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Capacity-Based Account of Theory-Building:

A Case of Two-Stage-Theory-Test Process in Economics[†]

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In contrast to some prominent methodologists maintain that the practice of economic theory-building follows a regularist methodology, this paper defends a capacity-based analytic piecemeal image of the so-called causal structuralist approach of theory-building in economics. This piecemeal image indicates that the process of restructuring the theoretical model in economics reflects a general picture of economic theorizing by showing that the failed predictions obtained from a two-stage-theory-test process constitute a piece of information that is fed back to the theoretical model as a clue to help economists manipulate a rearrangement of the model. By conducting such a model-rearrangement, economists can thus provide empirical content to their models in the process of theory-building with respect to various concrete cases; and this piecemeal characterization of the approach should be regarded as arising from the fact that although economists 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 can do is to try their very best to use the capacity-based analytic piecemeal method to uncover the

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causal structure case by case and step by step.

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1. Does Economic Theory-Building Fit with the Regularist View of Economic Explanation? – Two Accounts of Theory-Development in Economics

When Bertil Ohlin, one of the founders of modern international trade theory, stated in his 1933 book[1] 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, 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

1) Ohlin, Bertil, 1933, *Interregional and International Trade*, Cambridge, MA: Harvard University Press; Revised Edition, 1967. Excerpts are contained in W. R. Allen (ed.), 1965, *International Trade Theory: Hume to Ohlin*, New York: Random House, Chapter 7, pp. 167-202.

"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 causal thinking is directly relevant to the 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[2].

This effect on the meta-theory can be illustrated by using an example in the following. In his 1954 empirical research, Wassily W. Leontief conducted an input-output analysis by using U.S. economic data for 1947[3]. Leontief found that U.S. import-competing production

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2) For those methodologists who endorse the regularist view of economic explanation, natural laws are regarded as 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 is a representative regularist explanatory model, this 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 and the law statement—i.e., the lawful regularity—in the premises of the model should thus be discarded. 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.

3) Leontief, Wassily, 1954, "Domestic Production and Foreign Trade: The American Capital Position Re-examined," *Economia Internazionale*, Vol. 7, No. 1, pp. 3-32. Abridged edition reprinted in Richard E. Caves and

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 and Mark Blaug.[4)]

Harry G. Johnson (eds.), 1968, *A. E. A. Readings in International Economics*, Homewood, IL: Irwin, pp. 503-527.

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."[5] 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."[6]

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."[7] According

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4) For De Marchi's study, refer to: De Marchi, N., 1976, "Anomaly and the Development of Economics: The Case of the Leontief Paradox," in Spiro J. Latsis (ed.), 1976, *Method and Appraisal in Economics*, Cambridge and New York: Cambridge University Press, pp. 109-128. For Blaug's research, refer to: Blaug, Mark, 1992 (1980), *The Methodology of Economics or How Economists Explain*, 2nd edition, Cambridge University Press, Chapter 11.

5) Blaug, *ibid.*, p. 191.

6) Kenen, Peter (ed.), 1975, *International Trade and Finance: Frontiers for Research*, Cambridge, UK: Cambridge University Press, p. xii, excerpted from Blaug 1992, p. 190.

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')."[8] 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, 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

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7) De Marchi, *ibid.*, p. 123.

8) De Marchi, *ibid.*, p. 109-110.

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. The Concept of 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."[9] Following

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[9] For Harré's idea, refer to: Harré, Rom, 1970, "Powers," *British Journal for the Philosophy of Science* 21, pp. 81-101; Harré, Rom, and Edward H. Madden, 1973, "Natural Powers and Powerful Natures," *Philosophy* 48, pp. 209-230; Harré, Rom, and Edward H. Madden, 1975, *Causal Powers: A Theory of Natural Necessity*, Oxford: Blackwell. For Cartwright's idea, refer to: Cartwright, Nancy, 1989, *Nature's Capacities and Their Measurement*, Oxford University Press; Cartwright, Nancy, 1989, "A Case Study in Realism: Why Econometrics Is Committed to Capacities," *PSA 1988, Volume 2*: pp. 190-197; Cartwright, Nancy, 1998, "Capacities," in John B. Davis, D. Wade Hands, and Uskali Maki (eds.), 1998, *The Handbook of Economic Methodology*, Cheltenham, UK, and

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

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Northampton, MA, USA: Edward Elgar, pp. 45-48.

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 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.[10)]

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

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10) The description of economic modeling here may give the reader an impression that causal structuralist approach is an instrumentalist approach. But, the author would like to point out that this approach regards the power of an entity being real—i.e., the capacity of an entity is real—and the causal structure is hypothetical. The capacity is real because an entity carries its full capacity from situation to situation as long as there is no disturbing factor intervening to prevent the entity from exerting its full capacity; the causal structure is hypothetical because theorizers would never know about the genuine image of the complete causal structure from which the targeted phenomenon is derived, what theorizers can do is to use the piecemeal method suggested in this paper to patch up the most likely simulacrum of the genuine causal system. Based on this reinterpretation of causal structuralist approach, the author would like to suggest that the metaphysical position of the approach can be characterized as the so-called entity realism and the model of scientific explanation endorsed by the approach is the so-called simulacrum model of explanation.

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 has 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.

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.

3. What is the Use of *Ceteris Paribus* Clause 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.

4. What is Fundamental in Theory-Building for Economists? Economic Causal Law or Capacity Claim?

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.[11)]

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.

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11) It may seem that this paper, by making a distinction between these two approaches, defends a view that regularity view is not compatible with capacity-based approach, but the author would like to respond to the concern by pointing out: although there are a great number of differences between these two approaches, they are, instead of being incompatible to each other, in fact complementary to each other. The supporters of the capacity-based or structuralist approach do not deny that a regularity has its practical value of being able to be used in conducting an explanation of a targeted phenomenon as long as the regularity is a net result of a stable causal structure from which the phenomenon is supposed to be derived, what these supporters disagree is the presumption that a usable regularity is coming from nowhere—i.e., an applicable regularity is not relative to a certain stable casual structure—and is readily applicable to all the cases of the similar phenomena.

5. An Account of Two-Stage-Theory-Test Process 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: Economists establish their theoretical model 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? Economists usually would like to start by conducting 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.

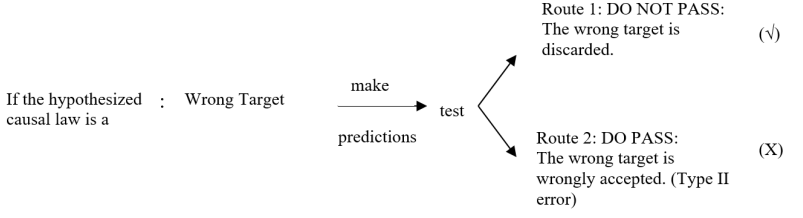
Imagine that these same economists did conduct a model specification test and got a positive result. In that case, their model specification test could be regarded as a *safeguard* to ensure that the result derived from a prediction test—in an attempt to see *whether* the prediction (or explanation) 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 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 1 shows this kind of mistaken decision by economists.



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

Figure 1: The Problem of Erroneously Accepting the Wrong Target

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. Or if the prediction 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. It should be noted 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 which is triggered by the structural inconsistency between two different causal structures.

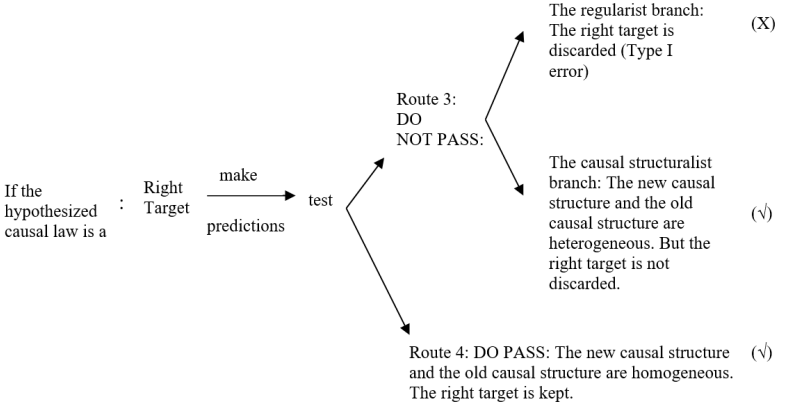


Figure 2: Three Possible Conclusions of the Second Round Test of the Right Target

It is obvious that the conclusions obtained in route 2 of Figure 1 and in route 3 of the regularist branch of Figure 2 can be likened to what statisticians call type II error and type I error, respectively. To see this, refer to Table 1.[12)]

12) Larsen, Richard J., and Morris L. Marx, 1986, *An Introduction to Mathematical Statistics and Its Applications*, Englewood Cliffs, NJ: Prentice-Hall, p. 299.

Table 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)

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, and grid B to the regularist branch of Route 3 in Figure 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 1, and grid D to Route 1 in Figure 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.

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 1 and focus only on the routes suggested in Figure 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 1 and in Route 3 of the regularist branch of Figure 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, 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.[13)]

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13) One anonymous reviewer supposes that the author would support the view that theory-testing is an intrinsically integral part of theory-building. The author agrees with the reviewer's observation, the reason for taking such a position is that the author regards economic reasoning as conducting a bootstrapping methodology of leveraging out the hypothesis from data or evidences. For the characterization of the methodology, refer to what Nancy Cartwright described in her 1989 book: "It is not enough that a scientific theory should save the phenomena; its hypotheses must all be tested, and tested severally. This, then, is an empiricism opposed at once to wholism and to the hypothetico-deductive method. The logic of testing for such an empiricism is probably best modelled by Clark Glymour's boostrap theory of confirmation: the evidence plus the background assumptions deductively imply the hypothesis under test… Scientific hypotheses should be tested, and the tests should be reliable. The should be powerful enough to give an answer one way or another. The answers will only be as sure as the assumptions that ground the test." (Cartwright 1989, p. 5)

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

Ever since Leontief published the result of his empirical testing against the H-O theorem in 1954, 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.

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.

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.

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 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. 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.[14)]

As we have seen from the discussion of our 2×2×2 case, the cause

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14) For Minhas's study, refer to: Minhas, Bagicha Singh, 1962, "The Homohypallagic Production Function, Factor-Intensity Reversals, and the Heckscher-Ohlin Theorem," *Journal of Political Economy* 70: pp. 138-156; Minhas, Bagicha Singh, 1963, *An International Comparison of Factor Costs and Factor Use*, Amsterdam: North-Holland Publishing Company.

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. By using the data of 24 industries from 19 countries in the time period of 1948 (or 1949)-1958 to estimate the parameter for the prevalence of the phenomenon, Minhas indeed found that the phenomenon 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." (Minhas 1963, p. 53)

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.

Causal structuralist thought in economic theorizing takes a different approach. *No take-over is involved*. Consider why we need to propose the theory of two-stage tests in section 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. A corresponding part of the original causal picture is revised 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.

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.

7. Conclusion: Why Structuralist Approach? – It Reflects an Analytic Piecemeal Methodology in Economic Theorizing

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 paper, 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: 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.

As for the idea of concretization of economic theories, 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*.

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이론 구축에 대한 수용량 기반 설명:
경제학의 사례로 본 두 단계의 이론 테스트 과정

첸 슈팅

경제학의 이론 구축이 실제로 규칙주의 방법론을 따른다고 주장하는 몇몇 유명한 방법론자들이 있지만, 본 논문은 경제학의 이론 구축이 소위 인과 구조주의 접근법의 수용량 기반 분석이미지를 옹호한다. 이런 이미지는 경제학의 이론적 모형을 재구성하는 것이 일반적인 경제학의 이론화 방식을 반영하고 있음을 말한다. 그것은 특히 두 단계의 이론 테스트 과정에서 얻어진 실패한 예측이 경제학자들로 하여금 그 이론적 모형을 재배열하도록 조종하게 한다는 점에서 밝혀진다. 그와 같은 모형 재배열을 수행함으로써, 경제학자들은 다양한 구체적 사례들에 대한 이론 구축 과정에서 자신들의 모형들이 경험적 내용을 지니도록 한다. 이러한 접근법의 특징은 경제학자들이 경제현상의 인과 구조에 대해서는 전체론적 관점을 가지지만 그와 같은 인과 구조를 얻기 위해서 전체론적 접근법을 취하지 않는다는 사실에서 비롯된 것으로 간주되어야 한다. 따라서 경제학자들이 할 수 있는 일은 수용량 기반 분석 방법을 이용해서 경제현상의 인과 구조를 사례별로 그리고 단계별로 밝혀내는 데 최선을 다하는 것이다.

핵심어: 인과 수용량, 이론 구축에 대한 규칙주의 대 구조주의의 설명, 인과 구조, 세테리스 파리부스, 국제 통상 이론, 경제학 법칙, 두 단계의 이론 테스트 과정