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ABSTRACT ECONOMIC MODELING: A SEMANTIC-PHILOSOPHICAL DEFINITION OF ECONOMIC MODELS

Porto Alegre
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Dissertação submetida ao Programa de Pós-Graduação em Economia da Faculdade de Ciências Econômicas da UFRGS, como requisito parcial para obtenção do título de Mestre em Economia.

Orientador: Prof. Dr. Marcelo Milan

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RESUMO

As fundações análogas das ideias de Kuhn e da visão pragmática das teorias favorecem uma união de pensamentos. Na concepção de Kuhn após a *Estrutura das Revoluções Científicas*, a filosofia da linguagem – especialmente as teorias de uso da linguagem – e suas ramificações nas ciências cognitivas são formas efetivas de julgar problemas científicos. Baseados nessas novas ideias, os interpretes de Kuhn propuseram a teoria psicológica dos Enquadramentos Dinâmicos como uma forma funcional de reavaliar a evolução científica. Uma aplicação dessa teoria para reler as definições pragmáticas de modelos foi realizada nessa dissertação, expondo a incomparabilidade entre estudos de caso, o que impede o avanço das discussões. Consequentemente, a criação de definições comparáveis é necessária para o desenvolvimento dos debates pragmáticos. Inspirada em Sugden (2000;2009), a solução proposta foi a criação de paradigmas plausíveis. Seguindo esta linha de raciocínio, um exame da história do pensamento econômico foi realizado buscando uma fundação crível para a definição de modelos econômicos abstratos. A pesquisa identificou os trabalhos de Tinbergen (1935) e de Von Neumann (1945) como os primeiros a usarem o termo ‘modelo’ em sentido abstrato e, portanto, como uma fundação sólida para um paradigma definidor do termo modelo econômico no período que transcorre de 1930 à 1950. Em seguida, a combinação da teoria dos Enquadramentos Dinâmicos e dos exemplares resultou na definição de modelos econômicos contendo cinco características: adaptabilidade, neutralidade, estrutura matemática, simplificação e objetivo. Uma avaliação subsequente da disseminação do termo de 1930 até 1950 sugere que os exemplares escolhidos são uma fundação plausível, ainda que a definição não tenha sido instantânea nem completamente disseminada entre os economistas.

Palavras-chave: Modelos. Thomas Kuhn. Enquadramentos dinâmicos. Paradigmas críveis. Filosofia da linguagem.

ABSTRACT

The analogous foundations of Kuhnian ideas and of The Pragmatic View of Theories favor a union of thoughts. In Kuhn's renewed ideas, philosophy of language – especially use theories - and its ramifications in cognitive sciences are an effective form of judging scientific conundrums. Based on this insight, Kuhn's interpreters proposed the psychological theory of Dynamic Frames as a functional form of reviewing scientific evolution. An application of Dynamic Frames was realized to reread pragmatic definitions of models, exposing the incomparability between case-studies, which hampers the development of discussions. Consequently, the creation of comparable definitions is necessary for the advancement of pragmatic debates. Inspired by Sugden (2002; 2009), the proposed solution was the creation of plausible paradigms. Following this mode of reasoning, an examination of history of economic thought was realized searching for a credible foundation for the definition of abstract economic models. The exploration suggested Tinbergen's (1935) and Von Neumann's (1945) works as the first ones to use the term "model" in an abstract sense and thus as a solid foundation for a paradigm intended to define economic models. The following combination of Dynamic Frames ideas and the exemplars resulted in a definition of models containing five characteristics: adaptability; neutrality; mathematical structure; simplification; and objective. A subsequent examination of the dissemination of the term from 1930s to 1950s suggested the exemplars were a plausible foundation, even though the definition was neither instantly nor completely disseminated among economists.

Keywords: Models. Thomas Kuhn. Dynamic frames. Credible paradigms. Philosophy of language.

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1 INTRODUCTION

Logical views of the philosophy of science were severely contested in the second half of the XXth century by two different strands. On the one hand, Thomas Kuhn opposed to naïve, non-historical interpretations of scientific evolution that understood science as a homogeneous and cumulative enterprise. On the other hand, the approach known as The Pragmatic View of Theories (PVT) confronted the philosophical explanations about scientific tools and behaviors which were too strongly based on logic. Case studies and closer observations of scientific practices were the foundations of both antitheses. However, even though their oppositions were effective in a myriad of ways, their foundations proved to be fragile. Kuhnian ideas created relativistic theories, compelling Kuhn to review his initial works. At the same time, the PVT embarked on frustrating debates.

From Kuhn's revisions emerged an important insight: scientific paradigms might be seen as language communities. Therefore, in Kuhn's renewed ideas, philosophy of language and its ramifications in the cognitive sciences are an effective way of judging scientific conundrums. Based on this thought, some of Kuhn's interpreters (ANDERSEN;BARKER;CHEN, 2006) proposed the psychological theory of Dynamic Frames as a functional method for reviewing the evolution of science, given that it represents a psychological surrogate for the definition of concepts in Kuhn's and Wittgenstein's views.

On the PVT side, the definitions of scientific models became a prolific research agenda. Under the guideline of suppressing logic, the exploration of case-studies became the foundation of their definitions. Morgan and Morrison (1999), Weisberg (2013) and Gibbard and Varian (1978), for instance, all define scientific models based on different arrays of case-studies. As a result, although thought-provoking, each definition is considerably different. The definitions are solidly organized on the basis ofdiscordant case-studies, creating unconnected conceptions, which are uncappable of describing *credibly* the scientific tools intended to be explained. Thus, the definition of scientific concepts – and of scientific models in special - are a frustrating task for pragmatic philosophers, considering their definitions cannot be compared.

Thus, whereas Kuhn and his interpreters have renewed their ideas, pragmatists persist with their unproductive discussions. Nonetheless, the analogous foundations of the Kuhnian and pragmatic approaches favor a review of the PVT in the light of Kuhn's reconsiderations. As a consequence, the present work has the objective of conciliating both views, especially presenting a review of pragmatic definitions of scientific models in economics. In reviewing the contemporary definitions, the work intends to rescue Kuhnian ideas related to the philosophy of language, present a pertinent critic of how the selection of exemplars should occur, select exemplars in a credible manner and, finally, present a definition for economic models. In summary, the work aims towards answering “What are economic models?” and thus, also, presenting a plausible philosophical mode of responding such question and why previous definitions were unhappy in their quests. Considering this task is a historical and philosophical research overlapping with psychology, an extensive analysis of the literature and some minor theoretical applications of Dynamic frames will guide the work.

Consequently, to accomplish the proposed objectives, after this brief introduction, the second chapter begins the analysis of the literature reviewing Kuhn’s ideas. His relativism and the problems related to the incommensurability thesis commence the examination. Thus, the chapter defines the theoretical basis for the rest of the work, presenting Kuhn’s reconsiderations and the connection with the theory of the Dynamic Frames.

In the sequence, the Kuhnian ideas are applied on a review of the PVT. The application of the Dynamic Frames to the analysis of the latter evinces the reasons behind the pragmatists’ frustrations: incomparability of particular case-studies. Consequently, the creation of comparable definitions is exposed as a necessary condition for the advancement of pragmatic debates. Inspired by Sugden (2002, 2009), the creation of plausible paradigms is proposed as a solution worth of further analysis. Plausible paradigms imply the selection of case-studies constrained by discipline and time. Therefore, while contemporary definitions are based on arbitrary selections of case-studies, the formulation of plausible paradigms would result in the selection of credible exemplars, excluding models in which scientists would not have relied upon as the foundation of their concepts and forcing philosophers to work on similar case-studies.

Following this mode of reasoning, an examination of the history of economic thought (HET) is realized in the third chapter, searching for a credible foundation for the definition of abstract economic models. The inquiry suggested that Tinbergen’s (1935) and Von Neumann’s (1945) works, although substantially different in what concerns their methodological roots, are the first works to use the term “model” in an abstract sense, and thus are a solid foundation for a paradigm.

Finally, the fifth chapter combines the ideas about the Dynamic Frames, the PVT and the proposed plausible selection of exemplars for defining economic models. The combination creates a definition of models containing five characteristics: adaptability; neutrality; mathematical structure; simplification; and objective. Each characteristic is then briefly discussed. A subsequent analysis of the dissemination of the term from the 1930s to the 1950s suggests that the exemplars were a probable foundation, even though the definition was not instantly or completely disseminated among economists.

2 RESCUING KUHN: FROM INCOMMENSURABILITY TO CONCEPT FORMATION

Normal science, anomaly, crisis, and revolution: Kuhn’s proposed scientific cycle has been read and reread numerous times. Thesis about his relativism and unrealism are abundant. Counterarguments exist in the same proportion. Regardless of the effort, it seems that Kuhn is fated to be interpreted as the founder of scientific relativism.

Nevertheless, reaching this post has never been Kuhn’s intention. After writing *The Structure of Scientific Revolutions,* Kuhn rejected his association with any relativistic interpretation, attempting at his best to deconstruct such views. Kuhn included a postscript to his piece, distanced himself from psychology, steepedinto philosophy, proposed new analogies, and repeatedly contended his neglect of relativism. Unfortunately for Kuhn, his efforts never reached the same audience as *The Structure*.

However, the lack of wide hearing does not connote inferior quality to his later works. On the contrary, the decreased visibility may be, indeed, a result of an enhanced foundation, which reduced controversy. In other words, uncontroversial statements demand minor attention. Although the reasons behind Kuhn’s later works reduced public interest are not clear, those will not be discussed henceforward. Still, the arguments themselves will be essential.

Therefore, Kuhn, apparently, needs to be rescued. His efforts at detaching himself from relativism cannot be condoned, since in them lie, if not a complete dismiss of relativism, an important theory of concept formation. Amidst his struggle for disconnecting from relativistic interpretations, Kuhn created this theory, as stated by Andersen, Barker, and Chen (2006).

Therefore, the main objective of the present chapter is to rescue Kuhn’s theory of concept formation, defining the theoretical basis of the following ideas. To accomplish such task, an explanation for the reasons why Kuhn received the label offounder of scientific relativism will be initially presented. In the sequence, section 2.2 will demonstrate the minutiae beneath the relativistic readings. Section 2.2 will be the base for the arguments in section 2.3 concerning the theory of concept formation. The fourth part, then, by looking into cognitive psychology, presents an operationalization of Kuhn’s latter theories. Finally, the last section offers some concluding remarks.

2.1 The decline of Logical positivism and the ascension of incommensurability

The debacle of the scientific enterprise marked the end of the XIXth century and the beginning of the XXth. Einstein's theories superseded Newtonian dogmas, and non-euclidean mathematics voraciously emerged. The certainty once disposedon science destabilized as a result of its new doddering steps. A new perspective relying upon a heterogeneous and nonlinear perspective started to overshadow the homogeneous and cumulative view of science. In order to rethink science philosophically, scientists and philosophers had, roughly speaking, two possible solutions. First, they could accept that science was, in fact, heterogeneous and unpredictable, and from then on rewrite their thinking. Second, they could deny that science was under pressure and, as a consequence, seek for a bond connecting disparate scientific ideas.

The Viena Circle was one of the most prominent groups of thinkers concerned with defining this bond identified as the second option. The circle was formed in the 1920s Viena – from which it received its appellation –, having as main exponents: Moritz Schlick, Hans Reichenbach, Otto Neurath, Phillip Frank, Hans Hahn, Herbert Feigl, and Rudolf Carnap. Their ideas can be found under different nomenclatures, such as "logical positivism", "logical empiricism", and "neopositivism"[1]. According to Brzechczyn (2009), the circle members were united in the objective of finding a scientific uniformity.

Curiously, the circle had direct relations with Einstein and Hilbert, the main authors behind science's cataclysm. Howards (2007) affirms that Viena Circle's ideas were molded by Einstein's theory, once Schlick and Frank had contact with the physicist, especially through correspondences. On the other hand, Stolzner (2002) highlights Frank and Hahn studied with Hilbert. Consequently, both had the capacity of bringing Hilbert's mathematical concepts for the debate. Yet, Majer (2002) contends that the circle's interpretation of Hilbert's theories was mostly inexact. Indifferently, the main point is that Logical empiricists were not overlooking the scientific changes at the time.

In fact, they were so in touch with the idea of philosophy's incompatibility with science that their essential argument was about transferring the philosophical view from theories to language. Therefore, language was the bond between heterogeneous scientific theories allowing the circle to maintain an ideal of science as a uniform activity. For Gattei:

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[1] According to Uebel (2013), although the three terms are not identical, no simple distinction is avaiable. Therefore, henceforward the terms will be used interchangeably. Still, for analyzing differences it is possible to consult Uebel (2013).

> […] not dealing anymore with particular scientific theories or with their contents, [neopositivism] is immune from the vicissitudes which trouble the scientific enterprise, and devotes itself only to defining the requirements which any scientific theory must meet. (GATTEI, 2008, p. 4).

In the circle's point of view, uniformity would follow the existence of a unique "observational language". This language is characterized by the fact that "we have a basic vocabulary made up of terms that derive their meaning directly from experience. [...], and the meanings of its terms are established independently of any of our beliefs – and a fortiori independently of any scientific theories." (BROWN, 2004, p. 150). Along these lines, members of the circle defended a logical scientific foundation, absent of any historical roots. In addition, the unified view permitted a cumulative comprehension of science. In a summary and sketchy evaluation, logical positivism accepted science as a cumulative, homogenous, and non-historical endeavor.

Yet, while the circle was being founded, other different philosophical interpretations of science were developing. Karl Popper, for instance, was a contemporaneous compatriot of the circle whose philosophical ideas, even though influenced by the circle, never became totally attuned to logical positivism. According to Naraniecki (2010), the similarities between Popper and the circle stand solely in their Kantian roots, since Popper was opposed to the essence of the Circle's arguments. Specifically, Popper's theories were antipodal to the primacy of observation and to the existence of a unique observational language. For Popper (1974), every observation is preceded by its theory, impeding the existence of an *a priori* language. Following this argument, Gattei (2008) asserts that "the primacy logical positivists attributed to observation data Popper confers to theory."

However, the similarities between Popper and the Circle may go beyond their Kantian roots. For Richardson (2007)"[...] the logical empiricists as a group all rejected a naive inductivism that claims that scientific theory can be both expressed in observational language and derived from observational results." (Richardson, 2007, p. 353). More specifically, when taken separately, the members of the Vienna Circle were skeptical about the existence of a unique observational language. In opposition to the sketchy image, in practice, distinct approaches formed the logical positivism. Carnap's thought, for example, is frequently presented in dissonance with the cumulative, homogeneous and non-historical image of neopositivism. Reisch (1991) and Creath (1995), on this subject, state that Carnap and Kuhn had significant overlaps in their mode of thinking, which would be impossible if the above image well defined Carnap.

In this regard, Richardson (2007) observes that a “sort of everyday image of logical positivism” exists. Attempts to unify the circle’s thought into one single definition distort their members’ individual ideas. Consequently, a naïve interpretation of neopositivists’ understanding of science as cumulative homogeneous and non-historical, is commonly shared. Yet, eventhough the image misrepresents the neopositivsts’ ideas, their intents are not. Behind their arguments was the purpose of finding a bond to unify all the scientific enterprises. As a consequence, the image ends describing solely their intentions, once their ideas were, in fact, unable to reach their purposes.

Nonetheless, this image played an important role in the progression of the philosophy of science. During the 1950s the image of neopositivsm was severely attacked. Opposing the proposed uniformity, emerged, as reported by Gattei (2008, p. 18), authors such as “Thomas Kuhn, Norwood Russell Hanson, Michael Polanyi, Stephen Toulmin and Paul Feyerabend”. All of them - in some way - accepted science as a heterogeneous endeavor, aligning philosophy of science with the first option and discarding the second. This new harmony between the philosophy of science and a heterogeneous image of science extinguished the “logical empiricism” in Creath (1995) point of view. For Richardson (2007), the main actor of the harmonization and consequent extinction was Thomas Kuhn:

> All these developments [Polanyi, Hanson, etc.] placed logical empiricism, more or less explicitly, in issue. All of them sought to go beyond logical empiricism in doctrine and method. All of them played a role in decreasing the dominance and, ultimately, influence of logical empiricism. But all of the philosophers and movements mentioned so far pale in significance when compared to Thomas Kuhn’s historical philosophy of science as presented in his 1962 monograph The Structure of Scientific Revolutions. (RICHARDSON, 2007, p. 348)

Kuhn’s attack rearranged the philosophy of science intentions, once it was aimed at the neopositivist’s sketchy perception. As Kuhn himself acknowledges: “[…] it was against that sort of everyday image of logical empiricism – I didn’t even think of it as logical empiricism for a while – it was that that I was reacting to when I saw my first examples of history” (KUHN, 2000, p. 306). Therefore, Kuhn was rebelling against the then current view of science as non-historical: “History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed.” (KUHN, 1996 [1962], p. 15). Unlike logical positivists, Kuhn viewed science as “a rather ramshackle structure with little coherence among its various parts

(KUHN, 1996, p. 49). This instability permitted not only a dissension with nonhistorical arguments but also with a cumulative understanding of science:

> In recent years, however, a few historians of science have been finding it more and more difficult to fulfill the functions that the concept of development-by-accumulation assigns to them. [...] Perhaps science does not develop by the accumulation of individual discoveries and inventions. (KUHN, 1996, p. 2).

Kuhn’s (1996) offensive can be summarized in the following manner. Members of some determined scientific group have an implicit consensus about fundamental scientific questions and how to approach them: a paradigm. Regular activities of those paradigm’s adherents are analogous to solving puzzles and are called “normal science”. The analogy is essential since it determines that normal science’s problems always present a solution. Thus, the incapacity of finding this solution discredits only the scientist. Yet, eventually “anomalies” arise amidst normal scientific conundrums. Those are characterizedas unreasonable problems. While anomalies are normally negligible, their importance may grow if they become recurrent. An intermittent anomaly affects more than one single scientist and, at some point, may affect the entire paradigm. In such case, the anomaly installs a scientific “crisis”. The solution to the crisis is to question the paradigmatic consensus and to discover a new set of solutions capable of solving the recurrent anomaly. For Kuhn (1996) this settlement of new exemplary solutions is a “scientific revolution”, in which one paradigm overcomes another.

As a result, homogeneity and cumulativeness became implausible in the light of his historical examples. By merely stating that science evolves through revolutions and paradigmatic changes, Kuhn was implicitly affirming that languages can change and uniformity is not a reality. Yet, Kuhn (1996) went further than just raising indirect criticisms. In consolidating his arguments, Kuhn presented incommensurability as his cardinal argument against the then existing philosophy of science. Based on the analogy of Gestalt Switches[2] and persuasive case studies, incommensurability determined that two paradigms were incapable of unequivocal communication, diametrically opposing the established view.

However, even though incommensurability represented a massive impact on the philosophy of science’s logical view, it was rapidly evinced that the argument was an

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[2] Kuhn used different examples of psychological experiments to represent the Idea of incommensurability, most of them cited on chapter X of The Structure of Scientific Revolutions (KUHN, 1995). However, the change of perception in the image of a duck/rabbit usually displays a useful analogy for Kuhn and his interpreters.

internecine offensive. On the one hand, incommensurability hampered any attempt to perceive science as a uniform and cumulative activity. On the other hand, incommensurability raised questions about the logic of scientific evolution. Gestalt Switches' analogy resulted in interpretations of incommunicability. Without communicability, scientists are unable to logically choose between arguments. Therefore, while Kuhn vanquished the simplistic perception of homogeneity in science, he also built a relativistic aura surrounding and discrediting his own arguments. Actually, relativism was never an objective of Kuhn's initial ideas, as he frequently affirmed throughout his career (KUHN, 1970a, 1970b, 1973, 1974, 1983, 1990).

This rejection is regrettably overlooked. Whereas *The Structure* is one of the most cited monographs in the history of social sciences (GREEN, 2016). Kuhn's review of his arguments has not received the same interest. Nevertheless, his reassessment of *The Structure* ideas is essential for two significant reasons. First, Kuhn deconstructs his relativism, or at least he tries to. Second, and this is the main point of the rest of the present work, when reviewing his thoughts and deconstructing his supposed relativism, Kuhn formulated a theory of concept formation in an ambiance of plural and heterogeneous ideas. This theory is interesting, since it integrates Kuhn's thought with different philosophical ideas, and has links to history and psychology, creating a possible multidisciplinary view of scientific concepts. The next section will look into *The Structure* for glimpses regarding his concept formation theory. Consequently, it will briefly review aspects of Kuhn's relativism and how he already had a basis for his future thoughts inside *The Structure*.

2.2 Kuhn's 'post-structure' foundations

Kuhn's attack at neopositivist's image of science occurs in the first edition of *The Structure of Scientific Revolutions.* Using the concepts presented in the former section, Kuhn was clear about three important aspects of incommensurability:

a) each paradigm corresponds to a different perception of the world;
b) the relation between two paradigms is analogous to the relation between gestalt switches;
c) a neutral language does not exist. He used historical examples to bolster his concepts and arguments, and through them deconstructed the previous image of science as cumulative, homogeneous, and nonhistorical.

Those same issues represent, thus, the debacle of the naïve scientific image and are the source of controversies. Hoyningen-Huene (1990) points out three different relativistic interpretations of Kuhn's arguments. First, inordinate reads of points one and two induced the interpretation that *all* characteristics of a paradigm change during a revolution. While this is a possibility, Kuhn never contended this was a necessity for a revolution. Second, extreme interpretations of the second point implied the depiction of abrupt revolutions. Therefore, those who took the analogy as a direct representation of a revolution, believed paradigms changed in an unforeseen manner. Third, similarly to both previous readings, interpretations of non-logical scientific choices were abundant, all based on Kuhn's assumption of the non-existence of a neutral language. All understandings have one common line of thought: incommunicability.

However, as it is going to be presented in the sequence, incommunicability was never an intended apprehension of his arguments for four main reasons:

a) Kuhn never treated directly about inter-paradigmatic dialogue in *The Structure*;
b) Kuhn resorted to unmanageable analogies;
c) Kuhn had space and philosophical restrictions;
d) Kuhn had incipient philosophical ideas.

These four points are essential since - although they neither absolve Kuhn of permitting relativistic interpretations nor confirm he was not relativist in *The Structure* - their post-Structure correction or hyperbole created Kuhn's theory of concept formation, as it is argued in the next sections. For now, a review of these four points is important as a substance for subsequent discussions.

To begin with, the first point may be summarized by Kuhn's attitude towards the following questions: how is communication possible between paradigms? How are arguments presented? Whether Kuhn noticed or not the presence of these questions hidden behind his ideas is an open query. Still, it is evident he never answered them during *The Structure*. His interest was directed to solving a related problem: "what causes the group [of scientists] to abandon one tradition of normal research in favor of another? (KUHN, 1996, p. 144); "How, then, are scientists brought to make this transposition?" (KUHN, 1996, p. 150); "We must therefore ask how conversionis induced and how resisted. (KUHN, 1996, p. 152). More specifically, Kuhn was worried about how arguments persuaded and not how they were presented.

When solving this different puzzle, Kuhn used controversial statements. In his opinion, paradigmatic choices eventually “lie outside the apparent sphere of science entirely” (KUHN, 1996, p. 152 - 153). He even affirmed that “a decision of that kind can only be made on faith” (KUHN, 1996, p. 158). Nevertheless, while he believed persuasion was beyond logic and communication, he did not defend those were impossible to be used in paradigmatic choices. In Kuhn’s defense, there is only left his indication that communicability was possible: “This is not to suggest that new paradigms triumph ultimately through some mystical aesthetic. On the contrary, very few men desert a tradition for these reasons alone” (KUHN, 1996, p. 158). Unfortunately for Kuhn, aspiring non-relativistic interpretations based on few unclear passages about the mere possibility of communication is not as solid as expecting such interpretations when directly discussing the problem. As a consequence, the lack of discussions about communicability allowed his arguments about persuasion to be distorted into incommunicability and relativism.

The second point is Kuhn’s lack of control over the gestalt switches’ analogy. His metaphor was only capable of describing world *perceptions*. As follows, gestalt switches had little or no capacity of presenting a good analogy for paradigms’ *representation*. Without representation, communicability is disabled, since communication is a relation between representation and perception. One agent represents a thought while the other perceives it.In addition, Hoyningen-Huene (1998) highlights the analogy was improper, considering that gestalt switches are an individual experience, while paradigms concern communities. In light of such flaws, Malone (1993) avers Kuhn’s relativism could be erased if references to his unfortunate analogy were excluded. As Barker (2001) points out: “The Idea of Gestalt switch and the illustrations in terms ofduck-rabbit figures were dramatic, and easy to understand, but misleading in crucial respects”. (BARKER, 2001, p. 437). The most misleading aspects of the analogy were its indication that revolutions were abrupt and radical changes in perception, while in Kuhn’s view this was a mere improbable possibility.

These two points undeniably lead to the third point: Kuhn had space and philosophical restrictions. Bird (2002; 2004), Sankey (1993), Gattei (2008) and Demir (2008) agree that Kuhn acquired philosophical knowledge gradually during his career. Kuhn had graduated in physics and thus did not have philosophical training. Under such circumstances, it is overt that Kuhn could not possibly argument in an eloquent philosophical manner at the beginning of his philosophical career. Therefore, as *The Structure* was one of his first philosophy pieces, it was probably built under unclear philosophical reasoning.

Remarkably, Kuhn was conscious about his limitations. The preface of *The Structure* was devoted to present autobiographical clarifications. Kuhn was transparent about his influences when writing about them, frankly acknowledging his claims were as governed by philosophy as by history and psychology. Furthermore, Kuhn confessed the need of condensation forcedhim to "foregodiscussion of a number ofmajor problems." (KUHN, 1996, p. 11). In fact, Kuhn even affirmed he had preferences for non-philosophical reasoning in *The Structure* due to its concise characteristic.

Whether the controversies around the analogy and the discussions about the absence of communicability are connected to the lack of space or to the lack of philosophical capacity is indifferent to the present work. The relevant point is the fact that Kuhn was aware of his limits regarding *The Structure*. As Kuhn (1996) admits when writing about revolutions: "We must therefore ask how conversionis induced and how resisted. What sort of answer to that question may we expect? […] *We shall have to settle for a very partial and impressionistic survey*" (KUHN, 1996, p. 251, italics added). Therefore, an implicit desire to extend his thoughts was hidden in his monograph, especially in the preface.

This extension, as Kuhn pointed out when confessing the inadequacy of his philosophical reasoning, would probably occur through the improvement of his understanding of philosophy. The fourth point thus indicates that Kuhn had incipient philosophical knowledge not necessarily connected to relativism. His future philosophical support would come from two different sources: Wittgenstein and Polanyi. The first one was cited in *The Structure*: "In the absence of a competent body of rules, what restricts the scientist to a particular normal-scientific tradition? [...]Partial answers to questions like these were developed by the late Ludwig Wittgenstein, though in a very different context." (KUHN, 1996, p. 44). Indeed, Wittgenstein's concept of *family resemblance* suits Kuhn's paradigms very well. For Kuhn, scientists sharing a paradigm do not have a set of rules governing their reasoning. Instead, they share an array of similarities, likewise Wittgenstein's *family resemblance*. These similarities are acquired through exhaustive analysis of recurrent problems and solutions – the next section will look more profoundly into this aspect. Furthermore, according to Jacobs (2002), Polanyi was probably an important foundation for Kuhn's discussions, notably because of the similarities between Kuhn's incommensurability and Polanyi's (1958) logical gap.

Summing up, Kuhn had several argumentation flaws permitting a relativistic interpretation. Yet, Kuhn never wished the title of relativism put onto his monograph. D'agostino (2013) hence contends Kuhn was "verballed – i.e. represented as saying things he actually denied" (D'AGOSTINO, 2013, p. 536). This dissonance between Kuhn's point of view and Kuhn's monograph implied that "Kuhn was […] constantly challenged to articulate and refine his thesis in greater detail, in the course of which his position also shifted in some important aspects." (HOYNINGEN-HUENE 1998, p. 6). As a consequence, starting with point three, time provided for Kuhn more space and more philosophical knowledge, allowing him to review *The Structure*. When reviewing it, Kuhn directly discussed communicability (first point), rejected and changed his initial analogy (point two), and enhanced his incipient philosophical arguments (point four). The aggregation of post-structure works, presented in the next section, creates a theory of concept formation.

2.3 Exemplars, Communicability, and Kuhn's 'post-structure' theories

After becoming circumscribed by the relativistic interpretations, Kuhn first important reevaluation of *The Structure* happened seven years after the first edition of the piece, when the author added a postscript to the monograph[3]. The four points highlighted previously were the source of confusion and did not have any power in preventing the incommunicability readings. Therefore, the postscript intention was to clarify some ideas and the strategy applied to expurgate relativism was to reevaluate the concept of paradigm. Starting with a review of the term, Kuhn tried to revise the gestalt switch's analogy and communicability.

Kuhn (2013 [1969]), then, asserts that communities of scientists share methods and knowledge through sharing a "disciplinary matrix". This matrix is characterized by four different aspects: symbolic generalizations, beliefs, values, and exemplars. Generalizations simply stand for all common logical symbolic expressions shared without controversy among the community of scientists. Beliefs, by their turn, are the problems and methods equally perceived by the scientists. Values are moral, ethical, and logical aspects of their modes of thinking: whether predictions have to be quantitative or not; whether science has to be socially relevant or not;etc.. Those three aspects, even though presented in Kuhn's argumentation, have a small significance in Kuhn's overall review.

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[3] In 1974 Kuhn also presented a work reviewing paradigms, very similar to his postscript. In the present section the postscript is prefered as a source of information, even though Kuhn (1974) is also used when necessary.

Exemplars are the most imperative aspect of a paradigm in his view, since they bolster his review of the communicability and the analogy in the postscript, as well as support the developments of the rest of his career. It is through the concept of exemplar that Kuhn can connect with other philosophical ideas and, consciously or unconsciously, build his theory of concept formation. Exemplars are previously successful solutions for paradigmatic problems used as examples for future puzzle solutions. Even though the concept has a simple definition, the underlying facets of exemplars, especially how they are shared and apprehended, is the foundation of Kuhn's review of incommensurability and thus of his theory of concept formation.

In favor of facilitating the understanding of the complexities and importance of the exemplars, Kuhn opted for the following example: Newtonian equation, f = mxa. The equation is embedded in meanings such as force, mass, and acceleration. According to Kuhn (1996 [1969]), apprehending the equation as an exemplar goes beyond the simple verbal comprehension. In Kuhn's words: exemplars cannot be understood "by exclusively verbal means. [...] nature and words are learned together" (KUHN, 1996 [1969], p. 191). Sharing and understanding exemplars is not simply to memorize a set of rules. An exemplaris so inculcated in the scientist that it is part of his natural mode of reasoning, permitting him to grasp any sign of similarity between the exemplarand normal scientific riddles. As Kuhn (1977 [1974]) states:

> […] where rules exist to guide him, he, of course, deploys them.But his basic criterion is a perception of similarity that is both logically and psychologically prior to any of the numerous criteria by which that same identification of similarity might have been made." (KUHN, 1974, p. 326) Therefore, the apprehension of an exemplar allows similarity recognition to be "fully systematic as the beating of our hearts. (KUHN, 1996, p. 194).

This naturalistic quality of exemplars is illustrated by Kuhn (1974): In a zoo, a child learns how to distinguish geese, swans, and ducks. In such an environment, the kid is recurrently presented to *exemplars* of the distinct species. His father helps him in the process, always indicating whether he is right or wrong in his definitions. After trying a few times, the kid starts to gain confidence in his ability to distinguish the species, since s/he acquired the capacity of noticing similarities among members of a same kind and dissimilarities among animals from different species. An interesting point, besides the child acquiring the ability through identification of exemplars, is the fact that the child and his father probably do not use the same characteristics for distinguishing the animals. Once there is no set of rules defining how to categorize the species, the child *naturally* acquired the *tacit* ability of discriminating.

Considering the example, Kuhn noticed that adherents of a same paradigm learn scientific concepts by an exhaustive exposition to exemplars, learning similarities and dissimilarities. Hence, affiliates of a same paradigm share a language as a result of sharing the same exemplars, even though not sharing necessarily the same set of methods for distinguishing them. Communication between adherents of a same paradigm is consequently possible. On the other hand, communication between different paradigms, as in different languages, is solely possible through translation and interpretation (KUHN, 1983). These new potential channels of communicability were Kuhn's way of deconstructing his previous relativism.

Therefore, Kuhn's approach to the zoo's child example is similar – if not equal – to Wittgenstein's family resemblance. Loosely remembering Wittgenstein's ideas, a concept is identified by a set of similarities, analogously as to how a member of a family is identified. In a family, it is impossible to determine an array of characteristics essential for its members, although it is possible to notice that all members share a set of characteristics that allow their recognition as members of it. Wittgenstein (1953) opts for the concept of "game" as an illustration of how family resemblance determines concepts: card games, board games, ball games, etc., all share similar characteristics, while an exact set of characteristics cannot be identified. For Wittgenstein (1953): "there's no better expression to characterize these similarities than 'family resemblance'" (WITTGENSTEIN, 1953, p. 32)

In fact, Kuhn noticed Wittgenstein's discussions of language and family resemblance, and also his own ideas of scientific concepts, describe non limited concepts of tacit knowledge. Along these lines, Kindi (1995) and Barker, Chen and Andersen (2003) understand Wittgenstein as one of the most important foundations of Kuhn's post-Structure works. Barker, Chen and Andersen even affirm:

> Kuhn first adopted Wittgenstein's notion of family resemblance in The Structure of Scientific Revolutions to argue that research problems are related by resemblance. But gradually Kuhn extended his argument to cover concepts in general, and in developing his account of concepts He gradually refined the treatment of family resemblance beyond the notion He had adopted from Wittgenstein (BARKER;CHEN;ANDERSEN, 2003, p. 214).

Consequently, based on Wittgenstein, language was the analogy chosen by Kuhn (1983) for overshadowing the previous gestalt switches' analogy. Thus, his new philosophical foundation allowed the formulation of the language analogy capable of dissolving relativistic interpretations resulting from incommunicability.

Andersen (2000) highlights Kuhn’s theory is different from Wittgenstein’s family resemblance in one important respect. Family resemblance is subject to being indefinitely extended, since there is always the possibility of finding new similarities among members of distinct families. According to Andersen (2000), Kuhn’s theory of concept formation does not share this problem. Unlike family resemblance, Kuhn’s theory implants a significant role to dissimilarities (ANDERSEN; BARKER; CHEN, 1996, 2006; BARKER, 2001, 2011, CHEN; ANDERSEN; BARKER, 1998). As a result, Kuhn’s theory hampers the possibility of concepts extending indefinitely.

Summarizing, Kuhn dissolves relativism by dissolving incommunicability through comparing paradigms with languages. Thiswas accomplished through deepening his philosophical ideas and thus identifying what scientists share among them that allows communicability. Kuhn noticed that members of a paradigm share the same exhaustive set of exemplars, which permits them to create similar concepts. This insight was reached when Kuhn corrected communicability (first point), rejected and changed his initial analogy (point two), and enhanced his incipient philosophical arguments (point four). Critically supporting these reviews was his Wittgenstein-based understanding of how scientific concepts are formed.

As a result, Kuhn’s reviewed ideas can be used as instruments for analyzing scientific specificities, unlike his previous notions that were only applicable to scientific evolution. Kuhn (1970a, 1970b, 1973, 1974, 1983, 1990) introduced the ideal of science as language and, therefore, the possibility of viewing scientific terms analogously to current vocabulary. As a consequence, in Kuhn’s revised point of view, philosophy of language and its ramifications in cognitive sciences and in the philosophy of mind can be applied to the study of science.

Unfortunately, although credible, Kuhn’s theory, in the same manner as Wittgenstein’s family resemblance, is extremely abstract. Consequently, the acceptation of their theories has a paltry capacity of helping to understand scientific concepts in reality. More categorically, Kuhn does not demonstrate how scientists choose their exemplars or how many exemplars do they choose. Furthermore, Kuhn does not present any method for operationalizing his theory. Therefore, his theory may be theoretically interesting and even possess the capacity of erasing relativism, but it has reduced practical utility. Fortunately, in the last decades, psychology –

dismissed in Kuhn’s post-structure theories – has been finding different forms of representing Wittgenstein’s and other types of concept description. The next section thus presents how Kuhn’s theory may be further rescued, since for now it has only been really saved from its relativistic interpretations, especially through the affirmation of the possibility of communicability.

2.4 Kuhn’s ‘post-structure’ theories and Dynamic frames

Before detailing Kuhn’s theory in the light of cognitive psychology, it is important to notice that advances towards such direction are not a consensus among philosophers of science and/or Kuhn’s interpreters. Thagard (2009), for instance, is skeptical about the capacity of psychology to help Kuhn’s theory. Bird (2004), on the other hand, believes that further developments of Kuhn’s naturalistic aspects, noticeably those related to psychology, is one important advance for Kuhn’s ideas. In any case, Kuhn’s theory of concept formations concerns *tacit* knowledge, what by definition is something that cannot be transcribed. Therefore, the following attempts of harmonizing Kuhn’s theory with psychology will eventually fail in realizing Kuhn’s more profound intentions. Yet, even though prone to failure, these attempts are significant as ways of turning abstract ideas into practical devices.

Then, according to Garbacz (2013), cognitive psychology, represented here by the theory of “dynamic frames”, is just an operationalization of Wittgenstein’s and Kuhn’s ideas, and not necessarily a perfect surrogate. Attempts of operationalizing concept formation are scarce in the philosophy literature, and dynamic frames are one of the few methods utilized for analyzing incommensurability and concept definition[4]. Moreover, psychological developments of Kuhn’s ideas would probably receive Kuhn’s endorsement, especially when comparing them to other developments such as the “absurd strong programme”. (KUHN, 1991).

Kuhn distanced himself from psychology in the 1970s, after his failed experience with the gestalt switches’ analogy. However, during this same period, psychology was evolving and presenting evidence that concepts were formed accordingly to Wittgenstein’s ideas.

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[4] Andersen, Barker e Chen, for instance, attempt to explain incommensurability from the point of view of Dynamic Frames (ANDERSEN; BARKER; CHEN, 1996, 2006; BARKER, 2001, 2011; CHEN; ANDERSEN; BARKER, 1998; CHEN, 1997). Their thoughts may be found in Andersen, Barker e Chen (2006). And a synthesis may be found in Barker (2011). Moreover, Garbacz (2013) uses Dynamic frames as a tool for rebuilding a concept, without a Kuhnian view.

Those evidences were surprising, given that until the 1970s concept formation in psychology was a result of a strict view, which defended that concepts were formed by the existence of a set of necessary and sufficient conditions. It was Eleanor Rosch's studies that provided evidence contrary to the established view and corroborated Wittgentein's theories (ROSCH, 1973; ROSCH; MERVIS, 1975).

Initially, Rosch's arguments fomented the development of prototypical concept formation theory. This theory understands prototypes – or exemplars in Kuhn's terminology – as the devices responsible for representing a set of expected, rather than necessary, characteristics for defining concepts. A simple illustration of prototypes may be seen in the case of birds. A parrot, for instance, is a better representation of the category "bird" than a chicken, because the first one flies, while the second does not. Even though a chicken can be part of the category bird, it cannot be a prototype. Therefore, an example fits into a category depending on its degree of similarity with the prototype's characteristics. Rosch viewed this characteristic as representing a "graded structure", since categorization depended on levels of similarity. Considering that objects can be inserted into a category even in the absence of some characteristics, graded structures are more closely related to family resemblance than the classic previous idea of Wittgenstein. (NERSSEISIAN, 2003)

However, in the 1990s a different theory of concept formation was developed based on Rosch's evidences. Barsalou (1987, 1992) proposed that dynamic frames form concepts. Unlike prototypes, dynamic frames create an interconnected chain of dependent concepts. As a result, concepts can be built by different degrees, given thatone concept can be formed by subordinate concepts. Remembering the child in the zoo, the concept of "water birds" can be formed through exhaustive presentation to the exemplars of geese, ducks, and swans; or, following dynamic frames, it can be formed by the subordinate concepts of "multicolored water birds" and "white water birds", the first using ducks and geese as exemplars and the second using swans. Moreover, besides this subordination insight, Barsalou states that concepts are created according to two different layers of characteristics: attributes and values. Values are specific characteristics of attributes. Barker, Chan and Andersen (2003) affirm: "a frame is a set of multivalued attributes integrated by structural connections" (BARKER; CHEN; ANDERSEN, 2003, p. 224). The following image – based on dynamic frames literature[5] – is conducive for understanding Barsalou's proposal:

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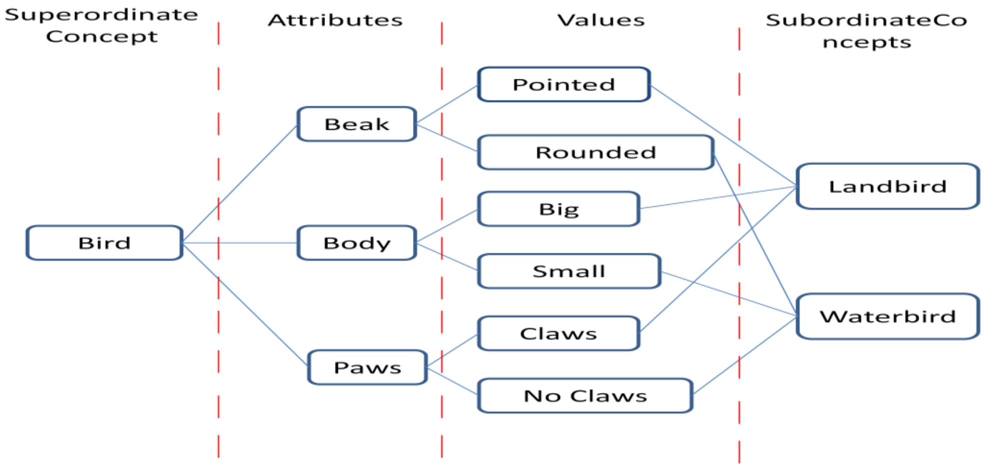
[5] Birds seem to be authors' preferred concept for describing Dynamic Frames, given that several authors have opted for it (ANDERSEN; BARKER; CHEN, 1996, 2006; BARKER, 2001, 2011; CHEN; ANDERSEN; BARKER, 1998; BARKER; CHEN; ANDERSEN, 2003; URBANIAK, 2010; VOTSIS; SCHURZ, 2012).

Fig. 1. Partial frame of bird



Source: prepared by the author (2017).

The concept of bird, thus, is defined by the subordinate concepts of waterbird and landbird. Each of the subordinate concepts has values capable of being represented by a unique attribute. The difference between values and concepts belongs to the fact that attributes define the concept, while values are the forms by which the attributes are more commonly manifested. Therefore, values do not define a concept. In addition, neither values nor attributes are an exhaustive list of characteristics. Both layers are malleable according to the insertion of new exemplars. Two important aspects emerge from this point. First, in some way, the causality is inverted, once exemplars determine the characteristics and concepts but not the other way around. Second, every complete concept is a partial concept open to the inclusion of new exemplars. Therefore, even though the dynamic frames seem like a list of characteristics, in fact they are not (VOTSIS; SCHURZ, 2012).

Four minor peculiarities can be briefly highlighted:

a) binary values for attributes are common, but not necessary;

b) since dynamic frames are exemplar-dependant, the insertion of new exemplars (or the exchange of previous exemplars for new ones) modifies attributes and values, changing the entire concept. Consequently, Andersen, Barker and Chen defend that Kuhnian revolutions can be studied through the lenses of dynamic frames, considering a revolution is the emergence of new exemplars;

Yet, different superordinate concepts are found in Garbacz (2013, 2016), Petersen (2007), Urbaniak (2010) and Votsis e Schurz (2012).

c) obviously, exemplars of a subordinate concept are exemplars themselves of a superordinate concept. For instance, a swan is an exemplar of "white water birds" and of "water birds";
d) according to Barker, Chen and Andersen (2003): "there are no birds with legs that attach to their necks" (BARKER; CHEN; ANDERSEN, 2003, p. 226).

As a consequence, dynamic frames allow the possibility of restricting the connections of values and attributes. For example, legs may be a characteristic only of birds with bodies.

One more important aspect connecting Kuhn's ideas and the dynamic frames approach, is the fact that one subordinate concept cannot be defined as another subordinate concept of the same superordinate concept (e.g. waterbirds cannot be landbirds). Dynamic frames, then, in the same manner as Kuhn's insight on concepts, delegate a significant importance to dissimilarities. Therefore, concepts are not indeterminate and infinite. Yet, even though overlaps within one determinate concept are impossible, the same exemplar may be utilized as asubstance for distinct concepts in terms of two different perspectives. First, an exemplar may be the foundation for concepts pertaining to different persons – as the child and his father. In this case, values and attributes may be different, but this does not interfere with communication. Second, the same exemplar may be part of different concepts. For instance, "paper" may be an exemplar of "writing surfaces" as of "inflammable substances".

These two different perspectives allow the interpretation of a relativistic theory of concept formation. However, from Kuhn's point of view, exemplars are *exhaustively* analyzed. Hence, adherents to the same paradigm would possess very similar concepts as a result of their meticulous analysis of the same exemplars. This would prevent scientists from creating disparate concepts inside their paradigms, even though between two different paradigms the same concept may be formed by the analysis of different exemplars. As an illustration of this difference, one could imagine a different child, who has been presented to ducks and chickens in the same zoo, trying to communicate with the previous child. While both kids could use "birds" to define their superordinate concepts, values and attributes would be significantly different. Theirprobable difficult of communication is what Kuhn defines as incommensurability in his post-structure works (KUHN, 1983). It cannot be forgotten that Kuhn's concept formation theory is the foundation of his new analogy of paradigms as languages. Consequently, incommensurability in his post-structure works results from exemplar incompatibility.

2.5 Final Remarks

After deconstructing neopositivists' ordinary image, Kuhn was labeled as the founder of scientific relativism. However, even though *The Structure* may suggest some relativism, Kuhn main objective was to reject the naïve view of science as cumulative, homogeneous, and nonhistorical; and not to propose a relativistic theory. His philosophical unpreparedness and lack of time may have contributed to his arousal to the relativist position. Then, correcting these misunderstandings became one of Kuhn's torment during his later career. As a result, Kuhn steepedinto philosophy, especially philosophy of language, divorced himself from his previous gestalt switches analogy, and presented his new analogy of paradigms as languages. Furthermore, Kuhn repeatedly belittled relativism.

Consequently, several theses about Kuhn's relativism on the one hand, and Kuhn's non-relativism on the other emerged. Kuhn's relativism seems like an endless discussion. Yet, beneath the debate inhabits Kuhn's analogy of paradigms as languages. Although communicability and the new analogy may seem just as one more defense against relativism, in fact they obscure one nonpartisan theory: Kuhn's theory of concept formation. Kuhn bolstered his defenses on a new solid philosophical ground. More specifically, Kuhn aggregated Wittgenstein, Polanyi, and his own idiosyncrasies for creating a theory of concept formation capable of explaining how paradigms are languages and what exactly incommensurability is.

Recapitulating, in Kuhn's post-structure point of view, paradigmatic concepts are formed through an exhaustive observation of exemplars. After fastidiously examining exemplars, scientists create very similar concepts allowing communicability. Incommensurability is the result of concepts created through the examination of different exemplars. However, while the explanation may be sufficient for deconstructing incommunicability and even relativism, it is incomplete for practical reasoning. How exactly concepts are created and how exactly incommensurability works is not clear through Kuhn's examples. In Garbacz (2013) understanding, this degree of abstraction, as Wittgenstein's family resemblance, incapacitates practical applications. Fortunately, as Garbacz (2013), Andersen Barker and Chen (2006) and Nersessian (2003) contend, Wittgenstein's and Kuhn's theory find in cognitive psychology a route to practical reasoning. Barsalou's (1987, 1992) dynamic frames are unusually similar to Kuhn's theory of concept formation, and frames' exemplar-dependence can be adequately demonstrated as the source of incommensurability (ANDERSEN; BARKER; CHEN, 2006). Yet, besides explaining incommensurability, dynamic frames are a fitting theory for operationalizing Kuhn’s theory of concept formation and explaining how scientific concepts are formed.

3 THE PRAGMATIC VIEW AND THE CONCEPT OF SCIENTIFIC MODELS: A KUHNIAN REVIEW

Theses of the exemplar-dependency of concept formation are empirically and theoretically prolific. However, while abundant in discussions pertaining to psychology and philosophy of language, philosophers of science are normally unfamiliar with these ideas, especially in what concerns the definition of theories and its surrounding concepts. Semantic and syntactic views of theories, on the one hand, overlook exemplar-dependency, since they have logical foundations exempting exemplar discussions. The pragmatic view, on the other hand, bases its arguments on case studies as a way of overcoming previous views, and thus implicitly accepts the importance of exemplars for arguments. However, integrations of the pragmatic view and psychology and/or philosophy of language rarely discuss exemplars explicitly. Along these lines, modern pragmatic definitions of scientific concepts could be competently integrated with neglected supplementary philosophical and psychological ideas. That is the goal of this chapter.

Yet, an integration could be made not only as it should be, since neglecting supplementary psychological and philosophical arguments - even though harmless for pragmatic view's main points - may be gradually disseminating imprecisions regarding the definition of scientific concepts in three important ways. Firstly, any concept is tacit, which by definition implies it cannot be clearly defined by words. This means that the pragmatic view is not being correct about its attempts at defining tacit concepts, especially models. Secondly, as already stated, concepts are exemplar-dependent. As a consequence, a more profound analysis of the role of exemplars inside the ideas of the pragmatic view is necessary. Thirdly, some philosophical and psychological ideas point towards the existence of systems of concepts opposing the previous interpretations, whose accepted concepts could be individual. The concept of a model, thus, cannot be understood separately from its connected concepts. While reviewing the pragmatic view in the light of unfamiliar psychological and philosophical ideas, especially around these three points, may bring forth imperfections in argumentation. It may also be essential for a self criticism of the pragmatic view.

Henceforward, Kuhnian post-Structure theories and psychology's dynamic frames will be integratedinto the pragmatic view in an attempt of reviewing the above identified aspects of the pragmatic view. Both theories present several similarities with the pragmatic view's normal methods of argumentation and are adequate for realizing a necessary self-criticism of the latter. In order to accomplish such integration, a brief review of the history of philosophy of science, demonstrating the emergence of the pragmatic view, will be realized first. In the sequence, three sections will realize distinct reviews of the pragmatic view in the light of Kuhnian theories. The rereadings intend to flash out the most prominent deficiencies of current pragmatic discussions around the topic of the role of models. The deficiencies will be processed separately in more three subsequent sections. In the end, final remarks are presented.

3.1 From logic to pragmatism

"Modelling" is a recurrent topic in the philosophyand in the philosophy of science. As pointed by Boumans (2005), by the end of the XIXth century and beginning of XXth, the concept of a model was already a common topic among scientists, such as Boltzmann, Hertz, and Ehrenfest. Initially, however, scientists approached modeling, in its philosophical aspects, merely for encyclopedic definitions and practical clarifications. Moreover, uses of the term model were diametrically different from modern ones, being exclusively used for defining material objects. This connotation resulted from the fact that models were a prevalent practice among physicists, particularly those interested in classical mechanics. As a consequence of their unimportance in the sciences, generally speaking, models were secondary in the philosophy of science's discussions as well. Indeed, the beginning of XXth century marked the rise of logical positivism and its defense of science as cumulative and homogeneous (see theprevious chapter). Models, in such atmosphere, could not burgeon, given that accepting science as a modeling enterprise is to accept the heterogeneity of the scientific enterprise.

Language, then, was the gravitation center of the debate. In the logical empiricists' point of view, theories were characterized as sets of sentences. Science thus was unified in its language. As French (2008) avers, logical empiricism understands theories in a syntactical form, observing its aspects under logical-linguistics eyes. As logical languages, theories relationship with reality is direct, since the logical sentences are the final representational instrument of theories (PORTIDES, 2013). Consequently, the method of theory evaluation is testing logical sentences validity in the face of empirical observations. This focus on the syntactical characteristics of theories, normally, endorses the appellation of "syntactic view of theories" for this branch of the philosophy of science.

However, while credible, the explanation of science as just a set of sentences was unsatisfactory in a myriad of ways. As a result, the syntactic view of theories was attacked severely, especially after the 1960s. One obvious instance was Kuhn (1996), for whom a single language was incapable of explaining the existence of scientific paradigms. Yet, one more subtle instance derived from the syntactic view's own logical weakness. The efficacy of creating a solid set of sentences representing theories was dubious. As a result, modernized instruments gradually flourished as escape routes for a logical view of scientific theories. In this ambiance, formal semantics prevailed among the revamped views, given its competency in inserting a new layer of logical analysis. As Odenbaugh (2007) states, in formal semantics, models are structures permitting the understanding of logical sentences. These interpretations, instead of the sentences themselves, are responsible for being tested against empirical facts. Hence, models were inserted in their thinking as logical structures capable of representing other logical structures (reality and/or theories). From then on, discussions about what types of logical structures define reality and what are their relationships with models became philosophers enigmas, as illustrated by Van Frassen (1980) isomorphism, Giere (1988) similarity, and Da Costa and French (2003) partial representations. Theories, then, became "families of models" and this view received the label of "semantic view of theories".

According to Liu (1997), irrespectively whether models or sentences are the central points of the debate, logic is still the beating heart of the semantic and syntactic view of theories. Thus, this characteristic cloisters both views in their logical interpretations of science, permitting the proliferation of innumerous interesting theses, yet distancing semanticists and syntacticists from practical aspects of science. Notably, models – beyond being logical structures connecting theories and reality for philosophers – have meaning for the scientists themselves. In Downes (1992) words: "[...] philosophers should focus on the nature of scientific theorizing. Theorizing is carried out by practicing scientists, and we cannot say what scientific theories are unless we appreciate the myriad ways they are used and developed in all of the sciences" (DOWNES, 1992, p. 142). Semantic and syntactic views' exclusively logical arguments secluded philosophers from the practical specificities of science.

In order to face the condoned practical dilemmas of the scientific enterprise, logic had to be replaced by a more pragmatic approach. Logical reasoning was distorting the philosophical apprehension of scientific specificities, as illustrated by the understanding of models: "There are many referents for the term 'model', [...] there are far greater differences between models in mathematics and logic and models in science than holder of the semantic view have been prepared to admit" (DOWNES, 1992, p. 144). From Odenbaugh (2007) point of view, philosophy was confounding models as structures with the very concept of a model: "We should not confuse the sameness of the particular structures considered to be models with the sameness of the concept of models." (ODENBAUGH, 2007, p. 512).

Aspirations for revision derived not solely from discontentment with logical reasoning, but also from skepticism about it. For example, Contessa (2006) addresses fierce critiques regarding the semantic view of theories. In his conception, the semantic view is a conservative agenda, discussing logical nuances when the real discussion should be whether philosophy of science really needs formal reasoning:

> The crucial divide in philosophy of science, I think, is not the one between advocates of the syntactic view and advocates of the semantic view, but the one between those who think that philosophy of science needs a formal framework or other and those who think otherwise. (CONTESSA, 2006, p. 376).

However, as Halvorson (2012) states, choosing may not be the best solution, since both logical and pragmatic reasoning have their own qualities and flaws.

Either way, critique and skepticism highlight that the semantic and syntactic views are unable to clearly explain science's practical problems, which bolstered the ascension of a more pragmatic attitude in the 1960s. This pragmatic turn established a new approach of philosophical analysis of science: the pragmatic view of theories (PVT). Visibly, the pragmatic view most impressive turnaround was its detachment from logical reasoning in the sphere of scientific tools and behaviors. Observation of science from the point of view of practice put philosophers on the track of looking at theories and models, searching for "better understand their construction and function" (MORRISON, 2007, p. 196). As a perspective independent of mathematical and logical constraints, the pragmatic view became free to approach science in a plural manner. Models and theories quickly began to be explained in a myriad of forms, accepting abstract and material interpretations, allowing linguistic and mathematical foundations, and comparing models to analogies, metaphors, caricatures, and several other different concepts. This multitude of interpretations resulted from the pragmatic view's new reasoning basis. Instead of logic, pragmatists' opted for studying scientific tools and behaviors from the perspective of the history of science (ODENABAUGH, 2007), similarly to how Kuhn was studying scientific evolution.

According to Winther (2015), the pragmatic view consolidated during the 1980s, especially after Cartwright (1983). Yet, it is interesting to notice, mainly considering Hesse (1966) , that pragmatic approaches to scientific behaviors already existed before, even though they received paltry attention. Somewhat, it can be argued that the pragmatic historical reasoning was initially directed towards the analysis of the scientific evolution as a whole - as illustrated by Kuhnian concerns. Thus, from the 1960s to the 1980s, the practice of studying philosophy of science based on historical examples gradually spread to the study of different questions. Therefore, after pragmatic concerns with the dynamics of science as a whole, case studies reached scientific specificities as well, focusing on matters such as models' ontology and function, the nature of assumptions, and representation in non-logical forms.

Hence, considering Kuhn's work and the pragmatic view of theories, logic alone – for more pragmatic philosophers - gradually became unable to explain both the evolution of scientific ideas in an aggregated point of view and science's minutiae. However, while both Kuhn and the pragmatic view of theories claimed for a historical analysis, their ideas are not usually found in combination. Kuhn's incommensurability and attributed relativism tend to repel philosophers. Also, Kuhn's initial focus on the dynamic of science was in disharmony with the pragmatic view, whose concerns were more specific. However, after the Structure, Kuhn corrected some of his misconceptions, specially substituting his former analogy regarding Gestalt Switches for an analogy with language. His modifications naturally flowed to more specific discussions and are an interesting form of conciliating Kuhn's dynamic focus with the Pragmatic View.

Although the pragmatic view may have superseded some of the flaws of the previous views, their discussions are not perfect. In the next sections a review of pragmatic arguments, in consonance with supplementary philosophical and psychological ideas, intends to demonstrate important neglected aspects in pragmatics' arguments. This review has the intention of condemning malpractices as well as the intention of proportionate a self-criticism of the pragmatic view.

3.2 Reviewing the pragmatic view I: complete frames

The Pragmatic approach to case studies is, as pointed henceforward, analogous to Kuhn's approach to exemplars. Remembering the discussion in the previous chapter, Kuhnian post-Structure theories affirm that scientific concepts are created from an exhaustive analysis of exemplars. Reading Kuhn in the light of the dynamic frames implies that scientific concepts are created with values and attributes which depend on a set of exemplars. In this section and in the next two it is demonstrated how pragmatist's claims use case studies exactly as Kuhnian exemplars in the dynamic frames, creating values and attributes for the concept of model.

Evidently, none of the authors observed next had in mind the conscious intention of formulating a frame for the concept of a model. Yet, either as a coincidence or as evidence favoring dynamic frames' validity, the resemblance between their works and the framing is at least curious. Unfortunately, similarity is not identity. As a result, some aspects may have been lost when converting the examples into frames. Still, the intention of this reformulation is eliciting a pragmatic self-criticism in the sense of illustrating how a pragmatic view could be well served with supplementary ideas from psychology and philosophy. As a direct manifestation of these goals, purposely the frames and the following reviews are lacking in some of the exemplars, since their presentation would solely increase the discussion complexity without aggregating any important points for reflection.

As a starting point, opposing to the previous logical views, Morrison and Morgan (1999) and Morrison (1999) affirm that models are autonomous mediation instruments. In the semantic view, for instance, models are necessarily a subset of theories and, consequently, have a structural connection with theories. Models as autonomous mediation instruments, differently, are independent of theories or reality, even though they mediate them. Assuming independence, besides being a rupture with previous logical interpretations, obliges the authors to assess models characteristic in a functional context. This assessment, as proposed by Odenbaugh (2007), occurs via case studies.

Morrison and Morgan's (1999) handling of case studies is similar to the use of exemplars in dynamic frames. Initially the authors highlight that models have three characteristics: function, representation, and a learning route. Each one of these characteristics is founded on different case studies. Therefore, in Kuhnian terms, it can be assumed that the authors determine attributes referring to distinct exemplars. The similarities, however, extend further. Morison and Morgan (1999) present additional similarities to frame reasoning when they elect characteristics of the characteristics, or values of the attributes in frames' vocabulary.

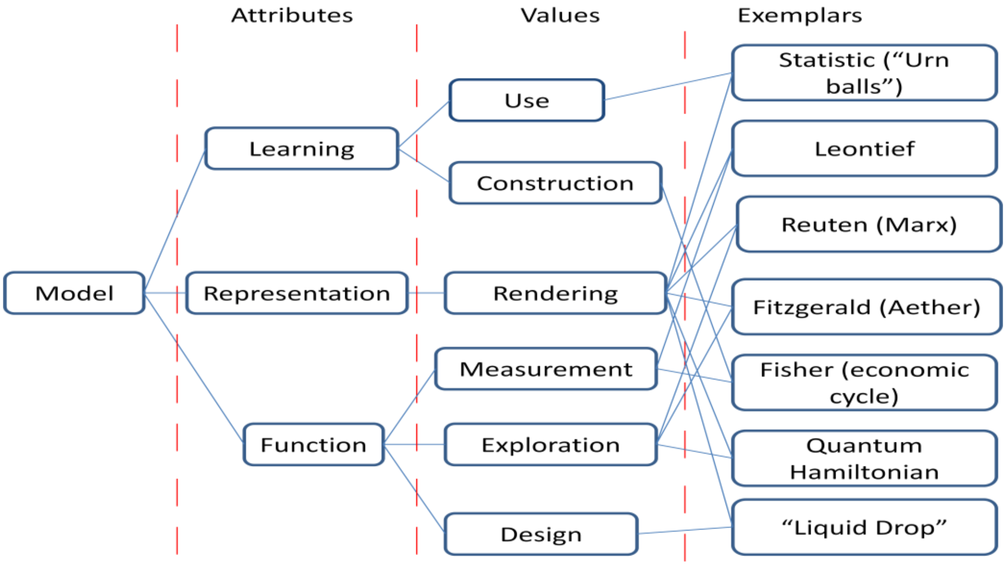
Function, for instance, can be found in three different manners (values) according to Morrison and Morgan's (1999) case studies: exploration, measurement, and design.

Exploration is the function of a model constructed for experimenting or exploring a certain well-defined theory, being capable of altering it or not. Sir George Francis Fitzgerald’s mechanical models of aether, Geert Reuten’s Marxian analysis model, the MIT-Bag model, the rational *homoeconomicus*, and the quantum Hamiltonian are some of the case studies used by the authors for providing support of exploration. Measurement, by its turn, is the function of models used as measuring instruments or for presenting measurements. The most significant exemplars bolstering this function are Irving Fisher’s and Leontief’s economic models. Finally, design corresponds to models functioning as a support for future technological developments. Nuclear physics’ liquid drop model and some models in optical geometry were the author’s case studies supporting such function.

The second attribute, representation, although more homogeneous among Morrison and Morgan’s (1999) exemplars was still presented in a form comparable to Frames. According to the authors, models differ in their capacity of representation according to their levels of rendering. Therefore, single values for representation are impossible to be determined, since rendering is theoretically a continuum scale. This nature of representation prevents it from having more than one value, but it does not prevent it from being presented as a possible single-valued attribute.

Lastly, the third attribute –learning - can be found in Morrison and Morgan’s (1999) exemplars in two different forms: learning from construction, and learning from use. The analysis of case studies generated these forms. On the one hand, constructing the first models of economic cycles was essential for teaching how to build future theoretical cycle models. On the other hand, when using the model of “balls in an urn”, rather than constructing it, one can learn several probability laws. Once more the authors’ main line of reasoning is displaying common forms of universal characteristics based on case studies, which in a frame would be equivalents of values of attributes based on exemplars. The similarity between Morrison and Morgan’s (1999) discussion and dynamic frames is presented in the following image:

Fig 2. Partial frame of Morrison and Morgan (1999)

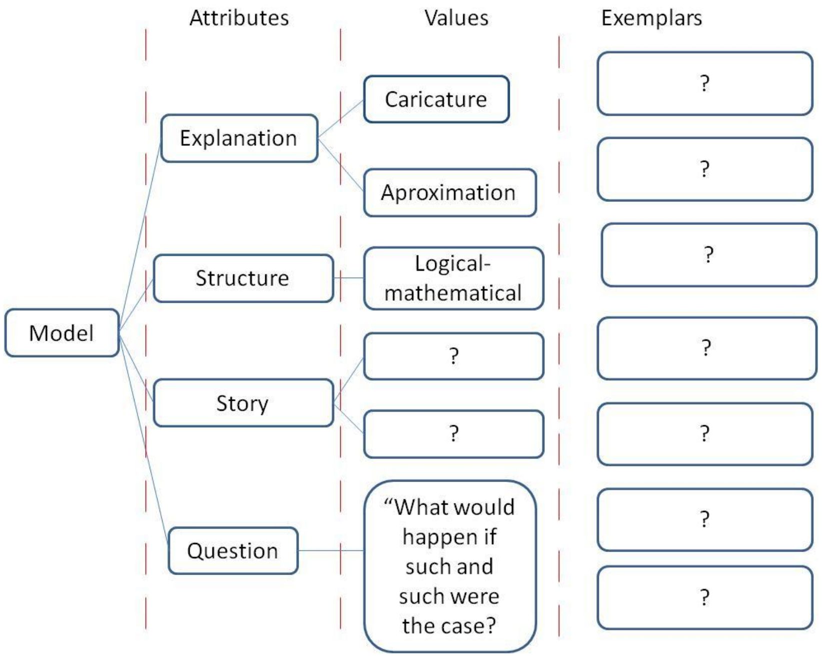


Source: prepared by the author (2017).

Moving forward, pragmatism enticed not only philosophers, but the scientists themselves. While logic secluded philosophers, hampering scientists from philosophizing about their own enterprise, pragmatism was an invitation for scientists' contributions. Gibbard and Varian's (1978) definition of models illustrate this characteristic from an economics angle. In their discussion about the definition of theoretical and descriptive economic models, the authors assert: "a[n] [economic] model [...] is a story with a specified structure" (GIBBARD;VARIAN, 1978, p. 666). Structure, for Gibbard and Varian (1978), corresponds to logical and mathematical aspects of the model's assumptions. History, on the other hand, is the interpretative element of the model and a way of directing the structure towards an explanation. Moreover, the authors also contend that economic models have a question and an explanation. While the explanation is heterogeneous, having two different forms (caricature and approximation), the question is monolithic, appearing solely as "what would happen if such and such were the case?" (GIBBARD;VARIAN, 1978, p. 668).

Therefore, Gibbard and Varian (1978) argue instinctively in the same pattern of a Kuhnian frame. The authors point the following attributes of a model: structure, question, story, and explanation. Each attribute has its specific values. The explanation may emerge in models as caricatures and approximations, whereas the structure materializes uniquely as logical-mathematical formulations. The question, also single-valued, appears as "what would...?". Still, instincts are unconscious. Consequently, Gibbard and Varian (1978) definition is not completely convertible to a frame. Firstly, the story does not have specified values. Secondly, the exemplars supporting Gibbard and Varian's reasoning are not explicitly showed, being defined solely as theoretical and descriptive economic models. Nevertheless, the framework of argumentation is similar to a frame and demonstrates how pragmatism intuitively defines models in a Kuhnian psychological scheme. The following image exposes Gibbard and Varian's as a frame:

Fig 3. Partial frame of Gibbard and Varian (1978)



Source: prepared by the author (2017).

Returning to philosophy, a recent pragmatic definition of a model belongs to Weisberg (2013). Once again, as in alternative pragmatic definitions, Weisberg (2013) discussion develops resembling Kuhnian frames. His definitions start identically to Kuhn's, since defining the exemplars is Weisberg's first concern. For Weisberg (2013), a model definition is adequately funded by "The San Francisco Bay-Delta Model", "Lotka-Volterra model" and "Schelling's Segregation Model". According to the author, the three models are capable of simulating the most part of exemplars used in philosophical discussions. From the selection of the exemplars, in a Kuhnian glimpse, Weisberg (2013) initiate his model definition: "So what do these models have in common such that we can develop a unified account of them?" (WEISBERG, 2013, p. 15)

Although Weisberg's commencement is essentially Kuhnian, his following definition is not as easily paralleled with a Kuhnian frame. Still, the similarities in the starting point encourage the comparison. For Weisberg (2013), each exemplar exteriorizes a structure in a singular form: the San Francisco Bay-Delta in a concrete form; the Lotka-Volterra in a mathematical mode; and the Schelling's model in a computational manner. In Kuhnian terms, the structure is an attribute materialized in different values according to the exemplars. A conundrum is settled when Weisberg defines models "description" and "interpretations". On the one hand, "[...] each of these [models] is composed of a structure along with an interpretation of that structure" (WEISBERG, 2013, p. 23). On the other hand, "When we talk about models, write about them, or show a picture or diagram, we are employing a model description" (WEISBERG, 2013, p. 33). Therefore, only interpretation and structure are explicitly clear attributes. Whether descriptions are either attributes or values of the structure is unclear.

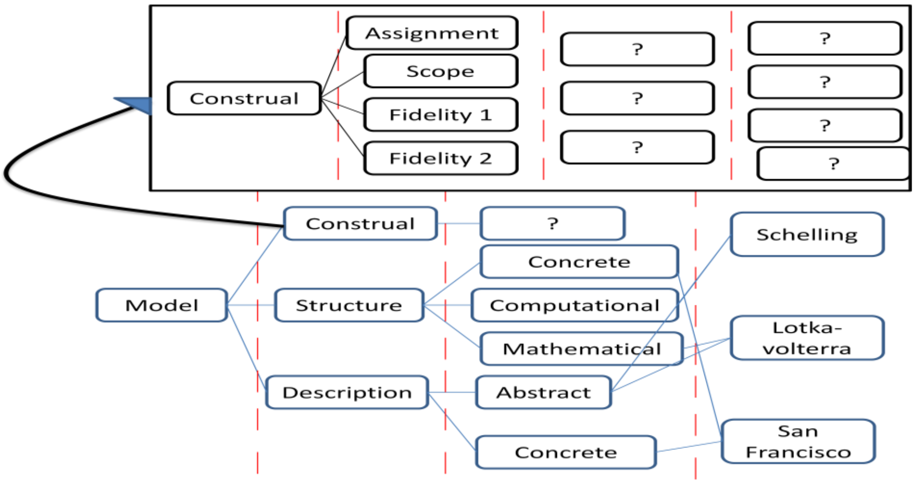
However, Weisberg clearly defined structure values as being concrete, mathematical, and computational. As a result, "description" hardly could be employed alongside these unfamiliar values. In addition, Weisberg's exposition suggests description can be an attribute, once values are apparently defined:

> While there is considerable latitude for the kinds of representations that can serve as model descriptions, concrete models are usually represented with concrete model descriptions [drawings, pictures, etc.], and mathematical and computational models are usually represented with abstract (algebraic or pseudocode) descriptions. (WEISBERG, 2013, p. 38).

In order to accommodate Weisberg's discussion adequately in a Kuhnian framework, description has to be understood as an attribute, whose values are concrete and abstract.

Finally, returning to interpretation as an attribute, Weisberg names model interpretations as "construals". Weisberg's discussion of construals exposes how scientific definitions may follow a convoluted Kuhnian reasoning. When discussing construals, instead of defining materialized values and connecting them to their exemplars, Weisberg (2013) opts for presenting immaterialized characteristics. In other words, Weisberg takes a step back in the frame mode of argumentation and starts the definition of a new frame within the frame of models. In his point of view, an assignment, a scope, and two different fidelity criteria form a construal. The exact definition of the frame of construal is unrelated to the frame of a model. Still, demonstrating the pattern of Weisberg's discussion illuminates how dynamic frames can be implicit in pragmatic definitions. The following image intends to simplify this pattern:

Fig 4. Partial frame of Weisberg (2013)



Source: Prepared by the author (2017).

3.3 Reviewing the pragmatic view II: attributes and values

Complete frames are rare in the literature, and there is no reason for this not being the case, considering that the pragmatic authors do not have the conscious intention of building frames. The previous examples are a rereading of the pragmatic literature accommodated to the Kuhnian framework, and, thus, they highlight only essential elements for the construction of frames, while leaving untouched innumerous discussions of the authors. Beyond the complete pragmatic definitions of the concept of a scientific model, the pragmatic view also concentrates its efforts around some focal points, such as specific attributes or specific values.

Representation is commonly discussed as if it is an attribute of the models. While the semantic and the syntactic views are logically conceived and have a uniform understanding of what a model representation is – even though disagreeing about how this representation may occur -, the pragmatic view, without the aid of logic, frequently debates the nature of the representation. The primary concern of pragmatists is surpassing exclusively analytical discussions. As Frigg (2006) and Suarez (2010) affirm, the pragmatic view can achieve an explanation about representation more closely related to what it means to the scientists themselves. Along these lines, as already discussed, Morrison and Morgan (1999) contend that representations are a characteristic materialized in infinite forms of rendering. Similarly, for Hughes (1997, p. 325): “The characteristic – perhaps the only characteristic – that all theoretical models have in common is that they provide representations of parts of the world.” Yet, defining representation demands a remarkable effort from the pragmatic view. Consequently, even though modest discussions may appear in the literature, generally representation deserves the formulation of its own scientific concept. As a result, the next section will observe representation in greater detail.

Another noteworthy attribute-lookalike discussion of the pragmatic view concerns idealizations and simplifications. Maki (1992, 1994, 2009) and Sugden (2002; 2009), for example, discuss how economic models idealize reality. For Maki (1992, 1994, 2009), economic models realize isolations in order to divorce the objects of study from unnecessary interferences. As an illustration, the author usually compares economic models to gravity models, which isolate gravitational force from air resistance. Depending on the broadness of Maki's exemplars and discussions, isolations may appear either as a value (of idealization) or as attributes. On the other hand, according to Sugden (2002; 2009), instead of isolating, economic models create fictional assumptions when idealizing. Sugden's example is the rational *homo economicus*¸who has unlimited access to information and is always capable of maximizing his utility[6].

Similarly, Weisberg (2007; 2013) discusses idealizations comparably to attributes. Based on a set of exemplars, Weisberg (2007; 2013) contends that idealizations are materialized in three different forms: galilean, minimalist, and multiple models. Galilean idealization "is the practice of introducing distortions into models with the goal of simplifying, in order to make them more mathematically or computationally tractable." (WEISBERG, 2013, p. 98). Minimalist idealization occurs when models are formulated solely with the central causal factors of phenomena. Multiple models idealization is the practice of building several incompatible models, each one having a partial role in model explanation. In the Kuhnian framework, Weisberg may be asserting that idealization is an attribute with three values.

Finally, a third discussion surrounds models' purposes. Gibbard and Varian (1978) argue that economic models intend to answer a specific question. Morgan (2002) expands Gibbard and Varian's point of view, and defends that models' questions may go further than simply asking "what would happen if such and such were the case?". In any case, both Gibbard and Varian (1978) and Morgan (2002) discuss the act of answering questions as a model purpose. In a similar manner, thus, Morgan and Morrison (1999) "function" also penetrates the debate, given that the concept has a sense of purpose. Following these arguments, Weisberg (2013) - who states that models have "targets" – and Hedoin (2012) – who affirms that models have a "purpose" – may be inserted into the debate as well. Each

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[6] Maki (2009) counterarguments affirming that unlimited access to information is an isolation, requiring solely that its costs are voided to be understood as such.

author highlights different arrays of values for their model's purposes. Gibbard and Varian (1978) restrict models to a single question, whereas Weisberg (2007; 2013) asserts that models have a vast set of values. Yet, in Weisberg's point of view, unlike Stefanov (2012), models cannot be expected to be descriptive instruments.

Unlike the discussion in the previous section, the similarity between the pragmatic discussions of representation, simplification, or purpose, and the Kuhnian framework, is not highly satisfactory. Not necessarily the discussions apply to Kuhnian reasoning, given that some are not even definitions of concepts *de facto*. Further evaluations of the pragmatic view are necessary to really demonstrate that discussions around those problems involve dynamic frames. Yet, this brief examination still have a supplementary character to the discussion insofar as pragmatic discussions, as observed in the previous section, implicitly assume Kuhnian reasoning.

3.4 Reviewing the pragmatic view III: models as exemplars

The implicit Kuhnian reasoning behind the pragmatic view may develop beyond the definition of scientific models as a result of two different obstacles. First, the attributes of scientific models, sometimes are hard to define. Consequently, the definition of the attribute has to be expanded, which eventually implies the formulation of a complete frame. Second, models may be part of broader definitions and, instead of being the object of the definition, become solely an exemplar of another primitive concept[7].

To begin with, Toon (2010; 2012) defends the view of "models as make-believe". In his point of view, in order to properly explain models capacity of representing something, a comparison is necessary. Scientific models must be compared to games of make-believe present in Walton's (1990) theory, since "Models [...] function as props in games of make-believe" (TOON, 2012, p. 61). Therefore, scientific models, in Toon's argument, at some moments become merely exemplars of games of make-believe, and representation is an attribute of the last materialized in the first, while at other moments games and models are exemplars of a different superordinate concept.

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[7] In fact, Kuhnian ramifications may be confounded with mere comparisons between models and different objects, since comparisons are the foundation of dynamic frames - and Kuhn's post-Structure theories are highly correlated to dynamic frames. In other words, distinguishing simple comparisons from the building of frames is complex – if not impossible, considering that Dynamic frames have supporting evidence that this is indeed the way our mind understands concepts. Therefore, the following examples demonstrate how the task of comparing can be benefited from a psychological framework. Differently from the third section, values and attributes are not clearly defined when using models as exemplars.

A recent tendency of the pragmatic view is to compare models with fictions. Frigg (2010) supports a reasoning that attempts to understand “model systems in analogy with entities that occur in literary fiction” (FRIGG, 2010, p. 268), while Godfrey-Smith (2006; 2009) contends that “Scientific modelers often treat model systems in a ‘concrete’ way that suggests a strong analogy with ordinary fictions” (GODFREY-SMITH, 2006, p. 739). Both Frigg and Godfrey-Smith opt for using models and fictions as exemplars of the concept of representation. Godfrey-Smith (2009) even argues in a kuhnianmode, presenting attributes of representations, such as the fact that both fictions and models are neither necessarily mathematical objects nor concrete objects, even though both are candidates for concrete and mathematical representations.

When discussing models and fictions, Contessa (2010) partially confesses his Kuhnian and Wittgensteinian tendencies, admitting that models and fictional entities belong to the same “ontological genus”: “it is not an accidental resemblance – it is, so to speak, a family resemblance. Fictional models and fictional characters are two species of the same ontological genus – that of imaginary objects” (CONTESSA, 2010, p. 219). The author goes beyond family resemblance and defends that fictional entities and models are not identical, having important dissimilarities. Therefore, Contessa’s arguments are peculiar in the pragmatic view, insofar as they are conscious about the search for similarities and dissimilarities of exemplars. As the author points out (2010, p. 218): models have characteristics “strikingly similar to the features of another kind of entities that are philosophically puzzling – fictional entities.”

In opposition to the fictional tendency, Ducheyne (2008) avers that models cannot be understood as mere fictions. Unlike fictions, models are physically realizable in Ducheyne’s point of view. For instance, although null air resistance gravity models are not physically realizable, their assumptions allow empirical tests – a form of realization. Moreover, Ducheyne’s thinking is also defined as a species of the Kuhnian framework, taking into account that the author compares models to mental representations.

Cartwright (2010) is more specific about the kinds of fiction comparable to scientific models: fables and parables. For the author, fables and models are alike, considering that “the conclusion of the model *like* the moral of the fable, can be drawn in a vocabulary abstract enough to describe the things we do want to learn about” (CARTWRIGHT, 2010, p. 20). On the other hand, while fables have their morals explicitly defined, Cartwright’s exemplars of models do not present this characteristic. Concerning models conclusions, the author believes that models are better compared to parables. In a Kuhnian framework, thus, Cartwright creates a family of exemplars and searches for similarities and dissimilarities between them.

Apart from fictions, Maki (2005) contends that “models are experiments, experiments are models”. In Maki’s point of view, theoretical models are “thought experiments”, which use isolations identically to material experiments. According to him, the main similarity between models and experiments is both having “characteristics of representations that are manipulatedin order to effect isolations.” (MAKI, 2005, p. 311). Moreover, McCloskey (1983) highlights that “The most important example of economic rhetoric […] falls outside the border of self-consciousness. It is the language economists use, and in particular its metaphors.” (MCCLOSKEY, 1983, p. 502). The language to which McCloskey refers to is economic modeling. As a consequence, McCloskey compares models to metaphors and discusses both in a somewhat Kuhnian way.

3.5 Remarks about the pragmatic view I: explicit and tacit

As observed above, the pragmatic view is far from being consensual about the definition of models. Gibbard and Varian (1978) definitions are distinct from Weisberg (2013), which, by its turn, are different from Morgan and Morrison (1999). Consequently, pragmatists’ discussions about the ontology of models are uncomfortably frustrating. As Godfrey-Smith (2009, p. 104) affirms: “[...] the word “model” itself is useddiversily. So it is not a good idea to organize discussion around the question: ‘what are models?’” . Analogously, Morgan (2012) avers:

> Fifteen years of researching, thinking and writing about models have convinced me that there are no easy answers to questions about what models are, and how modelling works. Some questions are more helpful than others. Asking: what qualities do models need to make them useful in science? And what functions do models play in a science? Are more fruitful than asking what are models. (MORGAN, 2012, p. 16).

Therefore, unsatisfactory discourses frequently arise: “more or less anything that is used in science to describe empirical phenomena is a model” (BAILER-JONES, 2009, p. 4).

Although identity is not expected in pragmatic definitions, profound dissimilarities hamper the progress of subsequent discussions. Representation and idealization, for instance, depend to some degree on the definition of models. Opportunely, knowing that the pragmatic view overlaps with Kuhnian frames in different forms makes it possible to reassess their thoughts in a reformulated manner. Consequently, by examining the pragmatic view's frustration, the reasons become achievable.

As a starting point, it is interesting to contrast philosophers' and scientists' tasks. Bailer-Jones (2009) presents itself as a solid basis for such comparison. Perhaps facing the pragmatic philosophers' incapacity of agreeing about model definitions, Bailer-Jones (2009) interviewed scientists searching for their definitions of models. From them, Bailer-Jones (2009, p. 8) concluded: "there are a number of characteristics of models that were identified by interviewees as being typical of models". Notably, scientists highlighted simplification, approximation, relation to reality (possibly the philosophers' representation characteristic), testability and, finally, the capacity of teaching as important aspects of modeling. Interestingly – but not surprisingly -, scientists definitions were attuned to framing definitions, underlining attributes and eventually exemplars. Furthermore, even though scientists were mainly physicists and chemists, their interpretations of models were similar to definitions in the social sciences, such as Gibbard and Varian (1978).

Still, some scientists neglected the possibility of defining models adequately. Remembering Kuhn, scientific concepts are tacitly acquired, resulting from implicit agreements on scientific exemplars and exhaustive studies of them. Concepts merely exist for scientists, without demanding explicit discussions. Bailer-Jones notices this tacit characteristic: "there is what philosophers think, and there is what scientists think, *and* there is what scientist practice" (BAILER-JONES, 2009, p. 14). Scientific practice is purely tacit. Scientists are unconcerned with transforming their ordinary practices into explicit rules or definitions. On the contrary, science develops tacitly. Therefore, the pragmatic view attempts to paradoxically transform the tacit into explicit, opposing scientists' indifference to rules. Part of pragmatic's frustration results from such inglorious task.

Tacit knowledge by definition is something incapable of being described in words. According to Turner (2012, p. 399) "[...] we, as tacit knowers, are the product of our interactions". Yet, "we know more, or different things, than the things we get simply through our interactions" (ibid.). Along these lines, concepts are not static and cannot be apprehended completely during interactions. Tacit concepts are in constant change. Starting from a selection of exemplars – in a "pool of exemplars" - we create a concept and subsequently use it. Therefore, this new concept is inserted into the pool of exemplars and can now be selected for forming new concepts. Innumerous individuals and groups select exemplars and create concepts in an uninterrupted manner.

Accepting this fluidity of concepts implies in accepting a high degree of relativism in two different forms. (1) First, a tacit concept constantly modifies itself over time. Any selection of exemplars is always lacking in future exemplars – resulting from present interactions - and thus may quickly become obsolete. (2) Second, a tacit concept is not constrained by distance. Groups and individuals under interaction in different places do not necessarily create the same concepts and select the same exemplars. As a simple illustration, Brazilian concept of beauty in the 1950s is different from the modern American concept of beauty. Why should it be different with the concept of scientific models?

Aligning the previous argument with a Kuhnian framework: scientific paradigms may be seen as a limitation of space and time for the choice of exemplars. Therefore, *the* concept of model does not exist. Instead, several concepts related to groups in specific times and places may exist. Yet, relativismis not unconstrained in the Kuhnian reasoning. Scientific exemplars, unlike mundane selections, are not volatile. Kuhn (1996) is emphatic about the importance of exhaustive presentation to the selected exemplars. In Kuhn's point of view, not all exemplars may be used for creating concepts, only those accorded between scientists pertaining to the same paradigm. And these accorded exemplars must be intensively analyzed, what prevents abrupt modifications of the set of exemplars. Unfortunately, the criteria for exemplar selection were not defined by Kuhn (1996) and are a topic of debate until modern days. Hence, how can a philosopher select the exemplars of a model in order to recreate a credible scientific concept? How to uncover scientists' tacit agreement over exemplars?

3.6 Remarks about the pragmatic view II: selection of exemplars

Scientists do not have to debate about the selection of exemplars when setting their concepts and behaviors. For members of scientific communities, exemplars are determined tacitly and naturally, and so are their concepts and behaviors. As a manifestation of the tacit characteristic of scientific practice, Bailer-Jones (2009) noticed that scientists, when asked about the definitions of a model, are incapable of describing it properly. On the other hand, philosophers of science are outsiders from paradigms and mere observers of scientists. As a result, they do not have the natural privilege of tacitly assimilating exemplars. Yet, pragmatists' interests impose the necessity of uncovering scientist's tacit agreements.

On the other hand, if not even scientists are able to identify exactly their agreed exemplars and explain their behaviors, philosophers' prospects of uncovering scientific exemplars are at a minimum. As a direct effect, the pragmatic view of theories incurs in arbitrary selections of exemplars, resulting in different basis for argumentation and disharmonic discussions. Returning to the examples, it is evident that comparing definitions based on distinct sets of exemplars is complex. Weisberg (2013) uses three heterogeneous exemplars for recreating his concept of a model, while Morrison and Morgan (1999) present lots of other exemplars. Since the authors opt for different exemplars, it is not possible to evince how the role of each exemplar in their scheme impacts their definitions. As a consequence, an important part of the differences in attributes and values result from their differences in the selection of exemplars. Thus, in order to prevent future frustrations, the pragmatic view needs to find an ordered way to expose scientists' agreements.

A possible solution may be found in economics. Inspired by Sugden (2002; 2009) philosophers – especially those associated with the pragmatic view -, instead of recreating *the* concept of a model, may create *a* concept of a model. Therefore, rather than selecting *the* exemplars, philosopher have uniquely to select *possible* exemplars. In other words, philosophers may assume their incapacity of representing properly all science's tacit and natural foundations, just like economists in Sugden's view accept their incapacity of mirroring economic reality. Alternatively, philosophers may attempt to create *plausible* foundations.

In this regard, the proposal that scientists have an agreement around one exemplar of the physics of the 1920s and another exemplar of the psychology of the 1990s is not credible. Hardly a group of scientists would agree with using such disparate set of exemplars as its paradigmatic foundation. Time and space (discipline) boundaries are not respected since the two exemplars are decades apart and both disciplines have little in common - especially considering the differences in time. Although both exemplars are scientific and can be categorized in the same ontological genus on the point of view of the philosopher or historian of science, they would not be categorized in the same category in the point of view of a scientist. Pragmatism, in the sense of analyzing science in the perspective of the scientists, cannot happen with non-credible options of exemplars. On the other hand, opting for three exemplars belonging to economic models in the 1930s has higher credibility as a paradigm support. Not necessarily the three exemplars were the exemplars that scientists agreed tacitly. Still the established boundaries of time and disciplineguarantee that the scientists could have chosen these as exemplars. In contrast with current pragmatic practices, comparing plausible paradigms prevents exemplar-dependency of overshadowing important questions.

Nevertheless, it has to be pointed out that creating possible paradigms is a hoax not seen by Kuhn, since it avoids defining the criteria of selection of exemplars and explaining scientific evolution, exactly Kuhn’s main intentions. But, unlike historians of science, the pragmatic view of theories is not concerned directly with these criteria. Therefore, the problem of exemplar selection for the pragmatic view is similar – if not identical – to the quandary regarding case studies. According to Pitt (2001):

> On the one hand, if the case is selected because it exemplifies the philosophical point being articulated, then it is not clear that the philosophical claims have been supported, because it could be argued that the historical data was manipulated to fit the point. On the other hand, if one starts with a case study, it is not clear where to go from there for it is unreasonable to generalize from one case or even two or three (PITT, 2001, p. 373).

Exemplar selection for the pragmatic view is the selection of case studies. Pitt (2001) skepticism about the capacity of philosophers using case studies may be expanded to the ability of pragmatists choosing exemplars. However, Burian (2001), in agreement with the previous proposed solution, asserts that case studies may be groupedin order toconstitute a solid basis for reasoning. Compatible with the proposal of plausible paradigms, Burian (2001) suggests two grouping methods: longitudinal and comparative. The first contends that the analysis of case studies has to be organized over time, while the second defends that case studies must be grouped according to their discipline (even though being different in their methods).

It cannot be denied that space and time limitations have importance in pragmatic definitions. Morgan and Grüne-Yanoff (2013, p. 143-144), for instance, admit concepts vary according to their corresponding disciplines. In their mode of thinking, pragmatic definitions focused on economic and physics, what neglects important differences between disparate scientific enterprises: “[...] it has become increasingly evident that the way models are conceptualized is very different in some other sciences, where philosophers’ accounts of models’ characteristics and functions might have to begin again” . Thus, the authors confess the concept of a model corresponds to its specific discipline.

It is important to notice that current pragmatic definitions are not incorrect. Exemplar selections cannot be right or wrong. Therefore, any set of attributes and values presented – excluding minor misunderstandings – are valid and exist for the exemplars selected. The dilemma consists in discovering what those selections actuallyrepresent, since they do not represent a credible paradigm. Considering this, pragmatic definitions of models have value and all pointed characteristic may be true in their imaginary paradigms. Yet, comparing them results hardlyin development.

3.7 Remarks about the pragmatic view III: disparate frames

A third and last brief remark deserves consideration: exemplars are not limited to one unique concept; and concepts are not constrained to be only one concept. A frame, either of psychological or of scientific concepts, is created in an interconnected manner. Each attribute and value is a defined concept, while each exemplar may be the basis for innumerous different concepts. Therefore, whole systems of complementary frames coexist. As already argued in the case of models, Weisberg (2013) construal earned its own frame definition. Moreover, models were used in different works as exemplars for the concepts of representation, rhetoric, and fiction. Somewhat, the pragmatic view recognizes that pragmatic concepts are not singular, and then it attempts to define relevant interconnected concepts for completing a functional explanation of science and modeling.

Eventually, comparisons are interpreted as surrogates: “models are fictions” or “models are experiments”. In fact, while models may pertain to the same family of fictions and experiments, they clearly are not identical to those. Resemblance is not identity. Furthermore, in a Kuhnian reasoning, simply presenting substitutes for models may be misleading for two different motives:

a) models and the proposed substitutes may be exemplars of the same family, and thus dissimilarities are not being accounted for when defining the concept to which they belong;
b) models and the proposed substitute are both exemplar and concept – in any order – and attributes and values are being overlooked.

As Contessa (2010) highlights the point, it is important to avoid explaining the obscure with the more obscure.

3.8 Final remarks

Although the authors did not consciously intend to formulate Kuhnian frames, their mode of reasoning implicitly hid concept definitions as frames. Therefore, performing a rereading of the pragmatic view in Kuhnian lenses was possible. Complementing the pragmatic view with a philosophical reasoning embedded in psychology demonstrated some frailties of modern pragmatic definitions, specially the ones around the concept of model.

First, after reviewing how the pragmatic view may be reread in three different forms, the conundrum of evincing tacit knowledge was approached. Scientific concepts are tacit and natural for scientists and attempting to demonstrate them explicitly will probably incur in neglecting information. Second, the pragmatic view bolsters its discussion on case studies – or selection of exemplars. Still, pragmatists do not recognize that scientific concepts are exemplar-dependent, and debates around characteristics without a proper selection of exemplars are innocuous. In addition, arbitrary selection of exemplars hardly represents a credible paradigm. Therefore, even though the attributes and values of proposed concepts may be true, they do not represent real scientific practice. Inspired by Sugden (2002; 2009), selecting exemplars using space and time constraints may enable the representation of plausible paradigms. Third, the pragmatic view eventually overlooks concepts that are not singular and exist as systems.

Consequently, pragmatists' frustration and lack of consensus result from inconsistencies around these three focal points. While it is possible to compare pragmatic definitions, it is not clear what those comparisons are supposed to explain. For instance, what does the fact that Weisberg (2013) and Gibbard and Varian (1978) assert that models have structures mean when the authors use disparate and arbitrary – in a Kuhnian context – sets of exemplars?

Time and space constraints allow pragmatic comparisons to explain new variables, such as when and why of scientific concept differences. Considering this, the next chapter will look into the history of economic thought after the mention of the first exemplars of abstract economic modeling. Therefore, time and discipline will be limited and the exemplar will allow the formulation of the concept of economic models between 1920 and 1940.

4 TRANSFORMING ABSTRACT INTO CONCRETE: THE SEMANTIC ROOTS OF ABSTRACT ECONOMIC MODELS

Models are possibly one of the few unanimities of economic thought. From Marxists to Neoclassicists, from Keynesians to Shumpeterians: every modern economist has already used or interpreted an economic model. Somewhat, understanding models is a requirement for contemporary economics. Surprisingly, “economic models” have not always existed. In fact, “model” in economics is an immature term.

The existing connotation of “Economic model” emerged only after the 1930s. Until then, models were almost exclusively a concern of the natural sciences, notably physics. As a consequence, models were far from being purely abstract, having to present a direct relationship with concrete objects or realizable physical theories. Abstract reasoning in economics, back then, received different appellations, such as schemes, systems, and diagrams. Still, if economic reasoning fulfilled the obligations of modeling, then the right to be called “model” was acquired. Thus, both Frisch’s (1933) comparison between economic cycles and a pendulum’s movements, and Williams’ (1934) cardboard production function could be called models. On the other hand, Harrod (1939) and Domar (1946) works, even though currently known as models, have never used the term to refer to their formulations.

Obviously, economists’ necessity of using cardboard or comparing economic facts and pendulums to use the term model does not exist anymore. However, this change provokes an important question: When and why schemes, diagrams, and systems became models? Those are still open quandaries in the history of economic thought and economics’ methodology. The present chapter’s purpose is to look into the history of economic thought, searching for some explanations regarding these changes. The problem will be observed from a semantic point of view. Consequently, the study will be organized in a broad manner, since languages are created in communities rather than by individuals. In the case of the scientific term ‘model’, this communitarian aspect is even more salient, since the term already existed in different scientific approaches. Therefore, the history behind the insertion of the term into economics is somewhat the history of the import and adaptation of a term from a different scientific field.

In this regard, although Duarte and Giraud (2014) affirm that the History of Economic Thought (HET) may be losing space exactly as a result of the use of broad, unspecific methodologies, studying the creation and transformation of vocabulary in archives may not be the adequate solution for the present inquiry. Furthermore, as a semantic analysis has not yet been carried out, it seems plausible to start the research with secondary sources, building a solid basis for future studies of archives. Lastly, this chapter supports a philosophical argument present in the rest of the work: HET, besides being the source of important reflections concerning theory, should also be the source of methodological and philosophical reflections. These considerations were important in the last quarter of the XXth century, with Kuhnian and Lakatosian reviews, but have been losing attention in the recent decades. Hence, it is important to keep in mind that the objective of the chapter is to reformulate a broad view of how the term was introduced - and not necessarily find the specificities of the emergence of the new term - in order to present a proper support for the philosophical content of the thesis.

To complete such objective, the chapter is organized in the following way: Initially, political economy in the thoughts of Mill and Whewell is observed, seeking the methodological and historical roots of the term “economic model”. In the sequence, complementing the political economy basis for the concept of a model’s, mathematics is observed as a foundation of the new connotation. These two sections clarify both mathematics and economics divided into two different ways each. Mathematics has divided into expansionist and formalist aspirations, while political economy has divided into psychological and empirical focuses. The next sections, then, worry about how these different methodologies merged. In the fourth section, it is observed how mathematics expansionist image and political economy’s empirical reasoning merged, resulting in the models of economic cycle. In the fifth section, axiomatic mathematics and economics’ deductive reasoning are observed looking after the sources of general equilibrium economic models. Finally, in the last section, some final remarks are presented.

4.1 Political economy’s views on observation and mathematics

The XIXth century and its methodological tensions determined economics’ professionalization. According to Schabas (2005), the period concurrently regulated economics’ denaturalization. Until the 1830s, economic facts were thought to be part of nature. *Homo economicus* was viewed as any other animal, whose motives were guided through passions. Therefore, natural laws ruled both the economy and its agents, who were not considered independent social beings: “For the eighteenth century economists, economic regularity stemmed, not from the uniformity of individual reason, but from the cohesive nature of human groupings in conjunction with nature" (SCHABAS, 2005, p. 153). As a result, political economy's methodology was analogous to natural sciences methodology, following the inductive reasoning. Newton's classical mechanics, thus, was political economy's methodological paragon.

John Stuart Mill had an important role in transferring economists' view from natural phenomena to individuals. Liberty and individuality were central in Mill's arguments, as the "the only source of any real progress, and of most of the qualities which make the human race much superior to any herd of animals" (MILL, 1976 [1871], p. 444). Thus, liberty and individuality distinguished humans, characterizing them as progressive and superior. Progress and superiority, by their turn, were evident in human's control over nature:

> "Of the features which characterize this progressive economical movement of civilized nations, that which first excites attention, through its intimate connexion (sic) with the phenomena of Production, is the perpetual, and so far as human foresight can extend, the unlimited, growth of man's power over nature. (MILL, 1976, p 235)

Yet, Mill's works were not a complete defeat of previews beliefs:

> In many respects, Mill may be viewed as putting the capstone on the classical theory of political economy. But, in certain fundamental respects, he ushered in our current conception of a denaturalized economic realm. There is, to be sure, a sense in which economists still believe that they study a physical world, even though, when it comes to explanations of production, concrete definitions of capital and labor are riddled with inconsistencies. (SCHABAS, 2005, p. 133).

Therefore, for Schabas (2005), even though denaturalized ideas emerged in Mill's suggestions, classical influences were still preventing a total "revolution". Nevertheless, the seeds of a new economic thought were sown. Nature and its laws were being leftbehind and the human mind was rising as the dominant field of inquiry for economists. "This was a sharp contrast to political economy before 1830, where reason was subordinate to the passions. In the classical theory, the individual mind did not make choices that determined the pricing and distribution of economic goods." (SCHABAS, 2005, p. 140)

From a methodological point of view, the realm of economics became more complex. Agents, instead of merely following nature's laws, acquired laws of their own. Consequently, the economic thought searched for different explanations and methods. As Hausman (1992) points out, once the complexity of social ambiance increased, political economists started to look for "genuine universal laws of *human nature*" as the foundation of their analysis. Introspective exploration of essential economic motives, deprived of disturbances, was XIXth century economists' option for analyzing agents' psychological laws. In Blaug's (1980, p. 56) words: "we should not take the whole man as he is, staking our claim on correctly predicting how he will actually behave in economic affairs. [...] What Mill says is that we shall abstract certain economic motives, namely, those of maximizing wealth subject to the constraints of a subsistence income and the desire for leisure..." .

For Mill, discovered psychological laws were irreducible fundamental laws, which should be the foundation of any economic study. Therefore, the "genuine universal laws of human nature" were solid "*a priori*" knowledge bolstering posterior deduction of economic laws. Roughly, introspection allowed economists to discover psychological foundations, and deduction permitted them to evince economic rationality. Data, observation, and evidence had little importance considering that the laws of theeconomy resulted from deductions. Those who used observation as a source of information were practical and not theoretical economists, who used the "*a posteriori*" method:

> Suppose, for example, that the question were, whether absolute kings were likely to employ the powers of government for the welfare or for the oppression of their subjects. The practicals would endeavor to determine this question by a direct induction from the conduct of particular despotic monarchs, as testified by history. The theorists would refer the question to be decided by the test not solely of our experience of kings, but of our experience of men. They would contend that an observation of the tendencies which human nature has manifested in the variety of situations in which human beings have been placed, and especially observation of what passes in our own minds, warrants us in inferring that a human being in the situation of a despotic king Will make a bad use of power; and that this conclusion would lose nothing of its certainty even if absolute kings had never existed, or if history furnished us with no information of the manner in which they had conducted themselves. The first of these methods is a method of induction, merely; the last a mixed method of induction and ratiocination. The first may be called the method à posteriori; the latter, the method à priori. (MILL, 1844, p. 109)

Even though Mill discussed practical quandaries, De Marchi (1972) contends that Mill did not believe that observation could be a source of review for deductions[8]. Social reality's complexity hampered the analysis of observation:"And here only it is that an element of uncertainty enters into the process— an uncertainty inherent in the nature of these complex phenomena, and arising from the impossibility of being quite sure that all the circumstances of the particular case are known to us sufficiently in detail, and that our attention is not unduly

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[8] Hollander (1985), on the other hand, understands observation as an essential form of reevaluation of economic laws for Mill.

diverted from any of them" (MILL, 1844, p. 115). Moreover, the social sciences for Mill were different from the natural sciences. In Mill's point of view, during the XIXth century, data regarding social phenomena was far from being as reliable as natural phenomena's ones (SCHABAS, 1985).

Consequently, from a theorist's understanding, psychological introspection and deduction were not susceptible to being blurred by unreliable and convoluted observations. Therefore, Mill's proposals opposed to Newton's paragon of science. Psychology was preferred over mathematics and observation was solely a problem for the practicals, having little or no capacity of modifying economic laws[9]. Thus, the rising denaturalized economics was a science of the human mind and not of nature. Obviously, the "*a priori*" method is distant from contemporary econometric models and even from pure mathematical ones. As a result, although logic was an important part of the definition of economic laws, the term "model" was not yet used. The term belonged to the natural science's realm, and some facts were still due to materialize, allowing the term to be imported by economists.

To begin with one of those facts, concurrently to the rise of Mill's ideas, statistics was growing institutionally and practically in urban centers. In the United Kingdom of Mill's time, for instance, between 1830 and 1840, both the F section of the British Association for the Advancement of Science – a section dedicated to the exclusive study of statistics and economics – and the Statistical Society of London (lately renamed as Royal Statistical Society) were created. Among the founders of both societies was William Whewell. Therefore, the author is as in interesting figure to contrast the practicals' and theorists' proposals, as well as the rise of statistics – essential facts bolstering the modification of economics vocabulary.

William Whewell, theauthor of the *History of the inductive sciences*and of*Philosophy of the inductive sciences*, was one of the main opponents of Mill's deductions. Whewell understood that observation could be used beyond the natural sciences (BLUM, 1976). Unlike Newton, who believed in the divine characteristics of phenomena, Whewell believed that observation was formed through the reunion of facts, experiences, and perception. Therefore, for Johnson (2011, p. 400), Whewell's method could be divided into four different parts: "(i) the decomposition of facts, (ii) the explication of concepts, (iii) the colligation of facts, and (iv) the verification of the resulting proposition". Hence, observation was part of Whewell's method at the beginning and at the end of the process.

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[9] Blaug (1980) and Reiss (2013) assert that this disharmony between deduction and observation may prevent economic laws from being falsified.

Whewell's colligation of facts relied upon scientists' imaginative capacity and when formulated was similar to a new inductive process, which used scientists' experience and perception. As an inductive process, observationwas necessarily correlated with his proposed mode of reasoning. Practically, for Whewell, economic laws were discovered by the inductive sum of inductive hypothesis. Consequently, data and evidence were central to Whewell's ideas and approximated economic formulations to current econometric modeling.

Still, even though closer to today's econometric models, the term was not used by the author. Practical's ideas were considered inferior to theorist's, specially in the Victorian age, when Mill's ideas were rapidly spread. Economists preferred diagrams, schemes, and systems to refer to their deductive analysis. Hence, at the end of the XIXth century, although political economy was divided into what concerned its relationship to data and observations, economists' vocabulary was mainly based on psychology and deductive reasoning. Both practicals and theorists discussed mathematics and natural sciences, but the application of their mode of thinking was unimportant. However, the outburst of a mathematical crisis at the end of the XIXth century would reach economists in different ways. First, mathematics transformed "political economy" definitely into contemporary "economics", inserting mathematical reasoning in the majority of it. Second, after mathematizing economics, the relationship between the natural sciences and the social sciences became greater, and economists' language had to be transformed. Its multiple foundations – psychology, mathematics, natural sciences, and deductive reasoning – obliged a modification in the form of communication. Among the alterations, the insertion of the term model occurred.

4.2 Newtonian crisis and the emerging mathematical images

Similarly to economic thought, XIXth century mathematics also had a divided methodology. However, the division was less evident, especially as a result of the hegemony of Newtonian mathematics. Still, as Martins (2012, p. 15) affirms: "since the modern age started there has been a tension between a Cartesian approach to mathematics, and a Newtonian approach to mathematics". The Newtonian approach viewed mathematics as the instrument to discover the truth behind natural phenomena, since geometry was attuned with nature. On the other hand, Cartesian mathematics was an abstract tool used for studying immaterial and infinite concepts.

This mathematical division was, in part, explained through regional divergences during the XVIIth and XIXth centuries. While the European continent was under the influence of Descartes, Victorian England was under the influence of Newton. United Kingdom’s opulence and the esteem of Newton’s discoveries put Newtonian mathematics in a dominant position. In this ambiance, Newton’s methods spread through academic circles and overshadowed alternative methodologies. Along these line, it is interesting to observe how Newton’s ideas spread through Europe.

Warwick (1998) highlights the importance of the “Cambridge Tripos” in the propagation of Newtonian mathematics. The Tripos was Cambridge’s selection mechanism, distinguishing students apt to pursue higher level studies from those who were not apt. The test started at the beginning of the XVIIth century, gradually increasing its relevance throughout the years. Time not only altered the test importance but also modified its requirements. During XVIIIth century the Tripos was an oral test. The reduced quantity of applicants promoted such structure. However, with the increased number of applicants, testing all orally was unmanageable. As a consequence, the Tripos gradually acquired a paper format. This action directed towards aiding the evaluation of a greater quantity of students had dramatic influences on the formation of Cambridge Students. (WARWICK, 1998)

The transition from oral tests to the paper format allowed the focus of questions to be transferred. The preferred philosophical content of oral questions progressively turned into technical contents. Therefore, mathematics, which was already part of Cambridge’seducation, achieved a more important role: “[...] the discipline was especially well suited to a system that sought to discriminate between the performance of large numbers of wellprepared students” (WARWICK, 1998, p. 299). Obviously, the mathematics required was from England’s genius: Newton. The depth of knowledge required was astonishing, being a challenge even for the smartest minds of Victorian England (WEINTRAUB, 2002).

Newtonian mathematics was demanded in its most profound technicalities, changing students’ minds and even the focus of university classes. Progressively the study of Newtonian mathematics became necessary for every student aspiring to participate in the academy. This burgeoning of mathematical reasoning was disseminated to different fields considering that students pursued a myriad of interests beyond mathematics. Therefore, it can be interpreted from Weintraub (2002) that in the XIXth century the idea of a mathematical economics was already alive in the mind of some economists. Still, the natural characteristic of Newtonian mathematics was in disharmony with Mill’s hegemonic ideas of a psychological political economy. According to Blum (1976), any metaphysical or non-phenomenological hypothesis could not be part of Newtonian reasoning, opposing political economy and mathematics. Moreover, statistics institutions were still consolidating during the XIXth century. As a consequence, economic data was not yet totally reliable or even available. Newtonian mathematics could not be properly implemented without qualified data.

Furthermore, confirming the methodological frictions, the term model remained an expression pertaining solely to the natural sciences. Thus, the overt methodological distinctions implied obvious language incongruities. Although economics never became a social physics, at some point similarities had to occur permitting terminology overlaps. In other words, in order to mathematicsbe definitely inserted into economics, and models become a semantic reality for economists, either mathematics or political economy's hegemonic methods had to change. And infact this happened, and the end of XIXth century marked the beginning of the debacle of Newtonian reasoning. According to Weintraub (2002), three distinct difficulties of explanation gradually emerged in mathematics:

> 1) the foundations of geometry, specifically the failures of euclidean geometry to domesticate the non-Euclidean geometries; 2) the failures of set theory made manifest through Georg Cantor's new ideas on "infinity" and 3) paradoxes in the foundations of arithmetic and logic... . (WEINTRAUB, 2002, p. 10)

A crisis was manifested in the mathematical enterprise. Using Corry's (1996) distinction between image and body of mathematics, the crisis not only affected mathematical theorems and definitions (its body), but also affected what mathematics understood as proof, rigor, and its ideals of evolution (its image). More specifically, at least two images of mathematics materialized after the crisis. At the end of XIXth century an expansionist image of mathematics emerged, affirming that mathematics should increase its importance in different scientific fields. At the same time, Cartesian economics evolved into a more sophisticated idea, asserting that mathematics should become self-contained and molded by its inherent logical axioms. The first image developed maintaining an association with the Newtonian method and was bolstered by the evolution of statistics, while the second resulted from Hilbert's works.

Each image evolved following different paths. Yet, both are at the root of the transformation of economics into a mathematical form of reasoning (WEINTRAUB, 2002). Consequently, these images, beyond playing an important role in transforming economics' method, also allowed the concretization of economics as a field of inquiry, transforming economics methodology and making it closer to the natural sciences. This approximation was added to the rise of statistics' practical and theoretical ideas, and allowed the insertion of a new term into economists' vocabulary, the term model. Therefore, a brief definition of the images mentioned above is necessary before explaining the import and adaptation of the term in the next sections.

4.2.1 *Volterra's image*

The incapacity of Newtonian mathematics of answering the problems presented by the crisis and the ascendance of new physics' theories, such as Einstein's and Planck's, at the beginning of the XXth century, infused in some mathematicians the ideal that mathematics should expand its frontiers to adjust to the new times. In Corry's (1996) terminology, this ideal was the mathematician's image of mathematics. Evidently, this image was based on the application of mathematics and should be supported by the evolution of measurement. According to Weintraub (2002), the expansionist image evolved specially in the figure of Vito Volterra. For Volterra, mathematical rigor was completely correlated with the capacity of experimentation, and as some sciences were still incapable of properly testing their theories, mathematics and measurement should attain that to them.

Volterra was not alone in his aspirations. Francis Ysidro Edgeworth and Felix Klein also aspired to spread mathematics in a similar manner (WEINTRAUB, 2002). The authors believed that sciences should maintain physics as the paragon of rigorous methodology. Therefore, the greater the similarity between a particular science and physics, the better. Hence, as in classical mechanics, good instruments of measurement were necessary to advance different scientific ideas. “For Volterra, as for Klein, the need in a field like economics was for measurement. For Volterra, as for Edgeworth, concepts had to be developed that would allow exact calculations, for that was the route to a mathematical science like the physics that was the paradigmatic mathematical science” (WEINTRAUB, 2002, p. 34).

Curiously, though, is the fact that the mathematical crisis was a result, partially, of the difficulties of measuring the “new” physical reality. Einstein's and Planck's ideas could not be measured by the same tools and foundations of the Newtonian mathematics. Perhaps Volterra, Klein, and Edgeworth were unconsciously trying to hide mathematics and physics own flaws by studying different fields. Anyhow, the expansionist image flourished and it was essential in the prospective mathematical economics.

4.2.2 *Hilbert's image*

The second mathematical image emerging at the end of the XIXth century was the formalist image. Volterra's image could hardly correct mathematical imperfections, and can be better understood as the simple propagation of malpractices, since Newtonian mathematics was starting to be selectively used. The second image was diametrically opposed, proposing mathematics to be a self-contained device to properly overcome the crisis. Hilbert's works were at the center of the formalist image.

In order to successfully become an independent tool, mathematics had to separate itself from reality. Therefore, rigor was not to be found outside mathematics, but inside it. Mathematics itself should provide the support for its proofs. In Hilbert's ideas, the inherent axioms of mathematics should be uncovered and studied. Consequently, data and observation should play no role in pure mathematics. Proofs, then, would be based on the consistency and independence of the axioms. According to Weintraub (2002, p. 87), independence "means that each axiom is neither derivable from, nor can be used to establish or prove, any other axiom" , while consistency means "that there is no contradiction to be produced in the theory by assuming the truth of the set of axioms, such that all members of the set are true under that interpretation or model, for if that is the case, then there is no logical contradiction that can arise, no theory based on those axioms will contain an internal contradiction" (WEINTRAUB, 2002, p. 87).

Hilbert's main objective was to prove the consistency of arithmetic. However, Gödel (1931) proved the impossibility of this task. Still, Hilbert's ideals represented mathematics as an instrument for the discovery of scientific truth through logic. Gloria-Palermo (2010) affirms that Hilbert's pupils, then, worked on applying Hilbert's ideas, searching for inconsistencies in different scientific fields. Considering this, unlike Volterra's image, Hilbert's ideals intended to reach different fields to validate them, not the other way around.

4.3 Political economy's preparation for receiving the expansionist image

In order to consolidate the expansionist ideas of mathematics in economics, solid data was required. The measurement of social phenomena should be similar to the measurement of natural phenomena. Therefore, statistics was the connection point between Volterra's ideals and economics. And, in fact, according to Porter (2001), statistics ascended rapidly after the XVIIth century's formation and centralization of states. During this period, the measurement of population and wealth became increasingly necessary for governors wishing to understand their political and economical performances. Also, Newton's success over the century permitted the evolution of practical aspects of measurement.

As a result, when the XIXth century began, statistics was reasonably consolidated. Klein (2001), for instance, highlights William Playfair's works as one important representation of the consolidation of statistical reasoning. Graphical and data analysis were already used as instruments of popular persuasion and even took place in some academic circles. Clavin (2014), by its turn, points out that graphic reasoning was essential for the understanding of the increasing abstractness of mathematical ideas. Hence, although Volterra's image of mathematics would appear solely at the end of XIXth century and Mill's methods were hegemonic in economics, Whewell's ideas and statistics were already being incipiently used for mathematical reasoning in economics.

Jevons is the classical example of how mathematical reasoning was already present in XIXth century political economy. During 1850s Jevons acquired scientific training in mathematics and chemistry and years later opted for studying economics. As one of the possible founders of the marginalist school (BIRKEN, 1988; COATS 1972), Jevons naturally utilized a different method than other thinkers. Already in the middle of the XIXth century Jevons accepted mathematics and statistics as important instruments of economic thought.

Jevons lived during the institutional emergence of statistics, being part of the Manchester Statistical Society. Therefore, his methodology was more attuned with Whewell's ideas than with Mill's. The sum of statistics, economics, and mathematics can be seen in Jevons' work around the value of gold (JEVONS, 1863). Collecting data and manipulating it in graphical forms was an essential part of Jevons work in the middle of XIXth century (MAAS, 2014; PEART, 1993). Nevertheless, he persuasively discussed the problem using a price index and calculating the variation of prices.

Still, deductive and psychological analyses were hegemonic in political economy. As the Newtonian technique was still solid, mathematics was not emphatically spreading to different fields. Consequently, even though the differences between natural and social sciences were reduced in Jevons works; neither statistics nor mathematics was yet prepared for a new mathematical economics. As a result, the term model stood as a peculiarity of natural sciences.

Notwithstanding, Jevons played a pivotal role in the modification of economics' language. In his pioneer demeanors, Jevons was one of the first economists to study the problem of economic cycles, which in the subsequent years would be essential for the consolidation of mathematics in economics and the insertion of the term model in economists' vocabulary. Jevons used his previous knowledge of the natural sciences to study economic cycles, trying to conciliate meteorology and prices. Climatic changes and the solar cycle were the bases of his formulations, which unfortunately did not succeed for a myriad of reasons (MORGAN, 1990).

Starting with Jevons' work, business cycle analysis became more common. Morgan (1990) points out the works of Juglar, Mitchell, Moore, and Pearson following Jevons initial discussion. Still, all of them worked before the ascension of Volterra's image and the consolidation of statistics. As a result, their mathematical formulations were schemes rather than models. Yet, gradually economics was aligning its mode of reasoning with the methodology of mathematics and natural sciences. Also, statistics was being implemented as a reliable source of information for economic analysis.

With the consolidation of statistics and the rise of the expansionist image, the study of business cycles resulted in what came to be known as the economic barometers. These instruments had the objective of measuring the condition of production and commerce just as a barometer measures air pressures. Evidently, in such instruments the similarity between physics and economics was much closer, impacting the methods of economics. Therefore, a semantic bridge permitting the exchange of scientific terms was being created. This allowed the import and necessary adaptation of terms, as is going to be argued below.

4.3.1 *The expansionist image and the origins of the concept of economic models*

Jan Tinbergen was the leader of the Dutch economic barometer project in the second quarter of the XXth century. Similarly to Jevons, Tinbergen had his academic formation in the natural sciences. He had been mentored by Paul Ehrenfest, who supervised Tinbergen's application of concepts from physics into economics. Yet, unlike Jevons, Tinbergen had the opportunity of working and thinking during the mathematical crisis and the consolidation of statistics. Therefore, he was capable of going further than Jevons, using the new images of mathematics and the new statistical instruments.

According to Maas (2014), as the leader of the Dutch project, Tinbergen noticed that the barometer did not have theoretical foundations and, as a result, could not represent causal relationships. Thus, Tinbergen's objective was to transform the barometer into a project serving solely for data recording, becoming the source of information to the study of theoretical problems. Instead of predictive goals, the barometer became a descriptive instrument. (MAAS, 2014)

Unpredictable – or not - circumstances created the perfect ambiance for Tinbergen's aim at combining statistics and the expansionist image in order to study economic problems. Tinbergen:

a) had a unique background in natural sciences;
b) had the necessary social engagement to study economics;
c) commanded the study of an economic *barometer*;
d) lived through the zenith of the expansionist image;
e) counted on consolidated statistics institutions.

These circumstances also allowed him to elicit the modification of economists' language in a lasting and important way. As a consequence of his unusual position in the middle of different modes of reasoning, his work flourished amidst an exuberant semantic hodgepodge. Tinbergen both as a physicist and as an economist could transit through both languages, finding similarities between the modes of thinking and applying imported terms to his own peculiar forms. Therefore, Tinbergen's work had its semantic construction borrowing from economics, physics, statistics, the expansionist image of mathematics and obviously his own background.

Initially, though, Tinbergen's works - just like those of other economists - were schemes rather than models. Therefore, according to Boumans (2005), the use of a model as the definition of Tinbergen's studies occurred through a gradual change of perception. In physics, models were mathematical analogies connecting abstract mathematical formulations with material objects or realizable physics theories. Evidently, physics as a natural science, and specially Newtonian physics, was concerned only with concrete, materializable problems. Therefore, completely abstract formulations were not a reality in physics. Economics, on the other hand, using its psychological foundations and facing complex causality problems, was certainly more abstract than any natural science, what constituted a semantic conflict.

The first step towards the use of the term was basing economic thought on realizable theories. Therefore, economists understood the necessity of concrete, trustful formulations for the use of the term. Frisch (1933), for instance, used the term model to refer to his analogy between pendulum movements and economic cycles. Williams (1934), on the other hand, used the term to define his formulation of a cardboard production function. The perception of economic problems in a concrete form similar to realizable physics' theories and material objects was necessary to name completely abstract economic formulations as models.

The term, hence, was exclusively used to refer to trustful testable analogies. Economics' lack of concreteness, initially, prevented the use of the term without references to physics. However, the development of statistics and of the expansionist image proposed a different perception to economic problems. The consolidation of statistical institutions guaranteed a higher level of reliability to economic data and the expansionist image proposed the application of classical mechanics to different fields without the necessity of demonstrating clearly, as Frisch (1933) or Williams (1934), that such connection was being made.

Tinbergen and his peculiar circumstances created the perfect setting for him to use the term to refer to complete abstract economic formulations as models. Therefore, with the passage of time, "Tinbergen began to experiment with what he called 'schemata', 'mathematical machines' or 'models'" (MAAS, 2014, p. 49). According to Boumans (2005), Tinbergen wrote *Quantitative Fragen der Konjukturpolitik* in 1935: "[This] was the first time an economist used the term model to denote a specific mathematical product of one's empirical research." Tinbergen borrowed the term, but applied an economics identity to it, notably removing the necessity of a material analogy for it.

According to Alberts (1994, p. 300), Tinbergen's notion of models "not only conceptually superseded the notion of applied mathematics, but replaced it in many domains". Tinbergen perceived economic problems and data in a concrete manner, permitting the use of the new term. Economic models were embedded in their own theoretical formulations and based on reliable data. Along these lines, the expansionist image, in unison with Whewell's ideals, allowed the semantic bridge to be created, but statistics' consolidation was essential to the formation of a concrete perception of social reality. Realizable physical theories disappeared from economics, and one type of models was finally created.

4.4 Political economy's preparation for receiving the formalist image

As already stated, the connection between Mill's idea of economics and Newton's mathematics were difficult, given the disharmonies between the psychology of the former and the naturalism of the latter. Notwithstanding, both ideals were hegemonic in the United Kingdom. While it is true that the European isle was dominant in some cultural and scientific aspects, there is no reason to believe that different forms of thinking did not occur, specially in the European continent. In fact, in the continent, French economists were influenced by Cartesian mathematics as well as by Mill's and Newton's notions.

Along these lines, Menard (1980) points out two different perspectives prevalent among French economists. On the one hand, a segment discarded mathematics completely, following Mill's psychological understanding of economics. On the other hand, some tried to apply mathematics to economics. Jean Baptiste Say represented the first attitude towards mathematics and observation. For Say, data and mathematics did not have the capacity of explanation of political economy's conundrums. On the other hand, Cournot can be seen as an example of the second attitude. In Cournot's point of view, mathematics was a good instrument for studying extreme economic cases. Regrettably for Cournot, his works competed with Mill's and Say's hegemonic mode of thinking and had a difficulty to flourish in the XIXth century.

Yet, Cournot demonstrates that French economists were willing to introduce mathematical reasoning into economics. In this regard, Menard (1980) also pinpoints Léon Walras' works as an important form of mathematical economic thought. Interestingly, Walras' formulation was not completely incompatible with Mill's ideas. For both authors, applied and pure economics were different subjects, and applications could hardly modify pure theoretical constructions. Moreover, Menard (1980) affirms that "the rationality of homo economicus had to do, not with the calculation of averages, but with behavioral psychology" (MENARD, 1980, p. 535). Consequently, Walras and Mill agreed that economics was a psychological subject. The main difference lied in the fact that Walras accepted mathematical formulations of theoretical economic problems, using Cartesian mathematics in some of his works.

Cartesian mathematics was attuned with Hilbert's future image of the field. Yet, during the XIXth century Newtonian mathematics was hegemonic and Walras's formulations, analogously to Cournot's, were not always respected. Furthermore, Walras faced the problem of representing psychological concepts using mathematical formulations. This conundrum could hardly be overcome without the proper evolution of a mathematical and statistical innovation. Until the end of the XIXth century, besides statistics not being institutionally and practically consolidated, measurement was uniquely done throughout cardinal conceptions. Cardinal measurement must present measurement unities and must be capable of comparing proportions among its measured objects. This perspective is intrinsic in formulations following Newtonian methods, such as Tinbergen's, once the quality of measurement and comparisons are essential. Without a complete seclusion from cardinal measurement, abstract psychological concepts could not be properly represented.

According to Moscati (2013), ordinal measurement emerged at the end of the XIXth century. As a consequence, quantitative proportions between objects became unnecessary. As a consequence, previous immeasurable concepts, especially abstract ideas, became measurable. Among these concepts, psychological theories, such as utility and preferences, started to find in numbers a form of being represented. Somewhat, psychological concepts, which were totally abstract until ordinal measurement appeared, found in this innovation a way to become concrete even without being factually material. During the end of the XIXth century and beginning of the XXth century, thus, economic's works gradually started to use ordinal understanding for psychological concepts, a benefit Walras did not have.

The incipient Cartesian economics and the rise of ordinal measurement, therefore, created the foundations for the entrance of Hilbert's image in economics. As already seen, Hilbert's mathematics was similar to Cartesian mathematics, and the statistical innovation permitted psychological concepts to be written into mathematics vocabulary. Concreteness was being also created in the most abstract form of economic reasoning. As a result, the connection of Hilbert's image, ordinal measurement and possibly some ideas from physics or the maturation of the term model, allowed formalist economic schemes to fully become models, as is going to be seen in the next subsection.

4.4.1 *Formalist image and economics' models*

According to Weintraub (2002), the development of Hilbert's image in economics was greatly affected by Von Neumann's works, who was Hilbert's pupil. Von Neumann was a mathematician working on formalist mathematics and quantum mechanics. Additionally, he had interests in social problems (VON NEUMANN, 1996). Thus, Von Neumann, beyond being a formalist, had the peculiarities of working on a physics problem and concerning himself with economic issues. He is usually known as one of the founders of game theory, in which he started working on 1928.

His formalist background allowed Von Neumann to face reality problems from a different manner. The axiomatic method implies the evaluation of pure logical concepts, deprived of any meaning or interpretation. Therefore, his studies of quantum mechanics when reaching a pure logical level, were merely mathematical equations demanding the analysis of consistency and independence. As a result, Von Neumann interest in social reality probably took him to alter the interpretation of one of his formalist expressions. In fact, one of his quantum mechanics formulations really received an economic interpretation and came to be known in 1945 as "A Model of General Economic Equilibrium".

Gloria-Palermo (2010) highlights that the history behind his 1945 model is more extensive than a simple reinterpretation. The first steps of the model happened early during the first formulations of game theory in 1928. However, a version of Von Neumann's economic schemes was presented only in 1931 during a seminar at Princeton. The lack of interest in the new complex mathematical ideas left the paper resting until 1934, when it was discussed again in Karl Menger's seminar in Vienna. The presentation had a positive repercussion and the paper was published in 1937 under the German appellation of "Uber Eines Okonomishes Gleinchungsystem und eine Verallgemeinerung des Brouwerschen Fixpunktsatzes". Notwithstanding that, the term model was not yet part of the paper, not even in a German form. The term model and the consciousness of the modeling formulation emerged in 1945, when Kaldor's interest in the paper granted an English translation for the journal *Review of Economics Studies*. The moment the paper was translated, the title changed to "A Model of General Economic Equilibrium".

Von Neumann's model, hence, had to go through a process similar to Tinbergen's model. Hilbert's mathematics had to find an ordinal measurement innovation and an author related to physics and to economics' problems. Consequently, what was once abstract could become concrete and receive the appellation of a model. Therefore, completely abstract psychological concepts were represented in mathematical formulations. However, even though Von Neumann's paper received the name of a model, the name was not defined exactly by the author.

The term model was inserted in the paper solely on its translation. Therefore, although Von Neumann accepted the title, it was not him the one to propose the term. As a result, it is probable that, beyond the relationship with physics, the maturation of the label inside the vocabulary of economists played an important role in the use of the term in Von Neumann's paper. During the 1940s, for instance, Marschack (1941) in a discussion about methodology, called Marshall and Walras' ideas models. Also, Tinbergen (1941) used the term to refer to a work about economic equilibrium "*Unstable and indifferent equilibria in economic systems*". Lastly, Kaldor was the editor of the *Review of Economics Studies'* and, at the time, he already had used the term to refer to some of his works, as in Kaldor (1940) for instance.

By the same token, a complementary explanation for Von Neumann's paper achieving a model status exists. As Mirowski (1989) affirms by extreme propositions, economics at the turn of the century became a social physics, emulating the methods and thus the vocabulary of the natural science. Mirowski's argument in its most extreme understanding may not be real. Still, it is undeniable that scientists coming from physics were inserted into economics. In the case above, Von Neumann worked closely on physics' problems. As a consequence, this connection with physics may be the foundation of the new appellation. However, such assertion is not taking into consideration the fact that the term in economics received a slightly different meaning than the physics one. While models in physics kept their relation with realizable theories, economics' models determined a mathematical analogy with abstract unrealizable ideas[10].

Presumably, both the overlap between the natural sciences and the maturation of the term were behind the new name. Anyhow, the new understanding of social reality certainly allowed the removal of physical and material analogies of economic models, transforming abstract concepts into concrete ideas. In Tinbergen's case, the expansionist image and the consolidation of statistics were at the center of the new view, while on Von Neumann's case the ordinal innovation and the formalist image transformed social reality. As already stated, the term was borrowed, but economics identity was inserted into it. The process of maturation and adaptation thus is the process of using the term to refer to different types of models,

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[10] Remembering Sugden (2002; 2009), economic models are possible worlds and not necessarily real worlds.

searching for similarities in the methods allowing the same reference. These similarities will be observed in the next chapter.

4.5 Final Remarks

Completely abstract economic models, although a universal method of reasoning in contemporary economics, became a reality only after 1930. Previously the term was used for defining analogies with material objects or realizable theories of physics. Therefore, the abstract had to become concrete for the term to be inserted into economists' vocabulary.

XIXth century's economics, both from the point of view of data as from the point of view of theory, was an abstract subject. Statistics was not institutionally, practically, or theoretically consolidated. This was a sharp contrast with natural sciences, where data were reliable. On the theory side, economics was being denaturalized and thus was associated with psychology, a completely abstract issue during the XIXth century. In this ambiance, the term model could hardly be used by economists.

Nevertheless, a mathematical crisis by the end of the XIXth century modified the circumstances. The Newtonian crisis brought with it the ascension of two different aspirations for mathematics. On the one hand, an expansionist image of mathematics was formed, willing to expand mathematical thought to different fields in order to validate Newtonian mathematical methods. On the other hand, a formalist image of mathematics was formed under the leadership of Hilbert, proposing that mathematics should become a self-contained science. Formalism was supposedly capable of identifying inconsistencies in the logical formulations of mathematical theories. As a result, Hilbert's pupils intended to apply formalist ideas to different fields of knowledge, willing to uncover incongruities in the logical foundations of distinct scientific enterprises.

These two images played an important role in inserting mathematics into the method of economics. Moreover, these images, when combined with the consolidation of statistics, determined the insertion of the term model in economists' vocabulary. On the one hand, the formalist image connected with the psychological ideas of Mill's antique proposals, enjoying the rise of ordinal measurement The expansionist image, by its turn, connected to Whewell ideals once the institutionalization of statistics was complete. In the first case, Von Neumann represents the consummation of the different pieces, creating a general equilibrium model. In the second case, Tinbergen combined the other pieces, creating the first models of economic cycle.

Both models understood social reality in a concrete manner, not requiring material analogies anymore. Therefore, Von Neumann and Tinbergen’s models contrasted with previous formulations such as Frisch (1933) and Williams (1934). These result from the fact that both models were the representation of the appropriation of a term coming from a different science, which in the process of being imported was embedded in economics own idiosyncrasies. Furthermore, it is interesting to notice that both models are methodologically different. While Von Neumann is purely abstract and is mainly deductive, Tinbergen’s model uses data to formulate its equation and to validate its conclusions. This characteristic confirms that the term was maturing in economists’ vocabulary and transforming the contemporary semantic notion of economic models. This evolution even allows works which never used the term to be considered as so, such as Harrod (1939) and Domar (1946) works.

5 THE SEMANTIC-PHILOSOPHICAL DEFINITION OF ECONOMIC MODELS IN THE SECOND QUARTER OF THE XXth CENTURY

The second chapter of this dissertation presented Kuhn's post-*Structure* ideas and their operationalization through the theory of Dynamic Frames. Next, the third chapter reviewed philosophical definitions of scientific concepts in the light of Kuhnian theories. The review exposed the necessity of an ordered selection of exemplars, especially in what concerns pragmatic definitions of scientific tools and behaviors. As a consequence, the fourth chapter carefully analyzed the history of economic thought in order to define a plausible paradigm for a semantic definition of abstract economic models. Von Neumann (1945) and Tinbergen (1935) exemplars were presented as the semantic foundations of economic modeling in the second quarter of the XXth century.

Therefore, the fifth chapter intends to combine the previous ideas, defining a frame based on the proposed foundations. Thus, Tinbergen (1935) and Von Neumann (1945) exemplars will be inspected as a form of designating the main characteristics of economic models in the period. In Dynamic Frames vocabulary, the exemplars will be analyzed in a search for values and attributes of economic models.

Moreover, the chapter intends to understand whether the selection of Tinbergen (1935) and Von Neumann (1945) is plausible as a paradigmatic foundation for the term economic model. To accomplish such task, a set of different exemplars of models of the first half of the XXth century will be compared with the proposed characteristics of models. The comparison allows a brief overview of the dissemination of the term among economists in the period as well.

The present chapter is divided into six sections, beginning with this brief introduction. Sections 5.1 and 5.2, in the sequence, analyze the exemplars by exposing their main characteristics. Section 5.3, then, delineates the frame defining economic models. Each attribute is tersely examined in five subsections pertaining tothe section 5.3. Then, section 5.4 presents how models disseminated among economists as a term and as a concept. Finally, some final remarks are presented.

5.1 Tinbergen

Tinbergen’s 1935 model “Quantitative Fragen der Konjukturpolitik” was a set of 18 interconnected mathematical equations. Some expressed definitions, while others were classified as reactions. Thus, through its interconnections there was the possibility of formulating a mathematical structure representing economic cycles. Tinbergen’s academic training in the natural sciences supported this peculiar formulation. Notably, physics was a clear influence in Tinbergen’s works, since he had his academic training in it. He had been supervised by Paul Ehrenfest, who guided him through his application of ideas from physics to social problems, especially economics. Consequently, mathematical reasoning based on physics was a natural mode of thinking for Tinbergen. Then, building the model structure on differential equations from classical mechanics was an instinctive option for him, especially considering the resemblance between pendulum movements and economic fluctuations.

However, unlike theoretical and practical studies of pendulums in physics, whose focus was mainly scientific, Tinbergen was concerned with the social relevance of the study of cycles. Thus, when he opted for studying economics, Tinbergen intended to increase the social utility of his works (ALBERTS, 1998). As a result, his models were aimed towards practice. Along these lines, Hallet (1989) affirms: “Tinbergen’s models were therefore the first of a whole economy and the first to be aimed specifically at policy analysis” (HALLET, 1989, p. 189).

This characteristic was evident in Tinbergen’s works. Already in his first exemplar of model, Tinbergen had a clear idea of the intention behind his works. In the sixth section of his 1935 model he discussed how to manipulate the mathematical structure in order to apply its concepts for solving economic policy quandaries. For Tinbergen, the main benefit of his model was the possibility of applying his work for different practical concerns.

Nonetheless, Tinbergen was aware that his model was a simplification of reality. As a consequence, to maintain his model practical value, Tinbergen affirmed that the balancing of reality and simplification was essential. As a set of simplifications, a model is not the reality:

> Zu diesem Zweck haben wir nun ein Modell der Wirtschaft zu kon- struieren, in dem nur die regelmäßigen Beziehungen bestehen. Eine detaillierte Abbildung der Wirklichkeit würde indessen wegen der großen Zahl der Variabein ein außerordentlich kompliziertes Modell ergeben.[11] (TINBERGEN, 1935, p. 370).

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[11] “To this end, we now have to construct a model of the economy in which only regular relations exist. A detailed picture of reality, however, would lead to an extraordinarily complicated model, because of the large number of variables” (Translated by the author)

Considering models are only a representation of reality, Tinbergen asserted models present a trade-off between representativeness and easiness of use. The simpler a model is, the easier its use become. In contrast, the increased simplicity reduces the model's capacity of representing reality. In Morgan (1990) words, Tinbergen "[...] saw that a successful applied model needed to replicate reality as closely as possible, but that the model would only be amenable to policy analysis if it were relatively simple." (MORGAN, 1990, p. 102).

Thus, for Tinbergen, although assumptions are necessary for a model, those should balance reality and practical relevance. Tinbergen (1935), for instance, opted for the following assumptions as a form representing reality, while also maintaining the possibility of practical applications:

> Es gibt nur Unternehmungen, die Konsumgüter produzierenmit Hilfe von Arbeit und Kapital. Die Arbeiter verausgabenihrenganzen Lohn auf dem Konsumgütermarkt. Die Unternehmer und Kapitalisten, die alseine Gruppe betrachtetwerden, investiereneinen Teil ihres Einkommens und erwerben für den Rest Konsumgüter.[12] (TINBERGEN, 1935, p. 372).

The first assumption assumes the existence of only two production factors, capital and labor. The second defines the spending habits of economic classes, such that workers spend all they earn. Finally, the third delineates capitalists' investment rules in a homogeneous manner. Seemingly, when defining model's assumptions, Tinbergen isolated the characteristics of the economy he was interested in studying (workers' consumption and capitalists' investment), but ensured none of his assumptions was severely disconnected from reality, opting solely for excluding unnecessary problems.

Yet, practical use was not guaranteed by the mere balance of reality and simplicity in a model. The political ambiance of Tinbergen's Netherlands was divided by several opposing parties, which hardly agreed upon policies. As a result, any tool had to be completely neutral in the view of the parties tobe acceptedonly for its scientific insights. Otherwise, parties would discard the instrument without acknowledging its ideas. Therefore, the modeling tradition initiated by Tinbergen had the purpose of being politically neutral as a mode of allowing social benefits of models to spread. In Van den Boogaard's perspective:

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[12] "There are only enterprises that produce consumer goods with the help of labor and capital. The workers spent their whole wages on the consumer goods market. The entrepreneurs and capitalists, who are considered as a group, invest part of their income and acquire consumer goods for the rest" (Translated by the author)

> […] one of the fundamental characteristics of the Dutch modeling practice is that the model is considered to be a neutral device – an umbrella over separate parties – which explains why model outcomes have more credibility than the different interpretations of national economy. (VAN DEN BOOGAARD, 1998, p. 347)

In fact, model neutrality had not to be completely real, neither in a theoretical nor in a political sense (since theory and politics are not necessarily distinct), but the model should be understood as if it were neutral. Therefore, the root of neutrality of Tinbergen's modelswas their empirical basis and the possibility of modifying their characteristics when facing different facts. This capacity allowed Tinbergen's ideas to be applied in different ways. Hence, even though "Tinbergen was clear that theory precedes the empirical, challenging the usefulness of the 'facts without theory' approach" (DOPFER, 1988, p. 678), he accepted the feedback from reality. In other words, Tinbergen's models had to be capable of confronting facts and modifying its ideas according to them.

Neutrality, then, is a characteristic resulting from the fact that models in Tinbergen's point of view are not an end in themselves. He demonstrated this aspect of models in his 1935 exemplar, as he affirmed the model should be observed in different formulations before being used in practical situations. Tinbergen (1935), for instance, contended the necessity of observing distinct models: "Es dürfte sich empfehlen, eine größere Zahl verschiedener Modelle zuuntersuchen, bevor man die Schlußfolgerungen als endgültig betrachte"[13] (TINBERGEN, 1935, p. 371). In the sequence, Tinbergen defended the requirement of adorning the model with more complex ideas: "Indessensollkeineswegsgeleugnetwerden, daß unser Modell ein gegenüber der Wirklichkeit sehrvereinfachtes Schema ist. Die Methode läßt sich jedochaufverwickeitere Fälle ausdehnen."[14] (TINBERGEN, 1935, p. 372).

As Morgan (1990) points out, in Tinbergen's models:

> […] the formation of each individual equation and the particular choice of variables were found by iterating between theoretical ideas and empirical investigations. [...] It should be clear that Tinbergen was not claiming 'statistical testing' of his model here, but 'statistical verification'. (MORGAN, 1990, p. 105).

That is, reality and theory interact in models, creating neutrality. Therefore, although Tinbergen's (1935) model had theoretical content, empirical verification was essential for its practical relevance:

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[13] "It is advisable to examine a larger number of different models before considering the conclusions as final" (Translated by the author)

[14] "However, it is by no means to be denied that our model is a scheme which is very simplified to reality. The method can, however, be extended to more complicated cases" (Translated by the author)

> Das Ziel ist die Bestimmung der in den Gleichungen angesetzten Koeffizienten (im Text durchgriechische Buchstaben angegeben) bzw. die Ersetzung der Gleichungen durchwirklichkeitsnähere und die Bestimmung der in diesenenthaltenen Koeffizienten.[15] (TINBERGEN, 1935, p. 382).

Neutrality based on the possibility of modification and the trade-off between reality and simplification were so important in Tinbergen's modeling practice that those were the essence of his speech for the Sveriges Riskbank Prize in Economics. Tinbergen (1992 [1969]) named 'refinement' the process of "the introduction of many more variables" into models. In his ideas, refinement is absolutely necessary to guarantee the practical validity of models. However, refinement must be carefully donein order to maintain the relation between models and reality intact. The practical relevance of models should not be lost due to a disconnection from reality. Consequently, empirical verification must occur when modeling.

5.2 Von Neumann

Von Neumann (1945) model established an innovation inside economic reasoning. While mathematical inquiries regarding economic equilibrium were supported "by the mere counting of numbers of equations and unknowns" (VON NEUMANN, 1944), Von Neumann (1945) exemplar studied equilibrium searching for the logical consistency of equilibrium assumptions. Therefore, Von Neumann (1945) was concerned with discovering whether equilibrium was really possible. In order to achieve his objectives, Giocoli (2003) affirms: "Von Neumann kept his argument on a strictly formal basis and employed twice a non-constructive demonstration technique called 'indirect proof method" (GIOCOLI, 2003, p. 227).

Thus, Von Neumann's mathematical method introduced an innovation. Instead of normal formulations, Von Neumann based his inquiry on formalist indirect proofs. According to Punzo (1991): "The novel formulation of the mathematical problem required the abandonment of the algebraic (even simply arithmetic) treatment and a plunge into combinatorial and topological methods..." (PUNZO, 1991, p. 9). Before his model, axiomatics was almost exclusively used in the natural sciences. Therefore, Von Neumann's peculiar mathematical background imported the new mathematical formulation.

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[15] "The objective is to determine the coefficients applied in the equations (indicated by Greek letters in the text), or to replace the equations by means of more realistic coefficients and to determine the coefficients contained in them" (Translated by the author)

Unlike Tinbergen, Von Neumann was a mathematician and not a physicist. In fact, von Neumann was an eminent mathematician researching especially Hilbert's formalism. According to Giocoli (2003, p. 227):"Hilbert and his school had made the mathematical models somehow autonomous from their empirical substrate; this entailed that a formalist model could be validated only internally, by proving non-constructively the absence of any inconsistency". As a result, Von Neumann produced models in a uniquely formalist way. However, even though based on a different mathematics, formalist models were still simplifications of reality. Thus, assumptions were an essential part of Von Neumann works.

In this regard, Champernowne (1945), when commenting on Von Neumann's (1945) formulation, affirmed that the general equilibrium model adopted "extremely artificial assumptions" or "drastic simplifying assumptions". This abuse of simplifications was not unnoticed. Cabral (2003) highlights that the model was extremely criticized for its unconscious suppositions, specially for assuming a non-monetary economy. Still, without such extreme simplifications, the proof of consistency would be constrained. Equilibrium should be transformed into tractable axioms to allow for the analysis of consistency.

As a result, even though worried about the simplifications, Champernowne (1945) underlined the elegance of the mathematical formulation. For him, the mathematical quality of the work could compensate the unrealism of the assumptions, since the main objective of Von Neumann's work was theoretical. Indeed, Von Neumann (1945) purpose was not practical, considering that Hilbert's pupils were more interested in uncovering the axiomatic foundations of scientific ideas than using their mathematics in practice.

However, even though applying extremely artificial assumptions, Von Neumann modeling practice for economics did not defend a complete disconnection from reality. The context in which his model was formulated compelled a dissension from Hilbert's drastic non-empirical aspirations for formalism. The first configuration of Von Neumann (1945) model appeared as a quantum mechanics application in 1928. Then, prior to his final economical formulation in 1945, the model was molded throughout the years (KJELDSEN, 2001). During this maturation process, the axiomatic method applied by Von Neumann was contested. Gödel (1931) highlighted the impossibility of proving arithmetic's logical consistency through purely logical sets of axioms. The proof even led Von Neumann a few years later to affirm:"Gödel has shown that Hilbert's program is essentially hopeless" (VON NEUMANN, 1961 [1947]). Before Gödel's proof, the axiomatic method intended to find axioms completely destitutedfrom empirical content. After it, the confidence on purely logical axioms devalued.

As a consequence, the initial proposal of 1928 for quantum mechanics had to change tobe presented as an economic model in 1945. The empirical basis became a reality for the axiomatic method. Therefore, Von Neumann (1961), already accepting a different kind of formalism, claimed: “As a mathematical discipline travels far from its empirical source, or still more, if it is a second or third generation only indirectly inspired by ideas coming from ‘reality’ it is beset with very grave dangers, it becomes more and more purely aestheticizing, more and more *l’art pour l’art*.”

Along these lines, although apparently unreal, Von Neuamman’s assumptions were not unprovided with empirical content. For Rashid (1994, p. 289) the relation with observed reality was inserted into Von Neumann’s ideas through the intuitive derivation of the axioms: “[...] he [Von Neumann] seemed to feel very strongly that in economics, as in physics, in order to derive meaningful explanations one must know very specifically what there is to explain” . Still, Rashid (2007) highlights Von Neumann had profound difficulties in discovering realist axioms for economic research, frequently becoming frustrated with the task.

As a result, according to Rashid (2007), Von Neumann’s relationship with economics was not rigid. Unlike his mathematical and physical concerns, economics was somewhat a hobby for Von Neumann. Thus, his lack of rigidness and his theoretical concerns eventually dismissed assumptions from being excessively complicated, allowing the mathematical ideas to flow more freely. Still, Von Neumann discarded models which could not be interpreted in real terms. As Gloria-Palermo (2010) contends:

> A prominent characteristic of hilbertian formalism is without doubt the strict separation between syntax and semantics. To formalize a theory in the sense of Hilbert means indeed emptying it from all its semantic content and giving an abstract representation of it. (GLORIA-PALERMO, 2010, p. 165)

For Hilbert, detachment from reality was essential for consistency proofs and the interpretation was unnecessary. The logical level of reasoning was Hilbert’s only concern. On the other hand, for Von Neumann, after mathematical proofs, axioms and models should be capable of being interpreted beyond logic. Therefore, models should be fulfilled with empirical content in their interpretations. As the author points out:

> This means that the criterion of success for such a theory is simply whether it can, by a simple elegant classifying and correlating scheme, cover very many phenomena, which without this scheme would seem complicated and heterogeneous, and whether the scheme even cover phenomena which were not considered or even known at the time when the scheme was evolved. (VON NEUMANN, 1961)

Therefore, models should adapt according to the phenomena they intend to explain, but they also should be capable of adapting to unintended situations. In Formica (2010, p. 489) words: “the model has to manifest its formal adaptability for further correct extensions”. Thus, the model has to be capable of being theoretically modified in a search for different interpretations. Real and unreal assumptions can be used in this process, provided that the interpretations are always aiming at reality. For Teixeira (2000) this adaptability allures scientists, once its possibilities are infinite. The model can always become more complex.

5.3 Frame

Once observed the two exemplars of models, it is possible to delineate a concept of a model based on their evident characteristics. The main characteristics of Tinbergen’s (1935) model are:

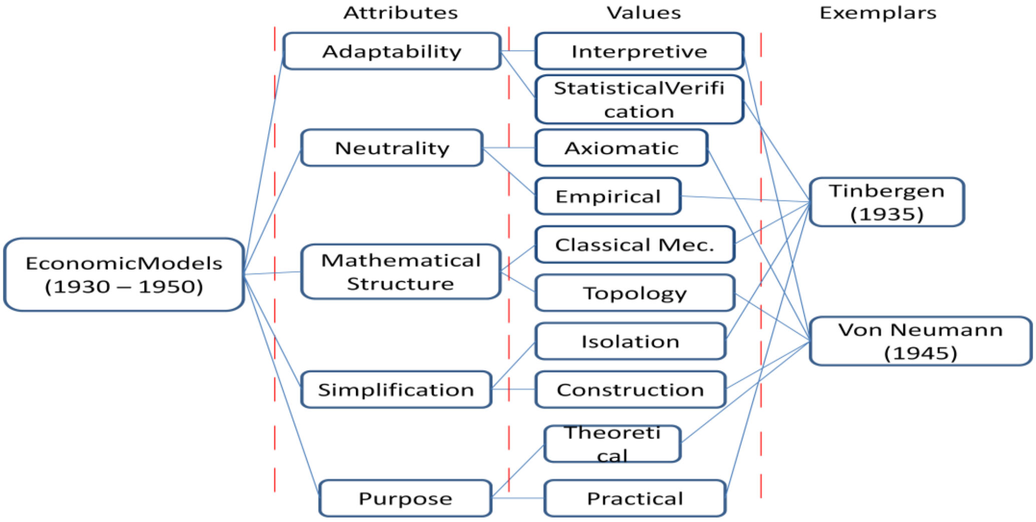
a) the model uses mathematical expressions to communicate, specially differential equations related to classical mechanics;
b) the purpose of the model is practical, intended to be used as a tool for policy analyses;
c) the simplifications are isolations of essential aspects of the problem at hand;
d) the model is neutral as a result of its empirical content;
e) directly connected to the fourth characteristic, the model adapts according to facts and statistical verification.

In what concerns Von Neumann’s (1945) model, the following aspects are evident:

a) topology and logic are the main forms of reasoning;
b) the purpose of the model is theoretical;
c) artificial assumptions are commonly found in its formulations;
d) the model is neutral as a consequence of its axiomatic formulation;
e) the model adapts to different interpretations.

Combining the characteristics of both models, the following frame can be presented:

Fig 5. The definition of Economic Model 1930 - 1950



Source: prepared by the author (2017).

According to the exemplars' characteristics, five attributes were defined: mathematical structure; neutrality; adaptability; simplification; and purpose. Each attribute has two correspondent values connected to their appearance in Tinbergen (1935) and Von Neumann (1945) exemplars. The attributes define abstractedly what economists searched in economic reasoning during the second quarter of the XXth century as requirements for classifying economic models. Values, thence, specify the manifestation formats allowed for the attributes. Consequently, works in order to be considered models could manifest their values in mixed compositions and even minor peculiarities could exist, provided that the attributes were identifiable. Therefore, considering the relevance of attributes in categorization, the next five subsections will discuss briefly each one of the attributes.

5.3.1 *Mathematical Structure*

Both Tinbergen and Von Neumann opted for mathematical structures to sustain their economic reasoning. However, while Tinbergen (1935) used mathematics based on classical mechanics, Von Neumann (1945) opted for topology and logic. Therefore, following Arnold (1989), any economic reasoning applying mathematical formulations based on Newtonian, Hamiltonian, or Lagrangian mechanics could be cogitated for a classification among economic models, since all three fields are part of classical mechanics. Along these lines, Franklin defines topology as: "the most general and most fundamental branch of geometry" (FRANKLIN, 1935, p. 39). As a result, mathematical rationality applying logical forms of geometry was also creating mathematical structures as in models. Thus, both topology and classical mechanics were acceptable manifestations of mathematical structures for models, although alone they did not define modeling.

In the light of the above frame, while themathematical structure was not sufficient for categorizing modeling, economic models in the second quarter of the XXth century still had to have mathematical structures. Non-mathematical formulations were schemes, diagrams, and alternative synonyms, but not models. Such judgment is comparable to previous observed views on models, which also understand that models are necessarily constructed as mathematical formulations. Gibbard and Varian (1978), for instance, affirmed that economic models have a structure which assumes one unique value: logic-mathematical. Weisberg (2013), by its turn, defines the structure of models in terms of three different values: computational, mathematical, and concrete. However, Weisberg (2013) is aware that all three values could be reduced to a single one, mathematical, since mathematics generally supports concrete and computational formulations.

Thomson-Jones (2012), however, contends that models may be more than just mathematical structures. In his perspective, models are merely a set of prepositions, which are not necessarily mathematical. Although a plausible allegation for contemporary concepts of model, it seems that economic models in their initial formulations had compulsory mathematical forms, as indicated by the two exemplars observed. Thus, when referring to models, economists were referring to reasoning bolstered by mathematical structures. Whether models must meet the requirement of mathematical structures is one direct problem of exemplar-dependence. When observing solely Tinbergen’s and Von Neumann’s exemplars, mathematic is undeniably a necessity. Therefore, Thomson-Jones (2012) affirmation may be an outcome of his singular selection of exemplars, which probably considers contemporary exemplars beyond economics.

5.3.2 *Purpose*

When observing Tinbergen’s (1935) and Von Neumann’s (1945) models, it is clear that both authors had defined purposes when formulating their ideas. In Tinbergen’s case, the purpose was practical as long as the model intended to be a tool helping in the analysis of policy, whereas in Von Neumann’s exemplar the objective was theoretical, seeking to uncover the mysteries of equilibrium. In frames vocabulary, models in the second quarter of the XXth century must have manifested the attribute “purpose” in any of two values: “theoretical” or “practical”.

Although the term purpose is not generally used to define a characteristic of models, several authors have already referred to similar aspects of modeling. Morgan (2002), for instance, points out that models are created with the unique intention of solving questions. Gibbard and Varian (1978), in the same sense, understand models as structures constructed for finding the solution to questions formulated as: “what would happen if such and such were the case?”. Thus, both for Morgan (2002) and Gibbard and Varian (1978), models have purposes, although more specific than Tinbergen’s and Von Neumann’s ideas: the purpose of resolving problems.

Morrison and Morgan (1999), similarly, point out that models have what the authors called ‘a function’.Yet, implicitly in their mode of argumentation is the fact that a function is the purpose of the model, since “function” is a synonym for “purpose”. In the same implicit manner, Weiberg (2013) affirms that models have targets. Therefore, models are not formulated arbitrarily, but with some specific target or, in other words, purpose. Finally, Hedoin (2012) is one of the few authors to affirm that models have purpose using this term directly.

As a consequence, despite purpose not being the common term found in the literature, the existence of some kind of direction for models seems to be a consensus. The pragmatic view of theories generally defines models as a subpart of theories, more specifically the application part. Therefore, while theory may exist without a defined purpose, models as the application of a theory to a theoretical or practical problem must have a defined objective.

When analyzing Tinbergen’s and Von Neumann’s exemplar, this characteristic becomes evident. Even though the scope of purposes is substantial, being both theoretical and practical in any sense, any economist applying the term model to define an economic reasoning in the second quarter of the XXth century would see a purpose behind the formulation.

5.3.3 *Simplification*

Indifferently whether models intend to explain practical problems or theoretical problems, they can represent neither reality nor theories completely, considering the fact that the complete representation would be the theory or the reality themselves. Therefore, models naturally use simplifications when representing. However, what exactly are those simplifications is still an open question.

Von Neumann, as observed in Champernowne's (1945) comments, used drastically artificial assumptions. Tinbergen, on the other hand, opted for more realistic simplifications. Clearly, the nature of their assumptions differed. Considering the discrepancies, a separation between isolations and constructions may explain the main modes of expression concerning the simplifications occurring in the models of the period[16].

For Mäki (1992; 2009) isolations are assumptions which follow the pattern of the natural sciences. An illustrative example is the case of gravity. When modeling gravity, a physicist determines the resistance of air as nonexistent. Therefore, the study of the force of gravity is isolated from interferences. Thus, isolation is merely the removal of aspects interfering in the examination of the object of study. Tinbergen, having an academic training in Physics, probably saw his assumptions analogously to the assumptions in the natural sciences. The appreciation of Tinbergen's simplifications as isolations seems to describe properly how his assumptions should work.

On the other hand, constructions are different from isolations. For Sugden (2002; 2009), beyond simply isolating factors of influence, economists eventually build completely artificial assumptions unconnected with empirical reality. As a consequence, fictional worlds are created, not just excluding characteristics from reality, but also adding nonexistent components. In this regard, Von Neumann's theoretical purpose eventually dismissed strict connections with reality. Therefore, his simplifications were revealed as constructions or "drastic artificial assumptions".

Still, both isolations and constructions are forms of simplifying reality or theories in order to explain some specific quandary regarding them. Their limits are not clear, and overlaps may occur. For instance, while it is possible to understand constructions as encompassing isolations, since isolation can be understood as a simplified form of construction, Mäki (2009) argues in the opposite direction. In his perspective, even the most unreal assumptions are just the withdrawal of interferences. The following example explains Mäki's point of view:

> In assuming perfect information on the part of economic agents, a model appears to add a feature that does not obtain in the real world: an excessively powerful mental

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[16] This brief overview was limited to the debate between isolations and constructions. However, simplifications may also be understood as idealizations. About them, important references are Weisberg (2007; 2013) and Wimsatt (2007).

> capacity seems to be attributed to the model agents. However, it seems to me that the correct reading of the function of this idealizing assumption is that it is used to remove certain real-world features from the model world: the search, acquisition and processing of information. (MAKI, 2009, p. 31)

In fact, a clear segregation of both may not be possible. Nevertheless, isolations and constructions are debated as distinct forms of simplification in the literature, which secure the existence of some level of difference between both. Moreover, it is possible to consider two different arguments favoring the existence of constructions. First, Grüne-Yanoff's (2009) ideas that economic models are similar to fictional works imply the existence of constructions, otherwise fictions would not be the correct resembling object. Second, Knuuttila's (2009) conclusion that researchers frequently have their result pre-determined suggests that scientists would be willing to construct their simplifications in order to achieve their goals.

5.3.4 *Neutrality*

A peculiar characteristic observed in both models is the fact that both are neutral. In Tinbergen's case, the model is theoretically and politically neutral as a result of its empirical basis, while in Von Neumann's case the model is neutral as a consequence from its axiomatic formulation, which allows infinite interpretations. Therefore, the exemplars of models are neutral modes of reaching their objectives. The mathematical methods played an important role in the neutrality aspect of modeling. Formalism guaranteed that at some stage the model would be completely logical and destituted from any interpretation. On the other hand, the empirical basis ensured that the model would be applied to practical concerns based on data. The reality of facts could overcome political and theoretical influences. As a consequence, for an economic reasoning to be considered a model, some type of neutrality should be confirmed. Normally, this neutrality was directly related to the capacity of adaptation of the model, which certified the model was not serving a persuasive purpose, but a dynamic scientific endeavor.

Moreover, it is interesting to notice that in both cases models are not necessarily neutral in reality. In fact, models have solely to be perceived as neutral for those who wish to rely upon their conclusions for theoretical or practical uses. Therefore, in Tinbergen's model, mathematics and adaptability guaranteed that policy makers would see his tool as a neutral device capable of being applied to social problems. Von Neumann's model, similarly, had to be understood as a neutral logical form for theorists who had the necessity of interpreting its axioms in different forms according to their intended applications.

5.3.5 *Adaptability*

The last attribute is adaptability. The concept of model based on the examplars of Tinbergen and Von Neumann does not have a fixed formulation As observed above, neutrality is an important characteristic of models and for the model tobe understood as neutral, the capacity of being adaptable to different situations is a necessity. In Tinbergen's (1935) model, alterations were possible considering that the model allowed statistical verification. Therefore, both parameters and the set of equations could be modified according to the facts. Von Neumann's (1945) model was adaptable since axiomatic formulation permitted innumerous interpretations. Therefore, neutrality for economists' in the first half of the XXth century was perceived when a model could modify its assumptions and results in the face of new facts or interpretations.

Consequently, in both cases refinement was a precondition for the models, such that in fact every model was the beginning of a new set of models and not an end in itself. Remembering Popper's (1974) ideas, theories must be falsifiable. However, not all theories are falsifiable. As a result, theories differ in their levels of testability. The higher the level of testability reached by a theory, the higher the possibility of it being falsified or accepted. Therefore, for Popper (1974), theories must increase their levels of testability. Assuming this is also a necessity for models, adaptability can be understood as if a model is always pursuing a test. Thus, adaptability is the pursuit of falsification[17], such that every modification is a refinement of the model. Netto's (2016) ideas are similar to this view. According to him, models adapt as a form of searching for new evidence. As a consequence, the model increases its credibility allowing it to be perpetuated through time. In any case, considering the proposed frame, adaptability is necessary for the models to be perceived as neutral. Still, reasons for why adaptability and neutrality occur are not yet clear.

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[17] Analagous to this pursuit, Robustness analysis affirms that models should be tested in different formulations in order to reveal the impact of their assumptions on conclusions. Regarding robustness analysis in a broad philosophical view see: Wimsatt (1981), Weisberg (2006). Concerning robustness analysis in economics see: Kuriokoski, Lehtinen and Marchioni (2010)

5.4 The credibility of Von Neumann' and Tinbergen's works as exemplars

While a frame is a dynamic structure, which may have its arrangement modified during the interaction with different exemplars, Kuhn contended that paradigms are at least partially static as a result of an exhaustive examination of communitarian exemplars. Innumerous concomitant concepts would exist without some degree of immobility. Different foundations of exemplars would hamper communication, as defined by incommensurability. Yet, even though Kuhn's proposal is theoretically credible, in practice all scientists of a same community would hardly have access to the same exemplars in the same period of time. As a consequence, an analysis of how the term and the concept disseminated may demonstrate whether there were differences in what economists understood as models in the first half of the XXth century. Concomitantly, this analysis may evince whether Tinbergen (1935) and Von Neumann (1945) really constitute a credible paradigm for the economics of the period. As contended in the second chapter, the selection of exemplars must be plausible, and plausibility may be acquired with a comparison manifesting the fitting of attributes and values in a few works of the period.

Thus, a historical comparison of the frame with works of the period is an interesting form of evaluating the credibility of the frame. Along these lines, in the following three subsections, two types of different works are going to be analyzed: works which used the term in the period and works which did not. Three results are possible from such examination. First, the proposed frame may not represent what economists understood as models. Second, although not being a perfect representation as a result of regional or time differences in the assessment of exemplars, the proposed frame symbolizes the meaning of economic models in the period. Minor distinctions, in this case, suggest the existence of different communities inside economics. Third, the frame represents perfectly the concept of a model in the period, being a close approximation of economists' tacit agreement. Moreover, the examination of different exemplars may expose the roles of the community in the dissemination of the concept and the term.

5.4.1 *Disseminating the idea and the term*

Meade (1937) presents itself as a good example for comparing modeling practice in the second quarter of the XXth century with the proposed frame. Meade (1937) study uses the term model in its title. Moreover, his model does not have an analogy with concrete objects or theories from physics. Therefore, Meade's model is clearly in the track of abstract modeling as proposed by Tinbergen's and Von Neumman's exemplars.

Right from the outset, Meade asserts:

> The object of this article is to construct a simple model of the economic system discussed in Mr. Keynes' 'The General Theory of Employment, Interest and Money', in order to illustrate: (i) the conditions necessary for equilibrium; (ii) the conditions necessary for stability of equilibrium; and (iii) the effect on employment of changes in certain variables. (MEADE, 1937, p. 98).

Therefore, Meade is defining his model purpose, which is definitely theoretical. Consequently, the attribute purpose receives the value theoretical in Meade's work.

In the sequence, the author also enumerates the simplifications of his model. However, whether the simplifications are constructions or isolations is not clear. His first simplification is "there is a closed economy", which is possibly a simplification aiming at isolating his object of study from interferences of different forms of trading. On the other hand, in his sixth assumption, Meade assumes that the fixed capital in the form of equipment has an infinite durability. This assumption cannot be simply defined as an isolation, given that its fictional character is evident. Somewhat, Meade's assumptionsare a combination of Tinbergen's isolations and Von Neumann's constructions. In fact, as pointed above, distinguishing between isolation and construction is difficult. Therefore, Meade's reasoning manifests simplifications, even though indicating exactly their format is not possible.

Following the purpose and the simplification, Meade adopts a mathematical structure. While in the proposed frame the theoretical objectives were related to topology, Meade conciliates his theoretical objective with classical mechanics. Thus, unlike Tinbergen, Meade does not use any statistical verification in his model. However, this does not impede the model of being adaptable. In his first footnote, Meade affirms that his simplifications could be transformedin order to permit different interpretations. As a consequence, Meade's model is adaptable in the sense of Von Neumann. As asserted in the fourth section above, neutrality is a direct result from adaptability and thus Meade's model could be perceived as neutral. Hence, his mathematical application, although not topological, is based on logic and can be transformed for creating new examinations.

Therefore, Meade's (1937) model seems to be a proper application of the attributes and values of the proposed frame. His structure and simplifications follow more closely Tinbergen's exemplar, while his objective, adaptability, and neutrality are connected to Von

Neumann’s exemplar. It is interesting to notice that Meade’s application is neither a perfect copy of Tinbergen’s nor Von Neumann’s values, but is completely based on their attributes. This resemblance with the attributes of the frame allows the nomination of his reasoning as an economic model. In other words, Meade’s work pertains to the same ontological genus as Von Neumann’s and Tinbergen’s exemplars.

A second appealing exemplar is Kaldor’s (1940) model, which also has the term model in his title. In his words, the purpose of his application is:“[...] to show, by means of a simple diagrammatic, apparatus, what are the necessary and sufficient assumptions under which the combined operation of these two forces [multiplier and investment demand function] inevitably gives rise to a cycle.” (KALDOR, 1940, p. 78). Therefore, like Meade, Kaldor’s work is a model with the attribute purpose represented by the value “theoretical”.

In order to achieve his objectives, Kaldor opts for a mathematical structure built using differential calculus and, thus, classical mechanics’ concepts. As it is normal to any type of model, Kaldor creates simplified assumptions to transmit his ideas. For instance, Kaldor assumes that the demand for capital increases with the level of production, a plausible, nonfictional assumption. Furthermore, Kaldor complicates his model during his exposition. While initially savings and investment were simple mathematical formulations presented as lines in a graph, at the end of the examination the mathematical and graphical formats were more sophisticated.Therefore, Kaldor gradually approximated his model to reality, adapting and making it neutral.

Kaldor (1940), therefore, is an application similar to Meade’s. Analogously, Kaldor’s model uses combined values of Tinbergen (1935) and Von Neumann (1945), achieving common attributes. His mathematical structure is not topological, while the objective is theoretical. His simplifications are mostly isolations, while adaptability and neutrality are related to Von Neumann’s ideals.

The third exemplar worth analyzing is Edelberg’s (1936) model. Unlike the previous two, Edelberg’s exemplar is an econometric model. As an econometric application, the purpose of the model is not theoretical, but practical. Edelberg (1936) intended to calculate the wage elasticity of the demand for labor in the United Kingdom. Evidently, the model was based on a mathematical structure capable of being employed with statistical methods. Thus, the model followed closely Tinbergen’s exemplar, organized as aset of differential equations formulated according to simplifications. Adaptability and neutrality resulted from statistical verification.

As in previous examples, Edelberg combined constructions and isolations as his simplifications. For instance, the simplification “one kind of labour, one kind of land, and one kind of consumption goods” (EDELBERG, 1936, p. 210) is plausibly an isolation, eliminating possible interferences. On the other hand, Edelberg aggregated labor, land, and capital into one single function as if the three were commensurable. However, as Felipe and McCombie (2013) affirm, this assumption is severely unreal and has several implications on conclusions. Therefore, unlike Kaldor (1940) and Meade (1937), Edelberg (1936) only combination is constructions and simplifications. Adaptability, neutrality, mathematical structure, and purpose are all manifested as in Tinbergen’s ideals.

When observing Kaldor (1940), Meade (1937) and Edelberg (1936), the credibility of the proposed frame seems to be high, although further analysis of different kinds of exemplars is still necessary[18]. The term model, the attributes, and the values are used by the authors in the same sense as the frame formed by Von Neumann’s and Tinbergen’s. Thus, according to this initial historical analysis, the dissemination of the term and the idea occurred concomitantly for this group of economists. Furthermore, none of the examples is an exact copy of the proposed examplars, which was expected since a dynamic frame is not a strict definition, but a definition opened to many arranges of similarities.

5.4.2 *Community and translations*

According to the previous subsection, Tinbergen’s and Von Neumann’s models represent a credible paradigm for the definition of the concept of a model during the second quarter of the XXth century. All the three examples analyzed used the term and presented the attributes necessary to be defined as abstract economic models. However, although the new meaning of the term was already spreading through the field of economics, the concept of model eventually existed without the terminology or in obsolete forms. The dissemination of the exemplars is inharmonic in a community and thus some members are more embedded in the new paradigm than others. Moreover, the acceptance of the new paradigm is not granted for all members.

Therefore, during the transition of paradigms the insertion of some works into the new paradigm may occur through the community and not necessarily through a volunteer option of

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[18] The selection of case studies was realized in credible manner, searching for works which used and did not used the term model during the period of analysis. Still, the credibility of the selection may be questioned and different ways of testing the validity of the frame may be necessary.

the scientist. As it is going to be argued henceforward, the dissemination of the frame in a disharmonic manner obliges scientists to categorize works according to their resemblance with the frame, even though they do not use the term. When works meet the required attribute, scientists may apply translations, transforming the previous nomination of the works and including them on the paradigm. In the case in question, economists apply translations understanding non-modeled works as modeled.

Edelberg (1936), at the end of his exposition, compared his model with Cobb and Douglas's (1928) formulation. In fact, both works are similar, using analogous mathematical and statistical methods. Yet, Cobb and Douglas (1928) did not use the term model over their argumentation. Nevertheless, Edelberg (1936) considers both works pertain to the same genus, what allows a satisfying comparability. Cobb and Douglas (1928) work originated the famous "aggregate production function" as a result of a mathematical exposition based on simplifications. Their initial propositions have been applied to innumerous different works, being adapted to infinite situations. Therefore, the methodological similarities between Edelberg (1936) and Cobb and Douglas (1928) probably guarantee the second work is also a model, even though without the terminology.

Like Cobb and Douglas (1928) case, additional works not named models, but which did not present the correct attributes, were observed – and translated - by economists of the period. Marschak (1941, p. 446), for instance, during his discussion regarding economic method affirmed: "But his [Mitchell's] work has the additional interest of formulating, albeit tentatively, a comprehensive system of elementary postulates: a 'dynamic model'. Methodologically, it has the same character as those static models familiar from Marshall and Walras" (MARSCHACK, 1941). Thus, Marschak assumes that the works of Walras, Mitchell, and Marshall were models. However, none of the authors used the term model to refer to their formulations. Consequently, what Marschak does is applying the new term to previous works. Therefore, Marschak goes beyond Edelberg and effectively effectuates a translation.

Schumpeter (1941), in the same manner, translates previous works using the new term: "The two parts of Walras' elementsd'economie Politique Pure were published in 1874 and 1877. These contained the theoretical skeleton of the static model in question much more fully than did Marshall's Principles." (SCHUMPETER 1941, p. 239). Hence, like Marschak (1941), Schumpeter assumes that previous works are models since their attributes allows them to be inserted in the frame. Therefore, Marshall's and Walras's works were models according to the second quarter of the XXth century comprehension, but this does not imply that they have always been or will always be considered models. Marshall (1890), for instance, used the term model one single time in his exposition, and it was not even in an economic context. In his work, Marshall preferred the term diagram to refer to his mathematical structures and simplifications. Thus, when the term disseminates, the mere comparison of attributes allows translations to occur, transforming the terminology used to refer to previous works. Then, it is the community who categorizes the works according to the frame.

Nevertheless, not solely previous works can be translated. The idea of modeling, as observed in the translations of works preceding Tinbergen's and Von Neumann's, was already part of economists' mindset. Therefore, while the frame was consolidated in Tinbergen's and Von Neumann's exemplars, different works were produced without using the terminology, but presenting the necessary characteristics to be considered as models. An interesting illustration lies again in a review of the General Theory. Concomitantly to Meade (1937) model, Hicks (1937) also realized a rereading of Keynes. However, unlike Meade (1937), Hicks opted for the terms 'theory' and 'apparatus' to indicate his mathematical reasoning.

Hicks (1937, p. 148) used the term model just once: "In these circumstances, it seems worthwhile to try to construct a typical 'classical' theory, built on an earlier and cruder model than Professor Pigou's." Still, Hicks's (1937) apparatus presents all the characteristics of a model. Initially, he highlights his theoretical purpose: "In order to elucidate the relation between Mr. Keynes and the 'Classics' we have invented a little apparatus." (HICKS, 1937, p. 156). In the sequence, simplifications are enumerated as the basis for his differential calculus formulation. For example: "I assume homogeneous labor. I assume further that depreciation can be neglected, so that the output of investment goods corresponds to new investment." (HICKS, 1937, p. 148). His simplifications resemble constructions considering their fictional characteristics. Also, as Von Neumann, Hicks emphasized the unrealism of his assumptions, but accepted it since his objective was purely theoretical. As a result, Hicks created a model capable of being adapted for different formulations, specially capable of having its assumptions modified. Indeed, considering Hicks's (1937) ideas as the vanguard of current IS-LM models, the apparatus has already been adapted countless times (VERCELLI, 1999).

Thus, Hicks's (1937) formulation has all the attributes of a model: mathematical structure, adaptability, neutrality, purpose, and simplifications. The example of Hicks suggests that the term model was not completely consolidated during the 1930s. Although the idea of models and all its attributes were already part of economists' mindset, the term was not yet commonly used. This is a symptom of the disharmonic form of dissemination and acceptance of a paradigm. Translations are necessary to properly nominate the works inside the new vocabulary.

Hicks (1937) is not an isolated example of works that have done modeling, but not naming themselves models. Besides the rise of the IS-LS model, during the second quarter of the XXth century emerged a prestigious research agenda: growth economics (PUNZO, 2009). Harrod (1939) and Domar (1946) originated the famous Harrod-Domar model in that period. Their contributions were essential for posterior works such as Solow (1956) and post-Keynesian models (HEIN, 2014). Notwithstanding, neither Harrod nor Domar used the term model.

Harrod (1939), for example, devoted his work for the investigation of cycles and economic growth. His purpose was to present a different mode of reasoning regarding economic dynamics, overcoming the previous static analysis commonly found during the 1930s. Evidently, hence, his purpose was theoretical. Similarly to previous examples, Harrod also opted for using differential equations for supporting his arguments. Before starting his discussion, Harrod enumerated the following assumptions:

> The axiomatic basis of the theory which I propose to develop consists of three propositions-namely, (1) that the level of a community's income is the most important determinant of its supply of saving; (2) that the rate of increase of its income is an important determinant of its demand for saving, and (3) that demand is equal to supply. It thus consists in a marriage of the ‘acceleration principle’ and the ‘multiplier’ theory, and is a development and extension of certain arguments advanced in my Essay on the Trade Cycle. (HARROD, 1939, p. 14)

Interestingly, his set of simplifications is called “axiomatic basis”, highlighting the combination of Von Neumann’s and Tinbergen’s ideas, since the axiomatic method was introduced through topology and not classical mechanics. This characteristic is common to Meade’s, Kaldor’s and Hicks’s exemplars as well. As an axiomatic basis in the sense of Von Neumann, his set of simplifications is adaptable to different forms, which guarantees the model is neutral. As a consequence, Harrod’s (1939) work is easily described and named as a model, even though the term was not part of Harrod’s vocabulary. Domar’s (1946) piece is methodologically similar to Harrod (1939) and thus does not demands thorough explanations. Their purposes and results were similar, which allowed their combination and creation of the Harrod-Domar models. Still, as Harrod, Domar has not used the term in his work.

Translations expose how the dissemination of terms and concepts are neither homogeneous nor concomitant inside a community. The idea of models represented by its attributes and values formed through Tinbergen’s and Von Neumann’s exemplars was already a reality in the preceding works. The method and the term spread at different paces. The consolidation of the term apparently occurred after the dissemination of the method. Translation plays an important role in this consolidation as an indirect form of disseminating a term. Not all members of a community have direct access to the agreed exemplars instantly. As a result, they detect the attributes in indirect forms and the community may apply the correct nomination according to their fit in the exemplary frame.

Morgan (2012) investigates this aspect of the different rhythms of evolution concerning a term and a method:

> William Baumol (1951) used the term as naturally as one might refer to a domestic weed when he referred to Harrod’s (1939) small set of equations showing how an economy grows as ‘Mr. Harrod’s Model’, while Roy Harrod himself (of the same older generation as Hicks and Meade) still mused about the term as if it were some exotic imported plant. (MORGAN, 2012, p. 12).

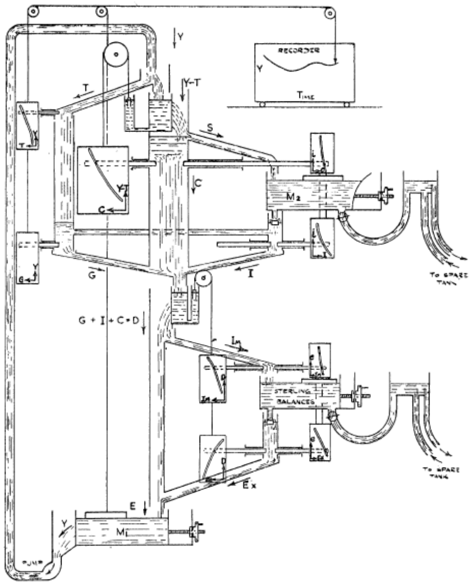
Although Harrod constructed his ideas about modeling, creating all necessary attributes to be considered part of the same ontological genus of Kaldor’s and Meade’s works, he had difficulties in referring to his ideas as models. Nonetheless, the community had not such difficulty. Furthermore, Tinbergen’s and Von Neumann’s exemplar continued to be credible foundations as the characteristics found in works translated as models were the same found in the frame.

5.4.3 *Concomitant paradigms*

The paradigm for the meaning of models formed by Tinbergen and Von Neumann overcome the previous paradigm. The previous relation with physics and concrete objects was not a reality anymore, as models became complete abstract concepts. However, analogous to how Harrod was unable to use the terminology in conformity with the new paradigm, some authors were completely incapable of divesting their ideas and vocabulary from the preceding order. Consequently, at the same time that the term model was being used mostly to refer to abstract formulations in the sense of Von Neumann and Tinbergen, eventually the term was used in the absence of the proper characteristics. Instead, concrete objects and physical analogies remained the foundations of models for some insular economists.

The most interesting case of an obsolete use of the term is the Newlyn-Phillips machine. Between 1949 and 1950, Walter Newlyn and “Bill” Phillips built a hydraulic machine designed to represent macroeconomic relations. The machine operated through an engine pumping water into pipes and compartments. The water flux represented money in an economy. The idea seems inconceivable in modern times. Contemporary economic models are abstract formulations, done with pen, paper, and computers. Still, no more than 70 years ago, economists still imagined the economy as the following image:

Fig 6. Newlyn-Phillips Machine



Source: Phillips (1950, p. 302).

In the second quarter of the XXth century, as observed above, economists were already using the term in a completely abstract mode. A physical relation could still exist, but they were not necessary for the use of the term. Economic data and the adaptability of models guaranteed their credibility, and physics was not necessary to offer methodological support for economists. Obviously, it is not possible to fit Newly-Phillips's reasoning into the frame of Tinbergen and Von Neumann. Notably, the model has its adaptability severely constrained as a result of its concrete formulation. Therefore, Phillips and Newlyn were delayed in their economic vocabulary and ideas. As Phillips points out at the outset of one of his works:

> There has been an increasing use in economic theory of mathematical models, usually in the form of difference equations, sometimes of differential equations, for investigating the implications of systems of hypotheses. However, those students of economics who, like the present writer, are not expert mathematicians, often find some difficulty in handling these models effectively. This article describes an attempt to develop some mechanical models which may help non-mathematicians by enabling them to see the quantitative changes that occur in an inter-related system of variables following initial changes in one or more of them. (PHILLIPS, 1950, p. 283).

Therefore, while the community of economists was overcoming its past methodological constraints, Phillips was incapable of following the development. Phillips still understood models in the obsolete idea of the previous paradigm. Thus, paradigmatic change is not harmonic in a community. Members of the community may learn at different paces the characteristics of a method and the new terms. Furthermore, some members may never overcome their previous beliefs. However, it is interesting to notice that the possibility of changing paradigms always exists. Phillips (1958), for instance, presented the formulation of the “Phillips curve” that could be easily fitted into the frame, although without using the term “model” during his exposition.

5.5 Final Remarks

Building a frame starting with Von Neumann (1945) and Tinbergen (1935) exposed the concept of a model in the second quarter of the XXth century as a set formed by the following attributes: mathematical structure, adaptability, neutrality, simplification, and purpose. Each attribute is valued differently depending on its basic exemplar. Therefore, when transmitting the meaning of a model through the community, the term could appear in different formulations, while always having the five basic attributes. As a consequence, all works categorized as models would pertain to the same ontological genus – the family resemblance.

Frame analysis, however, may present some limitations, especially as a result of its dynamic and tacit features. Therefore, to evaluate the credibility of the proposed frame, an examination of exemplars of models in the same period was realized. Through this analysis, it could be perceived that the concept of a model based on Tinbergen’s and Von Neumann’s exemplars is credible since both works which used the term and which did not use it demonstrated to be part of the same ontological genus of the frame. Consequently, the dissemination of a term may occur via direct and indirect forms:directly when the members of the community read the exemplars and indirectly when they read indirect sources. Indirect formulations eventually present the attributes in the absence of the term. As a result, the community realizes translations to integrate the works into the paradigm.

However, this same analysis highlighted that not all members of a community necessarily transpose the previous paradigms. Some economists used the term model in an obsolete sense more than ten years after the final publication of Von Neumann. Phillips (1950), for instance, contended that models should be concrete analogies in the 1950s.

Lastly, researches regarding the dynamic nature of frames are still necessary, specially in what concerns the selection of exemplars. Tinbergen and Von Neumann were selected for being the first authors in different methodological paths to use the term model to refer to their completely abstract formulations. Yet, probably Tinbergen (1935) and Von Neumann (1945) are not the foundations of contemporary economic vocabulary. Thus, uncovering possible transitions and the current foundation is still necessary.

6 CONCLUSION

The endeavor of defining scientific models is a recent tendency in philosophy. Until the 1960s, philosophy of science was mainly a logical enterprise divided into two segments: the semantic and the syntactic view of theories. As a consequence, the definition of scientific models was a logical problem disconnected from the perception of scientists. However, during the 1960s, a pragmatic turn happened. Kuhn’s ideas rose, defending the necessity of observing the logic of scientific evolution trough a historical view. His proposals claimed for a pragmatic observation of scientific dynamics. Concomitantly, pragmatism conquered space in the analysis of scientific tools and behaviors, which defined the pragmatic view of theories.

As a result, both scientific evolution and scientific specificities began to be studied with historical lenses. The definition of models thus became the task of highlighting models’ characteristics observed on case-studies, while philosophers concerned with evolution searched for the logic of choosing exemplars. Nevertheless, although the renewed views had similar foundations, the union of both is uncommon.

The present work attempted to merge the pragmatic view of theories and Kuhnian concepts. As pragmatic concerns with both evolution and specificities presented flaws, the combination intended to overcome their dilemmas. Kuhn’s relativism and the frustration of pragmatists were investigated in a multidisciplinary manner, combining cognitive science, history of economic thought, and philosophy.

The first concern was to expose Kuhnian initial ideas, their overcoming in the subsequent years, and finally their connection with cognitive science, specially with the theory of Dynamic Frames. In the sequence, paradigmatic definitions of models and their frustrations were analyzed in the light of Kuhnian ideas. The analysis exposed the incomparability of case-studies as the source of the frustration of pragmatists. As a consequence, a solution inspired by the economic models based on Sugden’s (2000; 2002) emerged. The creation of credible paradigms is an ordered manner of choosing exemplars, which allows comparability between pragmatic definitions.

Based on this insight, the third chapter realized an incursion into the history of economic thought searching for a plausible paradigm for the definition of abstract economic models. The examination suggested Tinbergen’s (1935) and Von Neumann’s (1945) works as the first studies to utilize the term model in an abstract manner.

Consequently, the fifth chapter inspected both exemplars and proposed a frame formed by the following attributes: adaptability, neutrality, mathematical structure, purpose, and simplification. In the sequence, the history of economic thought was one more time observed, now in a more specific form, evaluating the plausibility of the frame proposed and of the suggested exemplars. Both were confirmed as likely to be credible.

Finally, summarizing the main conclusions of each chapter, it can be pointed out first that Kuhnian ideas should be incorporated into contemporary analysis of the philosophy of science, specially his renewed ideas divested from relativism. Corroborating this first conclusion, the combination of Kuhnian ideas and the pragmatic view of theories exposed the source of the frustrating task of defining models in a historical manner. From this combination it can be inferred as a second point that the construction of credible paradigms is an interesting form of reevaluating pragmatic debates. Moreover, highlighting the third conclusion, semantic observations of the history of economic thought are a distinct way of analyzing thehistory of science and are worth of further works. Fourth, it is interesting to notice that the proposed definition of economic models seems credible and presented two peculiar characteristics (neutrality and adaptability) which should be observed in complementary forms. Lastly, a semantic analysis should be incorporated into the philosophy of economics as a mode of renewing debates about communicability and scientific evolution.

Nevertheless, the definition of economic models was constrained in time and was realized in a semantic-psychological manner. Therefore, discussions about the dynamic of the definition are still necessary since Tinbergen’s and Von Neumann’s works are probably not the foundation of current economic vocabulary. Moreover, debates about the capacity of a semantic definition being a conclusive definition for the ontology of concepts are necessary.

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