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The general equilibrium theory as economic metatheory

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Many economists show certain nonconformity relative to the excessive mathematical formalization of economics. This stems from dissatisfaction with the old debate about the lack of correspondence between mainstream theoretical models and reality. Although we do not propose to settle this debate here, this article seeks to associate the mismatch of mathematized models with the reality of the adoption of the hypothetical-deductive method as reproduced by general equilibrium. We begin by defining the main benefits of the mathematization of economics. Secondly, we address traditional criticism leveled against it. We then focus on more recent criticism from Gillies (2005) and Bresser-Pereira (2008). Finally, we attempt to associate the reproduction of the hypothetical-deductive method with a metatheoretical process triggered by Debreu's general equilibrium theory. In this respect, we appropriate the ideas of Weintraub (2002), Punzo (1991), and mainly Woo (1986) to support our hypothesis.

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SUPPOSED BENEFITS OF THE MATHEMATIZATION OF ECONOMIC DISCOURSE

One of the champions of economics formalization is Katzner (1991a). This author argues that formalization itself is not a problem. The difficulty lies in the nature of the questions that an economist intends to ask and in the contents of the assumptions of the analyses done to provide answers. In fact, for many economists, formalization stands as a powerful means of analytical expression. And if formal-

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ization were to be avoided, such avoidance would weaken economics and create far more problems. Although he agrees with formalization, Katzner claims that it has been misused, particularly, as is often the case, where it lacks an analog relationship with the phenomena under investigation.

McCloskey (1991) states that it would not be smart to criticize the mere presence of mathematics in Economic, and that the latter has been progressing more quickly with the former than it would without it. He emphasizes that a need exists for quantitative standards in economics, and illustrates by saying that it is one thing to conclude that an optimal import tariff is needed in terms of sectorial economic policy; this is a qualitative matter. It is quite another thing to calculate such a tariff.

Woo (1986), on the other hand, points out that no serious economist opposes the application of mathematics and formalization to economics. He does, however, criticize the existence of an absolute prevalence of mathematics, as this eventually leads to the virtual exclusion of all other methods of research by imposing mathematical finality on the subject. For Woo, the problem is not that mathematics is being used to solve problems, but that mathematization has become a prevalent approach in economic analysis. For this reason, the approach ends up directing the intellectual production and contents of the entire discipline. Behind the mathematization process lies the notion that only thinking that can be translated into mathematical form is worthy of respect in economics.

For the proponents of the formalization of economics, an axiomatic system may stand as an instrument for discovery and testing, enabling its application to virtually every topic or situation. As a result, one should not be surprised by the fact that mainstream economics is only concerned with determining how good a formalization can be, without ever asking itself whether a certain subject or situation can or cannot be formalized.

The appeal of the formal apparatus is so powerful that even the most moderate of economists take a defensive stance at best, arguing simply that non-mathematical approaches should not be discriminated against, and simply warning of the excessive and indiscriminate application of mathematics. The power of mathematization appears absolute and "guaranteed", and is taken as a given by the majority of economists.

Another benefit of the mathematization of economics lies in the assurance of rigor it lends to economics papers. This means that the more formally mathematized a paper, the more rigorous it is. This rigor is supposedly associated with the adoption of a hypothetical-deductive method in which logical coherence is the sole attribute necessary to this end. Another allegation for the mathematization of economics is that simplification by means of equations and graphs would help achieve a better understanding than natural language would allow.

On the other hand, Woo (1986, pp. 22-23) points out that any formalization would be useful if it met the following criteria: 1st. if it allowed selecting, based on pre-formalized concepts and relationships, the aspects that reflect essential properties of reality; 2nd if the syntactic relationships embedded in the formalization corresponded to a structural relationship found in reality; 3rd. if the relationships and properties selected for the formalization were sufficiently autonomous from other relationships that the formalization does not include, in such a manner that the formalized relationships may be taken as independent hypotheses.

For Woo, it would be difficult to imagine an economist who would differ from the above criteria. However, we must ask ourselves if formalization in social sciences does meet those criteria, and if economists have been making efforts to advance in that direction.

TRADITIONAL CRITICISM OF AND PROBLEMS CREATED BY THE MATHEMATICAL FORMALIZATION OF ECONOMICS DISCOURSE

The overwhelming majority of the criticism against the mathematization of economics has not been taken into consideration by the champions of the process; they believe that its beliefs are obvious and that the debate has been settled. Beed and Kane (1991) oppose this view to suggest that the gains that mathematical economists achieved for economics were obtained at the expense of equally valid, or even more appropriate, methodologies.

The critics of the indiscriminate mathematization of economics argue that the unilateral emphasis on mathematical techniques inevitably leads to an undesirable development of the science under which form dictates the contents of the investigation, and the availability of techniques determines the choice of problems, contributing to misled practices in academic production of theoretical economic models.

The range of techniques used in Academia is more appropriate to certain kinds of content. The more mathematized economics becomes, the more it ignores real problems to which such techniques cannot be applied. This has been leading scholars to emphasize areas in which the application of mathematical techniques is easier. With indiscriminate mathematization, economics ends up concerned exclusively with quantifiable phenomena and characteristics, and neglects those that cannot be approached in this manner, leading to the omission of historic and institutional characteristics.

Another line of criticism against the process of indiscriminate mathematization of economics concerns the interpretation of the analytical results of formal operations. Economists are guilty of neglecting the non-formal interpretation of the meaning of their theorems, as well as of failing to consider the relevance of those results to the real world.

No less importantly, there is absent evaluation in a non-formal language of what the formal language is capturing from reality and what it is leaving aside. Mathematical formalizations are divorced from reality and their contribution to knowledge in economics may be null, or even negative (Woo, 1986, p. 13).

This criticism does not deny the importance of the mathematization of economic discourse. They do, however, stress the need for prudent improvement to how mathematical techniques are applied. The question we raise largely concerns the attitude toward and awareness in the exercise of the profession. By acting with awareness, economists will not reduce the real contribution of formalization.

This, though, is not what typically occurs. Neo-classical economists do not usually adjust the theory to make it fit reality; instead, they attempt to "adjust" reality to unrealistic assumptions in an ad-hoc manner, however absurd this may appear.

DONALD GILLIES AND THE PROBLEM OF OPERATIONAL NUMBERS

More recently, Gillies (2005) developed another criticism. The author finds that the application of mathematics to physical sciences has achieved extraordinary success. As a result, Gillies asks why the same success would be repeated in economics.

The attempt to apply mathematics to economics began, in the manner no applied, with Jevons's 1871 book, "The Theory of Political Economy".[1] In it, Jevons points out that economic theory should be mathematical for the simple fact that it deals in quantities: price, money, etc.

Gillies adds that there is a large number of economic information that can be used to test economic theories, in the same way that astronomical information was used to test Newtonian mechanics. At first, a close analogy seems to exist between physics and economics and, therefore, so does some hope of successfully building a mathematical science of economics. However, other authors like, John Maynard Keynes, differ. Gillies notes that despite Keynes's cautionary warnings, the past sixty years have seen a renewed attempt to develop economics and a mathematical science modeled after physics. Many mathematicians have been working toward this goal. The results, however, have been of little expression. Gillies (2005, pp. 189-190) looks for examples of this inexpressiveness in two mathematical economics works conducted by respected scholars in this domain: the books "Foundations of Economic Analysis", by Paul Samuelson, and "Market Structure and Foreign trade", by Elhanan Helpman and Paul Krugman.

At first, Gillies asks how Samuelson's work compares with the orthodoxy of mathematical physics. The great success of mathematical physicists derives from the fact that calculations performed based on their theories resulted in predictions that can be compared with observed information to show a high degree of accuracy. Samuelson's book fails to meet this criterion, as, according to Gillies, it consists of 439 pages, almost entirely covered in mathematical formulae, without comparing a single result with observational information. Gillies's conclusion about Samuelson's book is that, instead of replicating the success of mathematical physicists, it is more like a work in pure mathematics, lacking any empirical content.

[1] There are obviously precursors in the application of mathematics to economics, such as Isnard, Cournot, Von Thunen, etc. We understand, however, that Jevons is the founder of the kind of mathematization introduced by the neo-classical school and that opened up the route that, with several changes, led to the current situation.

On the other hand, when he refers to Helpman and Krugman's book, the author notes that, out of its 266 pages, only one — page 173 — has data observed in reality that can be compared with the theory's results. The page is concerned with per capita income concepts and a direct relationship between increased intra-industry trade and bilateral trade volume. Gillies stresses two other similar empirical mentions made on the same page and indignantly wonders whether it was really necessary to write 172 pages of complex mathematics to explain a few qualitative results that find poor empirical support.

The author's question is why this kind of mathematical modeling, with practically no empirical content, is deserving of more prestige within the Economic academia, unlike in physics. He then suggests that a fundamental difference exists between physics and economics. The world of physics appears qualitative on the surface, but actually obeys precise quantitative laws; this is the reason why mathematics works for physics. Economy, on the other hand, appears quantitative on the surface, but is actually qualitative; and this is why attempts to create a successful mathematical economics have failed. What sets Gillies's criticism apart from the more traditional kind is the notion that the numbers we routinely use in economics are not absolute values that reflect intrinsic characteristics of the objects being quantified, but are operational numbers in reality. Such operational numbers provide a convenient, yet misleading, means of abstracting a complicated qualitative situation. As their values depend on conventional decisions and procedures, they are arbitrary to a degree (Gillies, 2005, p. 191).

The situation appears paradoxical because goods have prices, business firms have market values, and every item in the firm has an exact monetary value. In physics, unlike economics, falling rocks do not come with numbers attached to them, as do goods and services. It would then appear that economics would afford a better fit with mathematics. The author, however, argues that this is an illusion, as the numbers attached to economic phenomena are operational numbers.

In economics, Gillies (2005, p. 193) notes two examples of operational numbers: goodwill and brand. How does one estimate the selling value of a business firm's goodwill or of its brands? In the case of a piece of machinery, one calculates an annual rate of amortization based on the item's life, and brings it back to present value. But this cannot be done with the worth of goodwill and brands, as their values are intangible. As a result, such values are operational numbers determined by rather arbitrary conventions and decisions.

The value of brands, which can fetch astronomical figures, are calculated based on the profits made in recent years, with conventional weights assigned to each year, and on a series of values used to estimate "brand strength" (leadership, market, internationalization level, trending, protection, etc.), all of which are highly arbitrary. Similarly, the accounting conventions applicable to goodwill and brands change from one country to another, so that a value that might appear objective (the firm's profits) is ultimately essentially dependent on those conventions. Therefore, these numbers can easily lose any connection with reality. This is one of the main reasons why Gillies emphasizes the caution needed applying mathematics to economics, showing why the goal of creating an economics similar to physics is questionable.

What route, then, might be available to economics? Gillies does not believe that abandoning mathematics would make economics a minor science; for him, many scientific fields have been making important discoveries without using mathematics, and Economic should perhaps model itself on them over physics. One interesting suggestion is that economics should be based on Medicine, which made significant gains between 1860 and 1945 without the use of mathematics, and developed casually instead.

The work of Keynes followed a method similar to Medicine. Keynes (1985) identifies the economic problem the world faced as a result of the Great Depression, stagnation and unemployment, and then lays the groundwork for his theory as a political economy suggestion to address those problems.

BRESSER-PEREIRA AND THE INADEQUACY OF THE HYPOTHETICAL-DEDUCTIVE METHOD USED BY MAINSTREAM ECONOMICS

In reality, all of the foregoing criticism reflects, to a degree, some economists' inconformity with the economic policy prescriptions generated by the models derived from the neo-classical school. This dissatisfaction originates from the gap between this perspective's prescriptions and their effectiveness in terms of forecasts. No corroboration exists of the theory's predictions, which we believe to be associated with their unrealistic assumptions. This, we believe, is due to the inadequacy of the method that this theory proposes.

One author who has more recently discussed the issue objectively is Bresser-Pereira (2008), who splits sciences into two categories: on the one hand, we have the methodological sciences, which lack a particular subject of study and include mathematics, Statistics, Econometrics, and Games Theory; on the other hand, we have substantive sciences, which do have subjects and subdivide into natural (such as physics and Biology) and social sciences (like economics and Sociology).

Bresser-Pereira (2008, p. 3) maintains that, while the hypothetical-deductive method is appropriate for methodological sciences, the empirical-deductive, or historical-deductive, method is proper of substantive ones, both natural and social. "I make this claim because, given that the objective of economics is to analyze and predict the behavior of the economic system, the historical-deductive method should be the principal method applied." Both methods use deduction; the difference between them is not a matter of degree, but the fact that one sets out from aprioristic hypotheses, while the other is historic and part of a sequence of observed facts.

According to Bresser-Pereira, methodological sciences do not have a subject of study, but rather an instrumental purpose: they aim to facilitate or assist thinking. For them, the hypothetical-deductive method is the legitimate method, and the criterion for truth is logical consistency.

Substantive sciences, in their turn, have a subject of study and, therefore, their legitimate method is the historical-deductive method. Correspondence with reality is the truth criterion of this method's propositions and models. This is what lends them predictive power and the ability to drive action. Social sciences should only resort to the hypothetical-deductive method as a secondary choice.

Economics is a substantive science whose subject is economic systems, their stabilization, growth and distribution properties. Therefore, it should use the historical-deductive method. So did the mercantilist economists, and classical ones like Adam Smith and Marx. But neo-classical economists broke away from this tradition and started relying on the hypothetical-deductive method. When applied to economics, it enables economists, based on precise hypotheses or axioms, and quantifiable principles, to arrive at equally precise, but essentially, mistaken, conclusions. It all begins with *homo economicus*, whose behavior is entirely predictable, and proceeds to include a few assumptions to enable a precise, mathematical theory. The historical-deductive method,[2] in its turn, does not start from simple assumptions, but from observations of a complex, shifting reality.

The novelty in Bresser-Pereira's criticism of the neo-classical theory lies in the fact that it is a methodological criticism. The reason why this theory usually fails to match reality is twofold: firstly, it is impossible for one to comprehend economic systems based on the assumption of an entirely rational and predictable actor; secondly, neo-classical economists are not interested in such a match, but rather in the model's internal logical consistency.

For Bresser-Pereira, many in the economics academia (and not neo-classical ones alone), attracted by the scientific status mathematics lends, tend to use highly mathematized and logically consistent hypothetical-deductive models; these models, however, are closed and, in most cases, lack a relationship with the substantive subject they propose to analyze (the economic system). Bresser-Pereira proposes that, in a substantive science, induction and deduction must combine. He argues that the problem lies not in rejecting the hypothetical-deductive method within the domains of a substantive science, but to give it precedence over the historical-deductive method. For him, it is unacceptable to use logical consistency as a truth criterion instead of fit with reality and/or predictability. According to him, in social sciences, one should think not just historically, but also dialectically. Within reality, causes and consequences are muddled. Social reality is intrinsically historical because it is constantly changing, and is intrinsically contradictory because social systems are made up of individual agents who, though socially conditioned or determined, are free and responsible for making many choices that constantly confront one another; because they are learning agents who change with experience

[2] Bresser-Pereira also refers to the historical-deductive method as empirical-deductive method, which is a term more appropriate for natural sciences. While the empirical-deductive method is mainly analytical, the historical method is both analytical and dialectic. The analytical method is applicable to methodological and natural sciences, and to physics in particular.

and, by doing so, constantly change the social structures and, above all, create a culture and institutions that, in their turn, shift individual preferences.

Bresser-Pereira points out that neo-classical economists admit the need for a "positive or empirical method to understand their respective subjects" (Bresser--Pereira, 2008, p. 9). For this reason, they regard Econometrics as a powerful instrument to achieve this objective. But when econometric studies — as well as those of the new behavioral economics — falsify the theory, they do not hesitate to resort to the Popper-Lakatos protective belt and continue to defend the model.

Bresser-Pereira takes it as a given that the objective of economics is to study economic systems. If, instead, it aimed to help people make economic decisions, if it were a theory of economic decisions rather than a theory of economic systems, then it would be appropriate to embrace the hypothetical-deductive method. For him, contrary to what the neo-classical theory holds, Marshallian micro-economics does not lie at the heart of economic theory, but is a founder of the science of economic decision-making, next to games theory. They both use the hypothetical-deductive method, and rightly so, as theirs is a methodological science.

Bresser-Pereira emphasizes that, with the historical-deductive method, one first absorbs real facts to formulate hypotheses, then resort to deductive reasoning, and finally, inductively return to the facts in order to check them econometrically, all with the purpose of verifying a certain theory.

His criticism lies in the fact that neo-classical economic theory relies on a method more appropriate for methodological than substantive science. The historical-deductive method only observes rational motives a posteriori, rather than a priori, do define how reality is or should be. Economists must permanently observe the facts of reality, develop their theories, and go back to real facts to check their models.

For Bresser-Pereira, the neo-classical theory was able to mathematize Economic because it uses the same method as mathematics — the hypothetical-deductive method. This is what made it arrogant and mistaken. The arrogance lies in economics deeming itself as precise as a methodological science; the mistake, in its being a "castle in the clouds", distant from economic and social reality, since its final truth criterion is not correspondence with reality, but internal consistency — the same criterion as for mathematics.

WOO'S CRITICISM OF THE PROBLEM OF THE SUBSUMPTION OF QUALITATIVE TO QUANTITATIVE VARIABLES

What contributed to the dissemination of this thinking, seated on mathematical precision, was a series of theories linked to the ideas of general equilibrium that had been building from the Marginal revolution of the late 1800s through to Debreu's proof in the 1950s. Development of the proof of equilibrium was only possible given the paradigm change in mathematical physics in the early 20th centurytury, when hypothetical-deductive models started to prevail through increasingly sophisticated formulae and no need for empirical proof (Weintraub, 2002).

Woo (1986) states that, as mathematical formulae were introduced into economics, a sort of subordination of qualitative to quantitative variables took place. The meaning of this lies in the fact that, when one builds a theory, although some distortions relative to reality are tolerable, they should not be murky to the point of preventing interpretation of the real world. And the production function used in the general equilibrium model is murky indeed. This is because the formula that the model uses assumes empirically unsustainable hypotheses, such as the perfect substitution of capital and labor, factors that are subject to infinite divisibility. This ultimately obscures the limit of the interval at which input values may change without leading to the "collapse" of the formula, and also omits the question of what might be required for the interaction, growth and constitution of the variables before they developed within the phenomenon to which the formula refers. Because analysis of these subjects is obscured by the very hypotheses regarding input variables, no interval of exceptions can be read from the formalized theory itself.

As a result, economists are forced to add a series of exceptions that appear in reality in order to salvage the function. We believe that this fact has led to the prevalence of "ad hoc" in real situations that the theory does not predict. The function's formalist constitution includes metrics that obscure the interval at which the variables can be substituted, as well as the variables' interaction, growth and constitution. There are almost infinite possible exceptional cases, which may create a paradox: the more sophisticated a formula, or the larger the number of variables in it, the more it will lead to permutations of relationships subordinating unmeasurable variations. Such a formula would, as a result, distance itself from truth in the real world's. It would become a mere particular case among endless logical possibilities that are difficult to map out in reality. In fact, Woo (1986, p. 29) emphasizes that a reverse relationship exists between the level of abstraction and truth in social sciences. He calls this the law of the diminishing field of application.

This law shows a reverse relationship between the extent of unqualified applicability of a formula to reality and the apparent universality of the formula inherent to its equally apparent precision. Qualified applicability of a formula to reality would mean, given these definitions, a reduction of its empirical content. Therefore, the empirical content tends to decrease as the number of variables in a formula increases. Another problem with the subordination of qualitative variables to formulae (mathematical formalization) is that the number of relevant intervals of the mutually substitutable values, determined by the factors that govern a variable, and the interval limit of such values for a variable cannot be regarded as constant and, one cannot infer that they remain unchanged over time. The production function illustrates this situation: its main variables, according to neo-classical theory, are capital and labor. In this sense, Woo argues that the measure at which capital and labor are mutually substitutable does not depend only on qualitative variations subordinated to these variables, but also on factors such as technology, organizational structure, know-how, etc. Such "trellis" relationships can hardly be captured by joining variables provided by means of syntactic relationships from certain formalization techniques. Clearly, the more complex a formula that attempts to describe an economic or social reality, the lower its ability to explain this reality's complex relationships and structure becomes. That is, the more complex a formula, the more it ultimately reflects the syntactic structure of the formal apparatus from which it derives (Woo, 1986, pp. 30-31). As a result, the theory turns inside itself.

We can therefore conclude that the adoption of ever more sophisticated formulae by economics, in an onslaught of the hypothetical-deductive method of mathematics by means of the metatheory of general equilibrium, complexly assembled and incorporating the production function, contributes to obscure reality by subordinating qualitative to quantitative variables.

Mathematical economists forget this limitation of the process of mathematization of economic discourse; they attempt to give economy scientific status by using the method of a methodological science like mathematics. We may accept that certain variables subordinate less qualitative variations, such as money and prices, for example. And that, as a result, formalization or non-formalization of concepts is subject to the level of variable uniformity. This idea helps explain why some economic phenomena are, in principle, more susceptible to mathematization. For Woo (1986), however, mathematization would be more efficient in an environment where individual decisions were suppressed, and this would mean that the scope of economics would be greatly restricted.

Let us say that the scope of mainstream economics today allows infinite hypothetical-deductive developments disconnected from society. That is, we might argue that mathematical economists can always accommodate certain types of qualitative variations within their formulae. However, the approach loses focus, bringing about a qualitative distortion by incorporating ad hoc accommodations in order for a formula to survive.

As for the hypothesis tests of economic models, whose support lies in unrealistic assumptions, as defended by econometrists, we understand that they fail to provide conclusive evidence to falsify a theory, unless the equation is exceedingly simple and, as we know, this is not the case in Econometrics today.

According to Woo (1986, p. 44), the subordination of qualitative to quantitative variables grows mainly around theories whose endogenous variables are susceptible to analytical measurement, and therefore easily subject to formalization. In this case, external parameters are accepted due to their empirical relevance. However, it is assumed as a starting point that endogenous variables remain unchanged, while, at a later point, the inclusion of exogenous parameters is accepted to add empirical content to the domain in question. It remains a priority to formalize the relationships among endogenous variables. Mainstream economics believes that this primary task is crucial to ensuring the future incorporation of empirical content.

The problem with this conception lies in the fact that the development of relationships among formal endogenous variables departs from development in the direction of realism. It creates a sort of self-sustained formal reconstitution that prevents bringing outside parameters into this basic framework. This is what Woo (1986) refers to as the fossilization of parameter-variable relationships. On the other hand, economic theories that are free from this fossilization are normally characterized by the presence of core variables that are no subject to quantification. And, although such theories are regarded as inferior by more formal ones, given that they cannot establish precise relationships among endogenous variables, they escape the fossilization process and stand as open-ended theories, as illustrated by the theories of Marx, Keynes, etc., who use the historical-deductive method as discussed by Bresser-Pereira (2008).

While the parameters (exogenous variables endowed with empirical content) are left in the background, the formal structure becomes the foundation for new analyses, as illustrated by the production function mentioned earlier. In these latter developments, the product relates with different combinations of capital and labor, becoming the starting point for subsequent analyses in the theory of economic growth.

Development occurs in this direction because it is difficult to introduce a non-formalizable parameter into a formal framework. Formal reconstitution determines the theory's impenetrability to almost anything that is not formal, even as it gives the impression that knowledge is advancing. This impression stems from the argument according to which mathematization makes economic knowledge simpler and more transparent, in hopes that eventual inconsistencies may be repaired via formal reconstitution. Such reconstitution would supposedly lead to a "mature domain" of scientific economic knowledge, enabling investigation of the science's theoretical limits.

However, formal reconstitution implies the temporary fixation of relationships between the original concept and the experience from which such a concept emerged. This provisional initial fixation may be appropriate from the empirical point of view and, therefore, not harmful, as long as the purpose of the fixation (original experience) is kept in mind. The problem begins when the origin of the fixation is forgotten and the fixation itself becomes a given for the model. This leads to loss of the initial intuition linked to reality. As a result, "[...] ability or at least the mental agility to develop alternative concepts to capture the same part of reality is incapacitated" (Woo, 1986, p. 49). At this point, anything that is being regarded as a given for the problem ceases to be questioned. In fact, the theories that grow from this point by means of formal reconstitutions do so on bases that appear sounder than they really are, and require more and more formal fixations on the basis of the mathematical analytical rationale, and no longer on the substance of reality.

It so occurs that, in order to facilitate formalization, one must often alter a richer original concept. Woo (1986) points out the case of Menger's representation of marginal utility in terms of a hierarchy of desires which, despite being richer and more realistic, was incompatible with calculations (the formal technique of the time) and abandoned as a result. To circumvent this, there emerged the concept of the *homo economicus*, whose choices are transitive, which often not true, and complete, which demands a capability normal beings lack. And even the indifferent situation, which replaces the notion of utility, has little analogy with real situations. Consumers are seldom in a situation of indifference and, when they are, this is due more to decision-making problems than to a calibration of their preferences. That is, formal reconstitutions end up creating a new family of concepts within a domain that substitute existing concepts. And where general concepts are not easily substituted, formal reconstitution modifies them to generate a new set of notions whose mathematical logic becomes prevalent over empirical observation (Woo, 1986, p. 50).

Still, these "novel" concepts that emerge from formal reconstitutions are not new relative to knowledge of external reality, but rather to mathematical innovations in the theory's internal framework.

As formal reconstitutions occurred in economics, the criterion for correspondence with truth was exchanged for extra-empirical criteria. These include, according to Woo, formal rigor, the requirement of symmetry, simplicity, conformity with certain a priori conditions, etc. These new evaluation criteria make up the heuristics that drive the development of new reconstitutions. Under such circumstances, the domain is expanded not by means of its empirical base, but of new layers of reconstituted concepts and theories, turning into unlimited activity.

The consequences of the self-sustained formal reconstitution of extra-empirical models emerge through a series of successive steps. At first, axiomatization and construction require cutting off less relevant empirical concept. Secondly, as extra-empirical concerns begin to prevail, the empirical content that was left begins to change. This is due to countless reinterpretations that transform the original empirical content into several increasingly theoretical contents. Due to the countless reinterpretations made over time, a specific domain is created that can only be understood by decoding definitions and rules of correspondence. For this reason, recovery of the empirical contents in such a domain will be highly indirect, insofar as it must be decoded several times in order to return to the real world. As a consequence, as formal reconstitutions take place, the empirical contents become less and less relative to the accumulated hypothetical-deductive analysis. Over time, "[...] the domain in turn undergoes a kind of internalization process, with more and more research resources directed towards the buildup and elucidation of its internal structure" (Woo, 1986, p. 54). This ultimately leads to what Woo refers to as wholesale formalism, where several domains are rewritten in terms of formal concepts. One outcome of this kind of formalism is that scientific production efforts end up diverted to trivial matters, such as when a hypothesis is loosened to see how a result that is only valid in theory maintains itself under the new situation.

This wholesale formalization cannot be deemed neutral from the epistemic angle, and causes a kind of distortion of organized reality, which evolves with the continuous reconstitution of the domain. The criterion of evaluation by means of correspondence with reality ceases, and prevalence shifts to the hypothetical-deductive method's criterion of analytical coherence. This occurs because the empirical contents of models become difficult to test, as these models are overloaded with extra-empirical content. This leads to the development of auxiliary ad hoc assumptions in order to protect the hard core from being falsifiable. Another aspect is that, with the accumulation of extra-empirical knowledge, falsification becomes difficult:

> [...] it would be more difficult to falsify the more important propositions of the domains even if no conscious protection work is done. New empirical data that may be available to test a new proposition will naturally face increasing difficulty penetrating into the hard core which is now shielded by levels of reinterpreted content and proliferated peripheral structures. (Woo, 1986, p. 55).

This epistemic development in Woo (1986) may be regarded as a metatheory of the development of social sciences, and does apply to economics. The author, however, is concerned with describing an epistemic outline of how all theories in social sciences are born, mature, decline and are replaced. Not all theories fit this scheme, but Woo attempts to show certain universal aspects that emerge when a social science undergoes a mathematical formalization process. One example is the fossilization of parameter-variable relationships, the embryonic framework's limited ability to capture reality, the shift from empirical to extra-empirical concern, etc.

These formal reconstitutions have been taking place in economics, we believe, from Walras's general equilibrium model, which limited development of the science by basing itself on an imitation of Newtonian physics (whose criterion of truth was correspondence was correspondence with reality), through Wald, Morgentern, and Von Newmann, until the metatheory of equilibrium developed by means of Debreu's proof, supported by changes that took place at the foundation of mathematical physics itself, enabling infinite possibilities of constructing hypothetical-deductive model based on logical coherence. We understand that Debreu's equilibrium lies at the root of wholesale formalization.

THE GENERAL EQUILIBRIUM THEORY AS METATHEORY (EPISTEMIC ARCHETYPE) REPRODUCING THE HYPOTHETICAL-DEDUCTIVE METHOD IN ECONOMICS

Woo (1986) attempts to demonstrate the formal method's limited relevance enriching the contents of a given domain. The formal reconstitutions of a domain's embryonic framework, in which lie the original concepts that correspond with reality, end up metamorphosing the theory. The continued reconstitution of theories also generates specific, peripheral domains subordinated to the central metamorphosed domain. This is what Woo calls wholesale metamorphosis.

This development toward wholesale metamorphosis is activated by the connection between the existing domain and certain metamodels that standardize interpretation of the empirical contents. We interpret these metamodels as a metatheory.[3] For Punzo (1991), metatheory is a set of instructions for the selection of indefinite terms, the combination of these terms into well elaborated formulae and, finally, obtaining true propositions by means of deductive reasoning in the form of theorems. The metatheory (epistemic archetype) is characterized by high generality as well as openness to other domains. And it is able to capture a range of pseudo-empirical contents.

Metatheory appears to have enormous explanatory power and may be compared to a "tool box", with a standard set of formal instruments. As such, we understand it to be capable of providing the structural basis for standardizing the problems it formulates for any peripheral domain, and systematizing the way in which problems are to be evaluated and solved. These characteristics give the metatheory certain properties that excessively influence Academia.

According to Woo (1986, p. 75), by systematizing how problems must be solved, the equilibrium archetype establishes a hierarchy that peripheral domains must embrace. However, the fact that a domain interfaces with a metatheory will produce what calls a "curse in disguise": when a domain's economists become used to standardizing problems and solutions as dictated by the metatheory, wholesale metamorphosis takes place as a domain undergoing total reorganization and reinterpretation, constituted by means of the standardization of the metatheory. Over time, the domain will no longer borrow the metatheory's ideas, as it alters the original theory and prevents the production of genuine knowledge, with a weakening connection with reality. The continued reinterpretation of the original domain ultimately results in technical pseudo-knowledge foreign to the original domain. Mixture of pseud-knowledge and genuine knowledge may exists, and the more metamorphosed a domain, the harder it becomes telling its genuine from its non-genuine characteristics.

For Woo (1986), it would be naive to assume that any scientific domain could develop absent the influence of epistemic archetypes, which we interpret as metatheories for the purposes of this paper. The central issue, then, is how to best use such metatheories and, at the same time, prevent colonization of other domains. In the case of a social science, one would have to develop a methodology to prevent uneven dependency on any particular metatheory. Such a methodology must stablish objective criteria, on the one hand, and, on the other, render perceivable the point beyond which application of that particular metatheory would cause it to degenerate.

> One criterion would be whether or not application of an archetype would result in the discovery of even deeper non-syntactic relationships among the concepts within a domain, or if continued application would lead to aborting new discoveries of such non-syntactic complexities. (Woo, 1986, p. 89)

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[3] Woo (1986) refers to metamodels as epistemic archetypes.

Regarding economic thinking from this angle, we find that there is an enlightening example of how peripheral domains give in to the standardization that comes from a power, dominant metatheory like the equilibrium metatheory. For Woo, this helps understand how economics has been experiencing a lengthy degenerative process by means of theoretical metamorphosis, bearing in mind that the subject as a whole may be described as equilibriumized.

The question of equilibrium is not censurable in and of itself. In fact, the notion of equilibrium is highly convenient for economics. Many of the questions asked in economics have to do with the heuristics of equilibrium. The question is that we must go beyond equilibrium. For example, for Woo (1986) the central question of Adam Smith's "The Wealth of Nations" was to find out whether a harmonious social order could be obtained in an economic world with decentralized agents seeking to promote their personal interests, and whether this might be conceived of as a matter of equilibrium. For Smith, this equilibrium was non-stationary and, therefore, the question of the modern general equilibrium does not exhaust the Smithian notion of equilibrium. We might mention Keynes as another example. In his general theory (1985), he seeks to find out whether governmental action is helpful toward equilibrium. Both cases are related with self-equilibrium and open up a fruitful field for economic investigations. However, mainstream economics has limited itself to interpreting the notion of self-equilibrium capacity in favor of a technical representation that implies fossilization of the state of equilibrium and the investigation of a mathematical apparatus to ensure the existence of this economic equilibrium.

Woo's wholesale metamorphosis has transformed research programs in the domains of macro- and micro-economics. This has led to what may be referred to as the "equilibriumization" of economics.

If we look at traditional micro-economic, we may se that the advance of the hypothetical-deductive method has shifted the answers to economic problems to restricted maximization exercises. This laid the groundwork for the focus of the question to shift to how to represent an economy in equilibrium.

Attempts to prove the possible existence of the general equilibrium, which began with Walras in the late 1800s, met with relative success with the mathematical proof of equilibrium that von Neumann developed in the 1930s. The same author, together with Morgenstern, also formulated the general equilibrium theory from the angle of games theory. Von Newmann's main criticism was that, at the time, proof of equilibrium by purely mathematical meals failed to consider interaction among the agents, considering instead the maximizing attitude, such as the consumer's when maximizing the utility of the act of purchasing a good. Using powerful argumentation, games theory helps uphold a taxonomy of strategies that individuals can adopt in their interactions with other individuals in situations of conflict or cooperation. These interactions are mapped as *homo economicus* decision-making strategies. In alliance with the von Neumann and Morgenstern's games theory, as well as Nash's, the general equilibrium theory was reinterpreted as a cooperative game of *n* persons whose equilibrium outcome may be improved by the formation or dissolution of any coalition. With these ideas, games metatheory provided important support to equilibrium metatheory.

These developments, however, left by the wayside important methodological considerations, such as what Woo calls the isomorphism of mathematics and economic reality. This means that "validity of existence proof and the subsequent search for equilibrium values are contingent upon and relative to the isomorphism of relations between the real world and the set of mathematical equations" (Woo, 1986, p. 85).

This shows that the methodological problem is that such isomorphism is lacking from the most rigorous hypothetical-deductive representation of the equilibrium metatheory. This may seem obvious, as the subjects that prevail economic studies lie largely distant from equilibrium. Formal systems employ certain measurable variables. Unobservable factors, such as enterprise, which leads agents to take investment risks, are excluded from the analysis by means of formal instruments. As a result of this exclusion, any isomorphism has only a superficial relationship between reality and the instruments used. As a result, without an isomorphism with greater real content, the mathematical effort is reduced to a mere academic exercise. Such a formal academic exercise does not produce "genuine" knowledge. We are simply under the impression that economics is producing knowledge, when, in fact, all there is is a process of filing models whose results are known beforehand.

Although games theory may appear on the surface to have increased the general equilibrium's representation power by extending the theory from two to *n* persons, and form non-cooperative to cooperative, the notion of rational agents remains. That is, the formal properties developed by games theory are not isomorphic with the cognitive properties of a real consumer. Games theory fails to account for the cognitive weaknesses of an ordinary man carrying out actions and making economic decisions. As a result, games metatheory is incapable of addressing important economic questions. Consequently, games theory produces, at best, theoretical results for a rational marketplace.

We know that, in its turn, the Keynesian notion that the economy is incapable of self-equilibrium and therefore needs State intervention was reformulated into the neo-classical equilibrium theory (Ferreira, 1997). Neo-classical and neo-Keynesian economists converged on general equilibrium models and rational expectations in the 1970s and '80s, forming the new mainstream economics in macro-economics. The consequence was the loss of Keynes's ideal of disequilibrium factors in the labor market, such as uncertainty. Keynes, according to Woo (1986, p. 88), argued that equilibrium was a hypothetical notion that could never be achieved. Hicks, Samuelson, Patinkin, Lucas, etc., however, took the disequilibrium situation as shown by Keynes to represent a special case of the neo-classical theory. The idea of "equilibriumization" of the economy, which began with the Marginal revolution of the late 1800s and was subsequently developed by Wald, von Neumann, Arrow-Debreu, etc., consolidated during the 1970s and remains to this day as the mainstream economic metatheory.

One implication of "equilibriumization" is the hypothesis that the majority of economic phenomena are "equilibriumizable", and that those that are not can be captured by disequilibrium. That is, all economic phenomena are within range of equilibrium or disequilibrium. Therefore, all economic phenomena can be analyzed from the angle of equilibrium. Woo (1986) differs by stating that economic analyses should focus on the adjustment process, instead of final states of equilibrium.

By taking the equilibrium theory as a given, mainstream economists destroy the possibility of application and development of competing, alternative, theories. This, therefore, reveals the naiveté of adopting a new angle to formulate the problem. The equilibrium metatheory not only imposes the hypothetical-deductive method, as Bresser-Pereira (2008) describes, for the purposes of problem formulation and evaluation, but also provides a kind of metaheuristics for the formulation of new research programs. Because economics develops along the lines of equilibrium, its hard core grows around equilibrium itself, far more than around the study of the real economy. General equilibrium theory scholars are exploring the properties of the formal hypothetical-deductive structure of the equilibrium metatheory more than how equilibrium and disequilibrium forces operate in the real world. In the same manner, today's mainstream micro-economics may be regarded as a science of mathematical equilibrium instead of a science of the economy, as it makes no causal statements. This is due to the fact that equilibrium economics is no longer a real field of economics. Abstraction in micro-economics is such that it has no concern with real-world matters. This thinking is extensive to macro-economics, since it derives its fundamental from micro-economics.

For Woo (1986, p. 96), the consequence of equilibriumization is that economics has been cast into a state of "intellectual schizophrenia" that is nothing more than this departure from reality caused by the many formal reconstitutions. For Bresser-Pereira (2008, p. 185), the true hard core of economics lies not in the neo-classical triad (general equilibrium, Solow growth model and neo-classical macro-economic model), but in the contributions from the classical and Keynesian schools — the two schools of economic thinking that are based on the historical-deductive method. The classical school contributed its micro-economic theory, or prices and distribution theory,[4] and with its theory of growth based on capital accumulation and technical progress; the Keynesian school contributed macro-economics.

CLOSING REMARKS

In economics, the general equilibrium theory is largely responsible for the process of mathematization of economic discourse, and is based on the hypothetical-deductive method. Bresser-Pereira (2008, 2012) noted that the use of this meth-

[4] To which I have offered an alternative analysis, keeping the profit rate constant in the long run and letting the wage rate — the residual — grow with increased productivity and technical progress type (Bresser-Pereira, 1986).

od by neo-classical economists has necessarily led to mistaken mathematical models, because this method is appropriate for methodological sciences such as mathematics and the theory of economic decision-making, and not for a substantive science such as economics. Our main question in this paper, however, was: how did this method disseminate in economics? Bresser-Pereira has an answer: the same reason that misled neo-classical economic theory — the use of the hypothetical-deductive method — was also the reason that enabled the complete mathematization of economic theory. In his words, "insofar as the method's starting point is a principle — the *homo economicus*, whose behavior is entirely predictable — complemented with a handful of additional assumptions, such a method enables a precise, mathematical theory".

In addition to this general answer, we borrow ideas from Punzo (1991) and mainly Woo (1986), to try to show that this dissemination took place by means of the incorporation into economics textbooks of the general equilibrium model, which stands as a metatheory (Weintraub, 2002; Punzo, 1991). General equilibrium as metatheory was the main culprit of the still ongoing increase of mathematical formalization by disseminating the hypothetical-deductive method.

General equilibrium became a metatheory due mainly to the influence of Gerard Debreu, who in 1950 had become a permanent member of the University of Chicago's Cowles Commission. As a member, he followed a method very similar to Bourbaki's theory of sets, which established a series of formal rules for scientific production in mathematics. The need for economics to attain the status of science led several economists, from the Marginal revolution to Debreu, to search for proof of the general equilibrium, although Weintraub (2002) launched a series of questions on whether such proof had in fact been provided. Equilibrium was accepted as truth by the late 1950s, and incorporated without much criticism into the main graduate-level micro-economics textbooks. And is even now accepted by mainstream Economic and used as metatheory in the reproduction of hypothetical-deductive economic models.

According to Punzo (1991, p. 13), post-Debreu models and deductive frameworks became coextensive. In other words, since they lacked empirical fundamentals, these were replaced with the required formation of a full set of independent and coexisting axioms, and metatheoretical principles to produce appropriate answers to the questions formulated based on the theory.

General equilibrium, by acting as metatheory and reproducing the hypothetical-deductive method by means of formulae such as the production function, for example, ultimately led to the subsumption of qualitative to quantitative variables, as Woo (1986, p. 28) notes. A formula developed according to economic models may even appear to be true amid a range of phenomena. But this only happens in reality if the qualitative variations across the values of variables are more restricted and if there are few constraints on the interval of inter-substitutable values, Woo emphasizes. This is characterized by a large distance from a state of equilibrium, he adds: "It is inescapable that a large part of the concepts and variables we employ to theorize about the world are liable to subsume important qualitative variables".

Actually, Woo's (1986) objective was to show how social science theories are born, mature, decline and are replaced. To this end, the author attempted to show the main problems created by mathematical formalization in economics.

The purpose of this paper goes a little beyond Woo's. We attempted to understand how the hypothetical-deductive method disseminated in economics by means of the general equilibrium theory.

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