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INTERNATIONAL TRADE AND FINANCE

FRONTIERS FOR RESEARCH

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The economist, as such, is under no obligation to assume the role of the statesman, and he is doing his full duty if he supplies the statesman with sound advice with respect to economic means and economic ends. But he owes it to the statesman to take into account as full a range of the economic means and ends as he can recognize and has the skills to analyze and deal with, and to be aware of and to make evident to others the limitations of his analysis and the possible significance of what he has not taken into account. We should be suspicious ourselves, and we should encourage suspicion on the part of non-economists, of a trend toward greater simplicity and rigour of models in economics at a time when it is apparent to even the man in the street that the real economic world has been getting more complicated and its problems less susceptible of exact and clean-cut solutions.

Jacob Viner
International Trade and
Economic Development, 1953

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PREFACE

International trade and finance are among the oldest subjects studied by economists. Along with public finance, money, and what we would now call economic development, they were the chief concerns of the "consultant administrators" whose doctrines and advice for emerging nation-states too many of us know only through their critics' eyes. And because so much of classical economics was written on and for an island, the international aspects of economic life played an extraordinary role in the development of economic thought and the theory of economic policy.

The concentration on problems and policies relating to foreign trade inspired many major contributions to analytical economics. Mill's treatment of price determination, the first full formulation of the law of supply and demand, appeared in his essay, *Of the Laws of Interchange between Nations,* to round out Ricardo's account of international specialization. Edgeworth's taxonomic work on tariffs and the terms of trade contained and contributed importantly to the theory of tax incidence in general equilibrium. Positive and normative concerns with the gains from trade gave birth to classical welfare economics, a contribution which Schumpeter described as the "most important exploit" of the trade theorists. And, on the monetary side, debates about bullion and the balance of trade, going back to Hume and earlier writers, furnished the first formal statement of the quantity theory of money.

In our own century, of course, international economics, like most other specialties, has been a net importer of techniques. The most important innovations in trade theory, the opportunity-cost approach set forth by Haberler and the factor-endowments approach developed by Heckscher and Ohlin, are adaptations of more general ideas – the Austrian theory of value and the Walrasian theory of markets (especially in its Casselian variant). International monetary theory has been a borrower too. It has used Keynesian propensities and multipliers to analyze balance-of-payments adjustment, the transfer problem, and the international propagation of business fluctuations. Most recently, portfolio and monetary theories constructed to do other work have been applied to the analysis of capital movements and the implications of changes in exchange rates.

In one vital respect, however, there was too little borrowing for too long. A full decade after other specialties had been transformed by the application of econometric methods, international trade and finance displayed a stubborn immunity to quantification. They became the last refuge of the speculative theorist. One can cite several significant exceptions, including early efforts to measure the price elasticities and income propensities pertaining to import demand. But little was done to verify the fundamental propositions of trade theory or to measure the effects of trade restrictions. The theory was deemed to be immutably true. The task of the trade theorist, then, was merely to spell out its implications for welfare and policy. Recall, for example, the manner in which Leontief interpreted his own results on the factor content of U.S. trade. Because the Heckscher–Ohlin theorem *had* to be true, one would have to set aside common-sense judgments about the American factor endowment. Or read again the closing lines of Richard Caves's *Trade and Economic Structure,* published in 1960:

> Even if a massive breakthrough comes in the statistical testing of economic models, surely statistical verifiability will never be the only test for keeping a theory on the books. Indeed, should such a standard come to rule, one can predict that the audience for professional airings of economic ideas may be reduced to statistical clerks and baseball fans between seasons.

Quantification cannot get ahead of speculation. When indeed it tends to do so, its practitioners are reduced to rerunning equations with one more year's data. Theorists must keep on throwing off ideas, not only as grist for the computers, but also to keep up with human ingenuity. But speculation without quantification, even the most rigorous derivation of theoretical propositions, cannot tell us how an economy functions or what we should do in case of a malfunction.

It is fun to speculate about the reasons for the long delay in the coming of econometrics to international economic research. I can think of three. First, trade theory is a species of general-equilibrium theory, and, in the multi-country, multi-commodity form that has to be used as the framework for estimation, it is wonderfully complex. Trade theorists enamored of their complicated constructs have been loathe to simplify those constructs sufficiently, to make do with partial-equilibrium approximations. Trade theory is very hard to test in ways that do justice to its sophistication. Second, trade data are notoriously poor. They may be no worse than the other statistics economists use all the time, often incautiously, but their flaws are more conspicuous because the statistics exist in ubiquitous pairs. One country's exports of goods, services, or assets are another country's imports, yet the two numbers never agree. Third, economists attracted to the subject have often come with normative, not positive, inclinations. My own meticulous investigation, conducted late one evening at a famous conference site, disclosed that four of seven well-known colleagues were fugitives from political science. They had been told that they must master international economics in order to understand international politics. This background may not be a handicap. Anyone who studies international economics without noticing that governments are decisive actors has missed the distinguishing characteristic of his subject. It is the clash of sovereignties, not the role of distance, that causes us to study international transactions separately from other economic phenomena. But someone who approaches the subject from this vantage point is apt to disdain the study of numbers and to shun the acquisition of quantitative tools.

Reasons like these, however, no longer matter much. Quantification has come, late but massively. Ten years ago, few papers on international trade or finance contained a single regression equation. Papers published today look naked without them. I have just compared the reading list I use in a graduate course with the one I used a decade ago. Half of the articles on the current list were written in the last ten years, and half of those contain regression equations or other quantitative work. Half of the articles on the old list are not on the new one, and most of those that I deleted had no numbers.

Right now, in fact, we face the danger I mentioned before. Quantification is getting ahead of speculation. Too many papers are written to show that some colleague's test of someone else's theory can be refined, but that the test is inconclusive even when refined. One longs to know where we are – what we have learned during the last decade, what else we might learn from the numbers, and what the theorist should be doing next to pave the way for subsequent rounds of testing and measurement. These are the questions I asked of the authors whose papers appear in this book.

Emerging from two years of university administration, I spent a year of intellectual rest and recuperation trying to catch up with my subject. Like most of us, I had already fallen behind being a full-time economist. But my plight had grown worse while I was away from the subject, and I did not have the usual excuse – that I was too busy writing to read. Eventually, I thought it best to ask for help. I organized a conference.

The meeting took place at Princeton University in March 1973, under the joint auspices of the International Finance Section and the Woodrow Wilson School of Public and International Affairs. My colleague, William Branson, served as co-chairman. The program was much like the table of contents of this volume.

The first session was devoted to the three papers published in Part I, Robert Stern's paper on the testing of hypotheses concerning the sources of comparative advantage and the structure of merchandise trade, Max Corden's paper on the costs and consequences of protection, and Carlos Diaz-Alejandro's survey of the implications of trade policies for efficiency and growth in the less developed countries. Drafts were circulated in advance, and one discussant was assigned to each of the three papers. The fourth discussant was allowed to roam more freely, to look at the broad issues raised by the three papers taken together.

The second session was devoted to econometric work on international payments, the papers published in Part II. Stephen Magee was asked to look at econometric models of merchandise trade, with special attention to the problems of specification and the testing of hypotheses concerning the role of exchange rates. Gary Hufbauer was asked to look at recent research on the determinants and implications of direct investment, including the broad economic issues raised by the growth of multinational production. Ralph Bryant was asked to review and appraise studies of capital flows, linkages between capital markets, and the external effects of monetary policies. Again, one discussant was assigned to each paper, and one was urged to wander at will.

The third session of the conference, Part III of this book, was devoted to a heterogeneous collection of questions. John Helliwell was asked what we have learned about balance-of-payments adjustment under fixed and flexible exchange rates, especially to tell us whether and to what extent inferences drawn from Canadian experience are susceptible of generalization. Jerry Cohen was assigned to survey positive and normative work on international liquidity, to ask if economists have useful things to say about the management of world reserves. And Lawrence Klein was invited to furnish a progress report on Project LINK, including an estimate of the costs and benefits of this ambitious multinational enterprise. Discussants were assigned to each of these three papers, but, as you will see, they took some useful liberties.

The fourth session was devoted to a stock-taking. A panel of academic and government economists, under the chairmanship of Charles Kindleberger, was asked to survey what we know and what we need most urgently to know from the standpoint of policy formation. The summary of the discussion, in Part IV, will give you some idea of what transpired, even though it contains only the comments of the appointed oracles, not of the self-appointed who spoke from the floor.

I dislike editors who comment invidiously on the efforts of their authors, and I will not do so here. It is, in fact, my duty to shoulder the blame for some of the flaws you may detect.

When I commissioned the papers that appear in this book, I asked the authors a number of questions. Some of these were very broad: What has recent research told us about the causes and effects of international transactions and international economic policies? What more do we need to know, and how do we go about learning it? Where, in other words, is the frontier of ignorance right now, and how can we push it back most efficiently? Some of my questions were more technical: What does the most recent theoretical work tell us we should test or measure? And what has recent empirical work taught about designing theories? Do we pay sufficient attention to the institutional and political environment? Do we specify carefully enough the structures of national and world markets? Or are we apt to neglect certain important realities, political and economic, merely because they are difficult to model or measure? Finally, I asked a series of questions that were meant to elicit cost–benefit analyses of research strategies: Have we been responding effectively to the requirements of those who make policy? Or have we perhaps been responding too well to their short-run needs and neglecting the long-run problems of the international economy? Most important, have we allowed our skills to define our priorities, choosing to study only the problems we can solve with techniques we have at hand instead of trying to fashion the techniques required to study the problems we ought to be solving?

None of the nine authors whose papers appear here has tried to answer all my questions. Some of the questions were not germane to their special topics. Each of us, moreover, has his own comparative advantage. Some of us are better at surveying what others have done, some at doing and describing their own work, and some at telling others what they should be doing. And no one, regardless of versatility, could answer every one of my questions in a single paper of the size I allowed. You will find, then, that some of the papers are long on descriptions of recent research but short on suggestions for further work, while others are long on suggestions for new tests and estimates but short on descriptions of work done to date. (None of the papers, I trust, is just plain long.)

I have also to take responsibility for the form of the book. The choice of topics to be surveyed is to some extent a reflection of my personal curiosity. The choice, moreover, has a backward bias. It is much easier to map and subdivide the world we already know than one we have not yet explored.

Finally, I must take responsibility for a decision that some readers will deplore. Although I asked the authors to appraise the implications of our knowledge and ignorance for the conduct of public policy, my instructions emphasized positive economics – the relationships between pure theory and quantification. This is not a book about political economy or about international economic relations. It can perhaps be viewed as a complement to Bergsten's recent study for the Ford Foundation, *The Future of the International Economic Order,* not as a competitor.

Although I promised to refrain from comment on the papers, I am moved to end this Preface with two observations inspired by my reading of the manuscript. First, I detect a dangerous ambiguity in our quantitative work. We do not distinguish carefully enough between the *testing* of hypotheses and the *estimation* of structural relationships. The ambiguity is rampant in economics, partly because the development of econometric method has been so heavily influenced by the requirements of economic forecasting. But this is no reason to perpetuate it. We should be spending more time and thought on the construction of tests that will help us to discriminate between hypotheses having very different economic implications. It is not enough to show that our favorite theory does as well as – or better than – some other theory when it comes to accounting retrospectively for the available evidence. Second, I would echo Kindleberger's plea for the resurrection of historical studies – and not just cliometric studies. Regression equations are not the only forms of scientific evidence, the contents of this book notwithstanding. The phenomena with which we deal and those with which we should be dealing are far too complicated to be investigated by a single method. To answer affirmatively the question I posed earlier, we *do* tend often to neglect those aspects of our subject that defy analysis by techniques we have at hand. This is a sure road to triviality.

I accumulated many debts planning for the conference and editing this volume. I am especially grateful to John P. Lewis, Dean of the Woodrow Wilson School, for joining the resources of the School to those of the International Finance Section as co-sponsor of the conference. I am grateful to Jane Armistead and Elizabeth Search of the International Finance Section for managing flows of paper and people before, during, and after the conference, and to Ellen Seiler, our expert and patient editor, who prepared this volume for publication. Above all, I am grateful to the authors of the papers and to the discussants. They were remarkably responsive to my questions, delivered their manuscripts on time, and, after the conference, dealt graciously and energetically with my extravagant suggestions for revision.

PETER B. KENEN