

“Complex Dependence in Foreign Direct Investment: Network Theory and Empirical Analysis”

This paper is a sophisticated network analysis of foreign direct investment. It clearly involves a great deal of work, and knowledgeable application of network methods. Its biggest problems lie in the centrality of the network model to the paper’s contribution and perhaps in measurement. I therefore recommend revise and resubmit, with some suggestions that will refocus the paper on its contribution to the FDI literature in the context of other political science papers.

I completely agree with their primary criticism of the FDI literature: structural dependence across countries is a real problem in assessing any determinants of foreign direct investment. This paper is not the first to make that point; in fact spatial models FDI date back at least to Coughlin and Segev (2000), and perhaps earlier. In economics, Bruce Blonigen and Peter Egger have both investigated this topic with co-authors. But the real question for this paper is how to contribute to the political economy literature. The authors highlight several contributions in the paper, but I’m not sure that any fully succeeds in this version.

Contribution number one is incorporating third-party effects on FDI. But are these network methods the best way to do that? On page 16, the authors suggest some other NETWORK estimators that might be appropriate, but they do not offer much evidence that network methods are better than other spatial methods. I have some skepticism on this point, given that the authors must use a count model to estimate the correlates of foreign direct investment stock. They choose their dependent variable based on the model’s needs, and their hypotheses are limited to “canonical forms of network structure” (p. 21). Surely other spatial models are better equipped to test multiple types of spatial effects that allow for more sophisticated spatial effects. For example, the network model cannot discriminate between third-country effects that are positive because FDI happens between countries A and B and where it is negative because of that transaction. An auto parts plant in Czech Republic may mean more FDI for Hungary, but a toothpaste plant may mean less because it uses one plant to serve all eastern countries in the European Union. In other words, it depends on the logic of the investment, but global supply chains have complex effects, not just increasing FDI everywhere. In fact, it remains highly concentrated.

Contribution number two is yet another take on political institutions and FDI. This may appear to be settled given the extensive meta-analysis in Li, Owen, and Mitchell (2018, henceforth LOM), but few if any of the papers they review account for third-party FDI effects. As such, this paper can reignite that literature, as all other papers can be critiqued as misspecified. Additionally, LOM find that the debate over whether democracy attracts FDI is governed by the choice of measure, but FDI stocks are not as commonly used as flows and therefore may show different results. Any finding here must deal with the critique by Arel-Bundock (2017) that political institutions are just not very important for FDI.

Contribution number three is the focus on expansion of global supply chains, given the unique timeframe of the data. The relationship between FDI and trade may therefore be unique to this time period, and care should be taken regarding generalization of the results. Again the network setup limits the analysis of panel data to either pooled or multiple cross-sections, but the authors do as well as can

be expected with these limitations. Strangely, the first hypothesis is justified with older motivations for MNCs (who saturate domestic markets and must look abroad for profits) rather than theories that are more attuned to cross-border supply chains. Their logic seems appropriate for rich country pairs, but much less so for North-South dyads. I expect the results do not hold for non-OECD countries.

A few issues should be settled in any revision. First, the authors exhibit a deep knowledge of the foreign direct investment literature, but they use that literature to justify the limited hypotheses that can be tested in network models. In its current form, the paper finesses this point, but it seems that in the end the authors are relatively honest, noting their “network theory of FDI that includes reciprocity and transitivity as the core structural dependencies” (p. 29). If the others can make the case why network models have unique advantages, then why be indirect about the limitations of those theories. Be honest about what can be tested and what cannot. Crucial here is justifying how network models are better than other spatial models.

Second, the dependent variable is imperfect in ways that are not addressed. UNCTAD has done great work on this data set, and I’m glad the authors are making use of it. Furthermore, I like that they use a lagged dependent variable, which might initially make the setup seem similar to a model of FDI flows because it looks for variation in the one year changes of stocks. FDI stocks, however, can vary from year to year for multiple reasons, including both new flows and revaluation. For example, the book value of a factory may deteriorate from one year to the next due to the depreciation of capital assets. To the extent that these depreciation’s are reflected in the FDI stocks measure, it is more than just a representation of flows. Perhaps controlling for inflation would solve this problem, but I am not optimistic of any real solution. Furthermore, the measure still aggregates data at the country level, although the decision-makers are firms. These are important limitation of the data, and should be mentioned in the text.

Overall, I appreciate the work that went into this paper. The authors are quite careful with their network methods and robustness checks, and they exhibit a good reading of the FDI literature. Even so, I’m unsure how to contextualize these results in the broader FDI literature, and I think the current framing doesn’t quite get this right.

Works Cited

- Arel-Bundock, V., 2017. The political determinants of foreign direct investment: A firm-level analysis. *International Interactions*, 43(3), pp.424-452.
- Coughlin, C.C. and Segev, E., 2000. Foreign direct investment in China: a spatial econometric study. *World Economy*, 23(1), pp.1-23.