

COMMENTARY

Macprudential Policies in a Global Perspective

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I enjoyed reading this paper. The models are simple and focus on important issues. The approach assumes the existence of externalities and discusses Pareto-improving policies along Pigouvian lines. A basic assumption is that, beyond a certain point, credit expansion carries with it the virus of financial crisis which, however, is not internalized by individual agents. Then, it proceeds to discuss Pareto-improving borrowing taxes and other forms of government intervention.

The first part of the paper shows that if the dominant distortion resides at home, then *domestic* macroprudential policy, and not controls on capital inflows, are in order. This is a straightforward result in terms of the model, but it is a real eye-opener for those in the policy arena, where there are numerous instances in which policymakers implement controls on capital inflows without paying much attention to domestic distortions. The model is extended to account for several countries facing domestic financial distortion. In this setup, one country's macroprudential policy can have a negative effect on the others. Hence, there may be room for international policy coordination. However, if countries are small, the "invisible hand" is still capable of working its magic: the paper shows that if each country pursues macroprudential policies that are individually optimal, the equilibrium outcome is Pareto optimal! This result is far from obvious, and provides a benchmark case that should make policymakers think twice before engaging in policy coordination initiatives prompted by international financial externalities. To be true, some countries may feel short-changed, but the fact that the uncoordinated equilibrium is Pareto optimal is likely to make global alternative agreements hard to achieve. For instance, achieving alternative agreements may call for a nontrivial subset of underdogs to get together and threaten to take autarkic actions. However, this is unlikely to pay off since these initiatives are likely to be riddled with hard implementation problems.

The second part of the paper shifts attention to macro waters infested with wage stickiness and binding zero interest rate bounds. It abandons the world of atomistic economies in order to center on a world dominated by two large

blocks, called China and U.S. In this scenario, which can be employed to portray currency wars, the paper shows that there is room for policy coordination in which, for example, China commits to slow down the accumulation of international reserves and use the proceeds to increase aggregate demand, while U.S. jacks up its policy interest rate. This is a model-specific result but it helps to show that policies that are on the table for these two blocks could be Pareto-improving relative to the outright currency war that seems to be playing out these days. There is no doubt that much work is needed before these insights become operative, but the paper succeeds in showing that there is no blind alley ahead. In my opinion, this is macroeconomic *theory* at its best.

The paper addresses a slew of interesting and policy-relevant issues, as illustrated by these examples. It is an ideal tool to introduce economists to a discussion that so far has mostly been left in the hands of policy experts and policymakers, without solid analytical underpinnings. Moreover, it has the great advantage of showing that some issues can be addressed in Econ 101. However, there is still a large distance between these models and the scenarios faced by policymakers who are keen on cushioning the economy from the effects of capital flow volatility. Even if one agrees with the basic assumptions in these models, policy implementation becomes a very challenging issue. The Pigouvian methodology, for example, necessitates knowledge of how credit booms, say, eventually translate into credit busts. Without specific knowledge of these issues, the policymaker is left with only the vague notion that she should be doing something to pare down the boom. Unfortunately, knowledge about the specifics of credit booms and busts is extremely limited, and the economics profession displays a wide variety of opinions. The spectrum of opinions ranges from those who believe that these boom/bust episodes boil down to financial frictions that are subject to rigorous statistical estimation, to those who claim that these episodes reflect the existence of multiple equilibria and, therefore, there are sharp discontinuities or strong nonlinearities that are very hard to assess. Part of the problem here is that financial distortions are much harder to assess than the ones emphasized in the global warming controversy, for example. To be sure, global warming is still strongly debated but, at least, one hopes that basic science—which is aeons ahead of macro-finance—will help to identify and quantify some key relationships. This is bad enough news but, in addition, one has to reckon with the fact that when taxation is involved, the whole government gets drawn in, and the issue becomes highly politicized. These problems are even thornier in the context of international coordination.

Moreover, there are a slew of pedestrian implementation problems that must be taken into account. A prominent case in point is tax avoidance. For instance,

many countries have imposed taxes on short-maturity inflows to keep hot capital at bay. The effectiveness of those controls appears to be limited: While taxing short-term capital inflows lengthens the maturity of those flows, the effect on the current accounts may be negligible (Magud and Reinhart 2007). This may result from the fact, pointed out in Garber (1998), that investors can easily resort to off-balance-sheet tricks (such as interest rate swaps) to bypass capital-inflow taxes, changing reported debt maturity without significantly affecting total capital inflows.

The models employed in the present paper do not distinguish between *gross* and *net* credit flows, and center entirely on net flows. But, in practice, it is hard to tell gross from net borrowing. For instance, a firm may extend credit to its customers and simultaneously borrow from its suppliers. A literal reading of the paper implies that the government should tax borrowing and subsidize lending. However, this is not how controls on capital inflows work in practice. Until recently, for instance, Brazil imposed stiff taxes on gross capital inflows without countervailing subsidies on outflows. Is this a major distortion? I think the issue is worth pursuing because, as far as I can tell, it has also been ignored in the outstanding literature. I suspect, though, that if countervailing subsidies on outflows are allowed, it will make it even easier for the financial sector to find new ways to avoid paying taxes on net borrowing.

Finally, the paper ignores *quantitative* controls, possibly combined with taxation. Quantitative controls are especially relevant when policymakers know little about the market's reaction to a financial tax but have a much better idea about excessive credit growth. An example, with some practical relevance, is bank credit. It may be easier to assess if bank credit is growing too fast than figuring out the tax rate that would stop banks from exceeding the speed limit.

In summary, this paper helps to address central questions related to controls on capital flows and offers a clear-cut framework that helps to see the big picture. Future extensions addressing some of the issues raised here will certainly increase the policy relevance of this research project.

REFERENCES

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