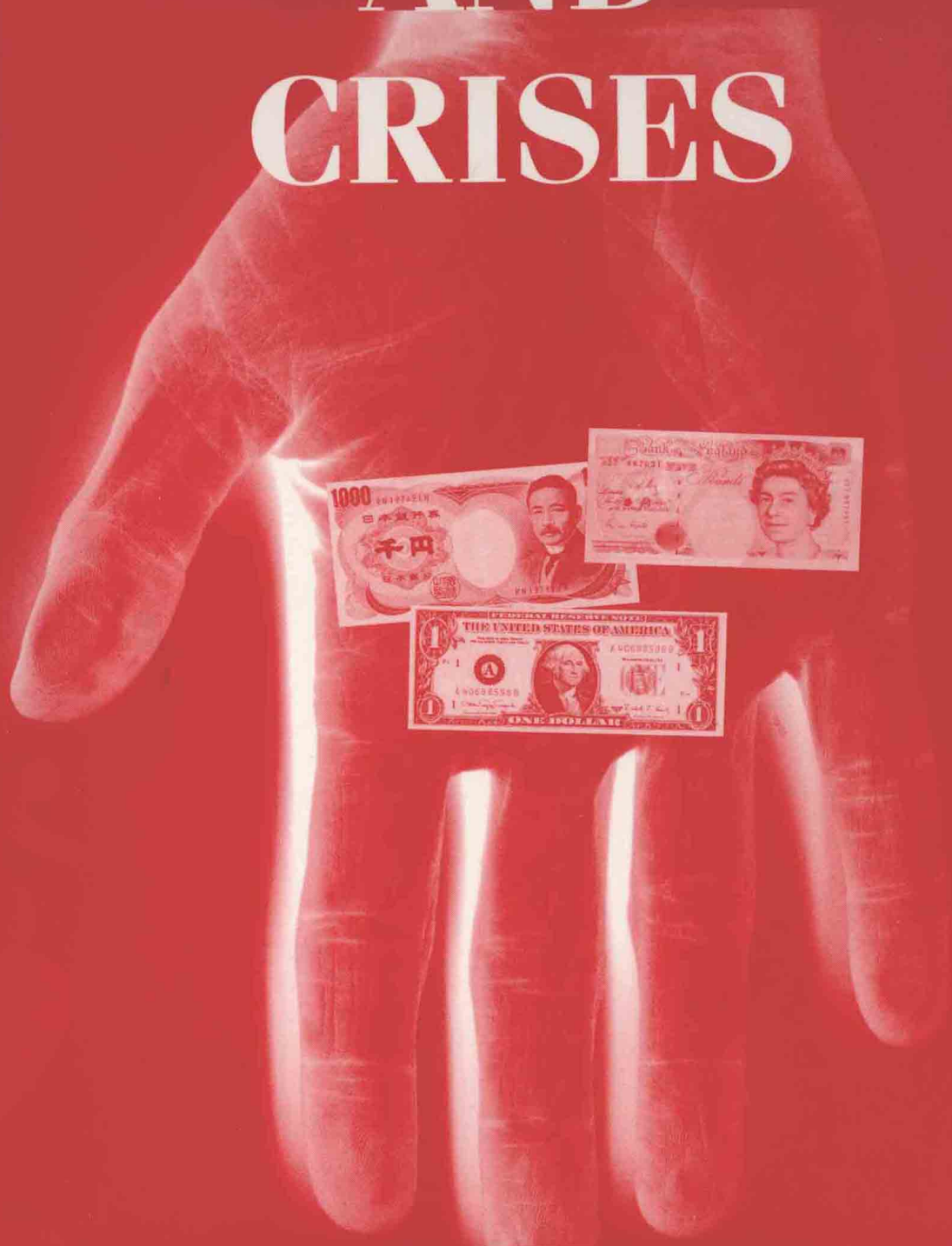


CURRENCIES AND CRISES



Currencies and Crises

Paul R. Krugman

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Preface

This is my second book of collected papers. The first, *Rethinking International Trade*, was a collection built around a single theme: the reconstruction of international trade theory to take account of increasing returns and imperfect competition. My work on international monetary economics, the subject of this collection, has often involved the construction of analytical models, but it has been motivated less by the desire to reconstruct theory than by the attempt to make sense of a rapidly changing world. So this is a book of less intellectual coherence but perhaps more immediate real-world relevance.

This kind of work cannot be done in isolation. My ideas about international monetary economics have emerged out of countless bleary-eyed discussions on the 6:40 AM flight to Washington, heated exchanges at Cambridge blackboards, and unplanned seminars that spontaneously developed over bitter coffee drunk from paper cups. So every one of these papers in a way has several coauthors. Since my graduate student days Rudiger Dornbusch, whose intellectual sharpness has been reinforced by ever-growing policy reach and acumen, has been a crucial source of ideas and advice. My colleague and coauthor Julio Rotemberg has been an invaluable sounding board over the years. Another colleague and coauthor, Ken Froot, played a key role in the development of several of the papers in this book. And all of my work has benefited immensely by the intellectual environment provided both by MIT and by the National Bureau of Economic Research.

Finally, as always, it is family—my parents and my wife—who provide the bedrock on which everything else depends.

Introduction

When the Bretton Woods system of fixed exchange rates broke up in the early 1970s, most international economists were not dismayed. Not only did they believe that greater flexibility of exchange rates was a good thing; they also believed that they understood reasonably well how the new system would work.

They were wrong. The last 20 years of the international monetary system have involved one surprise after another, most of them unpleasant, all of them forcing economists to scramble to keep up with new issues and unexpected turns in old ones.

The 11 papers collected in this book record my own international monetary scramblings from the late 1970s to the beginning of the 1990s. They come from a period in which all economists writing on international money and macro, myself very much included, were repeatedly getting it wrong: making incorrect predictions, or simply failing to perceive the imminent importance of a new issue and thus being caught flat-footed by events. It was also, however, a period of great intellectual excitement. For it is paradoxically true that times of radical change, in which economic theory is most likely to go wrong, are also the times in which such theory is most useful and interesting. In calm times the theorist mostly gets it right, but he has little to offer the experienced practitioner and may find it hard to come up with new ideas. In crazy times the theorist mostly gets it wrong; but the experienced practitioner, who has never seen anything like what is happening, hasn't a clue; and new ideas are virtually forced upon the researcher by each day's newspapers. And although one might expect that research that responds to events would quickly come to look dated, my own experience has been that papers written in an effort to understand current issues are often the ones that best stand the test of time.

It would be nice to go even further, to say that all of these pieces represent steps on the road to a unified theory of international money and

macroeconomics. Unfortunately, they don't. Partly this is because they were written not in pursuit of a general theory but in response to specific issues as they arose. Mostly, however, it is because a unified monetary theory (domestic or international) is a goal that seems to recede further every year. So this volume is more like a song cycle than a sonata. Yet there are some unifying themes, both those that apply to all the papers and those that link the papers into specific groups.

General Unifying Themes

The main overall unifying theme in this volume is methodological. All of the papers use the same basic approach: take a real-world problem that is a source of puzzlement or dispute, and construct a small theoretical model that in some simplified way seems to get at the essence of the problem. While some of the papers try to be quantitative as well as conceptual, the essential purpose of this kind of economic analysis is to build intuition. Success in the effort is measured by the "aha" index: A paper has achieved something if the informed reader feels that she has gained a significantly greater insight into an important problem, an insight whose major content is often that of providing a language to discuss that problem. This is the kind of economics that Robert Solow did when he introduced the basic concepts of growth theory, that James Tobin did when he introduced the idea of q , that Robert Lucas did when he introduced the concept of a rational expectations business cycle.

This kind of economics is not universally popular, to say the least. On one side, many contemporary economists now believe that economic analysis is valid only if it is built on microeconomic foundations from the ground up, that there is no room for models that start somewhere in the middle; an alarmingly large number of academics also tend to dismiss simple models on the ground that if they are easy to understand they must also be trivial. On the other side, some economists and many practical people see little utility in abstract theorizing of any sort; they want to see quantitative results ready for immediate application.

Obviously I disagree with both positions. The growing demand for rigor and mathematical difficulty in economics has, I would argue, gone much too far; if we refuse to think about anything until we can base our thoughts rigorously on the axioms of microeconomics, we will abandon most of what makes economics interesting and useful. In any case supposedly rigorous analysis is not necessarily actually reliable. To take the most glaring example, many economists have rejected the idea that nominal prices are sticky

because they believe that such stickiness is inconsistent with fully rational behavior. But there is abundant evidence that prices are in fact sticky, and no evidence at all that people are fully rational, so an economist who assumes sticky prices may well be on more solid ground than the purist who condemns her. Eventually economics may become a truly integrated structure in which it is possible to address all issues using a consistent set of assumptions. But that millenium is nowhere near, and meanwhile I refuse to give away the insights that a little ad hockery can bring.

On the other side, in the real world there is little patience for economists who want to offer insights rather than produce specific predictions, and a general preference for the judgment of people who have experience and look prosperous over academics who base their ideas on models. Yet in times of radical change this preference is exactly wrong. An anecdote may make the point. I once helped prepare a background report for a Third World debtor country, which, among other things, was critical of plans for a repurchase of part of that country's debt on the secondary market. (The reasons for that critical position are discussed in chapter 9). The country's central bank, with advice from its bankers, dismissed the criticism, with some waspish remarks about the silliness of relying on abstract arguments rather than the experience of practitioners. But of course there are no experienced practitioners in the area of Third World debt reduction—nothing remotely like the current experience has happened since the 1930s. Guesswork is all that we have to go on, and those who discipline their guesses with models are more reliable than those who fly by the seat of their pants, no matter how well tailored. In fact, in the particular area of market-based debt reduction, the bankers have persistently been wildly impractical, arguing that a little financial engineering will produce perpetual motion machines, while the economic theorists have correctly pointed out the limitations of clever financial schemes.

So the papers in this volume are linked by a methodological approach that uses abstract models but is willing to cut corners on rigor in order to gain insight. This methodology brings with it a natural attitude toward modeling style: a very strong preference for maximum simplicity. For economists who believe that maximization and equilibrium are unchallengeable axioms rather than frequently convenient metaphors, simplicity and elegance are less important than Truth. For economists who are actually building empirical models, complexity may be necessary in order to save the phenomena. If one is trying to build insight, however, it is essential to clear away as much clutter as possible. That means looking for the simplest, cleanest model that makes the point.

Simplicity does not mean an absence of technique. Sometimes one needs to introduce fancy mathematics in order to express an idea simply. For example, the analyses of the J-curve and adjustment in chapter 2, and of speculative attack in chapter 4, use the idea of saddle-path instability to describe succinctly the key role of expectations; the analysis of exchange rate target zones in chapters 5 and 6 relies on stochastic calculus to simplify an otherwise messy description of speculative behavior; the discussion of exchange rate regimes in chapter 11 implicitly appeals to the idea of subgame perfection to pin down the nature of the credibility problem. The point, however, is to wear one's technique lightly, to use it to make things easier, not harder. One should not fall into the trap of assuming that only papers with lots of theorems and lemmas are sophisticated, when often the truest sophistication is finding a way to express novel ideas with no more than a diagram or a numerical example.

The common ground of all of these papers then is one of method and style. The papers also, however, fall into four groups organized around particular substantive themes. The subthemes of the book are, first, the role of exchange rates in balance of payments adjustment; second, the role of speculation in the functioning of exchange rate regimes; third, the problem of Third World debt; and finally, the problem of constructing an international monetary system.

Within each group the papers use related models so that each group of papers at least gropes toward a unified approach to its particular subject. Thus the three papers on balance-of-payments adjustment focus on the role of the real exchange rate and move from a static toward a dynamic description of that role; the three papers on speculation use the same stripped-down monetary approach, and the third of the papers essentially integrates the first two; the three papers on debt share the common theme that debt reduction represents a trade-off of option value against incentives and build toward an increasingly policy-oriented statement of that framework; and the two papers on the international monetary system share an emphasis on the microeconomics of money. Any suggestion of small-minded consistency within each set of papers is, however, dispelled by the nearly complete absence of common assumptions between sections.

Exchange Rates and International Adjustment

The first three chapters in this volume examine an old issue: the relationship between currency devaluation and the trade balance.

It may seem surprising that this issue could still be a subject of dispute. After all, during the 1940s and 1950s there were a series of classic papers

on the theory of devaluation by Joan Robinson, Gottfried Haberler, Sidney Alexander, and above all James Meade. By the beginning of the 1980s there had also been extensive empirical work on the short- and medium-run determinants of trade flows, whose conclusions have mostly stood up fairly well over the past decade. One might have thought that little new was left to say.

Yet the rise and fall of the dollar during the 1980s turned the topic of exchange rates and trade adjustment into an area of heated dispute. One major reason was that influential economists—represented in particular by Ronald McKinnon and Robert Mundell—fiercely denounced the traditional view that a country could most easily reduce a trade deficit by allowing its currency to depreciate. Their view, which was echoed by policymakers and journalists, was that the growing international mobility of capital had ended any useful role for exchange rate adjustment. Changes in national savings-investment balances could now, they claimed, be translated directly into changes in trade balances, with depreciation playing no useful role. This was more than an analytical point. The McKinnon-Mundell argument was a key debating point for those who argued for a return to fixed exchange rates. Remarkably, few international economists rose to the defense of the conventional wisdom, perhaps because the mundane issue of trade balance adjustment had attracted so little research attention in the previous decade that the simple things had been forgotten.

Somebody needed to argue forcefully that this challenge to standard views was wrong both conceptually and empirically. In chapter 1, originally written for the unofficial advisory Group of 30, I took on that task. The chapter confronts the widely held idea that high mobility of financial capital somehow allows changes in saving or investment to influence imports or exports directly, rather than via a mechanism in which the exchange rate plays a crucial role. John Williamson has felicitously called this the doctrine of “immaculate transfer”; it is a misconception pure and simple. The chapter also shows that as an empirical matter, reducing a trade deficit requires substantial real depreciation by the deficit country and that there is overwhelming evidence that real depreciation is most easily accomplished by devaluing the nominal exchange rate. In other words, the chapter was an effort to defend the (hard-won) conventional wisdom on the usefulness of exchange rate adjustment against fashionable but misguided criticism.

When chapter 1 was written (in summer 1987), its defense of the usefulness and effectiveness of currency depreciation sounded a bit hollow. At the time the falling dollar had not yet had any noticeable effect in reducing the U.S. trade deficit, while the capital inflow that was the counterpart of

the deficit was increasingly taking the form of foreign purchases of U.S. corporations. This led to charges both by global monetarists and by protectionists that the fall in the dollar, instead of reducing the trade deficit, was leading to a "fire sale" of U.S. assets at distress prices. In chapter 2, written a year later, I tried to show that the perception of a fire sale was both misleading and a natural part of the adjustment process, that precisely because of long lags in the response of trade flows to the exchange rate, it was to be expected that during an interim period the United States would attract capital via a perceived low value of its currency.

Both chapter 1 and chapter 2 were essentially conservative intellectually, arguing that traditional views about short- and medium-run trade adjustment were still right in spite of trendy intellectual challenges and a few years of disappointing trade figures. Chapter 3 argues, however, that the demand-side framework that continues to work fairly well at explaining trade flows in the medium run is badly misleading for the long run. The standard framework sees each country as facing a fairly inelastic downward-sloping demand curve for its exports. This suggests that countries with rapidly growing exports, like Japan or Korea, should have rapidly falling relative export prices over time. Yet this is manifestly not the case, a fact that is explained in the standard model by a coincidental correlation between high growth and favorable income elasticities.

What chapter 3 argues with the aid of a simple illustrative model is that no coincidence is involved, that long-run trends in exports and imports reflect supply-side effects which conventional estimates mistakenly pick up as income elasticities. The main implication of this argument is that purchasing power parity is a much better guide to equilibrium exchange rates in the long run than the abject failure of PPP in the short run might have suggested.

Speculation and Exchange Regimes

Before the 1970s balance-of-payments analyses focused on flows: on the trade balance and on what were perceived as sustained, gradual movements of capital. Indeed, for a time it was standard for economists to gauge a country's position by a so-called basic balance that added the current account and long-term capital flows; this basic balance was assumed to be what really mattered, with short-term capital flows more of a nuisance than a really important issue.

With the growing international mobility of capital after 1970, however, it became increasingly clear that there were substantial stocks of capital that

were prepared to move en masse from one currency to another. Countries that tried to fix their exchange rates could find their reserves (or, in the case of surplus countries, their patience) quickly exhausted by massive speculative attacks; countries with floating exchange rates found that those rates fluctuated with the expectations of speculators. At the same time the rational expectations revolution in economics made it possible to say more interesting (if not always true) things about speculative behavior than had been possible before.

Chapter 4, which is the oldest paper in this volume, addresses the role of speculation in forcing the collapse of a fixed rate regime. When I wrote it early in 1977, I had in mind the collapse of Bretton Woods and the short-lived Smithsonian agreement that followed. But the model really came into its own a few years later with the huge capital flight from Argentina, Venezuela, and Mexico on the eve of the debt crisis. Building on a classic analysis of the gold market by Stephen Salant and Dale Henderson, the chapter shows that sudden runs on a country's currency need not be the result of irrational herd behavior on the part of speculators. On the contrary, when a country attempts to peg its exchange rate without following domestic policies consistent with that goal, a speculative attack that exhausts the country's foreign exchange reserves in a very short time is actually predictable. The concept of speculative attack has now become a standard tool of international monetary analysis. Unfortunately, policy-makers continue to give us opportunities to apply it.

Chapter 5 addresses the role of speculation in a seemingly very different context. During the 1970s and 1980s the unexpected volatility of exchange rates created considerable dismay; yet many people were reluctant to contemplate a return to fully fixed rates. When European countries formed the European Monetary System, they allowed exchange rates to fluctuate within considerably wider bands than under Bretton Woods. Influential economists who advocated stabilization of all major currencies called for still wider bands, which John Williamson (again) felicitously dubbed "target zones." Advocates of target zones hoped that they would preserve some flexibility, while curbing the speculative instability that they felt was playing a destructive role under floating rates.

As late as 1987, however, despite widespread discussion and even half-hearted implementation of the target zone idea, there were no clear models of how such zones would work. Some economists even claimed that target zones would destabilize exchange rates by offering speculators fixed targets to shoot at. What chapter 5 (initially presented at a 1987 conference) did was to introduce a simple approach to modeling target zones, showing that

rational speculation under such a zone would in fact be stabilizing. The interest of the paper was not only in its practical application but in the novelty of its technique (which was simultaneously and independently brought into international economics in a different context by Avinash Dixit): It was followed by scores of papers that applied the idea of “regulated Brownian motion” to more complex models, both in international economics and elsewhere.

Chapter 6 represents a different kind of extension; it links the target zone analysis to the speculative attack analysis in chapter 4. The model was at least partly motivated by concerns that target zone arrangements, like fixed exchange rates, would often fail to be backed up by adequate domestic policies. It also turns out that allowing for the possibility of speculative attacks helps to demystify some of the technical apparatus that has grown up around the original target zone model.

Third World Debt

In 1982 bank lending to developing countries dried up, and more than a dozen countries found themselves unable to service the debt they had accumulated over the previous decade. The debt crisis has been an enduring source of policy anxiety—some of which I shared in the early stages, as a staffer at the Council of Economic Advisers. It has also been, to be honest, a source of much intellectual fascination.

Chapters 7, 8, and 9 date not from the early stages of the crisis but from the period from 1986 on when the initial strategy for coping with the crisis began to unravel. In 1982 and 1983 most analysts—myself included—thought that if the principal on Third World debt were rescheduled and some of the interest recycled through “concerted lending,” economic growth would allow most debtors to work their way back into creditworthiness. By 1986 this was starting to look unrealistic, and there were growing calls for a shift to debt forgiveness. Yet there seemed to be a lack of clear thinking about how to decide what debt strategy was appropriate.

Chapter 7 was an effort to clarify that issue. At the time it was written (during the winter of 1986–87, when I was visiting at the IMF), the discussion on debt strategy was marked by absolutism. That is, bankers and U.S. officials insisted that debt should never be forgiven under any circumstances; some economists argued, on the contrary, that whenever countries are unable to service their debt it is in everyone’s interest to “recognize reality” and reduce the debt burden. Chapter 7 suggested that both positions were wrong because there is a trade-off involved. Creditors want to

keep claims high in order to benefit if the debtor happens to do better than expected, but reducing claims offers the hope that a lower debt burden will enhance a country's ability to service the remaining debt.

From 1986 to 1989, while official policy opposed any concerted debt reduction, there were widespread hopes that ingenious financial schemes, such as debt-equity swaps, could be used to engineer a decentralized solution to the debt problem. The point of chapter 8, written early in 1988, was to argue that these hopes were largely misguided. The paper offered a compressed summary of the trade-off discussed in chapter 7, the "debt relief Laffer curve." It then used this organizing device to show that the conditions under which financial engineering could benefit both debtors and creditors were essentially the same as those under which concerted debt reduction was in everyone's interest, so there was no magic in market-based approaches.

A year later the scene had changed again. Early in 1989 the U.S. Treasury Secretary Nicholas Brady declared himself in favor of an effort to reduce developing country debt and indicated that at least some resources from the World Bank and other sources would be made available to help finance such reductions. The actual content of the "Brady Plan" was, however, unclear. Initially, at least, U.S. officials seemed to believe that clever financial packages would allow them to leverage small outside contributions into large debt reductions, without any kind of coercion of private creditors. Chapter 9, which was presented that summer at the Latin American Econometric Society, was intended as a sort of primer. Using the same framework as chapter 8, and avoiding algebra as much as possible, it tried to lay out the case for concerted rather than market-based debt reduction. (The only really major Brady deal so far has been for Mexico. Although banks were given a menu of options, they were not given the option of not participating, so the principle of voluntarism was in effect abandoned.)

The International Monetary System

The Bretton Woods system, a system of fixed exchange rates in which the United States effectively dictated world monetary policy, ended in the early 1970s. Yet the new system (or nonsystem) that followed has never really settled down.

Despite the collapse of the dollar-centered exchange rate system, the U.S. dollar has continued to play a special international role. During the early years of floating exchange rates, however, there was considerable speculation about whether the dollar's key currency status would survive, and

whether a collapse of that role would pose dangers to the system. Chapter 10, written in 1980, argued that the self-perpetuating aspects of the key currency role could allow the dollar to retain its special role for the foreseeable future. A main purpose of the paper was to point out the importance of increasing returns, circular causation, and multiple equilibria in the international monetary system. These were unpopular themes at the time but have more recently become the cutting edge of theory in a number of fields.

Finally, at the end of the 1980s, when we thought that we had seen everything, Europe surprised us all with a sudden drive toward monetary union. Chapter 11 is an interpretive survey of the cases for and against monetary areas. Like the first two chapters of this volume, it is in large part a defense of a conventional wisdom that has come under so much attack that it now seems almost radical. The traditional analysis of currency areas stressed the painful trade-off between microeconomic efficiency and macroeconomic adjustment, a trade-off that could not be avoided but that could be best met by a world carved up into optimal currency areas. In the new enthusiasm for fixed rates and monetary union, however, many economists have come to deny the existence of that trade-off. They deny that exchange rate flexibility conveys any macroeconomic advantages (echoing some of the issues discussed in chapter 1) and invoke new issues, like the need to achieve inflation-fighting credibility. The chapter argues that the old trade-off view remains valid. It also offers the unpopular suggestion that there is little real evidence to back the current enthusiasm for European monetary union and that the case for EMU is essentially political rather than economic.

What Comes Next?

International money is the oldest field in economics: David Hume's "Of the Balance of Trade," published in 1753, was the first real example of an economic model as we now understand it. It is also one of the most innovative and rapidly changing fields because the best analysis in this area is driven not by deference to established theory but by the need to make sense of an ever-changing world.

It is always risky to predict where interesting areas of research will lie in the future, especially given the incredible succession of surprises over the past 20 years. My guess, however, is that the focus of international monetary economics over the next few years will shift from macroeconomics to microeconomics. As always, interesting work will be driven to an important extent by real world events, in this case the growth of regional trading

blocs. The EC has been reinforced by enlargement and 1992, the U.S.-Canada free trade area will probably be enlarged by the addition of Mexico, and (some would argue) a characteristically unlegislated but still *de facto* East Asian trading area centered on Japan is starting to emerge. The obvious next question is how these new trading arrangements will affect monetary arrangements. The Europeans think they know the answer: A recent EC document is called *One Market, One Money*. But does this mean that Canadian and Mexican currency must turn green if they are to realize the full benefits of free trade?

To answer this question, it will be necessary to examine the micro-economics of international money, a subject that has been largely avoided by formal theory until recently. Events now make the subject important, while new techniques may make it more tractable than before. So this may be another case of that happy marriage of new technique and real-world relevance that has led to the best international monetary economics in the past.

I do not expect to see a grand unified theory of international money emerge in my lifetime. What I do expect is that events will continue to provoke research, that this research will continue to yield important insights, and that in its messy but exciting way the field will continue to progress.

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