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There Was No Analytical Alternative to the Theory of Optimum Currency Areas

Peter B. Kenen[1]

Abstract

Lars Jonung and Eoin Drea have produced a remarkably thorough and fair account of U.S. economists' views on EMU during the years before it came into being. I cannot quarrel with their account of my own views, or those of others with whose work I am familiar, as they evolved during the period they cover. There was, I agree, excessive reliance by U.S. economists on the theory of optimum currency areas, which posited a binary choice between fixed and floating exchange rates, and these were not the options that bounded the evolution of European thinking.

The widening of the European Monetary System's bands resulting from the exchange-rate crisis of 1992 served as a stark warning that "fixed but adjustable" exchange rates were inherently fragile, a lesson that most economists had already learned from the dollar crisis of 1971, and if a reversion to floating was seen to be a blow to European integration, there was nevertheless no clear alternative to tighter monetary integration. This view was not always articulated clearly, but it was surely a powerful influence on those who believed that monetary union was

1. Walker Professor of Economics and International Finance, Emeritus, Princeton University, Princeton, NJ 08544.

the appropriate goal, even though there was much uncertainty in Europe itself about the speed with which that objective could be reached.

To say, as many Americans did, that EMU was a political project without adequate economic justification, was to ignore the European consensus in favor of closer economic integration. Whether the time was ripe for monetary union was nevertheless debatable.

All of which leads me to confess that reliance on the theory of optimum currency areas, to which I was myself a contributor and user, narrowed many American economists' perspective on the choices facing Europe. I must note, however, that there was then no other analytical framework available for assessing the benefits and costs of monetary union, and that there was much skepticism in Europe itself about the likelihood of monetary union. When the Delors Report was about to be published, I phoned someone close to the work of the Delors Committee. Acting in my role as Director of Princeton's International Finance Section, which published a well known series of papers on international monetary economics, I asked him whether I should organize a symposium on the Delors Report and whether he would write an introductory chapter. Without revealing anything about the content of the Report, he told me not to bother, because the forthcoming report would have no long-lasting effect. Americans were not the only EMU-skeptics!

Later, I spent a year as Houblon-Norman Fellow at the Bank of England, where I was invited to sit on the Bank's staff committee monitoring the Maastricht negotiations. That is how I came to write *EMU After Maastricht* (1992), which was based in part on the memoranda I had written for that committee. When I submitted the manuscript to a senior officer of the Bank to make sure that I had not committed any indiscretions, his only suggestion was that I refer to the British stance as an "opt-in" rather than an "opt-out"—a suggestion that I declined to adopt.

But I wander, so let me return to Jonung and Drea. I have two small quarrels. First, I question the relevance of their assertion that "European monetary unification since the Delors Report has been much faster than its U.S. counterpart was in the late 1700s." True enough, but comparing the degree of internal integration in the era of the horse and buggy with integration in the era of high-speed trains is hardly fair. (It also ignores the fact that U.S. states in the late 1700s were more closely integrated with Europe, especially Britain, than among themselves.) Second, I question their repeated criticism of American economists' reliance on the theory of optimum currency areas. It is, I agree, ahistorical and apolitical, but what other analytic framework did economists have for basing empirical judgments about the economic benefits and costs of monetary integration? Rather than criticizing us for relying too heavily on that framework, they should perhaps have criticized us for failing to produce a better multidimensional approach to the cost-benefit analysis of EMU, which was, after all, the matter before us. It was not for economists to assess the political benefits and costs of monetary union. The true failing of many American economists was rather their insistence on comparing EMU with floating exchange rates within the European Union rather than comparing it with crisis-prone pegged but adjustable exchange rates.

References

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About the Author

Peter B. Kenen is Walker Professor of Economics and International Finance, Emeritus, at Princeton University. His many writings include a key article on the theory of optimum currency areas (1969); widely used textbooks in international economics; and, recently, *Regional Monetary Integration* (2007, with Ellen Meade). He has been a consultant to the Federal Reserve Bank of New York, U.S. Treasury, other U.S. government agencies, International Monetary Fund, and United Nations. He has been a member of various groups that have promoted cross-fertilization between academic and policy ideas, including the Bellagio Group, the Council on Foreign Relations, and the Group of Thirty.

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