The Effects of Business Accelerators on Venture Performance: Evidence from Start-Up Chile

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Do business accelerators affect new venture performance? We investigate this question in the context of Start-Up Chile, an ecosystem accelerator. We focus on two treatment conditions typically found in business accelerators: basic services of funding and coworking space, and additional entrepreneurship schooling. Using a regression discontinuity design, we show that schooling bundled with basic services can significantly increase new venture performance. In contrast, we find no evidence that basic services affect performance on their own. Our results are most relevant for ecosystem accelerators that attract young and early-stage businesses and suggest that entrepreneurial capital matters in new ventures. (*JEL* G24, L26, M13)

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Business accelerators are an increasingly important institutional form of entrepreneurial ecosystems. Since the first investor-led accelerator debuted in 2005 (i.e., Y Combinator), thousands of business accelerators have sprung

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up worldwide.[1] These fixed-term, cohort-based programs offer start-ups a combination of cash, shared office space, and entrepreneurship schooling. Accelerators distinguish themselves from other early-stage financiers by their strong emphasis on entrepreneurship schooling, which is believed to provide "entrepreneurial capital" to participants who are otherwise lacking it. Although evidence about "managerial capital" constraints (e.g., Bruhn, Karlan, and Schoar 2010; Bloom and van Reenen 2010) seems to justify this emphasis, little rigorous evidence exists on the effect of business accelerators on new venture performance and on the importance of entrepreneurial capital in new firms.[2] This lack of evidence is particularly pressing given the importance of new ventures for economic development (Davis, Haltiwanger, and Schuh 1996; Haltiwanger, Jarmin, and Miranda 2013) and the relevant public and private resources being spent to foster entrepreneurial activity.[3]

This paper provides the first quasi-experimental evidence of the effect of business accelerator programs on new venture performance. It advances prior work on accelerators that, until now, has focused on these programs' conceptual definitions and has faced challenges in distinguishing the programs' effects from venture selection (Cohen and Hochberg 2014). This paper also provides first-time evidence on the importance of entrepreneurial capital in new ventures.[4] In contrast, prior related work has concentrated on the role of managerial capital in established firms and nontransformational ventures (cf. McKenzie and Woodruff 2014).

Business accelerators are an ideal context for studying the role of entrepreneurial capital in new ventures. Entrepreneurial capital refers to the set of skills and resources needed to start and grow nascent businesses. This type of capital can include know-how about seizing opportunities and growing a business (Bingham, Eisenhardt, and Furr 2007); cultivating a good reputation to attract employees, investors, and customers (Rao 1994; Zott and Huy 2007); and accessing valuable social networks (Granovetter 1973). Business accelerators' emphasis on entrepreneurship schooling has led practitioners to dub these institutions "the new business schools for entrepreneurs" (Golomb 2015).[5] The exact form of this schooling varies

[1] At least 4,397 institutions self-identify as an accelerator. See F6S available at https://www.f6s.com (last visited May 2016).

[2] By new ventures, we mean start-ups that aim at becoming large, vibrant businesses (aka transformational ventures) (Schoar 2010).

[3] For example, see https://www.sba.gov/blogs/sba-launches-growth-accelerator-fund.

[4] Our paper also complements the work by Klinger and Schundeln (2011) and McKenzie (2017), who look at the impact of formal and structured business training programs offered by business plan competitions.

[5] According to Natty Zola, the Managing Director of TechStars, business accelerators are "a proven way to quickly grow a start-up by learning from experts, finding great mentorship and connecting to a powerful network. They provide resources that reduce the cost of starting a company and the early capital a team needs to get their venture off the ground or to achieve key early milestones. They have become the new business school" (Brunet, Grof, and Izquierdo 2015). We illustrate this association between business and entrepreneurship schools in

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across programs, but generally includes formal instruction (e.g., workshops and seminars on a wide range of entrepreneurship topics), guidance (e.g., access to mentors), networking opportunities, and start-up progress monitoring or accountability (Cohen and Hochberg 2014). Similar to how business schools can increase managerial capital, entrepreneurship schools in accelerators can increase entrepreneurial capital by conferring certification (cf. Spence 1973; Arrow 1973) and increasing the productivity of founders (cf. Becker 1975). Start-ups can benefit from certification because of the large information asymmetries about the performance potential of these firms. They can also benefit from productivity increases, which are otherwise hampered by market frictions such as informational constraints. For example, some founders may simply be unaware of the importance of building business networks and not look for networking opportunities outside the accelerator.[6]

Our setting is Start-Up Chile, an ecosystem accelerator aimed at high-growth, early-stage ventures. In contrast to investor-led accelerators (e.g., Y Combinator and TechStars), which typically aim to make a return on their investment, ecosystem accelerators (e.g., Village Capital and Parallel 18) aim to stimulate start-up activity in their focal region (Clarysse, Wright, and Van Hove 2015). Similar to other ecosystem accelerators worldwide, Start-Up Chile offers participants an equity-free cash infusion, shared coworking office space, and the possibility of being selected into an exclusive subprogram, which we refer to as the entrepreneurship school. In addition to the potential certification from acceptance into the entrepreneurship school, participants are provided services typical of business accelerators: guidance and accountability, via monthly meetings with program staff, program peers, and industry experts; opportunities for networking (including representing the program at high-profile events); and advertisement on the Start-Up Chile Web page.

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 estimates of local average treatment effects of basic accelerator services (i.e., cash and coworking space) on start-up performance. Furthermore, 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 an informal qualification score cutoff—we are able to provide estimates of local causal effects of entrepreneurship schooling bundled with basic services, and distinguish this from the effect of basic services alone.

Appendix Table 1.1. By drawing a parallel between the services offered by entrepreneurship schooling in a prototypical accelerator program and those offered by business schools, we unpack the performance-enhancing mechanisms that potentially underlie entrepreneurship schooling.

[6] Bloom et al. (2013) find suggestive evidence that informational frictions help explain why modern managerial practices are not employed by participants (before the intervention). These firms are typically ex ante unaware of such practices or do not believe such practices will improve performance.

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We estimate that the combination of participation in the entrepreneurship school and access to the basic services of cash and coworking space leads to significantly higher venture fundraising and scale within the first 4.75 years of entry to the accelerator for the subpopulation "randomized in" by the pitch competition. Our more conservative results indicate that entrepreneurship schooling increases the probability of securing additional financing by 21.0%, which corresponds to a 0.29-standard-deviation increase over the sample mean. We further estimate that entrepreneurship schooling results in an increase of three times the amount of capital raised, helping firms increase their fundraising performance, resulting in an unconditional average increase of 37,000 USD to 112,000 USD, which is a 0.30-standard-deviation increase over the mean. Schooling also appears to increase venture scale: we estimate it results in a twofold increase in employees, helping firms go from an unconditional average of 0.9 employees to 1.8, a 0.34-standard-deviation increase over the sample mean. By contrast, we find no evidence that basic accelerator services of cash and coworking space have a treatment effect on fundraising, scale, or survival—at least not for the subpopulation of start-ups randomized in by the selection rule.

An important challenge of working with start-up performance data is the collection of outcome measures for all accelerator applicant start-ups. Similar to prior research, we hand-collected Web-based performance measures for all applicants (Kerr, Lerner, and Schoar 2014; Goldfarb, Kirsch, and Miller 2007; Hallen, Bingham, and Cohen 2016). In addition, we collected complementary outcome data using two surveys. Results based on both types of outcome metrics point to the same conclusion: the combination of basic services and entrepreneurship schooling in ecosystem accelerators is more effective than providing basic services only. This conclusion is consistent with recent work showing that the impact of consulting services for nontransformational ventures is much larger than simply improving access to capital (cf. Bruhn, Karlan, and Schoar 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, Karlan, and Schoar 2010).

A second empirical challenge of studying business accelerators is distinguishing between treatment and selection effects. The setting of Start-Up Chile provides us with the opportunity to advance in overcoming this challenge. Under the assumptions of no precise sorting of start-ups in the vicinity of the capacity cutoff for the basic services, or in the vicinity of the informal pitch competition qualification cutoff for the entrepreneurship school, our results estimate the local treatment effects of basic services and schooling around each respective cutoff. We present evidence in support of these identification assumptions. We also test and provide suggestive evidence against potential methodological concerns such as influential observations, survey- and Web-reporting biases, and demotivation effects on pitch competition losers. Although we cannot fully rule out these concerns (because we econometricians only have

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partial information), the preponderance of evidence suggests that the regression discontinuity design (RDD) estimates are valid.

In terms of the external validity of our findings, the program-level similarity between Start-Up Chile and other ecosystem accelerators suggests that the results are representative of these types of programs at large. Moreover, a cross-program comparison of average applicants with a sample of ecosystem accelerators worldwide sharpens the external validity of our predictions; the findings are particularly valid for ecosystem accelerators that attract young entrepreneurs and early-stage start-ups.

We contribute to several bodies of research. First, a growing body of literature focuses on the effects of early-stage financiers on new ventures, but has mostly explored venture capital and angel investors (e.g., Hellmann and Puri 2000, 2002; Kerr, Lerner, and Schoar 2014; Lerner et al. 2015). Our paper complements this stream of work by focusing on an increasingly important early-stage financier: business accelerators.

Second, our results complement the emerging work on business accelerators, which we roughly classify into three literature streams. The first stream focuses on conceptual descriptions of the accelerator model (Bernthal 2015; Cohen 2013; Cohen and Hochberg 2014; Kim and Wagman 2014; Radojevich-Kelley and Hoffman 2012). A second stream investigates the potential effects of accelerators on regional development (Fehder and Hochberg 2014). The third stream explores these programs' potential effects on new venture outcomes and founders (Hallen, Bingham, and Cohen 2016; Yu 2016; Smith and Hannigan 2015; Leatherbee and Eesley 2014). Our work is most closely related to this third stream, which generally faces identification challenges in rigorously distinguishing the value-added role of business accelerator services. Our contribution is the identification of the performance-enhancing effect of entrepreneurship schooling combined with basic services relative to basic services alone. Thus, we help distinguish, for the first time, which program services can affect new venture performance. Prior work finds suggestive evidence of how the bundle of services provided by accelerators can affect performance, but does not distinguish the role of any specific service. In particular, Hallen, Bingham, and Cohen (2016) argue that ventures can indirectly learn from the experience of others affiliated with the accelerator. Leatherbee and Eesley (2014) argue that founders can improve their entrepreneurial opportunity discovery behaviors through their interaction with more active peers. Yu (2016) and Smith and Hannigan (2015) argue that accelerators can help to speed up success or failure by resolving the uncertainty about the inherent potential of the start-up more quickly.

Third, our paper builds on the literature about firms' management practices and business-training programs. Consistent with the importance of managerial capital, empirical studies show a strong association between managerial practices and company performance (Acemoglu et al. 2007; Bloom and van Reenen 2010). However, the evidence pertaining to new ventures (and thus to

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the effect of entrepreneurial rather than managerial capital) is mixed (McKenzie and Woodruff 2014) and mostly relates to nontransformational ventures. 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; Bloom, Sadun, and van Reenen 2016), we focus on the input factors useful for emerging transformational ventures (Schoar 2010) *before* the firm is established, when entrepreneurs are searching for the business opportunity.

1. Institutional Setting

1.1 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.[7] As of August 2015, approximately 1,000 start-ups had participated in the program, and nearly 6,000 had applied.

Like other business accelerators worldwide, Start-Up Chile is a fixed-term, cohort-based program that offers participants shared office space and equity-free seed capital (roughly US$40,000 delivered in two installments: 50% at the beginning and the remaining 50% three months later, conditional on survival). In addition, it offers entrepreneurship schooling to a select few participants. On average, each cohort consists of 100 competitively selected participants, who, similar to other ecosystem accelerators worldwide, relocate to the programs' headquarters for six months.[8] As explained in more detail below (Section 1.3), the selection process is based on the relative quality of the submitted application, as evaluated by external judges. At the end of their term, participating start-ups "graduate" through a "demo day" competition (i.e., a formal presentation of the companies to external investors).

Like traditional business accelerators, Start-Up Chile also offers entrepreneurship schooling. The unique feature in our setting, however, is that these sought-after schooling services are only available to a few participants. On average, 20 participants in every cohort are competitively selected to take part in the entrepreneurship school. As explained in more detail in Section 1.4, the selection procedure for the entrepreneurship school consists of a competition, dubbed "pitch-day," where start-ups pitch their businesses and are evaluated by judges. The schooled participants are the poster children of the program and, similar to other accelerators, their names are advertised on the program's Web page and in specialized news releases.

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

[8] Relocation of founders is a common request for participation in business accelerators. See Section 5.1 for additional details.

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In addition to potential certification from acceptance into the entrepreneurship school, participants receive two key additional services, which are similar to the schooling services typically offered at business accelerators: guidance and accountability, as well as networking opportunities.[9] The guidance and accountability are imparted via 30-minute monthly meetings with program staff, program peers, and industry experts (no one is compensated or holds an equity stake), where milestones are set and entrepreneurs are held socially accountable for their self-defined strategic goals. Industry experts are generally Chileans connected to the Start-Up Chile network and are assigned to schooled participants according to industry. The networking opportunities arise because participants represent the program at high-profile public events and host (by holding one-on-one meetings) high-profile Start-Up Chile guests, such as Steve Wozniak and Paul Ahlstrom.

1.2 Sample

Start-Up Chile provided us with all the application data, including application scores and final selection decisions into the program and the entrepreneurship school, for seven cohorts. Our sample consists of 3,258 applicants (616 participants and 2,642 nonparticipants). Participants for generation 1 (7) arrived in Santiago, Chile, in June 2011 (June 2013) and graduated in January 2012 (January 2014).

In addition, Start-Up Chile granted us access to confidential records of the pitch-day competitions, including pitch-day scores and final selection decisions. Because the entrepreneurship school was launched in generation 4, these additional data are only available for generations 4–7 and amount to 276 pitch-day competitors (59 schooled participants; 217 nonschooled competitors).

Based on the program's records, we constructed 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 had a working prototype or had one in development at application (*Prototype*).

Table 1 provides summary statistics of our sample. On average, applicant founders are 30 years old; 21% of them are Chilean; and 14% are female. Applicant start-ups have between two and three employees, on average; 26% have previously raised capital; and 49% are working to develop a prototype or have already developed one.

Our sample is comparable to prior research on early-stage ventures, particularly in terms of the number of employees (e.g., Haltiwanger, Jarmin,

[9] Business accelerators typically offer guidance/accountability and networking opportunities as part of their entrepreneurial schooling services. There is, however, large variation in exactly how those services are imparted. See Section 5.1 for additional details.

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Table 1
Main variables

Variable	Obs.	Mean	SD	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	3,258	0.49	0.50	0.00	1.00
Selection process					
Rank	3,258	260.91	164.33	1.00	656.00
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 capital indicator	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. capital indicator	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. capital indicator	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	4.81
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 statistics of the main variables used in the analysis. The first and second sections include variables extracted from the applications and the Start-Up Chile records. The third section includes Web-based outcome variables, which were collected during first quarter of 2014 (mid-2015) from Facebook and LinkedIn (CB Insights). The last two sections include survey-based outcome variables. The first survey was distributed to all applicants during October 2014, and the second survey was distributed to all participants during the first quarter of 2016. For variable definitions, see Sections 1.3 and 3.

and Miranda 2013) and industry representation (e.g., Puri and Zarutskie 2012) (see Appendix 2 for further details). Our sample is also comparable to the ecosystem business-accelerator genre. Using information from the Entrepreneurship Database (ED) program at Emory University,[10] which has records of multiple ecosystem accelerators worldwide, we report in Appendix Tables 2.1 and 2.2 comparisons between the preapplication start-ups (founders) in our sample and those of the ED database (reported under the heading "ED"). The tables show that, relative to average applicants in other ecosystem accelerators worldwide, 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 capital prior to potential participation.

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

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1.3 Accelerator selection process

Selection into the basic Start-Up Chile program 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 rankings; thus it is impossible for applicants to manipulate the ranking process.

Three to five expert judges are assigned randomly to each application. YouNoodle's network consists of approximately 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, the identity of the other judges evaluating the same start-ups is unknown, and no single judge sees all applications. Thus, judges are unlikely to be able to precisely manipulate the rankings (e.g., to help an applicant friend qualify).

A committee at the Chilean Economic Development Agency (CORFO), which funds Start-Up Chile, handles the second part of the selection process, making the final decision based on YouNoodle's ranking. A capacity threshold is prespecified for each cohort (normally 100),[11] and the top-ranking companies—those ranking higher than the threshold—are typically selected.[12] 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 that meet the 100-company threshold ultimately participate, and not all of the accepted participants are ranked higher than the threshold. 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.

1.4 Entrepreneurship school selection process

Two months into the program, participants can apply to the program's entrepreneurship school. The entrepreneurship school is not available to all

[11] The only exception was generation 2, where the number of participants was set at 150. In unreported regressions, we exclude the first two generations of the program, and results are quantitatively unchanged.

[12] Highly ranked companies are assigned low rank values; for example, the top company has a rank of 1.

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participants, because monitoring requirements are too burdensome for the staff, and providing the preferential access to external speakers and staff's contacts to all participants is infeasible. On average, 80% of the accelerator participants chose to compete for a spot in the school, and roughly 20% of competitors are selected.

The selection procedure for the entrepreneurship school is also a two-part process, starting with a competition dubbed "pitch-day." On pitch-day, competing start-ups formally present, or "pitch," their business to a group of local judges (who are independent from the accelerator application-process judges), that is comprised of 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) members. Participants are allotted five minutes for their pitch, and, overall, the competition generally lasts for two hours. A guideline for the pitch is provided. Judges score competitors on five criteria: (1) the problem their business is trying to solve, (2) the proposed solution, (3) the business model, (4) the size of the market, and (5) fundraising needs. Judges keep records of the scores they assign to competitors by criteria.

The Start-Up Chile staff handles the second part of the selection process, making the final decision based on the average pitch-day scores—a weighted average of the scores per criterion across judges, where the weights (by criteria) are determined ex ante by Start-up Chile.[13] The potential for precise manipulation of pitch-day scores is small. Competitors do not know the identity of judges until minutes prior to the competition. Moreover, the external judges have no clear incentive for manipulation because they have no "skin in the game." In addition, although some judges might want to help a participating friend, they cannot precisely manipulate the average score: judges independently score each start-up across the five criteria and no one judge oversees all the scores.

No formal restriction exists on the number of participants allowed in the entrepreneurship school, and 15 participants win a spot in every cohort on average. However, an informal selection rule is implicit in the data—competitors scoring above 3.6 are 51.9% more likely to be chosen (conditional on the pitch-day score; see Column 1 in Table 4). Conversations with the staff indicate that there is an informal rule in the reviewing process: competitors below the normative "quality bar" of a 3.6 pitch-day score are generally desk rejected.

This informal rule is evident in Table 2, where we summarize the number of pitch-day competitors and schooled participants across generations—beginning with the fourth generation, during which the entrepreneurial school

[13] We have detailed information on the pitch-day scores by criterion and by judges only for generation 6. For the rest of the generations, we only have access to the final (weighted average) pitch-day score. In generation 6, the weights used for each criterion were as follows: problem, 30%; solution, 20%; business model, 20%; market, 20%; and fundraising needs, 10%.

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Table 2
Number of applicants and acceptance rate of the entrepreneurship school

		A					*B*				
		Applicants' school					Acceptance rate school (%)				
			Generation					Generation			
Pitch-day score bracket	All, share (%)	All	4	5	6	7	All	4	5	6	7
0.0–0.9	1.4	4	0	4	0	0	0.0	0.0	0.0	0.0	0.0
1.0–1.5	0.4	1	0	1	0	0	0.0	0.0	0.0	0.0	0.0
1.6–2.0	4.7	13	3	6	1	3	0.0	0.0	0.0	0.0	0.0
2.1–2.5	9.1	25	3	12	6	4	0.0	0.0	0.0	0.0	0.0
2.6–3.0	30.1	83	10	35	16	22	9.6	0.0	8.6	0.0	22.7
3.1–3.5	27.9	77	25	11	33	8	7.8	0.0	36.4	0.0	25.0
3.6–4.0	19.6	54	16	8	22	8	53.7	50.0	75.0	40.9	75.0
4.1–4.5	6.9	19	5	3	11	0	84.2	100.0	66.7	81.8	0.0
4.6–5.0	0.0	0	0	0	0	0	0.0	0.0	0.0	0.0	0.0

The table presents the number of applicants (panel A) and the acceptance rate (panel B) of the entrepreneurship school across pitch-day-score brackets.

was introduced. For ease of exposition, we group participants across brackets of 0.5 pitch-day-score units. Panel A shows the distribution of average pitch-day scores, which concentrates around mid-range values. Column 2 shows 78% of average pitch-day scores are between 2.6 and 4.0, inclusive, whereas 15% (7%) of average pitch-day scores are lower (higher) than 2.6 (4.0). Panel B shows a stark jump in the probability of acceptance in to the entrepreneurship school between average pitch-day scores of 3.1–3.5 and 3.6–4.0, where the average acceptance rate of all generations increases from 8% to 54%. This jump represents a distinct and permanent shift in the relationship between schooling and the pitch-day score: it is present across all generations and thus not due to cross-sectional variation in scores across batches.[14]

2. Empirical Strategy

2.1 Exploiting the accelerator's selection rule

We use the capacity-threshold rule in the selection process of Start-Up Chile to estimate a local average treatment effect of basic accelerator services on new venture performance. This rule implies the probability of acceleration changes discontinuously at the capacity threshold as a function of the applicant's ranking. Therefore, the difference in expected outcomes between start-ups on opposite sides of—but sufficiently near—the threshold can provide the basis for an unbiased local causal estimate. The main identification assumption is that ranks are not manipulated around the threshold. In this section, we begin by estimating the size of the discontinuity, and then present supportive evidence of the identification assumption. Finally, we describe the RDD empirical approach.

[14] This jump is also evident in Figure 4, where we plot the fraction of accepted participants in bins of 0.2 pitch-day-score units.

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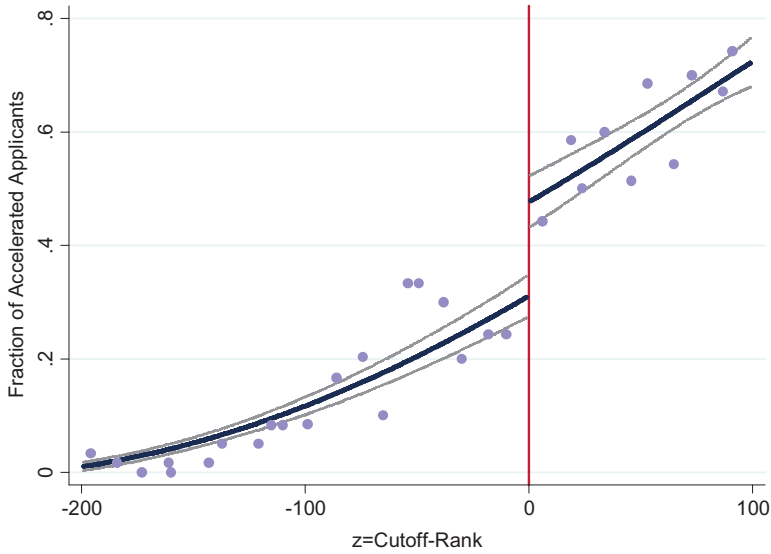

Figure 1
Fraction of accelerated applicants
The figure shows the average fraction of accelerated applicants (dots) in bins of 10 applicants, as well as the fitted values and 90% confidence interval from the regression $acceleration_s = \delta + \gamma higher_s + f\left(Rank_s - cutoff^g\right) + X_s'\sigma + \varepsilon_s$, 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 $f\left(Rank_s - cutoff^g\right)$ is a fourth-degree polynomial of the normalized rank. The vertical line represents the ranking cutoff normalized at 0 for the normalized rank. 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).

The discontinuity in acceleration at the capacity threshold is visible in Figure 1. 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) calculated across bins of 10 ranks and plotted in dots. Because we plot acceleration against normalized ranking, higher-ranking companies are represented to the right of the capacity threshold, which corresponds to the 0 on the x-axis.

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

$$acceleration_s = \delta + \gamma higher_s + f\left(Rank_s - Cutoff^g\right) + X_s'\sigma + \varepsilon_s, \qquad (1)$$

where s indexes start-ups, $acceleration_s$ indicates whether the start-up participated in Start-Up Chile, $higher_s$ is a dummy that equals 1 if the start-

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Table 3
Discontinuity probability of acceleration at the capacity threshold

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	p=1 & h=50	p=1, controls & h=50	p=2	p=2 & controls	p=3	p=3 & controls	p=4	p=4 & controls
Higher	0.303***	0.319***	0.207***	0.191***	0.176***	0.200***	0.166***	0.164***
	(0.071)	(0.083)	(0.035)	(0.044)	(0.042)	(0.048)	(0.041)	(0.049)
Obs.	682	499	3,258	1,906	3,258	1,906	3,258	1,906
R-squared	0.070	0.128	0.397	0.447	0.398	0.447	0.399	0.451

This table shows the discontinuity in the probability of acceleration around the capacity-threshold-ranking cutoff. Columns (1)–(8) report the coefficient of *Higher* (γ) of the regression: $Acceleration = \delta + \gamma higher + f(Rank - Cutoff) + X'\sigma + \varepsilon$. The variable *Acceleration* equals 1 if the applicant participated in the accelerator; the variable *Higher* equals 1 if the applicant ranks higher than the capacity threshold in its generation, and 0 otherwise; and $f(Rank - Cutoff)$ is a pth-degree polynomial of the modified rank (i.e., $z = Rank - Cutoff$). The type of specification is indicated at the top of each column, that is, inclusion of controls, the degree of the polynomial used (p), and the bandwidth (h), specified in terms of ranks included around the threshold. If no bandwidth is specified, then the full sample was used. The controls included are *Chilean*, *Female*, *Capital raised before*, *Prototype*, *Young*, and generation fixed effects. To conserve space, the estimated coefficients for the polynomial terms are not presented in the table. Robust standard errors are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

up ranks higher than the threshold, and X_s is a vector of controls including generation fixed effects, *Age*, *Chilean*, *Female*, *Employees before*, *Capital raised before*, and *Prototype*. We mitigate potential biases through high-order polynomials of the modified ranking (i.e., $f(Rank_s - Cutoff^g)$ of degree p) (cf. Lee and Lemieux 2010) or by restricting the sample to a small bandwidth of size h around the threshold (cf. Gelman and Imbens 2014). We consider different polynomial degrees and bandwidths in order to verify that the results are not dependent on functional form or sample restrictions (cf. van der Klaauw 2002).

The estimated discontinuity is sizable, significant, and robust. Table 3 presents robust estimates of γ across different specifications of Equation (1), with varying polynomial degrees (p) and rank bandwidths (h), including first-degree polynomials and 50 participants (ranks) around the threshold (Column 1), and additionally including generation fixed effects and controls (Column 2); as well as using the entire sample and including second-degree (Columns 3 and 4), third-degree (Columns 5 and 6), and fourth-degree polynomials (Columns 5 and 6) with and without controls. The coefficient of Column 7 (Column 8) implies that ranking higher than the capacity threshold increases the probability of acceleration by 16.6% (16.4%) relative to other start-ups in the same generation (and controlling for observable differences across start-ups). We plot the corresponding estimated probability of schooling of the estimates in Column 7 (and the 90% confidence interval) in Figure 1; the discontinuity is evident.

As mentioned in Section 1.3, the manipulation of rankings is hard in this context. Results from two formal tests confirm this notion. First, Figure 2 shows that no discontinuity exists around the capacity threshold in the density of applicants. The McCrary (2008) test for the distribution of applicant scores results in a discontinuity estimate of −0.026 with a standard error of 0.10. Second, Figure 3 demonstrates smoothness in observable covariates around

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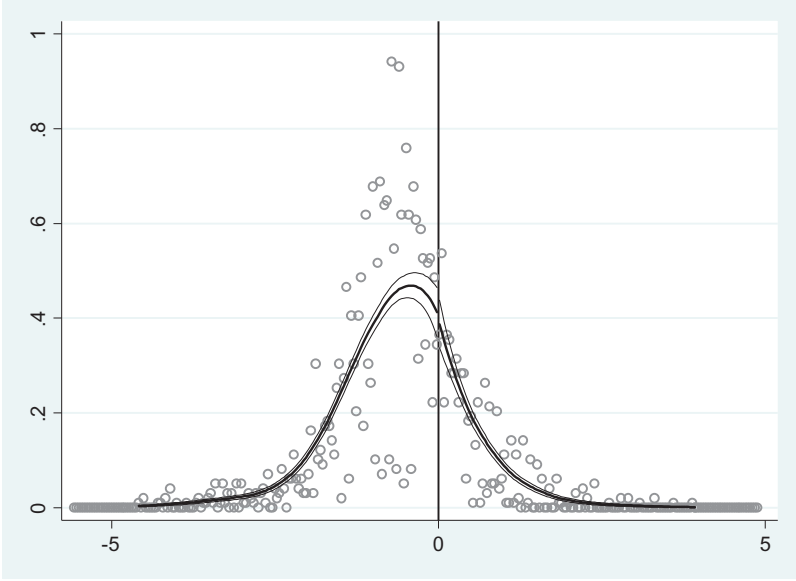

Figure 2
Density of judges' application scores
The figure presents a finely gridded histogram of the normalized application scores. For each applicant, the score of the capacity-threshold-ranking company (of its generation) is subtracted from the application score. Judges score applications from 1 to 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 -0.267 (log difference in height is -0.026 with standard error of 0.96). The McCrary test uses a local linear regression of the histogram separately on either side of the threshold to accommodate the discontinuity. For additional details, see McCrary (2008).

the threshold; that is, companies ranking closely on either side of the capacity threshold are similar. We estimate Equation (1) using pre-determined covariates as dependent variables; Figure 3 plots such estimates against normalized rank. In contrast to the probability of acceleration, in no case can we reject the null hypothesis of no jump at the capacity threshold.

We estimate a local average treatment effect of basic accelerator services on new venture performance by instrumenting $Acceleration_s$ with the selection rule (i.e., the indicator variable $Higher_s$) in a fuzzy RDD. We estimate a system of equations using (1) above and the following:

$$Outcome_s = \pi + \beta acceleration_s + \breve{f}(Rank_s - Cutoff^g) + X_s'\rho + \epsilon_s, \qquad (2)$$

where $\breve{f}(Rank_s - Cutoff^g)$ is a high-order polynomial of the modified ranking of the same pth degree like in Equation (1). As with all instrumental variable estimators, inference based on the fuzzy RDD is restricted to those observations that respond to the instrument; that is, applicants that are randomized into the accelerator by the selection rule (cf. Lee and Lemieux 2010). The main

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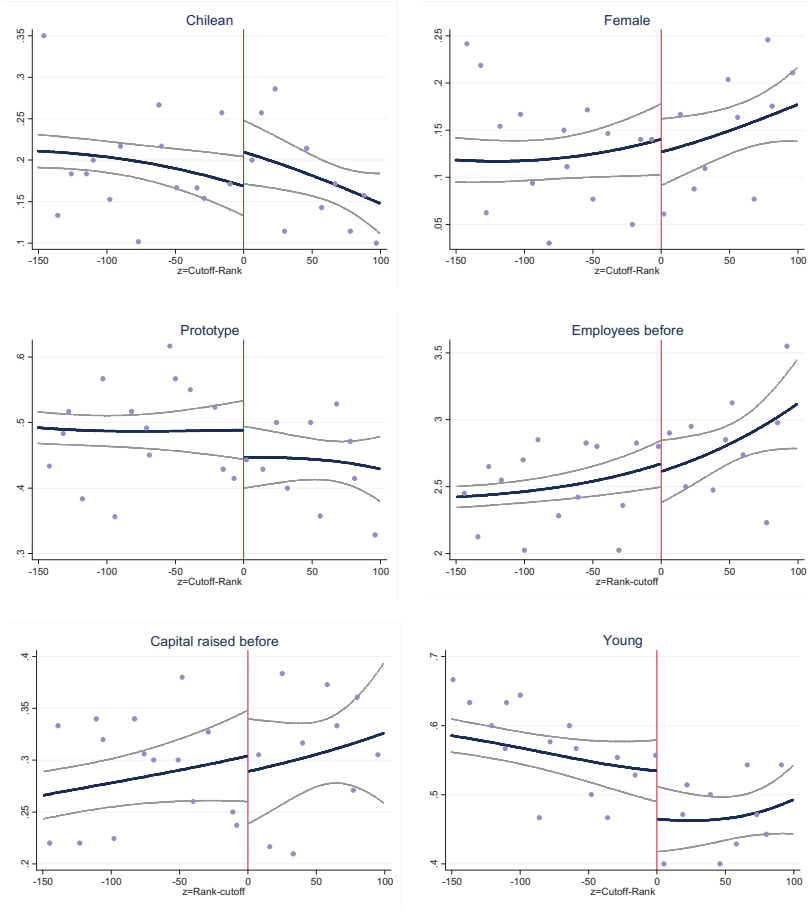

Figure 3
Balanced sample around the application capacity threshold
The figure shows evidence of a balanced sample near the capacity-threshold-ranking cutoff. *Chilean* (*Female*) is a dummy that equals 1 if the founder is Chilean (Female); *Employees before* is the number of workers the start-up reported 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 working prototype/or has one 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 $Outcome_s = \delta + \gamma higher_s + f(Rank_s - Cutoff^g) + X_s' \mathrm{M} + \varepsilon_s$, 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 normalized rank. The vertical line represents the ranking cutoff normalized at 0 for the normalized rank.

identification assumption is that ranking above the threshold is as good as random in the vicinity of the capacity threshold. In that case, the RDD estimates a local average treatment effect, even if selection into the program is made on the basis of prospective gains (cf. Roberts and Whited 2012).

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2.2 Exploiting the informal rule of the entrepreneurship school's selection process

We use the informal rule in the selection process of the entrepreneurship school to estimate a local average treatment effect of the combination of schooling and basic services on new-venture performance. Our approach is similar to other papers in the literature exploiting de facto discontinuities in selection systems (Kerr, Lerner, and Schoar 2014). Because the probability of schooling changes discontinuously at the 3.6 pitch-day score, the difference in expected outcomes between start-ups on opposite sides of—but sufficiently near—the threshold can thus provide the basis for an unbiased local causal estimate. The main identification concern is that competitors are manipulated around the 3.6 pitch-day score. As mentioned in Section 1.4, precise manipulation is hard: competitors do not know the identity of judges beforehand, judges independently score each start-up, no judge oversees all the scores, and final pitch-day scores are a linear function of the judges' scores. Nonetheless, such potential manipulation is of particular concern here because the pitch-day threshold does not correspond to a formal rule in the program (in contrast to the capacity threshold of basic services). In this section, we begin by estimating the size of the discontinuity. We then present evidence that is supportive of no manipulation. Finally, we describe the RDD empirical approach.

We estimate the size of the discontinuity in the probability of schooling at the 3.6 pitch-day threshold using the following equation:

$$School_s = \tau + \mu Above + g\left(Pitch_{day}\, score_s - 3.6\right) + Z_s'\phi + \varepsilon_s, \qquad (3)$$

where the outcome variable *school* is an indicator variable that equals 1 if the participant received schooling, *Above* is an indicator variable that equals 1 if the participant scored 3.6 or higher on the pitch-day, and Z_s indicates controls that vary across specifications. We mitigate potential biases by restricting the sample to a small bandwidth of pitch-day scores of size h around the threshold, and using polynomials (i.e., $g\left(Pitch_{day}\ score_s - 3.6\right)$ of degree p) (Gelman and Imbens 2014). The coefficient μ in Equation (3) is a measure of the size of the discontinuity.

The estimated discontinuity is sizable and significant. Column 1 (2) of Table 4 presents the estimate of μ from a specification of Equation (3) using a first-degree polynomial, the entire sample, and excluding (including) controls. The coefficient of Column 1 (2) implies that scoring above the informal quality bar of 3.6 in the pitch-day score increases the probability of schooling by 51.9% (50.9%) relative to other competing accelerator participants in the same generation (and controlling for observable differences across start-ups). We plot the corresponding estimated probability of schooling of the estimates in Column 1 (and the 90% confidence interval) in Figure 4; the discontinuity is evident. Table 4 shows the discontinuity estimate is robust to different specifications of Equation (3), including restricting the sample to start-ups scoring 1.5 points around the cutoff (Column 3), restricting the sample to those

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Table 4
Discontinuity probability of schooling at the pitch-day score of 3.6

	(1)	(2)	(3)	(4)	(5)
	p=1	p=1 & controls	p=1& controls & h=1.5	p=1 & h=1	p=2 & controls
Above	0.519***	0.509***	0.420***	0.440***	0.341***
	(0.072)	(0.073)	(0.085)	(0.094)	(0.097)
Observations	276	276	265	248	276
R-squared	0.398	0.435	0.440	0.385	0.455

This table shows the discontinuity in the probability of schooling around the pitch-day-score cutoff. Estimates are based on different specifications of the regression $school = \tau + \mu Above + g(Pitch_{day}Score - 3.6) + Z'\phi + \varepsilon$, where the outcome variable *school* is an indicator variable that equals 1 if the participant was selected into the entrepreneurship school, *Above* is an indicator variable that equals 1 if the participant scored 3.6 or higher during the pitch day, and $g(Pitch_{day}Score - 3.6)$ is a pth-degree polynomial of the normalized pitch-day score (i.e., pitch-day score minus the 3.6 cutoff). The type of specification is indicated at the top of each column including the degree of the polynomial used (p), and the inclusion of controls. Columns (3) and (4) include different bandwidth specifications (h) for pitch-day-score ranges of 2.1–5.0 (h=1.5) and 2.6–4.5 (h=1), respectively. The controls included are *Capital raised before* and generation fixed effects. To conserve space, the estimated coefficients for the constant and the polynomial terms are not presented in the table. Robust standard errors are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

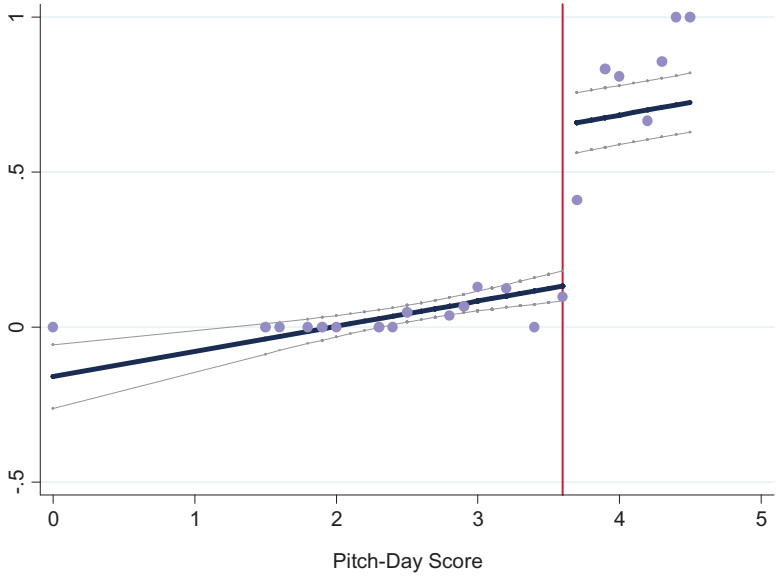

Figure 4
Fraction of schooled participants
The figure shows the average fraction of schooled participants in bins of 0.2 pitch-day scores, and the fitted values and 90% confidence interval from the regression $school_s = \tau + \mu Above + g(Pitch_{day}\ score_s - 3.6) + \varepsilon_s$, where the outcome variable *School* is an indicator variable that equals 1 if the participant was schooled; *Above* is an indicator variable that equals 1 if the participant scored above 3.6 on the pitch day; and $g(Pitch_{day}\ score_s - 3.6)$ is a first-degree polynomial of the pitch-day score. The vertical line represents the informal pitch-day-score cutoff of 3.6.

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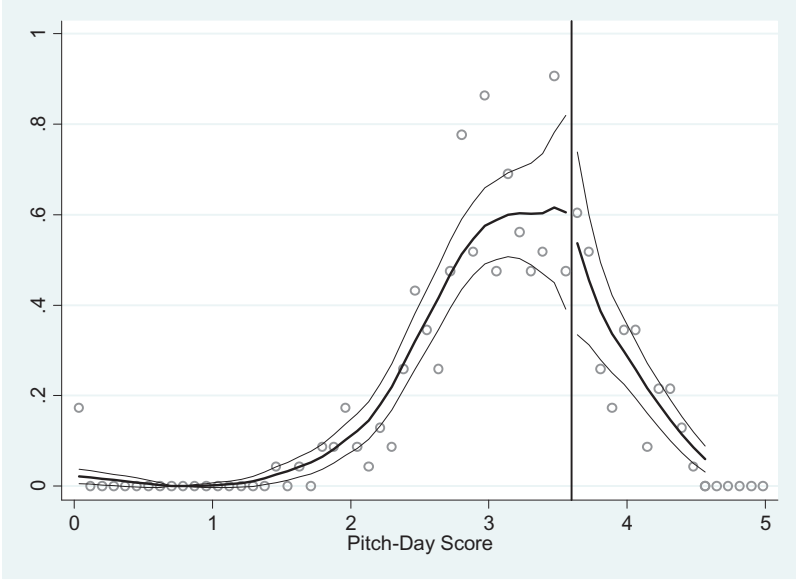

Figure 5
Density of pitch-day scores
The figure presents a finely gridded histogram of the pitch-day scores for all participants looking to qualify for the entrepreneurship school. Judges score applications from 0 to 5. In practice, average scores range from 0 to 4.45. The null hypothesis of no discontinuity 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 additional details, see McCrary (2008).

scoring 1.0 point around the cutoff (Column 4), and using a second-degree polynomial (Column 5).

We deploy two main tests for manipulation of start-ups around the 3.6 pitch-day score. First, in Figure 5, we plot the density of competitors by average pitch-day score and show that no discontinuity exists around the 3.6 cutoff. More formally, we cannot reject the hypothesis of local continuity in the distribution of average pitch-day scores at the cutoff (the t-statistic from the McCrary test is -0.191). This finding is as expected; given its informality, the quality bar threshold is unknown by participants and judges, which further limits the scope for manipulation. Second, we estimate Equation (3) using predetermined covariates as the dependent variable to verify that the start-ups above and below the cutoff are comparable ex ante. Figure 6 shows evidence of a balanced sample. The only significant 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 nonspecialized financiers such as family and friends; no difference is evident when restricting the type of capital to *Specialized capital*

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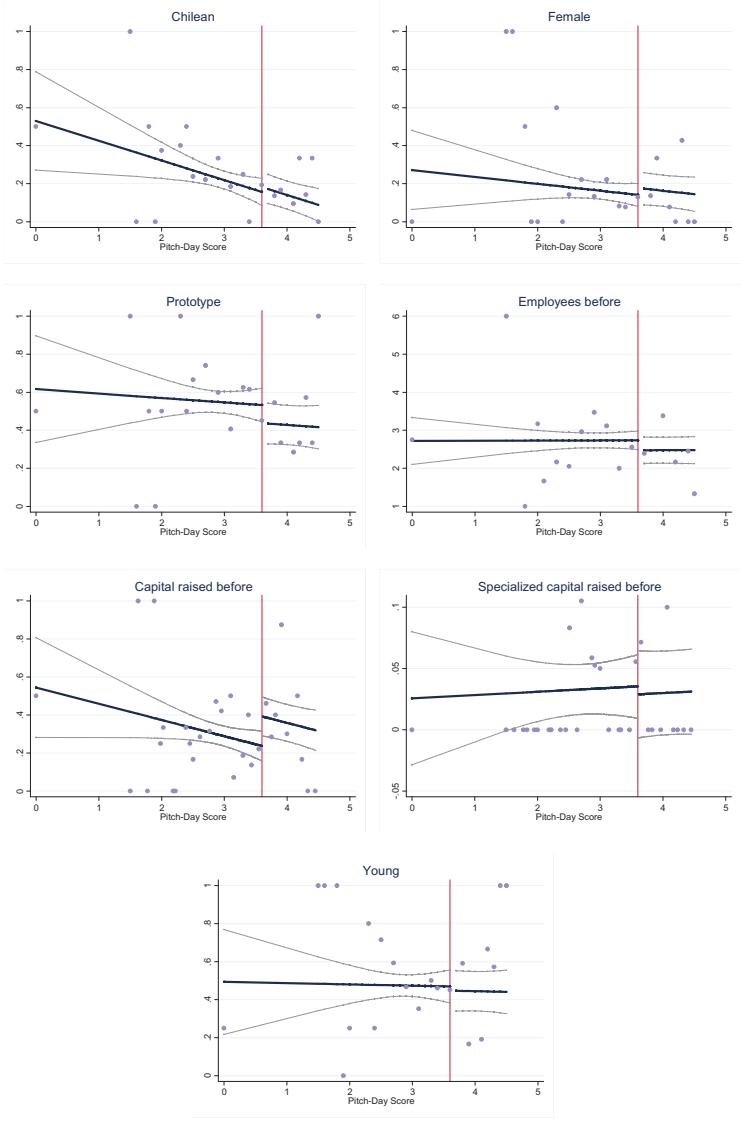

Figure 6
Balanced sample around the 3.6 pitch-day-score cutoff
The figure shows evidence of a balanced sample near the pitch-day cutoff for pitch-day competitors. *Chilean* (*Female*) is a dummy that equals 1 if the founder is Chilean (Female), *Employees before* is the number of workers the start-up 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 working prototype/or has one 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 version of the regression in Equation (2), $Covariate = \sigma + \omega Above + \breve{g}(Pitch_{day}Score - 3.6) + \epsilon$, with each of these variables as outcomes, *Above* is an indicator variable that equals 1 if the participant scored above 3.6 on the pitch day, and $\breve{g}(Pitch_{day}Score - 3.6)$ is a first-degree polynomial of the pitch-day score. The vertical line represents the informal pitch-day-score cutoff of 3.6.

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raised before (which includes angel, accelerator, or VC fundraising) as shown in the figure. We include the variable *Capital raised before* as a control in our regressions, and verify that the results are unchanged by its inclusion.

We implement the fuzzy RDD by estimating the system of equations using (3) above and

$$Outcome_s = \alpha + \beta school_s + \breve{g}\left(Pitch_{day}\ score_s - 3.6\right) + Z_s'\varphi + \epsilon_s, \qquad (4)$$

where the vector of controls varies across specifications and includes generation fixed effects and the covariate *Specialized capital raised before.* The system of equations identifies a local average treatment effect of entrepreneurship schooling under the assumption that no similar discontinuity exists in the unobservable quality of start-ups that scored close to 3.6 on the pitch-day. In Section 4.3, we conduct several tests for this assumption. Inference based on the fuzzy RDD is restricted to those observations whose treatment is randomized by the selection rule.

3. Outcome Variables

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 performance data on nonparticipating applicants. Therefore, we use two strategies to address this challenge. First, similar to prior research (Kerr, Lerner, and Schoar 2014; Goldfarb, Kirsch, and Miller 2007; Hallen, Bingham, and Cohen 2016), 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 4.75 years since potential entry to the accelerator. Greater details about this data-collection strategy and the definitions of each outcome variable can be found in Appendix 3.

For our Web-based measures, we searched through the Facebook and LinkedIn (CB Insights) platforms during the first quarter of 2014 (mid-2015). Because participating generations arrived from 2011 through 2013, these metrics represent new venture performance outcomes between 0.75 and 2.75 years (2 and 4 years) since potential entry into the program. Our first survey was sent to all *Applicants* on October 2014 (between 1.3 and 3.3 years since potential entry). The response rate was 9%.[15] During the first quarter of 2016, the accelerator conducted a second performance-outcome survey (between 2.75 and 4.75 years since entry), focusing only on *Participants*. The response rate was 72.4%. To distinguish between our three data sources, we identify the

[15] 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*.

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Web-based measures with the prefix "Web," the applicant survey measures with "Survey A," and the participant survey measures with "Survey P."

For each data source, we constructed five new venture performance proxies: *Capital indicator* as a binary variable for securing capital after potential participation in the accelerator; the natural logarithm of the value of *Capital raised* since inception, excluding the seed capital provided by the program to participants; the natural logarithm of the number of *Employees*; market *Traction* as the natural logarithm of the sales (or Facebook likes, in the case of the Web-based measure) during the six preceding months; and a binary variable to indicate *Survival*. In addition, we were able to construct *Valuation* as a sixth performance proxy from our survey data sources, which corresponds to the natural logarithm of the pre-money valuation of the start-up.[16] We used logarithmic transformations of continuous outcome variables to mitigate the potential impact of outliers (see also Section 4.3).

Table 1 presents the summary statistics of the five Web-based outcome measures. Within four years of potential entry into the program, the average applicant is 2.60% likely to secure specialized financing, raises 0.49 (log) dollars in capital, has 0.53 employees, has an average traction of 0.06 (log Facebook likes), and is 21.2% likely to survive.

Table 1 also presents summary statistics of the survey-based performance metrics. Within 3.3 years of potential entry into the program, the average applicant-survey respondent is 65.80% likely to secure external funding, raises 6.97 (log) dollars in capital, has 0.54 (log) employees, an average traction of 3.67, and is 61.80% likely to survive.[17] Within 4.75 years of entry into the program, 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.

In Table 5, we report correlations across our Web-based and survey-based proxies for new venture performance. All (except 2 out of 10) Web-based and survey-based performance metrics have a positive and statistically significant correlation, albeit a small one. This low correlation is likely due to differences across both data-collection systems in the timing of the collection, potential response biases, and variable definitions. Our *Applicants* survey lags (precedes) our Web-based metrics by 0.7 (0.7) years for Facebook and LinkedIn (CB Insights) data sources, whereas our *participants* survey lags our Web-based metrics by 2 (0.8) years for Facebook and LinkedIn (CB Insights) data sources. These lags can lead to important measurement differences: Haltiwanger, Jarmin, and Miranda (2013) and Hurst and Pugsley (2011) document large heterogeneity in young (less than 2 years) firm growth, and

[16] For those applicants that have not secured external funding, this variable corresponds to their perceived valuation.

[17] The average survival rate is lower than the fundraising rate because some of the companies that raised financing were not alive by the time of the survey, yet the founders answered the questionnaire. Results are robust to excluding these companies from the sample.

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Table 5
Correlation Web-based and survey-based performance proxies

A. Correlation of fundraising proxies

	Survey A. capital indicator	Survey P. capital indicator	Survey A. capital raised	Survey P. capital raised
Web capital indicator	0.04	0.17**		
	(0.53)	(0.05)		
Web capital raised			0.11**	0.34***
			(0.05)	(0.00)
Observations	319	145	319	145

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*	0.20***
			(0.07)	(0.00)
Observations	319	145	319	145

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% level, respectively.

de Mel, McKenzie, and Woodruff (2014) document time heterogeneity across short- and long-term effects of business-training programs. Moreover, survey respondents may be systematically different from average program participants. For example, successful firms may be more (or less) likely to answer surveys. Indeed, survival rates in our survey-based samples appear particularly high relative to the average survival rates reported by Haltiwanger, Jarmin, and Miranda (2013) and Puri and Zarutskie (2012) from the universe of firms. Furthermore, survey- and Web-based metrics are defined differently. For example, survey respondents include capital from family in their reported fundraising, whereas CB Insights includes only specialized capital sources. Also, whereas survey respondents report the number of employees in their firms, Web-based measures report ranges (see Appendix 3 for additional details).

4. Results

4.1 The effect of basic accelerator services on new venture performance

Table 6 summarizes the estimated effects of basic accelerator services on all outcome variables. Reported standard errors are heteroscedasticity robust. Columns 1 and 2 report ordinary least-squares (OLS) estimates comparing participants and nonparticipants, with and without controlling for covariates. Participants consistently outperform rejected applicants: results in Column 2

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Table 6
Venture performance and basic acceleration services

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Estimate	OLS	OLS & controls	p=1 & h=50	p=1, controls & h=50	p=2	p=2 & controls	p=3	p=3 & controls	p=4	p=4 & controls
Web capital indicator	0.062***	0.056***	−0.005	−0.047	0.037	0.062	0.065	0.088	0.049	0.056
	(0.011)	(0.012)	(0.089)	(0.119)	(0.071)	(0.110)	(0.098)	(0.116)	(0.103)	(0.142)
Web capital raised	1.215***	1.019***	0.956	0.355	0.439	0.106	0.160	0.056	0.117	0.108
	(0.159)	(0.168)	(1.244)	(1.501)	(0.999)	(1.454)	(1.443)	(1.558)	(1.497)	(1.876)
Web traction	0.079***	0.044**	−0.235	−0.405*	0.021	0.006	−0.053	−0.089	−0.039	−0.108
	(0.018)	(0.020)	(0.184)	(0.229)	(0.125)	(0.190)	(0.206)	(0.217)	(0.203)	(0.244)
Web employees	0.655***	0.315**	−1.704	−2.615	−0.674	−1.514	−1.255	−1.974	−1.385	−2.880
	(0.112)	(0.131)	(1.301)	(1.709)	(0.911)	(1.462)	(1.275)	(1.506)	(1.375)	(2.050)
Web survival	0.305***	0.250***	−0.037	−0.136	0.199	0.284	0.282	0.314	0.272	0.426
	(0.021)	(0.023)	(0.233)	(0.265)	(0.165)	(0.218)	(0.225)	(0.223)	(0.238)	(0.284)
Survey A. capital indicator	0.163***	0.181***	−0.352	−1.048	−0.456	−1.392	−0.328	−0.925	−0.593	−1.607
	(0.054)	(0.067)	(0.342)	(0.818)	(0.701)	(1.764)	(0.571)	(1.010)	(0.762)	(2.118)
Survey A. capital raised	2.477***	2.829***	−4.067	−11.296	−5.207	−14.214	−4.273	−10.467	−6.460	−16.954
	(0.601)	(0.753)	(3.845)	(8.811)	(7.919)	(19.055)	(6.598)	(11.675)	(8.566)	(23.435)
Survey A. valuation	1.068	2.058**	1.091	−2.250	−0.682	0.055	1.110	−0.776	1.672	−6.340
	(0.791)	(0.990)	(4.211)	(7.765)	(8.648)	(13.988)	(7.315)	(10.354)	(8.809)	(17.965)
Survey A. traction	0.288	0.249	−5.102	−10.221	−6.281	−13.333	−6.459	−11.140	−6.842	−15.270
	(0.569)	(0.696)	(3.208)	(8.211)	(6.998)	(16.935)	(5.837)	(10.847)	(7.350)	(20.882)
Survey A. employees	0.092	0.161	−0.575	−2.123	−1.274	−3.610	−0.967	−2.541	−1.319	−3.454
	(0.099)	(0.120)	(0.656)	(1.530)	(1.376)	(3.988)	(1.116)	(2.244)	(1.461)	(4.307)
Survey A. survival	0.178***	0.197***	−0.396	−0.114	0.422	0.545	0.187	0.357	0.070	0.549
	(0.055)	(0.069)	(0.348)	(0.679)	(0.640)	(0.954)	(0.540)	(0.674)	(0.654)	(1.077)

This table reports the effects of basic acceleration services (cash and coworking space) on venture performance. Estimates are based on the regression $outcome_s = \pi + \beta acceleration_s + \check{f}\left(Rank_s - Cutoff^g\right) + X_s'\rho + \epsilon_s$, where *Acceleration* is a variable that equals 1 if the applicant participated in the accelerator. The outcome variable is specified in the title columns of each row. The type of specification is indicated at the top of each column, that is, inclusion of controls, the degree of the polynomial used (p), and the bandwidth (h), specified in terms of ranks included around the threshold. If no bandwidth is specified, then the full sample was used. For the OLS estimate, the polynomials of the normalized ranking (i.e., $\check{f}\left(Rank_s - Cutoff^g\right)$) 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 controls included are *Chilean*, *Female*, *Capital raised before*, *Prototype*, and *Young* and generation fixed effects. Robust standard errors are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

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indicate participants are 5.6% more likely than nonparticipants to raise capital after the program.

Columns 3–10 report estimates using the fuzzy RDD with different combinations of bandwidths (h) and polynomial degrees (p), following the same structure used in Table 3. In contrast to OLS estimates, RDD specifications result in nonsignificant coefficients across all outcome variables, and generally smaller point estimates, which vary significantly across specifications in terms of both magnitude and sign.

In an unreported analysis, we verify that results are the same if we allow polynomials to differ on either side of the threshold, 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.

Two interpretations are possible for the negative differences between the RDD and the OLS effects, because RDD estimates a local average treatment effect (cf. Lee and Lemieux 2010). Under the assumption of underlying heterogeneity in the treatment effect, these negative differences can reflect lower-than-average returns of basic services in the subpopulation that the selection rule randomizes into the program. Under the assumption that no such heterogeneity exists, the negative difference can also reflect the program's ability to screen applicants.

4.2 The effect of entrepreneurship schooling on new venture performance

Table 7 reports the coefficients of each outcome variable regarding the estimated effect of participating in the entrepreneurship school. Column 1 (2) reports OLS estimates without (with) controls. Column 1 shows that schooled participants are 9.1% more likely to raise capital after the program. Column 2 shows fundraising ability prior to schooling does not explain the increase in fundraising performance—results continue to hold once we control for *Capital raised before*.

Columns 3–7 report estimates using different specifications of the fuzzy RDD, with and without controlling for covariates, and using different combinations of bandwidths (h) and polynomial degrees (p), following the same structure used in Table 4. Evidence suggests positive and large causal effects of the entrepreneurship school. The first rows of Column (3) show that schooling increases the probability of fundraising by 21.0%, the amount of capital raised by a factor of three, market traction by 23.8%, the number of employees by a factor of two, and valuations by a factor of five. Scale effects partly explain the large magnitudes. For example, a twofold increase in employees roughly means that firms hire one more employee (from an unconditional average of 0.9 to 1.8). Similarly, a threefold increase in capital raised means firms increase fundraising from an unconditional average of 37,000 USD to 112,000 USD. Controlling for observable covariates (Column 4)

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Table 7
Venture performance and the entrepreneurship school

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		OLS		p=1 &	p=1 & controls	p=1 &	p=2 &
Estimate	OLS	& controls	p=1	controls	& h=1.5	h=1	controls
Web capital indicator	0.091*	0.088*	0.210*	0.207*	0.250	0.312*	0.346
	(0.052)	(0.052)	(0.118)	(0.115)	(0.161)	(0.189)	(0.253)
Web capital raised	1.633**	1.560**	3.034*	3.008**	4.382**	6.019**	6.345*
	(0.745)	(0.683)	(1.576)	(1.504)	(2.175)	(2.667)	(3.374)
Web traction	0.142*	0.134**	0.238*	0.229**	0.354**	0.413**	0.490**
	(0.077)	(0.063)	(0.128)	(0.115)	(0.159)	(0.202)	(0.243)
Web employees	0.379	0.400	1.985*	1.890*	2.280*	2.891**	2.760
	(0.384)	(0.349)	(1.086)	(1.124)	(1.374)	(1.360)	(2.045)
Web survival	0.100	0.066	0.087	0.107	0.340	0.335	0.257
	(0.071)	(0.068)	(0.183)	(0.180)	(0.255)	(0.278)	(0.363)
Survey P. capital indicator	0.329***	0.346***	0.455**	0.422**	0.243	0.217	0.200
	(0.080)	(0.080)	(0.199)	(0.210)	(0.316)	(0.296)	(0.403)
Survey P. capital raised	4.246***	4.501***	6.253**	5.739**	3.551	3.613	3.130
	(1.038)	(1.031)	(2.533)	(2.661)	(3.890)	(3.711)	(4.983)
Survey P. valuation	2.411*	2.218	5.520*	4.984	8.794*	7.972*	13.752*
	(1.436)	(1.455)	(3.284)	(3.497)	(5.028)	(4.590)	(7.344)
Survey P. traction	1.345	1.399	4.226	3.662	2.118	−0.438	0.324
	(1.197)	(1.223)	(2.733)	(2.868)	(4.183)	(3.848)	(5.202)
Survey P. employees	0.548**	0.580**	0.871	0.693	0.897	0.779	1.009
	(0.250)	(0.252)	(0.550)	(0.581)	(0.787)	(0.749)	(1.008)
Survey P. survival	0.134	0.143	−0.044	−0.142	−0.082	−0.050	−0.007
	(0.088)	(0.090)	(0.219)	(0.232)	(0.316)	(0.303)	(0.414)

This table reports the effects of entrepreneurship schooling (bundled with the basic services) on venture performance. Estimates are based on the regression $Outcome_s = \pi + \beta school_s + \breve{g}(Pitch_{day}score_s - 3.6) + Z_s'\varphi + \epsilon_s$, where $school_s$ is a variable that equals 1 if the participant was selected into the entrepreneurship school and $g(Pitch_{day}Score - 3.6)$ is a pth-degree polynomial of the normalized pitch-day score (i.e., pitch-day score minus the 3.6 cutoff). The outcome variable is specified in the title columns of each row, and the type of estimate is specified at the top of each column (i.e., the degree of the polynomial used (p), the bandwidth (h), and the inclusion of controls). Columns (5) and (6) include different bandwidth specifications (h) for pitch-day-score ranges of 2.1–5.0 (h=1.5) and 2.6–4.5 (h=1), respectively. For the OLS estimate, the polynomials are excluded from the estimation. For the RDD estimate, $School_s$ is instrumented using *Above*, a variable that equals 1 if the participant scored 3.6 or higher on the pitch day. To conserve space, the estimated coefficients for the constant and the polynomial terms are not presented in the table. The controls included are *Capital raised before* and generation fixed effects. Robust standard errors are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

only marginally affects the statistical significance, and, importantly, does not affect the magnitude of the estimated treatment effect.

Table 8 shows that the economic magnitude of the entrepreneurship schooling effect is similar across different proxies of a given outcome. For example, Column 3 shows that entrepreneurship schooling increases the likelihood of fundraising by 0.29 and 0.39 standard deviations, respectively, according to the Web-based metric (Web Capital Indicator) and survey-based metric (Survey P. Capital Indicator). Similarly, the same column shows that, based on Web-based (survey-based) metrics, entrepreneurship schooling increases the amount of capital raised by 0.30 (0.43) standard deviations, market traction by 0.31 (0.30) standard deviations, and employees by 0.34 (0.28) standard deviations. Table 8 reports normalized coefficient estimates following the same structure used in Table 7.

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Table 8
Summary economic magnitude of entrepreneurship schooling effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Estimate	OLS	OLS & controls	p=1	p=1 & controls	p=1 & controls & h=1.5	p=1 & h=1	p=2 & controls
Web capital indicator	0.126	0.122	0.290	0.286	0.350	0.433	0.478
Web capital raised	0.159	0.152	0.295	0.293	0.429	0.587	0.617
Web traction	0.184	0.174	0.307	0.296	0.457	0.531	0.633
Web employees	0.065	0.069	0.342	0.325	0.405	0.529	0.475
Web survival	0.083	0.055	0.072	0.088	0.286	0.288	0.212
Survey P. capital indicator	0.285	0.300	0.394	0.366	0.213	0.194	0.173
Survey P. capital raised	0.291	0.309	0.429	0.394	0.246	0.255	0.215
Survey P. valuation	0.149	0.137	0.341	0.308	0.552	0.512	0.849
Survey P. traction	0.094	0.098	0.295	0.256	0.151	−0.032	0.023
Survey P. employees	0.105	0.116	0.284	0.222	0.168	0.395	0.126
Survey P. survival	0.119	0.127	−0.039	−0.127	−0.075	−0.046	−0.007

This table reports the effects of entrepreneurship schooling (bundled with the basic services) on venture performance. Reported coefficients correspond to normalized betas of results in Table 7, which can be interpreted as standard deviation changes in the outcome variables. Estimates are based on the regression $Outcome_s = \pi + \beta school_s + \check{g}(Pitch_{day} score_s - 3.6) + Z_s' \varphi + \epsilon_s$, where $School_s$ is a variable that equals 1 if the participant was selected into the entrepreneurship school and $g(Pitch_{day} Score - 3.6)$ is a pth-degree polynomial of the normalized pitch-day score (i.e., pitch-day score minus the 3.6 cutoff). The outcome variable is specified in the title columns of each row, and the type of estimate is specified at the top of each column (i.e., the degree of the polynomial used (p), the bandwidth (h), and the inclusion of controls). Columns (5) and (6) include different bandwidth specifications (h) for pitch-day-score ranges of 2.1–5.0 (h=1.5) and 2.6–4.5 (h=1), respectively. For the OLS estimate, the polynomials are excluded from the estimation. For the RDD estimate, $School_s$ is instrumented using *Above*, a variable that equals 1 if the participant scored 3.6 or higher on the pitch day. To conserve space, the estimated coefficients for the constant and the polynomial terms are not presented in the table. The controls included are *Capital raised before* and generation fixed effects. Robust standard errors are presented in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

The results in Columns 5–7 in Table 7 show that the findings are also qualitatively robust to using different bandwidths and polynomials in the RDD estimation (following the same structure as Table 4). They also continue to hold when we restrict the sample to the last two generations (for which data collection may be more accurate), and are stronger for companies in industries that require a Web presence, such as e-commerce, media, mobile, social media, and social networks. This finding is consistent with the notion that start-ups from the "new economy" grow faster than those from traditional industries, enabling us to observe performance differences earlier on.

Finally, we find no evidence that the entrepreneurship school affects new venture survival (as measured by Web- or survey-based metrics), and the implied economic magnitude of the point estimates (Column 3, Table 8) is very small. 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 are more likely to rely on entrepreneurial capital.

The positive difference between the OLS and RDD estimates (Table 7, Columns 2 and 3, respectively), suggests the RDD recovers the treatment effect for a subpopulation of start-ups with relatively high returns to entrepreneurship schooling. Such positive difference is a common result in the education literature, particularly in papers that exploit supply-side innovations for

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identification (such as selection processes based on qualification scores). In these papers, the instrumental variables estimates of the return to schooling typically exceed the corresponding OLS estimates, often by 20% or more (Card 2001). The leading explanation proposed for this pattern is that supply-side innovations are most likely to affect the schooling choices of individuals who would otherwise have relatively low schooling because they face higher than average access costs. In that case, the "local average treatment effect" (Imbens and Angrist 1994) reasoning suggests the instrumental variable estimator would yield estimated returns to schooling above the average marginal return to schooling in the population, and potentially above the corresponding OLS estimates. This explanation is also likely to hold in our setting. Start-ups that score close to the qualification threshold ("close calls") are less likely to secure schooling elsewhere relative to start-ups that, during the pitch-day, are deemed to have more potential, partly because they have more entrepreneurial capital to begin with and are thus scored highly by judges ("high scorers"). Hence, whereas high scorers may only marginally benefit from schooling, close calls are likely to benefit substantially, as lack of entrepreneurial capital can explain their worse performance during the pitch-day.

An alternative explanation for the positive difference, which is less likely to resonate in our setting, is based on the additional assumption that the treatment effect of schooling is homogeneous; that is, that high scorers and close calls benefit equally from schooling. Under this assumption, the positive difference between the RDD and the OLS estimates would suggest that the selection process for the entrepreneurship school picks lower-potential rather than higher-potential start-ups, and thus that the OLS is biased downward rather than upward. This alternative interpretation is unlikely to hold in our setting for two reasons. First, the assumption that treatment effects of entrepreneurship schooling are homogenous across start-ups is not realistic. Not only has the education literature shown evidence of heterogeneous effects from schooling (cf. Card 2001), prior work on accelerators has emphasized how the treatment effect of these programs is likely to be heterogeneous. In particular, accelerators appear to be associated with both the accelerated success of good start-ups and the accelerated failure of bad ones (Yu 2016; Smith and Hannigan 2015). Second, while a criterion to select contestants based on their need for an "entrepreneurial capital subsidy" may be reasonable in certain situations, this criterion is not used in Start-Up Chile. Indeed, the Start-Up Chile selection process is based on a pitch *competition*, where judges are required to select the apparent highest performers, and no directives exist that guide staff to pick the weaker start-ups.

4.3 Additional robustness tests

In this section, we use complementary tests to provide suggestive evidence against six potential methodological concerns including identification issues, influential observations, potential demotivation effects on pitch competition

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losers, multiple types of survey- and Web-reporting biases, and potential schooling effects on reporting (rather than on real outcomes).

One first concern is that the RDD estimate is a consequence of the selection process and not of schooling; that is, that the pitch-day score, not the entrepreneurship school, explains the superior performance of schooled ventures. Against this concern, the results in Figures 5 and 6 show little evidence of a discontinuity in the scores or in the covariates at the 3.6 threshold. Instead, we would expect to see a jump in sample characteristics if breaks in *observable* venture quality explained the results. Three additional tests provide further supportive evidence against this concern. First, Appendix Figure 4.1 shows that the dispersion of pitch-day scores across judges looks similar for ventures closely above and below the threshold. If a break in *unobservable* characteristics explained the RDD estimates, we would instead expect less dispersion of pitch-day scores (i.e., less disagreement among judges) immediately above the threshold. Second, Appendix Table 4.1 shows that pitch-day scores are generally uninformative about performance. In regressions projecting outcomes onto pitch-day scores, pitch-day scores are never significant once we control for *Above*. By contrast, if the selection process was driving the RDD estimates, we would expect the score to be informative about outcomes. The lack of pitch-day score predictability is not surprising; identifying high-quality start-ups is no easy task, even for expert investors who spend much more time and effort on due diligence than the judging time in the pitch competition (e.g., Kerr, Nanda, and Rhodes-Kropf 2014). Finally, Appendix Table 4.1 shows that *Above* only predicts performance among schooled entrepreneurs. If pitch-day scores fully explained the RDD estimates, *Above* should also predict the performance for nonschooled entrepreneurs. Together, these additional results suggest systematic quality differences between firms closely above and below the 3.6 pitch-day threshold are unlikely to drive RDD results.

A second potential concern is that influential observations drive our results, as outliers typically explain aggregate performance in early-stage financiers. To test whether such potential outliers drive our results, we ran multiple "leave-one-out" regressions that exclude observations from the estimation one by one. We classify observations as influential if their removal changes the significance of our results; that is, if the p value of the estimate falls below 10%. Appendix Table 4.2 confirms that outliers are not the main explanation for the estimated effects and that no influential observations exist for four out of six of the main results in Table 7. For the remaining two results (Web Capital Indicator and Web Employees), the removal of influential observations only slightly changes the significance (from 0.074 to 0.115, and 0.095 to 0.160), and does not dramatically change the point estimate.

A third potential concern is that the failure to enter the entrepreneurship school may demotivate pitch-day competition losers, inducing a relative increase in the performance of schooled participants. Against this possibility, Table 7 shows that schooling has no effect on venture survival. This finding

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implies that if any demotivation was taking place, it would not be enough to fully discourage entrepreneurs from pushing their ventures further.[18] Appendix Table 4.3 presents further supportive evidence against this concern. We restrict the sample to losers and project venture outcomes based on pitch-day scores, *Above*, and the fraction of pitch-day winners that are in the same reference group as the start-up. Panel A (B) uses the ventures in the same industry (and location) of the start-up as a reference group. The table shows that the performance of losers does not correlate with the fraction of winners. If demotivation were the main explanation behind the findings, we would expect a negative relation between these two variables. If most similar start-ups are rejected, then the start-up is likely to blame the industry for the loss rather than its own individual performance. Finally, Appendix Figure 4.2 shows no decreasing pattern in performance across pitch-day scores below 3.6. If demotivation were the main explanation behind the effect, we would expect a dip in survival and performance to the left of the threshold.

A fourth potential concern is that schooling increases the probability of Web and survey reporting, which could potentially bias the RDD estimates (e.g., Drexler, Fischer, and Schoar 2014). Against this possibility, the survey response rate is the same across schooled and nonschooled ventures (the estimated difference is 0.09 and the t-statistic is 1.18). In addition, although Start-Up Chile encourages the use of AngelList for communication purposes program-wide, the entrepreneurship school gives no additional nudge to open a profile on this site. Consistent with this notion, schooled and nonschooled participants were equally likely to have an AngelList profile in December 2013 (the estimated difference is 0.01; the t-statistic is 0.10). We refined this test using information from Google Insights, for which we timed Web activity (we downloaded information on Web activity per semester based on company name). We found that schooled and nonschooled participants have a similar Web presence immediately after their potential participation in the program (with a difference of 4.59 and a t-statistic of 0.80).

A related concern is that schooled ventures may over-report performance in surveys, which would positively bias RDD estimates. For example, schooled ventures may want to exaggerate how well their firms benefited from the program. This concern is mitigated by results using Web sources, where entrepreneurs have both less control over what is reported and the potential for misreporting to be heavily punished. Although participants have the freedom to choose whether to open up profiles on social media platforms (e.g., Facebook), industry providers, such as CB Insights, sweep all Web sites collecting nonself-reported news about start-ups, which founders are less likely to be able to fully manipulate. Reputational considerations also likely mitigate rampant lying; misreporting fundraising can seriously affect start-ups' chances of securing

[18] Because our sample is not comprised of subsistence entrepreneurs, common sense suggests that if entrepreneurs were not motivated, they would simply stop pursuing their ventures.

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fundraising in the future. Relatedly, RDD estimates of survey-based metrics may also be positively biased if better applicants are more likely to answer surveys and judges' scores reflect quality. This concern is mitigated by the little information content provided by pitch-day scores; whereas the best companies may be more likely to answer surveys, evidence suggests that judges do not appear to consistently assign them scores above 3.6 (see Appendix Table 4.1).

Finally, RDD estimates may also be positively biased if schooling induces better record keeping, even if outcomes remain largely unchanged. We test for this potential source of bias by examining survey-reporting errors and their association with schooling (cf. Drexler, Fischer, and Schoar 2014; Berge, Bjorvatn, and Tungodden 2015; de Mel, McKenzie, and Woodruff 2014). We find that the propensity to err is not statistically different across schooled and nonschooled participants (estimated difference is 0.02 and t-statistic is -0.99).[19]

5. Discussion

Our findings show evidence that entrepreneurship schooling, bundled with the basic services of cash and coworking space, appears to lead to significantly higher new venture performance within the first 4.75 years since entry to the accelerator. By contrast, basic services alone do not appear to have an effect on new venture performance. This is the first paper to provide this type of evidence for business accelerators, which have emerged as a new institutional form to support early stage start-ups. While nascent academic research has begun exploring this growing institutional form, very little rigorous evidence exists on the effect of business accelerators on new venture performance, and on distinguishing which of the services provided by these programs add value.

The pattern of results suggests that entrepreneurial capital is a key factor for new venture performance. This pattern is also consistent with the well-established findings on how interventions that combine finance (especially grants) and business training are more effective in supporting subsistence businesses than finance alone (e.g., Bandiera et al. 2013).

Moreover, the implied magnitude of the findings is also similar to prior papers on business training interventions. For example, Calderon, Cunha, and De Giorgi (2013) and de Mel, McKenzie, and Woodruff (2014) find a 20% and 41% increase in sales (within 12 and 8 months). These results are similar to our estimate of a 23.8% increase in Web-based venture traction (and are within the confidence interval of our survey-based results; see Column 3 in Table 7). 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 five

[19] We record as errors all instances in which start-ups report (1) higher sales during the previous year than since they began operations, (2) higher sales in Chile than overall sales, (3) higher capital fundraising in Chile than overall, and (4) more employees in Chile than overall.

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to seven months of a three-day training intervention. This estimate is within the standard error bands of both our estimates on employment reported in Column 3 of Table 7 (198.5% and 87.1%, Web-based and survey-based, respectively). Finally, similar to our own noisy estimates for survival, most studies also find positive but insignificant impacts (cf. McKenzie and Woodruff 2014).

In the rest of this section, we discuss three final points: the external validity of the results; the potential reasons why basic services alone have no apparent impact in our setting; and the potential mechanisms through which entrepreneurship schooling bundled with basic services can affect new venture performance. We also address the potential frictions preventing entrepreneurs from acquiring these services elsewhere. Although our setting does not allow us to pinpoint the exact mechanisms underlying entrepreneurship schooling, our discussion, grounded in the education literature, sheds light on future research avenues.

5.1 External validity

Two issues can affect the external validity of results. The first is that our estimates correspond to local average treatment effects around qualification cutoffs, and the subpopulations of start-ups around those cutoffs may not necessarily be representative of average start-ups in the population. For example, these start-ups may be particularly sensitive to schooling services, which may explain the large magnitudes of our estimates. However, the local nature of the estimates does not necessarily decimate their external validity (cf. Card 2001). From a practical standpoint, our local estimates are particularly useful to policy makers. After all, policy interventions of entrepreneurship schooling are best suited to target subpopulations that are particularly lacking in entrepreneurial capital.

The second potential issue is that Start-Up Chile is a unique program, and thus results from this setting may not generalize to other ecosystem accelerators. However, Start-Up Chile is actually very similar to the average ecosystem accelerator.

For example, reallocation requirements for participants and the location of the program (typically an underdeveloped economic hub) are common traits. Indeed, we estimate that 58.6% of ecosystem accelerators worldwide require full founding team relocation, and an additional 31.0% have at least some partial relocation requirement.[20] Moreover, 37.9% of ecosystem accelerators are located in underdeveloped regions (17.9% Africa, 10.3% Latin America, 10.3% India); 37.9% are in the United States, but outside Silicon Valley; and the rest are in Europe, the United Kingdom, or Canada, but not typically in the capital cities of these countries. In addition, concerns regarding the ability of

[20] 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.

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average ecosystem accelerators to replicate the visibility increase provided by Start-Up Chile do not appear first order. We estimate that news sponsors cover 76% of ecosystem accelerators (i.e., have at least one article in their press clips or the program futures in TechCrunch's CrunchBase database). Also, the vast majority of accelerators publish the names of their participants on their Web pages (in fact, this practice is common across early-stage financial intermediaries), and finalize the program with a well-publicized demo day (Cohen and Hochberg 2014).

Finally, the guidance and accountability and networking opportunities provided by the entrepreneurship schooling at Start-Up Chile are prevalent among ecosystem accelerators, and among investor-led and corporate business accelerators more generally. In surveys on business accelerators, network development as well as guidance and accountability appear extensive, although quite heterogeneous across programs. Cohen and Hochberg (2014) argue that accelerator programs generally include seminars given by staff or guest speakers who often provide one-on-one guidance after the talk. Some programs also schedule meetings with several mentors; others may make introductions on an as-needed basis, or simply hand entrepreneurs a list of preselected mentors. A recent report by Nesta and the Department for Business, Energy and Industrial Strategy (2017) on accelerators in the United Kingdom documents a great deal of variation in the way that the entrepreneurship schooling services are imparted. In particular, of surveyed accelerators, 85% offer some form of mentoring; 45% offer seminars or workshops; and 12%, 11%, and 6% offer more specific help in the form of access to experts, legal/accountancy support, and technology support, respectively.

Regardless of the similarities between Start-Up Chile and other ecosystem accelerators, we are, however, careful to emphasize the differences between average applicants to Start-Up Chile and other ecosystem accelerators worldwide, as mentioned in Section 1.3. We argue that the external validity of our findings is likely confined to other ecosystem accelerators that focus on young founders and early-stage start-ups.

5.2 Why did basic services of cash and coworking space have no apparent impact on new venture performance?

Other than the null hypothesis being true, one potential explanation is that start-ups near the capacity threshold are heterogeneous in their potential for success, and that basic services accelerate the inevitable outcomes (growth or failure) of participants relative to nonparticipants. In this case, the program might accelerate the success of high-potential business opportunities and expedite the demise of low-potential ones, with a resulting zero average treatment effect. For example, cash infusions can help founders discover fundamental flaws in their prototypes that justify the termination of the start-up or help them access new information that justifies the creation of a different start-up (Yu 2016; Smith and Hannigan 2015; Leatherbee and Katila 2017).

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Other potential explanations are less consistent with the results. One first alternative is that we do not have enough power to reject the null. That is, that the effect is indeed positive, but we do not have a big enough sample to distinguish it from zero. Against the notion that this possibility is a first order explanation, Table 6 shows that estimated effects are often negative, sometimes close to zero, and switch signs across specifications. In addition, Appendix Table 4.4 summarizes back-of-the-envelope power calculations that suggest we have enough power in the majority of specifications.

A second potential explanation is that the capital infusion is too small (not critical enough) to generate positive returns. While this explanation cannot be completely ruled out, it is not as likely to resonate in our setting for three 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. Third, the program estimates that relocation costs only represent 10% of the grant. In addition, the cash provided by Start-Up Chile is actually above the average stipend of $22,890 provided in business accelerators, as reported in Cohen and Hochberg (2014).

A third potential explanation is that rejected applicants secured acceleration services elsewhere, thereby dampening the estimated effect of the basic services. The analysis of supplementary data does not support this alternative story. We collected information from Seed-DB[21] regarding nonparticipants' acceptance into other programs, and found that 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 (Butcher 2014).

One last potential explanation is that the mechanisms to curb entrepreneurs' opportunistic behavior[22] are not strong enough through the program's basic services. Indeed, the value-adding monitoring and powerful allocation of control rights by financial intermediaries (Hellmann 1998; Cornelli and Yosha 2003) are most pronounced in the entrepreneurship school. According to the program's staff, however, even nonschooled 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 may be a due to reputational consequences acting as disciplinary devices (cf. Bernthal 2015).

[21] Seed-DB is an open-source accelerator database built on CrunchBase data (http://www.seed-deb.com/).

[22] The program takes no equity stake in participating start-ups. However, it engages in capital staging, and reputational consequences of opportunistic behavior are likely.

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5.3 How does entrepreneurship schooling affect performance?

While our setting is not suited to distinguish the specific mechanisms by which entrepreneurship schooling can affect performance, our results provide an empirical foothold for future research to explore how specifically schooling matters.

Based on the education literature, and consistent with interviews and surveys to participants, we distinguish two broad potential mechanisms (see Appendix 1): productivity increases (Becker 1975) and certification (Spence 1973; Arrow 1973). Productivity may increase via the instruction of entrepreneurship know-how from peers and staff (cf. Lerner and Malmendier 2013), access to valuable social networks (Granovetter 1973; Ketchen, Ireland, and Snow 2007; Cai and Szeidl 2016), the structured accountability imposed by regular meetings (cf. Locke and Latham 2002; Cialdini and Goldstein 2004), and increases in the self-efficacy of founders (Bandura 1982; Forbes 2005; Heckman and Kautz 2014). In the absence of business accelerators, start-ups may not realize these productivity increases because of market frictions, such as informational constraints. For example, accelerators may provide entrepreneurial know-how through experiential learning, which is a type of learning not typically found in traditional educational programs. Business school syllabi rarely provide such learning opportunities, as often professors lack the necessary hands-on business experience. Indeed, it is only until recently that universities have begun to replicate the business accelerator model in their educational programs. In addition, business accelerators may help overconfident entrepreneurs to recognize the value of accountability structures for performance, which they would not otherwise demand. Furthermore, some founders may simply be unaware of the importance of building business networks and not look for networking opportunities outside the accelerator.

Certification may also be at play because business accelerators typically increase the exposure and legitimacy of ventures (Zott and Huy 2007) via, for example, the promotion of start-ups on accelerators' Web sites and during the demo days at the end of the programs (Cohen and Hochberg 2014). Start-ups may need certification because of information asymmetries relative to the potential performance, which are typically prevalent in transformational ventures.

Future research may extend exploration of the effects of entrepreneurship schooling to the founder-level of analysis. 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 can seek to answer.

6. Conclusions

Whether accelerators affect new venture performance is an important question with both theoretical and practical implications. However, until now little

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rigorous evidence has existed about whether accelerators are effective, and, if so, which services 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, shedding light on entrepreneurship schooling as a topic that is ripe for future research.

We evaluated an ecosystem accelerator that provides participants with seed capital and coworking space. The accelerator also provides entrepreneurship-schooling services to a competitively select few. We find entrepreneurship schooling bundled with the basic services of cash and coworking space leads to significant increases in venture fundraising and scale. By contrast, we find no evidence that the basic accelerator services alone improve new venture performance.

Regarding the policy design of ecosystem accelerator programs, if the objective is to accelerate start-ups, our results suggest that more resources should be allocated toward combining basic services with entrepreneurship schooling, rather than providing basic services on their own. This conclusion is particularly valid for programs that focus on young founders and early-stage start-ups.

The findings are consistent with the view that entrepreneurial capital, similar to managerial capital, is a type of capital that is missing among certain populations (cf. Bruhn, Karlan, and Schoar 2010). Avenues for future research can include distinguishing the mechanisms through which entrepreneurship schooling and basic services in business accelerators affect entrepreneurial capital, including potential productivity increases and certification effects.

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