

Comments

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Jeffrey Frankel's chapter is a useful summary and extension of results in the literature on international capital mobility and crowding-out. He looks at the question of whether international capital mobility prevents fiscal policy from having an impact on domestic investment (that is, whether no crowding-out occurs). This question deserves the attention that Frankel devotes to it because it bears on policy issues that are currently very much in the public eye; for example: Do large government deficits crowd out domestic investment so that domestic capital formation is retarded? Can large U.S. budget deficits be the source of the current high real interest rates throughout the world? Can supply-side policies to stimulate domestic saving increase domestic investment and raise the capital stock? How effective are fiscal and monetary policies in influencing the business cycle?

Frankel's analysis leads him to the following conclusions:

1. International capital mobility does not fully prevent fiscal policy from having an impact on domestic investment, so that there is a distinct possibility that large U.S. government budget deficits do crowd out domestic investment.
2. Imperfect goods market integration is the reason why fiscal policy can have an impact on domestic investment.

While in general I agree with Frankel's conclusions, I do not always agree with the methodology used in his empirical analysis. It is important to distinguish between the validity of Frankel's conclusions and his empirical analysis because a literal reading of his empirical results might lead the reader to a conclusion that I believe is unwarranted. Specifically, a reader might come away from reading Frankel's evidence on saving–investment correlations thinking that there is a causal relationship from domestic saving to domestic investment that is not weakened by the existence of international capital mobility. I found the empirical analysis supporting this view to be

unconvincing. Frankel also has some doubts about this evidence because he states, “a better econometric approach than saving–investment correlations is to test real interest rate parity directly.” Thus, he and I have less disagreement than my comment suggests because I also find tests of international parity conditions to be the more relevant evidence supporting the two conclusions he reaches.

The Feldstein-Horioka Analysis

Frankel first approaches the effects of international capital mobility by focusing on the Feldstein-Horioka (1980) analysis in which the share of domestic investment in GNP is regressed on the share of national saving. Frankel seems to indicate that the most serious criticism of the Feldstein-Horioka regression analysis is the potential endogeneity of national saving. If this were the most serious criticism, then choosing a “good” set of instruments (ones that are exogenous) and estimating the regression with instrumental variables would solve the problem. I have no objections to the instruments Frankel chooses: Military expenditure and the age composition variables are about as exogenous as economists are ever going to find. However, I think that he has not focused on the most important criticism of the Feldstein-Horioka analysis discussed in the literature.¹

The most severe problem with the Feldstein-Horioka regressions is not that the national saving variable is endogenous, but is rather that a regression of domestic investment on national saving is not a well-specified model. In my reading of the investment literature I have never seen a structural model that suggests that investment is a function of saving. Finding good exogenous instruments and using them to estimate the relationship between saving and investment with instrumental variables does not solve the basic problem. The regression results on a misspecified model are still uninterpretable, no matter how good the instruments are.

I thus find the results in Frankel’s investment–saving regressions to be totally unconvincing. In fact, if they are taken literally, they imply that crowding out in an international context is practically complete and there are almost no effects of international capital mobility on domestic investment. I suspect that Frankel would also not be willing to accept this conclusion, and it is not a position that he advocates in his chapter.

It should be pointed out that my criticism of investment–saving regressions does not imply that government policies that affect domestic saving can have no impact on domestic investment. Such a link may exist, but it must be demonstrated by a more complicated structural model that describes what factors affect saving, which in turn also affects investment.

International Parity Conditions

The second approach for examining international capital mobility is to investigate international parity conditions. Frankel's empirical analysis of the covered interest parity condition is well thought out and it suggests that international capital mobility is very high. I found his discussion of Japan to be especially instructive, because it indicates that despite claims by American businessmen and politicians in 1983 that the Japanese were employing some form of capital market restrictions, the Japanese capital markets had become as open as those in other OECD countries such as Germany and Switzerland.²

Frankel's analysis of uncovered interest parity also leads him to conclude that international capital mobility is high. Although I too accept this conclusion, I find the route that he uses to arrive at this conclusion to be unconvincing. Frankel develops a model that indicates that risk premiums in the Eurocurrency market are small. He then takes these small risk premiums as evidence supporting high international capital mobility. My doubts about his reasoning center on two issues: (1) his view that the size of the risk premium provides important information for deciding on the degree of capital mobility and (2) his view that risk premiums are small.

First, a small risk premium is not at all necessary for international capital to be perfect. Large risk premiums in the foreign exchange market are consistent with perfect capital mobility, just as large risk premiums in the bond and stock markets in the United States are consistent with perfect capital mobility within the United States. The presence of large versus small risk premiums in the foreign exchange market thus has little bearing on whether one believes that international capital mobility is high.

Second, the evidence for small risk premiums is by no means clear-cut. An important assumption in Frankel's analysis of the risk premium is that the variance-covariance matrix of return differentials is constant over time—that is, return differentials are covariance-stationary. Models using this assumption have almost uniformly been rejected.³ Tests of this assumption in a recent paper by Giovannini and Jorion (1985) indicate that it is strongly rejected by the data. Giovannini and Jorion also show that if they allow for variation over time of the variance-covariance matrix of return differentials, they can derive large risk premiums using Frankel's framework. Indeed, in one illustrative example, they find that the risk premium is estimated to be forty times larger when allowance is made for a time-varying variance-covariance matrix.

Giovannini and Jorion's finding that risk premiums can be large is reassuring because Hodrick and Srivastava (1984) and others have shown that risk premiums must be large if expectations in foreign exchange markets are even close to being rational. Since many economists are unwilling to entirely

abandon the rational expectations (efficient markets) assumption because they do not see a viable alternative, they are far more comfortable with the view that risk premiums are large rather than small.

The evidence discussed in Frankel's chapter that both he and I find to be the most relevant to the degree of international capital mobility involves the tests of real rate equality. Real interest rate equality across countries is strongly rejected and this leaves open the possibility that domestic fiscal policy can indeed affect domestic investment. Frankel points out that the primary source of the rejection of real rate equality is the failure of ex ante relative purchasing power parity. This parity condition will fail to hold if international goods markets are not well integrated (or equivalently, goods in different countries are far from being perfect substitutes). Since it is plausible that international goods market integration is far weaker than international financial market integration, it is not surprising that ex ante purchasing power parity is rejected by the data. Frankel conducts one test of ex ante purchasing power parity with 116 years of U.S.–U.K. data and rejects this parity condition. His results are consistent with those of Cumby and Obstfeld (1984), who also find rejections of ex ante purchasing power parity when they use powerful statistical techniques on postwar data for several countries.

The evidence on real rate equality and ex ante purchasing power parity thus leads me to Frankel's conclusion that crowding-out can occur because goods markets are not well integrated.⁴ However, his chapter does not sufficiently stress several results that are germane to the issue of whether complete crowding-out occurs. First, although Friedman and Schwartz (1982), von Furstenburg (1983), Cumby and Obstfeld (1984), Mishkin (1984), and Cumby and Mishkin (1986) find that real interest rates are not equalized across countries, Cumby and Mishkin (1986) report that there is a strong and statistically significant tendency for real rates to move together in different countries, though the movement is not always one-for-one. This result suggests that international capital mobility *does* affect real interest rate differentials between countries and that complete crowding-out is unlikely. Second, approximately half of the United States's current huge budget deficit is being financed by capital inflows from abroad. This fact also suggests that complete crowding-out does not occur.

Conclusions

My reading of the evidence is as follows: Although zero crowding-out (as a result of perfect international capital mobility) receives little support so that fiscal policy can affect domestic investment, complete crowding-out is also not supported. Since this book is devoted to policy questions, I want to con-

clude by addressing the question: What does the evidence on international capital mobility and crowding-out imply for government policy.

The conclusion that complete crowding-out is not supported by the data suggests that international capital mobility ameliorates some of the harmful effects of the large budget deficits on the U.S. economy, specifically on real interest rates and on capital formation. The inflows of capital from abroad in the present U.S. situation have surely kept real interest rates lower than they otherwise would have been, which, in turn, has kept domestic investment and capital formation higher. However, the rejection of perfect capital mobility in which fiscal policy is offset by international capital flows suggests that current fiscal policy may be having undesirable effects on the U.S. economy.

It is also important to remember that the United States is a large country. Thus even in the presence of perfect international capital mobility, U.S. fiscal policy can affect the world economy and hence also the U.S. economy. Undesirable effects of U.S. fiscal policy will spill over to the rest of the world. To the extent that international capital mobility helps the United States to suffer less from its fiscal policy excesses, the rest of the world suffers more. Thus, even if the United States were to accept perfect international capital mobility and zero crowding-out, economists should not be complacent about possible harmful effects of U.S. fiscal policy.

Does the evidence on international capital mobility and crowding-out suggest that supply-side policies to stimulate private saving can help increase domestic capital formation? The answer, I believe, is no. First, high international capital mobility will certainly weaken the effects of policy-induced increases in domestic saving on domestic investment. Second, and more important, there is little evidence that saving responds substantially to supply-side incentives. This is particularly evident in recent years when incentives to promote saving have not resulted in a big expansion of private savings. In fact, the private savings rate in the United States has recently been hitting all-time lows. From this perspective, the supply-side revolution has not been a resounding success.

Notes

1. For example, see Tobin (1983) and Obstfeld (1985).
2. This conclusion was also reached in a careful study by Ito (1983).
3. In the exchange rate literature, examples are Hansen and Hodrick (1983) and Hodrick and Srivistava (1984).
4. Although rejection of real rate equality and ex ante relative PPP leaves open the possibility that crowding-out can occur, this rejection does not imply that crowding-out *must* occur. There could be zero crowding-out if deviations from ex ante relative PPP which lead to real rate inequality are never caused by fiscal policy but are rather attributable to some other factor.

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