0%	90	41.5%	91	37.6%
EUROPEAN PHYSICAL JOURNAL B	0	0.0%	27	12.4%	27	11.2%
JOURNAL OF ECONOMIC BEHAVIOR & ORGANIZATION	2	8.0%	9	4.1%	11	4.5%
QUANTITATIVE FINANCE	0	0.0%	10	4.6%	10	4.1%
PHYSICAL REVIEW E	0	0.0%	8	3.7%	8	3.3%

Table 2: Journals where the source papers have been published (*Web of Science*)

Since the appointment of J.B Rosser[17] as editor-in-chief in 2002, the *Journal of Economic Behavior & Organization* has begun publishing regular articles on the issue of complexity in economics, allowing econophysicists to publish their work in that journal. The third journal on the list, *Quantitative Finance* is a relatively new one. As explained below, it was created in 2001 and can be considered one of the first non-physics journals specifically devoted to the new field, as its editorial board includes many econophysicists and the editors include econophysicists (Jean-Philippe Bouchaud and Doyne Farmer) and a mathematician (Michael Dempster).

[17] His research focuses partly on complexity in economics, a topic that may allow him to be more open to the approach proposed by econophysicists.

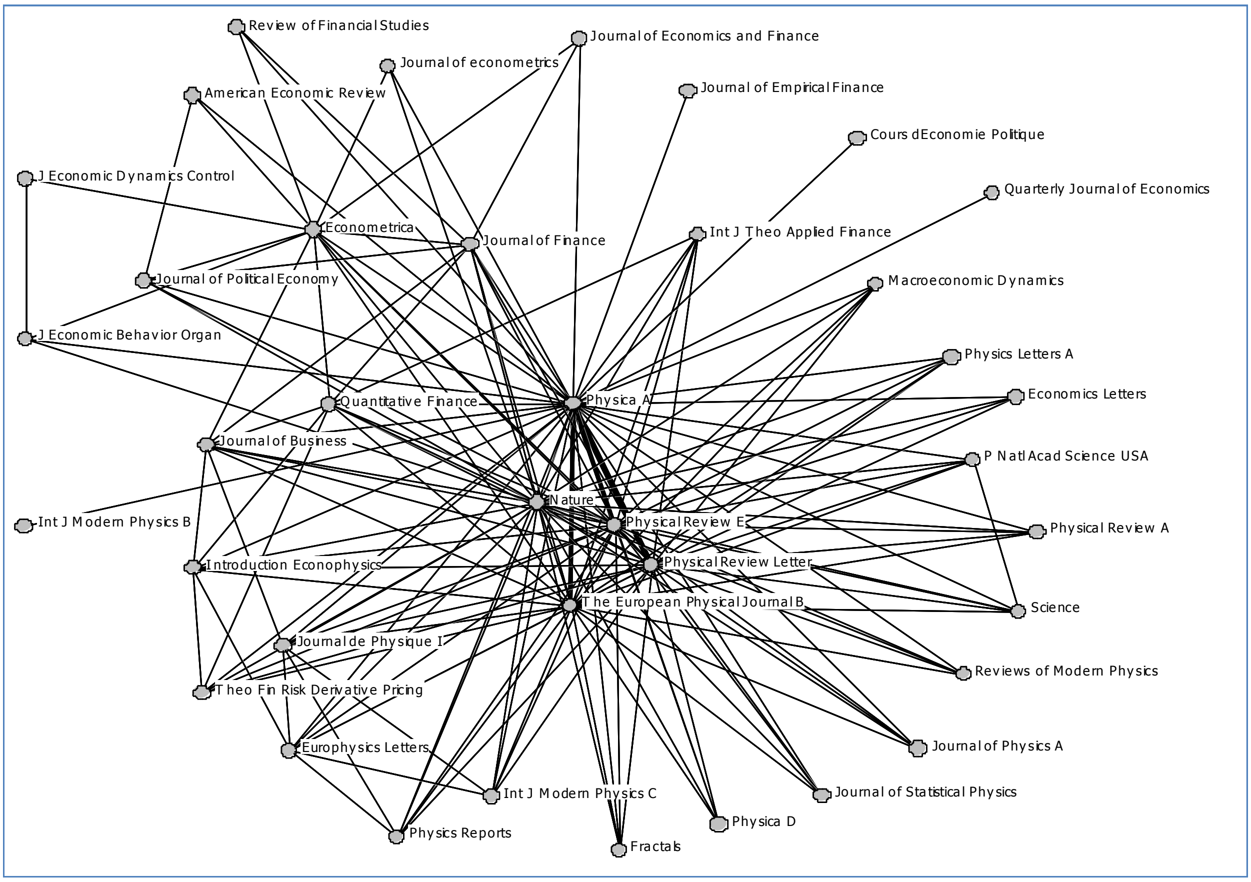

Figure 1: Most co-cited journals (and manuals) in papers citing our 242 source articles in econophysics (100 co-citations +)

The centrality of physics for econophysics is clearly visible in Figure 1, which maps the network of co-citations between journals cited in papers that cite our 242 source papers in econophysics. The dense core of the network is composed of physics journals, while economics and finance journals are peripheral (north-west of the map) and *Quantitative Finance* is in between.

Another way to look at the centrality of physics journals is provided in Table 3, which shows that between 1996 and 2008 only 12% of the citations came from economics or finance journals, even if the explicit topics of the econophysics papers were economic and financial phenomena. Interestingly, this trend was similar in the previous period (1980–1995), even as more than a half of the papers were published in economics and finance journals. Econophysics is thus essentially discussed in physics journals, a result confirmed by Table 4, which shows that, for both periods, about three-quarters of the citations come from papers published in physics journals that are usually devoted to condensed matter and statistical mechanics.

Discipline	1980–1995	%	1996–2008	%	Total	%
Physics	16	76.2%	2,489	76.1%	2,505	76.1%
Economics	2	9.5%	256	7.8%	258	7.8%
Finance	0	0.0%	143	4.4%	143	4.4%
Mathematics	1	4.8%	112	3.4%	113	3.4%
Other fields	1	9.5%	63	8.3%	64	8.3%
Total	**21**	**100%**	**3,272**	**100%**	3,293	100%

Table 3: Disciplines citing the source papers (*Web of Science*)

Journals	1980–1995	%	1996–2008	%	Total	%
PHYSICA A	3	14.3%	1,213	37.1%	1,216	36.9%
EUROPEAN PHYSICAL JOURNAL B	0	0.0%	326	10.0%	326	9.9%
PHYSICAL REVIEW E	2	9.5%	279	8.5%	281	8.5%
INTERNATIONAL JOURNAL OF MODERN PHYSICS C	1	4.8%	143	4.4%	144	4.4%
QUANTITATIVE FINANCE	0	0.0%	110	3.4%	110	3.3%
JOURNAL OF ECONOMIC DYNAMICS & CONTROL	0	0.0%	68	2.1%	68	2.1%
JOURNAL OF ECONOMIC BEHAVIOR & ORGANIZATION	1	4.8%	60	1.8%	61	1.9%
ACTA PHYSICA POLONICA B	0	0.0%	42	1.3%	42	1.3%
PHYSICAL REVIEW LETTERS	1	4.8%	36	1.1%	37	1.1%
CHAOS SOLITONS &	0	0.0%	35	1.1%	35	1.1%

FRACTALS						
JOURNAL OF PHYSICS A-MATHEMATICAL AND GENERAL	1	4.8%	33	1.0%	34	1.0%
MACROECONOMIC DYNAMICS	0	0.0%	33	1.0%	33	1.0%
JOURNAL OF THE KOREAN PHYSICAL SOCIETY	0	0.0%	30	0.9%	30	0.9%
EUROPHYSICS LETTERS	0	0.0%	29	0.9%	29	0.9%
PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCES OF THE UNITED STATES OF AMERICA	0	0.0%	25	0.8%	25	0.8%
ADVANCES IN COMPLEX SYSTEMS	0	0.0%	24	0.7%	24	0.7%
PHYSICS REPORTS-REVIEW SECTION OF PHYSICS LETTERS	0	0.0%	24	0.7%	24	0.7%
COMPUTER PHYSICS COMMUNICATIONS	0	0.0%	20	0.6%	20	0.6%
EPL	0	0.0%	20	0.6%	20	0.6%
INTERNATIONAL JOURNAL OF BIFURCATION AND CHAOS	0	0.0%	20	0.6%	20	0.6%
REPORTS ON PROGRESS IN PHYSICS	0	0.0%	19	0.6%	19	0.6%
INTERNATIONAL	0	0.0%	18	0.6%	18	0.5%

JOURNAL OF MODERN PHYSICS B						
JOURNAL OF STATISTICAL MECHANICS-THEORY AND EXPERIMENT	0	0.0%	15	0.5%	15	0.5%

Table 4: Main Journals citing the source papers (*Web of Science*)

In addition to the two journals already identified as being at the "core" of econophysics, we find *Physical Review E*, the major American physics journal that is devoted to research on "statistical, nonlinear and soft-matter physics". The only economic-related journals citing econophysics are *Quantitative Finance*, *Journal of Economic Dynamics & Control, Journal of Economic Behaviour & Organization* and *Macroeconomic Dynamics*. While the first is managed by econophysicists, the macro dimension of the latter leads its editors to be more open to an econophysics perspective. A special issue entitled "Applications of Statistical Physics in Economics and Finance" published in 2008 by the *Journal of Economic Dynamics and Control* explicitly proposed to "overcome the lack of communication between economists and econophysicists" (Farmer and Lux, 2008, p. 3). Doyne Farmer and Thomas Lux[18] were the guest editors for this special issue in which articles were written by economists and physicists. In order to overcome the gap between the two camps, this special issue offered 12 articles dedicated to econophysics, which were written by authors from economics as well as from physics.

Another journal, *Quantitative Finance,* appears to be the main economics journal that has published many papers devoted to econophysics. Interestingly, in 2008, the most cited journal is *Physica A,* followed by *Quantitative Finance* itself, the *Journal of Economics dynamics & Control* and then by two physics journals (*European Physical Journal B* and *Physical Review E*)[19]. It is worth emphasizing that economics-related journals that cite econophysics cannot really be considered as mainstream journals in economics, but rather as what Backhouse (2004, p. 265) called "orthodox

[18] The first is physicist and the second is economist and both were in our source authors.
[19] The data on the cited journals come from the "Journal of Citation Report 2008" published by Thomson Reuters and part of the Web of knowledge.

dissenter" journals, that is journals that are still rooted in mainstream theory but that are open to other approaches[20]. All this suggests that the really "mainstream" journals are not very open or interested in publishing papers dedicated to econophysics.

The complete absence of mainstream economic journals shown in Table 4 again confirms that, between 1996 and 2008 this discipline was not very influenced by econophysics and did not really acknowledge its existence. In contrast, Table 5 shows that econophysics does depend on economic and finance journals, since nearly half of the total of its citations (46.5%) goes to these disciplines, although physics still remains as an important reference with about a third of the citations going to papers published in physics journals, followed by mathematics journals for about 7% and a tail consisting of many different science journals (13%). During the first period (1980–1995), more than 56 percent of the references cited were from economics or finance journals. We thus observe a decreasing dependence of econophysics on the economics literature and a growing presence of physics journals as a source of knowledge for econophysics, up from 19.2% to 32.6%, which again is consistent with the idea that this field developed essentially outside of the field of economics.

Discipline	**1980–1995**	**%**	**1996–2008**	**%**	**Total**	**%**
Economics	148	50.7%	1,559	26.7%	1,707	27.9%
Finance	20	6.8%	1,162	19.8%	1,182	19.4%
Physics	56	19.2%	1,943	33.3%	1,999	32.6%
Mathematics	21	7.2%	419	7.2%	440	7.2%
Other fields	47	15.9%	752	13%	799	12.9%
Total	292	100%	5,835	100%	6,127	100%

Table 5: Disciplines cited in the source papers (two citations or more) (*Web of Science*)

[20] Following Backhouse (2004, p.265), I distinguish "orthodox dissenters" from "heterodox dissenters"; the latter reject the mainstream theory and aim at deeply changing conventional ideas, while the former are critical but work within mainstream economics.

This trend can also be observed in Table 6, which lists the main journals cited in the source papers. While economics journals (*American Economic Review)* were often cited in the key papers written between 1980 and 1995, physics journals became the main source of knowledge for the papers published after 1996.

Journals	1980–1995	%	1996–2008	%	Total	%
Physica A	3	1.0%	551	9.4%	554	9.0%
The European Physical Journal B	0	0.0%	260	4.5%	260	4.2%
Physical Review E	0	0.0%	196	3.4%	196	3.2%
Quantitative Finance	0	0.0%	179	3.1%	179	2.9%
Physical Review Letter	5	1.7%	162	2.8%	167	2.7%
Nature	2	0.7%	147	2.5%	149	2.4%
Journal of Finance	2	0.7%	128	2.2%	130	2.1%
American Economic Review	18	6.2%	107	1.8%	125	2.0%
International Journal of Theoretical and Applied Finance	0	0.0%	113	1.9%	113	1.8%
Econometrica	7	2.4%	101	1.7%	108	1.8%
International Journal of Modern Physics C	0	0.0%	107	1.8%	107	1.7%
Journal de Physique I	2	0.7%	93	1.6%	95	1.6%
Journal of Business	6	2.1%	85	1.5%	91	1.5%
Journal of Economic Behavior & Organisation	5	1.7%	84	1.4%	89	1.5%
Journal of Political Economy	5	1.7%	73	1.3%	78	1.3%
Quarterly Journal of Economics	10	3.4%	62	1.1%	72	1.2%
Economic Journal	10	3.4%	58	1.0%	68	1.1%

Table 6: Main Journals cited in the source papers (two citations or more) (*Web of Science)*

Taken together, these data confirm that as a field, econophysics is building on the existing institutional structures of physics instead of trying to impose itself onto the existing field of economics. A measure of the rapid growth of that field within physics is provided in Table 7, which shows the evolution of the annual number and proportion of papers devoted to that topic in *Physica* A since 1996.

Year	Number Papers dedicated to econophysics	Total Number of papers published	Proportion devoted to econophysics (%)
1996	1	486	0.2
1997	9	627	1.4
1998	7	582	1.2
1999	29	608	4.7
2000	53	636	8.3
2001	74	646	11.4
2002	44	674	6.5
2003	118	770	15.3
2004	162	853	18.9
2005	112	713	15.7
2006	115	848	13.5
2007	209	1,028	20.3
2008	131	715	18.3
2009	84	558	15

Table 7: Number of papers dedicated to econophysics published in the *Physica A* journal (*Web of Science*)

The trend is clear despite an exceptional year in 2007 when two special issues of the journal were devoted to econophysics. A similar trend (not shown here) is observed in the *European Journal of Physics B*. The growing presence of econophysics in the pages of physics journals has probably contributed to the official recognition of the field by the Physics and Astrophysics Classification Scheme (PACS) and, since 2003, econophysics is an official subcategory of physics under the code *89.65 Gh*.

The openness of physics journals to topics like econophysics contrasts widely with the closure of mainstream economics journals to that topic. Although more research will have to be done on that question, it is probable that this openness of physics to non-physical topics is not unrelated to the fact that model building has become a self-conscious and important part of the practice of physics as compared with the search for new laws[21]. As a consequence, there may have been more sensitivity on the part of physicists to search for new phenomena to be modelled using their tools, in order that such a wide view of their field could open up new job avenues for physicists at a time when the job market was difficult. While many physicists turned towards biology, some, especially statistical and condensed matter physicists, turned to social phenomena under the rubric of "sociophysics" and "econophysics" either in full-time or part-time research programmes, as many were, in fact, working in physics-related departments. It was thus easier to present their work to physics journals as examples of modelling exercises analogous to those found in physics than to try to pass through the gate keepers of economic and financial journals. The difficulty was compounded by the fact, which was already mentioned, that the conceptual foundations behind the mathematical techniques are very different than the ones found in mainstream economics.

In fact, the conceptual and methodological specificity of econophysics is closely linked to the different disciplinary origins of the authors who promote econophysics, as most of them have been trained as physicists and not as economists. This remark is important because although scientific papers appear contextless, they are social constructions that refer to a disciplinary culture whose knowledge is founded on the production, reception and use of texts. In their organization, these texts share assumptions about the types of persuasion that readers will expect

[21] We observe the same phenomenon with economic methodology that has been applied to numerous non-economic situations (related to politics, to military problems, to psychology, etc). However, this extension (sometimes called "economism") does not mean that economists are more open-minded for the importation of a non-economic perspective into economics. In a sense, econophysicists and economists share the same methodological attitude when they mainly work on the export of their knowledge out of their disciplinary borders by staying opposed to a potential importation of concepts from another disciplines (Pieters and Baumgartner, 2002).

(Bazerman, 1988). Economists and econophysicists do not share the same assumptions about readers' expectations: although the empirical dimension is emphasized in both communities, economists focus on an *a priori* approach (axiomatically justified argumentation), while the latter rather develop an *a posteriori perspective* (phenomenological data-driven models)[22]. The two communities have also different practices in terms of editing knowledge: while economists usually wait several months (sometimes several years!) for the finalization of editorial process, physicists consider that, once a paper is accepted for publication, it must be published because its analysis and the data it uses are significant only at the time the research is done and not several months or years after. Eugene Stanley told me, face to face[23], that after more than six years (!) he decided to cancel his submission to the *American Economic Review* (a key journal in economics). He also told me that this significantly long editorial process is one of the reasons why physicists do not want to submit papers in economic journals.

The codified knowledge relating to writing about science also involves conventions used in the organization of publications, allowing for a convenient and intelligible communication. Beyond the different stylistic conventions between economists and econophysicists[24], Bazerman (1988) noticed that economists and physicists tend to present their scientific writing in different way. A common practice in economics is to write an important literature review "demonstrating the incrementalism of this literature" (Bazerman, 1988, p. 274) in order to emphasize the accumulation of knowledge and the ability of authors to work within a pre-existing body of codified knowledge. At the opposite end of the spectrum, physicists focus rather on references that deal with their topic and some potential applications. According to Bazerman (1988), this difference results from cultural beliefs that rule both

[22] I will deal with the role of models in econophysics in Chapter 4.

[23] He invited me to participate to conference he organized about "Complex Systems in Physics" NATO Conference, Samarkand, Uzbekistan, 20–24 May 2013.

[24] Both communities use their own classification scheme: while economists use the JEL (Journal of Economic Literature) classification, physicists organize their knowledge through their PACS ("Physics and Astrophysics Classification Scheme") where econophysics has its own code (89.65*Gh*). Another stylistic difference between these two communities involves the use of reference style: economists usually use the stylistic conventions defined by the University of Chicago Press or the Harvard citation style where references are listed by alphabetical order, while physicists use the conventions used by the American Institute of Physics, where references are listed in the order in which they appear in the text.

communities: while physicists do not doubt the “scientificity” of their approach, economists feel compelled to justify it by making links with existing knowledge.

These dissimilarities between the norms of publication used in economics and those used physics can also explain the reasons why economics journals are less open to the publication of papers related to econophysics, which are mainly formatted using the publication norms used in physics.

This section suggests that econophysics emerged as a sub-field of physics in terms of publications. A complete disciplinary perspective of this new field also requires an analysis of the other ways of crystallizing knowledge, such as conferences, textbooks, or degrees. That is the aim of the following section.

II.2. The Institutionalization of Econophysics

If the nineties saw the emergence and growth of econophysics as a research programme, the next decade witnessed the growing institutionalization of this field. Although papers could be published in existing physics outlets, the specialty needed to develop further through having its own specialized conferences, journals, training programmes and textbooks. Though they do not necessarily appear in that order, I will now consider each of these in turn.

A simple and practical way to spread knowledge relating to econophysics as a new paradigm is to organize workshops and colloquiums. The first conference devoted to econophysics took place in Budapest in 1997 and, unsurprisingly, it was organized by the department of physics of the university. Two years later, the *European Association of Physicists* officially endorsed the first conference on the Application of Physics in Financial Analysis (APFA), which was organized in Dublin. The APFA colloquium was entirely dedicated to econophysics and it was organized annually until 2007. There are now several annual conferences in existence that are dedicated to econophysics, like the *Nikkei Econophysics Research Workshop and Symposium* and the *Econophysics Colloquium*. Combined with publications of papers in specialized journals devoted to the field and textbooks, these events contribute to the stabilization and spread of a common scientific culture among econophysicists. As for scientific societies, one can point to the creation in 2006 of the Society for Economic Science with Heterogeneous Interacting Agents (ESHIA), which aims at promoting interdisciplinary exchanges between economists, physicists and computer scientists (essentially in artificial intelligence), an objective consistent with econophysics. The absence of the label in the name of the organization may be a way of bringing more economists on board by letting their discipline keep its own name instead of being swallowed up by the new term, a gesture that would surely be perceived as hostile and imperialistic.

One can consider *Quantitative Finance*, created in 2001, to be a journal essentially devoted to questions of econophysics (as their editorial boards include many econophysicists) followed by the *Journal of Economic Interaction & Coordination (JEIC*), which started in 2005. As mentioned above, the *Journal of Economic Dynamics and Control* is also open to papers related to econophysics, since they recently published a special issue devoted to this theme.

The first textbook entitled *Introduction to Econophysics* was published in 2000 by Mantegna and Stanley, although several have appeared since (Roehner, 2002 and McCauley, 2004 for example). The publication of textbooks is very important step in the development of a new field because, they “contain highly elaborated models of linguistic forms for students to follow” (Bazerman, 1988, p. 155). Textbooks play a sociological and educational role for neophytes by defining the way of learning and formulating statements appropriate to the community they wish to join. As Figure 1 shows, this first textbook remains the most central to the field. The aim of such textbooks is to define and stabilize the contour of the field as well as its methods, thus helping create a shared culture uniting the members of the new specialty. As such, they constitute an important step in the process of institutionalization of the field. As Jovanovic notes (2008, p. 219):

> “Given that collections of articles are published before textbooks, the interval between the moment when the former were published and the moment when the textbooks were published gives an indication about the evolution of the discipline”.

The swiftness of the development of econophysics can be gauged by noting that it took twice as long (two decades) for the first textbooks to be written that are devoted to another recent specialty: behavioural finance (Schinckus, 2009b).

A last important component of a truly institutionalized research field is the creation of new academic courses and the organization of training for MA and PhD programmes that are uniquely devoted to that field. Here again, the physics discipline serves as the institutional basis and several physics departments have offered courses in econophysics since 2002 (universities of Ulm in Sweden, Fribourg in Switzerland, Munster in Germany, Wroclaw in Poland and Dublin in Ireland). Most of the time, these courses are framed for physicists and focus on statistical physics that are applied to finance. An additional step in the institutionalization of econophysics has been the creation of full academic programmes totally dedicated to econophysics. The first universities to offer complete programmes leading to a diploma were Polish ones; Warsaw proposes a Bachelor and Wroclaw a Master. In 2006, the University of Houston (the US) was the first to coordinate a PhD in econophysics[25] and in 2009, the University of Melbourne (Australia) planned to launch a similar programme[26]. All are situated within physics departments and are therefore physics-orientated. In order to familiarize students with the economic reality they are supposed to describe, these programmes also provide courses on the financial and macroeconomic *reality,* but they are not based on the *theoretical* basis of finance and macroeconomics[27].

All these new academic programmes show that econophysics is developing outside of the disciplines of social sciences economics and is emerging as a new scientific community with its own journals, conferences and training programmes. Since the middle of the 2000s, the conditions for the production of knowledge and the long-term reproduction of the group of econophysicists are thus in place and provide the basis for a sustained growth in the annual number of publications.

From a sociological point of view, econophysics clearly appears to be a sub-field of

[25] See: http://phys.uh.edu/research/econophysics/index.php
[26] http://physics.unimelb.edu.au/Community/Newsroom/News/Econophysics-scholarship-available
[27] For further information on these programs, see Kutner and Grech (2008, p. 644) and the website of these universities See University of Houston (http://phys.uh.edu/research/econophysics/index.php); on the organization of BSc and Master's degrees in econophysics at the university of Warsaw, see Kutner and Grech (2008).

physics, since the production of knowledge and the professionalization processes are controlled by physicists. It is interesting to observe that the development of econophysics in the 1990s coincides with what Kaiser (2012) called the "second bubble of Physics PhDs", which resulted from the 1980 defence policy under the Reagan Administration, which was combined at that time with increasing fears of economic competition with Japan, thereby justifying higher expenditures in bio-tech, engineering and physical sciences. Kaiser (2012) clearly showed this second bubble in the following graph,

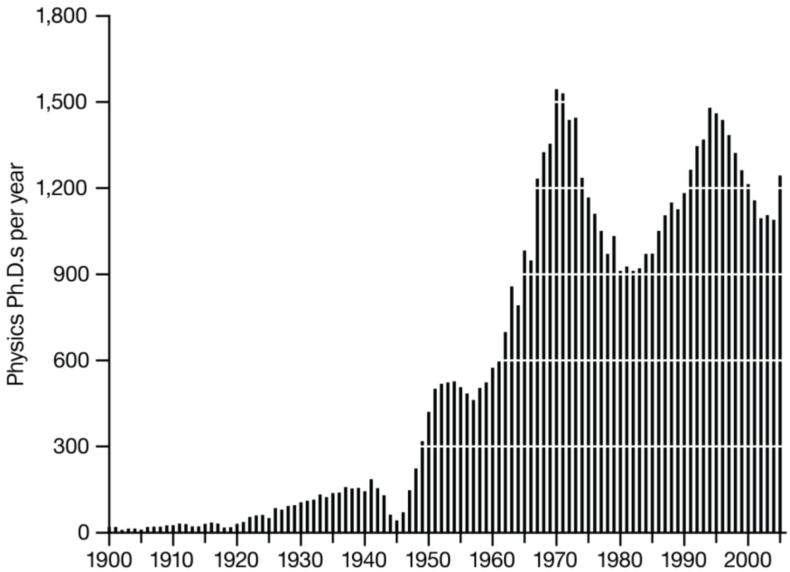

Figure 2: Number of physics PhDs granted by US institutions, 1900–2005. From Kaiser 2012, p. 299)

This rapid rise in funding for young physicists generated a form of "déjà vu" since it looked like the first bubble[28] (which also appears in Fig. 2) that was observed during the "Sputnik era" (in the 1960s), which was justified, at that time, by Cold War rhetoric (Kaiser, 2008). Cassidy (2011) also confirmed this trend, emphasizing that the second bubble was mainly favourable to physicists involved in condensed-matter physics because they "argued that in their field of research the line between fundamental physics and its practical applications was so close that it was often blurred" (Cassidy, 2011, p. 131). This rhetoric was directly in line with the political community's expectations in the 1990s, leading to a higher number of funding

[28] According to Standler (2009), the end of this kind of bubble can partly be explained by a generational shift in the administration: almost retired senior officers who were favourable toward funding a specific scientific research are no longer able to insist on generous financial support of scientific research.

opportunities for projects developed in this area of knowledge. Actually, the author explained that this trend was already observed in the first bubble (25% of physicists at that time worked in condensed-matter physics) but it has been strengthened in the 1990s with the second bubble, during which this area of research became the first choice for the new PhD physicists (in 2000, 41% of doctorates in physics were in condensed-matter physics).

This precision is important from an institutional point of view, not only because econophysics emerged in the 1990s, but also because all founders of this field were involved in condensed-matter physics. It is worth mentioning that it was two big names in condensed-matter physics (Eugene Stanley and Joseph McCauley, based in the US[29]), who promoted the development of econophysics. More precisely, these two founders of econophysics were, at one time or another, head of their department (of physics) and, in the 1990s (and continuing today), they promoted projects related to econophysics[30]. To sum up, the emergence of econophysics can be seen as a side effect of an institutional strategy of funding scientific research.

III. Econophysics and the origins of financial economics[31]

The bibliometric analysis developed in the previous section showed that this new field is controlled by physicists, with its own conferences, textbooks, education, etc. In a sense, econophysics can then be seen as mature sub-field that is able to define institutional norms and pre-given roles that perpetuate the reproduction of knowledge. However, a historical inquiry about concepts used by econophysicists will show that this disciplinary map is not so well defined. As suggested in the first part of this chapter, the disciplinary dimension of econophysics is more complex than it looks because it deals with specific knowledge that was already studied in financial economics in the 1960s. While the previous section showed what econophysics is in terms of existing scientific discipline (i.e. a sub-field of physics), this section will instead study where the concepts used by econophysicists come from. This historical inquiry will focus on a historical analysis of the main concepts used in

published at Cambridge University Press in (2004).

[30] Many econophysicists currently work or have worked in the past with the department of physics of the University of Houston or the Center for Polymer Studies of Boston University.

[31] This section is adapted from Chapter 2 of the book by Jovanovic and Schinckus (2017).

econophysics and see the extent to which these concepts were common in finance in the 1970s.

Since my major argument here will be to show that the advent of econophysics echoed the dead-end situation that financial economists were facing in the 1960s, it is convenient to focus on the history of financial economics that is closely linked with the history of modern probability theory. Moreover, one specific probability distribution plays a key role in the history of the discipline: Gaussian distribution (also known as normal distribution). This distribution underlies the creation of the majority of theories and models from the mainstream: Efficient Market Hypothesis, Modern Portfolio Theory, Capital Asset Pricing Model and the Black-Scholes model. One can therefore consider this distribution as a constituent of financial economics. But econophysics rejects the fact that financial distributions must only be described with a Gaussian distribution[32] and, as I will explain in the third part, this rejection even characterizes the key argument of econophysicists. In this context, this section will first explain the origin of Gaussian distribution in financial economics, what problems financial economists were faced with and how they solved these problems by trying to use stable Lévy processes. I will then explain the reasons why financial economists stopped using these stable Lévy processes. This historical perspective on financial economics will give me the opportunity to question the (uni) disciplinary perspective of econophysics.

III.1. The origins of the Gaussian approach in financial economics

Financial economics is mainly characterized by a high level of mathematization in the modelling of stock-market returns. Modelling stock-market returns or stock-market price variations is the first step in the development of financial models. This is why financial economists have always focused their attention and research on such problems. Stock-price variations and stock-market returns have been successively modelled using a random walk, Brownian motion and a martingale (Stabile, 2005; Poitras, 2006; Poitras et al., 2007; Jovanovic, 2009). Because these mathematical models require a statistical characterization of changes in price or returns, the work

[32] A Gaussian distribution refers to a symmetric statistical distribution characterized by a mean and a standard deviation.

of determining the statistical distribution of returns is a key problem in financial economics and, more generally, in the work of modern financial theory. Indeed, all models in finance assume specific parameters that can be valued through descriptive statistics of historical data supposing to describe the basic statistical coefficients and properties of the data in the study. Basically, the modern financial theory is mainly based on the first (mean) and the second (variance) statistical moments of financial returns; while the mean is usually associated with the expected return, the variance is rather presented as the financial risk[33].

The first statistical representations of variations in the price of financial assets were made on the basis of a Gaussian framework[34] in the 1860s when Jules Regnault (1863), directly influenced by Adolphe Quételet, worked on the application of normal distribution to social phenomena (Jovanovic, 2001, 2006b). Bachelier (1900), whose work was clearly influenced by Regnault's (Jovanovic, 2000, 2012), retained a Gaussian description of the evolution of variation in asset prices[35]. Similarly, all the empirical work that emerged from the 1930s onward (Cowles, 1933; Working, 1934; Cover 1937; Kendall, 1953) used this Gaussian framework because at the time it was difficult to use other kinds of statistical distribution[36]. Indeed, all non-Gaussian observations and "white noise" were characterized through a Gaussian standardization.

[33] By associating the future expected return with the past mean, and by doing the same with the variance, modern financial theory implicitly assumes that the future will be the statistical reflection of the past.
[34] A Gaussian perspective is the framework most used in science to describe random phenomena (Stewart 1992). Two arguments can explain this observation: the simplicity of Gaussian distribution (only two statistical moments, the mean and the variance, are needed in order to describe a random phenomenon) and the statistical foundations of this Gaussian framework, which are directly rooted within the central-limit theorem (Belkacem 1996).
[35] Bachelier needed normal law to demonstrate the equivalence between the results obtained in discrete time and in continuous time.
[36] Although some non-Gaussian distributions (Cauchy or Lévy distributions) existed, no author, except Amoroso (Tusset 2010), used them in a dynamic approach. And we had to wait for developments in modern probability theory in order to be able to use these statistical tools in finance.

This Gaussian description of financial reality progressively crystallized and was reinforced when Paul Samuelson[37] (1965) introduced geometric Brownian motion to describe the continuity of trajectories[38]. Since then, Gaussian distribution of returns on assets has strongly contributed to the development of modern financial theory. Indeed, Markowitz (1952) introduced the portfolio theory, which assumed that individuals will optimize a "mean-variance" strategy for their wealth. Markowitz (1952) showed that this "mean-variance" strategy is directly derived from the expected utility theory which is still a key conceptual framework in economics. Concretely, agents are assumed to maximize the expected return (mean) by minimizing the potential financial risk (variance). This portfolio theory represents the beginning of modern financial economics[39] in which all key models refer to this mean-variance optimization using the Gaussian framework (where the estimation of mean and variance is very convenient).

From Capital Asset Pricing Model (CAPM) and the Black-Scholes model, through to the recent development of Value at Risk (VaR), the Gaussian distribution of return on assets has played a central role in the construction of financial economics (Géman, 2002). However, from the time the first statistical databases of prices were constituted in the early 20th century, some authors[40] noted the occurrence of extreme values in empirical data that cannot be explained within a Gaussian framework. From a statistical point of view, the occurrence of these extreme values is associated with what statisticians call the leptokurticity of empirical distribution. Schematically, leptokurtic distributions (such as the distribution on the dotted line in the figure below) have higher peaks (characterized by long tails on both sides of the mean) around the mean in comparison to the normal distribution (i.e. distribution in plain line on the figure below), which has short statistical tails, as shown below.

[37] Paul Samuelson (1915-2009) was a famous American economist. He was the winner of the first Nobel Memorial Prize in Economic Sciences (1970). He contributed to several areas of economics, including finance, where he published some key articles about the pricing of warrants in the 1960s. He spent a large part of his career at the Massachusetts Institute of Technology.

[38] One of the principal characteristics of Brownian motion is precisely its normal distribution.

[39] See Bernstein (1992, 2007) or Jovanovic (2008) for further details about the beginning of financial economics.

[40] Mitchell (1915) and Mills (1927, chap. 3), who were among the first to collect financial data, stressed this leptokurtic character (i.e. presence of extreme value). Later, starting with the initial work in econometrics, this character was frequently mentioned, as in Kendall (1953) and Cootner (1962). Obviously, none of these authors can be considered as an econophysics.

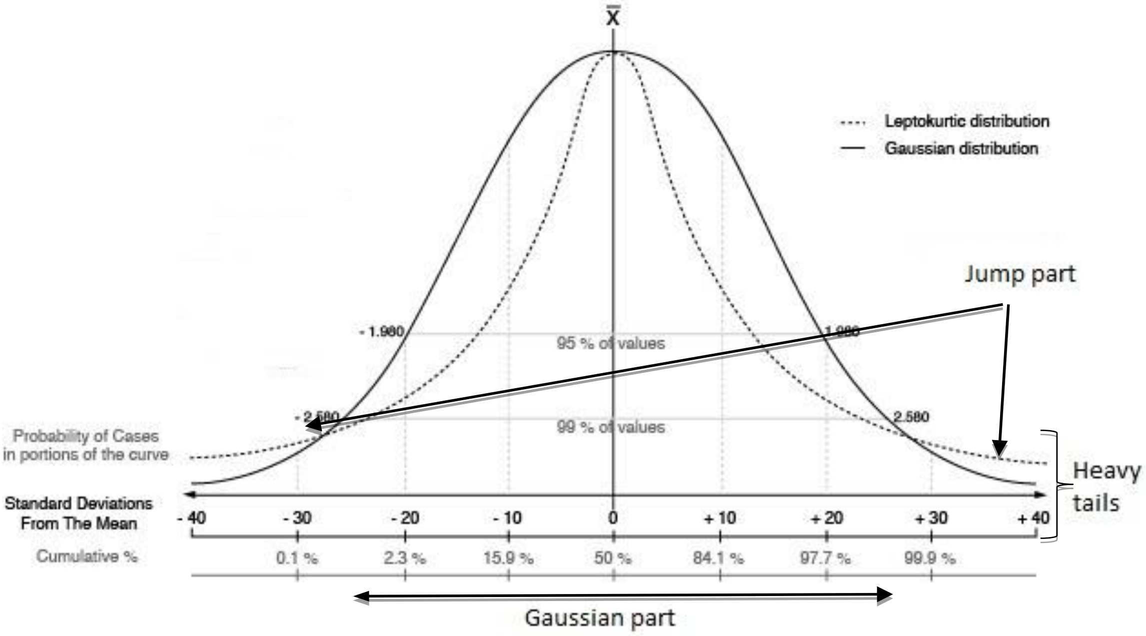

Figure 3: Visual comparison between Gaussian distribution (plain line) and a more leptokurtic distribution (dotted line) for an infinity of observations—Source: Jovanovic and Schinckus (2017, p. 27).

The long tails observed for the leptokurtic distribution (dotted line) refer to the portion of the distribution that has a large number of occurrences that are far from the mean. In other words, a long tail means that we can have more extreme variations. The real challenge is therefore to find the most appropriate statistical framework to describe the leptokurtic dimension of empirical distributions. Although the Gaussian framework has interesting statistical properties, it does not permit a full description of leptokurticity. Indeed, a leptokurtic distribution implies that small changes are less frequent than in a Gaussian distribution, but extreme price moves are more likely to happen and are potentially much larger than in a Gaussian distribution. Consequently, using a stochastic process with a Gaussian distribution does not allow for the reproduction of extreme variations of stock prices. Obviously, it is an important limitation of the Gaussian framework for reproducing stock price variations, and consequently, for analyzing risk.

At that time (in the 1960s), leptokurtic distributions were well known[41] and specialists were able to identify a non-Gaussian phenomenon, but they had no statistical tools for dynamic analysis of observations of this kind (some statistical moments can be undetermined, for example). Non-Gaussian distribution was then only a matter of observation and it was not modelled by a specific statistical framework. This apparent falsification of Gaussian distribution therefore required an improvement of the existing Gaussian framework. I introduce this topic in the following section.

III.2. The first attempt to generalize the Gaussian framework

In the 1960s, Benoît Mandelbrot[42] (1962, 1963, 1965), Paul Samuelson (1965) and Eugene Fama[43] (1965a) proposed studying financial markets using a non-Gaussian statistical framework that was directly inspired by Lévy's work (1924) on the stability of probability distributions and the generalization of the central-limit theorem proposed by Gnedenko and Kolmogorov (1954)[44]. Mandelbrot was the first to attempt to use an extended Gaussian framework in finance. Using two models that he called M1963 and M1965, he paved two new ways of describing empirical observations by focusing on the *stationary* character of these observations[45]. The first makes it possible to take into account observable and apparent cycles on markets, and the second makes apparent the discontinuity of the price of assets on the markets.

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[41] The leptokurtic nature of distribution tails was studied by Vilfredo Pareto (1848-1923) at the beginning of the 20th century when he analyzed the distribution of wealth in Italy. His study informed subsequent research throughout the 20th century (Barbut 2003). See also Tusset (2010).

[42] Benoit Mandelbrot (1924-2010) was a Polish-born, French mathematician who became well known for the development of fractal geometry that he tried to apply to a large variety of phenomena (including finance). In the 1960s, he was the first to use stable Lévy processes to describe the evolution of financial distributions (his doctoral advisor was Paul Lévy). He worked for IBM for more than 35 years and he had many visiting academic positions.

[43] Eugene Fama (born in 1939) is an American economist known for his Efficient Market Theory developed while he was doing his PhD in the 1960s. Fama mainly worked in financial economics and today he is considered a key author in this field. He had a position in finance at the University of Chicago after completing his PhD at the same university.

[44] In accordance with this generalization, the sum of random variables, according to Lévy laws, distributed independently and identically, converge towards a stable Lévy law having the same parameters. This generalization of the central-limit theorem is important because it represents a justification and a strong statistical foundation for the use of Lévy laws to characterize complex phenomena.

[45] *Stationary* means that variations in price remain the same over time and *independent* means that there is no link (no correlation) between variations in position.

Because it directly refers to the statistical framework used by econophysics three decades later, the first model (M1963) proposed by Mandelbrot is very important for my historical inquiry. Basically, Mandelbrot demonstrated how stable Lévy processes[46] can be perceived as a generalization of the Gaussian framework due to a statistical property called stability. This stability feature means that the statistical characteristics do not change with the time horizon[47]. Lévy's stable movements are processes whose accretions are independent and stationary and follow a *α*-stable law of type $P(X > x) = x^{-\alpha}$ in which it is possible to observe constancy of the parameter *α* (between 0 and 2). These laws are usually labelled "power laws" in the scientific literature and they can be visually illustrated as followed:

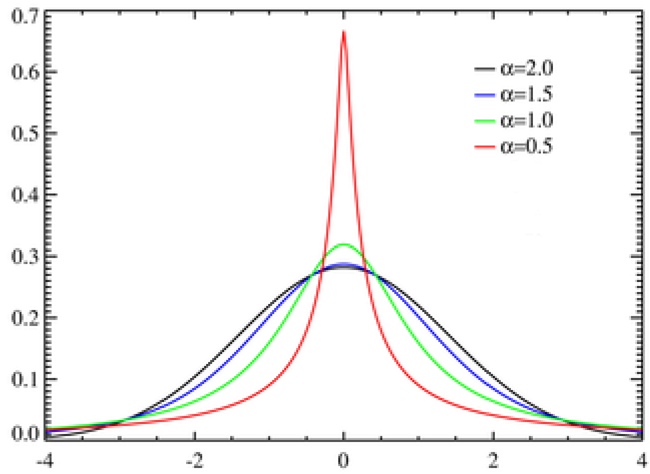

Figure 4: Different values of the characteristic exponent for power laws
Source: Schinckus (2009a)

Figure 4 clearly shows that Gaussian distribution (α = 2) is a specific case of stable Lévy processes. Basically, the parameter α is called the "characteristic exponent": it is an indicator of the leptokurticity of the distribution and it also shows its level of

[46] A Lévy process, named after the French mathematician Paul Lévy, is a time stochastic process with stationary and independent increments, càdlàg paths. In mathematics, a *càdlàg* (French "continu à droite, limite à gauche"), *RCLL* ("right continuous with left limits"), or *corlol* ("continuous on (the) right, limit on (the) left") function is a function defined by the real numbers (or a subset of them) that are right-continuous everywhere and that have left limits everywhere. Càdlàg functions are important in the study of stochastic processes that admit (or even require) jumps, unlike Brownian motion, which has continuous sample paths.
[47] This stability feature can be very important in finance because it suggests that a statistical evaluation of annual data can also be applied to another time horizon such as monthly or weekly.

statistical stability. The value of this exponent determines the shape of the distribution: the lower this exponent, the fatter the tails (extreme events then have a higher probability of occurring). In other words, the lower α is, the more often extreme events are observed. Depending on the value of this parameter, we can find some well-known statistical distributions: with $\alpha = 1$ it is a Cauchy distribution[48] and with $\alpha = 3/2$ it is a Pareto distribution[49]. If we have a $\alpha = 2$ then we find our way back to the famous Gaussian distribution[50]. This statistical parameter is very important because it can be shown that the variance does not exist when $\alpha < 2$ and that the mean does not exist when $\alpha \leq 1$. More generally, the p^{th} moment exists if and only if $p < \alpha$ (Nolan, 2005). Lévy processes, which have $\alpha > 2$, are said to be non-stable (meaning that their statistical characteristics can change with the time horizon).

Over several years, Mandelbrot (1963, 1966) and Fama (1963, 1965) gave empirical evidence of the leptokurticity of financial distributions, thereby justifying the use of stable Lévy processes. Fama (1965) also gave a mathematical reinterpretation of the modern portfolio theory by Markowitz (1952), evoked above, in a Paretian ($\alpha = 3/2$) statistical framework, but he was unable to provide a theoretical interpretation of his work because the parameter of risk (variance) was infinite (Fama, 1965, p. 414)[51]. When Mandelbrot (1962, 1963, 1966) and Fama (1963, 1965) proposed characterizing the uncertainty of the evolution of financial returns by using stable Lévy processes (in their Paretian form), they explicitly proposed to use stable Lévy processes in order to favour the development of more power laws-based finance. In other words, Mandelbrot and Fama promoted the use of stable processes to improve the phenomenological capture of empirical data; however, economists did not further investigate this research path that, interestingly, gave birth to econophysics. Why didn't financial economists work on this way of characterizing data? This is a key question in the historical analysis of concepts related to econophysics, since these

[48] Cauchy distributions are not really used in practical applications because they have an undetermined first statistical moment (mean).

[49] More generally, all Lévy processes with $\alpha < 2$ are said Paretian. Paretian distributions have a finite first statistical moment but an undefined second moment (variance). For a detailed presentation of Paretian distributions, see Schoutens (2003).

[50] For the stable Lévy processes in their Gaussian form, we can have a finite value for mean and variance making possible the mean-variance optimization developed by Markowitz (1952).

[51] As mentioned previously, Paretian distributions have a finite first statistical moment but an undefined second moment (variance), making the mean-variance optimization very complex.

stable Lévy processes play a central role in the emergence of econophysics. The following section will explain the technical and conceptual reasons for why financial economists reject the use of stable Lévy process.

III.3. The rejection of stable Lévy processes in financial economics

Although Mandelbrot (1962, 1963, 1966) and Fama (1963, 1965) showed that stable Lévy processes, in their Paretian form, seem to provide a better empirical description of the evolution of financial prices, these processes have not been used in financial economics[52]. To understand why, we must go back to the 1960s and specifically to the writings of Mandelbrot and Fama on stable processes.

Stable Lévy processes (see the dotted line in Figure 3) have thick tails, which allow them to take into account price variations that are very large in relation to average variations. This essential property enables them to integrate the possibility of price "jumps", but this characteristic, together with the stability of the distribution, means that variance can vary considerably depending on the size of the sample and the observation scale. Consequently, this variance does not tend towards a limit value. The variation is said, therefore, to be *infinite* because it does not tend towards a fixed value[53]. This infinite variance appears to be one of the major reasons for the difficulties of using stable processes in financial economics. Indeed, the infinite-variance hypothesis was meaningless within the financial economics framework. As explained previously, variance and mean are the two major statistical parameters used in modern financial theory, since the first is usually associated with the financial risk while the second is assumed to give the expected return. From this perspective, if variance were infinite (as it is in a stable Lévy process), it would become impossible to understand the notion of risk as Markowitz had defined it and as it was (and is still) used in the key financial models.

[52] There are some timid attempts in the literature. See Geman (2002) for further information about this topic.
[53] See Schoutens (2003) for a technical demonstration. The adjective "indeterminate" would be more accurately employed, but the literature uses "infinite".

In addition to this indeterminacy of variance, financial economists faced another problem: the absence of computational definition (at that time) for evaluating all parameters of stable Lévy processes: the second statistical moments of stable Lévy processes were known to be infinite but no alternative solution for estimating the variability of data existed, meaning that financial economists were at a standoff since they could not use parameters other than the traditional variance. Fama (1965) himself regretted this point, he wrote:

> "Although the model discussed in the previous sections provides a complete theoretical structure for a portfolio model in a stable Paretian market, there are several difficulties involved in applying the model in practical situations" (Fama, 1965, p. 414).

In the conclusion of his article, Fama explained that the next step in the acceptability of stable Lévy processes in financial economics would be "to develop more adequate statistical tools for dealing with stable Paretian distributions" (Fama, 1965, p. 419). A reminder of this statistical problem is found in several essays dedicated to the study of potential alternatives to replace the variance as variability estimator in stable distributions (Fama and Roll, 1968, 1971). In addition, some authors expressed their scepticism about the opportunity to use stable Lévy processes. Officer (1972, p. 811), for example, explained that financial data "have some but not all properties of a stable process" and that several "inconsistencies with the stable hypothesis were also observed". He concluded that the evolution of financial markets could not be described through a stable Lévy process (I will show in the fourth chapter that these debates on the statistical significance of stable Lévy processes are still important today for a potential rapprochement between econophysics and financial economics).

The indetermination of the variance combined with the absence of an established computational alternative for estimating statistical variability led financial economists to stop the development of a stable Lévy processes-based finance. The use of these processes was then progressively abandoned and this point has not been really discussed in the literature, since it implied a new measure of risk (Fama, 1965). Few economists tried to find a solution to this puzzle: Fama and Roll (1968, 1971), Blattberg and Sargent (1971) and Clark (1973) provided some alternative measures of risk but all these potential solutions were not really applicable (Fama, 1976) and their works did not generate further theoretical development. Even Fama (1976) himself preferred to use normal distribution to describe monthly variations, thereby abandoning stable distributions:

> "Statistical tools for handling data from nonnormal stable distributions are primitive relative to the tools that are available to handle data from normal distribution. Moreover, although most of the models of the theory of finance can be developed from the assumption of stable nonnormal return distributions, the exposition is simpler when models are based on the assumption that return distributions are normal. Thus, the cost of rejecting normality for securities returns in favor of stable nonnormal distributions are substantial and it behooves us to investigate the stable nonnormal hypothesis further" (Fama, 1976, p. 26).

In other words, the opportunity costs of using stable Lévy processes were too great at that time. In a sense, this lack of enthusiasm for finding alternative measures of risk was understandable because at that time (i.e. in the beginning of the 1970s), financial economics was a young, emerging field and it was very important for actors to emphasize their ability to provide scientific development about financial reality. Therefore, financial economists did not necessarily want to deal with scientific puzzles that could discredit the scientific reputation of their emerging field. This is what Fama (1976) implicitly meant when he wrote that there was a "substantial cost" (in terms of scientificity) for the field to deal with this puzzle. In a Kuhnian perspective, one could say that the discipline of financial economics was not mature enough to solve the problem of infinite variance. At the opposite end, financial economists were focused on what appeared to be theoretically accepted and well established: the mean-variance optimization developed by Markowitz (1952). Basically, the objective of financial economics in its early time was to develop a theoretical framework related to an area of business (investment) that was totally embedded in complex practices. The portfolio theory (and its mean-variance optimization) was the first theoretical formulation of a very old practice (financial diversification). This theory (and its Gaussian framework) was the bedrock of financial economics[54]. A few years later, the Capital Asset

[54] "Markowitz came along and there was light" (Bernstein, 2007, p. 6). See Frankfurter and McGoun (1996) for the seminal dimension of Markowitz's theory in finance.

Pricing Model (Sharpe, 1964) generalized Markowitz's approach[55] and the Black-Scholes model (1973) contributed to the extension of this mean-variance approach to the pricing of options[56].

Interestingly, discussions about the relevance of stable Lévy processes in finance re-emerged 30 years later. From a historical point of view, econophysics could therefore be looked on as the re-emergence of an old and forgotten research programme developed by Mandelbrot and Fama in the 1960s. This research programme existed in the 1960s and became degenerative in the 1970s due to a decreasing interest caused by a scientific context in which financial economists were not able to give meaning to the infinite variance implied by the use of stable Lévy processes. However, as explained in the previous section, economists acknowledged the high variability of financial data. In this context, a new potential conceptual framework needed to be expressed in terms of this approach which requires the possibility of valuing the mean and the variance (at least the variability) of empirical data. However, stable Lévy processes cannot meet the condition of finite variance, which was even worse, since there was no potential solution (at that time) for evaluating the variability of these processes. In absence of tools to deal with such statistical processes, financial economists simply abandoned research on this topic in the 1970s to focus on processes that met the necessary condition of having a finite variance. Research on extreme values in finance (i.e. leptokurticity of financial distributions) has progressively been transformed and studied through Gaussian compatible approaches. Empirical evidence [57] led financial economists to recognize that the mere Gaussian framework was not sufficient for describing the empirical data. However, the scientific context (emergence of their field) in which these financial economists operated invited them to avoid to work on anomalies to focus rather on an improvement of the existing Gaussian framework, which was (and is still) at the

[55] See McGoun (2004) about the epistemic role played by the CAPM model in the development of financial economics.

[56] See Haug and Taleb (2011) and Millo and Schinckus (2016) about the epistemic role played by the Black-Scholes model in financial economics.

[57] This empirical evidence was provided by Mandelbrot (1963), Fama (1963, 1965), Fama and Roll (1968, 1971), Sargent (1971) and Clark (1973).

core of the field[58]. In this specific situation, financial economists developed two categories of models that are called "jump-diffusion models" and "ARCH-types models". On the one hand, the jump models (Press, 1967; Merton, 1976; Cox and Ross, 1976) describe the leptokurticity of empirical data through a combination of two statistical processes: a Gaussian regime (in order to describe the main trend) and another (not necessarily Gaussian) process that characterizes the occurrence of extreme values (jumps)[59]. On the other hand, the ARCH-types models (Engle, 1982; Bollerslev and Engle, 1986) describe the leptokurticity of empirical data through a Gaussian distribution that is considered as an "unconditional distribution" whose variability can be described with a "conditional distribution" that is derived from historical values of the variance[60].

It is worth emphasizing that these statistical solutions can be looked upon as "corrective tools" or "ad-hoc solutions" to save the Gaussian framework. Econophysicists usually rejected these corrective methods promoting an analysis of data as they are (or appear). In this context, econophysicists do not necessary reject the Gaussian framework (i.e. which is a specific case of stable Levy processes) but they rather consider that this statistical framework cannot characterize the complex reality (the occurrence of extreme value) of economic systems. As the chapter 2 will detail it later, power laws (i.e. stable Levy processes with an exponent higher than 2) became a very common statistical tool in physics to deal with complex systems. In other words, econophysicists do not reject the Gaussian framework but, in line with their background, they rather use a more general formulation to characterize the dynamics of complex economic systems. This alternative path taken by econophysicists will be detailed in the following chapters. This chapter emphasized the historical links between econophysics and financial economics call into question the uni-disciplinary nature of the former. The emergence of econophysics is not totally independent from old research debates that appeared in the 1970s finance. Furthermore, the following section will

[58] The previous section explained why this Gaussian framework was so important to early financial economics. In this scientific context, the improvement of this framework can be seen as a positive heuristic of the field, according to McGoun and Frankfurter (1996).
[59] For further information on this literature, see Cont and Tankov, 2004.
[60] Chapter 4 will return to this in detail and discuss this way of modelling where Gaussian distribution plays the role of an idealization and the ARCH models can be seen as a de-idealization methodology.

emphasize some common practices between economists and econophysicists in the scientific justification of their field.

IV. For a constructivist history of econophysics

The previous section provided a *history of concepts,* showing that econophysics could be looked on as the re-emergence of an old research programme that deals with a particular statistical framework. This historical inquiry of concepts must be completed with a *history of practices* related to the emergence of econophysics. This section will present a more constructivist history by explaining how actors involved with the development of econophysics justified the apparition of this field and, related to the previous section, how this justification echoes the emergence of financial economics itself.

Although they can be considered as an institution dedicated to the production of a specific knowledge, emerging approaches must be studied in the light of disciplinary boundaries from which these fields derived. While the first part of this section will emphasize that econophysics seems to follow a classical model of disciplinary emergence in physics, the second section will show the historical similarities between the emergence of econophysics and the emergence of financial economics. Consequently, the topic (financial data) and the concepts (stable Lévy processes) presented in the previous section are not the only common points shared by econophysicists and the first financial economists.

IV.1. Foundational elements of econophysics

When Morrell (1990) studied the emergence of contemporary disciplines in the first half of the nineteenth century (in Europe and the US), he suggested six significant features of change: 1) an increased number of paid posts for scientific specialists; 2) the rise of specialist qualifications; 3) an increasing number of programmes or training for students; 4) increased specialization of publications; 5) the rise of institutions; 6) the creation of an autonomous reward system for career scientists. Basically, in order for an emerging field to become an autonomous, it would have to meet all these disciplinary requirements.

The section dedicated to the disciplinary analysis of econophysics (section II) showed points 2) (specialization of publications) and 3) (the growing number of programmes for students) through the increasing institutionalization of this field. The others elements were also observed: econophysics can now easily be studied in several prestigious institutes all around the world. The Institute of Theoretical Physics in Zurich, for example, has an important area of research dedicated to econophysics and it regularly enrols PhD and postdoc students in econophysics. In collaboration with the University of Fribourg, this institute launched a virtual interface that regroups all news related to econophysics (http://www.unifr.ch/econophysics/). The Santa Fe Institute (SFI) also dedicated some of its academic resources to the development of econophysics by offering annual fellowships for talented econophysicists[61]. In the same vein as the SFI, the Institute for Advanced Studies in Lucca (Italy) is a new research institution that promotes a multidisciplinary research approach between physics, economics and computer sciences. The prestigious Max Planck Institute (for physics of complex systems) annually offers grants for research proposals in econophysics, while the German Physical Society has introduced the “young scientist award for socio and econophysics” for more than a decade now (starting in 2001). Finally, the new Econophysics Network[62] recently created at the University of Leicester (but moved to King’s College) also offers research opportunities for PhD students and postdocs whose research deals with econophysics.

[61] The Santa Fe Institute is famous for its research on complexity. It played a key role in the combination of econophysics and agent-based modelling. I will deal with this specific point in the second chapter of this PhD.
[62] This network brings together more than 147 leading econophysicists—see: https://econophysicsnetwork.kcl.ac.uk/

These institutes and networks are only a few examples related to the increasing importance of econophysics in the physical sciences. Indeed, in addition to these specific research institutions, several universities provide a specific graduation in econophysics. It is also worth mentioning that all national research funding schemes (in physical sciences) also welcome proposals related to econophysics (see http://www.eps.org/, the website of the European Physical Society). Although all these elements confirm the institutional autonomy of econophysics, I showed in the previous section that this field has several historical links (in terms of concepts) with financial economics. The following sub-sections will complete this historical inquiry by emphasizing the historical similarities between the two fields in terms of practices (i.e. behaviours adapted by actors to justify the emergence of their field).

IV.2 Similarities between the emergence of econophysics and financial economics

Financial economics was born in the 1960s. It took less than one decade for the new discipline's main theoretical results (efficient market theory, option pricing model, CAPM, and modern portfolio theory) to become established, creating what is considered today to be mainstream financial economics[63]. And although several later theoretical movements in financial economics (for example, behavioural finance and microstructure of financial markets) have tried to challenge its pre-eminence, the mainstream approach remains dominant in financial economics[64]. Thirty years later, econophysics was created outside financial economics by physicists coming from statistical physics. Using statistical models (stable Lévy processes) that financial economists did not or could not develop when their discipline was taking shape in the 1960s, econophysicists propose an alternative way of describing financial data (Roehner, 2002; McCauley, 2004).

This section presents the historical similarities in terms of practices between the emergence of financial economics in the 1960s and that of econophysics in the

[63] On the history of mainstream financial economics, see Bernstein (1992), Jovanovic (2008), Melhring (2005), Poitras and Jovanovic (2007, 2010), or Whitley (1986a).

[64] In line with Frickel and Gross (2005, p. 208), the adjective "dominant" is used here to signify a progressive movement that urges a revival of past ideas to push the field forwards in new directions. Dominance must not be associated with the idea of truth but rather with the ability to provide a progressive evolution of knowledge. In our view, econophysics is not truer than financial economics, but interestingly, it offers a specific solution to an old problem in financial economics.

1990s. By means of a comparative analysis, I will then show that the actors involved in the emergence of these two fields used the same methodological arguments to justify the development of their works.

IV.2.a) The same institutionalization strategy

Regarding its institutionalization, econophysics followed a pattern observed during the emergence of financial economics: in both cases, a recognized discipline expanded towards a new field of research whose study had been hitherto dominated by another framework. In the 1960s, economics expanded to the study of financial markets, which at the time was dominated by a practical framework called "chartism"[65]; in the 1990s, statistical physics expanded to the study of financial markets, which at the time were dominated by financial economics. In both cases, the new community was made up of scientists trained outside the discipline, and hence outside the mainstream. A kind of colonization of finance has occurred. This colonization can also be detected in the new arrivals' publication strategy. As shown in section II of this chapter, econophysicists began by publishing in journals of their discipline of origin to make themselves known and disseminate their results—a sort of takeover of recognized scientific journals in the discipline of origin.

In the 1960s, the newcomers took control of the two main journals specializing in finance at the time, the *Journal of Business* and the *Journal of Finance*. The aim was to modify the content of published articles by imposing a more strongly mathematical content and by using a particular structure: presenting the mathematical model and then empirical tests. To reinforce the new orientation, these two journals also published several special issues. Once control over these journals had been established, the newcomers developed their own journals, such as the *Journal of Financial and Quantitative Analysis,* which was created in 1965.

Similarly, econophysicists chose to publish and gain acceptance in journals devoted to an existing theoretical field in physics (statistical physics) rather than create new journals outside an existing scientific space and hence structure. These journals are

[65] Chartism is a financial practice based on the visual observation of the historical evolution of assets' prices. See Schinckus and Christiansen (2012) for an epistemological analysis of this approach.

among the most prestigious in physics (they took control of editorial boards (as in the case of *Physica A* and *The European Journal of Physics B*). This editorial strategy is a result not only of the methodology used by econophysicists (deriving from statistical physics) but also of this new community's hope of gaining recognition from the existing scientific community quickly on the one hand, and to reach a larger audience on the other hand.

The new approaches had no alternative to this "colonization strategy" because partisans of the dominant approach (and hence of the so-called mainstream journals) rejected these new theoretical developments in which they were not yet proficient. Gradual recognition of the new discipline subsequently allowed new specialist journals to be created, such as the *Journal of Financial and Quantitative Analysis* (1965), *Quantitative Finance* (2001) and the *Journal of Economic Interaction & Coordination* (2006), which are officially indexed under human sciences, making it possible to reach a wider readership (especially in economics).

IV.2.b) Same arguments on scientific status

A final similarity between the two fields is the use of the same discourse to justify the scientificity of the new approach. The emergence of both financial economics and econophysics was accompanied by particularly virulent criticism of the existing framework.

In each case, proponents of the new approach challenged the traditional approach by asking its adepts to prove that it was scientific. This "confrontational" attitude is founded upon the challengers' contention that the empirical studies, the new mathematics and methodology they use guarantee a scientific status absent from the traditional approach[66]. The challengers maintain that the scientificity of a theory or a model should determine whether it is adopted or rejected. Consider Fama's three articles (Fama, 1965b, 1965c, 1970). All used the same structure: the first part dealt with theoretical implications of the random walk model and its links with the efficient market hypothesis, while the second part presented empirical results that validate

[66] See, for instance, Lorie (1966, p. 107).

the random walk model. This sequence—theory then empirical results—is today very familiar. It constitutes the hypothetico-deductive method, the scientific method defended in economics since the middle of the twentieth century. Basically, financial economists criticized the existing chartists for their inabilities to present their works with "scientific" arguments, accusing them of using instead a purely rhetorical justification rather than a strong theoretical demonstration of their findings[67].

Financial economists underlined the importance of the empirical dimension of their research from their very first publications (Lorie, 1965, p. 3). They saw the testability of their models and theories as a guarantee of scientificity, and concluded that "The empirical evidence to date provides strong support for the random-walk model" (Fama, 1965c, p. 59). Financial economists then developed a confrontational approach in their opposition to the chartists. As an example, James Lorie (1965, p. 17) taxed the chartists with not taking into account the tools used in a scientific discipline such as economics. Similarly, Fama (1965c, p. 59), Fisher and Lorie (1965, p. 1–2) and Archer (1968, p. 231–232) presented their results as a "challenge" to chartists. In this debate, financial economists argued that their approach was based on scientific criteria, while chartism was based on folklore and had no scientific foundation. Consequently, they believed that financial economics should supplant previous folkloric practices. Cootner's book (1964) was one of the first publications used by the proponents of financial economics to define the discipline. In his introduction, Cootner asserted that:

> "Academic studies have proven to be more sceptical 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" (Cootner, 1964, p. 1).

Cootner (1964) presented the first studies of the financial economists he discussed as the first scientific approach to stock-market variations, which would supplant previous practices, which were judged to be groundless. The method employed and the empirical tests of hypotheses were also presented as a guarantee of the scientificity of the results.

[67] In epistemological terms, this opposition between early financial economists and chartists shaded the classical opposition between deduction (used by financial economists) and induction (used by chartists) (Jovanovic, 2008).

Fama (1965c, p. 59) and James Lorie (Lorie, 1966, p. 110), two other emblematic figures in financial economics, denigrated traditional approaches in a similar manner. Hoffland (1967, p. 85–88) 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' […]".

What is interesting with the emergence of econophysics is that its scholars have proceeded in a similar fashion. In their work, they belittle the methodological framework of financial economics using similar vocabulary. They describe the theoretical developments of financial economics as "inconsistent […] and appalling" (Stanley, et al., 1999, p. 288). Despite his being an economist[68], Keen (2003, p. 109) discredits financial economics by highlighting the "superficially appealing" character of its key concepts or by comparing it to any "tapestry of beliefs" (Keen, 2003, p. 108). Marsili and Zhang (1998, p. 51) describe financial economics as "anti-empirical", while McCauley does not shrink from comparing the scientific value of the models of financial economics to that of cartoons:

> "The multitude of graphs presented without error bars in current economics texts are not better than cartoons, because they are not based on real empirical data, only on falsified neo-classical expectations". (McCauley, 2006, p. 17)

The vocabulary used is designed to leave the reader with no doubt: "scientific", "folklore", "deplorable", "superficial", "sceptical", "superstition", "mystic" and "challenge." All these wrangling words seem to dramatize the situation in which actors simply hold divergent positions. Econophysicists claim that their approach is more neutral (i.e. not based on an *a priori* model) with regard to the study of chance. They explicitly demonstrate a willingness to develop models that are, on the one hand, more coherent from a physics point of view, and on the other hand based on "raw observations"[69] of economic systems (Stanley, Gabaix, et al., 2008). By

[68] With Rosser (2006, 2008a), Keen is one of the rare breed of economists who have engaged with econophysicists.

[69] By "raw observations", econophysicists mean non-normalized data. Economics (and econometrics) is mainly based on a Gaussian framework and when economists (econometricians) observe abnormal data (by abnormal data, I mean statistically unusual from a Gaussian point of view), they normalize these data. They use data mining in order to consider that all abnormal data have an expected mean equal to zero. Econophysicists consider this normalization as a priori reasoning about the economic

"physically realistic models", the authors mean that econophysicists need to be able to give a physical meaning to the statistical parameters they use[70].

The approach used by econophysicists is then presented as more robust and more scientific than the empirical studies carried out in financial economics (Stanley*,* et al*.,* 2008, p. 3) and, in addition, "a claim often made by econophysicists is that their models are more realistic than those offered up by economists and econometricians" (Stanley et al., 2008, p. 3) whose fundamental concepts are "empirically and logically" (Keen, 2003, p. 108) erroneous, implying that a new, more "realistic" form of modelling needs to be developed. Here, the term realistic must be understood, according to econophysicists, as a way of describing the "true relationship governing changes in financial quotations"[71]. This empiricist perspective is very marked for econophysicists, who regularly point out that the empirical dimension is central to their work. Thus, although the "empirical data" are the same for financial economists and for physicists (financial quotations in the form of temporal series), physicists are quick to point to their "direct use of raw data," thereby criticizing the use of statistical transformations performed by financial economists to "normalize" data. Here is Mandelbrot on this point:

> "The Gaussian framework being a statistician's best friend, often, when he must process data that are obviously not normal, he begins by "normalizing" them […] in the same way, it has been very seriously suggested to me that I normalize price changes. I believe, quite to the contrary, that the long tails of histograms of price changes contain considerable amounts of information, and that there are a number of cogent reasons for tackling them head-on." (Mandelbrot, 1997, p. 142).

McCauley directly attacks this practice used by financial economists, explaining:

> "We [econophysicists] have no mathematical model in mind *a priori*. We do not 'massage' the data. Data massaging is both dangerous and misleading [...] Economists assume a preconceived model with several unknown parameters and then try to force-fit the model to a nonstationary time series by a 'best choice of parameters' " (McCauley, 2006, p. 8).

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phenomena that they study. Econophysicists claim there is no "abnormal data" but only data about the reality. See Schinckus (2010b) for further information about this point.

[70] That is, that accord with the theoretical principles of modelling in statistical physics—the fact, for example, that in the analysis of stationary physical systems, variance must always be finite, in accordance with the thermodynamic hypotheses (concerning the concept of heat).

[71] Although they are mainly focused on instrumental prediction, econophysicists often claim they deal with essential relationships existing in financial phenomena (McCauley, 2004).

This methodological position is widespread among econophysicists, who work in the spirit of experimental physics rather than with the standard methods of economics. This empirical perspective is also justified, in the view of econophysicists, by the evolution of financial reality. The computerization of financial markets has led to better quantification of the financial reality, which should now be studied as an "empirical science" (Bouchaud, 2002; McCauley, 2004). This radical viewpoint espoused by some econophysicists has an element of naivety. Indeed, in a sense, any sampling method is a massaging of data. Nevertheless, this viewpoint has led econophysicists to a better consideration of extreme values, while such values are considered as errors by the majority of financial economists[72].

These historical similarities between econophysics and financial economics suggest challenge the idea that econophysics is a mere sub-field of physics. However, despite the existence of historical similarities between the two fields in terms of practices, one can observe a clear difference: while financial economists in the 1960s took over the business schools by marginalizing the rival groups (Jovanovic, 2008); econophysicists do not try to take the place of financial economics; rather they have tried to carve out a place for themselves in finance from outside. In their attempts, econophysicists emphasize their potential contributions to finance mainly by claiming that their works can improve the modelling of uncertainty[73].

V. Discussion

An institutional analysis of econophysics showed which specialists control the production of knowledge and presented econophysics as a sub-field of physics with

[72] The way CRSP database was created provides a good example of *a priorism* from financial economists: "Rather than coding and punching all prices twice and then resolving discrepancies manually, we found a better procedure. We know that the change in the price of a stock during one month is very nearly independent of its change during the next month. Therefore, if a price changes a large amount from one date to a second date, and by a similar amount in the opposite direction from the second date to a third, there is a reason to believe that at the second date the price was misrecorded. A 'large change' was rather arbitrarily taken to mean a change in magnitude of more than 10 per cent of the previous price plus a dollar" (Lorie 1965, p. 7).

[73] See Schinckus (2009, 2011) for an analysis of the potential contributions of econophysics to financial economics.

its own channels to reproduce knowledge. This well-defined disciplinary perspective has then been nuanced through a more conceptual inquiry that emphasizes the great number of historical similarities between econophysics and financial economics. Precisely, the emergence of this first echoes the historical/conceptual debates that emerged in the financial economics of the 1970s. In the light of the two perspectives presented in the previous sections, the disciplinary nature of econophysics requires a deeper analysis. We know that econophysics is more than a mere sub-field of physics, but what kind of approach is it then? Is econophysics a telling example of interdisciplinarity? The purpose of this section is to further discuss this question.

Econophysics is an in-between field that deals with tools that come from one area of knowledge and topics that belong to another one. In this context, this field that appears can be looked on as a form of "pidgin", a "trading zone" or even as a new microcosm between two scientific tribes (financial economists and physicists). The anthropological notion of pidgin usually refers to an interim language, based on partial agreement on the meaning of shared terms. This concept of pidgin was introduced in science by Galison (1997), who called the Kuhnian incommensurability into question by explaining how people from different social groups can communicate[74]. Pidgin can be seen as a means of communication between two (or more) groups that do not have a shared language[75]. Galison (1997, p. 783) also used the metaphor of "trading zone" (because in situations of trade, groups speak languages other than that of their home country) to characterize this process of communication between people who do not share the same language. More specifically, "two groups can agree on rules of exchange even if they ascribe utterly different significance to the objects being exchanged" (Galison 1997, p. 783). The concept of pidgin shows the moving boundaries of scientific discipline by opening the way to the emergence of a new scientific community, which anthropologists call a microcosm.

[74] Galison (1997) explained how engineers collaborated with physicists in order to develop particle detectors and radar.

[75] The Creole language is often presented as an example of pidgin because it results from a mix of regional languages (Chavacano from the Philippines, Krio from Sierra Leone and Tok from Papua New Guinea); see Todd (1990).

The idea of econophysics as a "microcosm" that emerged between two different scientific communities is particularly interesting because physics and economics appear to be two self-referential disciplines. Indeed, according to the *Science & Engineering Indicator*s (2000, tables 6–54, p. 103), economics is the most hermetic field of the social sciences[76], with more than 87 percent of references being intra-disciplinary, compared to 50 percent in sociology. It is even more self-contained than physics, in which authors cite physics journals in about 80 percent of their references. On this point, Shumway and Messer-Davidow (1991, p. 209) wrote:

> "Physics and economics serve as instances of internally convergent fields that maintain uniform ideas, methods, and standards while geography and literary studies are often cited as examples of internally divergent fields that readily absorb ideas and techniques from neighboring intellectual territories".

This scientific homogeneity associated with economics and physics is very often emphasized in the literature, which makes the emergence of econophysics both troubling and very interesting from a historical and philosophical point of view. Econophysicists consider themselves as physicists, the disciplinary identity of which can be found in the first definition given to this new field: econophysics is a "field […] that denotes the activities of physicists who are working on economic problems to test a variety of new conceptual approaches from the physical sciences" (Mantegna and Stanley, 1999, viii-ix). However, from anthropological point of view, this definition appears to be tribal because it implicitly implies a "knowledge territory" that is defended by physicists. If physics can legitimately be considered as the purview of physicists, why should econophysics be seen as a "reserved area" for physicists? At the opposite end of the spectrum, economists also tend "to protect their territory": I showed in the section dedicated to the institutionalization of econophysics that economic journals are really not open to the publication of articles on econophysics. This analysis is directly in line with Whitley's (1986) characterization of economics as a "partitioned bureaucracy" that has strong control over its theoretical core.

[76] Pieters and Baumgartner (2002) explored intra- and inter-disciplinary communication of economics journals by means of citations analysis. They showed that the first-tier of economics journals did not cite articles published in journals of management, marketing, anthropology or psychology between 1995 and 1997.

In this well-defined disciplinary context, how can the emergence of a boundary field between economics and physics be explained? Beyond the institutional frontiers and protective strategies developed by actors, a historical inquiry of concepts and practices shows that econophysics is not such a well-defined disciplinary field. Basically, econophysics, by definition, requires a multidisciplinary approach since it refers to a carrying over of words that come from physics to a new object of reference that belongs to financial economists. In other terms, econophysicists assume they can translate a specific reality usually studied by another scientific tribe into their own language. Consequently, econophysics implies the creation of meaning through translation between two linguistic communities[77].

The cultural dimension directly influences the cognitive aspects of disciplines (and therefore education and research) since culture is a set of mental constructs that may serve to tell people how to know and to use things (Bailey, 1992). However, this academic tribalism, which was emphasized in the previous sections, does not make impossible the exchanges between communities, as Bailey (1977) explained:

> "Each tribe has a name and a territory, settles it own affairs, goes to war with others, has a distinct language or at least a distinct dialect and a variety of symbolic ways of demonstrating its apartness from others. Nevertheless the whole set of tribes possess a common culture: their ways of constructing the world and the people who live in it are sufficiently similar for them to be able to understand, more or less, each other's culture and even, when necessary, to communicate with members of other tribes. Universities possess a single culture which directs interaction between the many distinct and often mutually hostile groups" (Bailey, 1977, p. 35).

This cultural possibility for scientists to interact often generates the development of subdisciplinary fields: "Below the level of the discipline, there remains the important category of subdisciplinary specialisms, with their own more closely-knit but constantly shifting communities" (Becher, 1994, p. 152). In a sense, econophysics (and the debates it generated) has resulted from this process evoked by Bailey (1977), which led to the development of this new subdisciplinary field located between two recognized disciplines (economics and physics). This in-between

[77] This translation refers to the use of metaphor and analogy in sciences. I will study further this important aspect in Chapter 4.

situation is favourable to the emergence of what anthropologists call “pidgin”, which I defined earlier. The development of such an interim language can actually be favoured through the existing relationships that exist between econophysicists and economists: I mentioned earlier that collaborations are starting to arise between physicists and economists, which show that these two communities have a will to communicate with each other. As Farmer and Lux (2008, p. 6) wrote:

> “We hope that this selection of papers offers an impression of the scope and breadth of the growing literature in the interface between economics/finance and physics, that it will help readers to get acquainted with these new approaches and that it will stimulate further collaborations between scientists of both disciplines”.

More recently, one can observe some room for additional collaboration between economists and econophysicists. For instance, the *International Review of Financial Analysis* (a good journal in financial economics) recently proposed two special issues devoted to econophysics (Li and Chen, 2012 and McCauley et al., 2016). It is also important to emphasize that at the next American Finance Association, the first session dedicated to econophysics has been organized[78].

In this respect, all the conditions seem to have been met for the emergence of a new pidgin language, since regular contact between the language communities involved and the will to communicate are the required conditions for the emergence of a pidgin language (Chrisman, 1999). Pidgin requires the emergence of a common (interim) language that is founded on a partial agreement between the involved factions (Klein, 1994). This language implies a common conceptual scheme that results from a double movement: models from physics must incorporate the theoretical framework from financial economics and, at the same time, theories and concepts from financial economics must be modified so that they encompass the richer models from physics. This double movement is a necessary step towards a more integrative econophysics. This adaptation also implies the integration of theoretical constraints observed in each discipline in such a way that the new shared conceptual framework would make sense in each discipline. This issue is not without

[78] The American Finance Association is the major academic event for the mainstream financial discipline. For more information, see: https://www.aeaweb.org/conference/2018/preliminary/1721?q=eNqrVipOLS7OzM8LqSxIVbKqhnGVrJQMlWp1lBKLi_OTgRwlHaWS1KJcXAgrJbESKpSZmwphFSSmg1gWSrVcMEShGEU,

problems. As Morin (1994) explains, “the big problem is to find the difficult path of the inter-relationship [*l’entre-articulation*] between sciences that have not only their own language, but basic concepts that cannot move from one language to another”. A telling example of such a problem is the misunderstanding between economists and physicists about the use of stable Lévy processes: Both communities claim to be the first to have used these processes and they do not even consider what the other community is doing. However, economists and physicists use these processes in very different ways: the former use a combination of distributions (Gaussian combined with another one) to characterize the occurrence of extreme values in financial distributions, whereas the latter use only one distribution to describe the evolution of financial data.

The creation of a pidgin discipline implies a more integrative approach in which econophysicists and financial economists would share a common conceptual scheme that transcends both disciplines. This “integrative dimension” refers to two kinds of integration: on the one hand, a methodological integration to produce a common conceptual framework and, on the other hand, a sociological integration—meaning that theorists from the disciplines involved move beyond their cultural differences in order to work together on a common project. The sociological integration is a matter of “inter-professionality” related to the standardization of knowledge “through the background” (D'Amour and Oandasan, 2005) while the methodological integration refers to the knowledge itself. This sociological integration seems difficult given the strong disciplinary control observed in economics and physics (Pieters and Baumgartner, 2002). Although this trend is still in its infancy, we can observe an increasing number of collaborations between econophysicists and economists; these works take the form of special issues published in economic journals.

This potential integrative situation is still very new in the literature[79]. Econophysicists and economists accept that communicating and collaborating with these types of interactions might lead to the creation of a new conceptual jargon that would be understood by both communities. Chrisman (1999, p. 5) explained how the

[79] A telling example of such research is Jovanovic and Schinckus (2017).

elaboration of new transdisciplinary jargon between two disciplines can be associated with the emergence of pidgin. Two examples can be mentioned here to illustrate the potential emergence of pidgin: the truncation technique for stable Lévy processes and the asymmetric treatment of random matrixes.

In financial economics, the concept of risk is statistically associated with the variance of a distribution. In this context, statistical processes that have no defined variance (such as a stable Lévy process) are not appropriate for financial risk management. In the same vein, when statistical physicists use stable Lévy processes to describe the dynamic of a particular variable, they also require a finite variance in their analysis of the fluctuations that occur in finite physical systems. Because stable Lévy processes fit a high number of phenomena particularly well, physicists developed “truncation techniques” to make these processes “physically plausible” (i.e. applicable to physical systems). Precisely, these truncation methods refer to a mathematical treatment of a part of the distribution to ensure the finiteness variance. Such evolution for dealing with stable Lévy processes makes them appropriate for both communities (see Schinckus, 2013b for further details on this point).

Another example refers to the use of random matrixes in the analysis of financial data. In physics, statistical physicists usually work with symmetric random matrixes whose elements refer to (physical, electrical, magnetic, etc.) signals that characterize a physical system at a specific moment. This description of a system a particular time makes the matrix's element temporally symmetrical. Financial economists deal with time series that describe the dynamics of economic variables at different periods in time. Consequently, economists use random matrixes by defining their elements as temporally asymmetric (i.e. referring to different moments in the past). Econophysicists who have discovered this way of using matrixes tend to progressively integrate it into their mathematical techniques, which are now shared by the two communities (see Jovanovic, Mantegna and Schinckus, 2018 for further information). These two examples show how tools can evolve when a trading zone emerges between two areas of knowledge. In particular, the interactions between economists and econophysicists generate in-between situations whose complexity requires an adaptation of tools that can be used by the two communities.

VI. Conclusion

In this chapter, econophysics was introduced and analyzed through different lenses in order to better understand the disciplinary nature of this new field.

Firstly, I used a bibliometric analysis to identify the disciplinary space of econophysics. This investigation showed that this field can be seen as a sub-field of physics, since the vast majority of articles are published in physical journals, which means that the conditions related to the perpetuation of knowledge are controlled by physicists. Although this section emphasized different publication conventions, it shows that, from an institutional point of view, econophysics can be looked at as a "unidisciplinary field" (i.e. related to only one scientific discipline) since the majority of papers founding econophysics were originally published in physics journals.

However, a historical inquiry shows that this unidisciplinary dimension is not very well justified. Indeed, many historical similarities (in terms of concepts and practices) between econophysics and financial economics have been stressed in this chapter. This historical inquiry is the cornerstone of this chapter because it called the unidisciplinary dimension of econophysics into question by emphasizing two important points:

- In terms of the history of concepts, this field can be considered as the re-emergence (in physics) of an old research programme that was introduced (but abandoned) in the 1960s by financial economists.
- In terms of the history of practices, actors involved in the emergence of econophysics (in the 1990s) and that of financial economics (in the 1960s) adopted the same strategy (despite their disciplinary differences) in order to justify the development of their field.

This historical evidence paved the way for considering the possibility of creating a trading zone between econophysicists and financial economists. In particular I showed that the concept of pidgin is the most appropriate notion for describing the current epistemological status of econophysics; I discussed this point in the last part of this chapter. In the following table, I propose to summarize the different disciplinary perspectives I dealt with in this chapter:

Perspective	**Nature of econophysics**
Bibliometric	Unidisciplinary: econophysics appears to be a sub-field of physics since the majority of articles founding econophysics have been published in physics journals.
Historical	The unidisciplinary dimension of econophysics is called into question since this field has many similarities, in terms of history of concepts and practices, with financial economics, suggesting that it can be looked on as a boundary field between financial economics and physics.
Implications	The historical similarities of the concepts could suggest room for a more integrative movement between financial economics and physics. Although the collaborations between econophysicists and economists are still in their infancy, this direction is actually supported by the historical inquiry proposed in this chapter.

Table 8: Summary of the disciplinary perspectives discussed in this chapter and its implications for econophysics

In conclusion, at this point in time, econophysics is not a discipline. Specifically, the notion of discipline makes no sense in an epistemological analysis of econophysics. In this context, econophysics can be seen as a subfield of physics or it can be perceived as an intermediary zone (or a pidgin) between physics and economics.

Although the bibliometric analysis developed in this chapter suggested that econophysics could originally be perceived as sub-area of physics, a more precise historical analysis rather suggests the later statement. Econophysics appears to be a boundary field that can be defined as “an agreement and an awareness between the groups involved through which each can understand that the other may not see things in the same way” (Chrisman, 1999, p. 6). The second chapter will investigate the scientific context in which this boundary field progressively emerged.

Chapter 2: Where did econophysics come from?

> *What Santa Fe did was to act as a gigantic catalyst for all that [research on complexity]. It was a place where very good people—people of the caliber of Frank Hahn and Ken Arrow—could come and interact with people like John Holland and can deal with inductive learning rather than deductive logic, we can cut the Gordian know of equilibrium and deal with open-ended evolution, because many of these problems have been dealt with by other disciplines. Santa Fe provided the jargon, the metaphors, and the expertise that you needed in order to get the techniques started in economics* – Arthur (cited in Waldrop, 1992, p. 325).

I. Introduction

By analyzing the disciplinary nature of econophysics, the first chapter showed why econophysics can be seen as a hybrid area of knowledge that exists between economics and physics. However, the first chapter did not really explain the context that favoured the emergence of such an in-between field. The application of statistical physics to economics is not necessarily an obvious research approach, and econophysics did not spring from nowhere. Furthermore, the importation of physics into economics is nothing new, since econometrics, for example, was mainly developed by scientists with a background in physics (Miroskwi, 2002). In this context, it is worth asking what the contextual elements were that promoted the emergence of econophysics as a hybrid branch of knowledge and what the differences (or similarities) were between that field and the previous importation of physics into economics. In addition to this relation to economics, econophysics is often presented as a part of complexity studies. Precisely, the few works dealing with philosophy of science and econophysics (Juhn et al. 2017; Thebault et al. 2017, Rosser 2010) explicitly emphasized the link between econophysics and complexity. Rickles (2007; 2008) who was, to my knowledge, the first to write on this topic explained that econophysics can be presented as the study of financial systems from the perspective of the physics of complex systems. This chapter, and more generally, this thesis is a step further in the investigation of the philosophical questions, initiated by Rickles (2007, 2008), about the link between complexity and econophysics. Where does this association of econophysics with complexity come from? What is the link between complexity, econophysics and economics?

This second chapter aims to detail the historical context that facilitated the development of econophysics in order to understand where this field comes from. Although the econophysics field has officially existed since the 1990s (Mantegna, 1991; Mantegna and Stanley, 1994), my study will take the form of an analysis of works that created the environment in which econophysics emerged. This chapter will examine the roots of econophysics and show how these roots still influence the field today. This investigation will lead me to mention the historical importance of the Santa Fe Institute (SFI), which played a key role in the development of complexity studies and, therefore, of econophysics (Holt et al., 2011). The Santa Fe Institute is directly and explicitly associated with the notion of complexity, as is mentioned on their official logo:

(www.santafe.edu)

The Santa Fe Institute contributed to the extension of complex studies to other areas of knowledge and this chapter will investigate how this institute influenced the emergence of econophysics and how this influence plays a key role in the differentiation between econophysics and econometrics. Actually, it is impossible to understand the contemporary evolution of econophysics without mentioning its methodological links to seminal studies developed by SFI, because this institution created a specific scientific context that promotes the hybridization of physics (Dillon, 2001). The clarification of the role of the SFI will allow me to highlight on the one hand, the place of econophysics in economic complexity and, on the other hand, the origins of computational techniques used by econophysicists. The Santa Fe Institute is a well-known independent research centre based on Hyde Park Road (on 32 acres) in Santa Fe, New Mexico.

Source: (www.santafe.edu)

The institute employs a small number of resident faculty (50) combined with around 100 visiting/external faculty. The SFI offers a number of education programmes that take the form of "program camps", workshops or summer schools that focus on complexity and the understanding of complex systems. In this chapter, I will show how this institution played a key role in the emergence of econophysics.

This institute was created in the early 1980s by leading scientists who were directly involved in Cold War science. In this particular context, the first part of this chapter will trace the roots of the complexity issue in the balkanization (fragmentation) of the Cold War science, which was mainly characterized by physics-based research and an emerging of the behavioural sciences. Both perspectives had military purposes and they worked on the elaboration of an optimal problem solving framework in which rationality was seen as an optimizing process that provided the most appropriate decision in a given situation. This scientific culture led the vast majority of the post-war scientists to associate complex situation problem solving with a complex process (i.e. dynamic complexity). After having clarified this point about complexity, I will explain how the Santa Fe Institute emerged in this Cold War context and how it played an important role in the diffusion of this dynamic complexity outside of physics. Specifically, the following sections will explain how SFI scholars associated dynamic complexity either with the emergence of a spontaneous order (agent-based modelling) or with the emergence of a macro statistical regularity (statistical perspective that will be presented as the origins of econophysics)[80].

[80] These two computational techniques that are associated with dynamic complexity are sometimes considered as two faces of the same complex reality (Langston, 1986, 1990, Langston and Wootters, 1990), as I will explain in the conclusion of the first part of this chapter.

Thanks to works developed by the SFI, these two forms of complexity have progressively been extended in economics.

The second part of the chapter will present this development of complexity studies in economics by highlighting how (and why) the Santa Fe Institute initiated them. From this perspective, the identification of the computational approaches developed by the SFI is very important because it offers a conceptual framework for a better understanding of the historical links between econophysics and economics. It is worth mentioning that, except for Mirowski (2002), who notes in passing a parallel between the Santa Fe Institute and the Cowles Commission, the historical differences and similarities between the emergence of econophysics and the development of early econometrics have not been a subject of research. By identifying the kind of complexity (dynamical complexity) that econophysics deals with and by focusing on the history of the institution (SFI) that promoted this complexity, this chapter will investigate further this aspect and clarify the relationship between econophysics and economics. Interestingly, the presentation of the role played by the SFI in this history clarifies the reasons why econophysics failed to impress economists.

Existing historical works on econophysics usually present the field as a contemporary development of the mathematical intuitions[81] developed by Mandelbrot in the 1960s (Roehner, 2009; Mantegna and Stanley, 1999; Jovanovic and Schinckus, 2013); however none of them have clarified the scientific context that promoted the crystallization of these intuitions. Despite the fact that one can find several articles that associate econophysics with complexity, these works clarify the historical context in which these two words have been combined. This is the major contribution of this chapter: by proposing a historical explanation that favours the development of econophysics, this chapter offers a kind of pre-history (i.e. before the official) of the field for a better understanding of its relations, on the one hand, to the umbrella of complexity; and on the other hand, to debates that emerged in economic history regarding complexity.

[81] These intuitions were developed in the first chapter when I mentioned Mandelbrot's works about the occurrence of extreme values on financial markets.

II. The Santa Fe Institute

In the beginning of the 1980s, a physicist called George Cowan had a driving influence on the creation of the Santa Fe Institute. George Cowan was an American physicist who dedicated his career to the development of the Los Alamos National Laboratory (where he entered in 1951 as a nuclear physicist). In 1981, Cowan accepted an appointment to the White House Science Council (WHSC) under the Reagan administration. In his memoires, Cowan (2010) explained how the new administration relied on science for the development of their new Manhattan Project, the Strategic Defence Initiative also called the “Star Wars project” by the popular press, which was supposed to protect the US from potential nuclear attack.

Cowan came back to the Los Alamos National Laboratory (LANL) in 1982 and his return “reawakened my [his] interest in finding common ground between the relatively simple world of natural science and the daily, messy world of human affairs” (Cowan, 2010, p. 142). However, the director of the LANL at that time, Donald Kerr, wanted to keep the research line that he initiated in the 1970s by dedicating resources to military projects in order “to protect our nation [the US] and promote world stability” (www.lanl.gov). Donald Kerr got his PhD in Plasma Physics in 1966 from Cornell University and he was appointed director of the LANL in 1979 (until 1985). Prior to becoming director, Kerr conducted research on high altitude weapons and nuclear test detection at the LANL[82]. His nomination as director in 1979 gave him the opportunity to support the research he initiated in the 1970s. In this specific context, in 1982, Cowan took the initiative to contact a group of his senior colleagues (David Pines, Stirling Colgate, Murray Gell-Mann, Nick Metropolis, Phil Anderson, Peter A. Carruthers and Richard Slansky) at LANL for weekly discussions about complexity in science. Two years later, these discussions led to the organization of a workshop on “Emerging Synthesis in Science”. These scientists were internationally recognized and well known for their interest in combining physics with other disciplines (Cowan, 2010). David Pines was a specialist in theoretical physics, a professor at the University of Illinois and founder of the Center

[82] Donald Kerr served as assistant director of the FBI (from 1997 to 2001), as director of research for the CIA (from 2001 to 2005) and as Principal Deputy Director of U.S. National Intelligence from October 2007 to January 2009. He is currently a member of the board of Iridium Communications.

for Advanced Study (University of Illinois at Urbana-Champaign Urbana). He became an active actor of the Santa Fe Institute and a member of the Los Alamos National Laboratory in the 1980s. Stirling Colgate was an American nuclear physicist famous for his research on the hydrogen bomb. He was a professor at the New Mexico Institute of Mining and Technology while being a leading researcher at the LANL. Murray Gell-Mann won a Nobel Prize in physics in 1969 for his works on elementary particles. He was a professor at the University of New Mexico and well known for having a strong interest in history and historical linguistics[83]. Nick Metropolis was a Greek-American physicist, a professor at the University of Chicago and a member of the LANL. He mainly worked on the use of computers in physics (Monte Carlo simulation). Phil Anderson was the physicist I presented in the previous section. Peter A. Carruthers (1935–1997) was an American physicist who led the theoretical division of the LANL in the 1970s, where he remained scientist until 1986 when he joined the University of Arizona as head of the physics department. Richard Slansky (1940–1998) was an American theoretical physicist who worked for the LANL while being an adjunct professor at the University of California at Irvine.

Two things connected these scientists: they were known to be interested/involved in interdisciplinary research and/or they were working for the LANL. All of them were invited to the meeting organized by Cowan in 1984, which became the founding event of the Santa Fe Institute[84]. The SFI was initially presented as an "educational institute" (Cowan, 2010, p. 142) whose campus had no intellectual territory at the interface between the conventional disciplines. The objective was explicitly to promote research that involved several disciplines: "These interdisciplinary subjects do not link together the whole of one traditional discipline with another; particular subfields are joined together to make a new subject" (Gell-Mann, 1984, p. 1). Although there were an increasing number of works promoting interdisciplinarity at that time, Cowan (2010) explained how this dimension of the SFI was a barrier to getting money from usual funding bodies (National Science Foundation, Atomic Energy Commission, etc.) because these institutions allocate funds to conventional

[83] In 2001, Murray Gell-Mann initiated the Evolution of Human Language Project at the Santa Fe Institute.

[84] The initial name of the SFI was the Rio Grande Institute because the label "Santa Fe Institute" belonged to an existing organization that helped alcoholics and drugs addicted people. When this institution became defunct a few months later, the final name of the research institute was "Santa Fe Institute".

disciplines. However, thanks to the excellence of the founding committee and thanks to the network he made when he was at the WHSC, Cowan[85] was able to raise capital to launch the Santa Fe Institute. More precisely, he knew Al Trivelpiece, who was the head of research at the US Department of Energy and who agreed to provide $250,000 annual funding for four years to launch the institute (this financial support was not renewed, as I will explain it in the second part of this chapter).

Despite his initial reluctance towards interdisciplinarity[86], Phil Anderson accepted the invitation and thought his intervention as a good opportunity to present his paper, which was published in 1972, about the theory of broken symmetry as a description of emergent properties. This theory questioned the classical form of reductionism that was used in science and it generated debates because, for many scientists, “there is always a reductionist bridge between the phenomenological and the fundamental level of explanation” (Gell-Mann, 1984, p. 5). This theory raised deep questions in the philosophy of science about interactions between the macro level of a system and the behaviours of its micro elements. The scope of Anderson’s publication seemed to be in accordance with the objectives of the new SFI. Indeed, on that point, the founder of SFI explained that, beyond the will to “take into account the enormous and increasing complexity of our modern society” (Gell-Mann, 1984, p. 8), the Santa Fe Institute was partly created for solving this puzzle between the micro and the macro levels.

The objective was therefore clear: reforming the classical disciplinary reductionism in order to adapt it to the (apparently) increasing complexity of society. In a sense, the SFI was consistent with the critiques of the time since its members worked on a reductionist understanding of a moving and uncertain context (which has progressively been associated with a complex system). The first SFI workshop was the first of a series of monthly meetings dedicated to themes related to the “messy world of human affairs” (associated with complexity in the Cowan’s perspective). Several disciplines (physics, biology, mathematics, medicine, archaeology, psychology) were dealt with during this first meeting; however, the following

[85] In the chapter of his memoirs, Cowan commented on how he contacted people he met at WHSC.
[86] Waldrop (1992, p. 80).

meetings were mainly focused on biology and physics[87]. Articles presented in these workshops were then published in proceeding volumes that often presented a collection of heterogeneous papers about complexity and emergence, but which did not provide a coherent and unified framework interlinking these themes. Although these publications were called *Proceeding Volume in the Sciences of Complexity,* this term "sciences of complexity" stayed undefined, and the published papers mainly emphasized the conceptual similarities [88] that appeared between the disciplines involved. Monthly meetings progressively evolved towards questions related to the way of modelling complexity. From that perspective, computers became more and more important in the research on complexity, as I will detail hereafter.

The progressive call for the development of interdisciplinary research was not the only factor that contributed to the emergence of complexity studies. Indeed, the eighties were also the decade during which computers began to be used widely (Johnson, 2007). Personal computers were booming and scientists learnt, at that time, how to integrate this new tool in their practices. Computers contributed to science in two ways: on the one hand, they were used as "bookkeeping machines" recording data related to phenomena and, on the other hand, they provided a higher power of computation paving the way to simulation. As Waldrop[89] (1992, p. 63) explained, "properly programmed, computers could become entire, self-contained worlds, which scientists could explore in ways that vastly enriched their understanding of the real world". Computers can be looked on as technical tools that enlarge our access to, on the one hand, the past phenomena (through recording of historical data); and on the other hand, the hypothetical future phenomena (through simulations). This high number of data was a necessary condition for dealing with complexity since a high number of data allows modellers to identify statistical

[87] The first four *Proceeding Volumes* summarizing these meetings were mainly (but not totally) dedicated to topics directly or indirectly related to biology and physics. It is worth mentioning that the fifth proceeding volume will be dedicated to economy.
[88] These first publications also aimed to clarify the difference between chaos theory and complexity era (see Mitchell, 2009).
[89] Waldrop (1992) wrote an interesting book about the history of complexity and the role played by the Santa Fe Institute in the popularisation of complexity issue—although this book is a well-written monograph, it is worth mentioning that it appears more as an elegant novel about people who contributed to the emergence of complexity. The book does not present detailed concepts and models developed in the SFI, and his historical perspective on economics is more narrative than detailed.

patterns. The development of computers therefore created a favourable environment for the emergence of the complexity paradigm since "scientists were beginning to think about more and more complex systems simply because they could think about them" (Waldrop, 1992, p. 63).

These two ways of producing data (recording and simulating) provided by computers implicitly determined an epistemic classification in ways of studying dynamic complexity: some scientists tried to find a specific regularity or statistical patterns in the recorded data about past phenomena, while others tried to generate computerized future phenomena by using programming and simulation. The following section will introduce these two ways of characterizing the dynamic complexity. A detailed understanding of these two computerized tools is important because they directly contributed to the emergence (and the development) of econophysics.

II.1. From cellular automata to computers and agent-based modelling

Agent-based modelling is a computational technique that is based on a computerized simulation of interactions between a high number of agents whose plausible rules governing their behaviour are inspired from the real world. Basically, these agents form an artificial world in which commitments emerge from a great numbers of iterations/interactions (O'Sullivan and Haklay, 2000). Because this computational approach starts from simple atoms following simple local rules from which a complex behaviour can emerge, it finds its origins in cellular automata, which was initially developed by Stanislaw Ulam and John von Neumann (1951) who worked on self-replication of systems by using a universal Turing machine[90]. It is worth mentioning that in the 1940s and 1950s, these two scientists were both members of the Los Alamos National Laboratory when they developed this new computational framework. Because the Santa Fe Institute was founded by seven physicists, of whom five were based at the Los Alamos National Laboratory (Nick Metropolis was a close friend of von Neumann), the SFI was a natural place for investigating the potential contribution of computers outside of physics (Erickson,

[90] See Chopard and Droz (2005) or Schiff (2011) for further details about the early history of cellular automata.

2014). Except for a few research projects in the sixties[91], cellular automata were not really studied until the seventies when Conway introduced them into biology (Gardner, 1970) and Toffoli (1977) used them to model physical laws.

Since the mid-1960s, computers have become important in physics for data acquisition and analysis (Galison, 1987). However, Cassidy (2011, p. 161) explained that the purchase of these “heavy number crunching machines” was often associated with the generous funds for promoting military research (Department of Defence, Atomic Energy Commission) or engineering research (Department of Energy, National Aeronautics and Space Administration, National Science Foundation); both promoting a mechanizing approach of problems solving. Starting in the 1980s, the advent of smaller, personal computers provided a huge impetus for the use of computers by individual researchers or small teams of researchers; the use of computers became the norm in scientific research.

The increasing importance played by computers in physics led to the common view that computation can be used to describe physics processes. Of course, the use of computers in physics was nothing new (see Pang, 2006 for a history of computers in physics). Two elements contributed to the emergence of a real “digital physics” (Fredkin, 2003) in the 1980s: 1) the generalization (democratization) of personal computers in the beginning of the 1980s and, 2) the works of some scientists (Jaynes, 1957; Zuse, 1969; Levin, 1973) that showed that physical systems can be described by computational simulations on the condition that they are compatible with principles of information theory, statistical thermodynamics and quantum mechanics. Progressively, physicists associated physical systems with computational processes founded on an information structure in which “classical matter/energy is replaced by information, while the dynamics are identified as computational processes” (Muller, 2010, p. 5). The basic metaphor was quite simple: physical particles (or spins) can be seen as simple bits—every switch from one quantum state to another can therefore be described through a binary change (0 to 1 or 1 to 0) for a bit (the same reasoning can be used for the magnetization of a spin in

[91] See Moore (1962), Myhill (1963) or Hedlung (1969).

magnetic field). With the gradual improvement of computers, the metaphor became more and more complex—some physicists began to believe that the physical universe could be described through computational processes[92].

Steven Wolfram was an important actor in this computerization of physics since he explicitly associated modelling in this field with the use of cellular automata in order to compute all possible computable solutions. Roughly, cellular automata can be looked on as a specific way of using the computational power offered by computers. Research on this theme witnessed a boost in the 1980s at the Santa Fe Institute[93], which acted as a real catalyst for computerized complexity (he already used this word in the early 1980s).

Because cellular automata can easily be developed through simple rules from which can emerge a very complicated behaviour, they were an ideal starting point for studying complexity in accordance with the conceptual framework initiated by Simon. Indeed, by defining simple constraining rules that govern interactions between micro elements (individuals), the use of computers can characterize the agents' limited rationality by providing computerized rules for characterizing their macro behaviours. Cellular automata are unquestionably the computational origins of agent-based modelling. In terms of implementations, these cellular automata require a particular methodology that takes the form of an adaptive agent-based behaviour. Two important works contributed to the emergence of such approach: 1) the famous Schelling's (1969, 1971, 1978) model of racial segregation and, 2) the adaptive methodology promoted by Arthur (1986) and Holland (1986). While the first model is now renowned for explaining (in a limited rationality framework) that segregationist residential structures can emerge from local behaviour of non-segregationist people[94], Arthur and Holland introduced the notion of a "complex adaptive system" that is implicitly based on adaptive individual components (i.e. agents). As Holland

[92] This idea that the physical universe is a computer is called "pancomputationalism", see Muller (2010) or Milkowski (2007) for a presentation of debates related to this view.
[93] Wolfram attended the first meeting where the Institute was founded and he has always been an active member of this community.
[94] Without a priori segregationist structure (such as ghettos, for example), agents generate a global segregation by behaving in line with their local preferences relating their neighbourhood—See Schelling (1969, 1971, 1978).

explained, “a complex adaptive system has many levels of organization, with bounded rational agents at any one level serving as the building blocks for agents at a higher level” (Waldrop, 1992, p. 148). By agent, Holland meant an entity whose initial configuration (which can be associated with beliefs, preferences or capabilities) allows it to change or adapt its behaviour in an evolving system. The adapting behaviour implied that decision makers are ruled by a bounded rationality that leads them to adapt their behaviour (Lee, 2010).

In the 1980s, the Santa Fe Institute appears to be a natural place for the gradual emergence of agent-based modelling. Indeed the computational perspective associated with cellular automata promoted by physicists such as Wolfram (1984)or Kauffman (1984) combined with the adaptive agent-based modelling enhanced by economists (Arthur and Arrow) and Holland (1986) progressively led to the emergence of what we now call agent-based modelling (Waldrop, 1992; Mitchell, 2007). On the website of the SFI, Arthur (2014) explained that in the 1980s, the institute had the computational power at its disposal to develop the agent-based approach: “instead of reducing all situations to a simple set of equations, we decided to study them by creating artificial worlds within the computers”. O’Sullivan and Haklay (2000, p. 4) explained that the success of agent-based modelling is “closely related to a view of the economy as an evolving complex system promoted by the Santa Fe Institute”[95]. This computational approach has mainly been extended to other disciplinary contexts in the 1990s: voting behaviours (Lindgren and Nordahl, 1994), military tactics (Ilachinski, 1997), organizational behaviours (Prietula, Carley and Gasser, 1998), epidemics (Epstein and Axtell, 1996), traffic congestion patterns (Nagel and Rasmussen, 1994), etc. Agent-based modelling has been used in so many fields that it is not possible to number them in this section in which the objective was to present this technique as a privileged way of modelling dynamic complexity.

This section explained how the SFI contributed to the development of agent-based modelling. I will come back to this technique and its implementation in economics

[95] “The economy as an evolving complex system” was the title of all proceeding volumes related to the workshops that Santa Fe Institute organized about economics.

later in this chapter. In the meantime, the following section will further discuss another important computational method (also initiated at the SFI) to deal with dynamic complexity: power laws and their scaling properties.

II.2. From power laws to dynamic complexity

The increasing computational power of computers in the 1980s was accompanied by a growing expansion of storage capacities. Scientists quickly understood that computers offered an important source of knowledge in terms of simulation but also in terms identifying patterns in historical data. Indeed, the growing storage capacities of computers allowed modellers to deal with large databases, which paved the way for better statistical analysis. It is worth mentioning that this computerization of science (Waldrop, 1992; Hughes, 1999) contributed to the "re-emergence" in physics of an old statistical framework that describes the statistical dynamics of a system through a power law. These statistical processes were already evoked in Chapter 1 when I explained how the emergence of econophysics echoes the old methodological debates in financial economics. Interestingly, power laws also generated debates in physics, where they were progressively associated with dynamic complexity by members of the SFI. This section aims to explain this point.

In the 1980s, SFI scientists wanted to use the maximum potential of computer power. Regarding their storage capacities, the question was simple: is it possible to extract a macro pattern from historical (computer-recorded) data related to complex phenomenon? That question made sense for physicists who "look for patterns in things or events and construct snapshots" (Cowan, 2010, p. 129). Actually this question already existed in the scientists' minds before the computerization of science, since one of the most famous macro patterns identified in data was probably the one identified by Pareto (1897), more than a century ago, where he observed a strange linearity in the repartition of wealth in the population (many people seem to have a low amount of wealth, while the richest are not so commonly observed), as illustrated below,

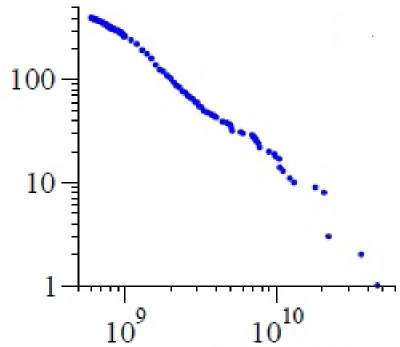

Figure 1: Linearity in the repartition of wealth (log-log plot). X-axis refers to wealth while the Y-axis indicates the number of people. This graph shows that a small number of people have large amounts of money—Source: Newman (2005, p. 6).

This pattern has a long story, since several scientists in different disciplinary contexts observed that linearity in their observations. Kleiber (1932) and Brody (1945), for example, also identified this linear relationship in their biological data: they found that the metabolic rate of various animals had a linear function of their body mass,

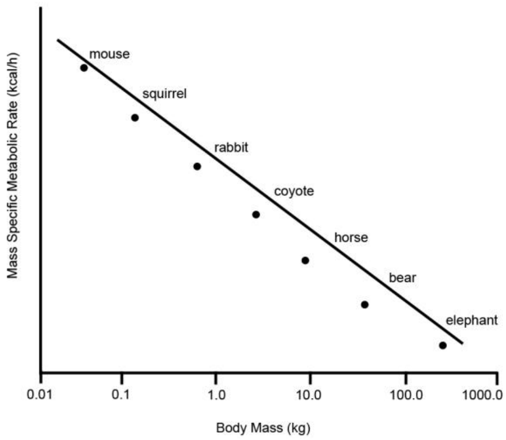

Figure 2: A log-log plot showing the link between metabolism (consumed energy in kcal/h) and body mass—Source: Brody (1945, p. 35).

In the same vein, the linguist Zipf (1935) also observed this linear relationship in the occurrence of words[96] in the vast majority of texts he studied[97], as illustrated below with the number of times that words occur in a typical piece of English text (here the novel *Moby Dick* by Herman Melville),

[96] Although Estoup (1916) was the first scientist to discuss this linearity in relation to words.

[97] Let us mention that Zipf observed this linearity in different languages.

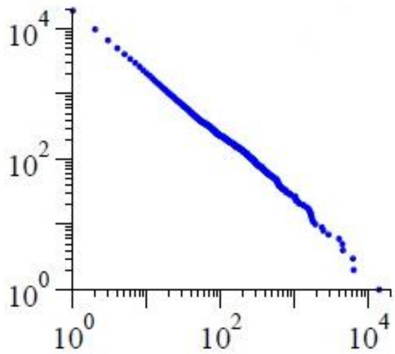

Figure 3: Word Frequency (log-log plot) in the novel *Moby Dick*—Source: Newman (2006, p. 6).

Precisely, the figure 3 shows that the frequency of one word in the novel Moby Dick is inversely proportional to its rank in the frequency table. These empirical observations are not simply due to a (un)happy coincidence, since this linear relationship has also been identified in several contemporary events: the magnitude of earthquakes (Newman, 2005), citations of scientific papers (Redner, 1998), internet hits (Adamic and Huberman, 2000) telephone calls (Aiello et al., 2000), copies of books sold (in the US) (Hackett, 1967), diameter of moon craters (Neukum and Ivanov, 1994), etc. In other words, this pattern appears in the observation of social and natural phenomena, which led some authors (Bak et al., 1987, Mantegna and Stanley, 1999) to consider this linearity as a law of nature (I will discuss this claim in Chapter 3). The increasing importance of this linearity seems to be a new scientific fashion for statistically treating the growing number of computerized data. This new paradigm of power laws is philosophically interesting because it offers a simple representation (straight line) of an increasing numbers-based complexity.

All of these statistical observations show a linear relationship on a log-log plot, meaning that the numbers on both axes increase by a power of ten with each tick on the axis. In other words, variables expressed on the two axes can be related through the following equation: $\ln p(x) = -\alpha \ln x + c$ where α and c are constants: while the first is the slope of the line (i.e the sensitivity of the x-axis variable related to the y-axis one), the latter is a scale parameter referring to the unit of measure used in the observations. This formula can also be reformulated (by taking exponential of both sides) as a power law: $p(x) = C\, x^{-\alpha}$ where the α is the characteristic exponent of the power law (this parameter is an indicator of stability since it refers to the sensitivity of potential variations). I already detailed this kind of law in the first chapter when I introduced how Mandelbrot (and econophysicists) characterized the occurrence of extreme values in the financial markets. In other words, these power laws have been a source of inspiration for many scholars, regardless of their background. A reason for this intellectual interest in power laws is due to their scaling property (i.e. the relationship does not change if scales are multiplied by a common factor) which is the expression of a statistical invariance (for this reason, power laws are called scaling laws). As Mitchell (2009, p. 258) explained, “scaling describes how one property of a system will change if a related property changes”. Because power laws keep a “particular proportionality” between each level of analysis, they offer the ideal statistical framework for describing scale-invariance phenomena. Given the statistical features of a specific level of analysis (data on metabolism of mice or weekly financial data, for example) it is easy to deduce information related to another level of analysis (in line with my previous example: information about the metabolism of elephants or the major statistical features of monthly financial data).

The first studies of this statistical invariance were those of Kolmogorov (1941, 1942) when he tried to find a scale invariance in data related to phenomena associated with turbulence in the 1940s. According to Hughes (1999), power laws (and their scaling property) appeared in physics during the same period, when Kolgomorov’s research (1941) about turbulence had progressively become widespread in the discipline[98]. Progressively scaling laws have been studied by physicists such as Kadanoff (1966), Domb and Hunter (1965) or Fisher (1957). However, as Stanley (1971) explained, there were no physical justifications, at that time, for the existence of scaling laws. Because these laws have an infinite second statistical moment, they appeared to be inappropriate for describing physical systems. On this topic, Stanley (1971, p. 18) wrote, “the scaling hypothesis is at best unproved and indeed, to some workers represents an ad hoc assumption entirely devoid of physical content”[99]. In

[98] Although modern probability theory was properly created in the 1930s, in particular through the works of Kolmogorov, it was not until the 1950s that Kolmogorov’s axioms became the dominant paradigm in this discipline thanks to the popularizing works of Doob (1953) and Feller (1957). These two writers had a major influence on the construction of modern probability theory, particularly through their two main books published in the early 1950s, which proved, on the basis of the framework laid down by Kolmogorov, all results obtained prior to the 1950s, thereby enabling them to be accepted and integrated into the discipline’s theoretical corpus (Shafer and Vovk 2005, p. 60)

[99] See Stanley (1971) for a review of theoretical literature related to the scaling laws in the 1960s.

other words, physicists seemed to face the same kind of conceptual problems as financial economists regarding the empirical application of power laws. In the 1990s, some physicists (Mantegna, 1991; Mantegna and Stanley, 1994; Stanley et al., 1996) revalued the use of power laws in physics by developing truncation techniques to deal with the infinite aspect of the volatility for such processes. Precisely, these techniques aim at making this volatility finite, thereby easing their physical interpretation (Schinckus, 2013c). Before the 1980s, power laws appear as a strictly conceptual framework in physics, but the implementation of computerized tools at the SFI paved a way for their observability. Furthermore, the empirical application of power laws was made possible by the development of truncation techniques that SFI scholars (econophysics' founders) developed in the earlier 1990s.

Identifying power law behaviour is not easy. As suggested previously in this section, the standard strategy consists of visually checking if data plotted on a double logarithmic scale align so that they are straight. This technique has several drawbacks: the visual line is sometimes not so "straight" and moreover, some data can show only power law behaviour for a part of the histogram. The most significant disadvantage of this visual technique is that it requires the highest number of data possible in order to identify a power law behaviour, meaning that only when a large volume of data is available, is it possible to distinguish between the two types of law (Newman, 2006). Miztenmacher (2004) also emphasized that, from a mathematical point of view, the linearity is a necessary but not a sufficient condition for having a power law[100].

As mentioned in the previous section, in the 1980s, computers became a physical and intellectual extension in the process of providing data about the world. Algorithms could generate work that could not be realized in any other way, while screens provided a new standpoint on data, emphasizing visual properties that could not be seen before (Mardia, 2000). By providing a high number of data combined with a visual analysis of these data, computers contributed to the renewed interest in power laws. In relation to that, Hughes (1999) explained that through the computerization of science observed in the 1980s, physicists got more and more

[100] See Mitzenmacher (2004) for mathematical considerations on power laws.

experimental evidence supporting the scaling laws and the existence of power laws in a variety of physical phenomena.

This section explained how SFI scholars contributed to the renew interest in the use of power laws in physics and interdisciplinary studies. The following section will further investigate the reasons why the SFI scholars focused on this category of laws. Specifically, I will explain how the scaling properties of power laws are particularly interesting for characterizing complex dynamics.

II.3. Scaling properties at the Santa Fe Institute

The improvement of computing equipment of the institute was a first priority of Cowan when he set up the SFI (Cowan, 2010). The SFI got the best computerized tools available for identifying statistical patterns and therefore potential power laws.

The increasing use of computers was a necessary condition for associating power laws with dynamic complexity, but it was not a sufficient condition. Indeed, such an association also required a theoretical justification, which had been proposed by a member of the SFI: Per Bak et al. (1987) who developed what he called "self-organized criticality". Bak was a Danish theoretical physicist who specialized in phase transitions; he worked at the Brookhaven National Laboratory at that time. He became member of the SFI in 1987 and became well known for his focus on the scaling property of power laws for characterizing complex dynamics. In particular, Bak claimed that the linearity visually identified on a log-log diagram describing the dynamics of two variables is the expression of the complexity of this phenomenon: "This simple law is impressive in view of the complexity of the phenomenon" (Bak, 1994, p. 478). Statistically, this linearity means that variables involved in the dynamics evolve simultaneously by keeping a scaling property (i.e. proportional relationship). Of this particular property, Bak (1994, p. 478) wrote:

> "This is an example [plot with occurrences of earthquake] of a scale-free phenomenon: there is no answer to the question 'how large is a typical earthquake?' Similar behaviour has been observed elsewhere in Nature […] The fact that large catastrophic events appear at the tails of regular power-

> law distributions indicates that there is ‘nothing special’ about those events, and that no external cataclysmic mechanism is needed to produce them”.

In other words, we have a “self-organized criticality” in which “slowly driven dynamic systems that have many degrees of freedom naturally self-organize into a critical state that obeys power-law statistics” (Bak, 1994, p. 480). The basic idea of self-organized criticality is that certain phenomena maintain themselves near a critical state. A telling example of that situation is a quiet sand pile in which the addition of one grain generates mini-avalanches. At some point, these mini-cascades stop moving and the sand pile has integrated the effect of this additional grain. The sand pile is said to reach its self-organized critical state (because the addition of a new sand grain would generate the same process). Physicists talk about “critical state” because the system organizes itself into a fragile configuration based on a knife-edge (the addition of only one sand grain would be enough to modify the sand pile). Bak et al., (1987) showed that the dynamics of critical state (i.e. the statistical characterization of the micro avalanches of the sand pile) follow a power law distribution.

As a member of the SFI (in 1987), Bak found the perfect environment for promoting his theory of criticality, which gradually became widespread in several disciplinary contexts in the 1990s (Frigg, 2003). In the second part of the eighties, eminent physicists (including Bak, 1987, 1994) of the SFI associated the observation of power laws with dynamic complexity because these laws characterize the evolution of a system whose micro configurations are so complex and unstable that only a description of the macro dynamics is possible. Boosted by the development of these works, the nineties were the decades of power laws since empirical evidence was been increasingly observed and published (Dubkov et al., 2008). These included: chaotic dynamics of complex systems (Zaslavsky, 2005; Solomon et al., 1993); front dynamics in reaction-diffusion systems (del-Castillo-Negrete et al., 2003), thermodynamics of anomalous diffusion (Zanette et al., 1995), dynamic foundation of non-canonical equilibrium (Annunziato et al., 2001), quantum fractional kinetics (Kusnezov et al., 1999), diffusion by flows in porous media (Painter, 1996), kinetic Ising and spherical models (Bergersen and Racz, 1991). According to Shalizi, in the physics literature, one can find a real fascination for these power laws”

> “Why do physicists care about power laws so much? […] The reason […] is that we're conditioned to think they’re a sign of something interesting and complicated happening. The first step is to convince ourselves that in boring situations, we don't see power laws” (Shalizi’s notebook http://vserver1.cscs.lsa.umich.edu/~crshalizi/notebooks/power-laws.html).

This section clarified the historical affiliation between econophysics and the SFI. Power laws and their scaling properties are founding concepts of econophysics and, because SFI scholars contributed to the renewed interest (and popularization) of this statistical framework, this institution played an important role in the advent of this new field. In light of this section, the SFI’s influence on the emergence of econophysics can be summarized by three statements related to power laws: 1) their observability through computerized tools was implemented at the SFI; 2) their theoretical importance in physics was conceptualized by SFI scholars, and; 3) their empirical application was made possible by the development of truncation techniques that SFI scholars (econophysics’ founders) developed in the earlier 1990s.

Beyond these three points, the general research atmosphere (i.e. interdisciplinary research) promoted at the SFI acted as a real catalyst for the emergence of alternative approaches. In this context, SFI scholars combined their new considerations on statistical descriptions of complex dynamics with computerized power to model and simulate their works. Such momentum contributed to the development of agent-based modelling. The next section will clarify the link between this new approach and the power laws whose importance at the SFI was discussed in this section.

III.4. Two computational sides of the same complex coin

The previous sections presented the importance of the SFI and how this organization contributed to the development of two computational ways of dealing with dynamic complexity: agent-based modelling and statistical characterization of the evolution of critical states. Although these two computational approaches both emerged in the same institution (SFI), one could wonder what these two computational approaches have in common. As Waldrop (1992, p. 307) explained, many scientists working on complexity in the 1980s acknowledged that at first sight, “Bak’s critical state [statistical perspective] didn’t seem to have anything to do with life or computation [usually modelled with an agent-based approach]”. However, it is worth mentioning that these two approaches share the same foundations since they study complex systems through the dynamics of numerous components interacting in a non-simple manner. Moreover, these two computational techniques use a methodology that is based on empirical verifications[101]. Some scientists (Langston, Kauffman) affiliated with the Santa Fe Institute were fascinated by the potential connection between these two computational approaches of dynamic complexity. More precisely, Langston (1986, 1990a, 1990b) proposed a formal connection between the dynamics of critical states and the one observed in computerized computation:

> “computation may emerge spontaneously and come to dominate the dynamics of physical systems when those systems are at or near a transition between their solid and fluid phases, especially in the vicinity of a second-order or critical transition” (Langston, 1990b, p. 13).

In other words, “we observe surprising similarities between the behaviors of computations and systems near phase [critical] transitions, finding analogs of complexity classes” (Langston, 1990, p. 12). By using this kind of similarity through statistical and agent-based approach, econophysics is a contemporary result of this progressive movement of physicists who are willing work on “the messy world of human affairs” by seeing communalities between the behaviours of economic and physical systems. The third part of this chapter will investigate how dynamic complexity was imported from physics into economics and how the two computational techniques associated with this kind of complexity have been implemented in economics. These two computational perspectives on dynamic complexity will also give me the context 1) to understand the current methodology used in econophysics (which will be studied in detail in the third chapter); and 2) to clarify to the historical debates that emerged after the advent of econophysics in economics, where the former is often presented as a pale copy of econometrics. These debates cannot be studied without understanding the role played by the SFI in the emergence of econophysics.

[101] This point will be discussed in the third chapter of this doctoral research.

III. The complexity era in economics

While the previous part dealt with the emergence of complexity studies and the role played by the SFI in their expansion, this second part will focus on the development of these studies in economics. In particular, it is important to mention that, independently of SFI, complexity studies were also influenced by economics. Such a situation leads me to contextualize here the role of the SFI in the emergence of econophysics and this, for two reasons: 1) this institution was a place where physics was extended to economics, and; 2) the role of this institute also explains why econophysicists failed to impress economists. This section aims to deal with these reasons by presenting in detail on the one hand, how SFI shaped the emergence of econophysics and, on the other hand, how this historical influence clarifies the fundamental differences between econophysics and econometrics.

The initial interest of SFI scientists for economics was directly related to the financial situation of the institution which, starting from 1987, sought funds (Waldrop, 1992)[102]. As previously mentioned, Cowan's network (after his work at the WHSC) enabled him to find funds to launch the SFI and to secure the financial situation of the institution for the first two years (1984–1986). However, in 1987, when it was time to extend existing financial supports, Cowan was faced with an increasing reluctance from the funding bodies that preferred to allocate their funds to more conventional and clearly defined research (Cowan, 2010). In this difficult context, George Cowan (the director) contacted several potential donors, such as the Russell Sage Foundation, and during a meeting at that Foundation he met a man who had significant influence on the research agenda of the Santa Fe Institute: John Reed.

John Reed was the CEO of Citicorp[103] and, although he has an economic background (MIT), he was very critical of the existing neoclassical economics, which,

[102] In 1975 Mitchell Waldrop earned a PhD in elementary physics from the University of Wisconsin and afterwards he obtained a Master's in journalism in 1977. He worked as writer and editor for several scientific journals and magazines such as *Science, Chemical and Engineering News*. He is currently a feature editor at *Nature*.
[103] Citicorp is still a funding body of the Santa Fe Institute.

according to him, were not very useful in a real economic context (Waldrop, 1992, p. 91). Reed's feeling was in line with an increasing wind of revolt against economics that appeared to be more and more abstract and disconnected from reality (Mirowski, 1989b; Morgan, 1990). After a discussion with Reed at the Sage Foundation, Cowan invited Reed to give a speech about existing problems in economics. The CEO agreed and presented a survey he coordinated about econometrical models whose conclusion incited a better appreciation of the dynamics of the economy in which we live (John Reed implicitly associated economic systems with changing phenomena, implying a dynamic complexity, see Waldrop, 1992, p. 89-96). Reed awakened interest of physicists who decided to open their meetings to economists, which explained thus on the website of the Institute:

> "In August 1986 a small group of Institute researchers and invited economists met in Santa Fe at the request of Citicorp CEO John Reed, who was frustrated with his own economists' past failures to foresee market catastrophes" (http://www.santafe.edu/about/history/).

That meeting had a huge impact on the SFI since it "took the intellectual agenda in the service of society to the extreme" (Pines, 2014, SFI website). Indeed, after this meeting, Reed decided to commit $1 million for an initial period of four years to fund research on economic complexity. That financial support was salutary for the institute, as explained on its website:

> "Unrestricted funding like that from Citicorp became an important element of the Institute's success. Its scientists sought refuge at the Institute from research environments where funding was assigned to individual projects that required specific results" (http://www.santafe.edu/about/history/).

The event led to a major programmatic orientation for the SFI because the sustainability of the Institute depended on the necessity of dealing with economic issues. This re-orientation did not mean that the Institute had to deal only with economic topics, but the conditional funding influenced the research perspectives developed by the institution. Because of that perspective, several economists were even invited to be involved in the research activities. Why would economists be interested in joining such a scientific project? Simply because the timing was good: in line with the optimizing way of dealing with rationality promoted by the Cowles Commission and the RAND Corporation, neoclassical economics was mainly ruled by a growing mathematical axiomatization focused on the Walrasian general equilibrium theory. This approach generated more and more debates (Mirowski, 1989): some economists called into question the axiomatic-based methodology while others began to work on new emerging theoretical frameworks inspired by the recent development of behavioural sciences. In this challenging context, in the early 1980s there was a demand for a conceptual renewal in economics. James Tobin refused the invitation. Tobin was an American professor of Economics at Yale University and member of the Council of Economic Advisors for several US presidents. He won the Nobel memorial prize in Economics in 1981 for his work on state interventions for avoiding recession. According to Waldrop (1992), this recent prize (in 1981) led Tobin to decline the invitation to join the Santa Fe Institute (in 1984) because he was too embedded and exposed in the economic mainstream. Another laureate of this Nobel memorial prize (in 1972), Kenneth Arrow, accepted the invitation. Arrow is an American economist, professor of Economics at Stanford University and was a member of the Council of Economic Advisors in the 1960s. He is famous for his works on the mathematical formulation of the general equilibrium. In the early 1980s, Arrow (1962, 1964, 1982) was well known for his awareness of the problems of neoclassical economics and, moreover, he was "intrigued with the possibility of using the mathematics of nonlinear science and chaos theory in economics" (Waldrop, 1992, p. 168) leading him to accept the invitation to join the SFI as well as suggesting the names of other economists: Michele Boldrin (University of California), William Brock (University of Wisconsin), Hollis Chenery (Harvard University), Timothy Kehoe (University of Minnesota), Thomas Sargent (Stanford University), Jose Sheinkman (University of Chicago), Mario Simonsen (Brazil Institute of Economics), Lawrence Summers (Harvard University) and Brian Arthur (Stanford University). In this list of economists (Bulletin of SFI, 1988, vol. 3, p. 18), the last name played a very specific role: Brian Arthur is a British economist who became well-known for his work on increasing returns and complexity. He was a professor at Stanford University where he founded the Institute for Population and Resources Studies—The role played by Arthur is particular for two reasons: 1) he was the only non-mainstream economist invited for the first meeting with physicists (Fontana, 2009); 2) he became the first director of the economic programme of the SFI. Why did the only non-mainstream economist succeed in becoming the director of the economic programme? Simply because the other economists considered the SFI as a research project in which physicists could contribute to the integration of non-linear modelling and stochastic analysis into the existing economic knowledge (Arrow, 1988). In accordance with this view, Fontana (2009, p. 3) wrote that “the agenda for the economics side of the meeting [first meeting between economists and physicists] was to teach to physicists the fundamentals of orthodox economics”. This first meeting was indeed interesting since it exhibited conceptual differences but also different expectations between physicists and economists. In his summary of the meeting, Arrow wrote “The general perspective of mainstream (the so-called neoclassical) economic theory had certainly had some empirical success […] But it is clear that many empirical phenomena are not covered well by either theoretical or the empirical analyses based on linear stochastic systems, sometimes not by either” (Arrow, 1988, p. 278). Actually, Arrow’s idea was to consider the research conducted at the SFI as an addition and not as a potential alternative to the neoclassical framework (Fontana, 2009). This claim is also supported by Colander (2003) who explained that economists were mainly defending their axiomatic approach “facing sharp challenges and ridicule from the physicists for holding relatively simplistic views” (Colander, 2003, p. 8); and also by Waldrop (1992, p. 141) who reported on the reaction of the physicist Phil Anderson, who straightforwardly asked the economists “and you guys really believe that?”. Brian Arthur was the only economist who asked the same question and he really wanted to develop an alternative to the neoclassical economic framework. This view influenced his nomination as the first director of the economic programme of the SFI in 1988 when the Science Board of the SFI appointed him as director and gave him the opportunity to shape the future research agenda of the institution (Bulletin of SFI, 1988, vol. 3, p. 13).

Despite the disciplinary challenges, the Santa Fe Institute progressively integrated economics into its research agenda and workshops specifically dedicated to economics were periodically organized [104]. By combining different disciplinary perspectives, the Santa Fe Institute played a very important role in creating a strong interest in complexity in economics, as Arthur (cited in Waldrop, 1992, p. 325) explained:

[104] The first one was organized a few months (September 1987) after the financial support was provided by Citicorp. It is worth mentioning that of the 21 contributors, six were working in a department of Economics, 12 in a department of Physics, on in a Food Research Institute, one in a department of Computer Sciences and one in a school of Medicine (See Anderson et al., 1988).

> “What Santa Fe did was to act as a gigantic catalyst for all that [research on complexity]. It was a place where very good people—people of the caliber of Frank Hahn and Ken Arrow—could come and interact with people like John Holland and can deal with inductive learning rather than deductive logic, we can cut the Gordian know of equilibrium and deal with open-ended evolution, because many of these problems have been dealt with by other disciplines. Santa Fe provided the jargon, the metaphors, and the expertise that you needed in order to get the techniques started in economics”.

Economic systems were considered an obvious candidate for complexity treatment because they are composed of multiple components that interact in such a way as to generate the macro properties. The dynamic complexity was explicitly mentioned as the major research target, as was written in the foreword of the proceeding volume related to this first workshop: “the purpose of the workshop was to explore the potential usefulness of a broadly transdisciplinary research programme on the *dynamics* of the global economic system” (Anderson et al., 1988, p. xiii). In his introductory speech for the workshop, Pines (1988) explained that the economic topics studied at the Institute were apportioned among working groups whose general schemes were “Cycle”, “Webs” and “Patterns”. The first scheme refers to nonlinear deterministic behaviour of systems, the second one concerns theories of large numbers of interacting units that generate emergent properties, while the last scheme focuses more on theories of statistical invariance. That distinction is interesting because it determined the methodological orientations for the research that the SFI initiated on economics: while the “Cycle scheme” does not really deal with complexity (but rather with chaos theory[105] as I will argue in the following section), webs and patterned themes were directly related to the two distinct computational approaches of dynamic complexity that I presented in the first part of this chapter: agent-based modelling (webs) and power laws (patterns). Interestingly, this categorization of SFI works influenced the methodological evolution of econophysics, as will be detailed later in this chapter.

Before presenting these three categories that are in line with the three research axes (“cycles”, “patterns” and “webs”) identified by Pines, it is worth mentioning that

[105] I will deal with the distinction between chaos theory and complexity in the following section.

economists were aware of complexity as witnessed by the existence of a strictly historical literature on economic complexity[106]. These economic historians provide interesting perspectives on complexity; they try to locate this topic within the history of economic thought, but not in a way that helps us to understand econophysics. In the following section, the three schemes mentioned above will be detailed by emphasizing their contemporary links with econophysics. That presentation will give me the context to clarify the major differences (but also to emphasize the similarities) between econophysics and another area of knowledge that also emerged through the application in economics of statistical tools imported by physicists: econometrics.

IV. Chaos theory associated with economic complexity

As noted previously, Pines (1988) explained that economic topics studied at the Santa Fe Institute were apportioned among three working groups labelled “Cycle”, “Webs” and “Patterns”. The first scheme refers to nonlinear deterministic behaviour of systems, which does not really deal with complexity but rather with chaos theory. Although chaos is sometimes used as synonymous with complexity, these two issues are not the same thing. Chaos is a non-linear dynamic that describes a situation in which the output’s system varies so erratically that it looks random. The chaotic character of the system can be associated with two notions: the dependence on initial conditions and strange attractor. The former is often associated with the “butterfly effect” according to which a tiny difference in initial conditions can lead to a very different system outcome, while a strange attractor is rather “paths of complicated and irregular geometric shapes […] which might be [seen as] an equilibrium trajectory” (Rosser, 1999, p. 174).

[106] An edited volume (Colander, 2000) providing a collection of papers devoted to historical perspective on economic complexity showed that this topic has some roots in early economists’ works (such as John Stuart Mill and Friedrich von Hayek). In his book dedicated to complexity in the history of economic thought, Colander (2000) proposed several other historical examples. Basically, these historical studies about complexity in economics usually aim to emphasize the pioneering dimension of these previous studies. These historical works allow heterodox economists to show that the new hype called “complexity” has some roots in their tradition. In a sense, this historical reconstruction appears as an indirect way of creating a crisis in the established economic knowledge.

Roughly speaking, chaos issues can be seen as a predecessor of the complexity era[107], whose non-linear dimensions paved the way for the development of research on complexity. This claim seems to be confirmed by the evolution of the themes dealt with in the Santa Fe Institute publications devoted to economics. Indeed, the first book, entitled "Economy as an evolving complex system", published in 1984, offered 13 articles; five were dedicated to Chaos [108] five focused on micro interactions and only two of these papers dealt with "patterns" (identification of statistical invariance). A decade later, in 1997, the second volume of "Economy as an evolving complex system" collected twenty articles of which seventeen were exclusively dedicated to micro interactions (i.e. agent-based modelling) and only three (Durlauf, 1997; Lane and Maxfield, 1997; Arthur et al., 1997) were devoted to the identification of invariance. Chaos issues totally disappeared from this volume and the word "chaos" was used only four times (in three papers) in the book. The third volume, published in 2006, definitively confirmed the historical nature of the chaos theory and its relationship to complexity since the word "chaos" did not even appear in the index anymore. This decreasing interest in chaos theory appeared therefore obvious; as Mirowski (1996, p. 38) put it, "the physical scientists at Santa Fe generally regard chaos theory […] as uninteresting or a dead end". Twelve out of the fourteen articles proposed in this third volume were dedicated to micro interactions and agent-based modelling while only two (Stanley et al., 2006 and Lévy, 2006) dealt with the identification of patterns. However, although the latter topic was not the central theme of the book, it is worth mentioning that these two articles directly came from econophysics[109]. This evolution in the themes dealt in the books published by the Santa Fe Institute is very informative. Basically, the computational prospects opened up by cellular automata combined with a

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[107] Rosser (1999) identified three predecessors of complexity: Cybernetics, Catastrophe theory and Chaos theory, which all proposed a specific framework for dealing with non-linear dynamics. Within the complexity framework, this non-linear dynamics is combined with emergent properties. See Rosser (1999) for further details about these issues and their links with complexity.

[108] All applications of chaos theory in economics were not necessarily related to the Santa Fe Institute. One can mention, for example, the chaotic description of the macroeconomic environment (Kaas, 1998), the new chaotic econometric models (Dechert and Gencay, 1996, Bask, 1998) or the development of a chaotic *tâtonnement* price adjustment (Goeree et al., 1998). The collection of papers edited by Prigogyne and Stengers (1984) on chaotic characterization of economic systems can also be associated with this literature. However, although there are some articles devoted to the application of chaos theory in economics, this theme has gradually been abandoned in economics (Rosser, 1999).

[109] One of these articles was written by one of father of econophysics (Eugene Stanley); moreover, the word "econophysics" appeared for the first time in the works proposed by the Santa Fe Institute on economics).

methodological adaptive individualism[110] that was enhanced by economists involved in the SFI (Arthur; Arrow) progressively led the institute to focus mainly on the modelling of evolving micro interactions. This evolution also resulted from the choice of Brian Arthur as the first director of the economic programme of the SFI in 1988. Indeed, while Arrow had more of a role of "steersman, in the ambit of the science board, than as an active researcher" (Fontana, 2009, p.6); Arthur was the only heterodox economist working at that time on the stochastic/dynamic method in economics based on an algorithmic approach. His role, combined with the influence of computer-orientated scientists such as Holland or Wolfram, determined the concepts and tools developed by the future research agenda that was implemented by the SFI. As explained in the first part of this chapter, that research paved the way to the development of agent-based modelling in economics (I deal with this approach in the following section).

This section explained that research on one of the key themes initiated by the SFI has been progressively abandoned, implying that the literature devoted to chaos did not really contribute to the development of econophysics, in contrast with the two other categories of works that were enhanced by the Santa Fe Institute: works on the identification of patterns in economic dynamics and research on economic interactions (associated with the group working on "webs"). These computational approaches have been extended in economics, mainly by SFI scholars. The rest of this chapter will present 1) how the SFI shaped these two themes, which laid down the methodological foundations of econophysics, and; 2) how this influence explains why econophysics can be seen as a different field from econometrics.

V. Statistical patterns in economics

V.1. Patterns and origins of econophysics

The scheme labelled "Patterns" was supposed to study the statistical behaviour of complex economic systems. I explained previously how the progressive

[110] This methodology refers to an algorithmic calibration of individual properties allowing agents to change their behaviour depending on stimuli they get from the context.

computerization of society and science observed in the 1980s contributed to the development of the self-criticality theory (Bak et al. 1987, Hughes, 1999), which requires a high number of observations in order to characterize power laws in complex systems. By promoting the application of this theory to other areas of knowledge, the SFI played a key role in the genesis of econophysics. Indeed, as discussed in the first chapter, the economic mainstream (neoclassical economics) is mainly based on Gaussian law (Jovanovic, 2008). However, because this statistical framework requires that no critical (extreme) variations can happen, it was not appropriate for describing the “criticality of complex [economic] systems [i.e. extreme variations in financial prices]” (Bak et al., 1987). From this perspective, Bak (1987, 1993) proposed an economic extension of his self-organized criticality through a model in which a shock in the supply chain (which acts as an additional sand grain in a sand pile) generates economy-wide fluctuations (like mini-avalanches in the sand pile) until the economy critically self-organizes (i.e. at a fragile state that could easily be modified by an additional small shock). This model showed that the dynamics of large fluctuations in the economy/finance can statistically be described through the scaling properties of a power law. That extension is very important for the emergence of econophysics, since the self-organized criticality is used to justify the use of power laws as the most appropriate macro description of complex economic/financial systems.

The methodological birth of econophysics is usually associated with the publication of Mantegna (1991) in which the author compared the occurrence of extreme variations on the Milan financial market with the occurrence of earthquakes from which observations can statistically be described through a power law that is in line with the self-organized criticality framework. In other words, the self-organized criticality framework originally defined by the SFI physicist Bak (1987) is the conceptual foundational justification for the importation of power laws as they are used in statistical physics into financial/economic spheres. That conceptual bridge would generate, in the 1990s, an increasing number of empirical works that observe power laws in socio-economic phenomena: Mantegna and Stanley (1994), Lux (1996), Bak et al. (1997) and Gabaix et al. (2000) observed that the large fluctuations on the financial markets can be captured through the scaling property of a power law while Lévy (2003) confirmed the conclusion made by Pareto (1897) one century earlier by showing that wealth and income distribution can both statistically be characterized by scaling properties. In the same vein, Amaral et al. (1997) explained that the annual growth rates for US manufacturing companies can also be described through a power law, whereas Axtell (2001), Luttmer (2007) and Gabaix and Landier (2008) claimed that this statistical framework can also be used to characterize the evolution of a firm's size as a variable of their assets, market capitalization or number of employees.

This research, which is based on the identification of a specific statistical pattern (power law) as the signal of dynamic complexity, was explicitly initiated in the inaugural workshop (1987) on economic complexity that was organized by the Santa Fe Institute. Although this theme of "statistical patterns" did not become the research priority of the SFI in the 1990s, it did not disappear from the research agenda of the institution. Between 1987 and 2006, the SFI published three collections of articles dedicated to economic complexity and one can observe that the theme of "statistical patterns" maintained its importance in these publications, while the "cycles" scheme (chaos theory) totally disappeared and the works dealing with "webs" (agent-based modelling) took on an increasing importance. Progressively, the theme of statistical patterns became an independent area of research that led to the advent of a field called econophysics. Because the SFI members clearly identified the characterization of statistical patterns as a path of research associated with economic complexity, and because the SFI was the place where the conceptual background (self-organized criticality) justifying this path was promoted, this institution played a key role in the crystallization of ideas that led to the emergence of econophysics. It is important to emphasize the role played by Prof. Eugene Stanley (who was a member of the SFI) in the development of econophysics. Precisely, the renowned scholar[111] offered an important institutional support to the field – being the director of the Polymer Center at the Department of Physics (at Boston University), Stanley allocated to econphysics-oriented research a part of the annual budget that

[111] To remind, Eugene Stanley (born in 1941) is an American physicist and professor at Boston University who is well known for his works on statistical physics and on interdisciplinary studies in physics. He coined the name "econophysics", the discipline of which he is said to be the father. He is also the author (with Rosario Mantegna) of the first textbook on econophysics, which was published in 1999 by Cambridge University Press. Eugene Stanley has also been the editor of Physica A, a journal that was originally dedicated to condensed-matter physics and which appears to be today the first journal in econophysics.

his center got from the University. In doing so, Prof. Stanley contributed to the development of a community of econophysicists. Interestingly, a lot of post-doctoral scholars who joind Stanley came from Europe explaining partly the reason for why econophysics is today more developed in Europe[112]. In addition to this aspect, it is worth mentioning that Eugene Stanley has been the chief-editor of *Physica A* for more than 2 decades where he always promoted and welcomed articles dealing with econophysics.

Despite the presence of economists in the SFI, it is worth mentioning that this scheme of "statistical patterns" did not arouse enthusiasm among economists. Indeed, the absence of a compelling set of theoretical models for explaining how the laws emerged (Durlauf, 2005) does not match with the usual micro approach enhanced by economists for whom an explanation in terms of individual characteristics is a disciplinary way of thinking (Hoover, 2013).

Beyond this methodological gap between economists and econophysicists[113], this lack of interest from the first in the works of the latter is enhanced by a feeling of "déjà vu", since the development of the Santa Fe Institute/econophysics is often seen as a pale copy of the emergence of the Cowles Commission/econometrics in the 1930s. Because this historical issue is important and very often mentioned by economists[114] (Durlauf, 2005, 2012), I propose hereafter to clarify this distinction in two steps: first, I will deal with the comparison between the Santa Fe Institute and the Cowles Commission to better understand why econophysics failed to impress economists. Secondly, I will emphasize the methodological differences between the way econophysicists and economists use statistics.

[112] Econophysics is still important at Boston University where Stanley's group still produces a lot of research outcomes in econophysics. Houston University (with Prof. McCauley) is also another university promoting econophysics oriented research. It is worth mentioning that the list of universities supporting econophysics is today longer in Europe (University of Leicester, UK; University of Palermo, Italy; University of Liege, Belgium; Kings College London, UK; Trinity College Dublin, Ireland; Univeristy of Warsaw, Poland etc.)

[113] The fourth chapter will investigate the epistemological differences in terms of modelling practices in the two communities in more detail.

[114] I must confess, this is the most common question I am asked by economists when I give a talk about econophysics.

V.2. The development of the SFI, a feeling of déjà vu?

Because the SFI and the Cowles Commission both hired physicists for promoting the use of statistics in economics, it is reasonable to draw a parallel between these two institutions in which the historical similarities are, at first sight, amazing, as Mirowski (1996, p. 19) wrote:

> "The Econometrics Society founded in 1930 by twelve Americans and four Europeans in a climate of economic contraction and academic hostility to mathematical formalism, might not have gone anywhere had it not found a long-term sponsor in Alfred Cowles. Cowles thought he was buying a better stock market predictor, but the trained physicists and mathematicians that had been taken on board reoriented the centre of research towards their own abstract concerns. Change the numbers, move the calendar to 1983, replace widespread hostility to mathematical formalism with a disdain for anything but formalism, replace Alfred Cowles with John Reed, and you have a fair characterization of the inception of the economics programme at the Santa Fe Institute".

Although Mirowski (1996) suggested this parallel, he did not further study this perspective. Let us investigate in more detail this comparison between the two institutions by retracing a quick intellectual evolution of the Cowles Commission: In the 1930s, the Cowles Commission was the spearhead of neoclassical economics, stressing the importance of mathematical formalism and the unicity of the scientific method (Mirowsky, 1989, 1996; Morgan, 1990). Starting in the 1940s, the Cowles Commission became more and more statistics-orientated and its leading members (Jacob Marshak and Tjalling Koopmans) developed their famous estimation methods, which were in line with the inference approach promoted by Pearson (1924). After the 1950s, none of the leading members of the Cowles Commission were still involved in empirical works and none of them investigated their econometric techniques further (Christ, 1994). The Commission became increasingly abstract since it "opted for pristine Bourbarkist mathematical abstraction, best represented by Debreu's *Theory of Value* and Koopmans' *Thee Essays on the State of Economics Science* in place of structural econometrics" (Mirowski, 1996, p. 17). From this perspective, adherence to the Walrasian general equilibrium theory combined with the use of a Bourbakist axiomatization became the required conditions for being part of the economic orthodoxy. Historic investigations (Mirowski, 1989b, 1996; Morgan, 1990) on the Cowles Commission concluded that the general project of the institution collapsed because it evolved towards a more abstract and disconnected (non-empirical) research agenda. That general feeling seems to be shared by leading economists, since Kenneth Arrow and David Kreps, for example, claimed “that very little truly novel took place in economics after the triumph of the Cowles programme in the 1960s” Mirowski (2012, p. 166). In response to this lack of interest in empirical works in economics, some rival areas of knowledge emerged such as game theory, behavioural economics and artificial intelligence, which progressively emerged to fill the vacuum opened by the Cowles Commission.

When the Santa Fe Institute began to deal with economic complexity in the beginning of the 1980s, no rival perspectives governed the orthodoxy of economics in which, according to Stiglitz (2003, p. 572), “something was [still] wrong—indeed, seriously wrong—with the competitive equilibrium models which represented the prevailing paradigm”. Economists who joined the SFI were usually looking for new intellectual challenges: Brian Arthur has never been considered as a mainstream economist and his first motivation to join the SFI were related to his critical thinking of the economic mainstream (Waldrop, 1992) while other big names (such as Kenneth Arrow) appeared to seek an new intellectual project by considering that the Santa Fe Institute could be the Cowles Commission of the 1990s (Mirowski, 1996). Here the similarities between the two institutions end. The Santa Fe Institute and the Cowles Commission can legitimately be associated: both institutions were funded to develop a research agenda based on statistical investigations of economic phenomena.

The comparison between the SFI and the Cowles Commission does not go beyond the parallel evoked above. Actually, these two institutions have more differences than similarities, since they did not implement the same research programme. While the objective of the Cowles Commission was to formalize and axiomatize the Walrasian general equilibrium theory, the SFI, in contrast, was interested in the development of empirical and evolutionary research. Such opposition explains why the two institutions methodologically evolved in a very different way. The Cowles

Commission promoted a single method science by formalizing economic knowledge wherein all economic phenomena must be described in axiomatic terms consistent with core assumptions initially defined (such as agent's perfect rationality, equilibrium as final state, etc.). For members of the SFI, this way of working was judged mathematically too abstract and disconnected from reality. In contrast with the Cowles Commission, the SFI did not focus only on an axiomatic methodology but it rather enhanced the cross-fertilization among disciplines by combining the empirical perspective promoted by physicists with the adaptive aspect enhanced by biologists and the computational techniques developed by computer specialists. Beyond these historical similarities and differences, it is also worth stressing that the two statistical ways of dealing with economic phenomena (econometrics and econophysics), which emerged from these two institutions, are technically very different. As a reminder, the first chapter explained how econophysicists mainly work with the description of the whole distribution (unconditional approach) when they describe the dynamics of economic/financial variables. In contrast, economists use technical solutions to save the Gaussian framework by working with conditional approaches consisting of describing the major trend of dynamics through a normal distribution whose large variations are characterized by a second distribution.

This section emphasized how the SFI contributed to the development of econophysics by having initiated the works dealing with power laws (and their scaling properties). This key role played by the SFI explains why econophysics failed to impress economists for whom this field appears as a pale copy of the Cowles Commission. This section clarified this aspect by claiming that these two institutions contributed in very different ways to the understanding of economic phenomena.

As an extension of this usual confusion between the SFI and the Cowles Commission, the existing literature usually presents econophysics as a pale copy of econometrics (initiated at the Cowles Commission). I would like to end this section by clarifying this point.

V.3. Statistics in economics? Nothing new!: The case of statistical economics

If econophysics is often compared to econometrics, it is not only due to the background of their members and their significant use of statistics but also because of the development of the former echoes a debate (called "measurement without theory") that emerged in the early days of econometrics. Economists are aware of the use of statistical patterns as they witnessed the existence of what is called "statistical economics" (Morgan, 1999, p. 55). Roughly speaking, that research programme[115] can be summarized as wanting to describe and measure business cycles through the identification of statistical patterns. More precisely, authors involved in this approach try to isolate fluctuating macro trends. In a sense, statistical economics can be seen as a precursor of econophysics for several reasons: this research programme focused a phenomenological description of economic systems through the identification of statistical macro patterns by criticizing econometrics' dependence on the Gaussian distribution and its conditional approach. Moreover, in accordance with econophysics works, economists emphasized the potential for "infinite probable error" (Mills, 1927, p. 336), referring to the "fat tails" of the distributions of price changes. As Mirowski (1989) explained, this observation was persistently ignored by neoclassical economics.

Beyond this historical similarity between econophysics and statistical economics, additional differences exist between these two research programmes: in contrast with the first, the latter did not identify statistical patterns as universal laws. Indeed, as Rutherford (2011) explained, the influence of the pragmatic school (especially John Dewey) on economic works at that time led economists to focus on contextualized treatments of statistical patterns. In line with the self-criticality theory, econophysicists see identified statistical patterns (i.e. power laws) as a signal of a universal framework. Concerning the statistical methodology, economists and econophysicists do not treat economic data in the same way since the first think the

[115] Most of works dedicated to this approach between 1910 and 1950 were associated with the American Institutionalist School, which mainly worked on the understanding of business cycles and the influence of institutions on economic behaviours. This school was mainly affiliated with the NBER and funded by the Rockefeller Foundation, which had decided to sustain the Cowles Commission starting in 1947 (for further details about the history of the Institutionalist School, see Craver, 1986).

regularities of data were visible in the patterns of events of the cycle but not in the statistical characteristics, while the latter deal with the identifiable patterns and the statistical features of data. Another significant difference between econophysics and statistical economics is the way of thinking about how phenomena related to an emergent macro law. The economists saw statistical patterns as instruments for both investigation and social control by considering that the society was too complex to be associated with a natural order that had to be “replaced by a social order, maintained by social controls including public opinion, belief, social institutions and laws” (Rutherford, 2011, p. 13). In this context, statistics and macro laws were perceived as instruments for “an active intelligence guidance of social processes” (Ross, 1991, p. viii). As previously mentioned, econophysicists explicitly associate economic systems with a self-organized criticality that no external actor/factor can influence[116].

As shown in this section, the research scheme associated with the “statistical pattern” developed by the Santa Fe Institute echoes the historical debates in economics. By clarifying the specific role played by the SFI in the development of econophysics, this section showed that the emergence of this field is not a pale copy of existing economic works. In the same vein, the importance of the SFI and these echoes of historical debates explain why econophysics failed to impress economists, and is why, as explained in the first chapter, this field emerged in physics.

VI. Webs in economics or agent-based modelling

As detailed in the first part of this chapter, the early 1980s were characterized by a fragmentation of Cold War science leading to balkanization of scientific research. This challenging context combined with an increasing number of personal computers owned by scientific institutions favoured the development of several computer-based methodologies in science: agent-based modelling is one of them[117]. Brian Arthur (the first director of the economic programme of the SFI) was a pioneer of this methodology, which became progressively dominant in the research agenda of the

[116] This perspective is often emphasised by econophysicists who compare the self-organized dimension to the agents’ free will, making their approach more in line with the Hayekian idea of spontaneous order (Bouchaud, 2002; Schinckus, 2016d).

[117] One can also mention the development of the Monte Carlo simulations.

institution. In the 1990s, (with David Lane [118] as director of the economic programme), the SFI contributed to the extension of this agent-based modelling to other themes, which explains why this modelling gradually become the most widely used tool for capturing economic complexity (Axelrod, 2005). Although that approach allows economists to define some behavioural features, this methodology explicitly associates human behaviours with sets of abstract algorithms that are supposed to describe the "fundamental behaviour" of agents [119]. In other words, models are formulated as computer programs in which agents' behavioural characteristics are inputs while outputs are associated with the macro behaviour that results from micro interactions.

As mentioned in the first part, agent-based modelling was developed at the SFI in the 1980s and 1990s (Arthur 1988; Holland, 1988). An analysis of the works published by the SFI shows how agent-based modelling progressively became the key area of research for this institution. In the three collections of articles dedicated to economic complexity that were published by the SFI between 1987 and 2006, we can observe that the theme of "statistical patterns" maintained the same importance in these publications, while the "cycles" scheme (chaos theory) totally disappeared, and the works dealing with "webs" (agent-based modelling) had an increasing importance, where this approach is used to describe diverse situations: the opinion transmission mechanism (Deffuant, 2006; Amblard and Deffuant., 2004); the development of industrial networks and the relationship between suppliers and customers (Brenner, 2001; Gilbert 2007; Epstein, 2006); the addiction of consumers to a brand (Janssen and Jager, 1999); the description of second-hand (cars) markets (Izquierdo et al., 2006); and the evolution of financial markets (Lebaron, 2006), etc.[120]. The best introduction to this literature is doubtless the three publications published by the Santa Fe Institute on economic complexity (Pine 1988; Arthur et al., 1997 and Blume et al., 2006), which offer an impressive collection of works devoted to agent-based modelling as it is applied in economics.

[118] David Lane is an American economist known for his theory of artefact innovation and his work on economic complexity based on evolutionary processes.
[119] From this perspective, "the entire market system is then seen as a network of interrelated individual automata\markomata whose profusion of forms may nonetheless be seen as relatively coherent if explained in terms of computational hierarchies" (Davis, 2013, p. 9).
[120] See Cristelli (2014) for a detailed literature review of agent-based modelling applied in economics.

VII. Conclusion

This second chapter presented a pre-history of econophysics and showed how this field can be connected with complexity studies. The first part of the chapter emphasized the key role played by the Santa Fe Institute in the development of computational techniques for dealing with dynamic complexity. These techniques are directly related to the rapid expansion of computers that allowed scientists on the one hand, to record and visualize a great number of historical data, and on the other hand, to simulate potential situations through computerized simulations. Basically, the two computational techniques promoted by the Santa Fe Institute for dealing with dynamic complexity refers to these two dimensions since the statistical (but also visual) identification of macro patterns has been favoured by the increasing databases, while agent-based modelling is a specific way of simulating complex situations.

By promoting the extension of dynamic complexity outside of physics, the Santa Fe Institute also contributed to the development of complexity studies in economics. In this chapter I exposed the financial reasons for why this institution decided to work on economic issues. Although the Santa Fe Institute favoured the development of complexity studies in economics, it did not have a monopoly on the topic. The first section of the second part of this chapter mentioned other categories of works that are usually associated with economic complexity. From this perspective, some words were given on the historical studies and on the application of chaos theory. Broadly speaking, the historical presentation offered in this chapter could be summarized through the following graph:

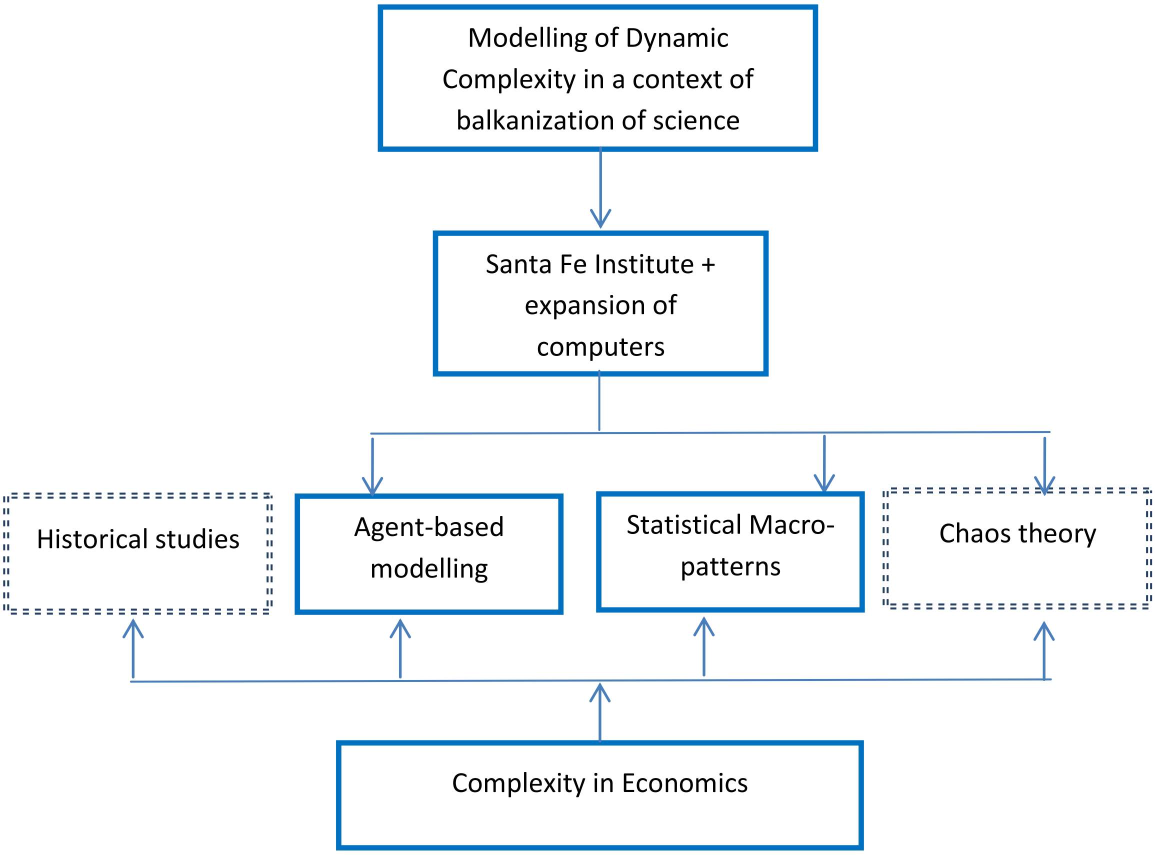


Figure 5: Processes presented in this chapter.

I quickly evoked the historical studies as an attempt to root complexity studies in the history of economic thought and I explained how chaos theory has progressively been abandoned in statistical physics[121] (and economics). Because historical studies and works dedicated to chaos theory did not have a historical or methodological affiliation with econophysics, this doctoral dissertation will focus, in the next chapters, on the works dealing with the two computational techniques enhancing by the Santa Fe Institute to deal with complexity (agent-based modelling and identification of statistical macro patterns). Finally, the investigation of econophysics' historical roots also gave me the opportunity to clarify some debates that exist in the literature, which regularly equate econophysics to a feeling of déjà vu in the history of economic thought. The last sections of this chapter emphasized the specificity of econophysics by explaining the extent to which this field differs from a previous influence of physics on economics. The following chapter will be more methodologically orientated since it will explain how econophysics originally emerged as an extension of works dedicated to the statistical identification of macro patterns,

[121] It is worth mentioning this field is still well and alive in other fields of physics (Quantum chaos for instance – see Wimberger (2014).

and how it progressively witnessed a methodological diversification. These methodological issues will lead me to deal with several key philosophical notions, such as emergence.

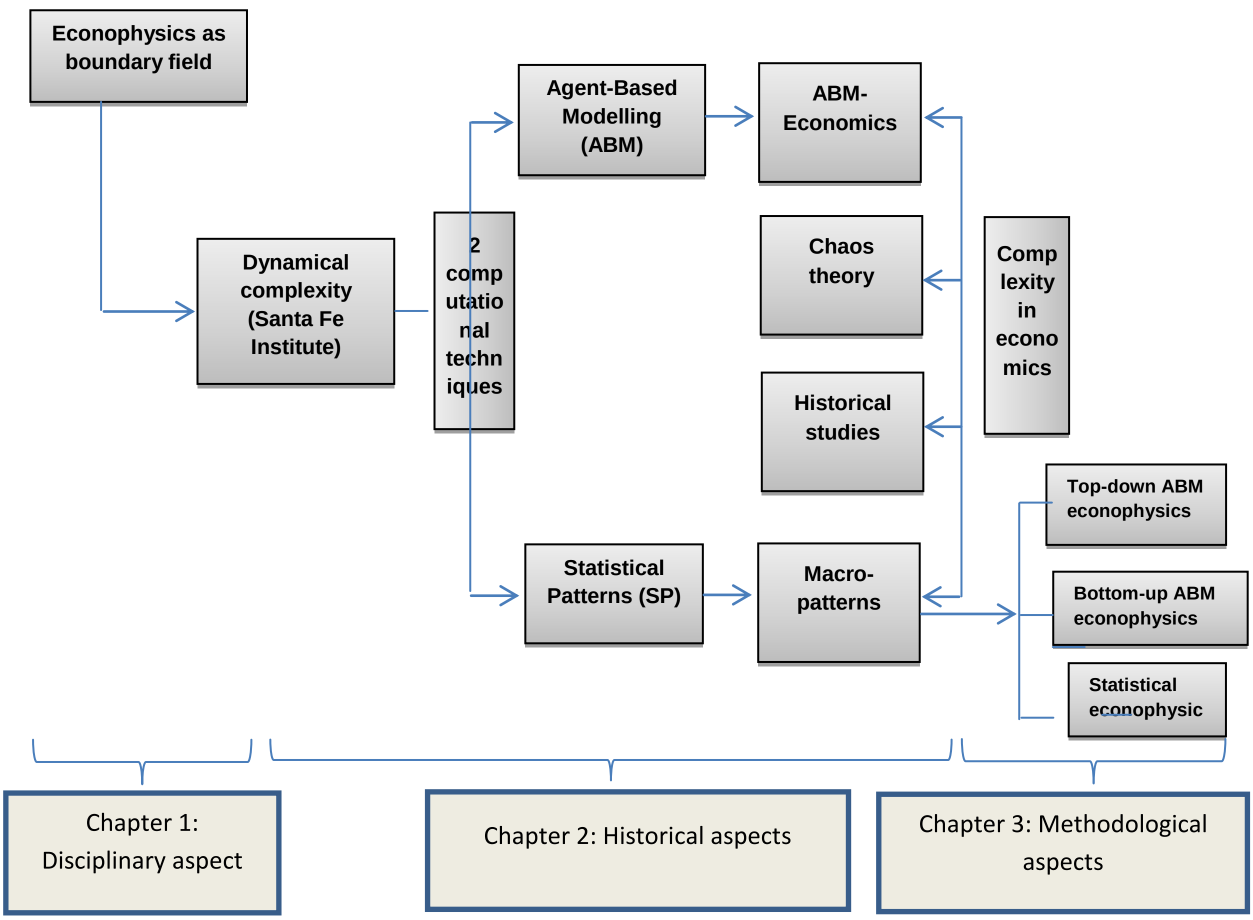
Chapter 3: The methodological diversification of econophysics

I. Introduction

We now know that econophysics is not a discipline in a traditional sense (Chapter 1) and we have some ideas of where it came from within 20th century science (Chapter 2). Now it is time to investigate the methodology of econophysics. In this chapter, I argue that there is not one methodology, but rather a diversification. Indeed, as explained earlier, econophysics is often associated with the umbrella of complexity and two of its computational techniques (statistical macro patterns analysis and agent-based modelling). What is interesting is to study how these two techniques are really implemented in econophysics. This chapter aims to clarify this point by showing how these two approaches can be actually decomposed into several methodological perspectives that have a different justification in their practitioners' eyes. Although it is always risky to generalize across different projects in any science, especially a new and controversial one such as econophysics, I will attempt to present a categorization of three methodological perspectives. This is a non-trivial task because econophysics is a very recent field, and no clear statement of its goal and methods has been articulated. The main contribution of this chapter is to categorize the ideal types of this field that, in the final chapter, I can evaluate for their adequacy.

In the history and philosophy of science, Kuhn, Lakatos and Laudan have proposed conceptual frameworks to characterize the diversity of perspectives in scientific knowledge. In accordance with these philosophers, I will assume a distinction between the system of procedures (methodology) used in a specific area of knowledge and the set of concepts/standards through which we can evaluate existing methodologies. Specifically, this chapter will use a Lakatosian angle to characterize the methodological diversity of econophysics because this philosophical approach offers a particular way of characterizing diversity within a unified and coherent field. I explain later in this chapter the reasons of this choice.

The story of this chapter is at once one of the diversification of econophysics into three traditions and a story of a progressive rapprochement between these traditions. The best way to appreciate the dynamics of this rapprochement is to adopt the Lakatosian concept of research programme to characterize the almost simultaneous existence of three research approaches under the umbrella of econophysics. Interestingly, this evolution suggests a convergence that will be nicely represented with a progressive articulation of a common hard core. Despite its Lakatosian background, this chapter differs from the usual historicist theories developed by Lakatos (Kuhn or Laudan) simply because my intention is not to deal with the polemical issue of unity of science but rather to highlight the existence of different ways of doing science in econophysics. To situate this chapter, I provide hereafter a schema (figure 1 below) that summarizes the process developed in the first three chapters:



The first chapter dealt with the disciplinary nature of econophysics by presenting the field as a boundary field that emerged in the 1990s. The second chapter focused on the history of econophysics in which the emergence of econophysics is clearly associated with the identification (and analysis) of statistical patterns in economic/financial data. This third chapter will show that this field has evolved towards a diversified situation where econophysicists combine their original statistical methodology with agent-based modelling. Precisely, echoing the two computational techniques developed at the SFI, econophysics can be decomposed into three ways of doing econophysics (statistical econophysics, bottom-up agent-based econophysics and top-down agent-based econophysics)[122].

Although the ways of using the label "econophysics" presented above deal with economic complexity, they conceptualize this notion differently. Indeed, these approaches explicitly associate economic complexity with the idea of emergence, but they all propose a different way of modelling this notion. This chapter aims to clarify the links that can be made between this concept of emergence and the different ways of doing econophysics. Before investigating these three traditions in econophysics, I will define (section 2) the key concept of emergence, which plays a central role in methodological characterization of econophysics. Afterwards, the chapter will present the three approaches, starting with the strictly phenomenological approach used in statistical econophysics, which will be associated with a "strong emergentism" in the philosophy of science. The methodological foundations of the bottom-up agent-based econophysics will then be discussed before the presentation of the more recent top-down agent-based modelling econophysics by presenting this very recent trend as the more integrative approach. This chapter will also pave the way for additional philosophical debates about the role of models and explanations in econophysics and financial economics (Chapter 4 will deal with this aspect).

[122] See my paper published in *Contemporary Physics* (Schinckus, 2013) for a classification and a complete review of the main works published in these sub-categories of econophysics. This chapter is partly based on this article (for the literature review) but it will go further in the analysis of the epistemological foundations of econophysics.

II. Preamble: Econophysics and the notion of emergence

The previous chapter introduced two computational approaches, both of them mainly developed by the SFI scholars. I explained how the agent-based perspective progressively became the major research theme of the SFI for describing economic systems. In this context, the literature related to statistical patterns has gone its own way and gradually gave birth to econophysics in the 1990s. What this chapter will show is the methodological affiliation between these statistical patterns and agent-based modelling. Roughly speaking, the two approaches did not interact for more than a decade until the 2000s when scholars started to combine them. Interestingly, this late interaction echoes earlier works initiated by the SFI, and they give birth to three different approaches in today's econophysics: statistical econophysics, bottom-up agent-based econophysics and top-down agent-based modelling. These three approaches explore the relationships between the micro and the macro levels in different ways, but all of them deal with the phenomenon of emergence, which can therefore be seen as a key element of econophysics.

Emergence is a complex notion that generates a great deal of philosophical discussions (Kauffman, 1993; Hodgson, 1997; Jean, 1997; Kim, 1999; Batterman 2002; Butterfield, 2011a, 2011b). Although emergence can take various forms[123], it is often associated with the claim that "things can be greater than the sum of their parts". More formally, emergence can be defined as "the arising of novel structures, patterns and properties during the process of self-organization in complex system" (Goldstein, 1999, p. 50). In this context, this notion of emergence often indicates the presence of properties that cannot be explained as the consequence of the simple aggregation of micro components. This situation suggests that emergence appears as the opposite of what philosophers call "reducibility". This chapter will review the major debates about this tension between emergence and reducibility, which became a "widespread ideology" (Butterfield, 2011, p. 3) in the philosophy of

[123] See Cunningham (2001) for a taxonomy of emergence.

science. Specifically, this chapter will investigate this tension in the context of econophysics and its methodological diversification.

Cunningham (2001, p. 62) reminds us that emergence is an old idea that was reemployed in the 1990s with the development of “complexity science” in which we observe a “re-emergence of emergence”. The idea of emergence dates back to the old British Emergentism attributed to Mill (1843), Alexander (1920), Morgan (1923) and Broad (1925). In opposition to the reductionist framework that dominated science between the 1930s and the 1960s, the emergentism inspired by Broad (1925) considered that emergence referred to the properties of the whole, which, on one hand, cannot be deduced from the properties of the parts; and on the other hand, is not reducible to the laws governing these parts. From this perspective, emergence appeared as a macroscopic phenomenon with no micro foundations, leading some reductionist authors (Epstein, 2006; Gregersen, 2002) to consider that emergentists favoured an “anti-scientific explanation” (Epstein, 2006, p. 32). In relation to this anti-scientific critique of the emergentist approach, in the 1940s, Hempel and Oppenheim (1948, p. 568) explained that:

> “This version of emergence is objectionable not only because it involves and perpetuates certain logical confusions but also because not unlike the ideas of neovitalism, it encourages an attitude of resignation which is stifling to scientific research”.

The confusion emphasized by Hempel and Oppenheim (1948) arises because logically speaking, emergent properties (from an emergentist perspective) are not deducible from the micro ones. However, we can only deduce propositions in a formal language from other propositions formulated in this same language. If a macro proposition (theory explaining the whole) contains terms that are not terms of the micro propositions (theory explaining the parts) then, of course, it is impossible to deduce the macro level from propositions describing the micro level. In this context, the “whole” is not deducible from its parts for purely logical reasons and then, the emergence is *trivially* not deducible. Hempel and Oppenheim (1948), and more recently Stephan (1992), showed that the non-deducibility is always relative to the proposition (i.e. to a specific formal language) used to characterize the micro and macro level. So non-deducibility does not establish absolute or ontological emergence, as the classical emergentists claimed it did.

The linguistic perspective initiated by Hempel and Oppenheim (1948) was developed by Nagel (1961), who associated scientific process with reduction. More precisely, Nagel (1961, p. 338) assumed that "reduction [...] is the explanation of a theory or a set of experimental laws established in one area of inquiry, by a theory usually though not invariably formulated for some other domain". Reduction is therefore defined through the logical idea, according to which a theory can be a definitional extension of another (Nagel, 1961, p. 351). As Butterfield (2011, p. 6) explained:

> "Writing *t* for "top" and *b* for "bottom", we say: T_t is a definitional extension of T_b if one can add a set *D* of definitions, one for each of T_t's non-logical symbols, in such way that T_t becomes a sub-theory of the augmented theory $T_b \cup D$".

When the reduced theory (T_t) is derivable from the descriptive premises contained in the reducing theory (T_b) and that terms used in T_t have approximately the same meaning that they have in T_b, then Nagel used the label of "homogeneous reduction". Although Nagel (1961) illustrated this kind of reduction through the reduction of the Galilean laws of falling bodies to Newtonian mechanics, the idea of approximation used by the author was problematic. Sklar (1967) underlined the logical problems generated by this "approximation", which, strictly speaking, made incompatible the process of reduction. Nagel (1961) was aware of this point since he also developed the idea of "heterogeneous reduction", which refers to a reduction process that involves revising the reduced theory. In other words, the reduced theory (T_t) contains terms or concepts that do not appear in the reducing theory (T_b). The classical example of heterogeneous reduction is the one of thermodynamics to statistical mechanics because the first "contains the concept of temperature (among others) that is lacking in the reducing theory of statistical mechanics" (Batterman, 2012, p. 2)[124].

[124] Batterman (2012, p. 4) explained that the reduction of classical thermodynamics to statistical mechanics fails because the reducing theory cannot associate a non-statistical feature with the concept of temperature. Thermodynamics is not originally a statistical theory and the possibility finding a bridge relationship "that captures the concept of temperature and the strict, non-statistical role it plays in thermodynamics seems impossible" (Batterman, 2012, p. 4). See Batterman (2012) for a detailed analysis of the reduction of thermodynamics to statistical mechanics.

Although this heterogeneous version of reduction allows the modeller to avoid the blurred approximation evoked above by integrating the idea of potential new properties, it is still problematic from a logical point of view: if the reduced theory contains terms that do not appear in the theoretical assumptions of the reducing theory, then the logical derivation of the first from the latter required a *condition of derivability* (Nagel, 1961, p. 352). Butterfield (2011a, 2011b) explicitly associated this condition of derivability to the idea of deducibility. This condition can take the form of "suitable relations" between the two theories (these relations are also called "bridge assumptions or laws"[125]). Schaffner (1976) explained that these bridge relations between reduced and reducing theories require an empirical justification[126].

This concept of heterogeneous reduction paved the way for a connection between the concepts of reduction and of emergence: by allowing the reduced theory (T_t) to contain concepts that do not appear in the reducing theory (T_b), the heterogeneous reduction opened the door for a semantic shift in which what appears as the emerging properties is actually associated with the new concepts evoked in the reduced theory. In that context, the question is, how did these properties emerge? This is a deep philosophical question whose scope goes far beyond the object of this work[127]. In this dissertation, the notion of emergence will be analyzed through the way econophysics characterizes emergent properties (in so doing, I will not assume whether these properties really emerge in reality or not). This research deals with epistemological dimension of emergence and not with the metaphysical aspect of the phenomenon. While ontological emergence raises controversial questions (what are the true causes of it?), the epistemological emergence is sufficient for understanding what is happening in econophysics. Indeed, the methodological diversification of this field can actually be illustrated through a variety in the ways of dealing with

[125] An example of bridge law is the association of heat with the mean molecular motion in the reduction of thermodynamics to statistical physics. See Kim (1998) for a discussion of this aspect.
[126] Shaffner (1976) illustrated his claim with the following example: "Genes were not discovered to be DNA via the analysis of meaning; important and difficult empirical research was required to make such an identification" (Schaffner, 1976, p. 615).
[127] This creation of new properties can take two forms: *ontological emergence* or *epistemological emergence.* The first refers to the idea that reality contains emergent properties, while the latter considers emergence as a semantic gap between the way of describing the micro and the macro levels. I will not deal with these philosophical debates here. See Siberstein and McGeever (1999) or Butterfield (2011a, 2011b) for further detail on this topic.

emergence in economic phenomena (Schinckus, 2013). Precisely, the three computational approaches (statistical econophysics, top-down agent-based econophysics and bottom-up agent-based econophysics) in question consider emergence as what has to be modelled (i.e. explanandum). This common perspective opens a door for a methodological classification based on the idea that these traditions have a common set of assumptions (i.e. hard core) that are implemented in different ways.

III. Methodological diversification of econophysics: Overview

The era of complexity in economics combined with the last financial/economic crisis has generated several debates about the relevance of economic theory. This challenging context favoured the emergence of a variety of new approaches that are trying to deal with complexity in economics. In this fragmentation of economic knowledge, Colander et al. (2004) explained that the "profession will, over time, adopt certain kinds of technical, mathematical, analytical and statistical tools to deal with that complexity [in economics]" (Colander et al., 2004, p. 358). Econophysics is a good illustration of this fragmentation of economic knowledge.

On the one hand, statistical econophysics offers a strictly phenomenological perspective of economic/financial systems and is founded on the self-criticality theory, according to which a dynamic system composed of a large number of interacting elements tends to have a critical point as an attractor. Such framework led physicists to describe the evolution of financial markets by using macro laws (taking the form of a power law). On the other hand, agent-based econophysics emerged because of increasing demand for a microscopic approach in econophysics whose original methodology was considered by some physicists (Farmer, 1999; Sornette, 2003) as too phenomenologically orientated. In this challenging context, some key econophysicists (Cont and Bouchaud, 1997; Farmer and Foley, 2009; Sornette, 2003), in the 2000s, promoted the creation of a methodological bridge between agent-based modelling and the statistical perspectives originally used in econophysics[128].

The literature dealing with agent-based models, which come from physics but are applied in economics can be separated into two categories: on the one hand, we have research characterizing the emergence of specific macro properties without using a pre-defined macro pattern, and on the other hand, one can find works whose objective is to reproduce existing macro statistical patterns that are taken as being given from previous empirical observations. It is worth mentioning that the first approach is quite similar to the one used by economists i.e. a modelling to characterize the emergence of a specific macro result (without conceptual assumption information about the emerging macro output) in which all micro interactions are defined through plausible assumptions. The second approach instead uses agent-based techniques in order to reproduce existing data in which all micro interactions between agents are initially calibrated in order to generate a pre-existed (given) macro pattern. In other words, the first category of works does not expect any kind of macro patterns (bottom-up approach) whereas the latter aims at reproducing (through calibration of micro interaction) a specific given macro pattern (top-down approach).

These two perspectives that deal with micro-macro interactions combined with the statistical approach, as mentioned above, lead us to consider three ways of conceptualizing the notion of emergence in econophysics: 1) statistical econophysics (or the original econophysics); 2) bottom-up agent-based econophysics; and 3) top-down agent-based econophysics. The next section will detail how these three methodological traditions are related (and how they refer to the concept of emergence).

The argumentation will be developed through a series of key points identified for the three traditions: for each of them, I will propose how they implement the asymptotic

[128] See, for example, the recent publication of a book entitled "Agent-based econophysics" at Springer Press—Abergel et al. (2014).

reasoning in their works (I will label this aspect with the word "machinery"). Afterwards, other aspects will be discussed for each approach: I will define the initial conditions (starting points) these traditions require to implement their methodology and I will clarify the goals, the outcomes and the way they deal with the notion of emergence.

IV. Statistical econophysics

Roughly speaking, statistical econophysics can be defined as an area of knowledge that deals with the phenomenological characterization of statistical patterns that macroscopically describe the dynamics of complex economic systems. In this approach, the notion of emergence is often associated with statistical regularity that is observed in a very high number of past macro interactions. However, the phenomenological identification of the macro patterns implicitly requires the existence of a micro activity; the way the micro dynamics finally fits with a specific macro patterns is not detailed. Furthermore, because micro states are judged as being too complex to be micro defined, the identification of macro properties can help in characterizing a potential description of the micro level (McCauley, 2004).

Beyond the presentation of this methodological tradition, this section aims to deal with several philosophical questions that the idea of statistical regularity raises. These aspects will be investigated in the fourth sub-section. The first sub-section will present the initial conditions/results as well as the kind of machinery (computational tool) this approach uses. The second sub-section will introduce the scientific justification of such an approach. Afterwards, I will discuss how the idea of emergence can be perceived as a phenomenological invariance that avoids the description of micro states.

IV.1. An asymptotic machinery

As previously mentioned, statistical econophysics is mainly founded on statistical processes. This machinery refers to a particular objective through a specific combination of inputs and outputs. This methodological tradition considers that economic systems are composed of multiple interacting components (not learning agents) that are assumed to interact in such a way that they generate macro properties for systems (Rickles, 2008). Knowledge developed by this kind of work mainly results from the analysis of past data that scholars try to describe through complex statistical processes. In other words, the objective of this area of knowledge is to describe the statistical regularities that arise in the observation of financial (or economic) time series/data and that seem to be persistent across various time periods, places, markets, assets, etc. (Chakraboti et al., 2010, p. 994). The more data econophysicists have, the more reliable the statistical machinery will be. From this perspective, the input (initial conditions) of such a technique is the high number of past data related to the system, which has to be explained/clarified. In terms of output (results), the accumulation of observations allows econophysicists to observe a specific statistical regularity, which often takes the form of a power law. Concretely, the way to produce this output consists of visually checking on a simple histogram that the frequency distribution of the quantity of x appears as a straight line when plotted on double logarithmic axes. If a distribution falls approximately on a straight line, then one can consider that the distribution follows a power law, with a scaling parameter *α* given by the absolute slope of the straight line. Such visual investigation has guided econophysicists' empirical research (Mantegna and Stanley, 1999; Jovanovic and Schinckus, 2017) and can be illustrated with the following figure:

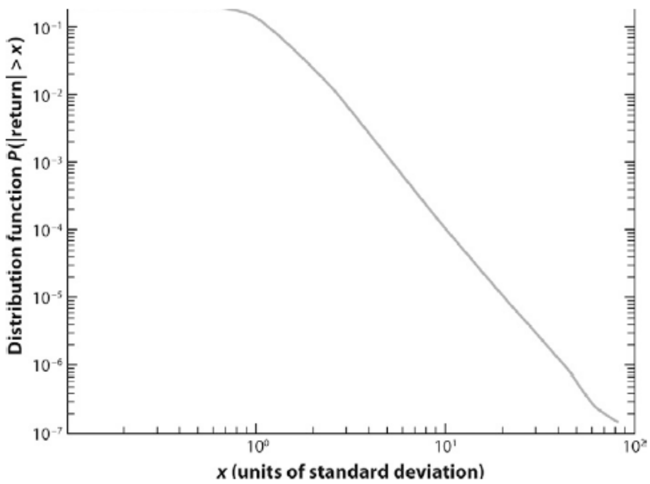


Figure 2: Empirical cumulative distribution of the absolute values of the normalized 15-min returns of the 1,000 largest companies in the Trades and Quotes database for the 2-year period 1994–1995 (12 million observations)—source: Gabaix (2009, p. 276).

This kind of visual relationship has been observed in many financial and economic phenomena. The reasons and the way econophysicists use the log-log system was explained in detail earlier. Statistical econophysicists tend to see this linearity in a large collection of empirical observations and, according to them, this repetition is not due to a(n) (un)happy coincidence; rather it stresses the phenomenological universality of power laws that emerge in different contexts. This idea of universality has supported the claim, according to which methods and models from statistical physics could be applied outside physics (McCauley, 2006; Mantegna and Stanley, 1999).

In the light of the elements presented above, power laws appear as an emergent behaviour that is totally unexpected from the mere analysis of interactions between individual components. Analytically speaking, this novel (not expected) and robust (regularly observed) results from the idea that the macro system can be perceived as a sequence of micro systems whose parameters can go to infinity. In order words, power laws appear as a novel behaviour by taking the limit $n \to \infty$ where *n* is the number of observations in accordance with,

$$\lim_{n \to \infty} T_2 = T_1$$

In this relationship, T_1 refers to the power law observed at the macro level while T_2 rather characterizes the (unknown or unnecessary) description of micro interactions. However, the concept of limit is a mathematical artefact that could be questioned in the physical world where systems always appear as finite. To put it in other words, one could ask whether this use of asymptotic reasoning makes sense from a physical point of view. As previously mentioned, econophysics comes from statistical mechanics (and more precisely phase transitions analysis) where physicists are used to working with what they call the "thermodynamic limit"[129], according to which a theoretical description of a phase transition requires that one take a limit towards

[129] It is worth mentioning that phase transitions analysis exists that does not use this concept of thermodynamic limit. See Gross (2001) for further details on this approach.

infinity of the number of constituents. The usual justification for this limit is mathematical convenience, since a collection of 10^{23} particles is infinite from a practical point of view. Indeed, the high number of variables—as many as Avogadro's number, 6×10^{23}—generates a gigantic number of equations of motion that have to be resolved[130]. This high number of relationships makes a strictly equations-based analysis unworkable, even for a computer. "Quite plainly, this is impossible … [the] subject is so difficult that [physicists] are forced to adopt a radically different approach to that employed in other areas of physics" (Fitzpatrick, 2012, p. 4). In this context, the implementation of this mathematical artefact does not mean that the limit $n \rightarrow \infty$ is physically real, but just that its use makes sense from a computational and physical point of view[131].

This infinite limit that provides a finite result offers an interesting mathematical structure for describing the evolution of a system composed of a high number of elements. Furthermore, this asymptotic characterization of emerging properties provides a mathematical derivability in line with the heterogeneous version of reduction. Since no physical systems are infinite, a key question for scholars is to know when an emergent limiting behaviour (i.e. the ability to identify a macro pattern) will appear. The next section will investigate the theoretical framework for justifying emergence in statistical econophysics.

IV.2. Phenomenological invariance and renormalization group theory

In this section, I clarify the scientific justification (renormalization group theory) used by physicists to apply their asymptotic reasoning. This presentation will also give me the opportunity to further discuss the relationship between statistical econophysics and the idea of emergence. For econophysicists, financial/economic systems can be seen as a self-organized entity whose large fluctuations seem to follow a power law, which is often presented as an emerging statistical macro outcome. Two aspects of

[130] As Fitzpatrick (2012) noticed, to solve a system with 6×10^{23} particles exactly, we would have to write down 1,024 coupled equations of motion, with the same number of initial conditions, and then try to resolve the system.
[131] The use of limit can also be physically motivated by the idea that the density ($\rho = n.v$) remains fixed when both the number of components (n) and the volume (v) tend to infinity (Butterfield, 2011b).

this invariance are philosophically fascinating: 1) the “universality” of these power laws that seem to describe several self-organized phenomena; and 2) the meaning of these macro laws (what econophysicists conclude from these macro patterns).

The first aspect (universality of power laws) probably enhances the modellers’ expectations and the prospect of observing the emergence of such invariance in different phenomena. The universality of these emerging laws is so widespread in the minds of some scientists[132] that they associate the emergence of this statistical invariance with “the essential dynamic process for everything that evolves and becomes complex” (Paczuski and Bak, 1999, p. 2). From this perspective, an expected universality of power laws can implicitly influence the detection of patterns observed by analysts (Crutchfield, 1994). This is supported by the analysis proposed by Newman (2005), which showed that the distinction between a power law and an exponential one is not so easy, explaining that scholars’ expectations can play a significant role in the identification of macro patterns. Although this aspect does not totally explain the reason why power laws are so important for physicists, it partially highlights why economists and econophysics describe the same macro emergent properties (i.e. the occurrence of extreme values) with different statistical interpretations. Both have disciplinary expectations: economists expect to observe a Gaussian law that is justified by micro economic foundations, whereas statistical econophysicists believe that these properties can be characterized macroscopically through a power law (I will come back to this aspect in Chapter4).

Let us here consider the second interesting aspect: what does the existence of power laws imply? What do these macro patterns mean for a system? Basically, the observation of power laws means that there is a constant ratio between the probability of observing an event of magnitude x and observing one of x’. This ratio does not depend on the standard or measurement; it is constant, regardless of the “scale of observation” (as explained in an earlier chapter). In other words, when a system is characterized through a power law, a constant relationship is presupposed

[132] Newman (2005).

between components (micro level) and the system (macro level), explaining why power laws are also called scaling laws. This constancy is therefore considered as a macro property that results from the behaviour of a large number of interacting components from lower levels. The renormalization group theory is related to the idea of self-criticality (introduced in the previous chapter) in which interactions between micro components are associated with "degrees of freedom […] allowing them to barely survive under different conditions" (Bak, 1994, p. 493). As Frigg (2003) explained, the self-organizing criticality does not provide a clear definition of "interaction". Instead, this word of "interaction" appears to be a filler term[133] (Craver, 2003) in order to characterize the fact that "something is moving" between units whose behaviours generate a power law. This notion of interaction is blurred in self-criticality because this conceptual framework does not require a clear identification for what happens at the micro scale. Actually, micro interactions are even judged to be too complex to be reduced (and then defined) through a mere analytical form (McCauley, 2004). The question now is to clarify how physicists can justify this approach, assuming that micro interactions are too complex to be captured. This is the contribution of the renormalization group theory presented in the next section.

IV.2.1) Renormalization Group Theory

The theoretical foundation of the asymptotic reasoning used in statistical econophysicists refers to what physicists call the "renormalization group theory". In 1982, the (high energy or elementary particle trained) physicist Kenneth Wilson received the Nobel Prize in Physics for his contribution to the connection between macroscopic and microscopic levels. More precisely, Wilson was awarded the prize for having developed the renormalization group theory for critical phenomena in connection with phase transitions[134]. The systematic study of such a critical phenomena emerged in the 1960s when physicists observed the emergence of

[133] A filler term refers to a word with a "minimal lexical content [that] play[s] a strategic role in an unfolding utterance (Fox, 2010).
[134] As Lesne and Laguës (2011, p. 3) point out, the study of critical phenomena was initiated by Cagnard de Latour in 1822 and then boosted with the work of Andrews from 1867 onwards. In 1869 Andrews observed a spectacular opalescence near the critical point of carbon dioxide. However, "the 1960s saw the emergence of a new general approach to critical phenomena, with the postulation of the so-called scaling laws, algebraic relations holding between the critical exponents for a given system" (Hughes 1999, p. 111).

macro regularities in the evolution of complex systems. Before going further, it is worth explaining what physicists mean by “critical phenomena”. This concept is used to describe systems whose configuration evolves through a dynamics of critical states. A critical state is a particular configuration of the system in which two phases (or two states) are about to become one. The most telling example is water. Water is commonly known to be liquid in a room condition but when the temperature or the pressure of this environment changes, the state of water changes as well (Figure 3).

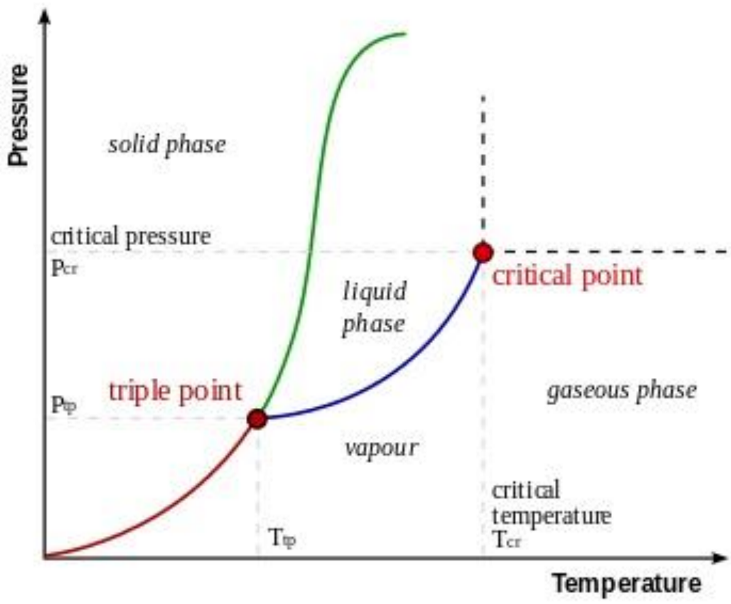


Figure 3: Temperature-pressure phase diagram for a fluid—source: adapted from Batterman (2002).

The transition of one state into another one is due to the gradual change of an external variable (temperature or pressure); it is simply called “phase transition” in physics. This transformation can be likened to the passage from one equilibrium (phase)[135] to another. When this passage occurs in a continuous way (for instance, a continuous variation of temperature), the system passes through a critical point that is defined by a critical pressure and a critical temperature and at which neither of the two states is realized (Figure 3). This is a kind of non-state situation with no real difference between the two configurations of the phenomenon—both gas and liquid

[135] It is important to mention that the concept of equilibrium can be associated with the notion of phase in an “all other things being equal” based analogy. Indeed, in the case of continuous variations of pressure/temperature, the phase is progressively moving toward a critical state, implying that it cannot be associated with a static equilibrium.

coexist in a homogenous phase. Indeed, physicists have observed that at the critical point, the liquid water, before becoming a gas, becomes opalescent and is made up of liquid water droplets, made up of a myriad of bubbles of steam, themselves made up of a myriad of droplets of liquid water, and so on. This is called critical opalescence. In other words, at the critical point, the system appears the same at all scales of analysis. This property is called “scale invariance”, which means that no matter how closely one looks, one sees the same properties. In contrast, when this passage occurs in a discontinuous way (i.e. the system “jumps” from one state to another), there is no critical point. Phenomena for which this passage is continuous are called critical phenomena (in reference to the critical points). Since the 1970s, critical phenomena have captured the attention of physicists due to several important conceptual advances in the characterization of scale invariance through the theory of renormalization[136] on the one hand, and to the very interesting properties that define them on the other. Among these properties, and probably the most important one for this study, the fact that the dynamics of critical states can be characterized by a power law deserves special attention because this is a foundational element of econophysics. As explained in Chapter 1, power laws refer to a specific statistical process that can be characterized by a critical exponent. This parameter refers to the “dimensionless number [that] characterizes the (virtually) identical behaviour of systems as diverse as fluids and magnets near their critical points” (Batterman, 2002, p. 37). In a fluid context, this exponent characterizes, independently of the chemical constitution of the fluid, the behaviour of its density as a function of temperature near a critical point, whereas it describes the behaviour of magnets when they undergo a ferromagnetic transition[137]. From this perspective, the micro details about the structure of a particular fluid are irrelevant for describing the behaviour of the system. As Bouchaud (2001) and Galam (2004) explained, physicists began to observe these power laws in more and more interacting components based systems. The so-called modern theory of phase transitions along

[136] Lesne and Laguës (2011) and Lesne (1998) provide an extremely clear and exhaustive presentation of renormalization methods. These papers make a very good introduction to intuitions and formalisms.
Stanley (1999) provides a short presentation. See also Wilson (1982 [1993]), Jona-Lasinio (2001), Calvo et al. (2010) or Stanley (1971) for further details.

[137] In this section, I will focus only on an illustration related to the fluids without presenting the details of the Ising model, which is related to the ferromagnetic transitions. See Hughes (1991) for a philosophical analysis of this model or the next chapter for the importance of an Ising model in econophysics.

with renormalization group techniques brought condensed matter physics into its golden age, leading several hundred young physicists to enter the field with a great deal of excitement. Of course, one could ask for an explanation of such phenomenological regularity and why one can observe it on very diverse systems. That is the objective of the renormalization group theory.

As previously mentioned, Wilson won the Nobel Prize for his method of renormalization, which he used to mathematically demonstrate how phase transitions occur in critical phenomena. His approach provides a conceptual framework for explaining critical phenomena in terms of phase transitions and enabling exact resolutions.

> “The development of [the renormalization group] technique undoubtedly represents the single most significant advance in the theory of critical phenomena and one of the most significant in theoretical physics generally” since the 1970s (Alastair and Wallace, 1989, p. 237).

The renormalization group theory has been applied in order to describe critical phenomena that are characterized by the existence of critical states in which the phenomenon shows the same properties independently of the scale of analysis. The major contribution of the renormalization group theory is to propose a conceptual framework offering a better understanding of phase transitions. The simplest way to illustrate this framework is to consider a fluid whose difference in densities of the vapour and liquid present in the container is given by the term $\emptyset = \rho_{liq} - \rho_{vap}$. The behaviour of this fluid depends on a critical temperature T_c below which one can observe a simultaneous presence of two phases of the fluid as illustrated in the following graph:

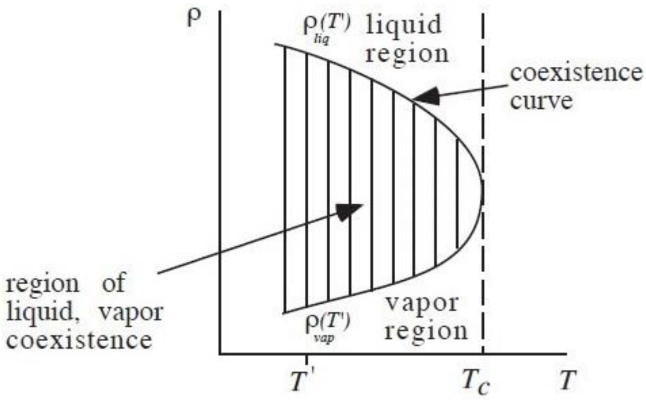


Figure 4: Illustration of the behaviour of a fluid at critical state—source: Batterman (2002, p. 40)

Experimental data show that near the critical point, fluids exhibit a coexistence state as illustrated by the curve on Figure 4. Physicists observe that this curve can be characterized by a particular relationship between the difference of densities and temperature since $\emptyset \sim t^{\alpha}$ (where $= \frac{T - T_c}{T_c}$, which refers to the difference in temperature from the critical temperature in dimensionless units). The clarification of such behaviour is given by the renormalization group theory, which assumes that every system (fluids, magnets, etc.) can be represented by a function (i.e. its Hamiltonian). This function describes the kind of interactions between the system's components. When the fluid is in its gaseous or liquid phase, components are weakly correlated with each other, implying that these elements interact only with the nearby component (and therefore that they are almost uncorrelated with others). When the system is near its critical point, interactions between components increase such that that all of them contribute to the physics of the system. In other words, the length of correlation[138] between components grows without bound. This statement is very interesting and Chapter 4 will detail how econophysicists analogically apply their framework to financial/economic systems.

This section presented the renormalization group theory; the following one will detail how this theory is used as a scientific justification for exporting condensed-matter physics to other areas of knowledge.

[138] The length of correlation refers to the average length-scale on which microscopic elements are correlated. I will investigate this aspect in more detail in Chapter 4.

IV.2.2) Renormalization Group Theory as scientific foundation

To characterize such phenomena, renormalization group theory proposes an abstract space (the space of Hamiltonians) in which all transformations preserve the form of the initial Hamiltonian that describes the real physical system. Such method enables the renormalization of parameters in such a way that the new renormalized function characterizes a system that exhibits similar behaviour. This theory can be looked on as a set of transformation combining components by keeping a specific scale invariance (which is a key property of power law). While the concept of invariance refers to the observation of recurrent characteristics independently of the context, the notion of scale invariance describes a particular property of a system/object or law that does not change when scales of length, energy or other variables are multiplied by a common factor. In other words, this idea of scale invariance means that one (or some) recurrent features can be found at every level of analysis. Concretely, this means that a macroscopic configuration can be described without describing all the microscopic details. This aspect is a key point in the renormalization theory developed by Wilson, the scholar who extended Widom’s (1965a, 1965b) and Kadanoff’s (1966) discovery of “the importance of the notion of scale invariance which lies behind all renormalization methods” (Lesne, 1998, p. 25). More precisely, his method considers each scale separately and progressively connects contiguous scales to one another. This makes it possible to establish a connection between the microscopic and the macroscopic levels by decreasing the number of interacting parts at the microscopic level until one obtains the macroscopic level (ideally a system with only one part). Such characterization of scaling invariance allows physicists to capture the essence of a complex phenomenon by identifying key features that are not dependent on the scale of analysis.

Consider a phenomenon (a magnet, a fluid) whose interactions of micro components can be described with the following sequence: $X = X_1 + X_2 + ... + X_{kn}$, composed of *kn* random independent variables and identically distributed. The renormalization group method consists of using a scaling transformation to group the *kn* random variables into n blocks of k random variables. The pairing process implies a reduction in the number of coupled components (or degrees of freedom) within the correlation length. The transformation S_n takes the sequence X into a new sequence of random variables—still independent and identically distributed. This transformation becomes truly fruitful when it is iterated, when each renormalization leads to a reduction in the number of variables, leading to a system that contains fewer variables while keeping the characteristics of the original system—thanks to the fact that the system stays independent, identically distributed and stable[139]. For instance, considering the previous sequence X with kn = 8, n = 4 and k = 2, we can renormalize the sequence three times in order to obtain a single random variable that characterizes the initial sequence.

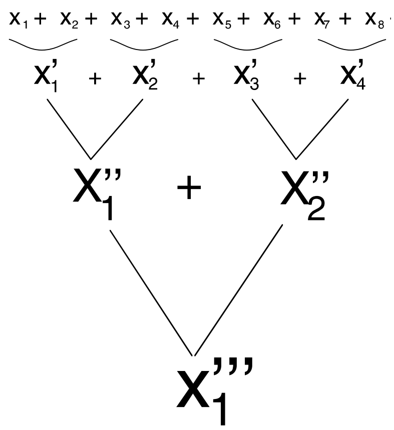


Figure 5: Renormalization group method applied to a stochastic process
Source: Sornette (2006, p. 53).

When applied several times, this pairing method allows modellers to “climb” the scales by reducing the number of variables (kn) without losing key features of the phenomena, which are captured in the scaling invariance of the process. In other words, this technique allows the grouping of random variables into (n) blocks of variables in order to reduce the initial complexity of the phenomenon. Roughly speaking, the technique can be summarized by the following equation,

$$S_n\left([X],\alpha\right) = n^{-\alpha}\sum_{j=1}^{n} X_j ,$$

[139] For more details, see Samorodnitsky and Taqqu (1994) and Lesne (1998).

where S_n is the sequence at a specific scale of analysis describing the phenomena, while X_j refers to the number variables used at that level of analysis. The α is called the "critical exponent" and it describes the scale invariance of the phenomena. Here is the important link between the power laws and the renormalization group theory: the latter can be seen as a scientific foundation of the former. The two frameworks are based on exponents that describe a universal property observed without regard for the scale. Considering the renormalization group method, the system at one scale is said to consist of self-similar copies of itself when viewed at a smaller scale, but with different ("rescaled") parameters describing the components of the system.

The scale invariance assumption was not new in physics[140], but the properties allowing the mathematical demonstration of invariance were only established at the end of the 1970s. This demonstration makes it possible to mathematically study macroscopic regularities that occur as a result of microscopic random interactions without to having study these microscopic interactions[141]. The focus is therefore on the macroscopic level, which is directly observable for physical phenomena. In other words, since the 1970s, thanks to scale invariance, from the microscopic constituents, physicists can infer some key parameters allowing them to capture and describe the dynamics of macroscopic behaviours without studying in detail what happens at the microscopic level. For these reasons, the renormalization group theory is the foundation of any modern approach of statistical physics[142] aimed at understanding the collective behaviour of systems with a large number of variables that interact with each other.

[140] For instance, it exists in the work of Euclid and Galileo.
[141] To understand the importance of this approach, it is worth remembering that the macroscopic level is supposed to be directly observable—for instance a table—but the microscopic level—the molecules that constitute the table —is not directly observable (one needs a tool, such as a microscope).
[142] It is worth mentioning this foundation is widely accepted by statistical econophysicists who implicitly assume that their readers are aware of it. Several econophysicists orally confirmed for me this implicit assumption (see Jovanovic and Schinckus, 2016 or Sornette and Cauwels, 2015 for further details on this point).

IV.2.3) Renormalization Group Theory and econophysics

Renormalization Group Theory is a very useful tool because it offers a set of transformation through which a set of variables can be replaced by another set of (usually) coarse-grained variables without changing the key physical properties of the system. These transformations are computed in a space of Hamiltionians allowing modellers to estimate fixed-points taking the form of Hamiltonians characterizing the dynamics of a system near a transition phase. Methodologically speaking, the identification of these fixed points indicates that microscopic details of the system are irrelevant in the study of its close-transition dynamics. This method suggests that different systems (e.g. fluids and ferromagnets for instance) might have same characteristics in terms of fixed points and dynamics – physicists therefore consider that these systems belong to the same universality class justifying the use of the same model (e.g. Ising model) to describe the behaviours of these systems. The identification of such universality class results from empirical observations of systems - when two systems have some empirical properties (e.g. following a power law with the same exponent) that means that these systems flow to the same fixed points by iterations of the renormalization method. In this context, they are said to have the same dynamical properties and therefore belong to the same universality class. Renormalization group theory is mainly used in econophysics to characterize either the dynamics of financial prices or to describe the occurrence of financial crashes. I discuss further these two issues hereafter.

The first issue is the major theme studied in statistical econophysics since it can be associated with the methodological origin of the field (Mantegna, 1991). Generally speaking, econophysicists acknowledge that financial prices evolve by following a power law whose exponent is estimated at 3 (Mantegna, 1991; Mantegna and Stanley, 1995; 1999; McCauley, 2004; Stanley et al. 2008). The universality of this observation is justified by the repeated occurrence in diverse contexts. Precisely, this exponent is has been observed for the majority of stocks in several financial markets: USA, UK, France, Australia, Canada, Hong Kong, Spain, South Korea, Germany and Netherlands (see Gabaix et al. 2003 for further details on the estimations). Although some authors (Farmer and Lillo, 2004 or Durlauf, 2005) explained that this universality could result from a methodological bias, they do not negate that financial markets appear to be complex with a particular universality (Rickles, 2008) [143]. From the empirical studies, one can observe the repeated occurrence of a power law exhibiting the same exponent in different countries. This situation might suggest a kind of universality for these markets. Consequently, these markets can therefore be studied as systems that are perpetually in a near-critical state[144], as Rickles (2008, p15) explained it,

> "Econophysicists, by contrast [with economists], use the statistical properties as their starting point; the basis from which to construct realistic models: the universality of the statistical properties – i.e. the fact that they reappear across many and diverse financial markets – suggests (to physicists at least) a common origin at work behind the scenes and points towards the theory of critical phenomena (with its notion of universality)".

Interestingly, although econophysicists emphasize the universality of the power law in financial dynamics, they do not necessarily detail how the renormalization group theory could actually estimate the exponent that they find. Very few works (Canessa, 2000; Wu 2012) detail the link between power laws and the renormalization group theory in econophysics. Often, scholars involved in econophysics associate this universality of exponent to the observation of a universal class consistent with the renormalization group theory. In this context, the real question is to know if the repeated observation of power law with the same exponent (i.e. universality) is a sufficient condition for mobilising the renormalization group theory. This aspect would deserve further research[145].

[143] As Rickles (2008, p.26) explained these critiques actually suggest that the complexity of financial markets should be treated with more care and attention but they do not refute the idea that there is *something in common* between all these financial markets.

[144] This statement is actually supported by the recent computerization of financial sphere which deeply changed the dynamics of financial prices by making it constantly moving.

[145] This aspect is actually a future research theme for the author of this dissertation.

The second theme to which econophysicists apply the renormalization group theory is the dynamics of financial crashes (Sornette, 2003; 2006) that generated more debates in econophysics (Bouchaud, 2001, 2002; McCaulley, 2004). Precisely, even if the renormalization group theory is mentioned as a methodological justification in the studies of financial crashes, it is worth mentioning that, although dynamics of financial crashes appear to follow a power-law, the exponent of this law does not converge to the same value. This situation therefore complicates the identification of a universality class of phenomena. Graf et al. (2003) showed that different financial crashes exhibited different critical exponents confirming that all financial crashes do not belong to the same universal class of phenomena. Related to this theme, the real question is therefore to know if the occurrence of power laws with different exponents (no universality) is a sufficient condition to mobilise the renormalization group theory as a scientific foundation. Several authors (Jhun et al., 2017; Butterfield, 2011 or Butterfield and Bouatta, 2015) explicitly rejected this possibility.

The use of the renormalization group theory as a scientific foundation for statistical econophysics is debatable depending on the kind of dynamics under consideration. Precisely, the use of this framework to characterize the dynamics of financial prices seems to be acceptable in accordance with a broader definition of a universality class propose by Batterman and Rice (2014) who promoted the use of such notion as a justification for minimal model to describe systems outside of physics exhibiting same critical exponents. However, regarding the characterization of financial crashes, it appears that there is no real methodological justification for the use of the renormalization group theory since these phenomena do not exhibit universal behaviour.

IV.3. Phenomenological invariance as an emergent property

This section aims at clarifying how statistical econophysics deals with emergence. For statistical econophysicists, economic systems are composed of multiple components interacting in such a way as to generate the macro properties for systems that take the forms of power laws. The emergence of macro patterns requires a large sample of data. In this context, the macro laws are assumed to be encapsulated, “out there”, in the existing observations, but their description requires a specific recurrence through the observations of a high number of events. This methodology, which was implemented by statistical econophysicists, is implicitly associated with the frequentist tradition in statistics. More precisely, key models used in econophysics usually consider that the probability of an event is given by the long-term relative frequency (allowing the use of asymptotic reasoning like in statistical physics [146]). The macro invariance is theoretically founded on an asymptotic reasoning in which the system is assumed to contain an infinite number of data in order to make prediction/infer information on the behaviour of real finite systems. This methodological perspective results directly from the conceptual framework used in statistical physics where:

> “The assumption that the system is infinite is necessary for the symmetry breaking [i.e. the fact that the whole is more than the sum of its parts] associated with phase transitions to occur. In other words, we have a description of a physically unrealizable situation (an infinite system) that is required to explain a physically realizable phenomenon (the occurrence of phase transitions in finite systems) […] A good deal of asymptotic behaviour that is crucial for describing physical phenomena relies on mathematical abstractions ” (Morrison, 2015, p. 28–29).

As Morrison (2015) explained, the complexity of physical systems is therefore associated with the observation of emergent properties that cannot be reduced to the sum of the system’s components. While Morrison uses the word “abstraction”, I would say instead that phenomenological invariance is conceptually founded on a minimalist idealization (I will detail this claim in the following chapter). By idealization, I mean here a voluntary distortion of reality, i.e. a process that describes situations in a way that cannot be realized in the physical world (here infinite population). This aspect is very important because it can provide theoretical representations for the description of complex phenomena. However, idealization-based reasoning calls for a simple question: how can mathematical properties [asymptotic behaviour] provide physical/economic information for a real system? This is a big question that is largely debated in the philosophy of science since it generates debates in quantum physics

[146] It is worth mentioning the distinction between the Bayesian tradition and the frequentist one: The Bayesian approach is used when it is possible to update the existing knowledge with a new information arriving from an external environment (implying a revision of beliefs in accordance with the new data) whereas the frequentist method is rather used to study events with large samples of data for which unbiased (non-subjective) estimators implemented.

(Zim-Justin, 1998), in thermodynamics (Huang et al., 2005) and in renormalization group theory (Batterman, 2002; Butterfield, 2014)[147]. A key question now is to know how this asymptotic reasoning can characterize emerging properties, such as power laws.

By considering the statistical macro patterns as a novel quality of physical or social systems, econophysicists implicitly consider these complex systems as entirely constituted by composite structures that are not mere aggregates (nor definitional extension) of the simple ones. The emerging statistical macro laws suggest the existence of a gap between micro and macro scales. Moreover, many physicists acknowledge their inability to predict these macro laws (McCauley et al., 2016). From this perspective, one can legitimately wonder whether econophysicists who deal with statistical invariance are not implicitly promoting a strong emergentism within which emergent properties cannot be reducible/deducible or predictive (Morgan, 1923; Kim, 2006). At first sight, statistical econophysics does not provide a clear formulation for the occurrence of emergent properties for two reasons: macro laws look to emerge suddenly after a high number of observations, while physicists confess they cannot predict the exact form of these laws. From this perspective, the emergence of these macro laws would be intractable/non-deducible and therefore incompatible with the classical Nagelian reduction evoked in the first part of this chapter. The rest of this section will discuss these points by analyzing the relationship between statistical econophysics and the notion of emergence.

The emergence of macro patterns through a high number of observations results from an asymptotic reasoning founded on the renormalization group theory. This conceptual framework has given scholars a comprehensive way of characterizing the emergence of power laws, implying that this phenomenon is not a mysterious figure that exists in econophysicists' minds. Despite the existence of this theory to describe how macro properties can emerge from a huge number of interacting micro elements, the association of emergence with asymptotic reasoning generates many

[147] See Morrison (2015, Chapter 2) for a good literature review on the topic.

debates in philosophy of science. Roughly speaking, the literature can be divided into two perspectives: some philosophers (Batterman, 1997, 2002; Kim, 1999) consider that asymptotic reasoning cannot related to emergence phenomena while others (Butterfield, 2011a, 2011b, 2014; Morrison, 2015) instead claim that an asymptotic reasoning offers a framework from which emerging properties can rigorously be deducible (with some limitations). These debates are interesting for econophysics because they refer to the way physicists justify their use of macro patterns to describe economic/financial systems. From a physicist's point of view, the observation of power laws in financial/economic phenomena seems to be justified, since their emergence can be characterized through the renormalization group theory. However, from an economist's viewpoint, the extension of this theory in economics/finance does not make sense simply because they cannot give an economic justification for this conceptual framework. To put it in other words, although from a physicist's point of view, this asymptotic reasoning offers a particular deducibility of emergent power laws through a heterogeneous emergence, this approach is associated with a strong sense of emergentism by economists. This interesting point directly echoes a perspective emphasized by Butterfield (2011a, p. 17) when he wrote that the "claims of deducibility are of course sensitive to exactly which theories are being considered". I will come back in Chapter 4 to these meaningful differences between economists and econophysicists.

The second aspect I would like to deal with in this section is regarding the predictability of macro laws. The ability to identify a power law in a complex system does not mean that one can predict the behaviour of this system. Indeed, the predictability must be understood in a specific way: these macro laws are not predictive (i.e. deduced or anticipated) because they are observed from past empirical data. The only thing econophysicists are able to predict is that a statistical invariance will appear in a specific form in complex systems, but they are unable to deduce the evolution of these complex systems. The emerging invariance is not deducible from initially defined rules, but rather it can be deduced from an asymptotic reasoning based on an accumulation of empirical data observed in the past. Although the exact form of the macro laws is still unpredictable, the nature of these macro laws (power laws) can be deduced from an asymptotic reasoning. Such deduction is interesting since power laws have important statistical (scaling) properties that give room for further analysis (or actions/decisions in the case of financial data).

Statistical macro pattern analysis is the first computational method used by econophysicists in their extension of physics to finance and economics. In so doing, they implemented a classical asymptotic reasoning founded on the scientificity of the renormalization group theory to characterize emergent properties. The macroscopic methodology initially implemented by econophysicists can visually be represented by the following schema:

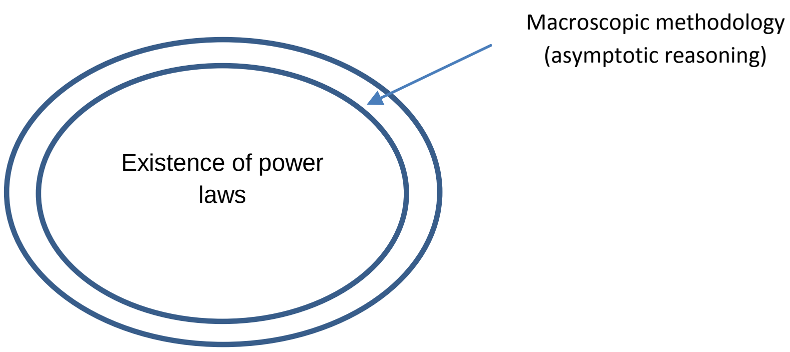


Figure 6: Lakatosian visualization of the core elements and the protective belt of the original (statistical) econophysics.

In this conceptualization, dealing with emergent properties in economic/financial systems by using an asymptotic reasoning appears as the core of statistical econophysics. This way of implementing a macroscopic method defines what can be improved or changed (without modifying the core elements). This Lakatosian characterization is interesting because it offers a way of introducing the first debates about the micro foundations of econophysics that emerged in the 2000s (Chakraborti et al., 2016). In this context, econophysics literature has progressively provided another sub-category of works that deal with micro realization of these emerging macro results. I discuss this approach in the following section.

V. Agent-based econophysics

Statistical econophysics is based on a macro approach of complex systems. In the early 2000s, one can observe an increasing demand for a microscopic approach in econophysics whose original methodology was considered by some physicists (Cont et al., 1997) as too phenomenology-orientated. Although this methodological perspective is interesting, it generated many debates and critiques that can roughly be summarized by the words of Durlauf (who was an economist involved in the Santa Fe Institute):

> "The empirical literature on scaling laws [i.e. power laws] is difficult to interpret because of the absence of a compelling set of theoretical models to explain how the laws might come about. This is very much the case if one examines the efforts by physicists to explain findings of scaling [power] laws in socioeconomic contexts" (Durlauf, 2005, F235).

The economic mainstream is founded on a microscopic approach of economic phenomena, and the phenomenological description of macro patterns proposed by statistical econophysics does not help economists understand how these macro laws emerge. Consequently, "The econophysics approach to economic theory has generally failed to produce models that are economically insightful" (Durlauf, 2005, F236). At the end of the 1990s, some econophysicists tried to address such critiques by introducing the agent-based modelling in econophysics. The main idea of this approach was to provide microscopic foundations to econophysics. In this challenging context, some key econophysicists (Farmer, 1999; Sornette, 2003; Cont et al., 1997) promoted the creation of a methodological bridge between agent-based modelling and statistical perspectives originally used in econophysics[148]. The debates about the micro approach generated a specific literature that can be divided into two traditions: bottom-up agent-based econophysics and top-down agent-based econophysics. The next two sections aim at presenting these two approaches.

[148] Although this conceptual combination between a macro-based and inductive approach (observation of statistical patterns) and a micro based and deductive method (agent-based modelling) could be seen as contradictory, the previous chapter showed that these two frameworks have a direct link with the historical roots of econophysics, since they were mainly developed by scholars of the Santa Fe Institute.

V.1. Bottom-up agent-based econophysics

This first category of papers concerns research that imports a bottom-up agent-based modelling in econophysics. Roughly speaking, this corpus works on the definition/calibration of interactions that rule the micro elements' behaviour in order to generate a macro spontaneous order. In such an approach, micro interactions are considered as inputs and the emerging macro result is seen as outputs of the process. According to Turing (1936), there is no systematic analytical method to select the best algorithm. That means that initial conditions defining the process play a key role. In this context, the initial assumptions made by the modellers for the implementation of their algorithmic have a crucial role.

Authors involved in modelling economic micro interactions will try to calibrate the basic behaviour that rules agents' interactions, which leads the system to a complex situation (i.e. within macro properties emerged). The way of defining the rules of behaviour determines the methodological perspective enhanced by modellers. Inspired by Moss (2009), I provide hereafter a specific methodological classification for works using agent-based modelling in economics and econophysics.

- the perfect agent-based modelling,
- the adaptive agent-based modelling,

The *perfect agent-based modelling* is the classical methodological individualism used in economics. This approach results from the mechanizing way of modelling rationality that was promoted by the Cowles Commission and the RAND Corporation: interactions' rules are defined in a "utility function" that is associated with a rational optimization of contextually defined theoretical constraints. The system's macroscopic behaviour is therefore deduced from the addition of individual characteristics. In this context the hypothesis of perfect rationality combined with an assumed perfect additivity of agents defines the aggregative rule at the macro level of the system.

In opposition to this perfect agent-based modelling within the principle of additivity allowing the modeller to deduce the macro level, the *adaptive agent-based modelling* instead required a large number of computerized iterations to infer the macro result[149]. In accordance with the idea that human beings have limited computing abilities, this methodology integrates the heterogeneity and the autonomy of agents considering that “individuals may differ in myriad ways—genetically, culturally, by social networks, by preferences, etc.” (Epstein, 2006, p. 6). This heterogeneity, which found its roots in the bounded rationality framework, implies some differences with the neoclassical framework. Indeed, because adaptive agent-based modelling does not require the condition of perfect rationality, this approach enlarges the means of modelling economic incentives since the algorithmically defined decision functions can integrate some concepts that come from behavioural economics, such as loss aversion, overestimation or conservatism, for example. Regarding the agents’ autonomy, the adaptive/learning abilities defined for agents ensure them a particular degree of freedom, since they can evolve depending on their plausible interaction rules inspired from economic world (Gallegati et al., 2006). These simple interaction rules are then expected to generate an emergent order far beyond individual capacities or wishes. In a sense, the only difference between the perfectly rational and the adaptive agent-based modelling refers to the method of inferring the macro level of the system: while the first is explicitly based on deduction, the latter instead requires an algorithmic simulation. When implemented in economics, these approaches are based on an *incentives-based modelling* in which (economic or/and behavioural) motivations must be initially pre-defined.

The economic mainstream promotes a modelling based on perfect rationality, while adaptive agent-based modelling has instead been promoted by alternative approaches (behavioural finance). The perfect agent-based modelling can be presented as a complementary approach to the adaptive agent-based framework (Lebaron, 2006). Some works combine perfectly rational agents with irrational agents, showing that the two frameworks can support and complement each other. As Lévy (2009, p. 20) explained:

> “The Agent Based approach should not and can not replace the standard analytical economic approach. Rather, these two methodologies support and

[149] See Epstein (2006) or Cristelli (2014) for a review of this huge body of literature.

> complement each other: When an analytical model is developed, it should become standard practice to examine the robustness of the model's results with agent based simulations. Similarly, when results emerge from agent based simulations, one should try to understand their origin and their generality, not only by running many simulations, but also by trying to capture the essence of the results in a simplified analytical setting".

Although agent-based econophysics looks methodologically similar to what economists do, there is a major difference: in opposition to the latter, the first use non-economic assumptions to calibrate the micro interactions.

As noted earlier, agent-based modelling requires a particular calibration/definition of micro interactions that are based on specific assumptions regarding the behaviour of microscopic elements that can generate an emerging order. Aggregate phenomena that exhibit unanticipated properties are not limited to social systems. In physical systems, such phenomena can also appear to show macro properties that are distinct from the properties associated with the micro components. In this context, agents are considered as interacting particles whose adaptive behaviours create different structures (such as molecules, crystals, etc.). The progressive emergence of this order is algorithmically described through computerized simulations. In accordance with what I previously called the core elements, agent-based econophysicists work on the emergent properties of economic phenomena.

An increasing literature exists that is based on this specific methodology: Pickhardt and Seibold (2011), for example, explained that income tax evasion dynamics can be modelled through an "agent-based econophysics model" based on the Ising model of ferromagnetism, while Donangelo and Sneppen (2000) and Shinohara and Gunji (2001) approached the emergence of money through studying the dynamics of exchange in a system composed of many interacting and learning agents. In the same vein, some authors used an agent-based approach to characterize the emergence of non-trivial behaviour, such as herding behaviour: Eguiluz and Zimmerman (2000), Stauffer et al. (1999) and Wang et al. (2005), for example, associate the information dissemination process with a percolation model among traders whose interactions randomly connected their demand through clusters.

Some econophysicists applied agent-based approach for studying the dynamics of order-driven markets. Bak et al. (1997) used a reaction diffusion model in order to describe the order dynamics. In this model, orders were particles moving along a price line, and whose random collisions were seen as transactions (see also Farmer et al. (2005), for the same kind of model). Maslov (2000) tried to make the model developed by Bak et al. (1997) more realistic by adding specific features related to the microstructure (organization) of the market. Challet and Stinchcombe (2001) improved the Maslov (2000) model by considering two particles (ask and bid), which can be characterized through three potential states: deposition (limit order), annihilation (market order) and evaporation (cancellation). Slanina (2001) also proposed a new version of the Maslov model in which individual position (order) is not taken into account, but is rather substituted by a mean-field approximation.

These works can be methodologically characterized by a non-economic agent-based approach since non-economic assumptions are initially made/used for the calibration of micro interactions. Actually, econophysicists define algorithmic rules that generate micro interactions in terms of “physically plausible” events, implying that agents and their interactions are described with notions such as potential states (deposition, cancellation, annihilation, etc.), thermal features (heat release rate, ignition point, etc.) or magnetic dimensions (magnetic permeability, excitation). In other words, the input in such modelling is a pre-defined set of micro interactions that are physically plausible/meaningful. By applying these existing models to describe economic phenomena, econophysicists implicitly assume a kind of physicality since they consider them a social reality that can be explained in physical terms. Indeed, by using physical concepts to deal with economic/social reality, econophysicists don’t deny that the world contains non-physical elements, such as items of a biological, psychological, moral or social nature, but, as Stoljar (2010) explained, “they insist nevertheless that at the end of the day such items are either physical or supervene the physical” (Stoljar, 2010, p. 1). In a sense, econophysicists use this “physically plausible dimension of micro interactions" as an analogy for economic relations (I will study in detail the nature and the role of analogy in econophysics in the following chapter). This way of modelling is far from the economic-incentives-based models developed by economists. Consequently, there is no link with usual economic knowledge, which explains why this kind of agent-based econophysics is largely ignored by economists (who instead implement an economic incentives-led agent-based modelling). This not economic calibration; in econophysics, modelling can be described by the following schema:

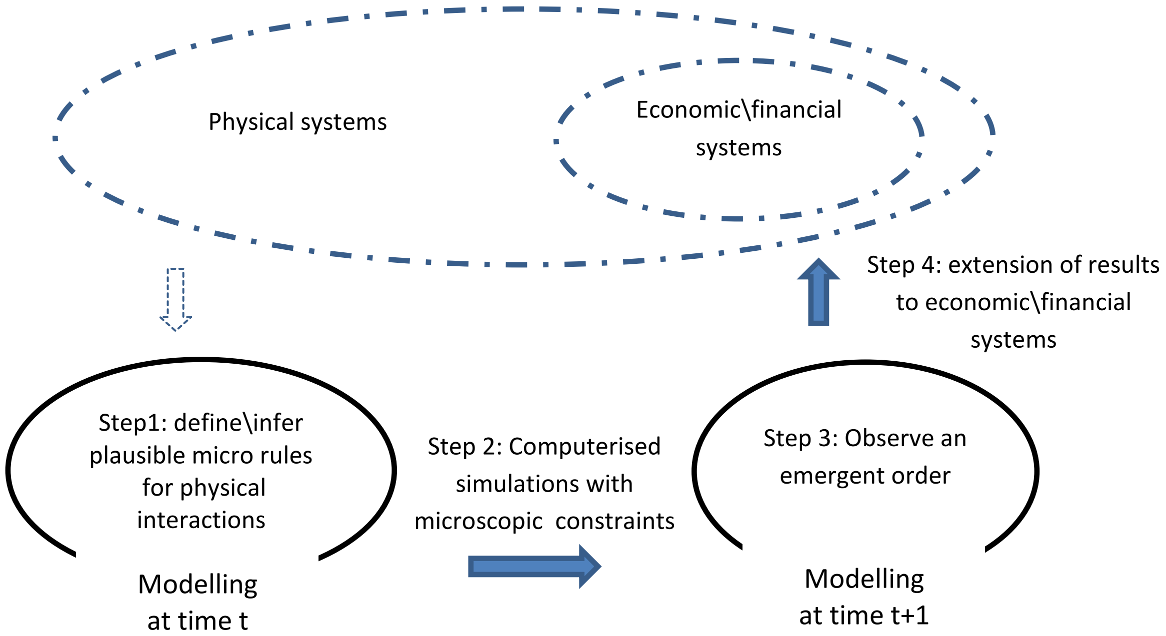


Figure 7: Modelling process for this application of agent-based modelling.

By defining a particular derivability of reduced theory (macro state) from reducing one aspect (micro level), the agent-based approach subscribes to the principle of the Nagelian definition of reduction since it transforms the first into a "definitional extension" of the latter. The modelling process starts with the characterization of the micro level whose dynamic is pre-defined in accordance with a particular conceptual framework (i.e. assumptions are physically plausible for agent-based econophysics). Afterward, computerized simulations generate an increasing number of interactions between micro components. When the number of simulations is large enough for the micro rules to become macroscopic, an invariant pattern, presented as a spontaneous order, emerged from the computerized iterations. This emergent macro order is the output of this bottom-up agent-based econophysics.

Except for the definition of the initial conditions and the physicalism mentioned above, these studies applied the same modelling processing as the one used by economists when they developed agent-based modelling. Roughly speaking, this bottom-up methodology can be associated with what Arthur et al. (1997a) called the "small tent complexity" that the authors identified through six joint characteristics: 1) dispersed interactions among locally interacting heterogeneous agents, 2) no global controller who could exploit opportunities resulting from these dispersed interactions, 3) cross-cutting hierarchical organization with tangled interactions, 4) continual learning and adaptation of agents, 5) novelty and mutations of the system and 6) out-of-equilibrium dynamics.

The agent-based modelling used in econophysics implicitly assumes an equivalence between the macro level physical and economic systems. This perspective is often justified by a naïve and approximate association of physically plausible interactions with economic interactions. For example, some physicists describe the formation of coalitions or the fragmentation of opinions on the market by using the physical phenomenon of spin glasses[150] (Galam, 2008; Pickhardt and Seibold, 2011), while others instead associated herding behaviours with a slow-diffusing process (percolation phenomenon) likely to generate sudden "breakthrough" (Eguiluz, 2000; Wang et al., 2005). It is worth mentioning that this implicit physicalism allows physicists to not study the potential reciprocal effect between the micro and the macro levels that Arthur (2014) called the "meso level". Specifically, because they deal with human behaviours, economists tried to integrate a "reciprocal causation [that] operates between different levels [… and that] may even give rise to new patterns and entities at both higher and lower levels" (Arthur, 2014, p. 94).

[150] "A spin glass is a disordered magnet with frustrated interactions, augmented by stochastic positions of the spins, where conflicting interactions, namely both ferromagnetic and also antiferromagnetic bonds, are randomly distributed" (Zhang, 2013, p. 10). This magnetic phenomenon exhibits both quenched disorder and frustration, and has often been cited as an example of "complex systems" (Stein, 2003). In the final chapter, I will come back with more detail on the use of spin in econophysics.

Authors involved in a bottom-up agent-based modelling assume that the macro level is considered derivable from the micro level whose interactions have beforehand been defined in line with specific assumptions. In this context, the emergence of properties at the macro scale is derivable from rules defining micro interactions. I discuss this aspect further in the following section.

V.1.a) Agent-based econophysics and the emergence as a spontaneous order

In the literature devoted to agent-based modelling, emergence often refers to a "stable macroscopic order arising from local interaction of agents" (Epstein and Axtell, 1996). Lévy (2009) explained that the agent-based modelling approach is a new way of investigating the complex and messy world through a micro based framework in which a spontaneous order emerged from the heterogeneity of agents. In accordance with this logical analysis of emergence, Epstein (2006, p. 33) showed how agent-based modelling and classical emergentism (i.e. emergent properties that are not derivable from the lower levels) are incompatible. Here is the summary of this logical argument: Let C stand for what is emergent in a classical sense, D for what is deduced, E for what is explained and G for what is generated or simulated (through an agent-based model), by using classical texts from literature dedicated to classical emergentism and agent-based approach, we can have the following predicates:

(1) $C \rightarrow \neg D$: classically emergent implies that it is not deducible (Broad, 1925)

(2) $C \rightarrow \neg E$: classically emergent implies that it is not explainable in lower level (Alexander, 1920)

(3) $\neg G \rightarrow \neg E$: not generated implied not explained (Epstein, 2006)

(4) $G \rightarrow D$: generated implied deduced (Epstein, 2006)

Then, $G \rightarrow \neg C$: generated (by agent-based models) implies that it is not classically emergent. In other words, the reducing theory T_b determines what is generated (G which refers to micro interactions) and allows the macro state to be deduced (i.e. a reduced theory T_t). Given this result, Epstein (2006, p. 33) concluded that "*Agent-based modelling and classical emergentism are incompatible"* (emphasis in original). The author added that classical emergentism wanted "to preserve the mystery gap between micro and macro" (Epstein, 2006, p. 33), while the agent-based approach seeks to provide an explanation for this gap by giving micro foundations to the emergence process by providing some rules on the micro scale, which will ensure the derivability of the macro level. However, although these macro properties can be algorithmically derivable from micro components, they are not deducible from the lower levels. This point is epistemologically interesting because it implies a tractable but not deducible notion of emergence. Butterfield (2011, p. 21) also mentioned this point when he wrote, "I should register the importance for heuristics of computer simulations […] In particular, computer simulations of T_b (or models of T_b) with finite N often show, regardless of deduction, the approximate behaviour characteristic of T_t—and often the approximation is very accurate. Besides, the deduction/simulation distinction is not so sharp". In other terms, the association between reductionism and agent-based modelling depends on the meaning we give to the derivability process that cannot, strictly speaking, be perceived as a deduction. Interestingly, this undefined area explains why econophysicists and economists perceive this derivability of macro levels in a different way.

In this context, the bottom-up dimension refers to the fact that macro regularities are not strictly deducible from individual features, but that they are rather inferred from interaction between individuals. However, despite the situation, agent-based modelling allows an "a posteriori computerized traceability" of these macro regularities, inviting some authors (Gallegati et al., 2009, p. 7) to present that approach as a "bridge between methodological individualism and methodological holism". If we give a broader meaning to the notion of derivability, in line with Dudau (2006), who wrote that reductionism can be seen as the thesis in which predicates in the higher-level theories are definable in terms of lower-level predicates, then the bottom-up agent-based modelling can be regarded as an algorithmically reducible (but not deducible) framework. In the light of this analysis, it seems that the bottom-up agent-based approach mainly deals with what Bedau (1997) called a "weak emergence" (i.e. those which need to be simulated to be revealed but, at the same time, which can be reducible to their microscopic entities).

Because the methodological analysis proposed in this section is valid for all potential applications of agent-based modelling, Rosser (2008, p. 19) and Lux (2009, p. 35) wrote that this "way of modelling [in economics] can be seen to be very compatible with what is implied by many econophysics models". Those words paved the way to a potential collaboration between economists and econophysicists, since they use the same methodology when they develop a bottom-up agent modelling. However, there is an econophysics literature that uses agent-based modelling in the same way that economists do (Gallegati et al., 2009), I explained previously that collaborations between the two communities are not common, simply because economists and physicists do not have the same conceptual framework: economists consider that interactions between agents can generate the emergence of intermediary (meso) orders (associated with economic institutions as market or money for instance) that could reciprocally influence the micro interactions. This way of modelling for economists appears to work contrary to the second law of thermodynamics, since adaptive systems evolve by transforming initially simple rules/structures into increasingly complex ones (Davis, 2013): order emerges from simple micro interactions whose basic features can be algorithmically described. From that perspective, macro order is not taken for granted and instead, it results from algorithmic micro configurations that allow economists to define a computable order. From this computerized simulation of this reciprocal influence, a spontaneous macro order emerges. In contrast, agent-based econophysicists do not deal with this meso order and they focus instead on the emerging pattern that results from the physically plausible interactions between micro elements.

V.1.b) The macro derivability as an asymptotic property

All econophysicists consider that complex economic systems consisting of a high number of interacting elements can be studied through their emergent properties, which are often associated with recurrent macro patterns. In the first part of this chapter, I identified this claim as a core element of econophysics. I also wrote that econophysicists share another common point: the use of asymptotic reasoning. Depending on the methodological tradition econophysicists are involved in, the use of such reasoning can vary. While statistical econophysicists implement an asymptotic technique by referring to the large number of micro elements, agent-based econophysicists instead use this reasoning in relation to a high number of computerized simulations[151]. The asymptotic nature of the analysis is not necessary in the observation of the phenomenon, but instead in the way data is generated and associated with this phenomenon. From this perspective, computers are used as the apparatus and computerized simulations can be regarded as a numerical experiment (Morrison, 2015). This is an important difference from statistical econophysics, where data are directly collected from the observation of the phenomena. In contrast, agent-based modelling aims at generating computerized data that reproduces existing observations related to phenomena. There is another element of distinction between statistical econophysics and agent-based modelling: while the first considers that microscopic interrelations are too complex to be described, the latter founds its methodology on the assumption that these micro interactions can be characterized through a particular calibration. When rules governing the micro level are defined, a high number (ideally an infinity) of computerized simulations is required to capture the macroscopic domain of the system, as explained. Let us illustrate the way agent-based econophysics implements the asymptotic reasoning. For this purpose, let us consider that X_0 refers to the macro behaviour of a system composed of a high number of components with a specific configuration of micro states at time t_0. Agent-based modelling will then characterize the dynamics associated with the evolution of that system $X_0 \rightarrow X_n$ where X_n is the macro behaviour of the system associated with the configuration of micro states after *n* computerized simulations. The dynamics characterizing the evolution of the system

[151] This point is not incompatible with the assumption of having a large number of interacting agents.

are defined by an algorithmic implementation of rules that describe micro interactions between components. Those computerized iterations will evolve the system's micro states over time in accordance with the following schema:

$$\underbrace{X_1 \rightarrow \ X_2 \rightarrow \ \ldots}_{\text{Macro-behaviour for which microscopic interactions did not generate a recurrent pattern}} \rightarrow \underbrace{X_n \rightarrow X_{n+1} \rightarrow X_{n+2}}_{\text{Macro-behaviour where a macro-regularity can be observed}} \quad \ldots$$

After *n* iterations, the micro interactions begin to generate a macroscopic result that takes the form of a persistent pattern [P]. That macro result is then looked on as an emergent order whose macroscopic domain, P, can be modelled as a macroscopic domain [χ] regrouping all potential microstates after *n* iterations:

$$P = [\chi] = \{\sum X_i; \text{ with } i \text{ between } n \rightarrow \infty\}.$$

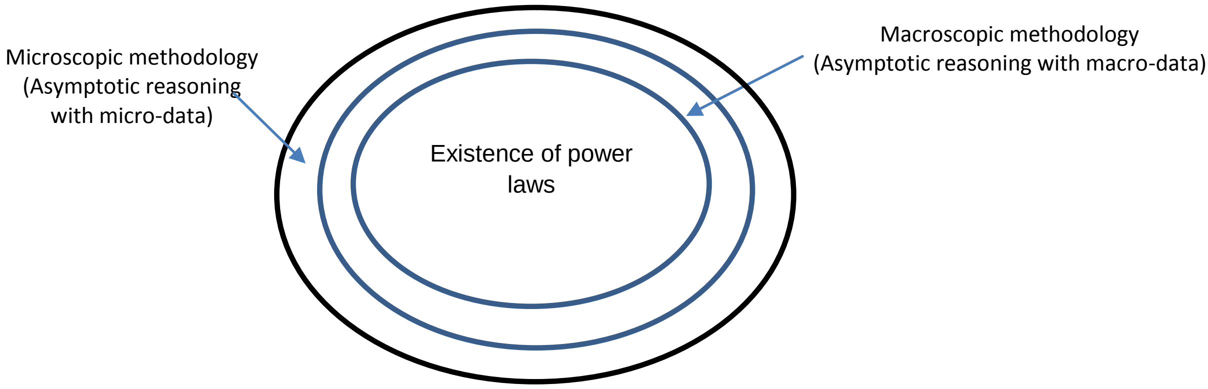
Between $X_0 \rightarrow X_n$, the switch between X_{i-1} and X_i ($\forall$ i < n) is not an invariant function (Γ) of the previous switches that justifies a new iteration (i+1). When a persistent relationship Γ begins to appear X_n, then the system is said in to be in the macroscopic domain. Conceptually, that limit is reached after *n* iterations when the pattern (Γ), which rules the dynamics, appears as persistent for a great number of iterations for $X_{n+2} \rightarrow X_\infty$. Actually, the macro pattern (Γ) appears to be "a set of states invariant under the dynamics"[152] since all additional iterations will reinforce its analytical form. To put it another way, in the course of iterations, the system tends to evolve towards a macro result whose the derivability from a micro level can be reduced to an asymptotic property. The emerging macro pattern is not initially contained in the description of the micro level, but it can be expressed (not deductively) in terms of the micro scale description (reducing theory) since we know the process required in order to generate the first from the latter. The reduced theory

[152] This is the definition of what we call an attractor (Weinstein, 2008).

(macro pattern) can therefore be looked on as an asymptotic definitional extension of the reducing theory (description of the micro interactions). Such a way of modelling can also be looked on as a heterogeneous version of reduction. Although there are several dissimilarities to statistical econophysics, it is worth stressing that these two approaches interestingly preserve the same core element, as explained in the following section.

V.1.c) Agent-based econophysics as a first methodological extension of econophysics

Although bottom-up agent-based modelling keeps the core elements[153] used by statistical econophysicists, it gives another meaning to these elements. However, in contrast with the latter, the first start their studies from the definition/calibration of microscopic elements' interactions. In so doing, agent-based econophysicists have extended the range of econophysics by adjusting the initial condition in an acceptable way (keeping the core elements and the physically plausible aspect of their analysis). The contribution of agent-based econophysics is to extend the way of dealing with this hard core by implementing a new computerized method that still makes sense for physicists. It is important to emphasize that this agent-based econophysics has never been presented as an alternative to the original (statistical) econophysics, but rather as a complementary approach. In other words, bottom-up agent-based econophysics can be perceived as a methodological extension of the econophysics as visually illustrated here:



[153] As a reminder, studying emergent properties in complex economic systems by using an asymptotic reasoning.

Figure 8: Lakatosian visualization of the core elements and the protective belt of the bottom-up agent-based econophysics.

This perception has been progressively promoted, leading to the advent of a third methodological approach attempting to bridge these two existing perspectives. In the following section, I will discuss this last extension of the econophysics protective belt by dealing with what I call top-down agent-based econophysics.

V.2. Top-down agent-based econophysics: An in-between approach

The second category of works developing an agent-based econophysics refers to research whose objective is to reproduce existing statistical data. In opposition to the previous category, authors dealing with this area of knowledge usually refer to existing empirical statistical patterns as inputs. Once a specific macro pattern is identified in economic/financial phenomena, the objective is to derive information for the calibration of micro interactions (these will be the outputs in this process). These simulated interactions are supposed to generate the macro patterns that were initially targeted/identified. The real target in this research is not directly the description of the system, but rather the kind of calibration needed to reproduce the initial emergent properties (patterns observed in data). In contrast with agent-based economics, individual incentives are not the constraint for the calibration of micro interactions. The real micro constraint for these works is actually defined by the information that can be derived from the initial macro laws in order to reproduce it with an agent-based modelling. While the initial condition of this approach (existence of a macro pattern) can be regarded as an extension of the initial condition used by statistical econophysicists (high number of observations), the machinery used by scholars involved in this approach is very similar to the one implemented (algorithmic rule) in bottom-up agent-based modelling. In line with the latter, the asymptotic reasoning is also used as a conceptual bridge between the observed macro patterns and the high number of iterations of micro interactions. Regarding the treatment of emergent properties, top-down agent-based econophysics do not offer something new in comparison with the two other traditions. In a sense, the real target in this research is not directly the emergent properties, but rather the kind of calibrations we need in order to generate the initial observed macro input.

The literature related to this tradition is quite recent. Although there is a corpus of some papers that were published in the 2000s, the real development of this literature began around 2010. This consists of a set of papers that use the analogy "agent-particle" to develop what econophysicists call the kinetic wealth exchange models whose objective is "to predict the time evolution of the distribution of wealth, by studying the corresponding flow process among individuals" (Chakraborti et al., 2011, p. 1,026). All these studies use a macro pattern as initial constraint for the calibration of micro interactions which are expected to generate the pre-defined macro pattern identified in the literature related to the statistical econophysics evoked above. By using a power law as initial macro pattern, Heinsalu et al. (2009) or Patriarca et al. (2010) provided models that describe the transfer of wealth for homogeneous agents (i.e. with the same statistical properties) while Chakraborti and Patriarca (2009) developed a more complex kinetic wealth exchange model in which agents are diversified (in terms of initial wealth and savings parameter for example). Some studies started their analysis with other kind of macro patterns: Richmond et al. (2013) used Lotka-Volterra equations to describe the wealth distribution, while others expressed wealth exchange by using matrix theory (Gupta, 2006), Markov chains (Scalas et al., 2007) or the Boltzman equation approach (Slanina, 2014; During et al., 2008).

In contrast with the bottom-up agent-based approach, initial assumptions (inputs) are formulated by integrating information from a particular macro pattern observed in the past evolution of the complex system. The following diagram summarizes the modelling process of this category of works:

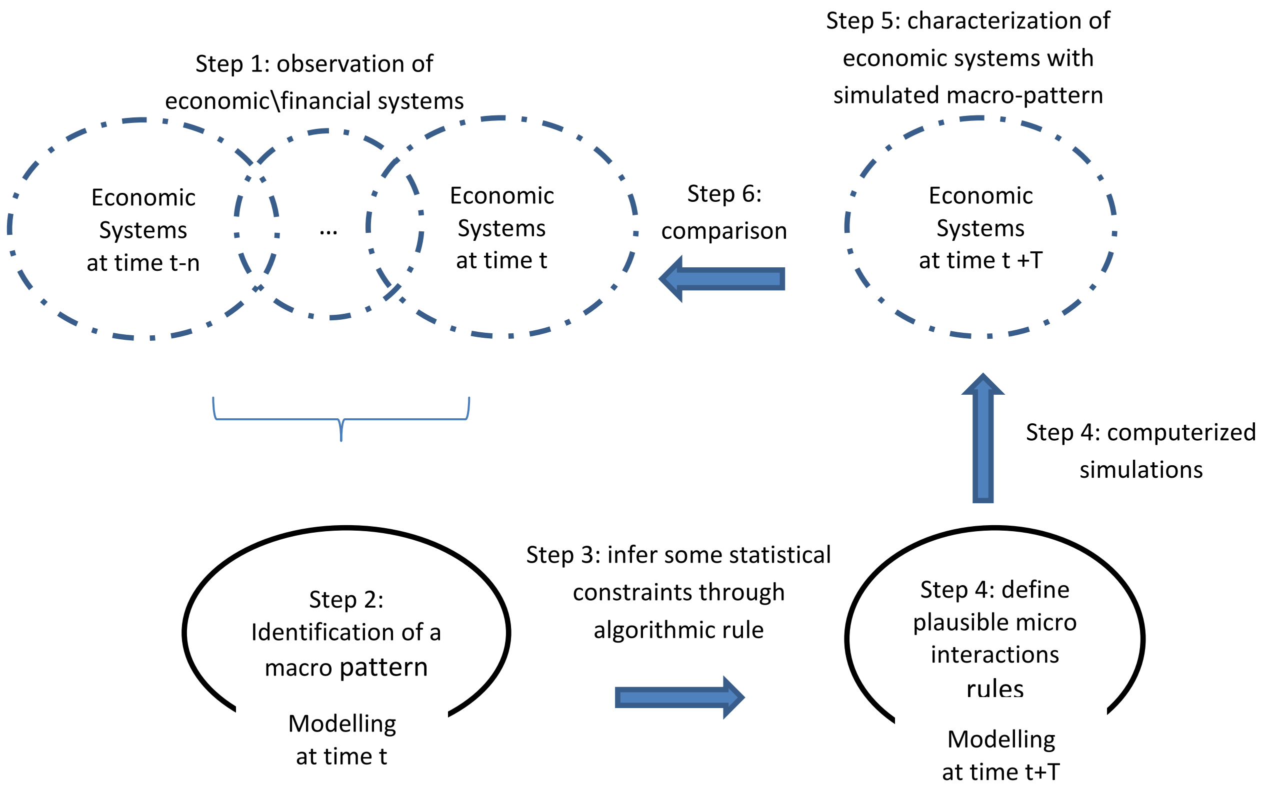


Figure 9: Modelling process of econophysics agent-based modelling.

As previously evoked, the emergence of econophysics is directly associated with the identification of statistical regularities in complex economic/financial dynamics. When the statistical approach is combined with agent-based modelling, the analysis begins with the phenomenological observation of a statistical regularity in a particular economic phenomenon. Afterwards, conditions are derived from the observed macro pattern to calibrate the micro interactions of individual market participants. These micro interactions will then be algorithmically generated with the hope of quantitatively reproducing the initial macro pattern. Concretely, the scaling properties of power laws are very interesting since they allow the modeller to consider that what is observed at the macro level can be proportionally found at a micro level. For instance, the scaling property of a distribution describing a financial time series implies that statistical features observed annually or semi-annually can be extended to monthly or weekly data. Such characterization means that these statistical features are not time dependent. In context, the macro pattern initially identified for this financial system will then be constraining for the calibration of the rules governing interactions between agents, as Feng et al. (2012, p. 8,388) explained, “the interaction strength between agents need to be adjusted with agent population size or interaction structure to sustain fat tails in return distributions [i.e. power law]”[154]. In their top-down agent-based modelling, econophysicists consider that statistical macro characterization can influence micro interactions. Indeed, econophysicists describe macro regularities that emerge from economic/financial complex systems in terms of statistics, and the characterization of that macro pattern is supposed to determine the behaviour of lower-level components. As Rickles (2008, p. 7) explained:

> “The idea is that in statistical physics, systems that consist of a large number of interacting parts often are found to *obey* ‘universal laws’—laws independent [causally] of microscopic details and dependent on just a few macroscopic parameters”.

The recent advent of this top-down agent-based econophysics illustrates the coherent and unified perceptions that econophysicists have about their fields: the macroscopic and microscopic techniques can be complementary combined to offer a global comprehension of the target system. On the one hand, statistical econophysics macroscopically describes emergent properties (power laws) in economic/financial systems; and, on the other hand, the bottom-up agent-based econophysics methodologically extended this objective through a more micro approach. More recently, top-down agent-based econophysics provides a methodological bridge between the two previous perspectives. Visually, one can summarize this process as follows:

[154] It is worth emphasising how econophysicists keep a physical vocabulary in their definition of t since they talk about “interaction strength” or “interaction structure” while economists instead use words like “interactions” and “network”.

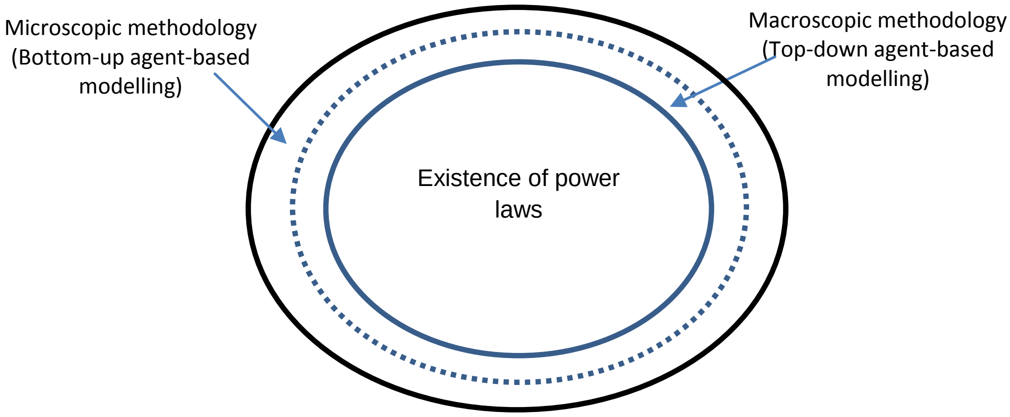


Figure 10: Lakatosian visualization of the core elements and the protective belt of the bottom-up agent-based econophysics.

In this visualization, I represent the contribution of the top-down agent-based modelling as a dilution of the separation between the micro and the macro methodologies. While the previous section presented bottom-up agent-based modelling as a first methodological extension of econophysics through a micro approach, this section shows that the top-down agent-based econophysics provides a coherent framework that integrates the two initial approaches discussed in this chapter. Figure 10 suggests an interesting Lakatosian perspective of econophysics that will be investigated in more detail in the following section.

VI. From diversity to unity: A Lakatosian coherence

The literature devoted to econophysics is extremely scattered. This section presents a set of criteria through which the three approaches evoked above will be presented as a coherent and unified research programme. Precisely, this section will structure the differences and similarities identified between the three methodological perspectives through a Lakatosian lens. Writing about what defines econophysics necessarily implies a methodological choice. Precisely, I use, in this section, a Lakatosian perspective to define the methodological core of econophysics. Alternative philosophical frameworks (Kuhn, 1962; Toulmin, 1972 or Laudan, 1984) could have been used here - however, given the way I introduced and analysed econophysics in the two first chapters, a Lakatosian lens is probably the most appropriate one for several reasons. First, the Lakatosian division of research programs in terms of hard core and protective belt fits the evolution of econophysics especially well and no such resources are available in other frameworks. A second reason for chosing a Lakatosian approach to characterize the methodological evolution of econophysics refers to the acknowledged statement, among econophysicists, that power laws are at the heart of the field. Such generalized agreement in the econophysicists community paves the way to a more straightforward Lakatosian analysis. More importantly, another reason refers to the fact that the evolution of econophysics can be perceived as a 'linear evolution of knowledge'. Indeed, although there are some cultural differences between economists and physicists, the field of econophysics emerged without conceptual break with economics. The theoretical core of econophysics is based on the use of power laws that are actually well-known in economics. Power laws have even been investigated in financial economics in the 1960s and 1970s (as evoked in the first chapter) but abandoned due to their lack of substantial empirical insights (e.g. indetermination of the variance). In this context, econophysics can be presented as an extension of an old research program that has been abandoned in financial economics in the 1970s (the first chapter investigated this aspect). Before detailing this characterization of econophysics, the next section will present the key Lakatosian concepts that will be used in my analysis.

VI.1. The concept of "hard core"

The concept of "hard core" is known in history and the philosophy of science through Lakatos' theory of knowledge. Imre Lakatos associated a set of theories with what he called a research programme whose hard core refers to common features of the theories shared by all scientists acting in this research programme. Lakatos used this idea of hard core to summarize what members of a scientific community take for granted in their activity. Lakatos (1978) explained that these fundamental assumptions that comprise the hard core are usually protected by what he called a "protective belt" (i.e. features of theories that may be altered in the research). This protective belt opens a door for the evolution of the research programme since it may evolve in line with a positive heuristic (i.e. what paths to pursue), exploring new issues/puzzles and their formulations required to preserve the hard core statements. This positive heuristic can be seen as a sequence of injunctions to not change the hard core or as an "implicit long-term research policy that anticipates refutations" (Lakatos, 1978, p. 50). For Lakatos, a research programme is said to be progressive when its alterations allow scholars to make and confront novel predictions. This idea of progress is very important for the Hungarian philosopher because it offers a demarcation between progressive and degenerative research programmes in science. The latter characterizes a programme whose alterations are no more than ad-hoc adjustments or reformulation of the existing protective belt to preserve the hard core assumptions. Roughly speaking, the dynamics of a research programme can be illustrated as follows,

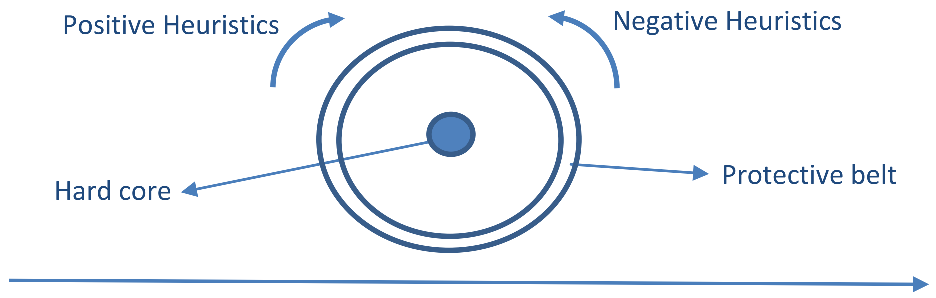


Figure 11: Illustration of the dynamics of a Lakatosian research programme

This schema shows that a progressive research programme is supposed to evolve in line with increasing empirical progress (the horizontal arrow). This is the role of the positive heuristic (arrow going the same direction as the horizontal one): to ensure this specific movement. On this illustration, the arrow going in the opposite direction represents the regression of the research programme when scholars do not follow the positive heuristic (i.e. injunctions that members must follow in order not to break the progress of the research programme). In other words, negative heuristics refer to a "classical conventionalism [that decides] not to allow refutations to transmit falsity to the hard core as long as the corroborated empirical content of the protective belt of auxiliary hypotheses increases" (Lakatos, 1978, p. 49).

As an illustration of his methodology, Lakatos analyses in detail Bohr's research programme, which was founded on the claim that light emission results from electrons jumping from one orbit to another within atoms. This explanation of light emission was quite debatable since it generated an opposition between two well-corroborated theories: the Maxwell-Lorentz theory of electromagnetism and the Rutherford theory of atoms[155]. In this context, Bohr suggested to ignore this inconsistency and develop a research approach whose refutable elements were inconsistent with the Maxwell-Lorentz theory. In so doing, he defined the core elements of his research programme, which Lakatos (1978, p. 56) summarized as follows:

> "1) The energy radiation [within the atom] is not emitted (or absorbed) in the continuous way assumed in ordinary electrodynamics, but only during the passing of the systems between different "stationary" states.
>
> (2) That the dynamical equilibrium of the systems in the stationary states is governed by the ordinary laws of mechanics, while these laws do not hold for the passing of the systems between the different states.
>
> (3) That the radiation emitted during the transition of a system between two stationary states is homogeneous, and that the relation between the frequency v and the total amount of energy emitted E is given by E = hv, where h is Planck's constant.
>
> (4) That the different stationary states of a simple system consisting of an electron rotating round a positive nucleus are determined by the condition that the ratio between the total energy, emitted during the formation of the configuration, and the frequency of revolution of the electron is an entire multiple of 1/2h. This assumption is equivalent with the assumption that the angular momentum of the electron around the nucleus is equal to an entire multiple of h/2π.
>
> (5) That the "permanent" state of any atomic system, i.e. the state in which the energy emitted is maximum, is determined by the condition that the angular momentum of every electron around the centre of its orbit is equal to h/2π".

In this specific research context, Bohr implicitly defined his negative heuristic specifying the irrefutable element of his hard core through methodological decision.

[155] According to which electrons are moving around atoms in a planetary-like system.

In the same vein, Bohr's research on light emission implied a positive heuristic, indicating a research direction that was focused on adjustment of some refutable variants in the core elements (i.e. elements that could potentially be inconsistent with the Maxwell-Lorentz theory).

In this section, I will use this conceptual framework to describe the methodological diversification of econophysics. More precisely, I will show how the three traditions share the same hard core, but how they investigate/protect it in different ways. In this context, the methodological diversification observed in econophysics will be presented as the result of different crystallizations of the protective belt in accordance with a positive heuristic. In other words, all econophysicists will continue to share the same common feature, regardless of the approach they subscribe to.

VI.2. The hard core of econophysics

First of all, the three econophysics traditions deal with the extension of knowledge from physics to economics/finance. In this extension, the vast majority of econophysicists consider economic/financial phenomena as complex systems that are composed of a large number of interacting elements. This methodological point is important because the high number of components requires a specific process of generalization in order to transform the accumulation of facts/statements into a structured knowledge. Regarding this aspect, econophysicists consider that "something happens" between the micro and the macro levels of complex systems by considering that macro results can be presented as emergent properties that transcend the micro components' behaviour. In this context, emergent properties take the form of power laws that are said to emerge from the complexity of systems. I explained earlier in this chapter that each tradition comprising econophysics developed a specific way of dealing with the emergence of power laws that, paradoxically, all of them derive from an asymptotic convergence that results from a large number of implementations of the reduced theory T_2. This way of conceptualizing emergence can be looked on as

$$\lim_{n \to \infty} T_2 = T_1$$

In this schema, a more encompassing (macro) theory T_1 reduces a specific (micro) theory T_2 if the laws of T_1 can be asymptotically derived from the observations/ iterations of T_2. This way of characterizing the notion of emergence is inspired by Batterman (2002), who promoted the development of an "asymptotic reasoning" (Batterman, 2002, p. 3). Through this definition, Batterman (2002) claimed that many of why-question based theories "are explanatorily deficient" for understanding how universality can arise (by universality, Batterman refers to "a feature of the world—namely that is in certain circumstances distinct types of systems exhibit similar behaviors", Batterman, 2002, p. 9). When the philosopher presented his approach, he wrote that "Sometimes, science requires methods that eliminate both details and, in some sense, precision […] I call these methods 'asymptotic methods'" (Batterman, 2002, p. 13). Generally speaking, the latter can be defined as methods describing the limiting behaviour of a specific phenomenon. These techniques assumed the existence of a sequence of data that were related to a particular configuration of systems composed by noisy elements/variables. In such a context, only the asymptotic domain (behaviour at the limit-situation) is considered as information worthy for understanding the emergence of universality because it avoids details that could obscure the understanding of the phenomenon (Batterman, 1997). In other words, the asymptotic reasoning is appropriate for describing a behavioural similarity observed in diverse systems. Through a mathematical characterization of their elements, an asymptotic method captures what is universally common between diverse dynamics under study.

The term "universality" does not necessarily have good press in the philosophy of science where some authors (Berry, 1987, p. 185) associate this notion with "the slightly pretentious way in which physicists denote identical behaviour in different systems". However, according to Batterman (2002), some systems exhibit, to some extent, the same macro behaviour, while we obviously know that their micro details differ significantly. To illustrate this notion of universality, Batterman (2002) took the example of the behaviour of pendulums—when one wants to understand why pendulums with different characteristics (masses, lengths, composition), one focuses on the fact all of these items have all periods that are proportional to the square root of the length of the rod from which the bob is hanging. More formally, the period θ (i.e. angular displacement) of pendulums exhibiting small oscillations is given by

$$\theta = 2\pi\sqrt{\frac{l}{g}}$$

where *g* is the acceleration due to gravity and *l* is the length of the pendulum. Whatever the differences we can find between the potential pendulums, all of them will depend on few parameters that are expressed in terms of units of length, mass and time. In this context, the dimension of θ is T (time), the one of *l* is L (length) and the one of *g* is LT^{-2}. Any changes in the units of time or length would imply a variation of the ratio *l/g.* For instance, if one considers that the unit of length can be decreased by a factor *x* and that the unit of time is decreased of a factor *b*, the acceleration due to gravity will increase by a factor xb^{-2}, implying that the quantity

$$\Delta = \frac{\theta}{\sqrt{l/g}}$$

remains constant under a change in the fundamental units. In other words, this ratio is dimensionless. For Batterman (2002), such invariance is a good example of universality: it is a dimensionless invariant feature observed in the behaviour of different pendula whose individual characteristics are irrelevant for the behaviour of interest because they generate an "explanatory noise" (Batterman, 2002, p. 15).

According to Batterman (2002), asymptotic reasoning is essential for understanding how universality can arise. The example of the pendulum is the simplest way to understand the notion of dimensionless universality. However, in most of the cases, this universality does not necessary take the form of a constant, but it is rather associated with an equation in which the parameter Δ evoked above is a function as expressed hereafter:

$$\Delta = fct\ (\Delta_{1,} \dots, \Delta_m)$$

where Δ_i can be considered as extremely small or extremely large. In such a context, one can reduce the problem by taking the limit so that Δ_i can be replaced by a constant $\Delta_i(0)$ = C or $\Delta_i(\infty)$ = C.

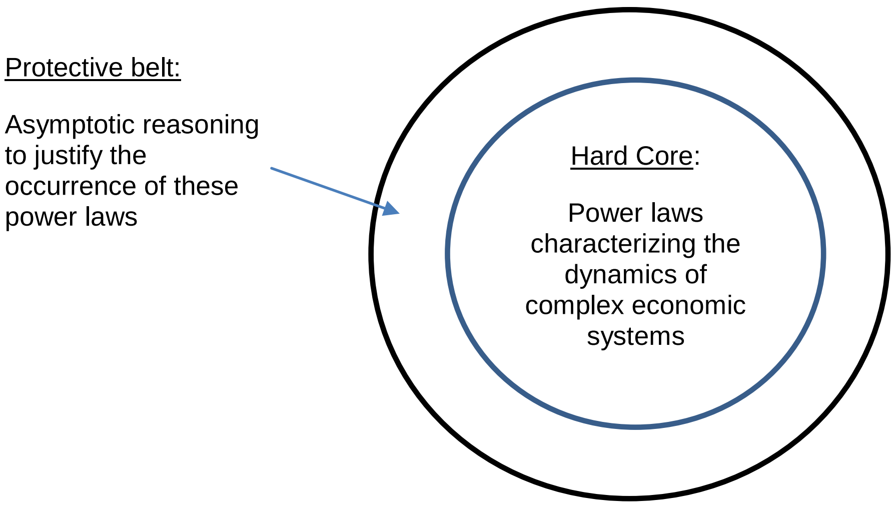
Generalizing this example, Batterman (2002, p. 17) explained that the role of an asymptotic reasoning is to formulate the equations that describe universal features (like Δ instance for instance) of systems by assuming that the limits $\Delta_i(0)$ or $\Delta_i(\infty)$ exist. In line with this reasoning, all econophysicists (whatever approach they use) assume that complex phenomena can be captured through the analysis of a high number of observations/iterations. Specifically, econophysics can then be looked on as a way of describing interacting-element-based systems for which we can deduce/derive a novel and robust behaviour by investigating the limit $\Delta_i(\infty)$. The idea behind asymptotic reasoning is to develop a method that eliminates micro details in order to highlight a significant regularity echoing an identical asymptotic behaviour in different systems/phenomena. As noted in the first chapter, this regularity often takes the statistical form of a power law presented as a "universal feature" whose statistical stability guarantees the dimensionless aspect of the analysis[156]. Although the three econophysics traditions deal in a different way with the emergence of power laws, they all use a particular asymptotic reasoning in their justification of the occurrence of these macro-patterns.

In a sense, econophysics offers an interesting context for studying the link between emergence and asymptotic reasoning. There is an important literature dedicated to asymptotic methods, and some philosophers (Pexton, 2014; Hooker, 2007) have emphasized that although asymptotic methods provide a limiting value that characterizes the behaviour of functions/systems, this limiting value is supposed to be approached indefinitely closely, but it is never really reached. In other words, if asymptotic reasoning can be used to describe the phenomenon of emergence, it is based on physically uninterpretable mathematics. Batterman (1997, 2002a, 2002b) did not clarify this aspect because he mainly focused on the dimensionless

[156] As a reminder: the statistical stability refers to a property that ensures the existence of a scale-free parameter that characterizes a particular aspect of the dynamics at every level of analysis.

properties in order to justify his use of asymptotic reasoning. However, his justification is strictly mathematical and, to some extent, the passage to the limit, which provides the limiting value, does not belong to the sequence of data associated with the system under investigation. This passage to the limit requires non-physically interpretable elements, which generates many debates [157] in philosophy. I discussed earlier in this chapter that although some authors (Butterfield, 2014; Batterman, 2002) associate this asymptotic reasoning with a principle of derivability, this reasoning cannot, strictly speaking, be looked on as a deduction. Such blurred perspective on the explanative power of asymptotic method keeps room open for interpretations and debates, as witnessed by the way econophysicists and economists disagree on the way of thinking about such reasoning.

In light of the analysis suggested in this chapter, the core elements conventionally shared among all econophysics can be summarized as follows:



This chapter illustrated that these elements define the intellectual scope of econophysics and they tell econophysicists what paths of research to pursue. First of all, it is commonly accepted that econophysics refers to the extension of statistical

[157] It is also worth mentioning that the singularity related to these non-physically interpretable elements generates many debates about the explanatory power of asymptotic methods (Batterman, 1997, 2002, 2009, 2012; Pexton, 2014; Hooker, 2004; Buenon and French, 2012).

physics to finance and economics. That being said, econophysics agree on what has to be modelled: complex economic systems can be described through power laws. This statement can be seen as the core of econophysics. As discussed in this chapter, econophysicists consider that these systems can be observed through a specific way of modelling, which is based on an asymptotic reasoning according to which a high number of observations/iterations can reveal the exact form of these power laws. Despite the existence of common ideas that are conventionally accepted among econophysicists, the literature (Chakraborti et al., 2011a, 2011b; Schinckus, 2013; Ausloos, 2013) devoted to this field indicates that there are several ways of implementing these ideas.

VI.3. Beyond the hard core, the diversity!

Econophysics is still a young field and the main core elements identified in the previous section do not offer a unifying framework for understanding the state of art in econophysics. The existence of this core statement is the necessary condition to present econophysics as a coherent field, but the comprehension of the methodological richness of econophysics requires a more peripheral analysis. That is the purpose of this section, which will show that the notions of a positive heuristic and the protective belt can be used as a source of innovation for the evolution of a research programme. By definition, the protective belt refers to the evolving/dynamic dimension of a research programme since it provides a sequence of auxiliary hypotheses that can be altered by future research. In other words, the protective belt offers many potential paths of research for the future evolution of the programme and, combined with a specific positive heuristic, this belt can be progressively transformed.

I claim here that the proliferation of methodological traditions in econophysics results from the intellectual dynamics generated by different treatments of puzzles that original (statistical) econophysicists were faced with. More precisely, I will show that this fragmentation is due to a diversity of potential solutions for solving existing puzzles. In this context, the last two decades can be seen as having a fertile momentum that may speed up the growth of econophysics[158] without upsetting the coherence of the field (i.e. the hard core evoked in the previous section is not called into question). In contrast, this pluralistic situation takes the form of a converging evolution of econophysical methodologies, leading me to consider that these traditions are ruled by common concerns about the original puzzle: how to find micro foundations for the initial macroscopic approach proposed by econophysics. A decade after the advent of statistical econophysics, a bottom-up agent-based econophysics emerged in the literature (Abergel et al., 2014). Works using this technique require the definition of fundamental constraints, which takes the form of pre-defined rules that describe the micro interactions between elements. Afterwards, a large number of interactions are simulated due to computational power. The use of computer simulations refers to the repeated applications of a set of instructions that describe the initial configuration of the target system. This initial setup will evolve according to algorithms used to define the complex interactions between the microscopic system's components, which are defined in the initial step of the modelling. Because it is based on a microscopic approach, this way of modelling is conceptually closer to what economists do. Indeed, as Feng et al. (2012) explained, the mere implementation of agent-based modelling is not an intrinsic feature of econophysics, whose origin refers to the characterization of macro patterns. With the purpose of combining the two methodological perspectives, some econophysicists (Stanley et al., 2012) very recently proposed a movement to adapt the bottom-up agent-based modelling to statistical econophysics. Specifically, scholars have used information inferred from statistical observations of systems as key elements in the definition of the micro interactions that are then computationally iterated. From this perspective, one observes the emergence of a top-down agent-based econophysics in which the input of the technique includes specific statistical characteristics inferred from the observed system (i.e. the existence of a power law with defined parameters, for instance). In accordance with the purpose of the bottom-up agent-based econophysics, the objective is to reproduce data observed for existing systems through a high number of computerized iterations of statements that are algorithmically constrained. However, in contrast with the bottom-up approach,

[158] This momentum can easily be observed by the increasing number of publications using the label "econophysics" (Ausloos, 2013).

assuming that macro results can be derived from the micro levels, the top-down perspective tries to combine the two other traditions discussed in this chapter.

The co-existence of these three methodological approaches is very interesting from a philosophical point of view. Beyond the question of the field coherence, this methodological diversity characterizes different ways of dealing with asymptotic reasoning and concepts such as emergence, reduction, derivability/deducibility, etc.

VI.4. Asymptotic reasoning as a source of diversification

The implementation of one of the core elements defined in the previous section characterizes the dissimilarities between the three econophysics traditions. Because the use of asymptotic reasoning actually ensures the link with physics (through its link with the renormalization group theory), it constitutes a conclusive protective belt giving a room for each tradition to extend the econophysics research without altering the core statement. As a reminder, such reasoning can be schematized as,

$$\lim_{n \to \infty} T_2 = T_1$$

This equation describes the situation in which a more encompassing (macro) theory T_1 reduces a specific (micro) theory T_2 if the laws of T_1 are asymptotically approached from the observations/iterations of T_2. Even though an implicit agreement exists about the use of this asymptotic reasoning in econophysics, the three traditions evoked in this chapter implement it in very different ways. Statistical econophysics, for instance, considers that the parameter *n* must be the *number of observations*; that T_1 is the statistical macro pattern (i.e. power law) which phenomenally emerged from the data, while T_2 refers to the undefined characterization of the low-level (microscopic) complexity ruling the individual level. From this perspective, T_2 describes a specific configuration of the micro level, which takes the form of a recorded number. In other words, T_2 is considered as a meso level since it characterizes a particular configuration of the components that can be associated with the value of a parameter that summarizes this micro level. This methodology raises particular issues, which I discussed in the first part of this chapter.

Regarding works devoted to agent-based econophysics, they also use asymptotic reasoning but in a different way since they explicitly associate the parameter *n* to the *number of computerized iterations* required to let the power law emerge, characterized by T_1 while T_2 refers to the description (pre-definition) of the micro level that must be defined (for bottom-up agent-based econophysics) or estimated (for top-down agent-based econophysics). In this bottom-up agent-based econophysics, T_2 is not given, but it must be pre-defined according to realistic assumptions about the microscopic interactions. T_1 (power law) will therefore be estimated through "an infinite number" of computerized interactions. Concretely, authors involved in this tradition define micro interactions inspired by existing theoretical frameworks (for instance, magnetism) to generate computerized simulations with the objective of reproducing the dynamics of the financial markets.

The top-down agent-based econophysics provides another schema: T_1 is given since it results from a macro pattern originally observed in the target system (this T_1 refers to the T_1 evoked in statistical econophysics) whereas T_2 will be estimated/adapted in order to generate T_1. The objective is to find a realistic definition of micro interactions that will generate the same macro pattern as the one observed in the evolution of the system under study. In other terms, the limit evoked above must read from left to right since T_1 is taken as given and that the target of the research is T_2. A telling example is the work of Feng et al. (2012), who used macro statistical parameters (i.e. variance, critical exponent, etc.) derived from a macro pattern (power laws) characterizing the financial markets dynamics in order to define micro interactions for their agent-based modelling simulation whose objective was to reproduce the macro evolution of markets.

The existence of these three methodological traditions in econophysics indicates a methodological coherence in accordance with the historical roots of the field. As a reminder, Chapter 2 explained how the Santa Fe Institute initiated the two major computational approaches (statistical and algorithmic techniques) used in econophysics to deal with complex economic/financial systems. Even though statistical econophysics is often presented as an independent literature and that the use of with agent-based modelling is more recent in the field, the diversification of econophysics directly results on the one hand, from its historical roots, and the other hand, from puzzles that led econophysicists to diversify their approach (but without changing the initial hard core of the field). Since the existence of power laws as an indicator of complexity is still central for all approaches. From this perspective, by combining the macro perspective enhanced by statistical econophysics with the micro approach implemented by agent-based modelling, top-down agent-based econophysics appears to be the more integrative tradition. This methodological diversification of econophysics indicates a movement between the macro and micro perspectives; and the recent emergence of the third tradition (top-down agent-based econophysics) could appear as a non-winning compromise, since the three traditions still co-exist in the current literature. In Lakatosian terms, this coexistence can be explained by the fact that although the development of new perspectives improved the explanatory power of econophysics, they do not refute the pre-existing one.

VI.5. The role of the positive heuristic in the evolution of econophysics

One purpose of this chapter is to show that the evolution of econophysics implies a methodological diversification and that it can be combined with a conceptual coherence, since by doing so, econophysics does not lose its original hard core. According to Lakatos, this development suggests an empirical progress that is characterized by the observation of novel predictions. Through novel predictions, scholars improve their understanding of unknown phenomena, such as the view that scientific progress refers to an increasing of advancement of scientific knowledge (cognitive progress). However, this way of describing the enrichment of knowledge mainly focuses on the goal of a research programme, and it underestimates the other aspects of scientific progress[159] that can also be expressed in more technological (increased effectiveness of techniques), societal (social increasing

[159] For more details about these debates, see Niiniluoto (2015)

quality of life and justice in society), professional (rising status of the scientific institutions) or methodical (invention of new method of research) forms (Niiniluoto, 2015). In this context, I will use this idea that a research programme can evolve at the same time, at the level of empirical characterization of phenomena (in accordance with the linear perception of Lakatos) but also at the methodical level. Such evolution implies a double progress since it combines a classical Lakatosian evolution of knowledge with a progressive improvement of research methods. This improvement takes the form a specific evolution of the protective belt induced by the positive heuristic of the field.

In my analysis then, two aspects of the research programme evolution will be studied[160]. First of all, I acknowledge that the three econophysics traditions evoked above keep the same conceptual hard core and the same major objective, which is to make predictions regarding phenomenon (emergent properties-based systems) whose mechanism appears for a long time as unknown for scholars (cognitive dimension). The second aspect refers to the methodical evolution of a scientific enterprise. By keeping its fundamental statements, as identified in the previous section, and by following the objective mentioned above, econophysics investigated/used different scientific instruments that led to a methodological diversification of the field. While the original econophysics focused on the statistical description of economic/financial systems without dealing with their individual components, other econophysicists explored methodological paths that were based on computer simulations. In so doing, they contributed to the methodical development of econophysics, which can be schematized as follows:

[160] I will not deal with the progressive vs. degenerative aspects of the research programme.

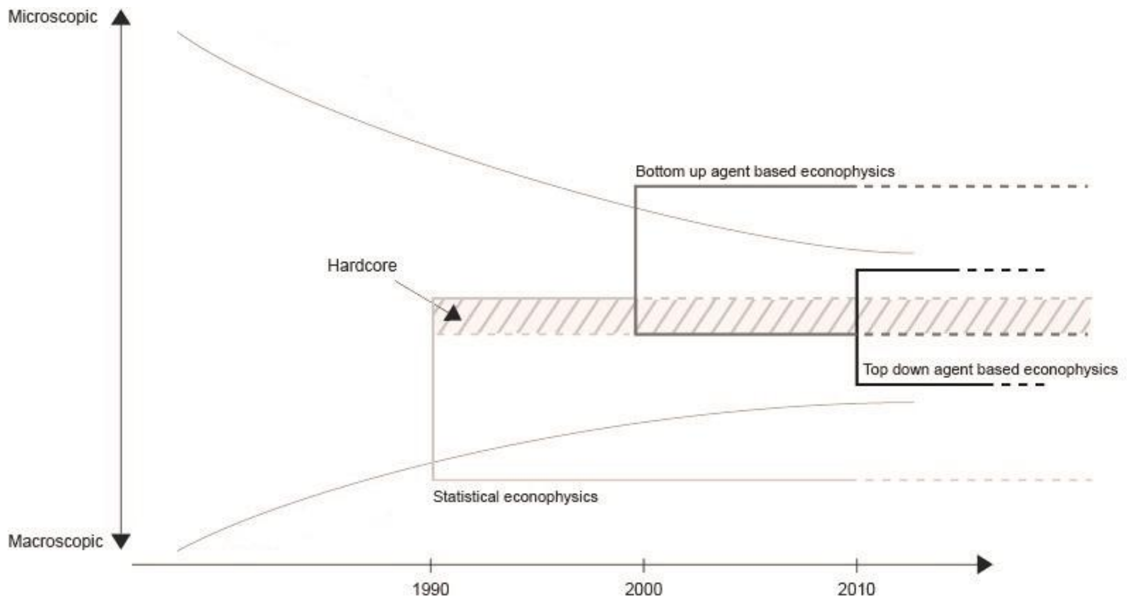


Figure 12: Illustration of the methodological evolution of econophysics.

This figure schematizes the evolution of econophysics through two dimensions: cognitive (horizontal axis referring to the evolution of the research program) and methodical (vertical axis describing the refinement of instruments used in econophysics). While the first rectangle describes the original (statistical) econophysics, which mainly used a macroscopic based approach, the second rectangle refers to a corpus of works founded on more a microscopic perspective and, from this perspective it illustrates the bottom-up agent-based econophysics that emerged in the 2000s. Finally, the last rectangle on Figure 11 is associated with the top-down agent-based econophysics, which has appeared very recently. It is worth mentioning that these three traditions co-exist in the current literature. Figure 11 shows that the proliferation of approaches results from a specific evolution of econophysics in accordance with a methodological diversification whose objective was to solidify key assumptions of the field by keeping its hard core unmodified. In a sense, this diversification is an extension of the protective belt resulting from what Lakatos called a "positive heuristic", which consists of an articulated set of suggestions on how to change and solidify the protective belt. Lakatos explained:

> "The positive heuristics sets out a programme which lists a chain of ever more complicated models simulating reality: the scientist's attention is riveted on building his models following instructions which are laid down in the

> positive part of his programme. He ignores the actual counterexamples" (Lakatos, 1978, p. 51).

When he wrote about these positive heuristics, Lakatos associated them with:

> "A set of initial conditions (possibly together with the observational theories) which one knows is bound to be replaced during the further development of the programme" (Lakatos, 1978, p. 51).

This quotation opens the door to a methodological evolution (and therefore a potential diversification) of the research programme. By considering the evolution of the research programme through the lens of the refinement of instruments, I directly illustrate this potential replacement of the initial conditions evoked by Lakatos. In the case of econophysics these initial conditions refers to the implementation of the asymptotic reasoning that evolved (as shown on the figure 12) but still ensures the protection of the hard core of econophysics. While original (statistical) econophysics used a macroscopic approach with its specific initial conditions (the existence of a high number of observations), the bottom-up agent-based econophysics that emerged several years later instead focuses on a microscopic perspective, implying different initial conditions based on the pre-definition of the micro interactions between components. Finally, the last tradition (top-down agent-based econophysics) provides a modelling that requires an initial condition that combines the ones used by the two other approaches. As a reminder, top-down agent-based econophysics requires the existence of a statistical macro pattern from which statistical properties will be induced to define micro interactions that are likely to reproduce the initial macro behaviour. Although the high number of iterations is still required, the set of initial conditions has changed since it assumes the pre-existence of a macro pattern whose statistical information will help to the identification of micro interactions that can generate this macro pattern.

In Lakatosian terms, agent-based modelling extended the way of implementing the asymptotic reasoning to produce a progressive problem shift in the protective belt of econophysics. Indeed, agent-based econophysics did not emerge because of radical (or accumulation of) refutations, but rather as the results of debates (Abergel et al., 2014) about the micro foundations of econophysics, which emerged in the 2000s.

Through a Lakatosian lens, this evolution can be characterized from two points of view: the perspective of adopted by agent-based econophysicists who have to justify their works; and the viewpoint of statistical econophysicists who see the evolution of their field. Agent-based econophysicists did not change the hard core of econophysics; they keep the same fundamental aspects shared by all authors involved in the statistical tradition. While the emergence of the bottom-up agent-based econophysics extended the protective belt of the research programme, the top-down agent-based econophysics solidified this belt by integrating the existing methodological approaches.

VII. Conclusion

This chapter dealt with the methodological diversification of econophysics. More precisely, I showed that although econophysics is often presented as a unified area of knowledge (Abergel et al., 2014; Slalina, 2013) this new field is rather characterized by a profusion of works that deal with complex economic phenomena.

Statistical econophysics emerged in the 1990s and it defined the original core assumptions of the field. A decade or so later, a bottom-up agent-based econophysics progressively appeared to solve the increasing number of anomalies that scientists were first faced with. Even though this second way of doing econophysics kept the core assumptions of the field, it also faced some problems for which a third approach (top-down agent-based econophysics) emerged in order to investigate. This evolution of the field is interesting since it shows, at the same time, a conceptual coherence (the three approaches keep the same core assumptions) and a methodological diversification (development of a micro and macro methodology). The combination between a micro and a macro approach, although incompatible at first sight, found its origin in the historical roots of econophysics, which dated back to the works on complexity that were promoted by the Santa Fe Institute. In this chapter, I used a Lakatosian framework to show how this diversification can be seen as an extension of the conceptual protective belt protecting the core of econophysics. The following two tables briefly summarize the major points discussed in this chapter and the role played by the traditions in the research programme that I called econophysics.

	Statistical econophysics	**Bottom-up agent-based econophysics**	**Top-down agent-based econophysics**
Hard core: **Existence of power laws**	Defined	Preserved	Preserved
Protective belt: **Asymptotic reasoning**	Defined	Extended	Solidified

Table 2: Lakatosian comparison between the three approaches in econophysics

The use of asymptotic reasoning to justify the occurrence of power laws gradually took several forms. Originially, scholars working on (statistical) econophysics explained the existence of these power laws by using an asymptotic reasoning applied in a macro-analysis of data. Progressively, this perspective has been extended with the development of bottom-up agent based econophysics that enlarged the methodological scope of econophysics. Precisely, this approach shows that the emergence of power laws can actually be justified at a microscopic level. Finally, the recent category of works dealing with top-down agent-based econophysics provides a methodological link between the two previous categories of works; solidifying therefore the protective belt and the use of asymptotic reasoning as a way of dealing with power laws in econophysics.

Beyond this categorization, each methodological approach can also be summarized through their dissimilarities, as illustrated in the following table:

	Statistical Econophysics	Bottom-up agent-based Econophysics	Top-down agent-based Econophysics
Methodology	**Phenomenological**	**Bottom-up**	**Top-down**
Initial conditions	**High number of observations**	**Pre-defined micro interactions**	**Statistical assumptions**
Outcomes	**Statistical macro patterns**	**Emerging macro order**	**Micro interactions compatible with a pre-existing macro pattern**
Goal	**Backward-looking (Fitting data for descriptive purposes)**	**Forward-looking (Reproducing data for predictive purposes)**	**Combination of fitting and reproducing data for predictive purposes**
Machinery (implementation of the asymptotic reasoning)	**Statistical processes**	**Algorithmic processes**	**Statistical/Algorithmic processes**
Emergence (treatment of emergent properties)	**No condition of derivability between the macro and the micro level**	**Definition of the micro level from which the macro level must be derived**	**Definition of the macro level from which the micro level must be derived**

	Heterogeneous version of reduction	Heterogeneous version of reduction	Heterogeneous version of reduction

Table 3: Comparison between the three approaches in econophysics

The first line of this table refers to the methodological angle chosen by the three methodological traditions to deal with complex economic/finance systems. The second one echoes the initial conditions (i.e. starting points) required by these traditions in order to implement their methodology. Afterwards, I evoke the form of knowledge that these three approaches propose by clarifying the outputs they offer. My presentation also differentiates these traditions in terms of goals and machinery (techniques) used by econophysicists. The goals refer to what scholars want to do by implementing their research. Their goal can be backward-looking or forward-looking since it can consider the present as either the starting or the final point of the research. While statistical econophysics aims at fitting historical data for description (backward-looking), bottom-up agent-based econophysics instead algorithmically reproduces data for predictive purposes (forward-looking). The following elements of the table refer to the way each of the three econophysics approaches characterizes the concepts of emergence and reduction.

It is worth emphasizing that the diversity of elements presented in this table does not call the conceptual hard core of econophysics into question. In contrast, the first table above shows that this diversity rather indicates a particular dynamics of research that is trying to keep the original assumptions of the field intact. While this chapter focused on the internal diversification of econophysics, the fourth chapter will deal instead with the way econophysicists produce their knowledge and how they justify the analogical extension of their works to financial economics.

Chapter 4: Modelling practices in econophysics and economics

Part I: Modelling practices in econophysics

I. Introduction

This chapter deals with the treatment of models in econophysics. Because econophysics is a new boundary (in-between) field (Chapter 1) founded on a conceptual coherence and a plurality of methodologies (Chapter 3), this area of knowledge appears to be an interesting ground for investigating philosophical issues that are usually associated with the theme of models in science. In line with the methodological categorization suggested in the third chapter, this final chapter studies how econophysicists implement models and how they justify/use them in their research.

Chapter 2 explained what the historical and contextual factors were that favoured the emergence of econophysics. The idea of importation of techniques and concepts from physics to economics is extremely important because it denotes a specific way of developing knowledge that involves two different disciplines. If physicists export their conceptual tools into another disciplinary horizon, that means they probably "see" something familiar outside of their borders. Therefore, this in-between situation provides a unique environment wherein the modelling practices can be studied through the lens of analogy. This chapter investigates the role of analogy in the extension of the econophysics model in financial economics.

By definition, an analogy is a comparison between two objects/systems that have similarities. Analogies play a key role in scientific practices: several authors have emphasized their pedagogical utility (Hodstrater, 2001; Weisberg, 2016) while others have detailed their heuristic role in the aid of discovery (Bartha, 2013; Bailer-Jones, 2009). In the context of the development of econophysics, which is characterized by an extension of physics outside of its borders, the issue of scientific analogy became particularly interesting. What do econophysicists see in financial economics that could appear so familiar to them? What are these similarities that paved the way for physicists to export their knowledge to finance? More precisely, I will investigate how econophysicists justify their modelling practices through a formal analogical reasoning in contrast with economists' way of understanding this modelling.

If one considers econophysics to be a new field developed by physicists for physicists, the issue of justification can be perceived as unidisciplinary. In this context, econophysicists generate abstract works that are published in physics journals without any economic justification or implication. This kind of situation would associate the field with a purely abstract intellectual game that involves economic data, and it would not raise a special philosophical interest. Although this way of considering econophysics is well spread among economists, the explicit objective of econophysics is to go out of physics since its scholars aim at developing tools that could be useful for practitioners and policy makers (Johnson et al., 2003; McCauley et al., 2016).

In this challenging situation, this chapter shows that the status of econophysical models differs radically, depending on the context in which they are considered. Because econophysicists develop models that capture the dynamics of economic actors/systems by using methods and concepts coming from physics, they do not really take into account the existing theories that were developed by financial economists (Jovanovic and Schinckus, 2017). In this context, it is important to understand the role of analogies and what is meaningful for econophysicists in the extension of their models in finance/economics.

The first part of this chapter presents how econophysicists formulate their reasoning by presenting the first econophysical model (Stanley et al., 1996). This model can be labelled as working in statistical econophysics, which is still, today, the largest part of the literature in the field (Gingras and Schinckus, 2012). Afterwards, I will investigate how this knowledge is stabilized. This analysis of the justification will be deconstructed into two steps: what makes sense for econophysicists and what makes sense for economists. This peculiar analysis will clarify how these two communities differ in their way of developing models and what is required in order for these two communities to accept the explanatory dimension of a model. The analogical nature of models will be presented as an essential aspect for econophysicists, whereas financial economists have different modelling practices that are based on a testing methodology. Finally, the last part of this chapter will explain the reasons for why one can observe epistemological gaps between econophysics and economics. This part will clarify the reasons for what can be seen as an explanation for physicists is simply perceived as a non-justified induction for economists. Beyond developing a better understanding of the status of models in econophysics, this chapter also contributes more generally to debates in philosophy of science about the use of analogies in science and Thomas Kuhn's thesis of incommensurability.

II. Econophysical Modelling: A telling example

Chapters 2 and 3 explained the turning point that took place in physics in the 1980s and the 1990s concerning the new connection between the theories of statistical mechanics (also called statistical physics) and social sciences. Statistical physics' main purpose is to explain the macroscopic behaviour of a system and its evolution, in terms of physical laws that govern the motion of the microscopic constituents (atoms, electrons, ions, spins, etc.) that make it up. Statistical physics distinguishes itself from other fields of physics through its methodology, which is based on statistics. This is due to the enormous number of variables on which statistical physicists have to work; for instance, Avogadro's number (6×10^{23}) refers to a gigantic number of equations of motion that have to be solved[161]. This high number of relationships makes a strictly based-equations analysis unworkable, even for a computer. "Quite plainly, this is impossible … [the] subject is so difficult that [physicists] are forced to adopt a radically different approach to that employed in other areas of physics" (Fitzpatrick, 2012, p. 4). From this perspective, statistics became a very important tool in physics where particles' behaviour is described

[161] As Fitzpatrick (2012) noticed, to solve a system with 6×10^{23} particles exactly, we would have to write down 1,024 coupled equations of motion, with the same number of initial conditions, and then try to resolve the system.

through the statistical properties of each particle motion. The methods used in statistical physics are thus essentially dictated by the complexity of the systems, due to the enormous number of constituents. This situation leads statistical physicists to start with statistical information about the motions of the micro constituents' properties of the system in order to statistically infer some macro properties for this system. The statistical approach is so common that "in most situations physicists can forget that the results are statistical at all, and treat them as exact laws of physics" (Fitzpatrick, 2012, p. 6)[162]. This integration of statistics into physics occurred in the 1970s as the direct result of this problematic of extremely voluminous data. The second chapter explained how the progressive computerization of society and economic sphere generated a huge amount of data that began to attract the attention of physicists. The computerization of financial marketplaces and the systematic recording of all transactions have created huge databases that have become attractive for all disciplinary profiles that have a strong background in statistics. This section presents how the first econophysical model emerged and how statistical physics has been gradually extended to economics and finance.

II.1. From DNA to econophysics

The term "econophysics" was created in 1996 in an article written by Stanley et al. (1996), strangely entitled, "Anomalous fluctuations in the dynamics of complex systems: from DNA and physiology to econophysics". This section presents this paper in more detail and shows how authors developed the first econophysics model for describing the dynamics of companies' growth rates. At first sight, one could ask what the link is between DNA and econophysics. As the abstract of the paper notes, the authors aimed to export physics into other disciplinary contexts. Precisely, they wanted to:

> "discuss examples of complex systems composed by many interacting subsystems […] These includes the one-dimensional sequence of base pairs in DNA, the sequence of flight time of the large seabird Wandering Albatross

[162] For instance, as Fitzpatrick (2012) commented, the familiar equation of state of an ideal gas, P V = n R T, is actually a statistical result. In other words, it relates the average pressure (P) and the average volume (V) to the average temperature (T) through the number (n) of particles in the gas. "Actually, it is virtually impossible to measure the pressure, volume, or temperature of a gas to such accuracy, so most people just forget about the fact that the above expression is a statistical result, and treat it as a law of physics interrelating the actual pressure, volume, and temperature of an ideal gas" (Fitzpatrick, 2012, p. 6).

and the annual fluctuations in the growth rate of business firms" (Stanley et al., 1996, p. 302).

How can DNA, seabirds and business growth rate be related? How (and why) can physicists model these different phenomena within the same conceptual framework? The major idea connecting these complex phenomena refers to the existence of anomalous fluctuations in their dynamics. According to Stanley et al. (1996), these anomalous changes indicate analogies in the underlying mechanism in totally different systems. Concretely, the authors focused on correlations between the anomalous variations in the sequence of DNA, sea birds movements and the sales fluctuations of firms. Such statistical analysis aims at identifying common patterns in these complex large fluctuations. Stanley et al. (1996) began their argument by studying the anomalous variations in the DNA walks (frequency of each pairing nucleotide couple changes). After having observed the existence of anomalous fluctuations, the authors gave a visual representation of how nucleotides couple each other. Here is an illustration of such visualization:

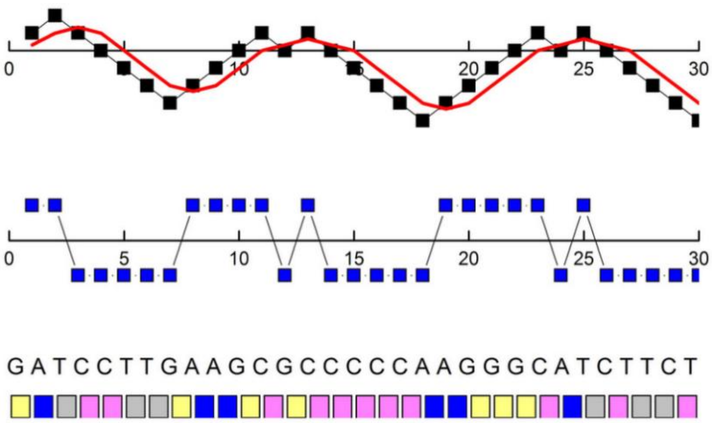


Figure 1: DNA fluctuations—Source: Carbone (2013)

This graph shows three levels of visualization (linear, discontinuous and continuous) of different kinds of nucleotides (characterized by three levels of colours: white, dark grey and light grey). What is important here is the evolution of the DNA where the movement of the nucleotides can move either up (u(i) = +1) or down (u(i)=-1) for each step of the walk. In other words, positive fluctuations (going up on the graph above) corresponds to what geneticists call a "purine-pyrimide" pair, while negative fluctuations (going down on the graph above) refers to a "hydrogen bond" pair. This visual representation is very important because it allows Stanley et al. (1996, p. 303) to go further in their reasoning by proposing the analogy of the DNA sequences and the Ising system, which characterizes the polarization of metallic entities in a magnetic field. In other words, Stanley et al. (1996) associated the move (up or down) with the potential orientations of ferromagnetic particles in a system submitted to an important change of the temperature (see next section). In this analogy, all nucleotides going up would be associated with a positive polarization of metallic entities, while those pointing down would view a negative polarization. Before continuing the presentation of the first econophysical model, it is worth presenting this Ising model, which Stanley et al. (1996) seem to consider as well-known by the readers, in more detail.

II.2. The magnetic appeal of the Ising model

Because the Ising model is a foundational element (an "exemplar", as I will explain) of econophysics, it is important present this framework. Let us begin this section with a peculiar phenomenon: beyond a critical temperature (770°C), iron exhibits paramagnetic rather than ferromagnetic behaviour, implying that it loses its magnetic feature above this temperature. The idea of the Ising model is to describe microscopically why the system exhibits radical changes in its properties at a critical temperature. This situation was modelled in 1925 by Ernst Ising, who, by uncovering concepts that were not yet developed (universality, renormalization and emergence), correctly demonstrated the phenomenon of magnetization for a system composed of two-state spins[163].

This model is considered to be the simplest description of a system that has a critical point; it played a central role in the development of research into critical phenomena and it occupies a place of importance in the minds of econophysicists. Briefly, the Ising model consists of discrete variables that represent magnetic moments of

[163] Ernst Ising (1900-1998) was a German physicist who worked on modelling of ferromagnetism. The Ising model published in *Zeitscrift of Physik* in 1925 is his major contribution to physics—it is quite interesting to mention that although Ising became professor of physics at the Bradley University (Illinois, USA), he never published again after 1935 and he instead focused mainly on teaching activities. For more information on the history of the Ising model, see Taroni (2015) and for more biographical elements on Ising, see Kobe (1996).

atomic spins, which can take one of two states, +1 (“up”) or −1 (“down”), the two states refer to the direction taken by the spins. The concept of spin characterizes the circular movement of particles (electrons, positrons, protons, etc.) implying that they have a specific rotation as described below.

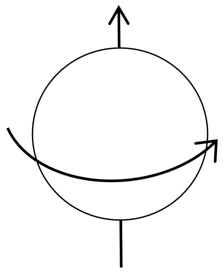


Figure 2: Schematisation of a particle’s spin—Source: Jovanovic and Schinckus (2017).

There is no way to speed up or slow down the spin of an electron (i.e. its revolution on itself) but its direction can be changed due to particular physical conditions, such as an important change of temperature. The interesting element is that the direction of one spin directly influences the direction of its neighbour spins.

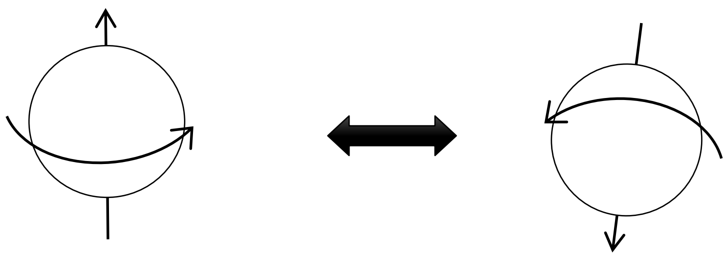


Figure 3: Schematisation of the interaction between particles’ spins—Source: Jovanovic and Schinckus (2017).

This influence can be captured through a function of correlation that measures the extent to which the behaviours of spins are correlated. The major idea of the Ising model is to describe this interaction between particles’ spins. From this perspective, the spins are arranged in a graph, usually a lattice, in which each spin exerts an influence on its neighbours. This influence is measured by the distance over which the direction of one spin affects the direction of its neighbour spins. This distance is called the correlation length; it has an important function in the identification of critical phenomena. Indeed, the correlation length measures the distance over which the behaviour of one microscopic variable is influenced by the behaviour of another. Away from the critical point (at low temperatures), the spins of an iron specimen point in the same direction. In such a situation, the thermal energy is too low to play a role; the direction of each spin depends only on its immediate neighbours making the correlation length finite.

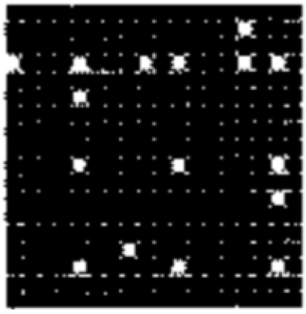


Figure 4: Two-dimensional Ising model at low temperature.
Source: http://www-f1.ijs.si/~vilfan/SM/ln4b.pdf (p. 98).

Figure 4 shows an almost black graph that indicates that all the spins are pointing in the same direction. In terms of relations (correlation length), this implies that each spin is directly dependent on and influenced by its close neighbours. In this situation, iron can be magnetized simply because all the microscopic entities are pointing in the same direction, thereby easing the diffusion of a magnetic field through the system. At the critical point, when the temperature (770°C) has been increased to the critical temperature, the situation is completely different. The spins no longer point in the same direction because the thermal energy influences the whole system and the magnetization spin-spin vanishes. In this critical situation, spins point in no specific direction and follow a stochastic distribution.

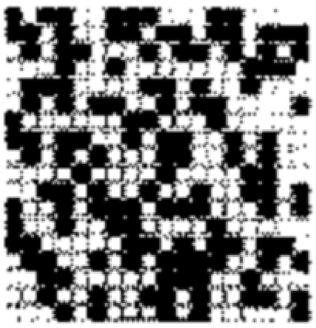


Figure 5: Two-dimensional Ising model at the critical temperature
Source: http://www-f1.ijs.si/~vilfan/SM/ln4b.pdf (p. 99).

As we can see in Figure 5, there are regions of spin up (black areas) and regions of spin down (white areas), but all these regions are speckled with smaller regions of the opposite type, and so on. In fact, at the critical point, each spin is influenced by all other spins (not only its neighbours) regardless of their distance[164]. From this perspective, the Ising model offers a particular description in which the coupling of neighbour pairs (taking an up or a down direction) can explain the magnetization of iron, and where the correlation length is presented as a measure of this magnetization (the higher, the less magnetic the specimen is). When the system reaches the critical temperature, we have a specific configuration in which the correlation length is very important (it is considered to be infinite). At this critical state, the whole system appears to be in homogeneous configuration, characterized by an infinite correlation length between spins (whatever the size of the system, all spins influence each other).

What is interesting is the statistical dynamics of these correlation lengths that phenomenologically follow a power law. Indeed, physicists have observed that the magnetization (M) evolves as a power law, depending on the level of temperature (t). Statistically speaking, this phenomenon takes the following form,

$$M \sim \left|t^{\beta}\right| \qquad (1)$$

Where β is called the critical exponents. This statistical characterization is very important because it offers an important tool for analyzing the system. In particular, Onsager (1944) showed that power laws exhibit scaling properties, implying that the spin system has the same statistical properties regardless of the scale (microscopic or macroscopic) considered. The scale invariance assumption was not new in physics[165], but the method allowing the mathematical demonstration of invariance was only established at the end of the 1960s by Kadanoff (1966) and Wilson (1971) with his renormalization group theory (which I presented in the previous chapter)[166]. As a reminder, this theory is "a method for establishing scale invariance under a set of transformation that allows us to investigate changes in a physical system viewed at different distance scales" (Morrison, 2016, p. 57). Before the development of this theoretical framework, universal behaviours (for instance, the fact that the correlation

[164] This is due to the magnetization of the spins pointing in the same direction.
[165] It exists in the work of Euclid and Galileo, for example.
[166] For further information about the link between scaling law and the renormalization group theory, see Goldenfeld (1992).

lengths between spins follow a power law) were observed experimentally without theoretical foundation (Morrison, 2016). This theory makes it possible to study mathematically macroscopic regularities that occur as a result of microscopic random interactions without to having study these microscopic interactions[167]. The focus is therefore on the macroscopic level, which is directly observable for physical phenomena. In other words, since the 1970s, due to scale invariance, physicists can infer from the microscopic constituents some key parameters that allow for the capture and description the dynamics of macroscopic behaviours without studying, in detail, what happens at the microscopic level. For these reasons, scale invariance is the foundation of any modern approach of statistical physics that is aimed at understanding the collective behaviour of systems that have a large number of variables that interact with each other. From this perspective, the renormalization group method can then be applied. By performing successive transformations of scales on the original system, one can reduce the number of interacting spins and therefore determine a solution from a finite cluster of spins.

Beyond the ability to describe the spin's movement, there is another point of interest in the Ising model. Because of its very simple structure, it is not confined to the study of ferromagnetism. As the philosopher of physics, R.I.G. Hughes wrote, "[p]roposed as a model of ferromagnetism, it [Ising model] 'possesses no ferromagnetic properties' " (Hughes, 1999, p. 104)! Its abstract and general structure has enabled its use to be extended to the study many other problems or phenomena:

> "The Ising model is employed in a variety of ways in the study of critical point phenomena. Ising proposed it […] as a model of ferromagnetism; subsequently it has been used to model, for example, liquid-vapour transitions and the behaviour of binary alloys. Each of these interpretations of the model is in terms of a specific example of critical point behaviour. [T]he model also casts light on critical point behaviour in general. Likewise, the pictures generated by computer simulation of the model's behaviour illustrate […] the whole field of scale-invariant properties" (Hughes, 1999, p. 124–125).

This model has been implemented to describe the behaviour of gases (Eyring, 1939) and, afterwards, it has been widely used to characterize various physical systems,

[167] To understand the importance of this approach, one has to keep in mind that the macroscopic level is directly observable–for instance a table—but the microscopic level—the molecules that constitute the table—is not directly observable (one needs a tool, such as a microscope).

such as fluid and gas dynamics[168] that exhibit radical changes in their properties at a crucial temperature. For these reasons, statistical physicists consider the Ising model as the perfect illustration of the simplest unifying mathematical model. Their looking for such models is rooted in the scientific view of physicists for whom "the assault on a problem of interest traditionally begins (and sometimes ends) with an attempt to identify and understand the simplest model that exhibits the same essential features as the physical problem in question"[169]. According to Hughes, (1999, p. 99), the advantage of the Ising model meets this requirement and its use is not restricted to statistical physics because "the specification of the model has no specific physical content"; its content is mathematical. Therefore, this model is independent of the underlying phenomenon studied and it can be used to analyze any empirical data that share the same characteristics. With these new theoretical developments, statistical physicists had a powerful mathematical model and method that could solve crucial problems in physics or in all areas of knowledge wherein phenomena can be interpreted in accordance with the foundation of the model. They were able to establish the behaviour of systems at their macroscopic level from hypotheses about their microscopic level, but without analyzing this microscopic level.

This section aimed to clarify the Ising model to which Stanley et al. (1996) refer in their paper where they coined the term "econophysics". After having explained the importance of this Ising model in physics, I come back now to the presentation of this seminal paper. We will see that the statistical characterization (power laws) of the magnetization (correlation lengths between micro entities) is the heart of the explanatory dimension of statistical econophysics.

II.3. Back to DNA

As noted previously, Stanley et al. (1996)[170] proposed an analogy between the magnetization in the Ising model and the walks of DNA sequences. Starting from the visualization of these DNA walks (Figure 1), the authors wrote:

[168] For further information on the potential extension of Ising model, see Taroni, 2015.
[169] See Alastair and Wallace (1989, p. 237) for further information.
[170] It is worth mentioning that the first author of Stanley et al. (1996) wrote an important book in the seventies on the importance of the Ising model for explaining transition phases. For further information, see Stanley (1971).

"coding sequences typically consists of a few lengthy regions of different strand bias, *resembling domains in the system in the ferromagnet state*. These observations can be tested by rigorous statistical analysis. Such DNA landscapes naturally motivate a quantification of these fluctuations by calculating the 'net displacement' of the walker after *l* steps, which is the sum of the unit steps *u(i)* for each step *i*" (Stanley et al., 1996, p. 309, my italics).

From this perspective, the dynamics of the DNA sequence after *i* steps can be considered as a sum where the trajectory (*l*) can be expressed as follows:

$$y(l) = \sum_{i=1}^{l} u(i) \tag{2}$$

Another important indicator in this walk is given by the root mean square fluctuation about the average of the displacement (*l*). Statistically, this quantity can be estimated with the following relation:

$$F^2(l) = \overline{[\Delta y(l)]^2} - \left[\overline{\Delta y(l)}\right]^2 \tag{3}$$

where the $\Delta y\,(l)$ is defined by $y\,(l_0 + l) - y(l_0)$ and the bars indicate the average over all positions l in the gene. This quantity informs us about the average sequence in the dynamics of the DNA sequences. What Stanley et al. (1996) wanted to describe is the anomalous fluctuations around this average (i.e. dispersion) and they observed that the statistical distribution of these variations follows a power law taking the following form:

$$F(l) \sim l^{\alpha} \tag{4}$$

with the critical exponent α < 2 (implying that the variable follows a stable Lévy process, as I explained in the first chapter). Visually, that means that the coding of the DNA sequences behaviour is linear on a log-log plot of $F(l)$ as the following graph shows:

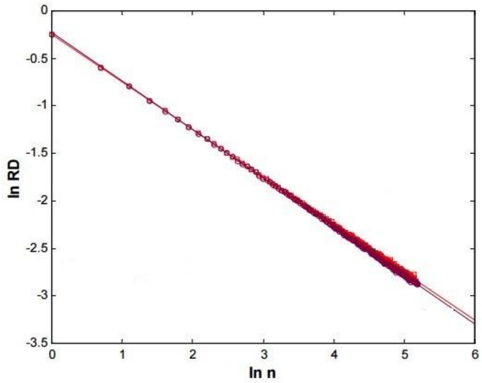


Figure 6: DNA sequence behaviour - Source: José et al. (2009, p. 12)

This diagram indicates a power law in the relative dispersion (RD) of fluctuations after *i* steps. One can observe that these fluctuations evolve in line with a power law according to which every step generates a variation that is exponentially correlated to the previous one. But how can this empirical observation be related to the Ising model? Interestingly, Stanley et al. (1996) questioned their own methodology by asking "how can power law correlations arise in the one-dimensional system such as DNA in analogy with spins of one-dimensional Ising models?" According to the authors, these two phenomena belong to the same category of events that exhibit the same statistical structure. This formal structure is clarified by Stanley et al. (1996) when they assume the existence of only two kinds of nucleotides (say *a* and *b*), each of them can be represented by a step up or a step down in the DNA sequences (one can notice the first similarities with the Ising system discussed in the previous section). After *k* steps, the dynamics will generate a sequence of 2^k nucleotides, whose total excess of *a* nucleotides over the *b* ones is given by the following relationship:

$$\Delta y = \sum_{i=1}^{2^k} u(i) \qquad (5)$$

Schematically, this process can be summarized by the figure following on which each tree-like structure can be associated with a step in the dynamics of the DNA sequences.

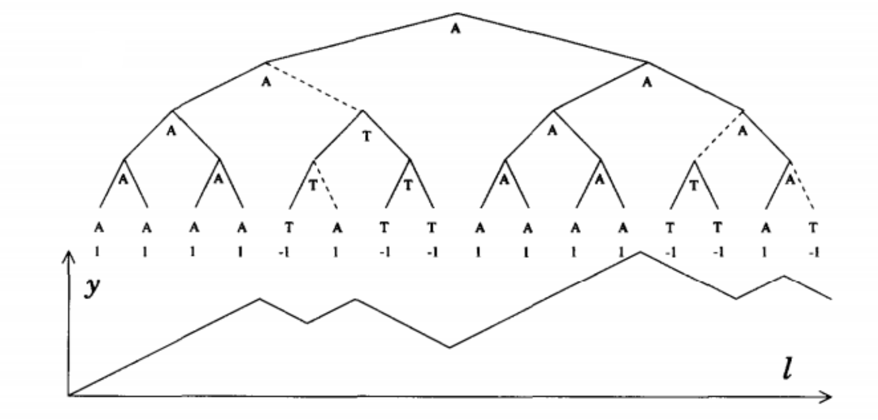


Figure 7: Step in the dynamics of the DNA sequences - Source: Stanley et al. (1996, p. 310)

This graph is important in the reasoning because it shows the long-range correlations that result from the fact that all nucleotides are descendent from a common origin. So statistically speaking, the move (*l*) decays exponentially with a factor *k* and acts therefore as a power law. In other words, this way of describing the DNA sequence is similar to the way the Ising model describes a critical phenomenon: all micro entities can take only two directions (up or down); these entities are correlated and the length of their correlation appears to follow a power law (straight line on a log-log graph). This similarity led Stanley et al. (1996) to use the Ising model to describe the DNA walks.

II.4. What is the link with econophysics?

In their article, Stanley et al. (1996) deal with unrelated phenomena, which they describe through the same conceptual framework. After having characterized the evolution of the DNA sequences in terms of the Ising model, the authors used the same analogy[171] to describe the fluctuations of annual growth rates for firms by showing that the dynamics of sales generate the same statistical situation as the one observed for spins movement in the Ising model. Using public data published by American companies, the authors worked on the average annual fluctuations of sales σ (S_0) (and number of employees), which they presented as a function of the initial value of sales (initial number of employees) S_0. Observing the evolution of this

[171] The analogical nature of econophysical models will be studied in detail later in this chapter.

variable, they noticed that "the remarkable linearity of the $\sigma\ (S_0)$ vs S_0 function on a log-log scale over many orders of magnitude may indicate some universal law of economics that is applicable for small companies [...] as for giants of size" (Stanley et al., 1996, p. 311). This power law discovered by the authors takes the following form:

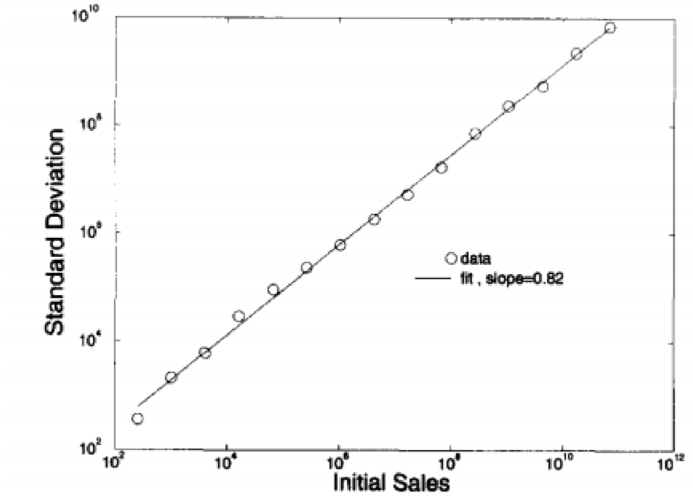


Figure 8 Companies sales growth: —Source: Stanley et al. (1996, p. 311)

This diagram shows a power law dependence between the standard deviation $\sigma\ (S_0)$ of sales and the initial level of sales (S_0) as expressed in the following relationship:

$$\sigma\ (S_0) \sim S_0^{\alpha} \tag{6}$$

where α is empirically estimated at 0.82. This power law characterizes the evolution of sales, which increases by following a constant pattern. The authors assumed that this evolution has its origin in the internal structure of each firm. In so doing, they considered that the evolution of sales (or the employee number) results from N independent units, which can be computed as follows:

$$S_0 = \sum_{i=1}^{N} \varepsilon_i \tag{7}$$

where the unit sales ε_i have an average of $\varepsilon = S_0/N$ and an annual variation $u(i)$ independent of S_0. In this context, the annual change in sales can be estimated by:

$$\Delta S = \sum_{i=1}^{N} u(i) \tag{8}$$

The familiarity of this equation with the statistical description of the DNA sequence walk (see eq. 2) caught the attention of Stanley et al. (1996) who wrote that the evolution of sales for companies and the DNA sequence walks can be explained through the same conceptual framework (i.e. Ising model): “Remarkably, the hierarchical structure of the company can be mapped exactly onto the diagram of the DNA mutations” Stanley et al. (1996, p.312). Visually, we have:

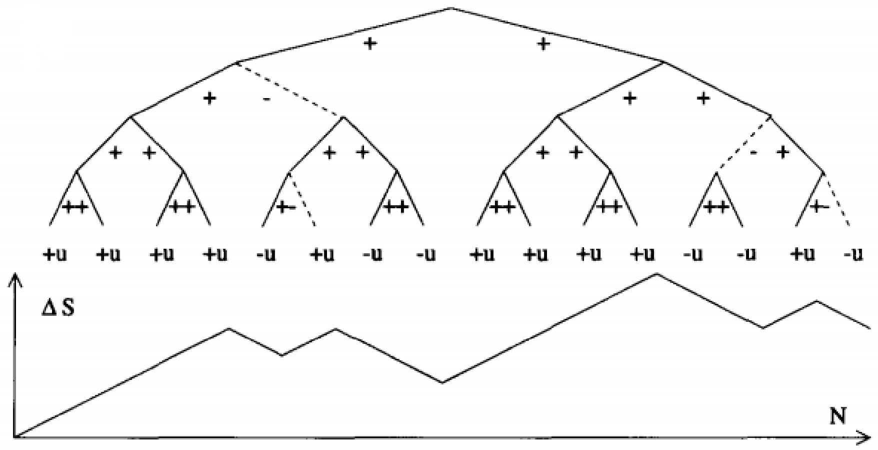


Figure 9: Hierarchical structure of the company - Source: Stanley et al. (1996, p. 310)

Stanley et al. (1996, p. 312) described this diagram as follows:

> “Each level of the firm hierarchy corresponds to one generation of repeat family and each modification of the head decision by the lower level management corresponds to a mutation. Note that the σ (S_0) for firm sales is exactly $F(l)$ for DNA sequences” [see the similarity between eq. 4 and eq. 6 for an illustration of these words].

Considering the duality (flying or sitting on the water) of sea birds’ behaviour, the authors extended the conceptual framework to the description of sea birds’ migration by quantifying their behaviour with the help of an electronic recording device that was placed on the legs of several birds.

It is worth mentioning that the vast majority of statistical econophysicists do not detail the statistical structure of variables as Stanley et al. (1996) did. However, this literature often quotes Stanley et al. (1996) as a seminal paper (see Gingras and Schinckus, 2012 for further information on the importance of this paper for the literature in econophysics). Methodologically speaking, models developed in statistical econophysics are founded on the existence of a power law pattern in which scholars adopt the reasoning proposed by Stanley et al. (1996). In other words, these authors provided the methodological foundations for statistical econophysics by making the Ising model an "exemplar" of the field (I will detail this claim later in this chapter).

This extension of the Ising model was used for coining the term "econophysics" and it is today a seminal article that founded the scientific justification of the field. It is worth mentioning that although this paper is largely quoted in the econophysics literature, no work questions the scientific justification of the approach. Another seminal article (and probably the first econophysics paper even though the term did not exist yet), by Mantegna (1991) is a telling example of the way econophysicists work: the author observed phenomenologically that the anomalous fluctuations of the Milan stock exchange follow a power law with a critical exponent lower than two. Considering the scaling properties of this particular statistical pattern, Mantegna (1991) made some recommendations in terms of analysis of the financial markets. However, he did not go beyond the phenomenological description of the financial data (Mantegna (1991) was aware of this aspect since he mentioned it in his discussion section). Interestingly, this paper also contributed to the crystallization of econophysics since it initiated the macro approach that is widely used today by statistical econophysicists. The following section will study in more detail the way econophysicists sustain their methodology and how they justify their analogical extension of the Ising model to a different disciplinary context.

Part II: The analogical extension of the Ising model

III. The econophysicists' view point

Econophysicists describe the dynamics of different phenomena through a particular statistical pattern that is founded on a specific statistical framework developed in the Ising model. Two aspects must be studied in relation to this way of modelling: the explanatory nature of the Ising model and the relevance of its analogical extension for characterizing a variety of different events. In this section, I will investigate these aspects by analyzing, in a first step, the explanatory nature of the Ising model. Afterwards, I will discuss the extension of this frame to economics and finance. Finally, the third sub-section will conclude by showing how econophysics can implicitly be perceived as a Duhemian field (or, more precisely, a field based on a Duhemian use of analogy).

III.1. The explanatory nature of the Ising model

The Ising model generated an important body of literature in physics (Taroni, 2015) as well as in the philosophy of science (Hughes, 1999). The existing works on this model usually consider it to be a particular idealization of reality. Rohwer and Rice (2013, p. 338) wrote that "*Idealized models* aim at accurately representing differences makers and use idealization to indicate those causal factors that are irrelevant". As Strevens (2011) explained, such models are evidently false, but their function is not to focus on what does make a difference in the characterization of a target system. From this perspective, some characteristics of the phenomenon are deliberately omitted or changed with the objective of having a more tractable analysis. Scientific works are full of such idealizations ("isolated systems", "infinite velocity", "frictionless movement", "perfectly competitive markets", "perfectly rational agent", etc.). The case of a perfect pendulum is a classical example of idealization, where some properties of the pendulum are changed or ignored in order to have a situation in which the Newton's force law can be applied. Precisely, physicists usually assume the strings have no mass; that the length of the string is inextensible and not rigid, etc. Such idealization creates circumstances in which the treatment of the pendulum can be represented through the classical Newton's second law.

Wiesberg (2016) explained that idealization is a necessary condition for modelling and he identified three kinds of idealization: Galilean idealization, minimalist idealization and multiple–models idealization. The first refers to "the practice of introducing distortion into models with the goal of simplifying, to make them more mathematically or computationally tractable" (Wiesberg, 2016, p. 99)[172]. The second idealization focuses only on the core factors that give rise to a phenomenon, whereas the third form of idealization concerns situations in which a model is built by using related but incompatible models[173]. This categorization of idealizations will be useful in this chapter since I will later associate economists' modelling practices with a Galilean idealization, whereas econophysicists' practices will instead be analyzed afterward through the lens of a minimalist idealization.

The Ising model proposes a mathematical structure used to represent states (how spins are orientated) and relations between states (how spins can move), especially transitions (when spins are all orientated in the same direction). Because this model captures the key interactions that occur in a ferromagnetic phenomenon, this conceptual framework is usually presented as a *minimalist idealization* (Strevens, 2011). By suggesting that the only recurrent element that allows us to characterize complex economic/financial system is the macro occurrence of a power law, econophysicists explicitly identified what does not make a difference in our understanding of such systems: the detailed description of micro interactions. A minimalist characterization of this micro level (à la the Ising model) is enough to have an explanation. The objective of minimal models is to "contain only factors that make a difference to the occurrence and essential character of the phenomenon in question" (Weisberg, 2016, p. 100). This definition raises an interesting question about the explanatory nature of minimal models: are these core factors that are evoked by Weisberg (2016) causal? Can the Ising model be associated with a classical explanation? Regarding the explanatory nature of the minimalist model, Strevens (2011, p. 155) wrote that:

[172] The example of the pendulum mentioned above is good illustration of a Galilean idealization.
[173] Wiesberg (2016, p. 103) gave the example of the United States National Weather Service (NWS), which uses several different models of global circulation patterns to model the weather. Although these models do not necessary share the same kind of assumptions, their combination can generate a prediction.

> "the content of an idealized model can be divided into two parts. The first part contains the difference-makers for the explanatory target and if the model is perfect, is identical to the canonical model. The second part of all idealization, its overt claims are false but its role is to point to parts of the actual world that do not make differences to the explanatory target".

In the case of the Ising model, these false causal factors refer to the dual representations (only up or down) of spins. Even though all physicists acknowledge that this assumption is false (too simplistic), another (and more detailed) characterization of this movement would not add something to our understanding of this ferromagnetic phenomenon. That means that the plurality of directions in the characterization of the spins' orientation make no difference to the phenomenon—this assumption is a way of asserting that the potential spins' multi-orientation is irrelevant in the description of the phenomenon[174]. Although minimal explanation is presented by Strevens as the most appealing dimension of idealized models, the authors also emphasized other aspects that contributed to the use of such models in science:

> "Though an idealizing explanation is in certain way inferior to a canonical explanation, there are considerations of communicative effectiveness, descriptive and computational simplicity, and scientific economy that motivate the widespread use of idealization in explanation" (Strevens, 2011, p. 150)[175].

Hartmann (1998, p. 118) wrote that these core factors that are at the heart of minimalist models offer "partial understanding of the relevant mechanisms for the process under study" by providing cognitive tools for characterizing highly complicated dynamics. In the same vein, Cartwright (1989, p. 187) emphasized the cognitive dimension of these factors since they result from a mental operation in which "we strip away—in our imagination—all that is irrelevant to the concerns of the moment to focus on some single property or set of properties".

In the third chapter of this dissertation, I explained that although the methodological diversification of econophysics into three different approaches all based on the same conceptual hard core, they aim at explaining the emergent properties based systems by using an asymptotic reasoning. This point is important, because in the literature

[174] There is a body of literature that is trying to solve the Ising model in several dimensions (for further information on this technical aspect, see Lundow and Markstrom, 2014).

[175] By canonical explanation, Strevens (2011, p. 152) means "a causalist account of explanation".

(Hartmann, 1988; Batterman; 2000, 2002; Strevens, 2011; Weisberg, 2016), the use of asymptotes in physics is a telling example of minimalist idealization used to study the behaviour of systems at the limits of certain physical magnitudes. As a reminder, Chapter 3 presented this asymptotic reasoning as a novel (not expected) and robust (regularly observed) pattern resulting from the idea that the macro system can be perceived as a sequence of micro systems whose parameters can go to infinity. More formally, for a complex system where *n* is the number of observations, we can write:

$$\lim_{n\to\infty} T_2 = T_1 \qquad (9)$$

where T_1 is the emerging property and T_2 refers to the theory that represents the micro interactions. For each methodological approach, I justified the use of an asymptotic reasoning. Specifically, for statistical econophysics, I explained that, in this relationship, T_1 refers to the power law observed at the macro level while T_2 instead characterizes the description of micro interactions. I also noted that the usual justification for the use of this limit refers to the necessity of dealing with a collection of 10^{23} micro components, which is infinite from a practical point of view.

Why do I mention the asymptotic reasoning here? Simply because the asymptote plays an important role in the extension of the Ising model outside physics. To be precise, this conceptual framework is supposed to describe the behaviour of a large number of spins so that its extension in a non-physical environment requires a necessary condition: this model can be applied in an environment characterized by a high number of components. In biology, the human body is composed of a wide range of cells, implying that the study of DNA sequences meet this necessary condition (the polarization of spins being associated with the orientation of the nucleotides in the DNA walks). When the Ising model is imported into social sciences, modellers have to justify this methodological jump by showing that their reasoning is founded on interactions between a great number of micro components. In their article, Stanley et al. (1996) emphasized this necessity by using it as a justification for applying the Ising model in economics: “It is difficult to obtain large databases on human behaviour unless we turn to economics where not only does a wealth of data exist but also the ‘human behavior’ is subject to well-defined rules” (Stanley, 1996, p. 316.) In the second chapter, I explained how the computerization of financial/economic reality contributed to the creation of huge databases, which facilitate the importing of statistical tools from physics. The role of the asymptote in physics has been emphasized by Batterman (2002), who explained that the use of this mathematical entity provides information about how complex systems would behave when some effects are removed. Specifically, the asymptotic reasoning offers “highly idealized minimal models of the universal, repeatable features of a system” (Batterman, 2002, p. 36). From this perspective, a model based on an asymptotic method aims at exhibiting a universal pattern for which adding more details to the minimal idealization would not improve the understanding of the target event.

The Ising model can be presented as a minimalist model that is implicitly based on what Batterman (2002) called an *asymptotic explanation,* which assumes that the system has an infinite number of micro entities in order to explain and predict the behaviour of the real (and therefore finite) systems. Mathematically, this assumption of an infinity of components is a non-physical (necessary) condition required to explain a physically plausible situation (a transition phase or a sudden alignment of all spins). This situation is possible because of the properties of the asymptote offering an analytic method for which a system with an infinite number of components converges towards a singular behaviour whose characteristics can be describe in finite terms. This way of dealing with complex physical systems is quite common in contemporary physics, as Morrison (2016, p. 29) explained:

> “A good deal of asymptotic behaviour that is crucial for describing physical phenomena relies on exactly these kinds of mathematical abstractions [asymptotes]. What we classify as ‘emergent’ phenomena in physics such as the crystalline state, superfluidity, and ferromagnetism [Ising model] to name a few, are the result of phase transitions whose theoretical representation requires singularities; hence their understanding depends on just the kinds of mathematical abstractions described above […] our understanding of phase transitions is inextricably linked to the mathematics of singular limits”.

Therefore, as a minimal model, the Ising model provides econophysicists with the necessary information to describe phenomena by exhibiting “how fundamental structural properties of a system generate common patterns among disparate phenomena” (Weisberg, 2016, p. 102). In so doing, the Ising model provides a mathematical characterization (how the spins behave) of a physical phenomenon (magnetization of an iron specimen) that will help physicists to infer physical properties (the dynamics of the magnetization) about the system they are studying. An interesting question now is to ask how this physical information about the magnetization can be transferred in a non-physical environment. I will investigate this aspect in the following sub-section.

III.2. The analogical extension of the Ising model to financial economics

The Ising model is now “part of the common culture of physics, as the simplest representation of interacting elements with a finite number of possible states” (Sornette[176], 2014, p. 17). This popularity of the model results from its mathematical structure, which can easily be applied to different contexts. As Taroni[177] (2015, p. 997) wrote, “Ising studied a deceptively simple model that, unknown to him at the time, captures the essential physics of an extremely wide category of problems. He may have been wrong in his 1925 work, but he tripped over a veritable physics goldmine”. This goldmine has been investigated by many physicists in different areas of their discipline (see McCoy and Maillard, 2012 for further details on the importance of Ising model in physics). By founding econophysics on the use of Ising model, Stanley et al. (1996) showed that this goldmine is not only restricted to physics. This trend was not new in the 1990s since other scholars had already modelled social interactions and organizations through the lens of the Ising model (Wiedlich, 1971, 1991, 2000; Callen and Shapiro, 1974; Montroll and Badger, 1974; Galam et al., 1982; Orlean, 1995). In these extensions of the model, authors usually characterize social phenomena, such as decisions in organizations, opinion polls or elections, by associating the formation of decisions with the magnetic orientations of spins (this specific literature is labelled “sociophysics”, see Galam, 2008 for a review of these works)[178].

[176] Didier Sornette is a professor of physics and professor of entrepreneurial risks at the Swiss Federal Institute of Technology in Switzerland. Sornette is a worldwide recognized econophysicist who mainly worked on prediction of financial crashes.
[177] Andrea Taroni is a statistical physicist currently working as editor in chief for *Nature Physics*.
[178] Sociophysic” refers to all articles extending physics to sociological issues, such as decision processes, mimetic behaviours, political choices, etc. This field is diversified and, in contrast with econophysics, it does not really offer a unified alternative based in a core statement to classical sociological analysis.

Some extensions of the Ising model also exist in economics[179], where the polarization of the spins can be analogically used to describe the formation of decisions of bounded rational agents (Roehner and Sornette, 2000) or to result from optimizing agents whose utilities incorporate a social component (Phan et al., 2004). Such extensions of the Ising model are mainly focused on the binary choice model of socially interacting agents, which allows modellers to obtain an Ising-like system. For instance, a spin taking the value +1 can be associated to a buyer, while a "-1 spin" is presented as a seller. This way of modelling starts with a particular description of micro entities from which a macro behaviour will emerge—this way of modelling can be related to agent-based econophysics (see Eckrot et al., 2016 and Chapter 3 for an analysis of this methodology).

The extension proposed by Stanley et al. (1996) when they created econophysics is quite different. They deal with what I called statistical econophysics. Indeed, the authors focused on the statistical evolution of large fluctuations of random variables around a normal (average) level of activity. In so doing, they started their analysis with specific results that were observed at the macro level of the systems, and they proposed an upstream reasoning showing that this macro behaviour can be perceived as the result of an Ising-like micro dynamics. Why did they think about this way of connecting the micro and the macro level? Simply because the macro patterns they observed exhibited specific statistical properties that are common for Ising-like systems. The core of this analogical extension is based on two components: the observation of a similar statistical structure (power law) and the element from which this pattern emerges (the dynamics of large fluctuations). The non-Gaussian nature of the evolution of financial prices is well known and called "stylized facts" in financial economics—these facts refers to anomalous that are not expected in relation to the theoretical mainstream (for which financial prices have a Gaussian dynamics)[180] whose large fluctuations are expected to be quasi non-existent. By having an indeterminate variance, power laws that are at the heart of the Ising model violate the Gaussian assumptions (possibility to have a finite variance).

[179] See Phan et al., (2004) for an overview of works dealing with this issue.
[180] While the first chapter presented how financial economists deal with the appearance of extreme values on financial market, the next section will clarify the explanatory nature (from an economist's viewpoint).

In other words, technically, what the Ising model allows the capture of is the statistical characterization of the correlation between the large variations. In a ferromagnetic environment, the Ising model describes that the length of correlations between spins (the process of magnetization) follows a power law, while in a financial context, the same model states that the length of correlations between large variations (which can be positive or negative) also follows a power law. Econophysicists extended the Ising model to the study of financial markets because they consider that the two systems or phenomena have something in common. From this perspective, the Ising model has been used as an analogical model for describing/representing an unfamiliar target system (extreme values in finance) in terms of a well-known/familiar framework (Ising model). In this context, it is important to understand the analogical reasoning used by econophysicists in their extension of the Ising model. This will be the aim of the following section.

III.3. Analogy in econophysics

An important literature exists for distinguishing analogical and metaphorical models (Hesse, 1953, 1964; Hutten 1954; Miller, 1996; Bradie, 1998; Bailer-Jones, 2002). Metaphorical models refer to a linguistic statement that has been transferred from one domain of application, where it commonly understood, to another domain in which it is unusual; whereas analogical models instead characterize statements that describe relational information through a transfer of a mathematical framework from one domain to another. In other words, metaphor is a simple descriptive comparison between two relevant domains (Bailer-Jones, 2009), while analogies are more likely to be mathematically formulated since they deal with similar dynamics, relations or processes observed in different domains—from this perspective, the extension of the Ising model by econophysicists in finance can be perceived as an analogical model. Roughly speaking, analogies are based on the understanding of something in terms of something else that is well understood and familiar. However, as Bailer-Jones (2009, p. 117) explained:

> "Being familiar does not equate with being understood, but familiarity can be a factor in understanding. This is also not to suggest that understanding can be reduce to the use of analogy, but having organized information in one domain (source) of exploration satisfactorily can help to make connections to and achieve the same in another domain (target). The aim is to apply the

same pattern assumptions of structural relationship in both source and the target domains".

In their seminal article, Stanley et al. (1996, p. 316) wrote, "The analogy between economics and critical phenomena [described by the Ising model] is sufficiently strong that a similar story might evolve." It is worth emphasizing that the authors wrote the word "might", showing therefore their deflationary perspective on the use of power law as a form in which the mind can grasp the complex nature of the phenomenon. In this context, the real question is to know if this Ising model (which gives physicists the opportunity to understand the process of magnetization very well) can really help to understand economic/financial phenomena. As mentioned earlier, the Ising model is used by econophysicists as an analogy between the source domain (physical systems) and the target domain (economic/financial systems). This reasoning can be summarized through a tabular representation found in Hesse (1966):

Physical systems	Economic/financial systems
Known similarities	
Interacting elements	Interacting agents
High number of micro components	High number of individual agents
Complex micro interactions	Complex micro behaviours
Observational similarity	
Dynamics following a power law	Dynamics following a power law

Table 1: Similarities between physical and economic/financial systems.

Hesse (1966) suggested clarifying the known similarities and observational ones to better understand the role of analogy in science. In accordance with this suggestion, I propose Table 1 above, where the horizontal relations are the relations of similarity in the mapping of the source and target domains, and the vertical relations are those between the objects and properties within each domain. What is interesting to emphasize here is the way of listing the horizontal similarities, because econophysicists and economists might agree on these aspects. However, the kind of conclusions one can draw from these characteristics would be totally different: while econophysicists consider that the emergence of a power law is an indication of complexity (Hughes, 1999); economists who use another statistical lens simply do not see this power law. As Bartha (2013, p. 6) noticed, the “manner in which we list similarities and differences, the nature of the correspondences between domains: these things are left unspecified [in Hesse’s works]”. In the third part of this chapter, I will explain that these aspects are directly related to the modeller’s disciplinary matrix.

Extending an earlier discussion on an analogy introduced by Keynes (1921), Hesse (1966) distinguished three kinds of analogies: negative, positive and neutral analogies. The former refers to relations that we know to be different between the two domains; the second one concerns the known (and acceptable) similarities and the latter characterizes what we do not know or what was not known before the association between the source and the target domains. In this sense, a negative analogy between physical systems and economic/financial ones could refer to the fact that in opposition with the former, the latter is composed of micro elements in economic/financial systems that have a human and social consciousness. The horizontal (known) similarities mentioned in the table above illustrate positive analogies, and the observational similarities could be seen as a neutral analogy in a sense that this similarity was neither assumed nor expected in the analogical association of the two domains. On this point, Frigg (2012, p. 14) wrote that “neutral analogies play an important role in scientific research because they give rise to questions and suggest new hypotheses”. In this occurrence, we can summarize the aforementioned table as follows:

Physical Systems	**Economic/financial systems**	
Interacting elements	Interacting elements	
High number of micro components	High number of micro components	Positive analogies
Complex micro interactions	Complex micro interactions	
Non-human micro entities	Human agents	Negative analogy
Dynamics following a power law		Neutral analogy
Plausibility	Dynamics following a power law	

In this analogical reasoning, econophysics consider that the statement according to which the dynamics of an economic/financial system follows a power law is plausible because of certain known similarities in physical systems that generate this kind of dynamics. Of course such analogical extension requires a particular interpretation of the "plausibility criteria" evoked above. Hesse (1966, p. 87) explained that this plausibility must be "acceptable in a scientific sense" and she added that "a tendency to co-occurrence" is an essential requirement for a good analogical association. In the case of econophysicists, they explicitly associate this plausibility with the statistical patterns they observe in economic/financial data. From econophysicists' perspective, the fact that power laws are regularly observed in empirical data and that these patterns can be explained mathematically appear to be an acceptable scientific reason for considering the plausibility evoked above. In other words, for econophysicists, this "co-occurrence" of power law in the source and target domains takes the form of a formal analogy.

Hesse (1966) distinguishes between two categories of analogies: formal ones and material ones. When the analogous refers to material entities (material analogy), the association between two domains is mainly based on the sameness or resemblance of common properties. These similarities being observable, the three levels of analogy evoked above are always present, but the negative one appears to be more obvious (Mellor, 1968). For instance, Earth and Mars are both stellar bodies, spherical, have moons and orbit the sun (positive analogy) but the observability of these common properties also makes obvious their differences: the absence of water/atmosphere on Mars, the distance between these two bodies and the sun, the periodicity of their respective circumvolution, etc. When two systems are related by formal analogy, they are both interpreted through the same mathematical framework. Very often, this kind of analogy concerns a situation in which the dynamics between certain ingredients within one domain are perceived as identical (or comparable) to the relations between elements of another domain (Bailer-Jones, 2009, p. 57). According to Mellor (1968) and Falkenhainer et al. (1989), when the analog between two domains refers to relations or dynamics, then, although the negative analogy is still present, it is less important, since only the formal evolution of the domain is taken as a formal analogy that focuses on the interrelationships between elements rather than their resemblances.

The Ising model has been analogically extended from physics to economic/financial systems because the formal characterization of its dynamics seems to be applicable for describing complex economic/financial dynamics. In other words, the Ising model is a formal analogy between a ferromagnetic system and an economic/financial system where the neutral analogy takes the form of a mathematical characterization (power law) of what was considered a statistical anomaly (occurrence of large fluctuations) by the existing financial (mainstream) knowledge. Given that, one could legitimately question the explanatory nature of a model that is used to explain several diverse phenomena. What do economists think about this extension? Do they consider it as scientific?

When Stanley et al. (1996) extended the Ising model in economics they also implicitly imported the scientific fabric usually associated with this model. Articles dealing with econophysics are mainly published in physics journals and assume that readers have a specific disciplinary background for understanding this type of research. For instance, the Ising model and the renormalization group theory are both well known for all statistical physics; and these two frameworks are often considered as the theoretical foundations econophysics[181]. For econophysicists, the epistemological justification of their works is quite simple: they use a familiar theoretical framework to describe complex phenomena that exhibits the same key features required to be studied through this frame. In other words, econophysicists did not produce their models out of nowhere: given the specific characteristics (emergence of extreme values in a particular dynamics) that they observe as physicists, they choose what appears for them to be an appropriate model (Ising model) to describe this phenomenon. This approach is justified in two ways: by scientific foundations of this familiar framework and by the empirical adequacy of results (what I previously associated with the co-occurrence of power laws in the physical and financial systems). Such extension of physics to another context is implicitly based on a justification that is internally (disciplinary) warranted but that can be questioned by scholars who are not familiar with physics. To better understand the disciplinary differences between financial economists and econophysicists, these questions must be analyzed from a different perspective: first from the viewpoint of an econophysicist and then from an economist's perspective. I will deal with these questions in the rest of this chapter. In the following subsection, I will initiate this analysis by explaining how econophysicists implicitly promote a Duhemian way of perceiving scientific research by extending their work into economics.

III.4. Econophysics as a Duhemian field

Econophysics has been developed by physicists who applied their methods to economic data. In so doing, they went out of their discipline and they cannot avoid facing the judgement of economists willing to protect their "disciplinary territory". Although economists acknowledge the technical knowhow of econophysicists, they are reluctant with such kinds of research simply because they consider that these works do not meet their scientific standards (as I will detail in the following section). These disagreements are rooted in a set of communal cognitive values/tools that shape the foundations of scientific justification in both communities. As explained next, these foundations are read/understood differently in the two disciplinary contexts.

[181] This claim has been confirmed by several personal conversations I had with econophysicists (Eugene Stanley, Marcel Ausloos, Tobias Preis, etc.)

The way econophysicists have applied their knowledge to economics and finance is in line with a Duhemian use of analogy. Pierre Duhem (1861–1916) was a French physicist and philosopher well known for his works on the "Newtonian" (inductive) and the "Cartesian" methods (Ariew, 2014). Although the notion of analogy is not ubiquitous in Duhem's works, he referred to this concept when he wrote about how physics as a field can evolve. More precisely, he explained that "The history of physics shows us that the search for analogies between two distinct categories of phenomena has perhaps been the surest and most fruitful method of all the procedures put in play in the constructions of physical theories" (Duhem, 1954, p. 95). The French physicist illustrated his claim with a study on the Maxwell's analogy between electrical flow and heat, where he considered analogies as a final relationship between phenomena and theoretical treatment of phenomena. Precisely, he wrote:

> "it may happen that the equations in which one of the theories is formulated is algebraically identical to the equation expressing the other […] [analogies are] intellectual economy, a method of discovery by associating two abstract systems; either one of them already known or both being formulated, they clarify each other" (Duhem, 1914 [1954], p. 96–97).

This reasoning *per analogiam* is also presented by Duhem as a way of understanding science as a human activity that develops in time and requires transgressions across the borders of the domain under investigation (Schafer, 2006); the development of econophysics seems to result from such a way of defining scientific activity. According to Duhem, scientists are not free in their choice of assumptions or models at a given time. Scientific knowledge, experience and even scientists' common sense are always somewhat related to a specific tradition. In this sense, theories of the past act as the "nuclei of the victorious theories of the future" (Schafer, 2006, p. 80). In other words, the analogical extension of knowledge is always constrained by a particular conceptual framework in which what is observed and how this thing is observed cannot be totally separated (Duhem, 1914 [1954]). Such a perspective is interesting because it offers a mode of transfer for analogies. Regarding econophysics, in particular, the previous section showed that thanks to Hesse's works, this kind of analogy (formal one) is the one that econophysicists are implementing in their modelling practices. However, the justification of this transfer of this formal analogy from physics to economics/finance requires a Duhemian analysis in order to understand what happens in the econophysicists' minds. By applying the Ising model and its statistical characterization (i.e. power law) in economics and finance, econophysicists gradually and analogically extended the epistemic domain of this well-known model to be in line with Duhemian use of analogy. What is specifically Duhemian in the formal analogies proposed by econophysicists is the way these scientists conjointly extend the analogical properties and the theoretical framework justifying these properties to economics and finance. To propose a formal analogy between economic/financial systems and the Ising model is one thing, but to simultaneously extend analogues and the theoretical framework into financial economics is a Duhemian step further. Analogies (and their consequences), like assumptions, cannot be formulated in isolation from the peculiar theoretical frame that supports them. Duhem (1914 [1954]) explained that this kind of extension does not pop up from nowhere as the result of scholars' individual arbitrariness, but rather that it results from the gradual development of a logic that belongs to a specific tradition. Regarding this aspect, Schafer (2006) wrote:

> "Reasoning by analogy has to start with previous knowledge. It has to rely on ideas that are familiar and have proved to be useful in a particular field of research. These ideas are, then, *per analogiam*, carried over in a new domain. Applying familiar ideas to new domain implies usually modifications in the inherited body of knowledge; every genuine development of science does not only add new materials to former knowledge but does single out certain sections as no longer tenable. New knowledge, if new it is, will negate some part of other if the received knowledge" (Schafer, 2006, p. 84).

This Duhemian use of analogy has some epistemological consequences, as Schafer, 2006, p. 80) explained:

> "his [Duhem] reconstruction of physics required the strict abolition of explanatory ambition […] and restriction to the descriptive function of physical theory. According to this, the only appraisal of physical theory that could claim to be rational consisted in the check of empirical adequacy which is restricted to the purely internal context of justification" (Schafer, 2006, p. 80).

From this perspective, econophysics is not perceived by econophysicists as a simple analogy, but rather as a justified new way of dealing with financial/economic systems. This situation explains why econophysicists believe that they could replace (or they are totally indifferent to) the existing economic knowledge. Such a Duhemian way of dealing with an imported analogy as a replacement[182] for existing knowledge will allow me to clarify how econophysicists bring their reasoning into economics and finance. First of all, Duhem acknowledged that a mathematical structure of a model is the core of physics—precisely, he considered that "a physical theory is a system of mathematical propositions, deduced from a small number of principles that aim to represent as simply as completely and exactly as possible a set of experimental laws" (Duhem, 1914 [1954], p. 9). In so doing, Duhem emphasized the dominance of the mathematical deductive method in physics. By combining the Duhemian use of analogy in the extension of knowledge and the importance of this deductive reasoning, we can now summarize the analogical reasoning econophysicists have in mind:

> *Statement 1: Complex phenomena are composed by a high number of interacting micro elements that generate a dynamics that can be described by a power law.*
> *Statement 2: Financial markets/economic systems are complex phenomena.*
> *Conclusion: Financial markets/economic systems exhibit power laws.*

Beyond the plausibility of the conclusion, which is often justified through the co-occurrence of power laws in physical and economic/financial systems, what is interesting in this reasoning is the association between the power law and the notion of complex phenomenon. This way of associating an observable statement with a scientific fact is quite common in science, as Feyerabend wrote:

> "As soon this method [here the association of power law with critical phenomena] is generally accepted and has been standardized, it is only a question of time until no conscious distinction is drawn between the presence of the criterion and the presence of S itself. The presence of the criterion no longer comes into consideration on its own, but one immediately says without further ado that S itself occurred: S has become directly observable" (Feyerabend, 1999, p. 19).

This way of developing physics is in accordance with Duhem's approach in which only abstract and general principles (experimental law) can guide the scholars' mind in unknown situations. The epistemological foundations of this reasoning are regularly emphasized by econophysicists (Mantegna, 1991; Stanley et al., 1996;

[182] See Mellor (1968) for further information on this aspect.

Gabaix, 2009). This "deductive import"[183] of statistical physics into economics and finance appears to be an extension of physics itself rather than an interdisciplinary attempt to develop knowledge in collaboration with another existing field. The fact that the majority of articles dealing with econophysics are published in physics journals is an indicator of the disciplinary background expected to understand this kind of research. As explained above, the theoretical foundations of econophysics refer to the Ising model and renormalization group theory, which are well known for all statistical physicists. Therefore, for econophysicists, the epistemological justification of their works is quite simple: they used a familiar theoretical framework to describe complex phenomena that all have key features required to be studied through this framework. This approach is then justified in two ways: 1) the familiarity/scientific foundations of the imported framework and, 2) the empirical adequacy of results (the co-occurrence of statistical patterns in physical and economic/financial systems). From a Duhemian perspective, this analogical extension of physics is justified for econophysicists only because the internal logics of their field are respected.

While the previous section clarified, through Hesse's work, the kind of analogy econophysicists use; this section defended that Duhem's writings offer an interesting framework for understanding how econophysicists justify their analogical extension of physics into economics/finance. I claimed here that, from a physicist's point of view, statistical econophysics can be perceived as an analogical and idealized extension of the Ising model (and renormalization group theory), which appear to be theoretically, empirically and logically justified. This perspective is not shared by financial economists, as the following part will detail.

[183] As I will detail in the following section, what is deductive for econophysicists appears to be not so logical for economists.

Part III: Modelling practices in financial economics

IV. Justification of knowledge: Financial economists' view point

As mentioned earlier, this dissertation deals only with financial economics, which is the major area of knowledge where physicists extended their works. Although the previous section detailed how econophysicists justify their research, financial economists do not perceive this extension of physics in the same way. Financial economists and physicists belong to two distinct scientific communities that are structured by a different disciplinary fabric. In this context, concepts, methods and even standards vary. For instance, I explained in the first chapter why the visual tests (used by econophysicists) confirming the existence of power laws are not considered as a sufficient test for financial economists[184]. In the light of these disciplinary considerations, one could wonder about the following questions: to what extent is the generation of economic knowledge different from the one produced by econophysicists? What kind of justification do economists use? How do financial economists perceive econophysics in light of their disciplinary standards? The rest of this chapter will deal with these questions.

IV.1. The heritage of econometrics

The Nobel memorial prize laureate, James Heckman (2000, p. 46), explained that "Economists are the people of the model". Although these words witness the importance of the notion of model in financial economics, it would be naïve to consider that this area of knowledge is based on a unique way of modelling. Financial economics is methodologically dominated by a particular mainstream (so called neo-classical financial[185] and that shape all textbooks of the field), which I plan to present here. More precisely, I will discuss how financial economists produce knowledge related to the treatment of financial data (since this is the area investigated by econophysicists). To illustrate the modelling practices in financial economics, it is important to understand the influence of econometrics on the field. In

[184] As a reminder, financial economists emerged in the 1960s from a methodological war against chartism, which promoted, at that time, visual analyses of the dynamics of financial markets.
[185] Ross (2004) explained in detail all the foundations of this neoclassical finance.

particular, econometrics defined the methodological foundations of modern finance. This section aims to provide an overview of the key elements of this influence. This analysis will give me the opportunity to show that, surprisingly, financial economists and econophysicists have a very different disciplinary fabric, so that the communities seem to face an incommensurability of perception and standards. I will detail these claims in the following sub-sections (and discuss the incommensurability aspect in the last section of this chapter).

In the 1930s, the emergence of econometrics marked the first mathematizing period of economic theory that was based on statistical measurement of economic/financial facts. In the second chapter, I explained how the scientific imaginary coming from physics and physicists themselves played an important role in the emergence of econometrics (Mirowski, 1989b; Morgan, 1990; Legall, 1994). Physicists like Ragnar Frisch, Harold Davis, Tjalling Koopmans, Henry Schultz, Trygge Haavelmo, Gerhard Tintner, Harold Hotelling, Charles Roos and Jacob Marshak contributed to the development of new techniques of dealing with economic data (Miroswki, 1989b). Their efforts allowed the rise of "econometrics", which was institutionalized under the roof of the Cowles Commission, which was founded in 1932. The commission promoted the mathematical formalism (Mirowski, 1989b, 1996; Morgan, 1990) that was supposed to reinforce the scientific method in economics. Because this Commission legitimated and defined the scope of econometrics, it became progressively an important institution that supported by other big foundations (for example, the Rockefeller Foundation, see Rutherford, 2011, p. 28 or Rockefeller Foundation archives 1903–2013). After the 1940s, the Cowles Commission became more and more statistics-orientated and its leading members (Jacob Marshak and Tjalling Koopmans) developed their famous estimation methods that were in line with the inference approach promoted by Pearson (1924; Neyman and Pearson, 1928).

Econometrics can be considered as a statistical way of making visible hidden causal relationships between economic variables (Hoover, 2013). As explained in the second chapter, the identification of these variables requires an a priori formulation of potential interactions. In other words, financial economists do not consider that data speak for themselves; a particular hypothesis must be assumed to "say something" about the data. As evoked in the first chapter, this *a priori* statement usually comes from economic theory and it is used to set up the initial conditions of the formalized systems. In this approach, hypotheses are the first step of the modelling process where the "model becomes an *a priori* hypothesis about real phenomena" (Haavelmo, 1944, p. 8). As I will explain in the following section, this *a priori* hypothesis often takes the form of a Galilean idealization (Kuoriskoski et al., 2007) that comes from economic theory. It is worth mentioning here that the idea of "*a priori* knowledge" used by financial economists does not refer to the same concepts in the philosophy of science. Precisely, economists consider that *a priori* knowledge is an idealized assumption that is based on a theoretical framework, whereas philosophers instead associate this notion with a statement that one can have without any input from the world[186]. In other words, because econometrics analysis requires a particular assumption to be tested, this way of modelling consists of applying "statistical methods to economic data to *test economic theories*," (Sowey, 1983, p. 257—my italics). This reference to a priori economic theory is often explicitly mentioned in the definition of the field, as the following example shows:

> "Econometrics as it is taught in textbooks—and even as it is sometimes practiced—focuses on the second use of statistical tests as if we had *a priori knowledge* of the structure of the model to be estimated" (Hoover, 2013, p. 53—my italics).

In this methodological context, one can notice and wonder with Hoover (2013):

> "That knowledge is not in the observable data. How do we know it? The standard answer to this question among economists—going back at least to Haavelmo's seminal 'Probability approach in econometrics' (1944)—is that it is a priori knowledge based in economic theory. But how did we come to have such knowledge? Indeed, this question is hardly ever addressed […] We need to have a model with known properties that maps well onto properties of the world. Is there a systematic method for obtaining such knowledge? The answer must be, no, if econometrics, as it is presented in many (perhaps most) textbooks, is limited to the problem of statistical estimation of the parameters of structures assumed to be known in advance" (Hoover, 2013, p. 49).

The justification of such an *a priori* method is justified by the fact that economics deals with variables that are not observable by themselves. Indeed, variables used

[186] To my knowledge, only the Austrian school of economic thought refers to such way of defining a priori knowledge in economics.

by economists result from interactions among these unobservables and, “without further information it is, in general, not possible to infer the behaviour of the unobservables from the observables” (Hoover, 1994, p. 68).

Since econophysicists mainly deal with the dynamics of financial markets, I will focus my attention here on the *a priori* knowledge used by financial economists in their way of modelling this dynamic. Roughly speaking, this knowledge is composed by the combination of two conceptual frameworks: one dealing with the behaviour of micro entities (agents are considered as perfectly rational) and another one related to the macro behaviours of these elements (macro dynamics of the system is assumed to follow a Gaussian distribution).

The statement according to which economic actors are perfectly rational is today well known and it dates back to the early time when economics tried to change itself into a more scientific area by mathematizing its foundations. From an abstract value-orientated theory, economic methodology evolved towards a more mathematized formulation in which agents maximize their utility function. This idea is quite simple: an economic actor is assumed to act rationally because she/he maximizes her/his utility logically consistent with a given set of conditions (i.e. set of goods and their prices). Since real economic agents are not perfectly rational as defined by economists, such representation can be presented as an idealization. Financial economists acknowledge this aspect as mentioned by Hahn and Hollis (1979):

> “The rational man of pure theory is an ideal type in the sense not only of being an idealization where the theory holds without qualification but also of being a model to copy, a guide to action. In pointing the way to satisfy a given set of ordered preferences, the theorist gives reasons for action” (Hahn and Hollis, 1979, p. 1,979).

The perfect rationality frame appears as a mathematical idealization that has been “designed for interpreting observed consequences of men’s actions” and not for interpreting the actions themselves (Machlup, 1978, p. 281). This use of mathematics as an idealization echoes what philosophers call a *Galilean idealization,* which is associated with “the practice of introducing distortion into models with the goal of simplifying, in order to make them more mathematically or computationally tractable" (Wiesberg, 2016, p. 99). The example of the pendulum is a good illustration of such in which modellers subtract all elements that are not related to the studied phenomenon in order to focus on "a mathematical formalism in the hope that essentials of that situation (from the point of view of the science one is pursuing) will lend themselves to mathematical representations" (McMullin, 1985 cited in Morgan, 2012, p. 148). Galilean idealization is mainly justified pragmatically to provide a simplification in order to learn something from a complex situation.

The use of Galilean idealization as a starting point for an explanation is often emphasized in the literature. Batterman (2008) and Weisberg (2016) promoted the use of such idealization as a first modelling step in the identification of minimal parameters that characterize a particular phenomenon. In economics, Hausman (1994) and Kuorikoski et al. (2007) explained that Galilean idealizations played a key role in the foundational knowledge (perfect rationality, perfect competition, etc.) while Morgan (2015) justifies such idealization through the Ceteris Paribus clause according to which "all other things [than components of the idealization] remain unchanged". For Cartwright (1999), the Galilean idealization would be a price worth paying for financial economists to have a kind of robustness in their models. In this context, financial economists focus on general tendencies as an element of the idealization, allowing them to develop an internal validity in their field (I will come back to this aspect shortly). Maki (2012) discussed the Galilean idealization in economics through the lens of what he called "the method of isolation" (Maki, 2012, p. 218) by explaining the methodological reasons for why economic modellers have to start with isolations [187]. In this context, one can wonder how a partial representation can explain a phenomenon. This question refers to two aspects: the modeller's epistemological objectives and the ability of an idealization to be improved or de-idealized. Because it covers the first step of the modelling practices, the use of Galilean idealization is also related to the epistemological aims of the modellers. On this point, Cartwright (1999) and Kuorikosli et al. (2007) explained that idealizations provide financial economists with the perfect conceptual tools to build a Babylonian (Euclidian) field (Kuorikosli et al., 2007) based on "an internal

[187] Maki (2012) explicitly associated the concept of isolation with a Galilean idealization (see Maki, 2012, p. 222).

justification" (Cartwright, 1999). In a sense, the Galilean idealization would be an appropriate starting point in modelling to guarantee the coherence of the reasoning in accordance with an existing theory.

The idea of de-idealization refers to a process of removing distortion and adding back details to the representation (Weisberg, 2016). In so doing, models can be made more specific by eliminating simplifying assumptions and de-idealization. From this perspective, the process of de-idealization can become a basis for a continuing research programme (McMullin, 1985). The next section will detail further what the major Galilean idealizations are that are used by financial economists and how de-idealization makes sense for these scientists.

IV.2. Galilean idealization in financial economics

The frame based on the perfect rationality is at the core of economics and finance and many economists (and many textbooks) still use this assumption in their modelling tasks. This idealized vision is still strong and it has been presented as a pillar of financial economics (Arrow, 1986). The concept of perfect rationality has its logical roots in the well known expected utility theory developed by Von Neumann and Morgenstern (1944). Specifically, these two authors defined a list of axioms (comparability, transitivity, continuity, independence, interchangeability and risk aversion[188]) that allow them to characterize the decision-making process of a rational economic agent. As Frankfurter (2007, p. 9) detailed, "financial economics

[188] Comparability: the agent prefers A to B, B to A or is indifferent to A and B.
Transitivity: if A is preferred to B and B is preferred to C then A is preferred to C.
Continuity: if A is preferred to B and B to C, then there is a probability P that the agent would be indifferent to the certain outcome of B and an uncertain outcome.
Independence: if the agent is indifferent to the certain outcomes of A and B; and C is any other certain outcome then he/she is also indifferent to the uncertain outcomes.
Interchangeability: "if the agent is indifferent to two uncorrelated risky incomes then the sequences that produce them are interchangeable in any investment strategies" (Frankfurter, 2007, p.9).
Risk aversion: if A and B produce the same outcome but that there is a probability $P_a > P_b$ of having this outcome then the agent prefers A.

adopted these axioms without much questions or criticisms from the early start of Markowitz (1952) and thereafter". Indeed, while these axioms allowed Markowitz (1952) to explicitly refer to Von Neumann and Morgenstern's theory to set up the theoretical foundations of the emerging financial economics with his portfolio theory (Jovanovic and Schinckus, 2013), the other key models of the field (Capital Asset Pricing Model; Black-Scholes model, Efficient Market Model, etc.) also assume that all investors are perfectly rational. In so doing, financial economists implicitly agreed on what the relevant assumptions are to pose to characterize the agents' behaviours. This perfect rationality has been presented in the literature as a Galilean idealization (Morgan, 2015), which some economists tried to de-idealize by integrating more realistic assumptions while preserving the initial (core) framework. The emergence of behavioural finance in the 1980s, for instance, can be presented as an attempt at de-idealizing the perfect rationality. The seminal work of this field is called the "prospect theory" developed by Kahnemann and Tversky (1978) who showed that perfect rationality framework is a specific case of a more generalized formulation of human rationality, integrating some psychological biases such as over/under-estimation, aversion for losses (instead of risk), etc.[189].

The second major component of the *a priori* knowledge assumed by financial economists refers to the way of dealing with the macro dynamics of the financial markets where the evolution of prices is assumed to follow a Gaussian distribution. Although there is a huge body of literature that deals with the technical way of using the Gaussian framework in financial economics (see Jovanovic and Schinckus, 2017 for a good review), very few works focus on the epistemological aspects of this *a priori* knowledge. The first two writers to use tools that came out of modern probability theory to study financial markets were Harry Markowitz and A. D. Roy in 1952. Both published an article about portfolio theory by formalizing an old saying advising investors: "Don't put all your eggs in one basket". Precisely, these two authors showed that this adage can mathematically be described by the fact that the expected value of a weighted sum is the weighted sum of the expected values, while

[189] For further information about the development of behavioural finance, see Schinckus (2009).

the variance of a weighted sum is not the weighted sum of the variances (because we have to take covariance into account). In financial terms, that means that diversification of investments can statistically reduce the variance (risk) of the portfolio. These two seminal papers (Markowitz, 1952; Roy, 1952) assumed that investors were perfectly rational and they used the Gaussian distribution to describe the evolution of financial markets. Markowitz (1952) acknowledged that this statistical frame was not the most appropriate for characterizing the evolution of financial prices, but he considered the Gaussian distribution to be the most convenient. He decided to adopt it for pragmatic reasons, as he explained in his book that he published some years later, "Some [other] measures which seem reasonable offhand produce completely unsatisfactory portfolio solutions […] The standard deviation [Gaussian distribution] is easier to use, more familiar to many and perhaps easier to interpret" (Markowitz, 1959, p. 77). This argument is still very common in financial textbooks where Gaussian distribution is often presented as an idealized approximation of the dynamics of financial prices, as illustrated by the words of Fama (1976):

> "Although the evidence also suggests that distributions of monthly returns are slightly leptokurtic relative to normal distributions, let us tentatively accept the normal model as a working approximation for monthly returns […]. If the model does well on this score [on how well it describes observed relationships between average returns and risk], we can live with the small observed departures from normality in monthly returns, at least until better models come along" (Fama, 1976b, p. 38).

The major advantage of this mathematical idealization refers to the fact that the Gaussian distribution is the only class of distributions for that the mean and the standard deviation are sufficient to describe the whole dynamics. Such a situation led some critical authors like Nicolas Taleb[190] or George Soros[191] to use provocative formulation to describe the current importance of the Gaussian distribution in

[190] Nicolas Taleb (born in 1960) is an American (originally from Lebanon) academic and essayist working on probability and uncertainty in finance. A statistician and former trader, Taleb is well-known for his book "Black Swan", which criticized the financial mainstream for its inability to characterize rare events or fluctuations occurring on the financial markets.

[191] George Soros (born in 1930) is an American (originally from Hungary) investor and business magnate. He is recognised worldwide as a philanthropist and essayist. His books (Soros, 1987, 2006, 2008) mainly deal with his personal experience of financial markets and his critical opinions about financial theory.

finance. Taleb (2006), for instance, wrote that “you need nothing else. The bell curve satisfies the reductionism of the deluded” (Taleb, 2006, p. 242). Such a provocative statement illustrates the fiery debates generated by the Gaussian distribution. There is another aspect that can be mentioned regarding the influence of the terminology used to characterize the Gaussian distribution. Although the mathematician Carl Frederich Gauss (1777–1855) worked on the mathematical nature of this distribution he did not extent it to characterize processes in reality. This extension is mainly due to Adolphe Quételet (1796–1894) who popularized the Gaussian distribution by using this framework to describe human features (weight, height, etc.). Quételet came up with the notion of “average human” whose association with “normal human” coined the usual synonymy between “Gaussian law” and “normal law” (Fendler and Muzzafar, 2008). This association had an important influence on the terminology used to present the statistical characteristics of the Gaussian distribution. This is a debatable aspect of this distribution: all deviations from the average (that refers to the “normality”) have been called “errors”. Gradually, “divergence from the mean was treated precisely as an error” (Taleb, 2006, p. 245). This is exactly what one can observe in finance textbooks where financial prices are assumed to follow a normal law whose larger variations are presented as “errors” or “anomalies”. Frankfurter and McGoun (1999) explained how this terminology tended to marginalize the works dealing with large statistical deviations that are associated with the literature of “anomalies” in finance. As mentioned in the first chapter, there is an important body of literature devoted to the treatment of extreme values[192] —actually, this literature even become an important part of finance where, although scholars acknowledge that the Gaussian distribution is not the best description of the evolution of prices, they keep it as a first idealization that they de-idealize through the development of a conditional analysis. Precisely, in the first chapter, I explained that the statistical description of financial prices can roughly be characterized by the following relationship that characterizes the evolution of a particular variable:

$$\mathrm{X}_t = \mathrm{N}\left(\mu + \sigma_t^2\right) + \varepsilon_t \qquad (10)$$

[192] See Jovanovic and Schinckus, 2017 for an extended review of literature of these works.

Where μ is the mean, σ² is the variance and $\mathcal{E}$ is the statistical error. This statistical equation can also be expressed as:

$$\mathrm{X}_t = \mu t + \bar{\sigma}_t^2 + \varepsilon_t \tag{11}$$

I discussed these elements earlier in this dissertation. What is interesting for the purpose of this chapter is the decomposition of the analysis into two elements: an unconditional (Gaussian) ingredient $\mathrm{N}(\mu + \sigma_t^2)$ and a conditional ingredient (ε_t). While the first refers to the idealized part of the explanation (Gaussian description), the conditional component refers to the de-idealization of the explanation, since this second part has been added to deal with empirical data that cannot be captured by the normal distribution.

This will to keep the Gaussian framework in the financial mainstream is mainly due to the fact that all key models[193] were developed in a Gaussian world (Bernstein, 1994). In other words, since the first works in modern finance in the 1960s, Gaussian distribution has been considered to be the law ruling any random phenomena. Indeed, the authors based their stochastic models on results deduced from the central-limit theorem, which led to the systematic use of Gaussian distribution. From this perspective, the major objective of these developments was to “reveal” the Gaussian distribution in the data or at least to show that we can use this distribution to describe the evolution of financial prices. When observations did not fit with the normal distribution or showed extreme values, authors commonly used a log-linear transformation to obtain the normal distribution[194]. This situation also opened a door for several potential improvements that consist of capturing extreme variations with an additional (and not necessary Gaussian) distribution—this is the idea of the combination exposed above. The conditional distribution characterizing the deviances is a “corrective tool” that can be perceived as a de-idealization of the main (Gaussian) trend. The former must be kept and assumed for theoretical reasons, as Fama (1976) explained clearly:

> “although most of the models of the theory of finance can be developed from the assumption of stable non-normal return distributions […] the cost of

[193] Portfolio Theory, Capital Asset Pricing Model, Black-Scholes model, Arbitrage Pricing Model, etc.
[194] However, in the 1960s, prices were recorded monthly or daily, implying a dilution of price volatility.

> rejecting normality for securities returns in favour of stable non-normal distributions are substantial" (Fama, 1976b, p. 26).

Beyond the historical importance of the Gaussian distribution[195], some authors (McGoun and Frankfurter, 1999) have also stressed the ideological dimension of a Gaussian random. The economic mainstream is well known to be opposed to all kinds of interventionism and, in this context, a normal law would be the best statistical justification for the fairness of financial markets simply because it implies that actors have the same probability of losing or wining money. Therefore, all forms of intervention would make the markets inefficient.

In this part, I identified the major rules for the production of models in financial economics where Galilean idealizations play a key role in the starting representations of the behaviours of agents and markets. Methodologically speaking, the financial mainstream is therefore based on an *a priori* statement (Galilean idealization) that is statistically implemented in financial/economic data. The question is now to see how this knowledge can be justified. This will be the topic of the next sub-section.

IV.3. The importance of the statistical significance

This section will investigate the standards by which financial economists justify their modelling practices. The objective of the econometric method largely used by financial economists is to test statistically admissible models that are used to evaluate economic theory (Hendry, 1980). In this sense, the process of validation is often presented as a statistical problem. The relationship between economic theory and economic data is formalized through a probabilistic framework in which econometrics models provide a particular explanation in terms of measurement of errors (Morgan, 1990). In other words, these models specify the probabilistic conditions under which the (*a priori*) theory is expected to hold. Such approach is

[195] See Berstein (1992) or Jovanovic (2002) for further information about this historical importance of the Gaussian framework.

justified by the necessity of measuring the gap between theory and facts. In so doing, financial economists became increasingly obsessed with statistical tests in order to ensure the significance (validity) of their works. To illustrate this claim, Hendry (1980, p. 390) wrote that "Economists have found their Philosophers' stone: transforming data into significant results" before adding:

> "The three golden rules of econometrics are test, test and test; that all three rules are broken regularly in empirical applications is fortunately easily remedied [by corrective methodology presented in the previous section]. Rigorously tested models, which adequately described the available data, encompassed previous findings and were derived from well based theories would greatly enhance any claim to be scientific" (Hendry, 1980, p. 403).

Economists use and abuse[196] statistical tests to give a particular legitimacy to their fields. But what kind of legitimacy are we talking about? For the economic mainstream, the significance of statistical tests is a required methodological condition to be accepted as a justified explanation. In this context, financial economists implement *a priori* knowledge to explain the evolution of financial prices, justifying this methodology through a panoply of statistical tests aiming to provide a quantitative validation of this *a priori* knowledge. The idea is therefore to determine whether there is enough evidence to reject or accept this initial knowledge. When statistical tests are significant, that means that modeller has good statistical reasons to "believe" in assumptions suggested in the modelled process. However, as Clauset et al. (2009, p. 19) mentioned, "statistical tests can be used to rule out specific hypotheses, but it is up to the researcher to decide what a reasonable hypothesis is in the first place". In other words, these tests provide information about the way of dealing with knowledge but not about the knowledge itself. If statistical tests estimate the degree of belief we can place on the theory, the latter cannot be used as a benchmark to define the significance of the former. However, "to interpret an estimate of a parameter, we must have a model in which the parameter is meaningful" (Hoover, 2013, p. 52). This specific part of modelling refers to what

[196] This reference to a potential abuse of statistical tests echoes a reflexive paper published in 1995 by Keuzenkamp and Magnus (1995) in which the authors wrote that in some articles, statistical tests seem to be the major purpose of the economic research. More precisely, they explained that "Sometimes one wonders about the abundance of tests reported in empirical papers, as the purpose of many of these tests is not always communicated to the reader. Occasionally, the number of test statistics reported in a paper exceeds the number of observations used in calculating them! In many cases, the implications, of a positive or negative result are not made clear" (Keuzenkamp and Magnus, 1995, p. 6).

economists call the “data generation process—DGP” (Keuzenkamp, 2000, p. 5) which defines the set of statistical conditions under which empirical data can be studied. Statistically speaking, the Gaussian distribution can be perceived as a particular set of conditions that define the theoretical benchmark for interpreting statistical tests. It is worth mentioning here that, like econophysicists “who see power laws everywhere”, economists also expect to see Gaussian distribution in every sample of data. This particular situation has been discussed by Keuzenkamp (2000, p. 6), who wrote that, “although the DGP is sometimes presented as hypothetical, there is a tendency to view the DGP as fact or reality […] The DGP is reality and model of reality at the same time”. In this context, all statistical procedures, such as correlation, regression, t-tests and analysis of variance, etc., which are usually called parametric tests, are based on the assumption that data follow a normal law (Ghasein and Zahediasl, 2012). A diversity of statistical tests exists to estimate the significance of empirical data: Kolmogorov-Smirnov, Lilliefors test, chi square test, t-test, shapiro-test, Jarque-Bera test, etc.—all of them developed to capture technical aspects (variation, skewness, asymmetry, etc.) of a process described through a Gaussian lens. These tests share the hypothesis of normality because the vast majority of statistical tests have been based on the properties of the central-limit theorem. Financial economists, particularly Fama and Mandelbrot, discussed this issue and its consequences in the 1960s[197] by acknowledging the lack of statistical tools:

> “... there are admittedly difficult problems involved in applying [portfolio models with non Gaussian distributions] to practical situations. Most of these difficulties are due to the fact that economic models involving stable Paretian [non-Gaussian distribution] generating processes have developed more rapidly than the statistical theory of stable Paretian distributions [non Gaussian distributions]” (Fama, 1965b, p. 418).

Consequently, as explained in the previous section, the community of financial economists decided to keep the Gaussian framework as an idealization (the best approximation) of the financial dynamics. This embeddedness of statistical tests in the Gaussian framework is very important and even “critical [because] when this assumption does not hold, it is impossible to draw accurate and reliable conclusion[s] about reality” (Ghasein and Zahediasl, 2012, p. 486). This situation is

[197] I mentioned this point in the first chapter of this dissertation.

problematic for the elaboration of a dialogue between financial economics and econophysics simply because the vast majority of statistical tools have been developed in the Gaussian framework, which is not suitable for testing power laws. Satisfactory statistical tools and methods for testing power laws simply do not yet exist (Jovanovic and Schinckus, 2017). This is a big challenge, and one that very few authors have been working on. Moreover, from a financial/economics perspective, there are several obstacles (like infinite variance, for instance) to the development of statistical tests dedicated to power laws[198].

As Chapter 1 explained, the existence of a power law is commonly tested by econophysicists through a visual inspection (see Figures 1, 2 and 3 in Chapter 2): the authors plot the data in a double logarithmic scale and attempt to fit a line to part of it. This procedure dates back to Pareto's work at the end of the 19th century. Unfortunately, this method generates significant systematic errors by wrongly attributing power law behaviours to phenomena[199] (Clauset, Shalizi and Newman, 2009; Stumpf and Porter, 2012; Gillespie, 2014). On this point, the conclusion of the article written by Clauset, Shalizi and Newman (2009) suggested that the identification of a power law might result from what modellers want to see:

> "The study of power laws spans many disciplines, including physics, biology, engineering, computer science, the earth sciences, economics, political science, sociology, and statistics. Unfortunately, well-founded [mainly visual] methods for analyzing power-law data have not yet taken root in all, or even most, of these areas and in many cases hypothesized distributions are not tested rigorously against the data. This leaves open the possibility that conjectured power-law behavior is, in some cases at least, the result of wishful thinking" (Clauset, Shalizi and Newman, 2009, p. 700).

From a financial/economics viewpoint, such visual tests have two major drawbacks. Firstly, they provide no objective criteria for determining what a "good fit" is. Secondly, as already explained in this section, financial economists consider only statistical tests as scientific. Therefore, empirical investigations from the literature of econophysics tend to be regarded with suspicion by financial economists who implicitly promote a naïve Popperian justification of their work, as I will detail in the following sub-section.

[198] I will come back on this point in the conclusion of this dissertation.

[199] The visual tests make it difficult to distinguish between power law, log-normal and exponential distributions. See Clauset et al. (2009) for further details on this point.

IV.4. Financial economics as a naïve Popperian field

"An important reason for the popularity of testing is that it is often thought to be a major if not the main ingredient to scientific progress and the best way to move from alchemy to science" (Keuzenkamp and Magnus, 1995, p. 7). This situation results from the influence of Karl Popper (1902–1994) on scientific practices: scholars cannot prove theories but they must be able to falsify them. In a Popperian perspective, there is no way to confirm a theory or an assumption: "even observation statements, Popper maintains, are fallible and science in his view is not a quest for certain knowledge but an evolutionary process in which hypotheses or conjectures are imaginatively proposed and tested in order to explain facts or to solve problems" (Thornton, 2016, p. 7). Popper emphasized the importance of the severity of tests to which conjectures have to be subjected. A test will never confirm a theory or an assumption but it will provide a temporary reason to proceed in a particular research direction. Such a way of thinking about knowledge associates scientific progress with a regular evolution (improvement) of the standards by which we measure the achievements of past accepted theories and conjectures (Robinson, 1971). This ability to test theories/hypotheses is often presented, in social sciences, as a naïve criterion of scientificity. While he was dealing with this aspect, the famous economist Fisher (1973) wrote that "statistical methods are essential to social studies, and it is principally by the aid of such methods that these studies may be raised to the rank of sciences" (Fisher, 1973, p. 2). An important body of literature exists that deals with the Popperian dimension of economics and finance. It is beyond the scope of this dissertation to give an overview of these debates between scholars (Hendry, 1980; Blaug, 1992) who promote the Popperian character of (financial) economics and those (Summers, 1991; Caldwell, 1992, Keuzenkamp and Magnus, 1995) who criticize this aspect.

In this section, I focus on the fact that statistical tests are mainly used as a naïve Popperian justification in financial economics. Regarding this point, it is noticeable that Popper is the only philosopher of science quoted in the mainstream journals such as *Econometrica* or the *Journal of Finance*. In line with Popper's opinion according to which "we never argue from facts to theories" (Popper, 1974, p. 68), financial economists start their modelling with *a priori* statements that must be tested. In their modelling practices, this starting frame provides the conditions that articulate a well-defined set-up; acting therefore as a "nomological machine" (Cartwright, 1989). The role of this a priori statement is to provide the identification required to render data economically interpretable. As detailed in the previous section, the two conceptual ingredients of this *a priori* framework are the perfect rationality of agents and the Gaussian characterization of the evolution of financial prices. These two idealizations define the relevant aspects by specifying *what can be said* about the dynamics of financial markets: the perfect rationality of actors ensuring the mechanistic decisions of agents allows researchers to focus on their behaviours' outcome (so on the prices themselves), whereas the Gaussian framework defines the statistical meaning (and ideological dimension[200]) of the modelling practices. In this context, the way of producing and justifying financial knowledge can roughly be summarized by the following process:

$$P_1 \longrightarrow TT \longrightarrow T \longrightarrow EE \ (\longrightarrow P_2)$$

> "All scientific discussions start with a problem (P1) to which we offer some sort of tentative solution—a tentative theory (TT); this theory is then criticized [and tested T], in an attempt at error elimination (EE); and as in the case of dialectic, this process renews itself (P_2): the theory and its critical revision give rise to new problems" (Popper, 1974, p. 105).

In a financial context, P_1 refers to the evolution of financial prices; TT denotes the combination of perfect rationality of agents and the Gaussian framework; T summarizes the existence of statistical tests implemented in the process, while the EE refers to the development of corrective methodology (which I presented as a de-idealization in the previous section) such as ARCH-type models. Eventually, these new models also generate specific empirical contradictions that raise new forms of models (GARCH), and so forth. Although the Popperian rhetoric is largely used in the mainstream finance, there is a major difference between what financial economists do and what Popper proposed they do. Actually economists do not read

[200] As mentioned in the previous section, Gaussian distribution is often used by financial economists to justify the fairness of markets since actors have the same probability of losing or winning money. For further information on this aspect, see Frankfurter and Mcgoun (1999).

the process evoked above in the same way that Popper would. For the Austrian philosopher, the error elimination (EE) and the evolutionary aspect of the process (P_2) will generate new standards, and these two steps are required in the progress of scientific knowledge. In contrast, financial economists don't really consider these aspects, but rather they focus on the first step of the process (T) regarding the possibility of testing their assumptions. In other words, the existence of tests is perceived as the key point of the process. However, if the existence of a testing methodology can be perceived as a necessary condition to be labelled "Popperian", it is not a sufficient one. In accordance with Popper, such testing methodology should evolve by integrating more severe scientific standards. When financial economists test their assumptions, they mainly look for the statistical significance as a confirmation of their works. This perspective is not really Popperian, since this philosopher rejected the idea that we can confirm a theory or an assumption.

Although several authors (Caldwell, 1992; Keuzenkamp and Magnus, 1995) emphasized the naïve falsificationism usually promoted by financial economists, this way of developing (and justifying) knowledge is very common in mainstream economic/financial journals where, "most economists have heard of Popper's theory of falsification, although they have no personal familiarity with his works, and most do believe that economic theories can be falsified in the sense of being refuted" (Redman, 1994, p. 76). Popper, himself, contributed to the development of such naïve falsificationism when he wrote "it must be admitted, however, that the success of mathematical economics shows that one social science at least has gone through its Newtonian revolution" (Popper, 1957, p. 60). The usual critiques (Redman, 1994; Keusenkamp and Magnus, 1995; Keuzenkamp, 2000) against this naïve falsificationism considered the panoply of ARCH and GARCH models[201] as an ad-hoc solution for preserving the Gaussian framework despite all empirical contradictions observed in data.

Beyond these well-documented critiques of the statistical adjustments, there are two other aspects that question the Popperian dimension of financial economics. The

[201] As a reminder, these models are corrective methods for capturing the extreme fluctuations that occur in the Gaussian description of financial markets.

first point refers to the circularity of the statistical reasoning: economists seek to test a Gaussian assumption by using statistical tests that take their meaning only in a Gaussian framework, thereby reducing the significance and the interpretability of measurements. In so doing, economists seem to avoid non-Gaussian distribution by keeping a question opened: what about a non-Gaussian situation? In addition to that, there are some technical limitations in using such statistical tests: Leamer (1983) shows that the significance level of statistical tests varies with the sample size, but this aspect has been largely ignored in practices where a fixed significance level is used (Keuzenkamp and Magnus, 1995). Moreover, some authors (Glymour, 1985; McCloskey, 1994) have raised questions about the informative nature of the statistical tests that economists use. As Glymour (1985) mentioned, “Statistical tests don’t inform us as to whether or not a model is approximately true. They don’t permit us to compare false models to determine which is closer to the truth” (Glymour, 1985, p. 78). The second point that tarnishes the Popperian dimension of financial economics refers to the fact that by seeing Gaussian distribution everywhere, economists seem to be confused between trends and laws. However, Popper distinguished between these two notions: “trends exist, or more precisely, the assumption of trends is a useful statistical device. But trends are not laws. A statement asserting the existence of a trend is existential, not universal” (Popper, 1960, p. 15). In line with this quotation, financial economists use the Gaussian distribution as a first assumption, but they transformed it into a law through the development of corrective ad-hoc statistical methods (ARCH-type models).

Financial economists start their modelling practices with *a priori* statements that they know are unrealistic but that they justify through statistical significance. In so doing they don’t really discover something, but instead they propose a particular explanatory fiction that justified through a naïve Popperian epistemology based on a (statistical) verification of the *a priori* representation. However, I explained in this section that the use of statistical tests is not a sufficient condition to be in line with what Popper promoted. In this context, financial economics can be associated with a naïve falsificationism that contrasts with the Duhemian perspective used by econophysicists.

One might wonder why it is that econophysicists are not promoting naïve falsificationism by visually testing the existence of power laws in financial/economic systems. In this context, the Popperian characterization of such an idea could take the following form:

$$P_1 \longrightarrow TT \longrightarrow T \longrightarrow EE\ (\longrightarrow P_2)$$

where P would refer to the evolution of financial prices, TT would denote the expectation of having a power law and T would represent the visual test used by econophysicists in their works. This characterization is not convincing for two reasons. First, the expectation of having a power law does not refer to a theoretical explanation (contrasting with what economists do) of the data—this expectation is hardly a phenomenological description of these data). Furthermore this expectation of having a power law results from another assumption[202], which makes sense only because physicists find the extension of their knowledge to finance meaningful. In other words, the Duhemian analogy, as exposed above, is a necessary condition for having such an expectation. The second reason refers to the absence of a real testing methodology in econophysics, where the empirical justification for the existence of power laws is based on mere observation[203].

As detailed in the previous section, financial economists and econophysicists have two different epistemologies for justifying their works: while the former refer to a Popperian rhetoric to establish the legitimacy of their research, the latter rather found their works on a Duhemian way of using analogies. In this context of epistemological dissimilarities, which generate an incommensurability of scientific standards between the two communities, it is worth investigating further the reasons for this lack of dialogue between econophysicists and financial economists. This is the purpose of the next part.

[202] Systems composed of a high-number of interacting components are complex and can be described through the self-criticality framework.
[203] There are still no statistical tests to test the existence of power laws—I will come back on this detail in the conclusion of this dissertation.

V. Conclusion

This chapter explained why financial economists and econophysicists do not interact: they have different ways of doing science, implying different disciplinary standards. From their perspective, economists have good epistemological reasons for doing it. On the one hand, given their way of thinking about what is empirically adequate, economists have their reasons for rejecting econophysics, which appears to them as an inductive export of physics that presents no potential links with the existing knowledge and standards used in economics. Economists developed modelling practices based on Galilean idealization that was implemented through a naïve falsificationism in which statistical tests define the extent to which a model fits to the real world. A telling example of the financial economists' position can be illustrated by the work of the economist Blake LeBaron (2001), who showed how a number of simple stochastic volatility models (i.e. models describing the occurrence of large fluctuations on financial markets) can visually produce power laws and long memory effects similar to those that have been reported in econophysics literature. LeBaron did not call to reject econophysics' results; on the contrary: "It does not say that power-law results are wrong. It is only that they should be viewed as less conclusive than they often are, since there may be many explanations beyond those related to critical phenomena" (2001, p. 629). LeBaron added that "The search for reliable scaling laws in economics and finance should continue […]. The visual indication of a straight line going through some points should not be taken on its own as a 'test for complexity', or critical behavior […]. It would be best not to abandon these concepts, but to improve statistical understanding of both the empirical tests and the theoretical models under consideration" (2001, p. 630).

On the other hand, econophysicists implement their scientific methods and standards based on a minimalist idealization, which they extend formally and analogically to another area of knowledge by adopting a Duhemian epistemology. In so doing, they deal with economic/financial data with a set of explanatory demands that is quite standard in statistical physics and, therefore, they don't understand the rejection by economists. This feeling clearly emerged from a survey realized by

Jovanovic and Schinckus (2013) showing that the major actors of econophysics have tried to publish their works in the mainstream economics journals, but with little success. Jovanovic and Schinckus (2010) sent a questionnaire to 27 leading econophysicists (identified through a bibliometric analysis) about the degree of closure of economic journals to econophysicists. To the question "have you submitted a paper to a ranked journal in economics", a large majority of econophysicists replied "yes". However, very few econophysics papers are now published in economic journals. Thus, when econophysicists were asked to give the main reasons for the rejection of their paper, they replied that referees in economic journals often have difficulties with the topic or/and the method used in their paper[204]. Although based on a small sample, these results emphasized the incomprehension of econophysicists of the reasons for why economic journals are reluctant to publish their papers. To conclude this chapter, I propose the following table, which summarizes the major epistemological differences between econophysicists and financial economists in terms of modelling practices:

	Econophysics	Financial economics
Justification	Duhemian analogy	Popperian rhetoric
Idealization	Minimalist	Galilean
Argumentation	Asymptotic	Additional
Validation	Visual tests	Statistical tests
***A priori* knowledge**	Complexity/power law	Gaussian distribution

Table 2: Comparision between modelling practices in econophysics and financial economics.

The epistemological differences summarized above and detailed in this chapter are essential for clearly understanding why there is no current conceptual bridge between economics and econophysics. These two fields have currently different "construals" (i.e. way in which people perceive and interpret the world), making all kinds of interaction hardly possible[205]. Wiesberg (2016) explained that in science,

[204] In the survey, econophysicists had to choose between five reasons for having been rejected and were invited to comment on their choices: 1) the topic of the paper; 2) the assumptions used in the paper; 3) the method used in the paper; 4) the results of the paper; or 5) another reason.

[205] I am not claiming that there are no joint works between economists and econophysicists—I am myself involved in such projects—I am just mentioning that such existing works are not very common in the literature, which is still quite hermetic to interdisciplinary attempts. I will come back to this dimension in the following section.

“construals provide an interpretation for the model’s structure, they set up relations of denotation between the model and real-world targets, and they give criteria for evaluating the goodness of fit between a model and a target” (Wiesberg, 2016, p. 39). For instance, a technical test confirming the statistical significance of a power law would be seen as a construal for financial economists, who will take into consideration the existence of such law as a fact only when this fact can be viewed through the usual lens they use to interpret the world. In the same vein, a linear relationship on a log-log graph will evoke the existence of a power law for econophysicists whose construals are more based on a visual analysis (as mentioned earlier in this work).

Construals are very important because they define the implicit rules of interpretation that a community shares about models. Although econophysicists and economists agree on the existence of large fluctuations in the evolution of financial prices, they disagree on what is empirically adequate. This dissimilarity is due to a different way of thinking about the link between the model and the reality.

I used the word construals to define this way of connecting the model with the reality. To some extent, this notion of construal echoes the basic background shared by all members of a scientific community. Thomas Kuhn (1962) emphasized the importance of this tacit knowledge and it is rooted in what he called “examplars”, which refer to “the concrete problem-solutions that students encounter from the start of their scientific education” (Kuhn, 1962, p. 107). Exemplars characterize these logical frameworks that scientists who belong to a specific community accept without question simply because these frames are part of their culture, and therefore part of their tacit shared knowledge. As explained previously in this chapter, financial economists and econophysicists have different examplars, making them use different symbolic generalizations. This situation illustrates the fact that scientific knowledge is embedded in theory and rules (detailed by Thomas Kuhn in the postscript of the second edition of his book). Specifically, Kuhn explained that the exemplars contribute to the crystallization of disciplinary intuitions shared by members of the same scientific community, which helps them to recognize a given situation as like other that has seen before. Scientists belonging to the same groups thus share education, experience and a language, leading them to perceive the world in the same way. However, due to their different disciplinary backgrounds, financial economists and econophysicists do not see the same things in the same empirical data. Regarding this point, Kuhn (1962) explained that two scientific communities can have different sensations and react in different ways to the same stimuli, implying that, to some extent, these two groups live in different worlds. Such dissimilarities can generate situations in which scientists respond to the same empirical observation with incompatible descriptions and generalizations (Kuhn, 1962, p. 201). These circumstances echo two important issues: scientists' choice of theory and the incommensurability of descriptions.

The implicit knowledge (examplars, language, etc.) shared by scientists belonging to the same community directly influences the way they perceive the world. In other words, scientists have good reasons, from their perspective, to consider stimuli as a familiar situation that they can describe through the theoretical framework they know (and share). On this aspect, Kuhn (1962, p. 199) wrote that scientists have no good reasons for being persuaded that this way of perceiving the world is not appropriate—actually, they have no other conceptual access to stimuli observed in the world. In this context, the choice of the theoretical lenses through which scientists study phenomena is not a matter of algorithm or rational choice but it instead results from the way scientists share value, education, language and culture in a common community. Kuhn (1962, p. 199) added to this point that "debates over theory choice cannot be cast in a form that fully resembles logical or mathematical proof". Science is diversified and dissimilar disciplinary contexts offer different descriptions of the world.

It is worth emphasizing that scientists, as practitioners of a common large community, may share some common values, such as: that quantitative analysis is preferable to qualitative analysis; that results must be consistently justified; that results must be compatible with existing theories; etc. On these aspects for instance, financial economists and econophysicists agree. However, these common values often take different forms, depending on the disciplinary context within which they are implemented. Judgement, simplicity, consistency, plausibility, etc. often vary greatly from community to community (Kuhn, 1962). This chapter illustrated this variety of perceptions between econophysicists and financial economists: while the former experience their value by justifying their works through a Duhemian use of analogy, the latter, despite claiming the use of the same values, instead found their modelling practices on a naïve Popperian rhetoric. Philosophers of science do not escape this influence of language and education on the way of analyzing their topic. From this perspective, because there is no neutral or deliberative process without influence of a particular background (I have a background in economics), I am not convinced that makes sense to seek criteria that demarcate which community has "the best tools" for characterizing the evolution of economic/financial data. Given the fact that econophysicists and economists do not define the term "characterize" or "explain" in the same way (I illustrated this point in detail in this chapter), all kinds of judgements on this point would appear as a personal (and disciplinary) opinion. In this chapter, and more generally in this dissertation, I used a perspectivist attitude[206] to try to understand what the "internal reasons" are for why econophysicists and financial economists do not really have a dialogue. A particular familiarity with physics (I have a background in engineering) combined with my role as a financial economist doubtless helped me (or influenced me badly) in this perspectivist investigation. Furthermore, as a member of the economists' community, I tried in this chapter to acknowledge conceptual differences in order to initiate a potential dialogue between the two communities. In a sense, this chapter is an illustration of what Thomas Kuhn (1962, p. 201) suggested: "what the participants in a community breakdown can do is to recognize each other as members of different language communities and then become translators". I will comment and illustrate this further translation process in the final conclusion of this dissertation.

[206] This perspectivist attitude is in line with Giere's position that acknowledges that models and science are social and historical constructions created to serve human ends. However, Giere also acknowledges that such constructions correspond to a part of the world in a way that can legitimately be labelled objective. In other words, econophysicists and economists have their own "objective reasons" for implementing their different ways of doing science. For further information on this perspectivism, see Giere (2006).

General conclusion: Is a bridge with economics possible?

This dissertation investigated the emergence and the methodologies of econophysics. The first chapter presented the current disciplinary situation of econophysics through a bibliometric study, showing how it can be considered institutionally as a new sub-field of physics. However, the bibliometric evidence does not sit comfortably with the fact that the very debates that gave birth to econophysics locate it firmly in the field of financial economics. This, I argued, exposes the dual nature of econophysics: substantively a branch of economics but sociologically a branch of physics.

The second chapter examined the historical environment that favoured the advent of econophysics. This analysis rooted econophysicists' practices in the computational techniques (statistical pattern analysis and agent-based modelling) initiated in the Santa Fe Institute in the 1980s. Accordingly, I clarified the role played by this institution in the exporting of physics outside its borders. The major contribution of chapter 2 was to propose historical analysis of econophysics by clarifying the scientific context that linked this field with complexity studies.

The third chapter showed how the development of these studies progressively shaped the literature in econophysics. Precisely, while the SFI focused more and more on studies dealing with agent-based modelling, the works on statistical (macro) patterns gradually in the 1990s became an area of knowledge on its own that I called statistical econophysics. A decade or so later, when this new field faced the first critiques regarding the lack of micro-foundations, econophysicists began to integrate the agent-based techniques into their research. The contribution of this third chapter was to show that this methodological diversification of econophysics preserved the same conceptual hard core built on the importance of asymptotic reasoning.

The fourth and last chapter of this dissertation mainly focused on the original (statistical) econophysics to investigate the way in which its practitioners produce knowledge. In parallel, I studied the financial economists' modelling practices to compare the eventual differences with econophysics. This analysis led me to explain in detail the seminal paper (Stanley et al., 1996) that coined the term econophysics. Because this article laid down the conceptual foundations of the field, it is one (if not the most) quoted article in the current econophysics literature. My study showed how econophysicists used minimalist idealization and extended it to financial economics by implementing a Duhemian way of using analogies. In contrast, financial economists founded their works on Galilean idealizations presented through a Popperian rhetoric. These dissimilarities suggested that economists do not simply reject econophysics but that they have their epistemological reasons for acting in such a way. If the lack of dialogue between economists and econophysicists is due to different disciplinary construals, one can wonder whether an emergent dialogue could be initiated. I hope to have contributed to a better understanding of the reason for this field not really being accepted by economists. Being an economist myself, this question is meaningful, and I will conclude this dissertation with a discussion on this point.

I. Is a dialogue between econophysics and financial economics possible?

Although econophysicists and financial economists try to describe the evolution of financial prices statistically using idealizations in their modelling practices, they do not perceive and justify their works in the same way. What are the major reasons for such a gap between the two communities? What about the possibility of creating some bridging principles between the two fields? These questions will be discussed here by structuring my argument on the claims recently made by James Weatherall. Precisely, he asked the same kind of questions in two presentations about econophysics that he gave at the LMU Munich (July 2016) and at CAMPOS (November 2016) at the University of Cambridge. These two presentations echoed his book, entitled *Physics of Wall Street*, in which he provided a historical/biographical perspective on the key physicists who imported their knowledge to finance. In a sense, these presentations can be seen as a continuation of his book, because, unlike in the latter, the author investigated the reasons for economists' continuing reluctance to engage with the development of econophysics. As Weatherall (2016) explained it, "econophysicists are motivated by questions that they think they have the resources to answer, and yet, economists have (generally) rejected these answers – and perhaps the questions". I agree with him on this point, which is clearly supported by the bibliometric analysis performed in the economic literature (presented in chapter 2). Financial economists do not accept/publish econophysical papers in their major journals. Why do financial economists reject econophysics? I will answer this question in two steps. I will first deal with what James Weatherall (2016) called "cheap explanations" by reminding readers of the existence of two different disciplinary cultures that complicate all forms of interactions between communities. Afterwards I will investigate the nuances of the two "deeper reasons" proposed by Weatherall (2016) to explain the gap between economists and econophysicists. Weatherall did not really detail what he meant by "cheap" and "deeper" reasons but he implicitly associated the former with cultural differences and the later with methodological dissimilarities. While I generally agree with Weatherall that the situation must be studied through different lenses, I disagree with him regarding the importance that he gave to these lenses. Precisely, I will explain how some elements of his "cheap explanations" are actually crucial to the understanding of the current lack of dialogue between econophysicists and economists. In the same vein, I will suggest some nuances regarding what he called deeper reasons that, from an economist's viewpoint, must be reinterpreted.

I.1. "Cheap reasons" for this epistemological gap

At first sight the rejection of econophysics by financial economists might seem strange, because both these communities are familiar with the statistical analysis of empirical data. Why do they not interact with each other? This question is all the more meaningful regarding the influence of physics on finance, since several physicists (Osborne, 1962; Black, 1971) have contributed to the foundations of financial economics (Bernstein, 1994). Referring to the key disciplinary aspects, Weatherall (2016) listed four cheap reasons purported to explain why economists reject econophysics.

The first reason refers to the ideological dimension of financial economics (i.e. economists commonly believe they contribute to society through the development of markets) that contrasts with the descriptive nature of (econo)physics. I agree with

Weatherall on this point, which is a classical opposition between social and hard sciences. Economic institutions (including financial markets) are created and organized in accordance with a specific way of thinking about/defining them. In other words, the organization of financial markets is explicitly based on the way in which financial (mainstream) economists describe these markets. Related to this, Millo and Schinckus (2016) showed how the assumptions of the Black and Scholes model shaped the structure of the early derivatives markets. In the same vein, Frankfurter and McGoun (1999) and more recently Schinckus (2017) emphasized the ideological reasons for financial authorities' explicit promotion of a Gaussian description (with a few large fluctuations) of financial markets after the recent flash crashes (quick and large variations in financial markets due to a "loop effect" in the automatic trading algorithms).[207] In this context the financial authorities try to justify the non-intervention attitude on the market through the "Gaussianity" of financial returns that (would) show that investors have the same probability of winning or losing money: the market being fair in this context, no intervention is required. This interconnection between financial knowledge and financial institutions makes financial economists more reluctant to accept a new theoretical framework that has the potential to shake/change the political recommendations promoted by financial authorities.

The second reason evoked by Weatherall refers to the importance of the "rational expectations assumption" in economics that would be rejected by physicists. I partly agree with him on this matter. If it is indisputable that (financial) economics is based on the hypothesis that agents have perfect rationality and therefore rational expectations, the idea that this dogma is challenged by econophysicists is not so obvious. Indeed, it is even common to find econophysical works (Bouchaud and Potters, 2003; McCauley 2006; Sornette, 2009)[208] that assume that actors are perfectly rational. Although econophysicists, in their critiques of economics, refer rhetorically to the unrealism of the perfect rationality assumption (Mantegna and Stanley, 1999; McCauley, 2006), they rather seem to be methodologically indifferent towards this assumption, which is not a particular requirement (or a challenged notion) in their works. Moreover, by ignoring what is happening at the micro-level, it

[207] See Schinckus (2017) for further information on these events.
[208] See Schinckus (2013) for an overview of these works.

cannot be said that econophysicists would propose a more realistic hypothesis regarding individuals' behaviour (Schinckus, 2012). In other terms, this cheap reason (as labelled by Weatherall) is not an explanation.

The third reason that would explain the rejection of econophysics by economists refers to a corollary implication of the previous reason: the lack of respect for the principle of utility maximization that is sacred to economists. This methodological aspect can indeed contribute to the lack of dialogue between the two communities. In contrast to all mainstream financial economists, econophysicists do not assume an individual utility function that should be maximized or optimized in relation to an existing set of initial conditions (McCauley, 2006). It is worth distinguishing the use of rational expectations assumptions and the principle of utility maximization. The former refers to the abilities of agents to deal with all the available information, but that does not necessarily require the formulation of a particular utility function. The principle of utility maximization means that rational agents will optimize a specific function to maximize their satisfaction. While some econophysicists assume in their works that agents can deal rationally with all the available information, none of them use the principle of utility maximization in their way of modelling the agents' behaviour - that does not comply with economists disciplinary expectations.

Last but not least, the final reason leading to the rejection of econophysics is the fact that econophysicists challenge what Weaterall (2016) called the "Gaussianities" of economics. Gaussianities refer here to economists' tendency to see Gaussian distribution in many of the phenomena that they study. I agree with Weaterall on the importance of "Gaussianities" in (financial) economics; however, unlike him, I do not consider it to be a cheap reason for the absence of dialogue with econophysicists. Chapter 4 showed that Gaussian distribution is an important element of the Galilean idealization used by financial economists, who have a different "way of doing science", implying that they have different standards in terms of explanation. Regarding this point, Feyerabend explained, "taking the demand for explanation for granted, only such theories are admissible (for explanation and prediction) in a given domain which either contain the theories already used in this domain, or are at least consistent with them" (Feyerabend, 1995, p.55). Even if econophysicists and financial economists agree on the existence of large fluctuations in the dynamics of financial prices, they do not “see the same thing”, simply because the same set of observational data is compatible with different and mutually inconsistent theories. In this context I think Weatherall underestimated the importance of this reason. The theoretical and technical importance of the Gaussian framework is actually the major obstacle between econophysics and (financial) economics – I will provide details of it in the following subsection.

I.2. Deeper reasons for this epistemological gap

According to Weaterhall (2016), the deeper reasons for this opposition between the two fields can be summarized as follows:

> First, econophysicists are motivated by a set of explanatory demands that are natural in physics, but which economists reject […] Second, econophysicists have a different understanding of the relationship between their models and the data than do economists. (Weaterhall, 2016, slide 31)

Although Weaterall (2016) did not clarify the kind of explanation that he referred to, I generally agree with these two reasons, especially the first one, regarding the explanatory demands in which “econophysicists seek to explain (and predict) large-scale (macro/market level) phenomena by appeal to the dynamical and statistical properties of micro-level descriptions” (Weaterhall, 2016, slide 32). This claim is supported by the first part of chapter 4, explaining that the theoretical foundations of this explanatory framework are based on minimalist idealization (the Ising model and the renormalization group theory) and that econophysicists extended their work outside their original field through a Duhemian use of analogy. The rejection of this set of explanatory demands by economists is due to the fact that they implement other modelling practices based on Galilean idealizations and justified by a Popperian rhetoric (as explained in chapter 4). Such an epistemological gap means that *what counts as an explanation* differs in the two fields.

However, at first sight this rejection might seem surprising. As mentioned above, several physicists contributed to the statistical development of financial economics, and many economic/financial models are micro-founded. Therefore, Weaterhall (2016) wondered rightly why economists reject the set of explanatory demands used by econophysicists. According to him, economists “reject the expectation that large-scale phenomena should/can be explained via micro-scale dynamics", and this observation would lead to the idea that "economists do not like agent-based modelling" (Weaterall, 2016, slide 37). It is worth remembering that agent-based modelling is neither an economic computational technique nor one based on physics. Economists might not like this approach, but many of them are familiar with it and use it in their works. Although agent-based modelling indeed challenged the foundational idea that no interactive agents are described by a fixed utility function, there is an important literature showing that this way of modelling is logically compatible with the neoclassical framework (Arthur, 2014). Hamill and Gilbert (2016) offered a very good review of this growing literature. A quick look at the list of the recent winners of the Nobel Memorial Prize in Economic Sciences also gives an indication about the acceptance of agent-based modelling in economics. Three people have won this award for their contributions to the development of agent-based economics: Thomas Schelling was the laureate of this prize in 2005 for his contributions to game theory[209]; Elinor Ostrom won this prize in 2009 for her work on the agent-based governance of complex economic systems; and Angus Deaton was awarded in 2015 for his contributions to the micro-foundations (agent-based modelling) of consumption, welfare and poverty. The growing importance of agent-based modelling can also be observed in finance, in which Lengwiler (2006) and more specifically Meyers (2009) showed how agent-based modelling also contributes to the financial mainstream.

Contrary to Weatherall's opinion, economists do not reject the idea that macro-phenomena can be explained in terms of micro-dynamics. The importance of the economic literature dedicated to the so-called micro-foundations of macroeconomics illustrates the key role played by this aspect in economists' mind. In 1979 the economist Weintraub published a book entitled *Microfoundations: The Compatibility of Microeconomics and Macroeconomics*, urging that economic macro-dynamics must be explained in terms of micro-dynamics. Afterwards, "during the last quarter century, the micro-foundations approach to macroeconomic theory has become dominant" (Van den Bergh and Gowdy, 2003, p.65). Today, one can find dozens of books and articles (Weintraub, 1977; Weintraub, 1979; Boumans and Davis, 2010)

[209] Game theory is a mathematical framework that can be tested or implemented through the methodology of agent-based modelling (Bonabeau, 2002).

presenting or debating the pertinence of the micro-foundations of macroeconomics that "the majority of (influential) economists take for granted" (Gowdy, 2004, p.78). In 2006 Edmund Phelps won the Nobel Memorial Prize in Economic Sciences for his work on the micro-foundations of a macroeconomic theory of employment. Today, it is impossible for a contemporary economist to avoid this specific theme, since the vast majority of textbooks deal with the micro-foundations of macroeconomics (Hoover, 2010). Given this context, where is the problem evoked by Weaterall (2016)? If econophysicists and (financial) economists both offer models to explain macro-dynamics in terms of micro-dynamics, why cannot they interact? As detailed in chapter 4, financial economists and econophysicists have different scientific methods, implying different standards. In this sense, the reason proposed by Weaterall (2016) must be read differently depending on the disciplinary lens used. From economists' point of view, they do not reject the idea that a large scale can be explained via micro-scale dynamics, but they certainly reject physicists' method. In other words, the major difference is in the way in which econophysicists and financial economists make the link between the micro- and the macro-level. While econophysicists accept that the macro-level can be "more than the sum of the micro-entities" (see the link of the renormalization group theory detailed in chapter 3), economists usually consider the macro-level in a different way, in which the most important aspect is not the process of aggregation but rather the compatibility between the macro-dynamics and the rational individual choice of micro-agents.

In a sense, this common use of agent-based modelling and this assumption to begin all reasoning with micro-foundations could be perceived as a potential conceptual bridge between econophysicists and economists, since these two communities seem to be familiar with these themes (Schinckus, 2010a, 2010b; Walstad, 2010). However, this is not the case. As Weatherall (2016) argues, this is due to two points: 1) the fact that economists strongly reject the kind of explanation provided by econophysicists; and 2) the fact that the former and the latter conceive their modelling practices fundamentally differently. Although I agree with this general formulation of these points, I explained in this section why I disagree with Weatherall in the detailing of these points. Precisely, economists reject econophysicists' explanation not because they reject the importance of the micro-scale (as argued by Weatherall) but rather because they consider that the macro-scale must be compatible with the rational individual choice of micro-agents to maximize the utility function. Economists and econophysicists indeed conceive their modelling practices differently. However, such a situation is directly related to the importance of what Weatherall called the Gaussianities of financial economics. In contrast to his claim, I would not associate this aspect with a cheap reason for the lack of dialogue between the two communities. Chapter 4 of this thesis showed how Gaussianities play a key role in the definition of what is empirically adequate for economists (while power laws define this aspect for econophysicists). Interestingly, the Gaussian framework can be presented as a specific case of power laws implying that there is a potential common path for future research between the two fields. I conclude this dissertation on by discussing this point in the following section.

II. Implications for the future of econophysics

In this context of difficult dialogue between the two communities, there is now a pressing question about the future of econophysics: what are the implications of my research for econophysics? Is a profitable dialogue between econophysics and financial economics possible? In this conclusion I would like to discuss these two questions by suggesting some future research paths that could create a conceptual bridge between the two communities.

This thesis showed that, despite some historical similarities, econophysics and financial economics have different ways of conducting science. Indeed, by dealing with different aspects of complex economic phenomena, these two fields are not in themselves enemies. They play their part in our understanding of such phenomena. The question in this context concerns the identification of potential future research paths that could be investigated for the development of a more integrative understanding of financial phenomena. Chapter 4 detailed how economists and econophysicists have different disciplinary construals that implicitly influence their modelling practices. Construals “are often not made explicit in discussions of models. This is because communities of modelers have standard conventions for reading model descriptions” (Weisberg, 2016, p.40). These implicit conventions shared by each community are probably the first obstacle to the initiation of a dialogue between the two fields. In other words, the first step towards this dialogue therefore refers to the clarification of these construals, which must be made explicit for all to study the extent to which these conventions can evolve towards a more transdisciplinary perspective. As Weisberg (2016, p.79) argued, "The construals can change through time, or with the application of the same structure to a different modelling domain". The very few works (Jovanovic and Schinckus, 2016; McCauley, Roehner, Stanley and Schinckus, 2016) existing on this difficult task "to open black boxes" have focused on what must be undertaken to make econophysics meaningful for economists and reciprocally to make financial economics meaningful for econophysicists. This difficulty for the two communities to understand each other is well illustrated by the following quotation made by the economist Steven Durlauf[210] (2005):

> … the implications of this new literature [econophysics] for economic complexity are still very unclear […] Within the physics community, there has emerged a subfield known as "econophysics" in which major research activity is represented by efforts to find power and scaling laws in different socioeconomic data sets … [however, these] findings in the econophysics literature are unlikely to persuade economists that scaling laws are empirically important. (Durlauf, 2005, p.231)

Very often the two communities stay behind their disciplinary frontiers by presenting their methodological approach as completely new when it is not, as Lux (2009b) explained:

> One often finds [in the literature of econophysics] a scolding of the carefully maintained straw man image of traditional finance. In particular, ignoring decades of work in dozens of finance journals, it is often claimed that "economists believe that the probability distribution of stock returns is Gaussian," a claim that can easily be refuted by a random consultation of any of the learned journals of this field. In fact, while the (erroneous) juxtaposition of scaling (physics!) via Normality (economics!) might be interpreted as an exaggeration for marketing purposes, some of the early econophysics papers even gave the impression that what they attempted was a first quantitative analysis of financial time series ever. If this was, then, performed on a level of rigor way below established standards in economics (a revealing example is the analysis of supposed day-of-the-week effects in high-frequency returns in Zhang, 1999) it clearly undermined the standing of econophysicists in the economics community. (Lux, 2009b)

Until now, the models developed by econophysicists have mainly stayed within the boundaries of statistical physics. In a sense, the aforementioned quotations are more

[210] Durlauf (who was an economist involved in the Santa Fe Institute) set out his position more clearly in a later paper (2012, p.14).

a call for collaboration than a criticism of econophysicists. The problem is not the econophysics concepts per se but rather the lack of links with the existing financial economics knowledge. Indeed, the majority of econophysicists apply the concepts and models of physics as they exist today, ignoring the features of financial economics, particularly the need for quantitative tests validating the power laws and the necessity to have generative models explaining the emergence of such as patterns.

One must admit that, for a long time, research into power laws has suffered from these two major problems. On the one hand, there were no statistical tests, the only tests being based on a visual comparison method. On the other hand, no generative models existed for explaining the emergence of power laws. These two absences are crucial for financial economists, because, from their Popperian perspective, statistical tests on the use of a theoretical statement are the foundations of their discipline. Indeed, from the most common financial-economics viewpoint, a scientific model must not only reproduce reality but must also be validated by econometric/statistical tests of assumptions that are compatible with the recognized theoretical framework. Some econophysicists do not feel especially concerned about these aspects, because, from their scientific perspective, they do not need to meet these conditions to propose a model. By contrast, these two conditions have largely contributed to the maintenance of the Gaussian framework by financial economists even when they describe the occurrence of extreme variations. Consequently, this epistemological gap has strongly supported the misgivings of financial economists about the potential contribution of econophysics to their field. Currently these contributions are still difficult to value in the light of the theoretical mainstream used by financial economists.

Interestingly, the gap evoked above is not only due to the methodological dissimilarities between the econophysicists and financial economists. It also results from the current lack of knowledge about the statistical treatment of power laws. In a seminal paper on power laws, Michael Mitzenmacher (2005) asserted that the characterization of empirical distributions by power laws is only a part of the challenge that faces researchers involved in explaining the causes and roles of these laws. Precisely, he pointed out the need for theoretical models that could explain them:

> While numerous models that yield power law behavior have been suggested and, in fact, the number of such models continues to grow rapidly, no general mechanisms or approaches have been suggested that allow one to validate that a suggested model is appropriate. [W]e have beautiful frameworks, theory, and models – indeed, we have perhaps far too many models – but we have been hesitant in moving to the next steps, which could transform this promising beginning into a truly remarkable new area of science. (Mitzenmacher, 2005, p.526)

In other words, the lack of dialogue between econophysics and financial economics not only results from different disciplinary contexts but also emphasizes a peculiar lack of knowledge (research) on a specific aspect (i.e. the lack of statistical tests) related to the use of power laws. The most interesting point is that, given their disciplinary construals, econophysicists and financial economists do not need to initiate such research. However, the diffusion of econophysics works into financial economics could favour the potential future development of such research.

For a philosopher of science, there is an interesting point here: the lack of dialogue between two scientific fields illustrates the disunity of scientific practices. Economists and econophysicists certainly have different construals and modelling practices, but the lack of dialogue between them also echoes another problem: the validation of the use of statistics. While economists work with a specific statistical (Gaussian) frame to ensure the existence of statistical tests, econophysicists rather use an analogical extension of their statistics that is conceptually justified by their disciplinary beliefs. Such a difference (detailed in chapter 4) shows the disunity of science in the method of validating the use of statistics. Although power laws are nothing new in science, there is still a lack of tools to testify to their validity. The following schema illustrates the current situation of these tools:

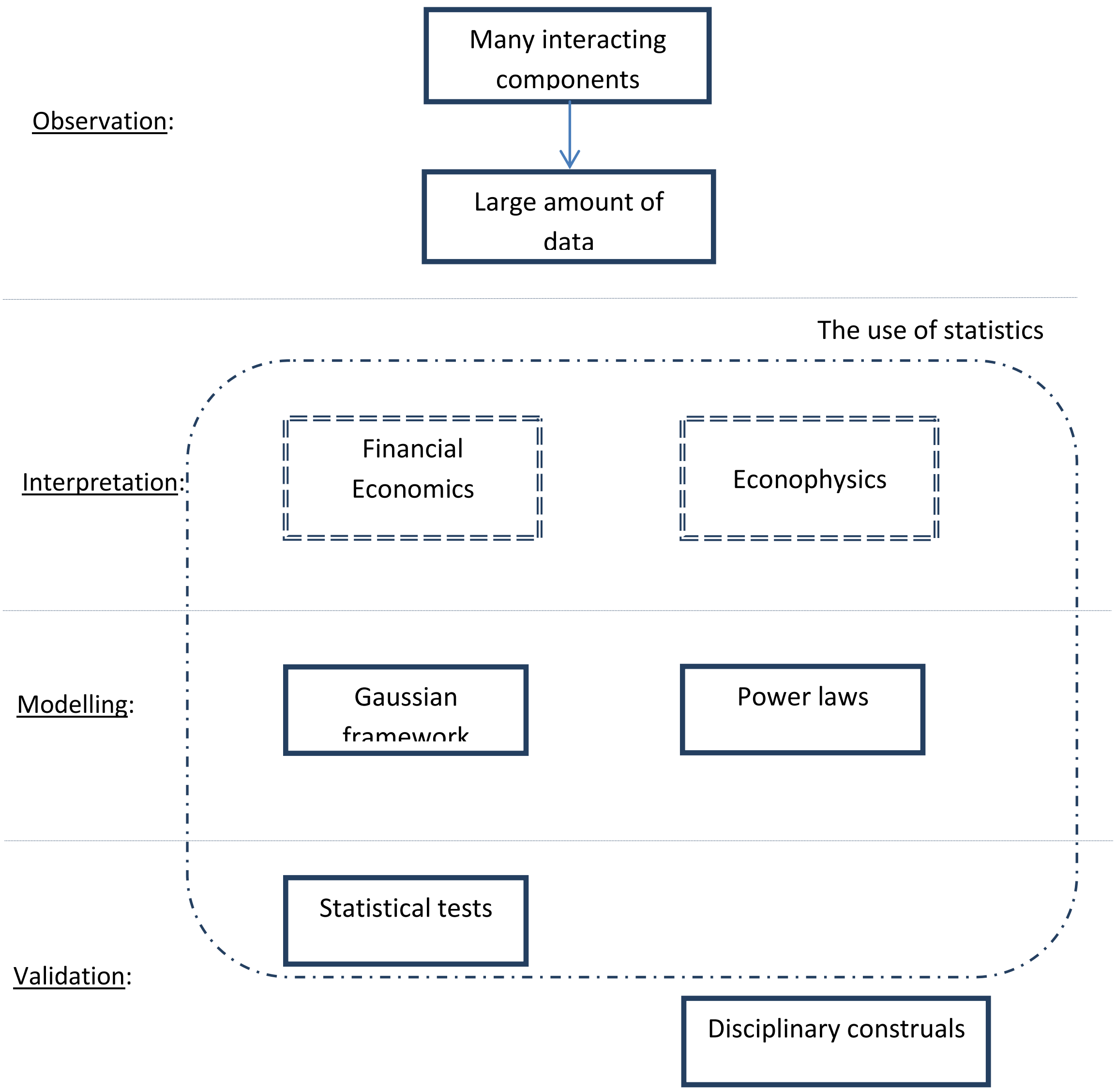


Figure 1: Schema illustrating the current situation of econophysics in relation to financial economics.

Economists and econophysicists aim to characterize the increasing complexity of economic/financial phenomena. Although these two communities use statistics to describe these phenomena, the key difference between them refers to the way in which they justify/validate their use of statistics. Economists mainly work with a statistical frame that allows them to justify their approach using statistical tests (well-defined in the domain of statistics). In contrast, in the absence of statistical tests for power laws, econophysicists justify their approach through an analogical extension of their disciplinary construals. Because such validation is outside the domain of statistics, their statistical approach is perceived by economists as a circular (ad hoc) justification that only physicists can understand. Discussing the current statistical knowledge about power laws, Mitzenmacher (2005, p.526) suggested a taxonomy in five steps for studies on power laws:

> (1) Observe: gather data on the behaviour of a system and demonstrate that a power law distribution appears to fit the relevant data.
> (2) Interpret: explain the significance of the power law behaviour to the system.
> (3) Model: propose an underlying model that explains the power law behaviour.
> (4) Validate: find data to validate, and if necessary specialize or modify, the model.
> (5) Control: use the understanding from the model to control, modify and improve the system's behaviour.

Mitzenmacher's analysis was particularly relevant to econophysics. Like other fields (geography, biology, etc.) that use power laws in their research, econophysics had not really been able to move beyond the third step when Mitzenmacher published his article in 2005. Mitzenmacher's argument is very important, because, on the one hand, it underlines that the claims made by economists have an echo in other fields dealing with the use of power laws; and, on the other hand, it paves the way for a potential research agenda that would ease the collaboration between econophysics and financial economists.

The development of statistical tests for power laws combined with the adjustment of economists' Popperian rhetoric could contribute directly to the development of an integrative approach between econophysicists and financial economists. Precisely, this kind of test would even be in line with a more restrictive Popperian approach based on an improvement of standards through which the theories and results of the past must be evaluated. It is worth mentioning that such statistical research has been conducted very recently and has not been disseminated[211] widely among econophysicists. Moreover, to date, the very few works dealing with statistical tests of power laws have not yet been used with financial data (they have been used with wealth, income, city sizes and firm sizes). Despite their drawbacks and the fact that

[211] It is worth mentioning the existence of a very few works on this topic: Ausloos (2014) or Schinckus (2013).

further investigation is needed, we can consider that these tests have opened the door to some research on statistical tests. Although one can observe the emergence of common works between economists and econophysists, the bridge between the two fields is still to be written. Up to now, the story still generally appears a failed takeover by one discipline famous for being imperialist (physics). This failed takeover is also due to the resistance of economics (also famous for being imperialist) that creates a situation in which the two empires are still standing, staring at one another by identifying (very) slowly the potential paths for a future dialogue.

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