Representation as a Process of Model-Building:

A Case from Economics

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## Abstract

What does it mean to say that a theory *represents* the targeted phenomenon that it aims to explain? Our interpretation of "representation" is closely related to the methodological position that we would adopt in answering the question of realism in science. As is pointed out by Nancy Cartwright, according to the traditional syntactic approach of explaining scientific theorization, the question of realism is about how *accurately* the sciences can represent the world; in the semantic approach, however, the focus of the question shifts to a concern about the range of science—i.e., how *much* of the world the sciences can represent. This shift in the methodological concern is by no means trivial; it indicates that there is a change of content in the concept of representation from a static idea to a dynamic one. The static idea of representation concerns how reliably the formal structure of a class of sentences—i.e., the formal structure of a theory—can stand for the targeted phenomenon. The dynamic idea, however, perceives a theory as a class of models and explores the development of these models; that is, the dynamic idea of representation investigates how a theorizer uses these models to stand for reality. As a consequence of this shift from a static to a dynamic mode of thinking, it seems that model-building constitutes the main content of the concept of representation. By comparing two differing contemporary accounts of the nature of economic models and presenting a case study in economic theorizing, this paper argues that representation is a process of economists' repeatedly using "realistic representation of the isolated unrealistic world" at each step of their theorizing to build up a class of "unrealistic constructed credible worlds."

## I. Introduction

What does it mean to say that a theory *represents* the targeted phenomenon that it aims to explain? Our interpretation of "representation" is closely related to the methodological position that we would adopt in answering the question of realism in science. As is pointed out by Nancy Cartwright (1999), the usual philosophical topic of realism in science is about how *accurately* the sciences can represent the world; but her focus of the question shifts to a concern about the range of science—i.e., how *much* of the world the sciences can represent. This shift in the methodological concern is by no means trivial; it indicates that there is a change of content in the concept of representation from a static idea to a dynamic one.

What then does it mean to say that the concept of representation is a static idea? According to the traditional syntactic, or orthodox, approach of explaining scientific theorization, a theory is conceived as a set of sentences—or, more precisely, a set of hypotheses—which are expressed in terms of first-order predicate logic and constitute a network of hypotheses. In this sense a theory is a logical structure that includes the most abstract hypotheses—the so-called axioms, which are expressed solely in theoretical terms—along with those hypotheses that are the logical deductive consequences of the axioms and are expressed in both theoretical and observational terms. Within this structure, there is also a set of correspondence rules that help make connections, through various stages, between the theoretical terms and the so-called topsoil of experience; and the anchoring points of these connections are the geneses of the meaning of the entire theoretical structure. The idea of "a theory representing what we see the world" that is captured by this description is a *static* idea, because it concerns *how reliably*, *at a particular point in the development of a theory*, the formal structure of a class of sentences—i.e., the formal structure of a theory—as a whole can *stand for* the targeted phenomenon in the world.

What then is a dynamic idea of representation? According to the semantic account of scientific theorization, a theory is still regarded as an object containing a class of hypotheses that together account for the targeted phenomenon of the world; however, these hypotheses are not conceived, as in the syntactic approach, as free-standing propositions located in the logical structure of a theory. Instead, each of these hypotheses is regarded as being derived from a specific concrete environment indicated by the theory. If we regard each specific concrete environment as a model, then each hypothesis is said to be *derived* from this model and to be *true* of it. From this perspective, a theory can thus be regarded as comprising a class of component models, each of which is used to represent the corresponding part of the targeted phenomenon. The idea of representation manifested in this description of a theory is a *dynamic* idea, because its focus is no longer a matter of investigating whether a theory

as a whole at a particular time reliably represents the targeted phenomenon; rather, its focus is a matter of examining the *long-range development of theorizers' practice of using a class of models to stand for* a class of corresponding parts of the phenomenon; that is, its focus is on examining how much of the world the theory can represent.

As a consequence of this shift from a static to a dynamic mode of thinking, it seems that model-building constitutes the main content of the new concept of representation; but the immediate question is, How should we characterize model-building? Are there any competing philosophical accounts of the nature of model-building? The question is important, because different answers may result in different interpretations of the new concept of representation. By comparing two differing contemporary accounts of the nature of economic models—one proposed by Nancy Cartwright, and the other by Robert Sudgen—and presenting a case study of economic theorizing in international trade theory, this paper argues that, by combining the most characteristic features of Cartwright's and Sudgen's ideas about economic models, representation can be regarded as a process of economists' repeatedly using "realistic representation of the isolated unrealistic world" at each step of their theorizing to build up a class of "unrealistic constructed credible worlds."

**II. Two Ideas of Economic Modeling: Isolationists or Fictionalists?**

In January 2009, the journal *Erkenntnis* published a special issue titled "Economic Models as Credible Worlds or as Isolating Tools?". Because this journal is a very important one in analytical philosophy, this publication indicates that the model-based approach—which was applied and further developed by philosophers of economic methodology in the middle of the 1980s in conducting the meta-theoretical discussion of economic theorizing—has been accepted by the analytical philosophers as a pioneering topic in mainstream philosophical discussion. Philosophers of economic methodology who have adopted the model-based approach have reached consensus that the gap between a theory and the fact it aims to explain—i.e., the issue of realism in theory—should not be dealt with from the perspective of hypothesis-testing but, instead, by model-building.

In spite of this accord, however, there is still disagreement among those philosophers about the role or function of economic models—i.e., what feature or characteristic they contain—that makes them credible in providing a meta-theoretical account of the issue at hand. The main objective of the publication of the special issue was to clarify the nature of economic models by comparing and discussing two competing positions. One view is held by *fictionalists*, and one of their representative scholars is Robert Sudgen, an economists. Supporters of the other position are called *isolationists*, and Nancy Cartwright, a philosopher of science and economics, is their

representative scholar.

According to the fictionalists, from the perspective of the practice of economic theorizing, economic models are "credible but counterfactual worlds." What does "counterfactual" mean in this context? For fictionalists, the worlds depicted by the models are parallel to the actual world; and if these parallel worlds have any features, these features do not necessarily correspond to any relevant features of the actual world because these features are derived from constructed fictional worlds; these fictional worlds are obviously different from the actual world, and so these features correspond solely to the features of the fictional worlds but not at all to features of the actual world. Thus, the fictional features are regarded as counterfactual.

What then does "credible" mean? When economists deal with practical economic problems and are consulted by governments for policy advice, the fictionalists suppose that notwithstanding the counterfactual nature of the constructed fictional worlds, these worlds are still parallel worlds to the actual world and therefore credible sources for economists to use in constructing tips and advice for tackling targeted economic problems. In other words, according to the fictionalists, economists clearly understand that the hypotheses derived from their models are suitable only for describing the features of the models, but they are confident in supposing that these features can still help them, to a certain degree, to deal with practical economic affairs.

Why are economists so confident in this belief? The key point is that they think they can conduct reliable inductive inferences on the basis of various criteria—such as similarity, salience, credibility, and so on—that they derive from the comparing the relevant features of these two kinds of worlds (Sudgen 2009, p. 4). For example, Sudgen himself applies the concept of similarity--which was originally proposed by Ronald Giere to explain the similarity between a model and the actual world—as a tool to explicate how economists' confidence is derived and established.

Just as Giere did, Sudgen characterizes a theory as an entity comprising a class of models and a set of hypotheses that connect these models to the systems of the actual world; Sudgen also, like Giere, uses the case of Newtonian mechanics as an example to explicate this idea. According to Giere, the Newtonian theory contains a family of models; for each model in the family, there is a common general schema that states that force equals mass multiplied by acceleration. One model in the family is about the two-particle gravitational system, in which force is related to distance and mass by the Newtonian inverse square function. The relative motion between two particles, which is derived from this model, is a property of this model and is not presumed to correspond to any property of the actual world. But this model contains a hypothesis stating that the relative motions between Earth and the moon are very

similar to those of the particles in the model; it is this hypothesis that connects the model to a system in the actual world. Is this connection adequate? To answer that question we must further consult other relevant empirical evidence to determine whether the hypothesis is well supported. In other words, as for the question of whether the Newtonian laws of motion, which are derived from the two-particle model, also hold in the planetary motions, the answer will depend on whether scientists can discover evidence in which the interaction of two planetary objects indeed follows what is depicted in the laws of motion. (Sudgen 2009, pp. 16-17)

By this example, Sudgen argues that, according to his observation, economists possess the same ideas and follow the same procedures as physicists do in their theorizing; that is, economists possess the same ideas concerning the general structure of a theory, and they follow the same procedures for constructing theoretical models and testing hypotheses that are derived from the models; but, as with physicists, economists do not presume that features derived from the theoretical models correspond to any features of the actual world. Instead, the validity of the correspondence must be judged by whether the hypothesis connecting the two systems—the models and the world—is found to be supported by the relevant empirical evidence. Consequently, unlike the isolationists, who characterize model-building as a work of "deducing the effects of known laws in controlled or idealized real-world systems," Sudgen maintains that "a model is a construction, not a stripped-down description of the world" and that what economists are doing by model-building is "investigating the properties of a model," and that's it—no more and no less. (Sudgen 2009, p. 17) As for comparing the similarity of the features of the actual world and those of the fictional world, that task belongs to the work of inductive inference and is not a part of the work of model-building.

Contrast Sudgen's view with the view of the isolationists, who assert that a model is an isolating tool that is used to isolate the targeted economic phenomenon from other possible disturbing factors. As is maintained by Cartwright, this isolation is what allows economists to identify the reason that possesses the capacity to cause the targeted phenomenon. From this perspective, we can say that when economists investigate the various properties of a theoretical model, they, at the same, also investigate those various properties of the targeted phenomenon that is represented by the model. For isolationists, a model is not a full-fledged representation of the targeted phenomenon; instead, it represents the most salient features of the phenomenon. Cartwright offers the example of 2005 Nobel laureate Thomas Schelling's famous checkerboard model of segregation to illustrate how the patterns of exertion of the capacity of a cause are formed in a model. (Cartwright 2009, pp. 46-47)

In Schelling's model, black and white checkers represent, respectively, the black and white citizens (or any two different ethnic groups) in a U.S. community. Initially, these checkers are distributed randomly, with some spaces left empty on the board; the way these checkers are placed represents the initial way that the black and white citizens are dispersed in an actual community. Suppose that these checkers—or citizens—can move freely on the board except that they must obey the following rule (or preference): that they "desire not to live in a neighborhood where they are badly outnumbered." Surprisingly, the deduced result is that checkers of the same color—or citizens of the same ethnic group—tend to gather together to form obvious segregated areas, although no one prefers to live in a segregated neighborhood.

According to Cartwright, Schelling's model can be regarded as a minimal model within which there is only one factor that carries the causal power, or capacity, that enables the model to produce the aforementioned result. The capacity that leads the hypothetical simple society to tend toward the segregated result is the preference of the agents in the model—i.e., the preference "not to live in a neighborhood where their own group is heavily underrepresented." Cartwright maintains that although the knowledge of the preference's capacity cannot completely reflect all the aspects of an actual society, it nevertheless represents an important aspect of the phenomenon of segregated communities in a complex society.

The main reason that the knowledge of capacity can represent an important feature of a targeted phenomenon is that, by using the technique of assumption-manipulation, the knowledge is derived from a disturbance-free environment; as a result, the derived outcome is regarded as a piece of information that reflects what the causal factor would exert with its full causal power (or capacity). Therefore, this element of the derived knowledge of the capacity of citizens' common preference can thus be used by economists as a foundation or as valuable information to construct an explanation for the segregation that occurs in an actual community—even though the actual phenomenon is a result of the operation of a great number and variety of causal factors.

Based on the fictionalist and isolationist positions, our immediate question is this: Is there a significant difference in their ways of characterizing an economic model? For Cartwright, who is an isolationist, the answer is no. Cartwright analyzed all of Sudgen's cases of economic modeling to illustrate his view that economic theorizing is nothing more than an activity of constructing counterfactual but credible models. Cartwright concludes that Sudgen shows two things: first, that there are in a counterfactual world some specific factors that are supposed to be the causes of that world's features, and, second, there is a specific way that those factors interact to produce the targeted features of the world. According to Cartwright's reinterpretation,

what Sudgen calls "counterfactual worlds" are in fact what she calls the "isolated environments" of hypothetical models; therefore, Cartwright asserts, Sudgen's practice is in fact applying models as isolating tools to explore what might happen in the actual world. Following Cartwright's alternative interpretation, it may seem that Cartwright aims to resolve the debate between the fictionalists and isolationists by redefining a model as an isolated environment or an isolated experiment. (Cartwright 2009, p. 53-54)

But is Cartwright's mediating work successful? It may not seem so, because the focal point of the debate between the fictionalists and the isolationists is to define the nature of an economic model, and Cartwright simply replaces the fictionalist idea with her isolationist idea, which is squarely contradictory to the fictionalist position. It may thus seem that Cartwright's mediating work is not only out of focus but also a futile attempt to communicate that would not be welcomed and accepted by the opposing party.

What then would an acceptable mediatory proposal look like? One of the main features of such a proposal is that it should start with finding common ground that would be accepted by the two parties. It may seem that Margaret Morrison and Mary Morgan's classic idea about the nature of models—that models are autonomous mediators between theories and the world—would be an ideal starting point. But before we move on to discuss how Morrison and Morgan's idea can help us to find a possible resolution for the debate, in order to have a more solid background for our discussion let us first take a look on a case of economic theorizing in international trade theory.

## III. A Case Study: The Pattern of International Trade and the Leontief Paradox

Our case study focuses on the pattern of international trade. The classical theory of international trade argues that the determinants of international trade are based on specific technological differences between nations. By contrast, the neoclassical theory asserts that these determinants are found simultaneously in the differences between the technologies, the available quantities of factors of production (i.e., factor endowments), and the tastes of different nations. However, by assuming identical production functions and tastes (i.e., the same technological level and taste between different nations), the modern Heckscher-Ohlin-Samuelson (H-O-S) model—the mainstream model of neoclassical tradition—attributes these determinants solely to the differences between the factor endowments of different nations.

This loss in degree of generality is, according to some authors (Gandolfo 1987, p. I.5), the price that must be paid if one wishes to obtain specific conclusions about the structure of a nation's international trade. These conclusions, which were produced by

an attempt to *predict* the pattern of trade on the basis of the observable characteristics of the pre-trade autarkic equilibria, can be summarized in the following two propositions (Chacholiades 1978, pp. 205-6):

1. The cause of international trade is to be found largely in differences between the factor endowments of different nations. In particular, a nation has comparative advantage in the production of—and will export—commodities that use more intensively the nation's more abundant factor. This proposition is known as the *Heckscher-Ohlin theorem.*

2. International trade tends to equalize factor prices between nations and thus substitutes, to some extent, for factor mobility. This proposition is known as the *factor-price equalization theorem.*

Let us focus on the Heckscher-Ohlin (H-O) theorem. Suffice it to say here that the H-O theorem asserts that the ultimate cause of international trade in goods lies in the differences between the available quantities of the producing factors, such as labor and capital, of different nations. The main reason is that differences in factor endowments will give rise to international variations in comparative costs of production which in turn give rise to international trade of goods. Therefore, according to the H-O theorem, the upshot of a nation's international trade will be that a nation will have comparative advantage in producing and exporting commodities that use more intensively the nation's more abundant producing factor. For example, if a nation has comparatively more capital than any other producing factor, the nation will benefit more from producing and exporting commodities that use more capital. Therefore, such a capital-abundant country will tend to produce and export capital-intensive commodities.

Apparently, as mentioned earlier, the H-O theorem is a conclusion that is derived from a highly simplified model; thus, its validity depends on certain factual background knowledge that remains to be analyzed. Accordingly, any attempt to derive specific empirical implications directly from this proposition without referring to the model's background assumptions would be dubious.

Indeed, the H-O theorem was generally accepted until the results of the first serious empirical test challenged its validity (Leontief 1954). Leontief demonstrated that the United States, the most capital-abundant country in the world, exported labor-intensive commodities and imported capital-intensive commodities. This unexpected result, which seemed to contradict the prediction of the H-O theorem, became known as the Leontief Paradox.

The Leontief Paradox stimulated enormous amounts of empirical and theoretical research that aims to explain this anomalous phenomenon. Among the explanations that have been put forth, one is worth noting for our purpose: B. S. Minhas's studies

(1962, 1963) of the empirical validity of the assumption of strong factor-intensity—i.e., the study of the relaxation of commodity's strong factor-intensity assumption and its consequence of the commodity's *factor intensity reversal* phenomenon.

The main idea behind Minhas's studies is as follows: Suppose that there are only two kinds of goods in our economy—capital-intensive goods and labor-intensive goods—and there are only two producing factors in our economy—capital (K) and labor (L)—that are used for producing these two goods. Now suppose that the price of capital (rent) becomes relatively cheaper than the price of labor (wage); i.e., the rent-wage ratio (rent/wage) decreases. As a result, the producers in both industries are willing to substitute the relatively-cheaper capital for the relatively-more-expensive labor in their production. The idea of factor intensity reversals is as follows: If the amount of capital (the relatively more expensive producing factor) that producers of labor-intensive goods substitute for labor (the relatively cheaper producing factor) changes the intensity ratio (capital/labor) of labor-intensive good to a certain degree that is greater than the new intensity ratio of capital-intensive good, which is resulted from the substitution of capital for labor by producers of capital-intensive goods; the factor intensity reversal occurs because the original labor-intensive goods become capital-intensive goods. In other words, because of a change in the relative factor price, the rate of substitution of the relatively cheaper factor—in our case, capital—for the relatively more expensive factor—in our case, labor—in the labor-intensive good industry is greater than that in the capital-intensive good industry; and this *difference in the substitution rate of producing factor between two industries* is so substantial that it is sufficient to change the original factor-intensity classification of the commodity produced in labor-intensive good industry relative to that of the commodity produced in capital-intensive good industry.

Let's see how the notion of factor intensity reversal contributes to the explanation of the Leontief Paradox. We know that, according to the H-O theorem, in a two countries-two producing factors-two commodities model, when a country is a capital-abundant country, it has a comparative advantage in producing and exporting capital-intensive goods because the cost of capital (i.e., rent) becomes cheaper relative to that of the other countries in the world. However, at the same time, the cheaper price of capital will not only cause producers of capital-intensive goods to increase the intensity of use of capital in producing capital-intensive goods, but it will also induce the producers of labor-intensive goods to substitute the relatively cheaper capital for the relatively-more-expensive labor in the course of their production. This action will change the intensity ratios of these two goods in this capital-abundant country. When the intensity ratio of labor-intensive goods (i.e., $(K/L)_l$) is greater than

that of capital-intensive goods (i.e., $(K/L)_k$), at the new factor price level the original labor-intensive goods become capital-intensive goods and the original capital-intensive goods become labor-intensive goods. Therefore, after the factor intensity reversals, this capital-abundant country will produce and export labor-intensive goods and thus exhibits the Leontief Paradox.

The example is important because it exhibits a case of common cause. The difference in factor endowments has the *dual power to cause* a country to export two different commodities with different factor intensities. In our example, on the one hand, the abundance of a country's capital (D) will enable the country to have a comparative advantage (A) in producing capital-intensive goods and exporting them (C). On the other hand, the abundance of capital will also cause the relative prices of factors to differ and will thereby cause the producers in both industries to substitute the relatively cheaper producing factor for the relatively expensive one and the substituting action may trigger factor intensity reversals (R). After factor intensity reversals, this capital-abundant country will produce and export labor-intensive goods (L) and therefore exhibits Leontief Paradox. The causal path is shown in the following (Figure 1), where $t_1$, $t_2$, and $t_3$ represent the time sequence:
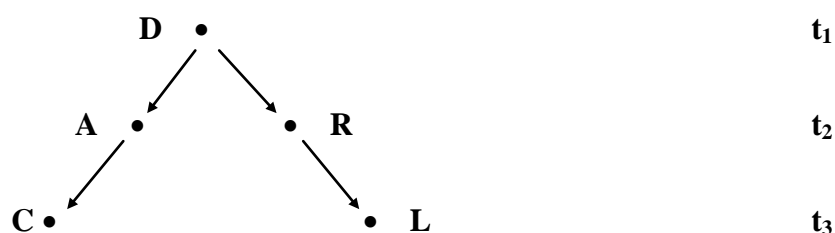


**Figure 1: Dual Causal Power of the Abundance in Capital (D)**

The main point of the case study for our purposes is that, to take the view from a meta-theoretical perspective, note that if we regard the original H-O theorem as a hypothesis that is used to describe a certain feature of international trade in a specified world, then we can say that this hypothesis—i.e., the H-O theorem—is true of the hypothetical world—i.e., is true of this specified world, and let us call this hypothetical world the H-O model. So, when Minhas offers his factor-intensity reversal explanation of the Leontief Paradox, Minhas is actually using a reformulated model to explain an anomalous phenomenon that happened in the actual world that the old model cannot explain or predict. That is, by dropping the assumption of the strong factor-intensity, Minhas creates a revised version of the H-O model, a version that is obtained by consulting the original theory that was proposed in the early twentieth century by two pioneers of modern international trade theory, Eli F.

Heckscher and Bertil Ohlin—let us call the theory the H-O theory—and can then be used to explain or predict the Leontief Paradox—i.e., can then be used to accommodate the originally anomalous actual phenomenon. From this perspective, we can say that when the result of Leontief's empirical study shows that the H-O theorem founders, this information can be fed back to economists as a clue to help them manipulate a rearrangement of the theoretical model that can then be used to explain or predict—i.e., to accommodate—the originally unexplainable or unpredictable phenomenon. In other words, in our case, it means that Minhas now has a new causal model that can be used to represent the new status of the similar phenomenon in the actual world.

### IV. Models as Autonomous Mediators between Theories and the World

I mention in Section II that, as is pointed out by Morrison and Morgan, models can be regarded as autonomous mediators between theories and the world; and I presume that this idea of models is generic enough to encompass both Sudgen's and Cartwright's ideas and so will be accepted by them. Thus it can be used to resolve the debate between them. Let us use our example to explain why it is so.

When we regard models as mediators between theories and the world, we mean that models are autonomous of both their relevant theories and the world. What does "autonomous" mean in this context? According to Morgan, it means that models are "halfway houses, formed to capture the correspondence between theory and data [i.e., the world]," and she adds that models are "needed to satisfy both sides." (Morgan 1988, p. 208) How can models satisfy both sides of theory and the world? Hsiang-Ke Chao, after reviewing Morgan's position, points out, "At one extreme, there are theoretical models which are not measurable. At the other extreme, there are measured data. Since theoretical models are not proper tools for empirical investigations, we need some kind of measurable or empirical models to do the task. Empirical models are not just those which are derived from data, but those which are measurable and estimable so that they match both theoretical and empirical properties." (Chao 2009, p. 5)

Based on Morrison and Morgan's generic idea about models, let us recap what Sudgen says. According to the fictionalist perspective, theoretical models are credible but counterfactual worlds that are parallel to the real world. Not all the characteristics of the fictional worlds of models correspond to those of the real world, but this defect does not prevent economists from regarding models as credible parallel worlds that function as important references consulted by economists when they are asked to provide policy advice. The reason that economists have such confidence in the information gleaned from fictional worlds is that they do not accept the information as

being reliable unless they are assured that there is a sufficient degree of similarity between the structures of the two worlds. For fictionalists, a model is a construction and not a stripped-down description of the real world.

Let us also recall how Cartwright characterizes the nature of models: Cartwright maintains that, according to isolationists, theoretical models are isolation tools that are used to separate disturbing factors that may interfere with the occurrence of the targeted economic phenomenon. According to this idea, theoretical models are embodiments that simulate the most salient features of the real world. By repeatedly using this tool, economists acquire knowledge about the capacity of the posited cause of the most salient features of the targeted phenomenon. According to isolationists, the phenomena or properties manifested in models represent certain important or interesting aspects of the real world.

Also recall that, in our case study, there is initially a hypothesis—the H-O theorem—which is derived from a highly restricted environment; in other words, economists initially have a highly idealized theorem that is derived from a model that is far from realistic in that it carries a class of assumptions that specify conditions very different from the conditions of the actual world. Let us call the H-O theorem an abstract theoretical claim. Admittedly, there is always a gap between an abstract theoretical claim and the real phenomenon it is intended to explain—such as the gap between the H-O theorem and the anomalous phenomenon of international trade (or, to be more precise, the anomalous phenomenon of the content of a country's international trade). As long as we are theorizing something, such a gap always exists. It simply reflects the limit of the scientific method that we can apply. The genuine concern here, however, is not the existence of this gap. Rather, we should focus our concern, within the limit of the scientific method, on whether and in what way this gap can be reduced. This genuine concern in fact reflects Cartwright's concern that I mention in the very beginning of this paper: The current concern of realism in science is not about how *accurately* the sciences can represent the world, but about the range of science—i.e., how *much* of the world the sciences can represent.

If we regard the concern of the gap between an abstract theoretical claim and the real phenomenon it aims to explain as a question of how an abstract theoretical claim bears on the relevant real phenomenon, an immediate question is, Why should we bother with it? The answer is that we want to know whether an abstract theoretical claim can be used to explain a real phenomenon. A further question is, Why should we doubt that an abstract theoretical claim can do the job of explanation? The answer is that we know that an abstract theoretical claim is at best derived from a theoretical model whose structure singles out the main, but not all, causal features of the structure of the real world. Therefore, we know that any explanation made from an abstract

theoretical claim will not precisely correlate with the real phenomenon. This imprecision—let us call it the *gap of abstractness*—raises our doubts.

In economics, the most often applied method to bridge the gap is the piecemeal method of assumption-manipulation, as we have seen in our study of Minhas's case; this method involves the changing of the ideal theoretical assumptions—i.e., to relax the assumption of strong factor-intensity of a commodity—in the original theoretical model. At first sight, it may seem that the objective of the assumption-manipulation approach is simply to design a theoretical structure that can be used to derive the abstract theoretical claim—in our case, the H-O theorem—more smoothly; and it is also hoped that, by using the same approach, as many theoretical models as possible can be established so that there are more and more hypothetical worlds from which the same abstract theoretical claim can be derived; in other words, the abstract theoretical claim can thus be regarded as being true of these many hypothetical worlds.

Recall the semantic explanation of scientific theorization I mention in the introduction of this paper, which states that the structure of a theory is constituted by a class of different models, and each model is used to derive a specific theoretical claim. To use the example in our case study, we can say that the H-O theorem, which is derived from the original H-O model, is a hypothesis that belongs to the H-O theory; and the theorem of the dual causal power of the abundance in capital, which is derived from the revised H-O model within which the assumption of strong factor-intensity is removed, is another hypothesis that is also a part of the H-O theory. Following a similar pattern, it is predictable that, by using the technique of assumption-manipulation, economists can produce as many theoretical models with different hypothetical structures as they wish so that they can derive as many theoretical claims as they wish; and these theoretical claims can be used to describe the features of these theoretical models from which they are derived. It may thus seem that we can conceive of a theory as a grand hypothetical world that contains a class of component hypothetical worlds—i.e., a class of theoretical models with different hypothetical structures—as its constituent parts. From this perspective, we may maintain that the practice of a theorist's constructing a theoretical model to derive a theoretical claim can thus be interpreted as using a theoretical model to represent a relevant part of the grand hypothetical world. This interpretation can thus help to shed new light on interpreting Sudgen's position: When Sudgen says that the worlds depicted by the models are parallel to the actual world and if these parallel worlds have any features, these features do not necessarily correspond to any relevant features of the actual world; what he means is that, according to our interpretation, these features derived from the models reflect simply the relevant features of the

grand hypothetical world that is depicted by the relevant theory.

This further interpretation of Sudgen's position illustrates the notion that the main concern of Sudgen's fictionalist account of models is to focus on explicating the relation between a theory and its component models; therefore, it is no wonder that Sudgen expresses the following view: "[Theorists] typically say very little about how their models relate to the real world. It seems to be seen as sufficient to describe the properties of the model world *in parallel with* those of the real world, …The most natural interpretation of this practice is that, in the relevant scientific community, this *counts as* explanation." (Sudgen 2009, p. 25, emphases original)

But, again, should the purpose of changing the ideal theoretical assumptions in the piecemeal method be interpreted simply as intending to design a theoretical structure that can be used to derive the abstract theoretical claim more smoothly and thus be regarded as simply a theoretical practice or game conducted by theorists to investigate the relation between a grand hypothetical world and its component hypothetical worlds? It may not seem so. Sudgen again: "Perhaps a theorist is entitled to present a model in the hope that it will prove useful, without being able to say how. Still, a model cannot prove useful unless someone uses it, and whoever that person is, he or she will have to bridge the gap between model world and real world… if we then try to imagine how that model could be used, we find we need someone to…advance the hypothesis that some part of the real world works like the model. And if the model is supposed to give us confidence in that hypothesis, we are entitled to ask how it does so. There is still a gap to be crossed, and that requires inductive inference." (Sudgen 2009, pp. 25-26) So even a fictionalist like Sudgen would still maintain that there is a gap to be crossed; in other words, he would still maintain that a model should somehow—surely, as mentioned in the quotation, Sudgen would suggest an inductive inferential approach—be proved to be useful in the sense that some part of the real world *works like*—or, *is represented by*—the model. Our question is, How can we rationally explain that Sudgen is still concerned about whether a model can adequately represent a certain part of the real world even though we know that Sudgen is a fictionalist who would care only about "the properties of the model world in parallel with those of the real world"?

**V. An Isolationist Account of How Abstract Theoretical Claims Can Be Relevant to the Real World**

To interpret Sudgen's aforementioned seemingly contradictory position, the best strategy is to reconsider why economists conduct assumption-manipulation in their theorizing. It is first supposed that the purpose of using assumption-manipulation in economic theorizing is simply to set up as many theoretical models as possible so that,

from these different theoretical models, as many theoretical claims can be derived and can in turn be used to describe the features of these models; but contrary to this supposition, we might interpret the use of assumption-manipulation as trying to add *more causal considerations* to the original theoretical model. These causal considerations can result in changes in the ideal theoretical assumptions—changes such as dropping restrictive assumptions, revising the content of the original assumptions, or even adding new assumptions. The final choice of the changes depends on the real situation of each case to be explained. I mention these changes to point out that they reflect economists' attempts to revise the *causal structures of their theoretical models* so that the revised models will be more pertinent to the *real causal structure* underlying the targeted real economic phenomenon.

Economists conduct such revisions because they want to obtain more-accurate causal models that they can use to derive more-accurate causal laws that can in turn be used to explain the targeted economic phenomenon. If economists can somehow show that the revised theoretical model can produce a more accurate causal law, this fact will indicate that their attempt to de-abstract—i.e., to remove assumptions from—the original theoretical model is successful; moreover, the causal law derived from this de-abstracted theoretical model will be less abstract than the causal law derived from the original theoretical model. Furthermore, because the less abstract causal law must have been shown in some empirical test to be a more accurate causal law that can be used to explain the targeted phenomenon, it can be regarded as more relevant to the targeted real economic phenomenon. By showing this relevance, economists can reduce the gap of abstractness. And this also explains why Sudgen maintains that "there is still a gap to be crossed" and why remains concerned about whether a model can adequately represent a certain part of the real world.

We may further illustrate that there is a connection between our causal interpretation of Sudgen's position and Cartwright's idea of the nature of economic models—i.e., the idea that models are isolating tools—by referring to our case study. Recall the H-O theorem: A country has a comparative advantage in producing and exporting those commodities that use more intensively the country's relatively more abundant factors. This theorem is a highly abstract theoretical claim, because it is produced from a highly abstract theoretical model within which a long list of assumptions is added. As mentioned in Section II, this long list of assumptions is used to set up a disturbance-free environment to guarantee that the cause—the difference in factor endowments—has the *capacity* to determine the content of exported commodities. That is, the purpose of setting up this highly abstract model is to try to discover the *essential behavior* of the difference in factor endowments in determining the content of exported commodities. But, at the same time, this long list of

assumptions makes this theoretical model very unrealistic in the sense that the causal structure for producing this essential behavior is very different from the one within which the exported commodities of a specific country are determined. But, as is pointed out by Cartwright in her remark on Schelling's checkerboard model of segregation, the knowledge of the capacity of a cause that is derived from a highly unrealistic model cannot completely reflect all the aspects of an actual society, but it nevertheless represents an important aspect of the targeted phenomenon in a complex society.

It is no wonder, then, that Leontief found that this capacity claim foundered when it was used to explain the content of U.S. exports in 1947. The discrepancy between what the H-O theorem asserted and what Leontief found is an example of what we have called the gap of abstractness. Does this gap lead international trade theorists to abandon the H-O theorem outright, or do they simply ignore the gap? Do international trade theorists simply tolerate the gap of abstractness and not try to improve the situation? It seems not. The Minhas study that we discuss in Section III represents a theorist's attempt to bridge the gap.

Notice that, following Cartwright's isolationist causal interpretation of the nature of economic models, Minhas's study should not be interpreted as trying to revise the H-O model by simply doing theoretical tricks such as changing some theoretical assumptions to enable him to derive the desired theoretical conclusion more smoothly. On the contrary, Minhas's study is also concerned with the problem of whether the causal structure of the theoretical model is consistent with the causal structure of the targeted real economic phenomenon—i.e., the problem of heterogeneous testing structures.

Following this isolationist interpretation, we may assume that Minhas has a different idea regarding the Leontief Paradox. According to Minhas, the fact that the United States did not export the commodities predicted in the H-O theorem must arise from a discrepancy between the theoretical causal structure and the real causal structure. What is this discrepancy? Minhas notices that, in the real world, the factor-intensity of a specific commodity does not always stay the same. Depending on the ease of substituting one production-factor for another factor in an industry in response to a change in the prices of these production-factors, the factor-intensity of a commodity will reverse in the industry that allows easier factor substitution. If this factor-intensity reversal occurs, it can be used to explain the Leontief Paradox. But how can Minhas show that this concern is not an arbitrary guess? How can he show that factor-intensity reversal is a general feature of the real causal structure? Recall what Sudgen says: "There is still a gap to be crossed, and that requires inductive inference." Minhas makes an inductive inference.

To show that factor-intensity reversal is a characteristic of the real causal structure, Minhas must show that, in most industries, factor-intensity reversal occurs. In an ingenious empirical test not mentioned earlier in this paper, Minhas indeed shows that factor-intensity reversal is a prevalent phenomenon. After Minhas showed that factor-intensity reversal is a general phenomenon in most industries in the United States, this phenomenon should be regarded as an additional causal consideration to be added to the original H-O model. Therefore, according to our causal interpretation, Minhas's finding should suggest that one more causal factor should be included in the original H-O model, and the addition of this causal factor will be reflected in the dropping of the restrictive assumption of the strong factor-intensity in the H-O model.

To mention Minhas's practice is to point out that trade theorists do not care only about the ease of the derivability of their theoretical models; they are more concerned about whether the causal structures specified in their theoretical models are consistent with the real causal structure of the targeted real economic phenomenon. If the theorists' causal structures are more complete than their previous versions, the causal laws derived from these more complete models will generally be more accurate than the ones derived from the old models. In any case, the derivation of more-accurate causal laws that can be used to explain real economic phenomena is the second concern in these theorists' work. Constructing more-complete causal structures is their first theoretical concern.

On the basis of our isolationist account of economic modeling, if we compare the H-O theorem and the result derived from Minhas's study, it is obvious that the H-O theorem is more abstract than Minhas's result with respect to the real phenomenon of the content of U.S. exported commodities in 1947. The reason is that Minhas's result is readily applicable to explain Leontief's Paradox, but the H-O theorem seems to be unhelpful or even provides a contradictory result. But note that this situation does not mean that the H-O theorem or the H-O model is useless or is empirically refuted. We must remember where Minhas's result came from: not from the original H-O model but rather derived from a revised H-O model with the assumption of strong factor-intensity dropped. In other words, his result is derived from a *more complete* H-O model with a new causal factor—the factor-intensity reversal—added to the original theoretical causal structure. Although the final result derived from the revised H-O model is contradictory to what is asserted in the H-O theorem, this fact does not prevent us from regarding the H-O theorem as a general guideline that has shaped the general direction of the research of international trade in the past 70 years. The later empirical research into the modification of the original H-O model can be regarded as trying to fill up the phenomenal content of the H-O theorem. It is the persistent fact of economic theorists' supplying the phenomenal

content to their abstract theoretical claims that gives us a reason to believe that the problem of the gap of abstractness is being reduced.

Given that we can narrow the gap of abstractness by making the theoretical model or theoretical claim more causally realistic or concrete via supplementing more phenomenal content with respect to the targeted real phenomenon, we can form a rough guideline for determining the order of abstractness (or concreteness) between any two theoretical models. This *guideline* is as follows: If a theoretical model can provide a *more complete causal structure* than another theoretical model, the more complete theoretical model can be regarded as *more concrete* (or less abstract) than the other model. The *corollary* of this guideline is this: If a theoretical claim is derived from a more complete causal model, it will generally be a *more accurate* causal claim that can be used to provide a fuller causal explanation of the targeted real phenomenon. As a result, this more accurate causal claim can be regarded as more concrete (or less abstract) than the causal law derived from a less complete causal model. If we use this guideline, it is obvious that the revised H-O model—i.e., the model formulated by adding a new causal factor found in Minhas's empirical study—is more concrete than the original H-O model. So the result derived from the revised H-O model is more concrete than the H-O theorem.

By proposing the guideline for determining the order of abstractness (or concreteness) between any two theoretical models and the corollary of the positive relation between the accuracy of a causal claim and the completeness of a causal model, I complete the following three tasks: (1) the task of applying the isolationist idea of economic modeling to explain Sudgen's seemingly contradictory position; (2) the task of analyzing an isolationist account of how abstract theoretical claims can be relevant to the real world; and (3) the task of illustrating that the main concern of *Cartwright's isolationist account of economic models is to focus on explicating the relation between models and the world—i.e., explicating how models access the actual world*.


## VI. Conclusion: Representation as a Process of Model-Building

Economic theorizing is an activity composed of two opposite processes: abstraction and concretization. When economists are interested in a specific class of repeated economic phenomena, they, like most theorists in other disciplines, start thinking about how to construct an account that will explain why this class of phenomena occurs repeatedly. They know that this class of repeated phenomena is probably not a result derived from the operation of any specific cause in an economic system; rather, they think that this class of phenomena is the result derived from the operations of countless causal factors in the system. But, at the same time, they also know that to

recognize the full list of these causal factors is not possible.

To formulate an explanatory account that is manageable within their recognition limits, then, they *assume* that although countless causal factors are responsible for the occurrence of a class of phenomena, there often is a class of causal factors that constitutes a causal structure that can also produce the same class of repeated economic phenomena within some reasonable approximation. With this assumption, economic theorizing begins and a process of abstraction is triggered. When economists conduct their theorizing in this way, they are constructing what Sudgen calls counterfactual but credible worlds in their theoretical models. Note that these constructed worlds of the theoretical models are called counterfactual because they are set up under the aforementioned assumption; and they are regarded as credible because the theoretical conclusions derived from the causal structures of these counterfactual worlds, with respect to the actual targeted phenomena, are always within some reasonable approximation.

This process of abstraction starts when economists begin to set up a theoretical model by abstracting from the real economic situation those causal factors that they think are most important. They then use these factors to lay out the causal structure that they think can represent the main causal features of the real causal system that produce the targeted real economic phenomena. Economists then use various ideal conditions, such as ceteris paribus clauses, to act as shielding devices—or as what Cartwright called isolating tools—to prevent the results derived from their models from suffering the disturbing influence of other, less important causal factors. The purpose of using these isolating tools is to ensure that the derived result is purely the exhibition of the essential behavior—i.e., the capacity—of this class of selected causal factors and nothing else.

The conclusion derived from economists' theoretical models must be very imprecise because, as we have mentioned, a theoretical model does not include all the causal factors. But this conclusion reflects economists' first attempt to provide an explanation that captures or represents the main causal features of the targeted economic phenomena. This initial account is highly abstract and cannot explain every detail of the phenomenon. But as long as it can provide a general description of the most important causal features, it can be regarded as a general guideline and so be accepted as an economic explanation.

Note that obtaining this general guideline is not the end of economic theorizing. A question often encountered in economic theorizing is this: Can such a general guideline be used to explain or predict other classes of economic phenomena? This question is critical, especially when economists are asked to provide policy suggestions based on their theoretical models. Is it reasonable for economists to

suggest, simply by using policy parameters suggested in a theoretical model based on the data of the past ten years, that a government reduce its money supply to a certain amount in order to fix the price level at a certain level in the next year? If the abundant factor of production in a country is capital, should the government of this country encourage its domestic manufacturers to focus on producing capital-intensive commodities in order to improve the welfare of these domestic makers simply because the government's policy would exploit what Heckscher and Ohlin suggested in their famous theorem?

Again, the concept of causal structure should figure in economic theorizing. The point is not whether or not these basic theoretical claims are correct; it is whether they are used in the same causal structure. We should not expect that a theoretical claim would be applicable within different causal structures. We can expect only that a theoretical claim can provide a general direction for our research. It is widely accepted that there is indeed a causal relation between money and price. But the point is in what way they are connected. The intuitive idea is that an increase in the supply of money will increase price levels. But it may, in the real world, turn out that an increase in the money supply, contrary to what is predicted in monetary theory, does not affect price levels at all. Should we simply refute the theoretical claim? No. This seemingly refuted theoretical claim should serve as a starting point to begin our search for a new causal structure. As is suggested in Minhas's case, when the real phenomenon contradicts what is predicted in the H-O theorem, it is time for the process of concretization to begin.

Just as Minhas's study produced suggestions about the original H-O model, a new study of the relation between money and prices should also suggest which new causal factors—i.e., which new phenomenal contents—should be added to the original monetary model and what kind of new causal structure should be laid out in order to capture the real causal system of the new economic situation. If, at the end of a process of economic modeling, it can be shown that the new causal structure specified in the new theoretical model is indeed consistent with the real new causal system, then what is derived from this new theoretical model must be able to explain, predict, and represent the targeted real phenomenon. By completing this entire procedure, the original abstract monetary model is said to be concretized.

In a review of his own thirty-year exploration of the issue of realism in economics, Uskali Mäki, a Finnish philosopher of economics, recollects on what he was brought into the study of the philosophy of economics. He was perplexed about why economists always use unrealistic models to discuss real economic affairs. He concluded that, for philosophers of economic methodology, the harshest challenge is to defend realism in economic science against the fact it is a prevalent practice among

economists to use unrealistic models to explain actual economic phenomena. (Mäki 2009) After reviewing Sudgen's and Cartwright's accounts of the nature of economic models, Tarja Knuuttila, another new-generation Finnish philosopher of science, points out that we may summarize the ideas held by both camps as follows: For fictionalists, theoretical models are credible constructions; for isolationists, they are isolating representations. (Knuuttila 2009)

Based on our case study, which examines how economists manipulate their assumptions to construct a new model with a new causal structure at each step of their theorizing, we may conclude by maintaining that, in economics, representation should be conceived as a dynamic idea. Economic theorizing (or economic explanation) is a process of repeatedly using "realistic representation of the isolated unrealistic world" (this part of the idea is motivated by Cartwright's idea of models as isolating tools) at each step of economic theorizing to build up a class of "unrealistic constructed credible worlds" (this part of the idea is, however, derived from Sudgen's idea of models as counterfactual but credible worlds).

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