Journal of Business Venturing Insights 3 (2015) 12–15

Contents lists available at ScienceDirect

Journal of Business Venturing Insights

journal homepage: www.elsevier.com/locate/jbvi

Data replication and extension: A commentary

Per Davidsson [a,b,*]

[a] Australian Centre for Entrepreneurship Research, Queensland University of Technology, GPO Box 2434, Brisbane 4001 QLD, Australia
[b] Jönköping International Business School, Sweden

ARTICLE INFO

Article history:
Received 7 February 2015
Accepted 11 February 2015
Available online 21 March 2015

Keywords:
Replication
Nascent entrepreneurship
Null findings
Business Planning

ABSTRACT

Honig and Samuelsson (2014) and Delmar (2015) recently had an exchange in this journal related to a replication-and-extension attempt of two papers which originally arrived at different conclusions based on the same data set. This commentary provides further clarification on the issues and links the debate to broader issues scholarly culture and practices in entrepreneurship research.

Honig and Samuelsson (2014) recently published in this journal an interesting example of re-examining the evidence underlying conclusions regarding the efficacy of business planning for venture creation success. Delmar (2015) has since published a rebuttal. The debate focuses particularly on two previous papers on the efficacy of business planning among nascent ventures. The papers, which I will refer to as "D&S"(Delmar and Shane, 2003) and "H&K" (Honig and Karlsson, 2004), used the same data base but arrived at different main conclusions. I wish to add this commentary because as initiator and leader of the research program from which the underlying data emanates I feel partly responsible for the apparent controversy. I may also be in a unique position to provide further clarification on the debated issues. Further, this case of contradictory findings and replication attempt provides an excellent opportunity to reflect more broadly on the scholarly culture and practices in entrepreneurship research.

I have considerable familiarity with panel studies of nascent entrepreneurship through deep involvement in three major projects and as reviewer of the research stream (Davidsson, 2004b, 2005; Davidsson and Gordon, 2012; Davidsson et al., 2011a, 2011b; Delmar and Davidsson, 2000). The people involved are respected colleagues whose contributions to the nascent entrepreneurship literature I have singled out as particularly valuable (Davidsson and Gordon, 2012: 871–872; Davidsson et al., 2011a, 2011b: xxiv–xxix). With all but one of them I have published research based on the very same data set (Delmar and Davidsson, 2000; Honig et al., 2005; Samuelsson and Davidsson, 2009). I acclaim Shane's scholarship in Davidsson and Wiklund (2009). Further, I devoted a whole chapter in *Researching Entrepreneurship* to the importance of replication (Davidsson, 2004a). As regards the value of business planning for nascent ventures the collective evidence I have seen makes me remain a sceptic.

To put the current debate in perspective it is also important to realize how little was known some 15 years ago about what entrepreneurial processes looked like in a random (-ish) sample, or how they could best be theorized and analyzed. Before scores of researchers sunk their teeth into the PSED and its sister projects, we had very vague notions of the nascent entrepreneurship phenomenon. The dominant, implicit mental model of the phenomenon was arguably that motivated,

* Correspondence address: Australian Centre for Entrepreneurship Research, Queensland University of Technology, GPO Box 2434, Brisbane 4001 QLD, Australia.
E-mail address: per.davidsson@qut.edu.au

http://dx.doi.org/10.1016/j.jbvi.2015.02.001
2352-6734/

single individuals or custom-built teams purposefully entered the process at a distinct point in time with an intention of working full time to implement a well-defined, for-profit venture idea as quickly as possible, following one of a small set of possible types of process, and being at the risk of losing significant amounts of money should they have to abandon the attempt. Early, descriptive results – published or unpublished – proved most such assumptions wrong and revealed a phenomenon of greater complexity, variability and (for the most part) modesty than anyone had thought possible (Aldrich et al., 2006; Carter et al., 2003; Davidsson and Gordon, 2012; Delmar and Davidsson, 2000; Liao et al., 2005; Reynolds, 2007; Reynolds et al., 2004; Ruef et al., 2003; Van Gelderen et al., 2005). We should also remember that we started with limited experience in the particular challenges and opportunities associated with longitudinal, panel data.

In this environment, D&S and H&K were both pioneering studies, providing data, theoretical angles, results, and method solutions that were novel for their time (Davidsson and Gordon, 2012: 871 Davidsson et al., 2011a, 2011b: xxiv–xxix). As a result, they both got published in leading, mainstream management journals – a rare event for entrepreneurship research at the time – and have been widely cited. In order to achieve this, the respective authors had to make a series of methods choices in partly novel terrain. Using the same data set, it is possible that making better and worse choices led one team but not the other to arrive at the correct conclusion on the substantive issue: the efficacy of business planning for nascent ventures.

Do Honig and Samuelsson (2014) settle the issue? I do not think so. Their temporal extension of H&K confirms the original results, but does so without including what we have come to realize is the most important control variable, namely how far progressed the start-up already was when first sampled (Davidsson and Gordon, 2012). Their dissection of D&S reads as if they have identified fundamental and indisputable flaws in D&S' procedure, such that sample bias and reverse causality drive the positive results for business planning. Reading all sides of the arguments, I am not convinced this is the case. The ideal study of the effects of business planning in new venture creation would have what Delmar (2015) calls an "incident cohort design", i.e., capture all cases at a unique and indisputable process inception point and follow them forward over time. As I understand it, what D&S tried to do was to take their study closer to this ideal. They seem to have been the first to realize and try to counteract the sampling and retrospection biases associated with the PSED methodology's inclusion of "long in process" cases that were "still trying" at the time of sampling (while their terminated or operational cohort peers were excluded by the sampling mechanism, i.e., left-censoring). They were also the first to re-organize the data set into monthly spells using the time-stamped gestation activities, rather than relying on the timing of interview waves, which occur at different and arbitrary stages of development across cases. In order to do so, they needed to develop a criterion for process inception. The problem is that because people drift into – and proceed with – start-up processes in all manner of ways there will never exist a distinct and indisputable criterion for when venture creation processes are started (Bhave, 1994; McMullen and Dimov, 2013; Reynolds, 2007; Reynolds and Miller, 1992). Honig and Samuelsson's (2014) analysis indicates that D&S choice of inception criterion may be associated with considerable problems. However, as I understand the set-up of D&S analysis, it is only *change* in planning status associated with *subsequent change* in the respective dependent variables that can affect the analysis results.

By contrast, the H&K analysis approach disregards the fact that "short in process" cases are under sampled, and that the cases are unequally far progressed in the start-up process when they enter the sample. This is understandable as most of us did not realize the importance of this back then (cf. Davidsson and Honig, 2003). However, H&K have one major advantage over D&S, namely that they apply a much stronger criterion for venture creation success than D&S do. The latter rely on indicators that can better be labeled "persistence" and "progress". This is also understandable, because 15 years ago we had limited understanding that this lumps together (a) cases on their way to successful entry; (b) cases that will never be successful and therefore should better be terminated quickly, and (c) cases that tinker about without ever seeming to get to a resolution either way (Davidsson, 2008; 184–185; Reynolds, 2007: 54). H&K's results indicate that planning may be positively associated with persistence but not with venture creation success, which I would say accords with what the collective evidence in nascent entrepreneurship research to date suggests (Davidsson and Gordon, 2012: 858–860). Following up this result after 5 and 10 years, so as to obtain "final outcomes" for a larger share of the sample, is a laudable undertaking. However, due to the methodological limitations of the H&K approach and possible idiosyncrasies of the particular data set I do not think the extended re-analysis settles the substantive issue of the merits of business planning for nascent venture.

This said, there are other important lessons to be learned from their effort and Delmar's rebuttal:

1. It reminds us that *doing good social science is very hard.* In developing their studies, researchers face limitations and are forced to make choices of such nature that close scrutiny may lead to valid questioning of the conclusions. The immense difficulty of the task is reason to respect the honest efforts of our peers, even if we disagree with some of the choices they made. I cannot bring to mind a case where a single study provides rock solid evidence on an issue, no matter how far up the journal hierarchy we look. This means that the current culture in management research, which requires a theoretical contribution of every empirical paper while actively discouraging replication, is fundamentally unsound (Hambrick, 2007; Hubbard and Lindsay, 2013a). We should therefore welcome journals like JBVi, which broaden the view of what constitutes valuable scholarly contributions.
2. The difficulty of getting all methods choices right is one reason why *replication* – in a broad sense – *is critically important.* Other reasons are that idiosyncrasies of the original empirical setting may drive the results in a particular direction, and that authors may – consciously or subconsciously – gear their methods choices towards results that match their