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Out of sight but not out of mind: Why failure to account for left truncation biases research on failure rates

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ABSTRACT

We note at least three major issues in entrepreneurship theory that can be clarified by studying the survival chances of new ventures: the extent to which entrepreneurs are so constrained by initial founding conditions that they are unable to learn; the degree to which heterogeneity and innovative capabilities are lost due to the failure of new ventures; and the imprinting effects of new ventures' early days on their subsequent development. However, previous research on these issues has been inconclusive because of problems in research design and data analysis. In this paper, we shed light on new venture failure rates by assessing the validity and generalizability of previous findings. We argue that research using registration data to study new ventures is very likely to generate biased results and that research attempting to track new ventures from a very early stage can still suffer from selection bias due to left truncation. Using a sample of new ventures from the Panel Study of Entrepreneurial Dynamics II, we provide evidence for the extent of such biases. We offer a statistical solution to left truncation that can be easily applied in widely used statistical programs.

1. Executive summary

Despite the best efforts of their founders, many entrepreneurial startups do not succeed. Many fail early, whereas others continue for months or even years before their founders abandon the effort. Understanding why the fates of startup attempts differ so dramatically has been a major concern of scholars over the past several decades, and they have made significant progress in theorizing about organizations and their environments. In addition to the practical question of whether researchers can identify resources and practices that might benefit entrepreneurs creating new ventures, studying startup attempts also carries theoretical benefits. Investigating the survival of emerging ventures in the very early weeks or months after founders have initiated their activities can shed light on at least three theoretical issues, including the extent of initial selection and adaptation among startups, the heterogeneity of their organizational forms and routines, and new ventures' capabilities in developing effective strategies. However, research designed to investigate these issues has run into difficulties. When scholars have designed research to study organizational emergence, most of them have focused on the organizing attempts that resulted in viable entities and ignored the organizing efforts that prematurely failed.

In the past decade, new research designs have made it possible for scholars to identify new ventures from a very early stage. Nonetheless, their research is still subject to a selection bias because of a built-in time lag between when a new venture is initiated and when an observation starts. Both scenarios pose problems for investigators – the earlier problem of only studying hardy organizations and the recent problem of over-selecting hardy organizations – because selective sampling threatens the validity of

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empirical findings. They lead to biased estimates of new ventures' failure rates and obscure the causal mechanisms that explain new ventures' failure. Although scholars have called for more empirical investigations of new ventures' life chances, less effort has been devoted to state-of-the-art research design and analytical methods.

In this paper, we moved beyond previous efforts in making three contributions. First, we identified and explained the methodological problems that have biased previous results and thus impeded our understanding of organizational emergence. Drawing on principles of sampling and statistical inference, we have reminded researchers of the limits to drawing inference to the whole population of emerging organizations if they study only the ones that can be located in public records. When investigators must rely on registration data, they should offer explicit caveats regarding generalizing from their results.

Second, we illustrated the bias created by registration data that only include well-performing new ventures and the bias created by left-truncated data that cover incomplete coverage of new ventures' lifespans. Using the Panel Study of Entrepreneurial Dynamics II, a data set that followed nascent entrepreneurs from when they initiated their new ventures and collected timing information on many important milestone events, we showed that there is a significant difference in survival probabilities for ventures that have been registered (and could be found in public records) and ventures that have not been registered (and thus would not be located in public records). Consistent with our argument regarding left truncation, our results showed that investigators would over estimate new ventures' survival probabilities if they failed to take account of over-selecting hardy new ventures. Our results regarding the effects of not controlling for left truncation suggest not only that the magnitude of the explanatory variables' effects would be biased but also that the significance of the effects would be mistakenly estimated. Our study is the first one to provide evidence showing the biases caused by using registration data and left-truncated data in studying new ventures' failure rate.

Third, we provide a statistical solution to left truncation. Entrepreneurship scholars have been searching for ways to fix the bias created by left-truncated data, and we have identified a powerful analytical strategy that has rarely been applied. Our proposed statistical solution to left truncation is widely available and can easily be implemented in many standard statistical packages. Were investigators to use this technique, much of the mystery regarding emerging firms' failure rates would be cleared up. Accordingly, entrepreneurship researchers could have more confidence in the strategies they recommend to entrepreneurs starting new ventures.

2. Introduction

Since the 1980s, research on new venture creation has shown that new businesses create jobs, improve economies, increase social mobility, facilitate innovation, and generate other benefits (Aldrich and Ruef, 2006; Delacroix and Solt, 1988; Gibb, 2000; Ruef et al., 2003; Shane, 2008; Sorensen, 2007). Realizing that these important functions can be achieved only if promising ventures survive, scholars have investigated failure rates of newly created ventures and have commented on the high portion of new ventures that fail before they become established organizations (Brüderl et al., 1992; Brush et al., 2008; Gimeno et al., 1997). Despite numerous investigations, findings from previous studies have been inconclusive regarding not only the patterns but also the magnitude of failure rates (Hannan et al., 1998; Levie et al., 2011). Whereas some scholars have insisted that new ventures suffer a substantial failure rate (Audretsch and Talat, 1995; Wiklund et al., 2010), others have contended that failure rates have been exaggerated (Gibb, 2000; Levie et al., 2011; Watson and Everett, 1993). Reconciling these views is difficult because past studies often used data that excluded a significant fraction of emerging ventures and did not take into account ventures that were initiated prior to when a study began observing them. The problems were not easily discerned and thus remained out of sight, suggesting to us that a more vigorous effort was needed to call the field's attention to them.

In this paper, we examine several concerns that arise in studying new ventures' failure rates. First, we identify a number of theoretical issues whose resolution would be enhanced by a better understanding of rates of failure among emerging ventures. Second, we consider the representativeness of previous studies' sampling frames and explain how their sampling frames introduced bias by over-selecting hardy new ventures. Third, we explore the extent to which another methodological issue, left truncation, leads to underestimates of failure rates. We argue that this occurs even though some studies attempted to track new ventures from their very early stages. Fourth, we provide evidence for the magnitude of the biases introduced by the two problems we have identified, using a representative sample of emerging organizations from the Panel Studies of Entrepreneurial Dynamics II (PSED II). Fifth, we provide a solution to left truncation and identify the circumstances under which it could be applied. We conclude the paper with implications for future research designs.

3. Theoretical issues: the process of organizational emergence and its outcomes

Beginning with a few pioneering articles in the 1960s, organization theorists have noted the dynamic relationship between organizations and their environments (Buckley, 1967; Campbell, 1969). These early efforts emphasized the contingent nature of organizational characteristics, positing that their features depended upon conditions in their environments. Explanations focused on organizations adapting to their contexts, implicitly assuming that most organizations would somehow muddle through, although perhaps at the cost of reduced performance. In the 1970s, however, organizational ecologists and others began calling attention to the problematic nature of adaptation for many organizations (Aldrich, 1979). Theorists began offering models in which a high proportion of organizations struggled to survive selection forces and many ultimately ceased to exist.

We have identified three issues raised by this literature, as applied to the study of entrepreneurship and emerging ventures. First, at the most general level, organization and entrepreneurship theory has pursued two lines of inquiry with regard to adaptation and survival among new ventures. One line of inquiry, typified by organizational ecology, posits that selection pressures are so severe that few, if any, organizations are capable of adapting to them (Aldrich and Ruef, 2006). A second line of inquiry, typified by organizational learning theory, posits that some nascent entrepreneurs are capable of learning from previous experience and feedback obtained from initial organizing efforts (Powell and Colyvas, 2008). Addressing the conundrum posed by these arguments, we argue that the most opportune time to observe the role of founders and founding teams is during their very early days, before other people have begun to play major roles (Katz and Gartner, 1988).

Second, the heterogeneity of organizational forms and routines is probably greatest at the moment when new ventures are initiated and thus investigators need to find them as early as possible. Founders undertaking radically innovative activities usually put themselves at substantial risk. The consequences of founders' risk-taking activities for startups' survival will be overlooked if researchers do not begin studying them in the months immediately following their initiation. By observing which new organizational forms and routines survive, investigators can assess the nature and strength of selection forces before isomorphic pressures have eliminated the most innovative. From a population standpoint, chances for the expression of commercially valuable creativity depend upon what founders do to keep their emerging organizations alive until they can build a stable platform and find a place in the market.

Third, early choices made by founders in conjunction with strong external selection forces can leave a lasting imprint on the structures and practices of new ventures. The early months are the time in a business's life when its basic business platform develops and becomes the potential bedrock on which subsequent practices will be built (Davidsson and Klofsten, 2003). Later adaptation may only be possible if a solid foundation is laid at the beginning. Indeed, the early foundation may create path dependence in the sense that some kinds of subsequent adaptations are ruled out. Many investigators have studied the impact of *initial* founding conditions on an organizations' later development, finding imprinting effects with regard to employment systems (Burton, 1995), managerial turnover (Burton and Beckman, 2007), organizational culture (Johnson, 2007), level of financing (Beckman et al., 2007), and financial performance (Stuart and Sorenson, 2003). Understanding how populations evolve requires studying the promising paths that were available early on but then subsequently closed.

The three critical issues we have identified, as well as many others, focus explicitly on the survival of emerging ventures in the weeks and months after founders have begun their initial activities. Despite strong arguments for investing in research designs and statistical methods that enable investigators to empirically pursue these issues, researchers have been slow to do so. In this paper, we identify some major methodological issues that have compromised the value of research on emerging organizations and offer some promising solutions.

4. Inconclusive findings from previous studies

Research on organizational survival has used multiple phrases to describe the event of interest: firm exit, failure, death, termination, quitting, and so on. In our paper, we use "termination" to describe the status of organizations at the end of their life course, meaning that these social entities no longer exist. We use "failure rate" or "hazard rate of failure" as the statistical terminology to describe organizations' transition rates to termination. We review previous findings on organizational survival in terms of survival probabilities and hazard rates. Researchers have often confused the terms, even though the two statistics have different functions. To be clear, "survival probability" is defined as the probability that a subject (a new venture) survives longer than time t. It is the *cumulative* distribution function, indicating the percentage of a sample surviving longer than time *t*. Related to the survival probability but having a very different meaning, the hazard rate of failure is the *instantaneous* failure rate. It indicates the approximate probability that a new venture is terminated in the next time unit, conditional on its probability of surviving to time t. Given this definition, the hazard rate is an *instantaneous age-specific* failure rate (Allison, 2010; Tuma and Hannan, 1984).

4.1. Survival probability

Previous findings on new ventures' survival probabilities have been highly divergent. Several studies reported a survival probability of 0.2 in the fifth year, i.e. they estimated that 80% of the new ventures they sampled died within 5 years (Dickinson, 1981; Nystrom and Starbuck, 1981). Relatively higher fifth-year survival probabilities, between 0.4 and 0.55, were found in several other studies. Using a sample of 11,000 firms established in 1976, Audretsch (1991) found that about 75% of new firms survived more than two years but less than 50% survived more than six years. Comparing enterprises in 21 OECD countries in 2005, Levie et al. (2011) showed that in the fifth year, the average survival probability of new ventures from all countries was about 0.52.

Some studies found even higher survival probabilities around the fifth year. Using a sample of 1849 business founders in Germany, Brüderl et al. (1992) reported a life table in which 75% of businesses survived more than two years and 63% survived more than five years. Romanelli (1989) analyzed longitudinal data on startups in the minicomputer industry and found that about 67% of 103 firms survived more than five years. Using data from the federal individual income tax returns of people who received inheritances, Holtz-Eakin et al. (1994) found about 72% of the enterprises alive in 1981 still existed four years later. Analyzing a sample of firms in Sweden from three cohorts (registered in 1994, 1995, and 1996), Wiklund et al. (2010) found that almost two thirds of firms were still alive in early 2004.

4.2. Hazard rate of failure

Results regarding the hazard rate of failure are even more diverse than for survival probabilities. Using a life-table estimator, Brüderl et al. (1992) found the hazard rate for new ventures in Germany was about 0.0075 per month initially, then increased to 0.0175 in the 10th month, and decreased monotonically afterwards. Using the same data, Brüderl and Schussler (1990) found a similar inversed U-shaped pattern of hazard rates, with the highest hazard rate at 0.023 per month for simple tradesmen.

Some scholars have reported age-specific death rates from parametric models. Singh et al. (1986) predicted the death rate of voluntary social service organizations using a Gompertz model. The hazard rate they found was 0.052 in the initial year, and 0.037 in the fifth year and 0.027 in the 10th year. Applying a Makeham model, which is an extension of the Gompertz model, Freeman et al. (1983) found an asymptotic death rate of about 0.012 for National Labor Union organizations, 0.065 for semiconductor manufacturing firms, and 0.024 for newspaper publishing organizations. In a comparative study, Carroll (1983) reanalyzed 52 data sets using three models: constant rate, Gompertz, and Makeham. Most of his findings supported the monotonically decreasing pattern of death rates. However, there was huge variation in the size of age-specific death rates across the different data sets.

Several studies reported cumulative (integrated) hazard rates, which is the accumulation of hazard rates over time, ranging from zero to infinity. Freeman et al. (1983) reported cumulative hazard rates for three populations of organizations. Around the 20th year, the cumulative hazard rate was about 0.7 for National Labor Union organizations, 1.4 for local newspaper firms, and about 2 for semiconductor manufacturing firms. For all three populations, the cumulative hazard rate increased considerably after the 20th year. By contrast, Halliday et al. (1987) found the 20th year cumulative hazard rate for state bar associations was about 1 and there was almost no increase after the 20th year.

Our review shows that previous studies have reported a wide range of estimates for both survival probabilities and hazard rates of failure. The fifth-year survival probabilities reported in previous studies vary from 0.2 to 0.7, and hazard rate estimates differ by orders of magnitude across studies, with some two or three times larger than others. Some of these diverse results might be due to substantive heterogeneity among new ventures in different periods, industries, geographies, countries, and other social contexts (Aldrich, 2009). However, we believe these divergent results partially stem from methodological problems that have systematically biased previous findings.[2] In the next section, we identify and explain a number of methodological problems that have produced such discrepant results: incomplete conceptualization of what "emergence" means and thus incomplete coverage of a new venture's lifespan, definitions of "new" too closely wedded to what is administratively available, and failure to use statistical procedures that correct for left truncation bias.

5. Emerging organizations and representative samples

Studies in entrepreneurship should use representative samples if researchers intend to apply inferences from their results to a target population (Aldrich et al., 1989; Kalleberg et al., 1990; Short et al., 2010). Researchers have demonstrated that both internal validity and external validity are threatened if a sample is not representative (Berk, 1983; Denrell, 2003; Denrell and Kovacs, 2008). Studying the theoretical issues of organizational emergence and the risks of failure requires a suitable design that follows entrepreneurs throughout the entire founding process. Accordingly, we need a theoretical construct that takes account of the intrinsic dynamic property of emerging organization, and allows us to identify organizations from an early stage. Based on Katz and Gartner's (1988) definition of "emerging organizations", we define the target population as the population of social entities which are still going through the organizing process on their way to becoming established firms. Obtaining a representative sample of emerging organizations poses considerable challenges to sample selection.

5.1. Are samples of registration data on emerging organizations representative?

Most investigators who wish to study emerging organizations have used registration data that are collected for regulation or businesses purposes. Because these registration data are designed to collect information on new ventures that have achieved a certain level of performance, very few registration data sets allow investigators to create a representative sample of emerging organizations (Aldrich et al., 1989; Kalleberg, et al., 1990). According to Reynolds and Curtin's (2009) review, only 7 out of 26 relevant data sets for research on entrepreneurship provided longitudinal information on new venture creation. We found that none of the 7 data sets applied selection criteria that would lead to a representative sample of emerging organizations. To a large extent, these registration data applied selection criteria that are strongly correlated with emerging organizations' survival and performance.

Some data sets were designed to examine innovative firms, and thus intentionally excluded less innovative ones. An example is the Longitudinal Research Database (LRD) maintained by the U.S. Census Bureau. This data set oversamples companies with sizable numbers of employees. Furthermore, firms which did not report any research and development activity were removed from the sample (LRD, 2011). Unfortunately, these less innovative firms might be the ones which were most constrained by a lack of financial resources and human capital and thus more likely to be terminated than the innovative firms.

In other data sets, researchers began with the intention of including small businesses, but their samples mostly consist of businesses hiring at least one employee. Data from the Business Employment Dynamics (BED), maintained by the Bureau of

[2] Schmidt (2010) argued that empirical results can be inconsistent with each other because of random errors. In this paper, we address the methodological problems that *systematically* bias the estimates of population parameters.

Labor Statistics, were derived from quarterly reports submitted by private sector employers. The data set was designed to reveal information on employment dynamics – job loss and job gains – at established businesses. BED explicitly excluded self-employed workers, most agricultural workers, businesses without any employees, and so on (BED, 2011). Although a few data sets such as the Integrated Longitudinal Business-level Database (ILBD) attempt to include non-employers, they rely on payroll tax records, corporate and individual income tax returns, or applications for an Employer Identification Number (EID) to identify new ventures without any employees (ILBD, 2011). Accordingly, new ventures which did not report tax returns or apply for an EID are excluded. By excluding new ventures that do not meet their sample inclusion criteria, the samples of LRD, BED, and ILBE are no longer random probability samples of new ventures because the selection probability for certain firms is known to be zero.

Unlike the above data sets that use only a few sources to identify new ventures, the Dun and Bradstreet (D&B) data set identifies new firms by compiling records from about ten different sources, including direct investigation, payment data, government registries, U.S. public records, company financial information, courts and legal filings, telephone directories, diversity data, web sources, and news and media sources (D&B, 2011). Even with such extensive coverage, however, new ventures which were registered with D&B still tend to have significantly higher survival probability than the ones not registered, as we will show in this paper.

Note that selection bias from using registration data might vary across countries, depending on institutional conditions. While new ventures in the U.S. might operate for months or years before registering with a government agency, new ventures in North European countries are required to register at a very early stage. For example, only 26% of new ventures in the U.S. were formally registered with an appropriate government agency (Reynolds and Curtin, 2009). By contrast, Delmar and Shane (2004) found that legitimating activities involving government were the first step in entrepreneurial activities for most new ventures in Sweden. This contrast suggests that registration data may provide more extensive coverage when founders are required to register with an appropriate government agency before taking action.

5.2. Identifying emerging organizations by locating entrepreneurs

Realizing the problems of selective sampling in registration data, a group of researchers from several dozen universities, led by Reynolds, attempted to identify emerging organizations by locating nascent entrepreneurs involved in creating new ventures (Gartner et al., 2004a). They designed elaborate screening interviews to identify nascent entrepreneurs based on their entrepreneurial activities. Their research design was explicitly intended to follow nascent entrepreneurs from a very early time to the point when new ventures were terminated or disbanded (Aldrich, 2001; Gartner et al., 2004b; Reynolds and Curtin, 2009; Van de Ven and Engleman, 2004). The resulting two data sets are PSED I and PSED II. By March 2011, the two data sets had been widely used in research on entrepreneurship, including more than *two hundred* articles, books, reports, and theses. We provide a timely assessment of PSED by addressing the extent to which their samples of emerging organizations are representative.

5.2.1. A nascent entrepreneur as the observation unit

In the research design of the Panel Study of Entrepreneurial Dynamics II (PSED II), a nascent entrepreneur – an owner of a new venture – serves as the sampling unit and the observation unit. Whereas the unit of observation is the element on which we collect information, the unit of sampling is the element for sample selection (Kish, 1965; Lohr, 2009). In PSED II, respondents are qualified as a nascent entrepreneur if they met four criteria: general criteria of entrepreneurial status, behavior criteria, ownership criteria, and profit criteria (see Appendix A in Reynolds and Curtin, 2009 for detailed information on the research design). First, three general qualification questions were asked to identify people who were creating a new business: (1) Are you, alone or with others, currently trying to start a new business, including any self-employment or selling any goods or services to others? (2) Are you, alone or with others, currently trying to start a new business or new venture for your employer, an effort that is part of your normal work? (3) Are you, alone or with others, currently the owner of a business you help manage, including self-employment or selling any goods or services to others?

If respondents said “yes” to at least one of the three questions, PSED II then applied the behavior criterion by asking them if they took any action in the past 12 months. If the behavior criterion were met, then they were asked questions on ownership in order to screen owners. The final criterion is about profitability, which is used to differentiate fully established firms and emerging organizations. In order to have a sample of *emerging* organizations, PSED II excluded fully established firms which had achieved stable positive cash flow for more than six months in the past twelve months.[3] By using the four criteria, PSED II improved upon previous research designs by identifying a sample of valid nascent entrepreneurs who are active in business creation but have not yet created a profitable firm (Reynolds and Curtin, 2009). In particular, PSED II reduced sample selection bias by including new ventures which would not be located by registration data.

[3] As their Table 1 shows (Reynolds and Curtin, 2009: 309), the profit criterion reduces the number of eligible new ventures from 3029 (9.5% of full sample) to 2393 (7.5% of full sample). So 20% of the 3029 new ventures which meet the other three selection criteria were excluded by further applying the profit criteria. There is unavoidable arbitrariness in deciding how much revenue a new venture should obtain in order to become an established firm. One might argue that the highly profitable “entities” defined by PSED II are still new ventures, instead of established firms. Indeed, by the 4th annual follow-up interview, only 20% of new ventures in the sample have become established firms based on this profit criterion. We think this criterion is already a high standard in defining an “established firm.”

5.2.2. A new venture as the sampling unit

Using a nascent entrepreneur as the sampling unit, a representative sample of nascent entrepreneurs can be obtained by applying sampling weights. The final weights for the PSED II sample were developed following three steps: survey weights were developed to adjust non-coverage and non-response; after screening interviews were conducted, post-stratification weights were developed given respondents' information on income, sex, age, and race; and the final weights were obtained by multiplying survey weights and post-stratification weights, $\mathrm{WT_{respondent}} = \mathrm{WT_{survey}} * \mathrm{WT_{post-strat}}$. If we apply the final weights, the sample of PSED II is a representative sample of nascent entrepreneurs in the U.S. – the owners of emerging organizations – and the unit of sampling is a nascent entrepreneur.

For analyses of new ventures, the appropriate unit of analysis should be a new venture. Whenever the selection probability for a nascent entrepreneur is not equal to the selection probability for a new venture, we need to adjust the sample weights. As Ruef (2010) noted, a consequence of the sampling in PSED II is that new ventures with more owners are over-represented. In repeated probability sampling using an individual owner as the unit of sampling, the probability of selection for a new venture with *n* owners is *n* times of the probability for a new venture with a solo owner. Given that the sampling weight is the inverse of the selection probability, the sample weights for a new venture with *n* owners should be one *n*th (n is the number of owners for a new ventures) of the weight for a new venture with a solo owner. Thus, conditional on the final weights developed to achieve a representative sample of nascent entrepreneurs, when the unit of analysis is a new venture we need to further adjust the weights by multiplying by the inverse of the number of owners. Accordingly, the final adjusted weights for new ventures are: $\mathrm{WT_{venture}} = \mathrm{WT_{survey}} * \mathrm{WT_{post-strat}} * \frac{1}{\mathrm{Teamsize}}$.

5.3. What is left truncation and why does it happen?

Ideally, we want to follow startups from the conception date when the business creation process was initially launched, because emerging organizations are at risk of *experiencing termination* the instant after the organizing process begins. To conduct this ideal design, researchers have to either follow individuals from the pre-startup period on a weekly or monthly basis to observe when these individuals begin creating new ventures or only include the emerging organizations that were initiated in the *same* week or month as the screening interview. The tremendous financial cost and time needed for this ideal design make it impossible to carry out in practice. A more feasible approach is the one used in PSED II: include emerging organizations in the sample as long as they have not evolved into established firms and their owners are still active in entrepreneurial activities, even though they were initiated prior to *when* the observation started. Some were initiated only days before, whereas others had been underway for months.

In the PSEDII, emerging organizations have been exposed to the risk of termination before they enter into the observation and thus the sample is characterized by left truncation. To formally define left truncation, let T_i be a continuous variable representing the duration time that a subject of interest has been exposed to the risk of an event, t_i be the observed duration time that a subject of interest has been exposed to the risk of an event, and u_i be the duration time that a subject of interest has been exposed to the risk of an event at the sample selection time. To be eligible for a prospective study which intends to observe the occurrence of the event, we require that subjects are at risk of exposure to the event at the time of selection and thus we have $t_i \geq u_i$ and $u_i > 0$. For a subject which has been exposed to the risk and is currently eligible for the study, its duration time T_i is left truncated at the sample selection time u_i. Whereas the original hazard function is $\Pr(T_i = t_i | T_i > t_i)$ where $t_i > 0$, the hazard function for a left-truncated T_i is $\Pr(T_i = t_i | T_i > t_i, T_i \geq u_i) = \Pr(T_i = t_i | T_i > t_i)$ where $t_i \geq u_i$ (Lawless, 2003: 67).

Left truncation was built into PSEDII's design, given that it included nascent entrepreneurs who had initiated their startups before they were selected at the date of the screening interview. This method of selection means that all of startups had already been exposed to the risk of termination for a certain period (depending on when they were initiated) when they came under observation. Using this research design, it is logically impossible for any nascent entrepreneurs selected by PSED to have terminated their startups before the date of the screening interview.

We illustrate this problem in Fig. 1, which shows a research project designed to study emerging ventures when the startup process has already been underway for some time. The horizontal axis indicates calendar time. T3 is the beginning of the study's observation period and T4 is the ending time. T1 is the start time of being at risk for startup P1, T2 is the start time of being at risk for startup P2, and T3 is the start time of being at risk for startups P3 and P4. P1 and P2 begin at different points in calendar time than P3 and P4. The pre-observation period is between a new venture's start time (origin time) of being at risk and the beginning time of the observation. It is clear that the length of the pre-observation period varies for these new ventures, depending on their own start time. It is possible that some of the four new ventures which were selected into this sample were begun at the same time as other attempts that did not survive long enough to enter into the observation period. For example, P1* began at the same point as P1 and P2* began at the same point as P2. However, neither P1* nor P2* survived long enough to reach time T3, when the observation starts.

The scenario depicted in Fig. 1 illustrates that in a prospective study of new ventures' termination, left truncation occurs when the observation period starts after new ventures have already been exposed to the risk of termination. Left-truncated data are incomplete and include hardy subjects. The longer the duration period that startups have been exposed to the risk of termination at the sample selection time, the greater the possibility that they over-represent the resilient cases. By contrast, the most fragile cases were quickly selected out and we would never observe them. For example, of the four startups, P1 is likely to lose the most startups of the same age because the number of terminated startups *monotonically* increases over time. As a result, if we estimate

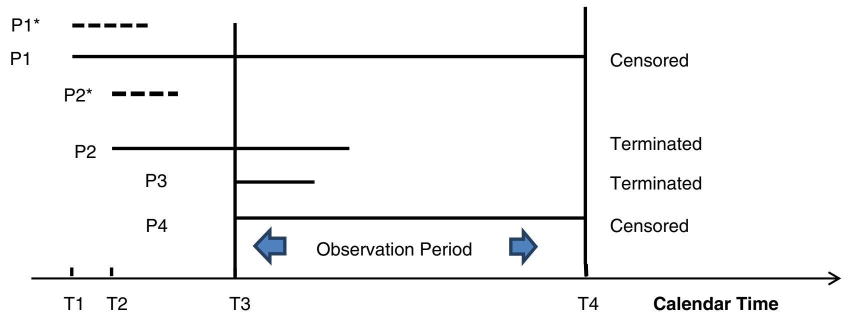

Fig. 1. Left-truncated data with known origin-time.
This Figure is adapted from Figure 1 in Guo (1993).

the survival function of startups without controlling for left truncation, we will over-estimate the survival probability because we leave out the cases terminated earlier than when our observation started.

To further clarify left truncation, we differentiate truncation from censoring, and explain their major differences. While reviewing previous literature, we found that scholars often confused *truncation* and *censoring*, mistakenly treating *left truncation* as *left-censoring*.[4] In fact, censoring means that an outcome or an event of our interest either has happened before the observation period starts or will happen after the observation period ends. In other words, censoring occurs when investigators do not observe the event during a study's observation period. Right-censoring occurs in longitudinal data sets on new ventures because scholars usually end their follow-up studies before they observe the outcome for all new ventures. Left-censoring refers to events that happened before the study got underway. Unlike left truncation and right-censoring, left-censoring can *only* happen for repeated events. If a subject has already experienced an event, the event has to have a likelihood of being repeated to make the subject eligible to be selected in a study in which that event is the outcome variable. For unrepeated events, such as new venture failures, once they occur, they entirely remove the possibility of a new venture being subsequently selected into a sample. Following this logic, PSEDII does not have left-censored new ventures when the outcome of interest is a new venture's failure. However, it does have information on events which in principle could be left censored, such as obtaining outside funding, as such events can occur before the observation period begins and are also repeatable events.

5.4. How can we control for left truncation?

Most research on the survival of startups has failed to take left truncation into account, let alone control for the problem (Hannan et al., 1998). However, we noted a few exceptions and the various ways they had tried to control for left truncation: (1) cases were only included if they were observed completely (Allison, 1984); (2) cases were only included for new ventures started within a short period prior to their observation, such as the previous 9 months (Delmar and Shane, 2003; Delmar and Shane, 2004; Lichtenstein et al., 2007); (3) a dummy variable was included in models to control for the effects of existence prior to observation period (Haveman, 1992; Haveman and Cohen, 1994; Hiatt et al., 2009); (4) an organization's age was used as a control variable (Gimeno et al., 1997); (5) a piecewise exponential survival model was applied to control for variations in order to reduce bias (Guo, 1993; Ruef and Scott, 1998).

However, these approaches are problematic for several reasons, and there is no statistical evidence that they are effective. In particular, if we only use cases for which we have complete observations, we might throw away a large number of cases. Accordingly, sampling errors will increase and coefficients will be less likely to be significant. Dealing with the problem by including a new venture's age is also problematic, as it could not be applied to descriptive statistics or the Cox Proportional Model in which age itself is the dependent variable. In fact, statisticians have developed more advanced statistical procedures to control for left truncation, but they have rarely been applied in entrepreneurship research.

With left-truncated data, if the start time of being at risk is unknown, we face the problem of a biased estimation of the hazard of failure because the full length of exposure to risk is unknown. For example, when researchers used registration data to study organizational termination, they mistakenly treated a registration date as the start times of an organization's life course. The actual length of time that organizations were exposed to the risk of termination before they were registered was unknown. However, if we know the actual start time of exposure to the risk of termination, we can handle left truncation with the Conditional Likelihood Approach (Allison, 2010; Guo, 1993). Fortunately, PSED II collected timing information on about fifty activities, including making a plan, marketing, getting funding, hiring employees, developing products, and so on. Using the information on these activities, we can operationalize the start time for an emerging organization.

[4] Left truncation and left censoring were treated the same when organizational ecologists first introduced them to the field of organizations (Tuma and Hannan, 1984). But in their later works, Hannan et al. (1998) explicitly used “truncation” to refer the “late-entry” problem arising when subjects only come under observation after they have already been exposed to the risk of death for some time. We differentiate the two concepts to reduce confusion as much as possible.

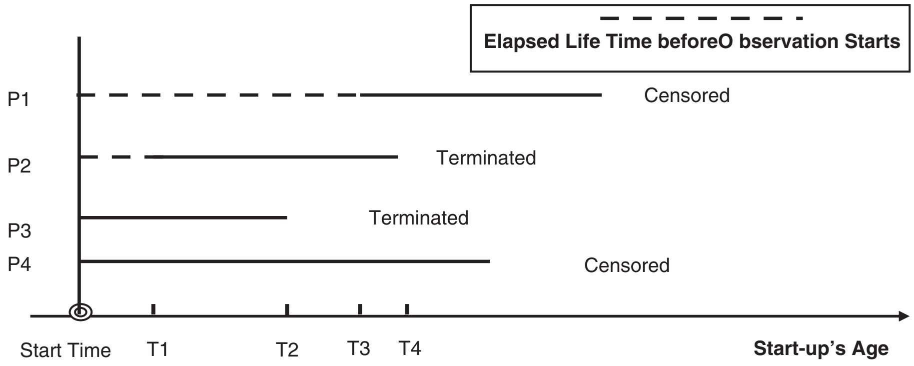

Fig. 2. Risk set for left-truncated data.
This Figure is adapted from Figure 3 in Guo (1993).

We explain how to appropriately estimate the hazard of termination using Condition Likelihood Approach in the Cox Proportional Hazard model, given that the panel data are left-truncated but the start times are known. Statisticians have convincingly demonstrated that this approach can effectively correct for left truncation and maximally use information on the termination events observed during a study (Guo, 1993; Howard et al., 2006; Lamarca et al., 1998; Tsai et al., 1987). This approach has been incorporated into widely used statistical programs, including SAS, STATA, and S-plus. The simple idea behind the conditional likelihood approach is that it excludes subjects from the risk set when they have yet not entered into our observation (Guo, 1993; Lyness and Judiesch, 2001). To put it positively, it only includes subjects in the risk set when we began actually observing them. To apply this approach, we need to specify the length of time that subjects existed before their entry into the observation. In an analysis of startups' survival using PSED II, the duration of the pre-entry period is equal to the age of a startup at the date of screening interview.

We give an example in Fig. 2. The horizontal axis is the age of emerging organizations rather than calendar time. The length of the dashed line indicates a startup's age at the date of screening interview. P1 is much older than other startups when it enters into the observation. It does not enter into the observation until its period of being at risk reaches T3. When case P3 is terminated at the T2 point, the risk set of efforts that could have been terminated at the T2 point is (P2 P3 P4). P1 is excluded from the risk set at time T2 because it has not entered into the observation yet; when case P2 is terminated at time point T4, the risk set at T4 is (P1 P2 P4) because P1 has been under observation since T3 and P3 is terminated before T4.

6. Empirical evidence from PSED II

We use data from PSED II to demonstrate how findings regarding new ventures' failure would be biased if we used registration data or left-truncated data. PSED II started in 2005 with the selection of 1214 nascent entrepreneurs based on screening a sample of 31,845 adults in the U.S. Once the sample was selected, PSED II followed respondents until they reported that they were disengaged from the startup effort. So far, PSED II has conducted six waves of interviews: the initial detailed interview and five annual follow-up interviews.

6.1. Operationalizing the start time and the end time of organizational emergence

To define an emerging organization's age (the length of the gestation period), we need to be clear about how to operationalize the start time and the end time of the gestation period. To determine the date of termination, we first evaluate a respondent's entrepreneurial status: “do you consider yourself to be actively involved with the new business (startup) or disengaged from it?” If the answer is “disengaged,” we further use the question of “are there any other people still involved?” to differentiate business termination from individuals' exits. Unlike the diverse routes of new firm exits found in previous studies (Bates, 2005; Wennberg et al., 2010), the types of new venture exits revealed by PSED II are quite homogeneous: 87% of 648 entrepreneurial exits overlap with new venture exits, meaning that only 13% of existing respondents reported that there were still other people involved in creating the new businesses. Also importantly, only 7 of the 648 exiting entrepreneurs reported that their startups were sold. For terminated businesses, we obtain the termination time from the question of “in what month and year did you end your active role in working on this business startup?” When individual respondents exit, the organizing effort may continue, but we treated such cases as right-censored at the point of the respondents' exit because the respondents who provided information on their businesses were no longer in the sample. Previous studies have shown that such *non-informative censoring* does not bias estimates of the survival probability and the hazard rate (Allison, 2010; Collett, 2003; Tuma and Hannan, 1984).

As Davidsson and Gordon (2011) noted, it is difficult to define the start time of a gestation period, given the complexity of the venture creation process. Although scholars have agreed that the start time should be defined by entrepreneurial action rather than by entrepreneurs' thoughts about creating a new business, there is little agreement on which activity or set of activities is most appropriate (Reynolds and Miller, 1992). For example, several scholars used the month of the first activity as the start time. Delmar and Shane (2003; 2004) defined the start time as the time when the first activity was undertaken and also restricted their sample by only including new ventures started within the 9 months prior to the time when their observation began. Similarly, Lichtenstein et al. (2007) restricted their sample by only including new ventures started within 24 months prior to the time when their observation began.

Reynolds and his colleagues provided several promising solutions to defining a starting time. In an early paper, given results from an analysis of more than 3000 established firm, Reynolds and Miller (1992) suggested that the date of first sale might be an appropriate indicator of a new venture's "birth." Later on, in an initial assessment of PSED I, Reynolds suggested defining the starting time as the time of "the earliest activity in any pair where both were initiated within a 12 month" (Reynolds, 2007: 113). His argument was that the real start time of a gestation period involves intensive entrepreneurial efforts.

We made our decision on operationalizing the "start time" of organizational emergence by assessing the concerns reported by scholars regarding model specifications and the screening procedure used in PSEDII. For Delmar and Shane (2003), their major concern with including new firms that were started more than 9 months prior to observation time was that nascent entrepreneurs might not be able to accurately recall information on activities, especially regarding the activity of making a plan, which they planned to use a key independent variable. However, by definition, the length of the pre-observation period is part of a new venture's gestation period. If we exclude new ventures based on the length of their pre-observation period, we actually introduce bias by sampling new ventures *given the dependent variable*, a new venture's survival time. Furthermore, eliminating efforts older than 9 months might be very conservative, as it excludes about 60% of new ventures in the PSED II. Indeed, we believe that entrepreneurial activities, such as making a plan and receiving revenue for the first time, are important events for new ventures. Although there might be measurement errors in timing information on activities, we believe nascent entrepreneurs should be able to provide information on activities that happened within the past several years.

For Reynolds and others, intense effort is a required element for a genuine nascent entrepreneur and thus respondents who only undertake one activity a year might not be serious about creating a new business (Reynolds and Curtin, 2007; Reynolds and Miller, 1992; Schoonhoven et al., 2009). However, we view diversity in the intensity of activities across nascent entrepreneurs as crucial information. In fact, using a sample of 1547 entrepreneurs in the U.S., Gimeno et al. (1997) demonstrated that there is a large number of underperforming but strongly persistent firms. Such under performers but strong survivors might be created by nascent entrepreneurs who are reluctant to undertake additional efforts. Furthermore, as Schoonhoven et al. (2009) suggested, some entrepreneurial activities included in generic research designs might not be applicable for certain kinds of new ventures. A deletion of new ventures based on the density of entrepreneurial activities covered in a research design might result in excluding valid new ventures. Thus, we argue that as long as all the new ventures were qualified by well-developed selection criteria, there is no reason to exclude respondents based on the intensity of their efforts.

Given the above reasoning, we define the first activity – carried out after nascent entrepreneurs first thought about starting a new business – as the start time of new venture gestation. Unlike previous researchers who deleted cases that had been in gestation longer than an arbitrarily chosen period, such as 24 months or less prior to observation, we take a more inclusive approach. We include any new ventures whose gestation period started within the previous 120 months (ten years) prior to our observation. By this criterion, 1100 new ventures (90%) in the sample of PSED II were included in our analysis. Among the 1100 new ventures included in our analysis, 25% were begun in the previous 5 months of the screening interview date, 50% in the previous 12 months, 82% in the previous 36 months, and 95% in the previous 72 months. Probably because new ventures initiated 72 months prior to the screening interview only constitute 5% of the sample, and the survival probability remains almost the same after the new venture organizing efforts reach the 80th month (meaning that new ventures were initiated about 30 months prior to the initial screening interview), we found that restricting the sample at cut points of 70 months, 80 months, 100 months, 120 months does not substantially change our results.

6.2. Does it make a difference when using registration data?

PSED II collected information on whether and when a new business has been registered with Dun and Bradstreet, registered with an appropriate government agency, or joined a trade or industry association. These questions were asked of respondents every year until they reported that a new business had been registered with each organization or group, after which they were no longer asked. Regarding the time of registration, not only the year but also the month was obtained. By the final interview wave, the 4th follow-up interview, about 10% of new ventures in the sample had been registered with D&B, 20% had joined a trade or industry association, and about half had been registered with an appropriate government agency. In particular, the average age when a new venture was listed with D&B is about 62 months, 27 months when a new venture joined an association, and 18 months when a new venture was registered with a government agency. These pre-registration periods would not be included in new ventures' gestation times if researchers used registration data as the "birth date" of a new venture.

In Fig. 3, we report the Kaplan–Meier estimate of the survival function, contrasting unregistered ventures and new ventures which were registered in any of the three ways: D&B listing, government agency, or trade association. To demonstrate how results would be biased if one uses registration data, in constructing this figure we intentionally treated "*registration date*" as the starting time of gestation period for registered ventures. If a new venture were registered with more than one agency or group, we use the earliest registration date as the starting time. We also controlled for left truncation.

As indicated in Fig. 3, there is a considerable difference in survival probabilities between registered ventures and non-registered ventures over time. The survival probability for registered ventures is much higher than unregistered ventures throughout the life course. Indeed, our findings for the magnitude of the fifth year survival probability of registered ventures is

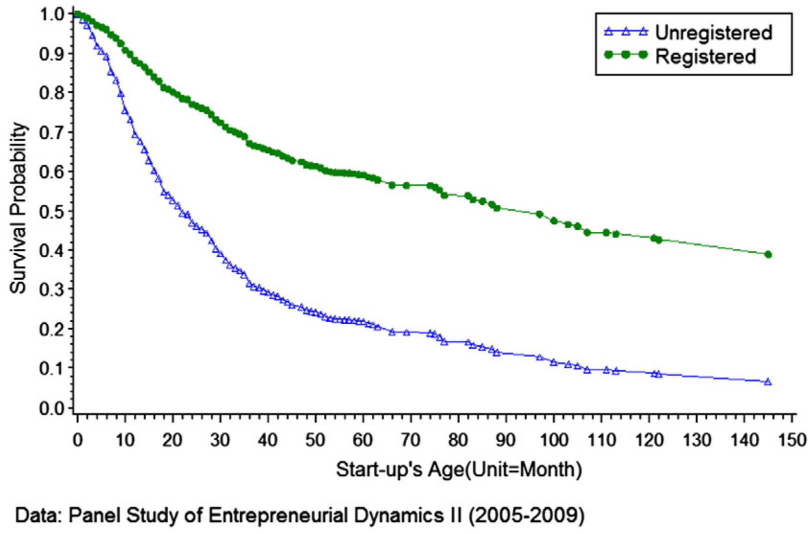

Fig. 3. Kaplan–Meier estimate of survival function given registration status.

close to that of several previous studies using registration data. We further conducted a Likelihood Score test in order to see whether there are significant differences between survival probabilities for the two groups. The Chi-square value of the Score test is 138.924, which is significant at the level of 0.0001 with one degree of freedom. It indicates that there is a statistically significant difference between the survival probabilities for registered ventures and unregistered new ventures over their life course.

6.3. Bias created by left-truncated data

New ventures which survive a longer time are over represented in left-truncated data. Accordingly, investigators would overestimate survival functions if they used left-truncated data. In Fig. 4, we show the contrast between the survival function estimated without controlling for left truncation versus the survival function estimated while controlling for left truncation. The difference between the two survival functions is considerable. Especially before an emerging venture's 40th month, the survival probability when controlling for left truncation decreases dramatically, whereas the survival probability changes very little when left truncation is not controlled.

We demonstrate how the coefficients of independent variables would be biased if we do not control for left truncation. Table 1 reports the descriptive results – means, standard deviations, medians, and the correlation coefficients – for all the variables that are included in our analysis. Table 2 reports the estimated coefficients, the standard errors of coefficients, and the P-values. The left side of Table 2 shows models results controlling for left truncation, whereas the right side of Table 2 shows models results

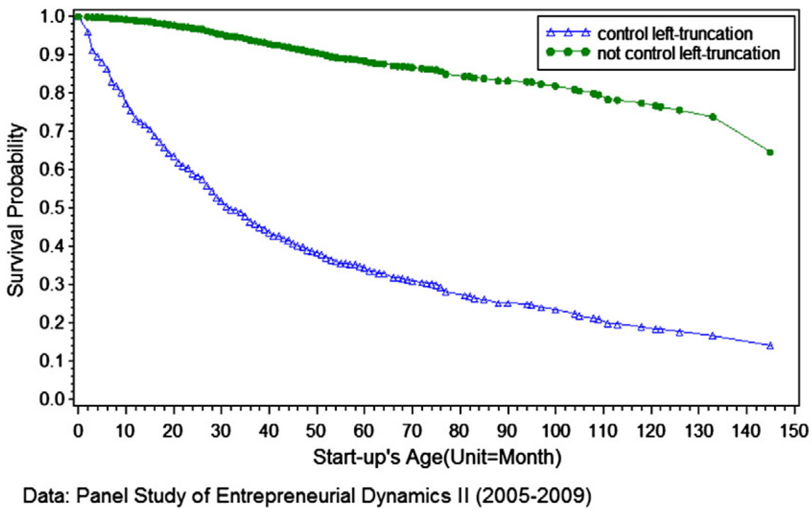

Fig. 4. Kaplan–Meier estimate of survival function of new ventures.

Table 1
Descriptive results from weighted data (N = 1100).

		Mean	Std	Median	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23
1	0<revenue<expense	0.26	0.43	0.00	1.00																						
2	Revenue>expense	0.24	0.42	0.00	−0.34	1.00																					
3	Revenue>expense +salary	0.13	0.32	0.00	−0.23	−0.21	1.00																				
4	Managerial exp	8.69	8.17	6.00	−0.04	0.03	0.07	1.00																			
5	Exp in ind of start-up	8.01	8.35	5.00	−0.03	0.01	0.06	0.36	1.00																		
6	No. of start-ups created	0.90	1.51	0.00	−0.01	0.03	0.02	0.29	0.07	1.00																	
7	No. of helper	0.97	1.90	0.00	−0.03	0.04	−0.02	−0.08	−0.07	−0.04	1.00																
8	No. of non-owner	1.03	1.86	0.00	0.00	−0.02	−0.02	0.02	−0.05	−0.01	0.02	1.00															
9	Joint experience	0.05	0.21	0.00	−0.02	−0.01	−0.02	−0.01	0.02	0.03	−0.05	−0.01	1.00														
10	Same household	0.17	0.37	0.00	0.02	0.04	−0.04	0.00	−0.08	−0.04	−0.05	−0.07	−0.10	1.00													
11	Listed in D&B	0.08	0.27	0.00	−0.01	0.03	0.15	0.07	0.02	0.06	−0.01	0.01	0.00	−0.05	1.00												
12	Team size	1.41	0.75	1.00	−0.03	−0.02	−0.04	0.02	−0.05	0.02	−0.05	−0.04	0.33	0.35	−0.02	1.00											
13	Teams having friend(s)	0.11	0.30	0.00	−0.05	−0.03	−0.05	−0.02	−0.03	0.03	−0.07	−0.05	0.63	−0.14	−0.01	0.52	1.00										
14	Family teams	0.20	0.39	0.00	0.00	0.04	−0.02	0.04	−0.05	−0.03	−0.05	−0.05	−0.11	0.89	−0.05	0.39	−0.17	1.00									
15	Average age	39.60	11.58	39.00	−0.01	−0.02	0.05	0.63	0.43	0.19	−0.13	−0.06	−0.01	0.02	0.06	−0.01	−0.03	0.03	1.00								
16	Age diversity	1.48	3.71	0.00	−0.02	−0.03	0.02	0.10	0.00	0.01	−0.07	−0.07	0.19	0.24	−0.03	0.49	0.34	0.38	0.09	1.00							
17	Gender diversity	0.10	0.20	0.00	0.00	0.02	−0.05	0.02	−0.07	−0.04	−0.06	−0.10	0.07	0.83	−0.04	0.53	0.14	0.77	0.02	0.33	1.00						
18	Race diversity	0.04	0.13	0.00	−0.01	−0.04	0.01	−0.03	−0.03	0.00	0.00	0.05	0.14	0.10	−0.01	0.36	0.24	0.10	−0.06	0.11	0.17	1.00					
19	Retail/wholesale	0.23	0.41	0.00	0.06	−0.05	0.01	0.01	−0.10	0.01	0.00	−0.03	−0.06	0.03	0.07	−0.01	−0.04	0.02	0.03	−0.01	0.00	−0.02	1.00				
20	Consumer	0.13	0.33	0.00	−0.06	0.08	0.01	−0.02	−0.08	−0.02	0.04	0.05	0.10	−0.11	0.03	0.01	0.08	−0.11	−0.10	0.00	−0.04	0.07	−0.22	1.00			
21	Business support	0.14	0.34	0.00	−0.01	0.03	0.00	0.02	0.10	0.01	−0.07	0.00	0.01	−0.02	0.03	−0.01	0.01	−0.02	0.06	−0.05	−0.02	−0.05	−0.22	−0.16	1.00		
22	Professional service	0.30	0.45	0.00	−0.01	−0.07	0.02	−0.04	−0.01	0.01	0.06	0.01	−0.03	0.01	−0.10	−0.01	−0.06	0.02	−0.04	0.00	0.00	0.05	−0.36	−0.26	−0.27	1.00	
23	Age at entry	23.22	22.98	15.00	−0.01	0.00	0.00	0.04	0.14	−0.04	0.03	−0.04	−0.03	0.04	0.03	0.03	−0.01	0.05	0.10	0.02	0.03	0.00	−0.03	−0.04	0.04	−0.02	1.00

Table 2
Results from Cox Proportional Model (Ties = Efron).

	Controlling for left-truncation									Not controlling for left-truncation								
	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
Parameter	Beta	S.E	P	Beta	S.E	P	Beta	S.E	P	Beta	S.E	P	Beta	S.E	P	Beta	S.E	P
Performance																		
0<revenue<expense							−0.49	0.12	<0.0001							−0.54	0.12	<0.0001
Revenue>expense							−0.61	0.12	<0.0001							−0.72	0.12	<0.0001
Revenue>expense + salary							−1.47	0.20	<0.0001							−1.98	0.19	<0.0001
Team characteristics																		
Managerial exp				0.00	0.01	0.552	0.00	0.01	0.956				0.00	0.01	0.593	0.00	0.01	0.902
Exp in ind of start-up				−0.02	0.01	0.002	−0.02	0.01	0.007				−0.03	0.01	<0.0001	−0.03	0.01	<0.0001
No. of start-ups created				−0.08	0.04	0.054	−0.05	0.04	0.178				−0.11	0.04	0.006	−0.08	0.04	0.048
No. of helper				0.02	0.02	0.360	0.02	0.02	0.484				0.02	0.02	0.331	0.02	0.02	0.381
No. of non-owner				0.01	0.03	0.609	0.01	0.03	0.715				0.00	0.03	0.876	−0.01	0.03	0.771
Joint experience				−0.31	0.29	0.292	−0.17	0.29	0.553				−0.18	0.30	0.534	−0.05	0.30	0.860
Same household				−0.04	0.36	0.905	−0.01	0.34	0.970				0.00	0.35	0.995	0.10	0.33	0.761
Listed in D&B	−0.93	0.23	<0.0001	−0.93	0.23	<0.0001	−0.76	0.23	0.001	−1.23	0.23	<0.0001	−1.24	0.23	<0.0001	−0.95	0.23	<0.0001
Control variables																		
Team size	0.06	0.09	0.539	0.06	0.09	0.470	0.07	0.09	0.423	0.06	0.08	0.446	0.06	0.08	0.444	0.08	0.08	0.321
Teams having friend(s)	0.08	0.21	0.706	0.23	0.24	0.330	0.05	0.24	0.836	0.13	0.21	0.523	0.25	0.24	0.296	−0.02	0.24	0.950
Family teams	−0.05	0.24	0.828	0.00	0.33	0.995	0.07	0.32	0.825	0.03	0.23	0.908	0.09	0.33	0.780	0.21	0.32	0.501
Average age	−0.01	0.00	0.186	0.00	0.00	0.365	0.00	0.00	0.886	−0.01	0.00	0.060	0.01	0.00	0.286	0.00	0.00	0.856
Age diversity	−0.02	0.02	0.304	−0.02	0.02	0.243	−0.02	0.02	0.237	−0.02	0.02	0.194	−0.03	0.02	0.122	−0.02	0.02	0.156
Gender diversity	−0.07	0.42	0.864	−0.16	0.48	0.745	−0.28	0.47	0.553	−0.08	0.41	0.851	−0.25	0.46	0.577	−0.66	0.46	0.147
Race diversity	−0.12	0.38	0.760	−0.14	0.38	0.719	−0.23	0.38	0.553	−0.43	0.38	0.252	−0.45	0.38	0.234	−0.36	0.38	0.355
Retail/wholesale	0.19	0.14	0.173	0.15	0.14	0.285	0.28	0.14	0.047	0.33	0.14	0.015	0.26	0.14	0.059	0.43	0.14	0.002
Consumer	−0.16	0.17	0.344	−0.19	0.18	0.290	−0.05	0.18	0.779	−0.19	0.17	0.277	−0.25	0.18	0.151	−0.03	0.18	0.877
Business support	−0.29	0.17	0.097	−0.26	0.17	0.141	−0.17	0.18	0.334	−0.34	0.17	0.044	−0.30	0.17	0.079	−0.19	0.17	0.266
Professional service	−0.07	0.14	0.585	−0.10	0.14	0.483	−0.05	0.14	0.699	−0.22	0.14	0.100	−0.23	0.14	0.091	−0.17	0.14	0.212
Age at entry	0.01	0.00	0.026	0.01	0.00	0.047	0.01	0.00	0.299	−0.06	0.00	<0.0001	−0.06	0.00	<0.0001	−0.07	0.00	<0.0001
Events	453			453			453			453			453			453		
Total episodes	4983			4983			4983			9472			9472			9472		
−2LL	5411			5390			5316			7095			7058			6904		

not controlling for left truncation.[5] We measured financial performance using three dummy variables: revenues are less than expenses; revenues are exceeding expenses; and revenues are exceeding expenses plus salaries for the owners. The reference category is no revenues earned. Because controlling for left truncation excludes spells before the age at which late-entering new ventures came under our observation, the numbers of spells in such models are much smaller than when left truncation is not controlled.

We first examine the effect of being listed in D&B on the hazard of termination, using models controlling for left truncation. In model 1, which includes all control variables but no other independent variables, we found the hazard ratio for being listed in Dun and Bradstreet is 0.394 [equal to exp(−0.93)], meaning that the hazard rate for new ventures listed with D&B is only 39% of the hazard rate for unregistered new ventures. The effect of a D&B listing is unchanged in model 2, which adds a set of independent variables: average managerial experiences, work experiences in the same industry as a new venture launched, entrepreneurial experiences, joint work experience, founders' social connections, the number of helpers, and the number of non-owner founders.

In model 3, as a check on whether our results for a listing in D&B simply reflected quality differences across emerging ventures, we further controlled for new ventures' performance levels. The performance indicators have significant and substantial effects on new ventures' survival chances. More importantly, controlling for financial performance reduces but does not eliminate the effects of a D&B listing, as model 3 shows that the hazard ratio for D&B registered ventures is about 47% for that of unregistered ventures, rather than the 39% we found in the models without controls for performance. We thus found significant difference between the hazard rates of registered ventures and unregistered ones, even after controlling for both team characteristics and performance.

Next, we investigated the extent to which *not* controlling for left truncation would substantially change the effects of being listed with D&B, using the 7 team characteristics and 3 performance variables in our full model. As shown in the right panel of Table 2, in models 4 through 6, when we ran the three models without controlling for left truncation, we found noticeable changes in many coefficients and standard errors. First, ignoring left truncation would lead us to overestimate the effects of being listed with D&B. Comparing only the final models in the left and right panels, we see that a D&B listing reduces the hazard rate by 53% [equal to 1−exp(−0.76)] in model 3, controlling for left truncation, whereas in model 6, the hazard rate is reduced by 61% [equal to 1−exp(−0.95)]. Both effects are substantial, but not controlling for left truncation could lead investigators to overestimate the significance of a D&B listing as a sign of a venture's quality.

Second, if we do not control for left truncation, we would also overestimate the effects of all three levels of performance. In model 3, when controlling for left truncation, we found that receiving revenue which is less than expenses reduces the death rate by about 39%, receiving revenue which is more than expense reduces the death rate by about 46%, and the highest performance level – receiving revenue which is more than expenses including owners' salaries – reduces the death rate by 77%. By contrast, if we do not control for left truncation in model 6, the three performance levels would reduce the hazard rate by 42%, 51%, and 86%. Taking the results of model 3 as the base, the effects of the three performance levels are exaggerated by 8% [equal to (42–39)/39], 13% [equal to (51–46)/46], and 12% [(86–77)/77] in model 6. These differences suggest that by ignoring left truncation, model 6 allows the hardy survivors from the pre-observation period of the study to inflate the apparent power of better performance.

Our two findings regarding over-estimation are not surprising. When not controlling for left truncation, we actually assume that late-entering new ventures were subject to a risk of termination before they entered our observations, although in reality such ventures have zero possibility of being terminated. (By definition, such failed ventures could not be in our sample.) We would thus artificially exaggerate the effects of financial performance and registration. Our first and second findings suggest that the internal validity of the results for the explanatory variables' effects on survival would be threatened without controls for left truncation.

Third, we found big changes in P-values for some coefficients. For example, the P-value of the coefficient for one industry type, Retail/Wholesale, is 0.047 in model 3 when controlling for left truncation, whereas it is 0.002 in model 6 without controlling for left truncation. In model 3, the P value for age at entry is 0.299 controlling for left truncation but is smaller than 0.0001 without controlling for left truncation. These differences suggest that we could mistakenly reject or accept a null hypothesis regarding the effect of an independent variable if we failed to control for left truncation.

In summary, as shown in Fig. 4 and Table 2, we have demonstrated not only that estimates of survival probabilities but also estimates of independent variables' effects would be biased if researchers ignored left truncation. To capture the precarious existence of emerging organizations in their early days, we need to take account of over-selecting hardy survivors in our research designs by using appropriate statistical models. Some of the independent variables included in our analyses are ones that researchers have used to measure organizational initial conditions (such as managerial experience, startup experience, team size) and subsequent financial performance (the three performance levels). Obtaining unbiased estimates for the effects of these variables is crucial for investigating what factors affect the survival of emerging ventures.

[5] Given the coefficients reported in Table 2, we can obtain hazard ratios, which are the exponential functions of coefficients, indicating the effects of covariates. A hazard ratio larger than 1 means that a variable has positive effects on the death rate, while a hazard ratio smaller than 1 indicates that a variable has negative effects on the death rate. For dummy covariates, the hazard ratio is the ratio of the estimated death rate for new ventures with a value of 1 to the death rate for new ventures with a value of 0. For researchers who might be interested in the confidence interval of hazard ratios, given that the exponential function is a monotonic function, the confidence interval of the hazard ratio can be directly computed as the exponential function of the confidence interval of the coefficient $e^{\hat{\beta}-Z_{1-\frac{\alpha}{2}}se(\hat{\beta})}, e^{\hat{\beta}+Z_{1-\frac{\alpha}{2}}se(\hat{\beta})}$.

7. Contributions and implications

In the past decade, emerging organizations have become a widely accepted target of intellectual inquiry in the field of entrepreneurship, but entrepreneurship scholars have been slow to recognize the merits of new and more powerful statistical methods for such investigations. Driven by the attraction of studying "successful" entrepreneurs, scholars have paid more attention to the organizing attempts that have effectively created "something" than to the organizing efforts that prematurely failed. Despite progress made in theorizing the process of organizational founding, the methodological problem of selective sampling remains a threat to the validity of empirical studies of emerging organizations in entrepreneurship research.

In this paper, drawing on principles of sampling and statistical inference, we have argued that previous findings regarding the rates at which new ventures fail might be biased for two reasons. First, scholars have mostly used registration data to examine new venture failures. Such data sets clearly include stronger survivors which have achieved a sufficient level of performance to be noted in public records. By focusing on the phases of organizational emergence immediately prior to the creation of fledging firms, previous studies not only left out emerging organizations that failed to weather their precarious early phases but also truncated the lifetime of emerging organizations that were selected. Second, although very recently scholars have attempted to reduce selection bias by using survey data on nascent entrepreneurs, their research is still subject to a selection bias if they fail to take account of the differences between the actual initiation date of new ventures and the starting time of a research project's observation period. These problems have remained out of sight partially because of the widespread availability of registration data and partially because statistical techniques for dealing with the problems have not been widely diffused in the entrepreneurship literature.

Our paper goes beyond previous efforts in making three contributions. First, drawing on organizational theories, we proposed three theoretical motivations for studying emerging organizations' survival. Examining the early weeks and months of an emerging venture sheds light on the role of entrepreneurial learning and leadership, highlighting the conditions under which entrepreneurs and their teams can adapt to their environments and build the platforms that will shape subsequent organizational development. Early-stage startups represent the time of maximal heterogeneity in organizational forms and routines and by focusing on them, we gain insight into the forces shaping the composition of the eventual population of established firms. We emphasized that further theoretical development regarding emerging organizations requires research designs that follow emerging organizations from the very early stage of their founding process.

Second, we examined several methodological issues which have plagued entrepreneurship researchers for years. Although scholars have called for more empirical investigations of new ventures' life chances, less effort has been devoted to state-of-the-art research design and analytical methods (Aldrich, 2009; Davidsson and Gordon, 2011; Martinez et al., 2011). We have reminded researchers of the limits to drawing inference to the whole population of emerging organizations if they study only the registered ones. When investigators must rely on registration data, they should offer explicit caveats regarding generalizing from their results. For example, they might point out that their sample contains organizations that survived their early "emergent" phase and are hardier competitors as a result. They might speculate on the ways in which organizations found in registration data differ from the larger population out of which the survivors emerged.

Third, we offered an analytical solution to the problem of left truncation. Entrepreneurship scholars have been searching for ways to fix the bias created by left-truncated data, and we have identified some powerful and well-developed analytical strategies that have rarely been applied. Our proposed statistical solutions to handling left-truncated data are widely available and can easily be implemented in many standard statistical packages. Were investigators to use these techniques, much of the mystery regarding emerging firms' failure rates would be cleared up.

A limitation of our paper is that we do not show the relative proportions of disparities in previous research that could be attributed to left-truncation, sampling error, and substantive moderators. Further research might shed light on this question. For example, several scholars have developed a program to correct for sampling errors (Hunter and Schmidt, 2004),[6] which might help investigators determine the relative impact of ignoring left truncation on estimates of survival rates, after controlling for sampling error. Such information would help investigators efficiently allocate their resources in creating research designs that produce reliable and valid results.

With regard to future research, we see four ways to improve research designs and data analyses. First, multiple dimensions, instead of a single performance indicator, should be examined when differentiating emerging organizations from established ones. In future research, scholars can try to operationalize the four properties of emerging organizations identified by Katz and Gartner: intentionality, boundary, exchange, and resources. Second, more sophisticated behavioral criteria could be applied in screening entrepreneurs. For example, if respondents reported positive answers to the three questions regarding general criteria used in PSED II, we could verify their entrepreneurial status by asking them what activities were conducted and when they were completed. Third, given the importance of understanding the duration of organizing processes in entrepreneurship studies, such as how much time elapses between the initiation of ventures and various milestones, future research should develop procedures for accurately identifying when new ventures start and end. Fourth, future research can obtain more dynamic information on organizational emergence by repeatedly interviewing entrepreneurs about their activities. Some events, such as making a plan, ownership, conducting marketing, hiring employees, are potentially repeated events. Researchers need to understand the rhythm and periodicity of such events and plan their data collection intervals accordingly.

[6] See Roth (2008) for a review of the program.

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