Referee Report 2

Referee report on

Wasseem Mina (2015). Political Risk Guarantees and Capital Flows: The Role of Bilateral Investment Treaties. Economics Discussion Papers, No 2015-24, Kiel Institute for the World Economy. http://www.economics-ejournal.org/economics/discussionpapers/2015-24

The paper examines the impact of bilateral investment treaties (BITs) on capital flows. Using the system GMM estimator and a panel dataset for 66 middle-income countries and the period 1984-2011, the paper finds that BITs have a positive impact on non-guaranteed debt flows and on portfolio equity flows in these countries. While the topic of the paper is potentially relevant from a policy perspective, the empirical analysis of the paper is not entirely convincing. I have three major comments.

The first one refers to the identification strategy. Capital flows take place between countries, that is, at a bilateral level. BITs are ratifies at a bilateral level as well. The empirical analysis, on the other hand, uses total capital inflows and the total number of BITs ratified (with OECD countries). This is not very convincing. As a consequence, the paper focuses on the signaling effect of BITs rather than the protection effect. This approach is highly questionable in terms of the identification of the main linkages between BITs and various forms of capital flows. I recommend using a gravity model with bilateral capital flows and BITs.

Response:

Thanks for your point. Let me first clarify that the FDI literature has examined the influence of BITs on FDI using either total or bilateral FDI inflows. UNCTAD (1998) and Neumayer and Spess (2005) are examples of the former approach, which I am adopting in this paper.

Your point about the signaling effect of BITs is an interesting one. What you are referring to is how to model the protection effect of BITs. Ideally we need a detailed study of individual signed or ratified BIT by experts (including economists and lawyers) to evaluate the level of protection offered in each treaty. This exercise is beyond the effort of a single researcher. What I adopted is a simple way to capture that protection effect by dividing the number of treaties *ratified* by the total number of OECD countries. The assumption here is that the relatively more treaties a country ratifies with OECD countries the higher is the level of commitment to protecting investment. I should also note that ratification of a treaty indicates a commitment to property rights protection.

My second major comment refers to the treatment of BITs in the paper. In all regressions, BITs are assumed to be homogenous, as the total number of BITs ratified with OECD countries (relative to the total number of OECD countries) is computed. Since the analysis focuses on a rather long time series (beginning in 1984), this can be problematic for the results. So far, we have seen three different types (or generations) of BITs. They differ with respect to the coverage and extent of investor protection. More recent BITs, for example, are far more comprehensive than those ratified in the 1980s. Treating all of them as homogenous is not appropriate and can lead to biases results.

Response:

Thank you again for an interesting point. As mentioned above, distinguishing between the different treaties over time is a challenging and ambitious task. I have addressed this point in three different approaches. First, in this revised version, I have controlled for heterogeneity arising from unobserved country, region, and time effects using an LSDV approach.

Controlling for unobserved time effects (table 6) should capture to a large extent the point you are raising. Second, one can also argue that the development in BIT design and protection is correlated to the development of real OECD output. In robustness checks, I weighted BITs by aggregate OECD real output index. The results are provided in table 7. Finally, I have included a "BIT generation" dummy variable, which took the value 1 for 1984-1990, 2 for 1991-2000, and 3 for 2001 and above, while controlling for unobserved country effects. The coefficient was statistically insignificant though.

My final comment refers to the econometric methodology used in the paper. For all regressions, the system-GMM estimator has been used. While the system-GMM itself is an appropriate estimator for the hypothesis to be examined, the reported results do not convince the reader that the estimations are unbiased. For a start, the system-GMM estimator has been established for "small T and large N" datasets. In view of 66 middle-income countries and 28 periods, this is hardly the case. Major problems show up in the test statistics. For example, the number of instruments is relatively high (due to the long time series), which weakens the Hansen test. In many regressions the J test is equal to 1 or close to 1, making that test unreliable. As shown by David Roodman (2009), the number of instruments can severely affect the GMM results. I fear that too many lags have been used in the paper. In view of the findings reported by Roodman, that would be a mistake. I recommend using different lag structures and – in the majority of regressions – applying not only the collapse option but also only one instrument per endogenous variable. Also, the test statistics for the Arellano-Bond test in first differences – AR(1) – should be reported in addition to the AR(2) results. To sum up, I fear that the empirical results in the paper are seriously biased.

Response:
Thanks for raising this point. Another reviewer has expressed a similar concern about the use of system GMM methodology. Accordingly, I have decided to follow Binici et al. (2010) and use LSDV approach.

Finally, some minor points:
• Why are middle-income countries used only? I would assume that information on at least some low-income countries must be available.

Response:
In this revised version, I have also included low income countries.

• Are 3-month US treasury bills really a good proxy for the risk-free cost of capital? (page 11).
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Response:
Thanks for the provided link. Italics and bold fonts are added. "In theory, the risk-free rate is the minimum return an investor expects for any investment because he or she will not accept additional risk unless the potential rate of return is greater than the risk-free rate. *In practice, however, the risk-free rate does not exist because even the safest investments carry a very small amount of risk.* Thus, the interest rate on a three-month U.S. Treasury bill is often used as the risk-free rate."

• Using system-GMM regression is quite standard these days. I recommend shortening the presentation of that estimator on pages 15 and 16.

Response:
Thank you for the point. I have now adopted LSDV approach and focused only on the rationale for using it.

• The information provided below the tables can be shortened considerably (at least in Tables 6A and higher).

Response:
Thanks! Information is much shortened now.

Reference

Roodman, David (2009), A Note on the Theme of Too Many Instruments, Oxford Bulletin of Economics and Statistics, Vol. 71, No. 1, pp. 135-158.