MODELS AS SPEECH ACTS: THE TELLING CASE OF FINANCIAL MODELS

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Models as Speech Acts: The Telling Case of Financial Models

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Abstract: This paper intends to bring Austinian themes into methodological discussion about models. Using Austinian vocabulary, I argue that models perform actions in and outside of the academic field. This multiplicity of fields induces a variety of felicity conditions and types of performed actions. If for example, an inference from a model is judged according to some epistemological criteria in the scientific field, the representation of the world which the model carries, will not be judged by the same criteria outside the scientific field. A model can be considered as a standard in a strict scientific framework, while not being used as part of public policies, or vice versa. However, we focus on the dynamics between different fields.

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Having observed the ignored or repressed difference between the common world and the scientific worlds, one can endeavour (...) to conceptualize what remain practically inaccessible to any self-respecting scholastic thought: the logic of practice. And this must be done by trying to carry through to the end the analysis that even the boldest of philosophies often abandon in mid-course, at the point where it would encounter the social. (Bourdieu, 2000: 50)

I. Introduction

Following Morgan (2012) and Hédoin (2012), to consider model making as an act in a scientific environment opens an interesting perspective. This perspective can be extended insofar as the scientific field is not the only praxis area for economists. Models participate in the making and the spreading of representations inside and outside of the scientific field. Using Austinian vocabulary, I argue that models perform actions in different fields (scientific, academic, practical and political). This multiplicity of fields induces a variety of felicity conditions and types of performed actions. If, for example, an inference from a model is judged according to some epistemological criteria in the scientific field, the representation the model carriers will not be judged by the

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same criteria outside the scientific field. A model could well be considered to be standard in the strict scientific framework, while not being used as part of public policies, or *vice versa*. In this paper I focus on the dynamics between different fields.

After exploring the implications of such an approach to modeling activity, I illustrate the potential interest in this approach by investigating the case of financial models. Specifically, I focus on the awakening phase in the financial modeling during the 1950s, 1960s and 1970s. There are several potential answers to the question of what theoreticians of finance did with their models:

- They attempted to integrate particular scientific and academic fields.
- They took a position in the analysis of stock movements against technical analysts (practical field).
- They participated in the recapture of a symbolic power from the defenders derivatives markets (political field).

It is impossible to understand financial modeling without appreciating what modelers as speech-act makers performed by their models. This has been well known following various works on the history of finance. My goal is to illustrate the interest of a conception of models as speech acts in light of these works (e.g. Jovanovic, 2002; Bernstein, 2005; MacKenzie, 2006; Preda, 2007; Poitras, 2009; Mehrling, 2012).
II. From model-to-world, to world-to-model.

There is a huge body of work on the nature, functions and uses of scientific models which might make one skeptical about the possibility of a unified account of modeling. The semantic approach is clearly one of the most discussed nowadays (Suppes, 1961; van Fraassen, 1980). Far from being homogeneous, the general idea is to get away from the received view established in the context of logical empiricism, according to which a model is a model of a theory: a model is another interpretation of a theory conceived as an axiomatic system expressible in formal language. The semantic view considers theories to be non-reducible to linguistic entities and what they refer to. Theories are construed « as what their formulations refer to when the formalizations are given a (formal) semantic interpretation » (Suppe, 1989: 4). This refers to defining a theory as a structure which can be related to phenomena and axiomatized in a set of theoretical predicates; models are defined as structures which satisfy these predicates (Frigg, 2006). Thus, a theory can be defined as a family of models considered both in terms of the formal linguistic representations of the theory’s predicates, and of descriptions of the outside world the theory describes. Concerning the first relation, models are non-linguistic structures which can be instantiated in a variety of languages. The second relation is a semantic one. Of course, there are fierce debates concerning the evaluation of this truth relation between models and the world (Chakravartty, 2001).
The semantic view has been criticized for not accommodating the nature and the role of models in scientific practices. The conception of models as a set of passive reproductions of a theory seems too restrictive:

This account gives us a kind of homunculus image of model creation: Theories have a belly-full of tiny already-formed models buried within them. It takes only the midwife of deduction to bring them forth. On the semantic view, theories are just collections of models; this view offers then a modern Japanese-style automated version of the covering-law account that does away even with the midwife. (Cartwright, Shomar, and Suarez 1995, 139)

It would be short-sighted to see models only as go-betweens between theory and the real world. In practice, they often are built without explicit underlying theories. Moreover, model making is not neutral regarding theories, it is a creative process: models bring unexpected “surplus content” that is heuristically fruitful (McMullin, 1968).

Beyond these debates, the semantic and the post semantic views intermingle in considering model making only within a model-to-world perspective. By model-to-world perspective, I mean the common epistemological account which consider a model only as a tool for representing an external world, whatever what we mean by “representation” - description, idealization, explanation, caricature, credible world, exploration, and so on. Although often models are seen as autonomous worlds elaborated for their own structures and

\[ \text{ARCH class of models is a telling example. See Lux (2006).} \]
\[ \text{This position was criticized by Da Costa and French (2000) who retorted that even if there were models that were developed independently of theory, they could still be represented in terms of structures.} \]
\[ \text{This concept is freely inspired by the work from John Searle’s philosophy of mind (Searl, 2004).} \]
properties (Morgan’s (2012) “world in the model”), the kind of criteria used to judge their external validity echoes the issue of epistemological reflections on models. This interest is exemplified in the debates around Julian Reiss’s “explanation paradox” (Reiss, 2012): how economists can simultaneously admit their models are wrong in the strong meaning of verisimilitude, and defend that they provide information about the external world?

In relation to economic modeling, Hédoin (2012) and Morgan (2012) criticize both semantic and post-semantic views. Both authors pave the way to a pragmatic conception of models. Hédoin criticizes the most influential post-semantic conception, namely Nancy Cartwright's causal capacity approach. The notion of capacity relies on the singular power of things to bring about other events. When a causal law is the expression of a statistical link in a particular context – X causes Y in a context V – the capacity is “something they can be expected to carry with them from situation to situation. So if the probability goes up in one test situation, thus witnessing to the capacity, it will do the same in all the others” (Cartwright, 1989, p. 106). As a consequence, talking about causal capacity is akin to talking about the ontological capacity of one thing to cause another. Models are concerned with the discovery of capacities that are “fixed (enough) arrangements of components, or factors, with stable (enough) capacities that in the right sort of stable (enough) environment will, with repeated operations, give rise to the kind of regular behavior that we represent in our scientific laws” (Cartwright, 1999: 50). Cartwright is talking about “Nomological Machines”. 
Cartwright\textsuperscript{5} makes the isolation of causal capacities in the world the alpha and omega of modeling activity. As a consequence, a failure in that task appears to be insurmountable: a model that does not isolate the causal capacities of external reality is \textit{per se} a wrong model since it does not fulfill its role as a nomological machine. Hédoin stresses that not only do many economists seem to be accepting of this kind of failure, it also does not appear to disturb their practices. On this ground he offers a pragmatic approach to economic modeling:

\begin{quote}
[T]he \textit{pragmatics} of economic modeling suggests that economists build and use models to make various kinds of inferences whose relevance cannot be understood without referring to the kind of problems (both theoretical and empirical) that community of economists is actually facing. (Hédoin, 2012, 437)
\end{quote}

The idea of a pragmatic approach to modeling is rooted directly in the claim that the function of a model as well as the epistemological criteria against which it is validated are defined by the scientific community within which the modeling practice takes place. What Weintraub (2002), following Corry (1996), calls the image of mathematical knowledge: standard proof accepting, ideas about the nature of models, ideas about rigor, etc.

These epistemological criteria set by a scientific community highlight the issues deserving attention, and the truth-value of specific claims. Hédoin builds on and extends Mauricio Suárez’s claim that a model supposes the presence of a modeler (Suárez, 2004):

\textsuperscript{5} See also Mäki (1992; 2009).
Establishing and maintaining the representational force of a model supposes the presence of an agent (the modeler) with a specific purpose. The agent must intend a specific use of the model and, as Suárez (2004, 773) notes, these uses are driven by pragmatic considerations. The second requirement makes the role of the modeler even more important in the determination of the cognitive value of the model. It underlines one of the main functions of scientific representational devices: to permit surrogate reasoning. A given model will not have the same value for different agents who are endowed with different competencies and knowledge and who are pursuing different goals. (Hédoin, 2012, 438-439)

In *The World in the Model*, Mary Morgan (2012) adopts a similar approach: contrary to what a semantic approach might indicate, the model is not a neutral artifact inserted between theory and reality. Morgan insists primarily on the fact that (1) the model is an exploratory tool designed to produce certain inferences, (2) these inferences concern both the external and the theoretical worlds, (3) models are not passive objects, not mere reflections of the theory or the world, but are active which makes it possible to explore these entities, and (4) inferences produced using models are judged against criteria from specific scientific communities. Modeling creates a new world, a world “in the model”, which allows us to say things about the theory on which it is grafted, or about the world that this theory describes. If we return to the classical problem of induction, the problem is that there is no way to judge the descriptive validity of a model:

Economists create models in an effort to find out how the world works and it is because they don’t already know how it works that they also don’t know whether they have an accurate model representation! (Morgan, 2012: 286).
This relativity of validation criteria takes us into the field of the epistemic specificity of different scientific communities.

Hédoin's and Morgan’s approaches consider modeling as a creative process in a community. The model is far from being a mere representation; it is a tool that is used to say things about the world, which can be evaluated only using the criteria defined within a particular community. Modeling is an intentional practice, an inferential practice, in a scientific community which sets inferential norms:

[T]he inferences drawn from the model depend on the modeler’s intentions, which are themselves constrained and defined by what the modeler expects to be the community’s inferential criteria. (Hédoin, 2012, 442)

In the Prologue in his standard, *How Economics Became a Mathematical Science*, Roy Weintraub comments on Mary Morgan’s position compared to classical philosophical approaches:

A philosopher asking “what is a model really?,” and creating distinctions among ways of modeling or thinking about modeling, is not of much help to Morgan in her project. Instead, for Morgan, modeling is embedded in practice, in the craft of the economist, and thus she grounds her investigation in the history of practice [...]. (Weintraub, 2002: 7)

In my view, the switch from *models as representations* to *models as inferential tools used in a community* is fruitful for the philosopher. In this perspective a model is not only designed to account for the external world the modeler
intends to explore (model-to-world). The modeler's goal is also to fulfill his community’s criteria which define what is a good model. This *model-to-community* view substitutes the question “what is a model?” by the question “what does the modeler want to do with his model?”. More than a sociological perspective on models (*à la* Kuhn, 1996), I want to stress that modelers not only want to describe the world while meeting the epistemological criteria of their communities, they also want to participate in the dynamics of their community. From Hédoin's and Morgan's examination of models from the perspective of their making we can go a step further and consider the act of modeling as a performance in several directions. An economist who makes a model does more than just describe the world; he does something in the world. First he does something in his community by spreading new representations, and potentially, new epistemological criteria, second he does something in the social world in general. I think the pragmatic view expressed by Morgan and Hédoin opens the perspective of a *World-to-Model view*: modelers do something in the world they study. A model is no longer a representation (model-to-world), it is a speech act (world-to-model). This perspective could help, if not to resolve at least to understand why despite the several paradoxes concerning the essence of modeling (Reiss, 2012; 2013) economists still work with models.

III. Models as speech acts.
What does it mean to say that modeling is acting? This question brings us directly to the theory of speech acts and performativity. The use of Austinian theory in the context of the history of ideas was a historiographical impulse, orchestrated by the Cambridge school. In his critique of standard approaches to the history of political ideas, Quentin Skinner points to the limits related to understanding texts as expressions of beliefs which the historian should look for carefully through exegesis. Instead, texts should be considered as speech acts, determined by illocutionary intentions: authors have certain intentions when they produce their speech (Skinner, 2002). In accordance with an ordinary language philosophy, anyone who considers that understanding a speech necessitates seeing it as an act, should subscribe also to the idea that this speech is likely to fail. The success of a statement to do what it is supposed to produce will depend on a set of standard conditions.

Skinner calls for a suspension of the mythological history built on the idea of texts’ conceptual autonomy. The history of ideas needs to analyze illocutionary intentions in conventional and conversational contexts (Skinner, 1970). Although he defends analysis of the contextual rationality underlying the texts (Skinner, 2011), Skinner does not neglect the fact that a statement can have unwanted effects. Therefore, texts can take some autonomy from the initial

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6 Michel Callon borrowed the Austinian vocabulary of performativity in order to analyze economists' speeches, while redefining performativity around the concept of “socio-technical devices.” For Callon, a theory is performative when, beyond simply describing the outside social world, it changes its structure. For Callon, this world-to-economic theory motion is achieved through the fact that economists produce formal models which are parts of economic devices. Without returning to the critics of this view of performativity (Brisset, 2014, 2015a, 2016), this paper attempts to articulate two alternative understandings of use of the Austinian concept.
wishes of the speakers. Reference is made explicitly to the *surplus of meaning* evoked by Ricoeur (1969, 1975). Because they are based on broad symbolic foundations, texts impose some reserve on interpretation and re-interpretation, especially through tradition.

Writing is doing something which produces an effect, either desired or not. Implementation of these actions depends of certain conventional conditions of enunciation. Skinner considers the effect of texts on the uses of concepts, and thus he considers the constitution of the social world:

I accept, of course, that we are all limited by the concepts available to us if we wish to communicate. But it is no less true that language constitutes a source as well as a constraint (...). This means that, if we wish to do justice to those moments when a convention is challenged or a commonplace effectively subverted, we cannot simply dispense with the category of the author. The point takes on an added significance when we reflect that, to the extent that our social world is constituted by our concepts, any successful alteration in the use of a concept will at the same time constitute a change in our social world. (Skinner, 2002: 117-118)

This provokes a first observation that it is entirely possible to do multiple things with a single statement or set of statements, namely with the same theory or model. Specifically, the same statement can participate in many conversations, perform different moves in different social contexts, and therefore, be subject to several sets of felicity conditions. Individuals are involved in several environments (Lahire, 2001), and develop several types of rationality.
There is another use of Austinian theory that is relevant for the present paper, which is rooted in the work of Pierre Bourdieu. Bourdieu begins with a critique of Austin's work. Although this critique emphasizes the importance of felicity conditions concerning the “happiness” of speech acts, it gives little substance to these conditions. Following Bourdieu, we observe the importance of constructing a space of legitimacy that allows some speakers the power to do things with some of their words beyond just being understood. There is a significant gap between sufficient language proficiency to produce comprehensible statements, and possession of sufficient social (symbolic) competence to be heard (Bourdieu 1982: 42).

Skinner and Bourdieu seem to exhibit a certain complementarity, to the extent that the latter gives consistency to the conventional conditions governing acts of theoretical language: we always speak from a certain position within a certain domain of the social space, and each speech tends to strengthen this position and this order. Seen from this perspective, domination consists precisely of the ability to dictate the legitimate conditions of felicity⁷. Thus, we can perceive a duality in the speech act (Brisset, 2015b). When a town mayor opens a new building, the speech act consists of providing the building with a recognized status. One can speak of a speech act as “conventional” insofar as it is obviously part of a process allowed by the force of precedents. If my speech challenges the legitimacy (status) of the mayor to inaugurate a public building, this is different. In that case, the illocutionary intentionality is to reverse the

⁷ For a critique of the narrow vision of the context in Skinner’s works, see Meiksins Wood (2012).
symbolic (conventional) order within which the performative power of the mayor is enacted. It is possible to speak of an “unconventional” speech act, because it precisely changes the established order. It goes without saying that these categories are intuitive rather than strictly exclusive: challenging the conventional order requires some configurations which themselves are conventional. This is reminiscent of the debate concerning the robustness of the distinction between illocutionary effects of speech and perlocutionary ones (Millikan, 2014). However, from the perspective of the history of ideas, it is relatively intuitive to differentiate between the speech act strengthening the established order (the act of transubstantiation reinforces the legitimacy of the priest to perform other acts of the same kind), and the speech act of performing a symbolic revolution in the meaning of Bourdieu: a symbolic revolution disrupts the cognitive and social structures (Bourdieu, 2013).

IV. How to do things with models?

To consider modeling as an act of inference in an academic environment opens an interesting perspective. This perspective can be extended in that the scientific field is not the only praxis area for economists. Models participate in the making and the spreading of representations of the world, inside and outside the academic field. This multiplicity of fields induces a variety of felicity conditions and types of performed actions. For example, although a model's inference may be judged according to some epistemological criteria in a
scientific field, the model’s representation of the world will not be judged by the same criteria outside the scientific field.

A recent contribution of Jovanovic, Andreadakis and Schinkus (2016) shows how the Efficient Market Hypothesis (EMH) has been used in the legal field through the Fraud on the Market Doctrine. To maintain a claim for securities fraud, a plaintiff must prove that it relied upon defendant’s allegedly fraudulent conduct in purchasing or selling securities. The Fraud on the Market Doctrine entitles plaintiffs to a presumption of reliance on the defendant’s misstatements when the relevant market is proved to be efficient. Since on an efficient market, public information is reflected in the prices of shares, « a person relying on the integrity of the market price can be said to rely indirectly on the public information it contains », which have been distorted by the alleged misstatement of the defendant (Jovanovic, Andreadakis and Schinkus, 2016: 179). The authors show that while using the same concept, jurists and economists use EMH in a totally different ways since both what they intend to do and their criteria of judgment are different. For instance, the issue of fundamental value is major for economists whereas it is superfluous for jurists since their only preoccupation is the public availability of information and not the origin of this information. Jovanovic, Andreadakis and Schinkus focus on the way these different uses and criteria come produced an alternative meaning of EMH within the legal field. This is an interesting example of the idea that models are not only descriptive but used to perform acts in different fields.
A model can also be considered as a standard in the strict scientific framework, while not being used in public policies, or *vice versa*.

Considering models as taking place in and shaping different and partly autonomous spheres implies a sort of disconnection between models and reality that could lead to see these models as pure fictions close to what Baudrillard calls “hyperreality”: they would be models without origin or reality (Baudrillard, 1994). Baudrillard’s hyperreality has been recently applied to financial models (McGoud, 1997; Muniesa, 2014; Schinckus, 2016; Jovanovic, 2017): there would be no relation between financial model and a pre-existing reality. This idea is reminiscent to the so-called “performativity of economics” thesis, except we are here in an extreme version of it, where the reality is no more “shaped” by theory, but is replaced by it.

Today, abstraction is no longer the abstraction of the map, of the double, of the mirror or of the concept. Simulation is no longer about a territory, about a referential being, about a substance. It is a model-generated real with no origin and no reality: hyperreal. The territory does not precede the map – *precession of simulacra* – it is the map that engender the territory. (Baudrillard, quoted by Minuesa, 2014: 20)

Taking the case of Efficient Market Hypothesis, Jovanovic comes to claim:

[T]he EMH hyper-reality has replaced the real financial markets, which are not accessible anymore. In this perspective, this hyper-reality is not plainly compatible with the idea of performativity because it is not the real financial markets that are shaped. (Jovanovic, 2017: 7)

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8 An interesting case is the political use of the Keynesian multiplier in the context of the last French presidential election. *La France Insoumise*, represented by his candidate M. Jean-Luc Mélenchon, put forward the concept of multiplier in order to convince electors of the benefits of an economic recovery policy that would momentarily increase the public dept. A concept attached to a simple model became central in the political field, while remaining a heterodox within the economic academic field.)
Let us add that to claim that the real financial markets are “not accessible anymore” also means this was not always so. To come back to classic sources, since Hume’s problem of induction (Hume, 1740) and Kant’s criticism (1781) the possibility of a direct accessibility of the external world is one of the main issues of the philosophy. If the world is never directly accessible, this means, on the one hand, that our knowledge cannot reflect the world, on the other hand, that our world is per se made of representation. Nevertheless, this is not to say our models (in a general or in a specific meaning of “economic models”) have nothing to do with such a thing as an epistemically objective external world made of ontologically subjective representations (Searle, 1995; Brisset, 2016). Following the pragmatist tradition (Peirce, 1878), we know things through practices. So we get some knowledge with models when we use them within an external world made of epistemically objective representations. As a consequence, the position I support about models do not necessarily lead to a pure relativist stance (Brisset, 2017).

We define a model as a reasoning tool which gives birth to representations (or denotations) of the world. From a model making perspective, we follow Boumans (1999, 2005) who sees models as recipes mobilizing formal, empirical, theoretical and stylistic ingredients. What does the modeler do? As a first approximation, she chooses ingredients of varied origins, and assembles them in a certain way, using certain narratives (Morgan, 2012). We can consider the types of illocutionary intention to explain the choice of these elements and their
agglomeration in specific ways. These illocutionary intentions are not unique since they will take place in several communities. For instance, the following structure is possible. A model is first appraised in the scientific field against epistemological criteria that define what should be considered to be scientific. Then, it is judged in the academic field. By academic judgment, I mean judgment of the interest in the object denoted by the model. The model can also be considered a practical posture: is the model convenient in the context of strategic decision-making? Then, a model may need to be construed from a political point of view, as a useful representation for political decision-making.

This general framework highlights two elements. The first is the distinction made above. If a model is built with the aim of positioning it in a field, it must be understood that following Bourdieu’s approach, the positioning has a dual status. Positioning tries to respect the specific criteria in a particular field. However, a specific position in a field allows agents to change that field’s symbolic criteria.

The second element concerns the relationship between various fields. It should be emphasized that the recognition of an economic model in a specific field often rests on its acknowledgment by other fields. A model not initially recognized as being scientific rarely allows its author any academic recognition. This allows us to return to the distinction between academic and scientific. By scientific field, I mean the community that recognizes a model or a theory as scientific, as worthy of being published. However, in the academic field the modeler receives symbolic remuneration: the modeler is worthy of attention, of
being cited, and is recognized as the author of a model against which everyone else must position themselves. It provides its author with the legitimacy to appraise other models. In other words, references to the model are necessary for academic symbolic recognition.

These two elements have important consequences for our analysis of modeling: a good positioning in a field $x$ can be an important resource to get a good position in another field $y$ and to change the $y$ field's symbolic criteria. Such an inter-field dynamics resting on multi-positioning (Boltanski, 1973) constitutes a space for strategic use of the positions in different communities: a social field is no longer a simple set of rules (felicity conditions) that modelers have to follow; it becomes a resource they can use (Bourdieu, 1977). This avoids Garfinkel’s “cultural dope”, defined as “the man-in-the-sociologist’s society who produces the stable features of the society by acting in compliance with pre-established and legitimate alternatives of action that the common culture provides” (Garfinkel, 1967: 68).

In this theoretical framework, this paper argues that the illocutionary intention behind a model, what we do when modeling, may be understood in terms of how the different elements (ingredients) of a model are mobilized in order to do things in various fields. In the succeeding sections, I illustrate the relevance of my framework for the case of the financial model performance in the 1960s and 1970s that has been much discussed by historians of economic thought and sociologists.
V. The Context of the Emergence of Financial Models

A performance always takes place in a specific context. This paper is an attempt to illustrate the necessity of a pragmatic view of economic models. By “pragmatic”, I mean a quasi-sociological investigation on model building. In this perspective, I mobilize the concept of “field” in order to follow the different kinds of rationality implied in the process of model making. As a consequence, the contextualization of modeling is an important (if not essential) step of my approach. I will first emphasize the context of elaboration of financial models. In accordance with the framework exposed in the previous section, by “context” I mean a set of various fields in which some specific models will perform various acts in positioning themselves in those fields. There are three kinds of rationality behind the financial models (academic/scientific; political; practical), and these three kinds of rationality take place in three social fields. Each of these fields contained specific rules (“conditions of felicity”) and dynamics that are the fruits of history.

5.1. The Political Field

The 1929 crisis left deep stigma on finance and financial markets in the United States. This stigma implied the need for more regulation which was accomplished mainly through the Securities Exchange Act of 1934, a symbolic
act of the Roosevelt administration to control the financial sector through the creation of the Securities and Exchange Commission (SEC). The creation of the SEC called into question the authority of the Boards of Trade (BOT) concerning regulation of financial businesses. The case of the Chicago BOT is a well documented example. Created in 1848 by private actors in order to provide a framework for the development of trade in cereal products, in 1859 the Chicago BOT received political legitimacy from the Governor of Illinois which ensured that the BOT could enforce its decisions. In practice, this meant that decisions made by the BOT had legal value. Development of the BOT went hand in hand with the aim of maintaining the credibility and independence established in 1859. Inevitably, the creation of the SEC undermined this independence. The responsibility of derivatives for the onset and worsening of the financial crisis seemed sufficiently clear to warrant fairly strict supervision of their use⁹.

The next enforcement of this context of widespread distrust vis-à-vis the financial sector was the famous attack from Alfred Cowles. Heavily affected by the 1929 crisis, Cowles directly questioned the ability of investors to predict price moves. In a 1933 article provocatively entitled “Do Stock Market Forecasters Forecast?” he questioned the predictive capacity of institutional investors (insurance companies), professional financial advisors and financial journalists¹⁰. Cowles observed the performances of these actors but tried also to

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⁹ Although futures appeared very early in the history of BOT (1865), options (known as privileges in the second half of the 19th century) were quickly banned. Their exchanges were not supervised by the BOT and they were banned because of their excessive proximity to gamble. The 1929 crisis confirmed the already present suspicions.

¹⁰ Cowles launched a frontal attack on William Peter Hamilton, editor of the highly respected Wall Street Journal
understand whether their performance was the result of skill or chance (Cowles, 1933, 1944, 1960; Walter, 1999; Dimand, 2009; Dimand and Veloce, 2010). Although Cowles acknowledge some gains in his later work, they were too small in his view to justify the brokerage fees. One of the consequences of this representation was the loss of investors’ credibility regarding their ability to make profit. Such a loss was formalized in 1958, the CES commissioned a report from the Securities Research Unit at the Wharton School of Finance and Commerce of the University of Pennsylvania. This report, submitted in 1962, was the basis for the Investment Company Amendments Act of 1967, which proposed regulation of investors' remuneration.

5.2. The Practical Field

The second important consequence of the 1929 crisis was the devaluing of the profession of trader. The figure of the gambler stands out as social marquing (de Goede, 2005). Trader’s profession and practice was built around a set of rules of prediction well illustrated by what is known as the Dow Theory, after Charles H. Dow, founder with Edward Jones and Charles Bergstresser, of Dow Jones & Co and the Wall Street Journal. The Dow theory is relatively simple to grasp. It consists mainly of a classification of specifics patterns of price movements. The goal is to make rational speculation without denying the

11 The Dow theory corpus consisted of Dow's editorials in the Wall Street Journal and especially, those written by Hamilton in The Stock Market Barometer (1922), and Robert Rhea's The Dow Theory (1932).
importance of practical knowledge. Rhea (1932) defends that despite the specific theoretical tools it provides to identify market motions, nothing can replace the eye sharpened by daily practice. It is a matter of how things look since each stock movement can be understood as the result of a long, medium or short-term trend. Ultimately, Rhea's stated aim was to defend the legitimacy of speculation.

Yet it is precisely this attempt of legitimization that Alfred Cowles attacked through the performance evaluation of professional investors. Cowles's criticism was harmful to the professionals and defenders of the financial markets. He said that the low returns were related to an intrinsic characteristic of the market, namely the randomness of stock price fluctuations. Although the idea that prices follow the shape of a random walk dates back to the work of Jules Regnault (1863), the 1930s saw a resurgence of this notion following Cowles's empirical work and that of Holbrook Working (1934). This idea was in perfect elective affinity to the image of the traders as players in a casino. The courses had the same characteristics as the dice: every shot was uncorrelated from the previous one, and there is no trend to help to ensure a gain. The basis of the separation between speculation and betting disappeared: randomness was fatal to Rhea's idea that traders created trends by integrating information on the intrinsic value.
The 1929 crisis and Cowles's attacks began to diffuse the idea that asset prices behaved like dice: they followed random trajectories, incompatible with chartists’ practices\(^{12}\).

5.3. Scientific and Academic Fields

In the 1910s, the economic literature did not treat financial markets as theoretical objects. If the situation had changed in the 1930s (Preda, 2007, 54), economic studies of finance remained mainly descriptive until the mid-1960 (Fourcade and Khurana, 2013). A look on the same literature after 1973 (the date of publication of the BSM model) shows that theoretical finance became a model-based discipline, well established in the academic landscape: Markowitz, Miller, Sharpe, Merton and Scholes had been awarded a Nobel Prize in Economics.

Between the 1950s and the 1970s, theoretical finance acquired its pedigree (MacKenzie, 2006, chap. 6; Jovanovic, 2009b). When in 1954, Harry Markowitz defended his dissertation, Milton Friedman famously said:

I’ve read your dissertation and can’t find any mistakes in it. There is just one problem: this is not a dissertation in economics. We cannot award you a Ph.D. in economics for a dissertation that is not economics. (Markowitz, 1991: 382).

\(^{12}\) Nevertheless, it is important to note that in spite of this loss of legitimacy regarding the regulatory authorities, technical analysts maintained their authority within the markets, as deeply analyzed by Preda (2007), who shows how technical analysis was in position of ‘epistemic monopoly’ (Preda, 2007, 57).
The road was long to acceptance of financial modeling as a research field – a situation that contrasts with the Nobel prizes awarded to the founders of modern finance.

This context of hostility saw the emergence of modern financial modeling. Three essential dates can be identified, corresponding to four publications and three models: Harry Markowitz’s paper 'Portfolio Selection' (1952), 'Capital asset prices: A theory of market equilibrium under the conditions of risk' by William Sharpe (1964) which outlined the famous capital asset pricing model (CAPM), and the articles by Fisher Black and Myron Scholes (1973) and Robert Merton (1973), which proposed development of what would become the Black-Scholes-Merton (BSM) model.

VI. Financial Models as Speech Acts

There is a dual timeframe: firstly the emergence of financial modeling, secondly, the development of financial markets. It is well known that financial theory is a powerful vector of the social and political acceptance of markets (MacKenzie, 2006). It is important to emphasize three positions financial theorists assumed in their models: an academic position, a political position and
a practical one. It is through these positioning that the symbolic revolution that occurred in the years 1960-1970 can be understood. Financial markets defenders have reconstituted the legitimacy of markets by using the symbolic resource of economic theory (Jovanovic, 2008). We saw interventions from great names in economic theory such as Paul Samuelson, Eugene Fama and William Baumol who were not however, the authors of the above mentioned models. Indeed these well known economists served the cause of the finance theorists (Scholes, Merton, Black, Sharpe, etc.), both in the theoretical struggle that pitted supporters of technical analysis (the proponents of the Dow theory) and their desire to become economists.

To the question "what acts theoreticians of finance performed with their models", there are several answers:

- They attempt to integrate particular scientific and academic fields.
- They take a position in the analysis of stock movements against technical analysts (practical field).
- They participate in the recapture of symbolic power from the defenders of derivatives markets (political field).

These acts are linked, insofar as it is precisely the fact of integrating economic theory’s types of reasoning (thinking in terms of efficiency and equilibrium) that legitimates scientific acceptance and an academic and scientific position (Jovanovic, 2002), and allows defenders of the financial markets to use the new
financial theory to level the credibility deficit inherited from the crisis of 1929.
In other words, the performative act of creation of the Chicago derivatives was
made possible by a re-legitimating of financial markets. Thus, there is a
succession of felicity conditions implemented in different fields. Note that
sometimes these actions are contrary to the intentionality of the actors.
Although the BSM model was a decisive factor in the opening of the CBOE, this
was clearly not the intention of Fischer Black who in the 1970s held a negative
image of options.

6.1. Integrating the scientific and academic fields.

Markowitz's article “Portfolio Selection” (1952), is to modern financial theory
what the two small papers by Friedrich Ludwig Gottlob Frege written in 1892
and 1918 were to the analytic philosophy of language. Markowitz's article
defines the conceptual contours from which all financial reflection must start.
Suffice it to recall that all these papers were the source of three key ideas:

(1) Securities are individually characterized by an expected return and risk ,
i.e. the variance of returns.

(2) Agents have preferences in terms of mean / variance of returns of
securities.

(3) It is possible to construct a set of optimal portfolios based on these
preferences (minimum risk for given returns).
This approach, based on the tradeoff between risk and expected gain, combines prescriptive and descriptive considerations. Markowitz began his 1952 paper with a critique of standard approaches to financial behavior oriented mainly to the idea that agents behave in the market only to maximize expected gains. He quotes the book by John Burr Williams, *The Theory of Investment Value* (1938), and also *Capital Value* by John Hicks (1939). For Markowitz, expected gain maximization does not account for the type of behavior that occurs in markets, namely diversification. The theoretical references Markowitz quotes have some specificities. Burr Williams was among the first in United States to introduce modeling to analyze finance, and particularly the link between price and fundamental value. Hicks’s contributions were quite clearly outside the financial scope at this time. 

Markowitz positions his model within the field of economic theory\(^\text{13}\). The story of the development of this idea to the standard financial model is well known. Implementation of the Markowitz model is contained in the contribution of Tobin (1958) and his *Separation Theorem*. This reduces the agents' choice to a simple choice between a risk-free asset and a portfolio replicating the market. The second step in the operationalization of Markowitz’s model followed naturally with the CAPM, paving the way to index management. Sharpe (1964) argued that it was possible to differentiate for each asset a particular risk, and a systemic risk (i.e. the risk affecting all securities). The first

\(^{13}\) This point was also raised by Andrew Donald Roy (1952), who is today recognized as the forgotten co-inventor of portfolio theory (Sullivan, 2011, Dimand, 2009; Markowitz, 1999). Without entering in the details, even if Roy’s conclusions are the same as Markowitz, the kind of behaviors he considered, the “safety first” principle, is different See also Telser (1955-1956).
risk is diversifiable insofar as it is possible to compose a portfolio of securities such that the singular price fluctuations balance each other. The second risk is not since it affects all securities. So, it is always possible to cover a position constituting a diversified portfolio, so that it reduces the risk to the systemic component. Based on this idea, Sharpe proposed the CAPM which has become the cornerstone of modern financial management. CAPM simplified and systematized Markowitz's (1952) and Tobin's (1958) work, by identifying an "optimal portfolio", i.e. a portfolio of securities for which it is impossible, for the same level of profitability, to identify a less risky portfolio. Minimizing risk, i.e. volatility approximated by the variance, leads ultimately to composition of the market portfolio: the safest strategy to compose a portfolio replicating the composition of all of the market. This idea is the basis for the contemporary approach of dominant asset management, namely index management: the safest way to manage capital is to diversify its funds across the entire market.

The main ingredient in this model is the hypothesis of Brownian motion. Therefore, there is no foreseeable tendency (contrary to what the Dow theory supposes). This idea is an important pillar of diversification: if there is no trend and no predictability, the concentration of capital in a small number of stocks is riskier. The use of log-normal law is the second pillar. Ultimately, the concept of an optimal portfolio allows a connection between two seemingly contradictory concepts: rationality and randomness. An optimal portfolio is the result of a rational choice in a financial market whose prices follow a Brownian motion.
Of course, the very idea of Brownian motion was hotly debated by market practitioners. The situation was delicate for the chartists. On the one hand, the notion of randomness challenged their practice (Cowles). On the other hand, an emerging literature was proposing incorporation of the notion of random walk in financial practices. This approach theoretically led to the end of active management in favor of passive management. At this point, the economists' intervention was decisive. Research groups were set up, including MIT and the University of Chicago, to conduct further research on three levels (Whitley, 1986; Jovanovic, 2009; Fourcade and Khurana, 2013): intensification of econometric research, improvement of random walk theory and research into the theoretical foundations of randomness. From an econometric point of view, randomness became an established fact. The problem with Brownian motion was its lack of economic foundations (Working, 1956): what is the economic significance of the randomness? Does it mean that investors act without taking account of other transactions? An implausible suggestion. Two theoretical elements filled this gap: martingale, and informational efficiency.

The concept of martingale, which comes from the work of Samuelson (1965b) and Mandelbrot (1966), intimately binds randomness and information: courses are random insofar as the information does not indicate that it will change in the next period. The most common argument used at that time was the argument of arbitrage. Samuelson expresses it with great simplicity: if the available information on the market shows a price increase, then it will already
have exceeded the expectations of speculators (Samuelson, 1965b)\(^\text{14}\). As Walter (1996) and Jovanovic (2009: 67) highlight, the mid-1960s marked a turning point: the link between the random variations in stock’s prices, the concept of the market equilibrium, and the integration of information into prices was gradually achieved. Indeed, while the random walk seemed to destabilize the classical results of economics, Samuelson reaction was to incorporate this concept in the standard approach. This connection became the heart of the theory of informational efficiency (Fama 1965a; 1970): the link between financial theory and economic theory equilibrium gradually emerged, to the extent that the prices of securities are rooted in market values (Jovanovic, 2009: 67). It is interesting to recall that this was not the goal of many contributing authors to the vision in terms of random walk. Samuelson’s case is in the spirit of the "surplus of meaning". Samuelson’s work was used to connect informational efficiency and random prices. Nevertheless, this connection was rejected by Samuelson himself (Samuelson, 1965b; Jovanovic, 2009). This led him to distance himself from the Chicago finance’s liberal stance:

> One should not read too much into the established theorem. It does not prove that actual competitive markets work well. It does not say that speculation is a good thing or that randomness of price changes would be a good thing. It does not prove that anyone who makes money in speculation is ipso facto deserving of the gain or even that he has

\(^{14}\) Cootner (1962) had developed a similar argument.
accomplished something good for society or for anyone but himself. All or none of these may be true, but that would require a different investigation. (Samuelson, 1965b, p. 48)\textsuperscript{15.}

It seems clear that the economic justification for randomness was a determining factor in the acceptance of a set of financial models. Proponents of modern finance developed models using economics components and based on an assumption (randomness) that had been justified theoretically through positioning these models within a specific scientific field. This justification made financial theorists scientifically and academically credible.

6.2. Symbolic revolution and market building.

In the previous section we emphasized the act of legitimation of theoretical finance as a scientific discipline to enable a credible interlocutor to challenge an economist. This legitimation process followed a particular path and came to link some econometric results (successive price changes are random and unpredictable) to a particular theoretical perspective (general equilibrium). This connection was made by using various elements (Brownian motion, martingale), at the expense of another financial market analysis practice - technical analysis.

As noted in the first part of this text, it is possible to do several things in different fields with the same set of ingredients connected in a model. We now

\textsuperscript{15} In a subsequent article he demonstrates the possibility of a martingale model being consistent with the fact that some agents make systematic profits (Samuelson, 1973).
examine the performance of major contemporary finance models in the political field. As we noted already, in the early 1930s following the 1929 crisis, there were two visions opposing visions of financial markets in the context of debate over the ability to predict market trends. Cowles (1933, 1944, 1960); Working (1934, 1956) and later Kendall (1953), Roberts (1959) and Cootner (1962) defended a random vision, prohibiting forecasts and assimilating recognition of trends in courses to pure illusions.

Rhea (1932, 1933), and proponents of technical analysis, defended a rational and predictable image of market prices. As we have seen, the idea of randomness contributed to the bad reputation of financial markets. In the 1960s, advocates of financial markets lacked symbolic capital. Relegated to the rank of irrational players, they struggled to defend the opening of new markets for innovative products (options, warrants). The investment industry, represented by the Investment Company Institute (ICI), responded to the 1967 Investment Company Amendments Act, and employed the consulting firm Arthur D. Little (ADL). ADL has been the heart of the rebuilding of credibility of speculative markets through a strong symbolic reversal, initiated by the finance theorists involved directly or indirectly in ADL: while the very idea of a random walk seemed to be in opposition with analysts’ view, financial models were showing that it was a condition of the scientific and effective management of financial assets. Traders, considered to be irrational players, acquired the status of cold calculators. Fischer Black had occupied a position in ADL since 1965. His work focused on portfolio choices at a time when many researchers were following in
the footsteps of Harry Markowitz, and were diverting from the active art of the Dow theory. Black’s thinking included the issue of assessing investor performance. In 1967 he organized a meeting involving a young economist from Chicago, Michael Jensen, and William Sharpe. Both developed performance evaluation models of investment funds which took account of their risk exposure. This meeting was an important milestone. It was indicative of the need for a change of perspective among the defenders of financial markets. The results of Jensen's work are clear. He confirmed the attacks made since Cowles which necessarily upset the report's sponsors who demanded Jensen’s results be deleted from the final report. Jensen was still far from Cowles’s vision. As discussed in the previous section, according to the arbitrage argument, it is precisely because analysts do their job, taking advantage of the differences in what they consider as fundamental value, that they can smooth divergences.

The debate on the regulation of remuneration allowed a connection between the defenders of markets and the proponents of efficiency who became theorists of finance. This concept is crucial in that it binds together unpredictability, "rationality" of markets and technical analysis leaving no need to oppose the random walk and technical approaches since speculators contribute to market price movements and unpredictable, random, and efficient moves. It is significant also that in his first theoretical model, Fama (1965a) assumes the existence of traders able to analyze the intrinsic value of courses, and to read the graphs correctly. These agents might beat the market while making prices walk
randomly. Thus, the defenders of derivative markets now had the opportunity to adopt the randomness worldview of the regulatory authorities, while defending market efficiency. The argument of arbitration is a key narrative (Morgan, 2012) that re-legitimated the financial markets.

In Bourdieu's view (2015: 316), Cowles, Kendall and Roberts questioned the very meaning of speculative game, indicating that in part markets were random. The speculative game itself was challenged by Cowles as no longer ensuring profitability for everyone. This was not only because it generated financial crises but also because speculators were unable to justify their remuneration. Rhea’s argument was aimed precisely at trying to remake the speculative game from a negative zero-sum game to a positive sum game. This quest for the common welfare became a condition for access to the political field: in front of an increasing regulation governing financial practices the claim that speculation was not only a rational activity but one of general interest, was to reverse the trend:

To enter in the political game, the legitimate one, is similar to access to the resource one call “the universal”, to the universal speech, to the universal position in which we can talk on behalf of everybody (...) we can talk in the name of the public welfare. (Bourdieu, 2012: 162, our translation)

The reconstruction of symbolic capital to counter the heavy regulations on building new markets for derivative products was carried out in two stages. The first, as we have seen, was to domesticate theoretical randomness by making it the counterpart of market efficiency. This argument allowed acceptance of the qualification of financial markets inherited from the 1929 crisis, and the
requalification that randomness was not the result of the "luck of the devil" but a sign that markets are always in equilibrium. This is the apparatus of economic modeling which accompanied nascent financial theory.

The second step consisted of showing that this unpredictability was synonymous with efficient financial management: it recategorized traders. Fama's argument linked martingale to speculators' ability to erase market anomalies. To this initial indication he added implementation of financial models involved in the emergence of new portfolio management which opened the way to passive portfolio management. Efficiency and hedging rhetoric was instrumental to the formation of the CBOE. When in 1968 the leaders of the Chicago Board of Trade considered creation of the CBOE, they understood the importance of that rhetoric. Legitimization of options on securities, banned by the Commodity Exchange Act of 1936, must pass through the discourse of efficiency and of rational management (MacKenzie and Millo, 2003). Fischer Black, Myron Scholes and Robert Merton extended this analysis to the case of options. The idea of their model was to provide a way to evaluate the price of options by extending the principle of hedging: the price of an option (call or put) must be equivalent to the cost of hedging the position of the option in question via a replica portfolio where any difference between these two prices would be absorbed by arbitrage behavior consisting of buying the cheapest and sell short undervalued underlying options. Here again, we see the pacification markets through arbitration: speculators take advantage of market anomalies and restore the "right" price for the option. The scope of the argument is
relatively ambiguous. Like any theory based ultimately on the rationality of agents, it can assume both a descriptive and prescriptive status. From a descriptive point of view, the BSM model served to qualify options markets. As Fourcade and Khurana (2013, 149) say, ‘the mastery of the language and techniques of financial economics soon became an indispensable credentialing device not only for finance professors but also for practitioners in the financial markets.’

In the late 1960s a committee was organized around the CBOT to assess the impact of the introduction of market options. The committee was led by James Lorie, founder of the Center for Research in Security Prices (attached to the University of Chicago and composed mostly of efficiency advocates). CBOT then hired Robert Nathan Associates Services (NAS). The Chicago economist Merton Miller acted as an intermediary between the RNA and the innovative ideas proposed by Black and Scholes in an article argument first put forward in 1969. Black joined the University of Chicago in 1971-1972, and Merton Miller actively supported publication of his article in the *Journal of Political Economy*. We now approach the normative use of the model. The report also contained a crucial contribution from William Baumol which emphasized the public interest in the possibility of options trading (MacKenzie and Millo, 2003). Following the report provided to the SEC by the RNA, permission was granted for the establishment of the CBOE on April 26, 1973. Of course, the report was not the only trigger. The SEC had been aware of the kind of arguments put forward in NAS’s report, mostly because Nixon appointed William Casey as head of the
SEC. My point here is not to argue that new financial modeling is entirely responsible for the skewness of financial markets but rather that we cannot understand financial modeling if we ignore the speech-act that modelers intended to perform through their models.

The defenders of CBOT were obliged to adopt the language of the regulatory authorities: the language of the unpredictability. The idea was for passive and safe management. Thus, work on accumulation of symbolic capital by economic theorists began. This made credible the idea that randomness was not synonymous with chaos. Conditions were met for a performative act: the creation of an options market could be successful. However, the model can be considered also as prescriptive of good management practices. Again the question of intentionality and meaning illocutory surplus arises. For example, Fischer Black attended a seminar at the Center for Research in Security Prices where he appeared to be skeptical about options on markets. He went so far as to call them casinos! However, once the CBOE was created, he used the BSM model to produce decision-making tools to pacify this market, to lead it to efficiency (Mehrling, 2012: 138 and 168-9).

VII. Concluding remarks

My objective in this work was to extend the pragmatic view of economic modeling proposed by Morgan (2012) and Hédoin (2012). Their ways of considering modeling as a scientific and creative process in specific
epistemological contexts sheds light on elements often hidden by more essentialist views. Instead of asking “what is a model”, the open question of “what do modelers do while modeling” seems more productive. The *magnum opus* by Mary Morgan is proof of what that kind of method brings to the debate. The goal of my contribution was to show that investigating models as speech acts necessitates consideration of the several fields in which economists act through their models: scientific, academic, practical, political. The rich literature on the making of financial theory after WWII provided a useful foundation to evaluate the potentiality of this perspective. Of course, more space is required, especially to address the relation between the performativity of models (models as speech acts) and the validity of these same models. This issue is partly addressed in Brisset (2014; 2016; 2017). In addition, it would be useful not only to study great models in the history of economic thought but also to study actual and contemporary processes of model making from below up.

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