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Volume Title: Regional and Global Capital Flows: Macroeconomic Causes and Consequences, NBER-EASE Volume 10

Volume Author/Editor: Takatoshi Ito and Anne O. Krueger, editors

Volume Publisher: University of Chicago Press

Volume ISBN: 0-226-38676-7

Volume URL: http://www.nber.org/books/ito_01-1

Conference Date: June 10-12, 1999

Publication Date: January 2001

Chapter Title: Comment on "How Were Capital Inflows Stimulated under the Dollar Peg System?"

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Chapter URL: <http://www.nber.org/chapters/c13066>

Chapter pages in book: (p. 192 - 195)

nancial turmoil period should also show that there were large capital outflows. Perhaps regressions using updated data will show stronger results than those reported in the paper.

Simulations based on estimated parameters in the regressions constitute the second set of results. The main one is that capital inflows in Korea and Thailand would have decreased if a basket peg had been adopted. It would be advisable to have a more detailed theoretical discussion on why these results are obtained. There are two issues that may be of concern here.

First, the estimated parameters used in the simulations may not be robust, in view of the low *t*-values for some parameters. Second, moving to the basket peg means that U.S. investors have to face more exchange rate volatility, whereas Japanese investors are less affected by exchange rate fluctuations. These cause opposite effects on capital inflows. The net outcome depends on a lot of institutional and historical factors not discussed in the paper.

Finally, the paper has gone some distance in assessing the impact of moving to a basket peg. However, further improvements in the estimation method and a longer data series are still desirable.

Comments Pranee Tinakorn

As a native of Thailand, I started reading this paper with great interest. This is because among the many factors alleged to have caused the crisis in Thailand, capital account liberalization under a fixed exchange rate has been seen as one of the main causes. Although, in my view, the crisis in Thailand was a result of both real sector and financial sector problems, the capital inflow and its reversal have been in the limelight.

First, I would like to summarize my reading of the paper and then offer my comments. In this paper, the authors tried to examine how the exchange rate, which was tied to the U.S. dollar, affected capital inflows to the three crisis-hit Asian countries: Thailand, Korea, and Indonesia (all of which had sought IMF financial support).

I agree with the authors' point that although these countries may be said to adopt the managed float system, as in Indonesia and Korea, or the basket peg system, as in Thailand, they all, as a matter of fact, were pegging to the U.S. dollar.

As can be seen from the movement of local currency to the dollar in figures 5.1–5.2 of the paper, this is more true in the case of Thailand than in the other two countries included in the study. The nominal baht value

during the 1990s hardly moved away from its par value with the U.S. dollar until July 1997, when the Bank of Thailand could no longer defend the value of the baht. A majority of Thai economists tended to believe that the weight of the dollar in the Thai basket was over 90 percent prior to the crisis.

If these crisis-hit countries were in fact pegging their currencies to the dollar, what was the consequence on their capital inflows? And if they had in fact used the basket system, would the pattern of capital inflows have been different? These were the two main questions posed by the authors, and they tried to answer these questions by postulating that capital inflows are influenced by the following factors: domestic interest rate, exchange rate-adjusted foreign interest rates, and foreign exchange risks. A positive exchange rate-adjusted interest rate differential is expected to have a positive impact on the country's capital inflows, and the foreign exchange risk is expected to have a negative impact on capital inflows.

Although I admire Ogawa and Sun's effort in trying to understand these important issues, I have some concerns about their estimation results based on two major grounds. The first reason has to do with how one interprets the diagnostic statistics from the estimation, and the second with how one should treat different types of capital inflows. As we know, capital inflows can be either long-term or short-term depending on the nature of their movement.

Let me first address the estimation results. In the section reporting the results of the regression analysis, one finds many instances of statistically insignificant effects of the independent variables on capital flows. One can clearly see this in the estimation results reported in table 5.2 for all the countries under study. It is also a little disturbing to have most of the coefficients on interest variables (especially di and yi) not significant. Given the statistical reliability of the estimates, I am afraid I should not proceed to discuss the simulation results, which are based on these estimated coefficients.

Let me turn now to the second issue: whether we should treat all kinds of capital inflows as being determined by the same set of variables. It is true that in macroeconomic analysis, we use the interest parity reasoning to explain capital flows. When it comes to the behavioral estimation, however, I think there are some types of capital flows that respond not only to the interest rate differentials and foreign exchange risks but also to other economic factors, depending on the nature of the flow.

Therefore, lumping all types of flows together or separating them into only two categories as Ogawa and Sun did may overlook some other important determinants of capital flows, resulting in possible specification error.

For example, when I look into the balance of payments of Thailand, I can distinguish between five different types of capital flows:

1. Foreign direct investment (FDI)
2. Portfolio investment
3. Private loans
4. Nonresident baht account (currency and deposits)
5. Other loans, such as trade credits and borrowing in the banking sector.

Among these, FDI can be regarded as long-term. Although we may generally consider portfolio investment as short-term like the other categories, the determinants of portfolio investment could also be different from those of private loans and other short-term flows.

Whereas long-term inflows are based on economic fundamentals and are reversed only when fundamentals change, short-term inflows, even though they are also influenced by economic fundamentals, tend to be speculative and easily reversible. Therefore it is more difficult to estimate a behavioral equation for them unless we take other factors into account.

For example, in the case of FDI, interest rate differentials may not be as important a determinant as much as the other pull factors in the host country, such as real GDP growth rate (reflecting return to investment), real wages (reflecting cost), and real exchange rate (reflecting competitiveness). Some push factor from the investing country may also be important.

In contrast to FDI, the flow of nonresident baht account is obviously short-term. This is foreign-owned money deposited in local commercial banks to do many activities, such as to gain from interest differentials, speculate in the foreign exchange market, and wait for other trade and investment opportunities.

In the 1980s, the net capital flow of nonresident baht account was about 7 percent of the total private nonbank flows. In 1991, Thailand's foreign exchange control was greatly relaxed, and in 1993 the capital account was liberalized and enhanced by the establishment of the Bangkok International Banking Facility (BIBF). As a consequence, the share of nonresident baht account increased to 40 percent of the total net flows in 1993 and swung down to 22 percent in 1996. However, if we look at the total inflow, and not net flow, the nonresident baht account share in the total private nonbank inflow was over 90 percent. The same is true for the total private nonbank outflow.

I think we should model the behavior of the nonresident baht account quite differently from that of FDI. I raise these two items as examples to suggest that if we don't differentiate for their behavioral differences, it may be difficult to obtain reliable estimates for the capital flows. I understand that one cannot be so detailed in working with international data across countries because the International Financial Statistics produced by the IMF do not show these details, but I think they should release the data

upon request because they obtain these detailed data from their member countries.

I would also like the study to separate out the estimates of interest differentials and exchange risks on each type of capital flows by controlling for other important factors in each category so that the estimates will not suffer from the bias arising from exclusion error.

Finally, I thank both authors for initiating our interest in this area, and I hope that they continue to expand on this study.