ECONOMIC THEORIZING: A CAUSAL STRUCTURALIST ACCOUNT WITH EXAMPLES FROM INTERNATIONAL TRADE THEORY

by

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A THESIS

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Abstract

This thesis applies the concept of causal structure to the discussion of economic explanation. It uses this discussion and examples from international trade theory to provide an account of economic theorizing. Three subthemes are pursued:

(1) The concept of economic laws manifested in economists' practice of theoretical model-building is not compatible with the regularist view that economic laws are unifying regularities from which all other regularities are derived. Rather, it sees these laws as derivatives of the causal structures specified in theoretical models. In these models, economists hypothesize a complete causal structure that is thought to identify the main causal features of the real economic system within which the economic regularities occur. Based on this idea, I propose a new explanatory account—the causal structuralist account—of economic theorizing.

(2) What is the nature of an explanatory relation between the explanans and explanandum? For some philosophers, an explanatory relation is a causal relation that is fundamentally about the invariance of a relation between variables under some interventions. This account, however, raises another question: Can causality be identified with invariance? For the causal structuralists, in contrast, a causal relation (and so an explanatory relation) cannot be reduced to any other relation; it can be explained only under a hypothesized complete causal structure that is supposed to represent the real causal structure underlying the concrete phenomena of interest.

(3) A causal structure specified in an economic theoretical model is meant to single out the main causal features of an economic phenomenon and omit the less relevant features; inevitably, the causal law derived from this structure possesses some abstractness. Some methodologists regard this abstractness as the main source of the inaccuracy of predictions made from abstract causal laws. What, then, is the value of abstract laws in explaining or predicting real economic phenomena? A complete causal structuralist explanatory framework should provide a plausible account that bridges the gap between what is abstract and what is real in economic theorizing.

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Chapter 1

Adjusting the Focus of Methodological Discussion in Economics

1.1 Introduction

Many modern methodologists of economics argue either that economics is in crisis or that it is not a scientific discipline. In the former case, the most often cited reason is that economic theories do not produce refutable implications, and, for the latter case, that economic theories do not make better predictions over time. Although these two criticisms seem to differ, they have one common feature: They both judge the success of economics by referring to the results of testing conducted to examine the implications derived from economic theories. To single out this point is not to question the importance of theory-testing in the practice of economic theorizing. Instead, the concerns raised here are twofold: (1) is theory-testing indeed what theorizing is all about? and (2) how should theory-testing be interpreted and practiced?

The tendency of these methodologists to emphasize the decisive status of empirical test results reflects their common underlying subscription to a *regularity* view of natural laws. For these methodologists, natural laws are universal regularities. In their view, statements of universal regularities should express the associations among all relevant phenomena. If a law claim cannot fulfill this requirement, it should be discarded. This regularity view of natural laws is closely related to the Deductive-Nomological (D-N) model of scientific explanation. The D-N model requires that the explanandum of a scientific explanation be an instance to be expected from the lawful regularity stated in the explanans. If the explanandum is an instance that cannot be derived from the law statement, the explanation in question is not qualified as a scientific explanation. Because the same type of argument can be applied to an economic theory that makes predictions, regularist economic methodology emphasizes the importance of empirical test results.

The main purpose of this chapter is not to devalue the importance of empirical testing. Rather, its main purpose is to point out that empirical testing is not the endpoint of the overall procedure of economic theorizing; instead, testing is a part of the process of model-choice. Economic theorizing begins with economists' considerations of the main features of a certain class of phenomena. These considerations are then formulated into theoretical models. Economic laws are derived from these models and are then used to explain the class of phenomena in question. The important point here is that economic laws are derivatives, and not fundamentals; therefore, they are not universal regularities but rather are regularities within the scope defined by the model. Empirical testing is called for only when the derived economic laws are to be used to explain a concrete phenomenon in the world. The concept of empirical testing used here, then, has a very different meaning from that used by the regularist methodologists.

For a regularist, when a prediction fails, it means that the universal regularity contained in the theory is not a true universal regularity. As a result, the regularist will continue to search for a different regularity that can cover the association that was missed by the false regularity. For regularists, empirical testing is used to test whether an economic theory contains a true universal regularity. According to our account, however, economists conduct empirical tests to determine whether their derived economic laws can be exported from the theoretical models to explain the concrete phenomenon. If the model fails this test, these economists conclude that the model fails to capture some structural features and thereby fails to produce the right kind of economic laws to explain the phenomenon in question. To obtain the correct economic laws, their task is to reconsider how to re-specify the environment of the model so as to generate the right kind of economic laws. Contrary to the regularists, who are *associationists*, these latter economic methodologists, because of their serious concern about the relationship between the economic law and the structure of the theoretical model, are what I shall call *structuralists* or, to be more specific, *causal structuralists*.

The main thesis of this chapter is that methodological discussion in economics should extend its focus to include not only theory-testing, as at present, but also theory-building, an important topic that is worthy of detailed discussion. In addition, the communication between the theory-building part of theorizing and the theory-testing part of theorizing should also be paid greater attention. Finally, this chapter argues that theory-testing should be interpreted in the context of the structuralist rather than the associationist view.

1.2 A Misconception in Economic Theorizing

When Mark Blaug (1992, pp. 59-62 and pp. 237-43) maintains that modern economics is facing a "crisis," what does he mean? The crisis is that modern economics is reluctant to produce theories that yield refutable implications that can be compared with the facts. According to Blaug, one of the main reasons for this crisis is the prevalent application of ceteris paribus clauses in economic theorizing. Blaug argues that economists attach ceteris paribus conditions to their statements of economic laws or general economic propositions as a methodological trick to immunize their theories from empirical testing. If a theory produces faulty predictions, the blame can be easily assigned to extraneous disturbing factors that are supposed to be held constant by the ceteris paribus conditions. Blaug maintains that unless these disturbing factors can be explicitly spelled out, the vague statements of economic laws will make it hard to test, and thereby uphold or falsify, economic theories that contain such laws.

Another attack on the lack of empirical content in economics follows a similar line of reasoning. Alexander Rosenberg (1992, pp. 112-6) argues that economics is not an empirical science because it cannot improve the predictive power of its theories over time. In Rosenberg's explanation, ceteris paribus clauses also play the main role in limiting the predictive power of economic theories. For Rosenberg, ceteris paribus clauses are ineliminable as well as useful because they hold constant certain unquantifiable factors, such as human beliefs and desires. Ironically, however, these ineliminable and useful clauses become ineliminable burdens, preventing these theorists from improving the accuracy of their theories in the face of faulty predictions or explanations. What happens is this. Economists try to repair their theories by adding relevant variables from the list of disturbing factors, which are thought to be held constant in ceteris paribus conditions, to the antecedents of the statements of economic laws contained in their theories. These disturbing factors can be found, but the problem is that they are unquantifiable; as a result, they cannot be converted into the needed variables.

Rosenberg's criticism seems to be stronger than Blaug's. Blaug contends that economic theories might lay the foundation of a falsifiable discipline if the content of ceteris paribus conditions for the statements of economic laws can be fully identified. In Rosenberg's argument, economics cannot become an empirical science because the conversion of disturbing factors—which are held constant in ceteris paribus conditions—into identifiable variables is simply impossible in economic theorizing. The discussion of the differences between these two views could be continued, but our main concern here is that these comments on economics reflect a common view of natural laws—namely, that natural laws are universal regularities.

For Blaug, economic theories can become more falsifiable if we unpack the mysterious box of ceteris paribus clauses and then identify the disturbing factors inside. Blaug's argument seems to contain an implicit presupposition: There is always a possibility that some economic law can be found that exhibits itself in the phenomenon of interest and includes a wider collection of associations among relevant factors. Suppose this law implies a narrower-scope law whose collection of associations of relevant factors is a subset of the collection from the wider-scope law. Then this wider-scope economic law can be used to show that the narrower-scope economic law is incomplete and fails to account for all the relevant associations among relevant factors. For Rosenberg, economic theories can be improved to become more comprehensive, thereby making better predictions or explanations. To do this, theorists must extract more relevant factors from ceteris paribus conditions and add them to the antecedents of the economic laws that underlie the theories. If the problem of converting disturbing factors into quantifiable variables can be resolved, according to Rosenberg it will be possible someday to construct a perfect economic theory containing the most comprehensive economic law, which will cover all economic phenomena and associations among them. Furthermore, this economic law will enable the economic theory generated from it to produce perfect predictions and explanations.

Both Blaug's wider-scope economic law and Rosenberg's most comprehensive economic law fall into the category of universal regularities known as the covering-all-narrower-scope-regularities type. These regularities are presumed to exist, to operate on their own to encompass the narrower-scope regularities, and to await discovery. This *regularist* world view, apparently held by both Rosenberg and Blaug, depicts the world as a structure possessing a hierarchy starting from the narrowest local regularity to the broadest universal regularity, with various middle-level regularities in between. The most characteristic idea of this world view is that the narrower regularities are normally regarded as instances derivable from the broader regularities. According to this world view, it is no wonder that a regularist methodologist would think that economists should, in economic theorizing, try to extend the scope of the regularity law as wide as possible by adding to the regularity law's antecedent conditions increasing numbers of relevant factors that are released from the ceteris paribus condition and in this way to explain the phenomenon in question as an instance of the extended regularity law. As a result, a way regarded by the regularist as the only empirical way is motivated that establishes whether an economic theory, with respect to the phenomenon in question, is an adequate explanation: The success of an economic theory is determined by whether the theory can explain the phenomenon in question as an instance that is covered by the regularity law contained in the theory.

To test the coverage of the regularity law, the practice suggested most often by regularists is to test the implication that is derived, based on this regularity law, from the economic theory. The implication is compared to the facts to check whether it is consistent with them. Apparently, this methodology puts the entire burden of the judgment of an economic theory on the success of the derived implications.

Admittedly, there is nothing wrong with the regularists' concern about extending the antecedent condition of a regularity law by adding to it increasing numbers of relevant factors; nor is there anything wrong with their concern about whether the phenomenon in question can be explained as an instance of the regularity law. Neither should there be a complaint about their emphasizing the importance of empirical testing of implications derived from economic theories. Rather, the concern here is that, by focusing exclusively on the pattern of the associations expressed in the regularity law and on the validity of the implications derived from it, the regularist methodology ignores the causal structure.

The idea is this: Normally, what can be observed by economists from the data that describe the targeted phenomenon or from the phenomenon itself are *the net results* of the operations or interactions of various causal factors under a certain causal structure. If, as the regularists suggest, the economists' job is to establish their theories by simply discovering the pattern of association between these causal factors, it is very likely (as is also pointed out by the regularists) that economists will repeatedly face the challenge that the regularity laws contained in their theories are often unable to explain or predict the targeted facts in the real world that these laws supposedly explain or predict. The main reason for this repeated failure is that there are always ignored disturbing factors that may intervene in the existing causal structure and destroy the supposedly stable association stated in the regularity laws. Thus, the regularists regard the theories' repeated failure in explanation and prediction as a crisis in economics because so far no one economic theory can be shown to possess a regularity law that is genuinely broad enough to cover whatever phenomenon is in question.

This pessimistic conclusion is, however, based on an unfounded presumption: that a good economic theory contains a true (or approximately-enough true), exceptionless regularity law that describes a general pattern of association that can be used to explain or predict any phenomenon in question—and that's it, no more and no less. The question of how and from what source this regularity claim is derived does not bother the regularists because they take for granted the existence of the lawlike association between the relevant causal factors and the net results they produce. By taking this lawlike association as it is without asking where it comes from, this regularist presumption implicitly disregards the causal structure. The danger of this presumption is easily illustrated by the following classic example: the relationship between a barometer reading and the approach of stormy weather.

By ignoring the causal structure and focusing exclusively on the pattern of association that a falling barometer reading is always followed by a storm, the regularists are likely to conclude wrongly by saying that, ceteris paribus, a fall in the barometer reading always gives rise to a storm. Furthermore, if this strategy of making the causal statement doesn't change, even if the regularists later find out that an additional factor—a fall in the atmospheric pressure—is always present whenever the barometer reading is falling, the regularists will likely make another mistaken conclusion: that, ceteris paribus, a fall in the barometer reading *and* a fall in the atmospheric pressure together always contribute to the occurrence of an incoming storm.

1.3 An Outline of the Causal Structuralist View of Theorizing

If, however, *causal structuralist* thinking is involved in making the causal statement, the situation can be improved. Despite the appearance of a strong correlation between, on the one hand, a fall in the barometer reading and a fall in the atmospheric pressure, and, on the other hand, an incoming storm, the causal structuralists do not base their causal conclusions simply on the known association. Instead, causal structuralists usually hypothesize a complete causal structure that is supposed to capture the main features of the real causal structure and in turn is assumed to be responsible for producing the strong correlation among the factors.

One question may arise: Why do we have to start with a hypothesized causal structure? Can't we simply set up a causal model that mimics every detail of the real causal structure in question? The answer is, Yes, if we can. It's common knowledge that it requires a great number—indeed, numerous—factors to enable the occurrence of a phenomenon. So if our aim is to make a complete explanation for a targeted phenomenon, it seems that we must explain it by using all the relevant factors. The regularists know that it is impossible to cover all relevant causal factors and thereby make an explanation complete, so they set aside the issue and instead argue that a good explanation should contain a regularity law that is broad enough to cover any targeted phenomenon as one of its instances. The causal structuralists, on the other hand, also know that it is impossible to exhaust all relevant causal factors, but, for them, discovering such a broad-scope regularity law is as difficult as discovering a full set of causal factors, so they depend on the idea of the hypothesized complete causal structure.

The idea is this: The causal structuralists do not know exactly how many causal factors are involved in producing a class of targeted phenomena, but they do know that *some prominent causal factors* may be assumed to be responsible for some of the main features of the targeted phenomenon. Based on this assumption, they further assume that there exists *a hypothesized complete causal structure* within which these selected prominent causal factors are arranged in a certain way and by which they interact to produce the targeted phenomenon. (Note: This complete causal structure is described as "hypothesized" in order to contrast it with *a "real" complete causal structure*, which is constituted of *all* relevant causal factors in the real world that are responsible for the occurrence of the targeted phenomenon.) Now the problem is as follows: What is the exact way in which these selected causal factors are connected? Or to put it another way, how do these selected causal factors interact? Initially, the causal structuralists use all possible information sources to generate a general idea of the hypothesized complete causal structure. This general idea of the hypothesized complete causal structure is what I shall call *a theory*. To ascertain whether their general causal theory is correct, the causal structuralists, by using selected causal factors, set up *a causal model* to depict a causal network expressing the interrelationship among the causal factors; their hope is that the causal network depicted in this causal model coincides with the hypothesized complete causal structure and produces the targeted phenomenon. To distinguish this causal network from a hypothesized complete causal structure, I call the causal network a *hypothesized causal structure*.

The hypothesized complete causal structure and the hypothesized causal structure share a common characteristic: Each is the kind of causal structure within which the causal mechanisms operate under the ceteris paribus condition. In a real complete causal structure—i.e., the causal structure including all relevant causal factors—the level of causal power of each factor is hard to ascertain because the interaction of numerous causal factors makes it impossible to measure the causal power. In both the hypothesized complete causal structure and the hypothesized causal structure, however, the contribution of each selected factor to the occurrence of the phenomenon can be identified directly from the proposed causal structure because it is *assumed* that only those selected causal factors are responsible for the occurrence of the targeted phenomenon. Such an assumption amounts to a *presumption* that the disturbances contributed by other, unselected factors are negligible. In other words, the selected causal factors are supposed to interact within a disturbance-free—or, as Nancy Cartwright would put it, a "properly shielded"—environment. It is in this sense that the causal mechanisms within a hypothesized complete causal structure and within a hypothesized causal structure are said to operate under the ceteris paribus condition.

Within a disturbance-free hypothesized causal structure—i.e., a properly shielded causal model—each selected causal factor is said to have a *distinctive* causal power in that it interacts with other selected causal factors with its *full* causal power according to the path stipulated in the causal model. This distinctive causal power is labeled by Nancy Cartwright a *causal capacity* and is further explicated in Chapter 2. What is relevant here is that the operation among these stable causal powers may, in each different causal arrangement, end up with a specific pattern of association, and this association can be compared against the targeted phenomenon to see whether the specific causal arrangement specified in the causal model has captured the main features of the real causal structure from which the targeted phenomenon is derived.

At this stage, it may seem that what the structuralists have obtained from their model—i.e., a pattern of association—sounds just like the regularity law that is required by the regularists, and the causal structuralist comparison between the result of the operation and the targeted phenomenon is just like the empirical testing that the regularists emphasize. As a result, it may seem that there is no difference between these two approaches. This conclusion is, however, premature, because we still haven't gone through the entire procedure of the causal structuralist strategy for establishing an adequate causal explanation.

Let's tentatively set aside this query and complete the discussion of the causal structuralist approach. Yes, as with the regularists, if the result of the causal operation derived from the causal model is consistent with the targeted phenomenon, the causal structuralists conclude that the causal story described in the causal model is a correct description of the targeted phenomenon. If, however, the causal story explicated in the causal model fails to cover the main features of the phenomenon in question, the causal structuralists, unlike the regularists, do not abandon this causal model. Their motive stems from the conviction that the selected causal factors should exert their distinctive causal power in the existing causal model to produce the result that can be used to explain the targeted phenomenon. If it turns out that they fail to do that, the blame may fall not on the notion of the stable causal power but rather on the causal structure specified in the causal model. In other words, what should be revised is not the assumed stable causal capacities but the way these causal capacities are arranged.

Based on this belief, the causal structuralists may follow any of three paths to revise the existing causal model: (1) introducing new causal factors into the existing structure, (2) excluding some existing causal factors from the existing structure, and (3) rearranging the causal paths of the selected causal factors in the existing structure. All of these remedial actions point to an attempt to recapture the *structural features* of the hypothesized complete causal structure within which the targeted phenomenon is supposed to occur.

By, on the one hand, following these three remedial directions, and by, on the other hand, continuously referring to their original general causal theory and other reliable information sources, the causal structuralists keep searching for another causal structure by revising the original causal model until they generate one revised causal structure that can be used to produce the targeted phenomenon. An important characteristic in this entire process is that, in each trial of the search for the adequate causal structure, the causal structuralists' main concern is whether the causal structure depicted in their causal model is a correct picture of the underlying hypothesized complete causal structure within which the targeted phenomenon occurs. Their concern is not, as would be that of the regularists, whether the regular pattern of association can be used to explain the targeted phenomenon as an instance. It is in this sense that the causal structure is said to be *fundamental* and the regularity law to be *derivative*. Throughout this thesis, I use the term *model-manipulation process I (MMP-I)* to refer to the process of revising the existing causal model to search for a correct causal structure that will explain the targeted phenomenon. The practices conducted in MMP-I constitute what I shall call the *theory-building stage*.

Have economists finished their work when a correct causal model has been generated via MMP-I? No. Often, a new challenge arises when an attempt is made to use the causal model to explain a different concrete phenomenon. I say "often" because it is imaginable that, in most cases, the new concrete phenomenon is to be located in a new real causal structure that is somehow different from the causal structure depicted in the causal model generated from MMP-I. If this indeed happens, it triggers another round of searching for a correct causal model.

Admittedly, the model specification practiced at this stage seems to resemble the model specification that economists have practiced in the theory-building stage. But one important fact distinguishes them from each other: In the theory-building stage, economists are concerned about establishing a plausible *theoretical model* that can be used to explain *the main features of the phenomena that they have considered so far*. In the current stage, which I shall call the *theory-testing stage*, economists are concerned about whether the theoretical causal model established in the theory-building stage can be used to explain *the current phenomenon of interest*.

To complete the job at the theory-testing stage, the normal suggestion is to check whether the result derived from the theoretical causal model, which is established in the theory-building stage, is consistent with the current phenomenon of interest. In actual practice, however, economists make a prediction based on the existing theoretical model to see whether it is consistent with the current phenomenon. In this case, the result derived from the existing theoretical model is called a *prediction* because, from the causal structuralist viewpoint, this implication of the theoretical model is thought to *be exported* outside the original causal structure to a new causal structure to be used to explain what happens there. It is in this sense that a theory—

i.e., a general theory regarding a hypothesized complete causal structure—is said to *be tested.*

For the causal structuralists, the term "testing" is to be used under this special meaning to contrast it with the idea of *model specification testing (or causal model testing)*, which is practiced by economists in the theory-building stage to discover a correct causal model to explain the phenomena of interest. The regularists, however, always conflate one idea with another. The danger of conflating these two notions of testing is fully spelled out in section 2.5. Suffice it to say here that the causal structuralists' vigilant awareness of the difference between model specification testing and prediction testing—an awareness motivated by their serious concern about causal structure—is the most important characteristic that distinguishes their methodological idea from the regularists'.

Normally, if model specification testing shows that the existing theoretical causal model fails to explain the main features of the phenomena that economists have been considering so far, the causal structure specified in the existing model must be re-specified and re-tested until a new model can be generated that succeeds in this aim. A similar thing happens when prediction testing fails under similar circumstances. This re-specification of the existing model launches another round of model-searching, although it is triggered by the prediction test rather than the model specification test. I shall call this second round of model-searching *model-manipulation process II (MMP-II)* to differentiate it from MMP-I in the theory-building stage.

I must point out a final important feature of the causal structuralist approach. So far, we have been discussing the ideas of theory-building, theory-testing, and model-manipulation, and it seems that these ideas are interrelated, but what is the exact relation between a theory and a model? This issue is spelled out in Chapter 3, where I discuss Werner Diederich's idea about models and reality (Diederich 1998, pp.

206-214 in D. Anapolitanos, A. Baltas, and S. Tsinorema (eds.), 1998) and in Chapter 4, where I talk about the de-abstraction (or concretization) of a theory. Suffice it to say here that a *theory* is a general idea of a hypothesized complete causal structure within which a set of stable causal relations is assumed; a *model* is a hypothesized causal structure to be used to illustrate how these stable causal relations are generated from a specific arrangement of the selected causal factors.

The practice of theorizing, which starts with the theory-building stage, produces a theory whose content inevitably consists of two parts: the theoretical part and the empirical part. Correspondingly, the model of this theory is also divided into the same two parts. The aim of MMP-II is to try to reduce the theoretical part of a theory and to increase the empirical part by supplying additional empirical information—called "phenomenal content" in Chapter 4—to the existing causal model to make it more realistic (or complete) to the extent that the revised causal model can be used to explain the concrete phenomenon of interest. It is in this sense that a model is said to function as *a mediator* to bring a theory into contact with the real world.

Figure 1.1 presents a general framework of the causal structuralist view of theorizing. This framework shows the interrelationship among the theory-building stage, the model-manipulation process stage, and the theory-testing stage.

[Please refer to Figure 1.1 on page 15-1]

Now let's return to our example of the relationship among a fall in barometer reading (A), the approach of a storm (B), and a fall in atmospheric pressure (C). If we are causal structuralists, despite the high correlation between A and C, on the one hand, and B, on the other hand, it is unlikely that we will come to the regularist

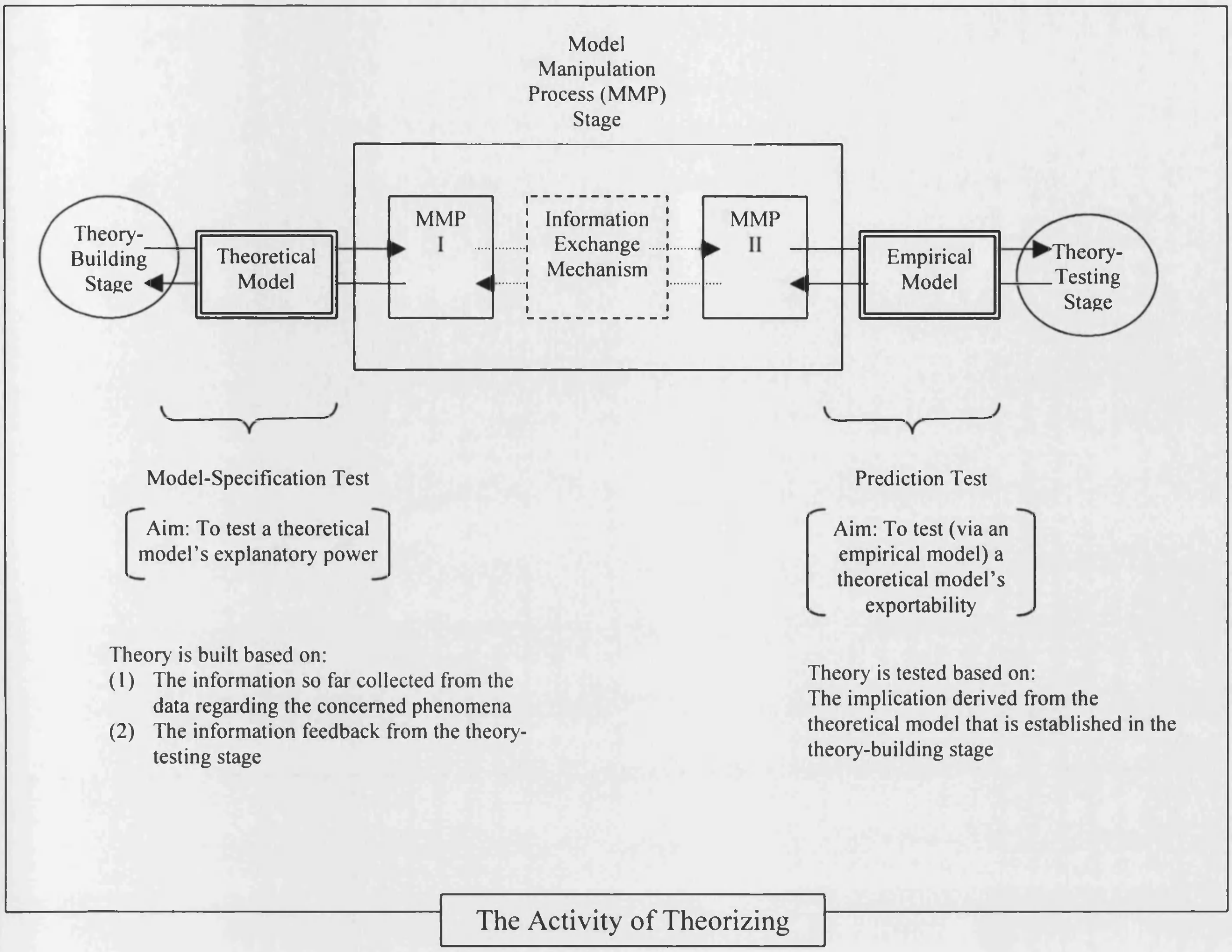


Figure 1.1: A Causal Structuralist Explanatory Scheme of Theorizing

conclusion that, ceteris paribus, A and C together always contribute to the occurrence of B. Before we draw a causal conclusion, as causal structuralists we will first avail ourselves of information in addition to the association among the three factors—namely, we will search for the causal structure that associates them. We have no guarantee that our hypothesized initial causal structure will correctly represent the real causal structure, but in the process of generating increasingly accurate and realistic causal models, we have a greater chance of coming to the correct causal conclusion based on a causal model in which the fall in atmospheric pressure is the common cause of both the fall in barometer reading and the approach of a storm. One of the main purposes of this thesis is to explain why this causal structuralist methodology has a better chance to obtain the correct causal conclusion.

People may then ask which approach—the regularist or the causal structuralist approach—has been adopted in the actual practice of economic theorizing. Contrary to the thinking of many modern methodologists of economics, it seems that economic theorizing follows the causal structuralist approach. The main reason is that the actual practice of economic theorizing has a distinctive characterization: It puts equal weight on both theory-building and theory-testing as is suggested by the causal structuralists. The following section presents an example to illustrate this characterization of economic theorizing.

1.4 Is an Economic Explanation a Deductive-Nomological Explanation?

To exemplify what I mean about the actual practice of economic theorizing, let's briefly review an example of economic theorizing from neoclassical international economics. This example is reexamined fully in section 2.6.

The gist of this economic theorizing is as follows: When neoclassical economists analyze the problem of the occurrence of international trade, their first step is to posit the cause, but not the economic law, of international trade. Then they construct a theoretical model and supplement it with assumptions concerning *boundary conditions* for specifying the environment reflected in the model. Next, by using the posited cause and the specified theoretical model, they derive a theorem for the occurrence of international trade.. Based on this theorem, they construct their theory of international trade.

Now let's look at the content of this economic theorizing. The neoclassical economists think that we can trace the occurrence of international trade to the differing endowments of production-factors among various countries. These economists first isolate a number of phenomena from the theoretical environment: production-factor movement between countries, differing production technologies between countries, differing preferences between countries, factor-intensity reversal, an imperfect market structure, increasing returns to scale, and so on. Under the assumption of the absence of these phenomena from the theoretical environment, the differences between the countries in their production-factor endowments constitute a condition of comparative advantage for each country, which then produces the goods that use more intensively this country's more abundant factors. Furthermore, under this condition of comparative advantage, countries seek to improve their own welfare and therefore exchange with one another the goods they have produced; as a result, international trade occurs. Therefore, according to this neoclassical theory of international trade, a conclusion can be derived from the theoretical model. This theorem of international trade is as follows: A country has a comparative advantage in producing and exporting the goods that use more intensively the country's more abundant factor.

Suppose that we are neoclassical economists and that we have been asked a question concerning the economic behavior of the United States in international trade: Comparing the United States with most of the other countries in the world, we know that the United States is relatively more abundant in capital than in labor. Does this mean that the United States exports a greater proportion of capital-intensive goods? As neoclassical economists, we would answer as follows: Given that the United States is a capital-abundant country and the proposition asserted in our theorem, the United States must export more capital-intensive goods in order to increase its level of welfare.

This neoclassical explanation, or prediction if you like, for the occurrence of international trade and for a specific country's international trade behavior was not questioned until Wassily Leontief used an input-output table analysis to conduct an empirical investigation of the content of U. S. exports. Leontief found that, contrary to what had been generally thought, the United States was, in fact, exporting more labor-intensive goods.

This observation, called Leontief's paradox, quickly prompted a great many studies conducted by neoclassical economists. This research was aimed at understanding the reason for Leontief's anomalous conclusion, but instead of abandoning their theories, these economists focused their efforts on attempts to reconcile this paradoxical conclusion with their theories. One example of such an attempt is the account of factor-intensity reversal. This account will be fully discussed in section 2.6. The general idea is this: Factor-intensity is an idea used to express the proportion of various production-factors that are used to produce a certain commodity. If we assume that there are only two production-factors (capital and labor) and two commodities (steel and cloth) in the market, the factor-intensity for each commodity can be defined as $(\text{labor/capital})_{\text{steel}}$ and $(\text{labor/capital})_{\text{cloth}}$. If $(\text{labor/capital})_{\text{steel}} < (\text{labor/capital})_{\text{cloth}}$, we say that steel is capital-intensive relative to cloth. In traditional neoclassical trade theory, the factor-intensity for a certain good is assumed to hold across all wage-rent ratios. This is the so-called strong factor-intensity assumption. This assumption aims to exclude a possible situation that when, say, the price of labor declines, the producers in the steel industry will substantially substitute the relatively cheaper labor for the relatively expensive capital to the extent that will convert the steel's factor-intensity from capital-intensive to labor-intensive. The proponents of the factor-intensity reversal account found that the phenomenon of Leontief's paradox was exactly what the strong factor-intensity assumption tried to exclude. So, by dropping this assumption and using the idea of factor-intensity reversal, they thought that they could explain away Leontief's paradox.

Beginning in the late 1970s, a group of international economists, including Paul R. Krugman, proposed an alternative explanation, also under the neoclassical theoretical model, for the occurrence of international trade. In this so-called new international trade theory, economy of scale—i.e., increasing returns to scale—was posited as the cause of international trade. In traditional neoclassical theory, the different endowments between countries constitute the condition of comparative advantage and thus determine what a country should specialize in producing and exporting. In the new international trade theory, however, it is the inherent advantage of specialization—i.e., the inherent advantage of increasing returns to scale—that determines the different endowment of each country and so determines the content of its production and exports. The change brought by the new international trade theory to the neoclassical theoretical model is that the original assumption of perfect competition is replaced by the new assumption of monopolistic competition, due to which the inherent advantage of economy of scale will create an imperfect market structure. Although this new trade theory was not proposed specifically to resolve the problem of Leontief's paradox, it can be used for that purpose.

This case reveals that, faced with anomalous facts, the neoclassical theory of international trade is forced to make revisions, such as the addition of the account of factor-intensity reversal or of increasing returns to scale, because of its faulty implications. This incident of economic theorizing, as practiced by neoclassical economists, illustrates how neoclassical theory is resistant to anomalous facts because its theoretical model, instead of being discarded, is preserved. It is this persistence in retaining a "refuted" theoretical structure while continuing to revise the theories derived from it that induces the frequent criticism that economists ignore the results of empirical testing and make ad hoc explanations for anomalous phenomena.

In the following, however, I argue that this criticism reflects a misconception of the methodology of the creation of economic explanations. As a first step, let's review the origin of the confusion, namely, the Deductive-Nomological (D-N) model of scientific explanation.

It can be argued that, in our example, when neoclassical economists are making the explanation for the U. S. behavior in international trade, they are in fact invoking the D-N model. This argument goes as follows: These economists use two things—the theorem of international trade as the economic law and the fact that the United States is a capital-abundant country as the initial condition—to derive the conclusion that the United States is producing and exporting capital-intensive goods. Figure 1.2 shows the format of the D-N explanation in this case.

[Please refer to Figure 1.2 on page 20-1]

By arguing in this way, these economists are to be regarded as regularists. The

The Theorem of International Trade (the H-O Theorem): A country has a comparative advantage in producing and exporting the goods that use more intensively the country's more abundant factor.	The covering law	Explanans
The U.S. is a capital-abundant country.	The initial condition	
The U.S. is producing and exporting capital-intensive goods.	Conclusion	Explanandum

Figure 1.2: The Format of the D-N Explanation

reason is that, viewed from this perspective, the theorem of international trade is regarded as a regularity law that is used to illustrate that the U. S. behavior in international trade is an instance that is under its coverage. In other words, the fact of the U. S. behavior in international trade is expected to have occurred *by virtue of* the regularity law. This is also the key requirement for an explanation to become a scientific explanation as is stated in the D-N model.

Since the publication of Leontief's work, the most frequent criticism made by the regularist methodologists of neoclassical economists' theorizing practices is that neoclassical economists, instead of rejecting their "refuted" theory, hold fast to their original theoretical model after revising it. Admittedly, the regularist methodologists may agree that neoclassical economists can supply additional factors, such as factor-intensity reversal, to their explanatory account to make it more plausible, but regularist methodologists do not agree that this addition will make neoclassical economists' explanation complete. The key element required by the regularists is a regularity law that is genuinely broad enough to cover any phenomenon in question and not only an additional factor.

This line of regularist thought can be further developed by adding the ideas of model and causal factor. The idea is this: The regularists may revise their position by agreeing that each individual regularity law is derived from an economic model within which a number of causal factors interact with each other. But even if the regularist position is revised, it does not help to solve the problem because, in their eyes, neoclassical economists still do not go far enough to find out *the* most general economic model that covers or unifies every economic detail in the world and thus can be used to generate *the* genuinely universal regularity law that is broad enough to cover or unify any phenomenon in question.

I am not prepared to argue whether scientists' practice in *making scientific* *explanations* actually follows the way depicted in the D-N model—i.e., to make an explanation is to subsume the instance to be explained under a regularity law. Instead, I argue that the actual practice of *making economic explanations* does not follow this way; rather, it involves different procedures.

Let's return to our example. The so-called traditional neoclassical international trade theory that we have been discussing is in fact a conglomerate of loosely related ideas dispersed in the trade theories provided by E. F. Heckscher in his 1919 paper and B. Ohlin in his 1933 book discussing the occurrence of international trade. Their theories are further developed to become a general equilibrium system by P. A. Samuelson in a series of papers on the issue of equalization of production-factor prices published during 1948-1979. Thus, the now so-called Heckscher-Ohlin (H-O) international trade theory indeed should be called the international trade theory of the Heckscher-Ohlin-Samuelson (H-O-S) tradition. The H-O-S international trade theory is in fact a highly abstract theory in that it includes a great number of assumptions. Some of the most important assumptions are quoted in M. Chacholiades's popular textbook and presented in Table 1.1. (Chacholiades 1990, pp. 63-66)

[Please refer to Table 1.1 on page 22-1]

We list all these assumptions because we want to discuss the causal structuralist approach by starting with the role that assumption plays in this approach. Normally, in addition to the explicitly stated factors representing the theoretical causes of a certain phenomenon in question, some factors stated in assumptions—such as those mentioned in assumptions 2, 3, 4, 7, and 8 in Table 1.1—can also be regarded as the main potential factors contributing to the occurrence of the phenomenon. But why

Table 1.1: The Basic Assumptions of the Heckscher-Ohlin Model

Assumption	Description
1. Number of countries, factors, and commodities	There are two countries; each country is endowed with two homogeneous factors of production and produces two commodities.
2. Technology	Technology is the same in both countries.
3. Constant returns to scale	Each commodity is produced under constant returns to scale.
4. Strong factor intensity	One commodity is always produced by using intensively a factor of production relative to the second commodity.
5. Incomplete specialization	Neither country specializes completely in the production of only one commodity.
6. Perfect competition	Perfect competition rules in all commodity and factor markets.
7. Factor mobility	Factors are perfectly mobile within each country but perfectly immobile between countries.
8. Similarity of tastes	Tastes are largely similar (but not necessarily identical) between countries.
9. Free trade	World trade is free from any impediments, such as tariffs, quotas, and exchange control.
10. Transportation costs	Transportation costs are zero.

are some factors impounded in assumptions and some others regarded as causes? The answer is directly related to my view regarding the distinction between a theory and its models.

A theory, as I stated in the preceding section, is a general idea about a hypothesized complete causal structure for a certain class of phenomena. In our example, the H-O-S theory conceives that there exists a complete causal structure that includes a number of causal factors, such as the endowment of production-factors, the level of production technology, the type of return to scale, the factor-intensity of a product, factor mobility, and consumer tastes. These six causal factors interact and follow the causal paths specified in the H-O-S theory to produce international trade. These six causal factors are *assumed* to be *all* the factors that are responsible for the occurrence of international trade; this is why I say that the H-O-S theory is a *hypothesized complete* causal structure.

People may find, however, that the Heckscher-Ohlin (H-O) theorem—i.e., the theorem of international trade mentioned at the beginning of this section—asserts that the endowment of production-factor is the only causal factor that is responsible for the occurrence of international trade and not that these six causal factors together are the causes for international trade. With respect to this question, my answer is that what is asserted in the H-O theorem is exactly a result derived from the practice conducted by economists in model-manipulation process I (MMP-I), mentioned in section 1.3 and illustrated in Figure 1.1. To see the extent to which the endowment of production-factors can contribute to international trade, Heckscher and Ohlin isolated this causal factor from the influences of the other five factors and derived from their simple model the so-called Heckscher-Ohlin theorem, stating that a country has a comparative advantage in producing and exporting the goods that use more intensively the country's more endowed factor. So what is normally regarded as the

H-O theorem should be regarded as the result derived from *the H-O model* (not the H-O theory), within which only one causal factor operates.

Why, then, did Heckscher and Ohlin perform a simplified version of model-manipulation with respect to their more complex theory? It might be that, during their time, the endowment of production-factor was regarded as the most important factor in determining each country's behavior in international trade. The real reason should be obtained from historical research. But whatever their motivation, what Heckscher and Ohlin have done is a kind of model-manipulation that is conducted against the background of their hypothesized complete causal structure—i.e., their theory.

Another example of model-manipulation in MMP-I is the so-called new international economic model mentioned earlier in this section. For these new international economists, international trading of some products should be considered the result of the effect of increasing returns to scale. The most often cited example to explain this model is the aircraft industry. As is pointed out by P. Krugman (Krugman 1990, p. 2), the initial selection of the location of the Boeing company in Seattle, Washington, U. S. A., may have been an accident, but the point is the ensuing effect of this selection. This industry's early establishment in Seattle (or the United States) enabled this city (or country) to gain the lead in manufacturing aircraft and thus to reach the economy of scale—i.e., the increasing returns to scale—earlier than any other city (or country). So the increasing returns to scale determine that Seattle (or the United States) exports airplanes to other cities (or countries). In this case, what Krugman did, also against the background of the hypothesized complete causal structure depicted in the H-O theory, was to single out the type of return to scale as the only cause for determining the direction of international trade, impounding the other five factors in the assumptions.

Thus, it seems that, under the hypothesized complete causal structure provided by the H-O theory, we have two causal models that can be used to answer questions such as, What is the cause of international trade? and What determines the content of a country's behavior in international trade—i.e., what determines the content of a country's export and import? At this stage, another question may arise: Which one is the better causal model to answer these questions? A crucial point here is that no one causal model can be regarded as *the* correct model that can be used to answer these questions in all economic situations. An example illustrates this point.

If we are asked to explain the cause of international trade, which model should we select? Just as we think about the structure of the model from which we derive the economic law (such as the H-O theorem and Krugman's conclusion), we must also think about the structure of the question itself; otherwise, we have no idea how to answer it. Consider the question, What is the cause of international trade? The problem with this question is that its range is too broad. We need to limit its range because if we do not, we may again be caught in the trap of regularist thinking. The question seems to ask for an *ultimate* cause of international trade, but it is obvious from common sense that there is no such fundamental cause. Therefore, instead of responding immediately to this regularist type of question, we should direct our questioner to revise the question to a more specific form.

Admittedly, there are various ways that this question can be revised. For convenience, let's take the revised form as, What is the cause of the international trade in X? This unknown X stands for a commodity, such as aircraft. Given these assumptions, we would think that the new international trade model is the right one to choose because the characterization of the aircraft industry is more pertinent to the features singled out by this model.

Now suppose that we replace the unknown X with stuffed animal toys. In that case, which model should we apply? At first glance, it may seem that both models are applicable. It seems to be true that the country exporting the greatest number of stuffed animal toys is China. The new international trade model may argue that China has the largest economy of scale in producing stuffed animal toys, so China has the advantage in exporting these products. But our structural rethinking suggests that this should not be the case. China's advantage in producing and exporting the world's largest number of stuffed animal toys comes from its advantage of having the world's largest pool of lower-cost labor. In this case, the main feature of the phenomenon in question is not increasing returns to scale but rather the abundance in labor. This feature is therefore more pertinent to the traditional production-factor-abundance model—i.e., the H-O model.

This example illustrates that no one causal model can be used to explain an economic question in different economic situations. So there is no reason to expect that there is one genuinely general causal law that can be derived from a genuinely general economic model that in turn covers every economic detail in the world. *Regularity*, however, can enter into this picture as long as the same causal model can be used repeatedly to answer the same type of question in the same kind of economic situation. For example, when the H-O model can be *repeatedly* used to identify the cause for exporting cloth, athletic shoes, or any other labor-intensive goods for a number of labor-abundant countries, then the H-O theorem derived from the H-O model can be regarded as a regularity law that can be used to explain the export of labor-intensive goods from these labor-abundant countries.

But note that this regularity law—i.e., the H-O theorem—is to be regarded, at this stage, as a regularity law *only within* a very limited domain, namely, only within this group of labor-abundant countries producing labor-intensive goods. As to the question of whether the applicability of the H-O theorem can be extended to explain the export of capital-intensive goods from a group of capital-abundant countries, the same examining procedure must still be conducted in this individual case. This examining procedure is an example of what I call model-specification test in section 1.3 and in Figure 1.1.

A more interesting point arises when we find that our selected model cannot explain a bizarre instance. Again, our example is the paradoxical result obtained in Leontief's testing. The result shows that, contrary to what is stated in the economic law derived from the factor-abundance model, the United States exports more labor-intensive goods than capital-intensive goods even though the United States is generally considered to be a capital-abundant country. As mentioned earlier, this paradoxical test result has been followed by a great deal of research to account for the origin of this problem. For our discussion we select one of them: the account of factor-intensity reversal.

Before we move on to discuss factor-intensity reversal, however, let's briefly consider a point regarding Leontief's testing. What Leontief tests is the general proposition derived from the traditional theoretical model: To increase its welfare, a country should produce and export the goods that use more intensively the country's more abundant production-factor. The instrument that Leontief uses to conduct his testing is the content of U. S. export. It is this practice that we are concerned about. Instead of being conceived as a test for a regularity law, this practice should be considered as a test for whether the abstract structural law—i.e., the H-O theorem—generated from our theoretical model—i.e., the H-O model—can be exported from this model to be used in explaining a concrete case—i.e., the case of the U. S. behavior in international trade. In addition, Leontief's input-output analysis should be considered, according to the causal structuralist explanatory scheme presented in Figure 1.1, as *an empirical model* that is formulated according to the content of the theoretical model to be tested and is used to do the test. In the overall picture of our explanatory scheme, Leontief's practice constitutes a model-manipulation phase that stands at the midpoint between theory-building and theory-testing.

Let's now return to the account of factor-intensity reversal. This phenomenon is presented in detail in section 2.6; suffice it to say here that, if the reversal situation occurs, it must be that the U. S. abundance in capital acts as a cause having *dual capacities* to produce two effects: One effect, as is stated in the economic law derived from the traditional theoretical model—i.e., the H-O model—is to constitute a condition of comparative advantage in producing capital-intensive goods, an advantage that enables the United States to promote its greater welfare by producing and exporting these goods. The second effect is to induce producers to use more of the country's relatively cheaper production-factor—i.e., labor—in their production and thus to trigger a *reversal.* This means that the producer's substitution of the cheaper production-factor results in a change in the production-factor proportion ratios of commodities, and that change acts as a countervailing factor, offsetting the positive capacity of the country's abundance in capital. This phenomenon thereby converts the previously capital-intensive goods into labor-intensive goods, and the United States becomes a country that exports labor-intensive goods. So the phenomenon of Leontief's paradox can be explained as nothing less than a net result of two countervailing effects under a specific causal structure.

Whether this explanation is successful is not the issue for our purpose. Our point is that when economists can supply this type of explanation to account for a problem, it proves that they are not regularists. The reason is that these economists are not concerned with the question of whether their derived causal law has been refuted by an anomalous instance; rather, the issue is how the anomaly is generated, which is a structural problem and not a regularity problem. Therefore, to explain the anomaly, economists think that they must reconsider the structure of their theoretical model, and then they will find the answer. In our example, the main feature of the revised model is that the assumption of strong factor-intensity is removed, and the factor released from this assumption is taken into consideration as an additional causal factor in combination with other existing causal factors to serve as a set of operating factors being used to derive a new economic law. This example illustrates a truth: that the history of finding a new causal structure to produce a new economic law is not a description of making an ad hoc explanation or providing an immunizing strategy for the refuted theoretical model. Rather, it is a history that records how economists struggle to uncover the true explanation for the phenomenon of interest.

What is the relevance of this account to the overall picture of our explanatory scheme? As shown in Figure 1.1, this account asserts that *a mechanism of information exchange*, which stands at the midpoint between the selected theoretical model and its corresponding empirical model, is active. When economists gain information from the theory-testing part—information that there is a problem with their theory being exported from the domain specified by the theoretical model to explain the concrete instance (in our example, the phenomenon of Leontief's paradox)—they then return to the theory-building phase to reconsider the hypothesized complete causal structure depicted by the H-O-S theory and the origin of the problem in the theoretical model, and then to discover which part of the model needs to be revised. In this description, the information acts as *feedback* to the theory-building part, telling economists how and when to restructure the theoretical model in order to produce the law that can be used to explain the anomaly in question. The practice of economists' theorizing described earlier constitutes another portion of the model-manipulation part of the process. This portion contains manipulation of the theoretical model's restructuring—i.e., the second round of MMP-I. Information exchange continues over time as long as there are communications between the theory-building part and the theory-testing part via the model-manipulation process.

My presentation illustrates that this strategy can provide a tool that can be applied to reduce the possibility of choosing an untenable model. Even if, for any reason, the model we choose is not well equipped to explain a concrete phenomenon, we can use the mechanism of information exchange, a practice of model-manipulation, to fine-tune our selected theoretical model. In this fine-tuning, we change the arrangement of the causal factors in the model and then use this new theoretical model to see whether it can generate the phenomenon in question. If it can, then we can apply the conclusion derived from the new model as an economic law to explain the previously unexplainable phenomenon.

In general, a new theoretical model that is revised or modified with respect to the points where the old theoretical model fails is *more concrete (or less abstract)* than the old theoretical model, because the new model will contain more concerns about the structural features of the targeted phenomenon—i.e., more phenomenal contents—than the old one. The main purpose of our entire searching procedure is to try to find out *the relatively most complete* model that can be used to explain the targeted phenomenon. Detailed discussion of these ideas is presented in sections 4.3 and 5.4.

1.5 Conclusion

This completes the analysis of the overall structure of my explanatory scheme. The schematic picture of this structure, illustrated in Figure 1.1, contains three main sections: a theory-building part, a model-manipulation part, and a theory-testing part. The theory-building part involves activities such as determining the causal factors to be included in the theoretical model, setting up the causal structure of the theoretical model, running the theoretical model and deriving conclusions (which we count as economic laws), and formulating an explanation for the targeted phenomenon in the theoretical model. When economists engage in theory-building, they are in fact manipulating their theoretical models, and this constitutes a portion of the model-manipulation part. After establishing their theoretical account of the targeted phenomenon, economists may then attempt to apply this account to explain a new concrete phenomenon in the world. They first apply this crude model directly to explain this new concrete phenomenon. If it works, then it goes. Otherwise, an empirical model, based on the content of the theoretical model, is set up and run to test the exportability of the theory to see whether it can really be brought out of the environment specified in the theoretical model. This empirical testing of the exportability of a theory constitutes another task in the model-manipulation part. This time, the model being manipulated is the empirical model. If this empirical model does not pass the test, then the information gained from the failure acts as feedback to the theory-building part. When the theoretical economists receive the message, they conduct a second round of theory-building to discover the correct causal structure that will produce the right economic law to explain the anomalous phenomenon. Again, this task involves model-manipulation; this time, the model being manipulated is the theoretical model. The entire procedure is repeated continuously over time because the information exchange mechanism is always at work between theory-building and theory-testing.

This chapter calls for an adjustment in the focus of methodological discussion in economics. As argued in this chapter, an exclusive methodological focus on the issues of theory-testing is insufficient. If regularist methodologists were to step back and view the entire structure of economic theorizing, they would discover how myopic it is to focus too narrowly on only one aspect of the process.

Chapter 2

Causal Capacity and Its Implications for Theory-Building in Economics

2.1 Two Accounts of Theory-Development in Economics

When Bertil Ohlin, one of the founders of modern international trade theory, stated in his 1933 book that free commodity trade between countries tends to equalize the prices of production-factors between these countries, what did he mean by "tends to"? Many international economists who follow the so-called Heckscher-Ohlin-Samuelson tradition contend that the main reason for the occurrence of international trade is the difference in production-factor endowments among nations. As discussed in Chapter 1, they argue that a specific structure of production-factor endowment gives a country a comparative advantage in producing and exporting the commodity that uses more intensively the country's more abundant production-factor. In other words, the comparative abundance of a certain production-factor tends to lead the country to export the commodity in question. Again, what does "tends to" mean here? Admittedly, both usages are a synonym of "causes to." So, to be more specific, we should ask, What kind of causal thinking is involved in making this type of tendency claim? Defining this concept is directly relevant to our discussion of theory-building and theory-testing in economics. The nature of the causal thinking involved in this type of tendency claim is often misidentified as the regularist view of causal laws. As a result, the direction of the development of the meta-theory that is used to describe theory-development in economics is shaped to fit the regularist view of economic explanation discussed in Chapter 1.

This effect on the meta-theory can be illustrated by using an example mentioned in Chapter 1. In his 1954 empirical research, Wassily W. Leontief conducted an input-output analysis by using U.S. economic data for 1947. Leontief found that U.S. import-competing production required a higher percentage of capital input per worker than U.S. export production. That is, U.S. import-substituting commodities (which were produced within the United States and competed with imported goods in the U.S. market) required more capital input per worker than did goods produced for export. If the content of U.S. import-substituting goods were regarded as the mirror image of the imported goods, the United States in fact exported labor-intensive commodities and imported capital-intensive goods. Because the United States has always been regarded as the most capital-abundant country in the world, this result ran directly contrary to the then widely accepted Heckscher-Ohlin theorem (the H-O theorem): A country exports those commodities that use more intensively the country's comparatively more abundant production-factor in their production. And because the underlying causal mechanism of the H-O theorem is that a country's more abundant production-factor will tend to cause the country to produce and export those commodities using more intensively this abundant production-factor in their production, Leontief's empirical result seems to provide evidence to reject the underlying mechanism.

In the field of international economics, the Leontief paradox soon invoked a great deal of empirical and theoretical research to try to reconcile this paradoxical conclusion or to provide further evidence to refute the H-O theorem. Also, in the field of philosophy of economics, this case has been repeatedly used as an example to illustrate the so-called falsificationist view of theory-development, which was widely adopted by economic methodologists in 1970s and 1980s. Two prominent examples are the studies conducted by Neil De Marchi (1976) and Mark Blaug (1980 and the 2^{nd} edition 1992, Chapter 11).

Blaug applied a more naïve—the so-called Popperian—version of falsificationist methodology. In reviewing the development of international trade theories since Leontief's paradox, Blaug expressed despair in pointing out that the ensuing theoretical development lacked an empirical underpinning because economists had not been willing to "perforce pass a qualitative judgment on the evidence for and against the theory in question." (1992, p. 191) On the contrary, in Blaug's opinion, the history of theoretical development of international trade theory following the Leontief discovery recorded that international trade theorists had tried to immunize their theories from empirical testing by invoking the ceteris paribus clause or by making ad hoc explanations. Therefore, Blaug seemed to agree with Peter Kenen that "international trade and finance displayed a stubborn immunity to quantification. They became the last refuge of the speculative theorist." (Kenen 1975, p. xii from Blaug 1992, p. 190)

On the other hand, De Marchi applied a more sophisticated version of falsificationist methodology, the so-called Lakatosian methodology. De Marchi's studies showed that the theoretical development of international trade theories in the same period can be regarded as a development of "the Ohlin-Samuelson research program." (De Marchi 1976, p. 123) According to De Marchi, a refuted theory within a research program can still be retained as long as it can be shown that, after being revised, it is "consistently predicting novel facts (is 'progressive')." (Ibid., pp.109-110) Viewed from this perspective, Blaug's proscribed immunizing strategies, including invoking the ceteris paribus clause and making ad hoc explanations for the refuted theory, can be regarded as a part of the heuristics, be it negative or positive heuristics, of this research program. The heuristics can be seen as an attempt to contribute a bit of effort to lead a research program in the direction of a "progressive problem-shift," that is, in the direction of growth in its truth-content.

By "truth-content," De Marchi meant the corroborated content of a research program, which is to be determined by the corroborated content of the newest theory developed within it.

My purpose in discussing falsificationist methodology is to point out that the approach being developed by philosophers of economics—to discuss the development of economic theories or economists' practices—must have been shaped by philosophers' own views about the nature of economic laws. For Blaug's case, as I have already explicated in Chapter 1, his view of economic methodology is indeed shaped by the regularist view of economic laws. The current focus here is whether De Marchi's view of theory-development in economics, which is based on the Lakatosian approach, is also influenced by the regularist view of economic laws.

One important feature of De Marchi's approach distinguishes it from Blaug's approach. By staying in line with the Lakatosian tradition, De Marchi allowed anomalies such as Leontief's paradox to figure in the theoretical development of the Ohlin-Samuelson research program (O-S research program) as long as anomalies can be explained by a later theory developed within the same research program. De Marchi's idea is this: Let's suppose that the initially developed H-O theorem constitutes the main content of the initial version of the Heckscher-Ohlin theory of international trade, T_n, developed in the O-S research program. Next, suppose that a revised version of theory T_n, call it T_{n+1}, has been developed within the tradition of the O-S research program in an attempt to cope with the Leontief paradox. If it turns out that T_{n+1} can be used both to explain what T_n has explained and to explain the originally unexplainable phenomenon pointed out in Leontief's study, T_{n+1} is said to be corroborated and is thus regarded to have more truth-content than T_n does. The consequence of allowing this revising procedure in economic theorizing is that it implies that theories in the O-S research program are not to be rejected out of hand simply because they make false predictions; by using a revising procedure to improve the predictive (or explanatory) power of its theories, the O-S research program could have increasing numbers of successes, with successive theories accounting for increasing numbers of international economic phenomena. It is this salient methodological implication that leads De Marchi's approach away from the tradition of Popperian falsificationism and toward that of Lakatosian falsificationism.

Blaug's approach suggests that good practice in economic theorizing should try to discover the most extensive possible regularity law, which can be used to construct the most splendid possible economic theory, which in turn can be put under test to justify its explanatory power against any economic phenomena in the real world. De Marchi's approach, by contrast, seems to suggest that good practice in economic theorizing is to formulate a good economic explanation that can be used to explain the occurrence of previously unexplained or unexplainable economic phenomena and so to increase the truth-content of the research program within whose tradition the theorizing practice is conducted. Note that what De Marchi requires is not that economic theorizing attempts to discover a genuinely true regularity law, but instead that it attempts to revise the existing theory so as to make it more applicable for use by economists to provide a good explanation for the economic phenomena in question. The comparison between the two accounts is interesting, but my main concern here is this: Even if De Marchi's account is true, why are economists so committed to their refuted theories? De Marchi's account seems to owe us an explanation.

2.2 Causal Capacity and the Causal Structuralist View of General Causal Claims in Economics

For a causal structuralist, the answer to the question raised at the end of the preceding section is that economists are so committed to their refuted theories because they believe in what Rom Harré calls "causal powers" and Nancy Cartwright calls "causal capacities." Following Cartwright, I think of a causal capacity as a stable causal power that a cause should carry with it from one situation to another situation to have its impact on an effect.

Following this characterization, we can view the causal claim derived from the primitive version of the Heckscher-Ohlin model as follows: The difference of factor endowment has a causal capacity to determine the content of a nation's commodity production and export. The model used to derive this causal claim is said to be a primitive version in that, in this model, there is only one cause—i.e., the difference of factor endowment—considered to exert influence in the simplified hypothesized causal structure; the influences of other disturbing factors have been ruled out by assumptions or ceteris paribus conditions. The point of having this highly simplified causal model is that economists want to know to what extent the difference of factor endowment as a cause can exert influence on the direction of a nation's export. For international economists, Leontief's paradox is only an indication that the causal structure specified in the highly simplified version of the H-O model should be adjusted so as to cope with the real causal structure in which the paradoxical phenomenon arises; they do not see the original causal claim regarding the causal capacity of the difference in factor endowment as problematic. In other words, the difference in factor endowment is supposed to have its stable influence on the direction of a nation's export; if this assertion fails in the real situation, there must be a reason for it, specifically a *structural* reason. For example, new causal factors should be added to the original causal model, and the original arrangement of causal relations should be readjusted to reflect the addition.

For a causal structuralist, economists' practice of repeatedly readjusting their causal model with respect to different economic situations—i.e., the continuous practice of revising their economic theories with respect to every anomalous phenomenon as described by De Marchi—reflects economists' endless effort to detect correct hypothesized causal structures that can be used to explain why what is asserted in the causal capacity claim does not work in these economic situations. It is this deep commitment to the truth of the causal capacity claims that shapes economists' theorizing practice to develop in a way similar to the theorizing procedure illustrated in Figure 1.1.

To see exactly how the idea of causal capacity has its impact on shaping economists' practice of theory-building and theory-testing and to examine the regularist's challenge to the idea of causal capacity, let's first assume that economic theories contain economic laws as their main hypotheses, and then examine the general formulation of the economic laws contained in the economic theories. We can characterize the formulation of the economic laws contained in economic theories in the following form: Other things being equal, in condition A, C's cause E's.

This way of formulating an economic law makes regularists uneasy, for at least four reasons. First, for regularists, the word "causes" used in the general formulation is misguided. The reason is that, following their Humean skepticism about causation, regularists think that there is no possibility that one can construct a causal relationship among events; one can grasp only the regularities among events. Therefore, the general formulation of the economic laws should at least be revised as the following form: Other things being equal, in condition A, C's are always followed by E's.

Second, for a naïve falsificationist such as Blaug, one of the most often proposed questions is, What are the "other things" mentioned in the formulation? Unless the *complete*, concrete content of these other things can be clearly identified, the economic law containing this vague specification can never be falsifiable, and thus the economic theory containing this unfalsifiable economic law lacks empirical content. Therefore, naïve falsificationists seem to suggest that, for an economic law to explain or predict a certain class of economic phenomena, it should at least possess the following form: In condition A, C's and *the complete concrete content of these (equal or unequal) other things* are always followed by E's.

Third, the phrase "in condition A" also causes trouble for the regularists. An economic law containing this kind of phrase is applicable only to a certain domain. In our case, this limited domain is the condition A. This restriction contradicts the regularist's notion of a natural law. For a regularist, a causal law should be genuinely broad enough to cover whatever phenomenon is in question. If the causal law contained in an economic theory does not possess such a broad-range characteristic, the theory, in order to be able to cover a wide range of economic phenomena, must then contain a lot of limited-range causal laws. This picture of a theory contradicts the regularist's notion of a theory, which supposes that a theory should have the feature of providing unification.

Furthermore, regularists argue that the theoretical economic models used for deriving economic laws are generally unrealistic. Critics of this point maintain that if economic theories are to be of practical use in the real world, economic theorists should not be content with their theories being applicable only within, say, condition A. They ask, What if condition A is incompatible with the real condition in question? Admittedly, this mismatch is generally the case. Does this mean that economic theories are without any empirical use and are simply academic exercises—what Blaug called "the last refuge of the speculative theorist"?

I maintain that this way of challenging the formulation of economic laws is misguided. To analyze why, let's start by considering how an economic theory is built. When economic theorists are constructing their theories, their practices are just like that used by natural scientists in their experiments. What is the purpose of doing an experiment? Most people would say that, in general, scientists want to know whether and in what way a certain factor (call it C) has a certain effect on the other factor (call it E). To have a decisive and reliable experimental result, scientists follow a crucial procedure: Using background causal knowledge that they have obtained from other scientific theories, they set up a contrived environment (or model) that will rule out the influences of all other imaginable disturbing factors that may intervene in the causal path from C to E. Their goal is to guarantee that the result derived from this model is the correct desired result—i.e., the operation of C on E.

When the experimental result is consistent with what the scientists expect, the causal path being tested is used to formulate a causal-law claim that constitutes a main part of a scientific theory. When the experimental result is inconsistent with what the scientists expect, the hypothesis of the causal path from C to E is not ruled out outright. Instead, scientists suppose that the inconsistency may be caused by some other reasons, and they then set out to discover these other reasons. New factors may be involved, but the scientists often start by examining their contrived model to see whether any disturbance thought to be put under control and so to be inactive in their model is in fact, for some reason, active in bringing about the inconsistency. Whether the final conclusion is that a supposed inactive factor is in fact active or that new factors need to be included, further tests result in changes in the causal structure of the original model—i.e., the causal structure of the original model will be restructured (or modified) to take into account the additional causal considerations. Each newly restructured model must be tested to see whether the result derived from the new causal structure can explain the inconsistency. The search for the contributing reasons continues as long as no convincing reason has been identified.

A question arises: Why are these scientists so committed to their causal claim that C causes E? Why don't they simply discard their causal claim? The reason is that they believe that C, within the well-contrived model, should have its stable influence on E. Or C has the *capacity* to cause E—i.e., C, because of its being C, carries with it the capacity to cause E. The simple idea underlying this capacity belief is that C, within a well-contrived model, will reveal its ability to cause E. If it turns out to be the contrary in a real-life case, there must be a reason that this happened; but, in any case, C has the ability to cause E as long as it is not disturbed and thus prevented from doing so.

This seemingly dogmatic belief is not dogmatic at all, because the capacity belief also requires that every contradictory conclusion to the capacity claim have its own reason to explain itself and that this proposed reason must also be tested. The latter requirement places the capacity claim within the empirical tradition. It is meant to show that the new reason is not simply an ad hoc explanation but rather is a solid empirical explanation. The explanation is justified because the test shows that the newly proposed reason and the original structural factors, both of which constitute a new causal structure, can now be used to produce the conclusion that can be used to explain away the contradictory conclusion to the capacity claim. If the conclusion derived from the newly structured model cannot provide a convincing explanation for the inconsistency, the proposed reason is discarded, and another new effort to determine the correct reason is launched.

It may be argued that the idea of capacity is a strong idea; as mentioned earlier, to believe that C has the capacity to cause E without any qualification added is to believe that, *in any situation*, C does cause E as long as it is not disturbed and thus prevented from doing so. Therefore, it seems that this idea is strong enough to be able to turn into a regularity idea. But to argue this point is, again, to ignore the causal structure. When we say that C has the capacity to cause E, we believe that C, because of its being C, has the ability to cause E. But unlike the regularity idea, the idea of capacity does not come without restriction; it includes the idea of causal structure. Admittedly, capacity is the stable influence of a causal factor that this causal factor carries with it to exert on another factor from situation to situation. But this exertion is fulfilled only under a stable causal structure that is well-contrived to guarantee that no disturbance is involved and that the capacity possessed by this causal factor can be fully exerted. If, for some reason, the exertion of the capacity is disturbed, its stable influence must then be affected by this disturbance. And this effect, depending on the way the disturbance affects this stable influence, may lead the originally supposed stable influence to result in any kind of net conclusion.

For example, the net conclusion may show that C's influence on E is in fact less than what is expected in the capacity claim. But this mismatch does not refute the capacity claim outright, because a search for the reason may show that C has a dual capacity—i.e., because of the effect of the disturbance, C now carries a dual capacity that exerts C's influence on E in two opposing directions. The net conclusion suggests that C's negative influence is stronger than C's positive influence on E. The theory of C's dual capacity is the reason for the shortfall in the expected influence, and it should be supported by the conclusion derived from the newly restructured causal model. If the new model is restructured according to the conditions provided by the dual capacity theory and if it can derive a result that is consistent with what is expected by the dual capacity theory, then the theory of C's dual capacity is said to have explained away the inconsistency between what is shown in the net conclusion and what is expected in the capacity claim. As a result, the capacity claim that C causes E is retained, and the capacity is in this sense said to be carried by the causal factor from situation to situation, although it sometimes does not demonstrate its full exertion in the net conclusion of a causal operation.

In section 2.6 we discuss the dual capacity of a cause in more detail when we examine the case study of Leontief's paradox and its economic explanation. Suffice it to say here that the construction of the idea of the ubiquity of capacity—i.e., the idea that capacity is carried by the causal factor from situation to situation—is heavily dependent on whether we have a stable causal structure to allow the fostering of this idea; therefore, contrary the regularist argument that capacity is also a regularity idea, capacity is indeed a causal structuralist idea.

2.3 The Role Played by Ceteris Paribus Clauses in Economic Theory-Building

Recall that when economic theorists construct their theories, it is as if they are conducting an experiment. When they invoke a ceteris paribus clause—i.e., other things being equal—and specify the special conditions for their theoretical models, it is as if they are setting up a well-contrived environment for their experiments. The ceteris paribus clause acts as a shielding condition, and the special conditions act as the initial conditions necessary for the operation of the factors of interest. Moreover, both of them are combined to form a safeguarded boundary for the theoretical model, thereby guaranteeing that the cause of interest will fully exert its capacity on the other factor of interest. The conclusion derived from the theoretical model is what we generally call an *economic (causal) law*.

If we accept this view of what an economic law is and how it is derived, the regularist's challenges to the traditional formulation of economic laws—"Other things being equal, in condition A, C causes E"—becomes pointless. As for the challenge to the phrase "other things being equal," we can now reject the Popperian view that this ceteris paribus clause is a trick to immunize economic theories containing this kind of vague economic law from falsifying. Neither should we expect that this vague specification of "other things" must be filled with concrete content and thereby constitute a part of the complete list of causal factors of interest. Instead, the ceteris paribus clause now plays a special role in causal structuralist thought concerning economic theorizing in that it acts as *a shielding condition* to keep disturbing factors from interfering with the experiments of economic theorists.

As for the challenge to the phrase "in condition A," we now know that to specify a set of special conditions such as A is to set up the necessary initial conditions for the operation of the factors in question; it should not constitute a ground for the regularist criticism that the law derived from this limited domain A will not be universally true and universally applicable. Also, the regularist criticism of the persistent mismatch between condition A in the theory and the real condition of interest in the real world should not constitute a reason to degrade economic theories as not being empirical. As causal structuralists, we would not easily conclude that economic theories lack empirical content solely because condition A is incompatible with the real condition of interest, because we know that economic theories obtain so-called empirical content in a different way, which I explicate in the following.

2.4 Is Economic Causal Law Fundamental?

Contrary to the regularist argument that economic theories should be proposed in a form containing the economic (causal) laws with universal quantifiers, economic theorists argue that what can be added with universal quantifiers are the capacity claims. Whether the capacity of this causal factor can exhibit its persistently stable influence depends on whether there is a well-contrived environment to allow it to do so. If economic theorists are lucky enough to have a well-contrived model to produce the desired result of this causal factor's stable influence, then this result not only justifies their capacity claim but also suggests some kind of economic causal-law claim that can be used to construct economic theories. If, for some reason, what these economic theorists derive from their model is not consistent with what the capacity claim predicted, they do not rule out the capacity claim outright; rather, they try their best to discover the reason for the inconsistency.

Note that in the process of searching for the reason for the inconsistency, what is stated in the capacity claim still constitutes part of the economic theorists' background knowledge. The theorists' search does not end until they discover a convincing reason that can be used to construct a new causal model in which a conclusion can be derived to explain away the aforementioned inconsistency. If this kind of new conclusion is indeed derived, both the new causal structure suggested in this newly restructured model and this newly derived conclusion can suggest a direction for constructing a new economic causal law that can be incorporated into their economic theories. Viewing from this perspective, we know that what is *fundamental* for economic theorists should be the capacity claim; the economic (causal) law should be regarded only as *a derivative* that is *relative to* a well-contrived model. This model must allow the causal factor's capacity to operate in the specified way, and this way of operation is expected to be expressed in the derived economic causal law.

It is this recognition—that capacities are fundamental and that economic causal laws are derivative—that fosters the structuralist's view of the practice of economic theorizing and that differentiates this approach from that of the regularists. Regularists believe that a good economic theory should contain a causal law that is genuinely broad—i.e., fundamental—enough to cover whatever phenomenon is in question. For this reason, they require two things of economic theorists in order to support the claim that their theories are empirically correct. First, theorists must, *at the start of theory-building*, list all the "other things" contained in the vague ceteris paribus clause of the causal laws in their theories. Second, they must try to specify the theoretical condition so that it more closely reflects the real condition of the phenomenon in the real world.

An economic theorist following the causal structuralist idea, however, rejects the regularist's ideal conception that we can be omniscient about all the relevant causal factors and current background conditions. For a causal structuralist, a kind of *piecemeal* methodology is more plausible. This theorist would say, at the start of theory-building, "I cannot provide the complete list of those other things or the precise description of the real background situation of the phenomenon in question. But I can tell you that, *in the process of theory-building*, whenever one or a few of those other things are unequal or one or a few of those background conditions are changed, I can observe the impact they have in my original causal system—which comprises the relevant causal factors with stable capacities—and the new causal laws that can thus be derived."

The point of describing the contrast between these two approaches is to explain that the causal structuralist approach does not attempt and does not assert that it is possible to build a grand theory containing universally true economic causal laws that can be used to explain the entire domain of economic phenomena. Instead, the causal structuralist approach determines that, for each economic situation, the capacities possessed by each of the relevant causal factors should be stable across situations; if they are not, there must be a reason, and the content of this reason can be illustrated by a corresponding causal structure that can be constructed by economists to represent the putative interrelations among the relevant causal factors. Furthermore, after all these procedures have been completed, the economic causal laws are then said to be derived from this causal structure. For causal structuralists, what is fundamental is the capacity of the causal factor; the causal law is a derivative that is relative to a causal structure constructed by the economists by referring to the relevant background causal knowledge and all other relevant local or general data about the situation in question.

It is this different conception of the status of economic causal laws that results in a different emphasis in economic methodology between these two approaches.

2.5 The Theory of Two-Stage Tests and Its Implication for Economic Theory-Building

Regularists argue that a successful hypothesized economic law should be broad enough to cover a wide range of economic phenomena in question and so it should be able to be used to make accurate predictions about the economic phenomena of interest. As a result, they tend to use the traditional hypothetico-deductive (H-D) method to test the implication of a hypothesized economic causal law against empirical observation to see whether the two things are consistent.

But, for causal structuralists, the testing of a hypothesis is not so simple. The testing procedure suggested by the H-D method is too abbreviated. Causal structuralists believe that such testing of predictions should be postponed until we can make sure that the hypothesized economic law is a *right target*. We can ensure that situation obtains if the causal law in question is derived from a correct causal structure (or a correct causal model).

The idea is this: Recall from Chapter 1 that economists establish their theoretical models at the model-manipulation process I (MMP-I) stage in an attempt to use this model to explain the main features of the phenomena that they have considered so far. But how can they know that their model is a right model for their purpose? As is illustrated in Figure 1.1, economists conduct a causal model test (or a model specification test). But what is the content of this test?

When economists start to construct their theoretical model for explanatory purposes, the *tools* that they normally have are (1) the data that they have so far gathered from direct observation and from other reliable sources, (2) other economic theories relevant to the phenomena in question, and (3) their own theoretical construct of a causal structure that they hope reflects the unknown real causal structure underlying the phenomena. These three tools constitute *a conglomerate of knowledge* that can be used to set up a causal model. Based on the characteristics of this putative causal model, economists then move on to *design a test* containing a certain *benchmark*, which is derived from economists' consideration of the characteristics of this causal model and the relevant causal information that they have gathered so far. As a result, this benchmark can be regarded as an *index* that can be used to faithfully reflect whether the supposed cause has fully exerted its power on the observed effect.

Note that, because different causal models reflect different characteristics and different causal information, the benchmark is thus a tailor-made index for each causal model test. That is, each causal model test is specially designed for each causal model. The result derived from the benchmark test can in turn be used to show that the putative causal model is indeed an adequate model that can be used to represent the real causal structure from which the data so far are generated.

Once the putative causal model can pass the benchmark test, the putative causal model is thus regarded as the correct causal model that can be used to represent the real causal structure in question, and the conclusion derived from this causal model—i.e., the hypothesized causal law—can then be regarded as the correct causal law that should be expected to be derivable from the real causal structure. It is in this sense that I say that the hypothesized causal law is a right target.

Note that the causal model test can in fact also ensure that a negative result derived from the prediction test—i.e., that the implication derived from the causal law is inconsistent with what is exhibited in a class of new economic phenomena—reflects a structural inconsistency between the new real causal structure from which the class of new economic phenomena derived and the old real causal structure from which the old class of economic phenomena derived.

Why do I say this? Suppose that economists establish their theoretical model and use it to explain a class of economic phenomena, but they do not conduct a model specification test for it; as a result, even though the model can be used for explanatory purposes, the economists do not know whether it is a correct causal model for this class of economic phenomena. Then one day, a new class of economic phenomena calls for an economic explanation. Suppose that these economists still use their old theoretical model to explain or make predictions about this new class of phenomena, and they find that their model is no longer applicable. What is causing this problem? The answer is, we don't know. It may come from the original incorrect model specification, or it may come from the structural inconsistency between the two real causal structures. The point is that, in retrospect, if this group of economists had put their theoretical model under the model specification test and had gotten a positive result, the only remaining problem would be simply to point out the structural differences between the two real causal structures.

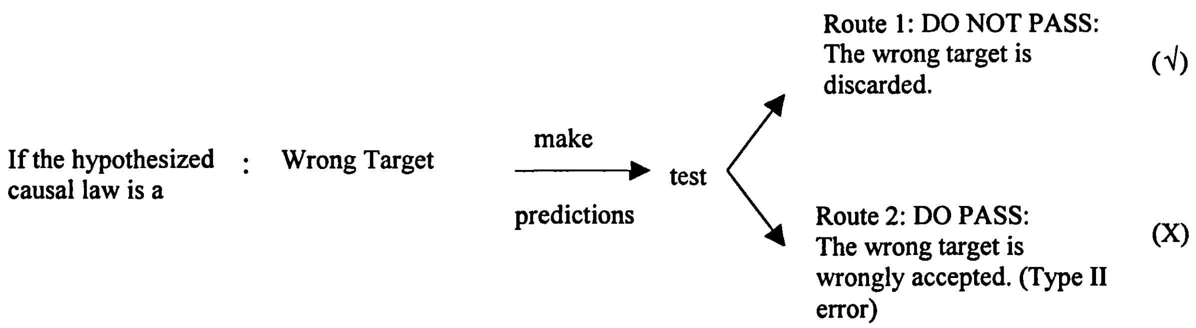
Imagine that these same economists did conduct a model specification test and got a positive result. In that case, their model specification test could be regarded as a *safeguard* to ensure that the result derived from a prediction test—in an attempt to see *whether* the prediction (or explanation) made from the hypothesized causal law is consistent with the new economic phenomena—would be guaranteed to reflect *whether* there is a structural inconsistency. If the hypothesized causal law, which is to be used to make prediction, is obtained from the aforementioned procedure, this law can be called a *right target* because the prediction made from it can be used *rightly* to determine whether there is a structural inconsistency between any two real causal structures. Furthermore, because the model specification test functions both as a causal model test and as a safeguard to guarantee the meaning of the result of a prediction test, we will call it the *first round test*. And, based on our argument so far, it is obvious that it is better to conduct the prediction test *after* the first round test; we will therefore call the prediction test the *second round test*.

One danger of bypassing the first round test and going directly to the second round test is the problem of erroneously accepting a wrong target. Again, let's use our simple example for illustration. Suppose that economists can use their theoretical model for explanatory purposes, although they do not conduct the first round test. Next, suppose that the causal law derived from their theoretical model is used to make a prediction for a class of new phenomena, and it passes the prediction test. What is revealed from this test result? There may be two revelations. The first one is that, although the theoretical model cannot be guaranteed to be a correct causal model for the old phenomena because no first round test result can be consulted, the result derived from the prediction test may suggest that the theoretical model happens to coincide with the real causal structure of the new phenomena. This is a happy ending. But there is another possible situation. Just like the situation in which the theoretical model is in fact a wrong causal model although it can be used for explanatory purposes, here the successful prediction may simply indicate that the model can be used for predictive purposes but it is not necessarily a correct causal model for the new phenomena.

Now, let's imagine the worst-case scenario, in which both dangers of bypassing the first round test are fulfilled: The economists' theoretical model is *neither* a correct causal model for the old economic phenomena in question *nor* a correct causal model for the new economic phenomena in question, *although* the model can *both* make a plausible explanation for the old economic phenomena *and* make a good prediction for the new economic phenomena. Figure 2.1 illustrates this problem. Note that, in this case, the hypothesized causal law, which is derived from economists' theoretical model, is said to be *a wrong target* in that the prediction made from it may lead economists to *wrongly* conclude that their theoretical model, which is in fact only a good predictive instrument but not a correct causal model, is a correct causal model for the new economic phenomena in question. Route 2 in Figure 2.1 shows this kind of mistaken decision by economists.

[Please refer to Figure 2.1 on page 51-1]

On the other hand, if economists do conduct the first round test for their theoretical model with respect to a class of old economic phenomena and if they obtain a positive result showing that their theoretical model is indeed a correct causal model for this class of economic phenomena, their theoretical model (and also the causal law derived from it) is then ready to be used to make predictions for the new economic phenomena. That is because the result derived from the prediction test is not only to tell economists whether the implication is consistent with what is exhibited in the new phenomena, but also to guarantee to tell them whether the causal structure from which the old phenomena are derived and the new causal structure from which the new phenomena are derived are homogeneous. That is, if the prediction result shows that the derived implication is consistent with what is shown in the new phenomena, the new causal structure and the old causal structure are homogeneous, which is represented by Route 4 in Figure 2.2. Or if the prediction



To avoid the mistake made in route 2, the predictions test must be postponed until the right causal model is obtained.

Figure 2.1: The Problem of Erroneously Accepting the Wrong Target

test shows an inconsistent result, these two causal structures are heterogeneous. When the latter case does present, the information of heterogeneous structures will be fed back to the economists, triggering another effort to determine a correct causal model that can be used to explain the new phenomena.

Note that the economists' new search mainly consists of the comparison they made between these two causal structures, their consideration of the re-specification (or revision) of their old causal model with respect to the difference between the two structures, and the consultation they made to their original theory (or their original hypothesized complete causal structure) for retrieving other relevant causal knowledge that is already existed in the theory. Why do they keep the apparently "refuted" causal model or "refuted" causal law? Remember that I have argued that economists are committed to causal capacity claims. They won't easily give them up simply because the predictions made from them foundered; they would rather think that these predictive failures come from a structural inconsistency between two different causal structures. It is in this sense that I say the old theoretical model is kept for further theory-development and that the old causal law—i.e., the right target—is not discarded outright. If economists do conduct this kind of practice, they are in fact following a causal structuralist framework of theorizing, which is represented by the causal structuralist branch of Route 3 in Figure 2.2. Also recall from Figure 1.1 that economists' practice of revising their theoretical model constitutes a part of the communication between the theory-testing part and the theory-building part via the channel provided by the information exchange mechanism.

[Please refer to Figure 2.2 on page 52-1]

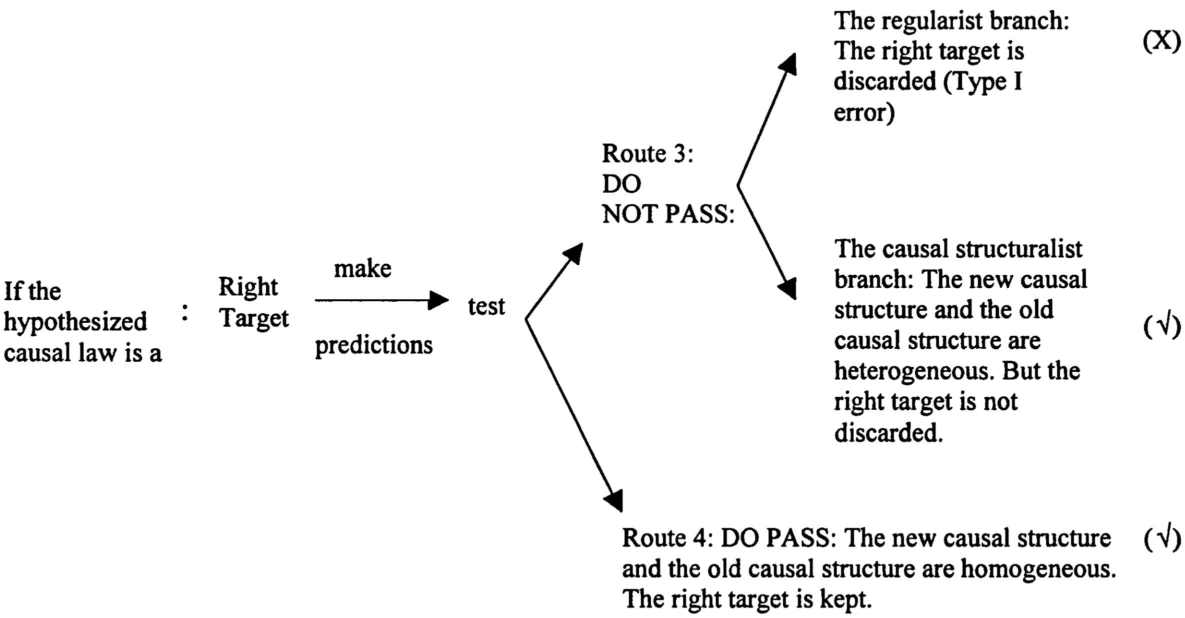


Figure 2.2: Three Possible Conclusions of the Second Round Test of the Right Target

It is obvious that the conclusions obtained in route 2 of Figure 2.1 and in route 3 of the regularist branch of Figure 2.2 can be likened to what statisticians call type II error and type I error, respectively. To see this, refer to Table 2.1. (Larsen and Marx 1986, p. 299)

[Please refer to Table 2.1 on page 53-1]

Let's suppose that the null hypothesis (H_0 in the table) is a hypothesized causal law derived from a theoretical model. Suppose further that this theoretical model is tested by benchmark test and passes it. According to our account, the causal law derived from this model can thus be defined as a right target for a prediction test because the prediction can be used rightly to determine whether there is a structural inconsistency. If we can indeed have a right target, we say that H_0 is true. Therefore, grid A is likened to Route 4 in Figure 2.2, and grid B to the regularist branch of Route 3 in Figure 2.2. Suppose, however, that the theoretical model is not tested by the benchmark test or it does not pass. In that case, the causal law derived from the model is defined as a wrong target for prediction because the prediction made from it may lead economists to make a wrong conclusion. If all we have is a wrong target, we say that H_0 is false. Therefore, grid C is likened to Route 2 in Figure 2.1, and grid D to Route 1 in Figure 2.1. Statisticians do not believe that we can know what the true state of nature is. This disbelief has led to the science of statistical inference and thus to these two types of error. By providing the theory of two-stage tests, causal structuralists seem to assert that it is possible to know what nature is like if we can identify a correct causal model with respect to the phenomenon in question. For causal structuralists, the statisticians' two types of error arise from the suspicious regularist methodological idea.

Table 2.1: Two Types of Error in Hypothesis Testing

		True State of Nature	
		H_0 is true	H_1 is true [or H_0 is false]
Our Decision	Accept H_0	Correct Decision (grid A)	Type II Error (grid C)
	Reject H_0	Type I Error (grid B)	Correct Decision (grid D)

Again, to single out this point is not an attempt to dwell on statistics. Instead, it is an attempt to point out that the causal structuralist is sensitive to causal structure, whereas the regularist tends to ignore it. In testing, regularists tend to overlook the first round test (the model specification test) and go directly to the second round test (the prediction test). It seems that they believe that the second round test can do the job of these two tests at once—i.e., they tend to convert two different tests into one test. Is this conversion successful? It seems not. The reason is that this conversion ends up resulting in both type I error and type II error. In type II error, the regularists bypass the first round test (the causal model test) and go directly to the second round test, and they do not have a chance to make sure that the causal law under test is the right target. The price is that they risk having a wrong target under prediction test. If by a felicitous coincidence, this wrong target passes the test, a type II error arises. Will the situation be better if the regularists get the right target for testing? No—in that case, a type I error may be waiting for them. Given that the regularists believe that a successful economic law should be a regularity law, they do not allow that there is any exception to this regularity. Therefore, if a prediction derived from the economic law, for some *structural reason*, contradicts what is observed in the real world, the regularists will reject the economic law outright. In that case, they commit type I error.

Will the situation improve if we follow the causal structuralist testing practice? It seems so. Because the causal structuralists are concerned about causal structure, they insist on conducting the first round test (the causal model test or model specification test) before going to the second stage. If the first round test is successful, the structuralists will avoid type II error because they have the rightly targeted causal law to put under the second round test. That is, the structuralists can completely rule out the routes suggested in Figure 2.1 and focus only on the routes suggested in Figure 2.2. If the second round test shows that the prediction derived from the rightly targeted causal law is inconsistent with the observation in the real world, they will not discard the rightly targeted causal law outright. Instead, they believe that this discrepancy may come from the differences between the structure specified in their theoretical model and the real situation in the world. Therefore, instead of rejecting their entire theory outright, the structuralists will first respecify or restructure their theoretical model to derive a different causal law that can be used to explain the real observation.

It may seem that the causal structuralists are cautious about not committing a type I error. The motivation for this caution is their belief in causal capacities. The causal structuralists believe that causal capacities should exhibit themselves in the real situation as they do in the theoretical model. If they fail to do so, the causal structuralists, because of their commitment to structural thinking, believe that the failure may be caused by structural reasons and not that there is necessarily a mistake in the idea of capacities.

The causal structuralists' commitment to structural thinking is generally misread by the regularists as the causal structuralists' regularly making ad hoc explanations of anomalous phenomena. It is no wonder that the regularists criticize the causal structuralist approach for its lack of empirical content. Imagine the case that theory A containing causal law B is directly put under the second round test (the predictions test) and the result of the test is negative—i.e., the prediction does not pass the test. For the regularists, whether or not causal law B is the right target, this negative result constitutes a powerful reason to rule out theory A. This conclusion is suggested both in Route 1 of Figure 2.1 and in Route 3 of the regularist branch of Figure 2.2. Given that the test result is negative, the causal structuralist conclusion goes directly to Route 3 of the causal structuralist branch of Figure 2.2, and so the causal structuralists will be busy restructuring the model and producing a new causal law to explain the real-world phenomenon. The regularists will regard these activities as making ad hoc explanations.

It may be true that the structuralists are making ad hoc explanations, but this does not show that the structuralist approach lacks empirical content. Contrary to the regularists' argument, it is in this activity of model restructuring that the structuralists' theories gain empirical content. Admittedly, the first model, being created to express the way that causal capacities operate, must be abstract. A set of various conditions and the ceteris paribus clause must be added to the theoretical model to guarantee that the causal capacities will exhibit themselves. Imagine the case that the capacity claim derived from this highly abstract theoretical model is used to make predictions for the real-world phenomenon and that this prediction founders; how will the structuralists react? According to the argument made so far in this chapter, the structuralists will not rule out the capacity claim; instead, they will consider whether the new situation has made an impact on the causal capacities and so has changed their nature. The structuralists will then restructure their model in an attempt to align the causal structure of their new model with the new situation and thereby to produce a new causal law that can be used to explain the phenomenon. It is obvious that whenever the structuralists restructure their theoretical model by dropping some of its conditions or assumptions, they will at the same time add more phenomenal content to the newly derived causal law. The final conclusion of the entire process can be perceived in this way: Whenever the causal structuralists seek to explain increasingly complicated real phenomena, they remove increasing numbers of conditions or assumptions in the theoretical model, and, in the end, the final causal law will have concrete content that will make it look very unlike the natural law of the traditional concept. The process of restructuring the theoretical model reflects the general picture of economic theorizing mentioned in Chapter 1: The failed predictions obtained from the theory-testing part constitute a piece of information that is fed back via the information exchange mechanism to the theoretical model as a clue to help the structuralists manipulate a rearrangement of this model.

The concretization of economic theories is the main topic of Chapter 4, and we will revisit this topic there. Suffice it to say here that, from this perspective, the causal structuralist approach should not be regarded as lacking empirical content; rather, it should be recognized that this approach provides empirical content to its theories *case by case*. This characterization of this approach may arise from the fact that although the causal structuralists have a holistic view of the causal structure of an economic phenomenon, they do not have a holistic approach to obtain the causal structure. What they have is only the aforementioned analytic piecemeal method to derive the causal structure *step by step*.

In the next section, we discuss Leontief's test of the Heckscher-Ohlin (H-O) theorem and explain how factor-intensity reversal dissolves the Leontief paradox.

2.6 A Case Study of the Causal Model Test (or the First Round Test): The Factor-Intensity Reversal Explanation for the Leontief Paradox

2.6.1 Background: The Heckscher-Ohlin (H-O) Theorem and the Leontief Paradox

The modern theory of international trade has its roots in Eli F. Heckscher's 1919 paper (English translation in 1949) and Bertil Ohlin's 1933 book. One of Heckscher's and Ohlin's main contributions to international trade theory is that they provide the cause of international trade. In the classical international trade theory of the Ricardo-Torrens tradition, a country's comparative advantage in international trade lies in its more efficient labor productivity in producing a certain kind of commodity relative to that of other countries. That is, a country's comparative advantage in producing and exporting a certain kind of commodity is determined by a particular production technology that uses more efficiently the country's labor forces to produce the commodity. But the classical international trade theory does not explain why different countries have different efficiencies in using their labor forces in production. It is modern international trade theory that provides an explanation for this difference.

Contrary to the classical model, which attributes the production-factor solely to human labor, the H-O model maintains that a commodity is not produced solely by labor input; instead, it is produced by a combination of various proportions of various input factors, such as the combination of 8 units of labor, 10 units of capital, 7 units of land, and so on. Different commodities require different proportional combinations of factor-inputs, and different countries have different factor endowments. So if a country is abundant in labor force relative to other countries, it will choose to produce and then to export those commodities whose production uses more units of labor. On the other hand, if a country is abundant in capital relative to other countries, it will produce and export those commodities whose production uses more units of capital. Because the H-O model uses the difference in factor endowment as a cause to explain both the occurrence of international trade and the classical puzzle of why technological differences exist in the use of labor input among different countries, this model is also known as the factor endowment theory of international trade, or the factor proportions theory of comparative advantage.

The H-O theory is in fact a limiting case of the general neoclassical international trade theory. According to the neoclassical theory, international trade is caused by the difference in production technologies, factor endowments, and tastes among countries. The H-O theory, by assuming that the production functions are identical and that tastes are similar among countries and considering only the difference in factor endowment, derives a definite result; it asserts that a country has a comparative advantage in producing and exporting those commodities that use more intensively the country's relatively more abundant factors. This conclusion is the so-called Heckscher-Ohlin (H-O) theorem.

Recall the analogy of the controlled experiment mentioned in section 2.2. What Heckscher and Ohlin have actually done is similar to conducting a controlled experiment. They must know that there are many causal factors that might influence the occurrence and the direction of international trade. But their chief concern is the effect brought about by the difference in factor endowment. In the terminology of section 2.2, the main concern of Heckscher and Ohlin must be the *capacity* of different factor endowments. To obtain the precise knowledge of this capacity, they, like the natural scientists who conduct an idealized experiment in our analogy, must create a disturbance-free environment by ruling out the influences of other possible causal factors. The disturbance-free environment created by Heckscher and Ohlin is their model, which contains a long list of assumptions and the ceteris paribus condition. Needless to say, this model is a highly idealized one. But the result derived from it—i.e., the H-O theorem—shows what the difference in factor endowment is determined to cause under a well-contrived environment, although the derived result may not be readily applicable to explain any concrete economic phenomenon. This lack of applicability seems to be the price that we must pay if we want to gain a precise knowledge of a causal factor's capacity.

As mentioned in section 2.1, the capacity claim asserted in the H-O theorem was seriously challenged by economists after Leontief published the result of his empirical testing against the theorem in 1954. Soon, the Leontief paradox was widely hailed as forceful evidence to refute the capacity claim asserted in the H-O theorem.

Since then, a great amount of empirical and theoretical research has been devoted to explaining the paradox in an attempt either to reconcile the H-O theorem with the paradox or to provide further support to the paradox and to refute the H-O theorem. Among these explanations, 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. Minhas's studies are worth noting not because they provide conclusive results that will allow economists to agree upon the empirical status of the strong factor-intensity assumption in the H-O model, but rather because Minhas's studies were conducted in a way that is congenial with the causal structuralist approach we have discussed, although Minhas's final conclusion does not stay within the line of this approach.

Before we discuss the details of Minhas's studies, we must clarify some economic ideas. Recall that the H-O theorem asserts the following capacity claim: A country has a comparative advantage in producing and exporting the commodity that *uses more intensively* the country's *more abundant* production-factor. Two ideas in the H-O theorem need to be clarified: What does it mean to say that a country is "more abundant" in a certain production-factor? What does it mean to say that a certain commodity is produced by "using more intensively" a certain kind of production-factor? For the convenience of our explication, let's suppose we have a simple 2×2×2 model that contains only two countries (the United States and Britain), two production-factors (capital [K] and labor [L]) and two commodities (steel and cloth). When we say that the United States is more abundant in capital, we do not mean that the absolute amount of available capital in the United States is larger than that in Britain. What we mean is that the United States' overall capital-labor ratio (K/L) is greater than Britain's, or that the autarkic equilibrium wage-rent ratio (w/r) in the United States is also greater than that in Britain because capital is relatively cheaper in the United States than in Britain. As for the meaning of factor-intensity of a certain commodity, we first assume that two production-factors (K and L) are both used in the production of both steel and cloth. When we say that steel is capital-intensive relative to cloth, what we mean is not that the absolute amount of capital being used to produce one unit of steel is greater than that being used to produce one unit of cloth. What we mean, rather, is that the ratio of capital (being used in producing one unit of steel) to labor (being used in producing one unit of steel) is greater than the same ratio for producing one unit of cloth. That is, if $(K/L)_{steel} > (K/L)_{cloth}$, we say that steel is a capital-intensive commodity relative to cloth because, per unit of labor, the production of one unit of steel requires more units of capital than the production of cloth. In a similar fashion, if $(L/K)_{cloth} > (L/K)_{steel}$, we say that cloth is labor-intensive relative to steel.

In the H-O model, the factor-intensity for a certain good is assumed to hold across all wage-rent ratios. That is, even if, say, the price of labor declines—i.e., w/r decreases—there will still be no substantial substitution of labor for capital in the production of both commodities. In other words, even in the face of declining labor prices, steel producers will still use the same capital-labor ratio to produce one unit of steel. Thus, according to this assumption, commodities can be classified and ranked by their factor intensities. In our case, steel is always a capital-intensive good and cloth always a labor-intensive good. This assumption, the so-called strong factor-intensity assumption, is used to rule out the phenomenon of factor-intensity reversal.

What, then, is factor-intensity reversal? In our 2×2×2 model, it means that, because of a change in the factor price, the rate of substitution of the cheaper factor for the more expensive factor in, say, industry 1 is greater than that in industry 2; and this difference in the substitution rate is so substantial that it is sufficient to change the original factor-intensity classification of the commodity produced in industry 1 relative to that of the commodity produced in industry 2. For example, if factor-intensity reversal does happen, say, in the steel industry, it must be that, because the price of labor declines—i.e., w/r decreases—the steel producers, in considering how to reduce their production cost, will substitute the cheaper labor for capital in an amount large enough to reverse the originally capital-intensive steel so that it becomes labor-intensive relative to the cloth industry.

A simple example will help to illustrate this point. Let's suppose that, when w/r = 2, steel producers use 18 units of capital and 6 units of labor to produce one unit of steel. So the capital-labor ratio for producing one unit of steel—i.e., $(K/L)_{steel}$—is 18/6 = 3. At the same wage-rent ratio, cloth producers use 12 units of capital and 9 units of labor to produce one unit of cloth. So, $(K/L)_{cloth} = 12/9 = 4/3$. Obviously, because $(K/L)_{steel} = 3 > 4/3 = (K/L)_{cloth}$, steel is a capital-intensive good and cloth a labor-intensive good. Now suppose that, for some reason, labor becomes cheaper. That is, for example, the wage-rent ratio decreases to, say, w/r = 1/2. At this new wage-rent ratio, steel producers will use 6 units of capital and 18 units of labor to produce one unit of steel. So in this case, $(K/L)_{steel} = 6/18 = 1/3$. At the same wage-rent ratio, cloth producers will use 9 units of capital and 12 units of labor to produce one unit of cloth. So $(K/L)_{cloth} = 9/12 = 3/4$. In this case, because $(K/L)_{steel} = 1/3 < 3/4 = (K/L)_{cloth}$, steel is now a labor-intensive good and cloth is a capital-intensive good, and so factor-intensity reversal occurs. In this example, the rate for substituting cheaper labor for capital in the steel industry is $[\Delta(K/L)/(K/L)]_{steel} = [(K/L)_{after\text{-}substitution\ of\ cheaper\ factor} - (K/L)_{before\text{-}substitution\ of\ cheaper\ factor}/(K/L)_{before\text{-}substitution\ of\ cheaper\ factor}]_{steel} = [(1/3 - 3)/3] = 8/9$. On the other hand, the rate for substituting cheaper labor for capital in the cloth industry is $[\Delta(K/L)/(K/L)]_{cloth} = [(3/4 - 4/3)/(4/3)] = -7/16$. From the comparison of the absolute values of the substitution rates in both industries, we can tell that substituting cheaper labor for capital in the steel industry is much easier than that in the cloth industry. In sum, we can say that, at a high wage-rent ratio—e.g., w/r = 2 (the high-wage situation)—steel is capital-intensive relative to cloth, but, at a low wage-rent ratio—e.g., w/r = 1/2 (the low-wage situation)—steel is labor-intensive.

Now that we have the concept of factor-intensity reversal, let's see how this concept can be fitted into the H-O model to explain the Leontief paradox. Remember that we will *assume* throughout that substituting cheaper labor for capital is much easier in the steel industry than in the cloth industry. Compared with Britain, we know that the United States should be regarded as the more capital-abundant country. So Britain is the more labor-abundant country. Recall how we defined the concept that a country is abundant in a certain production-factor. When we say that the United States is abundant in capital relative to Britain, we mean that the United States' overall capital-labor ratio is greater than Britain's—i.e., $(K/L)_{US} > (K/L)_{UK}$, or that the autarkic equilibrium wage-rent ratio in the United States is greater than that in Britain—i.e., $(w/r)_{US} > (w/r)_{UK}$. It is this latter definition that is relevant for the following discussion: Given that the commodity is no longer fixed to a certain kind of factor-intensity that is classified by the strong factor-intensity assumption, the difference in factor endowment—i.e., the difference in wage-rent ratio—between these two countries will have an impact on the content of the factor-intensity in both industries for one of these two countries. Note that this impact of different factor endowments (or the impact of different wage-rent ratios) on the change of the content of factor-intensity operates through the condition that there is a substantial difference in the rate of cheaper-factor substitution in both industries.

In our case, the United States has a high wage-rent ratio relative to Britain, so the United States is a high-wage country. In this situation, U.S. steel producers will be less likely to substitute labor for capital in the production of steel. So steel will still be a capital-intensive good and cloth a labor-intensive good in the United States, as is predicted by the H-O model with the strong factor-intensity assumption held. In fact, this conclusion is indicated in the summary of our simple example—i.e., in the high-wage situation, steel is a capital-intensive good. On the other hand, because Britain has a low wage-rent ratio relative to the United States, Britain is a low-wage country. Producers in both industries in Britain must be very keen to replace capital with cheaper labor in an effort to reduce their production cost. But given the assumption that substituting cheaper labor is much easier in the steel industry than in the cloth industry, steel will become a labor-intensive good and cloth a capital-intensive good in Britain.

Given that these two countries cannot export the same commodity, suppose that if the United States (a capital-abundant country) exports steel (a capital-intensive good) and Britain (a labor-abundant country) exports cloth (a capital-intensive good), then Britain will present the Leontief paradox. On the other hand, if the United States (a capital-abundant country) exports cloth (a labor-intensive good) and Britain (a labor-abundant country) exports steel (labor-intensive good), then the United States will present the Leontief paradox. That is, given that factor-intensity reversal occurs, at least one country will exhibit the Leontief paradox, and the occurrence of the Leontief paradox is thus explained (see Table 2.2).

[Please refer to Table 2.2 on page 64-1]

2.6.2 Minhas's Studies of the Factor-Intensity Reversal Explanation

Note that when we offer this explanation of the Leontief paradox, we are actually using a reformulated model to explain a phenomenon that the old model cannot explain or predict. That is, by dropping the assumption of the strong factor-intensity, we create a revised version of the H-O model, a version that is obtained by consulting

Table 2.2 Factor Intensity Reversal and the Leontief Paradox

I. The Original Classification of Factor-Intensities of Steel and Cloth: Steel: Capital (K)-intensive good Cloth: Labor (L)-intensive good	I. The Original Classification of Factor-Intensities of Steel and Cloth: Steel: Capital (K)-intensive good Cloth: Labor (L)-intensive good
II. The Factor Intensity Reversal: 1. In low wage situation—i.e., w/r is low 2. ∴ Labor is relatively cheaper Capital is relatively more expensive 3. If steel industry has greater rate of substituting cheaper labor for capital (i.e., to replace labor for capital is much easier in steel industry than in cloth industry) 4. Steel becomes L-intensive good So, conversely, Cloth becomes K-intensive good 5. ∴ Factor Intensity Reversal occurs	II. The Factor Intensity Reversal: 1. In low rent situation—i.e., w/r is high 2. ∴ Labor is relatively more expensive Capital is relatively cheaper 3. If cloth industry has greater rate of substituting cheaper capital for labor (i.e., to replace capital for labor is much easier in cloth industry than in steel industry) 4. Cloth becomes K-intensive good So, conversely, Steel becomes L-intensive good 5. ∴ Factor Intensity Reversal occurs
III. The Factor Endowments of USA and UK: USA: K-abundant ∴ w/r high UK: L-abundant ∴ w/r low	III. The Factor Endowments of USA and UK: USA: K-abundant ∴ w/r high UK: L-abundant ∴ w/r low
IV. The Classification of Factor-Intensities of Steel and Cloth in USA and UK: 1. Steel in USA is K-intensive good Cloth in USA is L-intensive good 2. Steel in UK is L-intensive good Cloth in UK is K-intensive good	IV. The Classification of Factor-Intensities of Steel and Cloth in USA and UK: 1. Steel in USA is L-intensive good Cloth in USA is K-intensive good 2. Steel in UK is K-intensive good Cloth in UK is L-intensive good
V. The Leontief Paradox (L-P): 1. If USA (K) exports steel (K), UK (L) exports cloth (K). UK exhibits L-P. 2. If USA (K) exports cloth (L), UK (L) exports steel (L). USA exhibits L-P.	V. The Leontief Paradox (L-P): 1. If USA (K) exports steel (L), UK (L) exports cloth (L). USA exhibits L-P. 2. If USA (K) exports cloth (K), UK (L) exports steel (K). UK exhibits L-P.

to the original H-O theory (or the original hypothesized complete causal structure) and can then be used to explain or predict the Leontief paradox. To use the terminology mentioned in Chapter 1 in the overview of economic theorizing, we can say that when the result of Leontief's prediction test shows that the H-O theorem founders, this information can be *fed back* to the theory-building part as a clue to help economists manipulate a rearrangement of the theoretical model that can then be used to explain or predict the originally unexplainable or unpredictable phenomenon. This means that we now have a new causal model. But do we have a new causal model test—i.e., a new first round test—to test this new causal model? Minhas's studies (1962, 1963) provide an excellent illustration of this kind of test.

As we have seen from the discussion of our 2×2×2 case, the cause that triggers factor-intensity reversal between industries is the difference in the degree of the ease of substituting a cheaper factor in both industries; this difference must be substantial enough to reverse the original factor intensities of the commodities produced by both industries. So if we want to test whether the revised H-O model, with the assumption of strong factor-intensity removed, can be used as a correct causal model to explain the Leontief paradox presented *in a certain class of economic data*, it seems that we must show that factor-intensity reversal is *prevalent* in this class of economic data. Part of Minhas's ingenuity is that he knows how to show this prevalence.

In Minhas's method, if we want to know whether factor-intensity reversal is prevalent, we must know whether the phenomenon of a very strong difference in the degree of the ease of factor-substitution is prevalent in the production of different commodities. To show this prevalence, we must show that, in any two industries, it is a normal case that the change in the capital-labor ratio—i.e., $[\Delta(K/L)/(K/L)] = [(K/L)_{\text{after-substitution of cheaper factor}} - (K/L)_{\text{before-substitution of cheaper factor}}] / [(K/L)_{\text{before-substitution of cheaper}}$

$_{factor}$]—in response to a change in the wage-rent ratio—i.e., [Δ(w/r)/(w/r)] = [(w/r)$_{new}$ - (w/r)$_{old}$] / [(w/r)$_{old}$]—in an industry is much greater than that in another industry. More specifically, we must show that *the elasticity of factor-substitution* in the first industry is much greater than that in the second industry. If we express this specific idea in a general formulation, it looks like this: $\{[\Delta(K/L)/(K/L)]/[\Delta(w/r)/(w/r)]\}_{industry\ 1} > \{[\Delta(K/L)/(K/L)]/[\Delta(w/r)/(w/r)]\}_{industry\ 2}$.

However, to tell whether the elasticity of factor-substitution is substantially different from one industry to another, we must first have an adequate production function that can be used to single out this feature. The production functions normally used by economists before 1961 were almost exclusively the fixed-coefficient and the Cobb-Douglas production functions. These production functions cannot do the job to single out the feature of the strong difference in the degree of factor-substitution among industries, however, because their algebraic forms allow them to have only the elasticities of zero and unit, respectively, for the fixed-coefficient and the Cobb-Douglas production functions. What is ingenious in Minhas's studies is that a new production function—originally proposed in a 1961 paper co-authored by K. Arrow, H. B. Chenery, B. S. Minhas, and R. M. Solow and christened the "Constant-Elasticity-of-Substitution production function," or the CES production function—is used to single out the feature of "inter-industry differences in the relative ease or difficulty with which factor inputs can substitute for each other in production." (Minhas 1963, p. 30)

Let's look how the CES production function does the job. The algebraic form of the CES production function for industry *i* is (Ibid., pp. 32-5) as follows:

$$V_i = f(K, L) = [A_i K^{-\beta i} + \alpha_i L^{-\beta i}]^{-1/\beta i} \quad (2.1)$$

where V_i, K, and L are all variables. V_i stands for the quantity of commodity *i* produced in industry *i*, K represents the amount of capital and L the amount of labor used for producing the aforementioned quantity of commodity *i*. α, β, and A are parameters determined by the technology—i.e., the combination of production-factors K and L—used to produce the aforementioned quantity of commodity i in industry i. That same formula and the definitions of the terms can also be applied to express the CES production function in industry j.

If we differentiate (2.1) with respect to K and L, we have the real rate of the rent of capital, r, and the real wage rate, w:

$$r = \partial V/\partial K = A_i\,(V/K)^{\beta i+1} \tag{2.2}$$

$$w = \partial V/\partial L = \alpha_i\,(V/L)^{\beta i+1} \tag{2.3}$$

From (2.2) and (2.3), we have the expression of the wage-rent ratio:

$$w/r = (\partial V/\partial L)/(\partial V/\partial K) = (\alpha/A_i)(K/L)^{\beta i+1} \tag{2.4}$$

By rearranging (2.4), we have the expression of the capital-labor ratio in industry *i*. We set this capital-wage ratio equal to x_i:

$$x_i = (K/L)_i = (A/\alpha_i)^{1/\beta i+1}(w/r)^{1/\beta i+1} \text{ where } 1/\beta_i + 1 = \sigma_i \tag{2.5}$$

Similarly, we also obtain the capital-labor ratio in industry j:

$$x_j = (K/L)_j = (A/\alpha_j)^{1/\beta j+1}(w/r)^{1/\beta j+1} \text{ where } 1/\beta_j + 1 = \sigma_j \tag{2.6}$$

Dividing (2.5) by (2.6), we have the ratio of relative capital intensity of industry i and j:

$$x/x_j = J\,(w/r)^{\sigma i-\sigma j} \qquad \text{where } J = (A/\alpha_i)^{\sigma i}/(A/\alpha_j)^{\sigma j} \tag{2.7}$$

Under the assumption of perfect competition, both industries i and j will face the same factor prices. So w/r will be the same for both industries. It is this conclusion of (2.7) that distinguishes the CES production function from the traditional fixed-coefficients and Cobb-Douglas production functions. Formula (2.7) is a general form of the ratio of relative capital intensity between any two industries in that both the fixed-coefficients and the Cobb-Douglas functions are to be regarded as simply two special cases of the CES function. When $\sigma_i = \sigma_j = 1$, the CES function reduces to the Cobb-Douglas function. When $\sigma_i = \sigma_j = 0$, the CES production function reduces to the fixed-coefficient production function.

Note that whenever $\sigma_i = \sigma_j$ occurs, it means that the relative factor-intensity of *i* and *j* (x_i/x_j) is independent of the wage-rent ratio (*w/r*). When this condition is obtained, we say that the assumption of so-called strong factor-intensity is fulfilled. That is, the factor-intensity of *i* and *j* is completely insensitive to the change in factor prices. But when $\sigma_i \neq \sigma_j$, it is possible that "at some critical value of (*w/r*), the relative factor-intensities of *i* and *j* will be reversed." So, Minhas concluded, in this case, "it is impossible to characterize industries as 'capital-intensive' or 'labor-intensive' irrespective of (*w/r*)." (Ibid., p. 34)

To get an idea of what the critical value of *w/r* is, let's return to equation (2.5) and (2.6) and take logarithmic forms for both equations:

$$\log x_i = \sigma_i \log (A/\alpha_i) + \sigma_i \log (w/r) \qquad (2.8)$$

$$\log x_j = \sigma_j \log (A/\alpha_j) + \sigma_j \log (w/r) \qquad (2.9)$$

Then we can solve this system of equations for the critical value of *w/r* where the capital-labor ratio is identical for both industries—i.e., $x_i = x_j$ or $(K/L)_i = (K/L)_j$. The following equation gives the solution of the critical value of *w/r* at which $x_i = x_j$:

$$\log (w/r) = -1/\sigma_i - \sigma_j [\sigma_i \log (A/\alpha_i) - \sigma_j \log (A/\alpha_j)] \qquad (2.10)$$

The graph in Figure 2.3 illustrates the critical value of w/r and its corresponding value of capital intensity by plotting the lines for equation (2.8) and (2.9) to cross each other at point C on a plane with the axis of log (*w/r*) and the axis of log *x* or log(*K/L*).

[Please refer to Figure 2.3 on page 68-1]

In general, this graph reveals that, as long as the constant slope of line log x_i is

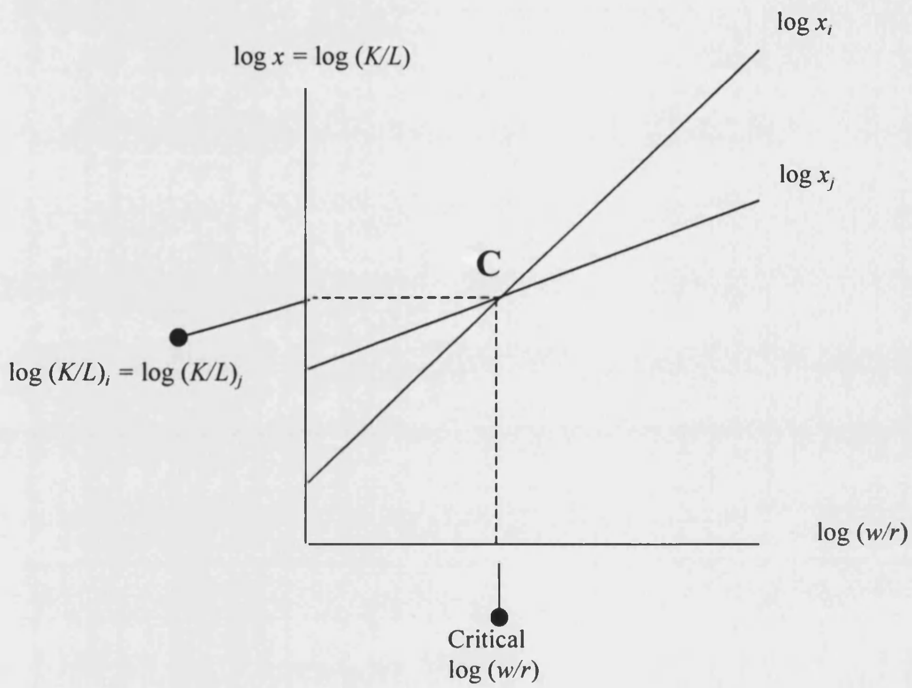


Figure 2.3: The Critical Point C

different from that of line log x_j—i.e., as long as $\sigma_i \neq \sigma_j$—these two lines will have a crossover point C. For our example, where $\sigma_i > \sigma_j$, this graph also shows that the originally labor-intensive industry *i*, because of its greater elasticity of factor substitution (σ_i), will turn into a capital-intensive industry in the region that is to the right of the critical point C. Conversely, the originally capital-intensive industry *j* will thus become a labor-intensive industry. In other words, whenever the two lines of the logarithmic forms of the capital-labor ratios for any two targeted industries cross each other, a factor-intensity reversal occurs between the two industries. The main purpose of Minhas's studies was to try to show that this occasion of crossover is prevalent among industries.

By using the data of 24 industries from 19 countries in the time period of 1948 (or 1949)-1958 to estimate the values of the parameter σ, Minhas indeed found that the crossover condition is prevalent among one-third of the cases he studied. This conclusion, according to the causal structuralist view of economic theorizing, should suggest that the revised H-O model, within which strong factor-intensity is dropped, is indeed a correct causal model for explicating the Leontief paradox shown in the Minhas data set. But according to Minhas's reading, this conclusion of prevalent factor-intensity reversal among industries should be regarded as further evidence to support the idea that the Leontief paradox is a prevalent phenomenon among countries; for this reason, the H-O theory should be ruled out because it "does not seem to possess the degree of generality in application that has often been claimed for it." (Ibid., p. 53)

2.6.3 Is Minhas a Causal Structuralist?

If this is indeed what Minhas read from the conclusion of his studies, we must regretfully admit that Minhas was still a regularist, although he conducted a wonderful test that can be regarded as a classical example of what I have called the first round test. Minhas seemed to suggest that the idea of a theory's degree of generality in application is a *fixed* idea that should come with the birth of the theory. Minhas's suggestion reveals that his underlying idea is still committed to the regularist idea of natural law: A good theory should contain a universally true regularity that can be applied to explain the concrete phenomena in the world. If a theory does not possess this kind of law, the theory should be regarded as incorrect and should be ruled out. A new theory containing a new regularity should be attempted. Minhas's underlying regularist commitment can be illustrated by his attempt to replace the capacity claim asserted in the H-O theorem with the new causal mechanism found in factor-intensity reversal: When Minhas found that factor-intensity reversal is prevalent among industries, he suggested that we should regard the mechanism connecting factor-intensity reversal with the effect it produces as a wider regular association, as compared to the scope of the capacity claim asserted in the H-O theorem, which exhibits itself more prevalently among the industries of interest. Thus, it seems that Minhas suggested that the mechanism maintained in factor-intensity reversal should *take over* the position that was originally occupied by the capacity claim suggested in the H-O theorem. But, this suggestion misses a portion of the complete causal structure of our case.

This point is illustrated in the causal structure graph in Figure 2.4.

[Please refer to Figure 2.4 on page 70-1]

Figure 2.4 is the correct causal structure that the revised H-O model (the H-O model with the strong factor-intensity assumption dropped) should represent in order to make a complete explanation of the Leontief paradox in the case of factor-intensity

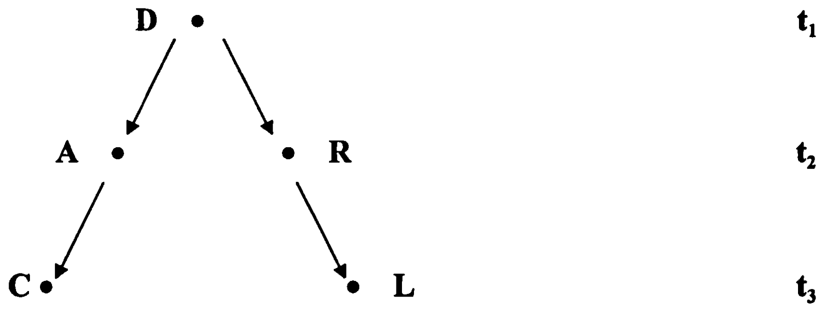


Figure 2.4: Dual Capacity of the Abundance in Capital (D)

reversal. The complete causal story should look like this: When the elasticities of factor-substitution between two industries are very different, a factor-intensity reversal must occur in the industry that has the greater elasticity of factor-substitution. A country's relative abundance in a certain factor will give it a comparative cost advantage in the production of the commodity that uses more intensively the country's more abundant factor. If the country's less advantaged industry happens to be the industry that has a greater elasticity of factor-substitution and if this country exports the commodities produced in the more advantaged industry, the country will exhibit the Leontief paradox.

This complete causal structure in fact records the story of a causal factor that has a dual capacity. If we assume that a country is relatively more abundant in capital, then D in Figure 2.4 represents the abundance in capital. From the content of the complete causal story depicted here, it is obvious that D has a dual capacity. On the one hand, it has the capacity to produce the comparative cost advantage in producing capital-intensive goods (A). On the other hand, if the less advantaged industry (in this case, the industry using the labor-intensive technology in production) happens to be the industry having greater ease in substituting cheaper capital in its production, D will at the same time also trigger factor-intensity reversal (R). The effect that follows from the comparative cost advantage will result in the country exporting the capital-intensive good (C). However, the effect that follows from the factor-intensity reversal will result in the country exporting the labor-intensive good (L) and thus exhibiting the Leontief paradox. The letters t_1, t_2, and t_3 in Figure 2.4 denote the time order in sequence.

The purpose of bringing out this complete causal structure is to point out that Minhas's suggested position rules out the left-hand side causal path and sticks to the right causal path. That is, the right-hand side causal path, which is newly discovered in the study of factor-intensity reversal, is supposed to cover a wider range of regularities among events and is then supposed to take over the place in international trade theory that was originally occupied by the left-hand side causal path—the causal path suggested in the H-O theorem.

But causal structuralist thought in economic theorizing takes a different approach. *No take-over is involved.* Instead, both paths are constituent parts of a complete causal story. Consider why we need to propose the theory of two-stage tests in section 2.5. The answer is that we think that the causal model test is different from the prediction test. But what is the difference? The causal model test places a causal model under a test against a situation that is similar to the stipulated situation of the causal model. The result of the causal model test is decisive. The prediction test places a causal model under a test against a situation that is very different from the situation specified in the causal model. A negative result derived from the prediction test is less decisive, but it does provide information to be fed back to the original causal model to trigger another round of model restructuring and then another round of causal model test. This *piecemeal* picture is the point here.

When what is asserted in the H-O theorem founders in a prediction test such as Leontief's test, a model restructuring is called for in an attempt to try to capture the correct causal model that will explain what is unexplainable by the H-O model. The correct causal path is added to the original causal picture (which is the left-hand side causal path) at the same time that the strong factor-intensity assumption is dropped from the original H-O model; this is an attempt to form a more complete causal picture to represent the correct causal structure underlying the originally unexplainable economic phenomenon. In turn, the newly restructured model is put under a first round test (such as Minhas's test) to see whether it really represents the correct causal structure underlying the originally unexplainable economic phenomenon.

Here, no new regularity replaces the old regularity; there is only a more complete causal story versus an incomplete causal story. There might be two laws simultaneously governing different regions of the plane in Figure 2.3. The region to the left of the critical point C is governed by the "law" stated in the H-O theorem, which is represented by the left-hand side causal path in Figure 2.4. The region to the right of the critical point C is governed by the "law" of factor-intensity reversal, which is represented by the right-hand side causal path in Figure 2.4. But each of them is only a part of a complete causal story about the dual capacity of the abundance in capital.

Note that the complete causal story that should be contained in the H-O theory is not to be built at the same time as the birth of this theory. Instead, the complete causal story is to be obtained by a piecemeal method—step by step and case by case—that is *relative to* the complete causal structure that is to be realized by the phenomenon in question. Therefore, the baseline seems to be that there is no grand theory, so there is no grand test. Every theory must be tested by a first round test, and every first round test must be designed in a precise way, such as what Minhas did in his test, so that it tests the theory decisively. All we have is the piecemeal method, and this method is to be used to construct the piecemeal theory, which is in turn to be tested by the piecemeal test.

Chapter 3

An Outline of Economic Explanation

3.1 Introduction

In the previous two chapters, I have pointed out that regularist methodologists adopt a misconceived methodology for appraising the practice of economists when the latter formulate economic explanations. I have argued that when economists make economic explanations, they do not usually follow the explanatory scheme laid out in the D-N model; instead, they follow what I call the causal structuralist explanatory scheme of theorizing, which is illustrated in Figure 1.1, to construct an explanation for the phenomenon in question.

The point of this critique is to try to single out a crucial issue in the study of economic explanation: What is the nature of the relation between the explanans and the explanandum in an economic explanation? By using the D-N explanatory model, the regularists seem to think that an explanandum should be regarded as an instance that is to be expected from or subsumed under a universal regularity law (the main explanan in a D-N model) that includes the targeted explanandum as one case among the associations covered by the law. So, for the regularists, an explanatory relation should be regarded as a relation of subsumption that reveals the unity of an underlying regularity. For the causal structuralists, however, an explanatory relation is not only a causal relation, but also it is, more specifically, a causal relation that is to be considered under a specific causal structure. So for the causal structuralists, the main component in their explanation is no longer a universal regularity law but rather is a causal model containing a hypothesized causal network of a number of hypothesized causal factors that can be used to produce or derive the explanandum in question. So even if the causal structuralists acknowledge that what is derived from their causal model can be used as a regularity law to explain an explanandum, this regularity law does not possess the feature of unification, which has been much vaunted by the regularists as the main characteristic of a regularity law. The reason is that the regularity law derived from the causal model is thought to be applicable only for explaining a specific case in a specific causal structure. For a different case in a different causal structure, there will be another causal law that can be generated from a different causal model. From this perspective, when a methodologist criticizes economists' practice because their theories fail to include the broadest economic covering laws, the methodologist may be barking up the wrong tree.

The relationship between explanans and explanandum in an explanation has recently been reexamined by James Woodward (1997). He writes that this relationship should be regarded as a causal relation rather than simply as a regular association. For Woodward, the traditional D-N account of explanation exaggerates the importance of the presence in the explanans of a natural law acting as a universal generalization. According to the D-N account, a natural law is a universal generalization that includes the explanandum in question as one of its expected phenomena. Therefore, except for minor help from the other auxiliary conditions in the explanans, the explanatory relationship between the explanans and the explanandum of interest is established mainly by the presence of the natural law.

For Woodward, however, a natural law's universal coverage is not the only condition for a successful explanation. Instead, he maintains, the presence of an invariant counterfactual dependence between the explanans and the explanandum of interest also makes a good explanation. Suppose that we can show that the explanandum of interest would change in a systematic way *if* the explanans *were to* change by various hypothetical or actual interventions in various ways. In that case, according to Woodward, we could then use this counterfactual information to determine that a causal relation exists between the explanans and the explanandum of interest and thus could use this causal relation as an explanatory relation to make a good explanation. For Woodward, this invariant counterfactual dependence need not be universally held in order to qualify as a plausible explanatory relation that can be used to make explanations. All that is required is that the counterfactual dependence hold invariant over some specified class of interventions.

Implicit in Woodward's theory of explanation is that Woodward tends to regard a causal relation as an invariant relation. This tendency can also be found in other writings in modern econometrics. These writings also discuss the topic of whether the concept of causation in the structuralist context, but not in the associationist context, can be represented by some other, more manageable concept that can be more easily identified by using modern econometric techniques. In contrast to the idea of Granger causality, which is pertinent to the associationist's idea of causation, an idea has emerged in the modern econometric literature that is most pertinent to the idea of causation in the structuralist sense. That idea, superexogeneity, is selected as the most pertinent candidate to represent the structural concept of causation because its definition contains the idea of invariance. The latter is reminiscent of the idea of autonomy, which was discussed by Trygve Haavelmo in his 1944 treatise on the probability approach in econometrics and recently has been the subject of much discussion among philosophers and economists, such as James Woodward and Kevin Hoover. As we have seen earlier, invariance involves manipulation or intervention—an idea that is applied by some philosophers, including Woodward, to explicate the idea of causation and explanation.

In the remaining sections of this chapter, I examine how the ideas of manipulation, invariance, superexogeneity, and causal structure are interwoven and argue that the concept of causation cannot be reduced to that of invariance. The conclusion derived in this chapter serves as additional support for the proposition that the concept of a causal law is a structural concept that cannot be reduced to any other concept, including the concepts of a regularity law and that of invariance. Consequently, the practice of making economic explanation cannot be regarded as a kind of D-N explanatory practice involving activities such as subsuming the economic phenomenon in question under a regularity law or checking whether the regularity law included in a theory can cover the economic phenomenon in question. Nor can it be regarded as simply conducting a test for the stability of a causal system. Rather, the making of economic explanation should be regarded as a continuous process of ascertaining the correct causal model, which represents the causal structure in question, to generate the correct causal law, which we can then use to explain the economic phenomenon in question. In short, in this chapter, I attempt further to show that causal explanation in economics is a structural concept.

3.2 Manipulation, Invariance, Superexogeneity, and Causal Structure

3.2.1 The Manipulability Theory of Causation

In a 1955 paper, Douglas Gasking maintained that a causal relation is a "means-end" relation or, to use his term, a "producing-by-means-of" relation. In a nutshell, Gasking's manipulative theory of causation is as follows: C causes E in cases in which we can, with the aid of a certain kind of general manipulative technique, produce an antecedent occurrence of kind (or sort) C as a means to bring about a subsequent occurrence of kind (or sort) E. What is new in Gasking's account is that a causal relation is defined in terms of human manipulation. Gasking maintained that the notion of causation depends on the notion of manipulation and that cause-effect relations can be explicated, directly or indirectly, in terms of means-end relations that can be initiated by human manipulation.

Woodward (1993, 1995, 1997, 2000) follows Gasking but refines this causal idea by adding a condition of invariance. As with Gasking, Woodward agrees that a relation, if it is to be regarded as having causal and explanatory import, must be explicated in terms of manipulation. Woodward further suggests that, for a relation R between C and E to count as being causal and explanatory, relation R must be invariant under the manipulation of C. That is, the manipulated change in C should bring about the change in E in the way stated in R. Otherwise, C does not cause E in the way stated in R and perhaps does not cause E at all. Clearly, for Woodward, a causal relation should be a relation that is exploitable by manipulation for the purposes of control. Woodward's account seems to imply that a relation R will express a causal relation only if R is invariant over a range of interventions. The implied danger here is that it is easy to confuse the causal relation with the invariant relation and, even worse, to equate the causation with the invariance or to reduce the causation to the invariance.

A more plausible position for treating the relation between causation and invariance is to think that a causal relation may have the characteristic of invariance in some cases but not in all cases. In other words, invariance can be taken as a sign for the existence of causation, but it does not equate with causation. An intuitive example of this position is the relation between the level of prices and the supply of money. Although a government's decision to increase the supply of money (that is, a manipulated change in the money supply) does not always prove to be effective in stimulating the economy (and thus increasing the price level), it rarely affects our opinion that the increase in the money supply is the main cause of the increase in price levels. That is, even though the relation between money and prices is not invariant, it is still believed that a causal path exists between them.

3.2.2 The Idea of Weak Exogeneity

The debate as to whether the characteristic of invariance is a necessary and sufficient condition for a relation to be regarded as a causal relation can be further illustrated by the recent discussion of superexogeneity in econometrics. Before we discuss superexogeneity, however, we must first talk about the idea of weak exogeneity. My discussion here follows that of Robert F. Engle, David F. Hendry and Jean-François Richard (1983, reprinted in Ericsson and Irons (eds.) 1994, pp. 39-70).

We know that, for each value of y within the range of variable Y, the conditional probability function of Y given X=x can be written as: $f(y|x) = f(x,y)/f(x)$. We can rewrite the formula as

$$f(x,y) = f(y|x)f(x) \qquad (3.1)$$

where x and y are values or realizations of two continuous random variables X and Y, respectively.

We want to decide *whether variable X is weakly exogenous for Y with respect to the parameters of interest*, which we represent by the vector of Θ. Now, suppose we use time series data to estimate and test a model. The joint probability function of Y and X can be represented as the following joint probability density function:

$$f(Y_t, X_t;\ \Psi) \qquad (3.2)$$

Suppose that we can divide the vector of parameters into Ψ_1 and Ψ_2— that is, $\Psi = (\Psi_1,\ \Psi_2)$, such that:

$$f(Y_t, X_t;\ \Psi) = f(Y_t|X_t;\ \Psi_1)\, f(X_t;\ \Psi_2) \qquad (3.3)$$

X_t then is *weakly exogenous for Y_t with respect to the parameters of interest* Θ if:

(1) Θ is a function of Ψ_1 only; that is, $\Theta = f(\Psi_1)$, or we can say that Θ can be recovered from Ψ_1 (the parameters of the conditional density function) only, and

(2) Ψ_1 and Ψ_2 are variation-free, that is, there are no cross-restrictions between Ψ_1 and Ψ_2. (Engle, Hendry, and Richard 1983, reprinted in Ericsson and Irons (eds.) 1994, p. 45, and Hoover 2001, p. 176)

The concept of weak exogeneity is related mainly to the problem of estimation and testing of the parameters of interest. For our example, the fulfillment of these two conditions implies that the estimation and testing of Θ can be efficiently conducted by referring only to the information contained in the density function of Y_t conditional on X_t but with no need to refer to the marginal density function of X_t, because the marginal function, under the weak exogeneity condition, is supposed to contain no relevant information for estimating and testing for the parameters of interest.

3.2.3 The Idea of Invariance and Its Relation to the Idea of Superexogeneity

In practice, decision makers such as government officials are not content with an efficient and unbiased estimation and testing of the parameters (including the coefficients of each variable and the error variance) of the model of interest. Generally, they are more interested in identifying variables that can be used safely as instruments for governmental intervention in certain economic activities. Therefore, in an economic model, economists are urged to concern themselves with determining the kinds of characteristics that can be extracted from the variables of interest to show that these variables can be reliably used for policy analysis.

For a general definition of superexogeneity, recall our formula (3.3): $f(Y_t, X_t; \Psi) = f(Y_t|X_t; \Psi_1)\, f(X_t; \Psi_2)$. X_t can be regarded as being superexogenous for Y_t

and Θ (the parameters of interest) if:

(1) X_t is weakly exogenous for Y_t with respect to Θ, and

(2) Ψ_1 is invariant to changes in the marginal density function of X_t (including the changes in X_t itself and in Ψ_2). (Engle et al. 1983, reprinted in Ericsson and Irons (eds.) 1994, p. 47, and Hoover 2001, p. 178)

The second condition for superexogeneity is the main theme that the manipulability theorists of causation apply most often to establish their argument. For example, Woodward, as we have mentioned, maintains that causality is fundamentally about the invariance of a relation between variables under some interventions. If there is a causality between the supply of money stock and the price level, then the relation between these two variables should hold under the government intervention of increasing the money supply. By taking a view of causality from this perspective, the discovery of a causation between variables can thus be regarded as identifying the superexogeneity between variables—i.e., as identifying the invariant relation between variables.

3.2.4 Can We Equate a Causal Relation with an Invariant Relation?

One concern about the manipulability theory of causation is that it seems to suggest that a causal structure can thus equate with an invariant structure of relevant variables or with a structure that has superexogeneity among variables of interest. Hoover, in his discussion about causality in macroeconomics (Hoover 2001), criticizes this line of thinking. He maintains that a causal structure cannot be reduced to an invariant structure.

Hoover agrees with manipulability theorists in that the fulfillment of the superexogeneity condition for a model does show that there exists a causality between variables of interest in this model. What Hoover criticizes is that, when the model fails to show that there is superexogeneity between variables of interest, the manipulability theorists too easily give up the assumption of the existence of a causal structure for the class of variables in the model. For Hoover, even when the superexogeneity condition (or the invariance condition) is violated, the causal structure might still exist. Therefore, he seems to suggest that superexogeneity (or invariance) in a model can only be an indication for a causal structure and that the notion of superexogeneity (or the notion of invariance) can never replace the idea of causation.

To illustrate his point, Hoover provides the example of the relation between the level of money stock and the price level (Ibid., pp. 64-66 and pp. 178-190). The following illustrations of the equations are heavily borrowed from Hoover's example. Let the general demand for money be represented as

$$m_t - p_t = \delta + \beta y_t - \alpha(r_t + ({}_tp_{t+1}{}^e - p_t)) + \nu_t, \quad (3.4)$$

where the subscript t indexes time, m represents the nominal money stock, p is the general price level, y represents the real income, r is the real interest rate, ${}_tp_{t+1}{}^e$ is the expectation at time t of the price level at time t+1—so that $({}_tp_{t+1}{}^e - p_t)$ represents the expected inflation rate between times t and t+1—and ν is an independent random error term. All variables are in natural logarithms, and $\alpha, \beta, \delta \geqq 0$. Our aim is to find out whether there is a causal relation between m_t and p_t, so we will designate y_t and r_t as background variables so that y_t and r_t are held to some constant values. Let's set $y_t = y^*$, $r_t = r^*$. If we use this assumption, equation (3.4) can be rearranged in the following form:

$$m_t - p_t = \mu - \alpha({}_tp_{t+1}{}^e - p_t) + \nu_t \quad (3.5)$$

where $\mu = \delta + \beta y^* - \alpha r^*$.

Let's assume that the central bank adopts the following rule for determining the level of the money supply:

$$m_{t+1} = \lambda + m_t + \varepsilon_t \qquad (3.6)$$

Three assumptions are given by Hoover for these two equations: (a) ε's are random error terms that are independent of one another, as are ν's, (b) ε_t and ν_t are serially uncorrelated, and (c) $E(\varepsilon_t) = E(\nu_t) = 0$. The parameter λ represents the rate of growth of the money supply and is regarded as a variable whose value is subject to direct control by the central bank.

Assume that the expectation for the future price level is rationally formed in the following way:

$${}_tp_{t+1}^e = E(p_{t+1} | \Xi_t) \qquad (3.7)$$

where Ξ_t represents information available at time t. An important assumption is made concerning the expected inflation rate by equating it with the rate of growth of the money supply:

$$({}_tp_{t+1}^e - p_t) = \lambda \qquad (3.8)$$

Substituting equation (3.8) into (3.5), we have

$$p_t = m_t - \mu + \alpha\lambda - \nu_t \qquad (3.9)$$

Hoover conjectures that equation (3.9) is the solution for the price level in this model. To verify his conjecture, Hoover suggests the use of a simple check to see whether "the mathematical expectation of the actual rate of inflation based on information available at time t" (Ibid., p. 65) is equal to the rate of growth of the money supply (λ). By using equation (3.9), rearranged equation (3.6) ($m_{t+1} - m_t = \lambda + \varepsilon_t$), and assumption (c): $E(\varepsilon_t) = E(\nu_t) = 0$, we have the following derivation:

$$\begin{aligned} E(p_{t+1} - p_t | \Xi_t) &= E[(m_{t+1} - \mu + \alpha\lambda - \nu_{t+1}) - (m_t - \mu + \alpha\lambda - \nu_t) | \Xi_t] \\ &= E(m_{t+1} - m_t - \nu_{t+1} + \nu_t | \Xi_t) \\ &= E(\lambda + \varepsilon_t - \nu_{t+1} + \nu_t | \Xi_t) \\ &= \lambda \qquad (3.10) \end{aligned}$$

By doing this, Hoover shows that the simultaneous system comprising equations (3.6) and (3.9) is a *consistent* model under the assumption of rational expectation because the expected value of the actual inflation rate (E $(p_{t+1} - p_t \mid \Xi_t)$) equates with the growth rate of the money supply (λ). Moreover, by combining equation (3.6) ($m_{t+1} = \lambda + m_t + \varepsilon_t$) and equation (3.9) ($p_t = m_t - \mu + \alpha\lambda - \nu_t$), we can "form a causal structure in which money causes prices in a non-linear manner." (Ibid., p. 65)

One point to notice here is that Hoover's conclusion—that the model containing equation (3.6) and (3.9) forms a causal structure within which money causes prices—is based on the assumption that the central bank is able to use money supply as an instrument to influence the level of prices in a way stated in equation (3.6). And by assuming this, Hoover, at the same time, also accepts the causal assumption that money causes prices.

An important feature of the causal structure is that the parameter λ appears in both equation (3.6) and equation (3.9) of this model. This fact causes a problem when the central bank tries to use monetary policy to fine-tune the economy. To put it simply, the problem is that the central bank's attempt to stabilize prices at a certain level cannot be successful because whenever the stock level of the money supply (m_t), which is used as a policy instrument, is changed through the direct manipulation of the growth rate of the money stock (λ), the price level (p_t) will also deviate, in a way not expected by the central bank, from the targeted price level. According to the famous Lucas critique of econometric policy evaluation, the main contributing cause of the deviation from the targeted price level is the impact of people's expectation concerning the effect of the policy variables.

The problem can be viewed from a different angle—i.e., from the perspective of exogeneity of a variable for another variable with respect to the parameter of interest. Recall equation (3.3): $f(Y_t, X_t;\ \Psi) = f(Y_t|X_t;\ \Psi_1)\, f(X_t;\ \Psi_2)$. For our analysis of the problem of rational expectation, equation (3.3) can be rewritten as $f(p_t, m_t;\ \Psi) =$

$f(p_t|m_t;\ \Psi_1)\ f(m_t;\ \Psi_2)$. Let's denote the vector of the parameters of the structural equations (equations (3.6) and (3.9)) as $\Phi = [\alpha\ \ \mu\ \ \lambda\ \ \sigma^2_\nu\ \ \sigma^2_\varepsilon]$. Recall that we have assumed in equation (3.7) that people's expectation for future price levels is formed rationally in the following way: ${}_tp_{t+1}{}^e = E\ (p_{t+1}|\Xi_t)$. If we conduct a process of reparameterization ($\Psi = f(\Phi)$) and then partition Ψ into two separate vectors (Ψ_1, Ψ_2), we can decompose the original joint density function into the following two functions: First, the conditional density function $f(p_t|m_t;\ \Psi_1)$ represents the same information as equation (3.9) does but possesses reparameterized vector $\Psi_1 = [\Pi_0\ \ \Pi_1\ \ \sigma_\omega{}^2]$: $p_t = \Pi_0 + \Pi_1 m_t + \omega_t$, where $\Pi_0 = \alpha\lambda - \mu$, $\Pi_1 = 1$, and $\omega_t = -\nu_t$ (we denote this reparameterized equation as (3.9')). Second, the marginal density function $f(m_t;\ \Psi_2)$ represents the same information as equation (3.6) does but possesses reparameterized vector $\Psi_2 = [\Omega_0\ \ \Omega_1\ \ \sigma_\xi{}^2]$: $m_t = \Omega_0 + \Omega_1 m_{t-1} + \xi_t$, where $\Omega_0 = \lambda$, $\Omega_1 = 1$, and $\xi_t = \varepsilon_t$ (we denote this reparameterized equation as (3.6')). The joint probability density function $f(Y_t,\ X_t;\ \Psi)$ represents the entire reparameterized system, as contrasted with the structural equations (3.6) and (3.9), which are presumed to lay out the causal relations. In this example, one particular case concerning the problem of the invariance account of causality is that when there is a change in $\Psi_2 = [\Omega_0\ \ \Omega_1\ \ \sigma_\xi{}^2]$ of the marginal density function, there will also be a change in $\Psi_1 = [\Pi_0\ \ \Pi_1\ \ \sigma_\omega{}^2]$ of the conditional density function.

Our example also allows us to interpret the problem of the lack of superexogeneity of a variable for another variable with respect to a parameter of interest. In our example, the central bank thinks that it can use the money supply to control the price level. Its confidence is based on the following two beliefs: First, it believes that it possesses the formula of the money supply—i.e., equation (3.6): $m_{t+1} = \lambda + m_t + \varepsilon_t$—which can be used to determine the level of the money supply (m_t) by manipulating the growth rate of the money stock (λ). Second, the central bank believes that the price level of the economy (p_t) is determined in a way similar to that stipulated in equation (3.5) ($m_t - p_t = \mu - \alpha({}_tp_{t+1}^e - p_t) + \nu_t$) but that people's expectation concerning the future price level plays no role in the mechanism of determining the price level; that is, it is assumed that, in equation (3.5), either ${}_tp_{t+1}^e = p_t$ or $\alpha = 0$. But contrary to the central bank's belief, in fact people's expectation does matter here: Whenever the central bank tries to reach a certain price level (p_t) by changing the stock level of the money supply (m_t) through the direct manipulation of the growth rate of the money stock (λ), the price level deviates, in a way not expected by the central bank, from the targeted price level. And, according to the famous Lucas critique, this deviation is induced by the impact of people's expectation concerning the effect of the monetary policy. We can illustrate the impact of people's expectation by pointing to a fact in our reparameterized system: that there is a simultaneous appearance of λ in both equation (3.6') and equation (3.9') and that, in equation (3.9'), the parameter $\Pi_0=\alpha\lambda-\mu$ is a function of λ. But the central bank ignores this fact when it makes the monetary policy decision. As a result, the government pays the price of being puzzled by the inefficacy of its monetary policy. The upshot is that, as Hoover points out, the central bank "cannot successfully follow the strategy of policy evaluation [based on the estimated structural system—i.e., the estimated equations of (3.6) and (3.9)] outlined above, since every change in monetary policy (that is, every change in λ) would alter the Π_is [the parameters of interest in the conditional equations of the reparameterized system], causing the true reduced-form equation to shift." (Ibid., p. 184)

Now let's turn to the examination of superexogeneity. In the aforementioned model of the demand for money with rational expectations, the impact of people's rational expectation on the determination of the price level is reflected in the violation of the invariance condition of superexogeneity for m_t to p_t *with respect to* the parameters of interest. In other words, the exogeneity of a variable for another variable is *relative to* the parameters of interest. Recall that when we transform our structural system into its corresponding reparameterized system we conduct a process of reparameterization: the parameter vector Ψ of the reparameterized system is derived from the parameter vector Φ of the structural system through the reparameterizing process of $\Psi = f(\Phi)$. The *relativity* of the exogeneity of a variable for another variable arises from the reparameterization, because a different set of parameters (such as the set of Ψ and Φ in our example) with respect to the same set of variables (such as the set of variables of (p_t, m_t)) will bring about a different condition of exogeneity for the variable (m_t in our example) under exogeneity examination.

According to the preceding analysis of the relativity of exogeneity, we can examine two cases in our reparameterized model of the money demand with rational expectation. Recall that our reparameterized model comprises equations (3.6') and (3.9'). We can represent this model as $f(p_t, m_t; \Psi) = f(p_t|m_t; \Psi_1)\, f(m_t; \Psi_2)$, where $\Psi_1 = [\ \Pi_0\ \Pi_1\ \sigma_\omega^{\ 2}]$ and $\Psi_2 = [\Omega_0\ \Omega_1\ \sigma_\xi^{\ 2}]$. Consider the following two cases.

(Case 1): If the vector of parameters of interest is $\Theta = [\Pi_0\ \Pi_1]$, then m_t would not even be weakly exogenous for p_t with respect to Θ because, although it satisfies the first condition stipulated in the definition of weak exogeneity, Θ can be recovered from Ψ_1 only—i.e., $\Theta = f(\Psi_1)$; it, however, fails to satisfy the second condition: There is a cross-restriction between Ψ_1 and Ψ_2 because there is a functional relation between Π_0 and Ω_0—i.e., $\Pi_0 = \alpha\lambda - \mu$. Neither is m_t weakly exogenous for p_t with respect to the central bank's intervention—i.e., the policy parameter λ—because there are cross-restrictions between Ψ_1 and Ψ_2.

In addition, given Hoover's assumption that the relations between the parameters of the marginal distribution of m_t ($f(m_t)$) and the parameters of the conditional distribution of p_t on m_t ($f(p_t|m_t)$) stay fixed, a change in $f(m_t)$ via a change in λ will change $f(p_t|m_t)$. The failure of the condition of invariant parameters, given Hoover's assumption, implies that the causal relation from m_t to p_t will be disrupted whenever there is an intervention such as the central bank's intervention (λ). Hoover's assumption plays an important role in his argument for non-superexogeneity in this case. The details are explicated in the next section.

(Case 2): If the vector of parameters of interest is $\Theta = [\alpha \quad \mu \quad \lambda]$—i.e., part of the parameters in the structural equation (3.6) and (3.9)—then m_t is not even a weakly exogenous variable for p_t relative to Θ, because Θ cannot be recovered from $\Psi_1 = [\Pi_0 \; \Pi_1 \; \sigma_{\omega}^2]$ only (that is, $\Theta \neq f(\Psi_1)$). And, as with case 1, because m_t fails to be weakly exogenous for p_t and Θ, m_t is, according to the hierarchy of exogeneity, surely not superexogenous for p_t and Θ with respect to the policy parameter λ.

According to the result of our examination of the superexogeneity in case 2, a manipulability theorist of causation, such as Woodward, might conclude that there is no causal relation between m_t and p_t. Woodward's main reason would be that, given Hoover's assumption that the relations between the parameters of $f(m_t)$ and the parameters of $f(p_t|m_t)$ stay fixed, the hypothesized causal relation from m_t to p_t is not robust under the central bank's intervention of manipulating the growth rate of the money stock (λ); this failure of robustness is reflected in the violation of the condition of invariant parameters.

Hoover maintains, however, that the manipulability theorist's conclusion disregards the fact that there is *a causal structure*, which is behind *the causal relation* from m_t to p_t. This causal structure is represented by the structural system comprising equation (3.6) ($m_{t+1} = \lambda + m_t + \varepsilon_t$) and equation (3.9) ($p_t = m_t - \mu + \alpha\lambda - \nu_t$), although a cross-restraint term— λ —appears in both equations of this causal structure, rendering the policy implications unstable. In Hoover's theory, despite the appearance of the instability of this structure, we can posit this causal structure, comprising equations (3.6) and (3.9), as a correct causal structure showing that the expectation of the actual rate of inflation given the information available at time t (that is, $E(p_{t+1} - p_t \mid \Xi_t)$) is equal to the growth rate of the money stock (λ), as we have shown in derivation (3.10), which backs this up to some small extent by showing that the causal assumptions are consistent. If this is the case, then the existence of the causal structure cannot be ruled out simply by the negative result of an invariance test showing that there is no invariant relation between m_t and p_t with respect to an intervention on λ.

According to this line of thinking, Hoover seems to suggest that the higher-order relation between a causal structure and the causal relation is, in a general sense, that the causal structure provides *a platform* on which the causal factors act or interact with each other so that the interaction among the factors produces various causal relations. But there is no guarantee that each action or interaction of the causal factors will succeed in generating a stable causal connection, because other background conditions (assumptions) must be fulfilled. In our example, this means that the factor m_t reserves the potentiality to exert on or to influence the factor p_t, but the success of the influence from m_t to p_t depends on whether the other features of the entire environment in which these two factors exist are suitable for the generation of the hypothesized causal relation. Therefore, according to this idea, satisfying the definition of superexogeneity so that there is an invariant relation between m_t and p_t (in this case, the relation in question is the conditional density $f(p_t|m_t)$) can indeed indicate that there is a successful exertion of influence between m_t and p_t. But failure to satisfy the definition of superexogeneity cannot rule out the possibility of the existence of the causal structure; it can indicate only that the way that the causal factors of interest connect or interact with each other has changed to the extent that it violates the invariance condition stipulated in the definition of superexogeneity.

Recall that Hoover provides a general model of rational expectations—i.e., the structural system comprising equation (3.6) and (3.9)—from which we can explain how p_t (the price level) is determined and ascertain its level. Also, recall our earlier exposition of Hoover's discussion of the case of *money demand with rational expectations of the effect of the governmental intervention* (Ibid., pp. 180-182): According to the result of non-superexogeneity, Hoover agrees with the manipulability theorists that there is no invariant relation between m_t and p_t, and therefore he agrees that there may be no stable causal connection from m_t to p_t. Furthermore, in a case of *money demand with extrapolative expectations of the price levels* (Ibid., pp. 178-180), which we do not cover in this chapter, Hoover shows that it is possible that *the fulfillment of a condition of an invariant relation* for superexogeneity happens to coincide with *the success of a causal connection* between m_t and p_t. In this case, a positive result states that there is an invariant or robust relation between m_t and p_t because m_t is superexogenous for p_t with respect to the intervention on λ; therefore, as suggested by Hoover, the manipulability theorists would be happy to confirm that there is a causal relation from m_t to p_t and then acknowledge that there exists a causal structure.

But what distinguishes Hoover from the manipulability theorists is as follows: Even when, in the case of money demand with rational expectations for the effect of policy parameter λ, the condition of invariant relation between m_t and p_t is violated with respect to interventions via λ, Hoover still maintains that it is the causal relation (which is represented by the causal structure) that determines the influence of

m_t on p_t, even though the relation that defines the influence is not invariant, and, consequently, the causal relation cannot be supposed to be an invariant relation. With respect to this point, Hoover writes (Ibid., pp. 181-182),

> This [inconsistency between the causal relation defined in the causal structure and the non-invariant relation that fails to be superexogenous] clearly distinguishes causal structure from invariance or robustness.... Causal structure governs the transmission of influence. A salient fact about one-way causal order is that influences do not transmit backwards against the arrow of causality. Invariance and robustness are relevant to causal order, but they do not define it. Rather it is the causal order that determines just what the invariance relationships are among variables... even though we reject basing a definition of causality upon them, invariance, robustness and super-exogeneity also are sources of relevant information, which must be interpreted in relationship to causal structures.

3.3 The Methodological Import of the Distinction between Causation and Invariance

The importance of making a distinction between causation and invariance can be further illustrated from a different perspective. Recall that when I discussed Hoover's examination of superexogeneity in case 1, I said that Hoover's assumption of the fixed relation between parameters of $f(m_t)$ and $f(p_t|m_t)$ is crucial. Let's examine why it is important. Let's first ask, How did Hoover draw his conclusions of non-superexogeneity for the two cases mentioned in the previous section? Surely, he made his conclusions based on his examination of superexogeneity in these two cases. Recall that, in Hoover's examination of superexogeneity in the two cases of the reparameterized model containing equation (3.6') and (3.9'), he concluded that m_t failed to be superexogenous for p_t with respect to various sets of parameters of interest.

But what we must note is that the reason for m_t's failure to be superexogenous for p_t with respect to parameters of interest in case 1 is not that the second requirement of superexogeneity—i.e., the requirement of invariant parameters, which is stated in section 2.2.3—is violated; it is, rather, because there is a cross-restriction between Ω_0 (a parameter of marginal density $f(m_t)$, where $\Omega_0 = \lambda$) and Π_0 (a parameter of conditional density $f(p_t|m_t)$, where $\Pi_0 = \alpha\lambda - \mu$). In other words, m_t fails to be superexogenous for p_t because one of the two requirements of weak exogeneity—i.e., the requirement of no cross-restriction between parameters—is violated. A similar situation happens in case 2. Here, m_t fails to be superexogenous for p_t with respect to parameters of interest (Θ) because another requirement of weak exogeneity—i.e., Θ should be a function of parameters of conditional density $f(p_t|m_t)$—is violated.

From this perspective, it seems that Hoover's two cases exhibit lack of weak exogeneity but cannot be guaranteed to exhibit (or not to exhibit) lack of superexogeneity otherwise. So our concern is that, to ensure that Hoover's conclusions about superexogeneity are definite, an additional assumption regarding causal structure is needed. Let's use case 1 for illustration. The idea is this: Let's use the joint probability density $f(p_t, m_t; \Psi)$, where $\Psi = [\Omega_0\ \Omega_1\ \Pi_0.\ \Pi_1\ \sigma_\xi^2\ \sigma_\omega^2]$, to represent the probability information revealed from the complete causal structure stated in the system containing equation (3.6') ($m_t = \Omega_0 + \Omega_1 m_{t-1} + \xi_t$) and (3.9') ($p_t = \Pi_0 + \Pi_1 m_t + \omega_t$). As we have seen in case 1, this joint probability density function has the characteristic that when we write it as $f(p_t|m_t; \Psi_1)f(m_t; \Psi_2)$, where $\Psi_1 = [\Pi_0\ \Pi_1\ \sigma_\omega^2]$ and $\Psi_2 = [\Omega_0\ \Omega_1\ \sigma_\xi^2]$, it turns out that Ψ_1 and Ψ_2 are functionally related—i.e., $\Pi_0 = \alpha\,\Omega_0 - \mu$.

Normally, what we mean when we say that we have interventions on m_t is that we have actually replaced the old joint density $f(p_t, m_t; \Psi)$ with the new joint density $f'(p_t, m_t; \Psi')$, where the old marginal density $f(m_t; \Psi_2) \neq$ the new marginal density $f'(m_t;\ \Psi'_2)$. But the crucial question for superexogeneity is this: Will the old conditional density $f(p_t|m_t;\ \Psi_1)$ = the new conditional density $f'(p_t|m_t;\ \Psi'_1)$? If Hoover wants his conclusions of non-superexogeneity in case 1 to be valid, the two things must not be equal. But what Hoover has shown in case 1 is merely that the condition of weak exogeneity is violated and not that the condition of invariant parameters fails. So how can he guarantee that $f'(p_t|m_t;\ \Psi'_1) \neq f(p_t|m_t;\ \Psi_1)$? To do this, he needs an additional assumption regarding the causal structure in question. This additional assumption is that the functional relation between the parameters of $f(m_t)$ and the parameters of $f(p_t|m_t)$ stays fixed as we move from $f(p_t, m_t;\ \Psi)$ to $f'(p_t, m_t;\ \Psi')$ or as we do interventions on $f(m_t)$. This assumption is required because otherwise a situation may arise showing that, after we do intervention on m_t, nature changes the functional relation between Ψ_1 and Ψ_2 in a way that makes this relation disappear and allows $f'(p_t|m_t;\ \Psi'_1)$ to equal $f(p_t|m_t;\ \Psi_1)$; as a result, there is no longer a violation of the condition of weak exogeneity, and the condition of invariant parameters is fulfilled. Therefore, m_t is superexogenous for p_t with respect to Ψ'_1, which contradicts what Hoover concludes in case 1. Therefore, for Hoover, to ensure that his conclusion of non-superexogeneity in case 1 is valid, he must hold this assumption throughout in his inference.

Hoover's assumption of invariant functional relation among parameters across envisaged change in the marginal density $f(m_t)$ is a kind of specification of *a causal structure* from which his non-superexogeneity conclusion is guaranteed to be derived. If, however, after an intervention on m_t, it turns out that there arises *a new causal structure* within which the functional relation among parameters no longer exists, it means that Hoover's assumption fails; as a result, there is no reason to rule out the possibility that the new causal structure may be arranged in a way that will allow the probabilities generated from it to exhibit that the new conditional density $f'(p_t|m_t;\ \Psi'_1)$

is the same as the old conditional density $f(p_t|m_t; \Psi_1)$, and this means that they have the same (or invariant) parameters. So the lesson learned from Hoover's case is that (the knowledge of) the causal relation (or causal structure) is more fundamental than (the knowledge of) the invariant relation (or the probability) between variables with respect to an intervention. The latter is, in a sense, a kind of special product of the former, a product that is generated only when the environment (or the causal structure) that contains the causal factors is free from any cross-restrictions between the factors of interest.

From this perspective, the Lucas critique of the inefficiency of policy evaluation is not surprising. Economists consistently face the challenge that the real economic situation is always far more complicated than the situation that they have posited in their economic theories and that various causal factors are frequently omitted from economists' models. When these omitted causal factors, however, eventually become the causal factors that systematically influence a certain class of economic phenomena, economists' theories, if they are not consistently revised by adopting these additional important causal factors into their models, will surely be insufficient to provide good explanations of the phenomena. In Lucas's case, people's expectations vary systematically with changes in policy. So if economists do not also systematically change the putative causal structure specified in their economic models by accommodating the current condition of people's expectation, the economic theories very likely cannot provide good economic models that can be used to conduct efficient evaluations of different economic policies. But this comment should not be interpreted as reflecting a concern that economists' theories cannot provide economic models containing stable parameters that can be used for such evaluation. Instead, it should be regarded as reflecting a concern that economists' models are still not good enough to describe a correct picture of the causal structure that underlies the phenomena in question.

What economists face is an ordinary causal question: Are people's expectations an important relevant causal factor to be included in the posited causal structures of economists' models? If the answer is yes, then economists should re-specify their models by accommodating this additional factor. The point here is that when the concern of obtaining correct causal structures can be settled, the concern of obtaining the intended probabilistic result—i.e., good statistical models containing invariant parameters—can also be settled.

As we have seen in Hoover's case, if what he assumed is exactly the way that the real causal structure of the targeted phenomenon operates, his conclusions of non-superexogeneity are guaranteed. But if his assumption is incongruent with the operation of the real causal structure, his conclusions are vulnerable; he must replace the old causal assumption with a better one in order to ensure that his new invariance test is conducted under a right causal context. The same argument applies in Lucas's case. If people's expectations turn out to be an important omitted causal factor and if economists indeed incorporate this factor into their models, their models will then possess a more complete causal structure. This will put them in a better position to assume that their models contain invariant parameters, which can in turn be used to conduct efficient policy evaluation. In any case, it is obvious that causal thinking (whether the posited causal structure is a correct picture of the real causal structure) comes first, and probabilistic inference (whether this model is an invariant-parameters model that can be used to conduct an efficient policy evaluation) follows—but not the other way around.

Furthermore, because the real economic world is constantly changing, there is no way that we can expect that we can someday discover an economic model that expresses the ultimate truth and can accommodate any kind of causal structure, and therefore can be used at any time in any situation to explain or predict the economic phenomenon in question. Every economic phenomenon constitutes its own causal structure, and so every economic model intended to be used to explain the phenomenon should also be regarded as a tailor-made individual model in order to capture the real causal structure underlying the economic phenomenon. Therefore, economic explanation or economic theorizing is a never-ending practice that continues to exist as long as there are various constantly unfolding causal processes in the economic world.

The everlasting nature of these processes can be illustrated by an explanatory model of theory development provided by Werner Diederich in his 1998 paper. (Diederich 1998, pp. 206-214, in D. Anapolitanos, A. Baltas, S. Tsinorema (eds) 1998) This paper discusses the relation between models and reality, on the one hand, and the process of theory-development, on the other hand. Diederich's main concern is how we can legitimately and realistically interpret scientific concepts and theories. In a nutshell, his idea is that, from the perspective of the history of the development of scientific theories, we can indeed interpret our scientific constructs realistically if we can stand at a "higher" position in the process of this development. Let's examine how Diederich's idea can help to resolve our main concern regarding how the idea of constantly unfolding causal processes shapes the methodology of economic theorizing.

Diederich supposes that the picture of the relation between theory and reality initially conceived by adherents of logical empiricism consists of a correspondence relation between the terms used in the theory and certain empirical findings in reality. This oversimplified picture is revised to accommodate the fact that not all theoretical terms can be linked directly to reality. For these unlinkable theoretical terms, logical empiricists think that they must somehow be reduced to observational (or empirical) terms—e.g., by using some bridge principles. Therefore, all scientific terms will finally be connected to some observable parts of reality. The revised logical empiricist idea is illustrated in Figure 3.1. (Ibid., p. 207)

[Please refer to Figure 3.1 on page 97-1]

The logical empiricist reductionist approach is used to contrast with the semantic or the model-theoretic approach. In the model-theoretic approach, all theoretical terms refer to the objects created in models, and the models are linked to reality by some kind of isomorphism (represented in Figure 3.2 by "~"). (Ibid., p. 208) But, as pointed out by Diederich, if Figure 3.2 is all that is provided by the model-theoretic approach, it may raise a doubt that it won't do any better than the reductionist approach to explain the relation between theory and reality because the model-theoretic approach merely uses the model to sidestep the question.

[Please refer to Figure 3.2 on page 97-2]

The concern about the redundancy of the model-theoretic approach can be settled once we distinguish the empirical and theoretical parts in a model and allow only the empirical part to be linked to reality by some kind of isomorphism. This is because, in this context, a model has the distinctive role of functioning as a surrogate of the theory (containing both the theoretical and the empirical parts of the theory) to connect to reality via its empirical part. The question of whether there are real objects corresponding to the theoretical part of the model and whether the total structure of the model represents a real structure is left open. This image can thus be used to explain what it means to say that "a theory explains." This image is

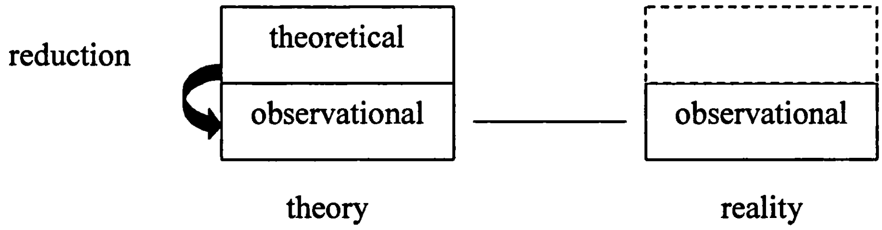


Figure 3.1: The Relation between Theory and Reality: A Logical Empiricist View

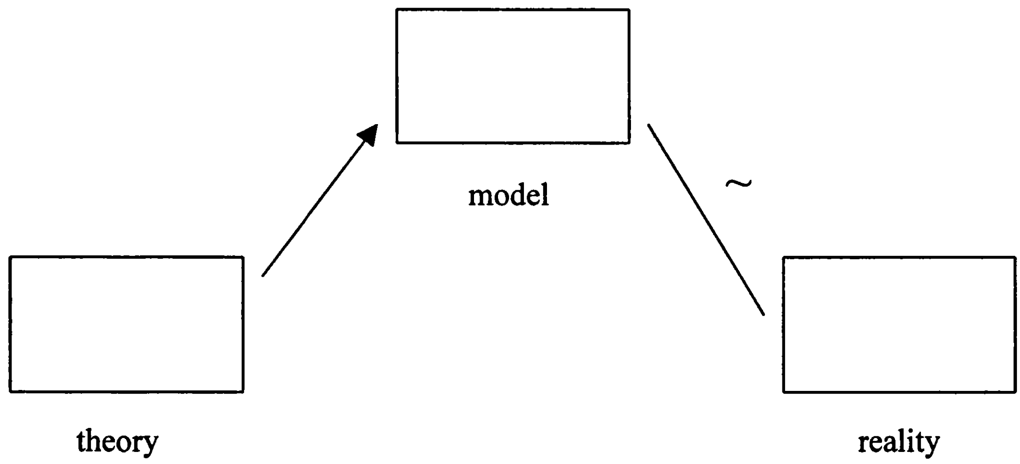


Figure 3.2: The Relation between Theory and Reality: A Model-Theoretic Approach

illustrated in Figure 3.3. (Ibid., 209)

[Please refer to Figure 3.3 on page 98-1]

We can apply this image to reconsider our discussion of the relation between an economic theory and its models in Chapter 1. I said that a theory provides a hypothesized complete causal structure. The causal structure is dubbed "hypothesized" because it is provided in the hope of capturing the real causal structure but is not necessarily consistent with it. It is also dubbed "complete" because economists suppose that they have included in their theory all the most salient causal factors and causal relations. So, in all, an economic theory consists of two parts. On the one hand, the theory captures *some main features* of a certain class of economic phenomena (this constitutes the so-called "empirical" part of an economic theory); on the other hand, the theory *is supposed to represent* a complete causal structure (this constitutes what I shall call the "theoretical" part of an economic theory). So, as to the problem of what it means to say that "a theory explains," the answer may be that "a theory explains" if it explains a certain concrete phenomenon by using its hypothesized complete causal structure, which is supposed to represent the real causal structure underlying the concrete phenomenon.

A model of the theory also possesses these two parts but uses only the empirical part to link to the real world. A model has two roles to play when it bridges the gap between the theory and the real world. The first role is to represent the theory in a more precise way, such as converting it into a formal model or a mathematical model that can be more easily applied for explanation. This part of the model's role is represented by the left-hand connection in Figure 3.3. The theory is used to construct a full (or complete) model, which contains a sub-model that plays a second

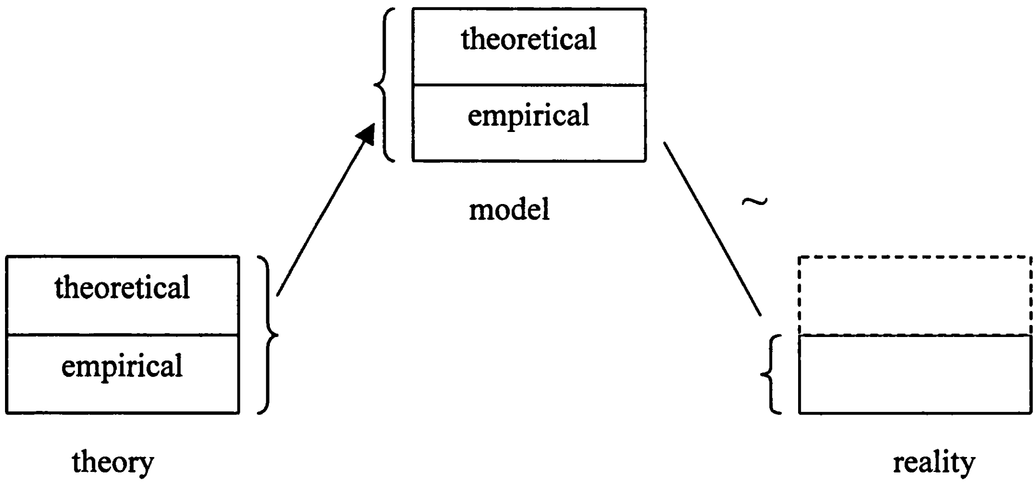


Figure 3.3: The Relation between Theory and Reality: A Revised Model-Theoretic Approach

role and corresponds to the empirical objects in reality. One example of a full model (recall our causal structuralist explanatory scheme in Figure 1.1) is the theoretical model that has been established in the model-manipulation process I (MMP-I) for the model-specification test. The second role a model plays is to model reality via its sub-model to provide a link to reality. In Figure 3.3, this part of the role is represented by the right-hand connection between the model's empirical part and reality. An example of such a sub-model is an empirical model such as Leontief's input-output model, which can be found in Figure 1.1 at MMP-II and is set up for the prediction test. The main function of an empirical model is to relate the full model to reality but not to the theory.

But according to the causal structuralist idea of economic theorization, an account such as the one illustrated in Figure 3.3 still does not accommodate the phenomenon of theory revision or extension, which is initiated by the finding of the original theory's failure to contain sufficient causal information in its hypothesized complete causal structure. In other words, the picture provided in Figure 3.3 seems to ignore the prevalent phenomenon of the dynamic side of theory-development, which works by the busy information exchange between theory-building practice and theory-testing practice as illustrated in Figure 1.1. This problem is also the main concern of Diederich's paper, so he may be of help here.

Diederich's idea is this: First, he suggests that we replace the absolute dichotomy of the "theoretical" and "empirical" parts with the distinction of "theoretical" and "non-theoretical" components of a theory, which are relative to the theory itself. Diederich points out that this distinction and the idea of relativity were originally suggested by Joseph D. Sneed in the discussion of his own structuralist approach. (Sneed, 1971, second ed., revised, 1979) Second, Diederich wants us to conceive a picture of the development of science in which a more advanced theory (or a higher theory) is built "on top of" another, less advanced theory (or a lower theory). Diederich calls this practice a "theoretization." This idea is illustrated in Figure 3.4.

[Please refer to Figure 3.4 on page 100-1]

Like Sneed, Diederich agrees that the main purpose of theorizing is to try to use the non-theoretical part of a theory to explain some non-theoretical objects in reality. What, then, is the use of a theory's theoretical component? As mentioned before, the theoretical component is used to construct a full (or complete) model that contains a sub-model that can be used to connect the complete model to non-theoretical objects in reality. Now, by using the image that a higher theory is built on top of a lower theory, we can say that the more advanced theory (or the higher theory) tries to use its non-theoretical part to explain non-theoretical objects in reality. But from the perspective of the less advanced theory (or the lower theory), what is used by the higher theory—i.e., its non-theoretical part—to explain non-theoretical objects in reality is exactly what is counted as the theoretical part of the lower theory. From this perspective, the aim of conducting a theoretization—i.e., building a more advanced theory on top of a less advanced theory—is to try to obtain a new layer of reality, which is supposed to remain as a layer of the theoretical part in the lower theory. So, as suggested by Diederich, from a *historical point of view*, "to build a theory 'on top of' another one is to presuppose (the existence of) the theoretical entities of the 'lower' theory." (Ibid., p. 211) In this sense, the theoretical part of the highest theory is regarded as tentative, because when a still higher theory is built, the theoretical part of the former highest theory becomes the non-theoretical part of the new highest theory. Therefore, according to Diederich, scientific concepts and theories developed before the existing highest scientific concepts and theories can all

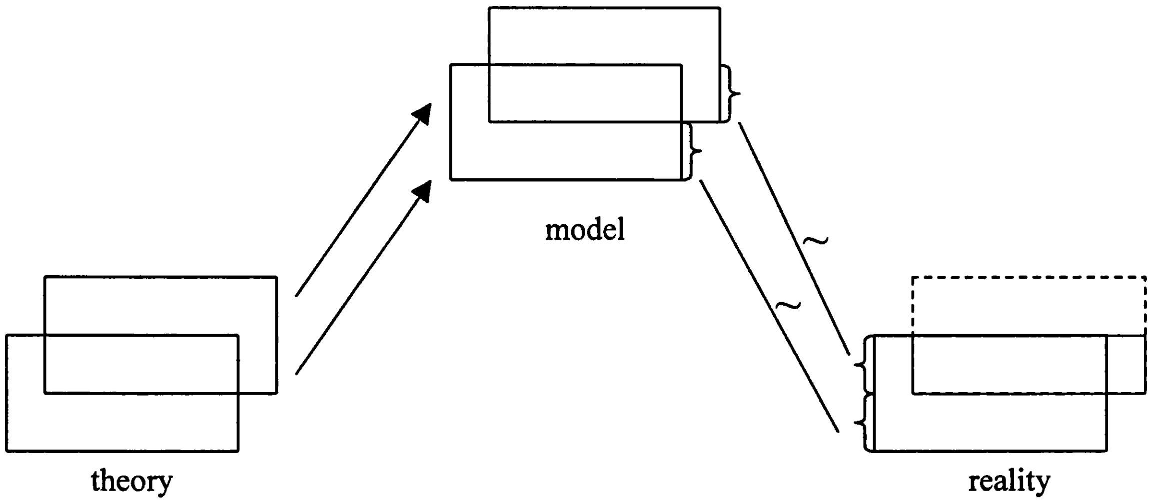


Figure 3.4: The Relation between Theory and Reality: The Sneedian Structuralist Approach

be interpreted realistically if we put them in a historical context.

This has been a long digression from my main topic. My goal is to use Diederich's idea to show that economic theorizing follows the same course. Economists establish their theory by combining in it their empirical studies of the real course of a certain economic phenomenon (the non-theoretical part) and their general theoretical construct of the causal structure underlying this phenomenon (the theoretical part). If economists eventually find that their theory cannot explain another class of concrete phenomena, they tend to suppose that some parts of their theoretical construct (or their posited causal structure) must be incongruent with the current situation. Rather than give up their original theoretical construct, they tinker with it, adding to it more relevant causal information obtained from the new empirical studies (these studies will constitute the non-theoretical part of a forthcoming new theory). After revising their theory, they construct a new complete causal structure (the new theory's theoretical part) in their new theory, and they use this new structure to explain the new phenomena. This same procedure continues as long as new causal facts are recovered from the real world (from the constantly unfolding causal processes), calling for revision of existing theories.

An important question is, How can we say that the new theory—i.e., the revised theory—is better than the old one? Diederich's explanatory model is helpful in pointing us in the general direction of the answer to this question. Recall Diederich's argument that the aim of conducting a theoretization is to try to obtain a new layer of reality that is supposed to remain as a layer of the theoretical part of the less advanced theory. In our case, what is the content of this new layer of reality? It is the new relevant causal information collected from new empirical studies! Note that, from the perspective of the old theory, the *total knowledge of the non-theoretical part of reality* increases after a new theory containing a more complete causal structure (or a more complete causal model) is built on top of it. We can analyze the content of this increase as follows.

What we must note is that the old putative causal structure contained in the old theory *is not replaced* by the new putative causal structure constructed in the new theory. On the contrary, the new putative causal structure is established by using both new causal information collected in new empirical studies and old causal information contained in the old putative causal structure. When I said that economists *do not give up* their original theoretical construct but instead *tinker* with it, I meant that (1) the old putative causal structure is *kept* in a way that the fundamental causal information (such as the causal capacities of certain factors), which is borne out by the new empirical studies, remains a part of the non-theoretical part of the new theory, and (2) what has changed is that the old causal network is *revised* to accommodate *the additional causal information* collected from the same empirical studies. The result of this tinkering is the new putative causal structure, which is the main content of the new theory. Although the new "theory" inevitably contains a new theoretical component (the new putative causal structure), it now possesses old causal information and new causal information collected from the new empirical studies; the new theory is in this sense said to be able to provide more total knowledge of the non-theoretical part of reality. (Chapter 4 provides more discussion regarding the relevance of abstract theoretical claims to the real world.)

A simple example illustrates my point. If people's expectations indeed turn out to be an important causal factor in determining the effect of a government's monetary policy, then economists should reconsider adding the influence of this causal factor to the causal structure posited in the traditional monetary theory and then rearranging the old causal structure to capture the current economic structure and thereby make the old causal structure more complete. According to our earlier discussion, because the revised monetary theory contains a more complete causal structure, it is thus said to be able to provide more total knowledge of the current economic structure (i.e., more total knowledge of the non-theoretical part of reality).

3.4 Conclusion

This chapter starts with the question, What is the nature of the relation between the explanans and the explanandum in an economic explanation? It ends by concluding that the idea of an explanatory relation is a causal structuralist idea, and this means that the explanandum is to be explained under a causal structure—i.e., under a specific arrangement of causal relations. Thus, to make an economic explanation is not simply to show that the economic phenomenon in question is an instance of a universal regularity law. A regularity law can be of help in making an economic explanation, but the law itself is to be generated from a causal structure. In short, a regularity law is a net result of the operation of various causal powers under a causal structure. Neither should the practice of making an economic explanation be regarded as a practice of discovering an invariant relation among relevant variables. An invariant relation is a probabilistic property that is a special result produced from a causal structure—i.e., a special result that is guaranteed only when the causal structure that contains the causal powers is free from disturbances so that it can produce a stable net result of operation among the causal powers. For causal structuralists, to make an economic explanation is to formulate a theory containing a causal model consisting both of economists' empirical findings of causal information (the non-theoretical or empirical component of a causal model) and their theoretical construct regarding the general form of the real causal structure in question (the theoretical component).

The causal structuralist idea of economic explanation, however, raises a question: If making an economic explanation inevitably involves formulating a theoretical construct, doesn't it mean that every economic explanation is nothing but a practice of theorizing, and so the upshot is that economics is not an empirical science but an abstract science? No. As we have seen in Diederich's model of theory-development, every stage of theorizing is just like building a higher theory on top of a lower theory; this means that the theoretical part of the lower theory is used with its non-theoretical (or empirical) part to set up a full (or complete) causal model, and this full causal model in turn is used to extract more empirical causal information from the next stage of theorizing. From the perspective of the higher theory, the theoretical part of the lower theory is thus not a defect but rather is a *lever* or *vehicle* that can be used by economists to help themselves to move the lower theory to a higher level in the next stage of theorizing if this lever or vehicle can be shown to be identical with the non-theoretical part of the higher theory. And because the real economic world is constantly changing and contains numerous constantly unfolding causal processes, the practice of economic theorizing (or making economic explanation) will continue as long as additional empirical causal information can be extracted from the processes.

From this perspective, therefore, we shouldn't try to determine whether economics is an empirical science simply by looking at the composition of its current content. We should instead stand back and take a bird's-eye view of the developmental history of economic theories, asking ourselves whether our total knowledge of the real economic world has been increasing since the birth of the first economic theory. If the answer is yes, economics can indeed be regarded as an empirical science, and the causal structuralist interpretation of economic theorizing should be widely accepted.

Chapter 4

Toward a Causal Structuralist Account of Economic Theorizing

4.1 Introduction

In Chapter 2, we have seen that economists frequently do not remove a capacity claim, such as the Heckscher-Ohlin (H-O) theorem, from their theories even if a prediction derived from the capacity claim is not consistent with the real-world economic phenomenon of interest. Why, then, do these economists tenaciously cling to such a claim, which seems to be empirically refuted? The reason is that they would rather think that any inconsistency between what is asserted in the capacity claim and what is happening in the real economic phenomenon should be attributed to the difference between the causal structure specified in the putative model that produces the capacity claim and the causal structure that generates the phenomenon. In this line of economic theorizing, therefore, the object that should be readjusted to attune with the real phenomenon of interest is the putative causal structure specified in the theoretical model and not the capacity claim. Chapter 2 then develops an explanatory framework to describe how the concept of causal structure figures in economists' practical economic theorizing.

Although the account provided in Chapter 2 explicates how the idea of causal capacity shapes the way an economic theory is built, it still seems to lack a well-articulated explanation of how a seemingly unrealistic capacity claim is relevant to real economic phenomena. Nor does it explain how a theory containing a seemingly unrealistic capacity claim can have a bearing on the real-world phenomenon of interest. This is a matter of concern because a causal capacity claim, or any causal law claim, is derived from a theoretical model that is specified by a specific causal structure, which in turn is meant to single out the main causal features of an economic phenomenon and omit the less relevant features. As a result, the causal capacity claim or causal law claim derived from such a structure inevitably possesses a certain degree of abstractness. This abstractness creates imprecision whenever the theory containing the abstract causal law (or abstract capacity claim) is used to explain or predict a new phenomenon. Let's call the resulting imprecision *the gap of abstractness*. According to some methodologists, this persistent gap of abstractness indicates that economic theorizing is an irrelevant discipline for explaining real economic phenomena.

Indeed, the persistent gap of abstractness presented by economic theories is a perplexing issue that most economic methodologists find hard to handle. But this situation does not in any way suggest that economic theorizing lacks empirical import or that derived economic theories are empirically irrelevant; at most, it indicates only that an economic theory that is totally consistent with every aspect of the real-world economic phenomenon is very difficult to come by, and the persistent gap of abstractness is the evidence of that difficulty. Rather than focus on the question of whether the gap of abstractness remains, the correct approach is to examine whether economists indeed tend to revise their originally highly abstract theories to make them more pertinent to real-world economic phenomena by adjusting the causal structures of theoretical models when important new causal features are called for. A simple criterion for determining whether a theory's gap of abstractness is narrowed is to examine whether the new relevant causal features are correctly incorporated into the theory.

It seems that we can take either of two approaches to look at the problem of reducing the gap of abstractness. The first way is to look directly at the change made to the content of an economic theory as the result of incorporating new causal features. The idea is this: When we add new causal features, they trigger a change in the original specification of the theoretical assumptions (or conditions) in the theoretical model. As a result, the content of the economic theory derived from the old theoretical model must also change.

In the second approach, theorists do not look at the problem by focusing on the change in content of the old theory but rather look further back, examining why and how a change in assumptions (or theoretical conditions) must be brought into the content of the old theory. A quick answer to this question is that the new causal facts discovered in the economic phenomenon will impel economists to revise the original specification of the causal structure in the old theoretical model, and this revision will be reflected in the change of theoretical assumptions. It is in this respect that we must consider the issue of why and how economists bring about a change to the content of their original theories.

An articulate account that explicates how the gap of abstractness is reduced by economists' theorizing can have an implication for the answer to the following more practical question: If we grant that a new causal law claim is derived from a readjusted causal model that has a different specification from the one that produced the old causal law, what then does it mean when the new causal law passes a specific causal model test, such as the invariance test mentioned in Chapter 3? More specifically, the question is this: If we put the fact of a-new-causal-law-passing-the-causal-model-test into the context of the general picture of economic theorizing mentioned in Chapter 1, how will this fact be interpreted against this general background? I suggest that this fact should be interpreted as indicating that an attempt to de-abstract the originally highly abstract causal theory has been successful, or as indicating that a model-manipulation intended to attune with the real situation has been successful.

Note that when we discuss in Chapter 2 that the idea of causal capacity shapes the way an economic theory is built, we are talking about how the abstract idea figures in economic theorizing. In this chapter, we reverse our direction, discussing how the newly discovered causal features in an economic phenomenon affect the structure of the original, highly abstract causal theory. A complete causal structuralist account of economic theorizing should explain economic theorizing conducted in these two opposite ways.

In what follows I first present Bert Hamminga's study of economic theory structure and theory development. Hamminga focused on the strategies economists have used to manipulate theoretical assumptions (or conditions) of their theories to produce theorems that have higher probabilities of being true in the real world. In this way, Hamminga intended to provide a criterion for determining whether a specific economic theory is successful in the real world. Because of its concern about the relation between theory and real economic phenomena, this study may help us to solve the problem of the gap of abstractness. But as you will see, although this approach tries to establish a criterion without invoking causal thinking, this lack of causal thinking raises doubts about the approach.

Next, I present an account that I think can bridge the gap between what is abstract and what is real in the practice of economic theorizing, something that Hamminga's account cannot do. Finally, I present a causal structuralist account of economic theorizing based on the results of the discussion of the former two studies and previous chapters.

4.2 Hamminga's Model-Theoretic Structuralist Account of Economic Theorizing

In a 1982 paper, Bert Hamminga, using as a case study the neoclassical international trade theory of the Heckscher-Ohlin-Samuelson (H-O-S) tradition, conducted a meta-theoretical investigation of theory structure and theory development in economics. Hamminga's study used the so-called structuralist approach. The general idea of this approach is this: If a model—i.e., for him, a set-theoretic object that represents the structure of a putative world—can be used to derive a theorem (or any theoretical result), then this theorem is said to be true in this putative world. The real world is only one of various putative worlds. Now let's suppose that there is a formal system including a class of various models within which the real world is only one model. Hamminga's approach tries to show that a theorem can be said to be very likely to be true in the real world without referring to what the real world is like; we need only ensure that this theorem can be derived from—i.e., can be proved to be true in—quite a few of the other models in the same formal system within which the real world exists.

Hamminga's main motivation for using this structuralist approach is to provide a solid ground for abstract theorizing in economics by connecting the abstract theorems derived from abstract theorizing to the real world through assumption-manipulation. In a nutshell, his idea is this: Abstract economic theorizing can be regarded as setting up a model that can be used to derive the targeted theorem. This initial model is generally supposed to be highly abstract because many theoretical assumptions—assumptions that specify idealistic conditions that often conflict with the real conditions in the world—are added to the model in order to derive a definite theorem. But there can be a series of economic theorizations representing a series of steps in which the various theoretical assumptions are dropped one by one from the original model to make the model's structure increasingly less restrictive. If we can show that this initial, highly abstract theorem is true in—i.e., is derivable from—this series of models, we can say that the fact that this highly abstract theorem can be derived from a series of *increasingly* less restrictive models indicates that the probability of this theorem to be true in the real world is *increasing*. Abstract economic theorizing is thus said to be relevant to the real economic world.

The appeal of this approach that is pertinent to the main concern of this chapter is that it offers a way by which we can connect the abstract economic theory directly to the real world without having to answer the problem of the gap of abstractness; it seems to sidestep this problem by reducing it from an empirical problem to a problem of theory construction, at the same time changing the focus of the problem from the gap between what is abstract and what is real to the gap between what is derivable and what is not derivable in a series of model-manipulation steps conducted under a formal system. But, as my presentation will show, this approach cannot avoid answering the problem of the gap of abstractness. Furthermore, to solve the problem, we must supplement this approach with a consideration of causal structure.

Let's next see how Hamminga established his structuralist account. For Hamminga, to conduct a meta-theoretical investigation, we must first define some unambiguous meta-theoretical terms that can be applied to represent the basic units of the structure of the exposition presented in the economic theories. He maintained that when a set of these unambiguous terms is constructed and is applied to describe and analyze the structures of theoretical exposition in various time periods, the research will show that some underlying theory-development strategies can be extracted from the analysis of the history of theory development. These theory-development strategies can in turn be used to explicate in detail how and in what way the structure of a theory changes in response to the economists' shifting of their emphasis to the so-called interesting theorems. In the following discussion, the italicized terms are those that Hamminga called the unambiguous meta-theoretical terms.

According to Hamminga, the practices of the international trade theorists of the H-O-S tradition mainly constitute activities of *proving* various economic propositions. The propositions being proven are called *theorems*, not because they are "logically or mathematically true in themselves" but because they are "proven to follow from certain conditions." The structure of the proofs of theorems can be represented in the following form (Hamminga 1982, p. 3):

$$V_{lmn} (C_1, \ldots, C_i, C_{i+1}, \ldots, C_j, C_{j+1}, \ldots, C_k \rightarrow IT) \qquad (4.1)$$

where V represents a *field* within which the proof is conducted, and the subscripts l, m, and n (to the right of V) together represent a specific situation in the field. For example, in the early version of international trade theory following the H-O-S tradition, the theorems are normally derived from a highly simplified field containing only two countries, two commodities, and two factors of production; this field is dubbed V_{222}. The various C's in formula (4.1) represent the various *conditions* (or assumptions) from which the *interesting theorem*—represented by IT in formula (4.1)—is derived. The use of the term "interesting" to describe the theorem is explained shortly.

Among the various conditions, those from C_1 to C_i represent the fundamental conditions that are borrowed from the neoclassical *foundation of economic analysis* and are called FEA-conditions. These conditions generally include those conditions that are stipulated in the neoclassical microeconomic theories for an individual economic regime, such as the assumptions about the range of variables to be covered in the production function and the consumer utility function of a single country.

Conditions from C_{i+1} to C_j represent *explanatory ideal* conditions—i.e., EI-conditions—that are used to ensure that the interesting theorem can be smoothly derived. In the example of international trade theory of the H-O-S tradition, these conditions include assumptions such as the assumption that the production functions of a certain commodity among different countries are identical and the assumption that there is no factor-intensity reversal. These conditions can be regarded as the core conditions that are used to guarantee that the general framework of the proof system of a specific branch of economics, such as international trade theory, does work in a way that economists expect it to work.

Conditions from C_{j+1} to C_k represent the *special* conditions. Special conditions are a set of tentative conditions that can be adjusted or revised by economists in response to a shift of their concerns regarding the relevant economic topic. These special conditions serve as supplemental conditions to FEA and EI conditions and ensure that the interesting theorem can be smoothly derived from the conglomerate of these conditions. Examples of special conditions include the assumptions of the algebraic form of the production function and the shape of the utility function. Based on the meta-theoretical terms defined above, formula (4.1) is then called a *proposition of economic theory* (PET). It represents an intellectual product produced by economists "in which a theorem is proven in a certain field from [a set of conditions that contains] always the same FEA and EI-conditions and ever differing special conditions." (Ibid., p. 5)

Now let's clarify the term "interesting" to describe a theorem. This term reflects the following two facts: On the one hand, the theorem is interesting because economists think that the theorem can be derived simply from FEA and EI conditions without calling on the help of strong special conditions. This fact makes the theorem *internally interesting*. On the other hand, the theorem is interesting because the theorem that economists are trying to prove in their theories must be the one that they think is most relevant to the economic issue of their current concern. This fact makes the theorem *externally interesting*. Both types of "interestingness" make economists think that the theorem is a target worth dwelling on.

After establishing a set of meta-theoretical terms, Hamminga then goes on to use these terms to discuss the development of an economic theory. Comparing a few research cases covering the same issue in international trade at various time periods, Hamminga concluded that the strategies for developing an economic theory can be categorized into four types, all of which involve the techniques of changing the field and the special conditions but leaving the FEA and EI conditions intact. These four strategies are (1) field extension, (2) the weakening of special conditions, (3) the construction of alternative conditions, and (4) the construction of conditions for conditions.

The strategy of field extension is referred to as a technique of changing the situation of the field with respect to economists' concern about the model-specification under which the interesting theorem is to be proven. For example, it can be argued that the traditional H-O-S model is too restricted within an unrealistic world containing two countries, two commodities, and two factors of production (the 2×2×2 case). Economists may then try to extend the range of the traditional model to cover the 3×3×3 case or even try to set up a general model to talk about the n×n×n case. The technique of field extension is revisited and illustrated in Chapter 5 in our case study of the debate on the theorem of factor-price equalization.

In the strategy of weakening the special conditions, the original special conditions are changed into the kind of conditions that will help economists to derive more easily the desired interesting theorems. The strategy of constructing alternative conditions is the technique of replacing old conditions by creating new conditions for the model in an attempt to prove the original theorem in a new case. The strategy of constructing conditions for conditions is used when an original condition that is used to derive the targeted theorem does not have an immediate relevance to the targeted theorem. In such a situation, an intermediate condition, which has direct relevance both to the targeted theorem and to the original condition, is established to represent the original condition in an attempt to connect the original condition with the targeted theorem. These strategies of theory development are generally used interchangeably to ensure that the derivation of the targeted theorems proceeds smoothly.

Like most meta-theoretical analyses, Hamminga's analysis of theory development inevitably must answer the following question: How and by what criterion can we judge that the theory derived by using one or more of the four strategies is a more successful or more valuable theory than its former version? To answer this question, Hamminga suggested that we first think about why economists try hard to conduct these four theory-developing strategies. Hamminga's answer is that "economists try to raise the plausibility of emerged interesting theorems as high as possible." By "plausibility," Hamminga meant "the probability of the theorem to be true in the real world." (Ibid., p. 8) So, for Hamminga, economists adopt the four theory-developing strategies in an attempt to maximize the probability that a targeted theorem is true in the real world.

How can this goal be achieved? According to Hamminga, "a world" is defined as a field having a set of completely specified FEA, EI, and special conditions. So the real world is only one of the worlds so defined. If we accept the underlying hypothesis of Hamminga's model-theoretic structuralist analysis—i.e., the hypothesis that if a theorem can be derived from a set of specified conditions, then the theorem can be said to be true in the world constituted by this set of conditions—it is then easy to recognize that economists' manipulation of theory-developing strategies is merely to highlight the economists' attempt to enlarge the class of the worlds within which the targeted theorem is true. Based on this line of reasoning, a criterion is proposed as a guideline for judging whether a targeted theorem will have a high probability of being true in the real world: "If a theorem holds in many worlds that can be expressed in the economists' language [i.e., can be defined by a class of various fields having various sets of well-specified conditions borrowed from other economic theories] the probability of the theorem to be true in our real world is high, even without considering at all what our real world exactly is like." (loc. cit.) Let's call this *the criterion of high probability.*

Hamminga's main motivation for proposing this idea of high plausibility is to try to provide a solid ground for abstract theorizing in economics. Hamminga provided one example to illustrate his attempt. In a 1941 paper, W. F. Stolper and P. A. Samuelson derived, from a set of very restrictive assumptions, a definite relation between the real wage of the scarce factor of production and international trade. The relation states that free international trade necessarily lowers the real wage of the scarce factor of production and raises the real wage of the abundant factor of production. This conclusion is the so-called Stolper-Samuelson (S-S) theorem. This abstract theorem provides a certain degree of support for international trade protection in some real-world issues such as the "cheap foreign labor" argument.

The methodology applied by Stolper and Samuelson is what Hamminga called the theory-developing strategy—i.e., the strategy of manipulating the field and its theoretical conditions (or assumptions). Stolper and Samuelson established their theorem by starting with a field containing only two countries, two commodities, and two factors of production and with a set of very restrictive assumptions. Then they dropped these restrictive assumptions one by one to see whether the theorem derived from the most restrictive model was still valid in the less restrictive models. They concluded that they had only to make slight modifications to their original theorem to cope with the change in the specification of theoretical assumptions. They thus concluded that their abstract theorem—i.e., the S-S theorem—had "shown that there is a grain of truth in the pauper labor type of argument for protection." (Stolper and Samuelson 1941, p. 73, from Hamminga 1982, p. 9)

Hamminga's main concern is where "the grain of truth" comes from. Because no statistical or observational evidence had been introduced in Stolper and Samuelson's paper, Hamminga concluded that this grain of truth "must stem from the possibility, shown by Stolper and Samuelson, to remove 'restrictive assumptions', and thereby allowing for more possible cases in which their theorem still is true. The theorem became more plausible up the road of 'generalization' and 'hence' the grain of truth." (Hamminga 1982, pp. 9-10) By offering this remark on the origin of the grain of truth for an abstract theorem, Hamminga is at the same time connecting the issue of the truth of a theorem with the issue of the value of the abstract theorizing in economics. To see this, let's analyze Hamminga's underlying idea.

Hamminga's idea is this: As is suggested by Hamminga elsewhere (Hamminga 1998, pp. 364-366 in John B. Davis, D. Wade Hands, and Uskali Maki (eds.) 1998), we can imagine that there exists a universe represented by a plane U. Each point on plane U represents a different combination of the various conditions (or assumptions) postulated in a theory. This specific combination of the various conditions is regarded as the specification of a special environment of a formal model that is used to derive an interesting theorem, which is in turn to be used to describe a certain state of the world. There are numerous such points in universe U, and the collection of these points constitutes a class of all logical possibilities within which each logical possibility represents a special kind of formal model possessing a special conditions-combination that can be used to derive a specific conclusion that can be used to explain a certain state in the world. Note that not every logical possibility has the same plausibility. Recall what Hamminga meant by "plausibility": the probability of being true in the world. Thus, the varying plausibilities among the logical possibilities means that not every conditions-combination, and not every theorem derived from it, has the same probability of being true in the world. Therefore, if we define the model so that each logical possibility maps to a real number between 0 and 1 representing its plausibility, we can then imagine that there is an uneven plane located right above plane U representing a plausibility plane, the integral of which equals 1. As Hamminga did in his short 1998 article, I also use a rectangle to represent the universe U in Figure 4.1.

[Please refer to Figure 4.1 on page 117-1]

To illustrate Hamminga's point, let's use Minhas's study of factor-intensity reversal (discussed in Chapter 2). Suppose that we derive an interesting theorem: the H-O theorem. We know that this theorem is derived from a highly abstract theoretical model—the H-O model—possessing a set of very restrictive assumptions (or conditions), so the H-O theorem is also a highly abstract theorem. Because of its high degree of abstractness, we therefore regard the H-O theorem as less likely to be true in the real world; it is represented by the small subset A of U in Figure 4.1. Now, suppose further that Leontief's paradox has been found and that Minhas's study of factor-intensity reversal in commodities has been proposed to explain not only this paradox but also the phenomenon explained by the H-O theorem. To do that, Minhas has set up a new model by weakening, or even removing, a restrictive assumption in the H-O model—i.e., the assumption of strong factor-intensity of commodities. As we have seen in Chapter 2, Minhas's conclusion can indeed achieve these two aims. So it is obvious that, by weakening the assumption of

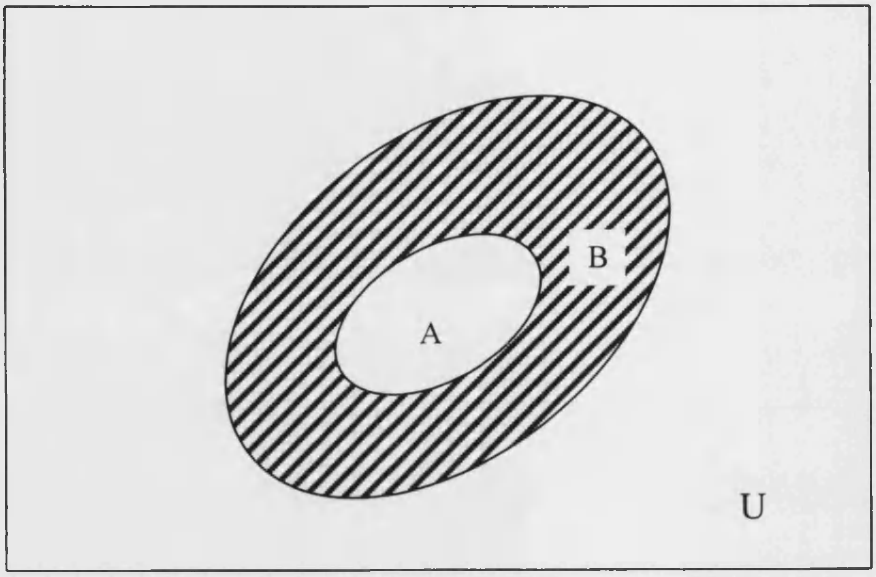


Figure 4.1: The Universe of Conditions-Combinations

strong factor-intensity, Minhas can accommodate one additional state of the world—i.e., the state of factor-intensity reversal—in his model. This means that, looking at Figure 4.1, he can move on plane U from A to B to increase the number of points of logical possibility; at the same time, he can increase the plausibility of his new model—i.e., increase the probability of its being true in the real world—because the new model now covers a larger portion of the integral of plausibility, which is correspondent to the larger collection of logical possibilities on plane U. Also note that what is asserted in the H-O theorem is now a special case of Minhas's model. It is in this sense that the H-O theorem is said to be retained in Minhas's new model, or to be proved to be true in or to be derivable from it. In this respect, the highly abstract H-O theorem can be shown to hold in more than one putative model or more than one possible world.

Minhas's strategy of economic theorizing—weakening or removing the restrictive conditions in a highly abstract model to make it less restrictive and so more applicable—is regarded by Hamminga as the main theorizing strategy that should be adopted by economists in order to develop their theories in a progressive direction—i.e., in a way that will make their theories more relevant to real economic issues. Indeed, when abstract economic theorizing can be shown to operate in this way in developing economic theories, the practice can be regarded as having higher value and so will be more successful; at the same time, the results derived from less restrictive models will also be regarded as having higher value and so thought to be more successful. This is why Hamminga said that "[i]t is widely consented by theoretical economists that the less restrictive assumptions are needed, the higher is the 'value' of the result [of economic theorizing]." (Hamminga 1982, p. 10)

It may seem that Hamminga's structuralist framework can provide an account of the relation between an abstract economic theorem and real economic issues without having to consider the problem of the gap of abstractness. The main reason for this impression is that the nature of the problem has been changed. Our original main concern was how an abstract economic theorem can bear on real economic issues. But putting this problem in the context of Hamminga's structuralist framework has changed the main concern to the following form: Is it very likely that an abstract economic theorem can be derived from as many putative models as possible? In this way, an empirical problem has been changed to a problem of model construction and derivation.

Hamminga's argument, however, has at least two flaws. To see this, let's first examine how his criterion of high probability is derived. This criterion comes from a background condition that Hamminga has maintained in his argument: If a theorem is proved to be true in quite a few model-theoretic systems, the theorem is then supposed to be true in a large class of worlds. I grant that this background condition is true, but it is puzzling that Hamminga seems to take for granted the truth of the following conditional simply because of the truth of this background condition: If a theorem is proved to be true in a large class of worlds, this fact strongly suggests that it is very likely that this theorem will also be true in the real world. This conditional is a non sequitur. The high probability of a theorem being true in the real world cannot be inferred from the sheer fact that it has been true in a large class of models whose structures differ from that of the real world. Whether or not the theorem in question has been true in a large class of worlds is irrelevant to the question of whether it is true in the real world. The simple fact is that anyone who wants to say that a targeted theorem is true in the real world must take the real world as another single case and must try to prove that the theorem is true under this case—i.e., under the model of the real world.

Second, even if Hamminga can successfully convert the problem of the gap of abstractness into a problem of model construction by asking whether an abstract economic theorem can be derived from a series of increasingly less restrictive models, his revised problem is dubious. The query is, Why is the fact that an abstract theorem can be derived from a less restrictive model so important for Hamminga's account? Or, Why does he regard this point as the most important characteristic of a successful economic theorization? One possible answer is that, as I have indicated in my illustration of Hamminga's point by using Minhas's case, less restrictive models are more relevant to real economic issues. This answer, however, begs another question: In what way are less restrictive models more relevant to the real economic world? If Hamminga cannot answer this question, there is no way that he can say that a certain abstract theorem, which is derivable from a series of increasingly less restrictive models, can be regarded as having higher plausibility—i.e., having higher probability to be true in the real world—because the definition of plausibility depends on the definition of the degree of relevance. One answer to the question is that "to be more relevant" means "to be increasingly realistic in some respects of the real world." But what are these respects? There must be certain right respects in the real world that can be used to compare *the degree of realism* between the old model and the new model. For a causal structuralist, these right respects should represent *the causal respects* of the real world. Regretfully, Hamminga's approach does not provide any account of these causal respects.

It thus seems that, even by using Hamminga's set-theoretic structuralist approach, we cannot avoid the problem of the gap of abstractness. We still must check empirically whether the abstract theorem is indeed still true in a real-world model; we cannot simply say that because the theorem has been shown to be formally derivable from a class of various models having various assumptions-specifications, it must have a high probability of being true in the real world. Admittedly, economic theorizing involves model-manipulation. But model-manipulation is not used simply as a tool to ease the derivation of the targeted theorem or to fulfill economists' special interests; most of the time, model-manipulation is used by economists to construct a correct causal model that explains the targeted real economic phenomenon. From this perspective, model-manipulation should be given *a causal interpretation*; Hamminga's type of model-construction interpretation is not sufficient to do the work of giving a complete account of model-manipulation. Giving model-manipulation a causal interpretation is the main topic discussed in the following section.

4.3 How Can Abstract Theoretical Claims Be Relevant to the Real World?

As we can see from section 4.2, Hamminga's account dealt only with the set-theoretic analysis of the structure of theory development within a formal system. This approach was deliberate on the part of Hamminga. The proposed idea of showing an abstract theorem to have a high probability of being true in the real world is evidence of Hamminga's intention. Hamminga did not require that economists know the real situation that the targeted theorem is to be applied to; rather, he required only that economists make sure that the targeted theorem is derivable from a class of various putative models. His idea was that if the targeted theorem can be shown to be derivable from this large class of models, the targeted theorem should be very likely to be derivable from the model that represents the structure of the real world. The underlying assumption of Hamminga's idea is that the real-world model is simply another model that can be designed by using assumption-manipulation and that economists should be able to invent such a real-world model that can be used to derive the targeted theorem. If all of these can be held, the targeted theorem is said to be true in the real world, and this targeted abstract theorem is said to be relevant to the real world. Hamminga's idea is then that, by following this seemingly self-contained argument, we can show that the abstract theoretical claims are relevant to the real world without having to deal with the problem of the gap of abstractness.

But as I pointed out at the end of section 4.2, the relation between abstract theoretical claims and the real world cannot be explained so easily. The conclusion that a specific abstract theoretical claim can be used to explain (or can be true in) real phenomena cannot be guaranteed merely by the fact that this specific abstract theoretical claim has been proved to be true in a large class of various models. The question of whether a specific abstract theoretical claim is a correct description of the targeted real phenomenon should be regarded as a single case to be examined individually. The answer to this question should not be affected by the fact that this abstract claim has been shown to be correct elsewhere.

Again, the relation between an abstract theoretical claim and the targeted real phenomenon should be considered in a causal structuralist way. Admittedly, there is always a gap between an abstract theoretical claim and the real phenomenon it is intended to explain. 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 point here is not the existence of this gap. Rather, we should focus our concern on, within the limit of the scientific method, whether and in what way this gap can be reduced. Contrary to what Hamminga thought, the problem of the gap of abstractness should not and cannot be sidestepped. We must face it and try to dissolve it.

Why should we concern ourselves with the question of how abstract theoretical claims bear on real phenomena? The answer is that we want to know whether an abstract theoretical claim can be used to explain a real phenomenon. 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, which I have called the gap of abstractness, raises our doubts.

In economics, the most often applied method to bridge the gap is the piecemeal method mentioned in Chapter 2. This method, like the assumptions-manipulation strategy described in Hamminga's account, also involves the changing of the ideal theoretical assumptions in the original theoretical model. But, in contrast to Hamminga's account, the purpose of changing the ideal theoretical assumptions in the piecemeal method should not be interpreted simply as intending to design a theoretical structure that can be used to derive the abstract theoretical claim more smoothly. Nor is the purpose merely to cover more of the plane of possible worlds. Rather, it should be interpreted 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 kind of change in assumptions depends on the real situation of each case to be explained. The main purpose for mentioning these assumption changes is 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 phenomena.

The economists' motivation for conducting such revisions is that 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, by using a specific causal model test such as the invariance test mentioned in Chapter 3, economists can show that the revised theoretical model can produce the more accurate causal law, this fact will indicate that their attempt to de-abstract 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 has been shown in the causal model 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.

To illustrate this discussion, let's use the example in Chapter 2. Recall the Heckscher-Ohlin (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 we have mentioned, this long list of assumptions is used to set up a disturbance-free environment to guarantee that the cause—i.e., the difference in factor endowments—determines 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.

For the reason mentioned above, it is no wonder that Leontief found that this capacity claim foundered when it was used to explain the content of U.S. exports in 1947. This 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 give up the H-O theorem outright, or is the gap simply ignored by these theorists? 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 Chapter 2 represents a theorist's attempt to bridge the gap.

Notice that although Minhas is a trade theorist, he does not follow the path predicted by Hamminga. Recall that Hamminga maintained that neoclassical international trade theorists will try to revise the H-O model by simply doing theoretical tricks such as changing some theoretical assumptions to enable them to derive the desired theoretical conclusion from this revised model more smoothly. Minhas's study, however, 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.

Minhas has a different idea regarding the Leontief paradox. Minhas argues that the fact that the United States did not export the commodities predicted in the H-O theorem must arise from the causal structure discrepancy—i.e., the discrepancy between the theoretical causal structure and the real causal structure. What is this causal structure discrepancy? Minhas noticed 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?

To show that factor-intensity reversal is a characteristic of the real causal structure, Minhas had to show that, in most industries, factor-intensity reversal occurs. Recall that in an ingenious empirical test, Minhas indeed showed that factor-intensity reversal is a prevalent phenomenon. In our reading of the Minhas finding, this phenomenon should be regarded as an additional causal consideration to be added to the original H-O model. Therefore, for a causal structuralist, 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 empirical test 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' theorizing. Constructing more complete causal structures is their first theoretical concern.

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 as I have argued in sections 1.4 and 2.6, 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—i.e., 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 to the theoretical claim 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.

4.4 Toward a Causal Structuralist Account of Economic Theorizing

Economic theorizing is an activity composed of two opposite processes. 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, they then *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 constitute 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.

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 to prevent the results derived from their models from suffering the disturbing influence of other, less important causal factors. The purpose of using these shielding devices is to ensure that the derived result is purely the exhibition of the essential behavior of this class of selected causal factors and nothing else.

The conclusion derived from economists' theoretical model must be very imprecise because, as we have mentioned, this model does not include all the causal factors. But this conclusion represents economists' first attempt to provide an explanation that captures the main causal features of the targeted economic phenomena. This initial account is highly abstract and cannot explain every detail of the phenomena. 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 problem often encountered in economic theorizing is this: Can such a general guideline be used to explain or predict other classes of economic phenomena? This problem is critical, especially when economists are asked to provide policy suggestions based on their theoretical models. Is it reasonable for economists to suggest that a government should reduce its money supply to a certain amount in order to fix the price level at a certain level in the next year simply by using policy parameters suggested in a theoretical model based on the data of the past ten years? If the scarce factor of production in a country is labor, should the government of this country set up a barrier to prevent the import of cheaper foreign labor in order to increase the welfare of domestic labor simply because this barrier is what Stolper and Samuelson 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 the price level. But it may, in the real world, turn out that an increase in the money supply, contrary to what is predicted in the monetary theory, does not affect the price level at all. Should we simply refute the theoretical claim that an increase in the money supply will increase the price level? 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 has suggestions about the original H-O model, a new study of the relation between money and price should also suggest which new causal factors 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, in the end, 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 and predict the targeted real phenomena. By completing this entire procedure, the original abstract monetary model is said to be concretized.

Chapters 1-4 complete my theoretical interpretation of a causal structuralist account of economic theorizing. In Chapter 5, I examine a case in international trade theory to illustrate the ideas discussed in these four theoretical chapters.

Chapter 5

Commodity Trade and Factor Mobility—Substitutes or Complements? A Case Study of Economic Theorizing

5.1 Background: The Heckscher-Ohlin-Samuelson Model and the Factor-Price Equalization Theorem

Recall from section 2.6 that the Heckscher-Ohlin model is highly idealized in that it aims to obtain precise knowledge of the effects of a difference in factor endowments on a country's pattern of international trade under a well-contrived environment. The definite result derived from this model is that a country has a comparative advantage in producing and exporting those commodities that use more intensively the country's relatively more abundant factors. In other words, to use the terminology established in Chapter 2, the difference in factor endowments has the capacity to determine which commodity a country should produce and export. This definite result is the so-called Heckscher-Ohlin (H-O) theorem.

The H-O theorem is generally used to delineate the relationship between a country's factor endowment and its pattern of commodity trade. In addition to this relationship, international trade theorists are interested in the impact of international commodity trade on real factor prices in both trading countries. Heckscher and Ohlin stated that international commodity trade either definitely equalizes or tends to equalize factor prices. But it was Paul A. Samuelson who established a systematic account, in a series of papers published in 1948 and 1949, of the relationship between international commodity trade and the prices of factors of production.

Let's discuss the gist of Samuelson's account. Note first that Samuelson's account was conducted under the framework of the H-O model. So, as with the H-O model, let's also assume that there are only two countries—nation 1 and nation 2—and two factors of production—in our model, labor and capital—involved in producing two commodities—commodity X and commodity Y. Let's suppose that nation 1 is a labor-abundant country and nation 2 is a capital-abundant country and that commodity X is a labor-intensive good and commodity Y a capital-intensive good. Under the other assumptions stipulated in the H-O model, such as the same production technology and the same tastes in consumption in the two countries, the pretrade, or autarkic, price of commodity X must be relatively lower in nation 1 than in nation 2 because the relative price of labor is lower in nation 1 (because nation 1 is a labor-abundant country). Similarly, we know that the autarkic price of commodity Y must be relatively lower in nation 2 than in nation 1. The condition of the difference in autarkic prices between the two countries creates a comparative advantage for each country to specialize in producing different commodities. For example, nation 1 will specialize in producing and exporting commodity X in exchange for commodity Y with nation 2, and nation 2 will operate in the opposite direction.

Let us further assume that both factors of production are fully employed in both industries in both countries. It's important to note that as international commodity trade occurs between these two countries and as nation 1 increasingly concentrates on producing commodity X, it must reduce its production of commodity Y in order to release additional factors of production to be used in manufacturing the additional amounts of commodity X called for in international commodity trade. But because commodity X is a labor-intensive good—that is, its production uses more units of labor than of capital—the relative demand for labor will rise faster than the relative demand for capital in nation 1. As a result, the price of labor—i.e., the wage (w)—

will increase, and the price of capital—i.e., the interest rate (r)—will fall in nation 1. The same logic applies to nation 2, with the result that the price of capital (r) will increase and that of labor (w) will fall.

Remember that nation 1 is a labor-abundant country, so the pretrade wage rate must be lower in nation 1 than in nation 2; and because nation 2 is capital-abundant, the pretrade interest rate must be lower in nation 2 than in nation 1. Thus, according to the mechanism mentioned above, when international commodity trade occurs between these two countries, this trade will increase the originally lower wage rate and reduce the originally higher interest rate in nation 1, and, conversely, it will reduce the originally higher wage rate and increase the originally lower interest rate in nation 2.

Accordingly, as long as there is a difference in the prices of factors between these two countries, there will be differences in the commodity prices, and there will still be a comparative advantage for each country to continue international commodity trade. The upshot is that the international commodity trade between the two countries will continue until the relative commodity prices are completely equalized. To be more specific, the international commodity trade will stop only when the relative factor prices are completely equalized, the relative commodity prices are then completely equalized, and the condition of comparative advantage no longer exists for the two countries to trade. So a succinct conclusion can be made: International commodity trade tends to equalize the factor prices between nations. This conclusion is the so-called *factor-price equalization (FPE) theorem.*

From the perspective of complete factor-price equalization, an important corollary can be derived from the FPE theorem. The corollary is that international commodity trade can be regarded as a *substitute* for factor mobility (or factor migration or factor trade) between nations. That is because, like factor mobility, commodity trade can also bring about a condition of international equilibrium in which all the homogeneous factors earn the same level of reward in both countries. That is, in the absence of factor mobility between nations, commodity trade between nations can act in the same way as factor mobility between nations to bring about the equalization of the factor prices between nations. It is in this sense that commodity trade is said to be a substitute for factor mobility. (see also Mundell 1957) Let's call this *the corollary of substitutability*.

Hereafter I will call the model that derives the H-O theorem and the FPE theorem the *Heckscher-Ohlin-Samuelson (H-O-S) model.*

5.2 The Focus of the Case Study

Casual observation of the world economy, however, will immediately indicate that neither factor prices nor commodity prices are equalized between nations. The discrepancy between what is asserted in the economic theory and what really occurs in the economic world always embarrasses economic theorists and prompts debates on economic theorizing among economic theorists and methodologists.

The traditional way to deal with this problem is, first, to raise the issue of realism versus theory by focusing on the concern of unrealistic assumptions in economic theories. To tackle the problem of inaccurate theory means to answer the following two questions: whether theories derived from unrealistic assumptions can actually represent and explain real economic phenomena, and whether these unrealistic assumptions are testable. One prominent example of this approach to economic methodology can be found in a series of discussion papers and responses exchanged among distinguished economists discussing economic methodology published in *American Economic Review* in 1963-5.

Although these traditional methodological concerns give us an angle to look at the problem of the inaccuracy of a theory when it is used to explain or predict phenomena of interest in the real world, they are not my main concerns in this case study. As I have pointed out in previous chapters, according to the causal structuralist account of economic theorizing, imprecise economic theories are inevitable. The imprecision of economic theories originates from what I have called the gap of abstractness between the abstract theories and the real phenomena of interest.

To a certain degree, as traditional wisdom would argue, the gap of abstractness can indeed be attributed to unrealistic assumptions. But, contrary to traditional wisdom, the application of unrealistic assumptions in economic theorizing is not a vice with respect to the empirical tradition; it figures in economic theory-building in just the same way as does the procedure of condition-control in theory-building in experimental physics. Unrealistic assumptions are introduced into theories to act as controlling devices to safeguard theoretical models against disturbing influences produced by other, less relevant or less important causal factors and to ensure that the main targeted phenomena can be elicited from these shielded theoretical models. The conclusions derived from these shielded theoretical models are what I have called *abstract causal laws* (or *abstract capacity claims*). They are the main targets of economic theorists. From this perspective, inaccuracy as a characteristic of economic theories is not surprising.

But the inevitable inaccuracy of economic theories, which troubles many empirical-minded economists, is not irremediable. As described in Chapter 4, the process of concretization is their salvation. This process attempts to provide additional phenomenal content to the originally highly abstract theory by supplying more complete knowledge of the causal structure of the real phenomenon in question.

This additional causal knowledge may come from economists' observation of the real economic situation, from their reexamination of other relevant economic theories, or from any other reliable sources. The effect of this additional causal knowledge on the practice of economic theorizing may result in any form of change in economic theories. It may, for example, cause economic theorists to remove the assumptions of their original theories. Or they may replace the production function used in their original theoretical model with other, more pertinent production functions, as shown in Minhas's studies of factor-intensity reversal in Chapter 2. Or new assumptions may be added to the original theoretical model. In any case, the main concern is no longer whether the gap of abstractness still exists or whether the original highly unrealistic theory can accurately represent the real phenomenon in question.

As pointed out in section 4.1, however, for a causal structuralist, the main focus of the study of economic theorizing should be to examine whether economists indeed tend to revise their original highly abstract theories to enable them to capture more complete causal accounts of real-world economic phenomena by adjusting the causal structures of their theoretical models when important new causal features are called for. More succinctly, the main focus of the entire study of economic theorizing should more closely mirror the focus of Nancy Cartwright's 1997 lecture at University College London. The focus is indeed not "[the question] of realism in science—i.e. how *accurately* can the sciences, including economics, represent the world, but rather the question of the range of science—how *much* of the world can it represent." (1997, p. 1)

The following sections present a case study of two models. The case study illustrates the reaction of international trade theorists—who are deeply committed to the abstract capacity claim (i.e., the corollary of substitutability), which is derived from a highly idealized theoretical model—to critics who decry the frequent discrepancy between economic theory and economic reality, calling for economic theorists to adjust their theories. An important finding of the study is that a seemingly empirically refuted capacity claim still figures in these theorists' theory-building. Indeed, a characteristic common feature of these theorists' practice is that, no matter what new theoretical conclusions they may draw from their revised theoretical models, what is asserted in the capacity claim continues to figure in their theorizing and to reappear in their new conclusions. The only difference involved in the capacity claim is that, given detailed information to explain a discrepancy between their theories and reality, theorists will reexamine the capacity claim under a new theoretical model. In an attempt to accommodate the additional causal knowledge, the new model contains a new causal structure that is revised from the old model. As a result of this reexamination, theorists may make some refinements to the original content of the capacity claim with respect to the features of the new causal structure. For example, if the new causal structure features more complicated interactions among various causal factors, theorists may add a restrictive condition to the original capacity claim to indicate the situation under which the cause stated in the capacity claim will influence the targeted effect in its own specific way. No matter what final conclusions theorists may draw from their new theoretical models, what they have done in their economic theorizing or model-manipulation represents an attempt to provide more complete causal accounts of the economic phenomena in question.

5.3 Outline of the Controversy

As pointed out in previous sections, for most empirical-minded economists, the status of the corollary of *the substitutability* between trade in goods and factor mobility—i.e., the corollary to the factor-price equalization theorem—is dubious. These critics' argument is this: The corollary's validity is based on the validity of the factor-price equalization theorem, and the validity of the factor-price equalization theorem depends on the unrealistic assumptions made in the Heckscher-Ohlin (H-O) model. These assumptions include the existence of free trade in goods and factor immobility; the same technology in the two countries; constant returns to scale; perfect competition both in commodity and in factor markets; and incomplete specialization in production. For these critics, not only are the unrealistic assumptions in the H-O model problematic, but also any observation of the factor prices and commodity prices in the real world raises doubts about the truth of the factor-price equalization theorem. So, according to the argument, the corollary of substitutability is not true in the real world.

Indeed, some economists, by revising some of the unrealistic assumptions in the H-O model, have reached a very different conclusion: Goods trade and factor mobility, to a certain extent, are *complements*. For example, James R. Markusen (1983) pointed out that if we assume identical factor endowments but different factor prices in a world of two countries, any revision of the traditional assumptions of the H-O model results in the conclusion that goods trade and factor mobility are complements between two trading nations.

Let's digress for a moment to clarify the terms "complement" and "substitute" used here. *Complement* here represents the ordinary economic meaning when it is used to describe two goods that are complements (or complement goods): If the purchase of good A induces the purchase of good B, we say that good A and good B are complements. In other words, if good A and good B are complements, good B tends to be purchased when good A is purchased, and vice versa. One example is cups and saucers. On the other hand, if good A and good B are *substitutes*, good B tends not to be purchased when good A is purchased, and vice versa. That is, the demand for good B is replaced by the demand for good A if the two goods are substitutes. One example is coffee and tea. In Markusen's case, commodity trade and factor mobility are said to be complements in that, in the process of heading for factor-price equalization, an increase in the amount of commodity trade will also induce an increase in the amount of factor mobility, and vice versa. In the H-O-S model, however, in the process of heading for factor-price equalization, an increase in the amount of commodity trade can occur in the absence of factor mobility, something that represents an extreme case of substitution.

Other prominent examples involved in the debate about substitutability and complementarity include Lars E. O. Svensson's 1984 paper and Kar-yiu Wong's 1986 paper and 1995 book. Unlike Markusen, whose model significantly violated most of the assumptions in the H-O framework, Svensson removed only the assumption of factor immobility; he retained all other assumptions in the factor-endowments theory. His goal was to create a model that reflected both the real situation in the world and the simplicity of a theoretical model. In this work Svensson developed a middle-ground conclusion: Goods trade and factor trade tend to be substitutes (complements) if traded and nontraded factors are cooperative (noncooperative) after a change in the endowment of the home country.

Kar-yiu Wong, allowing for possible differences in factor endowments, tastes, and technologies across the two trading countries, avoided the question of substitutability or complementarity. Instead, he developed necessary and sufficient conditions for substitutability and complementarity between goods trade and factor mobility.

These papers represent an active discourse between these trade theorists on the topic of the nature of the relationship between commodity trade and factor mobility. Markusen's and Wong's papers are discussed in the following two sections.

5.4 Markusen's Models

Although the main purpose of Markusen's paper was to argue for an alternative account of the cause of international trade, the conclusion derived from his argument can shed light on the question of substitutability. Markusen argued that instead of being substitutes, factor movements and commodity trade are complements. Markusen's work, using some different assumptions, presents several models in which factor mobility leads to an increase in the volume of world trade—i.e., both the volume of factor mobility and that of world trade change in the same direction, which means that they are complements. This work contradicts the corollary of substitutability between goods trade and factor mobility, which is inferred from the H-O-S type of factor-price equalization theorem.

Markusen listed six assumptions (Markusen 1983, p. 342):

(a) Countries have identical relative factor endowments;

(b) Countries have identical technologies;

(c) Countries have identical homothetic demand;

(d) Production is characterized by constant returns to scale;

(e) Production is characterized by perfect competition; and

(f) There are no domestic distortions in either country.

Markusen noted that if assumption (a) is removed and if the other five assumptions are retained, the model conforms to the standard H-O-S model. In fact, Markusen retained assumption (a) and assumption (c) and created four different models, each removing one of the remaining four assumptions. Because all the models retain the assumption of identical relative factor endowments before factor movement (note that this is contradictory to what is assumed in the H-O-S model), the models share the common characteristic that the basis for trade is something other than differences in relative factor endowments (note that difference in relative factor endowments is the cause of international trade in the standard H-O-S model). Table 5.1 compares the assumptions and the trade bases for the H-O-S model and the Markusen models.

[Please refer to Table 5.1 on page 141-1]

In the H-O-S model, differences in relative factor endowments create trade, and the mechanism described in section 5.1 results in factor-price equalization with incomplete specialization. In the Markusen models, on the other hand, equalization in relative factor endowments but differences in factor rewards induce factor mobility, which creates a factor proportion basis. Differences in relative factor endowments reinforce the other basis for trade. In the four models, factor-price equalization occurred after at least one nation was completely specialized.

Note that the phenomenon of "distribution of factors" does not matter in the H-O-S model because it assumes differences in factor endowments. This is an important issue for the Markusen models, however, because they assume that the relative factor endowments between two countries are equal before factor movement occurs.

How did Markusen conclude that commodity trade and factor mobility are complements? Let's take one of Markusen's models for illustration. In the model of different production technology (model (i) in Table 5.1), we see a series of chain reactions due to the removal of assumption (b)—i.e., dropping the assumption of identical production technology across countries. Suppose that, in this model, two countries—h (home) and f (foreign)—use two factors—L (labor) and K (capital)—to

Table 5.1: Comparison of the Heckscher-Ohlin-Samuelson Model and the Markusen Models

Model	Trade Basis	Assumptions
Heckscher-Ohlin-Samuelson	Different relative factor endowments with no factor movement between trading nations	• Identical production function (technology) • Constant returns to scale • Perfect competition • No domestic distortion
Markusen	Equal relative factor endowments with different factor rewards before factor movement	• Model (i): different production function (technology) • Model (ii): different returns to scale • Model (iii): imperfect competition • Model (iv): not free trade

produce two commodities—X and Y. Also suppose that X is a labor-intensive commodity and Y capital-intensive, and that there is no factor-intensity reversal. The production function for each industry in each country can be represented in the following form, where the total supplies of L and K are assumed to be fixed and denoted as $\bar{L}$ and $\bar{K}$ (Ibid., p. 343):

$$Y^i = G(L^i_y, K^i_y); \qquad \bar{L} = L^i_x + L^i_y,$$

$$X^i = \alpha^i F(L^i_x, K^i_x); \qquad \bar{K} = K^i_x + K^i_y, \qquad \text{where } i = f, h \qquad (5.1)$$

α^i is called the *technical efficiency parameter* in producing commodity X. Let's suppose that h is more efficient in producing X—i.e., h has superior technology in producing X—so $\alpha^h > \alpha^f$. Now, under the assumptions of equal relative factor endowments, identical homothetic demand between countries, and fixed amount of factor supplies in each country, country h apparently has a comparative advantage in producing more X to exchange for Y from country f. And, because X is a labor-intensive good and country h has a superior technology in producing X, it follows that country h can be even better off in its already superior efficiency of utilizing L if it can transfer more L from the production of Y to the production of X; then the relative marginal productivity of labor in country h must be greater than that in country f. Also, under the assumption that production is characterized by perfect competition (recall assumption (e) above), it follows that the reward for each factor must reflect its marginal productivity, so wage (w) = marginal productivity of labor, and interest rate (r) = marginal productivity of capital. So it must follow that $w^h > w^f$. The same reasoning can be applied to establish that country f has a comparative advantage in producing Y, and we can conclude that $r^h < r^f$.

Having outlined the production structures of both country h and country f and the effect of a difference in production technology on factor prices in each country, let's now look at what will happen when these two countries allow their factors to move across national borders. Under the factor reward structure derived above—i.e., $w^h > w^f$ and $r^h < r^f$—if the factor mobility between nations is allowed, L will move from country f to country h and K from country h to country f. This pattern of factor movement will create a Heckscher-Ohlin basis (or factor-proportion basis)—i.e., country h will be endowed with (or will accumulate) more L, and country f will be endowed with (or will accumulate) more K—to reinforce the existing direction of commodity trade. This pattern of factor movement will continue until at least one country is completely specialized in producing its more advantageous good. When this indeed occurs, the factor prices can also be equalized between nations.

The underlying mechanism of Markusen's model of different production technology can be summarized in the following steps:

> [1] Dropping assumption (b) to allow differences in production technology → [2] the factor price structure $w^h > w^f$ and $r^h < r^f$, where h (home country) is exporting labor-intensive good X and f (foreign country) is exporting capital-intensive good Y.
>
> If factor movement is now permitted, then, from [2], it follows:
>
> [2] → [3] in country h, K (capital) flows out to country f, and in country f, L (labor) flows out to country h → [4] h will have more L, and f will have more K → [5] forming a factor-proportion basis for commodity trade; this adds a Heckscher-Ohlin basis for trade, which acts to reinforce the existing direction of goods trade → [6] Finally, factor prices can be equalized between countries only if at least one country is completely specialized in producing its more advantageous good.

Note in this flow chart that the factor mobility occurs at step [3], and at step [4], as follows from the previous step, both countries have more of the factors that are used intensively in producing their export goods; at step [5], commodity trade increases. It is the same direction of variation both in step [3] and in step [5] that led

Markusen to conclude that factor movements and commodity trade are complements.

One of the main purposes of discussing Markusen's study is to illustrate an extreme case of model-manipulation. It is obvious that the H-O-S model and the Markusen model of different production technology begin with different assumptions: The H-O-S model begins with the assumption of different relative factor endowments with or without different factor rewards, and the Markusen model begins with the assumption of equal relative factor endowments and different production technology. These two models produce the two simple causal paths shown in Figure 5.1.

[Please refer to Figure 5.1 on page 144-1]

Recall our original question: What is the relation between commodity trade and factor mobility; are they substitutes or complements? As shown in Figure 5.1, the H-O-S model maintains that commodity trade and factor mobility are substitutes because commodity trade increases in the absence of factor mobility. In the Markusen model, however, they are complements because commodity trade increases as factor mobility increases. With respect to our original question, which model is the right one? The answer is that it depends. As I have emphasized in the previous theoretical chapters, there is no single model that can be used as a general model to answer our original question in every real situation. Which model is the right model to answer our original question depends on which model is better for illustrating the current situation of international trade under a certain causal structure.

Dominick Salvatore's popular international economics textbook presents the general equilibrium framework stipulated in the Heckscher-Ohlin *theory*. (Salvatore 1999, p. 120) This framework is reproduced in Figure 5.2.

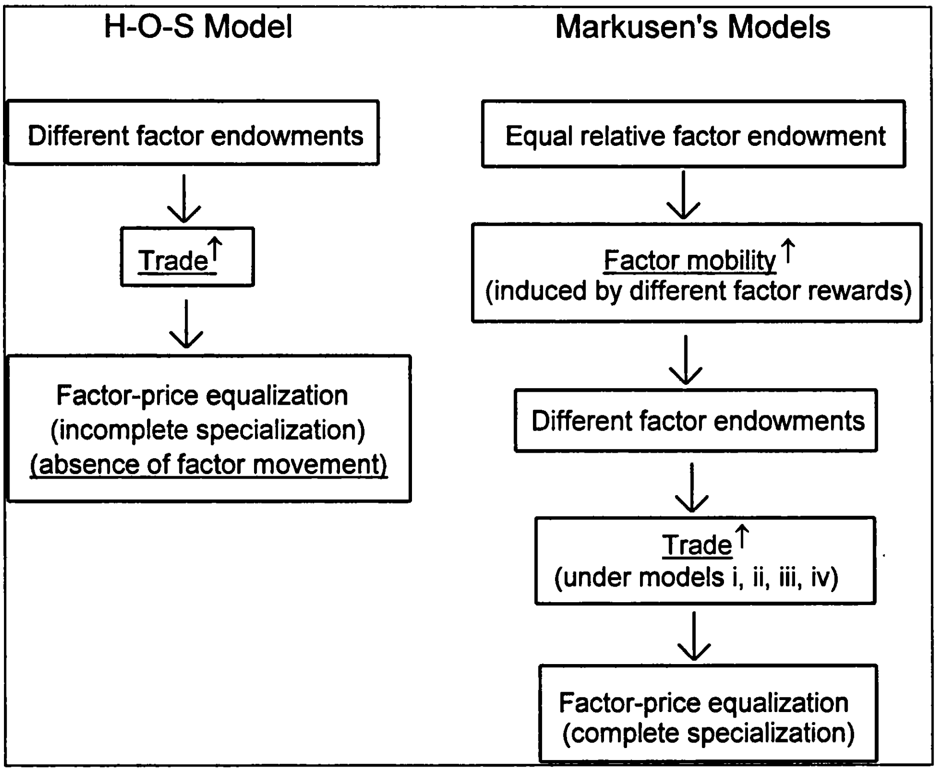


Figure 5.1: Cause Versus Result in the Two Models

[Please refer to Figure 5.2 on page 145-1]

In this general equilibrium framework, at least four fundamental economic forces contribute to the formation of commodity prices. These forces are those factors at the bottom of each route in Figure 5.2: technology, supply of factors (and so the difference of factor endowment), tastes, and income level. What the H-O-S *model* has done is to freeze the action of the forces of technology, tastes, and income level and so to rule out their latter influences along routes I, III, and IV. The purpose of this abstraction is to single out the force of the supply of factors (or the difference of factor endowment) in an attempt to see how it will exert its full influence on commodity prices. So, instead of the entire causal system expressed in Figure 5.2, what the H-O-S model derived is the simplified causal path starting from route II: Supply of factors → Factor prices →Commodity prices. It is from this causal path that the H-O-S model derived the conclusion of substitutability between commodity trade and factor mobility.

In Markusen's model of different production technology, however, route I is singled out by holding fixed all the influences coming from the other three routes. What is new to the general equilibrium system is that the Markusen model brings in a new causal factor from outside this system: factor movements between nations. This new causal factor in turn changes the system in that the original causal route II is now incorporated into route I. Therefore, instead of deriving the original simple causal route I—Technology → Commodity prices—Markusen's model comes up with a more complicated version: (Different) Technology → (Different) Factor prices → Factor mobility → Different supply of factors (or different endowment of factors) → Commodity prices. It is from this causal path that the Markusen model derives the conclusion of complementarity between commodity trade and factor mobility.

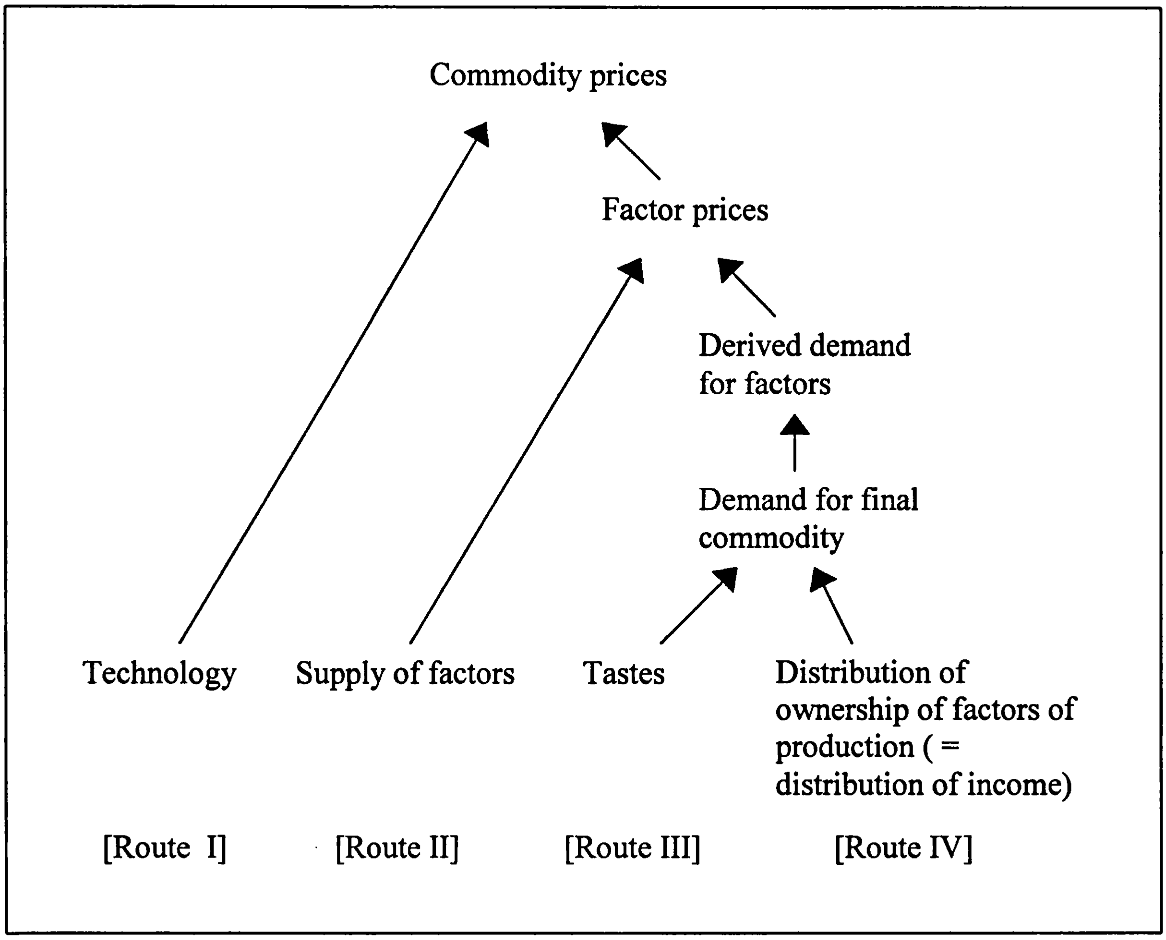


Figure 5.2: General Equilibrium Framework of the Heckscher-Ohlin Theory

Thus, both the H-O-S model and the Markusen model are highly idealized models in that to obtain their results they use numerous assumptions to rule out what many economic theorists thought were less relevant influences. If we are lucky and the causal system of the concrete phenomenon of international trade in question *happens to* have the same causal structure specified in, say, Markusen's model, we can readily apply this model to explain the concrete phenomenon. But, most of the time, nature acts in a way that is far more complicated than specified either in the H-O-S model or the Markusen model. If the real causal system is so difficult to grasp, what is the use of these abstract models?

Recall the simple criterion mentioned at the end of section 4.3: If a theoretical claim is derived from a more complete—i.e., more concrete—causal model, it will generally be a more accurate causal claim and can be used to provide a fuller causal explanation of the targeted real phenomenon. According to this criterion, it seems that Markusen's theoretical claim, asserting that commodity trade and factor mobility are complements, is indeed the more accurate claim compared with the corollary of substitutability. Why? The reason is that Markusen makes factor movement, a prevalent phenomenon in the real world, a part of his model, at the same time incorporating what the H-O-S model called the cause of international trade—the difference in factor endowment—into the model's causal path leading to general equilibrium. But if, based on this, we conclude that Markusen's model is *the* right model for identifying the relation between commodity trade and factor mobility, it would be a misguided conclusion.

If we assume that the real world operates as depicted in Figure 5.2, Markusen's model is neither concrete nor complete enough to cover a sufficient part of the entire causal structure and thereby claim the status of *the* right model compared with Kar-yiu Wong's model, which is discussed in detail in the following section. For our present purpose, it suffices to say that in Wong's model, *all* the economic factors presented in Figure 5.2 are released to act to determine whether commodity trade and factor mobility are substitutes or complements. What is derived from this model is a list of sufficient and necessary conditions indicating when commodity trade and factor mobility are substitutes or complements. Because all the causal factors mentioned in Figure 5.2 are included in Wong's model, according to our criterion, it should be more complete than Markusen's.

Note that we make this judgment with respect to the background of the general equilibrium framework depicted in Figure 5.2. We can make this judgment because we *assume* that the causal structure of the real world happens to be the same as this general equilibrium framework. If, for any reason, it turns out that the real world does not act in this way, the original judgment will need to be reexamined under a new causal structure that can represent the real world's structure. That is, any judgment of the ordering of the completeness of any two models is made relative to *a hypothesized complete causal structure* that theorists think can represent the real causal system of an economic phenomenon. When theorists find that this hypothesized complete causal structure can no longer represent the real causal system of the phenomenon in question, they need to construct another complete causal structure to try to represent the real causal system. Economic models are then rejudged for their completeness relative to this newly hypothesized complete causal structure.

One important point is that no single hypothesized complete causal structure can exhaustively list all causal factors of the phenomenon in question. So even if Wong's model is the most complete model relative to the hypothesized general equilibrium system, it is still not the most complete model in general. There is no such thing as *the* most complete model *in general*; every model is compared with another model *relative to* a hypothesized complete causal structure that is thought to represent the real causal system in the world. The most that we can get is *the relatively most complete* model with respect to a certain hypothesized complete causal structure. From this perspective, we can say that an abstract model such as the H-O-S model serves as *a prototype* to guide later theorists such as Markusen and Wong in developing their models so as to retrieve as much phenomenal content as possible from the hypothesized complete causal structure. If we see economic theorizing or economic model-building in this way, we are causal structuralists and will not ourselves commit the same mistake made by the regularists—to seek endlessly for *the* most general economic model that covers every economic detail in the world.

The next section discusses Kar-yiu Wong's model. This model represents a theorist's effort to try to establish the relatively most complete model under the general equilibrium system stipulated in the Heckscher-Ohlin theory.

5.5 Wong's Model

Our concern is not to find a model that is the most complete model in general that is readily applicable to explaining a real economic phenomenon. Instead, our goal is to find a model that is the relatively most complete model that can be used to explain the real phenomenon whose causal structure is supposed to be specified under a hypothesized complete causal structure of a general equilibrium system stipulated by economic theorists. Given that, we turn to Wong (1986) for a fresh perspective. Wong developed necessary and sufficient conditions pertaining to the question as to when substitutability or complementarity obtains, thus yielding more general insights into the debate of our concern.

Wong's model allowed differences in three variables: first, in factor endowments (the cause of trade in the H-O-S model but the result of factor mobility in the Markusen models); second, tastes (both in the H-O-S model and in the Markusen models, countries are assumed to have similar tastes); and third, technology (the H-O-S model assumes the same technology level between two countries, whereas the Markusen model of different production technology allows the technology level to differ between the countries). Wong's model is to be regarded as more complete than Markusen's model because Wong introduced an additional causal factor, and from this model he derived a more complete conclusion to the extent that Markusen's conclusion is to be regarded as one of its special cases.

5.5.1 Model Specification

Let's introduce Wong's method by beginning with the model specification. Let's define p = prices of good 2, w = prices of labor = wage, r = prices of capital = interest rate, k = the amount of foreign capital in the home country (negative for the amount of national capital in the foreign country), x_i = production of good i by the (national and foreign) factors in the economy, and c_i = the consumption of good i by the nationals (i = 1, 2). We denote foreign variables with asterisks. Suppose the following situations obtain (Wong 1986, pp. 27-8):

1. A world of two countries—the home country (h) and the foreign country (f)—initially endowed with a fixed amount of two factors: labor and capital.

2. The home country has a higher rental rate, i.e., $r^h > r^f$.

3. In autarky, both countries produce good 1 and good 2. Good 2 is the cheaper good in the home country (h), assuming a closed economy, i.e., $p^1_h < p^2_h$.

4. Differences are allowed in factor endowments, preferences, and technologies, but technologies are those which exhibit constant returns to scale and no factor-intensity reversal occurs in any prices.

5. The home country's export (import if negative) of good 1 is

$$E(p, k) = x_1(p, k) - c_1(p, I) - r(p, k)k \quad (5.2)$$

where I = national income, defined as total domestic output - payments to foreign capitalists.

To simplify formula (5.2), we further assume that in the absence of factor-intensity reversal, a one-to-one correspondence exists between factor prices and commodity prices. Thus, we can write $w = w(p)$, $r = r(p)$, and $I = I(p;\ \tilde{L},\ \tilde{K}) = w(p)\tilde{L} + r(p)\tilde{K}$, where $\tilde{L}$ and $\tilde{K}$ are the given labor and capital endowments of the economy. Thus, we can derive simplified (5.2) when the economy is diversified:

$$E(p, k) = x_1(p, k) - c_1(p) - r(p)k \quad (5.2a)$$

Next, we derive simplified (5.2) when the economy is completely specialized in producing good 1. Because only good 1 is produced, there will be no effect of the price of good 2 (= p) on the production of good 1 (x_1) and on the price of capital; thus, p is removed from the production function of x_1 and the interest rate function of r. So the simplified function of the home country's export of good 1 when the economy is completely specialized in producing good 1 is as follows:

$$E(p, k) = I - c_1(p, I) \quad \text{where } I = x_1 - rk \quad (5.2b)$$

Given that there is no export of good 1—i.e., $x_1(p, k) = 0$—and that the consumption of good 1 (c_1) depends upon import, the simplified function of the home country's export of good 1 when the economy is completely specialized in producing good 2 can be derived as follows:

$$E(p, k) = -c_1(p, I) \quad \text{where } I = (px_2 - pr_2k) \quad (5.2c)$$

Formulas (5.2a), (5.2b), and (5.2c) are used below to derive the sign pattern of $E_k = \partial E/\partial k$—i.e., the home country's export of good 1 when capital is traded.

5.5.2 Outline of Wong's General Equilibrium Approach

Wong's general equilibrium approach examines how the interaction between the world's commodity markets and the world's capital market determines simultaneously the relation between commodity trade and factor trade—i.e., whether they are substitutes or complements. In this approach, schedule GT, whose slope is denoted as S_G, depicts the equilibrium of the world's commodity markets under all possible combinations of E and k on an (E, k) plane. Schedule KM, whose slope is denoted as S_K, depicts the equilibrium of the market in the world's capital—the traded factor—under all possible combinations of E and k on an (E, k) plane. We will also see that some necessary and sufficient conditions for substitutability and complementarity between goods trade and factor mobility can be established by observing the behavior of the slope of schedule GT and that of schedule KM simultaneously.

To examine the behavior of the slopes of these two schedules, we must first determine the sign patterns of the slope of each schedule. However, these sign patterns can be determined only after we determine all the sign patterns of the variables that exist in the formula of the slope.

In the following sections, we will first see that Wong examined the condition for capital flow to have negative effects on the volume of commodity trade—i.e., the condition for $\dot{E}_k < 0$. Given that $E_k < 0$, Wong then moved on to derive the slope of schedule GT from three equilibrium conditions of the world's commodity markets. By using the same sign pattern of E_k, Wong also derived the slope of schedule KM from three conditions of the world's capital market. Finally, by observing the pattern of the intersection of schedules GT and KM, Wong derived the necessary and sufficient conditions for substitutability and complementarity between goods trade and factor trade.

5.5.3 Effects of Capital Flow on the Volume of Commodity Trade

Consider under what condition investment from abroad—i.e., the capital inflows from foreign countries—will have negative effects on the volume of commodity trade in good 1; that is, under what condition the sign of $E_k = \partial E/\partial k$ is less than zero. Proposition 1 in Wong's paper (1986, pp. 28) provided a condition for $E_k < 0$ in all three cases represented in formulas (5.2a), (5.2b), and (5.2c).

> *Proposition 1.* The case in which [if either]
> (a) the economy is diversified, or
> (b) the economy is completely specialized in the capital-intensive good and is a capital-receiving country, or
> (c) the economy is completely specialized in the labor-intensive good and is a capital-sending country[,]
> [then] [m]ore investment from abroad will, under constant terms of trade [i.e., under constant commodity prices], lead to a smaller export of good 1: E_k is negative if and only if good 1 is labor-intensive.

The condition for $E_k < 0$ is that good 1 is a labor-intensive good. Note that Wong's proposition 1 has already gone beyond the H-O-S model in that the economy in question is allowed to be completely specialized in producing only one good and to have capital inflows from foreign countries but still obtain the similar conclusion that is indicated in the theorem of factor-price equalization—i.e., commodity trade and factor trade tend to have a negative relationship. The purpose of identifying both the sign pattern of E_k, which is negative, and the factor-content of good 1, which is labor-intensive, under the three different cases specified in (a), (b), and (c) of proposition 1, is to attempt to clarify the further conditions that are to be used to ensure the derivation of the sign patterns of the slopes of both schedule GT and schedule KM.

5.5.4 Equilibrium of the World's Commodity Markets

According to Wong's proposition 1 we assume that (1) good 1 is labor-intensive in both countries and (2) both countries are diversified. Thus, the equilibrium of the world's good 1 market in the presence of capital movement requires the following three equilibrium conditions (Wong 1986, pp. 29):

$$E(p, k) + E^*(p^*, k^*) = 0 \tag{5.3}$$

$$k + k^* = 0 \tag{5.4}$$

$$p = p^* \tag{5.5}$$

Substituting (5.4) and (5.5) into (5.3), we have

$$E(p, k) + E^*(p, -k) = 0 \tag{5.3a}$$

Equation (5.3a) gives possible combinations of E and k that equilibrate the international goods market at the indicated price of good 2. Thus, all these possible combinations of E and k construct a schedule (GT) in a coordinate system with E or -E* as the vertical axis, and k or -k* as the horizontal axis.

By, first, totally differentiating (5.3a) and the export equation E = E (p, k) and, second, substituting the conclusion of the total differentiation of (5.3a) into the conclusion of the total differentiation of the export equation, we obtain the formula of the slope of schedule GT:

$$dE/dk|_{GT} = (E^*_{k^*})E_p + E_k(E^*_{p^*}) / E_p + E^*_{p^*} \tag{5.6}$$

Recall that, from proposition 1, E_k is supposed to be negative—i.e., $E_k = \partial E/\partial k < 0$. Wong pointed out that, in a Walrasian sense, the stability of the system of the international market of good 1 at any level of capital movement requires that $(E_p + E^*_{p^*}) < 0$. This inequality brings about two situations: $E_p < 0$ and $E^*_{p^*} < 0$, or either of them is positive but not of a significant magnitude. Thus, Table 5.2 summarizes the conditions for schedule GT to be negatively sloped—i.e., the conditions for the value of formula (5.6) to be negative—if good 1 is labor-intensive in both countries.

[Please refer to Table 5.2 on page 154-1]

Based on this information, these combinations of equilibrium E for various values of k are plotted in Figure 5.3 as negatively sloped schedule GT. The vertical axis in Figure 5.3 represents domestic export (E) or foreign import (-E*) of good 1. The horizontal axis represents the amount of foreign capital in the home country (k, -k*).

[Please refer to Figure 5.3 on page 154-1]

5.5.5 Equilibrium of the World's Capital Market

In the same manner as we did in section 5.5.4, we can derive another schedule, KM, to represent the equilibrium conditions of the capital market. The equilibrium of the capital market requires the following three conditions (Wong 1986, 31):

$$E(p(r), k) + E^*(p^*(r^*), k^*) = 0 \quad (5.3b)$$

$$k + k^* = 0 \quad (5.4)$$

$$r = r^* \quad (5.7)$$

Substituting (5.4) and (5.7) into (5.3b), we have

$$E(p(r), k) + E^*(p^*(r), -k) = 0 \quad (5.3c)$$

Equation (5.3c) gives possible combinations of E and k that equilibrate the international capital market at the indicated interest rate. Thus, all these possible combinations of E and k will construct a schedule (KM) in the same coordinate system where GT lies.

By, first, totally differentiating (5.3c) and the export equation E = E (p(r), k) and, second, substituting the conclusion of the total differentiation of (5.3c) into the conclusion of the total differentiation of the export equation, we obtain the formula of

Table 5.2: Conditions of Negative Slope for Schedule GT If Good 1 Is Labor-Intensive in Both Countries

E_p	$E^*_{p^*}$	E_k	$E^*_{k^*}$	$E_p+E^*_{p^*}$	$E_pE^*_{k^*}$	$E^*_pE_k$	$E_pE^*_{k^*}+E^*_{p^*}E_k$	dE/dk
<0	<0	<0	<0	<0	>0	>0	>0	<0
>0(wm)	<0(sm)	<0	<0	<0	<0(wm)	>0(sm)	>0	<0
<0(sm)	>0(wm)	<0	<0	<0	>0(sm)	<0(wm)	>0	<0

wm = weak magnitude

sm = strong magnitude

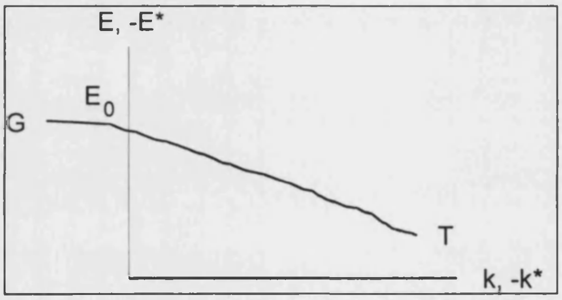


Figure 5.3: Map of GT

the slope of schedule KM:

$$dE/dk|_{KM} = E_p p_r E^*_{k*} + E^*_{p*} p^*_{r*} E_k / E_p p_r + E^*_{p*} p^*_{r*} \quad (5.8)$$

Recall from proposition 1 that E_k is supposed to be negative—i.e., $E_k = \partial E/\partial k < 0$. Note that because both economies are diversified, E_k and p_r, E^*_{k*} and p^*_{r*} always have opposite signs. The Walrasian stability also holds in the international capital market; it requires that $(E_p + E^*_{p*}) < 0$. This inequality brings about two situations: $E_p < 0$ and $E^*_{p*} < 0$, or either of them is positive but not of a significant magnitude. Table 5.3 summarizes the conditions for schedule KM to be negatively sloped—i.e., the conditions for the value of formula (5.8) to be negative—if good 1 is labor-intensive in both countries.

[Please refer to Table 5.3 on page 155-1]

Figure 5.4 shows a plot of these combinations of equilibrium E for various values of k plotted as negatively sloped schedule KM. The vertical axis represents domestic export (E) and foreign import (-E*) of good 1, and the horizontal axis represents the amount of foreign capital in the home country (k, -k*).

[Please refer to Figure 5.4 on page 155-1]

We can now combine Figure 5.3 and Figure 5.4 to create Figure 5.5. (Wong 1986, p. 31, here simplified) Figure 5.5 depicts the intersection point, point w, between schedules GT and KM. Point w represents the equilibrium of the world's good 1 and capital markets. Furthermore, according to the notion revealed in Figure 5.5, we can develop the necessary and sufficient conditions for substitutability and complementarity between goods trade and factor mobility by observing the slope

Table 5.3: Conditions of Negative Slope for Schedule KM If Good 1 Is Labor-Intensive in Both Countries

E_p	p_r	$E_p p_r$	E^*_{p*}	p^*_{r*}	$E^*_{p*}p^*_r$	$E_pP_r+E^*_{p*}p^*_{r*}$
<0	>0	<0	<0	>0	<0	<0
<0(sm)	>0	<0(sm)	>0(wm)	>0	>0(wm)	<0
>0(wm)	>0	>0(wm)	<0(sm)	>0	<0(sm)	<0

E^*_{k*}	$E_p p_r E^*_{k*}$	E_k	$E^*_{p*}p^*_{r*}E_k$	$E_p p_r E^*_{k*}+E^*_{p*}p^*_{r*}E_k$	dE/dk
<0	>0	<0	>0	>0	<0
<0	>0(sm)	<0	<0(wm)	>0	<0
<0	<0(wm)	<0	>0(sm)	>0	<0

wm = weak magnitude

sm = strong magnitude

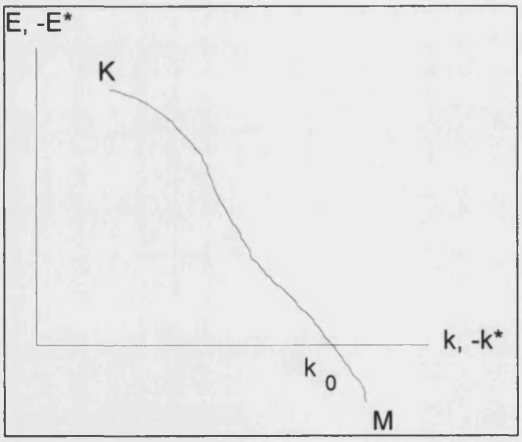


Figure 5.4: Map of KM

behavior of schedules GT and KM.

[Please refer to Figure 5.5 on page 156-1]

5.5.6 Necessary and Sufficient Conditions for Substitutability and Complementarity between Goods Trade and Factor Mobility

In Figure 5.5, points E_0, k_0, and w have their own meanings. Point E_0, with a coordinate of (0, E_0), represents the level of domestic export of good 1 under free commodity trade but no capital mobility. Point k_0, with a coordinate (k_0, 0), represents the level of foreign investment in the home country under free capital mobility but autarky in commodity trade. Point w, with a coordinate (E_w, k_w), represents the level of domestic export of good 1 and that of foreign investment in the home country under free trade and capital mobility.

In terms of E_0, k_0, E_w, and k_w, we quote the following definitions from Wong's paper (1986, p. 33, with renumbering):

(i) Capital mobility diminishes (augments) goods trade if and only if the volume of trade under free goods trade and capital mobility is smaller (greater) than the volume of trade under free trade but no capital mobility—i.e., if and only if $E_w < (>) E_0$.

(ii) Goods trade diminishes (augments) capital mobility if and only if the amount of capital transfer under free goods trade and capital mobility is smaller (greater) than the amount of capital transfer under free capital mobility but autarky in trade—i.e., if and only if $k_w < (>) k_0$.

(iii) Goods trade and capital mobility are substitutes if and only if they diminish each other.

(iv) Goods trade and capital mobility are complements if and only if they

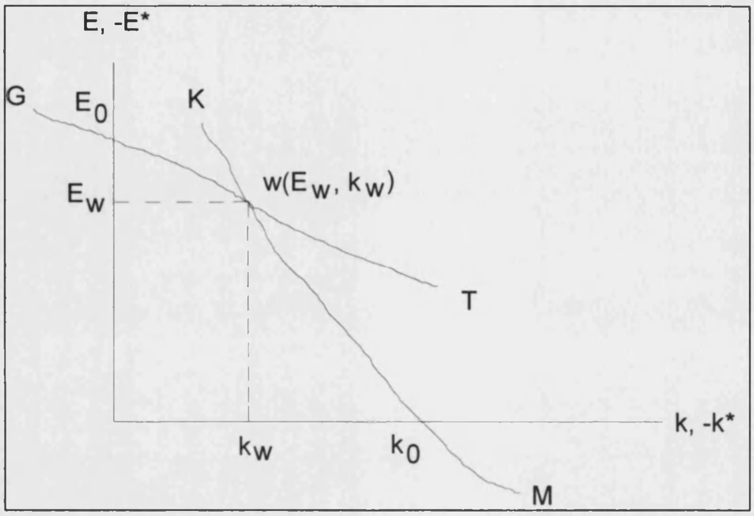


Figure 5.5: A Possible Intersection of Schedule GT and KM

augment each other.

We can connect points E_0 and w to form line E_0w (not shown) with slope S_G, and connect points w and k_0 to form line wk_0 (not shown) with slope S_K. S_G can be calculated as follows. We have

$w = w(k_w, E_w)$ and $E_0 = E_0(0, E_0)$

Thus,

$S_G = (E_w - E_0) / (k_w - 0)$

$$\Rightarrow S_G k_w = E_w - E_0 \quad (5.9)$$

In the same manner, we can obtain S_K as follows. By transforming k_w and E_w,

$w = w(k_w, E_w) \rightarrow w(E_w, k_w)$

$k_0 = k_0(k_0, 0) \rightarrow k_0(0, k_0)$

Thus,

$S_K = (k_w - k_0) / (E_w - 0)$

$$\Rightarrow S_K E_w = k_w - k_0 \quad (5.10)$$

Table 5.4 summarizes the sign structure among S_G, k_w, E_w, and E_0 for depicting the change in the equilibrium levels of goods trade in (5.9), and Table 5.5 summarizes that among S_K, E_w, k_w, and k_0 for explaining the change in the equilibrium levels of capital flow in (5.10). (Wong, 1986, p. 34, here rearranged)

[Please refer to Table 5.4 and Table 5.5 on page 157-1]

Figure 5.6 depicts the possible maps for conditions (1) and (4), (2) and (5), and (3) and (6) in Table 5.4 and 5.5.

[Please refer to Figure 5.6 on page 157-1]

Table 5.4: Sign Structure for $S_G k_W = E_W - E_0$

(1) $E_W > E_0$	iff	$S_G k_W > 0$	or iff	S_G and k_W have the same sign
(2) $E_W = E_0$	iff	$S_G k_W = 0$	or iff	either S_G or k_W is zero
(3) $E_W < E_0$	iff	$S_G k_W < 0$	or iff	S_G and k_W have different signs

Table 5.5: Sign Structure for $S_K E_W = k_W - k_0$

(4) $k_W > k_0$	iff	$S_K E_W > 0$	or iff	S_K and E_W have the same sign
(5) $k_W = k_0$	iff	$S_K E_W = 0$	or iff	either S_K or E_W is zero
(6) $k_W < k_0$	iff	$S_K E_W < 0$	or iff	S_K and E_W have different signs

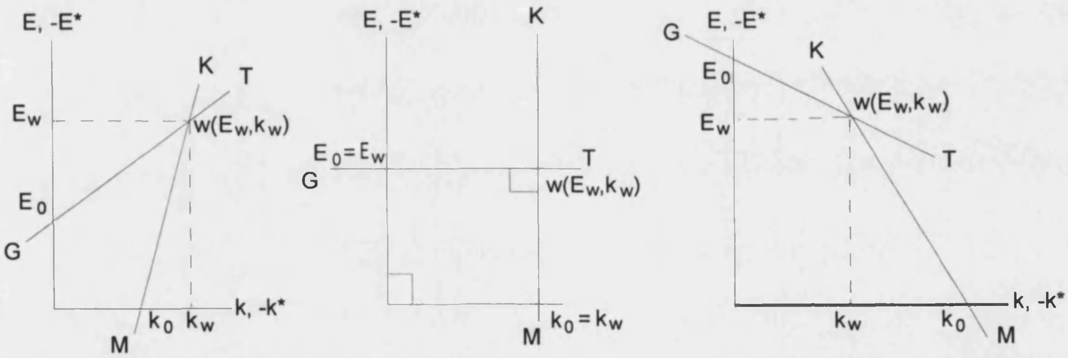


Figure 5.6: Possible Map for (from left to right) Conditions (1) and (4), (2) and (5), and (3) and (6)

By using definitions (i) through (iv) and conditions (1) through (6), we can infer the following three outcomes (Wong 1986, p. 34, here rearranged):

(a) The slope of schedule GT (= S_G) and that of schedule KM (= S_K) have the same sign.

(b) Foreign capital will tend to flow in under free trade if r_0 (home price of capital or home interest rate under free commodity trade but capital immobility) > r^*_0 (foreign price of capital or foreign interest rate under free commodity trade but capital immobility), and k_w will be positive; thus, (r_0 - r^*_0) has the same sign pattern as k_w does.

(c) E_w (export of good 1) will be positive if p_0 (price of good 2 in home country under free capital movement but autarky in commodity trade) > p^*_0 (price of good 2 in foreign country under free capital movement but autarky in commodity trade)—i.e., (p_0 - p^*_0) has the same sign pattern as E_w does.

Finally, by combining definitions (i) through (iv), conditions (1) through (6), and outcomes (a) through (c), we can derive the necessary and sufficient conditions for substitutability and complementarity between goods trade and capital mobility when capital is mobile between countries, which we quote from Wong (1986, p. 34):

> *Proposition 2.*
> (a) Capital mobility diminishes (augments) goods trade if and only if sign (slope of schedule GT) ≠ {=} sign (k_w) = sign (r_0 - r^*_0);
> (b) Goods trade diminishes (augments) capital mobility if and only if sign (slope of schedule KM) ≠ {=} sign (E_w) = sign (p_0 - p^*_0);
> (c) Goods trade and capital mobility are substitutes if and only if (i) sign (slope of schedule GT) ≠ sign (k_w) = sign (r_0 - r^*_0); and (ii) sign (slope of schedule KM) ≠ sign (E_w) = sign (p_0 - p^*_0);
> (d) Goods trade and capital mobility are complements if and only if (i) sign (slope of schedule GT) = sign (k_w) = sign (r_0 - r^*_0); and (ii) sign (slope of schedule KM) = sign (E_w) = sign (p_0 - p^*_0).

5.5.7 Wong's Empirical Testing and a Summarized Comment

In addition to this systematic theoretical treatment of the issue, Wong conducted, in a 1988 paper (Wong in Robert C. Feenstra (ed.) 1988, pp. 231-50; for a succinct summary of this empirical research, refer to Bowen, Hollander, and Viaene 1998, pp. 244-5), the first empirical research on the effects of factor movements (including labor and capital movements) on the volume of commodity trade and factor prices. In this study, Wong estimated the indirect trade utility function of the United States from the data over the period 1948-83, and then, using the results derived from this estimation, he calculated the elasticities of exports and imports with respect to factor supply to see whether these elasticities are negative or positive and thereby determine whether factor mobility and commodity trade are substitutes or complements. The simple idea is this: In the case of substitutability, elasticity is negative; in the case of complementarity, it is positive.

Wong found that, in the case of the United States during this period, the signs of the elasticities are mostly not only positive but also significantly greater than 0. This means that factor mobility and commodity trade are strongly complement to each other; whenever there is an increase in the foreign factor supply to the United States, this foreign factor supply will cause an increase in the volume of U.S. trade with foreign countries. This conclusion runs directly counter to the conclusion derived from the standard H-O-S model, which states that factor mobility and commodity trade are substitutes. But, as is shown in Wong's theoretical model, this result should not surprise trade theorists because it is a case that is already included in Wong's model—a relatively most complete model with respect to the general equilibrium framework of the H-O-S tradition.

One important conclusion regarding the nature of the method of empirical causal inference can be derived. Consider both Minhas's empirical test for determining the prevalence of the phenomenon of factor-intensity reversal and Wong's empirical test for substitutability or complementarity between factor mobility and commodity trade. Notice that each theorist devises his own approaches or criteria, with respect to the real situations they faced, to test for the existence or direction of the influence of the additional causal factors that are originally impounded in the H-O-S model. Our observation of the ways adopted by Minhas and Wong in their empirical testing seems to suggest that there is no single general empirical methodology of causal inference, nor is there a single general criterion that can be applied in all cases in all situations to determine the existence of the influence of certain causal factors. This suggestion is also applicable to Kevin Hoover's empirical approach for determining causal ordering (discussed in Chapter 3).

Let's return to the example of identifying the causal ordering between money and price in Chapter 3. Hoover's own practice has demonstrated that his structural approach of causal inference, which detects the causal asymmetries of these orderings by observing the patterns of the asymmetries of alternative conditional probability distributions, should be regarded as only *a part* of an entire procedure of economic theorizing. This is a part that is to be used, depending on the knowledge collected from the other parts of this entire theorizing procedure, to test the causal ordering of a *specific* case in a *specific* situation. Recall that Minhas and Wong cannot make definite causal conclusions simply by depending on their empirical testing approaches. Similarly, Hoover also reached his conclusions with the help of a conglomerate of knowledge, including the knowledge derived from his own empirical testing approach, the background causal knowledge provided by the monetary theories, and, most importantly, the detailed knowledge of the real situation of the economic phenomenon in question—i.e., in Hoover's case, the knowledge of the real situation of the U. S.

money market over the period 1950-1985. The main import here is that whenever there is a change in the knowledge of some part of the theorizing procedure, the existing empirical method being used to make causal inference may turn out to be inadequate under the condition of this new background knowledge, and the method may be replaced by another, more pertinent method.

As a result, whenever an empirical approach to causal inference has succeeded in identifying the causal influence of a specific cause in a specific economic phenomenon, the case should attest only that this empirical approach is adequate for use only in the case of this specific economic phenomenon—no more and no less. Any attempt to generalize the applicability of this empirical approach beyond the limit of a specific case should itself be tested for validity by other, independent approaches or ideas.

5.6 Conclusion

Instead of facing the problem of selecting which main assumptions in the H-O-S model to drop, as Markusen faced in his model-building, Wong chose to sidestep this problem by dropping all assumptions in the H-O-S model and then constructing, under the H-O general equilibrium system, a relatively most complete model whose conclusions can be used to identify when commodity trade and factor mobility are substitutes or complements. The lesson that we learn from comparing the theoretical structures of the H-O-S model, the Markusen models, and the Wong model (for a schematic comparison of these models, refer to Appendix A) is this: It is possible for theorists to develop full-fledged causal models *with respect to* a complete causal system that is hypothesized by these theorists themselves (or by other theorists who are from the same tradition) in an attempt to represent the main causal features of a class of real phenomena.

The entire procedure to produce such fully developed causal models can be summarized as follows: When theorists find that some real causal phenomena, which are originally supposed to be explainable or predictable by the theorists' causal models, cannot in fact be explained or predicted by them, the theorists generally start to think about which assumptions specified in their theories do not hold in the phenomena. These theorists often then regard their causal models as being too simple compared with the causal system that is hypothesized in theories by themselves or their colleagues. They then respecify their simple causal models to accommodate more causal factors, released from the theoretical assumptions, to make their models more complete and so more concrete. At this stage, they are still restructuring their causal models within a limited domain—i.e., within their hypothesized complete causal system. These theorists journey toward establishing increasingly complete causal models by adding increasing numbers of causal factors that can be released from the theoretical assumptions. In the end, they may reach the point that all imaginable causal factors presented in their theories are exhausted. When this situation is reached, these theorists' causal models can then be regarded as the relatively most complete causal models relative to their hypothesized complete causal system.

One question arises: Are these relatively most complete causal models models of everything? That is, can these models be used as general models to explain and predict every economic causal phenomenon in the real world? No. Notice that these causal models are relatively most complete only with respect to a specific domain—i.e., only with respect to the hypothesized complete causal system. When new causal factors occur in the real world that are missing from the original hypothesized complete causal system but now involve an economic phenomenon in question, these relatively most complete causal models cannot explain or predict the economic phenomenon in question. What, then, can the theorists do? At this stage, they must employ any possible means to ascertain relevant causal knowledge of the new causal factors. When they have obtained enough background causal knowledge of the new causal factors, they can then proceed to enlarge the domain of their hypothesized complete causal system by adding the factors. Then a new round of causal model structuring and restructuring with respect to a new hypothesized complete causal system runs again. The entire procedure continues infinitely.

People may ask, Does this description of causal modeling suggest that it may be possible to establish a relatively most complete causal model at each stage, but it is not possible to have *the* most complete causal model in general—one that can be applied to explain or predict anything in the real world, even in the long run? Yes, this is exactly what I mean. Then isn't this too pessimistic a picture of science? No, not at all. This is an exact picture of what science is for. Science, including economics, continues to exist for the purpose of discovering new causal factors in different stages in an attempt to establish the relatively most complete causal model at each stage. The pleasure of scientists and economists comes from their achievements in establishing the relatively most complete causal model at each stage. Here is what Paul A. Samuelson said in a keynote address presenting an anthology of recent papers on new directions in trade theory (Samuelson in Levinsohn, Deardorff, and Stern (eds.) 1995, p. 22):

> A science seeks perfection and closure. But success brings dull complacency. To the degree that challenging problems remain to be solved, a science stays vital and exciting.
>
> By this test, the theory of international trade is young and lusty. Our platter is full of delicious challenges. In science as elsewhere, it is

> better to travel than to arrive. And [the various new models of international trade] attest that trade theory is very much on its way.

Yes, far from being over, the journey has only begun, and there is still a long way to go. Economic theorizing, like any other scientific theorizing, engages in an endless journey of searching for the relatively most complete causal models in different stages of theory development.

Appendix A

Assumptions, Methodology, and Conclusions of the Four Models

The controversy concerning the relationship among factor endowments, the pattern of trade in goods, and factor mobility is rooted in the extensions and reinterpretations made by Markusen, Svensson, and Wong of the two powerful propositions made in the Heckscher-Ohlin-Samuelson model. These propositions are the Heckscher-Ohlin theorem, which posits a strong positive connection between factor endowments and the pattern of trade in goods, and the factor-price equalization theorem, which states that trade in goods equalizes factor rewards completely and thus serves to some extent as a substitute for factor mobility.

The criticisms, however, generally are aimed at the assumptions and methodologies of the Heckscher-Ohlin-Samuelson model, not the model's overall structure. The following comparative analysis delineates the main assumptions and methodologies of the models discussed in Chapter 5. The outline of Svesson's model is also presented, although it is not discussed in detail in Chapter 5.

The Heckscher-Ohlin-Samuelson Model

• Main assumptions: (1) different factor endowments (which forms the trade bases), (2) different or same factor rewards, (3) identical technology, (4) identical tastes, (5) factor immobile, (6) incomplete specialization in production, and (7) 2 countries × 2 factors × 2 commodities.

• Methodology: the orthodox theoretical logic

The Rybczynski theorem → the Heckscher-Ohlin theorem → the Stolper-Samuelson theorem → the factor-prices equalization theorem is used to establish the following two proposition.

• Conclusion: (1) a strong positive connection between factor endowments and trade in goods and (2) trade in goods and factor mobility are substitutes.

Markusen's Models

• Main assumptions: (1) Begin with same factor endowments, (2) identical tastes, (3) factors mobile, and (4) 2 countries × 2 factors ×2 commodities.

• Methodology:

(1) Assume same factor endowments.

(2) Relax each one of the following assumptions, respectively, in the Heckscher-Ohlin model to establish a situation of different factor rewards in each individual model: (i) identical technology, (ii) constant returns to scale, (iii) perfect competition in both product and factor markets, and (iv) free trade.

(3) Different factor rewards drive factors to move between countries, thereby increasing factor mobility.

(4) Thus, a situation of different factor endowments obtains.

(5) A factor-proportion basis is formed to reinforce the other basis for commodity trade.

(6) Accordingly, commodity trade increases.

• Conclusion: Goods trade and factor mobility are complements.

Svensson's Model

• Main assumptions: (1) begin with same factor endowments and same factor rewards, (2) identical technology, (3) identical tastes, (4) factors mobile, and (5) 2 countries × 2 factors ×2 commodities.

• Methodology:

(1) Assume same factor endowments and rewards between the two countries.

(2) Before the change in the endowment of the home country, trade in both goods and in some factors is allowed.

(3) The initial equilibrium will be zero trade in goods and factors.

(4) However, the change in home country endowment will result in a different equilibrium with generally nonzero trade in both goods and factors.

• Conclusion: Goods trade and factor trade tend to be substitutes (complements) if traded and nontraded factors are cooperative (noncooperative).

Wong's Model

• Main assumptions: (1) different factor endowments, (2) different factor rewards, (3) possible different technology, (4) possible different tastes, (5) factors mobile, and (6) 2 countries $\times$ 2 factors $\times$ 2 commodities.

• Methodology: Assume that there are only two factors in the world—one is internationally mobile and the other one is immobile—to establish Wong's own general-equilibrium system. In this system, two schedules are depicted. One schedule represents the equilibrium of the world's commodity markets, and the other schedule represents that of the world's traded-factor market, under all possible combinations of the level of the home country's export of nontraded factor-intensive good and the level of the amount of foreign traded factor in the home country. By observation of the intersecting point of these two schedules and the behavior of the slopes of these two schedules simultaneously, some necessary and sufficient conditions for substitutability and complementarity between goods trade and factor mobility can be established.

• Conclusion: Necessary and sufficient conditions have been developed for substitutability and complementarity between goods trade and factor mobility.

Summary

From these brief outlines it is clear that even those authors, such as Markusen, who most sharply disagree with the traditional Heckscher-Ohlin-Samuelson ideas, still depend heavily on the conventional theoretical assumptions and logic to develop their own models, although they argue that either the assumptions or the logic can be relaxed. Markusen relaxed the conventional assumptions but retained the traditional theoretical logic to develop a conclusion that differs significantly from the traditional ones. To develop his own middle-ground conclusions, Svensson held all of Heckscher-Ohlin's assumptions except factor immobility and conducted the logic using procedures that differ from those in the traditional model. A more interesting case is Wong's paper. By relaxing all the main assumptions in the Heckscher-Ohlin-Samuelson model, Wong developed the equilibrium system originally stipulated in the Heckscher-Ohlin theory to establish the necessary and sufficient conditions for deciding substitutability or complementarity between goods trade and factor mobility.

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