

Discussion Paper No. 2015-39 (Hollenstein/Berger)

Answers to the *second* Referee Report

Main comments

1st para.

According to the journal's "guidelines for authors" the length of a paper, in principal, is not limited. Therefore, 11'000 words (excluding the tables) may not be too long. Nevertheless, restructuring the paper according to the suggestions of the referee and some tightening of the text (eliminating redundancies although these are often appreciated by readers; deleting some illustrating examples (?)) may help to reducing the length of the text – *if needed at all (!)*.

In the latter case we could present the material in the following order (old numbering of the (sub)sections in brackets):

1. Introduction (1)
2. Model A: Conceptual framework and hypotheses (2.1); previous evidence (2.2); specification of empirical model A (5.1) including problem of sample selection (5.3.1)
3. Model B: Conceptual framework and hypotheses (3.1); previous evidence (3.2); specification of empirical model B (5.2) including endogeneity/causality problems (5.3.2)
4. Data (4)
5. Empirical results for model A (6.1) and B (6.2), including table 7 that summarises the results of the empirical tests of H1 to H4
6. Summary and conclusions (7)

We would highly appreciate to get a feedback from the editor with respect to the structure and the length of the paper. Does an adjustment of the structure of the paper along the lines mentioned above (what implies re-writing of parts of the text) enhance the quality of the paper to an extent that justifies the necessary effort?

The referee suggests to writing down the estimation equations. However, we do not see a necessity to do so as the specification of the model explaining RDFOR and MODE (model A) as well as the model explaining LINNL and LQL (model B) are clearly set out in Table 2 (model A) and 3 (model B). For each model, the tables provide a list of the explanatory variables, definitions and measurement of the variables as well as the expected signs.

2nd para.

As explicitly mentioned in subsection 5.3.1 (p.15), we used the two-stage Heckman model. The Mills ratio in the MODE-equation turned out to be insignificant in the Swiss ($p = 0.597$) as well as the Austrian case ($p = 0.767$) as reported – without mentioning the p-values – in the text (see subsection 5.3.1, p.16; we may provide the p-values in a footnote.) As a consequence, there is no need to use the Mills ratio in estimating the MODE-equation.

Furthermore, we explicitly have discussed the appropriateness of the identifying variables “degree of competition” and “export intensity” (see subsection 5.3.1, p.15); the referee’s suggestion is thus already accounted for. The two variables are significant in the propensity equation.

3^d para.

“Use present tense as much as possible and keep use of passive tense to a minimum” is accepted. We shall adjust the text accordingly.

4th para.

We agree to change the title of the paper by using “*relationship*” instead of “*impact*” because it effectively is impossible to identify causal links by model estimates based on cross-section data (what is explicitly admitted in subsection 5.3.2). In this respect, we are much more cautious than the authors of almost all entry mode studies which typically also are based on cross sections (see subsection 5.3.2 and footnote 10). In contrast to these studies, we clearly state that “rather than making causal claims, we interpret the estimated coefficients as partial correlations ...” (subsection 5.3.2).

2. *Minor points*

- a) All comments on wording/grammar are accepted and could thus be accounted for in a revised version.
- b) We mention *five* surveys of the entry mode literature which very specifically deal with this topic both at the theoretical and the empirical level. Contrary to the reviewer’s suggestion, the article of Dunning and Lundan (2009) cannot be added to the list of literature survey as it discusses the entry mode choice only very shortly. We rather suggest to referring to the book of these authors which we already cited in Section 1 and 2 (Dunning and Lundan, 2008, Ch. 9).
- c) We agree that there should be a statement on the paper’s contribution within the introduction, what, however, we already have already done. We mention in the introduction that the paper complements previous research in three aspects (see p. 3). We The same holds true for the final section (p.21) where our contribution is substantiated by linking the main empirical findings with the three points mentioned in the introduction.
- d) It is suggested that the statement “As early as in the 1970s, Dunning argued ...” (see p. 4) requires a suitable reference. We shall do so by mentioning:

Dunning, J.H. (1977). Trade, Location of Economic Activity and the Multinational Enterprise: A Search for an Eclectic Approach. In B. Ohlin, P.O. Hesselborn and P.J. Wiskman, *The Allocation of International Activity*. London: McMillan.

Dunning, J.H. (1979): Explaining Changing Patterns of International Production: In Defence of the Eclectic Theory, *Oxford Bulletin of Economics and Statistics* 41(4), 269-295.

- e) The reviewer suggests to referring, additionally, to the contributions of Rabbiosi (2011) and Moncada-Paterno-Castello et al. (2011). It appears to have escaped the reviewer that we cited Rabbiosi (2011) in subsection 3.1. The second paper (MPC), of which we are well aware, provides evidence and a discussion of the most relevant trends of the internationalisation of R&D. An important one is the increasing relevance of joint ventures/contractual R&D (what we emphasised on p.1). However, MPC only shortly deal with the governance of foreign R&D. In this respect, MPC primarily mentions that foreign R&D more often is based on merger/acquisition activities than on greenfield investments, both being equity-based modes of foreign R&D. The factors determining the choice between *equity*- and *non-equity* governance modes, what is the topic of the present study, is not discussed. Therefore, we do not feel a necessity to refer to MCP.
- f) In the text, we cited Chen and Chang (1996) whereas the correct reference is Chen and Chang (2011) which already is included in the references section. We shall check again the one-to-one correspondence of *all* references in the text and the reference list.
- g) The referee is not convinced that “insufficient IPR protection in the target country” is a relevant factor driving foreign R&D, arguing that the majority of MNEs invest in developed countries. However, a survey conducted by Thursby and Thursby (2006) shows that this variable is highly relevant for deciding on FDI in R&D both in developed and in less developed countries; it is even more relevant in case of developed countries. We thus shall cite this study.

Thursby, J. and M. Thursby (2006). *Here or There? A Survey of Factors in Multinational R&D Location*, Washington (DC): National Academic Press.