Better Flee From Freedom?

The Effects of Structured Accountability on New Venture Performance

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Working Paper
March 2021

We thank Anna Valero and participants of the Innovation Growth Lab 2020 Global Conference for thoughtful and constructive feedback. We gratefully acknowledge the financial support of IGL, Kauffmann Foundation and ANID (The Chilean Agency for Research and Development) through the FONDECYT Iniciación Project number 11170537. The support of Start-Up Chile and CORFO was invaluable for a successful implementation of the experiment. We recognize the support of David Fernández, Rocío Fonseca, Sebastián Díaz, Ángeles Romo and all the Start-Up Chile team. We are particularly grateful to Macarena Gonzalez, for her outstanding research assistance. Also, José Guridi, Rocío Ortega and Santiago Reyes provided helpful assitance. The collaboration of so many participants, mentors and scribes was key and is greatly appreciated.

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ABSTRACT

Policies to promote entrepreneurial activity have received much attention in recent years. However, there is still much to learn about which policies actually work, and what are the exact mechanisms by which they do. We explore the performance effects of structured accountability, a policy that encourages founders to periodically express their strategic plans and progress in front of others. We conduct a randomized controlled trial on a balanced sample of 361 business accelerator startups, quantify the subsequent performance of startups using standard measures, and conduct in-depth interviews to a randomly selected subsample. Prior literature has suggested that the effects of structured accountability vary across the dimension of task uncertainty. Our results shed light on a new dimension: structured accountability can aid or harm startups as a function of the founder's level of formal education.

1. INTRODUCTION

Business accelerators have become a relevant and effective organizational form to shape entrepreneurial ecosystems and support new ventures. In addition to becoming a prevalent institution in most entrepreneurship hubs across the globe (as of 2021 CrunchBase lists almost 3,000 accelerators worldwide), accelerators have garnered much scholarly interest (Cohen *et al.*, 2019b; Gonzalez-Uribe and Leatherbee, 2018; Hallen, Cohen, and Bingham, 2020; Yu, 2020). Early work focused on the effects of accelerators on new venture performance, finding consistent evidence to support their role as an organizational sponsor that supports and promotes entrepreneurship (Armanios *et al.*, 2017; Gonzalez-Uribe and Leatherbee, 2018; Hallen *et al.*, 2020). This was important work because entrepreneurship is responsible for a large proportion of new job creation (Haltiwanger, Jarmin, and Miranda, 2013), and providing evidence about the positive effects of accelerators is a fundamental building block for further research. After establishing the causal effects of accelerators an an instrument for endowing participant ventures with entrepreneurial capital[2] (Armanios *et al.*, 2017; Gonzalez-Uribe and Leatherbee, 2018; Gonzalez-Uribe and Reyes, 2021; Hallen *et al.*, 2020), scholars shifted attention towards understanding the specific mechanisms that drive this phenomenon. This was important because of the mounting evidence that design features of accelerators are responsible for the heterogeneity in the impact they have on new venture performance (Chan, Patel, and Phan, 2020; Hallen *et al.*, 2020), such as broad, intensive and paced consultation practices (Hallen *et al.*,

[2] While managerial capital has been found to improve the exploitation of existing businesses (Bloom and Van Reenen, 2010; Bruhn, Karlan, and Schoar, 2010), entrepreneurial capital improves the discovery and capture of new ventures (Gonzalez-Uribe and Leatherbee, 2018).

2020), customized advice and visibility (Gonzalez-Uribe and Reyes, 2021), regular updates with program managers (Cohen *et al.*, 2019b), and feedback about the viability of business ideas (Leatherbee and Katila, 2020; Yu, 2020).

Prior literature has highlighted the parallels between accelerators and business schools, as both have broadly similar features of capability-building and certification (Armanios *et al.*, 2017; Dutt *et al.*, 2016; Gonzalez-Uribe and Leatherbee, 2018; Hallen *et al.*, 2020): a competition for admittance (sorting); acquisition of knowledge about organizational practices (learning); access to unique information (networking); periodic examinations and a graduation challenge (accountability structures); and certification (signaling). While this prior literature has explored the broad features of sorting, learning, networking and signaling, little is known about the feature of accountability on new venture performance.

We focus on studying the causal relation between structured accountability (as a business accelerator feature) and new venture performance. Specifically, we ask "How does structured accountability affect new venture performance?" and "What are its boundary conditions?" By structured accountability we mean supervisory structures that prime or coherce entrepreneurs to report on their periodic progress, and to commit to execute specific tasks. It is important to answer this question because prior research has theorized about features that are conceptually close to structured accountability – such as regularly describing to program managers what founders have learned (Cohen *et al.*, 2019b), providing temporal structures that encourage entrepreneurs to converge quicker towards decisions and actions (Hallen *et al.*, 2020), and standardizing programmatic activities (Cohen, Bingham, and Hallen, 2019a) – and have called for stronger evidence about the features' causal effect on venture performance. Moreover, our paper distinguishes itself from prior accelerator literature that focuses primarily on the relation

between learning and decision-making. We do so by answering calls to better understand the psychology-based features that accelerators can leverage for the benefit of new ventures (McKenzie, 2020) and contributing to the literature studying the psychological mechanisms that affect strategic decision-making (Grégoire, Corbett, and Mcmullen, 2011; Grimes, 2017).

Our paper is also relevant for practice. Firstly, because the race of accelerators to establish a reputation of actually increasing new venture performance attracts higher quality applicants, pushing accelerators into a self-sustaining virtuous cycle (Hallen *et al.*, 2020). Secondly, many accelerator managers are experimenting with new feature designs, few of which are evidence-based. This trial-and-error approach can lead to supersticious learning and unwittingly perpetuate the use of ineffective acceleration practices.

To answer our research question we conduct a two-year randomized controlled trial field experiment with 361 business accelerator startups. Because new venture performance can express itself in different ways depending on its goals and business model, we keep with prior literature (Gonzalez-Uribe and Leatherbee, 2018; Hallen *et al.*, 2020) and we measure multiple ex-post performance variables. We complement our empirical results with a qualitative analysis based on in-depth semi-structured interviews of a random subsample of 22 startups. Moreover, since prior literature has found that accelerator features can affect new ventures in different ways, for example based on their inherent quality (Gonzalez-Uribe and Reyes, 2021), we explore treatment heterogeneity across founder education level. We chose education level due to the tight relation between self-regulation, procrastination and graduate-level education. Relative to undergraduate students, graduate students are more likely to think critically, and less likely to procrastinate (Artino and Stephens, 2009), especially when they are intrinsically driven (Cao, 2012), which is a hallmark of entrepreneurs.

We find that, despite the intuitive appeal for policy-makers and the strong theoretical support for using structured accountability in firms (Garg, 2013; Lerner, 1995; Sapienza and Gupta, 1994; Westphal, 1999), structured accountability has heterogeneous effects on new venture performance. It is useful for some types of founders, but not for others. Specifically, for new ventures whose founders have relatively lower education levels (i.e., upto undergraduate studies), structured accountability increases performance consistently across multiple measures. In contrast, for new ventures whose founders have higher levels of education (i.e., graduate studies), our point estimates suggest that structured accountability is detrimental (although not statistically so). It is clear from our findings that education level of founders is a key driver when it comes to deciding wether to provide structured accountability to new ventures. Interstingly, there appears to be a contradiction between what entrepreneurs need and what they want. Our qualitative analysis shows that, despite the fact that structured accountability has detrimental (or at least null) effects on the ventures of founders with high education, these founders appear to want more structured accountability instead of less of it.

Our findings make theoretical contributions to at least four literature streams. First, we contribute to the growing literature around business accelerators. Specifically, how to better design the features of programs aimed at fostering entrepreneurship (Gonzalez-Uribe and Leatherbee, 2018; Hallen *et al.*, 2020), by showing the first causal estimates of the impact of structured accountability on venture performance, and delving into the use of psychological mechanisms to drive entrepreneurial performance (McKenzie, 2020). Second, we contribute to a better understanding of the boundary conditions of supervisory structures (Barney *et al.*, 1996; Daily *et al.*, 2002; McGrath, 2001), sharpening the limitations of new venture oversight. While there is growing literature on new venture boards and their interaction with founder-CEOs (Garg

and Eisenhardt, 2017; Garg and Furr, 2017; Garg, John Li, and Shaw, 2019), there is little empirical and causal evidence about ways to improve such interactions. We show that structured accountability can be an effective tool for venture boards. Third, we contribute to the discussion about new venture founding team characteristics (Beckman, 2006; Boeker and Wiltbank, 2005; Eisenhardt and Schoonhoven, 1990; Furr, Cavarretta, and Garg, 2012; Vissa and Chacar, 2009). Prior studies have extensively examined how new venture performance is related to the characteristics of individual entrepreneurs (Brockhaus, 1980; Hsu, Roberts, and Eesley, 2007; Shane, 2000), and we contribute to the emerging literature about the relation between new venture teams' educational background and the entrepreneurial process (Leatherbee and Katila, 2020). Specifically, we show that the educational background of the founders is a key characteristic to sort startups into different support programmes. Fourth, we contribute on recent calls to provide more evidence-based insights to strategic management (Chatterji *et al.*, 2016), by providing, to the best of our knowledge, the first randomized controlled trial to test the effects of structured accountability on new venture performance.

The rest of our paper is structured as follows. We provide a background to frame our work, after which we describe the research setting and methods, and empirical setup. Next, we present our results, followed by a discussion and concluding remarks.

2. BACKGROUND AND MOTIVATION

2.1. Business Accelerators and New Venture Performance

How to improve entrepreneurial performance is a question that intrigues new venture stakeholders across the board. Founders strive to find methods and techniques that may help them reach their goals faster. Angel investors and venture capitalists continuously experiment and explore the landscape for strategies to improve the return on investment of their portfolio

companies. Governments are continuously under public scrutiny to have a favorable socioeconomic effect as they allocate taxes earmarked for entrepreneurship promotion. Despite the importance of new ventures for economic growth (Haltiwanger *et al.*, 2013), and the relevant public and private resources spent to foster entrepreneurial activity, little is known about which specific interventions actually speed up new venture performance.

Currently, one of the most popular institutional forms aimed at supporting new ventures are business accelerators. They typically are a fixed-term, cohort-based, financial intermediary that offer start-ups cash, shared office space, and business education. From only one in 2005—Y Combinator—thousands now exist worldwide, with governments sponsoring approximately 18% of the programs (Lewis, Harper-Anderson, and Molnar, 2011). Accelerators distinguish themselves by their strong emphasis on the business-education component (Cohen and Hochberg, 2014). The main purpose of this component is to guide entrepreneurs down the path that will lead to greater performance faster, a path that is assumed some entrepreneurs will not naturally follow on their own accord.

While testing the causal effects of accelerators has recently drawn the attention of entrepreneurship scholars (Bernthal, 2015; Fehder and Hochberg, 2014; Gonzalez-Uribe and Leatherbee, 2018; Hallen *et al.*, 2020; Yu, 2020), questions still remain regarding the specific programmatic features or mechanisms through which business accelerators affect new venture performance. Recent work has found causal evidence about the performance-enhancing effects of accelerator's role as "entrepreneurship school" (Gonzalez-Uribe and Leatherbee, 2018; Gonzalez-Uribe and Reyes, 2021). However, the entrepreneurship school has, thus far, been treated as a "black box" and it is not yet clear what are the exact mechanisms of the school that drive the superior performance.

Based on the education literature, we distinguish two broad potential mechanisms within the entrepreneurship school (see Figure 1): productivity increases (Becker, 1975) and certification (Arrow, 1973; Spence, 1973), both of which have been found to be present in similar entrepreneurship-promotion institutions (Armanios *et al.*, 2017). Productivity may increase via the instruction of entrepreneurship know-how from peers and staff (Hallen *et al.*, 2020; Lerner and Malmendier, 2013), access to valuable social networks (Granovetter, 1973; Ketchen, Ireland, and Snow, 2007), the structured accountability imposed by regular meetings (Cialdini and Goldstein, 2004; Locke and Latham, 2002) and increases in the self-efficacy of founders (Bandura, 1982; Forbes, 2005). In the absence of business accelerators, new ventures may not realize these productivity increases because of market frictions, such as informational constraints.

Certification may also be at play because business accelerators typically increase the exposure and legitimacy of ventures (Zott and Huy, 2007) via, for example, "signaling" (Hallen *et al.*, 2020) or "certification" (Armanios *et al.*, 2017) of participant ventures on accelerators' web sites and during the demo days at the end of the programs (Cohen and Hochberg, 2014). New ventures may need certification because of information asymmetries relative to the potential performance, which are prevalent in uncertain and fast-moving environments (Eisenhardt, 1989).

Regarding the accountability component of entrepreneurship schools, Gonzalez-Uribe and Leatherbee (2018) found suggestive evidence that structured accountability could play a role, but had no rigorous evidence. This paper takes a step towards opening the black box of the entrepreneurship school embedded in the business accelerator model, by specifically testing the causal effects of structured accountability on new venture performance. This focus is not only important because it contributes to an unexplored feature in the literature of business

accelerators, and delves into the psychological mechanisms that could be used to improve new venture performance (McKenzie, 2020). Moreover, it can be a cheaper mechanism to implement relative to other features, such as mentors.

2.2. Structured Accountability

By "structured accountability" we mean the process by which entrepreneurs are exogenously encouraged to articulate to a high-status third party (i.e., an accelerator staff member, a board member, or a peer), on a regular basis, the strategic activities they consider important to work on during a given time period (e.g., a month), and how they fared with the tasks they committed to during the previous period (i.e., their progress, achievements and challenges). Thus, by relying on preemptive self-criticism in anticipation of opinions by others (Hoorens, 1993; Leary and Kowalski, 1990), social pressure to execute the declared committed tasks (Thaler, 2000), and periodic deadlines (Ariely and Wertenbroch, 2002; Latham and Locke, 2006), the structured accountability policy may guide entrepreneurs to perform better than the alternative of leaving them to their own volition.

Accountability has been defined as the "expectation that one may be called on to justify one's beliefs, feelings, and actions to others" (Lerner and Tetlock, 1999: p. 255). As this definition suggests, accountability is a broad term that encompasses different actions or situations, such as the mere presence of others, identifiability, explanation-giving, and evaluation (Tetlock, 1985). In established organizations, accountability is typically reflected in performance evaluations, employment contracts, and reward systems, among other procedures (Frink and Klimoski, 2004). In the case of new ventures, such accountability structures are rare, as organizational routines are seldom in place, and the main drivers of the organization are the founders, who, by construction, rarely have a superior to report to.

2.3. Does Structured Accountability Improve or Hinder New Venture Performance?

It is unclear whether new ventures benefit from structured accountability. Innovation and learning are key pillars for new venture success, especially in uncertain and dynamic environments (Eisenhardt, 1989). When it comes to tasks that require exploration and learning, McGrath (2001) found that managerial oversight actually decreased performance, while oversight increased performance when exploration about business ideas shifted towards their exploitation. There are reasons to believe that imposing accountability structures on founders of ventures that have yet to become consolidated may actually not be a good idea. In fact, venture CEOs do not always find board advice valuable (Barney *et al.*, 1996; Ehrlich *et al.*, 1994), which suggests that perhaps, under certain conditions, founders would be better off spending their time running the firms rather than reporting to a board.

Moreover, prior literature has found that accountability negatively affects important entrepreneurial behaviors. For example, it reduces risk-taking and cognitively complex thinking (Lerner and Tetlock, 1999), as well as creativity and knowledge exchange (Son, Cho, and Kang, 2017). Moreover, there is an inverted U relation between accountability and performance, such that too much of it can negatively affect new venture performance (Garg, 2013; Guldiken and Darendeli, 2016). Furthermore, when startups are conducting exploratory efforts, which are important for learning processes and typically antecede exploitation processes needed once the business idea has been validated, managerial oversight appears to obstruct the important learning that is required during the discovery process (cf. Leatherbee and Katila, 2020).

Furthermore, founders may also reject accountability structures as entrepreneurs are typically driven by the appeal that creating their own companies provides independence from hierarchical structures (Hisrich, 1984), and conforming to such structures may create a negative

predisposition or motivate an avoidance of such instances. Thus, if a founder were forced to participate in such instances, perhaps they would do so with little interest and effort.

In contrast to the negative perspective of accountability on new venture performance, the psychology literature views it in a more positive light. An interesting perspective in accountability comes from judgment and choice research, which shows that human decisions are prone to a number of cognitive biases (Bazerman and Moore, 2013; Kahneman, 2011). In this context, accountability has been proposed as a prescriptive strategy to debias decisions (Larrick, 2004). Indeed, accountable decision makers tend to use more systematic decision strategies (Ashton, 1992; Kausel *et al.*, 2015; Murphy, 1994), decrease their overconfidence (Tetlock and Kim, 1987), and make fewer sunk costs errors (Simonson and Staw, 1992).

The social context of accountability is key, as it rests upon the power of social motivation. Several researchers have argued that people have generally a strong motivation in protecting and enhancing their social image (Hoorens, 1993; Leary and Kowalski, 1990)—especially when confronted with high-status peers. Accordingly, people put more effort in certain tasks under accountability to positively impress others and avoid embarrassment, thereby enhancing their own self-worth in important dimensions (e.g., competence, intelligence). In particular, accountable individuals engage in "preemptive self-criticism" by evaluating their judgments and decisions critically and anticipating counterarguments of potential critics (Connolly, Reb, and Kausel, 2013; Tetlock, 1983). As a result, they generally improve their judgment and decision-making processes.

Accountability is also important when people plan tasks and set deadlines, which is relevant to our idea of structured accountability (Latham and Locke, 2006). For example, studies by Ariely and Wertenbroch (2002) suggest that when individuals are accountable of delivering

their work progressively (i.e., at regular intervals), they tend to improve their performance. Moreover, having externally imposed deadlines and assessments can help individuals avoid spending effort on activities that may appear urgent or easy to complete, but are strategically trivial.

These two apparently competing views make it hard to anticipate the effects of structured accountability on new venture performance. While McGrath's (2001) findings would suggest that ventures that are in early phases of development may be negatively affected by an accountability structure, the boundaries between exploration and exploitation for these organizations are not quite clear.

Inspired by the psychology literature and recent findings that accelerator features can affect new ventures in different ways depending on their inherent quality (Gonzalez-Uribe and Reyes, 2021), we take an alternative contingency approach: heterogeneity in founder characteristics. One dimension that varies across founders in our sample is educational level. Our focus on this dimension is based on the principle that heterogeneity in education level explains differences in self-regulation, procrastination and critical thinking, which have been found to be governed by accountability structures (Ariely and Wertenbroch, 2002). In fact relative to undergraduate students, graduate students are more likely to think critically, and less likely to procrastinate (Artino and Stephens, 2009), especially when they are intrinsically driven (Cao, 2012), which is a hallmark of entrepreneurs. We are thus theoretically driven to explore this dimension within our study.

3. INSTITUTIONAL SETTING

3.1. Research Setting

In this section we summarize the details of our research setting. Start-Up Chile is a business accelerator sponsored by the Chilean government that was introduced in 2010. Its main aim is the attraction of early-stage, high-potential entrepreneurs from across the globe, and the transformation of the domestic entrepreneurship ecosystem. As of January 2021, approximately 1,960 start-ups had participated in the program, and nearly 14,000 had applied.

Like other business accelerators worldwide, Start-Up Chile is a fixed-term, cohort-based program. Once per semester, Start-Up Chile accepts roughly 90 startups into the program, who coexist in the accelerator during a six-month tenure.[10] Each startup receives an equity free grant of roughly $30,000 from the government. 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 startups "graduate" through a "demo day" competition (i.e., a formal presentation of the companies to external investors).

3.2. The provision of standardized activities as a feature of accelerators

One of the features of Start-Up Chile that we exploit in this study is a standardized activity (see Cohen *et al.*, 2019a) provided regularly to participants. We focus on a specific type of standardized activity: roughly seven monthly meetings throughout the program (see Table 1 for variation in the number of planned meetings in each of the cohorts in our study). These meetings are held in English, which is the main common language spoken by participants in the business accelerator. Participation rates are high (Section 4 discusses participation in detail) and considered good "citizenship."

The first type of meeting is known as *Platoons*. These meetings are moderated by a program staff *executive*, include four to eight program peers, and occur in roughly two-month intervals (see Table 1 for details on variation in number of startups per Platoon across cohorts).

Executives are assigned roughly 2 platoons per cohort. The assignment occurs at the start of the program based on industry and does not change during program roll-out. The purpose of the Platoons is to encourage the exchange of experiences and lessons among the entrepreneurs and are moderated by the corresponding executive.

The second type of meeting is known as *Boards.* These meetings are held between the lead founder and a panel of two to three industry experts who act as proxy board members or "advisors." Industry advisors are matched to startups such that participants meet with the same industry experts each time. Note that no other program peers attend these meetings. The purpose of the Boards is to provide team leaders with customized feedback. Boards are also are held at two-month intervals, interspersed with the Platoon meetings such that entrepreneurs participate in either the Platoon or the Board activity roughly once per month (every 3-4 weeks). Boards were implemented starting at cohort 19, replacing half the Platoon sessions such that all cohorts had roughly the same number of standardized meetings.

Participant startups are not allowed to change from their assigned platoon nor attend other platoon meetings. Thus, for a given entrepreneur, its executive, board advisors and Platoon peers remain constant throughout the program. Platoon and board sessions last 83 minutes on average, but there is variation across cohorts. The average length in cohorts 18 and 21 was roughly 110 and 70 minutes on average (see Table 1, row 12).

3.3. Sample

Our sample includes 369 Start-Up Chile participants across five cohorts of the program (cohorts 17-21). The experiment took place between February 2017 and August 2019 (the application and graduation dates for each cohort are summarized in Table 1). On average, each cohort includes 5

executives and the average number of startups per executive is 10. Each cohort includes between 12 to 14 Platoons, and each Platoon includes between 5 to 8 startups.

We pooled five cohorts to increase statistical power: the number of startups that participated in each cohort ranged between 54 and 85. Pooling makes sense in our context because the standardized activities offered by Start-Up Chile did not vary substantially across cohorts (see Sections 3.1-3.2). Naturally, it is still important to control for potential differences across cohorts (for example, the average quality of participants or skills of the program staff participating in a given cohort), and we do this in several ways as we explain in more detail in the empirical strategy section (Section 4).

Start-Up Chile provided us with all the application data, including application scores, for the participants in these five cohorts. Based on the program's records, we constructed six covariates to use as controls in our empirical strategy: age of the lead founder (Age), indicator variables for domestic and female applicants (Chilean, Female), the natural logarithm of the number of employees (Initial Employees), the natural logarithm of the (monthly) sales (Initial Sales), a discrete variable indicating the stage of the applicant (Initial Stage),[3] the application score (Score) and indicator variables for capital raised before application to the program (Capital Raised Before), and for highly educated founders (High Education)—i.e., Masters and above.

Table 3 provides summary statistics of our sample. Roughly 47% of participant startups have raised external financing prior to their application, the average number of employees is 4.83, and has monthly sales of $9.5K. Participants are concentrated in information technology

[3] A self-reported variable of 4 categories: Concept, Prototype, Functional Product, Scaling Sales.

related sectors—IT & Communication (17%), Education (11%), and Health (10%). The proportion of Chilean participants is 31% (see Appendix for a more detailed industry breakdown). Most founders are between 25 and 35 years old (average age is 32), and the proportion of women in the program is 22%.

Our sample is comparable to prior research on early-stage ventures, particularly in terms of the number of employees (e.g., Haltiwanger *et al.*, 2013) and industry representation (e.g., Puri and Zarutskie, 2012). Our sample is also comparable to the ecosystem business-accelerator genre (Clarysse, Wright, and Hove, 2015). Using information from the Entrepreneurship Database (ED) program at Emory University, which has records of multiple ecosystem accelerators worldwide, we report in the Appendix at-application comparisons between the startups (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 participant 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.

4. EXPERIMENT DESCRIPTION

In this Section we describe the Structured Accountability experiment design. We begin by describing the assignment of participants to groups, then we discuss details of the treatment and control offerings. Finally, we describe the mechanisms we used to ensure the proper implementation of the experiment.

4.1. Random assignment

We used a stratified randomization (Edovald and Firpo, 2016) with a 1:1 allocation ratio (see Figure 2), based on the executive assigned to the startup. The executive assignment was done according to industry – a criteria required by Start-Up Chile. The benefit of this randomization

approach is the reduction of the intrablock variance, which makes treatment estimates more accurate because of increased statistical power and precision of test statistics (Ariel and Farrington, 2010).

In detail, all participants in a given cohort were classified by Start-Up Chile according to industry and assigned to executives on that basis. Executives can have more than one industry assignation, and all founders in the same industry were not necessarily assigned to the same executive because of executives' capacity constraints. For a given executive, we then randomly allocated the founders to the Platoon treatment and control groups. The treatment assignment was also extended to Boards. That is, all participants in a given Platoon have the same treatment assignment, and all participants in the treatment Platoons also were assigned treatment Boards.[4]

Table 1 shows the distribution of startups intro treatment and control groups across cohorts (row 7). The assignment is roughly 1:1 (except in the cases whether the total number of participants is not an even number). The number of Platoons per executive varies across cohorts: 3 for the first two cohorts, and 2 for the rest. Note that Platoons have different sizes in each cohort, and the average size varies across cohorts too (row 6). However, the number of Platoons and size of Platoons across treatment groups is virtually identical in every cohort (rows 4 and 6). For cohorts 17 and 18 there is an imbalance in the number of Platoons by treatment assignment

[4] The only exception to this assignment rule occurred during cohort 18 where the assignment to treatment was done at the Platoon rather than at the individual level. This occurred because unbeknownst to the principal investigators, Start-Up Chile assigned participants jointly to executives and Platoons at entry. Therefore, when we implemented the random assignment we had to randomize the entire Platoon, rather than the individual participants as we did in the other cohorts. As a robustness check, we show in the Appendix that results are similar if we drop cohort 18 from the regressions.

per executive: some executives had 2 treatment and 1 control groups, whereas some had 2 treatment and 1 control group. The executives in both cohorts are the same individuals, so we made sure to flip the imbalance from one cohort to the next.

4.2. Randomization checks

We use the application data to provide evidence that the process resulted in treatment and control groups that are comparable in terms of their initial characteristics. Table 4 shows that the groups are balanced in terms of: lead founder education, gender and age, firm location, and capital raised at application. But, some differences remain in spite of the randomization: the treatment group has a slightly lower level of: initial sales, initial number of employees and application scores. Table 4 reports differences in characteristics at application across groups controlling by strata (we include cohort cross executive fixed effects and industry fixed effects—as we explain in more detail in Section 6 below; results are quantitatively similar if we use simple t-tests that do not control by strata). The coefficients in the table are based on log transformations of the variables. Relative to the unconditional means, they imply that treated firms have on average $6.5K less initial sales and 0.5 fewer initial employees.

There are several explanations for these small, yet statistically significant differences. First, the randomization was blind to founder and firm characteristics: we had no access to the application data at the moment of the randomization. Second, the table pools results from several individually randomized cohorts. In unreported analysis, we show that the differences are driven by cohort 19.

In the main analysis, we show that the differences in initial sales, employees and scores appear immaterial: results are robust to the inclusion of these variables as controls in the

regression. In addition, we show in robustness checks that results remain quantitatively similar if we exclude cohort 19 from the sample (see Section 7.3).

4.3. The structured accountability intervention

We build our experiment on the customized feedback structure provided by Start-Up Chile. Both groups were offered the Platoons and Board meetings, and were blinded to treatment status. The key difference was that treated lead founders (those subject to structured accountability) were asked to articulate the strategic tasks to be completed during the following weeks until the next platoon (board) meeting, and report about their progress since the last platoon (board) meeting.

In detail, during each Platoon meeting, executives asked treated founders two specific questions: "how was your progress on the committed tasks since our last meeting?" and "what would you say are the key strategic tasks you need to work on until our next meeting?" During Board meetings, these questions were asked by the board members as requested by Start-Up Chile. The only role program executives played in this case was to provide board members with the founder's list of previously committed tasks. Treatment participants are expected to think about these questions freely, and provide answers based on their own understanding of what "good progress" (or the "best strategy") actually means. Executives (board members) did not ask these questions to the Platoon (Board) control group. This means that while both control and treatment groups have the chance to get customized feedback from the program peers and the advisory board, only the treated entrepreneurs were asked these two specific questions in their meetings.

There were several reasons for offering the control group the standardized meetings rather than not providing any meetings at all. The first was that, from Start-Up Chile's point of view,

the regular meetings provide a control mechanism to oversee the proper use of the grants by participants, and therefore not meeting control participants was out of the question.

The second is that from an evaluation standpoint, offering both groups the standardized meetings lowers the risk of Hawthorne and John Henry effects, since both groups were told they were provided the same service in the Platoon and Board meetings. By having similar regular meetings that had a subtle but important difference (the questions and discussion about the progress of the committed tasks), we minimized the risk of treatment spillover. That is, control group entrepreneurs also got to meet with platoon moderators and board members on a periodic basis. Moreover, the meetings served to further reduce spillovers because, in the eyes of participants, potentially observed differences between control and treatment groups would most likely be attributed to the uniqueness of every platoon moderator or board members.

The third there is growing theoretical interest in how specific design features of accelerators affect new venture performance (Hallen *et al.*, 2020), and how to strike the right balance between customized interventions (McKenzie, 2020) and program standardization (Cohen *et al.*, 2019a). We provide a unique contribution to the literature by focusing on structured accountability as a specific feature that may improve (or hinder) new venture performance. Finally, we also believe that the provision of structured accountability within the context of a meeting should be more powerful than on its own (e.g., by simply filling out a form), due to the way the intervention should induce preemptive self-criticism in anticipation of opinions by others (Hoorens, 1993; Leary and Kowalski, 1990), and social pressure to execute the declared committed tasks (Thaler, 2000). While someone may argue that the Platoon and Board meetings naturally induce some sense of accountability on participants, if it were to do so, it should be ever so slight because the control group meetings are strictly about feedback and not

about defining tasks nor following up on them. Regardless, our specific structured accountability intervention must be considered within the context of a business accelerator that has the standardized feature of monthly meetings. Throughout, we are careful to interpret results with this caveat.

4.4. Mechanisms to ensure proper implementation of the experiment

Proper implementation of the structured accountability experiment depends on two main success conditions. First, participation of treatment and control participants in the Platoon and Board meetings. Second, correct implementation of the structured accountability treatment (and absence of it) in the treatment (and control) groups, conditional on meeting participation.

The conditioning of grants' disbursement to meetings' participation sets strong compliance incentives for the first condition. Consistent with this intuition, Table 1 shows that participation compliance is very high, and is no different between treatment and control groups, overall, and for the sub-samples of firms with high- and low-education lead founders.

Table 1 Panel A (row 10) shows that average participation rates almost always match the mandatory number of meetings per cohort. Attendance to all meetings was mandatory in cohort 17 and Table 1 shows virtually perfect compliance for that batch (row 10). Starting from cohort 18, lead founders were allowed to skip one meeting, and consequently the average difference between number of attended meetings relative to planned meetings (6) is one. Starting on cohort 19, lead founders could skip two meetings—one for each type of meeting (recall Boards are introduced in cohort 19); thus, the difference between planned and attended meetings in that cohort is two. The only cohort where participation compliance is not perfect is cohort 21: the difference between planned and attended meetings is on average three, rather than two. Conversations with the program staff revealed that most executives in that cohort cancelled the

meeting immediately prior the pitch-day, to allow participants further preparation time for the event. Importantly, Panel A (row 11) also shows that participation rates are virtually the same for treated and control participants across all cohorts (average participation rates of the two groups are very close in value and are not statistically significant). This is true even for cohort 21 where overall participation compliance was not perfect.

Panel B of Table 1, shows no differences in participation between firms in the subsamples with high and low education lead founders. No differences are visible either between treatment and control firms for both the high and low education sub-samples. We note that all participants, regardless of participation compliance, are included in the sample to avoid participation bias.

In order to ensure the compliance of the second condition—correct implementation of the structured accountability treatment (conditional on participation in the meeting), we used several diverse mechanisms that we now describe in detail.

First, we ran a Pilot intervention on Start-Up Chile's 16th cohort to align the expectations between the research team and Start-Up Chile's staff. The pilot lasted 6 months between August 2016 and January 2017. In the trial, we assigned two Platoons to the treatment condition and used two as the control. Based on the Pilot's experience, we initiated several protocols to ensure proper implementation that we used throughout the experiment: including, separate training sessions with the program staff (the executives) and the board members to explain the implementation design. In addition, we developed an *Operations Manual* to be used as guidance by the staff and that included answers to frequently asked questions.

Second, we hired an experiment implementation team of local assistants to implement and oversee the execution of the experiment. The team included a local coordinator that ran the training sessions with executives and board members. The team also included more junior

assistants, overseen by the coordinator, who worked as scribes in the Platoon and Board meetings and kept track of the implementation of the experiment. The scribes were present at each one of the meetings, taking notes for both control and treatment groups, and registering and reporting whether or not the treatment (control) group entrepreneurs were (not) asked the key questions. In addition, scribes took detailed notes of the articulated tasks, which were revised and discussed during the next meeting. The control group participants did not have any kind of explicit tracking of commitments that were spontaneously brought up by entrepreneurs during the meetings.

We made sure that scribes were independent from Start-Up Chile: they are students of the Pontificia Universidad Católica de Chile, selected and hired by the research team. We screened scribes based on their ability to understand different English accents (we tested their note-taking abilities from watching videos with pitches by entrepreneurs from different backgrounds, such as Scotland and India) in order to ensure adequate notetaking; the need to do this was made clear during the Pilot. Furthermore, they were trained for the job ahead of each cohort and required to sign a confidentiality agreement to safeguard the privacy of the entrepreneurs and avoid disclosure about the experiment.

Third, we designed and measured a series of key performance indicators (KPIs), which we revised on a regular basis. Scribes kept track of these KPIs, which in addition to correctly implemented meetings, also include: the timely execution of the meetings, the meeting duration, days since the last meeting, time spent on discussing the questions, number of participants at the meeting (for platoons), number of committed tasks (including spontaneous tasks provided by control-group entrepreneurs), number of failed and completed tasks committed during the

previous meeting, a qualitative description of the tasks, and, if necessary, a description of incidents.

Table 1 summarizes the average KPIs for each cohort. Panel A shows that errors in implementation were uncommon (row 11): virtually all meetings held were correctly implemented. The same panel also shows that the few implementation errors are equally distributed across treatment and control group.[5] Panels B and C break down the comparison of KPIs for the subsample of firms with high and low education lead founders, respectively. These panels show no heterogeneity in implementation: the few implementation errors do not concentrate in any given sub-sample.

Panel A in Table 1 summarizes the other KPIs for the whole sample. For reference, we note that the average meeting duration is not substantially different across treatment and control groups (row 12). Treatment and control meetings lasted 85 and 82 minutes on average. In treatment meetings, the average time allocated to reviewing past tasks was 7.28 minutes, and the time allocated to enunciate future tasks was 3.52. Consistent with the correct implementation statistics, the number of committed tasks are higher for treatment than control firms for every cohort, except cohort 21, where the difference between the two cohorts is not statistically significant. We note that for control ventures the number of committed tasks in the table corresponds to the spontaneous tasks enunciated by participants without solicitation form the executives or board advisors. In unreported analysis, we show that the null difference in committed tasks for cohort 21 is not driven by implementation mistakes. A possible explanation

[5] By "implementation errors" we mean that a control group was given the treatment and viceversa.

is cultural shifts inside the program: by the fifth cohort of the experiment, the structured accountability culture may have become so ingrained among participants that the conversations in Platoons and Boards lead founders to self-commit to tasks even when not explicitly prompted.

The fourth mechanism to ensure the proper implementation of the experiment, was taking corrective actions in the few instances that control group entrepreneurs did receive the treatment (because the platoon leader mistakenly asked the treatment question to the control group). These instances were reported by the scribes to the principal investigator, who took swift corrective action (such as talk with the platoon moderator or the program director to avoid future mistakes). We note that whenever control group entrepreneurs spontaneously talked about strategic tasks they planned to execute in the future (un-primed by the aforementioned questions), platoon staff moderators and board members did not impede the conversation. This was important in order to avoid creating an artificial context for the control group. Thus, staff moderators and board members did not encourage (nor discourage) this type of conversation, committed tasks were not brought forth during the subsequent meetings, and scribes registered the event and commitments for analytical purposes.

Finally, we conducted novel analysis to corroborate the correct implementation of the structured accountability treatment by testing the perceptions of participants. Starting on cohort 18, towards the end of venture's tenure in the program, we asked all participants a series of questions meant to elicit their perceptions and experiences regarding their participation in both *Platoons* and *Boards*. The questions and answers are summarized in Table 2.

Table 2 provides compelling evidence that the mechanisms to ensure proper implementation were successful. Panel A shows evidence that treated participants perceive

greater structured accountability than control participants. By contrast, treated and control participants report similar perceptions in terms of feedback.

In more detail, the table summarizes the answers to the questions we asked participants with the objective to elicit perceptions about structured accountability as well as feedback.[6] The table shows that participants in the treated group were more likely to perceive being asked by their executive to enunciate some goals or relevant tasks during the Platoon sessions relative to those in the control group (row 5). Similarly, participants perceived that executives in the treated group were more likely to follow-up about goals participants had committed to work on previous Platoon sessions relative to executives in the control group (row 6). We did not include a question in the implementation check asking participants to comment on whether the board members asked them to enunciate goals or followed-up on them, so we cannot report on the potential differences in the structured accountability perceptions of participants during Board meetings.

It is interesting to note that control group participants perceived a request to enunciate goals and follow-up on goals (4.3 and 4.2 on a 1-5 Likert scale), despite the fact that control-group executives did no such thing, and even though the treatment group perceived a significantly higher structured accountability than the control group. We also note that the participants in the treated group also felt more committed to accomplish such goals than those in the control group (row 9), but there were no significant differences across treatment groups

[6] The response rate of the implementation check was 62%. In unreported analysis we show that participants that responded the questions are no different from those that didn't.

regarding their perceived competence to meet these goals. In contrast to the differential perception about the perceived structured accountability, the table shows no evidence that treated and control participants perceived different feedback.

Panels B and C repeat the analysis splitting the sample into the subsample of firms with high-education lead founders (Panel B) and with low-education lead founders (Panel C). The results in these panels provide compelling evidence that the mechanisms to ensure proper implementation in both were equally successful in both sub-samples. The panels show no differences between treatment and control firms in the number of correctly implemented meetings. Also consistent with the correct implementation of the experiment, treatment firms have more committed tasks than control firms in both sub-samples.

5. OUTCOME VARIABLES

Collecting performance measures for all participants to the accelerator is challenging. The vast majority of participants are not registered in standard (local or foreign) business data sources. Moreover, the program collects participant data only irregularly. Therefore, we use three strategies to address this challenge. First, similar to prior research, we hand-collected web-based performance measures for all participants (Goldfarb, Kirsch, and Miller, 2007; Gonzalez-Uribe and Leatherbee, 2018; Hallen *et al.*, 2020; Kerr, Lerner, and Schoar, 2014). Second, we relied on surveys that we co-designed and co-implemented with program staff. Greater details about this data-collection strategy and the definitions of each outcome variable can be found in the Appendix. Finally, we conducted in-depth interviews with a sub-sample of participants. All outcomes are measured within 2.75 years since application to the accelerator.

5.1. Quantitative measures of venture performance

For our web-based measures, we searched through the Crunchbase and LinkedIn platforms during the Summer of 2020. Because participants in cohorts 17-21 applied to the program between February 2017 to February 2019, these metrics represent new venture performance outcomes between 0.75 and 2.75 years since application into the program.

We also conducted surveys in October 2019 (cohort 17) and November (cohorts 18-20) of 2019, and January (cohort 21) of 2020 (see the Appendix for a detailed explanation). In each survey, we asked participants about their performance after graduation in six months intervals. For example, for cohort 17 participants that were surveyed in October 2019, we asked about their performance by 0.5, 1, 1.5 and 2 years after graduating in August 2017. Depending on the cohort, we have performance data for a varying number of semesters after graduation from the program (1 semester for cohort 21, and 4 semesters for cohort 17).

To maximize the survey response rate. We deployed a wave-based approach. The first wave sent a generic email inviting participants to respond the survey. The second wave used individualized emails from the Start-Up Chile staff that developed a close relation with participants, inviting them to respond the survey. The final wave was a phone call from the program's staff. This intensive approach allowed us to achieve relatively high average response rates: 73% overall and, respectively, 76.1% and 71.4% for the subsample of firms with high and low education lead founders (for disaggregated information on response rates by cohort see the Appendix). In the appendix we show negligible differences between survey respondents and other participants. The appendix also shows that response rates do not differ between the sample of low and high education lead founders. In the appendix, we report positive and statistically significant correlations across our web-based and survey-based proxies for venture performance.

For each data source, we constructed five new venture performance proxies: the natural logarithm of the number of *Employees*; *Capital Raised* measured as a binary variable for securing capital after participation in the accelerator; the natural logarithm of the *Amount Raised* after participation and excluding the seed capital provided by the program; *Market Traction* measured as the natural logarithm of the sales (or LinkedIn Followers-results) during the six preceding months; and a binary variable to indicate *Survival*. We used logarithmic transformations of continuous outcome variables to mitigate the potential impact of outliers; we add one to the variable before the transformation and in unreported analysis show that results are robust to adding the minimum (non-zero) value of the respective variable.

Table 3 presents the summary statistics of the five web-based outcome measures. Within 2.75 years of application to the program, the average participant is 16% likely to secure specialized financing, raises 86K dollars in capital, has 10.7 employees, has an average market traction of 537.7, and is 80% likely to survive. Table 5 also presents summary statistics of the survey-based performance metrics. Six months after graduation, the average participant-survey respondent raises 27K dollars in capital, has 5.8 employees, an average market traction of 7.68 dollars in sales.

5.2. Interviews

We complement our quantitative results by conducting in-depth, semi-structured interviews with 22 participants of generation 19. We randomly selected 15 entrepreneurs from the treatment group and 15 from the control group. Four selected participants from each group declined our invitation to participate in the interviews, leaving us with 11 interviews for each group. Seventeen interviews were conducted in English and five in Spanish.

One of the research team members conducted all 22 interviews, unbeknownst to the assignment to treatment of each interviewee. Interviews were roughly one hour long, and a conversation guide was used to ensure that all issues of interest were covered while maintaining flexibility to tailor questions depending on the interviewees' responses. The interviews included general questions about the experience in the program, as well as the experience in the platoon and board meetings. As the interview progressed, the interviewer made more specific questions about the effectiveness of the meetings and gathered the interviewee's opinion about the content and structure at the meetings. In addition, the interviewer inquired about the entrepreneur's feelings regarding the sharing of experiences and the listening to the experiences of others, as well as receiving positive or negative feedback. The interviewer also asked entrepreneurs about the usefulness of the meetings in terms of how these meetings shaped the outcome of their ventures. Because the interviews were semi-structured, not all questions were explicitly articulated, particularly if the interviewee addressed the particular question on their own volition. Moreover, interviewees were allowed to discuss other aspects that may not have been capturable by the predefined questionnaire, thus providing potentially valuable insights that the research team may not have considered.

All interviews were recorded and transcribed by experts external to the research team. The data was analyzed using a four-stage coding technique employing MAXQDA 12 software. Firstly, we generated codes organically as specific concepts surfaced from reading the transcripts. Secondly, we classified these codes into eight broad topics: criticisms of the board meetings, approvals of the boards, criticisms of platoons, approvals of platoons, opinions about Start-Up Chile, general sentiments, differences between platoon and boards, and experience. The third stage consisted of teasing apart categories within the broad topics, in the cases where such a

separation was justified. Finally, we iteratively analyzed the recurrence of the codes and merged codes whenever increases in the robustness of the categorization ensued, after which we counted the frequency with which the code emerged in the transcripts.

Only after members of the research team were comfortable with the codification and categorization of the interviews, was the sample separated between treatment and control groups. We then compared the frequency between the experimental groups for each category to identify contrasting patterns between both groups. We discuss the findings in the results section.

6. EMPIRICAL STRATEGY

In this section we discuss the empirical strategy we use to measure the effect of the joint provision of structured accountability and customized feedback, relative to customized feedback alone, on venture outcomes. We start by describing the strategy to estimate the average effect of randomly offering the structured accountability intervention to participants that receive feedback. We then explore the heterogeneity of results across ventures with high and low education lead founders.

6.1. Average effects of treatment assignment

To estimate the average impact of structured accountability and customized feedback relative to customized feedback on its own on firm outcomes, we use the following base specification:

(1) $y_i = \alpha + \beta Treat_i + CohortFE \times Executive\ FE + IndustryFE + Controls_i + \varepsilon_i$,

where the sub-index *i* denotes firm *i*, and the variable $Treat_i$ equals one if the firm receives the structured accountability treatment. We include cohort fixed effects interacted with executive fixed effects to reflect the level at which the treatment was randomized among firms. We also include industry fixed effects to control for any residual variation across industries. These controls are important because the initial executive assignment was industry-based (see Section

4). In some specifications, we add controls for initial sales (in logs), initial employees (in logs), and score to absorb the unintended differences in baseline despite the randomization (see Section 4). We use robust standard errors throughout.

The coefficient of interest is β that measures the impact of being assigned to the treatment group and being offered the possibility to participate in the structured accountability meetings rather than the control meetings. That is, β measures the Intention-to-Treat—ITT (Angrist and Pischke, 2008). Because we include cohort fixed effects interacted with executive fixed effects, β is estimated as the average differences in outcomes between treated and control groups for a given executive in a given cohort. Therefore, the estimate controls for any potential differences across executives in specific cohorts; for example, in the tone, applicability and constructiveness of their feedback, and only captures the additional effect of the provision of structured accountability by the executive. Likewise, the estimate also controls for any differences across cohorts, for example in quality, and the effects of those differences in executives' role, for example, the possibility that executives may provide different feedback with different intensity in cohorts of different average quality.

We implement several robustness checks as we explain in detail in Section 7.3. We show that results are robust to a number of changes in the specification of equation (1) or changes in the estimation sample.

6.2. Heterogeneity across high and low education lead founders

We explore the heterogeneity of results across firms with high-education and low-education team leaders. As explained in Section 4, cutting the sample in this dimension is interesting because high and low education founders are expected to differ in their levels of self-accountability: Masters have higher degrees of autonomy and self-initiative.

We estimate the potential differential impact of offering structured accountability and customized feedback, relative to customized feedback alone, on high-education and low-education firm outcomes using the following specification:

(2) $y_i = \alpha + \beta_{Low} Treat_i + \alpha_{High} High_i + \beta_{High-Low} High_i \times Treat_i +$

$\times CohortFE \times Executive\ FE \times High_i + IndustryFE \times High_i + Controls_i + \varepsilon_i,$

where the variable $High_i$ equals one if the firm has a high-education team leader. We include interactions of the fixed effects in equation (1) with the variable $High_i$ to control for education differences among entrepreneurs assigned to different executives in a given cohort. Note that such interactions absorb the level effect of $High_i$, which is why the results reported in Table 5 do not report this variable.

We also interact the industry fixed effects with the variable $High_i$ to control for differences in founder education across industries. The variables in $Controls_i$ vary across specifications, but across all models we control for lead founder age. This way the regression captures differences across high and low education lead founders, after absorbing potential differences in age across the groups. In some specifications, we add equation (1)'s basic controls for initial sales (in logs), initial employees (in logs), and score to absorb the unintended baseline differences between treatment and control units despite the randomization. In other specifications, we interact the basic controls with the variable $High_i$ to allow for differences in baseline controls across different education levels, although results in Table 4 show no such differences exist in sales, employees, or scores. Finally, in other specifications, we include controls for the initial stage interacting with the education dummy to absorb the only difference in baseline characteristics for treatment relative to control firms of different founder education (see Table 4).

The coefficients of interest are β_{Low} that measures the average effect of offering the possibility to participate in the structured accountability meetings for firms with low-education founders, and $\beta_{High-Low}$ that measures the difference in treatment assignment between firms with high and low education founders. We also report in Table 5 (and Table 4) the estimate (and p-value) of the assignment to treatment for firms with high-education founders, estimated as $\beta_{High} = \beta_{Low} + \beta_{High-Low}$.

Similar to the base regressions, we implement several robustness checks as we explain in detail in Section 7.3. We show that results are robust to a number of changes in the specification of equation (2) or changes in the estimation sample.

7. RESULTS

In this section we discuss the results from implementing the empirical strategy described in Section 4. First, we show that the results point to no average meaningful differences in outcomes across ventures randomly assigned to treatment and control groups. Then, we show that such average null effects mask substantial heterogeneity of impact across firms with high and low education lead founders. The intervention has remarkable positive effects on firms with low-education founders, but little (and sometimes negative) effects on firms with high-education lead founders. At the end of the section we also discuss results from the qualitative analysis using the in-depth interviews.

7.1. Quantitative analysis: Results

Table 5 summarizes results based on the internet based outcome variables. We note that we report p-values in parenthesis, and omit using stars in the tables to signal statistical significance. Panel A presents the results from estimating equation (1). Panel B presents results from estimating equation (2). For each variable, we report results from the various specifications

varying the control variables as specified in the last columns of each row. To conserve space, we only report the estimate for the variables of interest, treatment in Panel A and treatment for low and high education and their difference in Panel B. But, the estimates on control variables for all the different models considered are reported in the Appendix.

The results in Panel A show no meaningful average differences in performance across treatment and control firms. We cannot reject the hypothesis of no effects across the different specifications, and for all outcome variables. Results are similar if we use survey based variables, as reported in the Appendix.

The pattern of results in Panel B show that the lack of results in Panel A mask a large heterogeneity. We find consistent evidence of positive effects on treated firms with low education founders (Column 1), and typically negative, yet not statistically significant effects for high education founders (Column 2). Moreover, column 3 shows that the estimated effects for low and high education lead founders are statistically significantly different to each other. The only exception are the results for survival: while they are only statistically significant for the low education founders, which cannot reject the null that the results are the same across with high and low education lead founders. Because we control for the age of the lead founder in all specifications, these patterns capture the differential effect of the assignment to treatment for different education levels, once we control for differences in age across groups.

The results in Panel B provide compelling evidence that assignment to treatment has positive and significant venture performance effects for firms with high education lead founders. This pattern of heterogeneity of results is robust across all outcome variables and specifications considered in the panel. The only exception is the most saturated model (last row) for the variable capital raised: the estimate for the effect on low education founders is positive and

almost statistically significant (with a p value of 0.106). This pattern of heterogeneity is also visible in Table 6 that summarizes results using the survey variables. To ease comparison, Table 6 only summarizes results of the most saturated model (last row for any variable in Table 5). Across the different survey based outcome variables, the point estimate for the effects on firms with low (high) education lead founders are positive negative, although they are never statistically significant. However, for many variables the estimates in column 3 show that the difference in the effects for firms with high and low education lead founders are statistically significant.

7.2. Quantitative analysis: Magnitudes

In terms of magnitudes, the estimated effects of assignment to treatment on the firms with low-education founders are sizable. The results in Panel B in Table 5 roughly show that assignment to treatment increases, within 2.25 years of participation, the number of employees by 40%, the probability of raising capital by 10%, the amount raised by 32%, the market traction by 33% and survival by 14%. Relative to the unconditional averages, these estimates imply an increase of 4 employees, 1 percentage points in the probability of raising capital (from 16% to 17%) and $27K in the amount raised.

The results in Tables 6 and 7 show that the economic magnitude of the assignment to treatment effect for firms with low education lead founders is similar across different proxies of a given outcome. To facilitate comparisons, Tables 6 and 7 present results based on standardized variables. For each variable the results in Table 6 are most directly comparable to the results of the last specification in Table 7.

The coefficients in column 1 of the first four rows of Table 6 and the fourth row in Table 7 show that assignment to treatment increases the number of employees by 0.3 (average across

surveys) and 0.46 standard deviations, respectively, as measured by the survey and internet based proxies. In terms of capital raised, the assignment to treatment increases the amount of capital according to survey proxies and internet proxies by roughly similar amounts: 0.30 (average across surveys) and 0.28 standard deviations, respectively. Finally, the effects on market traction increases by 0.30 (average across surveys) and 0.36 standard deviations, respectively, for survey and internet based proxies.

The implied magnitude of the findings is similar to prior work on Start-Up Chile (Gonzalez-Uribe and Leatherbee, 2018), as well as prior papers on business training interventions. For example, Calderon, Cunha and de Giorgi (2020), and de Mel et al. (2014) find a 20% and 41% increase in sales (within 12 and 8 months), which are similar to our estimate of a 33% increase in web-based market traction (and are within the confidence interval of our survey-based results; see the appendix). Our estimates on employees are close to those of Glaub et al. (2014), who estimate that treated firms have roughly twice as many workers as control firms after five to seven months of a three-day training intervention. Such estimates are within the standard error bands of both our estimates on employment (roughly 40%; see Panel A of Table 5). Finally, similar to our own noisy estimates for survival, most studies also find positive but insignificant impacts (see McKenzie and Woodruff, 2013).

7.3. Quantitative analysis: Additional robustness checks

We present a battery of robustness checks in the Appendix. First, we show that results are robust to excluding industry fixed effects from the regressions. Second we show that results are robust to excluding cohort 19, for which the average differences across sales, employees and scores at application between treatment and control groups is the most pronounced. Finally, we also show

that results are robust to excluding cohort 18 for which the randomization was incorrectly implemented.

7.4. Quantitative analysis: Discussion

Overall the results from the quantitative analysis point to strong positive effects on performance for low-education founders from the joint provision of structured accountability and customized feedback, relative to the provision of customized feedback on its own. The evidence for high-education lead founders is less conclusive: the estimated effects of treatment assignment are generally negative, but are not statistically significant, although they are statistically significantly different to the estimated effects on the low-education lead founders.

This heterogeneity in the results is consistent with the idea that low-education lead founders are less self-accountable, and therefore, can benefit more from the customized feedback if it is provided in combination with structured accountability. Instead, for high-education founders that already possess certain degree of self-accountability the joint provision of customized feedback with structured accountability has no added benefit relative to the provision of customized feedback on its own, and in fact can be detrimental for performance.

We can reject alternative and more mechanical explanations of these results. The contrasting results across lead founders with high and low education are not explained by differences in age between the groups, as we control for age across the different specifications. They are also not explained by differences in participation of the meetings. As shown in Table 1, participation is similar across the two groups. They are also not explained by differences in the proper implementation of the experiment across high and low education founders: the errors in implementation are negligible and no different between the two groups.

As limitations, we note that we cannot rule out that the lack of significantly negative effects for high education founders is driven by power issues. We have fewer firms with high-education lead founders than with low-education founders (see Table 1). We note too that the effects of structured accountability cannot be extrapolated outside our research setting of customized feedback provision. We cannot make any conclusions with respect to the potential effects of structured accountability on its own. Our experiment is geared towards understanding the effects that structured accountability can have on the effectiveness of customized feedback, and cannot be extrapolated outside the setting.

7.5. Qualitative analysis: Results and discussion

Results from the analysis of the 22 in-depth semi-structured one-hour interviews with participants from generation 19 are consistent with the results from the quantitative analysis, and provide additional insights that are theoretically and practically interesting. Table 8 lists the patterns that emerged from the interviews, as described in Section 5.2. Six topics clearly stand out: positive and negative aspects of platoon and board meetings, comparison between both types of meetings, and general sentiments towards the business accelerator. While other concepts did also emerge from the interviews, we only conserved codes with a frequency of 5 or higher.

The second colum shows the relative statement frequencies between treatment and control groups. Positive (negative) percentages indicate that treatment (control) participants expressed listed aspects more than control (treatment) participants. The greater the absolute magnitude of the values, the greater the relative difference between groups.

Results suggest that treated participants perceived the existence of structured accountability more than control participants. This is consistent with our experiment

implementation tests in Section 4.4. For example, relative to control participants, treated participants spontaneously perceived the existence of structured accountability (row 2).

Moreover, treated participants had more positive opinions as well as more constructive criticisms about platoons and board meetings. In contrast, control group participants systematically expressed negative criticisms. For example, treated participants valued the support of the board members, and appreciated the structured accountability. However, they were critical about the organization (or lack thereof), about the value of the feedback, and the lack of domain expertise. In contrast, control group participants felt strong negative emotions about the board meetings, such as frustration, anxiety and lack of sincerity from the board.

Regarding the platoons, treated participants appear to appreciate the opportunities to interact and learn from their peers, generate new business leads, friendship and emotional support. However, they were also more critical regarding the organization, lack of clear objectives and heterogenous expertise of their peers. In contrast, control group participants appear to be very critical about the value of the platoon meetings.

Finally, treatment group participants had strong opinions about the favorable support that Start-Up Chile provided regarding the efforts to expand their ventures internationally, but felt that the program required too many obligations – likely due to the accountability structure. In contrast, the control group had a strong negative opinion about the high levels of bureaucracy.

Overall, regardless of education level, participants appear to appreciate structured accountability, want more of it, and have more positive sentiments towards the accelerator. Now we turn to differences between high- and low-education founders.

When comparing contrasting opinions between high- and low-education founders relative to structured accountability (rows marked with *), a consistent pattern emerges: high-education

founders appreciate structured accountability and want more of it, while low-education founders appreciate it less and want less of it. For example, treated high-education founders appreciate structured accountability, while treatment and control low-education founders do not differ much (row 2). Also, treated high-education founders expected more from the boards (row 3), despite the fact that they were receiving greater structure relative to control high-education founders, and expected much more accountability (row 5). Moreover, structured accountability in the platoons appears to be more appreciated by high-education founders relative to their low-education peers (row 7), and had fewer negative opinions about the organization of platoons (row 8). Furthermore, treated high-education founders did not perceive the structured accountability to be onerous, while their low-education peers appear to find the structured accountability an unwanted obligation (row 11).

Surprisingly, our quantitative and qualitative results suggest that while structured accountability improves the performance of ventures led by low-education founders and is detrimental (or at least useless) to high-education founders, the former want less structured accountability while the latter want more of it. This is an important insight, because it suggest that founders are unaware of (or even incorrect) about what sort of oversight structures are beneficial for improving the performance of their new ventures. They want more of something they should have less of.

8. DISCUSSION

Improving new venture success is key for job and wealth creation in societies (Haltiwanger *et al.*, 2013). Research has found that organizational sponsors, such as business accelerators, can play an important role in this success by inoculating new ventures from the liability of newness (Stinchcombe, 1965) through the certification and capacity-building (Armanios *et al.*, 2017;

Gonzalez-Uribe and Reyes, 2021) provided by entrepreneurship schooling (Gonzalez-Uribe and Leatherbee, 2018). Because business accelerators have become a predominant organizational sponsor for new ventures (Cohen and Hochberg, 2014) with more than 3,000 accelerators worldwide, it is important to generate evidence-based insights to improve theory-building and practitioner effectiveness.

An emerging stream of literature has provided important insights about how specific business accelerator features can help to mitigate bounded rationality (Cohen *et al.*, 2019a), reduce uncertainty about the viability of business ideas (Yu, 2020), inspire venture teams to come up with new business ideas (Leatherbee and Katila, 2020) and improve venture performance through consultation practices (Hallen *et al.*, 2020) and visibility (Gonzalez-Uribe and Reyes, 2021). Using a randomized controlled trial of 361 new ventures in a business accelerator, we contribute to this literature stream by providing the first causal evidence about the effects of structured accountability on new venture performance. Our findings provide new insights and tools for organizational sponsors (e.g., policy-makers) as well as for entrepreneurs and new venture boards.

Public agency and fiduciary responsibility

Because new ventures are important for job and wealth creation, yet they face important market frictions to succeed, it is natural that public policy-makers design programs that use tax-payer money to reduce such frictions. In doing so, there are two potentially conflicting forces, particulary with public programs that provide funding to new ventures based on an application process that relies on startups articulating, ex-ante, how they plan to use the funds. On one hand, constituyents expect program managers to ensure that funds disbursed to new ventures be used by startups according to the plan defined in the application process. Thus, as agents to

constituyents, public-program managers have a strong incentive to oversee the progress of the startups so that compliance with the committed application plan is effectively executed. Implicit in this expectation is the mechanism of structured accountability, which we find actually helps one type of new ventures. On the other hand, constituyents expect the highest possible socioeconomic return of the public funds, and therefore, program managers are expected to implement policies that maximize such returns. We find that, for a different type of new ventures, this fidicuary responsibility would be better executed if program managers had a more hands-off approach (i.e., less structured accountability).

Our study provides important insights about the effectiveness and boundary conditions of specific features of organizational sponsors. Specifically, we find that the implementation of structured accountability can significantly improve new venture performance for founders who have relatively lower levels of education (a proxy for less self-accountability, less critical thinking and greater procrastination), while it can be detrimental (or at the least useless) for founders with higher levels of education. Because business accelerators are an important type of organizational sponsor that can evidently improve new venture performance, our findings provide an evidence-based tool that can help program managers improve the effectiveness of accelerartors.

Entrepreneurial freedom or voluntary subjection?

An important characteristic of entreperneurs is their desire for independence (Aldridge, 1997; Hisrich, 1984; Hornaday and Aboud, 1971; Shane, Locke, and Collins, 2003). However, as Wasserman (2012) describes, entrepreneurs commonly face a tradeoff between independence and venture performance. Our findings provide important new insights about this tradeoff.

For some types of entrepreneurs (relatively lower-education founders in our setting), subjection to accountability structures improves new venture performance. For other types of entrepreneurs (higher-education founders) freedom from such accountability structures appears to be the best choice. Ironically, our qualitative analysis suggests that founders desire more of that which is detrimental to their goals. Founders that are most likely to benefit from structured accountability appear to shun it, while founders that are least likely to benefit from such subjection appear to desire more of it.

Our findings shed light on a specific type of organizational feature, namely structured accountability, that can be very relevant for founders' goal of greater performance, but that can be at odds with founders' desires. This conundrum begs an important question: what other types of choices are founders irrationally making? We believe this is an interesting avenue for future research.

New venture boards

New venture boards of directors have been posited by prior literature as consequential to the outcome of the companies they supervise (Daily *et al.*, 2002). Venture directors are extensively involved in monitoring (Garg, 2013; Lerner, 1995; Sapienza and Gupta, 1994), and board monitoring and venture performance appear to go hand in hand, particularly in established firms (Westphal, 1999). However, it appears that overseeing new ventures is not a one-size-fits-all approach.

Our findings provide important evidence-based prescriptions for new venture boards. Understanding the boundary conditions and tradeoffs of monitoring new ventures can be consequential for the mission of boards of directors. Knowing when and how to provide structured accountability can benefit both founders and boards.

9. CONCLUSION

Our study provides evidence-based insights for improving new venture performance, which is an important activity for socioeconomic wellbeing. Despite founders' resistance against – or desire for – greater structured accountability, governance structures aimed at supporting new venture performance can be consequential to achieving this goal. We show that the proper use of structured accountability can, albeit counterintuitive to founders, help them create greater value for themselves and society at large.

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TABLES

Table 1: Descriptive statistics of experimental intervention

Panel A: Pooled dataset

Cohort	17	18	19	20	21
Start Date	Feb-17	Jul-17	Mar-18	Jul-18	Feb-19
End Date	Aug-17	Dec-17	Aug-18	Dec-18	Aug-19
Number of executives	4	4	7	7	6
Number of Platoons (treatment/control)	6/6	6/6	7/7	7/7	6/6
Number of Platoons per Executive	3	3	2	2	2
Number of Startups per platoon (treatment/control)	6.8/7	7.1/6.8	5.3/5.6	4.7/4.2	4.5/4.5
Treatment/control startups	41/42	43/41	37/39	33/31	27/27
Low educated treatment/control startups	25/24	28/25	22/27	20/20	16/20
High educated treatment/control startups	16/18	15/16	15/12	13/11	11/7
Number of meetings (treatment/control/total)	7/6.8/7	4.7/4.5/6	5.6/5.3/7	5.9/5.3/8	4.1/4.1/7
Number of correctly implemented meetings (treatment/control)	-	4.6/4.5	4.9/5.2	5.5/5	3.9/3.3
Meeting length in minutes (treatment/control)	-	112/109	75/75	77/74	75/69
Time allocated to reviewing past tasks – Platoons	5.2	8.1	11.1	6.4	5.6
Time allocated to enunciating future tasks – Platoons	4	3.1	4.5	2.7	3.3
Commited Tasks (treatment/control)	2.9/0.3	3.1/0.3	2.9/1.1	2/0.6	2.3/2.6
Commpleted Tasks	1.9	1.8	1.9	1.8	1.2
Failed Tasks	1.1	1.3	1.1	0.9	1.1

Note: Time allocated to discussing tasks and the count of tasks is at the founder level. Cohorts 17 and 18 had only platoons. Boards were implemented starting at cohort 19, replacing half the platoon sessions such that all cohorts had the same number of instances of structured accountability.

Panel B: Descriptive statistics separated by education level

	High Educated					High Educ. Avg. Treated	High Educ. Avg. Control	Diff	p-value
Generation	**17**	**18**	**19**	**20**	**21**				
Number of meetings (treatment/control)	7/7	4.8/4.5	5.3/5	6.2/6.2	3.6/4.1	5.5	5.6	0.1	0.8
Number of correctly implemented meetings (treatment/control)	-	4.7/4.5	5/4.7	6/6	3.7/3.5	5.3	5.5	0.2	0.50
Meeting length in minutes (treatment/control)	-	109/109	72/77	82/76	72/75	86	88	-2	0.65
Time allocated to reviewing past tasks – Platoons	5.6	8.9	11.9	8.1	8.0	8.4	-	-	-
Time allocated to enunciating future tasks – Platoons	4.8	2.4	4.1	2.9	2.1	3.4	-	-	-
Commited Tasks (treatment/control)	1.4/-	3/0.2	2.7/0.7	2.3/0.7	2.1/2.3	2.6	1.6	2	0.00
Commpleted Tasks	1.8	1.8	1.6	1.9	1.0	1.7	-	-	-
Failed Tasks	0.8	1.2	1.1	0.8	1.1	1.0	-	-	-

	Low Educated					Low Educ. Avg. Treated	Low Educ. Avg. Control	Diff	p-value
Generation	**17**	**18**	**19**	**20**	**21**				
Number of meetings (treatment/control)	7/6.7	4.7/4.6	5.7/5.4	5.7/4.8	4.5/4.2	5.6	5.2	0.4	0.04
Number of correctly implemented meetings (treatment/control)	-	4.6/4.5	5.2/5.3	5.3/4.5	3.2/4.1	5.3	5	0.3	0.11
Meeting length in minutes (treatment/control)	-	114/109	76/73	78/70	76/70	89	83	6	0.11
Time allocated to reviewing past tasks – Platoons	5	8.1	10.1	7.9	5.9	7.4	-	-	-
Time allocated to enunciating future tasks – Platoons	3.5	3.6	4	2.8	4.0	3.6	-	-	-
Commited Tasks (treatment/control)	1.7/-	3.2/0.1	3.1/1.2	1.9/0.5	2.3/2.6	2.9	0.9	2	0.00
Commpleted Tasks	2.0	1.8	2.1	1.7	1.2	1.9	-	-	-
Failed Tasks	1.3	1.4	1.2	1.0	1.1	1.2	-	-	-

Note: We used t-tests to compare High and Low Educated means.

Table 2: Experiment Implementation Check

	Treatment Group				
Question	**Treated**	**N**	**Control**	**N**	**p-value**
Attended at least one meeting (Y/N)	.98	203	.99	186	0.21
Number of meetings attended (1–7)	3.94	199	4.1	184	0.31
Received feedback by executive (Y/N)	.95	198	.97	185	0.19
Received feedback by others (mentors or other Start-Ups) (Y/N)	.98	198	.99	181	0.36
Executive asked to enunciate goals (1–5 Likert)* - Platoons	4.7	96	4.3	91	0.00
Executive requested to follow-up on goals (1–5 Likert)* - Platoons	4.6	96	4.2	91	0.01
Commitment with enunciated goals (1–5 Likert)	4.6	199	4.2	185	0.00
Competence in meeting enunciated goals (1–5 Likert)	4.5	199	4.3	185	0.03

* Not available for Gen 17

Note. We used Fisher's exact tests for binary (Y/N) responses; we used t-tests for Likert responses.

	Treated					Control				
Question	**High Educ**	**N**	**Low Educ**	**N**	**p-value**	**High Educ**	**N**	**Low Educ**	**N**	**p-value**
Attended at least one meeting (Y/N)	1.0	92	1.0	111	0.41	1	62	.99	124	0.48
Number of meetings attended (1–7)	3.8	91	4	108	0.49	4.4	62	3.9	122	0.02
Received feedback by executive (Y/N)	0.9	91	1.0	108	0.19	1.0	62	1.0	123	0.71
Received feedback by others (mentors or other Start-Ups) (Y/N)	1.0	91	1.0	107	0.06	1.0	59	1.0	122	0.48
Executive asked to enunciate goals (1–5 Likert)* - Platoon	4.8	45	4.6	51	0.06	4.1	31	4.4	60	0.17
Executive requested to follow-up on goals (1–5 Likert)* - Platoon	4.7	45	4.5	51	0.46	4.1	31	4.2	60	0.79
Commitment with enunciated goals (1–5 Likert)	4.7	91	4.5	108	0.13	4.3	62	4.2	123	0.73
Competence in meeting enunciated goals (1–5 Likert)	4.7	91	4.4	108	0.00	4.4	62	4.2	123	0.18

Table 3: Descriptive stats – media, SD, N (total, treatment, control, high-education, low-education)

	Pooled Sample			Treatment						Control					
				High Education			Low Education			High Education			Low Education		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Baseline															
High Education	.37	.48	361	-	-	-	-	-	-	-	-	-	-	-	-
Female Leader	.22	.42	361	.29	.46	70	.2	.4	111	.19	.39	64	.22	.42	116
Age Team Leader	31.37	6.6.	359	33.58	6.78	68	29.79	5.95	111	33.42	6.84	64	30.46	6.43	116
HQ in Chile	.39	.49	361	.4	.49	70	.41	.49	111	.34	.48	64	.41	.49	116
Chilean Entrepreneur	.31	.47	361	.29	.46	70	.34	.48	111	.27	.45	64	.34	.47	116
LatAm Entrepreneur	.37	.48	361	.3	.46	70	.35	.48	111	.36	.48	64	.43	.5	116
Cap. Raised Before Application	.47	.50	361	.49	.5	70	.43	.5	111	.39	.49	64	.53	.5	116
Initial Sales*	9.52	9.42	347	2.61	11.43	65	1.92	4.96	108	5.79	20.91	62	22.92	164.41	112
Initial Stage	3.09	.80	347	2.97	.88	65	3.14	.68	108	3.13	.8	62	3.07	.87	112
Initial Employees	4.83	3.95	360	4.3	3.32	70	4.41	3.02	111	4.66	4.42	64	5.65	4.66	115
Score	3.56	.42	361	3.56	.42	70	3.5	.43	111	3.62	.44	64	3.58	.41	116
Internet Variables															
Employees	10.68	16.19	361	9.39	16.63	70	9.9	15.01	111	15.88	23.55	64	9.34	10.8	116
Capital Raised	.16	.36	361	.17	.38	70	.14	.34	111	.23	.43	64	.12	.33	116
Amount Raised*	86.88	551.71	360	94.45	457.46	70	37.73	226.59	111	154.02	509.63	63	92.89	794.13	116
Market Traction	537.74	3838.67	361	476.39	1512.78	70	358.89	1728.75	111	1582.72	8653.4	64	169.35	311.04	116
Survival Online	.8	.4	361	.81	.39	70	.8	.4	111	.81	.39	64	.77	.42	116
Survey Variables															
Employees S1	5.78	7.59	260	5.11	6.97	54	5.84	8.35	76	5.92	8.58	48	6.09	6.67	82
Employees S2	9.86	65.9	190	4	2.8	35	20.25	121	56	6.54	9.39	37	5.77	7.48	62
Employees S3	4.66	7.34	146	2.88	2.95	25	3.73	5.31	45	7.45	11.42	29	4.77	7.16	47
Employees S4	3.55	7.25	100	2.37	3.65	19	3.06	7.14	32	6.16	11.73	19	3.17	5.02	30
Amount Raised S1*	27.26	100.09	247	20.48	67.15	51	8.55	25.14	70	42.56	167.49	47	39.12	104.77	79
Amount Raised S2*	51.21	199.83	180	9.16	22.65	32	37.92	207.27	53	148.43	345.55	36	26.65	69.1	59
Amount Raised S3*	42.29	309.31	140	12.42	27.76	22	7.31	22.52	43	185.63	681.35	28	2.87	10.97	47
Amount Raised S4*	97.67	909.73	98	0.78	3.23	17	3.13	17.68	32	497.83	2060	19	0	0	30
Market Traction S1*	7.68	23.24	249	8.66	30.6	52	4.24	9.53	71	10.68	25.29	48	8.33	25.07	78
Market Traction S2*	8.55	22.03	185	4.98	9.02	34	7.33	18.57	54	15.89	35.52	37	7.16	18.55	60
Market Traction S3*	6.85	18.41	142	4.76	11.23	24	8.98	27.49	43	11.83	19.11	29	2.82	5.1	46
Market Traction S4*	9.17	29.11	99	2.49	6.24	18	12.59	42.75	32	14.52	26.36	19	6.14	19.65	30

Notes: * Thousands of USD$

Table 4: Baseline regressions relative to the control group

	(1)			(2)				
Estimate	**Treatment**	**N**	**R^2**	**Treatment Low Education**	**Treatment High Education**	**Difference**	**N**	**R^2**
High Education	0.037	357	0.10	-	-	-	-	
	(0.870)							
Female	0.024	357	0.12	-0.011	0.068	0.079	346	0.24
	(0.623)			(0.878)	(0.43)	(0.478)		
HQ in Chile	-0.006	357	0.20	-0.054	0.043	0.097	346	0.31
	(0.916)			(0.493)	(0.66)	(0.437)		
Chilean Entrepreneur	-0.020	357	0.20	-0.045	0.039	0.084	346	0.30
	(0.691)			(0.558)	(0.65)	(0.465)		
LatAm Entrepreneur	-0.074	357	0.19	-0.036	-0.073	-0.037	346	0.29
	(0.175)			(0.657)	(0.45)	(0.768)		
Age Team Leader	-0.721	355	0.18	-1.708	1.220	2.928	344	0.32
	(0.351)			(0.077)	(0.46)	(0.128)		
Capital Raised before Application	-0.055	357	0.18	-0.115	0.054	0.169	346	0.29
	(0.332)			(0.141)	(0.61)	(0.201)		
Initial Sales	-0.797	343	0.21	-0.664	-1.472	-0.808	331	0.32
	(0.034)			(0.217)	(0.02)	(0.327)		
Initial Stage	-0.012	343	0.20	0.177	-0.398	-0.575	331	0.33
	(0.897)			(0.143)	(0.02)	(0.006)		
Initial Employees	-0.122	356	0.19	-0.149	-0.142	0.007	345	0.30
	(0.031)			(0.067)	(0.11)	(0.954)		
Score	-0.077	357	0.29	-0.034	-0.163	-0.129	346	0.39
	(0.067)			(0.548)	(0.05)	(0.204)		

Note: Robust p-values in parentheses. Model (1) includes fixed effects of Executive interacted with Cohort, and Industry. Model (2) include the same fixed effects interacted with the High Education dummy.

Table 5: Main results

Panel A: Pooled results

				Control Variables	
	Treatment	**N**	**R^2**	**Basic**	**Age Team Leader**
Employees	0.088	343	0.28	Y	N
	(0.499)				
	0.122	341	0.30	Y	Y
	(0.348)				
Capital Raised	-0.006	343	0.17	Y	N
	(0.890)				
	0.001	341	0.17	Y	Y
	(0.983)				
Amount Raised	-0.072	343	0.15	Y	N
	(0.604)				
	-0.040	341	0.16	Y	Y
	(0.766)				
Market Traction	0.119	343	0.30	Y	N
	(0.278)				
	0.137	341	0.30	Y	Y
	(0.218)				
Survival	0.073	343	0.18	Y	N
	(0.139)				
	0.079	341	0.19	Y	Y
	(0.112)				

Robust p-values in parentheses.
Notes: Basic controls include Initial Employees, Initial Sales and Application Score.

Panel B: Results cut by education level

						Control Variables			
	Treatment Low Education	**Treatment High Education**	**Diff.**	**N**	**R^2**	**Basic**	**Basic*Above**	**Initial Stage**	**Age Team Leader**
Employees	0.394	-0.319	-0.713	331	0.38	Y	N	N	N
	(0.013)	(0.18)	(0.012)						
	0.412	-0.356	-0.767	331	0.39	N	Y	N	N
	(0.010)	(0.13)	(0.007)						
	0.398	-0.289	-0.687	331	0.41	N	Y	Y	N
	(0.015)	(0.19)	(0.012)						
	0.461	-0.308	-0.769	329	0.44	N	Y	Y	Y
	(0.007)	(0.17)	(0.007)						
Capital Raised	0.107	-0.164	-0.271	331	0.32	Y	N	N	N
	(0.062)	(0.08)	(0.015)						
	0.103	-0.151	-0.253	331	0.32	N	Y	N	N
	(0.074)	(0.11)	(0.023)						
	0.090	-0.139	-0.229	331	0.33	N	Y	Y	N
	(0.132)	(0.14)	(0.041)						
	0.095	-0.128	-0.223	329	0.38	N	Y	Y	Y
	(0.106)	(0.20)	(0.056)						
Amount Raised	0.359	-0.572	-0.930	331	0.31	Y	N	N	N
	(0.026)	(0.09)	(0.013)						
	0.322	-0.496	-0.818	331	0.33	N	Y	N	N
	(0.041)	(0.14)	(0.029)						
	0.297	-0.450	-0.747	331	0.34	N	Y	Y	N
	(0.074)	(0.18)	(0.047)						
	0.331	-0.439	-0.770	329	0.35	N	Y	Y	Y
	(0.044)	(0.20)	(0.045)						

Market Traction	0.315	-0.367	-0.682	331	0.41	Y	N	N	N
	(0.019)	(0.12)	(0.012)						
	0.333	-0.428	-0.761	331	0.42	N	Y	N	N
	(0.014)	(0.08)	(0.007)						
	0.321	-0.382	-0.703	331	0.44	N	Y	Y	N
	(0.017)	(0.11)	(0.010)						
	0.360	-0.394	-0.754	329	0.45	N	Y	Y	Y
	(0.009)	(0.12)	(0.010)						
Survival	0.131	0.018	-0.114	331	0.31	Y	N	N	N
	(0.044)	(0.81)	(0.243)						
	0.140	0.001	-0.139	331	0.30	N	Y	N	N
	(0.036)	(0.99)	(0.145)						
	0.136	0.018	-0.118	331	0.32	N	Y	Y	N
	(0.040)	(0.78)	(0.211)						
	0.154	-0.002	-0.156	329	0.33	N	Y	Y	Y
	(0.024)	(0.98)	(0.114)						

Robust p-values in parentheses.

Notes: Basic controls include Initial Employees, Initial Sales and Application Score. Basic*Above are the basic controls interacted with the High Educated dummy variable. Initial Stage interact with the High Education dummy, however Age of Team Leader is added as control without interaction.

Table 6: Robustness 1 - Standardized survey variables

	Treatment Low Education	Treatment High Education	Difference	N	R^2
Employees S1	0.227	-0.323	-0.550	235	0.61
	(0.233)	(0.13)	(0.057)		
Employees S2	0.373	-0.575	-0.948	164	0.55
	(0.128)	(0.02)	(0.008)		
Employees S3	0.142	-0.791	-0.933	119	0.60
	(0.581)	(0.00)	(0.011)		
Employees S4	0.494	-0.099	-0.593	69	0.81
	(0.026)	(0.90)	(0.478)		
Amount Raised S4	0.085	-0.265	-0.350	222	0.37
	(0.670)	(0.49)	(0.421)		
Amount Raised S2	0.217	-1.000	-1.217	156	0.40
	(0.349)	(0.01)	(0.008)		
Amount Raised S3	0.384	-0.582	-0.966	115	0.51
	(0.101)	(0.39)	(0.177)		
Amount Raised S4	0.302	-2.779	-3.082	66	0.73
	(0.375)	(0.09)	(0.071)		
Market Traction S1	0.082	-0.357	-0.439	223	0.56
	(0.660)	(0.28)	(0.245)		
Market Traction S2	0.318	-0.366	-0.684	160	0.51
	(0.112)	(0.32)	(0.104)		
Market Traction S3	0.444	-0.797	-1.241	116	0.51
	(0.100)	(0.03)	(0.008)		
Market Traction S4	0.563	-1.027	-1.590	66	0.74
	(0.119)	(0.33)	(0.158)		

Notes: Robust p-values in parentheses. All models include Initial Employees, Initial Sales, Score, Initial Stage (interacted with High Education), and Age Team Leader as control variables.

Table 7: Robustness 2 - Standardized main results

						Control Variables			
	Treatment Low Education	**Treatment High Education**	**Difference**	**N**	**R^2**	**Basic**	**Basic*Above**	**Initial Stage**	**Age Team Leader**
Employees	0.343	-0.278	-0.622	331	0.38	Y	N	N	N
	(0.013)	(0.18)	(0.012)						
	0.359	-0.310	-0.669	331	0.39	N	Y	N	N
	(0.010)	(0.13)	(0.007)						
	0.347	-0.252	-0.669	331	0.42	N	Y	Y	N
	(0.015)	(0.19)	(0.007)						
	0.402	-0.269	-0.599	329	0.44	N	Y	Y	Y
	(0.007)	(0.17)	(0.012)						
Capital Raised	0.107	-0.164	-0.271	331	0.32	Y	N	N	N
	(0.062)	(0.08)	(0.015)						
	0.131	-0.151	-0.253	331	0.32	N	Y	N	N
	(0.074)	(0.11)	(0.023)						
	0.090	-0.139	-0.229	331	0.33	N	Y	Y	N
	(0.132)	(0.14)	(0.041)						
	0.095	-0.128	-0.223	329	0.34	N	Y	Y	Y
	(0.106)	(0.20)	(0.056)						
Amount Raised	0.308	-0.491	-0.800	331	0.31	Y	N	N	N
	(0.026)	(0.09)	(0.013)						
	0.277	-0.426	-0.703	331	0.33	N	Y	N	N
	(0.041)	(0.14)	(0.029)						
	0.255	-0.387	-0.642	331	0.34	N	Y	Y	N
	(0.074)	(0.18)	(0.047)						
	0.284	-0.377	-0.661	329	0.35	N	Y	Y	Y
	(0.044)	(0.20)	(0.045)						

	0.315	-0.367	-0.671	331	0.41	Y	N	N	N
	(0.019)	(0.12)	(0.007)						
	0.333	-0.428	-0.682	331	0.42	N	Y	N	N
Market Traction	(0.014)	(0.08)	(0.012)						
	0.321	-0.382	-0.761	331	0.44	N	Y	Y	N
	(0.017)	(0.11)	(0.007)						
	0.360	-0.394	-0.703	329	0.45	N	Y	Y	Y
	(0.009)	(0.12)	(0.010)						
	0.131	0.018	-0.114	331	0.31	Y	N	N	N
	(0.044)	(0.81)	(0.243)						
	0.140	0.001	-0.139	331	0.30	N	Y	N	N
Survival	(0.036)	(0.99)	(0.145)						
	0.136	0.018	-0.118	331	0.32	N	Y	Y	N
	(0.040)	(0.78)	(0.211)						
	0.154	-0.002	-0.156	329	0.33	N	Y	Y	Y
	(0.024)	(0.98)	(0.114)						

Robust p-values in parentheses.

Notes: Basic controls include Initial Employees, Initial Sales and Application Score. Basic*Above are the basic controls interacted with the High Educated dummy variable. Initial Stage interact with the High Education dummy, however Age of Team Leader is added as control without interaction.

Table 8: Qualitative analysis from interviews about standardized meetings

	Weight: Treatment (+) Control (-)	**High-Educ:** Treatment (+) Control (-)	**Low-Educ:** Treatment (+) Control (-)
Topic 1: Positive aspects of the board			
Positive perception about the role of the board included: access to new contacts, ideas, constructive criticism, domain knowledge, and encouragement.	+100%	+167%	+78%
* Structured accountability was appreciated as it helped founders to plan, focus and work within a given structure.	+52%	+139%	-8%
Topic 2: Critical aspects of the board			
* Negative perception about the role of the board members, primarily due to unfulfilled expectations about what a board should do. Critiques included that: boards were not startup-relevant domain experts, board participation was discontinuous and sometimes absent, board members were not chosen by the entrepreneurs, instead of constructive feedback it was negative.	+20%	+67%	-16%
Feedback was perceived as useless, meetings were disorganized, and the tasks requested were not consistent with the development stages of the startups.	+32%	+29%	+35%
* Very little accountability was perceived, tasks requested seemed unreal or contradictory to founder's own goals.	0%	+167%	-93%
Emotions of frustration, anxiety and perceptions of lack of honesty.	-94%	-91%	-100%
Topic 3: Positive aspects of the platoon			
* Founders appreciate platoons because of: the emotional support from sharing experiences, the support from peers, generation of new contacts and leads, interaction with domestic entrepreneurs (for foreigners), friendships.	+22%	+93%	-22%
Topic 4: Critical aspects of the platoon			
* Negative perception associated primarily to: disorganization, unclear objectives, heterogeneous domain experience of peers.	+22%	-100%	+43%
Perceived as useless gatherings that did not add value, and conversations were forced.	-58%	-50%	-67%
Topic 5: Comparison between platoons and boards			
Platoons are less formal, and thus there is a lower commitment in executing the enunciated tasks. Peers are too forgiving.	+116%	+200%	+86%
Topic 6: General sentiments about Start-Up Chile			
* Too many obligations.	+143%	-200%	+200%
Bureaucratic.	-120%	-67%	-200%
Support to expand internationally.	+111%	+100%	+100%

Note: Statement frequencies were counted from the 22 transcribed interviews. Weight is calculated as the percentage deviation of the frequencies over the mean frequency for the given statements. Nine (thirteen) interviewees were categorized as high-education (low-education) founders.

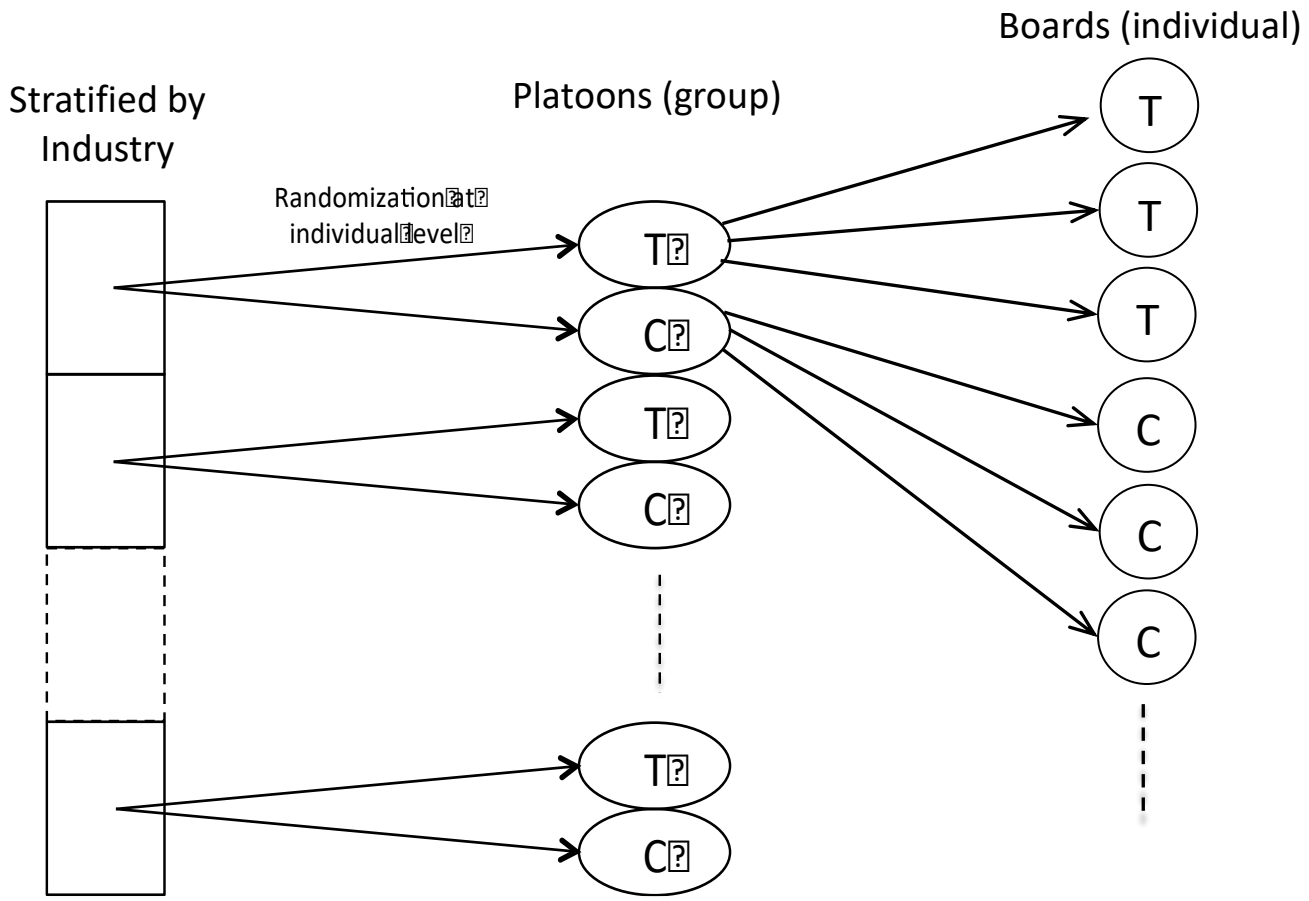
FIGURES

Figure 1: Entrepreneurship Schooling: Parallel between Business Schools and Accelerators

Sources positive returns to schooling	Mechanism	Business School	Business Accelerators
Signalling (Spence, 1973, Arrow, 1973)	**Reputation** (Rao1994; Zott and Huy, 2007)	Certification from selection, graduation from business school, diploma.	Certification from selection, graduation from entrepreneurship school, exposure to community.
Productivity (Becker, 1964)	**Know-how** (Lerner and Malmendier, 2013)	Developing and growing a company through classes, professors, guest speakers, career office, advisors, fellow classmates.	Developing and growing a start-up through workshops, staff, guest speakers, industry experts, mentors, fellow participants.
	Social Networks (Granovetter, 1973; Ketchen, Ireland and Snow, 2007)	Preferential access to peer and professor networks.	Preferential access to peer and staff networks.
	Self-efficacy (Bandura, 1982; Forbes, 2005)	Self-confidence from selection and graduation (in the form of business self-efficacy)	Self-confidence from selection and graduation (in the form of entrepreneurial self-efficacy)
	Structured Accountability (Locke and Latham, 2002; Cialdini and Goldstein, 2004)	Setting learning goals, class work, homework, exams.	Setting strategic tasks, monthly follow-up meetings, demo-day

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

Figure 2: Graphical representation of assignment to experimental groups



APPENDIX

Appendix 1: Start-up Characteristics at Application by Generation

Panel A: Applicants and Participants

Generation	Applicants	Rejections	Selections	Participants	Treatment	Control
17	619	519	100	83	41	42
18	719	619	100	84	43	41
19	582	491	91	76	37	39
20	467	394	73	64	33	31
21	156		60	54	27	27
Total						

Panel B: Capital Raised at Application

	Start-Up Chile							ED
	17	18	19	20	21	Total	%	%
No (Bootsrapped)	55	58	30	28	22	193	53.4	79.28
Yes	28	26	46	36	32	168	46.5	20.72
Total	83	84	76	64	54	361		

Panel C: Number of Full-Time Workers at Application

	Start-Up Chile							ED
	17	18	19	20	21	Total	%	%
-	0	2	7	2	2	13		
<5	61	60	45	41	21	228	65,5	68.54
5-10	20	15	19	20	24	98	28,2	16.85
10+	2	7	5	1	7	22	6,3	14.62
Total	83	84	76	64	54	348		

Panel D: Start-up Age at Application

	Start-Up Chile							ED
	17	18	19	20	21	Total	%	%
-	83	0	0	0	0	83		
Less than 6 months	-	18	17	10	3	48	17,3	21.86

6-12 months	-	41	33	30	26	130	46,8	29.36
12-24 months	-	22	18	23	22	85	30,6	17.10
More than 2 years	-	3	8	1	3	15	5,4	31.68
Total	-	84	76	64	54	278		

Panel F: Start-up Development Stage

	Start-Up Chile							ED
	17	18	19	20	21	Total	%	%
-	1	2	10	0	1	14		
Concept	2	4	2	3	4	15	4,3	22.10
Prototype in Development	11	12	10	11	9	53	15,3	4.12
Functional Product with Users	37	41	27	34	28	167	48,1	22.52
Scaling Sales	32	25	27	16	12	112	32,3	51.27
Total	83	82	66	64	53	361		

This table describes the composition of the sample, which includes 361 participant start-ups to the experiment. It also describes average applicants to ecosystem accelerators worldwide under the heading "ED," based on information from the Emory Entrepreneurship Database. Percentages are calculated over the number of non-missing responses.

Founder Characteristics at Application by Generation

Panel A: Location

	Start-Up Chile							ED
	17	18	19	20	21	Total	%	%
-								
Africa	0	2	1	1	2	6	1,7	19.1
Asia	7	10	8	8	4	37	10,2	19.1
Europe	15	6	6	9	5	41	11,4	6.6
N. America	11	8	12	5	6	42	11,6	34.8
Oceania	1	1	0	1	1	4	1,1	0.4
S. America (exc. Chile)	31	22	26	19	19	117	32,4	19.4
Chile	18	35	23	21	17	114	31,6	0.6
	83	84	76	64	54	361		

Panel B: Age

	Start-Up Chile							ED
	17	18	19	20	21	Total	%	%
-								

Younger than 25	13	8	7	4	5	37	10,2	10.6
Between 25 and 30	30	36	34	25	23	148	41,0	21.73
Between 30 and 35	23	21	20	17	14	95	26,3	21.64
Between 35 and 40	10	9	11	10	5	45	12,5	15.24
Older than 40	7	10	4	8	7	36	10,0	30.79
Total	83	84	76	64	54	361		

Panel C: Gender

	Start-Up Chile							ED
	17	18	19	20	21	Total	%	%
Female	19	18	21	12	10	80	22,2	28.6
Male	64	66	55	52	44	281	77,8	71.4
Total	83	84	76	64	54	361		

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

Appendix 2: Baseline without 19th Generation

	(1)			**(2)**				
Estimate	**Treatment**	**N**	**R^2**	**Treatment Low Education**	**Difference**	**Treatment High Education**	**N**	**R^2**
High Education	0.011	281	0.11	-	-	-	-	-
	(0.870)							
Female	-0.073	281	0.15	-0.161	0.214	0.053	268	0.25
	(0.193)			(0.035)	(0.109)	(0.63)		
HQ in Chile	-0.020	281	0.23	-0.053	0.079	0.026	268	0.35
	(0.753)			(0.563)	(0.594)	(0.82)		
Chilean Entrepreneur	-0.034	281	0.21	-0.076	0.106	0.030	268	0.39
	(0.577)			(0.401)	(0.452)	(0.78)		
LatAm Entrepreneur	-0.045	281	0.19	0.027	-0.113	-0.086	268	0.30
	(0.474)			(0.773)	(0.452)	(0.47)		
Age Team Leader	-1.551	279	0.21	-2.135	1.327	-0.808	266	0.32
	(0.099)			(0.083)	(0.565)	(0.68)		
Capital Raised before Application	-0.084	281	0.18	-0.172	0.255	0.083	268	0.30
	(0.195)			(0.053)	(0.091)	(0.49)		
Initial Sales	-0.742	277	0.20	-0.737	-0.437	-1.175	263	0.32
	(0.083)			(0.230)	(0.648)	(0.11)		

Initial Stage	0.024	277	0.23	0.173	-0.544	-0.371	263	0.33
	(0.818)			(0.232)	(0.029)	(0.07)		
Initial Employees	-0.094	281	0.22	-0.086	-0.119	-0.206	268	0.38
	(0.133)			(0.346)	(0.365)	(0.03)		
Score	-0.068	281	0.31	-0.007	-0.179	-0.186	268	0.43
	(0.160)			(0.915)	(0.134)	(0.07)		

Robust p-values in parentheses

(1) modelo con efectos fijos eje_num#generation num_ind

(2) modelo con efectos fijos eje_num#generation#above num_ind#above

Appendix 3: Development of Outcome Variables

Given the fledgling nature of startups, the standard metrics used to establish firm performance for more mature businesses (e.g., profits or stock price) are not generally available, nor are they particularly useful in new venture settings (cf. Puri and Zarutskie, 2012). For example, Facebook purchased Instagram for roughly $1 billion when it was only one and a half years old and had neither revenues nor profits. However, it had over 100 million active users. Therefore, in keeping with prior literature (Eisenhardt and Schoonhoven, 1990; Maurer and Ebers, 2006), we construct performance measures that proxy for venture fundraising, venture scale, and venture survival. We use two methods: web searches and surveys.

Our first web search (conducted during the months of October and November of 2020) focused on CB Insights and LinkedIn. Our second web search (conducted November 2020) focused on the Facebook and Twitter platforms. Our first survey (conducted during the fourth quarter of 2019 and first quarter of 2020) was focused on participants. Logarithmic transformations of the survey responses are used to reduce the impact of outliers. Following, we describe details of each of the data collection methods.

Web-based Measures:

Variables Names	Description	Construction logic
Employees	Number of employees	LinkedIn reports the number of employees in ranges (e.g., 1-10 employees), which we transform into point estimates using the median employee size in the range (i.e., we assigned an employment level of 5 when the reported range was 1-10 employees). We confirmed that the transformation rule is immaterial for the results.
Capital Raised	*Capital Raised* equals 1 if the start-up has a post-application fundraising record, and 0 otherwise.	If a start-up has relevant fundraising activity, that activity is most likely to appear on CB Insights. By construction, we also code this variable with zero for those that do not have a profile on CB Insights. We use detailed information about the fundraising date in the platform, together with the start-ups' application date, to classify fundraising rounds as post-application.
Amount Raised*	*Amount Raised* is the natural log of the value of capital raised.	This variable equals zero if the start-up has no post-application fundraising record on CB Insights, if such a record exists but does not specify an amount of capital raised, or if the start-up has no profile on CB Insights.
Market Traction	*Market Traction* is the natural log of the number of LinkedIn followers, standardized.	*

Survival Online	*Survival Online* equal 1 if the startup has online presence.	This variable equals zero if the start-up does not have a profile on LinkedIn or Crunchbase.

Survey based Variables:

We sent an email to all the 361 participants between the months of October 2019 and January 2020 asking for the survival (and last time the startup was active if the answer was negative), the average monthly values of revenue, the amount of capital raised, and the number of employees of each semester since graduation.

	Participation Dates	Survey Date	Participants	Responses	Response Rate	Number Semesters Surveyed	Semesters Surveyed	Semesters since graduation
G17	Feb-2017 to Aug-2017	Oct-2019	83	52	62,70%	4	2017-2 a 2019-1	4
G18	Jul-2017 to Dic-2017	Oct-2019	84	63	75,00%	3	2018-1 a 2019-1	3
G19	Mar-2018 to Aug-2018	Nov-2019	76	60	78,90%	2	2018-2 a 2019-1	2
G20	Jul-2018 to Dic-2018	Nov-2019	64	54	84,40%	1	2019-1	1
G21	Feb-2018 to Aug-2018	Jan-2020	54	36	66,70%	1	2019-1 a 2019-2	1
TOTAL			361	265	73,40%			

Appendix 4: Correlation Web-based and survey-based performance proxies

	Web Variables	Survey Variables	Observations
Employment	Employees Web	Employees 0.06 (0.28)	270
Capital Raised	Capital Raised dummy	Capital Raised 0.32 (0.00)	286
	Funding after SUP	0.55 (0.00)	286
Market Traction	LinkedIn Followers	Revenue 0.21 (0.00)	262
Survival	Survival Online	Survival 0.26 (0.00)	295

The table presents correlations across Web-based and survey-based venture performance metrics. Robust p-values are reported.

Appendix 5: Main Results without 19th Generation

	Treatment					Controls			
Estimate	**Low Education**	**High Education**	**Difference**	**N**	**R^2**	**Raw**	**Above**	**Initial Stage**	**Age Team Leader**
Employees	0.468	-0.404	-0.872	263	0.40	Y	N	N	N
	(0.013)	(0.12)	(0.006)						
	0.463	-0.369	-0.832	263	0.40	N	Y	N	N
	(0.015)	(0.15)	(0.009)						
	0.432	-0.325	-0.756	263	0.44	N	Y	Y	N
	(0.026)	(0.17)	(0.014)						
	0.498	-0.261	-0.759	261	0.46	N	Y	Y	Y
	(0.014)	(0.29)	(0.018)						
LinkedIn Followers	0.349	-0.257	-0.606	263	0.45	Y	N	N	N
	(0.017)	(0.35)	(0.051)						
	0.345	-0.289	-0.634	263	0.46	N	Y	N	N
	(0.020)	(0.31)	(0.050)						
	0.301	-0.255	-0.555	263	0.49	N	Y	Y	N
	(0.037)	(0.36)	(0.077)						
	0.346	-0.216	-0.562	261	0.51	N	Y	Y	Y
	(0.022)	(0.46)	(0.091)						
Web Traffic (Ranking)	0.303	-0.187	-0.490	263	0.30	Y	N	N	N
	(0.105)	(0.49)	(0.138)						
	0.311	-0.144	-0.454	263	0.31	N	Y	N	N
	(0.097)	(0.63)	(0.193)						
	0.300	-0.130	-0.430	263	0.32	N	Y	Y	N
	(0.118)	(0.66)	(0.218)						
	0.300	-0.013	-0.312	261	0.33	N	Y	Y	Y
	(0.121)	(0.97)	(0.363)						
Capital Raised dummy	0.098	-0.191	-0.289	263	0.34	Y	N	N	N
	(0.164)	(0.07)	(0.023)						
	0.095	-0.172	-0.267	263	0.35	N	Y	N	N
	(0.178)	(0.11)	(0.038)						
	0.084	-0.164	-0.248	263	0.36	N	Y	Y	N
	(0.251)	(0.13)	(0.057)						
	0.086	-0.138	-0.224	261	0.37	N	Y	Y	Y
	(0.231)	(0.23)	(0.103)						
Funding after SUP	0.296	-0.744	-1.040	263	0.35	Y	N	N	N
	(0.117)	(0.04)	(0.013)						
	0.267	-0.626	-0.893	263	0.37	N	Y	N	N
	(0.142)	(0.08)	(0.027)						

	0.256	-0.595	-0.851	263	0.38	N	Y	Y	N
	(0.180)	(0.10)	(0.039)						
	0.273	-0.531	-0.804	261	0.39	N	Y	Y	Y
	(0.145)	(0.16)	(0.061)						
Survival Online	0.160	-0.004	-0.164	263	0.27	Y	N	N	N
	(0.043)	(0.96)	(0.157)						
	0.160	-0.011	-0.171	263	0.28	N	Y	N	N
	(0.046)	(0.90)	(0.137)						
	0.145	0.002	-0.832	263	0.31	N	Y	Y	N
	(0.069)	(0.98)	(0.009)						
	0.166	-0.002	-0.168	261	0.32	N	Y	Y	Y
	(0.045)	(0.98)	(0.151)						

Robust p-values in parentheses

Appendix 6: Main Results without Industry Fixed Effects

	Treatment					Controls			
Estimate	**Low Education**	**High Education**	**Difference**	**N**	**R^2**	**Raw**	**Above**	**Initial Stage**	**Age Team Leader**
Employees	0.249	-0.265	-0.514	347	0.303	Y	N	N	N
	(0.086)	(0.23)	(0.048)						
	0.273	-0.293	-0.566	347	0.308	N	Y	N	N
	(0.064)	(0.18)	(0.032)						
	0.254	-0.254	-0.508	347	0.336	N	Y	Y	N
	(0.091)	(0.20)	(0.041)						
	0.285	-0.270	-0.556	345	0.348	N	Y	Y	Y
	(0.067)	(0.16)	(0.026)						
Capital Raised dummy	0.066	-0.113	-0.179	347	0.183	Y	N	N	N
	(0.180)	(0.19)	(0.071)						
	0.063	-0.103	-0.166	347	0.185	N	Y	N	N
	(0.206)	(0.24)	(0.098)						
	0.053	-0.098	-0.151	347	0.193	N	Y	Y	N
	(0.305)	(0.26)	(0.135)						
	0.054	-0.085	-0.139	345	0.197	N	Y	Y	Y
	(0.287)	(0.33)	(0.173)						
Funding after SUP	0.193	-0.441	-0.634	347	0.198	Y	N	N	N
	(0.165)	(0.15)	(0.064)						
	0.163	-0.404	-0.567	347	0.205	N	Y	N	N
	(0.230)	(0.19)	(0.095)						
	0.138	-0.379	-0.517	347	0.218	N	Y	Y	N

	(0.326)	(0.22)	(0.127)						
	0.153	-0.348	-0.502	345	0.223	N	Y	Y	Y
	(0.265)	(0.26)	(0.137)						
LinkedIn Followers	0.193	-0.233	-0.426	347	0.337	Y	N	N	N
	(0.111)	(0.28)	(0.083)						
	0.205*	-0.263	-0.469	347	0.341	N	Y	N	N
	(0.090)	(0.23)	(0.061)						
	0.184	-0.231	-0.415	347	0.368	N	Y	Y	N
	(0.127)	(0.25)	(0.079)						
	0.201	-0.251	-0.452	345	0.371	N	Y	Y	Y
	(0.104)	(0.24)	(0.070)						
Survival Online	0.087	0.017	-0.070	347	0.199	Y	N	N	N
	(0.151)	(0.81)	(0.443)						
	0.097	0.002	-0.095	347	0.205	N	Y	N	N
	(0.118)	(0.97)	(0.306)						
	0.091	0.013	-0.078	347	0.222	N	Y	Y	N
	(0.145)	(0.84)	(0.374)						
	0.100	-0.002	-0.102	345	0.229	N	Y	Y	Y
	(0.118)	(0.98)	(0.257)						

Robust p-values in parentheses

Appendix 7: Main Results without 18tn Generation

	Treatment					Controls			
Estimate	**Low Education**	**High Education**	**Difference**	**N**	**R^2**	**Raw**	**Above**	**Initial Stage**	**Age Team Leader**
Employees	0.287	-0.346	-0.633*	248	0.384	Y	N	N	N
	(0.128)	0.25	(0.071)						
	0.316*	-0.384	-0.700**	248	0.391	N	Y	N	N
	(0.099)	0.20	(0.048)						
	0.285	-0.316	-0.601*	248	0.417	N	Y	Y	N
	(0.147)	0.28	(0.088)						
	0.359*	-0.347	-0.706*	247	0.432	N	Y	Y	Y
	(0.083)	0.25	(0.058)						
Capital Raised dummy	0.123*	-0.181	-0.305**	248	0.360	Y	N	N	N
	(0.078)	0.12	(0.026)						
	0.133*	-0.189	-0.321**	248	0.371	N	Y	N	N
	(0.060)	0.12	(0.022)						
	0.096	-0.174	-0.271*	248	0.395	N	Y	Y	N

	(0.193)	0.14	(0.054)						
	0.105	-0.167	-0.272*	247	0.399	N	Y	Y	Y
	(0.154)	0.17	(0.060)						
Funding after SUP	0.357*	-0.427	-0.784*	248	0.335	Y	N	N	N
	(0.068)	0.31	(0.090)						
	0.372*	-0.466	-0.838*	248	0.344	N	Y	N	N
	(0.060)	0.28	(0.076)						
	0.284	-0.419	-0.703	248	0.362	N	Y	Y	N
	(0.174)	0.33	(0.140)						
	0.331	-0.427	-0.758	247	0.368	N	Y	Y	Y
	(0.113)	0.33	(0.121)						
LinkedIn Followers	0.337**	-0.265	-0.603*	248	0.420	Y	N	N	N
	(0.042)	0.35	(0.068)						
	0.376**	-0.294	-0.670*	248	0.435	N	Y	N	N
	(0.025)	0.35	(0.061)						
	0.356**	-0.253	-0.608*	248	0.448	N	Y	Y	N
	(0.035)	0.41	(0.085)						
	0.394**	-0.274	-0.669*	247	0.452	N	Y	Y	Y
	(0.023)	0.41	(0.083)						
Survival Online	0.067	0.031	-0.036	248	0.337	Y	N	N	N
	(0.377)	0.72	(0.747)						
	0.079	0.019	-0.060	248	0.348	N	Y	N	N
	(0.300)	0.82	(0.600)						
	0.071	0.037	-0.035	248	0.361	N	Y	Y	N
	(0.349)	0.66	(0.757)						
	0.090	0.021	-0.069	247	0.369	N	Y	Y	Y
	(0.255)	0.80	(0.556)						

Robust p-values in parentheses