The State of Theory in Relation to the Empirical Analysis

HARRY G. JOHNSON

LONDON SCHOOL OF ECONOMICS
AND UNIVERSITY OF CHICAGO

The decision to hold the Universities-National Bureau Conference on Technology and Competition in International Trade was largely motivated by the beliefs that empirical research on patterns of international trade had revealed and confirmed important technological influences which were not recognized in the established theory of comparative cost, and that in the interests of scientific progress it would be highly desirable to confront empiricists and theoreticians with a stock-taking of the current treatment of the technology factor in international economics. The purpose of this paper is to discuss, in the light both of the literature prior to the Conference and the papers prepared specifically for the Conference, how far and in what respects there may be said to be a conflict between the empirical findings and the theoretical models, and to suggest ways in which any such conflict may be reconciled in establishing new foundations for future scientific advance.

To begin with, it is, I think, important and necessary to recognize a crucial difference between the role of theory in the context of empirical

NOTE: This is a revised and extended version of the paper actually presented at the Conference. This paper and the first version draw heavily on ideas developed in two earlier attempts of mine to come to grips with the problem of the technology factor in trade, my Wicksell Lectures of March 1968 [5] and my International Economic Association World Congress paper of September 1968 [6]. In discussing the state of theory in relation to the technology factor, however, I have regarded these efforts as not belonging to the literature relevant to the Conference.

research, and its role in economics generally.[1] In the context of economics as an empirical science, the function of theory is to cast up empirically testable and refutable explanatory hypotheses, and the value of a theory is to be judged by its explanatory power in comparison with its rivals. In the broader context of economics as a systematic approach to the understanding of economic phenomena and as the organization of disciplined thinking about these phenomena and about policies relating to them, however, the purpose of theory is to abstract from the complexity of the real world a simplified model of the key relationships between dependent and independent variables, and to explore the positive and normative implications of changes in the "givens" of this hypothetical system. For this purpose, the validity of the empirical foundations of a theory is, obviously within limits, not of such crucial importance, in the sense that the principles of interrelatedness, of systematic response to change, and of optimization remain valid in the face of wide variations in assumed economic structure.

The theory of international trade has always been primarily theory in the second sense. Specifically, it has not been much concerned with the empirical problems of predicting or prescribing which goods will or should be traded by particular countries, or of specifying the characteristics of such goods. Instead, it has tended to draw on prevailing models of the domestic economy, and particularly of the domestic production system, as the bases for analysis of the positive and normative aspects of trade between national economies with differing production opportunities. In this connection, the central contribution to both positive and normative economics of the theory of international trade has been the principle of comparative cost—a principle which is not dependent on any particular assumptions about the nature of production.

Although this way of constructing a theory is legitimate enough—again within limits—for the purposes at hand, it does court the definite risk of freezing into the theoretical apparatus a model of the production process which is gradually being revealed by further research conducted by specialists on domestic economics as inadequate, and consequently of appearing increasingly irrelevant, if not downright in error, as a guide to understanding, interpreting, and operating in the real world of experience. As is well-known, the theory of international trade

[1] The argument of this and the next two paragraphs was inspired by some comments by R. E. Caves on my World Congress paper.

retained the labor theory of value as an explanation of domestic production equilibrium long after that theory had been superseded by the marginalist revolution in the theory of domestic value. Moreover, the efforts of certain neoclassical trade theorists to "patch up" the labor theory of value with elements of marginalism and Marshallian partial equilibrium analysis resulted in an increasingly cumbersome theoretical structure, which was eventually discarded in favor of the Heckscher-Ohlin-Samuelson model of general equilibrium.

The Heckscher-Ohlin-Samuelson model—which, it should be noted, is in its contemporary form largely the work of Samuelson and disregards some of the more penetrating insights of the other two economists credited with its authorship—builds on the formalization of production theory in the mathematical concept of the production function. That concept, in its simplest textbook form of a constant returns-to-scale relation between output of product and inputs of the two homogeneous factors capital and labor, constitutes an extremely powerful but an equally extreme and restrictive simplification of production economics. Assuming perfect competition, international indentity of production functions and factors, nonreversibility of factor intensities, and international similarity of preferences, the constant returns-to-scale application to international trade theory leads to the dual key theorems—the differential factor-endowment explanation of trade theorem and the factor price equalization theorem. These are undoubtedly elegant theorems. But they are essentially mathematical theorems whose relevance and interest depend not only on the assumptions listed but more fundamentally on the relevance and usefulness of the production function concept itself.

The fundamental question, which has been raised in general by a large body of recent empirical and theoretical work on the economics of domestic industrial production and competition and on the role of technological change in economic growth, and which has been raised in particular by the recent empirical work on the technology factor in international trade, is whether orthodox international trade theory has once again reached a position of commitment to an antiquated and misleading model of the production process, which has to be rejected to further the progress of economic science.

This is a question which needs to be approached with considerable care, since there are natural pressures on everyone to arrive at an answer that justifies and if possible exalts his own choice of research activity, and it is only too easy to define the problem so as to arrive at a preconceived answer. For purposes of attack, orthodox theory can be identified with a specific textbook model designed to illustrate general principles but required by the critic to provide a detailed guide to empirical research and economic policy-making; for purposes of defense, orthodoxy can be endowed with a flexibility capable of absorbing any reasonable concept, or defined so broadly and vaguely as to be potentially consistent with all observed phenomena. These contrasting temptations can be illustrated by reference to certain papers prepared for this Conference.

Bruno, for example, identifies orthodoxy with the static analysis of the two-country model, assuming identical given production functions and no capital flows, and sets up in its place a dynamic linear programming optimization model for a single economy with foreign aid receipts as the key variable. This model, although ingenious in some respects and presumably useful to the Israeli planning authorities, not only involves the purely arbitrary constraints necessary to make a linear programming problem both interesting and computable, but so far as I can see contains nothing logically inconsistent with the application of orthodox theory to the same problem and also nothing that adds to our understanding of comparative advantage or the role of technology in determining and changing trade patterns.

On the other hand, Hall and Johnson assert, as if it proved something, that their paper illustrates that "international flows of technology can be studied profitably by means of conventional market-force analysis." In fact, however, all they show—though in admirably careful detail—is that technological transfers can be subjected to cost-benefit analysis. Their case, moreover, is a politically motivated, not market-directed, transfer; and their most striking finding is that, *contrary to prevailing opinion in the industry itself,* the transfer reduced the aircraft's cost of production. Apparently expert opinion in the industry overestimated the importance of engineering efficiency and underestimated the potential cost-saving from substituting cheaper Japanese for more expensive American labor. If such miscalculations of potential comparative cost are commonplace in industrial management circles, serious questions arise about (1) the amenability of technological transplantation to treatment as a rational economic process, and (2) the welfare implications of relying on the process of competition among large industrial firms to diffuse technical progress through the world economy.

Again on the side of orthodoxy, Ronald Jones' elegant paper demonstrates a point known to international trade theorists for over a decade,[2] that the Heckscher-Ohlin-Samuelson model can be readily adapted to place the emphasis of the analysis on technological differences, and goes on to discuss in a perceptive way how the approaches of recent research on the product cycle and the technology factor can be incorporated in formal theory. His analysis, however, markedly fails to capture the dynamic orientation of that research, but it does underline the key problem of making the connection between them the development of a satisfactory inducement mechanism for innovation. Moreover, the extension of the model in the ways he discusses reduces it perilously close to eclectic taxonomy.

The purely theoretical paper by Chipman goes beyond Jones' paper in applying recent developments in the theory of induced innovations in growth models of international trade. In so doing Chipman confirms the flexibility of theory and its power to absorb and apply ideas drawn from empirical work; but the paper's results are equally remote from the real stuff of the world of observation.

The fundamental problem, the conflict between the empirical findings and the theoretical models, can be seen in part as a continuation in the international trade field of the debate begun in the 1930's over the issue of monopolistic, as contrasted with perfect, competition—with the important difference that monopolistic competition is now viewed as a rational corollary of the evolution of technology in a free enterprise system rather than as a manifestation of consumer irrationality. The new empirical research stresses the influence on international trade patterns of factors determining monopolistically competitive ability—technological leadership, economies of scale, and product variation (non-standardization). The orthodox theoretical tradition stresses differences in the classical determinants of wealth—specifically, capital per head—on the assumption of a broadly competitive international economy.

[2] For an early extension of the model to the case of technical changes, see my [3].

It may be observed parenthetically that the consternation caused to traditional theorists by the publication of the Leontief paradox can in the light of hindsight be attributed in large part to a misspecification, based on the identification in the English classical and neoclassical tradition of "capital" with capital equipment, and "labor" with human bodies regardless of skill, of the empirical counterparts of the arguments on the production function. The publication of the paradox led immediately, as Hufbauer rather scornfully points out, to tortuous efforts to rescue the factor proportions theorem by appeals to differences in demand conditions, or to the possibility of factor-intensity reversals. However, in the longer run, and partly in response to the development and application elsewhere in theory and empirical research of the concept of human capital, the outcome has been the recognition of human skills as an important constituent of capital together with material equipment and structures. This recognition goes a considerable way towards resolving the Leontief paradox; and, as I have suggested elsewhere (5), the paradox could probably be fully dispelled—at least formally—by extending the concept of capital to include the capitalized value of productive knowledge created by research and development (R&D) expenditure.

It must be admitted, however, that no one has yet performed the required calculations; and also that, as both the Hufbauer paper and the Gruber and Vernon paper have carefully confirmed, the Leontief paradox in its original form still stands. An explanation of it has been provided by proponents of the R&D explanation of U.S. comparative advantage in the form of an inverse correlation between material capital intensity and R&D effort in the United States (see [1] and [7]). Such a correlation is not necessarily a law of nature: Hirsch's paper finds a direct correlation in the case of Israeli industry.

Returning to the general framework of the debate in terms of classical comparative cost theory versus monopolistic competition theory, the difficulty and unsatisfactoriness of that debate in its late-1930's form arose from the fact that monopolistic competition theory is partial equilibrium theory—and therefore permits the insertion of a great deal of empirical detail—whereas comparative cost theory is general equilibrium theory—and therefore is useful and manageable only when it can be kept reasonably simple. The rock on which attempts to apply monopolistic competition theory to international trade theory—initially a hopeful enterprise—foundered, was the inability of theorists to translate monopolistic competition concepts into forms relevant for general equilibrium analysis.[3]

A parallel problem has emerged in the contemporary confrontation between empirical research emphasizing various aspects of the technology factor and its dynamic manifestations in international competition, and the grand simplicity of the neoclassical general equilibrium model of comparative advantage. Most of the empirical studies have been concerned with particular industries or the trade of the United States—by no means a typical country. And this has left to individual judgment the question of whether the empirical findings are to be considered as frontal contradictions of prevailing theory, or as curious or exceptional cases that are not inconsistent with the central theoretical propositions but require merely a more sophisticated interpretation and extension of them.

Hufbauer in his excellent paper emphasizes the dangers of testing hypotheses against a limited range of commodities or countries and the necessity of comprehensive testing of hypotheses against one another on a common (and extensive) set of data. The Gruber and Vernon study, though not explicitly devoted to testing rival hypotheses against one another, uses a comparable methodological approach. Hufbauer's study finds that the crude factor proportions theory performs surprisingly well according to the tests he runs. But so, embarrassingly, does every other theory proposed by a competent observing economist. The theories that cannot be confirmed empirically, to generalize beyond Hufbauer's study, are those proposed either in support of anti-orthodox policy prescriptions, or in more or less direct contradiction to comparative advantage theory in the broad sense—theories not really motivated by the spirit of scientific research and progress in understanding. The same eclectic conclusion emerges from the study by Gruber and Vernon: R&D expenditure as a surrogate for the technology factor is demonstrably significant as a determinant of international trade patterns, but it is not a powerful enough influence to supersede more conventional explanations of these patterns.

These results should be encouraging to all honest men—"little truths

[3] On this point, see my [4].

in every nook" is an undramatic but satisfyingly scholarly interim position to have arrived at, and one that opens up rather than closes off new avenues of theoretical and empirical research. It is, however, somewhat discouraging for those whose preference is for simple theories and for decisive testing of rival hypotheses against one another—especially if such testing can shatter orthodoxy in favor of heterodoxy or confirm the superiority of empirical-inductive over abstract-deductive approaches to international economic research.[4]

In the summary of his paper Hufbauer expresses something of this discouragement by posing again what I have described as the central issue of the Conference, in terms of the alternative "neofactor proportions" and "neotechnology" accounts of the determination of trade patterns, and suggests hopefully that if returns to scale, product age, and product differentiation could be combined into a single characteristic "that characteristic might prove as powerful as Lary's single measure (value added per man) of human and physical capital in explaining trade flows."[5] However, he adds rather wistfully, "the neotechnology approach is not geared to answering the traditional questions of economic inquiry. It can as yet offer little to compare with Samuelson's splendid factor price equalization theorem."

These remarks of Hufbauer's reveal a certain schizophrenia involving belief in the methodology of empirical testing to rival hypotheses (tempered naturally enough by concern for the chances of victory for one's favorite candidate) on the one hand, and admiration for the formal elegance of traditional theory on the other. In my judgment, they both sell the neotechnology approach short and obnubilate the theoretical and empirical issues. I conclude with three extended comments on these remarks.

First, it seems to me, as a rank amateur in empirical research in this field, that part of the power of Lary's statistic is that it picks up not only the neofactor proportion elements of material and human capital, but also to some extent the neotechnology elements of scale economies, and

[4] Actually, the difference is a matter of degree rather than of kind, for even the most abstract theory starts from some postulates about the nature of reality derived, however remotely, from observation, while even the most inductive approach requires some sort of theory specifying relevant facts.

[5] Editor's Note: See also the note by Lary in this volume for tests applying the value added variable to Hufbauer's trade data.

of product age and differentiation insofar as these are reflected in selling prices. Thus a composite neotechnology index, if one could be constructed, would be entered in a biased test against the Lary statistic, if the latter were identified uniquely with the neofactor proportions account of the courses of trade. If hypotheses are to be tested against each other on the basis of single indexes, the indexes must represent the hypotheses clearly. I would further suggest that something more might be learned about the neotechnology account from closer study of international differences in the Lary index, though I have not thought of a detailed research procedure that might be applied.

Second, the "adversary procedure" of testing one hypothesis against another is a useful scientific procedure up to a point; but, when both hypotheses perform well and seem to be fairly evenly matched, it is not necessarily the best scientific procedure to send the challenger back to training camp with good advice on how to prepare for the next month. In the realm of ideas, a conflict of equally well (or equally imperfectly) supported hypotheses may be more fruitfully resolved by merger into a composite hypothesis.

Specifically, as I have suggested above and elsewhere [5], the impression of strong conflict between the neofactor proportions and neotechnology accounts of international trade may reflect merely the domination of trade theory by the narrow concept of capital as material equipment, inherited from the English classical, neoclassical, and now neo-Keynesian economists. The concept of capital considered relevant has now been extended to include human capital—an extension which the neotechnology proponents in their roles as adversaries of the factor proportions theory may have accepted too easily for their own good—and could easily be extended further to include intellectual capital in the form of productive knowledge. Such an extension would be fully consistent with Irving Fisher's approach to the relation between capital and income. In that context, the neotechnology school can be interpreted as insisting on the importance of productive knowledge as a form of capital that is to be included in the theory of production, rather than as advancing a theory of international trade patterns to rival the factor proportions theory. Further, the neotechnology school can be viewed as emphasizing the process of obsolescence as well as the international mobility of capital in the form of productive knowledge, an aspect of capital which has generally been ignored in the neofactor proportions models.

More generally, a synthesis of the presumed rival hypotheses could be sought conceptually through revisions of the basic concept of the production function employed in standard trade theory. The standard concept implies capital equipment used by homogeneous labor to produce a given product according to a given technology. A more relevant concept would envisage capital embodied in various forms—natural resources, equipment and structures, human skills, and the productive knowledge used to combine them—cooperating to produce bundles of want-satisfying characteristics embodied in products whose nature changes as knowledge accumulates and demand changes. In this framework, the theory of comparative advantage would remain neoclassical in outline, resting on international differences in capital in the extended sense per unit of population, but would have to concern itself also with the influences governing the allocation of capital among the different forms in which it may be embodied, in countries of different economic sizes and with different institutions for the education of people and the support of knowledge production.

This observation brings me to my third comment. The appeal of the neotechnology account is not primarily that it is capable of explaining international trade patterns to a statistically satisfactory degree or better than an extended version of the factor proportions model, but that it accords more satisfactorily with prevailing ideas about, and observations of, the facts of competition in and between modern industrial states. Conversely, the dissatisfaction of empirical workers and policy advisers with the traditional factor proportions model is not so much that it does not explain trade flows to a satisfactory approximation as that its static and essentially mathematical formulation—and especially the production concept—is difficult to square with the apparent facts of competition in a dynamically evolving economy. What practical men—empirical workers and policymakers alike—see when they look at international competition is not national differences in factor endowments but corporations competing monopolistically either on the bases of superior technology, labor skill, managerial and selling techniques or on the more conventional bases of cheap labor and cheap capital.

This view is not, of course, sufficient to establish the neotechnology and dismiss the neofactor proportions theory; on the contrary, particularly in the field of international economic policy, preoccupation with the manifestations of neotechnology factors is likely to obscure understanding and facilitate erroneous conclusions. But it does suggest the need for a more sophisticated formulation of the Heckscher-Ohlin-Samuelson theory, and particularly for a more careful identification of the empirical correlates of its theoretical constructs, most notably with respect to "factors of production" and "products." More bridges need to be built between the abstract, theoretical model of trade, and the concrete phenomena that concern the practical man. I have suggested above how this problem might be approached.

Further, I would suggest that the neotechnology hypotheses embody both empirical and theoretical insights and point to both new problems and new formulations of old problems that ought to be pursued in their own right, unconstrained by the ambition to arrive at results as elegant as those of the Heckscher-Ohlin-Samuelson model. The Samuelson factor price equalization theorem is indeed a splendid proposition; but its chief practical relevance is to direct attention—by the indirect process of theoretical abstraction—to the many reasons why factor prices, and more still, incomes per head, are unlikely to be equalized in the real world as we know it. I believe that the neotechnological account offers a more direct and positive approach to this conclusion and also a more sophisticated and genuinely dynamic approach to the understanding of the persistence of inequalities of levels of development and of economic welfare in a developing world economy.

I would also suggest that the phenomena of modern international industrial competition, and the policy problems to which they give rise—especially those concerning policy toward science and policy toward foreign corporations—raise theoretical issues of far greater complexity than can be dealt with by the existing theories of tariffs. We are, as Ohlin emphasized in his paper for the Montreal World Congress [8], moving into a world of freer trade but much more direct governmental intervention in industry. Much of that intervention is directed at mastering the technology factor as a means of improving the comparative advantage and competitive power of the national economy, or of its nationally owned corporations. Traditional theory offers useful approaches to the analysis of some of these problems or apparent problems: conventional market analysis suffices to dispose of much popular nonsense. But as Posner [9] and Hufbauer himself [2] have pointed out (though in neither case with enough specificity to clarify the nature of the problem) there is a need for more fundamental analysis of the welfare effects of technological leadership and their diffusion, and correspondingly of the economics of government intervention in these matters. So long as one does not question the existing system of property rights in knowledge and in its productive application for profit, it is easy enough to elaborate on the traditional case for freedom of trade and freedom of factor movements and against government interference. But the essential problem is that reliance on the market principle of rewarding investment in the discovery of knowledge, which has the nature of a public good, by granting a temporary monopoly of the use of the knowledge, which makes the application of it suboptimal, is inherently inefficient. It is the recognition of questions of this kind and ultimately their solution, rather than the provision of new solutions to the traditional problems of "income distribution, migration, saving, and investment," that I would expect and hope to emerge from the challenge to traditional theory posed by the neotechnology approach.

REFERENCES

1. Gruber, W., Mehta, D., and Vernon, R., "The R&D Factor in International Trade and International Investment of U.S. Industries," *Journal of Political Economy*, February 1967, pp. 20–37.

2. Hufbauer, G. C., *Synthetic Materials and the Theory of International Trade*, London, 1965.

3. Johnson, Harry G., "Economic Expansion and International Trade," *The Manchester School of Economic and Social Studies*, May 1955, pp. 95–112.

4. Johnson, Harry G., "International Trade Theory and Monopolistic Competition Theory," in R. E. Kuenne (ed.), *Monopolistic Theory: Studies in Import*, New York, 1967, pp. 203–218.

5. Johnson, Harry G., "Comparative Cost and Commercial Policy Theory for a Developing World Economy," Wicksell Lectures of 1968, mimeographed.

6. Johnson, Harry G., "The Theory of International Trade," forthcoming in the Proceedings of the Third World Congress of the International Economics Association, or *The Future of International Economic Relations,* held in Montreal, Canada, September 2–7, 1968.

7. Keesing, D. B., "The Impact of Research and Development on United States Trade," *Journal of Political Economy,* February 1967, pp. 38–48.

8. Ohlin, G., "Trade in a Non-Laissez-Faire World," forthcoming in the Proceedings of the Third World Congress of the International Economics Association, or *The Future of International Economic Relations,* held in Montreal, Canada, September 2–7, 1968.

9. Posner, M. V., "International Trade and Technical Change," *Oxford Economic Papers,* October 1961, pp. 323–341.

10. Vernon, R., "International Investment and International Trade in the Product Cycle," *Quarterly Journal of Economics,* May 1966, pp. 190–207.

COMMENT

JAGDISH N. BHAGWATI
Massachusetts Institute of Technology

The papers of Harry Johnson and Ronald Jones [1] concur in the theme that trade theory has been somewhat left behind by the real world, especially in relation to the phenomenon of technological progress. While Jones essentially tries to develop the traditional Heckscher-Ohlin-Samuelson model of trade theory (which is basically a simplified general equilibrium model of the Hicksian variety) in directions implied by the consideration of technical change, the Johnson paper attempts to develop themes which are on a more "imaginative" scale but which seem to have no theoretical foundations as of the present moment.

Looking through the Johnson paper, one gets the strong impression of reading John Williams (a reference which, I assume, Harry Johnson would regard as complimentary): imaginative, insightful, stimulating, and pregnant with theoretical implications without actually offering a new theoretical framework for analysis. But one also gets an occasional impression of reading someone like Thomas Balogh (a reference which Harry Johnson would probably not approve of): imaginative and insightful but basically opposed to theoretical modes of reasoning, as when towards the end of his paper Johnson would gladly sacrifice such theories as those which yield powerful conclusions with respect to distributive shares in order to make his description of the real world move closer to observation.

I shall be returning to this question later, but let me say briefly at this stage that the real problem with Johnson's interesting paper is that the

[1] For comment on Jones see page 93 below.

"realistic" phenomena which he is dealing with, such as the development of new technologies in consumption and production, involve essentially phenomena of imperfect competition for which, despite Chamberlin and Joan Robinson, we still do not have today any serious theories of general equilibrium. Therefore, although we can certainly indulge in *partial* analyses of imperfectly competitive phenomena, we cannot yet replace the traditional value-theoretic models of general equilibrium on which even our welfare analyses are based. Unless therefore we have a new, powerful, theoretic system (built admittedly on many of the interesting insights about modern, affluent economies which Johnson offers us in his paper), we cannot really hope to make a dent in the traditional theoretical frames of analysis.

Having said this, however, let me offer some general remarks about the important point made by Johnson about the tendency of trade theory to get stuck with analytical models and questions that nontrade theorists have discarded in favor of more useful constructs. I *do* think that there are several problems for which the traditional Heckscher-Ohlin-Samuelson model has become obsolete: for example, those dealing with questions such as process, value-added or "effective" tariffs, as distinct from nominal tariffs, which require operating with a model involving traded factors of production or inputs rather than with the traditional model involving primary, nontraded factors producing traded consumer goods. But then, as we know, trade theory has indeed been adapting itself by using more variegated models, better suited to such problems. As for the kinds of questions asked, the recent introduction of dynamic analyses represents an important extension of traditional comparative-static treatments. For example, Michael Bruno's paper at this Conference represents an attempt at building a computational, planning model which takes into account the fact that comparative advantage shifts over time. The very fact that, if you had nonshiftable capital in a world where over time the foreign rate of transformation faced by a country could change, introduces the possibility that *someone* would have to "look ahead": the traditional, static models of gains-from-trade do not yield such insights, and recent interest in "structural" models has begun to penetrate through to trade-theoretic analyses as well to Jones.

But I do remain a pessimist at being able to handle the kinds of broader issues, such as imperfect competition, that Harry Johnson's stimulating paper raises. It would be valuable to hear from him what *precisely* is the manner in which he thinks we can begin to reconstruct our theories so as to bring them closer to his view of the role of technology in international trade in a developing world.