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A response to Honig and Samuelsson (2014)

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ABSTRACT

Honig and Samuelsson (2014) recently published an article in this outlet criticizing "Does business planning facilitate the development of new ventures? "a paper I wrote with Scott Shane nearly 15 years ago. They claim that their effort adds to the discussion of (a) the merits of business planning, (b) data replication and extension, (c) sample selection bias, (d) evaluation of normative research and (e) publication standards. However, most of the claims they make are incorrect.

1. Introduction: the importance of replication to scientific discovery

As part of an article in JBVi that discussed the importance of data replication and extension Honig and Samuelsson (2014) criticized the methodology in a paper I published on business planning in nascent ventures with Scott Shane in 2003 (Delmar and Shane, 2003). Honig and Samuelsson's (2014) work is based on the partial replication and extension of two papers (Delmar and Shane, 2003; Honig and Karlsson, 2004) that reached opposite conclusions on the merits of business planning while both using the Swedish Panel Study of Entrepreneurial Dynamics (PSED). In this article, I seek to stimulate the discussion about testing theory using data from prevalent cohort design by highlighting some of Honig and Samuelsson's (2014) misunderstanding of several key dimensions of such designs. I also discuss the methodological choices that Delmar and Shane (2003) made 15 years ago and how those choices influenced our research results.[1]

The PSED is *a prevalent or cross-sectional cohort design*. Respondents are identified through a range of questions that ensure they are in the process of starting a new venture. The first wave of interviews is therefore a cross-sectional sample of people who are in the process of starting a business. These respondents are then followed and sampled repeatedly over time.

This design is different from an *incident cohort design*, for which respondents and units are identified when they experience their first critical milestone or conception time. From that point on, units are followed over time or until they reach a terminal outcome (e.g., venture termination). Drawing accurate inference from a prevalent cohort design depends on the correct approach to analyzing such data.

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[1] The data and the code book used in the D&S paper have been available for some time of my Research Gate account (https://www.researchgate.net/publication/266630741_Swedish_PSED_Final_Data_1998) and (https://www.researchgate.net/publication/262796537_Coding_manual_for_Swedish_PSED_final_data).

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2. Dealing with left censoring

Honig and Samuelsson (2014) criticized that we did not use the full sample of cases. A problem with a prevalent cohort design is that the cases sampled began the process of starting a new venture at different points in time. As a result, a prevalent cohort design likely oversamples cases that are in the start-up process for a longer period of time. For example, if we sample start-ups in 1998, we mix ventures that have been in the process of starting up for two months with ventures that have been in the process of starting up for many years. The sampling procedure misses cases for which the venture start-up process was so rapid that the ventures have either become established businesses or been terminated. The exclusion of short-in-process cases is known as *full left-censoring* (Blossfeld and Rohwer, 1995).

The PSED sampling procedure therefore generates a biased sample of cases. That bias might influence results of studies examining those cases if not dealt with statistically. In the case of business planning, the biased sample could influence results either because business planning permits fast failure or rapid establishment or because more complex businesses need to plan more or cannot spend as much time on other organizing activities. The point is that the causal mechanisms governing long-in-process may be quite different from those governing short-in-process. You may identify risk factors that are associated with the duration of the process rather than causal factors. In fact, favorable factors that tend to prolong the process time may be misinterpreted as causal factors to outcomes of interest.

To mitigate the problem of left censoring, Delmar and Shane (2003) retained only ventures started in the first nine months of 1998 (a period concurrent with the first wave of interviews). As our milestone or conception point, we chose when respondents say that they committed to the specific venture based on this question "What year did the work of starting your business begin?" in the phone interview. This procedure allowed us to construct a sample similar to an incident cohort with a clear and theoretically relevant time point of conception: the commitment to this particular venture.

3. Dealing with left truncation

Honig and Samuelsson (2014) pointed out that Delmar and Shane (2003) threw out too many cases and that we did not make full use of the data, because we did not time code organizational activities happening before our conception point. They find that many ventures had actually initiated a number of activities before we start our process clock, and therefore that our causal claims may be invalid, especially when it comes to our dependent variable organizational activities. Yang and Aldrich (2012) made a similar argument proposing that Delmar and Shane (2003) were too conservative by excluding too many cases and thereby increasing the risk of not finding significant coefficients.

PSED suffer left censoring but provides left truncated data. Left truncation is closely related to left censoring. Left truncation happens when a number of prevalence cases are sampled and the researcher adds additional retrospective information about the time of entry and the process. Researchers can then reconstruct the whole process. Statistical analysis dealing correctly with left truncated data allows the researchers to correct for some of the biases introduced by missing fully left-censored cases (cases short in process). Two assumptions need to be made for these statistical analyses to be valid. First, respondents need to able to recall when milestones happened, thereby providing researchers with precise data on timing. Second, cases short-in-process are similar to cases long-in-process (Applebaum et al., 2011; Guo, 1992).

As stated earlier, Delmar and Shane (2003) used commitment to the specific venture as the conception point at which we started the process clock. This obviously limited the sample size as many nascent entrepreneurs long-in-process were not included. That is, we decided to only use 223 cases, because the other cases did not fulfill our criteria (too long-in-process). We coded all activities that happened before commitment as initiated at the time of the first month of conception. In table 1 (p.1176), Delmar and Shane (2003) presented the details per wave instead. At wave 1, the ventures had initiated on average 1.6 activities (s.d. 1.4). We did not code for how many months previous our commitment the activities had been initiated. The logic of the process clock guided that particular decision: the entrepreneurs had not yet committed to a particular venture.

Nevertheless, the inefficient approach used by Delmar and Shane (2003) is better than the alternative of not dealing with left censoring at all, especially when the researchers have strong beliefs that respondents might not recall activities and that cases short in process might be different from cases long in process.

4. Dealing with right censoring and time-varying covariates

Delmar and Shane (2003) used event history analysis with time-varying covariates to incorporate information about the timing of the activities into the prediction of new venture disbanding and to control for right censoring. Honing and Karlsson (2004) used the interview waves of the PSED data. Honig and Samuelsson (2014) used cross-sectional data (five and 10 years). Their analysis did not take into account timing of activities and right censoring included in Delmar and Shane (2003).

The PSED provides *time-varying covariates*, that is, information about the timing of various venture organizing activities and outcomes. Having this information allows for more precise estimation of the effects of different organizing activities, such as business planning on new venture performance by allowing researchers to incorporate information about the timing of the activities into their estimates. Thus, using logistic regression in place of event history models introduces bias in and reduces the precision of the coefficients (Blossfeld and Rohwer, 1995; Yamaguchi, 1991).

Right censoring happens when a case has not yet reached the outcome of interest at the study's end, such as having the disbandment of the venture by all its members. Because all cases are not equally likely to reach an outcome before the end of observation period, and because those cases might reach an outcome later (the problem of right censoring), event history models provide more precise estimators than logistic regression (Cain et al., 2011).

5. Using fixed effects regression to deal with unobserved heterogeneity

Honig and Samuelsson (2014) argued that the use of fixed effects is problematic and that our causal claims may be invalid because ventures had initiated organizational activities before undertaking business planning. However, this is not correct. Delmar and Shane (2003) used fixed effect regression, correcting for survival bias, to predict changes in two quantitative outcome measures: product development and organizational activities. Fixed effects regression has the attractive feature of controlling for all stable characteristics of the unit of analysis, whether measured or not because the fixed effect for the case captures all non-time varying characteristics by only measuring the change within the subject (Balgati, 2001; Wooldridge, 2002).

Failure to use fixed effects leads to biased estimates from unobserved heterogeneity. For example, if less talented entrepreneurs with worse ideas are less likely to succeed than more talented entrepreneurs with better ideas, venture performance might be correlated with business planning simply because more talented entrepreneurs with better ideas both choose to plan and perform better.

Fixed effect regression looks at changes over time, therefore, it can incorporate changes in business planning and other organizing activities at whatever time they occur. As argued previously, Delmar and Shane (2003) allowed for the fact that organizing activities and product development might have happened before business planning. In our model, organizing activities might have happen before business planning (mean=1.55, s.d.= 1.39 at wave 1, table 1 p.1176), but business planning increased the number of organizing activities initiated (mean=5.06, s.d.= 1.79 at wave 5, table 1 p.1176). The same argument is valid for product development (mean=4.43, s.d.= 1.01 at wave 1; mean=4.67, s.d.= 0.87 at wave 5; table 1 p.1176). As a result, Honig andSamuelsson's (2014) Figures 1 and 2 are incorrect interpretations of Delmar and Shane's (2003) model both when it comes to the data set and the analysis.

6. Proximal and distal outcomes variables

Honig and Samuelsson (2014) criticized Delmar and Shane's (2003) choice of venture disbanding, product development and venture organizing as dependent variables, suggesting that organizational performance, such long-term sales and survival are more appropriate variables. This criticism implies that Delmar and Shane (2003) saw their model as predictive of the long term financial performance of a new venture, i.e., distal outcomes. This raises an important question: what is the most appropriate dependent variable for entrepreneurship research for nascent ventures early in the organizing process?

Our theory was only about the role of business planning in the nascent stage of a new venture's life (i.e, proximal outcomes). We argued that business planning helps entrepreneurs develop products to be ready for the market and to initiate other organizational activities. They did not argue that business planning leads to the long-term financial performance of a new venture.

All theories specify boundary conditions for their predictions of why and how and our theory was that business planning facilitates the venture organizing process, thereby leading to better outcomes mechanisms. Once the venture is organized a theory about how business planning facilitates the venture organizing process is not relevant to the performance of the venture.

Honig and Samuelsson's (2014) study examining the effect of initial business planning on long-term financial performance (sales and survival) does not make sense without a theory that business planning leads to long term venture financial performance. Such distal variables are contaminated with noise. Too many things other than business planning affect a venture's survival or sales five and 10 years later.

7. Conclusion

I have here replied to Honig and Samuelsson's (2014) paper on the need to extend and replicate studies, especially on business planning. In their efforts to criticize Delmar and Shane (2003), Honig and Samuelsson (2014) made a number of incorrect statements about that paper and about how to test theoretical arguments on data from a prevalence cohort design.

Criticism and progress in theory, design and analysis is the nature of research. Future studies will, no doubt, inform us further. Possible venues are analyses dealing with truncated data incorporating full data (with the full use of time-dependent covariates) and dealing with left censoring.

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