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Comment

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This paper undertakes an empirical analysis of the degree of financial integration across EU regions and its determinants. The starting point is an innovative measure of gross regional income for EU regions constructed by the authors using survey data. This measure is then used to run two types of panel regressions to examine the degree of financial integration and the variables that drive it:

1. Diversification finance regressions

$$\Delta_{96}^{03} \left(\frac{OUTPUT}{INCOME} \right)_i = \mu_c + \delta X_i^{90-93} + \alpha \Delta_{92}^{94} \ln(GDP)_i \\ + \gamma (X_i^{90-93} - \bar{X}) \Delta_{92}^{94} \ln(GDP)_i + e_i.$$

2. Development finance regressions

$$\left(\frac{OUTPUT}{INCOME} \right)_i = \mu_c + \delta X_i + \alpha_c \ln(GDP)_i + \gamma (X_i - \bar{X}) \ln(GDP)_i + e_i.$$

These regressions are based on a canonical theoretical framework of financial integration that yields the prediction that, under full financial integration, the coefficient α should be equal to the share of capital income in gross domestic product (GDP). The key findings of the paper are that: (a) EU regions are less integrated than predicted by theory, (b) there is little evidence that country-level institutions matter, but (c) regions where confidence is higher are more integrated in terms of the indicator derived from theory.

This is one more contribution adding to the very interesting research program of the authors looking at the empirical implications of financial integration. The construction of regional income data for the EU was done in a clever way, and the database they produced is likely to be used

in many other research applications. In my discussion I will focus on three main points: first, I will argue that the empirical classification of de facto versus de jure financial integration commonly used in the literature is misleading at best and should be discarded. In the case of this paper, the authors should contrast their results with those obtained by using a de jure measure. Second, I will discuss some important limitations of the theoretical framework on which the empirical analysis is based. In particular, I will argue that the inability to track cross-regional asset portfolios makes it difficult to argue that the estimated α coefficient is a measure of financial integration. Third, I will make some comments about the policy implications of the analysis.

Why is the de facto de jure classification misleading? The problem is that this classification is logically flawed because it confuses an action (financial integration) with the outcome of that action (the volume of financial asset trading). *Financial integration* is defined as the removal of distortions and barriers affecting asset trading across countries, or in this case across regions in the EU. It is an issue largely about actions of policy (which in the case of the EU relate to the removal of capital controls of all kinds that are very well documented to have taken place during the 1980s and early 1990s), and it is also about technological innovations that have enhanced the efficiency of financial asset transactions significantly. *Financial asset trading*, on the other hand, is defined as the magnitude of gross and net financial flows that results from a particular economic environment, including in this case the degree of financial integration.

The problems with the de facto de jure classification of financial integration can be illustrated with three examples that show why the de facto measure can be very wrong: first, it is possible to construct theoretical examples in which countries can have full financial integration but zero asset positions and zero credit flows (e.g., a multicountry model with fully integrated, complete markets of contingent claims but perfectly correlated country-specific incomes). Here, there is full financial integration de jure, but the de facto measure would indicate financial autarky! Second, it is also possible to construct a theoretical model in which capital controls or asset trading costs are present, but countries maintain large positive or negative net foreign asset (NFA) positions (see, for example, Durdu, Mendoza, and Terrones 2007). Here, the de facto measure could indicate a high degree of capital mobility, while the de jure measure would indicate the opposite. Third, consider the well-known case of the saving-investment correlations, which have been

proven to be an inadequate measure of the degree of capital mobility (see Obstfeld 1986, and Mendoza 1991). For example, it has been shown that depending on the persistence of the shocks that drive output fluctuations, the saving-investment correlation can be positive or negative in model economies assumed to have perfect capital mobility. This example shows another instance in which an outcome (now the saving-investment correlation rather than the gross or net capital flows) cannot be used as a indicator of financial integration.

In short, the authors' empirical analysis is about the determinants of net factor payment flows across EU regions, which is a *very* interesting topic on its own, but it is *not* about financial integration. To examine the latter, the authors would need to explore similar experiments as the ones conducted in the paper, but use the standard (de jure) measures of financial integration, such as that constructed by Chinn and Ito (2005).

The theoretical benchmark that anchors the empirical analysis is based on three key premises: (a) Ex-ante arbitrage of differentials in marginal products of capital allocated to each region, K_i , under perfect foresight, $R = \alpha A_i K_i^{\alpha-1} L_i^{1-\alpha} = R_i \forall_i$; (b) constant shares of ownership of global capital by each region, \hat{u}_i ; and (c) a conjecture about portfolio structures according to which, if ownership is fully diversified, capital in a region will be mainly owned by nonresidents. Under these assumptions, the GDP-GNI ratio in a region reduces to:

$$GDP_i/GNI_i \stackrel{EQ}{=} 1/[\alpha\phi_i(K/K_i) + 1 - \alpha].$$

Notice that both the ratio of capital allocated to the region relative to total global capital (K_i/K) and the region's ownership share of global capital ϕ_i matter. Given that physical capital is significantly more costly to adjust than financial capital, it is quite likely that over the seven-year period used in the empirical analysis ownership shares moved more than physical capital allocations. This issue points to the fact that, in order to use this theoretical framework to derive robust testable predictions, the analysis needs to include a theory of ownership shares (i.e., portfolio choice).

Unfortunately, the determination of well-defined cross-country or cross-region portfolio structures is a difficult task. Under perfect foresight and perfect financial integration, or under uncertainty but with complete markets of contingent claims, marginal returns are fully arbitrated, but precisely because of that portfolio structures are indeterminate. Agents are indifferent across portfolio structures because all assets yield the same returns. Moreover, ignoring uncertainty is likely to be

problematic because uncertainty is a key factor driving portfolio choice. But canonical portfolio models have well-known problems of their own in terms of accounting for observed portfolios. In particular, they find it hard to explain the substantial home bias in the portfolios of agents resident in EU countries that still remains. Baele, Pungulescu, and Ter Horst (2007) show that the country time series means of the percent difference relative to optimal international capital asset pricing model (CAPM) portfolios range from 55 percent in Belgium to 99 percent in Poland. This evidence, albeit not aligned by regions as in the paper, casts serious doubt on the paper's key assumption that a region's capital is largely owned by nonresidents. In addition, as the recent work in modeling international portfolios under uncertainty and incomplete markets in dynamic stochastic general equilibrium (DSGE) models shows (e.g., Devereux and Sutherland 2006; van Wincoop and Tille 2007), pinning down closed-form solutions for optimal portfolio structures that can be tested by standard empirical tools may not be feasible.

Another issue with the theoretical framework relates to the potentially important role that differences in regional capital valuations due to adjustment costs, depreciation rates, and taxes can play, even in a canonical perfect foresight setup. If we modify the authors' framework to consider the typical capital-adjustment costs behind Tobin's Q model of investment, and differences in country- and region-level tax rates on dividends $\tau_{c,i}^k$ and capital gains $\tau_{c,i}^g$, the arbitrage condition under full financial integration becomes:

$$R \stackrel{EQ}{=} \frac{(1 - \tau_{c,i}^k)[\alpha A_i K_i^{\alpha-1} L_i^{1-\alpha} d_{c,i}] + (1 - \tau_{c,i}^g)q_{t+1}}{q_t} \dots R_i \quad \forall_i$$

where $d_{c,i}$ is a depreciation rate that varies across countries and/or regions and q_t is Tobin's Q (which differs from unity due to marginal adjustment costs, which in turn depend on the position of the region's capital at date t relative to its long-run trend). In this case, it will no longer be true that estimating a coefficient α equal to the share of capital on GDP is evidence of full financial integration, since this prediction was derived using the simple arbitrage condition without taxes and adjustment costs.

These issues are likely to be relevant not just as theoretical points but also for the empirical analysis. Countries and regions in the EU are at different stages of the growth dynamics of their capital stocks, so their Tobin Qs are likely to vary widely (consider, for example, Spain versus

the U.K., Greece versus France, East Germany versus West Germany, Southern Italy versus Northern Italy, etc.). Moreover, tax rates on capital income differ significantly across countries in the EU (see Mendoza and Tesar 2005). Country dummies can pick up the effects of these differences only as long as tax differences are purely country-specific, but it is very likely that tax rates also vary by region.

Setting aside the limitations of the theory behind the empirical tests, some aspects of the quantitative results are controversial and can affect the policy implications of the analysis. One key issue is whether institutions at the country level can be as clearly separated from regional trust and confidence as the paper suggests. Confidence is easier to gain and maintain with strong institutions, and similarly, institutions are likely to be stronger and more stable when confidence is high. Moreover, financial contracts clearly depend on trust and confidence, but these are also dependent at least in part on institutional enforceability. A second concern is that the empirical findings are strong for confidence, and less so for trust, but the confidence measure is a very mixed bag that includes confidence in church, army, education, media, unions, police, legislative, bureaucracy, social security, corporations, judiciary, EU, NATO, and so forth. Looking at this list it is hard to see how the data can split confidence from institutions, and the list includes many more aspects of confidence and/or institutions than the key ones for financial flows (which would be mainly corporations and judiciary). The paper should explore the robustness of the results to redoing this part of the analysis, considering only these two components of the confidence data.

In summary, this paper undertakes a very interesting empirical analysis of the determinants of cross-region capital flows in the EU, using an innovative measure of regional income based on survey data. It concludes after a careful empirical investigation that the data support the hypothesis that confidence matters for cross-region capital flows. Taking this result at full value, setting aside all my previous comments, it seems that the big unanswered question that remains is: what can countries or regions do about confidence? In this regard, we seem to arrive at a conclusion that is widely agreed on: the development of institutions or confidence levels that anchor financial markets is an important precondition for a successful process of global financial integration (see Rajan and Zingales 2003, and Mishkin 2006). The hard part is to figure out what strategy countries or regions can follow to develop their institutions and enhance confidence.

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