

ESSAYS IN INTERNATIONAL FINANCE

No. 190, July 1993

WHAT DO WE NEED TO KNOW ABOUT THE
INTERNATIONAL MONETARY SYSTEM?

PAUL KRUGMAN



INTERNATIONAL FINANCE SECTION

DEPARTMENT OF ECONOMICS
PRINCETON UNIVERSITY
PRINCETON, NEW JERSEY

ESSAYS IN INTERNATIONAL FINANCE

ESSAYS IN INTERNATIONAL FINANCE are published by the International Finance Section of the Department of Economics of Princeton University. The Section sponsors this series of publications, but the opinions expressed are those of the authors. The Section welcomes the submission of manuscripts for publication in this and its other series. Please see the Notice to Contributors at the back of this Essay.

The author of this Essay, Paul Krugman, is Class of 1941 Professor of Economics at the Massachusetts Institute of Technology. His publications include *Rethinking International Trade* (1990), *The Age of Diminished Expectations* (1990), and *Currencies and Crises* (1992). This Essay was presented as the Frank D. Graham Memorial Lecture on April 16, 1993, and concluded a conference on the international monetary system sponsored by the International Finance Section to celebrate the fiftieth anniversary of Essays in International Finance. A complete list of Graham Memorial Lecturers is given at the end of this volume.

PETER B. KENEN, *Director*
International Finance Section

ESSAYS IN INTERNATIONAL FINANCE

No. 190, July 1993

WHAT DO WE NEED TO KNOW ABOUT THE
INTERNATIONAL MONETARY SYSTEM?

PAUL KRUGMAN



INTERNATIONAL FINANCE SECTION

DEPARTMENT OF ECONOMICS
PRINCETON UNIVERSITY
PRINCETON, NEW JERSEY

INTERNATIONAL FINANCE SECTION
EDITORIAL STAFF

Peter B. Kenen, *Director*
Margaret B. Riccardi, *Editor*
Lillian Spais, *Editorial Aide*
Lalitha H. Chandra, *Subscriptions and Orders*

Library of Congress Cataloging-in-Publication Data

Krugman, Paul.

What do we need to know about the international monetary system? / Paul Krugman.
p. cm. — (Essays in international finance, ISSN 0071-142X ; no. 190)
Includes bibliographical references.
ISBN 0-88165-097-8 (pbk.) : \$8.00

1. International finance. 2. Foreign exchange. 3. Monetary unions. I. Title.

II. Series.

HG3881.K778 1993

332'.042—dc20

93-9099

CIP

Copyright © 1993 by International Finance Section, Department of Economics, Princeton University.

All rights reserved. Except for brief quotations embodied in critical articles and reviews, no part of this publication may be reproduced in any form or by any means, including photocopy, without written permission from the publisher.

Printed in the United States of America by Princeton University Printing Services at Princeton, New Jersey

International Standard Serial Number: 0071-142X
International Standard Book Number: 0-88165-097-8
Library of Congress Catalog Card Number: 93-9099

CONTENTS

1	INTRODUCTION	1
2	SILVER LININGS IN A EUROPEAN CLOUD	1
3	WHAT I THINK WE KNOW	4
	Linkage Problem 1: Trade Theory	9
	Linkage Problem 2: Intertemporal Analysis	13
	Linkage Problem 3: Rational Expectations	15
4	WHAT WE NEED TO KNOW	18
	REFERENCES	23

FIGURES

1	U.S. Unemployment and Inflation-Rate Change	7
2	U.S. Exchange-Rate Indices	8
3	Rates of Export Growth	9

WHAT DO WE NEED TO KNOW ABOUT THE INTERNATIONAL MONETARY SYSTEM?

1 Introduction

Frank Graham is today best known for his work in the pure, that is, nonmonetary, theory of international trade. His most famous paper is surely “Some Aspects of Protection Further Considered” (Graham 1923). This paper anticipated many of the themes that I and others have pursued under the unfortunate name of the “new trade theory,” and it did so with such insight that reading it makes one wonder whether the rest of us were necessary.

Yet Graham knew that trade takes place in a monetary world, and, unlike many real-trade theorists, he did not retreat from confronting the messier and less secure terrain of international monetary affairs. It is surely appropriate that, at a conference honoring the fiftieth anniversary of *Essays in International Finance*, I should commend to you Essay No. 2, Graham’s (1943) thoughtful discussion of the troubled choice between fixed and floating exchange rates—an issue that I shall argue remains at the heart of what we need to know in international monetary economics.

It is especially fitting for me to refer to the great Graham tradition in international economics, for I shall have a lot to say during this lecture about the virtues of traditional insights and approaches to international monetary economics. I shall not merely celebrate the past, however, but shall begin with a very recent event: the crisis that gripped the European Monetary System (EMS) only eight months ago, in September 1992.

2 Silver Linings in a European Cloud

A few days after a massive speculative attack forced the United Kingdom to pull the pound out of the Exchange Rate Mechanism (ERM) of the EMS, Chancellor of the Exchequer Norman Lamont denied that the events represented a policy defeat. He claimed that he had always regarded the defense of a fixed parity as a mistake (although, right up to the day of the debacle, he had asserted Britain’s absolute commitment to the ERM), and he went so far as to say that, after the pound was freed from its peg to the mark, he had been “singing in the bath” with relief.

I don't know whether the chancellor was actually singing in the bath or whether he was doing so more than usual. I can say, however, that, in late September of 1992, I myself was feeling pretty cheerful. Not that I wished the EMS harm; but the way the events of Black September unfolded encouraged me in my belief that we international macro-economists do in fact know a thing or two.

It is a slightly shameful but true observation that economists interested in policy find themselves pleasantly stimulated by economic crisis, just as professional military men are somewhat cheered by the prospect of war. This is particularly true when the events are dramatic without being too threatening in a personal sense: I have never seen as many happy people at the National Bureau of Economic Research as I did during the first few days after the 1987 stock market crash. There is extra satisfaction when the crisis is one that you and your friends think you understand, and to be around when a crisis that you have predicted actually comes to pass is very heaven.

The ERM crisis of September was one that many of us thought we understood quite well indeed, and one that at least some of us had predicted well in advance. (For the record: I wrote a column in *U.S. News and World Report* in February 1991, predicting that the fiscal consequences of German reunification would create strains on the EMS and force a realignment; of course I won't tell you about all the crises I predicted that *didn't* materialize). The story of the rise and partial fall of the EMS is deeply satisfying to tell, because it fits so well into the standard, workhorse models that most of us use to discuss international macroeconomic policy. The whole episode seems like a kind of textbook exercise designed to lead the student through the workings of a basic model of exchange rates, interest rates, and policy interdependence. (Indeed, I can guarantee that for a while, at least, the troubles of the EMS will be viewed as precisely such a textbook case; after all, Obstfeld and I [1991] write the textbook!)

The events of September, then, confirmed me in the view that we do, in fact, know quite a lot about the international monetary system. To be sure, our quantitative accuracy is limited. But, in a basic sense, we do know how monetary and fiscal policy work in open economies, and we know how they are transmitted internationally under fixed and floating exchange rates. This knowledge was enough to enable us to predict correctly that the combination of fiscal expansion and tight money in Europe's key currency nation would create a recession in the rest of the continent. And we knew enough about the behavior of exchange markets to anticipate correctly that the breakdown of the

system under these strains would be attended by massive speculative attacks (a point that gives me extra pleasure, for I have some personal intellectual property rights in such attacks).

So the policy debacle of 1992 was intellectually reassuring. I was certainly not the only economist who, behind his serious expressions of concern, was thinking “Ha! Told you so!.” And yet, even the intellectual satisfaction was not unalloyed. We may know a lot about the international monetary system, but we cannot rest easy with that knowledge. Indeed, hardly anyone is pleased with the state of our field.

One reason for our discontent is that the standard model that so nicely explains the ERM crisis works better in practice than it does in theory. It is essentially a slightly updated version of the Mundell-Fleming model, which is rooted in the kind of old-fashioned, *ad hoc* macroeconomics that nobody respects anymore. It is a short-run model that has never been clearly linked to the long-run stories we use to explain both trade and capital flows. And it is ugly. In Stephen Weinberg’s book, *Dreams of a Final Theory* (1992), Weinberg asserts that theories should be beautiful, and that a theory is beautiful if it seems “inevitable,” that is, if none of its assumptions can be changed without compromising its entire conceptual basis. By this measure, the standard model of international macroeconomics is exceedingly homely. It involves a set of plausible, but by no means overwhelmingly compelling, assumptions; indeed, some key parameters of the model do not seem to be tied down by any deep economic logic. It is better to use an ugly model that seems to work than to insist on beautiful falsehoods, but modified Mundell-Fleming does not comfort the economist’s soul.

Worse yet, the standard model leaves some crucial questions unanswered. We understand pretty well what Britain gained by dropping out of the ERM. We can even hope to make a rough quantitative estimate of the value of the monetary autonomy obtained by abandoning the fixed parity. But what did Britain lose by letting its rate float? What would it be worth to Europe if, despite the odds, the Maastricht Treaty were somehow to succeed in producing a unified European currency? We have some suggestive phrases—reduced transaction costs, improvement in the quality of the unit of account—to describe what we think are the benefits of fixed rates and common currencies. We even have a loose-jointed theory of optimum currency areas that stresses the tension between these hypothesized benefits of fixity and the more measurable costs of lost monetary autonomy. What we do not have, however, is anything we can properly call a model of the benefits of fixed rates and common currencies.

This is an unsatisfactory situation. I would suggest that the issue of optimum currency areas, or, more broadly, that of choosing an exchange regime, should be regarded as the central intellectual question of international monetary economics. We have formulated this question well enough to agree that it is a matter of trading off macroeconomic flexibility against microeconomic efficiency. Unfortunately, we are not completely happy with the way we model the macroeconomic side, and we have no way at all at present to model the microeconomics.

I shall eventually argue in this lecture that developing some kind of model of the microeconomics of international money ought to be our top research priority. Most of the lecture, however, will be spent on a different issue: that of defining what it is that we actually do know about the international monetary system. Unfortunately, macroeconomics in general and international economics in particular is a field marked by deep ideological divisions and much mutual incomprehension. It is hard to hold on to the things that we actually do know, let alone expand our territory. My initial task will therefore be to try to make a map, to sketch out the border between what I think we know and what I am sure we do not know about the international monetary system.

3 What I Think We Know

In a recent essay (1991), I used the term “Mass. Ave. model” to describe the slightly updated version of the Mundell-Fleming model that is the workhorse of international-policy analysis. Let me use a different term here and call it “modified-Mundell-Fleming,” or “MMF” for short. (I guess that’s pronounced “mmph.”) A typical version of MMF looks something like the following:

First, we assume a Keynesian demand-side determination of output, in which real output y (in terms of some composite domestic good) is the sum of domestic absorption A and net exports NX :

$$y = A(y, i - \pi) + NX , \quad (1)$$

where i is the interest rate and π is the expected rate of inflation.

We also assume a standard LM curve:

$$M/P = L(y, i) , \quad (2)$$

where M is the money supply and P the domestic price level.

Prices are assumed to be sticky. In the original Mundell-Fleming model, they were simply taken as given; in the MMF model, inflation is determined by the difference between real output and the “natural”

level y^N , and on the expected inflation rate π :

$$\frac{\dot{P}}{P} = \phi(y - y^N) + \pi, \quad (3)$$

and expected inflation is assumed to adjust only slowly in response to actual inflation:

$$\dot{\pi} = \lambda \left(\frac{\dot{P}}{P} - \pi \right). \quad (4)$$

All of this is just standard early 1970s macroeconomics. The specifically international side of the model comes in the determination of net exports and the exchange rate. We assume export and import equations that depend on incomes and relative prices, so that the net-export equation looks something like this:

$$NX = NX(y, y^*, EP^*/P), \quad (5)$$

where y^* is foreign output, E is the exchange rate, and P^* is the foreign price level.

In the original Mundell-Fleming model, international arbitrage was assumed to equalize interest rates. In MMF, we need something that is not so obviously untrue. A typical assumption is that markets expect the real exchange rate to revert toward some “normal” level, e^E , and that they set the expected return on domestic and foreign interest-bearing assets as equal:

$$i = i^* + \pi - \pi^* + \gamma \left[\ln(e^E) - \ln \left(\frac{EP^*}{P} \right) \right]. \quad (6)$$

I am not going to do anything with these equations. I put them here just to give some concreteness to what I mean when I talk about the standard model of international macroeconomics. I think it is fair to say that something like this model underlies most informed policy discussion of exchange rates, macroeconomic interdependence, balance-of-payments adjustment, and so on.

Of course, each of us would like to make a few changes in the details. The aggregate-demand equation is far too simple: all sorts of other factors should be included as determinants of expenditure. The *LM* curve is nastier than I have written it, especially given the problem of defining a useful monetary aggregate. The net-export equation definitely needs some lagged effects for the real exchange rate, and it

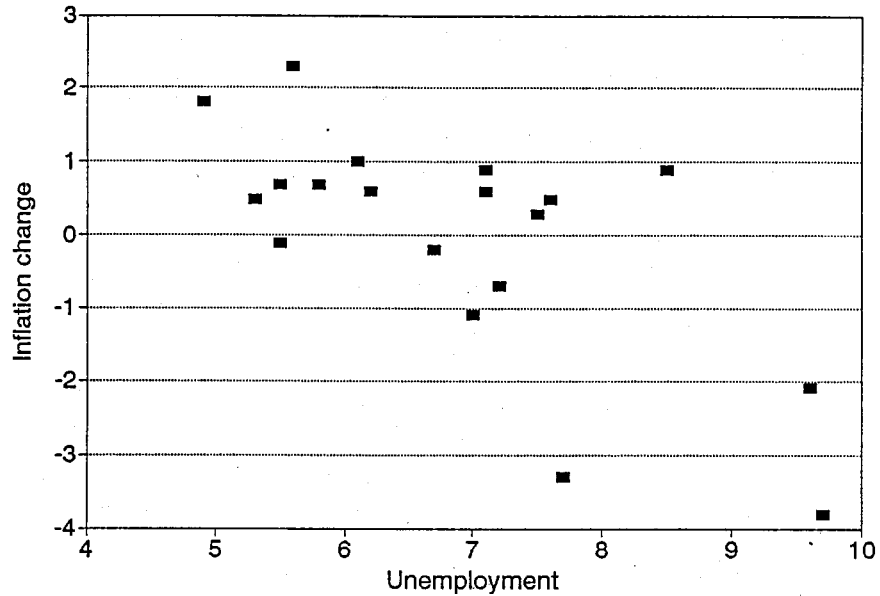
should probably be so constructed as to yield a J-curve. Some people would want to include risk premia in the exchange-rate equation, so as to allow some scope for the effectiveness of sterilized intervention. More broadly, the assumption of perfect capital mobility is questionable. As Obstfeld (1993) points out in his paper for this conference, there are substantial questions about the degree of long-run capital mobility even for advanced countries. Dooley's (1993) paper is a reminder that many developing countries were simply shut out of international capital markets for a decade. These are all, however, technical adjustments; they do not challenge the fundamental conceptual basis of the model.

There are many economists—although few of them actually engaged in making policy recommendations—who *would* challenge the fundamental conception. Indeed, a substantial number of economists regard the MMF model as pure nonsense. I shall get to those criticisms in a little while. First, however, I want to spend some time pointing out that the most controversial aspects of the MMF model have actually held up rather well in the face of recent experience.

In terms of the philosophical underpinnings, the most troublesome aspect of the MMF model has nothing to do with international economics. It is the assumption of gradual price adjustment. Indeed, by the early 1980s, after years of relentless criticism from Lucas and his followers, it had become inadvisable to write down anything like my equations (3) and (4) in a paper intended for a refereed journal. Yet this old-fashioned, *ad hoc* approach to aggregate supply has in fact fared rather well in the face of the actual experience of price behavior since 1980. We can see this in two ways.

First, “adaptive-expectations” Phillips curves do not do badly in fitting the actual interplay between the business cycle and inflation. Figure 1 shows a crude illustration of this point. It compares the U.S. rate of unemployment on an annual basis with the *change* in the inflation rate (measured by the GDP deflator) since 1973. The relation is far from perfect—I could no doubt do much better by playing with lag structures and demographically corrected unemployment rates—but two things are unmistakable: there is a negative correlation between the rate of unemployment and the change in the inflation rate, and the slope is not all that steep. That is, the picture is broadly consistent with the idea that there is a fairly flat short-run tradeoff between inflation and unemployment, but a vertical tradeoff in the long run. The important point is that this picture, some version of which has been appearing in textbooks since the late 1970s, still looks pretty good after all these years.

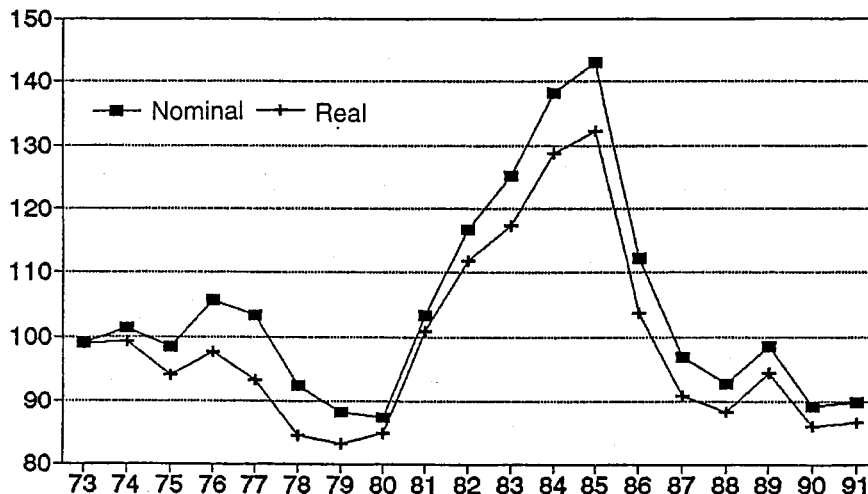
FIGURE 1
U.S. UNEMPLOYMENT AND INFLATION-RATE CHANGE



A second, crisper test of the idea of sluggish price adjustment comes from the relation between nominal and real exchange rates. During the 1970s, at the same time that new classical macroeconomic theorists were challenging the legitimacy of assuming nominal rigidities in domestic macroeconomics, "monetary-approach" international economists were asserting that it was unacceptable to assume that nominal-exchange-rate changes had any real effect: the exchange rate was the relative price of two moneys, not of two goods. In fact, however, the experience of the post-1980 period has been one of extremely high correlation between nominal and real exchange rates. Figure 2 makes the point for the United States: the nominal- and real-exchange-rate indices have moved almost perfectly together.

The other highly controversial part of the MMF model is the assumed linkage between the real exchange rate and net exports. This relation has been questioned from at least two sides. On one side are those whom I have elsewhere called "structuralists," usually noneconomists who insist that trade deficits are rooted in structural causes and cannot be cured by depreciation. On the other are those who like to think of trade imbalances as the result of an intertemporal maximization and who find the assertion of a simple partial-equilibrium relation between

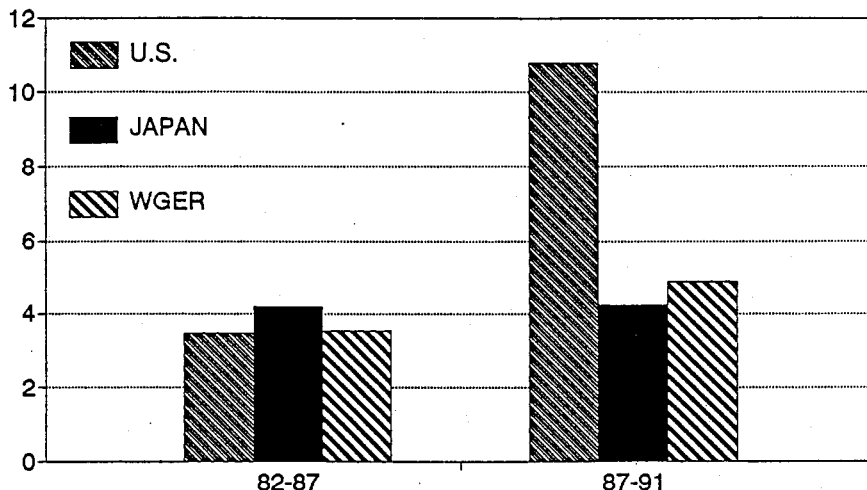
FIGURE 2
U.S. EXCHANGE-RATE INDICES



real exchange rates and the trade balance unacceptable. I shall not bother with the structuralists in this lecture but shall take the intertemporal approach more seriously. For now, let me simply point out that, in a gross, crude way, U.S. external adjustment since 1980 has seemed to confirm the idea that real exchange rates work the way that the standard model says they should. Figure 3 makes the point by comparing U.S. export growth with that of Japan and Germany over the 1982-87 and 1987-91 periods. (In each case, we begin the period two years after the trough and peak in the dollar, to allow for lags in adjustment). During the first, strong-dollar, period, U.S. exports stagnated; during the second, weak-dollar, period, they soared. Of course, one can offer other explanations, but, on the face of it, dollar depreciation seems to have done just what it is supposed to do.

My point, then, is that a framework something like the model described in equations (1) through (6) seems quite useful. Or, to put it another way, what we know about the international monetary system is that we seem to be able to track its performance and predict the outcomes of policy fairly well using a framework similar to the one I have described here. That does not mean that the framework is the last word. In fact, it is far too ugly and *ad hoc* to be our final theory on the subject. Still, when it comes to the issue that this framework addresses, we do seem to know quite a lot.

FIGURE 3
RATES OF EXPORT GROWTH



Nonetheless, the MMF model has been subject to a great deal of criticism and even outright rejection over the years, largely because it seems to fail to connect with other parts of economic theory about which many of us also have strong ideas. So let me now turn to the problems of linking the MMF model with several apparently competing economic doctrines.

Linkage Problem 1: Trade Theory

The problem of joining international macroeconomics with trade theory has not been at the top of many peoples' agenda in recent years. Nonetheless, it is a glaring gap in our understanding, and I believe that the absence of a well-explained link between trade and finance has been a major source of analytical and even policy confusion.

The nature of the problem should be obvious. In international trade theory, we are concerned with explaining the pattern of production and trade in a many-good, many-factor world. The model described above, however, seems to be one in which each country is simply assumed to produce a single good that is not a perfect substitute for goods produced abroad and in which there is nothing interesting going on in factor markets. Where are the trade-theoretic underpinnings of the macro model?

In a way, it is remarkable that economists have made so little effort to integrate international trade and monetary economics. The difficulty

has been apparent at least since Ricardo's time: there is no room in Ricardo's model, or certainly in the formalization of that model by John Stuart Mill, for the kind of price movements envisioned in Hume's story of balance-of-payments adjustment. Robert Mundell developed his macro analysis of exchange-rate regimes only a few years after making major contributions to real-trade theory; yet the two analyses seem to be referring to completely different worlds.

In fact, I can think of only one well-known paper that seriously tries to build a bridge between international trade and international money: the Ricardian model of Dornbusch, Fischer, and Samuelson (1977). That paper struck me like a bolt of lightning when I first read it—it seemed to me to legitimize international macroeconomics and to make sense of some of its characteristic assumptions in a way that had not been possible before. Not everyone appreciates what these three accomplished, however, so let me review briefly their argument, before I talk about what is missing.

The Dornbusch-Fischer-Samuelson approach envisages a world in which each country has only a single factor of production, which it can use to produce a large number of traded goods and perhaps a range of nontraded goods as well. It begins with a pure, static, real-trade model. With a little *ad hockery*, however—simply assuming domestic nominal expenditure proportional to the domestic money supply—the model becomes dynamic and monetary. With a little more *ad hockery*—assuming rigid nominal wages—the model becomes Keynesian as well.

The model immediately suggests answers to several major historical debates in international economics; indeed, it suggests that they are all really about the same thing. It offers a startlingly neat solution to the Keynes-Ohlin debate over the transfer problem: Ohlin is right in principle, but Keynes is right in practice if a large fraction of expenditure falls on nontraded goods. The model also offers a quick integration of trade theory with Hume's adjustment mechanism: allowing money to flow automatically generates a specie-flow mechanism; if nontraded goods are important, this then becomes a *price-specie-flow* mechanism in which a trade deficit is associated with an unusually high relative wage rate and domestic price level. In other words, the question of whether the price component of Hume's story is essential is the same as the answer to the transfer problem. Finally, when we turn to devaluation, we see that the debate between the absorption and elasticity approaches comes down to the same thing: relative price changes are an essential part of the adjustment process if, and only if, conventional wisdom on the transfer problem is right.

The model also shows that the conventional wisdom that exchange-rate adjustment helps reconcile balance-of-payments targets with employment targets is justified in the presence of sticky nominal wages. In so doing, it basically integrates Ricardo's trade theory not only with Hume's earlier monetary story, but with the external-and-internal-balance stories that Swan (1963), Johnson (1958), and others put at the heart of international macro analysis 140 years later.

All of this is wonderful. I have used what I learned from Dornbusch, Fischer, and Samuelson as an underpinning for a lot of work and, indeed, for some serious policy arguments. With its remarkable encapsulation of 200 years of thought into 17 pages of text, it is one of my favorite papers.

There is only one problem. Nobody thinks that the Ricardian model is an adequate representation of the forces driving international trade. And, unfortunately, the integration of trade and monetary theory achieved by Dornbusch, Fischer, and Samuelson does not easily survive introduction of a more complex trade model. To see the problem, let us simply imagine replacing the Ricardian setting with a standard two-factor model of trade and see what happens to the results.

In the Ricardian model, introducing nontraded goods is enough to give us the conventional presumption on the transfer problem. If country A transfers income to country B, the transfer will raise the demand for nontraded goods in B and lower it in A, even if they have the same expenditure patterns on the margin. Because nontraded goods are produced with domestic labor, the effect is to shift world relative demand for the two countries' labor, and thus to push up B's relative wage rate. This shift in the double-factorial terms of trade will produce a corresponding change in just about any measure of the real exchange rate.

It is easy to show, however, that, in a two-or-more-factor world, this need not happen. Suppose, for example, that there are two traded goods and one nontraded good, produced with two factors. And suppose that each country produces at least some of both traded goods. A transfer will then lead to a complicated reshuffle of resources within each country, with the nontraded sector releasing resources to traded-goods production in one country and absorbing them in the other, generating Rybczynski effects all over the place. If technologies and tastes are the same, however, the end result of the shuffle will be to allow the world to accommodate the shift in the location of consumption of nontraded goods without any change in relative prices or factor returns. The simple association of a large nontraded sector with a Keynesian view on the transfer problem is broken.

Worse yet, when there are multiple factors of production, one cannot introduce a Keynesian story about unemployment simply by assuming a rigid nominal wage rate. Fixed wage rates in two-factor models do weird things, leading to abrupt changes in specialization when the relative wage rates shift a little. Obviously, that doesn't happen in practice, and the reason why is clear: steel mills cannot be turned into textile mills over the course of a few months. What we learn, however, is that, once we try to get the realistic tradeoff between internal and external balance into anything more complex than a Ricardian model, we are immediately faced with the need to get into a lot of messy stuff. We cannot just assume sticky wages; we need to start worrying about things like the dynamic adjustment of sector-specific capital stocks. The simplicity of both the Mundell-Fleming model and the Dornbusch-Fischer-Samuelson model seems to get buried under a welter of detail.

How, as economists, do we deal with the glaring inconsistency between the models we use to think about macroeconomic and trade issues? One answer is simply to specialize: many trade economists profess a total lack of understanding of, or faith in, macroeconomic analysis, and many macroeconomists simply lack interest in trade. The world wants answers, however, and some of us try to keep abreast in both areas. How do we manage the cognitive dissonance? We do so largely, I believe, by telling ourselves that the MMF model is a short-run story, whereas modern trade theory is a long-run story. In fact, however, nobody other than Dornbusch, Fischer, and Samuelson has succeeded in making anything like MMF emerge as the short run of a long-run model.

Matters get even worse when we introduce the concerns of the “new trade theory,” increasing returns and imperfect competition. I have personally made a small stab at integrating monetary factors into a new-trade-theory model (Krugman 1987), using a framework shamelessly plagiarized from Dornbusch, Fischer, and Samuelson. That exercise suggested that, in a world of increasing returns, we may not even be able to assume that the long run is exempt from monetary influences: a large short-run overvaluation or undervaluation may permanently change the pattern of dynamic comparative advantage.

What I have argued, then, is that there is a glaring lack of consistency between the stories we tell about international trade and the way that we model trading economies when we want to talk about macroeconomics. One reaction to this inconsistency would be to dismiss the macro analysis. After all, the trade stories have coherent microeconomic bases,

and the MMF model does not. That is not, however, my reaction. The fact is that the MMF analysis seems to be extremely useful—it appears to work in practice much better than it ought to work in the light of trade theory. The question is why.

So here is a research challenge: let us try to build a link between the trade analysis that works in theory and the macro analysis that seems to work in practice.

Linkage Problem 2: Intertemporal Analysis

A number of years ago, when I was on the staff of the Council of Economic Advisers, I found myself obliged to defend the CEA free-trade position in a meeting in which most people were much more senior than I. Among them was the then U.S. trade representative. At one point, I tried to emphasize the domestic origins of the U.S. trade deficit by referring to the point that the trade balance equals the difference between domestic saving and domestic investment. Ambassador Brock was polite. “That’s an interesting theory,” he said.

Of course, the identity $X - M = S - I$ is not a theory. It is one of the few things in international economics about which we are absolutely sure. So one might think that an “intertemporal” approach to the balance of payments, one that treats current accounts as the outcome of long-run savings and investment decisions, would be at the core of the way we do open-economy macroeconomics. And we all invoke such an approach, at least informally, when we try to discuss enduring patterns in international capital flows, such as the persistent current-account surpluses of Japan and pre-reunification West Germany. There is also a growing formal literature on international economic models based on intertemporal optimization models. This is nicely surveyed by Razin (1993).

What seems striking to me, however, is that there has been very little contact between the world of more or less practical policy analysis and the intertemporal approach. If anyone has tried to discuss the travails of the EMS in terms of intertemporal optimization, I am not aware of it.

Why do we seem unable to make any use of these models? It could be that the policy-relevant types are simply too old-fashioned to be willing to use modern analysis—but I don’t think that’s a fair judgment. The real reason, I think, is that the intertemporal approach doesn’t seem to accord with what we think we know about what actually happens.

Let me start at the shallow end. The thing I find most striking about the predictions from intertemporal maximizing models is how compli-

cated they are compared with the fairly simple stories told by the MMF model. Suppose I ask how an extra percentage point of U.S. economic growth next year will affect U.S. trade. Even a very simple intertemporal model will respond with a request for more information. Is the shock temporary or permanent? Is the shock in traded or non-traded goods? Or what about the relation between the trade balance and the real exchange rate? The answer again seems to depend on a variety of questions about the source and persistence of shocks. The peculiar thing is that, although things are complicated and messy in theory, they are fairly simple in practice: the trade equations described by Hooper and Marquez (1993), equations that work rather well, tell us that 1 percent on U.S. GDP means imports rise by 2 percent; 10 percent on the real exchange rate means net exports decline by 1 percent of GDP—end of story. It is hard to sell a practical policy economist on a theoretical framework that seems to require her to throw away simple tools that have proved useful and to replace them with complicated ones that seem to give no answers at all.

A deeper problem with the intertemporal approach is that it may be rigorous but wrong. It assumes that people have a very high degree of rationality about the effects of shocks on their future income, to a degree that one can reasonably argue would actually require irrational expenditures of resources on gathering and processing information.

Consider, for example, what the intertemporal approach says about one of the issues surrounding the troubles of the EMS. Should Germany have tried to finance the rebuilding of the East with higher taxes rather than accept large fiscal deficits? Many economists believe that, with a different fiscal stance by Germany, the strains that led to Black Wednesday could have been avoided. According to the standard intertemporal approach, however, it would have made no difference. Robert Barro became famous for arguing that long-lived households should decide on their consumption based on what they expect the government to spend, not on the particular time path of the taxes it plans to collect to pay for that spending.

The point, of course, is that this story requires that ordinary West German households sit down over their evening meal and estimate the impact of likely subsidies to the East on their future tax liabilities. Is this plausible? Would the improvement in expected utility from doing so actually be worth the time and effort for the typical German family? I doubt it. Surely it is far more likely that people use reasonable, but not hyperrational, rules of thumb to decide on their consumption, rules that are not likely to provide an automatic offset to changes in taxes

unmatched by changes in spending plans.

We might note as an aside that many intertemporal models make the high rationality required seem more plausible by assuming an ergodic structure of recurrent random shocks. The idea is that, through long experience, households develop rules of thumb that approximate optimal behavior given the shocks they typically face. Unfortunately, the times when we really need our models are when *atypical* shocks come along; German reunification is not something that happens on a regular basis.

Finally, let me note the obvious point that the MMF model focuses crucially on the role of sluggish price adjustment, and that this focus seems to be correct. The intertemporal models currently available, however, are full-employment models in which there is no natural way to introduce the nominal rigidities that remain so critical to understanding the real issues that confront us.

Yet one cannot simply dismiss an intertemporal approach as useless. The present is linked to the future by saving and investment; what we do now matters for what we expect to happen in the future, and vice versa. We cannot ultimately rest easy with any short-run model that is not at least approximately embedded in some kind of intertemporal framework. The MMF model, once again, does not meet that criterion; it respects the accounting identities, but that's about it.

So here is another research challenge: let us try to build an intertemporal approach in which the balance of payments is determined by forward-looking (if not necessarily hyperrational) saving and investment decisions, yet which remains able to discuss the kind of issue that the MMF model seems to handle acceptably.

Linkage Problem 3: Rational Expectations

During the 1970s, the rational-expectations revolution swept all before it in macroeconomics. It became completely unacceptable in polite circles to make *ad hoc* assumptions about expectations or dynamic adjustment processes. Everything, from asset pricing to aggregate supply, was supposed to be grounded in rational behavior, albeit in the presence of incomplete information. At the core of the revolution was what we may call the Lucas Project, the effort to build business-cycle theory on maximizing microfoundations.

The initial effect of this revolution was exhilarating; its eventual effect was devastating. Traditional Keynesian macroeconomics was, as a matter of theory, completely vanquished, as were the various *ad hoc* models of asset markets that had been common ingredients of macro

analysis up to that point. The ramshackle, *ad hoc* intellectual structures of the 1950s and 60s were ruthlessly cleared away, making room for the erection of a new structure to be based on secure microfoundations. Unfortunately, that structure never got built.

The fact is that the Lucas Project succeeded in destroying the old regime but failed to create a workable new macroeconomics. The effort to explain the business cycle in terms of rational confusion over which shocks were nominal and which were real was, in the end, a failure: economic agents have too much information, and business cycles are too persistent. The true believers in equilibrium business cycles shifted to real-business-cycle theory (to which the intertemporal models of the balance of payments are related), while most theorists simply abandoned the subject of business cycles altogether. Meanwhile, forecasts and policy assessments had to be made. So, practical economists continued to use the old-fashioned models, like Cuban drivers stranded by the U.S. embargo doing the best they can with lovingly maintained 1959 Chevys.

The theoretical devastation wreaked by the rational-expectations revolution was perhaps most severe in international macroeconomics, for two reasons:

First, in international even more than domestic economics, the evidence for some kind of nominal rigidity is overwhelming. Domestic macroeconomists can point to the lack of clear correlation between any particular monetary aggregate and real output and deny that nominal variables have real effects; or they can claim that such correlation as there is represents reverse causation from real shocks to an endogenous Federal Reserve. International macroeconomists must face up to much stronger evidence, the nearly perfect correlation between nominal and real exchange rates in industrial countries since 1980. There are a few who try to make the reverse causation argument—but they must then confront the question of why the “real” shocks seem to change so much when the nominal regime shifts. Real exchange rates were far more volatile after 1973 than before; the formation of the EMS was associated with a sharp reduction in real-exchange-rate movement within the currency bloc. An extremist might dismiss even this evidence on the grounds that the changes in exchange-rate regime were endogenous. This is certainly true: physicists tell us that only a few basic constants are truly exogenous, and the rest is all quantum mechanics. But, as Eichengreen (1993) shows, the factors determining changes in exchange regime are far too subtle to produce such a raw, striking correlation. And one must, in the end, also confront such facts as the change in

Ireland's real-exchange-rate behavior from close correlation with the United Kingdom before its entry into the ERM to close correlation with Germany afterward. I personally think that the effort to explain away the apparent real effects of nominal shocks is silly, even if one restricts oneself to domestic evidence. Once one confronts international evidence, however, it becomes an act of almost pathological denial.

The problem, of course, is that Lucas made us all painfully aware that we lack good microfoundations for assuming any sort of nominal rigidities. This leaves international macroeconomics with a painful dilemma: to write a macroeconomic model with sticky prices is professionally dangerous, but to write one without such rigidities is empirically ridiculous. The result is a considerable degree of intellectual paralysis.

The situation is made worse by the second problem of international macroeconomics: the apparent failure of rational expectations even in the place where one might hope it would work, international asset markets.

For a number of years, there was a sort of academic industry that focused on testing the speculative efficiency of the forward exchange rate. A few early papers claimed to confirm that the forward rate was an efficient predictor of the subsequent change in the exchange rate (or more accurately, failed to reject the null hypothesis that it was an efficient predictor). Since the crucial paper by Hansen and Hodrick (1980), however, it has been obvious that this is not the case. Indeed, if anything, the correlation is negative. Now, this need not imply a rejection of efficiency if there are risk premia, especially shifting ones—although nobody thought large shifting risk premia were likely to be important until the devastating failure of simple efficiency ideas became apparent. In the end, however, it just won't wash. Taylor's (1993) paper summarizes the huge and dispiriting literature on foreign-exchange-market efficiency: after more than a decade of work, it seems clear that nobody has found any reasonable way to "save" the speculative-efficiency hypothesis within the data. This is devastating in its impact on our research. What we know how to model are efficient markets; what we apparently confront are inefficient ones. Nor can we, in international macroeconomics, tacitly put speculative behavior on one side. Under floating exchange rates, the role of market expectations is crucial to every aspect of policy analysis.

What practical policy analysts do, of course, is apply *ad hoc* rules about expectation formation, like the rule embedded in equation (6) in my exposition of the MMF. These rules are clearly wrong as a full description of how markets behave, yet they contain enough truth to

give some guidance, and they at least allow the model to be completed. This kind of brutal expediency, however, encourages the slightly disreputable reputation that international macroeconomics has among smart young economists.

In my last two linkage discussions, I have suggested that there is room for some research on trying to put what we think we know about international monetary economics together with what we think we know about related fields. Here, I have no such optimistic suggestion. It seems to me that macroeconomics is in a terrible state independent of its international aspects. Until we find some resolution of its difficulties, which I suspect will involve facing up to deep issues such as the role of bounded rationality, there is little that can be done on the international front. Perhaps a slender bridge can be constructed between international macroeconomics and “new Keynesian” macroeconomics à la Mankiw (1991, 1992), but I guess I wouldn’t expect more from that than a bit of rationalization for continuing to use the MMF model.

4 What We Need to Know

Up to this point, I have described a series of problems with the MMF model of international macroeconomics. I have pointed out that it is an *ad hoc* model that is poorly linked with the models that we use to explain international trade, even though an open macroeconomy is necessarily also a trading economy. I have pointed out further that the MMF model does not link up at all well with our best models of saving and investment decisions, even though it is a basic identity that the current account equals the savings-investment balance. And I have pointed out that the MMF model, along with virtually all relevant short-run macroeconomics, has been intellectually stranded by the way the rational-expectations macroeconomics first vanquished Keynesianism, then collapsed in the face of its own internal contradictions.

And yet, despite all of these problems, when it comes to making sense of the international monetary system, the macroeconomic side is not the biggest obstacle. The MMF model is crude, *ad hoc*, and in huge need of improvement. Nonetheless, it is a workable guide. If you ask me what will happen if, say, Mexico emulates Argentina and adopts a “currency-board” system that pegs the peso to the dollar; if you ask me what will happen if France gives up the *franc fort*, or Germany decides to slash public spending; if you ask what the consequences have been of Canada’s determination to achieve price stability, I think, in all of these cases, I know how to answer—and maybe even to

produce a rough quantitative assessment—using something along the lines of the MMF model.

But now, suppose you ask me some related questions, to which policymakers would very much like to know the answers. What will be the impact on European trade and, beyond that, on the efficiency of the European economy if the European Community (EC) actually adopts a common currency? What will be the effect on North American trade if Canada and Mexico permanently peg their currencies to the U.S. dollar? I can talk a good game on these questions when pressed, but I know, even if my listeners do not, that I do not have a model nearly as well developed as the MMF model to back up my assertions. What I have is only a set of nice words, backed by vague images. In particular, I have no real way of quantifying the forces to which I can allude. To put it briefly, we have a workable, if not beautiful, model of international macroeconomics; we have no real model of the microeconomics of international money.

The same is, of course, true for domestic macroeconomics. The truth is that there is no even halfway adequate model of the microeconomics of money, at least in the sense of a model that addresses the issues that everyone thinks really matter. The case in point is the welfare costs of inflation: existing models only let us get at the “shoe-leather” costs that arise from the use of non-interest-bearing money as a medium of exchange. These costs are small at anything short of hyperinflation. Most economists who worry about the issue believe, however, that the main costs of inflation lie, not in the degraded role of money as medium of exchange, but in its damaged role as unit of account—for which we have no model.

Nonetheless, in domestic macroeconomics, we do not usually find that our microeconomic ignorance is crucial. The consensus that there is no long-run tradeoff between unemployment and inflation, but that the short-run tradeoff is quite flat, has allowed the emergence of a policy consensus that inflation should be kept at its current fairly low levels, but that it is not worth a costly push to full price stability. To put it another way, the central issues in domestic monetary policy do not, at present, seem to require reaching a judgment about the microeconomic side of the equation. (Strictly, this is true only for advanced countries with low inflation. The relation between inflation and long-run growth is much more central for the kind of stabilization problems discussed by Bruno [1993]).

In international monetary affairs, however, I think it is fair to say that the central, canonical issue is that of choosing an exchange regime.

Of course, there are always problems of policy management within an exchange regime: Chancellor Lamont still needs all the advice he can get, and better open-economy macro models remain essential to many real policy issues, such as the coordination problems discussed by Bryant (1993). Still, the big issues involve fundamental regime choice. Should Mexico contemplate devaluation to restore some of its industrial competitiveness, or should it lock in its gains against inflation by permanently pegging to the dollar (or even adopting the dollar as its currency)? Should Sweden (Poland? Slovakia?) join EMU, if such a thing happens? These are all questions that are, in effect, variants of the optimum-currency-area problem.

Now, Mundell (1961), McKinnon (1963), and Kenen (1969) gave us a very nice intellectual structure for thinking about the problem of defining an optimum currency area. In all cases, we think of a country as asking whether it prefers the macroeconomic independence that comes with an independent currency and perhaps a floating rate, or whether it prefers the microeconomic benefits of stable rates and perhaps a common currency. We have a fairly good idea of what the macroeconomic tradeoff is: we know that fixed rates cost least when trade is large, when labor mobility is high, when shocks are symmetric, and when there are compensating fiscal transfers. Knowing this, we guess that some index based on these criteria will indicate when and if a country should join a currency area.

In fact, however, we know almost nothing about the other side of the comparison. To repeat: what we say about the microeconomics is a matter of metaphor and slogans rather than worked-out models. I am sure that a common European currency would save the transaction expenses now incurred in changing currencies—London and Paris could get by with far fewer foreign-exchange kiosks. Beyond that, we really don't know. Does confusion over fluctuation in units of account significantly inhibit the ability of European businessmen to reach mutually beneficial deals? To the extent that it does, how large are the costs? I don't think we even have an idea of the order of magnitude.

Of course, we must make judgments anyway. I would identify three different strategies that have been used to try to deal with, or perhaps to paper over, our almost total ignorance about the crucial microeconomic tradeoffs involved in the formation of monetary areas.

First, we seem to be able to resolve the issue in many cases by pointing to overriding political concerns, often involving seigniorage, that force monetary areas to coincide with nations. Goodhart (1993) makes this point effectively, and it is surely often valid. Yet I cannot

help noticing the relief with which economists seize upon discussions of seigniorage as a way to avoid the really difficult issues. After all, seigniorage is something we understand; we slip away from the optimum-currency-area argument into a discussion of inflation taxes with something like the attitude of a man changing from his business shoes into a pair of comfortable old slippers. Unfortunately, comfortable as we may be with this kind of argument, it will only sometimes be enough.

Second, quite a few economists have tried to assert that there are no macroeconomic benefits to independent currencies, so we don't have to worry about how big the microeconomic costs are. This line of argument usually rests on rational-expectations macroeconomics, which seems to suggest that highly visible nominal policies like currency depreciation should have, at most, very transitory real effects. Indeed, with some time-consistency stories thrown in, one may argue that a country with a propensity to inflationary policies is actually better off pegging its currency to a more disciplined partner, because this commitment will gain it credibility that actually improves the *ex post* tradeoff between inflation and unemployment. If fixed rates are a macroeconomic plus, then any microeconomic gains are icing on the cake; our ignorance about their size doesn't matter for policy purposes.

Unfortunately, this neat solution to our conundrum is just too neat. There was a time when it seemed reasonably plausible for non-German Europe: as long as the United Kingdom, Italy, and even France were preoccupied with regaining credibility in their fight against inflation, one could argue that pegging to the mark was an unambiguous good. But that was a special contingent circumstance. In the world of 1993, when inflation is nobody's top priority and recession is a big problem, when the vices of German fiscal policy have upstaged the virtues of German monetary policy, it becomes clear that the old-fashioned view that pegging one's currency will impose macroeconomic costs is once again the sensible one. For most of 1992, no European policymaker was willing to say as much, but, despite their protestations that they would never contemplate abandoning the ERM, it was obvious to everyone, speculators especially, that the non-tradeoff view was no longer viable. This is not to say that arguments about credibility may not be useful in their place, as in Rodrik's (1993) discussion of the problems of sequencing of reform. We are kidding ourselves, however, if we think that they can settle the optimum-currency-area problem.

Finally, we often try to deal with our microeconomic ignorance by leaning on analogies. In particular, the Great Analogy of international monetary discussion in the late 1980s and early 1990s has turned out to

be between potential currency blocs and the United States. Initially, this analogy was used to justify Europe's lunge toward monetary union. After all, the United States is a continent-sized monetary union that works pretty well, so why shouldn't the same be true for the EC? Subsequent research has driven home just how different Europe is from the United States on at least two of the dimensions of the optimum-currency-area argument—labor mobility and fiscal integration—and the comparison with the United States is now mostly used as a critique of monetary union.

The U.S.-Europe comparison is a useful intellectual strategy. It has led to a lot of very interesting economic research and has clearly raised the tone of the discussion of international monetary reform. I have used it as an effective debating tool myself. Yet it is clear if we are honest with ourselves that it is a bit of an intellectual scam. We can compare Europe (or the North American Free Trade Area, or any other proposed currency bloc) with the United States. But we have no reason to suppose that the United States defines an optimum currency area. Conceivably, the United States would be better off with a half-dozen regional currencies. Equally conceivably, the hidden microeconomic benefits of a common currency are so overwhelming in the United States that Europe should follow suit even though the macroeconomic costs would be much greater. We just don't know. It is not that there are conflicts among the estimates. There are simply no estimates at all. At this point, you may ask me how I propose to remedy this gap. The short answer is that I don't know. All I can do is assert that, if there is one crucial priority in international monetary economics, it is putting some analytical flesh on the microeconomic side of the optimum-currency-area argument.

This lecture is entitled "What Do We Need to Know About the International Monetary System?." Much of it, however, has dealt with things I would *like* to know. I would like to know how the macro model that I more or less believe can be reconciled with the trade models that I also more or less believe. I would like to know how to build a bridge between an intertemporal story about savings and investment and that macroeconomic model. And I would very much like to be able to rebuild a macro structure that I can believe in the desolation that rational expectations left behind. For many purposes, however, including the giving of policy advice, the existing macro model is good enough to serve for the time being. What we *need* to know is how to evaluate the microeconomics of international monetary systems. Until we can do that, we are making policy advice by the seat of our pants.

References

- Bruno, Michael, "Inflation and Growth in an Integrated Approach," paper presented at the Conference on The International Monetary System, honoring the fiftieth anniversary of Essays in International Finance, Princeton, N.J., April 15-16, 1993; forthcoming in Peter B. Kenen, ed., *Understanding Interdependence: The Macroeconomics of the Open Economy*, Princeton, N.J., Princeton University Press, 1994.
- Bryant, Ralph, "International Cooperation for National Macroeconomic Policies: Where Do We Stand?," paper presented at the Conference on The International Monetary System, honoring the fiftieth anniversary of Essays in International Finance, Princeton, N.J., April 15-16, 1993; forthcoming in Peter B. Kenen, ed., *Understanding Interdependence: The Macroeconomics of the Open Economy*, Princeton, N.J., Princeton University Press, 1994.
- Dooley, Michael, "A Retrospective on the Debt Crisis," paper presented at the Conference on The International Monetary System, honoring the fiftieth anniversary of Essays in International Finance, Princeton, N.J., April 15-16, 1993; forthcoming in Peter B. Kenen, ed., *Understanding Interdependence: The Macroeconomics of the Open Economy*, Princeton, N.J., Princeton University Press, 1994.
- Dornbusch, Rudiger, Fischer, Stanley, and Samuelson, Paul, "Comparative Advantage, Trade, and Payments in a Ricardian Model with a Continuum of Goods," *The American Economic Review*, 67 (December 1977), pp. 823-839.
- Eichengreen, Barry, "The Endogeneity of Exchange-Rate Regimes," paper presented at the Conference on The International Monetary System, honoring the fiftieth anniversary of Essays in International Finance, Princeton, N.J., April 15-16, 1993; forthcoming in Peter B. Kenen, ed., *Understanding Interdependence: The Macroeconomics of the Open Economy*, Princeton, N.J., Princeton University Press, 1994.
- Goodhart, Charles, "The Political Economy of Monetary Union," paper presented at the Conference on The International Monetary System, honoring the fiftieth anniversary of Essays in International Finance; forthcoming in Peter B. Kenen, ed., *Understanding Interdependence: The Macroeconomics of the Open Economy*, Princeton, N.J., Princeton University Press, 1994.
- Graham, Frank D., "Some Aspects of Protection Further Considered," *The Quarterly Journal of Economics*, 37 (February 1923), pp. 199-227.
- , *Fundamentals of International Monetary Policy*, Essays in International Finance No. 2, Princeton, N.J., Princeton University, International Finance Section, Autumn 1943.
- Hansen, Lars Peter, and Hodrick, Robert, "Forward Exchange Rates as Optimal Predictors of Future Spot Rates: An Econometric Analysis," *The Journal of Political Economy*, 88 (October 1980), pp. 829-853.

- Hooper, Peter, and Jaime Marquez, "Exchange Rates, Prices, and External Adjustment," paper presented at the Conference on The International Monetary System, honoring the fiftieth anniversary of Essays in International Finance, Princeton, N.J., April 15-16, 1993; forthcoming in Peter B. Kenen, ed., *Understanding Interdependence: The Macroeconomics of the Open Economy*, Princeton, N.J., Princeton University Press, 1994.
- Johnson, Harry, "Towards a General Theory of the Balance of Payments," in Harry Johnson, *International Trade and Economic Growth*, New York, Allen and Unwin, 1958.
- Kenen, Peter B., "The Theory of Optimum Currency Areas: An Eclectic View," in Robert A. Mundell and Alexander K. Swoboda, eds., *Monetary Problems of the International Economy*, Chicago, University of Chicago Press, 1969, pp. 41-60; reprinted in Peter B. Kenen, *Essays in International Economics*, Princeton, N.J., Princeton University Press, 1980, pp. 163-182.
- , *The International Economy*, 2nd ed., Englewood Cliffs, N.J., Prentice-Hall, 1989; 3rd ed., Cambridge and New York, Cambridge University Press, forthcoming 1994.
- Krugman, Paul, "The Narrow Moving Band, the Dutch Disease, and the Competitive Consequences of Mrs. Thatcher: Notes on Trade in the Presence of Dynamic Scale Economies," *Journal of Development Economics*, 27 (October 1987), pp. 41-55.
- , *Rethinking International Trade*, Cambridge, Mass., MIT Press, 1990.
- , *The Age of Diminishing Expectations*, Cambridge, Mass., MIT Press, 1990.
- , *Currencies and Crises*, Cambridge, Mass., MIT Press, 1992.
- Krugman, Paul, and Maurice Obstfeld, *International Economics: Theory and Policy*, 2nd ed., New York, Harper Collins, 1991.
- , *Has the Adjustment Process Worked?*, Washington, D.C., Institute for International Economics, 1991.
- Mankiw, N. Gregory, *Macroeconomics*, New York, Worth, 1992.
- Mankiw, N. Gregory, and David Romer, eds., *New Keynesian Economics*, Cambridge, Mass., MIT Press, 1991.
- McKinnon, Ronald I., "Optimum Currency Areas," *The American Economic Review*, 53 (September 1963), pp. 717-724.
- Mundell, Robert A., "A Theory of Optimum Currency Areas," *The American Economic Review*, 51 (September 1961), pp. 657-664.
- Obstfeld, Maurice, "International Capital Mobility in the 1990s," paper presented at the Conference on The International Monetary System, honoring the fiftieth anniversary of Essays in International Finance; forthcoming in Peter B. Kenen, ed., *Understanding Interdependence: The Macroeconomics of the Open Economy*, Princeton, N.J., Princeton University Press, 1994.
- Razin, Assaf, "The Dynamic-Optimizing Approach to the Current Account: Theory and Evidence," paper presented at the Conference on The International Monetary System, honoring the fiftieth anniversary of Essays in

- International Finance, Princeton, N.J., April 15-16, 1993; forthcoming in Peter B. Kenen, ed., *Understanding Interdependence: The Macroeconomics of the Open Economy*, Princeton, N.J., Princeton University Press, 1994.
- Rodrik, Dani, "Trade Liberalization in Disinflation," paper presented at the Conference on The International Monetary System, honoring the fiftieth anniversary of Essays in International Finance, Princeton, N.J., April 15-16, 1993; forthcoming in Peter B. Kenen, ed., *Understanding Interdependence: The Macroeconomics of the Open Economy*, Princeton, N.J., Princeton University Press, 1994.
- Swan, Trevor, "Longer-Run Problems of the Balance of Payments," in Heinz W. Arndt and W. Max Corden, eds., *The Australian Economy: A Volume of Readings*, Melbourne, Cheshire Press, 1963.
- Taylor, Mark, "Exchange-Rate Behavior Under Alternative Exchange-Rate Arrangements," paper presented at the Conference on The International Monetary System, honoring the fiftieth anniversary of Essays in International Finance, Princeton, N.J., April 15-16, 1993; forthcoming in Peter B. Kenen, ed., *Understanding Interdependence: The Macroeconomics of the Open Economy*, Princeton, N.J., Princeton University Press, 1994.
- Weinberg, Stephen, *Dreams of a Final Theory*, New York, Pantheon, 1992.

FRANK D. GRAHAM MEMORIAL LECTURERS

1950–1951	Milton Friedman
1951–1952	James E. Meade
1952–1953	Sir Dennis Robertson
1953–1954	Paul A. Samuelson
1955–1956	Gottfried Haberler
1956–1957	Ragnar Nurkse
1957–1958	Albert O. Hirschman
1959–1960	Robert Triffin
1960–1961	Jacob Viner
1961–1962	Don Patinkin
1962–1963	Friedrich A. Lutz (Essay 41)
1963–1964	Tibor Scitovsky (Essay 49)
1964–1965	Sir John Hicks
1965–1966	Robert A. Mundell
1966–1967	Jagdish N. Bhagwati (Special Paper 8)
1967–1968	Arnold C. Harberger
1968–1969	Harry G. Johnson
1969–1970	Richard N. Cooper (Essay 86)
1970–1971	W. Max Corden (Essay 93)
1971–1972	Richard E. Caves (Special Paper 10)
1972–1973	Paul A. Volcker
1973–1974	J. Marcus Fleming (Essay 107)
1974–1975	Anne O. Krueger (Study 40)
1975–1976	Ronald W. Jones (Special Paper 12)
1976–1977	Ronald I. McKinnon (Essay 125)
1977–1978	Charles P. Kindleberger (Essay 129)
1978–1979	Bertil Ohlin (Essay 134)
1979–1980	Bela Balassa (Essay 141)
1980–1981	Marina von Neumann Whitman (Essay 143)
1981–1982	Robert E. Baldwin (Essay 150)
1983–1984	Stephen Marris (Essay 155)
1984–1985	Rudiger Dornbusch (Essay 165)
1986–1987	Jacob A. Frenkel (Study 63)
1987–1988	Ronald Findlay (Essay 177)
1988–1989	Michael Bruno (Essay 183)
1988–1989	Elhanan Helpman (Special Paper 16)
1989–1990	Michael L. Mussa (Essay 179)
1990–1991	Toyoo Gyohten
1991–1992	Stanley Fischer
1992–1993	Paul Krugman (Essay 190)

PUBLICATIONS OF THE
INTERNATIONAL FINANCE SECTION

Notice to Contributors

The International Finance Section publishes papers in four series: ESSAYS IN INTERNATIONAL FINANCE, PRINCETON STUDIES IN INTERNATIONAL FINANCE, and SPECIAL PAPERS IN INTERNATIONAL ECONOMICS contain new work not published elsewhere. REPRINTS IN INTERNATIONAL FINANCE reproduce journal articles previously published by Princeton faculty members associated with the Section. The Section welcomes the submission of manuscripts for publication under the following guidelines:

ESSAYS are meant to disseminate new views about international financial matters and should be accessible to well-informed nonspecialists as well as to professional economists. Technical terms, tables, and charts should be used sparingly; mathematics should be avoided.

STUDIES are devoted to new research on international finance, with preference given to empirical work. They should be comparable in originality and technical proficiency to papers published in leading economic journals. They should be of medium length, longer than a journal article but shorter than a book.

SPECIAL PAPERS are surveys of research on particular topics and should be suitable for use in undergraduate courses. They may be concerned with international trade as well as international finance. They should also be of medium length.

Manuscripts should be submitted in triplicate, typed single sided and double spaced throughout on 8½ by 11 white bond paper. Publication can be expedited if manuscripts are computer keyboarded in WordPerfect 5.1 or a compatible program. Additional instructions and a style guide are available from the Section.

How to Obtain Publications

The Section's publications are distributed free of charge to college, university, and public libraries and to nongovernmental, nonprofit research institutions. Eligible institutions may ask to be placed on the Section's permanent mailing list.

Individuals and institutions not qualifying for free distribution may receive all publications for the calendar year for a subscription fee of \$35.00. Late subscribers will receive all back issues for the year during which they subscribe. Subscribers should notify the Section promptly of any change in address, giving the old address as well as the new.

Publications may be ordered individually, with payment made in advance. ESSAYS and REPRINTS cost \$8.00 each; STUDIES and SPECIAL PAPERS cost \$11.00. An additional \$1.25 should be sent for postage and handling within the United States, Canada, and Mexico; \$1.50 should be added for surface delivery outside the region.

All payments must be made in U.S. dollars. Subscription fees and charges for single issues will be waived for organizations and individuals in countries where foreign-exchange regulations prohibit dollar payments.

Please address all correspondence, submissions, and orders to:

International Finance Section
Department of Economics, Fisher Hall
Princeton University
Princeton, New Jersey 08544-1021

List of Recent Publications

A complete list of publications may be obtained from the International Finance Section.

ESSAYS IN INTERNATIONAL FINANCE

157. Wilfred J. Ethier and Richard C. Marston, eds., with Charles P. Kindleberger, Jack Guttentag, Richard Herring, Henry C. Wallich, Dale W. Henderson, and Randall Hinshaw, *International Financial Markets and Capital Movements: A Symposium in Honor of Arthur I. Bloomfield*. (September 1985)
158. Charles E. Dumas, *The Effects of Government Deficits: A Comparative Analysis of Crowding Out*. (October 1985)
159. Jeffrey A. Frankel, *Six Possible Meanings of "Overvaluation": The 1981-85 Dollar*. (December 1985)
160. Stanley W. Black, *Learning from Adversity: Policy Responses to Two Oil Shocks*. (December 1985)
161. Alexis Rieffel, *The Role of the Paris Club in Managing Debt Problems*. (December 1985)
162. Stephen E. Haynes, Michael M. Hutchison, and Raymond F. Mikesell, *Japanese Financial Policies and the U.S. Trade Deficit*. (April 1986)
163. Arminio Fraga, *German Reparations and Brazilian Debt: A Comparative Study*. (July 1986)
164. Jack M. Guttentag and Richard J. Herring, *Disaster Myopia in International Banking*. (September 1986)
165. Rudiger Dornbusch, *Inflation, Exchange Rates, and Stabilization*. (October 1986)
166. John Spraos, *IMF Conditionality: Ineffectual, Inefficient, Mistargeted*. (December 1986)
167. Rainer Stefano Masera, *An Increasing Role for the ECU: A Character in Search of a Script*. (June 1987)
168. Paul Mosley, *Conditionality as Bargaining Process: Structural-Adjustment Lending, 1980-86*. (October 1987)
169. Paul A. Volcker, Ralph C. Bryant, Leonhard Gleske, Gottfried Haberler, Alexandre Lamfalussy, Shijuro Ogata, Jesús Silva-Herzog, Ross M. Starr, James Tobin, and Robert Triffin, *International Monetary Cooperation: Essays in Honor of Henry C. Wallich*. (December 1987)
170. Shafiqul Islam, *The Dollar and the Policy-Performance-Confidence Mix*. (July 1988)
171. James M. Boughton, *The Monetary Approach to Exchange Rates: What Now Remains?* (October 1988)
172. Jack M. Guttentag and Richard M. Herring, *Accounting for Losses On Sovereign Debt: Implications for New Lending*. (May 1989)
173. Benjamin J. Cohen, *Developing-Country Debt: A Middle Way*. (May 1989)
174. Jeffrey D. Sachs, *New Approaches to the Latin American Debt Crisis*. (July 1989)
175. C. David Finch, *The IMF: The Record and the Prospect*. (September 1989)
176. Graham Bird, *Loan-loss Provisions and Third-World Debt*. (November 1989)

177. Ronald Findlay, *The "Triangular Trade" and the Atlantic Economy of the Eighteenth Century: A Simple General-Equilibrium Model*. (March 1990)
178. Alberto Giovannini, *The Transition to European Monetary Union*. (November 1990)
179. Michael L. Mussa, *Exchange Rates in Theory and in Reality*. (December 1990)
180. Warren L. Coats, Jr., Reinhard W. Furstenberg, and Peter Isard, *The SDR System and the Issue of Resource Transfers*. (December 1990)
181. George S. Tavlas, *On the International Use of Currencies: The Case of the Deutsche Mark*. (March 1991)
182. Tommaso Padoa-Schioppa, ed., with Michael Emerson, Kumiharu Shigehara, and Richard Portes, *Europe After 1992: Three Essays*. (May 1991)
183. Michael Bruno, *High Inflation and the Nominal Anchors of an Open Economy*. (June 1991)
184. Jacques J. Polak, *The Changing Nature of IMF Conditionality*. (September 1991)
185. Ethan B. Kapstein, *Supervising International Banks: Origins and Implications of the Basle Accord*. (December 1991)
186. Alessandro Giustiniani, Francesco Papadia, and Daniela Porciani, *Growth and Catch-Up in Central and Eastern Europe: Macroeconomic Effects on Western Countries*. (April 1992)
187. Michele Fratianni, Jürgen von Hagen, and Christopher Waller, *The Maastricht Way to EMU*. (June 1992)
188. Pierre-Richard Agénor, *Parallel Currency Markets in Developing Countries: Theory, Evidence, and Policy Implications*. (November 1992)
189. Beatriz Armendariz de Aghion and John Williamson, *The G-7's Joint-and-Several Blunder*. (April 1993)
190. Paul Krugman, *What Do We Need to Know About the International Monetary System?*. (July 1993)

PRINCETON STUDIES IN INTERNATIONAL FINANCE

55. Marsha R. Shelburn, *Rules for Regulating Intervention Under a Managed Float*. (December 1984)
56. Paul De Grauwe, Marc Janssens and Hilde Leliaert, *Real-Exchange-Rate Variability from 1920 to 1926 and 1973 to 1982*. (September 1985)
57. Stephen S. Golub, *The Current-Account Balance and the Dollar: 1977-78 and 1983-84*. (October 1986)
58. John T. Cuddington, *Capital Flight: Estimates, Issues, and Explanations*. (December 1986)
59. Vincent P. Crawford, *International Lending, Long-Term Credit Relationships, and Dynamic Contract Theory*. (March 1987)
60. Thorvaldur Gylfason, *Credit Policy and Economic Activity in Developing Countries with IMF Stabilization Programs*. (August 1987)
61. Stephen A. Schuker, *American "Reparations" to Germany, 1919-33: Implications for the Third-World Debt Crisis*. (July 1988)
62. Steven B. Kamin, *Devaluation, External Balance, and Macroeconomic Performance: A Look at the Numbers*. (August 1988)

63. Jacob A. Frenkel and Assaf Razin, *Spending, Taxes, and Deficits: International-Intertemporal Approach*. (December 1988)
64. Jeffrey A. Frankel, *Obstacles to International Macroeconomic Policy Coordination*. (December 1988)
65. Peter Hooper and Catherine L. Mann, *The Emergence and Persistence of the U.S. External Imbalance, 1980-87*. (October 1989)
66. Helmut Reisen, *Public Debt, External Competitiveness, and Fiscal Discipline in Developing Countries*. (November 1989)
67. Victor Argy, Warwick McKibbin, and Eric Sieglhoff, *Exchange-Rate Regimes for a Small Economy in a Multi-Country World*. (December 1989)
68. Mark Gersovitz and Christina H. Paxson, *The Economies of Africa and the Prices of Their Exports*. (October 1990)
69. Felipe Larraín and Andrés Velasco, *Can Swaps Solve the Debt Crisis? Lessons from the Chilean Experience*. (November 1990)
70. Kaushik Basu, *The International Debt Problem, Credit Rationing and Loan Pushing: Theory and Experience*. (October 1991)
71. Daniel Gros and Alfred Steinherr, *Economic Reform in the Soviet Union: Pas de Deux between Disintegration and Macroeconomic Destabilization*. (November 1991)
72. George M. von Furstenberg and Joseph P. Daniels, *Economic Summit Declarations, 1975-1989: Examining the Written Record of International Cooperation*. (February 1992)
73. Ishac Diwan and Dani Rodrik, *External Debt, Adjustment, and Burden Sharing: A Unified Framework*. (November 1992)
74. Barry Eichengreen, *Should the Maastricht Treaty Be Saved?*. (December 1992)

SPECIAL PAPERS IN INTERNATIONAL ECONOMICS

15. Gene M. Grossman and J. David Richardson, *Strategic Trade Policy: A Survey of Issues and Early Analysis*. (April 1985)
16. Elhanan Helpman, *Monopolistic Competition in Trade Theory*. (June 1990)
17. Richard Pomfret, *International Trade Policy with Imperfect Competition*. (August 1992)
18. Hali J. Edison, *The Effectiveness of Central-Bank Intervention: A Survey of the Literature After 1982*. (July 1993)

REPRINTS IN INTERNATIONAL FINANCE

25. Jorge Braga de Macedo, *Trade and Financial Interdependence under Flexible Exchange Rates: The Pacific Area*; reprinted from *Pacific Growth and Financial Interdependence*, 1986. (June 1986)
26. Peter B. Kenen, *The Use of IMF Credit*; reprinted from *Pulling Together: The International Monetary Fund in a Multipolar World*, 1989. (December 1989)
27. Peter B. Kenen, *Transitional Arrangements for Trade and Payments Among the CMEA Countries*; reprinted from *International Monetary Fund Staff Papers* 38 (2), 1991. (July 1991)

The work of the International Finance Section is supported in part by the income of the Walker Foundation, established in memory of James Theodore Walker, Class of 1927. The offices of the Section, in Fisher Hall, were provided by a generous grant from Merrill Lynch & Company.

ISBN 0-88165-097-8

Recycled Paper