

Reply to Invited Referee Report

First of all, we would like to thank to the Referee for his/her valuable comments. We are honored by the effort made to understand something that we tried to develop. Our responses are provided below.

Comments

Comment 1: 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 awed 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.

R/ Thanks for the comment. In the revised version we will present the springboard country and springboard subsidiary concepts in the introduction.

Comment 2: 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.

R/ Thanks for the comment. The referee is correct in that there is a clear connection between FDI and springboard subsidiaries. We took this relationship into consideration through the construction of the springboard country variable, which was created to serve as an index of distance that includes FDI as one of its aspects. The springboard country is the strategic role that a country can play when it is in an intermediary position between two countries that want to establish inflows and outflows of direct foreign investment. Rather than being

based on geographical distance, its advantageous position is based on institutional distance, which is decisive for these types of flows of capital, knowledge and final products.

Thus, the springboard perspective may be seen as a strategy available to multinationals in developed countries that seek to enter emerging markets. In this vein, Pla & Camps (2012) propose a springboard perspective in which European multinationals can develop a springboarding strategy. Using this strategy, the multinational's subsidiary takes advantage of a springboard country's strategic position relative to the destination countries considered in order to manage its network of subsidiaries, thus becoming a springboard subsidiary. This perspective is based on theoretical developments in literature on international business (Johanson & Vahlne, 1977), regionalization theory (Rugman & Verbeke, 2005; Rugman & Verbeke, 2007), and in research in the economic geography field on how multinational companies manage their foreign subsidiaries (e.g., Yeung, Poon, & Rerry (2001) and Phelps & Fuller (2000).

Finally, it is clear that in standard FDI theory (Rugman 1986, Caves 1971) firms invest in foreign markets in order to obtain rents from exploiting firm-specific capabilities (products and knowledge). FDI boosts firms' strategic position by providing access to scarce resources like labor and knowledge (Chen and Chen 1988).

Comment 3: 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.

R/ Thanks for the comment. We explain the hypothesis in greater depth in the final version. Please see our response to the invited reader report.

Comment 4. 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.

R/ Thanks for the comment. We agree with the referee that the variables in Table 2 do not match the econometric specification offered in Equation (1). In Table (2) we check the robustness of the variables and for this reason we modify the use of variables and controls in each estimation. In the final version we will make sure

that at least one estimation includes all of the variables in Equation (1).

Comment 5. 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.

R/ In this case, we respectfully disagree with the referee. Equation (1) is not a simple linear regression. It is clear that we estimate a panel data model (we follow N firms in t periods):

"The basic regression model for N firms and T time periods, where the firms are indexed by i and time by t , takes the following form:

$$\begin{aligned}
 Performance_{i,t} = & \alpha_i + \beta_1 PtEsp_{i,t} \times Springboard\ Subsidiary_{i,t} + \beta_2 PTFT \times \\
 & Reltec_{i,t} + \beta_3 Ind_{i,t} + \beta_4 ActIntFix_{i,t} + \beta_5 Year_{i,t} + \\
 & \beta_6 SectManuf_{i,t} + \beta_7 SectEdu_{i,t} + \beta_8 SecActFin_{i,t} + \beta_9 D2003_{i,t} + \\
 & \beta_{10} D2004_{i,t} + \beta_{11} D2005_{i,t} + \beta_{12} D2006_{i,t} + \beta_{13} D2007_{i,t} + \\
 & \beta_{14} D2009_{i,t} + \vartheta_{i,t}
 \end{aligned}
 \tag{1}$$

Where, $i = 2582$ subsidiaries and $t = 8$ periods, so the total number of observations is 20,656."

But we agree with the referee's concern about the explanation of the nature of the individual unobserved heterogeneity of the firms $\alpha_i + \vartheta_{i,t}$. We discuss this topic in greater detail in the final version. Finally, our estimations use random effects (we use random effects given the results of the standard Hausman test (1997)) and we will include a note on this in the final version.

Comment 6. 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.

R/ Thanks for the comment. It is true that the marginal effects (partial effects) are the same as those resulting from the parameter

estimation. But we are interested in a particular function of these partial effects (marginal effects) such as elasticities. Thus, we compute the elasticities (see the Table (3) and Equation (2)).

$$MFX_{Performance, X} = \frac{\partial Performance}{\partial X} \times \frac{X^{\circ}}{Performance^{\circ}}$$

Where the superscript ($^{\circ}$) indicates that the average value of the variable is evaluated".

Because of the unobserved individual heterogeneity effects, we compute the average partial effects. (We corrected the type in Equation 2, a Spanish word). In the final version we discuss these elasticities in greater depth and include the differences point made by the referee.

Comments 7. 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.

R/ Thanks for the comments. One solution for endogeneity bias is of course the use of instrumental variables in the context of the panel data models. This is an interesting discussion and in the final version we include an IV-Panel data estimation.

Minor Comments:

Comment 8. The marginal effect for the age of subsidiary seems to be way to 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.

R/ We agree with the referee's comments and thank you for the same. We have checked the results.

Comment 9. 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.

R/ OK. BvDEP (2009) explains that "...BvDEP has created an Independence Indicator to characterise the degree of independence of a company with regard to its shareholders.. Indicator A -Definition: Attached to any company with known recorded shareholders (excluding the 3 "collective" (p. 15) types indicated above) none of which having more than 25% of direct or total ownership." We use this definition in our paper.

Comment 10. 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, standard 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.

R/ Thanks for the comment. Yes, it is possible that the results will change if the cut-off values are changed. In the final version we will include and discuss different cut-off values.