

## Report on the manuscript

### “Political Risk Guarantees and Capital Flows: The Role of Bilateral Investment Treaties”

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This paper assesses the importance of bilateral investment treaties for international capital flows, including not only Foreign Direct Investment (FDI) but also portfolio equity and various debt instruments. It seems that the extant literature has mostly not examined the consequences of investment treaties protecting the investor’s property rights beyond their effects on FDI flows, and this paper intends to fill the gap. Furthermore, it distinguishes between private non-guaranteed debt and public or publicly guaranteed debt, making it possible to identify different sensitivities of those two types of debt instruments to the guarantees provided by the treaties against political or expropriation risks. Data from a panel of middle-income countries is used to estimate capital flow equations with GMM methodology, in order to account for both heterogeneity across countries and endogenous dynamics.

On the positive side, I think the distinction between different types of capital flows is interesting: FDI, portfolio equity, private non-guaranteed debt (PNG), public or publicly guaranteed debt (PPG). It is surely a contribution to think beyond FDI and to allow for the very likely possibility that international investment treaties matter differently for such different financial instruments. The author comes up with a smart proxy for political risk guarantees, namely, the proportion of OECD countries with which the country under consideration has a ratified bilateral investment treaty (as recorded in the UNCTADSTAT database). I think the underlying logic would be that all such treaties have comparable coverage and provide comparable guarantees against expropriation risk, and that the OECD essentially represents the rest of the (economically relevant) world. If this is an original idea it would be worthwhile to elaborate and emphasise it a little more; if it has been used elsewhere that should be mentioned. As for the expropriation risk of foreign investments, the author uses an indicator from the ICRG, the investment profile component of the political risk index. The international capital flow data are from the WDI, and several more variables are drawn from known sources.

Unfortunately, the interesting conceptual and data work is not followed by equally insightful econometric analysis. The author begins with unit-root tests of all the variables, so as to difference variables showing signs of nonstationarity. The discussion of the results for PNG are at variance with the results reported in Table 1, and for an obscure reason lead the author to take second differences of PNG debt flows. That means that all later analyses will juxtapose second differences (“accelerations”) in PNG with level measurements of PPG, FDI and portfolio equity flows. It is hard to make sense of such comparisons. The mechanical use of unit root tests leads further to second-differencing the financial openness index adopted from Chinn and Ito. As explained in the paper, the Chinn-Ito index is a codification of four restrictions, and can hardly be treated as a quantitative (interval-scaled) measurement generated by some stochastic process. Yet the usual testing battery is applied, leading incidentally to nonsensical contradictions between the different unit-root tests (LLC vs IPS and ADF-F). The author resolves the contradictions by taking second differences, which probably removes any information content from the original index.

After dealing with the issue of nonstationarity, the author pays attention to that of endogeneity. This arises in mainly two different ways: by the presence of dynamics (the lagged dependent variable) combined with heterogeneity (unobserved country effects), and by the possibility of reverse causality from other explanatory variables to the dependent variable. Both problems may cause correlation between regressors and the error terms. To deal with the first issue, GMM methodology (à la Arellano-Bond) is invoked. To guard against the reverse causality issue, Granger-causality tests are conducted based on a five-year lag window, and where this provides evidence of reverse Granger-causality the regressors are instrumented. I am not sure that Granger-causality tests will reveal contemporaneous correlation between regressors and error terms. Granger causality is all

about time precedence. Neither is it clear to me that delayed feedbacks would necessarily be problematic. Furthermore, in the Arellano-Bond GMM logic instruments are constructed from lagged values of the endogenous regressors. I would think that evidence of reverse Granger-causality in a five-year window casts a doubt on the appropriateness of such instruments for endogenous regressors based on their own lagged values.

On p. 16, the author argues briefly why system GMM will be preferred to the erstwhile difference GMM of Arellano and Bond. The argumentation is very incomplete, as are the moment conditions mentioned in eq. (7)-(8), and the reader is left unaware of some of the restrictive implications of system GMM (in spite of the reference to Blundell and Bond 1998). I think this issue would have deserved more attention in the context of the application.

In the GMM estimations, the explanatory variables are introduced “gradually”, meaning they are accumulated one by one. In spite of showing awareness of the implications, the author allows the instruments to proliferate as the regressions lengthen. With 62 to 66 countries and 60, 80 or even more than 120 instruments, it sounds somewhat off-key to appeal to a method because of its asymptotic properties in wide panels. The method responds promptly by producing conspicuous  $p$ -values of 1.000 for the Hansen test of the overidentifying restrictions. (I have to add, by the way, that the tables incorrectly suggest that the  $p$ -values are test statistics or “Hansen  $J$  statistics”. The same remark applies for the A-B test for second-order autocorrelation.)

In conclusion, however much I appreciated the original idea, I find it difficult to take the substantive conclusions drawn from the estimations very seriously.

### Some more quibbles

- The paper would benefit from thorough proofreading, as well as cross-checking of references and notations. The English is sloppy and doesn't read easily, with grammatical mistakes, unnecessarily complex sentences, telescoping adjectives, and missing articles. The first few references that I tried to check are missing in the list of references: Hallward-Driemeier 2003; Neumayer and Spess 2005; UNCTAD 1998; Egger and Pfaffermayr 2004; Tobin and Rose-Ackerman 2006; all on page 2! After this experience, I gave up checking, though I noted further inconsistencies.
- The parentheses don't match in eq. (1). Also in eq. (1),  $f(K_j)$  is never defined, discussed or justified.
- In equations (3) and (3'), the terms  $Z'\delta_{i,t}$  are mistyped and should undoubtedly read  $Z'_{it}\delta$ . Also, the coefficient notations are in conflict:  $\beta_3$  has a different meaning in (3) and (3').
- Amongst the  $Z$  control variables is real GDP per capita in 2005 US\$, to account for economic development. This is, it seems, the only variable that is not dimensionless. I would argue that its coefficient would be more likely to be stable across countries if GDP were measured in PPP \$ and, more importantly, transformed into logarithms. (Note that this variable will only appear in differenced form, so the log-differences will be growth rates rather than absolute growth in US\$.)
- The notation in eq. (4) is inconsistent with that in eq. (3)-(3').
- The notation in eq. (5) is, again, inconsistent with that in the preceding equations.
- Concerning the tables: I find it not necessarily a bad idea to emphasize “significant” coefficients by boldfacing them, although this should be mentioned in the notes; but the letter code (a, b, c) is definitely a nuisance (and, obviously, totally uninformative).