The Effects of Business Accelerators on Venture Performance:

Evidence from Start-Up Chile

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Do business accelerators add value? If so, how? We investigate these questions by focusing on Start-Up Chile, a government-backed ecosystem accelerator. Using a regression discontinuity design, we show that entrepreneurship-schooling services of accelerators can significantly increase new venture performance by improving the entrepreneurial capital of participants. We speculate about the existence of four performance-enhancing mechanisms: greater social clout, the provision of an accountability structure that induces entrepreneurs to articulate and reflect about specific strategic tasks, an increase in self-efficacy, and know-how about building a start-up. We find no support for causal effects of basic services of cash and co-working space.

JEL codes: G24, L26, M13

Keywords: Accelerators, Entrepreneurship, Start-ups, Entrepreneurial Capital, Managerial Capital

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An increasingly important institutional form in entrepreneurial ecosystems is the business accelerator: a fixed-term, cohort-based, financial intermediary that offers start-ups a combination of cash, shared office space, and entrepreneurship schooling. Since the first investor-led accelerator in 2005 (Y Combinator), thousands now exist worldwide.[3] Accelerators generally distinguish themselves from other early-stage financiers in their strong emphasis on the entrepreneurship-schooling component (Cohen and Hochberg, 2014). Although evidence about "managerial capital" constraints (e.g., Bruhn, Karlan, and Schoar, 2012; Bloom and Van Reenen, 2010) seem to justify such emphasis, little rigorous evidence exists regarding the mechanisms through which business accelerators affect new ventures.[4,5] This issue is particularly relevant given the importance of new ventures for economic growth (Davis, Haltiwanger, and Schuh, 1998; Haltiwanger, Jarmin, and Miranda, 2013), and the relevant public and private resources being spent to foster entrepreneurial activity.[6]

This article provides the first quasi-experimental evidence of the effect of accelerator programs on start-up performance, and of the importance of "entrepreneurial capital" in new ventures.[7] Our setting is Start-Up Chile, an ecosystem accelerator created in 2010. In contrast to investor-led accelerators (e.g., Techstars), which typically aim at making a return on their investment, ecosystem accelerators (e.g., Village Capital and Parallel 18) aim at stimulating start-up activity in their focal region (Clarysse, Wright, and Van Hove, 2015). Start-Up Chile offers participants an equity-free cash infusion, shared co-working office space, and the possibility of being selected into an educational subprogram, which we refer to as the entrepreneurship school. This subprogram is much like a business school in that it confers certification, provides access to distinguished international guests and encourages peer networking, increases exposure to the community, involves greater supervision and provides entrepreneurship know-how (in the form of

[3] At least 4,397 institutions self-identify as an accelerator. See F6S, https://www.f6s.com (last visited May 2016).
[4] By new ventures, we mean start-ups that aim at becoming large, vibrant businesses (Schoar, 2010).
[5] Exceptions include the work by Bernthal (2015), Cohen and Hochberg (2014), Leatherbee and Eesley (2014), Fehder and Hochberg (2014), Hallen et al. (2016), and Yu (2015).
[6] For example, see https://www.sba.gov/blogs/sba-launches-growth-accelerator-fund.
[7] Our paper complements the work by Klinger and Schundeln (2011) and Mckenzie (2015) looking at the impact of formal and structured business training programs offered by business-plan competitions.

monthly meetings with industry experts). Using a fuzzy regression discontinuity design (RDD), which exploits the fact that the program accepts a fixed number of participants every round based on an application score, we provide causal estimates of the effect of basic accelerator services (i.e., cash and co-working space) on start-up performance. Further, exploiting a unique feature of Start-Up Chile (that only 20% of participants are selected into the entrepreneurship school based on a business plan "pitch competition" and a qualification score cutoff) we estimate the value-added effect of entrepreneurship schooling (combined with the basic services), also using a fuzzy RDD.

We find participation in the entrepreneurship school, bundled with the basic services of cash and co-working space, leads to significantly higher venture fundraising, valuation, and scale within the first four and a half years since accelerator participation. We estimate a 21.0% to 45.5% higher probability of securing additional financing, which corresponds to a 0.29 to 0.43 standard deviation increase over the sample mean. We also estimate an increase of three to six times the amount of capital raised (a 0.30 to 0.40 standard deviation increase over the mean), and a fivefold increase in valuation (a 0.34 standard deviation increase). Schooling also appears to increase venture scale: we estimate a 23.8% increase in market traction and a twofold increase in employees, which correspond to standard deviation increases of 0.31 and 0.34 over the sample means, respectively. By contrast, we find no evidence that basic accelerator services of cash and co-working space have a treatment effect on fundraising, scale, or survival—at least not for the subpopulation of start-ups "randomized in" by the RDD. Although participants in the accelerator outperform rejected applicants on average, the selection skill of the recruiters appears to explain the performance differences. Finally, we find suggestive evidence of positive spillovers on local business registration rates, which is consistent with the broader goals of ecosystem accelerators.

How does participation in the entrepreneurship school add value to new ventures? Why did these start-ups not previously invest in the entrepreneurial capital conferred by the program? Our evidence, though speculative, points to four scarcely supplied entrepreneurial-capital-enhancing mechanisms: (1) an increase in the "social clout" (cf. Burt, 1997) of the start-up's founders—via access to business and fundraising connections of their peers, the program's staff, preferential access to foreign guest speakers,

exposure to the broader community, and certification from being admitted into the school; (2) the introduction of "structured accountability": a commitment to publicly articulate and be held accountable for monthly development goals and tasks; (3) an increase in entrepreneurial self-efficacy or self-confidence as the recognition of being accepted into the school may help to boost entrepreneurs' belief that they are capable of achieving tougher goals; and (4), the acquisition of know-how about building a start-up, as a consequence of workshops and the interaction with accelerator stakeholders and peers. We also speculate that the lack of investment in entrepreneurial capital prior to the program is due to a combination of supply, informational, financial and cognitive constraints.

An important challenge of working with start-up performance data is the collection of outcome measures for all accelerator applicant start-ups. Most applicants do not appear in standard business data sources, because they are seldom legally incorporated. Furthermore, the large likelihood that these early-stage start-ups pivot into new start-ups and change their names makes defining, let alone adequately measuring, post-application performance difficult. We address this challenge by hand-collecting data using extensive web searches about the start-ups and their founders in online platforms (i.e., CB Insights, LinkedIn and Facebook). This approach is similar to that used by previous studies such as Kerr, Lerner, and Schoar (2014), Goldfarb, Kirsch, and Miller (2007), and Hallen, Bingham, and Cohen (2016).

One concern with this data-collection method is that participation in the accelerator may change a start-up's likelihood of reporting to these sites, irrespective of whether it actually changes performance (cf. Drexler, Fischer, and Schoar, 2014; de Mel, McKenzie, and Woodruff, 2014). We address this issue by cross checking the information retrieved from the web with that from two surveys: one of applicants and one of participants. To the best of our knowledge, we provide the first-of-its-kind validation of web-based venture-performance measures by showing they significantly correlate with survey-based venture-performance measures, and produce similar results.

Business-training interventions and studies on the impacts of other hands-on financial intermediaries offer the most natural benchmark against which to compare our findings on the effect of entrepreneurship schooling. Our estimates are consistent in both magnitude and speed with prior literature.

For example, our estimates for the increase in the likelihood of securing additional fundraising are comparable to findings by Kerr et al., (2014), who estimate a 21% to 27% increase in the likelihood of securing (additional) venture fundraising within four years of receiving support from business angels. Similarly, our estimates of the effects on venture scale are consistent with those of Calderon et al. (2012) who find a 20% increase in sales within 12 months of business training interventions of small firms. Concerning the external validity of our findings, our results suggest that combining basic services and entrepreneurship schooling to transformational ventures in ecosystem accelerators is more effective than providing basic services only. This interpretation is consistent with recent work showing the impact of consulting services for small and medium-sized non-transformational entrepreneurial firms is much larger than simply improving access to capital (cf. Bruhn et al., 2010). It is also consistent with the view that entrepreneurial capital—similar to managerial capital—is a type of capital that is missing among certain populations (cf. Bruhn et al., 2010). Our findings seem particularly valid for ecosystem-accelerators focusing on early-stage start-ups; indeed, we show that relative to the average applicant to ecosystem accelerators worldwide, Start-Up Chile applicants tend to be younger and have earlier-stage start-ups.

We contribute to several literature streams. First, a growing body of literature assesses the value added by early-stage financiers to new ventures (e.g., Hellman and Puri, 2000, 2002; Kortum and Lerner, 2000; Kerr et al, 2014; Lerner et al., 2015). This literature has mostly focused on venture capital and angel investors. To the best of our knowledge, our paper is the first to provide rigorous evidence for business accelerators. In addition, our research setting allows us to advance on uncovering causal estimates and to trace the impact to specific mechanisms for value creation. Our evidence complements prior work that focuses on the importance of networks and human capital in the private equity industry (Hochberg, Ljungqvist, and Lu, 2007; Ewens and Rhodes-Kropf, 2015). Moreover, our paper offers suggestive empirical evidence to complement recent theoretical perspectives regarding the role of boards for new venture performance (Garg, 2013). As such, we distinguish the idea of structured accountability as a distinct mechanism to be used by new venture boards of directors.

Second, our results complement several emerging studies on accelerators. These studies focus on conceptual descriptions of the accelerator model (e.g., Bernthal, 2015; Cohen, 2013; Cohen and Hochberg, 2014; Kim and Wagman, 2014; Radojevich-Kelley and Hoffman, 2012), the cognitive and behavioral effects of social interaction (Leatherbee and Eesley, 2014), the emergence of regional early-stage financiers (Fehder and Hochberg, 2014), and the acceleration of new venture outcomes (Hallen et al., 2016; Yu, 2015; Winston-Smith and Hannigan, 2015). Our paper provides evidence regarding specific aspects of the accelerator model—capital, co-working space, and entrepreneurship schooling—and identifies the value-added role of these services.

Third, our paper builds on the literature about firms' management practices and business-training programs. That is, the effects of managerial capital, defined as the "abilities to manage an effective operations scale-up" of established companies (Bruhn et al., 2010). We contribute to this literature by distinguishing entrepreneurial capital from managerial capital. Whereas prior literature has defined and studied managerial capital as an input factor for established firms (Bertrand and Schoar, 2003; de Mel, McKenzie and Woodruff, 2008; McKenzie and Woodruff, 2008), we focus on the input factors useful for emerging ventures. For example, managerial capital literature focuses on management know-how, and includes topics such as "inventory management," "financial planning," "accounting," "employee management," and "customer service" (McKenzie and Woodruff, 2014), all of which are important once the firm is up and running. By contrast, entrepreneurial capital is an important input factor for *before* the firm is established, when entrepreneurs are searching for the business opportunity that will enable them to create an established firm. Our setting exposes a different set of assets, such as social clout, structured accountability, entrepreneurial self-efficacy, and know-how about building a start-up. By exploring the inputs that affect the creation of new companies, we provide quasi-experimental evidence that entrepreneurial capital could be profitable for new ventures to access, at least in the start-up phase. Our results complement those of recent field experiments in developing countries exploring returns to business-training interventions (e.g., McKenzie and Woodruff, 2014).

The paper is organized as follows. Section 1 details the institutional setting; section 2 describes the data; section 3 explains the empirical design and presents the main results; section 4 discusses the interpretation, magnitude, and external validity of the results. Section 5 concludes.

1. INSTITUTIONAL SETTING

1.1 Business Accelerators

Business accelerators constitute a new incubator model that has developed into an "umbrella term" for any program providing access to funding, co-working space, and—especially important—entrepreneurship-schooling services. Depending on their strategic focus, accelerators can be classified into one of three broad types: investor-led, matchmaker, and ecosystem (Clarysse et al., 2015). Investor-led accelerators (e.g., Y Combinator, Techstars) are typically aimed at discovering investment opportunities; thus, their main goal is profitability. Corporations set up accelerators (e.g., Microsoft Ventures, Google Ventures) to match potential service providers for their clients' needs. Finally, ecosystem accelerators (e.g., Start-Up Chile, Village Capital, and Parallel 18) typically have government agencies or non-profit organizations as stakeholders (principal), and their aim is to stimulate start-up activity in their focal region by supporting large numbers of start-ups and fostering contact points with the surrounding community.

The motivation behind accelerators' focus on entrepreneurial schooling is that market frictions challenge a start-up's development by imposing constraints on "entrepreneurial capital". The former can take several forms, including a founding team's inexperience (Gruber, MacMillan, and Thompson, 2008), limited legitimacy to attract good employees or investors (Zott and Huy, 2007), and deficient know-how about how to seize opportunities and grow a business (Ambos and Birkinshaw, 2010). The idea that entrepreneurial technology can affect the productivity of inputs is not new: it is central to Lucas (1978). Consistent with the importance of this type of capital, empirical studies show a strong association between managerial practices and higher productivity and profitability (Acemoglu et al., 2007; Bloom and Van Reenen, 2010). However, the evidence pertaining to new ventures (and thus to the value of entrepreneurial rather than managerial capital) is mixed (McKenzie and Woodruff, 2014) and mostly relates to non-transformational ventures (cf. Schoar, 2010).

In this article, we investigate the value of entrepreneurial capital and the role of business accelerators in its provision. Given the central role of entrepreneurial schooling in these programs, practitioners are dubbing accelerators "the new business schools for entrepreneurs."[8] We illustrate this association in Appendix A.1, were we classify the services provided by business schools into four main categories and construct a parallel with the services provided by business accelerators. As summarized in the table, the greater *social clout* from the certification of being accepted into a prestigious business school and the access to the social networks of classmates and professors can also be found in accelerators, in the form of certification from being accepted into the entrepreneurship school and the access to the social networks of schooled peers and affiliated members. The same is true for milestone-achievement structures through exams and assignments at business schools: at the accelerator's entrepreneurship school, *structured accountability* (imposed by regular meetings with program staff) induces entrepreneurs to be held accountable for milestone achievements. The *self-efficacy* gained from being accepted into—and graduating from—a prestigious business school, is also present in the accelerator's entrepreneurship school. Finally, similar to how business schools provide their students with *know-how* through lectures and access to professors and advisors, accelerators provide know-how through workshops, guest lectures, and access to industry experts and mentors. Thus, much like business schools provide students with managerial capital, accelerator entrepreneurship schools provide participants with entrepreneurial capital.

1.2 Research Setting

We focus on the case of Start-Up Chile, an ecosystem accelerator launched in August 2010 and sponsored by the Chilean government. Its main aim is the attraction of early-stage, high-potential entrepreneurs from across the globe, and the transformation of the domestic entrepreneurship ecosystem.[9] As of August 2015, approximately 1,000 start-ups had participated in the program, and nearly 6,000 had applied. The main long-term goal is to transform Chile into the innovation and entrepreneurial hub of Latin America. Like

[8] Article: http://techcrunch.com/2015/07/11/accelerators-are-the-new-business-school/ See also: http://www.economist.com/news/special-report/21593592-biggest-professional-training-system-you-have-never-heard-getting-up-speed

[9] For more details on Start-Up Chile, see Applegate et al. (2012) and Gonzalez-Uribe (2014).

other ecosystem accelerators worldwide, the expectations are that the policy will help domestic entrepreneurs access the resources of foreign entrepreneurial hubs (by building relationships with the foreign entrepreneurs), increase deal flow for early-stage domestic investors, and legitimize the occupation of high-growth entrepreneurship.

Like other business accelerators across the world, Start-Up Chile is a fixed-term, cohort-based program that offers participants equity-free seed capital (roughly US$40,000) and shared office space. In addition, it offers entrepreneurship schooling to a select few. Each cohort is comprised of 100 competitively selected participants, who remain in the program for six months. At the end of their term, participating start-ups "graduate" through a "demo day" (i.e., a formal presentation of the companies to external investors). Although the program takes no equity stake in participants, it relies on two mechanisms to mitigate opportunistic behavior by entrepreneurs: capital staging and social norms. The capital is delivered in two installments: 50% at the beginning, and the remaining 50% three months after, conditional on survival.

Homologous to other accelerators, participants are required to relocate to the host program's city (Santiago de Chile) for the duration of their term. This requirement is not unique—we estimate that 58.6% of ecosystem accelerators worldwide require full reallocation, and an additional 31.0% have at least some partial reallocation requirement.[10] The program is also not unique in terms of the stage of development of the hosting region's entrepreneurship ecosystem; 37.9% of ecosystem-accelerators are located in underdeveloped regions outside US (17.9% Africa, 10.3% Latin America, 10.3% India), 37.9% in US but outside Silicon Valley, and the rest in Europe, UK and Canada, and not necessarily always in the capital cities of these countries.

A key feature of Start-Up Chile is that participants have the option to compete for a spot in the accelerator's entrepreneurship school—a highly sought-out award. About 20% of participants are competitively selected. These participants are the poster children of the program: their names are advertised on the webpage of Start-Up Chile and in news releases. They receive two main additional services. First,

[10] We use hand-collected descriptions of ecosystem accelerators surveyed by the Entrepreneurship Database Program at Goizueta Business School of Emory University on ecosystem-accelerators worldwide to construct these estimates.

they get to represent the program at high-profile public events and host (by holding one-on-one meetings) high-profile Start-Up Chile guests, such Steve Wozniak and Paul Ahlstrom. Second, they get to hold 30-40-minute monthly meetings with program staff and industry experts (no one is compensated or holds equity stake) where milestones are set and entrepreneurs are held socially accountable for their self-defined strategic tasks. Industry experts are generally Chileans connected to the Start-Up Chile network. They are assigned to schooled participants according to industry. Ideally, the program would have kept records of this assignment, but it did not. In section 4, we come back to these additional services and speculate more about the channels through which they might affect venture performance.

1.2.1 Selection process into the accelerator

Selection into Start-Up Chile is a two-part process. First, entrepreneurs submit their applications through an online platform operated by YouNoodle—a private company based in California that runs application processes for accelerator programs worldwide. YouNoodle sends the applications to a network of entrepreneurship experts, who judge and evaluate applications based on three criteria: the quality of the founding team, the merits of the project, and its potential impact on Chile's entrepreneurial community. For every generation that applies to Start-Up Chile, YouNoodle averages the judges' scores and ranks start-ups from best to worst. No ties are permitted—if companies tie they are ranked randomly. Importantly, applicants do not know who their judges are, nor do they know their position in the rank; thus, ranking manipulation by applicants is not possible.

Three to five expert judges are randomly assigned per application. YouNoodle's network is comprised of circa 200 entrepreneurship experts—roughly 40% from Silicon Valley, 25% from Latin America, 20% from EMEA, and 10% from the rest of the United States. Each expert evaluates approximately 10 start-ups per generation, does not know the identity of other judges evaluating the same start-ups, and no single judge observes all applications. Thus, judges are unlikely to be able to precisely manipulate the ranking (e.g., to help an applicant friend qualify).

A committee at CORFO handles the second part of the selection process, making the final decision based on YouNoodle's ranking. A capacity threshold is pre-specified for each cohort (normally 100), and

the top ranking companies—those ranking higher than the threshold—are typically selected. The threshold corresponds to the predetermined size of the cohort, and the government determines the threshold as a function of its budget before the application process begins. Perfect compliance with the selection rule does not occur: not all applicants ranking higher than the 100th company threshold ultimately participate, nor do all accepted participants rank higher than the threshold start-up. Two reasons explain the less-than-perfect compliance: (1) earlier stage start-ups (as opposed to established businesses) receive preference, especially in sectors that are not traditional to the Chilean economy; and (2) some selected applicants ultimately reject the offer. In the latter case, other candidates, usually ranking lower, are selected.

1.2.2 Selection process into the entrepreneurship school

Two months into the program, participants can apply to its entrepreneurship school. The entrepreneurship school was launched during the fourth generation of Start-Up Chile. On average, 80% of the accelerator participants chose to compete for a spot in the school.

The selection procedure for the entrepreneurship school consists of a competition dubbed "pitch day." On the pitch day, competing start-ups formally present or "pitch" their business to a group of local judges (an independent group from the accelerator application-process judges), both external (i.e., staff at other private accelerators in Chile, e.g., Telefonica's Wayra) and internal (i.e., staff at Start-Up Chile). Participants do not know the identity of these judges until minutes prior to the competition. The external judges have no clear incentive for manipulation, because they have no "skin in the game." Furthermore, although some might want to help a participating friend, they cannot precisely manipulate the scores: judges independently score each start-up (from 1 to 5), and no judge observes all scores.

Participants are allotted five minutes for their pitch. A guideline for the pitch is provided. Applicants are expected to discuss (i) the problem their business is trying to solve, (ii) the proposed solution, (iii) the business model, (iv) the size of the market, and (v) fundraising needs.

Based on the pooled scores from the pitch day judges, the accelerator's staff selects roughly 20% of the participants. The entrepreneurship school is not available for all participants, because monitoring

requirements are too burdensome for the staff, and providing the preferential access to external speakers and staff contacts to all participants is infeasible.

Although in each generation the number of accepted participants into the entrepreneurship school is not strictly or ex ante determined, an implicit selection rule is evident in the data. Start-ups scoring at least 3.6 (out of 5) on the pitch day are unconditionally 34% more likely to be selected into the entrepreneurship school. When asked, staff recalled no formal use of this rule other than that it highlighted those start-ups that "had just passed" the pitch competition.

2. DATA: Start-Up Chile Applicants

Start-Up Chile provided us with the application data for seven generations of the program, including rejected applicants. We have information on a total of 3,258 applicants (616 participants and 2,642 non-participants). Participants for generation 1 (7) arrived during June 2011 (June 2013) to Santiago, Chile, and left on January 2012 (January 2014).

Table 1 displays the number of applications judged per generation, as well as the number of the following: rejections (i.e., the program extends no offer), selected participants (i.e., the program extends an offer), participants (i.e., the start-up accepts the offer), pitch-day competitors (i.e., participants who competed to get accepted into the school), and participants in the entrepreneurship school.[11] The proportion of accepted applicants dropped from roughly 31% in generation 1 to approximately 7% from generation 5 onward, reflecting the increasing legitimization of the program in the international entrepreneurship community. [12]

Panel A in Table 1 illustrates some of the discrepancies in the selection rule: across generations, the capacity restriction never binds. In generations 5 and 6, Start-Up Chile extended extra offers to make up for rejections. Ideally, the program would have kept records of the order in which the offers were made, but it did not.

[11] The program imposes no restrictions on reapplications, which constitute 5% of the sample. We kept them in our main analysis, but results are not materially changed by their removal.

[12] The almost four-fold increase in the number of applicants for generation 2, motivated and increase the capacity threshold to 150. However, space restrictions prompted a reset of capacity back to 100 for generation 3.

We report in Tables 1-2 start-ups' and founders' characteristics retrieved from the application forms. The application format changes over time, explaining most missing observations in the tables. We compare applicants to Start-Up Chile with average applicants to ecosystem accelerators worldwide, using information from the Entrepreneurship Database (ED) program at Emory University.[13] We report characteristics of the latter under the heading "ED" in the tables.

Table 1 shows that 25.87% of applicant start-ups have raised external financing prior to their application (Panel B), 91.77% have less than five full-time employees (Panel C), and 56.27% are less than six months old (Panel D). The employee size of these companies is comparable to the average company size reported by Haltiwanger et al., (2013) for young firms (less than a year old) in the US: 33% have between one and four employees. Table A2.1 in Appendix A.2 shows further information about the stage of development of applicant start-ups and the distribution of applicant industries. The distribution is concentrated in IT related sectors such as E-commerce (18%), IT & Enterprise Software (17%), Mobile and Wireless (9%), and Social Media (12%), which is comparable to the industry representation of VC-backed firms in Computers, Electronics and Telecom reported by Puri and Zarutskie (2012).

[INSERT TABLE 1 HERE]

Table 2 describes founder characteristics across generations. Consistent on the objectives of the accelerator of attracting foreign entrepreneurs, on average, only 21.3% are Chilean (Panel A). The distribution of founders' age and gender across generations is relatively stable: most founders are between 25 and 30 years old (Panel B), and the female proportion ranges from 7% to 16% (Panel C).

A comparison between "Start-Up Chile" and "ED" columns in Tables 1-2 reveals the average Start-Up Chile applicant is younger, less likely to be female, has a younger and more underdeveloped business, and is less likely to have raised finance prior to potential participation than average applicants in other ecosystem accelerators worldwide. Indeed, 54.4% (32.33%) Start-Up Chile applicant founders (average ecosystem-accelerators—"ED") are younger than 30 years, 91.77% (68.54%) have less than five

[13] See http://goizueta.emory.edu/faculty/socialenterprise/resources/database.html.

employees, and 56.27% (21.86%) have start-ups younger than 6 months. Start-Up Chile applicants are, however, similar in fundraising to average ecosystem-accelerator applicants: 74.13% (79.28%) of applicants to Start-Up Chile (average ecosystem-accelerators) have not raised external financing (not even from family or friends) at the time of the application. In section 4, we come back to these differences when we discuss the interpretation and external validity of our findings.

[INSERT TABLE 2 HERE]

Based on the information from the application forms, we construct six covariates to use as controls in our empirical strategy: the age of the entrepreneur (*Age*), indicator variables for domestic and female applicants (*Chilean, Female*), the natural logarithm of the number of employees (*Employees Before*), and indicator variables for capital raised before application to the program (*Capital Raised Before*) and for start-ups that already have a working prototype/or have one in development (*Prototype*). Table 3 presents summary statistics.

[INSERT TABLE 3 HERE]

2.1 Performance Metrics

Collecting performance measures for all applicants to the accelerator is challenging. The vast majority of applicants are not registered in standard (local or foreign) business data sources. Moreover, the program did not collect data on non-participating applicants. Therefore, we used two strategies to address this challenge. First, we hand-collected web-based performance measures for all applicants. Second, we relied on two surveys: a post-application survey to all applicants and a post-participation survey to all participants. All outcomes are measured within four and a half years since potential accelerator participation.

2.1.1 Internet-based performance measures

We conducted Internet searches of the names of the founders and their start-ups, looking for indications of entrepreneurial performance (cf. Kerr, Lerner, and Schoar, 2014). Based on this information, we construct five performance metrics that proxy for venture fundraising, venture scale, and venture survival (cf. Eisenhardt and Schoonhoven, 1990; Maurer and Ebers, 2006). The standard metrics used to establish firm performance for more mature businesses (e.g., profits or stock price) are not generally available, nor are

they particularly useful in new venture settings, given the fledgling nature of start-ups (c.f., Puri and Zarutskie, 2012). For example, Facebook purchased Instagram for roughly $1 billion dollars when it was only one and a half years old and had neither revenues nor profits. However, it had over 100 million active users.

We hand-collected applicants' web information from CB Insights, LinkedIn and Facebook by searching each of these websites for the start-up name specified in the application form. We were careful to try out different versions of the names to ensure we did not miss observations due to spelling issues. We also used complementary information to ensure the match was correct. Whenever possible, we matched on location and the founder name given in the application.

CB Insights is a platform that mines data available online, gathering information about venture capital financings, merger and acquisition transactions, investor websites, and social media. We relied on CB Insights to construct a proxy for venture survival. If a start-up becomes relevant enough, it is likely to appear on CB Insights. Thus, we create an indicator variable that equals 1 if the company has a profile in CB Insights, and zero otherwise (*Web Survival*). We verified other indicators of survival (e.g., whether or not the start-up has a profile on LinkedIn or Facebook, or a website), and our results are consistent.

Using the information from CB Insights, we construct two proxies for venture fundraising. Because an important part of building a venture is knowing how to raise external capital (e.g., from angel or venture capital investors), and the action of raising capital serves as a third-party validation that the start-up has upside potential, venture fundraising is a relevant and appropriate outcome metric in our context. Our first fundraising measure is an indicator variable for securing specialized capital (i.e., seed, angel or venture capital funding) after potential participation in the accelerator (*Web Indicator Capital*). We code this variable with zero for all start-ups that have no post-application fundraising record on CB Insights. By construction, we also code this variable with zero for those that do not have a profile on CB Insights. We use detailed information about the fundraising date in the platform, together with the start-ups' application date, to classify fundraising rounds as post-application. Our second measure for venture fundraising records the natural logarithm of the value of capital raised (*Web Capital Raised*) by the start-up post-application.

This variable equals zero if the start-up has no post-application fundraising record on CB Insights, if such a record exists but does not specify an amount of capital raised, or if the start-up has no profile on CB Insights.

Using information from LinkedIn and Facebook, we construct two proxies for venture scale. Venture scale (or the potential for scale) is a key firm characteristic which specialized intermediaries such as venture capital firms focus on (cf. Puri and Zarutskie, 2012). Facebook and LinkedIn are two of the main social network platforms and are useful data sources for gathering information such as customer traction and employees. The first proxy corresponds to the number of employees as stated on LinkedIn (*Web Employees*). LinkedIn reports the number of employees in ranges (e.g., 1-10 employees), which we transform into point estimates using the minimum employee size in the range (i.e., we assigned an employment level of 1 when the reported range was 1-10 employees [cf. Kerr, Lerner, and Schoar, 2014]). We confirmed that the transformation rule is immaterial for the results. The second proxy for venture scale corresponds to the natural logarithm of the number of (thousand) Facebook "likes" (*Web Traction)*. This variable is an appropriate proxy for scale in this context because start-ups commonly use Facebook advertisements to run their marketing campaigns (cf. Miller and Bound, 2011). This is similar to web traffic measures used by Kerr et al., (2014). Because the prevalence of Facebook "likes" varies across industries, we verified that results continue to hold when we normalize the number of "likes" by industry.

We searched Facebook and LinkedIn (CB Insights) during the first quarter of 2014 (2015). Because participants in generation 1 (7) arrived in June 2011 (2013), the performance proxies based on our 2014 search results are roughly representative of outcomes between 0.5 years and 2.5 years since potential acceptance into the accelerator. Similarly, the proxies based on our 2015 search results correspond to outcomes between 1.5 years and 3.5 years since potential acceptance.

Table 3 presents summary statistics of the five web-based outcome measures. The average applicant is 2.60% likely to secure specialized financing, raises 0.49 (log) dollars in capital, has 0.53 employees, an average traction of 0.06 (log Facebook likes), and is 21.2% likely to be alive at the time of data collection within four and a half years since potential participation in the accelerator. The low

specialized fundraising rate is comparable to venture capital fundraising rates for young firms in the US as reported by Puri and Zarutskie (2012): only 0.11% of new firms created in the US between 1981 and 2005 were VC-backed. The survival rate compares to the low survival rates for US firms: 78.9% of non-VC-backed financed firms fail within the first five years (Puri and Zarutskie, 2012) and the average annual failure rate for small firms in the US is 20% (Haltiwanger et al., 2013).

One concern with this data-collection procedure is the potential effect of accelerator participation on web-reporting behavior (c.f. Drexler et al., 2014; Berge et al., 2014; de Mel et al., 2014). This concern is likely less of an issue when we compare participants in and out of the entrepreneurship school, because of a program-wide policy that encourages start-ups to build a web presence. However, we address the reporting concern in two ways. First, we exhaustively crosschecked the information retrieved from the different web sources. Second, we crosschecked the data with information retrieved directly from start-ups, using two independent performance surveys, which we describe next.

2.1.2 Survey-based outcome measures

The first survey was sent to all applicants during October 2014 (between 1.5 and 3.5 years since potential acceptance into the program). We received 298 valid responses, representing a 9% response rate. Because, we received few responses from participants who competed for a spot in the entrepreneurship school (45), and even fewer from schooled participants (13), we only use this first survey to test basic accelerator services for *applicants*. During the first quarter of 2016, the accelerator conducted a second performance-outcome survey (between 3 and 5 years since acceptance), focusing only on *participants*. The response rate was 72.4% and included 145 responses of participants who competed for a spot in the entrepreneurship school. Of these respondents, 35 correspond to actual schooled participants, which amounts to 60% of the alumni from the entrepreneurship school. Throughout, we refer to the first survey as the applicants' survey, and to the second survey as the participants' survey. A detailed description of the surveys is included in Appendix A.3 and A.4.

Using information from the applicants' (participants') survey, we construct six survey-based proxies of venture performance for applicants to the accelerator (participants who competed for the

entrepreneurship school). These metrics follow the same logic as those constructed using the information from the web searches. They thus share the same names, except the prefix, which refers to the type of survey: "Survey A." or "Survey P." for the survey of applicants and participants, respectively. Logarithmic transformations of the survey responses are used to reduce the impact of outliers. In unreported analysis, we verified that results are quantitatively similar when we code the outliers to missing.

The first variable is an indicator for venture survival (*Survey A. (Survey P.) Survival*), which equals 1 if the start-up was active by the time the survey was conducted. Next, we construct venture-fundraising proxies, starting with an indicator for securing capital after potential participation in the accelerator (*Survey A. Indicator Capital*). Analogously, *Survey P. Indicator Capital* is the proxy for securing fundraising after potential participation in the entrepreneurship school. We code these variables with zero for all start-ups that do not report having raised any financing post-application to the accelerator (or the entrepreneurship school). The second venture-fundraising proxy records the natural logarithm of the value of capital raised since inception, excluding the seed capital provided by the program to participants (*Survey A. Capital Raised* or *Survey P. Capital Raised*). The third venture-fundraising proxy is the natural logarithm of the pre-money valuation of the start-up (*Survey A. (Survey P.) Valuation*). For those applicants that have not secured external funding, this variable corresponds to their perceived valuation. Finally, we measure venture scale using the natural logarithm of the sales during the six months preceding the arrival of the survey questionnaire (*Survey A. (Survey P.) Traction*) and the natural logarithm of the number of employees (*Survey A. (Survey P.) Employees*).

Table 3 presents summary statistics of the survey-based performance metrics. The average applicant-survey respondent is 65.80% likely to secure external funding, raises 6.97 (log) dollars in capital, has 0.61 (log) employees, an average traction of 3.67, and is 61.80% likely to survive. The average participant-survey respondent is 57.90% likely to secure funding, raises 7.12 (log) dollars in capital, has 1.33 (log) employees, an average traction of 6.82, and is 64.10% likely to survive. A comparison between the third to last, and the last two sections of the table reveals that average web-based performance proxies are much smaller in magnitude than survey-based ones. These differences are most likely the result of

potential response bias. That is, only the best-performing start-ups answer the surveys. For example, survival rates in our survey-based samples appear particularly high relative to average survival rates reported by Haltiwanger et al., (2013) and Puri and Zarutskie (2012). This difference may also reflect dissimilarities in data collection criteria (e.g., survey respondents include capital from family in their reported fundraising, whereas CB Insights includes only specialized capital sources).

Table 4 shows the results of our crosschecking exercise correlating web-based and survey-based proxies for venture performance. Reassuringly, and despite the aforementioned differences in average levels, all (except 2 out of 10) web-based and survey-based performance metrics have a positive and statistically significant correlation. To the best of our knowledge, this paper is the first to show correlations that provide support for the use of web-based metrics as proxies for new venture performance.

[INSERT TABLE 4 HERE]

2.2 Basic Patterns

Table 5 compares average start-up performance across different subsamples of applicants within four and a half years since potential participation in the accelerator. Panel A compares accelerator participants and non-participants, and shows participants are better off according to both web-based and survey-based performance measures. Participants are more likely to raise capital, have larger scale, and are more likely to survive. Selection on observables may explain some of these performance differences. Indeed, they are also larger in size and are more likely than rejected applicants to have fundraised prior to participation, although they appear to be more immature in terms of development stage, because they are less likely to have a working prototype or be working on one.

Panel B compares schooled and non-schooled participants. It shows schooled participants perform better than non-schooled participants, according to both web-based and survey-based metrics. This outperformance is particularly salient in terms of fundraising and scale. By contrast, we find no significant differences in survival. Again, effective selection might explain some of the observed outperformance, as schooled participants are also more likely to have raised capital prior to applying, although they are less likely to be working on a prototype.

The differences in performance reported in Table 5, even if we were to condition on covariates, cannot be interpreted as evidence that the accelerator or the entrepreneurship school affect venture performance. The key point is that the accelerator and its entrepreneurship school may also be selecting start-ups based on unobservables. If this type of selection were to explain all of the conditional differences, the accelerator's basic services or its entrepreneurship school would not have an effect on venture performance.

[INSERT TABLE 5 HERE]

3. EMPIRICAL STRATEGY

We exploit the selection rules into the program and the entrepreneurship school to advance on distinguishing the causal effects of basic accelerator services (cash and co-working space) and entrepreneurial schooling on venture performance. In this section we summarize our findings and turn to their interpretation (i.e., magnitude and external validity) in section 4.

3.1 Exploiting the Selection Rule into the Accelerator

The accelerator's selection rule based on the capacity threshold implies the probability of acceleration changes discontinuously as a function of the application's ranking. This discontinuity is visible in Figure 1, where we plot the fraction of participating applicants against the normalized rank (i.e., the ranking of the start-up minus the generation's capacity threshold). For applicants ranking above the threshold—represented by the vertical line in the figure—we see a jump in average participation rates, calculated across bins of 10 ranks and plotted in dots.

[INSERT FIGURE 1 HERE]

We estimate the size of the discontinuity using the following equation:

$$(1) \qquad acceleration_s = \delta + \gamma higher_s + f(Rank_s - cutoff^g) + X_s + \varepsilon_s,$$

where s indexes start-ups, $higher_s$ is a dummy that equals 1 if the start-up ranks higher than the threshold, and X_s is a vector of controls including start-up and founder characteristics. We include in the estimation a fourth-degree polynomial of the normalized rank (i.e., $f(Rank_s - cutoff^g)$, where g indexes generation). Figure 1 also plots the fitted values and 90% confidence interval from this equation. As per visual

inspection, a discontinuity is present in the estimated probability of acceleration around the cutoff, which is sizable and significant, and corresponds to γ in equation (1).

Table 6 presents robust estimates of γ across different specifications of equation (1): including only the polynomials as controls (column 1), adding generation fixed effects (column 2), adding covariates (column 3), allowing the polynomials to differ on either side of threshold (column 4), and restricting the sample to a window of 73 observations around the cutoff as calculated using the optimal bandwidth procedure of Calonico et al. (2014) and differentially weighting observations using a triangular kernel (column 5). The coefficient in column (3) implies that ranking higher than the capacity threshold increases the probability of acceleration by 21%, relative to other start-ups in the same generation and controlling for observable differences across start-ups.

[INSERT TABLE 6 HERE]

We exploit this discontinuity in the probability of acceleration by using a fuzzy RDD. The key intuition is that conditional on ranking within a sufficiently small interval around the cutoff, ranking above the capacity threshold is as good as random. Hence, the difference in expected outcomes between start-ups on opposite sides of—but sufficiently near—the threshold provides the basis for an unbiased treatment-effect estimate.

We estimate a system of equations using (1) above and the following:

$$(2) \qquad outcome_s = \pi + \beta acceleration_s + \check{f}(Rank_s - cutoff^g) + X_s + \epsilon_s,$$

where we instrument $acceleration_s$ with the selection rule (i.e., the indicator variable $higher_s$).

We use all the data to estimate the system, borrowing strength from observations far from the cutoff to estimate the average outcome for observations near it.[14] We mitigate potential biases through high-order polynomials of the ranking (Lee and Lemieux, 2010). One potential concern is the results' dependence on functional form (cf. Gelman and Imbens, 2014). Thus, an important consideration is the choice of

[14] We check that our results are qualitatively similar when we use local linear regressions (e.g., Hahn, Todd, and van der Klaauw, 2001). This alternative RDD approach discards observations beyond some bandwidth h away from the cutoff, and estimates low-degree polynomial regressions on the remaining observations (e.g., Gelman and Imbens, 2014). Several methods for choosing the bandwidth exist; we used the approach by Calonico et al., (2014).

polynomial order. Although the statistics literature offers some help in the form of generalized cross-validation procedures (e.g., van der Klaauw, 2002), the correct order is ultimately unknown. Our approach is to verify that results are robust to using different polynomial orders.

As with all instrumental variable estimators, inference based on the fuzzy RDD is restricted to those observations whose treatment is "randomized" by the selection rule. Offsetting this restriction are the relatively mild conditions required for identification of the local average treatment effect (LATE). In particular, the identification assumption requires that the distribution of all pre-intervention variables is smooth around the threshold, which is testable on observables. Thus, inference based on the fuzzy RDD is valid even in situations where selection into the program is made on the basis of prospective gains from the treatment (cf. Roberts and Whited, 2013). The identification is guaranteed when applicants cannot manipulate their rank or score (i.e., the running variable [cf. Lee and Lemieux, 2010]). Under this assumption, $acceleration_s$ is as good as randomly assigned at the threshold.

[INSERT FIGURE 2 HERE]

Figure 2 checks the RDD assumption by looking at covariate balance. Applicants ranking closely below and above the threshold are similar to one another in their observable characteristics at the time of the application (regression results removed for space and available upon request). Figure 3 shows the distribution of application scores appears continuous around the capacity threshold. The McCrary test gives a discontinuity estimate of -0.03 (with a standard error of 0.10), which is close to and not statistically different from zero.

[INSERT FIGURE 3 HERE]

3.1.1 The effect of basic accelerator services

Results are summarized in Tables 7 and 8. Reported standard errors are heteroscedasticity robust. Table 7 summarizes differences in fundraising performance across selected and non-selected applicants using the web-based (Panel A) and survey-based (Panel B) metrics. Columns 1-2, 5-6, and 9-10 in the panels, report estimates from simple OLS comparisons between participants and non-participants, controlling and not for covariates. Across all fundraising proxies, participants outperform applicants, which is consistent with the

univariate differences reported in Table 3. The economic magnitude of this outperformance is sizable. Results in column (2) of Panel A indicate participants are 6.80% more likely than non-participants to raise capital after the program, which corresponds to a 0.14 standard deviation increase.

Columns 3-4, 7-8, and 11-12 in Table 7 report estimates of the effect of basic acceleration services on venture fundraising, using the fuzzy RDD, controlling and not for covariates. To conserve space, we do not include the estimates for the polynomial terms. Across all fundraising proxies, the estimated coefficients are lower in magnitude and not significant. For example, the point estimate in column (4) is -0.072, implying a 7.2% lower fundraising probability form acceleration. However, the estimate is not significant.

[INSERT TABLE 7 HERE]

Comparisons between OLS and RDD estimates in the table are informative about the program's ability to screen applicants. Although the differences in coefficients are generally not statistically significant, the point estimates suggest the positive difference in performance across participants and non-participants is explained by (ex-ante) heterogeneity in start-up quality—perceived by the accelerator yet unobserved by the econometrician. Thus, they indicate that naïve comparisons between accepted and rejected applicants overstate the program's effects. These results emphasize the importance and challenge of creating proper control groups in entrepreneurial finance studies as already argued by many before us (cf. Kerr, Lerner, and Schoar, 2014).

Table 8 summarizes the differences in our scale and survival measures across selected and non-selected applicants using the web-based (Panel A) and survey-based (Panel B) metrics. Results have a similar pattern to that of our fundraising measures: participants generally outperform non-participants (i.e., columns 1-2, 5-6, and 9-10 are generally positive), selection-on-observables by the program explains some of this outperformance (i.e., the point estimates in columns 2, 6, and 10 are generally smaller than in columns 1, 5, and 10, respectively), selection-on-unobservables appears to explain most of the differences in performance (i.e., columns 3-4, 7-8, and 11-12 are never significant), and results are qualitatively similar using web-based and survey-based metrics (i.e., patterns in panels A and B are similar).

[INSERT TABLE 8 HERE]

In unreported analysis, we verify that results are robust to using different degree polynomials, allowing polynomials to differ on either side of the threshold, and excluding observations from generations 1 and 2 and participants in the entrepreneurship school. We also find no evidence of heterogeneity in the effect across several covariates such as gender, nationality, and age.

3.2 Exploiting the Selection Process into the Entrepreneurship School

We exploit the informal selection rule into the entrepreneurship school to assess the casual effect of entrepreneurship schooling. This selection rule also implies a discontinuity—this time between the assignment to the school and the pitch-day score. The sample size for this exercise is much smaller than the analysis of the basic accelerator services, because only 276 participants competed for entry into the entrepreneurship school. Moreover, the schooling program was launched in 2012; hence, only participants from generations 4-7 had the option to compete for a spot.

[INSERT FIGURE 4 HERE]

Figure 4 shows, by bins of 0.2 (scores), the fraction of applicants participating in the pitch day that are selected into the entrepreneurship school—a clear jump occurs for pitch-day scores above 3.6. The figure also shows the OLS fitted values and 90% confidence interval of the regression:

$$(3) \qquad school_s = \tau + \mu Above_{3.6} + g(Pitch\_Day\ \ Score_s) + \varepsilon_s,$$

where the outcome variable $school_s$ is an indicator variable that equals 1 if the participant secured the education services, $Above_{3.6}$ is an indicator variable that equals 1 if the participant scored above 3.6 on the pitch day, and $g(Pitch\_Day\ \ Score_s)$ is a first-degree polynomial of the pitch-day score, which we choose given the small sample size. We verify that results are qualitatively similar to using higher-degree polynomials. However, because of the small sample size, these terms are never significant, and we thus exclude them (cf. Gelman and Imbens, 2014). The coefficient μ in equation (3) is a measure of the size of the discontinuity. As per visual inspection, and as confirmed in the regressions reported in Table 9, the discontinuity is large (51.9% and statistically significant at the 1% level), and robust to different specifications of equation (3), including generation fixed effects (column 2), covariates (column 3), a

second-degree polynomial (column 4), and restricting the sample to start-ups scoring between 3.3 and 3.8 on the pitch day (column 5).

[INSERT TABLE 9 HERE]

3.2.1 The fuzzy RDD for participation in the entrepreneurship school

We instrument participation in the entrepreneurship school (*school*$_s$) using the selection rule, and estimate the system of equations using (3) above and:

$$(4) \qquad outcome_s = \alpha + \beta school_s + \breve{g}(Pitch\_Day\ \ Score_s) + \epsilon_s.$$

Evidence of a balanced sample (Figure 5) and smoothness in the distribution of pitch-day scores (Figure 6) across participants scoring closely around the 3.6 cutoff validate the assumptions behind the fuzzy RDD. We cannot reject the null hypothesis of local continuity in the distribution of pitch-day scores (t-statistic from the McCrary test is -0.198). Indeed, we see little potential for manipulation: the judges score the pitches independently, no judge observes all scores, judges are external to the program, and their identity is unknown to the participants prior to the pitch day.

[INSERT FIGURE 5 HERE]

The only significant (observable) difference in covariates regards the indicator variable *Capital Raised Before*—participants who scored above 3.6 on the pitch day are significantly more likely to have secured external financing prior to joining the accelerator. Further inspection reveals, however, that such capital arises from non-specialized financiers such as family and friends: no difference is evident when restricting the type of capital to *Specialized Capital Raised Before* (which includes angel, accelerator or VC fundraising) as shown in the figure. We control for the significant difference in this pre-treatment observable by including the variable *Capital Raised Before* as a covariate and checking whether the estimated coefficients differ significantly with and without this control.

[INSERT FIGURE 6 HERE]

3.2.2 The effect of the entrepreneurship school

Tables 10 and 11 present the estimated effect of participating in the entrepreneurship school on venture performance, following the same structure as Tables 7 and 8.

Table 10 summarizes results for the web-based (Panel A) and participant survey–based (Panel B) measures for venture fundraising. Columns 1-2, 5-6, and 9-10 in the panels report estimates from simple OLS comparisons between participants and non-participants (controlling and not for covariates). The first column in Panel A shows that schooled participants are 9.1% more likely to raise capital after the program. Column 2 shows the fundraising ability of the start-ups prior to participating in the entrepreneurship school does not explain the increase in fundraising performance (i.e., results continue to hold once we control for *Capital Raised Before*).

Columns 3-4, 7-8, and 11-12 in the panels of Table 10 report estimates of the effect of entrepreneurship schooling on venture fundraising, using the fuzzy RDD, controlling and not for covariates. Columns 3 and 7 of Panel A estimate a positive and large causal effect of the entrepreneurship school: it increases the probability of fundraising by 21.0% and by three times the amount of capital raised. Controlling for observable covariates only marginally affects statistical significance, and importantly, does not affect the magnitude of the estimated treatment effect (column 4). The economic significance of the estimates in columns 3 and 7 is sizable: they imply a 0.29 and 0.30 standard deviation increase in the likelihood of fundraising and in the amount of capital raised, respectively.[15] Results from our participant survey–based measures (columns 3 and 7 in Panel B) are consistent with those from the web-based measures. They imply a 45.5% (0.43) and six-fold (0.40) increase (standard deviation increase) in the likelihood of fundraising and in the amount of capital raised, respectively. Finally, column 11 summarizes valuation estimates: schooling generates a five-fold (0.34) increase (standard deviation increase).

The positive difference between the RDD and OLS estimates in the table suggests heterogeneity in the treatment effect of the entrepreneurship school, and that the start-ups "randomized in" by the selection rule are particularly responsive to the schooling services (cf. Angrist and Pischke, 2008).

[INSERT TABLE 10 HERE]

[15] The calculation is based on standardized coefficients, which we do not report to conserve space.

Table 11 summarizes differences in our scale and survival measures across participants in and out of the entrepreneurship school, using the web-based (Panel A) and survey-based (Panel B) proxies. Similar to the findings for fundraising, participation in the entrepreneurship school appears to causally increase venture scale (columns 3-4 and 6-7). Panel A estimates a 23.8% increase in market traction and a twofold increase in employees. Panel B also shows positive effects using survey-based metrics, although less precisely estimated likely because of power issues. The economic significance of the effects in both panels is comparable: the estimates in columns 3 and 7 of Panel A (Panel B) imply participation in the entrepreneurship school yields a 0.31 (0.30) and 0.34 (0.30) standard deviation increase in market traction and employees, respectively. By contrast, we find no evidence that the entrepreneurship school affects venture survival (columns 9-10). Consistent with arguments by Maurer and Ebers (2006) this finding may reflect the notion that start-ups can survive through the persistence of their founders, but fundraising and growth must rely on entrepreneurial capital. Thus, the overall effect of entrepreneurship schooling seems to be the acceleration of participating ventures.

[INSERT TABLE 11 HERE]

In unreported regressions, we find no differential effect by stage of start-up (e.g., prototype in development). We find some evidence of a lower effect for female founders, somewhat consistent to related work on subsistence micro-entrepreneurs (de Mel et al., 2008). No consistent patterns are present in the estimates comparing Chilean and foreign founders. We also find results continue to hold when we restrict the sample to the last two generations of the program (for which data collection may be more accurate), and are also stronger for companies in industries that require a web-presence such as: E-commerce, Media, Mobile and Wireless, Social Media and Social Network.

4. DISCUSSION

4.1 How Does Participation in the Entrepreneurship School Add Value?

The natural follow-up questions ask how entrepreneurship-schooling services add value to new ventures, and why these start-ups had not previously invested in entrepreneurial capital. Although our empirical

setting does not allow us to directly answer this question, we use additional information from interviews with staff, schooled participants, and non-schooled participants to draw some suggestive conclusions.

We speculate the effect of the entrepreneurship school, much like that of business schools, stems from a combination of the four different types of schooling services explained in section 2 and summarized in Appendix A.1.

First, increasing the social clout of the start-up appears to play a key role: start-ups benefit from almost immediate higher exposure and certification from winning the pitch-day competition. Indeed, Start-Up Chile publishes the names of the winners on its website, and in press-articles. The certification effect combines the brand value constructed by Start-Up Chile over the years and the reputation of the pitch-day judges, generally renowned local investors such as the heads of other accelerator programs (e.g., the head at Wayra Chile) and prominent business angels. Although they have no equity stake in the participating start-ups, reputational considerations (i.e., the pitch-competition is open to the entrepreneurial community) incentivize judges to judiciously consider the competing teams. The start-ups' social clout may also benefit, and likely very rapidly, from the networks of: schooled peers, program's staff and high profile guest speakers. Indeed, Cai and Szeidl (2016), show that the value of new business contacts for young firms materializes very quickly—i.e., within a year of establishing the new relation. Moreover, schooled entrepreneurs are more likely to be invited to participate at high-profile events. For example, during generations 5 and 6, schooled start-ups were taken for a "roadshow" to Silicon Valley, where they held meetings with potential investors (see Gonzalez-Uribe, 2014).

Second, interviews with participants revealed that the monthly meetings also increase value by inducing participants to articulate, set-up and be held accountable for business milestones. During each of the meetings, a Start-Up Chile staff member takes notes and lists the tasks articulated by participants. This list is reviewed at the beginning of the next meeting, and entrepreneurs are encouraged to discuss their progress since the last meeting. This accountability structure may serve as a way to commit participants towards the achievement of business milestones (cf. March and Simon, 1993). That is, by being encouraged to set goals and be held accountable for their achievement, participants may become more persistent in the

pursuit of the challenges that are innate to the entrepreneurial process. Consistent with this notion, Berge et al., (2014) find significant short-term correlation between sales and profits and financial record keeping in microenterprises.

Further, interviews with staff revealed that entrepreneurial self-efficacy appeared to be boosted by the entrepreneurship school. The staff noticed that soon after winning the pitch-day and being accepted into the entrepreneurship school, participants appeared more self-confident and in control of their businesses. This is consistent with prior literature on entrepreneurial self-efficacy (Bandura, 1982; Chen, Greene, and Crick, 1998), which is an individual's belief in their ability to successfully execute entrepreneurial tasks. Prior literature has found that higher levels of entrepreneurial self-efficacy are positively related to new venture performance (Forbes, 2005; Hmieleski and Baron, 2008)

An increase in the know-how about how to build a start-up also appears to play a role, but mostly in the form of peer effects and more attention and support by the program's staff to schooled participants. Consistent with a potential role for peer-effects, 32% of respondents of a complementary program-value-assessment survey responded by 207 program alumni in December of 2012 mentioned that gaining access to a group of like-minded entrepreneurs was the most valuable aspect of the Start-Up Chile experience.[16] The potential role of peer-effects is also consistent with the related studies for investor-led (Hallen et al., 2016) and ecosystem (Leatherbee and Eesley, 2014) accelerators, which find evidence suggestive of peer effects within one-year of participation in the program. Instead, business instruction from industry experts is unlikely to play a large role in our setting, as the latter are not compensated and have no "skin in the game". They only formally met schooled entrepreneurs four times throughout the program for periods of roughly 40 minutes, whereas providing meaningful business advice requires time and effort for advisers to become familiarized with the start-up. This is confirmed by the responses of the complementary program-value assessment survey, where only 1% of respondents mentioned instruction from industry experts as a valuable aspect of Start-Up Chile. In contrast, investor-led accelerators typically do have expert mentors

[16] An open-ended question asked alumni "what was the most valuable aspect of participation in Start-Up Chile?" Answers were content-analysed and classified.

who act as staff members and have a financial interest in the start-ups they advise.

The question remains: if entrepreneurial capital effectively increases new venture performance, why did entrepreneurs not contract it in the first place? We speculate that informational, supply and financial constraints exist. For example, founders may be unaware or underestimate the importance of entrepreneurial capital a priori. As suggested by Bloom et al. (2013), management is a technology that diffuses slowly. Moreover, effective management is the type of knowledge that may be taught and "learned-by-doing". Likewise, we may expect knowledge about entrepreneurship best practices to also take time to diffuse. The program circumvents these informational frictions by providing examples, opportunities to gain experience, and by serving as a collective knowledge pool of best practices.

Cognitive restrictions may also help explain the limited pursuit of entrepreneurial capital prior to the program. For example, entrepreneurs might self-select into an entrepreneurial occupation because of their inclination to avoid being confined by a hierarchical structure that requires them to report to an authority figure. Therefore, building an accountability structure that feels like the hierarchy they are choosing to avoid may feel unnatural to them. An external entity like the entrepreneurship school may be necessary to push entrepreneurs out of their comfort zone.

Finally, consistent with supply and financial frictions, interviewed non-schooled participants argued that access to good connections was hard and costly to secure, much like securing a spot in a business school. For example, accessing a demo day elsewhere is costly (e.g., the cost for a start-up to present at the demo conference in Silicon Valley was $18,500 in 2011), and the ownership stake required by investor-led accelerators averages 6% (Cohen and Hochberg, 2014). Because of these high costs, entrepreneurs may be discouraged from securing the necessary entrepreneurial capital to grow their ventures. The program circumvents these financial costs by subsidizing the demo-days and tapping into the reputation and networks it has built over time (e.g., in 2013 Start-Up Chile was invited to present at the prestigious South by Southwest conference of Silicon Valley).

4.2 Why Did Basic Services of Cash and Co-working Space Have No Apparent Impact?

Other than the null hypothesis being true (i.e., basic accelerator services indeed add no value), one potential

explanation is that participants that are randomized in by the selection rule are of heterogeneous quality and the program accelerates the success of some but the demise of others: with a resulting zero average effect. For example, cash infusions can help founders discover fundamental flaws in their prototypes that justify the termination of the start-up (Yu, 2015). Cash infusions may also help founders find better business opportunities that justify the formation of a new start-up with greater upside potential, at the expense of terminating the previous start-up (Leatherbee and Katila, 2016). Consistent with this potential explanation, Table 5 shows that web-based outcomes for participants appear more disperse than for non-participants. Because by only measuring the performance of applicant start-ups we are precluded from observing the change and pursuit of better opportunities by entrepreneurs, we shift our level of analysis from the start-up to the founder. In unreported analysis, we find suggestive evidence that the program has a permanent effect on the entrepreneurial occupation of founders.[17] That is, treated applicants are more likely to start new ventures after they fail. Therefore, our results may be underestimating the treatment effect of the accelerator's basic services on founders, if the program actually accelerates growth, accelerates failure, or encourages the creation of more productive (new) ventures.

A second potential explanation, which is common in impact evaluations of business training programs, is that we don't have enough statistical power to reject the null. To explore this possibility, we construct back-of-the-envelope calculations of the sample size needed to detect outcome changes in the magnitude of the reported coefficients in tables 7-8 with a power of 80%. As a reference, many funding agencies consider 80% an appropriate power target (Duflo et al., 2008). Our approach is in the same spirit as McKenzie and Woodruff (2014), who use the coefficient of variations (ratio of the standard deviation and the mean) of the outcome variables in the baseline survey to estimate the statistical power of different studies. Because we have no baseline survey of outcomes, we use as proxy the standard deviation and mean for outcomes of non-participants. We report in the last row of tables 7-8 our back-of-the-envelope-estimates

[17] We record whether founders remain entrepreneurs after potential participation in the program. We collected information on the occupations of applicants' from LinkedIn, using searches based on the founder's name and crosschecking for location. We recorded occupations as *entrepreneur*, *student*, *analyst*, and *consultant*.

using a test size of 0.05 and the observed sample size ratio of participants to non-participants. Across most columns in the tables, the actual sample size is above our back-of-the-envelope estimate, which suggests that in most cases we have at least 80% statistical power.

A third potential explanation could be that the capital stock supplied to the participant start-ups is too low to generate positive returns (Banerjee and Newman, 1993; Aghion and Bolton, 1997). However, this explanation does not resonate in our case for two main reasons. First, applicant start-ups are predominantly from the "new economy," for which the necessary levels of physical capital stock to generate positive returns are generally low (e.g., Rajan and Zingales, 2000). Second, start-ups in our sample are mostly in the business model discovery and validation phase, which is typically characterized by low levels of fixed costs. Nonetheless, one can still argue, at least for foreign entrepreneurs, that the relocation costs to Santiago consume too much of their seed capital. However, this possibility also seems unlikely: we estimate no differences in the effect across foreign and Chilean entrepreneurs, and the program estimates that relocation costs represent less than 10% of the awarded grant.

A fourth potential explanation is that rejected applicants secured acceleration services elsewhere, thereby dampening the estimated effect of the basic services. Analysis of supplementary data does not support this alternative story. We collected information from AngelList and Seed-DB[18] regarding non-participants' acceptance into other programs, and find only 2% of rejected applicants secured financing in other accelerators. This low probability is consistent with recent estimates of low acceptance rates in accelerators worldwide. According to FS6.com a web platform that runs 90% of applications to accelerators globally, less than 3.98% of applicants ever make it into an accelerator.[19]

One last potential explanation is that the mechanisms the program uses to curb entrepreneurs' opportunistic behavior are not enough, and hence added-value gains are not materialized. Indeed, the value-adding monitoring and powerful allocation of control rights by financial intermediaries (Hellman, 1998; Cornelli and Yosha, 2003) are most pronounced in the entrepreneurship school. According to the program's

[18] Seed-DB is an open-source accelerator database built on CrunchBase data (http://www.seed-deb.com/).
[19] https://techcrunch.com/2014/04/20/who-gets-into-accelerators-persistent-men-with-saas-apps-says-study/

staff, however, even non-schooled participants are very motivated. Since the inception of the program, only one case of questionable use of funds has occurred, and corrective measures were taken. The lack of opportunistic behavior in our setting may be a due to reputational consequences acting as disciplinary devices (cf. Bernthal, 2015).

4.3 External Validity and Magnitude

Our findings suggest ecosystem accelerators that combine basic services with entrepreneurship schooling are more effective than those providing basic services only (e.g., cash). However, we are careful to emphasize the differences between average applicants to Start-Up Chile and other ecosystem accelerators, which we documented in section 2. Perhaps a more exacting interpretation of our findings is that for ecosystem accelerators that focus on young founders and early-stage start-ups, the combination of education and basic services can add value to participants. We are also careful to emphasize the differences between Start-Up Chile, which is a prototypical ecosystem accelerator, and other types of accelerators as explained in detail in section 1. Extrapolation of lessons for other types of programs, such as investor-led or matchmaker accelerators, is challenging.

It is useful to compare our results to that of prior work on the impacts of other types of intermediaries in private equity markets, and other types of business training/consulting interventions. Perhaps the closest paper to ours regarding private equity intermediaries is Kerr et al. (2014), which estimates an increase of 21%–27% in the likelihood of securing (additional) venture fundraising circa four years after being supported by business angels. Remarkably close are our estimates of a 21%–45% increase in (additional) venture fundraising within four and a half years since participation in the entrepreneurship school. Regarding training and consulting interventions, Calderon et al. (2012) and de Mel et al. (2014) find a 20% and 41% increase in sales within twelve and eight months of different business training interventions, respectively. These findings are similar to our estimate of a 23.8% increase in venture traction from entrepreneurship schooling (and our survey-based results have similar standard errors to those of these studies). Our estimates on employees are close to those of Glaub et al. (2012), who estimate that treated firms have roughly twice as many workers as control firms after 5-7 months of a 3-day training intervention

(10.7 workers relative to 5.0). This estimate is within the standard error bands of our 198.5% (column 7, panel A, Table 11) estimated increase in employees from participation in the entrepreneurship school. Similar to our own noisy estimates for survival, most studies find positive but insignificant impacts (cf. McKenzie and Woodruff, 2014). Finally and more generally, abundant evidence suggests that for certain types of subpopulations, interventions that combine finance (especially grants) and business training are more effective in supporting business start-ups than finance alone (e.g., Bandiera et al., 2013). This is consistent with our findings of no visible effects from the basic accelerator services, but positive effects from the additional entrepreneurship schooling services.

4.4 The Entrepreneurship Ecosystem

Given that the main goal of ecosystem accelerators is to influence the entrepreneurship ecosystem, we explored whether the program has any spillover effects. We collected information on new business creation in Chile. In particular, we focused on the annual number of new registered businesses by "comunas" (i.e., fiscally independent localities in Chile) and industry between 2005 and 2013. We extracted data from the Chilean Department of Economic and Tax Studies through a procedure akin to a Freedom of Information Act information request (Ley de Transparencia) on October 17, 2014.

Using a difference-in-differences methodology, we compare business-creation rates before and after inception of the program, across industries related and not related to the program, and in comunas close to and far from the headquarters of Start-Up Chile. Macroeconomic factors—such as the multiple policies and regulatory changes that occurred in Chile between 2010 and 2014—may certainly affect overall business-registration rates. However, if the program indeed had an impact on the local entrepreneurial community, it would most likely affect registrations in industries that are directly related to Start-Up Chile (such as software, as opposed to, say, timber), and in comunas close to the headquarters of the program (where regular "meet-ups" were held for aspiring and established entrepreneurs to network and share their experiences).

[INSERT TABLE 12 HERE]

Table 12 summarizes our results. The estimate in column 3 shows the number of registered businesses increased by 2.4% after 2010 and relative to the same types of businesses in other comunas and to other industries in the same comuna. We interpret these results as suggestive evidence of ecosystem spillovers of the program. They complement related work attempting to measure spillover effects of government interventions (cf. McKenzie and Woodruff, 2014) and investor-led accelerators more generally (Fehder and Hochberg, 2014), a task only a handful of studies have taken up. They are also consistent with the program's main objective, and with informal evidence on Chile's transformation into a regional entrepreneurial hub dubbed "Chilecon Valley."

5. CONCLUSIONS

How accelerators affect new venture performance is an important question, with theoretical and practical implications. However, little evidence exists about whether accelerators are effective, and if so, which underlying mechanisms make them so. This paper provides the first quasi-experimental evidence of the effect of accelerator programs and the importance of entrepreneurial capital on new venture performance.

We evaluated an ecosystem accelerator that provides participants with seed capital and co-working space. The accelerator also provides entrepreneurship-schooling services to a competitively select few. We find that entrepreneurship schooling (bundled with the basic services of cash and co-working space) leads to significant increases in venture fundraising and scale. By contrast, we find no evidence that the basic accelerator services of cash and co-working space improve venture performance. We also find suggestive evidence that the program has spillovers in the form of increased business creation, which is consistent with the broader goals of ecosystem accelerators.

Our findings suggest that policy interventions such as ecosystem accelerators that combine basic services and entrepreneurship training (specially those focusing on young founders and early-stage start-ups) are more effective than programs providing basic services only (e.g., cash). This result is consistent with the view that entrepreneurial capital, similar to managerial capital, is a type of capital that is missing among certain populations (cf. Bruhn et al., 2010). We speculate that the lack of investment in

entrepreneurial capital prior to the program is due to a combination of supply, informational, financial and cognitive constraints.

In terms of policy design, if the objective is to accelerate participants, our results suggest that more resources should be allocated toward entrepreneurship schooling. Naturally, it is challenging to scale scarce schooling resources, in contrast to cash infusions, which are more easily scalable. However, if instigating an ecosystem change is the policy objective, size may matter—as our evidence of regional spillovers and the persistence of an entrepreneurial occupation suggests. In this case, funding larger programs—even at the expense of not providing schooling to all participants—may be the better option.

Future research directions include directly testing the value-adding mechanisms of entrepreneurship schooling using randomized control trials. We can only provide suggestive evidence of four value-adding mechanisms: social clout, structured accountability, entrepreneurial self-efficacy, and business know-how. Moreover, our exploration of different levels of analysis (founder- and regional-level) has profound implications regarding the need for comprehensive evaluations of accelerator programs. Understanding how accelerators influence the persistence of individuals on an entrepreneurial career path, and how an entrepreneurial experience may influence an individual's entrepreneurial capital for the creation of economic value regardless of her career path, are two other important questions future research should try to answer.

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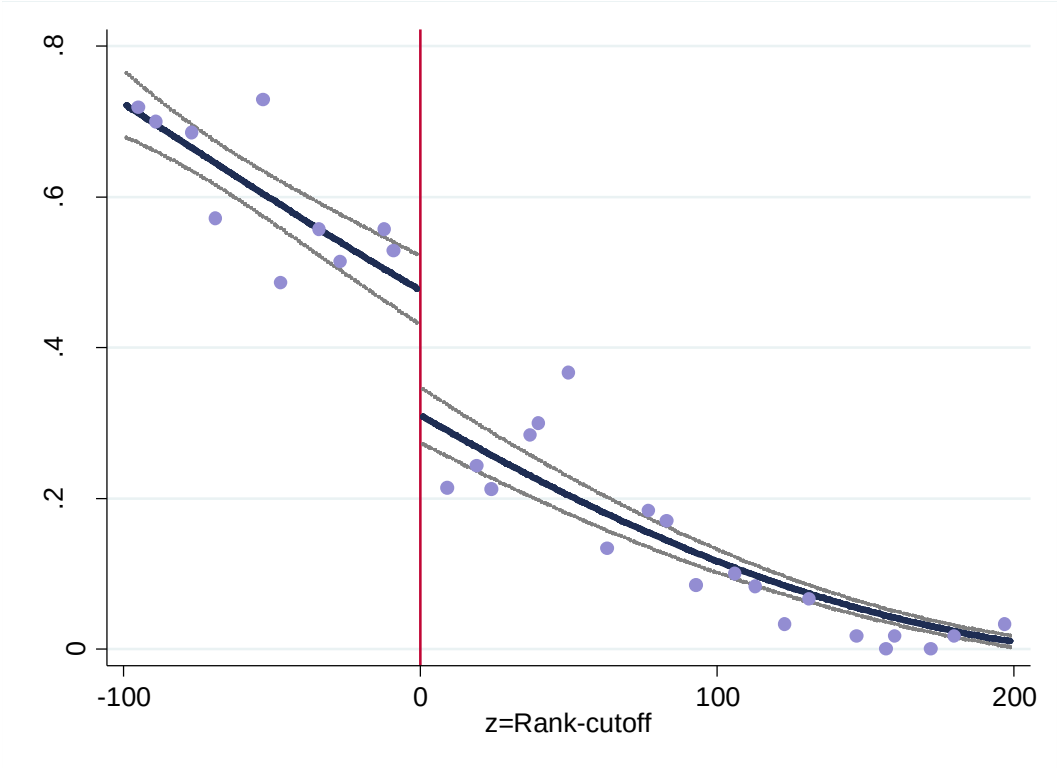
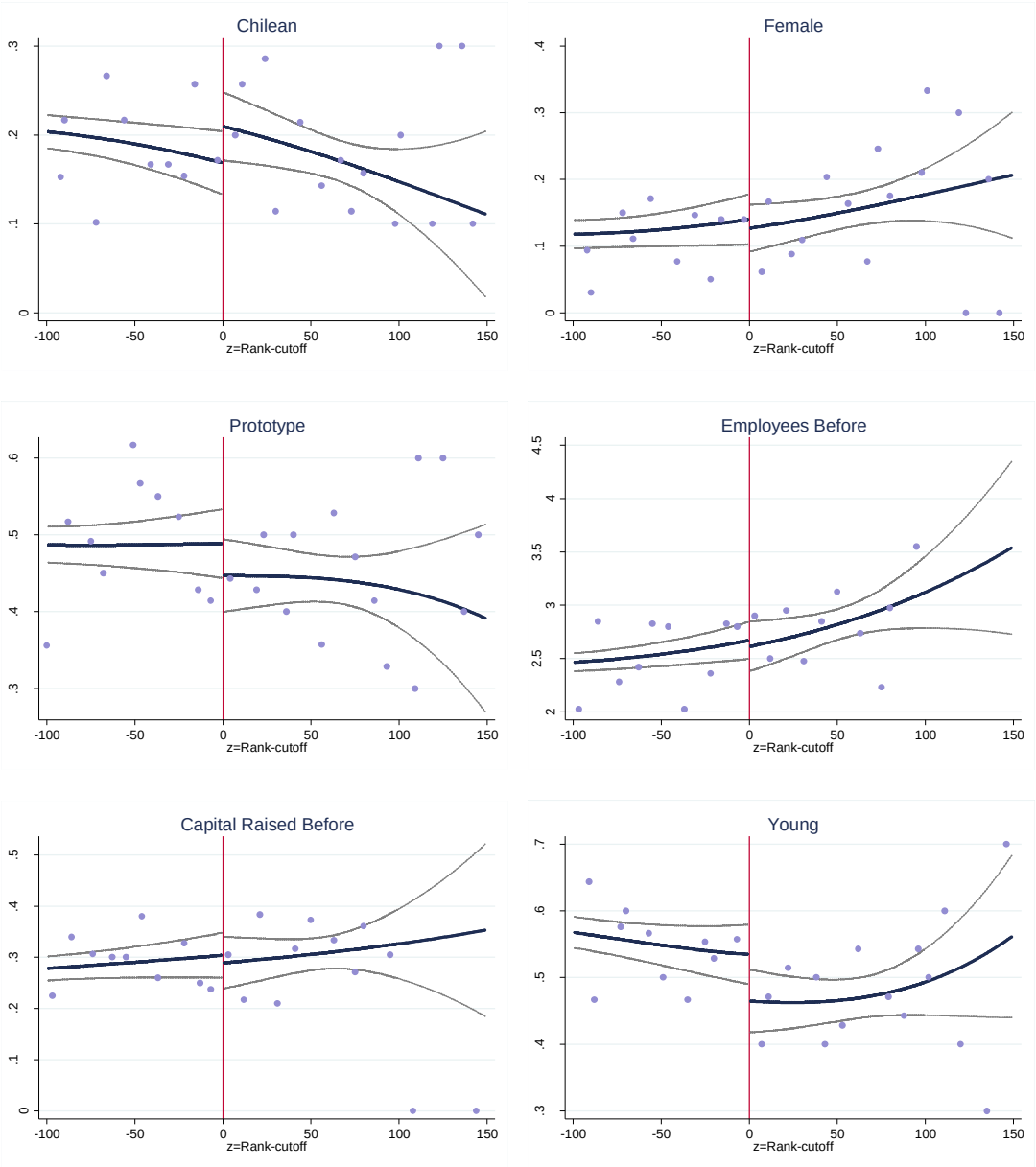
Figure 1 – Fraction of Accelerated Applicants



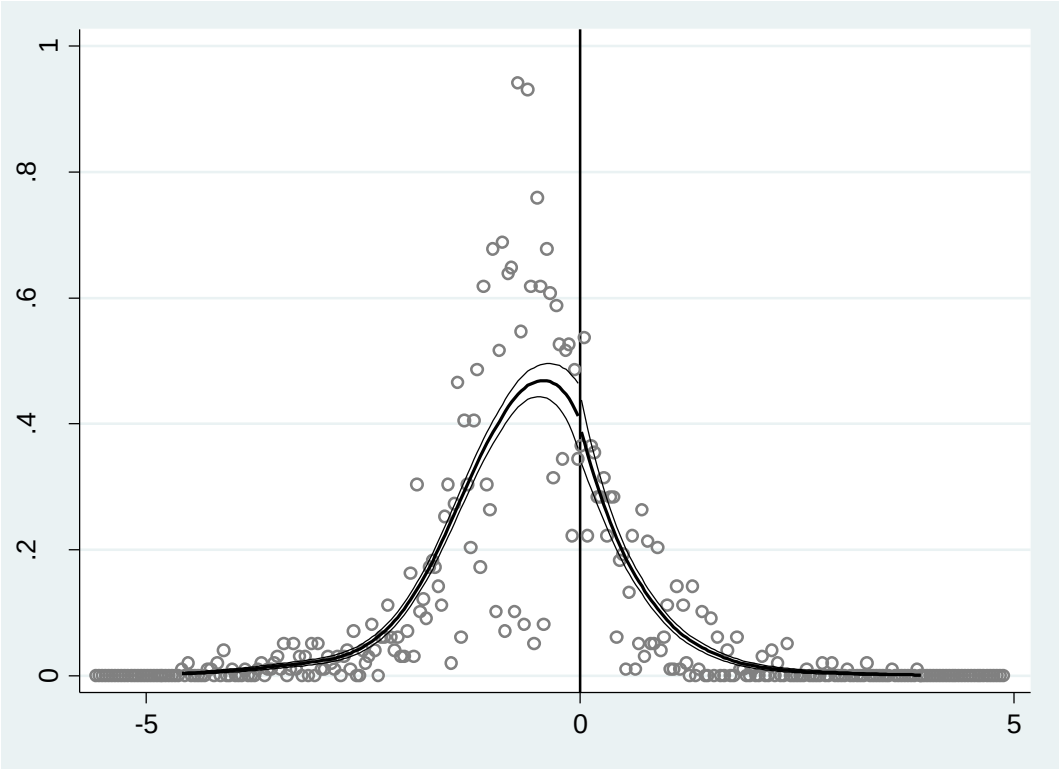
The figure shows the average fraction of accelerated applicants (dots) in bins of 10 transformed ranks (i.e., z), and the fitted values and 90% confidence interval from the regression $acceleration_s = \delta + \gamma higher_s + f(Rank_s - cutoff^g) + X_s + \varepsilon_s$, where the outcome variable *acceleration* is an indicator variable that equals 1 if the applicant participated in the accelerator, $higher$, a variable that equals 1 if the applicant ranks above the ranking cutoff of the capacity threshold in its generation, and 0 otherwise, and $f(Rank_s - cutoff^g)$ is a fourth-degree polynomial of the transformed rank. The vertical line represents the ranking cutoff normalized at 0 for the modified ranking. Only observations ranking below 301 are included in the plot. The relatively poor fit of the polynomial for companies ranking around 150 (+50 in the plot) is not mechanically driven by the change in capacity threshold in generation 2: the estimated participation rate for companies ranking in positions 150, 155, and 159 is lower than the observed probability of 0.6 across generations 3 to 8. In unreported analysis, we checked whether the participants ranking in these positions are observationally different (they are not) and whether a discontinuity exists here (it doesn't). Alternative explanations for the poor fit include a statistical issue (i.e., we have information about only 7 generations, and in this sample, start-ups ranking around 150 happen to be of comparatively good quality) and checking thresholds by program officials (i.e., start-ups around 150 and 160 constitute the final checking threshold for judges, such that if some spots are still available, they are filled in with these).

Figure 2 – Balanced Sample around the Application Capacity Threshold



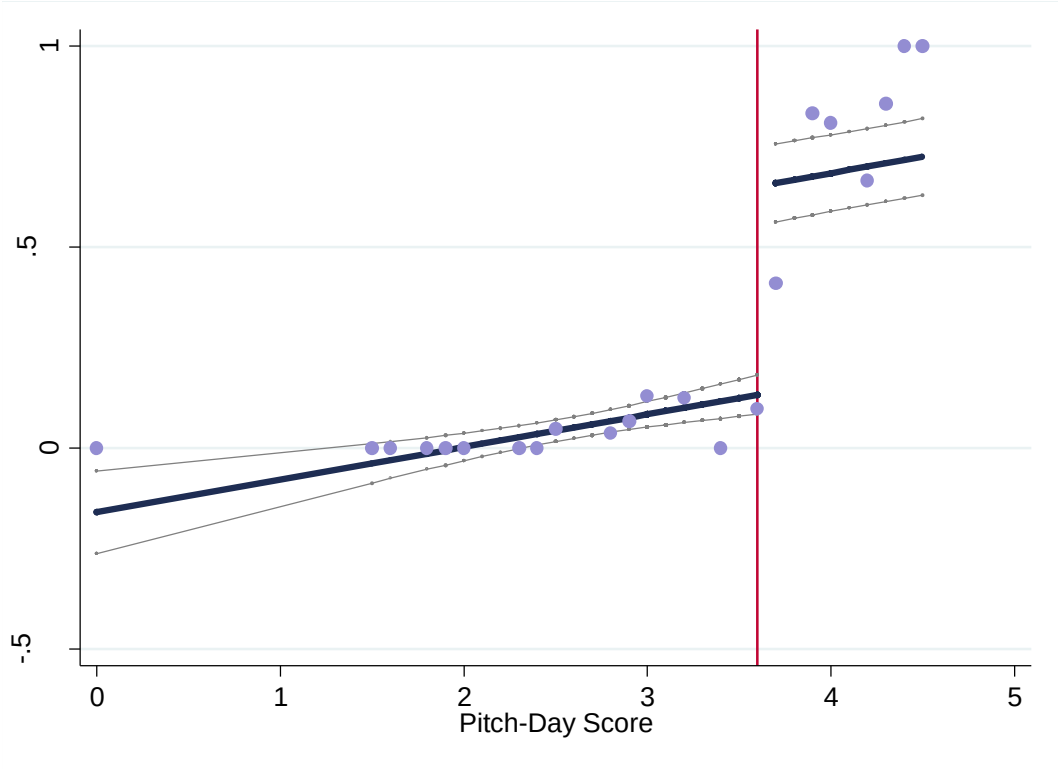
The figure shows evidence of a balanced sample near the capacity-threshold-ranking cutoff. *Chilean* (*Female*) is a dummy that equals 1 if the founder is Chilean (Female), *Employees Before* is the number of workers the start-up reported at the time of application (censored at 10), *Capital Raised Before* is a dummy that equals 1 if the start-up fundraised before potential participation in the program, *Prototype* equals 1 if the start-up has a prototype in development, and *Young* equals 1 if the start-up is less than a year old. All variables are as of the application date. The plots show averages grouped in bins of 10 applicants (dots), and the fitted values and 90% confidence interval from the regression $outcome_s = \delta + \gamma higher_s + f(Rank_s - cutoff^g) + X_s + \varepsilon_s$, with each of these variables as outcomes, $higher$, a variable that equals 1 if the applicant ranks above the ranking cutoff of the capacity threshold in its generation, and 0 otherwise, and $f(Rank_s - cutoff^g)$ is a fourth-degree polynomial of the transformed rank. The vertical line represents the ranking cutoff normalized at 0 for the modified ranking.

Figure 3 – Density of Judges' Application Scores



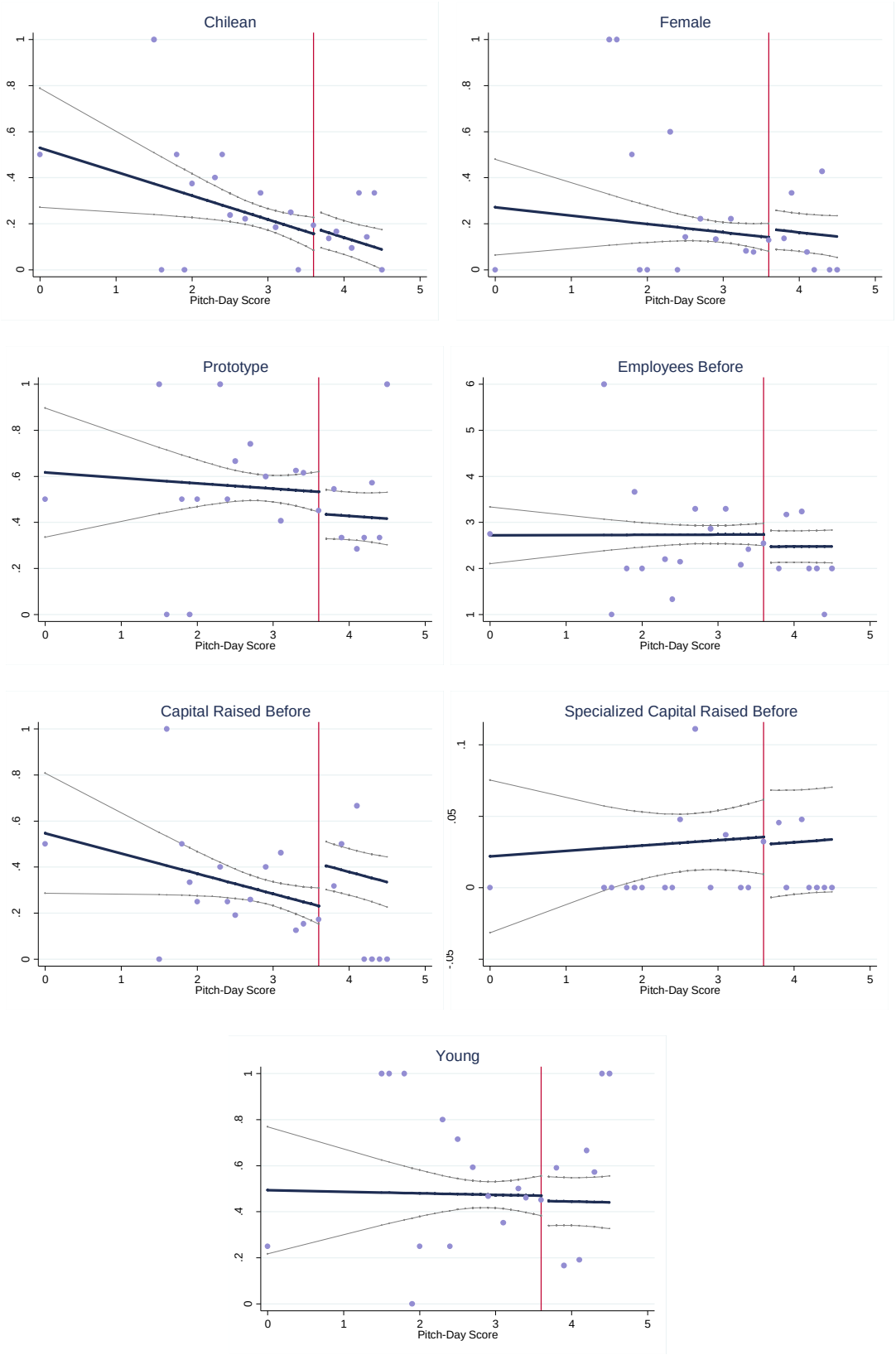
The figure presents a finely gridded histogram of the normalized application scores. For each applicant, the score of the capacity-threshold-ranking company (of its generation) is subtracted from the application score. Judges give applications a score from 1 to 10. Average scores range in practice from 1.28 to 8.9. The null hypothesis of no discontinuity in the distribution of the normalized application scores at the threshold cannot be rejected: the t-statistic from the McCrary test is -0.262. The McCrary test uses a local linear regression of the histogram separately on either side of the threshold to accommodate the discontinuity. For more detail, see McCrary (2008).

Figure 4 – Fraction of Schooled Participants



The figure shows the average fraction of schooled participants in bins of 0.2 pitch-day scores, and the fitted values and 90% confidence interval from the regression $school_s = \tau + \mu Above_{3.5} + g(Pitch\_Day\ \ Score_s) + \varepsilon_s$, where the outcome variable *school* is an indicator variable that equals 1 if the participant was schooled, $Above_{3.6}$ is an indicator variable that equals 1 if the participant scored above 3.6 on the pitch day, and $g(Pitch\_Day\ \ Score_s)$ is a second-degree polynomial of the pitch-day score. The vertical line represents the implicit score cutoff of 3.6.

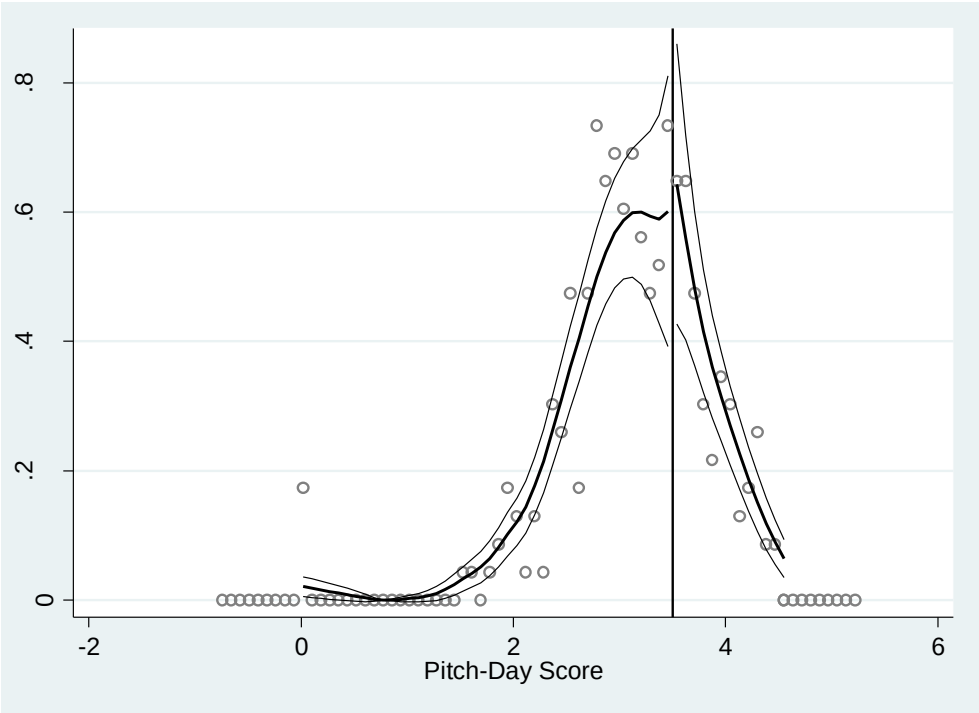
Figure 5– Balanced Sample around the 3.6 Pitch-Day-Score Cutoff



The figure shows evidence of a balanced sample near the pitch-day cutoff for participants. *Chilean* (*Female*) is a dummy that equals 1 if the founder is Chilean (Female), *Employees Before* is the number

of workers the start-up reported at the time of application (censored at 10), *Capital Raised Before* is a dummy that equals 1 if the start-up fundraised before potential participation in the program, *Prototype* equals 1 if the start-up has a prototype in development, and *Young* equals 1 if the start-up is less than a year old. All variables are as of the application date. Plots show averages grouped in bins of 0.2 in pitch-day score. The plots also show the fitted values and 90% confidence interval of a modified versions of the regression in equation (2), $covariate = \sigma + \omega Above_{3.6} + \breve{g}(Pitch\_Day\ Score) + \epsilon$, with each of these variables as outcomes, $Above_{3.6}$ is an indicator variable that equals 1 if the participant scored above 3.6 on the pitch day, and $f(Pitch\_Day\ Score)$ is a first-degree polynomial of the pitch-day score. The vertical line represents the implicit score cutoff of 3.6.

Figure 6 – Density of Pitch-Day Scores



The figure presents a finely gridded histogram of the pitch-day scores for all participants looking to qualify for the entrepreneurship school. Judges give applications a score from 1 to 5. Average scores range in practice from 0 to 4.45. The null hypothesis of no discontinuity in the distribution of the normalized application scores at the threshold cannot be rejected: the t-statistic from the McCrary test is -0.191. The McCrary test uses a local linear regression of the histogram separately on either side of the threshold to accommodate the discontinuity. For more detail, see McCrary (2008).

Table 1 - Composition Sample: Start-up Characteristics at Application by Generation

Panel A: Applicants and Participants

Generation	Applicants	Rejections	Selections	Participants	Competed in Pitch Day	Schooled
1	126	40	86	64		
2	474	324	150	125		
3	394	295	99	85		
4	472	374	98	74	62	13
5	655	554	101	90	80	15
6	581	476	105	95	89	18
7	556	456	100	83	45	13
Total	3,258	2,519	739	616	276	59

Panel B: Capital Raised

	Start-Up Chile									ED
	1	2	3	4	5	6	7	Total	%	%
-	1	462	3	13	0	0	0	479		
No (Bootsrapped)	107	10	290	354	492	450	357	2,060	74.13	79.28
< 50K	10	1	72	72	116	92	134	497	17.88	10.57
>50K	8	1	29	33	47	39	65	222	7.99	10.15
Total	126	474	394	472	655	581	556	3,258		

Panel C: Number of Full-Time Workers

	Start-Up Chile									ED
	1	2	3	4	5	6	7	Total	%	%
-	126	474	394	9	6	1	0	1,010		
<5	0	0	0	438	596	543	486	2063	91.77	68.54
5-9	0	0	0	23	45	36	66	170	7.56	16.85
10+	0	0	0	2	8	1	4	15	0.67	14.62
Total	126	474	394	472	655	581	556	3,258		

Panel D: Start-up Age

	Start-Up Chile									ED
	1	2	3	4	5	6	7	Total	%	%
-	0	2	0	9	6	1	0	18		
Less than 6 months	66	276	231	276	389	352	233	1,823	56.27	21.86
6-12 months	30	119	108	135	204	174	250	1,020	31.48	29.36
12-24 months	19	51	33	52	56	54	73	338	10.43	17.10
More than 2 years	11	26	22	0	0	0	0	59	1.82	31.68
Total	126	474	394	472	655	581	556	3,258		

Table 1 describes the composition of the sample, which includes 3,258 applicant start-ups to the accelerator. It also describes average applicants to ecosystem accelerators worldwide under the heading "ED," based on information from the Emory Entrepreneurship Database. Percentages are calculated over the number of non-missing responses. Observations are at the start-up level.

Table 2 - Composition Sample: Founder Characteristics at Application by Generation

Panel A: Location

	Start-Up Chile									ED
	1	2	3	4	5	6	7	Total	%	%
-	4	82	1	4	3	0	0	94		
Africa	2	4	0	2	7	4	2	21	0.7	19.1
Asia	10	23	22	40	47	51	80	273	8.6	19.1
Europe	26	81	79	82	94	110	101	573	18.1	6.6
N. America	56	142	118	122	112	106	103	759	24.0	34.8
Oceania	2	8	6	6	12	6	5	45	1.4	0.4
S. America (exc. Chile)	23	54	73	138	180	138	213	819	25.9	19.4
Chile	3	80	95	78	200	166	52	674	21.3	0.6
Total	126	474	394	472	655	581	556	3,258		

Panel B: Age

	Start-Up Chile									ED
	1	2	3	4	5	6	7	Total	%	%
-	138	462	394	472	136	65	10	1,677		
Younger than 25	0	0	0	0	80	86	70	236	14.9	10.6
Between 25 and 30	0	0	0	0	193	207	225	625	39.5	21.73
Between 30 and 35	0	0	0	0	147	122	141	410	25.9	21.64
Between 35 and 40	0	0	0	0	56	57	64	177	11.2	15.24
Older than 40	0	0	0	0	43	44	46	133	8.4	30.79
Total	126	474	394	472	655	581	556	3,258		

Panel C: Gender

	Start-Up Chile									ED
	1	2	3	4	5	6	7	Total	%	%
-	5	97	76	305	439	83	347	1,352		
Female	8	49	47	24	27	78	28	261	13.7	28.6
Male	113	328	271	143	189	420	181	1,645	86.3	71.4
Total	126	474	394	472	655	581	556	3,258		

Table 2 describes the composition of the sample across different characteristics of the founder. For those applicant start-ups with multiple founders, only the characteristics of the founder leader (self-reported in application) are described. It also describes average applicants to ecosystem accelerators worldwide under the heading "ED," based on information from the Emory Entrepreneurship Database.

Table 3- Main Variables

Panel A. Summary Statistics

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Application Form					
Age	1,582	30.33	6.76	19.00	84.00
Chilean	3,258	0.21	0.41	0.00	1.00
Female	1,906	0.14	0.34	0.00	1.00
Employees Before	2,248	2.46	1.46	1.00	10.00
Capital Raised Before	2,779	0.26	0.44	0.00	1.00
Prototype	3,109	0.74	0.44	0.00	1.00
Selection Process					
Rank	3,258	260.91	164.33	1.00	656
Pitch-day Score	276	3.14	0.70	0.00	4.50
Acceleration	3,258	0.19	0.39	0.00	1.00
School	3,258	0.02	0.14	0.00	1.00
Web-based Outcomes					
Web Indicator Capital	3,258	0.026	0.159	0.00	1.00
Web Capital Raised	3,258	0.491	2.336	0.00	16.93
Web Employees	3,258	0.534	1.939	0.00	11.00
Web Traction	3,258	0.063	0.284	0.00	4.78
Web Survival	3,258	0.212	0.409	0.00	1.00
Survey Applicants Outcomes					
Survey A. Indicator Capital	319	0.658	0.475	0.00	1.00
Survey A. Capital Raised	318	6.973	5.246	0.00	14.51
Survey A. Valuation	318	7.664	6.512	0.00	16.52
Survey A. Employees	319	0.542	0.799	0.00	3.43
Survey A. Traction	319	3.673	4.610	0.00	13.12
Survey A. Survival	319	0.618	0.487	0.00	1.00
Survey Participants Outcomes					
Survey P. Indicator Capital	145	0.579	0.495	0.00	1.00
Survey P. Capital Raised	145	7.118	6.262	0.00	18.60
Survey P. Valuation	145	4.673	6.957	0.00	19.56
Survey P. Employees	145	1.333	1.255	0.00	4.812
Survey P. Traction	145	6.823	6.142	0.00	16.81
Survey P. Survival	145	0.641	0.481	0.00	1.00

The table presents summary statistic of the main variables used in the analysis. The first and second sections include variables extracted from applications and Start-Up Chile records. The third section includes web-based outcome variables. The last two sections include survey-based outcome variables. The first survey was conducted on all applicants during October 2014, and the second survey was conducted on all participants during the first quarter of 2016. For variable definitions, see section 2 of the main paper.

Table 4—Correlation Web-based and Survey-based performance proxies

Panel A- Correlation of fundraising proxies

	Survey A. Indicator Capital	Survey P. Indicator Capital	Survey A. Capital Raised	Survey P. Capital Raised
Web Indicator Capital	0.04	0.17**		
	(0.53)	(0.05)		
Web Capital Raised			0.11**	0.34***
			(0.05)	(0.00)
Observations	319	145	319	145

Panel B- Correlation of scale proxies

	Survey A. Employees	Survey P. Employees	Survey A. Traction	Survey P. Traction
Web Employees	0.13**	0.23***		
	(0.02)	(0.01)		
Web Traction			0.10*	0.2***
			(0.07)	(0.00)
Observations	319	145	319	145

Panel C- Correlation of survival proxies

	Survey A. Survival	Survey P. Survival
Web Survival	0.21***	-0.02
	(0.00)	(0.80)
Observations	319	145

The table presents correlations across web-based and survey-based venture performance metrics. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5- Univariate Analysis of Applicants and Participants

Panel A- Accelerator Participants and Non-participants

	Participants			Non-participants			Difference
Variable	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	
Age	264	30.73	6.50	1,318	30.25	6.81	0.47
Chilean	616	0.19	0.40	2,642	0.21	0.41	0.02
Female	567	0.15	0.36	1,339	0.13	0.34	0.02
Employees Before	340	2.74	1.71	1,908	2.41	1.40	0.33***
Capital Raised Before	488	0.32	0.47	2,291	0.25	0.43	0.07***
Prototype	616	0.45	0.50	2,642	0.50	0.50	-0.05*
Web Indicator Capital	616	0.08	0.27	2,642	0.01	0.12	0.06***
Web Capital Raised	616	1.48	3.86	2,642	0.26	1.73	1.22***
Web Employees	616	1.06	2.66	2,642	0.41	1.71	0.65***
Web Traction	616	0.13	0.43	2,642	0.05	0.24	0.08***
Web Survival	616	0.46	0.50	2,642	0.15	0.36	0.31***
Survey A. Indicator Capital	100	0.77	0.48	219	0.61	0.49	0.16***
Survey A. Capital Raised	100	8.67	4.86	219	6.19	5.24	2.48***
Survey A. Valuation	100	8.39	6.60	219	7.33	6.46	1.07
Survey A. Employees	100	0.61	0.84	219	0.51	0.78	0.09
Survey A. Traction	100	3.87	4.80	219	3.58	4.53	0.29
Survey A. Survival	100	0.74	0.44	219	0.56	0.50	0.18***

Panel B- Schooled Participants and Non-schooled Participants

	Schooled			Non-Schooled			Difference
Variable	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	
Age	44	29.68	5.44	166	30.95	6.93	-1.27
Chilean	59.	0.10	0.30	217	0.24	0.43	-0.14**
Female	48	0.13	0.33	199	0.18	0.38	-0.05
Employees Before	59	2.69	1.83	215	2.67	1.68	0.03
Capital Raised Before	59	0.42	0.50	215	0.28	0.45	0.14**
Prototype	59	0.36	0.48	217	0.57	0.50	-0.21**
Web Indicator Capital	59	0.17	0.38	217	0.08	0.27	0.09**
Web Capital Raised	59	3.10	5.40	217	1.47	3.78	1.63***
Web Employees	59	1.20	2.71	217	0.82	2.29	0.38
Web Traction	59	0.22	0.58	217	0.07	0.18	0.14***
Web Survival	59	0.64	0.48	217	0.54	0.50	0.10
Survey P. Indicator Capital	35	0.83	0.38	110	0.50	0.50	0.33***
Survey P. Capital Raised	35	10.34	5.05	110	6.09	6.28	4.25***
Survey P. Valuation	35	6.50	7.68	110	4.09	6.65	2.41*
Survey P. Employees	35	1.75	1.32	110	1.20	1.21	0.55**
Survey P. Traction	35	7.84	6.23	110	6.50	6.11	1.34
Survey P. Survival	35	0.74	0.44	110	0.61	0.49	0.14

This table presents mean differences for the main variables used in the analysis across different subgroups of applicants. Observations are at the applicant level. For variable definitions, see section 2. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6- Discontinuity Probability of Acceleration Around the Capacity Threshold

	(1)	(2)	(3)	(4)	(5)
Higher	0.166***	0.169***	0.210***	0.278***	0.189**
	(0.041)	(0.041)	(0.056)	(0.103)	(0.085)
Constant	0.311***	0.285***	0.349***	0.337***	
	(0.022)	(0.043)	(0.075)	(0.083)	
Observations	3,258	3,258	1,519	1,519	513
R-squared	0.399	0.400	0.476	0.476	
Generation FE	No	Yes	Yes	Yes	No
Covariates	No	No	Yes	Yes	No

This table shows the discontinuity in the probability of acceleration around the capacity-threshold-ranking cutoff. Columns (1)-(3) report the constant (δ) and the coefficient of $higher$ (γ) of the regression, $acceleration = \delta + \gamma higher + f(Rank - cutoff) + \varepsilon$, where $acceleration$ is a variable that equals 1 if the applicant participated in the accelerator, on $higher$, a variable that equals 1 if the applicant ranks higher than the capacity threshold in its generation, and 0 otherwise, and $f(Rank - cutoff)$ is a fourth-degree polynomial of the modified rank (i.e., $z = Rank - cutoff$). To conserve space, the estimated coefficients for the polynomial terms are not presented in the table. Columns (2) and (3) present the estimates when the regression additionally includes generation fixed effects and covariates, respectively. The covariates included are *Chilean*, *Female*, *Money raised before*, *Prototype*, and *Young*. Column (4) allows the degree of the polynomial to differ on either side of the threshold, 4 and 3 (left and right), respectively. Column (5) reports the estimates when using a local linear estimation following Calonico et al. (2014) (CCT). The optimal bandwidth-selection algorithm is based on Calonico et al. (2014) (CCT) and generates a bandwidth of 73 (except generation 2 with 75) observations around the capacity threshold. Robust standard errors are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7- Start-up Fundraising and Basic Acceleration Services

Panel A– Web-based Fundraising Metrics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Web Capital Indicator				Web Capital Raised			
Acceleration	0.062***	0.068***	0.049	-0.072	1.215***	1.222***	0.117	-1.683
	(0.011)	(0.015)	(0.103)	(0.141)	(0.159)	(0.214)	(1.497)	(2.079)
Observations	3,258	1,519	3,258	1,519	3,258	1,519	3,258	1,519
R-squared	0.024	0.034	0.025		0.042	0.068	0.028	
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Estimate	OLS	OLS	RDD	RDD	OLS	OLS	RDD	RDD
#80% power	168	141	271	126	96	94	10,284	50

Panel B– Applicants' Survey-based Fundraising Metrics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Survey A. Indicator Capital				Survey A. Capital Raised				Survey A. Valuation			
Acceleration	0.163***	0.216***	-0.593	-6.429	2.477***	3.238***	-6.460	-77.348	1.068	1.780	1.672	-25.639
	(0.054)	(0.070)	(0.762)	(21.209)	(0.601)	(0.791)	(8.566)	(257.324)	(0.791)	(1.094)	(8.809)	(102.864)
Observations	319	184	319	184	318	184	318	184	318	184	318	184
R-squared	0.025	0.102			0.048	0.137			0.006	0.125	0.008	
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Estimate	OLS	OLS	RDD	RDD	OLS	OLS	RDD	RDD	OLS	OLS	RDD	RDD
#80% power	247	141	19	<10	141	84	22	<10	1,389	501	568	<10

This table reports the effects of basic acceleration services (cash and co-working space) on venture performance. Estimates are based on the regression $outcome_s = \pi + \beta acceleration_s + \check{f}(Rank_s - cutoff^g) + \epsilon_s$, where $acceleration$ is a variable that equals 1 if the applicant participated in the accelerator. The outcome variable is specified in the title of the panel and at the top of each column, and the type of estimate (i.e., OLS or RDD) is reported at the bottom of each column. For the OLS estimate, the polynomials of the normalized ranking (i.e., $\check{f}(Rank_s - cutoff^g)$) are excluded from the estimation. For the RDD estimate, $acceleration$ is instrumented using $higher$, a variable that equals 1 if the applicant ranks higher than the capacity threshold in its generation. To conserve space, the estimated coefficients for the constant and the polynomial terms in the second stage are not presented in the table. The covariates included are *Chilean*, *Female*, *Money raised before*, *Prototype*, and *Young*. The bottom row reports back-of-the-envelope estimates of the sample size necessary to distinguish the estimated effect from zero with an 80% probability. We use the sampsi command in Stata based on data for non-participants to proxy for the baseline mean and standard deviation, using a significance level of 0.05 and the observed participants vs. non-participants ratio (0.27 Panel A, 0.46 Panel B). For columns with large point estimates, relative to the mean of non-participants, the estimated sample sizes are naturally small—i.e., less than 10 observations ensure a power of 80%. Robust standard errors are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8- Venture Scale and Survival, and Basic Acceleration Services

Panel A–Web-based Metrics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Web Traction				Web Employees				Web Survival			
Acceleration	0.079***	0.028	-0.039	0.042	0.655***	0.251*	-1.385	-1.609	0.305***	0.254***	0.272	0.184
	(0.018)	(0.021)	(0.203)	(0.211)	(0.112)	(0.149)	(1.375)	(1.704)	(0.021)	(0.027)	(0.238)	(0.265)
Observations	3,258	1,519	3,258	1,519	3,258	1,519	3,258	1,519	3,258	1,519	3,258	1,519
R-squared	0.012	0.028	0.003	0.029	0.017	0.049			0.085	0.121	0.094	0.130
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Estimate	OLS	OLS	RDD	RDD	OLS	OLS	RDD	RDD	OLS	OLS	RDD	RDD
#80% power	418	3,347	1,701	1,497	319	2,161	72	54	67	96	84	182

Panel B–Applicant Survey-based Metrics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Survey A. Traction				Survey A. Employees				Survey A. Survival			
Acceleration	0.288	-0.573	-6.842	-57.901	0.092	0.124	-1.319	-14.679	0.178***	0.188**	0.070	-2.469
	(0.569)	(0.759)	(7.350)	(189.238)	(0.099)	(0.131)	(1.461)	(47.034)	(0.055)	(0.077)	(0.654)	(9.498)
Observations	319	184	319	184	319	184	319	184	319	184	319	184
R-squared	0.001	0.149			0.003	0.058			0.029	0.169	0.031	
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Estimate	OLS	OLS	RDD	RDD	OLS	OLS	RDD	RDD	OLS	OLS	RDD	RDD
#80% power	10,121	2550	19	<10	3,018	1,666	17	<10	224	202	1444	<10

This table reports the effects of basic acceleration services (cash and co-working space) on venture performance. Estimates are based on the regression $outcome_s = \pi + \beta acceleration_s + \check{f}(Rank_s - cutoff^g) + \epsilon_s$, where $acceleration$ is a variable that equals 1 if the applicant participated in the accelerator. The outcome variable is specified in the title of the panel and at the top of each column, and the type of estimate (i.e., OLS or RDD) is reported at the bottom of each column. For the OLS estimate, the polynomials of the normalized ranking (i.e., $\check{f}(Rank_s - cutoff^g)$) are excluded from the estimation. For the RDD estimate, $acceleration$ is instrumented using $higher$, a variable that equals 1 if the applicant ranks higher than the capacity threshold in its generation. To conserve space, the estimated coefficients for the constant and the polynomial terms in the second stage are not presented in the table. The covariates included are *Chilean*, *Female*, *Money raised before*, *Prototype*, and *Young*. The bottom row reports back-of-the-envelope estimates of the sample size necessary to distinguish the estimated effect from zero with an 80% probability. We use the sampsi command in Stata based on data for non-participants to proxy for the baseline mean and standard deviation, using a significance level of 0.05 and the observed participants vs. non-participants ratio (0.27 Panel A, 0.46 Panel B). For columns with large point estimates, relative to the mean of non-participants, the estimated sample sizes are naturally small—i.e., less than 10 observations ensure a power of 80%. Robust standard errors are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 9- Discontinuity Probability of Schooling around Pitch-Day Score of 3.6

	(1)	(2)	(3)	(4)	(5)
$Above_{3.6}$	0.519***	0.508***	0.509***	0.393***	0.351***
	(0.072)	(0.072)	(0.073)	(0.097)	(0.105)
Constant	0.124***	0.102***	0.101***	0.168***	0.125***
	(0.028)	(0.029)	(0.029)	(0.036)	(0.035)
Observations	276	276	276	276	223
R-squared	0.398	0.434	0.435	0.410	0.172
Generation FE	No	Yes	Yes	No	No
Covariates	No	No	Yes	No	No

This table shows the discontinuity in the probability of participation in the entrepreneurship school around the pitch-day-score cutoff. Columns (1)-(3) report the constant (τ) and the coefficient of $Above_{3.6}$ (μ) of the regression: $school = \tau + \mu Above_{3.6} + g(Pitch\_Day\ Score) + \varepsilon$, where the outcome variable *school* is an indicator variable that equals 1 if the participant was selected into the entrepreneurship school, $Above_{3.6}$ is an indicator variable that equals 1 if the participant scored above 3.6 during the pitch day, and $g(Pitch\_Day\ Score)$ is a first -degree polynomial of the pitch-day score. To conserve space, the estimated coefficients for the polynomial terms are not presented in the table. Columns (2) and (3) present the estimates when the regression additionally includes generation fixed effects and covariates, respectively. The covariates included are *Chilean*, *Female*, *Capital raised before*, *Prototype*, and *Young*. In column (4), we include a second-degree polynomial and in column (5), the observations are restricted to start-ups that scored between 3 and 4 on the pitch day. Robust standard errors are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 10- Venture Fundraising at the Entrepreneurship School

Panel A– Web-based Fundraising Metrics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Web Capital Indicator				Web Capital Raised			
School	0.091*	0.088*	0.210*	0.207*	1.633**	1.560**	3.034*	3.008**
	(0.052)	(0.052)	(0.118)	(0.115)	(0.745)	(0.683)	(1.576)	(1.504)
Observations	276	276	276	276	276	276	276	276
R-squared	0.016	0.121		0.096	0.025	0.213	0.007	0.195
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Estimate	OLS	OLS	RDD	RDD	OLS	OLS	RDD	RDD

Panel B-Participants' Survey-based Fundraising Metrics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Survey P. Capital Indicator				Survey P. Capital Raised				Survey P. Valuation			
School	0.329***	0.346***	0.455**	0.422**	4.246***	4.501***	6.253**	5.739**	2.411*	2.218	5.520*	4.984
	(0.080)	(0.080)	(0.199)	(0.210)	(1.038)	(1.031)	(2.533)	(2.661)	(1.436)	(1.455)	(3.284)	(3.497)
Observations	145	145	145	145	145	145	145	145	145	145	145	145
R-squared	0.081	0.106	0.084	0.112	0.085	0.118	0.084	0.124	0.022	0.056	0.010	0.050
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Estimate	OLS	OLS	RDD	RDD	OLS	OLS	RDD	RDD	OLS	OLS	RDD	RDD

This table reports the effects of entrepreneurship schooling (bundled with the basic services) on venture performance. Estimates are based on the regression $outcome_s = \pi + \beta school_s + \breve{g}(Pitch\_Day\ Score_s) + \epsilon_s$, where $school_s$ is a variable that equals 1 if the participant was selected into the entrepreneurship school. The outcome variable is specified in the title of the panel and at the top of each column, and the type of estimate (i.e., OLS or RDD) is reported at the bottom of each column. For the OLS estimate, the polynomials (i.e., $\breve{g}(Pitch\_Day\ Score_s)$) are excluded from the estimation. For the RDD estimate, $school_s$ is instrumented using $Above_{3.6}$, a variable that equals 1 if the participant scored more than 3.6 on the pitch day. To conserve space, the estimated coefficients for the constant and the polynomial terms in the second stage are not presented in the table. The covariates included are *Capital raised before*, *Prototype*, and *Young*. Robust standard errors are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 11- Venture Scale and Survival at the Entrepreneurship School

Panel A–Web-based Metrics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Web Traction				Web Employees				Web Survival			
School	0.142*	0.134**	0.238*	0.229**	0.379	0.400	1.985*	1.890*	0.100	0.066	0.087	0.107
	(0.077)	(0.063)	(0.128)	(0.115)	(0.384)	(0.349)	(1.086)	(1.124)	(0.071)	(0.068)	(0.183)	(0.180)
Observations	276	276	276	276	276	276	276	276	276	276	276	276
R-squared	0.034	0.220	0.023	0.210	0.004	0.046		0.007	0.007	0.108	0.010	0.109
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Estimate	OLS	OLS	RDD	RDD	OLS	OLS	RDD	RDD	OLS	OLS	RDD	RDD

Panel B–Participant Survey-based Metrics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Survey P. Traction				Survey P. Employees				Survey P. Survival			
School	1.345	1.399	4.226	3.662	0.548**	0.580**	0.871	0.693	0.134	0.143	-0.044	-0.142
	(1.197)	(1.223)	(2.733)	(2.868)	(0.250)	(0.252)	(0.550)	(0.581)	(0.088)	(0.090)	(0.219)	(0.232)
Observations	145	145	145	145	145	145	145	145	145	145	145	145
R-squared	0.009	0.039		0.036	0.035	0.088	0.052	0.098	0.014	0.050		0.019
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Estimate	OLS	OLS	RDD	RDD	OLS	OLS	RDD	RDD	OLS	OLS	RDD	RDD

This table reports the effects of entrepreneurship schooling (bundled with the basic services) on venture performance. Estimates are based on the regression $outcome_s = \pi + \beta school_s + \breve{g}(Pitch\_Day\ Score_s) + \epsilon_s$, where $school_s$ is a variable that equals 1 if the participant was selected into the entrepreneurship school. The outcome variable is specified in the title of the panel and at the top of each column, and the type of estimate (i.e., OLS or RDD) is reported at the bottom of each column. For the OLS estimate, the polynomials (i.e., $\breve{g}(Pitch\_Day\ Score_s)$) are excluded from the estimation. For the RDD estimate, $school_s$ is instrumented using $Above_{3.6}$, a variable that equals 1 if the participant scored more than 3.6 on the pitch day. To conserve space, the estimated coefficients for the constant and the polynomial terms in the second stage are not presented in the table. The covariates included are *Capital raised before*, *Prototype*, and *Young*. Robust standard errors are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 12- Regional Effects: New-Business Registration Rates

	(1)	(2)	(3)	(4)
	Number	Number	Log.	Log.
Post 2010× Contiguous	0.314***		0.024***	
	(0.097)		(0.005)	
Post 2010× Contiguous ×Venture		0.483**		0.060***
		(0.213)		(0.022)
Observations	426,180	426,180	426,180	426,180
R-squared	0.043	0.900	0.062	0.783
Comuna FE	Yes		Yes	
Year FE	Yes		Yes	
Industry×Year FE		Yes		Yes
Industry×Comuna FE		Yes		Yes
Comuna×Year FE		Yes		Yes

This table reports the regional effects of the program on new-business registration rates. Estimates in columns (1) and (3) are based on the regression $\text{New Business}_{cit} = \gamma_t + \gamma_c + \text{Post\_2010}_t \times \text{Contiguous}_c + \varepsilon_{cit}$, where $\text{New Business}_{cit}$ corresponds to the number and logarithm of new businesses registered in comuna c, industry i, and time t, respectively, and Post_2010 is a dummy that equals 1 after 2010 (i.e., the inception year of the program) and Contiguos_c equals 1 if the comuna neighbors the comuna where the program is headquartered. In detail, the contiguos comunas correspond to Independencia, Providencia, Nunoa, San Joaquin, San Miguel, Pedro Aguirre Cerda, Estacion Central, Quinta Normal, and Santiago Central. Estimates in columns (2) and (4) are based on the regression $\text{New Business}_{cit} = \gamma_{it} + \gamma_{ic} + \gamma_{cy} + \text{Post\_2010}_t \times \text{Contiguous}_c \times \text{Venture}_i + \varepsilon_{cit}$, where Venture_i equals 1 for all those industries similar to the industries of the program's participants (i.e., venture industries): activities of experimental research and development, auxiliary transport activities, business-to-business services, information services, other types of financial intermediation, and retail trade not realized in shops, telecommunications, and travel agencies. Robust standard errors are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix A.1. Parallel Between Business Schools and Business Accelerators

Schooling Services	Business School	Business Accelerators
Social Clout	Certification from selection, graduation from business school, diploma. Preferential access to peer and professor networks.	Certification from selection, graduation from entrepreneurship school, exposure to community. Preferential access to peer and staff networks.
Structured Accountability	Setting learning goals, class work, homework, exams.	Setting strategic tasks, monthly follow-up meetings, demo-day
Self-efficacy	Self-confidence from selection and graduation (in the form of business self-efficacy)	Self-confidence from selection and graduation (in the form of entrepreneurial self-efficacy)
Know-how	Developing and growing a company through classes, professors, guest speakers, career office, advisors, fellow classmates.	Developing and growing a start-up through workshops, staff, guest speakers, industry experts, mentors, fellow participants.

Note: Although business schools have traditionally not offered entrepreneurship-related instruction in their curriculum, in recent years some business schools have started to include it.

Appendix A.2. Composition Sample: Start-up Characteristics at Application by Generation

Table A2.1 -

Panel A: Industry of Start-up

	Generation								
	1	2	3	4	5	6	7	Total	%
-	5	95	64	135	206	83	347	935	
Consulting	0	0	0	0	3	0	0	3	0.13
E-commerce	32	81	54	57	73	95	35	427	18.38
Education	0	0	36	26	45	32	25	164	7.06
Energy & Clean Technology	6	24	10	4	13	10	9	76	3.27
Finance	6	12	10	7	5	12	5	57	2.45
Healthcare & Biotechnology	5	0	12	16	15	21	12	81	3.49
IT & Enterprise Software	29	97	59	48	57	67	30	387	16.66
Media	0	0	17	22	15	33	7	94	4.05
Mobile & Wireless	12	53	24	25	42	36	20	212	9.13
Natural Resources	0	0	6	4	13	10	2	35	1.51
Other	22	82	32	35	40	48	21	280	12.05
Social Enterprise	9	30	14	15	20	21	8	117	5.04
Social Media/Social Network	0	0	40	55	81	79	28	283	12.18
Tourism	0	0	16	23	27	34	7	107	4.61
Total	126	474	394	472	655	581	556	3,258	

Panel B: Start-up Development Stage

	Generation								
	1	2	3	4	5	6	7	Total	%
-	126	14	2	2	5	0	0	149	
Concept	0	118	100	124	155	137	53	687	22.10
Scaling Sales	0	21	11	24	19	18	35	128	4.12
Functional Product with Users	0	83	69	87	140	126	195	700	22.52
Prototype in Development	0	238	212	235	336	300	273	1,594	51.27
Total	126	474	394	472	655	581	556	3,258	

This table describes the composition of the sample, which includes 3,258 applicant start-ups to the accelerator. Percentages are calculated over the number of non-missing responses. Observations are at the start-up level. Panels A and B show the sample composition across different characteristics of start-up applicants.

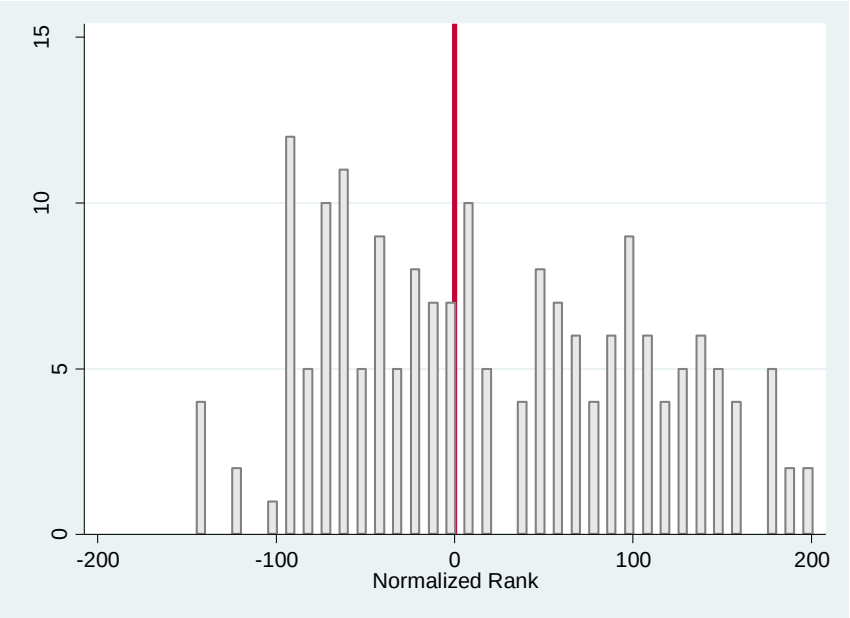
Appendix A.3. Survey-Performance Outcomes for Start-Up Chile Applicants

In October of 2014, we sent an email to all applicants to Start-Up Chile, generations 1–7, including the 3,258 applicants in our sample, inviting them to participate in our survey. Companies in generation 1 applied to the program in March 2011, and those from generation 7 applied in March 2013. Generation 7 graduated from the program in January 2014. Therefore, the surveyed population of start-ups had a considerable amount of time since inception and graduation from Start-Up Chile. Of the total number of invitations, 184 bounced due to email addresses that no longer existed, likely because individuals who applied to the program did so using their start-up's Internet domain name, which may cease to exist when the venture is no longer pursued. Of the remaining population, 332 submitted fully completed surveys; the rest initiated but did not submit the survey, or opted out. The response rate ranged from 6% for generation 1 to 16% for generation 7. The larger response rate for latter generations probably reflects a greater sense of commitment to Start-Up Chile for those more recently involved in the program.

We dropped 176 observations because the respondents did not answer beyond the first few questions. We further dropped 24 observations because of response ambiguity, that is, survey respondents who declared they had participated in Start-Up Chile, but who were not in the registry of the program, or who declared they had not participated in Start-Up Chile, but who were in the program's registry. This process left us with a total of 298 valid survey responses.

Figure A3.1 plots the distribution of the respondent's rank around the capacity threshold. The distribution exhibits "patches;" for example, no companies ranking between 30-40 below the capacity threshold completed the survey. Of the 298 survey responses, 198 correspond to non-participants, 100 correspond to program participants, and 13 to schooled ventures. Of respondents with a normalized ranking between -75 and +75 (-50 and +50), 62 (36) correspond to participants, and 39 (5) to non-participants.

Figure A3.1. Distribution of Survey Respondents across the Normalized Rank



The figure plots the distribution of survey respondents across the normalized rank. It plots the number of respondents in bins of 10 ranks, where observations are at the start-up level. The total number of survey respondents is 298.

Table A3.1- Distribution of Survey Applicant Respondents

Gen.	Survey	Non-resp.	Ambig.	Resp.	Participants	Competed Pitch Day	Schooled
1	126	118		8	3		
2	474	443		31	13		
3	394	366	1	27	9		
4	472	438	5	29	13	13	4
5	655	598	5	52	17	14	2
6	581	517	4	60	15	15	3
7	556	459	6	91	30	15	4
Total	3,258	2,939	21	298	100	57	13

The table describes the composition of the survey respondents, which includes a final sample of 298 ventures. Observations are at the start-up level. The table summarizes the number of respondents who participated in the accelerator, those who competed during the pitch day, and those who were ultimately selected into the entrepreneurship school.

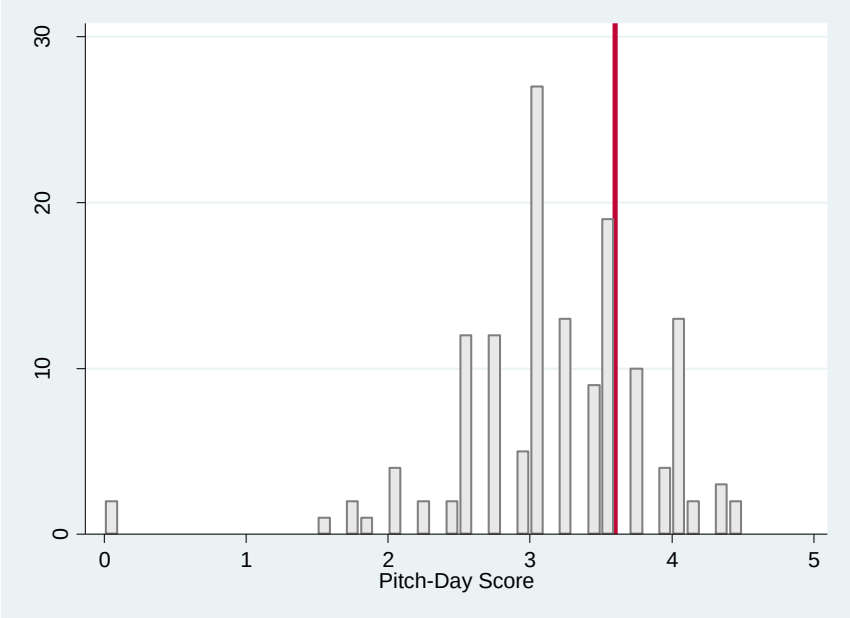
Table A3.2 Applicant Survey Questions and Variable Definitions

Question	Variable names and definition
What is the fate of the start-up? Potential answers: 1. The company is alive, but I sold or gave my shares to someone else. 2. The company is alive, and I still own shares, but I no longer work primarily at that company. 3. The company was sold to (or it merged with) another company, and it no longer exists as an independent entity. 4. The company is alive and I am currently working there. 5. I pivoted this company into my current start-up. 6. The start-up is currently on stand-by while I am working on starting a new company. 7. I closed that company and have started a new company. 8. I closed that company and I am not currently working at my own startup. 9. The start-up is currently on stand-by (nobody is working on it), and I am not currently working at my own startup.	*Survey A. Survival* equals 1 if answer was "The company is alive and I am currently working there," and 0 otherwise.
What are your accumulated sales in US dollars during the last 6 months?	*Survey A. Traction* equals logarithm of reported sales.
What is your start-up's "people count" for the following categories? • Full-time founders • Part-time founders • Full-time employees • Part-time employees	*Survey A. Employees* equals the number of reported full-time employees.
How much money have you raised in US dollars since the beginning of your start-up?	*Survey A. Indicator Capital* equals 1 if answer is not zero, and 0 otherwise. *Survey A. Capital Raised* equals logarithm of reported capital raised.
What is your estimated pre-money valuation in US dollars?	*Survey A. Valuation* equals logarithm of reported pre-money valuation.

Appendix A.4. Survey-Performance Outcomes for Start-Up Chile Participants

During the first quarter of 2016, the Start-Up Chile staff contacted Start-Up Chile alumni. Alumni were contacted by email and phone, requesting them to collaborate with a data acquisition effort. Table A4.1 shows the distribution of survey respondents (72.4% response rate). Table A4.2 shows the list of questions that were asked of participants. Figure A4.1 plots the distribution of respondent-participants' pitch-day score. Of the 183 participants from generations 4–7 who answered the survey, 145 participated in the pitch-day competition.

Figure A4.1. Distribution of Survey Respondents across the Pitch-Day Score



The figure plots the distribution of survey respondents across the pitch-day score. It plots the number of respondents in bins of 0.1 scores, where observations are at the start-up level. The total number of survey respondents in generations 4-7 is 183. Respondents are restricted to 145 start-ups that participated during the pitch day and for which we observe the pitch-day score.

Table A4.1- Distribution of Survey Participant Respondents

Gen.	Surveyed	Non-Respondent	Respondent	Competed Pitch Day	Schooled
4	61	26	35	31	8
5	74	42	32	28	6
6	83	26	57	53	10
7	78	19	59	33	11
Total	296	113	183	145	35

Table A4.2 Participant Survey Questions and Variable Definitions

Question	Variable names and definition
Is your start-up active?	*Survey P. Survival* equals 1 if answer was "yes" and 0 otherwise.
How much have you sold worldwide (including Chile) in the last 12 months? (USD)	*Survey P. Traction* equals logarithm of reported sales.
How many employees does your startup have worldwide?	*Survey P. Employees* equals the number of reported full-time employees
How much public capital have you raised worldwide (including Chile) not considering the Start-Up Chile program? (USD)	*Survey P. Indicator Capital* equals 1 if answer is not zero, and 0 otherwise. *Survey P. Capital Raised* equals logarithm of reported capital raised.
How much is your startup worth according to your last formal valuation?	*Survey P. Valuation* equals logarithm of reported pre-money valuation