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Discussion of "Capital mobility and international sharing of cyclical risk"

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ABSTRACT

This is a comment on "Capital mobility and international sharing of cyclical risk." The comment focuses on the authors' choice of portfolio adjustment costs in explaining limited international risk sharing. The comment raises two main issues. One is that investors hold large gross positions that vary significantly in value over the business cycle. The second is that it is difficult to identify portfolio adjustment costs from trade costs within the authors' environment.

This paper is motivated by the low level of risk sharing across countries observed at business cycle frequencies. Consumption correlations across economies at business cycle frequencies are low. This is true in recent periods despite an era of relatively large current account imbalances. The fact that output is not highly correlated suggests unexploited insurance opportunities.

The complete-markets benchmark implies perfect risk sharing.[1] While useful as a benchmark, perfect risk sharing will not hold in practice for a number of potential reasons. The authors consider two. The first is lack of state-contingent assets. In particular, suppose countries can only trade a noncontingent bond, subject to some upper bound on debt positions. The second is costs to adjusting portfolio positions. They model this in the bond economy as a quadratic adjustment cost on the stock of outstanding bonds. This latter friction is the most novel relative to the international business cycle literature.

The main quantitative finding is that portfolio adjustment costs improve the fit of the model, but nevertheless there remains a large gap between model and data. In fact, the adjustment cost model works particularly well in countries where the implied costs are so high as to essentially replicate autarky. In line with the previous literature, moving from state-contingent assets to a bond-only economy does not matter at business cycle frequencies.

My comments are designed to highlight what the authors leave in and what they leave out, providing context for the paper's main contribution. Before discussing the results in more detail, it is worth highlighting a fairly novel feature of the quantitative analysis. In particular, for each country, the authors re-compute a world general equilibrium in which that country trades with the remaining countries as a group. This allows the authors to draw general equilibrium conclusions without having to simultaneously solve for an N country portfolio problem. This keeps the model tractable without sacrificing the general equilibrium elements of the analysis. The improves on the usual "small open economy" assumption common in the literature.

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[1] All models under consideration allow for the presence of nontraded goods. The complete-markets model therefore will not generate a correlation of aggregate consumption of one, due to the differences in consumption baskets and idiosyncratic shocks to nontraded output.

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The authors begin with the natural starting point in considering deviations from complete risk sharing; namely, incomplete markets. They find that limiting assets to a single noncontingent bond does not help answer the question of why risk sharing is limited. This is so because of a well known mechanism—mean reverting shocks and patience lets countries "self insure," approximating a complete markets allocation. There are two issues with this result. One is that the model (and all versions considered in the paper) assumes full commitment. Self-insurance may require large debt positions which may not be compatible with realistic limited enforcement. The authors impose an upper bond on debt, but it is very loose. In fact, they note that the limit plays no role unless it is sufficiently tight to bring the economy close to autarky. However, an ad hoc, noncontingent limit is not the same as an endogenous borrowing limit, which may allow large debt positions in some states, but restricts borrowing severely in others. In particular, shocks to a countries cost of default, or "outside option," may be an important, uninsurable risk, similar to shocks to nontraded output already embedded in the authors' model.

Moreover, it is not entirely clear that noncontingent debt is realistic. It is true that debt (bonds) dominate international capital flows. However, default renegotiation and haircuts, and maturity structure provide a fairly rich amount of state contingency to the period-by-period payoffs. Capital gains and losses on both debt and equity are often large, leading to the "valuation effects" emphasized by Gourinchas and Rey (2007) and Gourinchas et al., (forthcoming). For example, the latter paper documents that in the 2007–2008 financial crisis, the US net foreign asset position declined by USD 863 billion, while the United Kingdom gained USD 542 billion. These are large swings in wealth (both in absolute terms and relative to GDP) achieved through asset markets.

This raises the issue of why the large shifts in wealth are not matched in net imports or consumption. The authors advocate portfolio adjustment costs. If it is costly to trade these large positions, then they will not be reflected in flows at business cycle frequencies. They propose a convex portfolio adjustment cost of the form

$$\frac{b'}{R} = y - c + b - \phi(b' - b)^2, \tag{1}$$

where b and b' are current and next period's net asset positions, respectively, y and c are tradable output and consumption, respectively, and ϕ is a parameter governing the severity of the adjustment cost to bond positions. Embedding this cost into a calibrated model does lower consumption correlations, as one would imagine. This partial success, however, still leaves the model's predictions far from the data. For example, for Canada, a canonical small open economy, the sum of squared errors between model and data for consumption is 0.107 with complete markets, 0.101 with a bond only, and 0.095 with portfolio adjustment costs. The portfolio adjustment costs do better for some other economies. For example, Mexico's sum of squared errors is 0.344 for complete markets, 0.333 for bond-only, and 0.142 with adjustment costs. However, in this case the adjustment cost is set to the maximal limit allowed in the calibration, which the authors note is close to autarky. More generally, as the authors point out in the paper, the portfolio adjustment costs improve the model substantially only when they are set to this maximal limit, which is the case for a majority of the countries considered.

The analysis therefore suggests that for many countries, it is prohibitively expensive to adjust portfolios at business cycle frequencies. This is reminiscent of the argument in Fitgerald (forthcoming). Fitzgerald argued that trade costs for real goods and services are large, and in many instances outweigh limits to asset spanning. In particular, using a multi-country gravity model, she decomposes the failure of risk sharing into the component due to trade costs and the component due to asset market frictions. Identification is obtained using the bilateral gross trade flows and geographic distance to pin down country-pair trade costs. Fitzgerald's analysis found that the failure to insure risk is due in large part to the costs of shipping goods.

While Fitzgerald considered the traditional iceberg transportation costs, the Mendoza et al. paper's portfolio adjustment costs are almost isomorphic to a convex cost to net trade. In their model, trade inclusive of transactions costs is given by $y-c$. If trade costs were convex, we would have a balance of payments equation of the form

$$\frac{b'}{R} = y - c + b - \tau(y - c)^2. \tag{2}$$

Note that the difference between this equation and the authors' is the gap between the trade balance $y-c$ and the current account $b'-b$, which is net factor income in terms of balance of payments accounting. As net factor income for most economies is relatively small, the difference between the two approaches may be negligible, particularly when raised to the power 2.

This raises the question of whether the lack of risk sharing the authors attribute to portfolio adjustment costs reflects real trade costs, a la Fitgerald (forthcoming). One possible route for future research is to perform an analysis similar to that of Fitzgerald's using gross capital flows. Fitzgerald used the volume of trade between many pairs of countries, plus geographic distance, to identify trade costs. Perhaps a similar analysis can be done using the volume of capital flows. For example, capital flows do obey a gravity equation, just like trade (e.g., Portes and Rey, 2005). Similarly, trade costs have implications for deviations from the law of one price in goods markets (something observed in the data). The same does not seem to be the case for asset markets, but a careful analysis may turn up large implicit costs to trading assets. A horse race between the two types of costs may shed light on which is

important, or if both, their relative severity. The authors' paper is informative in its own right, but also provides an exciting direction for future research.

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