Managerial Springboards: Employee Spillovers of Startup Accelerators [1]

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We study whether business accelerators improve workers' long-term career trajectories. Using (i) a cross-program Crunchbase–LinkedIn dataset for the Americas and (ii) a quasi-experimental design in a Colombian accelerator, we find large, persistent gains: more transitions into managerial roles, higher wages, and no increase in unemployment. These benefits occur outside the original startup, reflect shifts into more knowledge-intensive and less routine work, hold even when the startup fails, and are not explained by moves to other accelerator-backed firms. The patterns best align with human-capital accumulation—not certification or network effects—suggesting that accelerators help build the managerial talent pipeline of the innovation economy.

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Large income gaps persist both between rich and poor countries and across regions within advanced economies. A key reason is that many workers remain stuck in low-productivity firms with limited opportunities for learning or career progression. For most people, labor is their main asset, and jobs in larger, more productive firms provide higher wages and richer prospects for acquiring new skills (Bloom and Van Reenen 2010; Bloom et al. 2020; Akcigit, Baslandze, and Stantcheva 2021; Arellano and Bover 2021). Yet in low-income settings the typical firm employs only the owner, and even in richer economies poorer cities remain anchored in traditional, routine "old-economy" sectors—dominated by small, low-productivity firms where workers have few pathways to advance (McKenzie, 2017). Prosperous cities, by contrast, have shifted toward the "new economy": innovation-intensive, knowledge-driven industries powered by high-skill workers and offering far greater opportunities for learning and upward mobility. This divergence fuels widening regional gaps in productivity and opportunity(Hsieh and Olken 2014; Moretti 2012).

A natural question, then, is how to expand the supply of new-economy firms, the high-quality jobs they create, and the long-term benefits these jobs can generate for workers. This poses a core policy challenge: how to identify and support entrepreneurs capable of building scalable, productivity-enhancing ventures. Targeted support for high-potential startups—such as business accelerators—has become a leading approach, and existing evaluations show that well-designed programs can improve firm outcomes (McKenzie 2017; González-Uribe and Leatherbee 2018; González-Uribe and Reyes 2021). But these studies also highlight how rare high-growth firms are, and how many participant firms ultimately shut down (Yu, 2020; Lee et al., 2025). This leaves a key question unanswered: do accelerators improve outcomes for the workers employed in these early-stage ventures—regardless of whether the firms survive—or are the benefits limited to only the owners of the few firms that scale?

This paper tackles that question by examining how business accelerators shape workers' long-term career trajectories. We argue that the structured, high-pressure environment of accelerators—tight deadlines, milestone-based accountability, and investor scrutiny—raises employees' incentives to invest in on-the-job learning (Becker 1975; Hochberg 2016), regardless of the firm's eventual performance. There is much to learn in periods of rapid growth, but also in the experience of business closure, and the labor market appears to value both types of learning. Because these skills are portable, workers can redeploy them elsewhere in the economy, generating flywheel effects that are manifested in, and traceable through, their subsequent career paths. We test this hypothesis by assessing how exposure to accelerated startups shapes workers' subsequent employment, career progression, and long-run earnings—moving beyond prior work that has focused primarily on firm outcomes rather than worker trajectories.

We combine two complementary empirical strategies. First, we assemble a large cross-program dataset linking Crunchbase and LinkedIn to track workers' career histories at accelerated startups across the Americas and compare them with matched employees in non-accelerated firms. Second, we leverage a quasi-experimental setting in Colombia by linking social-security wage records to employees of startups that applied to the ValleE accelerator, where randomly assigned judges—who vary in scoring generosity—create exogenous variation in admission (González-Uribe and Reyes 2021). The cross-program analysis provides breadth and external validity across countries and programs, while the quasi-experimental design delivers clean causal estimates from administrative data.

Across both analyses, we find large and persistent employee spillovers. Within seven years of acceleration, workers from accelerated startups earn higher wages, face no higher risk of unemployment, and are far more likely to move into managerial roles. These gains occur outside both the originating firm and the accelerator's network and are driven by ex-ante high-potential participants rather than marginal admits. They apply to founders and rank-and-file employees alike, and to employees of both surviving and failed startups. Most transitions are into professional managerial jobs in established firms, with some employees also founding new ventures—evidence of both managerial and entrepreneurial upgrading.

The quasi-experimental evidence shows that these patterns are not driven by selection (e.g., accelerators picking high-growth firms already employing future managers), nor by mechanical channels such as within-firm performance improvements or internal progression through rent-sharing (Van Reenen 1996; Howell and Brown 2023). Instead, the evidence points strongly toward a portable human-capital accumulation mechanism, rather than matching or signalling. Workers move into roles demanding stronger problem-solving, critical-thinking, and managerial skills (Hansen et al. 2025; Autor, Levy, and Murnane 2003), with the largest shifts in settings where skill-building is most valuable: among workers without prior business training, in underserved regions such as Latin America, and in ventures supported by more experienced accelerators.

These findings have two broad implications. First, our findings show that business accelerators are emerging as institutions of human-capital formation for the new economy, contributing to a broader literature on how firms shape *managerial capital*—defined as the skills and practices managers use to organise production, lead teams, and make strategic decisions (Schoar 2010). This matters because managerial talent is a central determinant of firm productivity (Bloom and Van Reenen 2007; Bloom et al. 2013), yet we know relatively little about how such talent is cultivated inside young, innovation-driven firms. Existing research focuses mainly on CEOs of publicly listed or private-equity-backed companies (Cziraki and Jenter 2022; Gompers, Kaplan, and Mukharlyamov 2023), leaving open the question of where the next generation of managerial talent in the new economy comes from.

Our work complements and extends prior research showing that established firms can build managerial capability through improvements in management practices or targeted executive training (Bruhn, Karlan, and Schoar 2010; McKenzie and Woodruff 2012; Bloom et al. 2013; Bandiera et al. 2020; Custódio et al. 2025). Yet this literature focuses primarily on firms with limited growth prospects—such as microenterprises or underperforming mature firms—and on formal training settings, providing little guidance on how managerial talent is formed within young, high-potential firms.

Our contribution is to show that an important, previously overlooked stage of managerial-capital formation occurs inside early-stage, innovation-intensive firms. Accelerators expose employees—not only founders—to structured, high-pressure environments that compress learning cycles and accelerate the accumulation of managerial capabilities. These skills are portable and durable, helping to build the managerial talent base of the new economy.

This perspective reframes the role of targeted high-growth startup support, and accelerators in particular. Rather than serving only to improve startup performance, accelerators operate as institutional pipelines for managerial talent. Evidence from Figure 1—based on LinkedIn data that naturally overrepresent young, new-economy firms where online résumé posting is common—shows that nearly one in ten managers in such firms previously worked at accelerator-backed startups. This share has risen steadily over time for young new-economy firms while remaining flat in older ones. The pattern is striking: as the prevalence of MBAs among newly appointed managers has declined, accelerator experience has grown to a comparable level. Today, roughly 15 percent of new managers in young firms previously worked in accelerator-backed ventures, and roughly 15 percent hold an MBA, with little overlap between the two groups.

These patterns do not imply that accelerators will replace business schools. Rather, they show that accelerators provide a distinct and increasingly important pathway into management—one built on experiential learning, rapid problem-solving cycles, and hands-on exposure to growth environments that formal education often cannot replicate. In this sense, accelerators complement traditional business-education pathways, broadening the institutional channels through which managerial talent in the innovation economy is developed.

Second, our findings broaden how the impacts of entrepreneurship support should be evaluated. Accelerator programs—and the policies that fund them—are typically assessed through firm-centric outcomes such as survival, growth, and investment (McKenzie 2017; Campos et al. 2025). This narrow focus misses a central margin of impact. Governments and donors spend more than $1 billion annually on programs aimed at developing entrepreneurs (McKenzie et al. 2020), yet most employees of accelerated startups will not found new ventures, and many participating firms ultimately shut down. Our results show that accelerators generate substantial worker-level skill formation and diffusion, producing benefits that persist long after the original firms have ceased to operate.

By equipping not only founders but also their teams with transferable skills, accelerators seed managerial and entrepreneurial capabilities that diffuse through the labor market. These spillovers help explain how early-stage interventions can generate broader productivity gains even when most participating firms do not scale. Recognising these worker-level pathways is essential for evaluating entrepreneurship policy and for informing broader debates about whether managerial and entrepreneurial skills can be taught in adulthood (Guiso, Pistaferri, and Schivardi 2021) and how such talent is formed in entrepreneurial settings.

Taken together, the evidence argues for expanding the lens of entrepreneurship policy: accelerators shape not only the trajectory of the firms they support but also the careers, skills, and long-term opportunities of the people who work in them. The substantial worker-level gains we document make this clear. Exposure to an accelerator triples the likelihood of moving into a managerial role and raises subsequent wages by 1.5× relative to comparable employees in non-accelerated startups over seven years, with no increase in unemployment risk. In the Colombian quasi-experimental setting, these effects correspond to an earnings gain of roughly 10% of the minimum wage—about USD 600 per year, or 7.5% of Colombia's per capita GDP.

What explains these career gains? The quasi-experimental evidence rules out a simple selection story in which accelerators merely pick high-growth firms that would have hired future managers anyway. Following the classic framework on post-schooling wage determinants (Rubinstein and Weiss 2007), three mechanisms could in principle account for the positive causal impact of accelerators on employee career trajectories: human-capital accumulation, improved matching, or signalling.

We find that human-capital accumulation is the only explanation consistent with the full body of evidence. Accelerators immerse entire startup teams—not only founders—in intensive, experiential learning environments: structured mentorship, disciplined timelines, milestone-based accountability, and investor scrutiny. These features plausibly raise incentives for on-the-job learning, compress learning cycles, and accelerate skill formation (Becker 1975; Bender et al. 2018; Hochberg et al. 2019). Founders' curricula and management tools also spill over to non-founder employees through the diffusion of improved management practices (Bender, Bloom, Card, Van Reenen, and Wolter 2018).

Consistent with this mechanism, the cross-program analysis shows that workers move into roles demanding stronger problem-solving and critical-thinking abilities, more intensive managerial skills, and fewer routine tasks. The largest gains accrue to employees without prior formal business education. The quasi-experimental results mirror these patterns: accelerated employees disproportionately transition into innovation- and knowledge-intensive industries and occupations requiring non-routine cognitive skills.

These patterns are difficult to reconcile with improved matching or signalling. If matching—particularly through accelerator networks—were the driver, we would expect sorting into any available positions within the accelerator ecosystem, including lower-quality matches. Instead, employees sort into higher-order, skill-intensive managerial roles in firms entirely outside the accelerator's network. A pure networking "safety-net" channel is equally inconsistent with the selective shift toward knowledge-intensive sectors.

A certification mechanism also fails to fit the evidence. Signalling would predict wage premia concentrated among employees of successful ventures. Yet we find equally large gains for employees of failed firms—precisely where a negative failure signal should dominate any positive certification effect. Nor would certification explain the persistent skill demands of subsequent roles or the lack of demotions following managerial promotions.

We provide direct tests against these alternatives. If accelerators improved matching by broadening workers' outside options (Mortensen 1976; Feldman and Zoller 2012), we would observe higher employment probabilities or clustering into other accelerator-backed firms. Neither appears. Results are unchanged when excluding all jobs in other accelerator participants, and in the quasi-experimental setting not a single employee moves to another startup in the same cohort. Employees also spend more of their post-acceleration careers outside the program's headquarters city—contrary to predictions of a network-matching channel.

The quasi-experimental evidence further undermines certification. Causal effects are driven by firms with high ex-ante growth potential—the true compliers in our IV design. Low-potential firms that enter "by luck" rarely hire formal workers and thus drop out of the data. Our identification therefore compares high-quality firms correctly admitted with high-quality firms incorrectly rejected due to systematically tough judges. This comparison is incompatible with a certification story but fully consistent with accelerators building worker skills rather than merely conferring labels.

Our final analysis returns to the firm. A natural concern is that worker-level gains might come at the expense of the originating startups—for example, if accelerators raise employees' outside options and leave firms struggling to replace departing talent. Prior work, however, finds positive average firm-level effects, suggesting the opposite. We extend this evidence by examining whether accelerated startups replace departing workers and how the characteristics of their new hires compare with those of matched non-accelerated firms.

This analysis is challenging because employees choose when to exit and because acceleration may itself reshape a firm's hiring needs and talent mix. Even so, the patterns suggest that worker gains do not harm their firms. In both analyses, replacement hires in accelerated startups appear stronger than those in comparable non-accelerated firms: they have more prior experience and spend a larger share of their subsequent careers in formal employment. We find no consistent differences by gender or age.

Our interpretation is that, rather than being harmed by talent turnover, accelerated firms show signs of organisational upgrading consistent with maturation and professionalisation (Hellmann and Puri 2002). Taken together, the evidence underscores that accelerators function as institutions of human-capital formation in the innovation economy—shaping not only the trajectories of the startups they support but also the broader stock and distribution of managerial talent.

The remainder of the paper proceeds as follows. Section I describes business accelerators, their potential role in shaping startup employee careers, and positions the paper within the related literature on the importance of management and entrepreneurship policy. Section II details the construction of our datasets—the cross-program sample built from Crunchbase and LinkedIn and the administrative data from the ValleE accelerator in Colombia—and outlines our empirical frameworks: the event-time and staggered difference-in-differences designs used in the cross-program analysis, and the instrumental-variable strategy exploiting quasi-random judge assignments in ValleE. Section III presents the main results on employee outcomes—managerial transitions, wages, and employability—

and examines heterogeneity by program type, region, and employee characteristics. Section IV investigates mechanisms, focusing on human-capital accumulation, search, and signalling, and evaluates firm-level responses to employee turnover. Section V concludes by discussing policy implications for entrepreneurship support programs and their role in shaping the future stock of managerial talent in developing economies.

1. Business accelerators and their role in startup employee careers

More than $1 billion is spent each year worldwide on programs to train entrepreneurs. At their core, these initiatives seek to narrow regional disparities in productivity and income, driven by the belief that some entrepreneurs possess high growth potential but face binding constraints to expansion—and that effective policy design can identify these individuals and unlock their potential to build fast-growing firms.

Among these initiatives, business accelerators have become increasingly prominent. They now play a central role in both entrepreneurship policy and private-sector innovation, with leading programs such as Y Combinator, Techstars, and 500 Startups attracting thousands of applicants to each cohort. Unlike traditional entrepreneurship programs focused on basic training or mindset development, accelerators operate as "schools for entrepreneurs"—intensive, cohort-based programs built around experiential learning. Entire teams are immersed in high-pressure environments that combine mentorship, milestone tracking, and frequent feedback (Cohen and Hochberg 2014; González-Uribe and Leatherbee 2017), often culminating in high-stakes pitch events and seed investment in exchange for equity. Since Y Combinator's launch in 2005, accelerators have spread globally, with more than 1,600 programs listed on Crunchbase and growing academic attention (Cohen, Fehder, Hochberg, and Murray 2019; González-Uribe and Hmaddi 2023).

A growing body of evidence shows that accelerators improve average startup performance (Bone et al. 2019; González-Uribe and Hmaddi 2023). Yet they do more than raise the mean: accelerators amplify selection, helping strong startups scale while pushing weaker ones to fail faster (González-Uribe and Reyes 2020; Yu 2019).

Despite this progress, little is known about accelerators' impact on startup employees. While designed for founders, accelerators also shape the work environment of entire teams through goal-setting, mentorship, and structured accountability. These features may foster experiential learning and build higher-order capabilities—decision-making, collaboration, and adaptive problem-solving—critical for managerial success (Hansen et al. 2021). In contexts where shortages of managerial talent constrain firm growth, especially in developing economies, such skill formation may give employees valuable outside options (Hjort, Malmberg, and Schoellman 2022). Accelerators could thus generate important employee-level spillovers, acting as springboards into managerial careers. However, these benefits may come at a cost if key talent exits the startup (Choi, Goldschlag, Haltiwanger, and Kim 2023).

Still, employee benefits are not assured. Accelerators may heighten job instability by compressing timelines and exposing startups to intense performance pressure, increasing the risk of premature unemployment. Their emphasis on short-term milestones and investor pitches may also misalign incentives, diverting attention from long-run capability building—a form of the multitasking problem (Holmström and Milgrom 1991). Whether accelerators ultimately provide net career benefits for employees remains an open empirical question.

This study addresses that question. We confront two key challenges: (1) tracking detailed employee outcomes—wages, mobility, and employment spells—and (2) addressing the non-random selection of startups into accelerators, which tend to admit high-potential firms with strong teams. We overcome these issues using two complementary empirical strategies, described in the next sections.

2. Empirical Strategy

2.1. Cross-Program Analysis

We assemble a sample of companies that participated in accelerator programs, their employees, and a control group of comparable workers in non-accelerated startups. We focus on accelerator programs across the Americas to encompass both advanced economies (the United States and Canada) and emerging ecosystems in Latin America and the Caribbean (LAC). Data sources—originally accessed in October 2024—include Crunchbase, LinkedIn, and the ONET dataset from the U.S. Department of Labor. This section outlines the data construction and matching procedure, and summarizes the sample composition; full details are provided in the Online Appendix.

2.1.1. Cross-Program Sample Construction and Sample Composition

We begin by downloading information on all startups in North America (NA) and LAC and their acceleration participation from Crunchbase. From this set, we exclude accelerators with fewer than 20 investments, and focus on those targeting the NA and LAC innovation ecosystems. We further restrict the data to each startup's first accelerator participation during 2005–2019 to avoid Covid-related distortions, and require startups to be no more than five years old at entry. We classify accelerators according to whether they provide funding, using generative AI tools as detailed in Appendix F.

We implement firm-level stratified matching protocols to assemble a group of control companies. For each accelerated company in NA, we identify 5–6 control startups that never participate in an accelerator program during the sample period, matched on: (1) country and state, (2) founding year, and (3) industry, as classified by Crunchbase (with at least half of the claimed industries overlapping if multiple are listed). For each accelerated company in LAC, we identify 20 control startups matched on: (1) country, (2) founding year, and (3) industry similarity (maximal overlap).[2]

[2] In robustness checks we show results are the same if we use 5-6 control firms for LAC businesses. We used a higher control to treatment ratio for LAC, given the lower propensity of LAC employees to actively use LinkedIn.

Next, we collect LinkedIn profiles of all individuals employed at accelerated and control startups. Founders are included in the sample of employees except in exercises where we explicitly exclude them. Identifying founders is not always straightforward, as some do not clearly state their founding role in job descriptions; where possible, we triangulate information from LinkedIn and Crunchbase to classify them. Employees are divided into two groups: "incumbents," who are employed at the time of the acceleration event, and "new hires," who join afterwards. For control firms, we use the acceleration date of their matched treated firm to make this distinction. We exclude "former employees" who left the startup before the acceleration event, as well as firms with more than 500 incumbents or disproportionate service-sector employment (e.g., Rappi drivers), based on manual validation of LinkedIn job titles.

We then use propensity score matching (PSM) to pair employees of treated startups with employees of the matched control firms. We estimate a Probit model predicting the likelihood of working at an accelerated startup using rich pre-acceleration characteristics from LinkedIn, including education, age, average job description length, prior roles and employers, total work experience, time since labor market entry, employment rate since entry, and average tenure per job and per firm. We further require matched employee controls to (i) work at one of the startups matched firms, (ii) be of the same gender, and (iii) have joined in the same year as the treated employee. Each treated employee is matched to all control employees within a propensity score caliper of 0.05, achieving a 90% match rate.

The resulting set of treated employees and their matched controls constitute our "baseline sample" throughout the analysis. We refer to the matching group of control startups for a given treated startup as its "treated firm group"; similarly, we refer to the set of control employees matched to a treated employee as the "treated individual group."

Having assembled the baseline sample, we then select a random subsample of employee LinkedIn profiles that we match to the ONET database, due to the high computational demands of the matching process. ONET provides standardized occupational descriptors for nearly 1,000 roles in the U.S. economy, including detailed information on required skills and tasks. To ensure geographic diversity, we oversample companies from LAC relative to those from NA.

We use a commercially available system commissioned by the U.S. Department of Labor to match text to 2023 ONET codes using a weighted algorithm. For each job spell in the LinkedIn resumes, we extract job titles and descriptions and translate non-English text using large language models (GPT-4o-mini). The system tokenizes the text, matches it to a curated occupation-specific database, and calculates relevance scores based on linguistic distance between the job spell and ONET occupation descriptions. Based on the scores, we construct a detailed occupational representation for each job spell using all matched ONET occupations above a relevance threshold (match score $\geqslant 20$). To reduce noise, we discard occupations in the bottom 10% of scores. By incorporating all qualifying matches and their associated probabilities, we better capture the complexity of unstructured job histories and avoid the bias introduced by assigning a single occupation per job—a limitation of many commercial and survey datasets. [3] Appendix C provides examples illustrating this advantage.

Table 1 summarises the sample composition, with Panel A presenting the baseline sample and Panel B focusing on the ONET sample. The baseline sample comprises 107,092 incumbent employees, of which 6,401 (3,509 in NA and 2,892 in LAC) worked for 1,346 (957 in NA and 389 in LAC) accelerated startups and 100,691 (6,284 in NA and 94,407 in LAC) worked in 3,747 control startups (818 in NA and 2,929 in LAC).

Panel A in Table 1 shows that the average accelerator was founded in 2011 and had a portfolio of 403 companies as of February 2024. Approximately 60% of accelerators provide funding. The typical startup was founded in 2014 and entered acceleration two years later, in 2016. At the time of acceleration, the average incumbent employee was 31 years old, had held 2.7 prior jobs across 2.3 companies, accumulated 76 months of work experience, and had been in the labor market for 87 months—indicating a prior employability rate of nearly 90%. 60% of incumbent employees are male, which contrasts with the high male representation among founders and startup teams (Ewens, 2023).

Appendix B describes geographic and sector distribution of the baseline sample. US accounts for 78% of accelerators and 26% of firms in the full sample. The next three countries by accelerator (firm) concentration are: Canada with 6% of accelerators (Brazil with 26% of firms), Brazil with 6% (Mexico with 22%), and Mexico with 3% (Chile with 11%). This pattern reflects the relative prominence of startup ecosystems in LAC (Rudolph, Miguel, and Gonzalez-Uribe, 2023). Regarding industry composition, the top five sectors in the full sample are software (12%), information technology (7%), internet services (6%), data analytics (5%), and commerce and shopping (4%), consistent with these programs' focus on software and ICT-driven businesses.

The ONET sample includes 10,919 incumbent employees, of which 946 (266 in NA and 680 in LAC) worked for 316 (113 in NA and 203 in LAC) accelerated startups and 9,973 (1,122 in NA and 8,851 in LAC) worked in 774 control startups (180 in NA and 594 in LAC). A comparison between Panels A and B of Table 1 shows that the ONET sample has a similar composition as the baseline sample. Appendix B shows the geographic and sector coverage of the ONET sample is also similar to the baseline sample.

2.1.2. Cross-Program Outcome Variables

We track employee career trajectories, identifying employment spells up to the end of our sample period in October 2024. The data are organized in acceleration-event time. For control employees we set the acceleration date to that of the company of their accelerated counterpart. We define post-acceleration job spells as any positions held after acceleration and after the end of the

[3] Commercial datasets like Revelio and census data like IPUMS assign single occupational code (O*NET/SOC) to each job, as used in the literature as Cen et al. (2025), Autor et al. (2003).

incumbent role. Positions that occur after acceleration but before the incumbent role ends are excluded from the analysis; unreported results show that including them yields similar findings. Summary statistics for all variables are provided in Table 2, with observations measured at the employee level.

Employability. Our first outcome variable captures the share of time each individual was employed—either at the same firm or elsewhere—from the date of acceleration until the end of the sample period; following a similar approach to Agrawal and Tambe (2016). This measure, which we label *Employability*, captures the extent to which individuals remain attached to the labor market after acceleration. Once employment spells are identified, we characterize each employment spell along three dimensions: (1) managerial status, (2) skill and task requirements, and (3) expected wage levels.

Managerial Role Status. To classify jobs as representing managerial positions, we use two complementary approaches. Our first approach applies natural language processing (NLP) to the textual data in LinkedIn resumes, following recent finance research that uses similar methods to extract economic insights (for example, Hoberg and Phillips (2025) use NLP to define firm scope). We train a model to construct dictionaries for entrepreneurial and professional manager positions, and then compute job-level scores that reflect alignment with these dictionaries, as explained in detail in Appendix B. We define *Manager Word Index, Professional Manager Word Index* and *Entrepreneur Word Index*; which are standardized to have a mean of zero and a standard deviation of one. Higher values indicate stronger alignment of job positions with managerial roles.

The second approach classifies jobs as managerial if ONET assigns the position to its Management and Entrepreneurship career cluster. Since each LinkedIn job entry in our dataset may match multiple ONET occupations, each with a match score, we construct a *Manager Index*: the match score–weighted share of matched occupations in the cluster.

Job Required Skills and Tasks. To characterize jobs based on skill and task requirements, we use a multidimensional taxonomy rooted in the ONET database and informed by prior frameworks from Hansen et al. (2021), Deming (2017), and Autor, Levy, and Murnane (2003). A summary of the key variables is provided below, with full construction details in Appendix D.

We begin by assessing how the skills required for each job, as classified by ONET, align with the five core managerial skill clusters identified by Hansen et al. (2021). These clusters span cognitive, operational, and interpersonal domains that are fundamental to managerial roles.[4] The cognitive domain includes two indexes: (1) *Information Skills*, related to information processing; and (2) *Monitoring of Performance*, reflecting managerial oversight, strategic planning, and system evaluation. The operational domain includes the *Financial and Material Resource Management* index that reflects

[4] Hansen et al. (2021) identify the six executive skill clusters by analyzing a large corpus of C-suite job descriptions and linking them to occupational skill requirements from ONET. Their framework highlights the increasing importance of interpersonal and social capabilities in executive roles, particularly within information-intensive organizations.

budgeting, procurement, expenditure control, and cost estimation.[5] The interpersonal domain includes two indexes: (1) *Human Resources* encompassing communication, team-building, personnel development, and supervisory responsibilities and (2) *Social Skills* that include coordination, instructing, negotiation, persuasion, service orientation, and social perceptiveness.

We also construct a composite *Soft Skills* index that combines the social and thinking skills measures, as both are defined by ONET as essential interpersonal and cognitive abilities for effective workplace performance. The *Thinking Skills* component captures basic and cross-fucntional skills reflecting the ability to absorb information, engage in complex reasoning, and make sound decisions.

We also we evaluate the degree to which the jobs require technical skills given our focus on technology-oriented firms. The *Technical Skills* index is defined by ONET as the capacities used to design, operate, maintain, and troubleshooting machines or technological systems, including cross-functional skills, reflecting applied competencies relevant across a range of technical occupations.

Finally, following Deming (2017) and Autor, Levy, and Murnane (2003), we include an index of *Routine Task* intensity. This index captures the degree to which an occupation involves repetitive and automatable activities by averaging two ONET work context variables: (1) the degree of automation in the job and (2) the importance of repeating the same tasks. Both are categorized by ONET as structural job characteristics and serve as a proxy for routineness and susceptibility to standardization or technological substitution.

To estimate these indices for a given LinkedIn job entry, we use ONET's descriptions of skill and task requirements for the multiple occupations matched to that job, weighted by their respective match scores. Each skill (tasks) index represents the match score–weighted proportion of linked occupations that require those specific skills (tasks). In the regressions, we use log transformations of the skill indices.

Wages. Finally, we characterize occupations by their expected wage levels, estimated using data from the U.S. Occupational Employment and Wage Statistics (OEWS) program, as explained in detail in Appendix E.

The OEWS offers publicly maintained, nationally representative, and regionally disaggregated annual wage data and employment estimates for over 800 occupations. These estimates are derived from a large-scale survey of non-farm establishments across U.S. states, is widely used in economics research (e.g., Haltiwanger et al., 2024; Ilin and Terry, 2021; Agrawal and Tambe, 2016), avoiding common issues of sample selection and measurement error firm wage data derived from salary data from online job postings (Batra, Michaud, and Mongey, 2023). Each employee's job location is identified using the state where their employer is headquartered, as reported on LinkedIn. For

[5] We exclude administrative task variables from the broader functional skill category used by Hansen et al. (2021), as these are drawn from ONET's occupation-specific "Tasks" section, which lacks comparability across roles. By focusing on generalizable operational competencies, this measure captures the execution-oriented capabilities essential for leading complex and resource-constrained ventures.

employees at LAC-based firms, we assign the U.S. national average wage for the matched occupation and year. For each job spell each year, we estimate salary as the match score–weighted average of OEWS mean annual wages across all matched occupations. If an individual held multiple jobs in a year, we take the simple average of the estimated wages. In the regressions, we use log transformations of the wages.

2.1.3. Cross-Program Empirical Strategy

We compare post-acceleration changes in career trajectories of employees from accelerated startups with those of matched employees from control startups. Our analysis begins with event-time difference-in-differences regressions that track changes in employability and job characteristics after acceleration, relative to the period when each employee was still working at their original (accelerated or control) firm. We run the following type of regressions

$$Y_j = \alpha + \gamma_{\hat{\imath}} + \rho Acceleration_i + \theta X_j + \varepsilon_j \qquad (1),$$

where $Acceleration_i$ equals one if the original firm i participated in an accelerator, 0 if it is a control firm. Y_j represents the change in the average employability or characteristics of the jobs held by incumbent employee j after potential acceleration, compared to the characteristic of the jobs they held in the original firm i. If the individual does not change jobs after the acceleration date, then Y_j is set to zero, except for regressions involving wages where values can change over time even if the individual holds the same job. If the individual held multiple jobs after the acceleration date, then the variable compares the simple average characteristics of new jobs held after acceleration to the job held at the acceleration date.

We include treated individual group FE (denoted as $\gamma_{\hat{\imath}}$) to ensure that comparisons are made within the matched group of incumbent employees and companies. The vector X_j includes individual-level control variables to account for different sources of productivity differences such as age and education and acceleration year fixed effects. Standard errors are clustered at the individual group level to account for the multiple observations per treated employee and the variation driven by the acceleration assignment. Our primary coefficient of interest is ρ, which captures the average post-acceleration difference in employability and job characteristics between incumbent employees of accelerated startups and the matched control employees of the control startups.

Next, we use stacked difference-in-difference techniques to address any issues arising in traditional event-time regressions where treatment effects may be heterogeneous across units and time periods. We restrict this analysis to the wage data that includes variation across positions and time periods. Specifically, we estimate the "trimmed aggregate ATT (average treatment on the treated)" on wages within four years of participation; see Wing, Freedman and Hollingsworth (2024). We trim the data to create a balanced sample over a fixed event time window of 7 years around the acceleration time, 3 years before and 4 years after. In the estimation, we apply the corrective sample weights to eliminate potential biases from different implicit weights to treatment and control trends, and we focus on the weights that correspond to the trimmed aggregate ATT. We estimate the following type of regression,

$$Y_{jt} = \alpha + \gamma_{\hat{\imath}} + \sum_{\substack{h=-3 \\ h\neq -1}}^{h=4} \beta_h (Acceleration_i \times D_h) + \sum_{\substack{h=-3 \\ h\neq -1}}^{h=4} \mu_h D_h + \theta X_j + \varepsilon_{jt} \ (2)$$

Where Y_{jt} represents the log wage of employee j at time *t*. D_h are time event dummies around the acceleration event. All other variables are as defined in Equation (1). The coefficients of interest are the β_h capturing the average difference in wages within h years of acceleration, for incumbent employees of the accelerated firm and relative to the matched employees in the control firms.

2.2. The ValleE Quasi-experimental Analysis

ValleE is a local ecosystem business accelerator that was launched during 2015 after an intense local advertising campaign using social media and radio in the city of Cali, the third most important city in Colombia in terms of population.[6] The accelerator is operated by the city Chamber of Commerce, and as is common among ecosystem accelerators, ValleE's main objective is to encourage local growth by identifying and boosting high-growth entrepreneurs (cf. Clarysse, Wright, and Van Hove, 2015).[7] Examples of ValleE participants include "Luces projects" a company offering residential wind energy solutions and "Contratan.do," an information and communication technologies business-to-business hiring platform in Latin America.

Like other business accelerators worldwide, ValleE is a fixed-term, cohort-based program that selects participants based on the relative quality of applications submitted online, as evaluated by a panel of judges. As explained in more detail below, participants are selected based on *average scores* from partially overlapping 3-judge panels in order to satisfy pre-determined budget and space restrictions, as well as judges' time constraints. Any person proposing the creation of a new business or the scale of an existing young (0-3 years) business located in the region is in principle eligible for the program. However, the program focuses on high-growth entrepreneurs, and many applicants are *de facto* incompatible and thus rejected (as explained in more detail in Section 3.2).

Like traditional business accelerators, ValleE provides participants with a variety services, including standardized grouped business training, one-to-one customized advice, and increased visibility. It offers no cash as is common among the subset of ecosystem accelerators worldwide, cf. Clarysse, Wright, and Van Hove, 2015).[8] The perception is that for many young businesses the foremost constraint to growth is gaps in managerial and entrepreneurial skills. The business training sessions are

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[6] Ecosystem business accelerators are popular in low and lower-middle income countries: 37.9% of the ecosystem accelerators in the Entrepreneurship Database at Emory University are located in Africa (17.9%), Latin America, (10.3%), and India (10.3%).
[7] The top two impact objectives among ecosystem accelerators are employment generation (35%) and community development (30%). Source: The Entrepreneurship Database at Emory University.
[8] Circa 55% of the ecosystem accelerators in the Entrepreneurship Database at Emory University provide no seed capital. Source: https://www.galidata.org/accelerators/.

highly structured and simultaneously attended by all participants in the offices of the Chamber of Commerce. They consist of roughly 8 weekly hours of standardized content (totaling 100 hours over a space of three months) delivered by hired local and national experts. Bootcamps combine lecture-based conceptual sessions together with case-based sessions discussing real-life practical examples, and cover the topics of business modelling, early-stage financing, market validation, prototyping, accounting, and pitching. Two types of one-to-one customized advice sessions are also provided. First, bi-monthly strategic sessions with senior industry advisors—renowned CEOs and Chamber of Commerce managers—who provide high-level guidance and valuable introductions. Second, weekly hands-on sessions with program coordinators, who are more junior and focus on supporting entrepreneurs with day-to-day operations rather than long-term growth strategy. Finally, ValleE provides several opportunities to increase visibility: participants are showcased on the Chamber of Commerce's website and monthly publications, as well as exhibited at different events. At the end of their term, participating businesses "graduate" through a "demo day" competition (i.e., a formal presentation of the companies to potential investors).

Selection into ValleE is a four-part process. First, aspiring participants submit an online application that requests information about the entrepreneurs and their detailed business plans. Next, the accelerator filters applicants to exclude projects that are deemed to have no high-growth potential (e.g., taxi drivers, shopkeepers). Filtered applications are then randomly assigned to three judges that individually score the application. The total number of judges is 50, and thus judges only partially overlap across applicants. The judges evaluate the applications according to five criteria: (i) clarity of the business model proposal, (ii) innovation, (iii) scalability, (iv) potential profitability, and (v) entrepreneurial team. Finally, the staff at the accelerator makes the final decision by picking the top 35 applicants based on average scores. It is impossible for judges and applicants to manipulate the ranking process. Judges are unaware of the weight of each criteria in the final score; they independently score projects, are not aware of the identity of the other judges in the panel, and no judge sees all applications. Applicants do not know who their judges are, nor do they know their position in the ranking. The capacity threshold of 35 participants was determined prior to the launch of the program and is due to budget and space limits.

In the first cohort of ValleE (our sample source), there were 255 applicants who submitted a complete application online. Of these, only 135 businesses were deemed to have "high potential for growth" and therefore correspond to our analysis sample. The maximum length allowed for business plans submitted with the applications and read by the judges was 2 pages. The average number of projects scored by any given judge was 8, and the minimum (maximum) was 5 (14). The program picked the judges based on the relevance of their backgrounds to help sort applicants. Judges were not compensated for evaluating applicants, and their identities are private to us. The pool of 50 judges included individuals with substantial experience in business and entrepreneurship, such as C-level executives in local businesses, independent business consultants, and industry experts, as well as managers in entrepreneurship departments in development agencies and two staff members.

Compliance with the selection rule was exact: the top 35 applicants (based on judges' average scores) were selected, and all selected applicants participated (see Figure 2, Panel A). Gonzalez-Uribe and Reyes (2020) show statistically significant differences at the time of application between accelerated and nonaccelerated applicants: participants have bigger founding teams, are slightly more educated, have more sectorial experience, and are more likely to be serial entrepreneurs. The economic significance of most of these differences is, however, small, in part due to the filter applied by the program to remove the non-transformational entrepreneurs from the sample.

While the accelerator provided uniform criteria by which a judge should score proposals, Gonzalez-Uribe and Reyes (2020) show there was substantial variation in the interpretation of these criteria across judges in the first cohort of ValleE. This heterogeneity in "scoring generosity" (SG) is reminiscent of the systematic differences in "judge leniency" reported in other settings, such as in bankruptcy courts in the U.S. (e.g., Dobbie and Song, 2015). Below we discuss how we use this heterogeneity in SG across the randomly assigned judges to estimate the causal impact of participation in the accelerator on workers' careers.

2.2.1. The ValleE Quasi-experimental Analysis Sample

ValleE provided us with all the application data, including application scores by each judge and final selection decisions, for the program's first cohort. All selected applicants in this cohort participated in the accelerator for three months, during May, June, and July of 2015. Our initial sample consists of 135 projects (35 participants and 100 nonparticipants) that applied to the accelerator in March of 2015 and were deemed to have high-growth potential by the staff. We refer to this sample as the ValleE sample throughout. The size of the ValleE sample is standard for business accelerator programs that tend to take groups of 10-20 companies per cohort and exceeds that of similar papers exploring the impact of business training (e.g., 14 participants and 14 control plants: Bloom et al., 2013; 47 participants and 66 control business owners: Mano et al., 2012). Gonzalez-Uribe and Reyes (2020) show that the sample is comparable to the average applicant of ecosystem accelerators worldwide, based on information from the Entrepreneurship Database (ED) program at Emory University, is also comparable to that used in prior work on ecosystem business accelerators, and in prior work on early-stage ventures.

The main innovation in this paper relative to Gonzalez-Uribe and Reyes (2020) is to shift focus from the effects of ValleE on business to startup employees and founders. For that purpose, we assemble novel data by combining the ValleE data to social security administrative data from the *Planilla Integrada de Liquidacion de Aportes (PILA)*, the official registry and payment system of payroll taxes and social security contributions for formal employers and workers in Colombia. The PILA contains detailed information about all formal workers including their reported wage. We only observe formal employment because only formal workers are registered in the PILA, so we observe the creation of formal employment.

PILA data have monthly frequency, given that payroll taxes and social security contributions are paid at this frequency. For this study, we have access to data from January 2012 to June 2022. We match our ValleE sample PILA using company name and registration number. For each matched company, we then assemble data on all employees that were part of its payroll for at least one month from January 2015 to June 2022. For each of these matched ValleE employees, we then track their formal career history in PILA during the entire sample period of January 2012 to June 2022. The final sample consists of 47,381 employee-month non-missing observations, covering 682 ValleE matched employees that worked for at least one month in ValleE firms; 127 in 16 (46% out of 35) beneficiary ValleE companies and 555 in 51 (51% out of 100) rejected applicants.

Table 3 compares the businesses in the ValleE sample the PILA sub-sample. The PILA sub-sample covers individuals in relatively more established businesses relative to those at the ideation stage (64% relative to 47%), and that are larger in terms of their revenues at the time of application (41.84 M COP relative to 25.80M). The relatively higher maturity and size of the businesses covered by the PILA sample aligns with the idea that businesses are more likely to employee formal employees as they become larger and are more mature, given the large costs associated with formal employment in Colombia (Bernal, Eslava, Melendez and Pinzon, 2017).

As in the cross-program analysis, we distinguish between two types of individuals in the PILA sample. “Incumbent” employees are those that were working in their respective ValleE firm during the acceleration period: any month of 2015. The other type of employees are “New hires” that first joined their respective ValleE firm after 2015. We do not include in the sample former ValleE employees that separated from their jobs at ValleE firms before application to the accelerator (and never returned). There are 16,278 employee-month non-missing observations covering 195 incumbent employees and 32,881 employee-month non-missing observations covering 466 new hires. Note that because we cover the entire formal employment careers for the individuals after they work in a VallE firm, the sample covers three different types of firms: ValleE beneficiaries, ValleE rejected applicants, and Non-ValleE firms. The observations include time periods during which employees are working for their respective ValleE firm, and time periods during which they are formally hired in a Non-ValleE firm. Not all employees change jobs, and conditional on changing jobs, not all employees return to formal employment. This means that the data combines individuals that have stable jobs in the formal market, with other individuals that change firms, and with other individuals that transition in and out of the formal job market. This is not particular to our data: transitions in an out of the formal market are common among workers in young firms in LAC, and particularly Colombia (Bernal, Eslava, Melendez and Pinzon, 2017).

A comparison between Tables 1 and 3 show that the sample are similar: the average firm in the PILA sample is founded in 2013 and entered the ValleE program in 2015. At the time of acceleration, the average incumbent employee was 30 years old, had accumulated 60 months of work experience, having been in the formal labor market for 82% of the months since her first employment spell. 68% of incumbent employees are male.

For the regression analysis, we structure the dataset at the employee–month level. Each observation corresponds to an individual linked to a specific ValleE firm where they were formally employed for at least one month between January 2012 and June 2022. Table 4 reports summary statistics for the outcome variables, including *Log(Wage)*—the natural logarithm of wages. This variable is defined only for months in which the individual is formally employed and is missing otherwise.

We also estimate regressions using a collapsed version of the dataset at the employee level, defining variables that capture average career outcomes after acceleration. Since PILA lacks detailed occupational information, we proxy for managerial positions using a *Manager* dummy that equals one for individuals whose wage after 2016 at some point exceeds 2.56 minimum salaries—the average wage for "directors and managers" according to Colombia's Household Survey (Gran Encuesta Integrada de Hogares) from DANE (National Administrative Department of Statistics).

Finally, we examine *Employability* in the formal sector, measured as the share of months after potential acceleration in which an individual remains formally employed until the end of the sample period. We also construct a dummy variable *Leaves Formal Employment* indicating whether an employee leaves the firm after acceleration and does not take up any subsequent formal employment elsewhere.

2.2.2. The ValleE Quasi-experimental Analysis Empirical Strategy

We begin by comparing formal labour outcomes of the incumbent workers before and after the company participates (or not) in the accelerator and while they are still employed in the accelerator (or not). We estimate the following equation:

$$Y_{jt} = \alpha + \rho_1 Acceleration_i \times Post_t + X_{jt} + \vartheta_j + \mu_t + \varepsilon_{jt} \qquad (3)$$

where $Acceleration_i$ equals one if the original firm *i* participated in an accelerator, 0 if it is a control firm that applied to ValleE and was rejected. The variable $Post_t$ indicates the periods after acceleration (2016 onwards). Y_{jt} is the log wage for employee j at time t. To pin down the macro changes in wages, we estimate equation (3) using observations of both incumbent employees and new hires (indicated by a dummy $New\ Hire_j$) allowing the effects of acceleration to vary parametrically across types of employees using the interactions $Acceleration_j \times Post_t \times New\ Hire_j$ and $New\ Hire_j \times Post_t$. To simplify exposition, we do not report the coefficients of these interactions; full regression results are available upon request. We include employee fixed effects (ϑ_j) to control for fixed differences across employees and month fixed effects to capture aggregate macro changes affecting wages (μ_t). The vector X_{jt} captures additional controls including time-varying controls at the individual level such as age, experience, square of experience, and share of potential experience in the formal labor market. We report heteroscedasticity robust standard errors to account for any serial correlation over time for a given employee.

For variables defined at the employee (rather than employee-by-month) level including Manager and Employability, we estimate a simplified cross-sectional version of Equation (3) without individual or time fixed effects. In these specifications, the vector X_{jt} includes employee-level characteristics measured at the time of acceleration: age, experience, experience squared, share of potential experience in the labor market, an indicator for holding a managerial position prior to acceleration, gender, and adjusted score.

To identify the casual effects of acceleration, we follow Gonzalez-Uribe and Reyes (2020) and use an instrumental variables (IV) approach that instruments the variable $Acceleration_j$ with SG—the scoring generosity of the judges that scored the application of the ValleE company linked to the employee. This variable equals the average fixed effects of the application's judges, where we estimate the fixed effects by regressing scores against applicant fixed effects and judge's fixed effects. Gonzalez-Uribe and Reyes (2020) show that the judges' fixed effects are jointly significant, indicating systematic differences in the propensity of judges to allocate high very low scores. The authors run various robustness checks to show the joint significance is not an artifact of the small sample size.

The first stage associated with equation (3) includes the regression:

$$Acceleration_j \times Post_t = \alpha + \gamma_1 SG_j \times Post_t + X_{jt} + \vartheta_j + \mu_t + \varepsilon_{jt} \quad (4)$$ [9]

We present results using heteroscedasticity robust standard errors. We verify in unreported regressions that results are similar using bootstrap standard errors clustered at same to account for the fact that the adjusted score is a generated regressor (Wooldridge, 2002; Young, 2018). For variables at the employee level, we estimate a version of equation (4) without individual or time FE and excluding the interactions of the outcome and main dependent variable with the post acceleration indicator.

Using SG to instrument for acceleration yields a consistent two-stage least squares estimates of ρ_1 as the number of applicants grows to infinity but is potentially biased in finite samples. This bias is the result of the mechanical correlations between an applicant's own outcomes and the estimation of that applicant's judge fixed effects. Following the parallel literature exploiting judge leniency (Kling, 2006, and related papers thereafter), we address the own observation problem by using different leave-one-out measures of SG as explained by Gonzalez-Uribe and Reyes (2020). In unreported results, we verify that results are similar using the different SG measures as instrument.

The IV estimates of ρ_1 capture the local average treatment (LATE) of acceleration for the individuals in ValleE firms whose participation is altered by SG. These firms include both the type 1 and type 2 selection mistakes by the program—i.e., applicants that in spite of their potential were,

[9] The first stage also includes the regression $Acceleration_j \times Post_t \times New\ Hire_j = \alpha + \gamma_1 SG_j \times Post_t \times New\ Hire_j + X_{jt} + \vartheta_j + \mu_t + \varepsilon_{jt}$. We do not report results to ease exposition.

respectively, mistakenly rejected/accepted due to the generosity/strictness of their judges. Figure 2 shows that in the ValleE sample, the frequencies of Type 1 and Type 2 mistakes are similar (by construction, these two types of mistakes are closely linked). In contrast, in the PILA sample, Type 2 mistakes are very rare—most mistakes are Type 1. This asymmetry arises because low-scoring firms are less likely to hire formal employees and therefore drop out of the PILA sample. As shown in Figure 2, firms deemed to have a higher potential by judges (as measured by adjusted scores) appear to indeed be more likely to employ at least one employee in the formal labor market. We discuss the implications on interpretation of the PILA sample composition in the next section.

Three conditions must hold to interpret these estimates as the average (local) causal impact of acceleration: (1) SG is associated with participation in the accelerator, (2) SG only impacts venture outcomes through the probability of participating in the accelerator (i.e., the "exclusion restriction"), and (3) the impact of SG on the probability of acceleration is monotonic across applicants.

Gonzalez-Uribe and Reyes (2020) show ample evidence of the three identification assumptions for the ValleE sample. We extend their analysis to evaluate the evidence on the identification assumptions for the PILA sample. In terms of the first stage, Panel A in Figure 3 shows evidence of a positive association holding constant applicant quality (as measured by adjusted score) for both the ValleE and PILA samples. To produce Panel A in Figure 3, we classify applicants into quartiles of SG, and estimate for each quartile the distribution of acceleration over adjusted scores.[10] The figure shows that for a given adjusted score, the acceleration probability is always highest (lowest) for projects assigned to the top (bottom) quartile of SG. Confirming this positive association, in columns (1) and (2) of Table 5, we show that a simple cross-sectional regression estimating the probability of acceleration of a given applicant using SG as main explanatory variable and controlling for the adjusted score estimates a positive and significant coefficient for the SG with an F-test for the excluded instrument of 49.74 in the ValleE sample, and of 15.98 for the PILA sample (standard errors are robust). As expected, the power of the instrument is higher in the ValleE sample relative to the PILA sample given the differences in sample size at the firm level. However, the instrument still appears to have enough statistical power for the analysis. In columns 3 and 4, we examine how the strength of the instrument increases as we borrow statistical power from the observations at the employee level and employee-month level, respectively. We report the corresponding F-tests of the excluded instrument and show that the instruments are not weak (Stock and Yogo, 2005).

In terms of the exclusion restriction, the random assignment of judges to applicants ensures conditional independence in the ValleE sample. We check that no differences continue to exist between applicants with different scoring generosities for the PILA sample in the Online Appendix. Any

[10] Relative to a mean average score of 0.7, the breakpoints for the SG quartiles are -0.03, 0.001, and 0.05, and the max (min) SG is 0.21(-0.13). These numbers imply that projects classified in the top (bottom) quartile of judge generosity received between 0.05 and 0.21 (0.13 and 0.003) additional (fewer) points than their project fixed effects.

remaining concerns regarding the unintentional assignment of generous judges to high-quality firms are not consistent with the patterns shown in Panel B of Figure 3; i.e. projects with high adjusted scores do not systematically have higher average scores than expected. These concerns are also not consistent with the fact that observable characteristics are similar across applicants assigned to judge panels with low and high SG. Differences in the interaction between applicants and judges across applicants in different quartiles of SG are unlikely because only two of the 50 judges are ValleE staff members, the rest of the judges do not interact with participants as part of the program, and the judges' identities are not revealed to applicants throughout the process. Because applicants are not made aware of their scores, nor of the generosity of their judge panel, psychological reactions are also unlikely (e.g., feelings of grandeur or depression). Finally, Gonzalez-Uribe and Reyes (2020) show evidence that SG also does not measure differences in predicting ability across judges Ultimately, however, the assumption that SG only systematically affects applicants' performance through acceleration is fundamentally untestable, and our estimates should be interpreted with this identification assumption in mind.

3. Results

3.1. Baseline Results

Table 6 shows that business acceleration is strongly positively associated with incumbent workers' transitions into managerial roles, higher wages, and improved employability in both the cross-sectional analysis (Panel A) and the ValleE setting (Panel B).

Managerial roles. In Panel A, Column 1 shows that the increase in the probability of holding a managerial role—as measured by the change in the manager word index—is 5.2% higher for employees of accelerated startups relative to matched control employees. Unreported results indicate that this increase reflects both transitions into professional managerial roles and new firm creation by former employees. Column 2 shows similar effects using the ONET-based managerial index. Further unreported evidence indicates that these transitions begin within three years of acceleration, pointing to rapid and persistent shift in career paths. Column 3 shows similar effects using the ATT estimator.

Panel B shows comparable patterns in the ValleE setting. Column 1 shows that the likelihood of holding a formal managerial role increases 22% more for treated employees relative to control employees—controlling for incidence of managerial roles at acceleration. The estimated effects of business acceleration on transitions into managerial roles are sizeable, especially given how rare such transitions are in our data—for example, the average change in the manager word index for control employees is only 0.013. As shown in the final rows of each panel, the two empirical approaches yield similar economic magnitudes in relative terms: over a seven-year period, employees of accelerated startups are between 2 and 5 times more likely to move into managerial roles than matched non-accelerated employees.

Wages. Figure 6 presents unconditional event-time plots from the cross-sectional analysis, comparing expected wage trajectories for treated and control employees before and after acceleration. Panel A collapses the data to the employee–job-position level and plots average expected wages by event number, where positive (negative) values correspond to jobs after (before) acceleration. Panel B presents the same comparison over time, plotting average expected wages by event year. Both panels show a clear post-acceleration divergence in wages between the two groups.

Table 6, Panel A (Columns 4 and 5) confirms these patterns. Column 4 shows that business acceleration is associated with a 3.5% larger increase in expected wages relative to matched control employees. Figure 8 shows that this increase is not driven by continued employment in the accelerated firm but rather by transitions into other companies (see Panel B). Column 5 demonstrates that these results are robust to concerns about heterogeneous treatment effects in the simple event-time analysis of the cross-program analysis: stacked difference-in-differences ATTs yield similarly positive and statistically significant estimates. Figure 7 plots the corresponding stacked difference-in-difference estimates of the interacted event time dummies of Equation 2. Confirming the raw patterns and results from the simple event time analysis, Figure 7 shows that the increases in expected salaries occur rapidly, with positive and statistically significant coefficients from the third year after acceleration.

Panel B in Table 6 shows parallel results in the ValleE setting using administrative wage data. Column 3 documents an 11% post-acceleration increase in formal monthly wages for treated relative to control employees. This increase is visible in Figure 8 comparing average wages over time across treatment and control employees. Across both empirical approaches, the economic effects of business acceleration on wages are very similar. As shown in the bottom rows of each panel, wage increases for employees of accelerated startups are between 1.3 and 1.6 times higher than those of matched non-accelerated employees over a seven-year period.

Employability. The last columns of Panel A and B of Table 6 show that the positive managerial role and wage effects of acceleration do not come at the cost of reduced employability.

For the cross-sectional analysis, we begin by plotting employment probabilities for incumbent employees in treated and control groups over time. Panel C of Figure 6 shows that employment probabilities remain similar across groups for the first three years after acceleration. By year four, however, a slight divergence emerges, with treated employees exhibiting marginally higher employability (94% versus 92%). This overall similarity conceals important differences in mobility. Panel D of Figure 6 shows that treated employees are more likely to leave their incumbent firms. Taken together, these findings suggest that acceleration increases employee mobility but does not reduce long-term employability in economically meaningful ways. The regression evidence in column 6 of Panel A of Table 6 corroborates the unconditional patterns hold after we restrict the comparison to matched employees of matched companies and further control for demographic characteristics: treated employees spend 1.4% more time employed after acceleration than control employees.

In the ValleE analysis, Column 5 in panel B shows that accelerated employees have higher employability after acceleration, consistent with Figure 8. This employability difference is explained by longer time spent employed, not by a higher probability of remaining in the formal labor force: employees of ValleE participants are just as likely as controls to exit formal employment after acceleration (see Figure 9).

3.2. Heterogeneity and Sample Cuts

Table 7 presents results from the cross-program analysis for different subsamples and for the three main outcome variables change in manager index (Panel A), change in log wages (Panel B) and employability (Panel C).

There are three main findings. First, the effects of acceleration are not driven by natural within-firm wage progression following acceleration: the positive effects on managerial roles, expected wages and employability are driven by the sample to employees who leave their original company (column 10). These results rule out mechanical explanations for the findings associated with within-firm performance improvements such as internal progression through rent-sharing (Van Reenen 1996; Howell and Brown 2023).

Second, the effects are similar across accelerator programs that offer or not seed capital, as show in Columns 1 and 2. This result reinforces hat results are not driven by more mechanical explaantions, like potential alleviation of financial constraints through seed financing, which leads to rent-sharing and internal progression.

Third, the effects are robust to restrciring the sample to settings where skill-building is most valuable: among workers without prior business training (column 9), in underserved regions such as LAC (column 7), and in ventures supported by more experienced accelerators (column 3).

3.3. Evidence of Treatment Effects: the ValleE Quasi-experimental Results

The even columns in Panel B of Table 6 show results from the IV exercise and provide evidence of causal effects of acceleration effects on incumbent workers' managerial roles and wages, without facing higher employment risks compared to their counterparts in non-accelerated firms.

Column 3 shows that monthly wages increase on average by 13.6% after acceleration—about 1.5 times the increase observed for control employees. In monetary terms, this corresponds to an annual wage gain of roughly 2.0 million COP (about USD 500). To put this into perspective within the Colombian context, the rise in formal earnings represents about 10% of the minimum wage—equivalent to approximately USD 600 per year, or 7.5% of Colombia's per capita GDP.

Column 2 links the observed wage growth to transitions into managerial positions. The economic magnitude is comparable to that in the cross-program analysis: the quasi-experimental estimates indicate that employees of accelerated firms are about four times more likely to hold a managerial role, compared to a threefold increase in the cross-program results.

Column 6 shows no evidence of causal effects on employability risk. We interpret the findings as showing meaningful acceelratio effects on managerial trasjitions and wages that are not at the expenses of lower employability.

The IV regressions estimate the local average treatment effect (cf., Imbens and Angrist, 1994) for the applicants at the margin of selection, which include type 1 and type 2 selection mistakes. Intuitively, the IV estimate averages out two types of performance comparisons. First, the performance difference between high-potential participants and similar-potential type 1 applicants that were mistakenly rejected. Second, the performance difference between low-potential rejected applicants and similar-potential type 2 participants that were mistakenly accepted. A natural question asks which performance comparison drives the IV results. Gonzalez-Uribe and Reyes (2020) show that in the ValleE sample, the average positive effects of acceleration are driven by the type 1 errors that were mistakenly rejected. Because we have fewer type 2 errors in the PILA sample, the positive effects of acceleration we estimate on worker careers' are also driven from type 1 errors. However, this is not because there are no employee career effects for type 2 errors: we cannot test this in our sample because very few of these firms formally employ workers. The implication is that the evidence on casual effects is driven by employees working in accelerated firms that att he time of accelertaio were already high-potential.

We contrast the IV estimates with the naïve OLS estimates, which compare average career outcomes across employees of ValleE participants and nonparticipants. A comparison between estimates reveals a positive difference, although not statistically significant, between the IV and the OLS estimates for log wages and the manager dummy. This positive difference is consistent with the positive difference that Gonzalez-Uribe and Reyes (2020) document for outcomes at the business level in the ValleE sample for the period they cover (until 2018). These positive differences are unlikely to be driven by a weak instrument; Figure 3 and Table 5 shows compelling evidence of the strength of the instrument. Moreover, a comparison between the employability estimates reveals a negative rather than a positive difference between the IV and OLS. Instead, the positive differences are likely driven by a combination of two effects. First, the projects at the margin of acceptance due to the generosity of the judges are most sensitive to acceleration (cf. Card, 2001). Second, there is no standard positive bias in selection by ValleE because they do not account for the differences in generosity across judges; it is precisely the mistakes in selection that provide the variation to identify the causal effects of acceleration. Therefore no specific positive bias is expected when comparing the OLS and the IV estimates.

The evidence of treatment effects for the ValleE program does not mean that all business accelerators will necessarily have positive causal impacts on their participants' career trajectories. There is substantial heterogeneity among accelerators in both design and performance (Cohen et al., 2019). Nonetheless, several features of ValleE suggest that these findings are unlikely to be unique to this program. As discussed in Section 2, ValleE applicants closely resemble those of other accelerators in the cross-program analysis and those of ecosystem accelerators in particular. Likewise, ValleE mirrors average ecosystem accelerator on many dimensions. For instance, its location outside Colombia's capital mirrors a broader pattern—about 38% of ecosystem accelerators operate in less-developed regions. Similarly, 40% of such programs in the United States are located outside major innovation hubs such as Silicon Valley, Massachusetts, New York, and Washington, D.C., while most European ecosystem accelerators are also based outside capital cities. In terms of services, ValleE's offerings align closely with those of comparable programs worldwide (cf. Clarysse, Wright, and Van Hove, 2015).

However, ValleE also possesses some distinguishing features—most notably its access to highly qualified staff, mentors, and judges. Therefore, the external validity of the quasi-experimental findings likely extends to other ecosystem accelerators that attract young firms with initial traction and benefit from similarly high-quality human and institutional resources. This includes many of the programs featured in the cross-program analysis, where we observe consistent patterns.

4. Mechanisms of impact

Why are there such large and positive effects on workers' career paths caused by acceleration? The results in Section 3.3 suggest the effects cannot be entirely explained by a selection channel where business accelerators select already proven higyh-quality companies with incumbent workers that are at the cusp of significant career changes. Instead, the results indicate that business accelerators can have positive treatment effects on workers' career paths.

But what mechanisms explains the treatment effects of acceleration on employees' career paths? Do these effects reflect genuine human capital development or other alternative mechanisms?

To explore potential mechanisms of impact, we build on Rubinstein and Weiss (2007), who group post-school wage growth mechanisms into two additional categories beyond human-capital: search (matching), and information effects (signalling).

We now discuss the evidence for each of these mechanisms. Overall, this section shows that evidence is most consistent with the human capital accumulation.

4.1. Human capital accumulation

Human-capital accumulation is a natural explanation for the career effects we observe in accelerator settings. Structured mentorship, goal-setting, and peer learning increase the returns to on-the-job learning (Becker 1975), while fixed timelines and demo days impose discipline that accelerates skill acquisition (Hochberg et al. 2019). Prior work shows that accelerators close entrepreneurial and managerial skill gaps by providing training, feedback, and exposure (Gonzalez-Uribe and Leatherbee 2017; Gonzalez-Uribe and Hmaddi 2020). Although founders benefit directly, employees also gain from operating in lean, high-pressure environments in which small teams and fluid roles push them into cross-functional tasks, building the "jack-of-all-trades'' breadth described by Lazear (2005) and the experiential rotation-style learning highlighted by Sorenson et al. (2021). These settings cultivate problem-solving ability, managerial judgment, and adaptive collaboration—competencies linked to future leadership roles (Rosenbaum 1979; Kotter 1982; Troy et al. 2008). In this sense, startups function as training grounds for future managers (Stinchcombe 1965; Freeman et al. 1983; Sørensen 2007; Campbell 2013).

We provide two pieces of evidence that support the human-capital channel: first, changes in job-level skill requirements in the cross-program analysis, and second, parallel patterns in the industry destinations of workers in the quasi-experimental ValleE setting.

In the cross-program setting, we exploit detailed ONET-based information on the skill and task content of all jobs workers hold. Columns 1 and 2 of Table 8, Panel A show that, after acceleration, employees move into jobs requiring significantly stronger soft skills, driven especially by increases in thinking skills. These thinking skills—information processing, complex judgment, and critical reasoning—are general-purpose, transferable abilities that are typically acquired through experience rather than formal education. Their classification as "soft'' reflects their intensive use in collaborative and judgment-rich settings and their malleability through workplace learning. This distinction is important in light of debates on soft-skill development: while early-life interventions matter (Kautz et al. 2014; Algan et al. 2022), growing evidence suggests that such skills can be built in adulthood through on-the-job experience (Adhvaryu, Kala, and Nyshadham 2018). Our findings support this view, at least for thinking-related soft skills. Column 4 of Table 8 complements this evidence by showing that post-acceleration jobs involve fewer routine tasks, consistent with a shift toward more non-routine, cognitively intensive work (Autor et al. 2003).

Figure 4 provides unconditional evidence on managerial skills. Prior to acceleration, treated and control employees hold roles that already require above-average managerial skills—reflecting the responsibility levels inherent in lean startup teams. After acceleration, however, treated employees shift into jobs that demand higher managerial skills than both their own pre-acceleration roles and those of matched controls, particularly in cognitive dimensions such as information processing and monitoring. Interpersonal skills also show unconditional increases, whereas operational skills remain unchanged. Table 8 refines these comparisons by restricting the sample to treated employees and their matched controls and by controlling for demographic characteristics. Columns 1 and 2 of Panel A show statistically significant and economically meaningful increases in cognitive managerial skill requirements. On average, treated employees experience changes roughly 2.5 times larger than those of matched controls, with effects appearing within three years of acceleration and persisting for at least seven years. By contrast, once within-pair differences are accounted for, there is no evidence of significant changes in interpersonal or operational skills.

Given the overrepresentation of high-technology firms in the sample, we also explore shifts in technical-skill requirements. Column 3 of Table 8 shows an overall decline in technical requirements. Figure 5 demonstrates that this decline is not driven by reduced demand for core technical abilities—such as programming, technology design, and operations analysis—which remain elevated relative to ONET averages in both treated and control groups. Instead, the decline reflects a move away from lower-level technical tasks such as installation, troubleshooting, and quality-control analysis, which were already less central prior to acceleration. This pattern suggests a shift from routine technical work toward more advanced or managerial responsibility, rather than technological obsolescence.

In the ValleE setting, we lack job-level skill and task data and thus approximate the sample analysis by examining the types of industries employees enter after acceleration. Table 8, Panel B shows that treated employees are significantly more likely to work in knowledge-intensive and non-routine sectors, using both OLS and IV estimates. The results hold whether we measure industry transitions as a probability or as the share of time spent in each sector. Consistent with the cross-program analysis, the quasi-experimental evidence indicates that accelerated employees move toward occupations and industries characterized by higher cognitive and informational demands (Autor, Levy, and Murnane 2003; Drucker 2007; Hansen et al. 2021) and away from routine-intensive roles.

Taken together, the cross-program and ValleE findings point to a broad and durable transformation in the nature of work performed by employees of accelerated startups. Workers shift away from routine and non-core technical tasks and into managerial, cognitive, and non-routine roles. These directional patterns are fully consistent with human-capital accumulation and are difficult to reconcile with network-based matching or certification, neither of which predicts systematic changes in task content, skill requirements, or industry destinations. The evidence aligns most closely with a model in which experiential learning within accelerated startups produces portable, career-enhancing skills that shape long-term mobility into knowledge-intensive and managerial occupations.

4.2. Matching and networking effects

Acceleration effects on employee careers could also operate through improved job search and matching. Like dense entrepreneurial ecosystems, accelerators connect startups with funders, mentors, and intermediaries, potentially enhancing employees' access to job opportunities and increasing their bargaining power (Mortensen 1976; Feldman and Zoller 2012; Hochberg et al. 2019). If this channel were the primary driver of the results, we would expect to see positive career effects even in the absence of human-capital accumulation, with gains arising from better matches facilitated by accelerator networks. In particular, a dominant matching mechanism would imply higher overall employment probabilities or more frequent transitions into other accelerator-linked firms.

Table 9 tests this mechanism. Panel A splits the cross-program sample into employees who move into any firm connected to an accelerator—broadly defined as any company previously backed by an accelerator—and those who do not. The estimated effects remain unchanged even when we exclude all jobs in accelerator-backed firms, which is a strong, "acid test'' against a network-matching explanation.

For the ValleE analysis, we observe no internal mobility within the cohort: not a single employee moves to another startup that also participated in the first 2015 cohort. Because firm identifiers for later cohorts are confidential, we cannot test transitions into the broader ValleE alumni network. However, we can conduct an equally demanding test by excluding all firms located in the accelerator's immediate region of influence—the city of Cali. Panel B shows that employees of accelerated firms actually spend a larger share of their formal employment in firms outside Cali, which is the opposite of what a network-driven mechanism would predict and is inconsistent with network-based search (Rajkumar et al. 2022). Columns 4 and 6 indicate that accelerated employees are both more likely to migrate and devote a significantly greater share of their post-acceleration careers to jobs outside Cali, although statistical significance is stronger for the latter given the sample size. Taken together, these results rule out the possibility that accelerator networks are simply reshuffling workers across peer firms or within the local ecosystem, reinforcing that search is not the primary explanation.

4.3. Signalling and certification effects

A final potential explanation for the acceleration effects on employee careers is signalling. Because accelerators are perceived as identifying high-growth firms that attract strong talent, they may act as certifiers of worker quality, reducing information asymmetries for outside employers (Jovanovic 1979). Under this view, wage premia would reflect the market updating its beliefs based on accelerator selection rather than genuine skill accumulation, echoing certification effects documented for SBIR awards (Lerner 1999) and business plan competitions (Howell 2020).

Table 10 provides evidence from both the cross-program (Panel A) and ValleE (Panel B) settings that contradicts a pure signalling or certification mechanism. If certification were the dominant channel, effects should be strongest for employees at the margin of acceptance—who would benefit most from a positive signal—and weakest for employees of firms that eventually fail, where any positive belief about worker quality should be corrected once the firm collapses. Instead, the odd-numbered columns in Panel A show substantial career gains even for employees of startups that later closed. Panel B shows the same pattern in the ValleE setting and further demonstrates that the effects persist among employees who exit at the time of closure.

The quasi-experimental design strengthens this conclusion. As explained in Section 3, the ValleE sample consists primarily of Type 1 errors—high-quality firms incorrectly rejected by tough judges—rather than Type 2 errors, where low-quality firms are mistakenly admitted. Because very few low-quality firms employ formal workers, the IV results are driven by applicants with genuine high-growth potential rather than marginal entrants who would benefit most from certification. This pattern is inconsistent with a pure signalling story, which would predict the strongest effects among workers from lower-quality firms.

Moreover, the quasi-experimental evidence that judges make systematic selection mistakes challenges the notion that market participants interpret accelerator admission as a uniformly strong quality signal. If signalling were the causal mechanism, the effects should attenuate once firms fail or once markets observe selection errors; instead, they remain robust.

Taken together, these results rule out certification as the primary explanation. While search and signalling may contribute at the margin, they cannot account for the magnitude, persistence, or distribution of the career gains observed after accelerator participation. The evidence overwhelmingly supports human-capital accumulation as the central mechanism.

5 New Hires and Robustness Checks

Our final analysis returns to the firm. A natural concern is that the employee-level gains we document might come at the expense of the originating startups—if, for example, accelerators raise workers' outside options and make departures harder to absorb. Prior work generally finds positive firm-level effects, and our evidence aligns with this view. We examine whether accelerated startups replace departing workers and how the characteristics of their new hires compare with those of matched non-accelerated firms. Although this analysis is challenging—employees choose when to exit, and acceleration may itself alter hiring needs—the results consistently suggest that worker gains do not harm their firms.

Across both datasets, Table 11 sows that replacement hires in accelerated firms appear stronger than those in comparable non-accelerated firms. They have more prior experience, and in the cross-program setting are also more educated. We find no systematic differences by gender or age. These patterns indicate that accelerated firms are able to attract and recruit high-quality workers even as original employees move into managerial roles elsewhere.

This interpretation is reinforced by evidence consistent with organisational upgrading. As accelerated startups expand, they naturally shift toward hiring more experienced talent, consistent with maturation and professionalisation (Hellmann and Puri 2002). In this sense, employee turnover reflects not a loss but an evolution: accelerated employees transition into managerial roles in the broader economy, while their original firms replenish with stronger hires.

Taken together, the evidence shows that accelerators enhance both sides of the market. They improve workers' wages and managerial mobility without raising unemployment risks or disadvantaging the firms they leave. The effects are not driven by movements within accelerator networks, and they apply equally to employees of successful and failed startups. Overall, accelerators strengthen firm dynamism and enlarge the economy's stock of managerial talent, underscoring their role as institutions of human-capital formation.

We also conduct several robustness checks. For the cross-program analysis, we use alternative methods to construct the control group and obtain similar results. For both the cross-program and quasi-experimental settings, the findings hold when excluding founders—defined as employees present at inception with the longest tenure. Finally, alternative definitions of managerial roles and skills, including top-wage earners in the quasi-experimental setting and alternative ONET-based indexes in the cross-program setting, yield consistent patterns.

6 Conclusion

This paper shows that startup accelerators—long viewed as engines of firm-level growth—also play a formative role in shaping the careers of startup employees. Using cross-program and quasi-experimental evidence, we find that accelerator participation increases employees' upward mobility into managerial roles, with sustained wage and earnings gains and no increase in unemployment risk. These effects persist even among employees of failed startups, suggesting they are not driven by firm success alone.

Our findings point to human capital accumulation as the primary channel: accelerator structures—such as mentorship, goal-setting, and milestone-based evaluation—enhance the learning environment within startups, particularly for developing general and higher-order skills. We find little support for alternative mechanisms such as job search or signaling. In contrast to the private equity literature, where career gains are often tied to IT skill acquisition, our results highlight the importance of cross-functional, managerial, and soft skills in early-stage firms.

These findings contribute to three areas of research. First, they expand the literature on managerial formation by identifying accelerator-driven experiential learning as a novel early-career pathway. Second, they advance our understanding of human capital development in startups, where compensation often includes reputational and deferred returns. Third, they call for a broader evaluation of accelerator programs, which have traditionally been assessed through the lens of startup performance alone. Accelerators also generate substantial employee spillovers, and should be seen as institutions that shape labor markets—not just venture outcomes.

More broadly, our results speak to the growing role of entrepreneurial finance in workforce development. Like private equity and corporate restructuring, accelerators influence the distribution of human capital and career opportunity. But unlike those mechanisms, accelerators achieve this by building skills—rather than reallocating or replacing them—underscoring their importance as human capital–forming institutions in the innovation economy.

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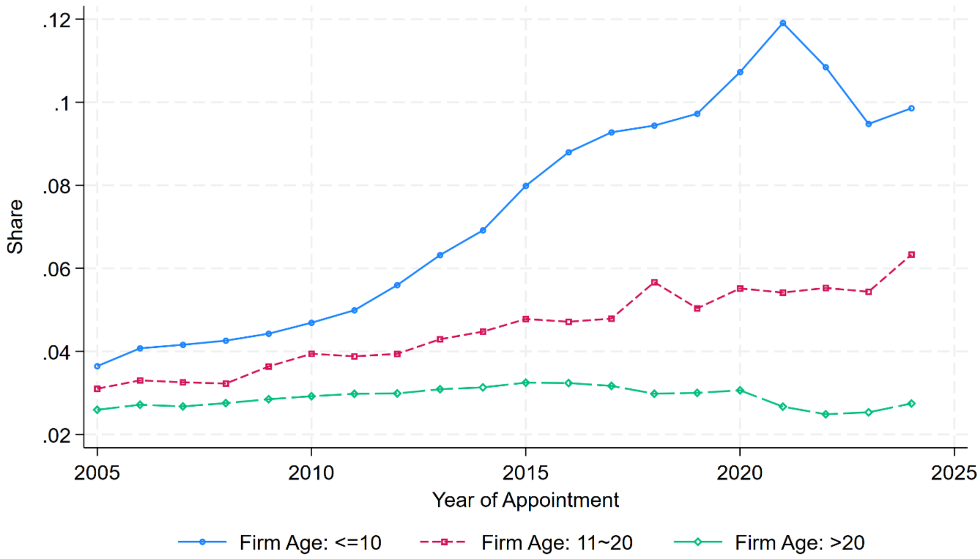
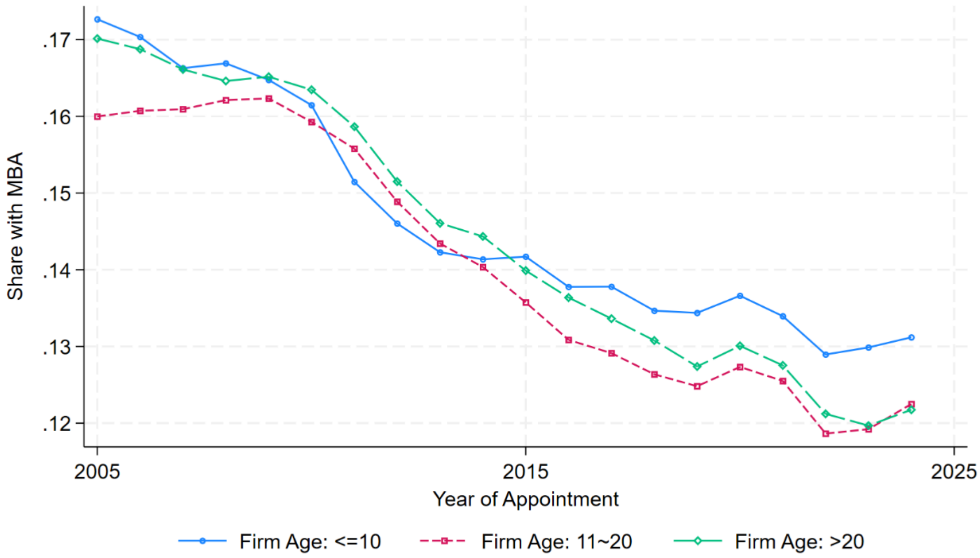
Figure 1. Previous Experience of Newly Appointed Managers by Firm Age

Panel A - Managers with Experience in Accelerated Firms



Panel B - Managers with MBAs

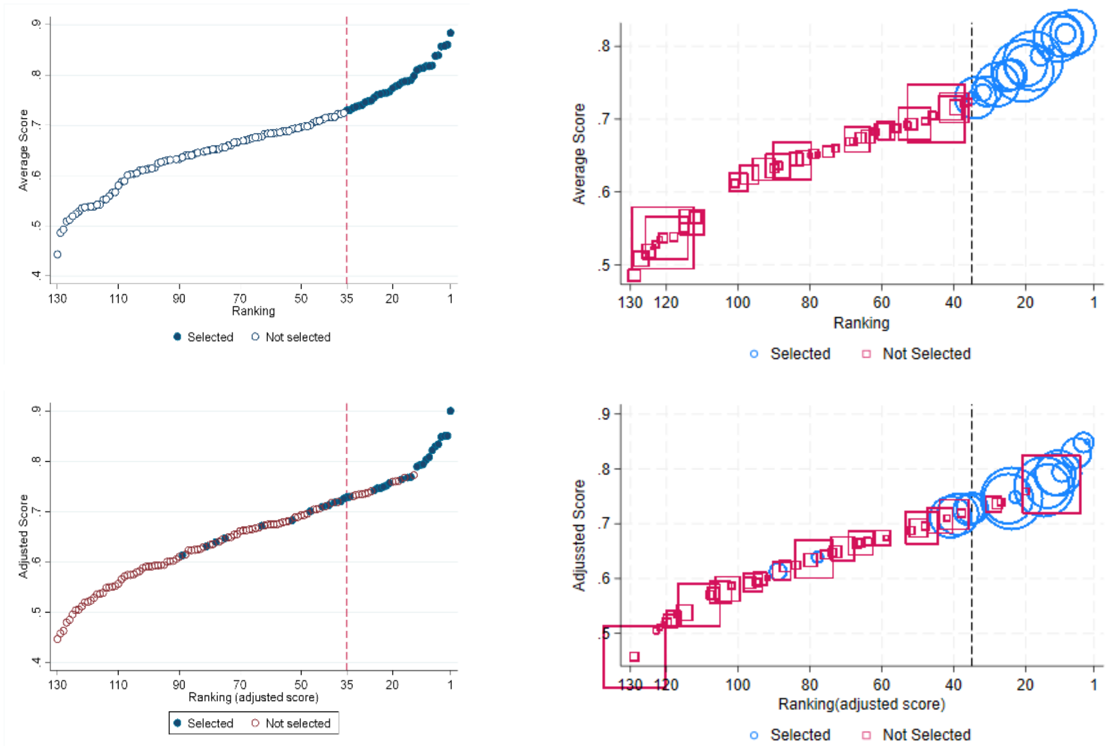


The figure depicts the fraction of managers appointed in a given year that have previously worked in an accelerated firm (Panel A) or held an MBA (Panel B). The universe of managers are LinkedIn users in countries in the Americas who in a given year are appointed to a job that is classified as a managerial position by ONET (i.e., "management and entrepreneurship cluster" https://www.onetonline.org/find/career?c=060200) according to their resumes. The sample of accelerated companies in American countries is obtained from Crunchbase (and matched to LinkedIn companies profile). We restrict firms with known founding date and we delete cases in which the managerial position is the individual's first job. There are 26,550 accelerated firms, 4.79 million managers who worked for 1.13 million companies in total in generating this plot. The unconditional (over the full sample period and across different types of firms) share of appointed managers with acceleration experience is 4.16 %.

Figure 2. Distribution of ValleE Applicant Scores and Selection

Panel A. ValleE Sample

Panel B. PILA Sample

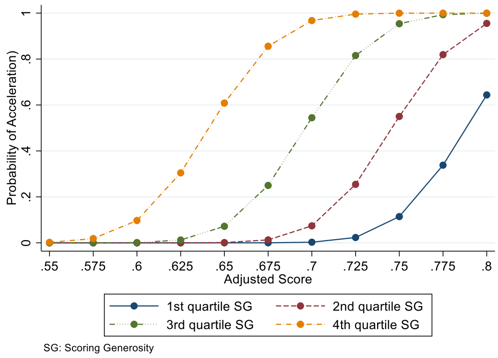


The top figure in each panel plots average scores against rankings based on the average score. The bottom figure in each panel plots adjusted scores against rankings based on the adjusted score, where adjusted scores correspond to the project's fixed effects when scores are regressed against project FE and judges' FE to clean the scores from differences in scoring generosity. The left panel uses the entire ValleE sample and the right panel uses the PILA sample. In each figure, each dot represents an applicant; the solid (open) dots indicate the applicants that were (were not) selected into the accelerator. Selected applicants correspond to the treatment applicants and rejected applicants to control applicants.

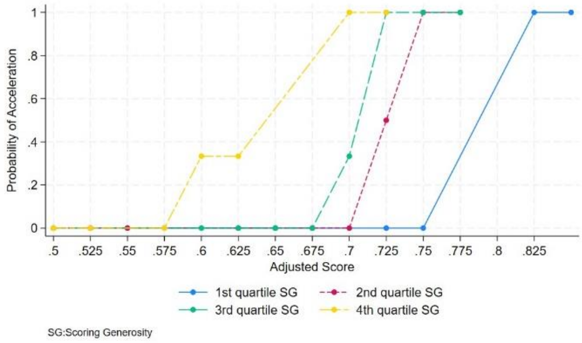
Figure 3. Acceleration Probability and Scoring Generosity

Panel A— Acceleration Probability and Generosity, by Quartiles of Adjusted Score

ValleE Sample

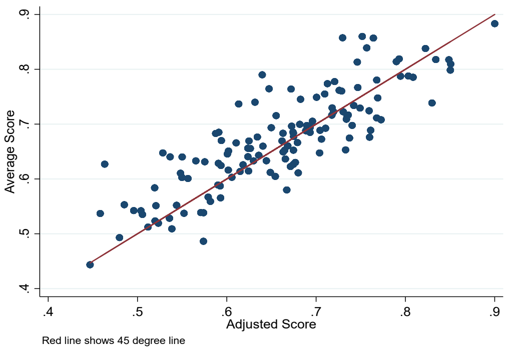


PILA Sample

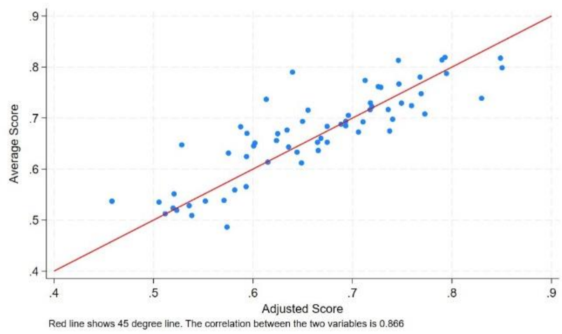


Panel B - Average Scores and Adjusted Scores

ValleE Sample

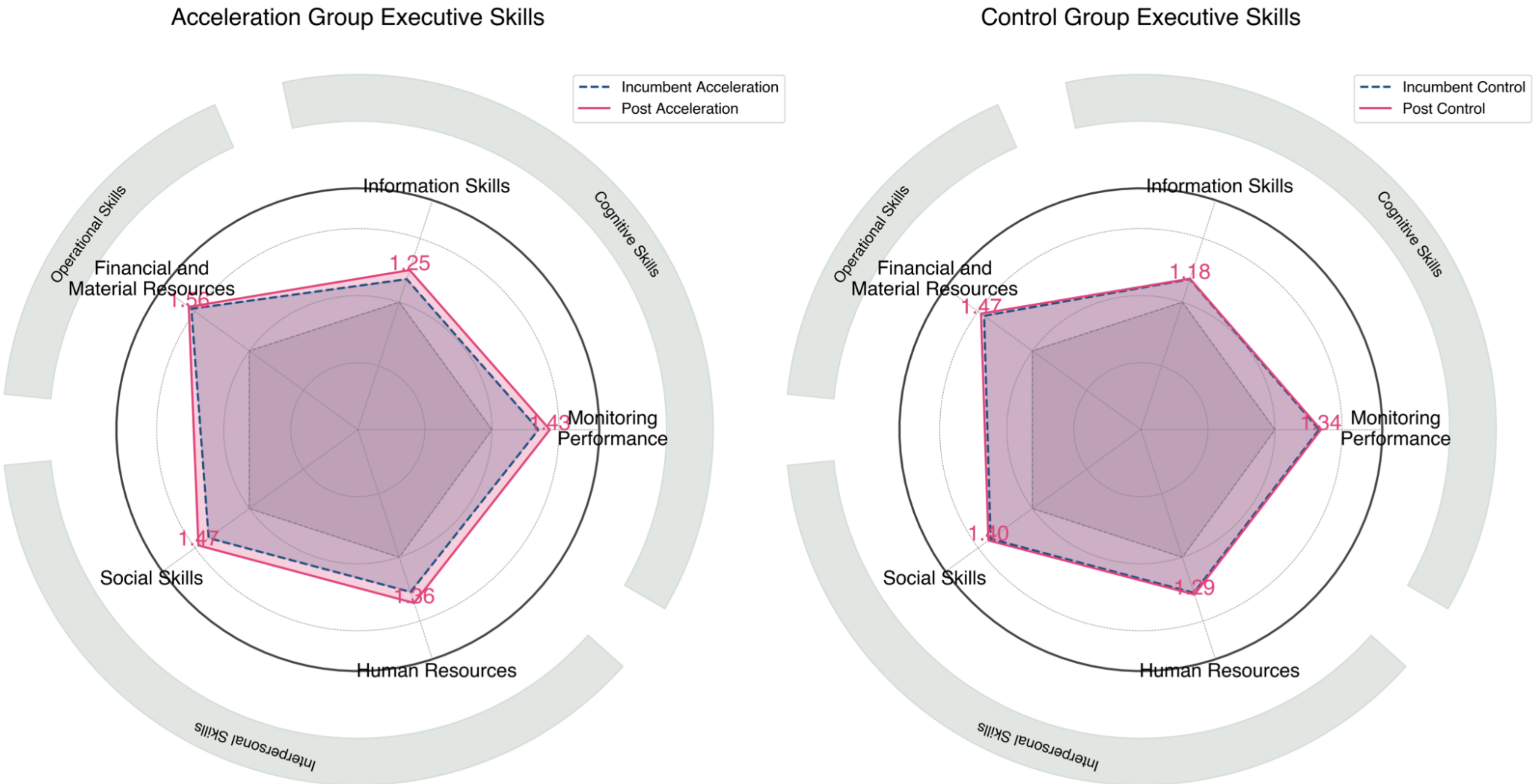


PILA Sample



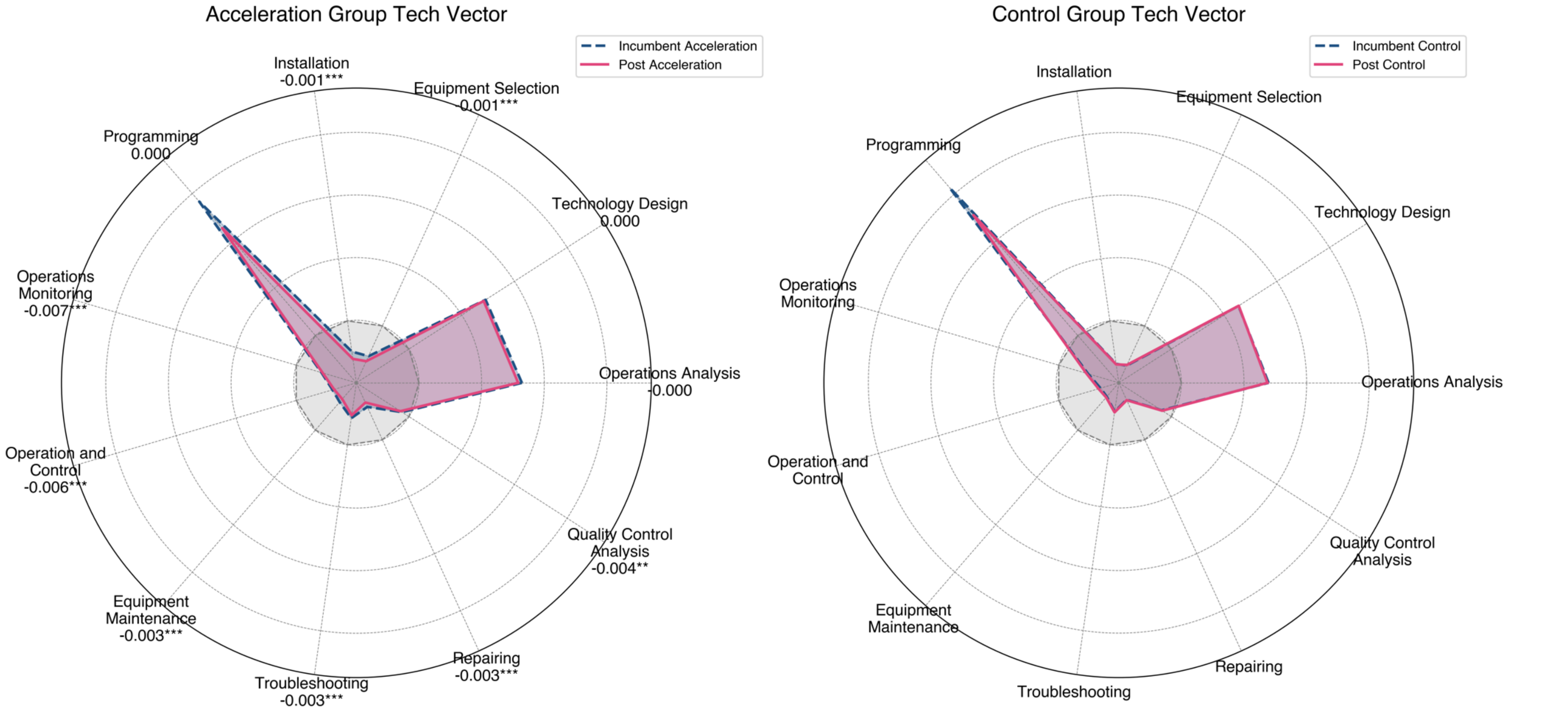
Each plot in panel A depicts the probability of acceleration against adjusted score by each quartile of scoring generosity for the ValleE sample (left) and PILA sample (right). The top (bottom) quartile of scoring generosity corresponds to the most (least) generous judge panels. Each plot in panel B depicts the average scores against adjusted scores for the ValleE sample (left) and PILA sample (right). Each dot represents an applicant. The red line shows the 45-degree line. Applicants with adjusted scores above the 45-degree line were "lucky" in that they drew a generous judge panel, while applicants with average scores below the 45-degree line were "unlucky" and drew a strict judge panel. The correlation between average scores and adjusted scores in the ValleE (PILA) sample is 0.825 (0.866).

Figure 4. Required Managerial Skills for Jobs During and After Acceleration



The figure shows radar charts for the job requirements in terms of Executive Skills following the taxonomy of Hansen et al. (2024) and O*NET taxonomy: Cognitive Skills that includes Information Skills and Monitoring Performance; Interpersonal Skills that includes Social Skills and Management of Human Resources; Functional and Operational Skills that include Management of Financial and Material Resources. The figure compares radar charts for treated employees (left) to control employees (right); raw vector values compared to the O*NET mean occupation. For each group of employees, we plot the radar chart during acceleration (blue, dashed lined) and post-acceleration (red, solid line). We also plot the average of all O*NET occupations (in grey area). The number below each category corresponds to the average value of each particular executive skill across all jobs after (potential) acceleration.

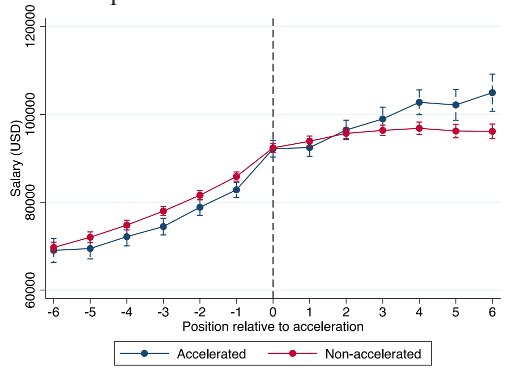
Figure 5. Required Technical Skills for Jobs During and After Acceleration



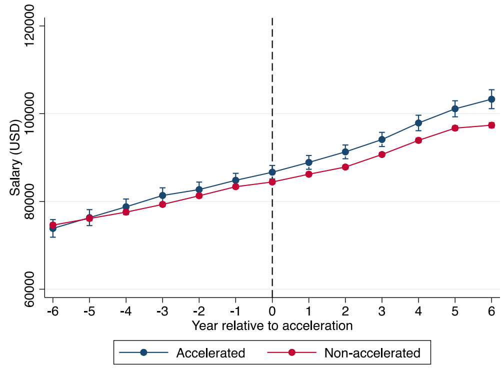
The figure shows radar charts for the job requirements in terms of Technical Skills following O*NET Content Model. The figure compares radar charts for treated employees (left) to control employees (right); raw vector values compared to the O*NET mean occupation. For each group of employees, we plot the radar chart during acceleration (blue, dashed lined) and post-acceleration (red, solid line). We also plot the average of all O*NET occupations (in grey area). The number below each category corresponds to the regression coefficient on the log difference between all subsequent positions and incumbent position. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Figure 6. Expected Wages, Employability, Departures, and Expected Earnings after Acceleration in the Cross-Program Analaysis

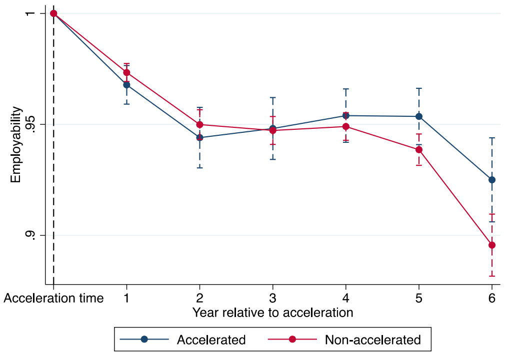
Panel A. Expected Wages in subsequent job positions after acceleration



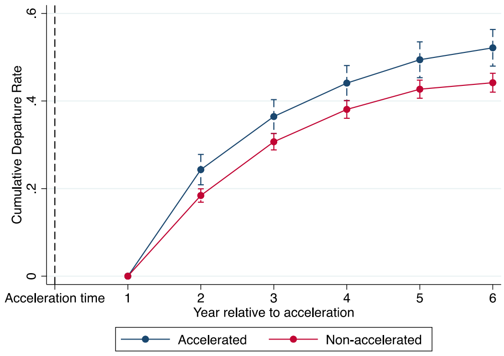
Panel B. Expected Wages in the years after acceleration



Panel C. Employability in the years after acceleration

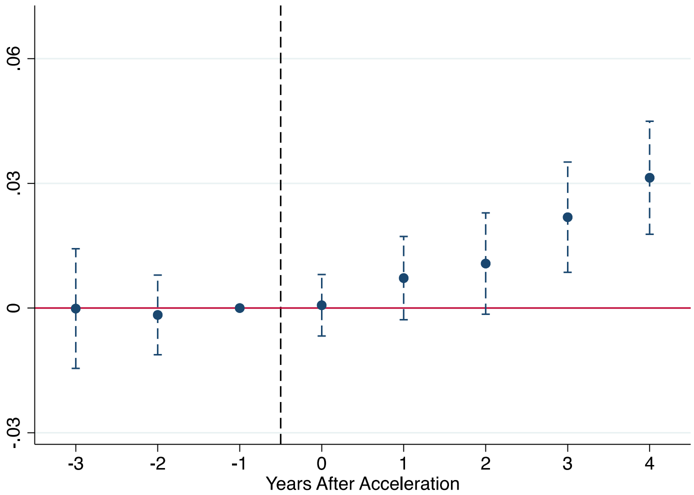


Panel D. Company departures in the years after acceleration



Panel A plots average expected wages across positions held before, during and after acceleration. Panel B plots average employability for treatment and control groups around acceleration. Panels C and D plot the cumulative job and company departure rates after acceleration. Panel E plots average (across job positions and employees) annual expected Wages for treatment and control groups around acceleration.

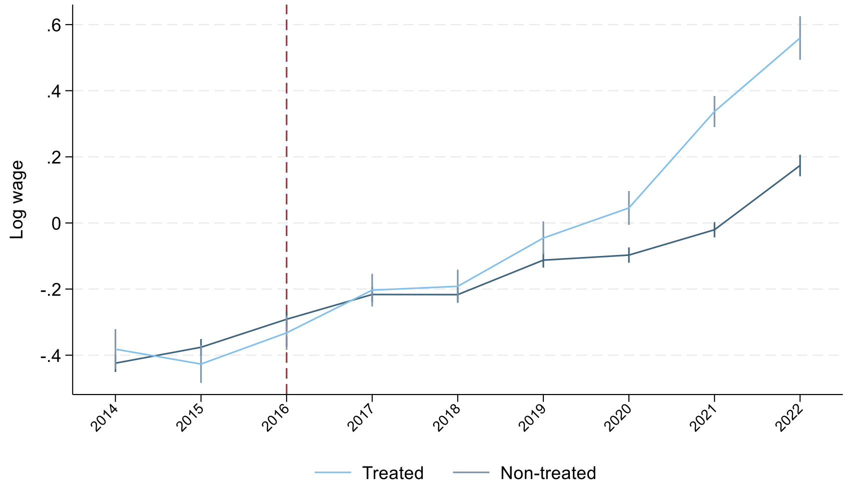
Figure 7. Stacked Difference-in-Difference in Log Expected Wages after Acceleration in the Cross-Program Analysis



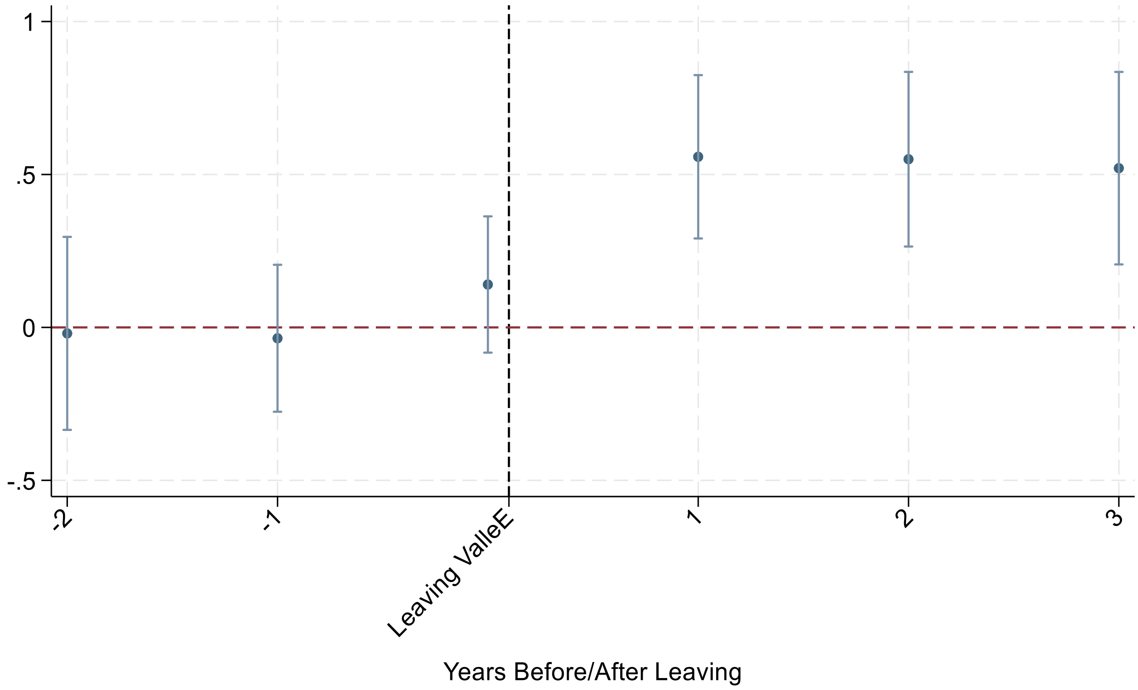
The figure plots the coefficient estimates β_h from equation (2) using the variable indicated in the title as the outcome variable. We include the treated individual group FE and acceleration year FE, and control for employee demographic characteristics including age and education. Standard errors are clustered at the treated group level.

Figure 8. Wages and Earnings After ValleE Acceleration

Panel A. Wages in the years after acceleration



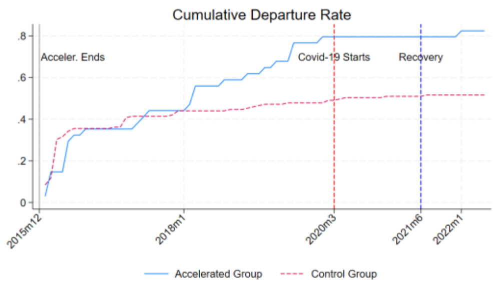
Panel B- Wages in subsequent job positions after ValleE Acceleration



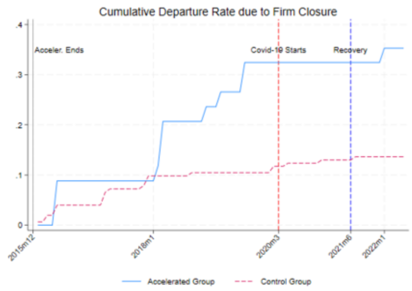
Panel A plots average (across job positions and employees) annual wages for employees of ValleE participants (Treated) and of the rejected applicants (Non-treated). Panel B organizes the data at the "leaving VAlleE" event time and plots estimated differences in wages for accelerated and control employees.

Figure 9. Departure Rates and Employability after ValleE Acceleration

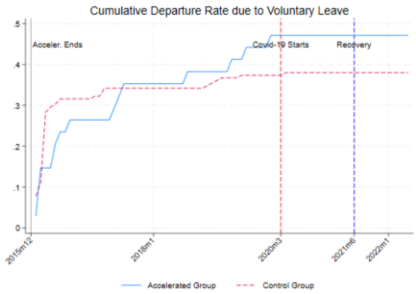
Panel A. Cumulative departure rate



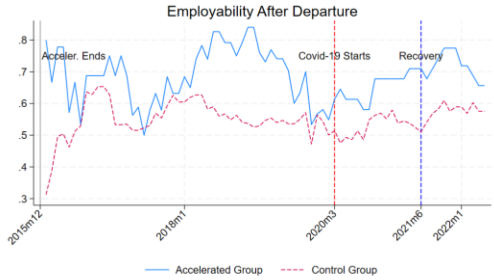
Panel B. Cumulative departure rate due to firm closure



Panel C. Cumulative departure rate not due to firm closure



Panel D. Employability after leaving applicant firms



Panels A-C plot cumulative departure rates across employees of ValleE participants (accelerated group) and rejected firms (control group). Panel D depicts the fraction of employees employed after leaving the applicant firms.

Table 1. Cross-program Analysis Sample Composition

Panel A – Baseline sample

	N	mean	std.	min	P25	P50	P75	Max
Firm level variables								
Founded year	5,093	2014.4	2.36	2001	2013	2015	2016	2018
Acceleration year	5,093	2016.49	2.24	2006	2015	2017	2018	2019
Firm age when accelerated	5,093	2.09	1.33	0	1	2	3	5
Employee level variables at the time of acceleration								
Age	106,926	30.54	5.77	20	26	29	34	50
Gender	106,926	0.47	0.5	0	0	0	1	1
Education level	106,926	0.25	0.6	0	0	0	0	3
Previous positions	106,926	2.41	2.39	0	0	2	4	10
Previous companies	106,926	2.14	2.05	0	0	2	3	8
Previous employability	106,926	0.9	0.16	0.35	0.85	0.99	1	1
Previous working tenure (in months)	106,926	71.51	60.05	2	26	56	102	313
Previous total time since entry labor market (in months)	106,926	82.05	67.26	2	30	66	116	345
Previous working tenure per position (in months)	106,926	23.38	24.97	0	0	18	33	129
Previous working tenure per company (in months)	106,926	25.52	26.76	0	0	20.4	36.33	140
Avg description length per position on LinkedIn	106,926	146.58	201.66	0	0	36	242.33	809
Accelerator level variables								
Founded year	94	2011.62	4.71	1991	2010	2012	2015	2018
Investment records	185	240.39	727.39	21	40	81	142	6013
Funding provided (RAG)	185	0.57	0.5	0	0	1	1	1

Panel B – ONET sample

	N	mean	std.	min	P25	P50	P75	Max
Firm level variables								
Founded year	1,091	2014.43	2.36	2006	2013	2015	2016	2018
Acceleration year	1,091	2016.43	2.28	2010	2015	2017	2018	2019
Firm age when accelerated	1,091	2.01	1.29	0	1	2	3	5
Employee level variablesat the time of acceleration								
Age	11,016	30.93	6.53	20	26	30	34	52
Gender	11,016	0.62	0.49	0	0	1	1	1
Education level	11,016	0.42	0.77	0	0	0	1	3
Previous positions	11,016	2.67	2.54	0	1	2	4	10

Previous companies	11,016	2.35	2.21	0	1	2	4	9
Previous employability	11,016	0.89	0.16	0.37	0.84	0.99	1	1
Previous working tenure (in months)	11,016	75.84	66.89	2	25	56	108	307
Previous total time since entry labor market (in months)	11,016	87.28	74.31	2	29	70	124	333
Previous working tenure per position (in months)	11,016	23.36	24.42	0	4.67	18	32	122
Previous working tenure per company (in months)	11,016	25.99	26.88	0	5	20	36.35	133
Avg description length per position on LinkedIn	11,016	153.68	207.96	0	0	52	250.17	877.09
Accelerator level variables								
Founded year	48	2011.6	4.66	1991	2010.5	2012	2014	2018
Investment records	89	403.01	1023.8	21	38	96	250	6013
Funding provided (RAG)	89	0.58	0.5	0	0	1	1	1

The table presents the composition of the baseline and ONET cross-program samples and selected summary statistics of the descriptive variables extracted from the LinkedIn and Crunchbase individual, company and accelerator program profiles. Panel A (Panel B) comprises the baseline sample (ONET sample). The observations are at the firm, employee and accelerator level as indicated in the row titles of each panel.

Table 2. Cross-program Analysis Summary Statistics

Panel A – Baseline sample

	N	mean	std.	Min	P25	P50	P75	Max
Change manager word index	106,926	0.02	0.62	-2.78	0	0	0	2.43
Change entrepreneur word index	106,926	-0.05	0.5	-3.45	0	0	0	1.38
Change professional manager word index	106,926	0.04	0.6	-2.41	0	0	0	2.74
Within 3 years								
Change manager word index	106,926	0.01	0.41	-1.85	0	0	0	1.85
Change entrepreneur word index	106,926	-0.02	0.26	-1.88	0	0	0	0.8
Change professional manager index	106,926	0.02	0.42	-1.81	0	0	0	2.11

Panel B – ONET sample

	N	mean	std.	min	P25	P50	P75	Max
Change in manager index	11,016	0.01	0.09	-0.3	-0.01	0	0.03	0.29
Change log information skills	11,016	0	0.05	-0.16	-0.01	0	0.01	0.18
Change log monitoring performance	11,016	0.01	0.08	-0.24	-0.01	0	0.02	0.27
Change log management of financial and material resources	11,016	0.01	0.08	-0.25	0	0	0.03	0.24
Change log management of human resources	11,016	0.01	0.07	-0.22	-0.01	0	0.03	0.23
Change log social skills	11,016	0	0.06	-0.18	-0.01	0	0.02	0.18
Change log soft skills	11,016	0	0.04	-0.14	-0.01	0	0.01	0.14
Change log thinking skills	11,016	0	0.04	-0.13	-0.01	0	0.01	0.15
Change log routine tasks	11,016	0	0.04	-0.12	-0.01	0	0.01	0.11
Log change technical skills	11,016	0	0.03	-0.09	0	0	0.01	0.09
Change log wage	11,016	0.06	0.18	-0.4	0	0	0.12	0.69
Long run employability	11,016	0.91	0.19	0.09	0.91	1	1	1
Remain employability at incumbent company	11,016	0.57	0.39	0.02	0.17	0.55	1	1
Long run employability after leaving incumbent company	6,931	0.74	0.34	0	0.65	0.92	0.99	1
Within 3 years								
Change in manager index	11,016	0	0.1	-0.36	0	0	0	0.35
Change log information skills	11,016	0	0.04	-0.15	0	0	0	0.16
Change log monitoring performance	11,016	0	0.07	-0.23	0	0	0	0.26
Change log management of financial and material resources	11,016	0	0.07	-0.23	0	0	0	0.22
Change log management of human resources	11,016	0	0.06	-0.21	0	0	0	0.21
Change log social skills	11,016	0	0.05	-0.17	0	0	0	0.17
Change log soft skills	11,016	0	0.04	-0.14	0	0	0	0.13
Change log thinking skills	11,016	0	0.03	-0.13	0	0	0	0.14
Change log routine tasks	11,016	0	0.03	-0.11	0	0	0	0.1
Log change technical skills	11,016	0	0.02	-0.08	0	0	0	0.08

Change log wage	11,016	0.03	0.15	-0.44	0	0	0.03	0.61
Long run employability	11,016	0.93	0.17	0.16	1	1	1	1
Stacked difference-in-difference analysis								
Annual Wage	24,672	11.39	0.23	10.71	11.27	11.41	11.54	11.92

The table presents summary statistics for the main variables in the cross-program analysis. The observations are at the employee level; except the variable in the last row where observations are at the employee-year level.

Table 3. PILA Sample Composition

	All Sample			PILA Sample			ValleE Sample		PILA Sample	
							Business Ideas	Established Firms	Business Ideas	Established Firms
Variable	Mean	Min	Max	Mean	Min	Max	Mean	Mean	Mean	Mean
Gender: Male	79%	0	1	79%	0	1	75%	84%	75%	81%
Education: High school	12%	0	1	10%	0	1	17%	6%	21%	5%
Education: Technical degree	21%	0	1	21%	0	1	22%	21%	17%	23%
Education: College	52%	0	1	57%	0	1	39%	67%	42%	65%
Education: Masters or PhD	15%	0	1	12%	0	1	22%	6%	21%	7%
Location: Cali	85%	0	1	87%	0	1	88%	83%	92%	84%
Motivation: Have stable income	12%	0	1	12%	0	1	13%	11%	8%	14%
Motivation: Own boss	1%	0	1	1%	0	1	0%	2%	0%	2%
Motivation: Business opportunity	87%	0	1	87%	0	1	88%	87%	92%	84%
Dedication: Sporadic	6%	0	1	4%	0	1	10%	2%	8%	2%
Dedication: Half-time	21%	0	1	19%	0	1	25%	17%	29%	14%
Dedication: Full-time	73%	0	1	76%	0	1	65%	81%	63%	84%
Sector experience (years)	5.6	0	30	5.6	0	30	4.7	6.6	3.7	6.7
Serial entrepreneur	61%	0	1	58%	0	1	61%	62%	58%	58%
Has entrepreneurial team	88%	0	1	90%	0	1	85%	92%	92%	88%
# of people on team	3.0	1	10	1.5	0	7	2.8	3.3	1	1.7
Sector: Agriculture	16%	0	1	15%	0	1	13%	19%	17%	14%
Sector: Manufacturing	21%	0	1	24%	0	1	24%	17%	33%	19%
Sector: Water and Electricity	3%	0	1	0%	0	0	4%	2%	0%	0%
Sector: Construction	3%	0	1	3%	0	1	3%	3%	0%	5%
Sector: Commerce	2%	0	1	4%	0	1	1%	3%	4%	5%
Sector: Services	56%	0	1	54%	0	1	56%	56%	46%	58%
Participated in other contests	59%	0	1	61%	0	1	56%	63%	58%	63%
% Established Firms	47%	0	1	64%	0	1	0%	100%	0%	100%
Year founded (established firms)	2013	2010	2015	2013	2010	2015	.	2013	-	2013
Revenue 2013 (million pesos)	10.62	0	290	17.39	0	290	1.27	21.48	2.17	26.1
Revenue 2014 (million pesos)	25.80	0	300	41.84	0	300	4.61	50.01	7.88	60.79
Number of applicants		135			66		72	63	24	42
Number of incumbent employees					195				27	168
Number of new hires					466				261	205

The table presents the composition of the sample and selected summary statistics of the variables in the application forms. The ValleE sample includes all applicants evaluated by judges. The PILA sample includes all businesses in the VallE sample linked to the PILA employer-formal employee database. The subsample of established firms (business ideas) corresponds to applicants that at the time of the application had (had not) registered as a business with the Chamber of Commerce.

Table 4. Summary Statistics PILA Sample

	N	Mean	SD	Min	p25	p50	p75	Max
Wage (COP millions)								
All	49,159	1.12	1.17	0.02	0.62	0.83	1.12	25
Incumbent Workers	16,278	1.12	1.26	0.02	0.62	0.78	1.03	16.49
New Hires	32,881	-0.22	0.9	-4.17	-0.48	-0.19	0.17	3.22
Age								
All	49,110	31.9	8.9	18	25	30	37	69
Incumbent Workers	16,278	32.5	8.4	18	26	31	37	60
New Hires	32,832	31.6	9.1	18	25	29	36	69
Years of experience in formal labor market								
All	49,110	4.2	3	0	1.7	3.6	6.2	13.5
Incumbent Workers	16,278	4.4	3	0	1.8	3.9	6.5	13.4
New Hires	32,832	4.1	3	0	1.6	3.4	6	13.5
% of potential work experience in formal labor market								
All	49,110	0.78	0.23	0.03	0.64	0.87	0.99	1
Incumbent Workers	16,278	0.78	0.22	0.04	0.66	0.86	0.98	1
New Hires	32,832	0.78	0.24	0.03	0.63	0.87	0.99	1
% of women								
All	49,110	0.32	0.46	0	0	0	1	1
Incumbent Workers	16,278	0.32	0.46	0	0	0	1	1
New Hires	32,832	0.32	0.47	0	0	0	1	1

The table shows summary statistics for the wage variables in the PILA sample.

Table 5. First-Stage: ValleE Selection and Scoring Generosity

	(1)	(2)	(3)	(4)
Observations	Firm level	Firm level	Employee level	Employee-month level
Sample	ValleE	PILA	PILA	PILA
	Acceleration	Acceleration	Acceleration	Acceleration×Post
SG	3.002***	2.638***	5.039***	
	(0.43)	(0.66)	(0.531)	
SG×Post				4.438***
				(0.081)
N	135	66	658	48,548
R-sq	0.529	0.492	0.447	0.764
F-test excl. ins.	49.74	15.98	58.25	2970

The table shows results from the first stage regressions for the ValleE quasi-experimental analysis. Columns (1)-(3) regress Acceleration against SG, the scoring generosity of the applicants' judges. Column (3) reports results from equation 4. Post is a variable that equals one after 2015. Column (3) includes employee level controls at the time of acceleration, including: age at acceleration, experience at acceleration, experience at acceleration squared, and share of potential experience in the formal labor market, and indicator for holding a manager role prior to acceleration, dummy for women and adjusted score. Column 4 include employee fixed effects, month fixed effects and time varying employee controls including age, experience, experience squared and share of potential experience in the formal labor market. Standard errors are heteroskedasticity robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6. Business Acceleration, Managerial Roles, Wages and Employability

Panel A – Cross-program Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
	Change manager word index	Change manager index	Manager index	Change log wage	Log wage	Employability
	Event study	Event study	ATT	Event study	ATT	Event study
Acceleration	0.052***	0.015***	0.013**	0.035***	0.039***	0.014**
	(0.012)	(0.004)	(0.005)	(0.007)	(0.011)	(0.006)
N	105,727	10,990	42,168	0.061	0.085	0.906
R-squared	0.068	0.088	0.185	10,990	42,168	10,990
Control group mean	0.013	0.004	0.016	0.061	0.085	0.906
Economic effect	5.1	4.9	1.8	1.6	1.5	1.0

Panel B – ValleE Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
	Manager	Manager	Log wage	Log wage	Employability	Employability
	OLS	IV	OLS	IV	OLS	IV
Acceleration	0.220***	0.463**			0.145**	-0.066
	(0.092)	(0.177)			(0.060)	(0.173)
Acceleration × Post			0.109***	0.136***		
			(0.0247)	(0.0399)		
N	658	658	48,548	48,548	656	656
R-squared	0.162	0.145	0.393	0.008	0.176	0.132
Control group mean	0.143	0.143	0.362	0.362	0.181	0.512
Economic effect	2.5	4.2	1.3	1.4	1.8	0.6

Panel A shows estimates of equation (1) and equation (2) comparing changes in characteristics of the jobs held after acceleration relative to the jobs before acceleration across treated and control employees in the cross-program analysis sample. Observations are at the individual level and regression controls include employee-level variables at the time of acceleration—age and education, acceleration year FE, and treated individual group FE to fix the comparison group for each treated employee to the specific matched control employees. For control employees, we use the years after the acceleration date of the matched treated firm as the period after acceleration. Standard errors are clustered at the treated individual group level. The control group mean corresponds to the mean change in the outcome variable for the control employees, and the economic effect corresponds to the ratio between the estimated change in the outcome variable for the employees of accelerated companies relative to the mean change for the control employees. *Panel B* shows OLS and IV estimates of equation (3). Columns 1, 2, 5 and 6 compare employment outcomes after acceleration between the treated and control groups. Observations are at the employee level, and regression controls include employee-level variables at the time of acceleration, including: age, experience, experience squared, share of potential experience in the formal labor market, an indicator for holding a manager role prior to acceleration, dummy for women and the adjusted score. In columns 2 and 6 we instrument *Acceleration* with the variable *SG*—the scoring generosity of the applicants' judges. Columns 3 and 4 compare employment outcomes before and after acceleration between the treated and control groups. Observations are at the employee-month level and the controls include time-varying employee-level variables including age, experience, experience squared and share of potential experience in the formal labor market. In column 4 we instrument *Acceleration* × *Post* using *SG*×*Post*—the variable

Post equals one after 2015. In columns 1, 2, 5 and 6, the control group mean is the mean for the control group, and the economic effect corresponds to the ratio between the estimated change in the outcome variable for the employees of accelerated companies relative to the mean for the control employees. In columns 3 and 4, the control group mean corresponds to the mean change in the outcome variable for the control employees, and the economic effect corresponds to the ratio between the estimated change in the outcome variable for the employees of accelerated companies relative to the mean change for the control employees. Standard errors are heteroskedasticity robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7. Heterogeneity Cross-Program Analysis: Business Acceleration, Managerial Roles, Wages and Employability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Program Seed Capital		Program Experience		Program Location			Employee Business Degree		Departed	
	Yes	No	High	Low	NA – bottom	NA – top	LAC	Yes	No	Yes	No
Panel A -Change in Manager Index											
Acceleration	0.019***	0.016**	0.010	0.014**	0.007	0.001	0.015***	-0.011	0.015***	0.019**	0.001
	(0.006)	(0.007)	(0.007)	(0.006)	(0.011)	(0.023)	(0.005)	(0.016)	(0.005)	(0.008)	(0.003)
N	8,084	2,784	5,534	5,385	1,001	365	9,598	1,979	8,743	6,788	3,917
R-squared	0.114	0.123	0.099	0.109	0.186	0.330	0.080	0.208	0.105	0.135	0.163
Panel B - Change in Log Wages											
Acceleration	0.047***	0.033***	0.035***	0.032***	0.012	0.096***	0.033***	-0.018	0.039***	0.059***	-0.009
	(0.010)	(0.011)	(0.011)	(0.010)	(0.017)	(0.027)	(0.008)	(0.021)	(0.008)	(0.012)	(0.006)
N	8,084	2,784	5,534	5,385	1,001	365	9,598	1,979	8,743	6,788	3,917
R-squared	0.117	0.184	0.102	0.155	0.282	0.444	0.099	0.248	0.131	0.162	0.204
Panel C - Employability											
Acceleration	0.015	0.015	0.019**	0.014	0.035**	-0.022	0.017**	0.041**	0.018**	0.024**	-0.003
	(0.009)	(0.010)	(0.009)	(0.009)	(0.017)	(0.029)	(0.007)	(0.017)	(0.007)	(0.011)	(0.002)
N	8,084	2,784	5,534	5,385	1,001	365	9,598	1,979	8,743	6,788	3,917
R-squared	0.124	0.171	0.140	0.140	0.186	0.425	0.107	0.342	0.141	0.162	0.206

The table shows estimates from equation (1) comparing changes in characteristics of the jobs held after acceleration relative to the jobs before acceleration across treated and control employees in the cross-program analysis sample. Observations are at the individual level and regression controls include employee-level variables at the time of acceleration—age, gender, education, a series of pre-acceleration metrics, including positions held, companies worked for, employability, total working tenure, time since labor market entry, average tenure per position, average tenure per company and average description length per position from LinkedIn, and acceleration year FE, and treated individual group FE to fix the comparison group for each treated employee to the specific matched control employees. For control employees, we use the years after the acceleration date of the matched treated firm as the period after acceleration. Program Seed Capital distinguishes accelerators that provide (column 1) or not (column 2) funding to participants. Program Experience distinguishes between accelerators with more (column 3) or less (column 4) than 6 years since foundation (program median). Program Location distinguishes between programs headquartered in NA top counties (column 5), NA bottom counties (column 6) and LAC (column 7). Top counties include Maricopa, AZ; Los Angeles, CA; Orange, CA; San Diego, CA; Santa Clara, CA; Cook, IL; New York, NY; Dallas, TX; Harris, TX; King, WA. Employee Business Degree distinguishes between employees with (column 8) and without (column 9) formal business degrees prior to acceleration. Departed distinguishes between employees ever (column 10) and never (column 11) left incumbent company after acceleration. Standard errors are clustered at the treated individual group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8- Business Acceleration and Human Capital Accumulation

Panel A- Cross-program analysis: business acceleration and job required skills and tasks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Managerial skills				
					Cognitive		Interpersonal		Operational
	Soft skills	Thinking skills	Technical skills	Routine tasks	Information	Monitoring	Human resources	Social skills	Financial and material resources
Acceleration	0.003**	0.003**	-0.003***	-0.004***	0.003*	0.006**	0.004	0.003	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
N	10,990	10,990	10,990	10,990	10,990	10,990	10,990	10,990	10,990
R-squared	0.098	0.097	0.103	0.110	0.092	0.096	0.093	0.096	0.094

Panel B- ValleE analysis: business acceleration and employment in knowledge intensive and routine intensive sectors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment in knowledge-intensive sectors after acceleration				Employment in routine-intensive sectors after acceleration			
	Indicator	Indicator	Share	Share	Indicator	Indicator	Share	Share
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Acceleration	0.168*	0.186*	0.129**	0.192**	-0.276***	-0.367***	-0.543***	-0.545**
	(0.088)	(0.110)	(0.057)	(0.076)	(0.0914)	(0.140)	(0.152)	(0.261)
N	658	658	658	658	658	658	658	658
R-squared	0.074	0.008	0.067	0.051	0.167	0.103	0.063	0.034

Panel A shows estimates from equation (1) comparing changes in characteristics of the jobs held after acceleration relative to the jobs before acceleration across treated and control employees in the cross-program analysis sample. Observations are at the individual level and regression controls include employee-level variables at the time of acceleration—age, gender, education, a series of pre-acceleration metrics, including positions held, companies worked for, employability, total working tenure, time since labor market entry, average tenure per position, average tenure per company and average description length per position from LinkedIn, and acceleration year FE, and treated individual group FE to fix the comparison group for each treated employee to the specific matched control employees. For control employees, we use the years after the acceleration date of the matched treated firm as the period after acceleration. Standard errors are clustered at the treated individual group level.
Panel B shows OLS and IV estimates of equation (3) comparing both incidence of employment and share of time worked after acceleration in companies in knowledge-intensive sectors (columns (1)-(4)) and in routine-intensive sectors (columns (5)-(8)). Observations are at the employee level, and regression controls include employee-level variables at the time of acceleration, including age, experience, experience squared, share of potential experience in the formal labor market, an indicator for holding a manager role prior to acceleration, dummy for women and the adjusted score. Standard errors are heteroskedasticity robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 9- Business Acceleration and Networks

Panel A – Cross-program Analysis: business acceleration and employment out(in)side accelerator's network

	(1)	(2)	(3)	(4)	(5)	(6)
	Manager index		Log wages		Employability	
	Outside	Inside	Outside	Inside	Outside	Inside
Acceleration	0.009*	0.053***	0.022***	0.117***	0.021***	-0.006
	(0.005)	(0.016)	(0.007)	(0.025)	(0.007)	(0.019)
N	9,394	1,596	9,394	1,596	9,394	1,596
R-squared	0.094	0.091	0.120	0.121	0.129	0.098

Panel B- ValleE analysis: business acceleration and employment outside accelerator's headquarters

	(1)	(2)	(3)	(4)
	Incidence	Incidence	Share	Share
	OLS	IV	OLS	IV
Acceleration × Post	-0.0305	0.261	0.0402	0.302***
	(0.103)	(0.180)	(0.0642)	(0.111)
N	658	658	658	658
R-squared	0.016	-0.031	0.022	-0.071

Panel A shows estimates from equation (1) comparing changes in characteristics of the jobs held after acceleration relative to the jobs before acceleration across treated and control employees in the cross-program analysis sample by type of employment. Outside refers to jobs in organizations that did not participate in the accelerator. Inside refers to jobs in companies that did participate in the accelerator. Observations are at the individual level and regression controls include employee-level variables at the time of acceleration—age, gender, education, a series of pre-acceleration metrics, including positions held, companies worked for, employability, total working tenure, time since labor market entry, average tenure per position, average tenure per company and average description length per position from LinkedIn, and acceleration year FE, and treated individual group FE to fix the comparison group for each treated employee to the specific matched control employees. For control employees, we use the years after the acceleration date of the matched treated firm as the period after acceleration. Standard errors are clustered at the treated individual group level.
Panel B shows OLS and IV estimates of equation (3) comparing both incidence of employment and share of time worked after acceleration in companies located outside of the city of Cali where ValleE is headquartered. Observations are at the employee level, and regression controls include employee-level variables at the time of acceleration, including age, experience, experience squared, share of potential experience in the formal labor market, an indicator for holding a manager role prior to acceleration, dummy for women and the adjusted score. Standard errors are heteroskedasticity robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 10- Business Acceleration and Signalling

Panel A – Cross-Program Analysis: acceleration and employee outcomes, by the performance of the participating company

	(1)	(2)	(3)	(4)	(5)	(6)
	Manager index		Log wages		Employability	
	Closed	Operating	Closed	Operating	Closed	Operating
Acceleration	0.023***	0.009	0.044***	0.028***	0.011	0.023***
	(0.007)	(0.006)	(0.012)	(0.008)	(0.011)	(0.007)
N	3,851	7,139	3,851	7,139	3,851	7,139
R-squared	0.105	0.087	0.131	0.113	0.155	0.108

Panel B -ValleE analysis: acceleration and employee outcomes, by the performance of the participating company and the timing of employee departure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Closed		Operating		Left at closure		Did not leave at closure	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Acceleration × Post	0.116***	0.133**	0.0990***	0.194***	0.134***	0.425***	0.107***	0.0836*
	(0.0340)	(0.0581)	(0.0354)	(0.0600)	(0.0428)	(0.0673)	(0.0312)	(0.0503)
Observations	18,330	18,330	30,218	30,218	8,303	8,303	40,245	40,245
R-squared	0.368	0.010	0.410	-0.003	0.432	-0.002	0.389	0.011

Panel A shows estimates from equation (1) comparing changes in characteristics of the jobs held after acceleration relative to the jobs before acceleration across treated and control employees in the cross-program analysis sample by participating company performance. Closed (Operating) company refers to companies no longer (still) in operation by the end of the sample, as proxied by not having any employee records at the sample end date and never experienced exodus (still having employee records and never experienced exodus). Exodus is defined as company whose over 50% of employees left incumbent company at the same date. Observations are at the individual level and regression controls include employee-level variables at the time of acceleration—age and education, acceleration year FE, and treated individual group FE to fix the comparison group for each treated employee to the specific matched control employees. For control employees, we use the years after the acceleration date of the matched treated firm as the period after acceleration. Standard errors are clustered at the treated individual group level.

P*anel B* shows OLS and IV estimates of equation (3) comparing changes in log wages after acceleration between treated and control employees by the performance of the participating firm (columns (1)-(4)) and timing of departure of the employee (columns (5)-(8)). Closed (Operating) company refers to companies no longer (still) in operation by the end of the sample, as proxied by not having any (still having some) formal employee records at the sample end date. Left at closure indicates employees that leave the company at the closure of the business. Did not leave at closure refers to employees who either (i) remained with the company throughout the sample period and the company is still operating, or (ii) left the company before it closed. Observations are at the employee level, and regression controls include employee-level variables at the time of acceleration, including age, experience, experience squared, share of potential experience in the formal labor market, an indicator for holding a manager role prior to acceleration, dummy for women and the adjusted score. Standard errors are heteroskedasticity robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 11. Acceleration and Characteristics of New Hires

Panel A: Cross-program Analysis

	(1) Young	(2) Woman	(3) Education	(4) Experience	(5) Prior Employability
Acceleration	-0.058**	-0.001	0.102*	0.106***	0.125***
	(0.024)	(0.033)	(0.057)	(0.038)	(0.026)
N	16,492	16,492	16,492	16,492	16,492
R-squared	0.210	0.093	0.155	0.116	0.115

Panel B: ValleE Analysis

	(1) Young	(2) Woman	(3) Experience	(4) Prior Employability	(5) Medium and Large Firms
Acceleration	0.135**	-0.0653	0.0120	0.0661*	0.0527
	(0.0554)	(0.0520)	(0.0562)	(0.0349)	(0.0423)
N	463	463	466	466	466
R-squared	0.012	0.003	0.000	0.007	0.003

The table reports cross-sectional comparisons of the characteristics of new hires between accelerated and control businesses for the cross-program analysis (Panel A) and the quasi-experimental analysis (Panel B). Young is a dummy equal to one for individuals under 30 years of age (the median age in the sample). In Panel A, Experience is a dummy equal to one for individuals with more than three years of prior formal-sector experience. In Panel B, Experience is a dummy equal to one for individuals with more than three years of prior formal-sector experience. Share Formal is a continuous variable measuring the proportion of an individual's working history (estimated as age minus 18) spent in formal employment. Medium and Large Firms is a dummy equal to one if the individual was previously employed in a medium- or large-sized firm. Standard errors are robust to heteroskedasticity. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.