

Referee Report on
**A Microeconometric Analysis of the Springboard Subsidiary: The
Case of Spanish Firms**

for *Economics: The Open-Access, Open-Assessment E-Journal*

Summary:

This paper empirically tries to identify firm- and organization-specific characteristics that affect the performance of so-called springboard subsidiaries. Thereby, the authors also highlight the crucial role of the interaction between subsidiary- and country-specific characteristics for differences in the performance of (springboard) subsidiaries.

Comments:

I unfortunately have to admit that I am rather unhappy with the general quality of this paper. To start with, I would like to mention that I agree with all comments put forward by an invited reader report which has been uploaded at the journal's homepage on April, 15th. However, I would like to also mention some of them explicitly and also add some additional thoughts.

To start with, the structure of the paper is flawed and the paper could benefit a lot from a careful language and style proof reading. The introduction provides some very (and in my point of view too) general statements on globalization. The authors do not really explicitly state their research question and also could better justify why investigating what they call springboard subsidiaries is important and interesting.

In a similar vein, to me the literature review is rather confusing and mainly reports a large list of findings from the literature without properly connecting these. Furthermore, I totally miss any reference to the economic literature on internationalization. To give only one example, I would argue that it is essential to differentiate between alternative motives for FDI when investigating the role of springboard subsidiaries. Among other things, I would have liked to see a discussion on how the importance of springboard subsidiaries might differ across horizontal, vertical and complex motives for FDI. Clearly, one could add many more issues from the economic literature which should be relevant for the research question at hand.

This confusion also carries over to Section 3 which tries to formulate some testable hypotheses. Consequently, the reader gets the impression that the hypotheses are rather arbitrarily formulated and not well deduced from theory. Moreover, for someone who is not perfectly aware of the cited management literature, it's almost impossible to identify the value added of this paper.

I also believe that the authors could do a better job in their empirical analysis. First of all, a more careful and extensive presentation of the data would help a lot. Second, the econometric specification provided in equation (1) does not fit the results provided in Table 2. Equation (1) suggests that the authors only include interaction terms for their variables of most interest while the columns in Table 2 also report estimates for the main effects of these covariates.

Furthermore, it remains unclear which estimation strategy the authors have applied. Again, equation (1) somehow suggests that they are using simple linear regression models. They include a common constant α_0 (although in the text the authors label it α_i) and formulate a linear equation which includes among other things industry- fixed effects. Table 3 suggests that they are applying subsidiary- fixed effects estimators (which would fit the α_i story in the text), because they report within- R^2 measures. In its Table 3, however, the paper reports marginal effects that differ from the point estimates reported in Table 2. This could only happen when one uses some kind of non-linear models. However, there is no proper explanation of all these issues offered in the manuscript. This, in turn, makes an understanding for the paper's results and a useful interpretation of them almost impossible.

Concerning the estimation of the parameters of interest, the authors assume that their information on the springboard status of subsidiaries is exogenous to their profit margins. This is an assumption that would need much for justification! Otherwise, one is tempted to think that the estimates are also prone to an endogeneity bias. I also think that the paper would be more interesting when the authors would assume that the springboard status is endogenously determined and then try to come up with some estimators which would still allow to estimate causal effects of a subsidiaries springboard status. Here I especially think about some methods extensively discussed and used in the program evaluation literature.

Finally (and as also already pointed out by one invited reader), the results section is substantially underdeveloped. The authors only spend one sentence on the results for each and every hypothesis and do not even accurately explain how the different columns in Table 2 differ in terms of the underlying empirical specifications, etc. One can also find almost nothing on what we can learn from the paper's finding for our understanding of multinational enterprises.

Minor Comments:

- The marginal effect for the age of subsidiary seems to be way too large. At any point in time and *ceteris paribus*, this would imply that subsidiaries increase their overall performance by an average of 100 percent in case they are able to survive for two more years.
- I was wondering how the information from BVDs independence indicator includes different information in comparison to the included ownership shares. As far as I know, the independence indicator classification is mainly based on the latter information.
- Regarding the included ownership information, I was wondering how the results would change if one applies alternative cut-off values in the ownership. For example, stan-

dard FDI definitions typically only require that the foreign firm (in this case the Spanish subsidiary) holds 10 percent of the foreign firm (the subsidiaries in Latin America). Alternatively, one could also well-justify a cut-off value of 50 percent or more of all outstanding shares (majority ownership) as being relevant for the research question at hand.