Sasi Iamsiraroj and Hristos Doucouliagos (2015). Does Growth Attract FDI?. Economics Discussion Papers, No 2015-18, Kiel Institute for the World Economy. http://www.economics-ejournal.org/economics/discussionpapers/2015-18

Brief report for the authors and the editor:

1) **Is the contribution of the paper potentially significant?**

The paper analyses an important dimension of the literature on the determinants of FDI. Does growth attract FDI? This is an important contribution of the paper, which fills a gap in the scattered evidence in the literature. In fact we do not have a clear understanding of the comparative importance of different economic dimensions (such as access to cheap resources, access to knowledge, availability of natural resources, access to more efficient allocation of production via the split of the global value chain in different locations, etc.) explaining the direction an intensity of the Foreign Direct Investment. This is a relative recent field and the data are still pointing towards different directions. The paper does contribute to this important investigation. In this respect, the main strengths of this paper are 'relevance', 'methodological rigor' and 'open-minded approach': the paper has no a-priori on the sign nor the significance/size of this effect and it builds up the necessary steps to convince the reader on the main findings. Another strength of the paper is the encompassing approach in looking at the literature, by analysing a very wide spectrum of keywords and permutations. The 140 papers result is remarkable.

On the weakness side the paper is blurred in the identification of the strong link between the growth-FDI literature and the so-called "market seeking motivation FDI" (vis-à-vis the other three, efficiency seeking, resource seeking and strategic seeking). The reasons why growth might attract FDI are in fact rooted in the strategy of multinational corporations in looking for expanding markets (e.g. in developing countries) that are growing relatively more than other on the global scale.

Even if the paper mentions these channels in different parts it suffers from a clear separation of this stream of the literature from the FDI-growth literature (Does FDI generated growth?) and it is not looking the market-seeking motive in the wider motivation strategy literature. For example, when discussing the growth-FDI relationship results, the paper still cites papers of the FDI-Growth nexus by rendering the punch line much blurred and by weakening the strength of the overall argument. It would be useful the mention the FDI-growth literature at the beginning but then stick to the growth-FDI when the empirical analysis starts. The exclusion of sector level studies and unpublished papers is not really well justified, but it should not change the overall message of the empirical analysis.

Furthermore, there is an enormous "endogeneity" concerns in all this literature (see also the second point on methodology and analysis) and the paper is not using convincing arguments to rule of the possibility that the effect is "purely" driven by endogeneity, as many economists suspect. There is much more work to be done here.

2) Overall the analysis is correct (main strength), but it is highly misleading and not transparent, i.e. potentially driven by statistical artifact (main weakness). The MRA is an excellent tool to investigate the growth-FDI

nexus in a systematic way: the paper is very well written and motivated, the battery of tests is encompassing and the robustness checks quite thorough. However there is no transparency on the main point of endogeneity and of identification of the genuine effect. Three consideration are in order: a) the subset of regressions with control for endogeneity (page 17) b) the inclusion of single countries estimates and the cross-countries in a unique regression (page 17) c) the sample split methodology for developing countries.

a. The last row of table 1 should be presented as a separate table with all the summary stats (number of obs., of studies etc.) and should not be confined to a single line in whole table. For example the FAT-PET test is not significant in columns two and four and this casts doubt on the validity of the overall conclusion that there is a positive (and significant) relationship between Growth and FDI when controlling for publication bias. Even if the PEESE (columns 3 and 6) is marginally significant, there is an indication (Stanley and Doucouliagos 2013, Research Synthesis) to rely on the latter only if the FAT-PET does reject the null (B=0), which is not the case here. Furthermore, the fact that there are not enough "single countries studies" controlling for endogeneity (bottom of page 16) is even more problematic, because it could indicate that the results are actually driven by very badly estimated single countries studies empirical analysis. In other words, it would be useful to show the results of cross-countries studies <u>only</u> and especially cross countries studies controlling for endogeneity. Table 1 is mixing two very much different type of regressions. This leads to point b.
b. Results in table 2 cannot lead to the conclusion (end page 18) that there is a much larger role for economic growth in attracting FDI because time series analysis are fundamentally biased by endogeneity problems. These single studies paper cannot be mixed with the cross-countries analyses to start with (table 1 should have covered only the latter) because they use fundamentally different econometric specification that are completely unreliable on the endogeneity side (single countries studies can use granger causality tests but they are prone to major critiques anyway). Table 3 tries to partially address this point by including the *singelcontry* moderator variable, but this is not anyway enough to convince the reader that cross-countries and single countries should not me mixed together in the first place. For example in single countries studies FE should also be included.
c. It would be useful to split the sample also for not developing and show a separate table to see the results separately. Alternatively, it is also possible to add a dummy in the whole sample and see whether it is significantly different from the constant (implicitly the estimate of the not-developing)

Overall this is a very interesting and well-structured analysis. Unfortunately, the results are partially convincing, due to a lack in transparency when explaining the endogeneity as well as *singlecontry*

estimates role within the overall effect. The latter can be much more muted than emphasized in the paper.