Comment

Thierry Tressel: This paper explores the impact of remittance inflows on the real exchange rate of recipient countries. While there exists a vast literature on the determinants of long-run real exchange rates, few papers have explored the role of remittances. The paper first documents the importance of remittances around the world. It shows that in many countries, remittances dwarf official development assistance and even private capital flows. Hence, because these flows have become large in percent of GDP, they could cause problems of macroeconomic management by appreciating the real exchange rate. For example, an appreciating real exchange rate may adversely affect tradable goods sectors and lower overall productivity growth, thus weakening the growth potential of these countries.

To address this potential concern of policymakers, the authors first present a workhorse model of a small economy with a fixed nominal exchange rate and flexible domestic prices and labor markets. The economy produces a nontradable good and a tradable good with a decreasing return to scale production technology requiring labor only as an input. There is perfect access to international capital markets, and the risk premium on foreign liabilities is rising with the existing stock of net foreign liabilities. They establish the standard result: a permanent transfer appreciates the long-run real exchange rate by permanently increasing the aggregate consumption that is consistent with external balance. Higher steady state consumption is associated with higher demand for nontradable goods and therefore a more appreciated real exchange rate and a larger nontradable goods sector.

They present several extensions of the model to illustrate how the standard result could be weakened or even overturned. First, remittances may not be entirely exogenous and may respond somewhat to the conditions of the domestic economy. For instance, remittances may be negatively correlated to domestic incomes expressed in foreign currency. An appreciating real exchange rate, by raising the foreign currency value of domestic income, would tend to

decrease remittances. Such a feedback effect of the real exchange rate to remittances would weaken the overall relationship between the exogenous components of remittances and the real exchange rate. Second, the country risk premium may depend on the net foreign asset position of the country *inclusive* of remittances. If this were the case, a permanent increase in remittances would cause a decline in other components of the net foreign asset position to maintain a country risk premium equal to the rate of time preference of domestic agents. This reduction in other components of the net foreign asset position would materialize through a temporary overshooting of consumption and the real exchange rate, followed by a return of consumption and the real exchange rate back to their initial levels. Hence there would be *no permanent effect* of the increase in remittances on the real exchange rate. A third possibility is that remittances would be spent primarily on tradable goods, the price of which is set internationally; hence there would be no effect on the real exchange rate as the increase in remittances would not affect the demand for nontradable goods. Finally, a monetary economy is considered in which there are transaction costs (in tradable goods). In this case, an inflow of remittances, by increasing consumption, would also reduce tradable goods supply. To maintain external balance, the positive link between the real exchange rate and remittances would be weaker.

In the empirical section of the paper, the authors present a very thorough analysis of the long-term cointegration relationship between the consumer price index–based real effective exchange rate and a set of fundamentals including the ratio of workers' remittances to GDP. They consider various country samples based on income, openness to trade and capital flows, or regional groupings. They find evidence of a positive link between the real exchange rate and remittances only in some subsamples, in particular lower-middle-income countries and countries that are relatively closed to trade and capital flows. The economic significance is very small, however. In other samples, they do not find any evidence of a systemic relationship between remittances and the real exchange rate. These findings suggest that remittances should not be a concern to policymakers.

This well-executed paper raises a number of interesting issues. But first let me make a few remarks on the model. There exists another possible theoretical justification for a possible weak link between remittances and the real exchange rate. Consider for instance an economy with two inputs in production instead of one: capital and labor, and assume that remittances are used as capital inputs. Depending on the capital intensity of the nontradable sector relative to the tradable sector, one may obtain an appreciation or a depreciation of the real

exchange rate. Indeed, more abundant capital would tend to depress the price of the good that is relatively more intensive in capital; if this happens to be in the nontradable sector, the real exchange rate would tend to depreciate. Doing some robustness analysis on the modeling of the monetary economy might also be interesting. What would be the consequences of using more standard approaches, such as a "money in the utility function" approach or a cash in advance approach?

The empirical part of the paper suggests that remittances have no economically significant effect on the real exchange rate. Further work could help clarify why the effect on the real exchange rate is small. For example, do we know how remittances are associated with domestic private consumption and investment? I also would be interested in knowing the extent to which remittances are spent on imports as opposed to nontradable goods and services, as suggested by the authors in the theory section of the paper. If remittances are indeed spent on imports, this may provide a very simple explanation for the finding of the paper. If this is not the case, alternative explanations may need to be explored further, including potentially the role of trade openness or openness to private capital inflows.

The empirical finding also contrasts very clearly with the finding that aid inflows seem to have a large *negative* effect on the real exchange rate. This clear discrepancy would be worthwhile exploring, maybe in future work. What aspects of aid flows versus remittances could explain these large differences in estimated impacts on the real exchange rate? Could it be related to the fact that aid flows are associated with policy conditionality, in contrast to remittances? Could it be because aid flows and remittances are not spent on similar bundles of goods? Or is it because they might have different effects on potential output growth, via diverse channels? This seems a promising avenue for future research.

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