The Double Edge of Case-Studies: A Frame-Based Definition of Economic Models

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Abstract

Starting on the 1950s, the philosophy of science rearranged its methods. Logic gradually lost space as philosophers' exclusive tool of analysis, whereas case-studies and historical methods emerged as viable instruments. However, the methodological transposition concealed a double-edged knife. The approximation of philosophy and scientific practice happened at the expense of exponentially widening the list of possible philosophical definitions for scientific concepts, creating numerous incomparable explanations. The present paper thus advocates in favor of the use of more objective tools for defining scientific concepts, looking to reduce incomparability of definitions. From this point of view, framing proposals are presented as suitable mechanisms for reasoned definitions, given that frames' exemplar-dependency entail the necessity of organized selections of case-studies. In this context, community targeting understood as a limit of time and discipline for the selections of case-studies stood out as an organizational criterion. Considering this criterion, the present paper selected as case-studies, for defining the concept of an economic model, the first two cases where the term 'model' was utilized for referring to abstract economic reasoning. The proposed frame presented the following attributes: mathematical structure, adaptability, simplification, neutrality and purpose.

Keywords: Frames; Economic Models; Case-Studies; Conceptual Pragmatism; Models

Resumo

Começando nos anos 50, a filosofia da ciência rearranjou seus métodos. A lógica gradualmente perdeu espaço como a ferramenta exclusiva de análise dos filósofos, enquanto estudos de casos e métodos históricos emergiram como instrumentos viáveis. Entretanto, a transposição metodológica escondeu uma faca de dois gumes. A aproximação da filosofia da prática científica ocorreu ao custo de exponencialmente aumentar a lista de definições filosóficas possíveis dos conceitos científicos, criando numerosas explicações incomparáveis. O presente artigo, então, advoga em favor do uso de instrumentos mais objetivos para definir conceitos científicos, visando reduzir a incomparabilidade das definições. Deste ponto de vista, os frames são apresentados como um mecanismo cabível para definições arrazoadas, considerando que a dependência de exemplares presente nos frames implica na necessidade de uma escolha organizada dos estudos de caso. Assim, apontamento de comunidade, entendido como um limite de tempo e disciplinas para a seleção de estudo de caso, se destacou como um critério organizacional. Considerando esse critério o artigo selecionou como estudos de caso, para definir o conceito de modelo econômico dentro da economia, os dois primeiros casos nos quais o termo modelo foi utilizado para se referir ao pensamento econômico abstrato. O frame proposto se constitui dos seguintes atributos: estrutura matemática, adaptabilidade, neutralidade, propósito e simplificações.

Palavras-chave: Frames; Modelos Econômicos; Estudos de Caso; Pragmatismo Conceitual; Modelos

Classificação JEL: B23; B31; B40

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1) Introduction

Starting on the 1950s, philosophy of science rearranged its methods. Logic gradually lost space as philosophers' exclusive tool of analysis, whereas case-studies and historical methods emerged as viable instruments. In this context, philosophers such as Kuhn and Feyerabend embarked on a thread of discussions regarding the logic of scientific evolutions. Concomitantly, the pragmatic view of theories specialized in the conceptual analysis of science. Conceptual pragmatism concerned with the definition of scientific concepts (e.g. explanation, causality, experiment, model) through the examination of case-studies.

However, the methodological transposition concealed a double-edged knife. On the one hand, pragmatism came closer to the reality of scientists. On the other hand, the renounce of logical instruments in favor of case-studies exponentially widened the list of possible definitions for scientific concepts. In the case of scientific models, the list counts with: models as make-believe (Toon 2010a, 2010b), models as mediating instruments (Morrison 1999; Morrison and Morgan 1999); models as fictions (Frigg 2010; Godfrey-Smith 2009); models as experiments (Mäki 2005); toy models (Reutlinger, Hangleiter and Hartmann forthcoming); minimal models (Grüne-Yanoff 2009; Fumagalli 2016) models as isolating tools (Mäki 1992; 1994; 2009); models as analogies (Gilboa 2014); models as mini-theories (Rappaport 2001), and it continues to grow. As a result, the pragmatic view is far from being consensual about the definition of models. Consequently, pragmatists' discussions about the definition of models are uncomfortably frustrating, what has been hampering the development of discussions.

The present paper thus advocates in favor of the use of more objective tools for defining scientific concepts, looking to reduce incomparability of definitions. In the light of cognitive sciences, frame-based accounts will be demonstrated as promising instruments of definition, especially considering that the focal point of the construction of frames is the selection of exemplars. Among framing proposals, Barsalou's (1987; 1992) frames are selected and presented as the mechanism of definition for the concept of economic model.

Given that the selection of exemplars is essential both for the definition of a concept and for allowing future comparability, a criterion for the selection of exemplars is also presented. Community targeting is presented as a criterion for such selections. Following Kuhn (1974; 1983; 1996 [1962]) and Burian (2001), limits of time and discipline are presented as tokens of credibility for the selection of exemplars.

Finally, after exposing the source of pragmatic frustrations and presenting how frames and organized selections of exemplars could aid future debates, a frame-based definition of economic models is formulated. Von Neumann (1945) and Tinbergen (1935) are chosen as a plausible foundation for a definition of economic models. After a brief discussion of both models, the frame is defined.

In order to present all these points, the paper is organized as follows. After this brief introduction, in section 2, Barsalou's frame account is explained. Section 3 discusses the frustrations of conceptual pragmatism and how frames could resolve them. Thus, in section 4, the selection of exemplars is highlighted and discussed. The following section demonstrates the selected exemplars and debates their nuances. In the sequence, section 6 defines the frame and indicates possible directions for future debates. In the end, the last section presents some final remarks.

2) Frame-based definition of Concepts

Whereas the nature of concepts and the relation between them and reality may be controversial quandaries in the philosophy of mind and language, the fact is that conceptualization occurs in our minds (or outside of it[2]). According to Rosch (1978), concepts and categorization are essential features of our reasoning once they organize the perceptual world and economize cognitive effort. As Levitin (2014) points out, it would be quite difficult to understand the world if each leave of grass of the backyard or each seed of bean on the lunch plate was itself a special category deserving special attention.

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[2] Whether concepts are abstracta or mental representations is a recurrent philosophical debate. See Margolis and Laurence (2007) for a discussion.

Thus, in a purely functional point of view, conceptualization is a reality. Moreover, following the empirical evaluation of the above cited cognitive scientists, concepts must be structured in a cognitively economical mode. However, before the ascension of the neurosciences in the second half of the XXth century, philosophers defined concepts without the aid of empirical studies. As Strawson (1992) and Ramsey (1992) highlight, the endeavor of the analytic philosopher has mostly been the "conceptual analysis". In philosophy, during the centuries of study before the emergence of empirical neurosciences, the structure of concepts was understood primarily by what came to be known as the 'classical view'. According to such view, concepts are categorized through a set of sufficient and necessary conditions. Hence, classically, to conceptualize was an 'all-or-nothing' task.

The most prominent philosophical blow at the classical view was Wittgenstein's (1953) family resemblance. For Wittgenstein (1953), concepts and words are identified analogously to how the classification of the members of a family occurs. In a family, it is impossible to determine a set of characteristics essential for its members, even though it is noticeable that all members share similar characteristics that allow their recognition as members of it. Wittgenstein (1953) presents the word "game" as an illustration of family resemblance: card games, board games, ball games, or any other game, 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, 67, p. 32). Evidently, Wittgenstein's language game cannot be conciliated with the classical view. There is no single set of characteristics determining concepts for him.

The rise of the cognitive sciences has brought with it the interest in the empirical validation of these philosophical views of concepts. How exactly our mind conceptualizes? Rosch and Mervis (1975) played a crucial role in the study of this question. The authors prepared an experiment in which the subjects had to list all the attributes they could think of for a set of exemplars from superordinate concepts. For *furniture*, the subjects would have to list the attributes of *chair, sofa* and *bed* for instance. If there were necessary and sufficient conditions, all the items of the category would present them. However, Rosch and Mervis (1975) noticed that hardly the subjects listed overlapped characteristics for the items of the categories.

Following their results, Rosch (1973) and Rosch and Mervis (1975) argued that superordinate concepts were described by family resemblance rather than by sets of necessary and sufficient conditions. Furthermore, their results demonstrated that certain items were more rapidly identified as members of a category and presented more characteristics also mentioned to the superordinate concept. These results have been generally designated as typicality effects, prototype theories and graded structures. In summary, all ideas have in common the perception that categories are defined by comparing items to prototypical exemplars (e.g. *robins* are prototypes of *bird*). Even though there were methodological challenges for the experiment, prototypical ideas conceived a whole new world of theories of conceptualization.

According to Medin (1989), Rosch and Mervis (1975) generated the probabilistic line of concepts' evaluation, given that in their view members of a category were defined through probabilistic comparisons with prototypical exemplars. In Komatsu's (1992) point of view, probabilistic theories evolved in at least three different directions: family resemblance, exemplar and schema views. The main dissidence lied in their interpretations of how prototypes should be structured inside the mind.

Amidst these interpretations, frame-based accounts emerged. Frames were a hybrid proposal, capable of coping with most criticisms directed towards probabilistic views (Barsalou 1992). Consequently, during the 1980s and 1990s, frames became a prolific research agenda in linguistics, computer sciences, cognitive sciences, and philosophy. Moreover – and more relevant for the purpose of the present paper -, frames presented themselves as a suitable methodology for operationalizing the representation of concepts (Garbacz 2016). Thus, while not necessarily reproducing how the representation of concepts occurs in the mind[3], the application of frame-based definitions flourished as an interesting mechanism for philosophical definitions and discussions.

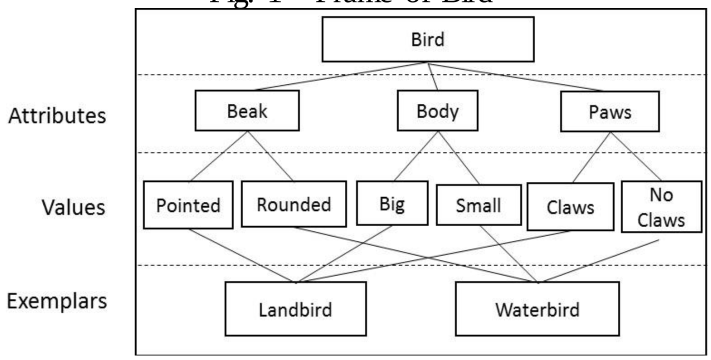
In the philosophy of science, Barsalou's (1987, 1992) frames endured as a regular tool of conceptual analysis. Under the spotlight rest the applications of Barsalou's proposals for the analysis of Kuhnian ideas

[3] See Lakoff (1987) for an argument against similarity-based theories as explanations for the representations of concepts in the mind.

and the definition of theoretical scientific concepts[4]. Alone, Barsalou's popularity in philosophical applications could already constitute an alluring justification for its utilization as an instrument for the definition of the concept of *economic models* in the following pages, especially considering that it has not yet been used for it. However, after a brief overview of the proposal, some weaknesses of frames may be observed and a second more prominent reason must come out. Still, before suggesting this second reason, a review of Barsalou's methodology is necessary.

First of all, frames are a species of bottom-up conceptual structure, in which subordinate and related concepts serve as exemplars for superordinate concepts. According to Barsalou (1992), the main component of frames is their attribute-value sets. Attributes are abstract characteristics expected in the different exemplars of a concept. Values, in their turn, are the materialized perception of such attributes. As an illustration, the concept of a *colored ball* may present the attribute color and the possible values for it (e.g. green, white, yellow). Following the example, values are the observational stimuli of attributes. Thus, components materialize from the analysis of subordinate exemplars. As a result, a colored ball could not be conceived without individual exemplars, such as yellow ball, green ball and blue ball. An illustration more commonly found in the literature is the case of birds:

Fig. 1 – Frame of Bird



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As observed, *Landbird* and *waterbird* have as salient observational stimuli the following characteristics: pointed beak; rounded beak; big body; small body; clawed paws; no-clawed paws. These characteristics instantiate the emergence of a set of features: beak; body and paws. While a quick observation could lead to the understanding that this abstract set of features determines the subordinate concepts, in fact the causality runs in the other way. The subordinate exemplars have determined the set of attributes required for the concept of *birds*. Consequently, Barsalou's frames are recurrently referred as partial dynamic-frames. Attribute-value sets are adjustable to the set of subordinate exemplars. For this reason, the bottom up peculiarity imposes that concepts become at least partially exemplar-dependent.

The hybrid nature of the proposal rests in this characteristic. Prototypical effects may be competently connected to exemplar-dependency. For instance, the set of attributes could be determined by a fixed set of prototypes. The set of resulting attributes thus becomes a list of expected characteristics for the exemplars of the category, which resembles the classical view. Following this line of thought, Andersen, Barker and Chen[6] have discussed how the modification and insertion of prototypes depicts the Kuhnian process of revolution.

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[4] See Andersen, Barker e Chen (1996; 2006), Barker (2001; 2011), Chen, Andersen and Barker (1998), Barker, Chen and Andersen (2003), Votsis e Schurz (2012).

[5] Birds seem to be authors' preferred concept for introducing Dynamic Frames, given that several authors have opted for it as an introduction to philosophical utilizations (Andersen, Barker e Chen 1996; 2006, Barker 2001; 2011, Chen, Andersen and Barker 1998; Barker, Chen and Andersen 2003, Urbaniak 2010, Votsis e Schurz 2012). Yet, different superordinate concepts are found in Garbacz (2013, 2016), Petersen (2007), Urbaniak (2010) and Votsis e Schurz (2012).

[6] Andersen, Barker and Chen attempt to explain incommensurability from the point of view of Dynamic Frames (Andersen, Barker and Chen 1996; 2006, Barker 2001, 2011; Chen, Andersen and Barker 1998; Chen 1997). Their thoughts may be found extensively in Andersen, Barker and Chen (2006) and a synthesis may be found in Barker (2011).

The complexity of frames can be exponentially increased when the fact that each node of a frame is itself a concept is considered. In other words, "there might be frames within frames" (Garbacz 2013, p. 423). Thus, frames may face constant changes, considering that all components of a frame may have their exemplars altered. However, Barsalou (1992) also points out that frames may present two different limits to their dynamicity: structural invariants and constraints. In summary, both determine prevailing activation patterns of attributes and values (Urbaniak 2009). Barker, Chen and Andersen (2003) exemplify the idea: "there are no birds with legs that attach to their necks" (Barker, Chen e Andersen 2003, p. 226). As a consequence, dynamic frames allow the possibility of restricting the connections between values (constraints) and attributes (invariants). For example, legs may be a characteristic only of birds with bodies. Most of these constraints and invariants are empirical truths more important for cognitive scientists than for philosophers. Barsalou (1992) himself neglects structural invariants during his analysis. Therefore, to maintain simplicity, both will be overlooked henceforward.

3) Why frames? The double-edge of case-studies

Although Barsalou's frame account presented a new perspective for the representation of concepts and achieved some level of popularity, its adoption is far from being a consensus among philosophers and cognitive scientists. Thagard (2009), for instance, discords from Andersen, Barker and Chen (2006) in what concerns the explanation capacity of a frame account. Zenker (2014), on the other hand, defends the use of conceptual spaces rather than dynamic frames for explaining changes in scientific concepts.

The controversy arises from the fact that frames have already been supposedly overcome by newer theories of conceptual representation, especially in the cognitive sciences. Medin (1989) affirms that conceptual representation has passed through at least three intellectual shifts. Probabilistic views substituted the classical view, but they have also been replaced by what the author calls the view of concepts organized as theories. Komatsu (1992) endorses Medin's argument, even though the author divides the probabilistic view into different segments. Margolis and Laurence (1999) and Margolis and Laurece (2007), while not necessarily accepting that the shifts implied in improvement (see Margolis 1994), reinforce that there are several alternative approaches for the definition of concepts, such as conceptual atomism, neoclassical views, and pluralistic views.

A core issue contesting frame-based accounts stems from the evidence that probabilistic views and their derivates are best suited for the definition of 'kinds' (e.g. natural kinds such as trees, water, and tigers) and 'artifacts' (e.g. human-made objects such as sofas, hammers, and houses). Osheron and Smith (1981) remind that complex concepts like *belief*, *desire*¸ *explanation* remain outside of the scope of capabilities of probabilistic views.

Considering these contentions, it is natural to question why frames should be the right approach for operationalizing the definition of philosophical concepts, particularly scientific models as the focus of the present paper. Beyond frames' popularity, one simple reason stands out as a defense favoring frames. The reason is purely operational and results of the historical accumulation of frustrated definitions of scientific concepts carried by pragmatic philosophers. In order to understand where the frustration rests and how frames could operate to dissolve them, a minor detour for overviewing the recent history of the philosophy of science is necessary.

To begin with, the second half of the XXth century marked a turning point in the philosophy of science. Until the emergence of Einstein's theories and non-Euclidean mathematics, logic was the predominant tool of analysis for philosophers of science. Cumulativeness and linearity were viewed as central characteristics of the scientific enterprise and logic suited this perspective perfectly. The debacle of the scientific enterprise questioned both the views and the tools of philosophers of science.

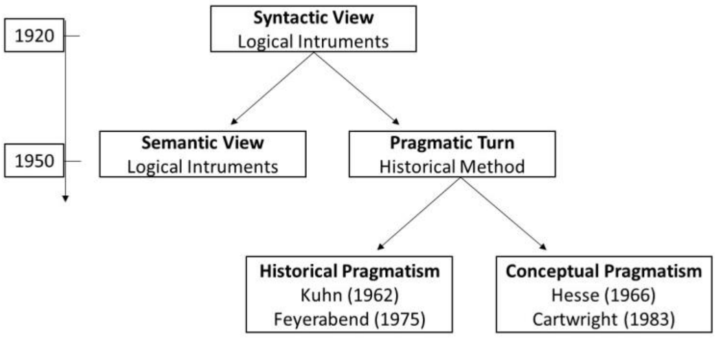
On the one hand, some philosophers of science upgraded their logical instruments. As a result, modernized instruments gradually flourished as escape routes for a logical view of scientific theories. In this ambiance, formal semantics prevailed among the revamped views, given its competency in inserting a new layer of logical analysis (Odenbaugh 2007). More specifically, philosopher of science's Syntactic view was questioned by the rise of the semantic view. For the purpose at hand, it is only important to notice that both views relied upon logical instruments (Liu 1997). On the other hand, the scientific crisis started a movement to approximate the philosophy of science to the practice of scientists. 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).

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. This transition created a methodological dissonance in the philosophy of science "between those who think that philosophy of science needs a formal framework or other and those who think otherwise." (Contessa 2006, p. 376). Thus, instead of the logic of syntacticians and semanticists, pragmatists opted for studying philosophy of science from the perspective of the history of science and case-studies (Odenabaugh 2007).

Roughly, the pragmatic turn can be divided into two stances. At one side, philosophers such as Kuhn (1996 [1962]) and Feyerabend (1975) commenced the branch of historical pragmatism, using case-studies to explain the evolution of the scientific enterprise. On the other side, the pragmatic turn established a new approach in the conceptual analysis of science: the pragmatic view of theories. According to Winther (2015), the pragmatic view consolidated during the 1980s, especially after Cartwright's (1983) seminal work. Still, before Cartwright (1983) there were already pragmatic conceptual analyses such as Hesse's (1966) for instance.

The pragmatic view of theories may be defined as: conceptual pragmatism. Conceptual pragmatism, thus, was the field which implemented the study of scientific concepts through case studies. At the spotlight stand concepts like explanation, causation, evidence, models, and experiments. As case-studies are the foundational ground of analysis, frequently conceptual pragmatism occurs in the sphere of specific scientific disciplines, such as economics, biology, physics and so forth. Thus, the pragmatic view of theories constitutes an important part of the expanding fields of philosophy of science (e.g. philosophy of economics, philosophy of biology, etc.). The following image summarizes this brief overview:

Fig. 2 – Overview of the Pragmatic Turn



Source: prepared by the author

Since the establishment of conceptual pragmatism, pragmatists have defined scientific concepts in numerous forms. In the case of models, the list counts with: models as make-believe (Toon 2010a, 2010b), models as mediating instruments (Morrison 1999; Morrison and Morgan 1999); models as fictions (Frigg 2010; Godfrey-Smith 2009); models as experiments (Mäki 2005); toy models (Reutlinger, Hangleiter and Hartmann forthcoming); minimal models (Grüne-Yanoff 2009; Fumagalli 2016) models as isolating tools (Mäki 1992; 1994; 2009); models as analogies (Gilboa 2014); models as mini-theories (Rappaport 2001). Frigg and Hartmann (2012) in their encyclopedic entry regarding scientific models point out an even bigger list of definitions: "Probing models, phenomenological models, computational models, developmental models, explanatory models, impoverished models, testing models, idealized models, theoretical models, scale models, heuristic models, caricature models, didactic models, fantasy models, toy models, imaginary models, mathematical models, substitute models, iconic models, formal models, analogue models and instrumental models are but some of the notions that are used to categorize models." (Frigg and Hartmann 2012, p.1)

From this wide list, it can be inferred that the pragmatic view is far from being consensual about the definition of models. Consequently, pragmatists' discussions about the definition of models are uncomfortably frustrating. As Godfrey-Smith (2009, p. 104) affirms: "[...] the word "model" itself is used diversily. So, it is not a good idea to organize discussion around the question: 'what are models?'". Analogously, Morgan (2012) avers:

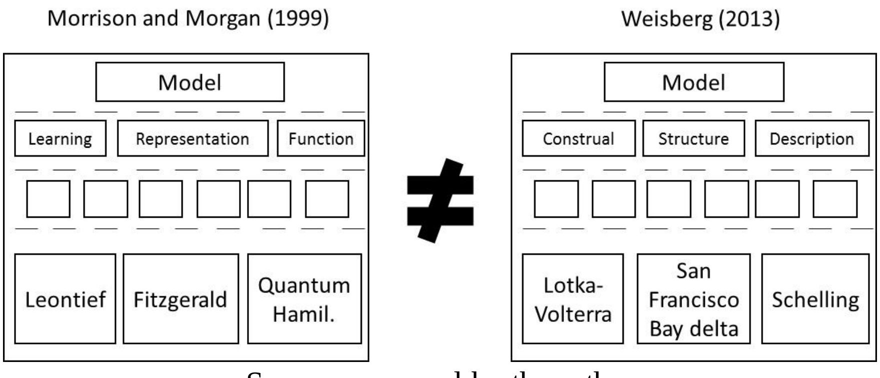
"Fifteen years of researching, thinking and writing about models have convinced me that there are no easy answers to questions about what models are, and how modelling works. Some questions are more helpful than others. Asking: what qualities do models need to make them useful in science? And what functions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

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.

A frame-based account for defining models reveals the root of pragmatic frustrations: case-studies. The line of reasoning is straightforward: if the definition of concepts occurs in a bottom up manner (from exemplars to attributes), and if case-studies are pragmatists' exemplars, pragmatic definitions differ due to the mismatch of case-studies. Logically, following frames, the dissonant definitions of models (and of other scientific concepts defined through conceptual pragmatism) derive from the lack of consensus in the selection of case-studies (exemplars). Thus, the discard of logic concealed a two-edged knife. The historical method approximated philosophers and scientists, but the renounce of logic widened the possibilities of definition exponentially.

To illustrate, two definitions where the dissonance is evident can be observed: Weisberg (2013) and Morrison and Morgan (1999). For Weisberg (2013), a model definition is adequately funded by the San Francisco Bay-Delta Model, the Lotka-Volterra model and Schelling's Segregation Model. On the other hand, for Morrison and Morgan (1999), the characteristics of models should be defined through Sir George Francis Fitzgerald's mechanical models of aether, Geert Reuten's Marxian analysis model, the MIT-Bag model, the quantum Hamiltonian, Irving Fisher's and Leontief's economic models, and finally nuclear physics' liquid drop model. Unmistakably, there is not a single match in their selection of case studies. As a result, while Weisberg (2013) talks about the attributes construal, structure and description, Morrison and Morgan (1999) discuss attributes such as learning, representation, and function. The following image illustrates the incomparability.

Fig. 3 – Incomparable definitions



Source: prepared by the author

It is important to notice that current pragmatic definitions are not incorrect. Selections of exemplars 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 how to compare the wide variety of attributes and values. Unfortunately, comparability seems uncomfortable without common points of view. Discrepant selections cause the development of individual and isolated theories, which rarely collaborate with each other.

Frame's exemplar-dependency clarify the importance of case-studies to single out the attributes of the concepts being defined. As so, frames expose the double-edge of case studies and the cause behind the widening of the list of pragmatic definitions. In exposing the weakness of unorganized pragmatic definitions, frames create a new level of discussions for pragmatists. When depicting any frame, the bottom-up characteristic (from exemplars to attributes) is ubiquitous and not solely attributes and values might be discussed, but also the selection of exemplars: "does the attributes and values represent the characteristics of the exemplars?"; "Were the exemplars significant for the community in question?"; "If different exemplars had been selected, would the attributes also be different?"; "Have these attributes and exemplars altered over time?"

Moreover, frames may organize the debate in two additional forms. First, conceptual pragmatism frequently compares models to different abstract categories (e.g. fictions, analogies, experiments). According to the frame approach, these comparisons may come in two separate forms, either models and fictions, for instance, are the exemplars of a high order concept or models are being exemplars for defining fictions (and vice-versa). As follows, frames oblige the awareness of the concept being defined. Confounding exemplar and concept distorts completely the process of definition, given that the causality runs solely from exemplars to attributes. Second, as cognitive scientists highlight, each node of the frame is a complex frame itself. Considering this, each node may be discussed exhaustively. Hence, conceptual pragmatism embarks frequently on debates about the definitions of a single attribute or value of models (e.g. the mathematical structure, the nature of simplifications, the purpose of models). Being conscious about these nuances of conceptualization may organize the debate and avoid unproductive discussions.

Apart from these supplementary organizational advantages, it should be clear that the main point supporting framing is the fact that a frame-based account sheds light on the incomparability of definitions which are based on disparate case-studies. Now, the adoption of frames will not organize the debate for itself. If discrepant selections of case-studies are really the case supporting the existence of such a wide list of pragmatic definitions, how should the exemplars be selected in order to allow the comparability of definitions?

4) How to select the exemplars?

As a theory of representation of concepts, frames are limited tools. A narrow selection of exemplars is necessary in order to define a concept outside the realm of our minds. In the context of conceptual pragmatism, these exemplars are case-studies. However, Pitt (2001) reminds that case-studies are precarious foundations for philosophical arguments (especially in the philosophy of science):

> "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).

Pitt's (2001) concerns are not thoughtless. The cited list of model's definitions and the even bigger list of attributes are an indication that philosophical discussions should be aware of the dilemmas regarding the use of case-studies. However, in opposition to Pitt's (2001) criticisms, Burian (2001) asserts that case studies may be grouped in order to constitute a solid basis for reasoning. Burian (2001) suggests two grouping methods: longitudinal and comparative. The first contends that the analysis of case studies should be organized according to time, while the second defends that case studies must be grouped according to their discipline.

Burian's (2001) proposal develops differently whether applied in historical pragmatism or in conceptual pragmatism. First, in the case of historical pragmatism, Burian's ideas correlate with Kuhn's works (1974; 1983; 1996 [1962]). After the controversies of The Strucuture of Scientific Revolution (1996 [1962]), Kuhn redefined scientific paradigms as tacit agreements of scientific communities around narrow sets of exemplary works, accountable both as theoretical and methodological paragons. In a schematic illustration, what Kuhn and Burian suggest is that imagining 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. The boundaries of time and discipline are not respected since the two exemplars are decades apart and both disciplines have little in common - especially considering the differences in time.

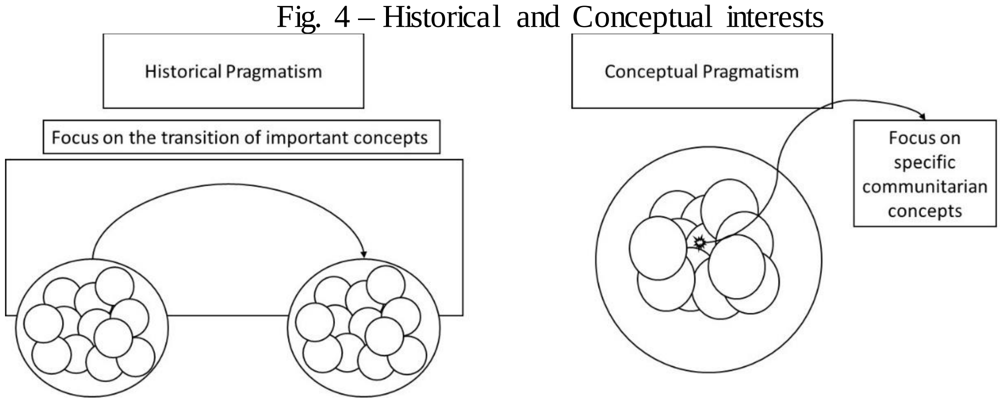
As follows, both Burian's constraints and Kuhn's paradigms may be seen as limitations of discipline and time for the choice of exemplars. In their views, rather than existing encompassing scientific concepts, several concepts related to groups in specific times and places exist. As a result, they accept that scientific concepts may change. This belief compelled Kuhn to study the logic behind the change of scientific concepts: what is the criterion of scientists for *changing* their exemplars? For such task, Kuhn was obliged to reveal where the longitudinal boundaries of scientific communities were. For instance, in order to study the logic behind the change of Ptolemaic ideas by Newtonian ones, the shifting point has to be observed carefully. Hence, in the context of historical pragmatism, the longitudinal constraint plays a crucial role.

On the other hand, given that the point of view of historical pragmatism is global, comparative constraints are secondary. Lakatos (1978) and Kuhn (1974; 1983) affirm that total consensus is not a reality inside scientific communities because those are, in fact, an association of smaller groups. However, both accept that usually scientists agree on their ideas about central theoretical concepts, while discordance occurs around methodological and radial theoretical concepts. Historical pragmatists look at science concerned with great theoretical transitions such as that of Phlogiston in Chemistry or Gravity in Physics. This macro point of view exempts historical pragmatism of defining exactly the borders of discipline.

In contrast with historical pragmatism, conceptual pragmatism - even though supported by historical case-studies - it is not a field of history, but of scientific conceptual analysis. Historians' study of the change of exemplars is substituted by a focus on stable scientific ideas. Therefore, for this different task, longitudinal and comparative constraints assume different roles. Longitudinal constraints still exist as it would be unwise to select a Copernican exemplar and a Newtonian exemplar to define a specific physics' concept. However, the exact point of change becomes unimportant, what reduces the responsibility of the longitudinal constraint. Thus, for conceptual pragmatism, longitudinal constraints are secondary and the only attention should be to not select exemplars from rival paradigms without clear justifications for doing so.

Comparative constraints, on the other hand, increase their importance. As the point of view moves from a macroscopic perspective to a more specific one, the smaller groups inside scientific communities flash out. In this context, minor nuances may cause great distortions. Quantum mechanics and general relativity, for instance, even though coexisting in contemporary physics, may substantiate different understandings of methodological and radial theoretical concepts (e.g. experiment) as their objects study differ - small particles and massive objects respectively. In economics, marxians and neoclassicists coexist, but their definitions of profit hardly match. Even though there may be overlaps between historical and conceptual pragmatism, the following schematic image promotes a basic understanding of their concerns and, consequently, of the relevance of longitudinal and comparative constraints for their analysis.

Fig. 4 – Historical and Conceptual interests

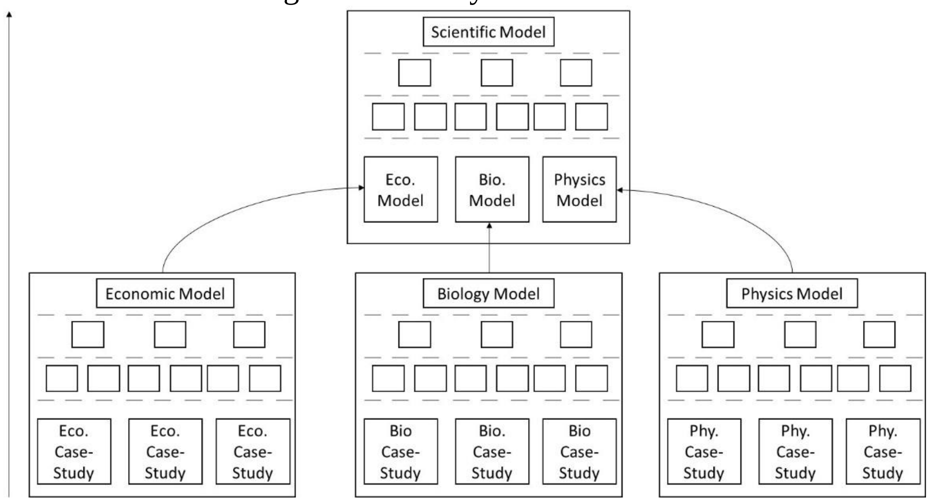


Source: prepared by the author

From this micro point of view, it is essential that pragmatists' selection of exemplars follows a criterion of *community targeting*. That is, conceptual pragmatism should consider to what community the concept belongs to before commencing the definition. Methodological and theoretical concepts differ according to the groups of scientists and an encompassing selection overlooks important nuances. Moreover, an encompassing definition of a concept is probably inconceivable without supportive definitions. Therefore, framing approach pairs appropriately with community targeting.

For example, in a frame mode of reasoning, an encompassing definition of models would rely upon specific exemplars such as economic models, biology models, and physics models, each one being a complex frame itself. Hence, according to this line of reasoning, the definition of specific exemplars should occur before the definition of an encompassing definition. In this sense, community targeting matches with frames' precondition of hierarchical conceptualizations. Case-studies are the exemplars of specific communities, which by their turn are exemplars of bigger communities. The following image explains the hierarchy:

Fig. 5 – Hierarchy of Definitions



Source: prepared by the author

New layers of hierarchy could be inserted in the network of frames. The concept of an economic model, for instance, could be defined by Post-Keynesian models, Neoclassic models and Marxian models. Those could also be divided into different categories. Post-Keynesian models may be defined by Kaleckian models, Minskyan models and Keynesian models for example. The frame network could grow indefinitely, which entails the necessity of targeting clearly the community. As follows, arbitrary selections do not target specific communities and have low credibility, considering that hardly a group of scientists would define their concepts arbitrarily. In fact, hardly a scientist would know well enough specific exemplars outside his own community.

Thus, encompassing definitions are still possible, but demand previous definitions or, at least, credible justifications for the selected exemplars. Why would a scientist define the concept of a model based on Lotka-Volterra's model and the San Francisco Bay delta model? As Bailer-Jones affirms: "there is what philosophers think, and there is what scientists think, *and* there is what scientist practice" (Bailer-Jones 2009, p. 14). If Downes (1992) is correct and Pragmatism is an effort to approximate philosophers to the scientific practice, conceptual pragmatism should define scientific concepts from the perspective of scientists. This demand constrained and justified selections of exemplars once encompassing definitions are in fact a philosophical perspective.

Considering these points, longitudinal and comparative constraints are tokens of credibility for the definition of scientific concepts. Not necessarily the exemplars must be the exemplars that scientists agreed tacitly. Still the established boundaries of time and discipline guarantee that the scientists could have chosen these as exemplars. In contrast with current pragmatic practices, comparing targeted selections prevents the total incomparability of definitions, given that time and discipline will create a common line of observation. In summary, community targeting is a claim for pragmatists to justify their selections of exemplars and to add the level of exemplars to the debate.

5) **A credible foundation**

In order to not remain solely a methodological criticism and to demonstrate an application of the previous ideas, the present section selects two exemplars of models for a definition of the concept of economic models. Thus, the following pages will define the concept of economic models for the period lasting from the 1930s to the 1950s. Tinbergen (1935) and Von Neumann (1945) will serve as the foundation for the definition. According to the previous arguments, the following question must be answered: why Tinbergen (1935) and Von Neumann (1945) constitute a credible basis for the definition of the concept of economic models?

The justification exploits a philosophical controversy. According to Boumans (2005) and Netto (2016), these two pieces were the first ones to utilize the term model for defining abstract economic reasoning. Until their works, economists used schemes, diagrams and systems as terms referring to their ideas. In this context: could the concept of economic models exist without the term? Answering this question is far from the purpose of the present paper. However, a common philosophical line of reasoning inserts both exemplars in a distinguished position for defining the concept at aim. Assuming that a concept (or mental representation) may only exist concurrently with its corresponded word requires that Tinbergen (1935) and Von Neumann (1945) are the exemplars which designed the first definition of economic models. As a consequence, their works settled the way for the modern economic method and for all subsequent definitions of models inside the community of economics.

Thus, a semantic-historical justification constitutes a solid pretext for the choice of the exemplars. Even if assumed that concepts exist without words, the connection between both will remain undeniable. The word plays a crucial role in the development of the concept inside communities. Along these lines, the fact that both authors were the first authors to use the term constitutes a solid foundation for framing an encompassing definition of economic models without defining subordinate concepts pertaining to subordinate communities. If both exemplars were truly the first ones to use the term and words precede concepts, the following frame is the pioneering definition of models inside economics and it may be assumed that there were no subordinate concepts to it. Moreover, both must not have remained as the foundations of the concept up to modern days, and thus probably defined the concept for a determined period. This period will be assumed to be from the 1930s to the 1950s.

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

"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" (Tinbergen 1935, p. 370, 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, increased simplicity reduces the model's capacity of representing reality. In Morgan's (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 of representing reality, while also maintaining the possibility of practical applications:

"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" (Tinbergen 1935, p. 372, translated by the author).

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 to be accepted only 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:

"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 models was 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 1989, 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: "It is advisable to examine a larger number of different models before considering the conclusions as final" (Tinbergen 1935, p. 371, translated by the author). In the sequence, Tinbergen defended the requirement of adorning the model with more complex ideas: "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" (Tinbergen 1935, p. 372, translated by the author).

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:

"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" (Tinbergen 1935, p. 382, translated by the author).

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 (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 done in 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 logically possible. In order to achieve his objectives, "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.

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's 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's (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 1947). Before Gödel's proof, the axiomatic method intended to find axioms completely destituted from 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 to be presented as an economic model in 1945. The empirical basis became a reality for the axiomatic method. Therefore, Von Neumann (1947), 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 1947)

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's (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.

6) Frame

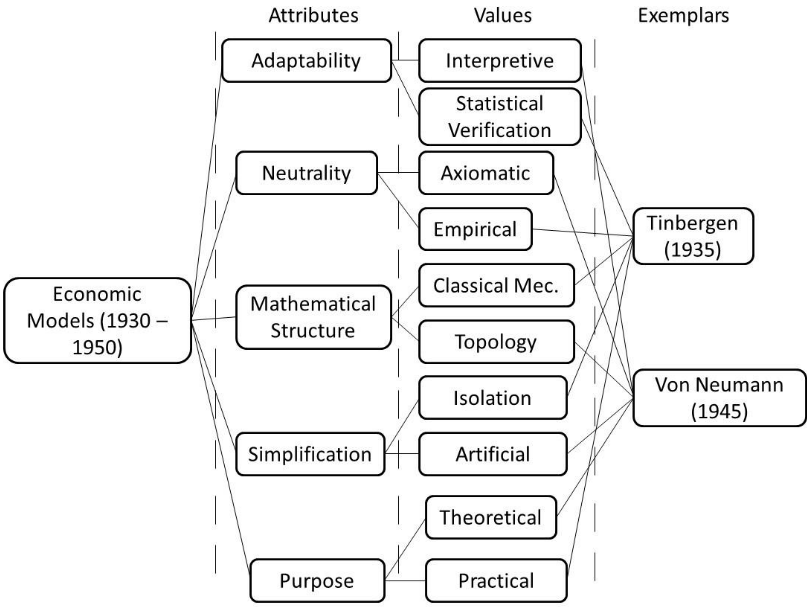
Once observed the two exemplars of models, it is possible to delineate a concept of a model based on their evident characteristics. However, as Murphy and Medin (1985) point out, to select a set of attributes based on similarity judgments generates several problems. Similarities are context-dependent in a way that it can always be found a characteristic that pertains to two different objects. Chairs and tables are both furniture, while chairs and horses may both have four legs. Thus, selecting attributes based solely on similarity creates distortions.

According to Andersen (2000), this problem is solved through enhancing the importance of dissimilarities. As a result, to select an attribute is to select a characteristic both similar and dissimilar between exemplars. Remembering the previous example, birds have beaks that can be pointed or rounded. The abstracting of this observational stimuli instantiates the attribute beak, which is essential for the conceptualization of birds. As follows, characteristics for which dissimilarities are unimportant may be neglected. In the case of Tinbergen's and Von Neumann's works, both are similar in the characteristic of being printable. However, no clear dissimilarity exists between this feature, and there is no reason for including it on a frame. Consequently, the following attributes were chosen based on the (almost paradoxical) condition of finding dissimilarities on the similarities.

The main characteristics of Tinbergen's (1935) model are: (1) the model uses mathematical expressions to communicate, specially differential equations related to classical mechanics; (2) the purpose of the model is practical, intended to be used as a tool for policy analyses; (3) The simplifications are isolations of essential aspects of the problem at hand; (4) the model is neutral as a result of its empirical content; (5) 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: (1) topology and logic are the main forms of reasoning; (2) the purpose of the model is theoretical; (3) artificial assumptions are commonly found in its formulations; (4) the model is neutral as a consequence of its axiomatic formulation; (5) the model adapts to different interpretations. When combining the characteristics of both models, the following frame can be presented:

Fig 6. The definition of Economic Model 1930 - 1950



Source: prepared by the author

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's (1935) and Von Neumann's (1945) exemplars. While outside the scope of the present paper, this definition paves the ground for at least four subsequent discussions. First, each attribute and value is a complex frame itself, and most of them are widely discussed in the philosophical literature - especially simplification, mathematical structure and purpose. However, frames and community targeting have not yet been used for defining these concepts. Second, to my knowledge, adaptability and neutrality are unusual characteristics appointed to models, and could be further discussed.

Third, assuming that the chosen exemplars are prototypes of economic models imply that the set of attributes may be seen as a list of required and sufficient conditions for a work to be considered a model in the period. In this view, 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. Following the prototype argument, works to be considered models could manifest their attributes in mixed compositions of values and not necessarily correspond exactly to either Von Neumann's or Tinbergen's exemplars. It would be possible, for instance, to categorize as a model works using artificial simplifications for practical purposes, provided that all the attributes were identifiable. Considering this idea, the frame could be used to categorize works of the period as models or not.

Finally, in consonance with the main purpose of the previous methodological critique, the selection of exemplars can -and probably should - be contested and further analyzed, which leaves open a wide variety of questions: Is a semantic-philosophical selection enough for considering the exemplars the foundation of the concept of economic models? Were the exemplars significant for the community in question? Have these attributes and exemplars altered over time? What is the role of the exemplars in the manifested attributes? Would they be different for distinct exemplars?

7) Final Remarks

The main objective of the present paper was to review the causes supporting the widening of the list of definitions of scientific models and present a solution for the problem along with an application. For the causes, it was demonstrated that conceptual pragmatism and its basis on case-studies created a hodgepodge of definitions, which has been hampering the development of debates and frustrating pragmatists. In summary, the source of frustrations comes from the incomparability of definitions that are based on disparate case-studies.

Frame-based accounts were shown as a viable solution for recreating comparability, considering that exemplar-dependency is at the core of their approaches. Exemplar-dependency puts the spotlight at the selection of case-studies. The recognition of case-studies as the main designers of the definition of scientific concepts alert for the necessity of reasoned selection of exemplars. Following Burian (2001), at a minimum, case studies should be selected according to their disciplines and generations. Thus, a reasoned selection of exemplars should target a specific community. In other words, there is no sense in defining scientific models based on disparate exemplars spanning from a wide variety of disciplines and time periods. What is a model defined by an engineering's exemplar of the 1930s and an economics' exemplar of the 1980s? What scientists of these paradigms have in common? Probably some, but not enough to justify such peculiar selection.

In favor of not remaining solely at the level of methodological criticism - following the proposed criterion - Tinbergen (1935) and Von Neumann (1945) were chosen as exemplars for the definition of economic models for the period going from 1930 to 1950. For the definition, the method presented by Barsalou (1987; 1992) was selected. The analysis of the case-studies formed a frame with five attributes: mathematical structure, purpose, neutrality, simplification and adaptability.

The proposed definition, thus, may be criticized at two different levels, that of the attributes and values and that of the exemplars. As a result, several reasoned questions arise: "do the attributes and values represent the characteristics of the exemplars?"; "Were the exemplars significant for the community in question?"; "If other exemplars had been selected, would the attributes be different?"; "Have these attributes and exemplars altered over time?" Therefore, frame-based definitions create a base of comparability and open a whole new world of questions.

At last, it has to be highlighted that not only frames may organize the debate. The important point is the awareness of exemplar-dependency in the definition of concepts. Hence, this consciousness is far more meaningful than the method itself. The above-cited questions could emerge only from being aware of the importance of exemplars. Frames were selected especially for their capacity of highlighting this point.

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