The Soft Skills Dividend: How Business Accelerators Shape Startup Employee Careers [1]

March 2025

Juanita González-Uribe
LSE, CEPR, JPAL

Marion Restrepo
iNNpulsa

Santiago Reyes
IFC

Xiang Yin
Tsinghua University

Yufeng Wang
Tsinghua University

High-growth startups fuel economic dynamism and receive substantial government support worldwide, often through acceleration programs. While the impact of these programs on businesses is well-documented, little is known about how they shape employee careers. We provide the first systematic evidence that startup acceleration enhances career trajectories through a "soft skills dividend." Using a two-pronged approach—analyzing a large cross-program dataset from the Americas and a quasi-experimental study with administrative wage data from Colombia's ValleE accelerator—we find that employees of accelerated firms earn higher wages over time without increased employability risks. These gains stem from transitions into other companies and roles requiring greater soft skills—behavioral and social traits that enable individuals to collaborate effectively—leading to more managerial, entrepreneurial, or leadership positions. Alternative explanations, such as certification, validation, or networking, are less consistent with the results. Our findings highlight the broader impact of accelerators on human capital development.

JEL Classification: G24, L26, M13
Keywords: High-Growth Entrepreneurship, Business Accelerators, Young Firms, Employees, Careers

[1] Corresponding authors: Juanita Gonzalez-Uribe (j.gonzalez-uribe@lse.ac.uk), Santiago Reyes (sreyesortega@ifc.org), Xiang Yin (yinxiang@sem.tsinghua.edu.cn) and Yufeng Wang (wangyufeng.22@pbcsf.tsinghua.edu.cn).

We thank Carloine Genc (discussant), Nick Flamang (discussant), Johan Hombert, Daniel Paravisini, Dong Yan and seminar participants at LSE Finance, Erasmus University, Nova's Entrepreneurship and Innovation Symposium, HEC Paris Entrepreneurship Workshop, Cardiff University, and Edinburgh University for their comments.

Over the past two decades, research has highlighted the outsized role of high-growth young firms in driving economic dynamism. Despite their small numbers and high failure rates, these startups are powerful engines of job creation, innovation, and investment (Haltiwanger et al., 2016; Gonzalez-Uribe and Paravisini, 2020). This shift in understanding has redirected academic and policy focus from small businesses to high-potential startups and their broader economic impact.

In response, markets and policymakers have ramped up efforts to identify and support these firms, lowering barriers to their growth. Governments worldwide collectively invest over $1 billion annually in training entrepreneurs aiming to close managerial and entrepreneurial skills gap (McKenzie et al., 2023). Business accelerators have emerged as a key intervention, fast-tracking high-growth startups by helping founders set milestones, providing rigorous feedback, and fostering rapid iteration. These programs offer training, mentorship, and sometimes funding, culminating in a high-stakes pitch that tests founders' progress. Evidence suggests accelerators reinforce the "up or out" dynamic—propelling promising ventures forward while hastening weaker ones' exit by improving access to critical resources (Gonzalez-Uribe and Hmaddi, 2020).

Yet, one key question remains largely unexplored: how do support programs for entrepreneurs including accelerators shape startup employees' careers? While these programs are designed to drive firm growth, they also aim to create quality jobs and promote economic mobility. However, research has focused almost exclusively on firm-level outcomes, overlooking potential spillover effects on workers.

The impact of accelerators on employee careers is theoretically uncertain. On the one hand, these accelerators intensify the "startup experience," accelerating learning in both success and failure—an asset in the labor market. Even when high-potential startups fail, their employees can acquire highly transferable skills, particularly soft skills. The U.S. Department of Labor defines these as "interpersonal and thinking skills needed to interact successfully with people and perform efficiently and effectively in the workplace."[2] These skills are closely tied to leadership and entrepreneurship (Rosenbaum, 1979; Stewman and Konda, 1983; Lazear, 2005) and are associated with higher wages (Almeida and Devedzic, 2022; Josten, Lordan, Krause and Yeung, 2024). This positive association between acceleration and employee careers aligns with "on-the-job" learning models, where workers accept lower wages—common in resource-constrained startups—in exchange for valuable experience (Becker, 1962; Rosen, 1972). Accelerators also provide certification, validation, and networking opportunities that may also independently enhance employees' market value.

Conversely, accelerators' fast-paced, high-risk environment may hurt employee careers by increasing job instability, as they can accelerate firm failure (Yu, 2020; Lee et al., 2024). They may also limit employees' ability and incentives to develop deep expertise, as frequent pivots and experimentation discourage specialization—particularly in technical skills, which the U.S. Department

[2] See definition of soft skills here: https://www.onetonline.org/skills/soft/

of Labor defines as the ability to design, set up, operate, and troubleshoot machines or technological systems. If the market highly values technical expertise, the "jack-of-all-trades" advantage could come at the cost of becoming a "master of none," potentially weakening long-term career prospects. While some evidence suggests that startup employment negatively affects employees' careers unless the business succeeds (Sorenson et al., 2021; Burton et al., 2018; Babina et al., 2019; Garcia-Trujillo et al., 2024), the impact of technical skills is more nuanced. For example, Josten, Lordan, Krause, and Yeung (2024) find that returns to data science skills are constantly evolving, with newer skills being rewarded while older ones become obsolete.

Assessing the impact of accelerators on employees is an empirical challenge. One major hurdle is tracking startup employees' careers—monitoring the performance of young businesses is difficult enough, let alone capturing employee compensation and subsequent career transitions. Measuring these effects requires detailed, hard-to-access data that tracks employment changes, including job mobility and periods of unemployment. A second challenge is addressing endogenous selection. Accelerators do not source and select firms randomly; applicants that are accepted often have high-growth potential and attract top talent. These same factors may independently shape employees' market value and career trajectories, making it difficult to isolate accelerators' effects.

This paper provides the first systematic evidence on how business accelerators influence employee career paths, addressing these empirical challenges with two complementary research methods that balance external and internal validity.

The first approach examines the career trajectories of employees in accelerated firms compared to matched employees in non-accelerated firms across multiple accelerator programs, using LinkedIn data. This broad scope enhances external validity but limits interpretability, as the sample includes diverse programs with varying offerings, making it difficult to pinpoint specific impact mechanisms. Moreover, the analysis relies on self-reported LinkedIn data, which may introduce biases.

The second approach prioritizes internal validity by focusing on a single partner accelerator, leveraging quasi-exogenous variation in selection and unique access to administrative wage data over time. This allows for a more precise identification of impact mechanisms but limits generalizability beyond similar contexts. Together, these methods provide a robust analysis of how accelerators shape employee careers.

In more detail, our first approach builds a comprehensive cross-program dataset of accelerator-backed startups across the Americas and a matching control group (US, Canada, and Latin America). Using Crunchbase to identify firms and LinkedIn to track employee careers over time, we apply natural language processing to job titles and descriptions, classifying occupations with the US Department of Labor's O*NET dataset. This allows us to map and compare career trajectories at accelerated versus non-accelerated firms across multiple programs, ensuring broad external validity. To estimate the impact of acceleration, we use a stacked difference-in-differences approach, comparing career paths before and after acceleration for treated versus matched employees (Wing, Freedman, and

Hollingsworth, 2024). This method accounts for potential treatment heterogeneity across firms and over time, reducing bias (Cengiz, Dube, Lindner and Zipperer, 2019; Baker, Larcker, and Wang, 2021).

Our findings reveal that employees of accelerated firms earn higher wages over time than their peers in non-accelerated firms, without facing increased employability risks. These results are robust to restricting the sample to employees and excluding founders. These wage gains stem from employees transitioning into managerial or entrepreneurial roles or joining larger companies. The effects persist even for employees from failed startups, are stronger for more experienced workers and more mature firms at the time of acceleration, are not driven by workers moving to other accelerated firms, and is there for programs providing financing support and those that offer non-monetary resources only. Consistent with a "soft skills dividend" from startup acceleration, these wage increases align with shifts in skill demands: employees increasingly move into roles that emphasize soft skills while relying less on technical expertise.

However, two key factors complicate interpreting these patterns as a causal effect of acceleration. First, endogenous selection—accelerators may favor firms whose employees are already well-positioned for career progression and that showcase budding soft skills. Since early-stage investors prioritize strong teams, acceleration may not be the primary driver of employees' improved career outcomes. Second, LinkedIn data, while extensive, relies on self-reported job titles and descriptions. Although reputational concerns may limit outright misrepresentation, individuals may still embellish their profiles. If accelerator participation heightens awareness of LinkedIn's importance, our findings could partially reflect improved profile curation rather than actual career progression.

To address these concerns, our second approach combines a quasi-experimental design with administrative wage data to complement the cross-program evidence. We exploit exogenous variation in participation in the ValleE accelerator, sponsored by Colombia's innovation agency, iNNpulsa.

Building on Gonzalez-Uribe and Reyes (2017), our empirical strategy leverages the selection process, where businesses were assessed by three randomly assigned, non-overlapping judges who independently evaluated their business plans. While the accelerator provided uniform scoring criteria, judges applied them differently, leading to systematic variation in average scores across judges. As a result, otherwise identical applicants had different probabilities of selection purely due to the judges they were randomly assigned: higher if their judge had a higher "scoring generosity" and lower if their judge was more stringent. Since the program did not adjust for these differences in scoring, this variation provides a natural source of quasi-random selection to the accelerator.

The key innovation relative to prior work is the use of Colombia's payroll tax and social security registry data, allowing us to track wages and career trajectories over time for formal employees, extending the analysis beyond firm-level outcomes.

The intuition behind our employee-level analysis mirrors that of Gonzalez-Uribe and Reyes (2017) at the firm level, but two key differences arise since not all businesses in the original applicant pool had formal employees during the analysis period.

First, our sample size is smaller. Although we observe employees at the employee-month level, the number of firms in the analysis sample decreases. Given this, we devote significant attention to ensuring the instrument remains valid and that our results are not driven by artificial statistical power from multiple employees within the same firm. Second, the group of business compliers whose treatment responds to the instrument differs. The original study exploited both Type 1 and Type 2 errors in the selection process. Type 1 errors provided a benchmark for high-potential firms, comparing businesses correctly selected for acceleration with similarly promising businesses wrongly rejected due to assignment to a stricter judge. Type 2 errors allowed estimation of effects for lower-potential firms by comparing businesses admitted by chance (due to assignment to a generous judge) with those correctly rejected. The firm-level analysis of Gonzalez-Uribe and Reyes (2017) revealed heterogenous treatment effects: the program significantly benefited already high-potential businesses but had no meaningful impact on lower-potential firms, indicating that acceleration unlocked, rather than created, growth potential.

In our setting assessing career effects for formal employees of applicant businesses, identification is primarily driven by Type 1 errors, as most businesses with formal employees at application were high-potential firms, making Type 2 errors scant in the analysis sample. As a result, our estimates capture the impact of acceleration on employees in firms that were already high-potential, some of which were mistakenly rejected, providing a valid counterfactual. This has important implications for interpretation: our findings speak to career effects within high-potential firms but offer little insight into outcomes for employees in lower-potential firms that were accelerated by mistake.

Our results from this second approach show similar patterns in the Colombian setting as those in the cross-program analysis: participation in ValleE increases employees' formal labor income, with no meaningful effects on employability, thus increasing return of formal employment without increasing risk. The significant increases in incumbent workers' wages after acceleration are linked to firm transitions, rather than wage increases while employees remain employed in the accelerated firm. Importantly, these benefits are not restricted to participating firms that eventually succeed and instead are visible in employees of ultimately failed companies. Instrumental Variables (IV) regressions using the administrative wage data and instrumenting participation with the scoring generosity of judges shows these career effects cannot be entirely explained by the selection of high-growth companies by business accelerators nor by LinkedIn window-dressing. Ordinary least squares (OLS) and IV estimates are similar in magnitude, with IV effects being economically larger. This aligns with the identification of local treatment effects (LATE) and the idea that identification stems from selection errors, reducing concerns that OLS estimates are biased upwards.

Why does acceleration lead to positive career effects for employees in high-potential businesses? The soft skills dividend explanation is reinforced by the ValleE results, which show that career benefits persist regardless of whether the business ultimately succeeds. Conceptually, the skills dividend should exist irrespective of a startup's fate—acceleration provides employees with

opportunities and incentives to develop new, market-valued skills through on-the-job learning (Rosen, 1972). This also explains why career gains are observed even when firms fail; failure itself can be a powerful learning experience.

Further supporting the soft skills dividend mechanism, we conduct three additional tests using the ValleE data. First, we find that acceleration increases the likelihood of employees moving into soft skills intensive sectors as defined using the ON*NET data to classify industries. Second, we show that acceleration facilitates transitions across industries, suggesting employees gain broadly transferable skills. Third, we find stronger effects for employees who remain longer at their accelerated firms, indicating that the benefits compound with experience.

Alternative explanations such as certification, validation, and networking cannot fully account for the positive career effects we identify.

Beyond the soft skills dividend, accelerator selection itself may enhance employees' market value by acting as a *certification* of quality, like in models where employers struggle to distinguish between productive and unproductive workers, and external signals—such as experience at an accelerated firm—improve job prospects (Spence, 1974). However, certification alone cannot explain why employees from failed firms still benefit, as poor business performance should send an (arguably stronger) negative signal to the market. Nor does it account for the increasing strength of career effects with time spent in the accelerated company. Further challenging this explanation, we find stronger effects for employees from lower-potential firms, as measured by internal program rankings. Since these rankings are not publicly available, a pure certification mechanism would predict similar career benefits across employees, regardless of firm quality, which is not what we observe.

The combined findings do not support a *validation* mechanism either, where acceleration boosts employees' confidence, effort, or productivity, like in models where entrepreneurs learn about their abilities through experience (Jovanovic, 1982). This mechanism cannot explain why career benefits persist even for employees of failed firms, where failure would likely weaken any positive signal from selection. Moreover, the effects are stronger for more experienced employees, who are less likely to be influenced by programme validation, having already established a career path.

Finally, the results are unlikely to be driven solely by *networking*, where acceleration expands professional connections beyond founders to employees, enhancing their employability and bargaining power (Rajkumar et al., 2022). Treated employees do not disproportionately move into jobs at ValleE peer firms and spend a larger share of their careers in Bogotá rather than Cali, where ValleE is based. If networking were the primary mechanism, we would expect stronger career effects within the accelerator's immediate ecosystem. Instead, the broader geographic distribution of career advancements suggests that networking alone cannot explain the observed employee career gains.

The findings strongly support the soft skills dividend channel, indicating that while other mechanisms may contribute, they are unlikely to be the sole drivers. A key question remains: does the skills dividend stem primarily from the intensified startup experience accelerators provide or from

accelerator training itself? While both may play a role, three pieces of evidence point to the importance of the startup experience.

First, the results hold even when excluding founders, who receive direct accelerator training, while employees do not participate in group sessions or mentoring. Second, the findings remain robust after removing small-team firms, where knowledge spillovers from founders to employees are most likely. Third, we show that type-1 mistake employees (those in high-potential firms mistakenly left out of the programme) still experience better career outcomes than similar rejected applicants, suggesting that simply working in a high-growth firm—regardless of acceleration—is associated with better career prospects.

The evidence pointing to the importance of an intensified startup experience does not mean acceleration training does not matter. We find evidence that acceleration appears to shape career trajectories in specific ways. While employees in both accelerated and non-accelerated high-growth firms see wage gains vis a vis similar employees form rejected companies, their career pathways differ. While employees from accelerated firms are more likely to transition into knowledge-intensive sectors and smaller firms, those from non-accelerated high-growth firms tend to follow more traditional career paths into medium and large-sized businesses. This suggests that, beyond the startup experience, accelerators may foster distinct skills or preferences that influence career choices.

Our research contributes to the growing literature on organizations aiming to identify and boost high-growth young firms, which has expanded rapidly over the past 15 years. Yet, scant research exists on their impact on employees (Gonzalez-Uribe and Hmaddi, 2020). We provide the first systematic evidence on how accelerators shape career paths, offering key insights for policy design and evaluation. Many accelerator programs are designed with workers in mind, yet their effects on employees had never been tested. We show that accelerators can influence career trajectories, highlighting the need for evaluations to extend beyond businesses and consider employee outcomes to capture their full impact. This also applies to the broader literature on entrepreneurial training (McKenzie et al., 2023), where expanding the focus to workers could help capture spillover effects on non-trained workers.

We also contribute to the literature on how startup employment affects workers' earnings. While extensive research has examined the financial consequences of entrepreneurship for founders, far less attention has been given to startup employees. Most existing work focuses on short-term wages, showing that young firms—especially small startups—pay less than large, established firms (Troske, 1998; Audretsch et al., 2001; Brixy et al., 2007). Part of this wage gap reflects worker sorting, as startups tend to hire younger, less experienced, and less educated employees who would likely earn less in any job (Ouimet and Zarutskie, 2014; Sorenson et al., 2021). Even after accounting for these differences, startups still pay lower wages (Nystrom and Elvung, 2014; Ouimet and Zarutskie, 2014; Burton et al., 2018).

Long-term career effects, however, remain largely unexplored, despite their likely importance in startup settings. Employees are not only compensated for current performance but also build

reputational capital that shapes their future career prospects (Holmstrom, 1982). Startups, often cash-constrained, frequently use back-loaded contracts (Howell and Brown, 2020), meaning lower initial wages could be offset by long-term earnings gains. Few studies have examined this dynamic. One exception is Sorenson et al. (2021), who find that Danish startup employees earn about 17% less over the following decade than those hired by large firms.

A key limitation in this literature is the failure to account for the heterogeneity of startups and how career impacts vary based on firms' growth potential. Some studies address this by conditioning on startup success, showing that employment in successful young firms correlates with positive career outcomes (Burton et al., 2018; Babina et al., 2019; Garcia-Trujillo, Gonzalez-Prieto, and Silva, 2024). However, this approach captures only ex-post success rather than the effects of working at firms with high ex-ante growth potential—businesses that could have significant career implications for employees, even if they ultimately fail.

We contribute fill this gap by using business accelerators, which specialise in supporting high-growth startups, as a research setting. Participation in an accelerator can be interpreted as an early signal of quality, which broadens research focusing solely on success cases. We show that working in accelerator-backed firms leads to positive career effects compared to employment in non-accelerated startups, underscoring the importance of selecting high-growth firms. Our findings also reveal distinct career pathways, differentiating between the effects of acceleration and those of naturally occurring high-growth firms (gazelles).

Finally, our research also contributes to the literature on employment and human capital in the private equity (PE) industry. Agrawal and Tambe (2016) find that employees of PE-acquired firms in the US experience long-term gains in employability and wages, suggesting that PE investment enhances transferable, IT-complementary human capital. In contrast, Antoni, Maug, and Obernberger (2018) show that employees of PE buyout targets in Germany face earnings declines, consistent with increased human capital risk.

More broadly, our work relates to the finance and labour literature on the effects of mergers and acquisitions (Tate and Yang, 2016; Lee et al., 2017), bankruptcies (Brown and Matsa, 2016; Graham et al., 2013), and capital structure decisions (Matsa, 2010; Agrawal and Matsa, 2013). We extend this research by shifting the focus to startup employees and the role of business accelerators—an increasingly prominent support mechanism for early-stage firms.

1. Business accelerators and their role in identifying and supporting high-growth businesses

Most firms benefit from adopting a wide range of business and management practices.[3] For micro-enterprises, they include using separate business and personal accounts. For growing businesses, they include goal setting and accountability structures. Systematic measurement of these types of practices across various countries and firm types documents that firms using better management practices are more productive and grow faster (Bloom and van Reenen 2010, McKenzie and Woodruff 2017). Despite the potential benefits, many firms fail to adopt better business and management practices. This perceived managerial and entrepreneurial skills gap has led governments to implement programs to train entrepreneurs to improve business performance. Current evidence suggests that traditional training programs have only a modest effect on the profits and sales of firms assigned to receive training.

However, significant heterogeneity exists in the types of entrepreneurs, firms, and training programs. High-growing start-ups aiming to grow rapidly and potentially attract outside funding is an essential separate category. They are particularly interesting to many policymakers because of their potential for innovation, rapid growth, and relative scarcity in developing countries (Eslava et al., 2019). Most of these firms are young and have few workers, but they differ from the micro and small firms that are also trained in other programs in terms of the types of entrepreneurs running these firms, the types of workers employed, and in the technologies and industries. Entrepreneurs starting these types of firms are often highly educated and highly motivated. The result is that these entrepreneurs are less likely to need training on basic business skills or on cultivating an entrepreneurial mindset, but instead need more specialized assistance with their business model, with positioning their firm to receive outside financing from investors, and with leading teams. The most common intensive approach is to support firms through business accelerators.

Business accelerators are 'schools for entrepreneurs', organizations that offer intensive programs of limited duration that are designed to help entrepreneurs build their ventures (Cohen and Hochberg, 2014; Gonzalez-Uribe and Leatherbee, 2017). These schools periodically take in cohorts of businesses, typically via a highly competitive selection process. Their aim is to fast-track startups by helping founders set milestones, providing rigorous feedback, and fostering rapid iteration. Additionally, these programs offer training and coaching culminating in a high-stakes pitch that tests founders' progress. Some accelerators also fund their participating businesses and often take an equity stake in the portfolio company in return.

Accelerators can be traced back to Y Combinator (YC), a US-based technology startup accelerator established in 2005 in Cambridge, Massachusetts. Four years later, the Difference Engine

[3] This background section on business accelerator research closely follows closely McKenzie et al (2023) and Gonzalez-Uribe and Hamadi (2023).

kick-started the accelerator model in Europe. Since then, the number of accelerators has grown rapidly worldwide. The online platform Crunchbase has listed more than 45,000 participants in close to 1,600 accelerator programs since 2005. These programs have attracted a substantial amount of research by academic and practitioner researchers. The online scholastic archive Social Science Research Network (SSRN), for instance, lists almost 800 entries on the topic since 2005, with over two-thirds of the posts dated 2018 or later.

Several review papers, including those by Bone et al, 2019 and Gonzalez-Uribe and Hmaddi (2023) have synthesized the collective knowledge from research on business accelerators. By now, there is good evidence that business accelerators usually succeed in increasing the average performance of participating businesses. Changes in average performance do not convey the full nature of the effects, however. Accelerators also tend to affect each end of the spectrum of entrepreneurs by both, ushering promising participants into the upper echelons of company growth and steering less apt participants to fold faster. However, the impact of business accelerators on startup worker's career paths remains understudied.

The main aim of this study is to fill that gap. We begin the analysis using a cross-program approach by building a large dataset of companies that participated in business accelerators from the online platform Crunchbase. We summarize the sample and analysis from this cross-program approach in Section 2. We then turn to a quasi-experimental approach and administrative wage data focusing on a single business accelerator in Colombia, ValleE that has been shown to be successful at identifying and supporting high-growth young firms and that offers only capability-building and no money and takes no equity—thus offering a cleaner set up to test a skills-based treatment story. We summarize the quasi-experimental setting, data, approach, and analysis in Section 3. Finally, in Section 4 we explore the mechanisms of impact and provide supportive evidence on the skill-dividend that cannot e explained by other mechanisms like certification, validation and networking.

1. The Cross-Program Analysis

To explore the startup workers' career paths in beneficiary firms of business accelerators, we begin by assembling a dataset of companies that participated in these programs. We combine several data sources that we originally accessed in October 2024, including the online platforms Crunchbase, LinkedIn and the O*NET dataset from the US Department of Labor.

1.1. Business Accelerators in the Americas

We restricted the search to business accelerator programs in the Americas to cover both the advanced economies of the US and Canada, and the developing countries in Latin America and the Caribbean (LAC). While many of the most famous accelerator programs like Y-combinator and Techstars are based in developed economies like the US and Europe, business accelerators have spread globally, with a strong presence in LAC (Gonzalez-Uribe and Hmaddi, 2023).

LAC exhibited the fastest growth in venture capital fundraising during 2021.[4] VC funding in the region surged to levels equivalent to the total equity financing raised through initial public offerings in the main local markets, surpassing the volume of corporate debt financing. These amounts became significant enough to draw the interest of policymakers which have further supported the emergence of local business accelerators and other support institutions. Although the region is home to a large, young, tech-savvy population, the startup ecosystem only began to develop more quickly with the spread of support programs for early-stage entrepreneurs.

The LAC region was an early adopter of accelerators during the 2000s. According to CrunchBase and PitchBook data, at least 80 programs have been created in the region since 2003. This estimate is most likely a lower bound, given that these data sources trace investments and many of the accelerator programs in the region provide no capital. A prominent example is Start-up Chile (SUP), which was launched in 2010 as a business policy response to Chile's earthquake and tsunami that year (see Gonzalez-Uribe and Leatherbee, 2017). It quickly became an important reference for the region, as well as for government-backed programs elsewhere. The LAC region was also home to the first local office of Endeavor, the leading non-profit organization supporting high-impact entrepreneurship globally.

Support programs by local accelerators for early-stage entrepreneurs are some of the most active investors in the region, according to a recent report by SlingHub. Topping the list is SUP, with nearly 1 percent (250) of all LAC startups receiving funds from this accelerator. Another active accelerator program is Wayra—Telefonica's corporate investment arm that started out as an accelerator—which ranks third on the list with 164 invested startups. Nondomestic accelerators topping the list include 500 Startups and YC, which rank fifth (114 invested companies) and eighth (91 invested companies), respectively. Notably, Endeavor's Catalyst Fund has backed 22 of the LAC unicorns and selected Argentinian "decacorn" Mercado Libre in 1999 as one of its first investments.

These business accelerators helped to create demand for VC in the region by selecting and training large pools of participants before they went on to raise venture capital. For example, more than one-third of LAC unicorn companies are alumni of accelerators. By several accounts, these programs have also spawned domestic entrepreneurs. For example, SUP led to higher business creation rates in the industries targeted by the program, as well as in areas close to the program's headquarters in Santiago de Chile (Gonzalez-Uribe and Leatherbee, 2016; Rudolph, Miguel and Gonzalez-Uribe, 2023).

1.2. Sample

[4] This background section on business accelerators and venture capital in Latin America closely follows Rudolph, Miguel and Gonzalez-Uribe (2023).

Our analysis leverages a multi-stage filtered sample of 9,435 accelerator-participating startups (8,781 in North America (NA), 654 in Latin America/Caribbean (LAC)) derived from Crunchbase's comprehensive accelerator and startup database. We excluded companies associated with small accelerators (<20 investments) to ensure program quality comparability, filtered by geographic constraints of accelerators with a focus on NA/LAC innovation ecosystems, restricted first accelerator participation to 2005-2019 to avoid Covid-related concerns and verified founding year requiring startups to be $\leqslant$5 years old at acceleration entry.

Matching at the firm level

To create a control group, we start by implementing stratified matching protocols at the firm level: for each treated company in NA, we identified 5–6 control startups based on the following matching criteria: (1) same country and state, (2) same founding year, (3) same industry (which is classified by Crunchbase; if there are multiple industries, they should have at least half of the claimed industries in common); for each treated company in LAC, we identified 20 control startups based on the following matching criteria: (1) same country, (2) same founding year, (3) similar industry (companies with maximal industry similarities).[5] The treatment to control ratio of the LAC is intentionally higher than for the NA sample, given the higher propensity of NA employees to report their career trajectories in LinkedIn. However, in robustness checks, we show that results are similar if we use the same treatment/control ratio for the LAC sample (5-6 control firms for every accelerated business).

Having constructed this firm level data, we then collect worker profiles from LinkedIn of all employees that have ever worked at each company. Through this process, we identified 198,593 NA employees (from 3,691 accelerated firms) and 447,515 LAC employees (from 462 accelerated firms). For most of the analysis, we focus on employees who were working at the company at the time of its first participation in the business accelerator program. We refer to this group as "incumbent employees". We excluded firms with with too many incumbents at acceleration (>500) and disproportionate service-sector employment (e.g., Rappi drivers) through manual validation of LinkedIn job titles. We identify the incumbents of each company based on worker profiles on LinkedIn and yield 6,642 NA incumbents from 1,777 accelerated firms and 14,928 NA incumbent controls, and 2,893 LAC incumbents from 389 accelerated firms and 48,171 LAC incumbent controls.

*Linking to O*NET*

We link a given job position and description to an O*NET job title to categorize jobs in several ways, like average expected salaries as well as the different knowledge, skills, and abilities required to perform the job responsibilities. ONET is a comprehensive system developed by the U.S. Department

[5] In robustness checks we show results are the same if we use 5-6 control firms for LAC businesses.

of Labor that contains hundreds of standardized and occupation-specific descriptors on almost 1,000 occupations covering the entire U.S. economy. The database, which is available to the public at no cost, is continually updated from input by a broad range of workers in each occupation by the U.S. Department of Labor/Employment and Training Administration (USDOL/ETA).

To perform this match, we use a commercially available system commissioned by the US Department of Labor to assign O*NET 2023 codes to job descriptions at an accuracy level that exceeds the level achieved by human coders. The system is based on an algorithm that splits the text of the job description into its individual words and phrases, which are then matched to a database of words and phrases associated with O*NET codes. The words in the database have been reviewed and weighted so that the most important words for a given occupation are given more importance in the match calculation. The result is a series of matched O*NET-SOC occupation codes with respective scores for any job position in the employees' LinkedIn profiles.[6] Using these occupation codes, we then characterize jobs in terms of the expected salary, and skills, knowledge, and ability requirements, using a commercially available interactive tool for job seekers and students to learn more about their career options developed and maintained by the National Center for O*NET Development, under the sponsorship of the US Department of Labor/Employment and Training Administration.

The underlying salary data source of O*NET corresponds to survey data in the Occupational Employment and Wage Statistics (OES) data developed by the Bureau of Labor Statistics (BLS) in the U.S. The OES survey measures occupational employment levels and wage rates for wage and salary workers in nonfarm establishments in the 50 states, the District of Columbia, Guam, Puerto Rico, and the Virgin Islands. Estimates of occupational employment and wage rates are based on six panels of survey data collected over a 3-year cycle. The final in-scope sample size when six panels are combined is approximately 1.2 million establishments. The OES data is widely used in the economics literature to document local employment and wage levels (Hershbein and Kahn, 2018). Unlike deriving salary data from job postings, which has several concerns as illustrated by Batra, Michaud, and Mongey (2023), the ONET code-specified salary level is maintained by the U.S. government and exhibits time-region-varying characteristics. We use the year-state-varying salary data spanning the 2009 and 2023 period. We use the average across US states for positions in LAC companies in a given year.

The underlying information on job-level skills, knowledge, and ability requirements is organized under the O*NET Content Model at the O*NET Resource Centre. These data have also been used in the economics literature (Autor, Levy and Murnane, 2003; Autor and Dorn, 2013). O*NET use a continual data collection process aimed at identifying and maintaining current information on the characteristics of workers and jobs. There are three primary sources: incumbents, occupational experts,

[6] If a code is the top selection of all methods (and meets expected match thresholds and separation from other codes), then the overall match score will approach 100, the maximum possible. Scores above a score of X accurately predict the correct code at least X% of the time. Accuracy rates drop off rapidly when scores are in the 60s or lower.

and occupational analysts. Targeted job incumbents provide ratings on knowledge (among others). Ratings on abilities and skills associated with occupations are collected from occupational analysts.

We focus on the ratings pertaining to skills, knowledge, and abilities. Skills and knowledge are descriptors referring to work-related attributes acquired and/or developed through experience and education (classified under *Worker Requirements* in the Content Model). Knowledge represents the acquisition of facts and principles about a domain of information. O*NET classifies knowledge into ten main areas: business and management, manufacturing and production, engineering and technology, mathematics and science, health services, education and training, arts and humanities, law and public safety, communications, and transportation. Experience lays the foundation for establishing procedures to work with given knowledge. These procedures are more commonly known as skills. O*NET considers 35 types of skills that are grouped into two broad categories, and seven sub-categories. The two board categories are basic skills and cross-functional skills. Basic skills facilitate the acquisition of new knowledge and include two sub-categories: content and process. Cross-functional skills extend across several domains of activities and facilitate the performance of activities that occur across jobs. They include five sub-categories: social, complex problem-solving, technical, systems, and resource management. Finally, abilities are enduring characteristics of the individual that influence performance (classified under *Worker Characteristics* in the Content Model). O*NET considers four sub-categories: cognitive, psychomotor, physical, and sensory. These are further classified into 15 sub-categories.

For this first type of textual analysis of LinkedIn job positions and descriptions based on O*NET, we restrict the sample to a random sub-sample of LinkedIn profiles, given the high computational demands associated with the method. We refer to this sub-sample as the O*NET sub-sample throughout.

To create the O*NET sub-sample, we followed a two-step approach. First, we randomly selected 400 unique treated startups (roughly 10% from the treatment group) that participated in 188 business accelerators from North America (NA), and their 1,104 control firms matches (control-to-treated ratio 2.8). These companies have 54,510 unique individual LinkedIn profiles. After restricted to incumbents who participated in the very first business accelerator program, we got 366 incumbents from 162 unique startups and matched them with 3,979 employees from 540 control firms (control-to-treated ratio 3.3). Then, to enhance geographic diversity and balance representation across developed and developing regions, we complemented this sample with a Latin America and Caribbean (LAC) sample, maintaining comparable matching ratios at both the firm and employee levels. From this, we randomly selected a LAC subsample with 691 incumbents from 204 treated companies and 9,174 employees from 659 control firms (control-to-treated ratio of 3.2). We then implemented the state-of-art NLP techniques (OpenAI GPT 4o-mini model) to translate each profile from foreign languages into English to conduct further analysis.

In total, the combined sample has 14,210 incumbents, including 1,057 from 366 treated firms and 13,153 from 1,199 control firms, achieving an overall control-to-treated ratio of 3.3. Our

construction ensures geographic representativeness (44.3% NA, 55.7% LAC in final sample), and counterfactual validity through matching (average 3.3 control firms per treated in NA, 3.2 in LAC).

Having assembled a dataset of career paths for incumbent employees of both treated and control firms, we use novel textual analysis of LinkedIn job positions and descriptions in two ways: linking the data to information from The Occupational Information Network, and characterizing positions in terms of the likelihood they are managerial or entrepreneurial jobs.

Matching at the employee level

To further polish the control group, we also match incumbent employees of treated firms to employees of the control companies based on individual characteristics. In detail, we use a propensity score matching (PSM) algorithm based on an extensive set of variables:

(1) employee from same treated firm group (2) year of joining the firm (3) same gender (4) age (5) education (6) number of LinkedIn followers, (7) number of LinkedIn connections (8) the average length (word count) of descriptions for each position before incumbent position on LinkedIn (9) number of positions taken before joining incumbent company (10) number of companies joined before joining incumbent company (11) employability (working time / total time) measured from the entry of labor market till the time joining the incumbent company (12) total working tenure before joining incumbent company (13) total time from the entry of labor market to the time joining the incumbent company (14) average tenure in each position before incumbent position (15) average tenure in each company before incumbent company.

The PSM process achieved a successful matching rate of 89.5%, yielding a refined subsample of 946 incumbents from 316 treated firms (35.8% from North America and 64.2% from LAC) and 9,973 control employees from 774 control firms, resulting in a control-to-treated ratio at the firm level of 2.4. A total of 111 treated incumbents could not be matched to suitable control counterparts, due to missing or invalid LinkedIn data across the matching variables. This outcome underscores the granularity and precision afforded by individual-level employee matching compared to firm-level approaches. Subsequently, we mirror the company-level matching process to conduct further analysis.

Textual analysis Managerial and Entrepreneurial Job positions

The second type of textual analysis we perform on LinkedIn job occupations and descriptions is applying natural language processing (NLP) models to systematically characterize jobs as “managerial” or “entrepreneurial”. The main challenges are defining managerial and entrepreneurial roles, and systematically coding job positions and descriptions as satisfying those definitions. Jobs can be considered by researchers to be managerial or entrepreneurial even though the terms “manager” or “entrepreneur” are not explicitly written in the title or description. However, systematically classifying job positions in a large sample manually is daunting given the sheer size of the data (over ten million

jobs for the entire sample) and can lead to inaccuracies and noise as any manual method would be subject to a subjective interpretation by researchers.

To address these challenges, we follow Li, Liu, Mai and Zhang (2021) and offer a neural network algorithm that starts with a word-embedding model to obtain word lists for "manager" and "entrepreneur" terms based on each word's proximity to the respective term in job titles and descriptions. Using these word lists, we then estimate scores for how possible a particular job position and description in each LinkedIn profile indicates a manager-like or entrepreneur-like job. We define the scores for "managerial" and "entrepreneurial" jobs in two different ways, and the output of both approaches corresponds to the job-level measures of the degree to which the job corresponds to a managerial and entrepreneurial role. An essential advantage of this type of analysis relative to the analysis based on the link with O*NET is that we can perform it on the entire sample as the computational requirements are lower. Figure 1 presents the flow chart of the neural network algorithm.

The word-embedding model is based on the simple linguistics concept stating that words that co-occur with the same neighboring words have similar meanings (Harris (1954)).[7] Thus, such a model converts the neighboring word counts of a word to a numerical vector, which captures the word's meaning and supports a synonym search using vector arithmetic. Although there are different variants of the word-embedding model, following Li, Liu, Mai, and Zhang (2021), we use a popular neural network algorithm, word2vec (Mikolov et al. (2013)), to efficiently learn dense and low-dimensional word vectors. In essence, word2vec "learns" the meaning of a specific word via a neural network that "reads" through the textual documents and thereby learns to predict all its neighboring words. The output from the process is a vector representation of the word once learning has been completed after several iterations through the documents. The vector has a fixed dimension and captures the properties of the original co-occurrence relationship between the word and its neighbors.

We utilized the *gensim* library in Python to train a Word2Vec model on our dataset. The model was configured with a word vector dimensionality of 300, a context window of 8 neighboring words (spanning a maximum distance of seven words within a sentence), and a minimum frequency threshold of 5 occurrences, excluding words appearing less frequently in the job positions and descriptions corpus. Named entities within the LinkedIn corpus were identified and filtered using the CoreNLP package. After training, the model transformed a total of 106,039 unique words in the LinkedIn job positions and descriptions corpus into 300-dimensional vectors, representing the semantic meaning of each word. These vectors subsequently served as the foundation for further natural language processing (NLP) analyses.

Using this capability, we construct the managerial and entrepreneurial word list by associating words gleaned from job positions and descriptions in the LinkedIn sample. We then select the top 1000 words with the closest associations (i.e., the highest cosine similarity between their word vectors) to the

[7] This section closely follows Li, Liu, Mai and Zhang (2021)

word vector for "manager" and "entrepreneur".[8] We use ChatGPT (4o-mini model) manual inspections to inspect all the words in the auto-generated list and exclude words that do not fit.[9] Most of the excluded words are either too general in meaning (e.g., corporation and organization) or too specific regarding industry context (e.g., E-commerce platform and biotech funding). Appendix 1 in the Online Appendix provides the word lists for managers and entrepreneurs, ordered by descending similarity to the words indicating managers and entrepreneurs. The procedure creates two 43-word and 39-word lists or dictionaries for entrepreneurial and managerial jobs, respectively.

With the dictionaries in hand, we classified jobs as managerial or entrepreneurial based on the prevalence of dictionary-defined keywords within job titles and descriptions. To quantify this, we employed three distinct scoring methodologies. The first is *word count*, measures the frequency of dictionary keywords within each experience corpus, normalized by the total length of each description. The second approach is *TF-IDF*, which leverages the product of two metrics: term frequency (TF), which reflects the occurrence of keywords within a specific job description, and inverse document frequency (IDF), which downweighs terms that appear frequently across all descriptions, thereby emphasizing more distinctive keywords. Intuitively, this method provides lower scores to words that appear with a high frequency across all documents. The third method computes the *cosine similarity* between the full set of words in the job description and the dictionary keywords scaled by the total length of each description, which captures a nuanced measure of semantic proximity with the high-dimension distance between the entire description and the target keyword dictionaries.

1.3. Empirical Strategy of the Cross-Program Approach

We are interested in examining changes in the career paths of employees after their employer participates in an acceleration program. To uncover potential changes in employee career paths, we conduct three types of empirical analysis.

First, we characterize the jobs that people hold before and after their high-growth employer is accelerated (or not). The characterization focuses on skills, knowledge and abilities required for the job, the managerial and entrepreneurial traits of these positions, and the average salaries paid in those jobs. We categorize job positions for every employee into two groups according to the experience date relative to the company's participation in the business acceleration program. For control firms, we use the date of participation in the accelerator of the treated company as the reference date. Because Table

[8] For entrepreneur-like job, we also use stem words "founder" and "self-employed".

[9] The prompt we use for entrepreneur-like job in ChatGPT is "*I am going to feed you 100 words that are probably related to a self-owned business experience in one's resume. These words are from job descriptions. But some of them are vague and noisy. I need you to remove all those words that fail to clearly suggest a self-owned business experience or an entrepreneur experience. Please filter these words and show me the most possible 10 to 20 words that can suggest and offer reasons for each selected word."* The prompt for manager-like job is similar.

1 shows that 63% of incumbent employees change positions within the sample period, with an average number of positions held after acceleration of 1.87. Most position changes involve employer changes: Table 1 shows that the fraction of incumbent employees that change employers is 63% and the average number of new employers after acceleration is 1.81.

We estimate the following type of cross-sectional equations comparing the average changes in the characteristics of jobs held by individuals before and after the acceleration of their employer:

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

Where Y_j represents the change in the average 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 the position after acceleration, then Y_j is set to zero. $Acceleration_i$ equals one if the original firm i participated in an accelerator, 0 if it is a control firm. For control firms, we fix the date of potential acceleration to the date that their matched participant firm was accelerated. To make sure that we fix the comparison within a given treatment firm and its matched control companies, we include in the regression fixed effects for accelerated firms and their matched control group, $\gamma_{\hat{\imath}}$. X_j is a vector of controls at the employee level including gender, age, experience, and education. We want to control for potential age effects; individuals become more productive as they get older, and experience effects: individuals become more productive as they get more experienced– this is independent of whether they choose to invest in on-the-job training. Standard errors are clustered at the firm i's group to acknowledge that the level of variation is at the treated firm level. The coefficient of interest is ρ capturing the average difference in job characteristics, after acceleration, for incumbent employees of the accelerated firm and relative to the employees of its matched controls. In some of the regressions we fix the end-time of the post-acceleration period to specific windows (1 year, 2 years etc.) rather than the end of the sample (Octorber 2024), to compare short-term and long-term job characteristics after acceleration.

Second, we examine differences in long-term employability. The first type of analysis is inherently limited to individuals who remain employed, excluding those who leave their employers and fail to secure a new job within the sample period. Even for individuals who do find new jobs, this analysis does not account for variations in the time it takes to secure employment. As a result, it overlooks potential differences in unemployment durations between individuals whose employers participate in an accelerator program and those whose employers do not. To investigate potential differences along these dimensions, the second type of analysis estimates, for each individual, the proportion of time they were employed—either with their current employer or another firm—from the moment of acceleration (or its absence for the control group) until the end of the sample period in June 2024, following a similar methodology as in Agrawal and Tambe (2016). Having constructed this

measure for every incumbent employee in our sample, we then test for average differences in employability across treated and control employees using the same regression (1) with Y_j equal to the long-run employability of employee *i*. In some of the analysis we fix the post-acceleration period to specific windows (1 year, 2 years etc.) rather than the end of the sample, to compare short-term and long-term employability after acceleration.

Third, we combine the first two types of analysis into a single regression that compares the time-series average *wage* and *earnings* differences across treated and control incumbent employees before and after acceleration. For every incumbent employee in each period, we define earnings as equal to zero if they are unemployed and equal to the expected wage if they hold a job position.

We re-organize the data in acceleration-event time, and run stacked differences-in-differences regressions as proposed by Wing, Freedman, and Hollingsworth (2024) to estimate the "trimmed aggregate ATT (average treatment on the treated)" on wages and earnings within three years of participation. We trim the data to create a balanced sample over a fixed event time window of 5 years around the acceleration time, 2 years before and 3 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; see Wing, Freedman, and Hollingsworth (2024). We estimate the following regression,

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

Where Y_j represents the wages or earnings of incumbent employee *j* at time *t*, $Acceleration_i$ equals one if the original firm *i* participated in an accelerator, 0 if it is a control firm; X_j is a vector of controls at the employee level (at the time of acceleration) including gender, age, experience, and education, and D_h are time event dummies around the acceleration event. For control firms, we fix the date of potential acceleration to the date that their matched participant firm. To make sure that we fix the comparison within a given treatment firm and its matched control companies, we include in the regression fixed effects for accelerated firms and their matched control group, γ_i. Standard errors are clustered at the firm i's group to acknowledge that the level of variation is at the treated firm level. The coefficients of interest are the β_h capturing the average difference in earnings within h years of acceleration, for incumbent employees of the accelerated firm and relative to the employees of its matched controls.

1.4. Characterizing Jobs by Required Skills, Knowledge and Abilities in O*NET sample

We begin our analysis by using radar charts to illustrate changes in the nature of job positions for treated and control employees separately as illustrated in Figure 2. For each job characteristic—skills, knowledge, and abilities—we create two radar charts: one for treated employees and one for control employees. Each chart includes two plots: one representing the characteristics of the job position held during the acceleration event and the other representing the average characteristics across all subsequent job positions after acceleration. For the control group, we use the date of the matched treatment firm's acceleration event as the reference point. In these charts, each spoke corresponds to a specific sub-category. For example, the knowledge radar charts include 10 spokes, one for each knowledge sub-category defined by O*NET. Job characteristics are expressed relative to the average job across the entire O*NET database, which is represented by a spoke of length 1 in the plots. A spoke with a value of $X>1$ ($X<1$) indicates that the job requires X times more (less) of that sub-category compared to the average job in the O*NET sample.

Panel A in Figure 2 depicts the skills radar charts. The left (right) plot corresponds to the treated (control) employees. The figure reveals two main findings for acceleration and the skill requirements of the job positions of employees. First, during the acceleration event, incumbent treated and control employees are in job positions with similar skill requirements. The most important skill requirements with scores (treatment vs. control) above 1.0 in these job positions are the cross functional skills of resource management (2.22 vs. 2.47), followed by systems (1.79 vs. 1.79) and social (1.10 vs. 1.21). All other subcategories, including basic skills, have average scores below 1.0. The second result from the skills radar chart is the significant shift, especially for cross-functional skills for the treated group versus the control group. The treated group sees a positive shift in cross-functional resource management, systems, and social skills, and a decrease in technical skills. The shifts in the control group are less pronounced, with a single significant positive shift in cross-functional resource management skills.

Panel B in Figure 2 reveals two main findings for acceleration and the knowledge requirements of the job positions of employees. First, prior to acceleration, the job positions for both treatment and control employees are very similar: the most important knowledge sub-categories with average scores (treatment vs. control) above 1.0 in these jobs are: Engineering and Technology (1.39 vs. 1.30), Business and Management (1.34 vs. 1.39), Communications (1.31 vs. 1.40) and Arts and Humanities (1.03 vs. 1.05). All other sub-categories have average scores below 0.10 including: Education and Training, Health Services, Law and Public Safety, Manufacturing and Production, Mathematics and Science, and Transportation. The second finding is that after acceleration, there are important shifts in the knowledge required in the job positions of treatment employees, which are not visible for control workers. The biggest positive changes are for the subcategories of communications and business and management, which increase 29% (1.70-1.31/1.31) and 8% (1.34-1.24/1.24), respectively. There are

also negative shifts in Engineering and Technology, Education and Training, Health Services, and Manufacturing and Production.

Finally, Panel D in the Figure shows the radar charts for job requirements in terms of abilities. The charts indicate the predominant importance of cognitive abilities for high-growth startup jobs, which are almost indistinguishable between treatment and control employees prior to acceleration. The chart also shows the significant increase in requirements for cognitive abilities for treated employees after acceleration, which is not visible for the control employees. The sub-categories corresponding to cognitive abilities with the most important changes are memory and quantitative abilities. Other cognitive abilities like spatial and attentiveness have very little or nil importance for the jobs of high-growth startup workers.

We complement the visual analysis on pre and post job positions with regression analysis comparing incumbent positions and all subsequent positions. In unreported analysis we also focus on the first subsequent position and the last position. For every employee and every subcategory of knowledge, skills and abilities, we define two variables: *Change dummy*, indicating whether the average score for the given subcategory of the subsequent positions differs from the score in the incumbent's position and *Change*, equal to the difference between the average score of the subsequent position and the score of the incumbent position. Using these variables as outcome variables, we estimate cross-sectional regressions comparing treated employees and control employees. We restrict the comparison to employees working for the treated company and their respective control company matches by including the treated group fixed effect. The coefficient of interest is the estimate on acceleration, which captures the average difference in the job requirements for a given knowledge, skill and ability subcategory after acceleration across treatment and control employees. To conserve space, summary statistics of these variables are in Appendix 2. Results from the regression analyses using these variables are not reported to conserve space.

Soft Skills

Figure 2 shows a significant shift in the skills, knowledge and capabilities demands of the jobs that employees of accelerated firms hold after acceleration. Interpreting these shifts is hard without any more specific structure to help analyze job skills. To that end we shift our attention to specifically focusing on Soft skills – the behavioral and social traits that enable individuals to work harmoniously with one another. In recent years, the importance of soft skills for productivity has been documented in several papers (Guadalupe and NG, 2022). A number of targeted eary childhood and school interventions have been proven to enhance soft skills and outcomes later in life (Kautz, Heckman, Diris, ter Weel, and Borghans, 2014 Algan, Beasley, Cote, park, Tremblay, and Vitaro 2022). But soft skills are malleable and can be developed in adulthood too. On-the-job training programs can play a part in

increasing the level of soft skills (Adhvaryu, Kala and Nyshadham, 2018). We investigate the evidence of accelerator programs in Table 3.

We begin by classifying social skills and thinking skills as soft skills following the O*NET definition. Social skills include: coordination, instructing, negotiation, persuasion, service orientation, and social perceptiveness. Thinking skills include: active learning, active listening, complex problem solving, critical thinking, judgment and decision-making, learning strategies, monitoring and time management. We then estimate the change in the requirements of these skills for each employee across the jobs they hold before and after acceleration using Equation (1).

Results in Table 3 show evidence of a strong and significant increase in soft skills after acceleration, driven by increases in both social and thinking skills. The results are robust to using the sample matching at the firm level (Panel A) and employee level (Panel B). Figure 2 shows that the change in skills does not represent an upward shift in all skills, which would instead possibly reflecting a trend that we do not properly control in our regressions. Indeed, Table 3 shows that while soft skills increase, there is a decrease in technical skills, robust across the two samples: matched at firm and employee level.

1.5. Characterizing Jobs as Managerial and Entrepreneurial Roles in the Full Sample

The shift in skills and in particular the increase in soft in skills may be tied to a greater likelihood of participants pursuing entrepreneurial roles after completing the program. Similarly, accelerated employees might be more likely to move into managerial positions. The intense startup experience provided by accelerators can reinforce the "generalist" work patterns seen in startups, as argued by Dahl, Canales, and Burton (2021). If acceleration amplifies this effect, it could fast-track individuals into top management, explaining their rapid ascent in the corporate world and associated salary premiums (Kaiser and Malchow-Møller 2011; Baptista et al. 2012; Campbell 2013; Luzzi and Sasson 2016; Custodio, Ferreira, and Matos 2013).

Startups, as noted by Sorenson et al. (2021), begin with limited resources and lack established structures, forcing employees to multitask and develop a broad skill set. This dynamic, characterized by frequent role changes and uncertainty, effectively serves as a training ground for future management roles (Stinchcombe 1965; Freeman et al. 1983; Yang and Aldrich 2017; Sørensen 2007; Campbell 2013).

To explore these possibilities, we turn to the second type of NLP analysis we perform on the LinkedIn job positions and descriptions described in section 2.2. We generate multiple variables to describe the degree to which job positions appear managerial and entrepreneurial. Like the skill analysis, we structure the data at the employee job position level. We define the following variables

comparing the propensity of managerial and entrepreneurial jobs across positions: *Change score dummy*, indicating whether the score in the next position differs from the incumbent position score; *Score change for next position*, the difference in scores between the incumbent and the next position, *Position score change ratio*, the change ratio in scores between the incumbent position and the next position, calculated as the difference divided by the incumbent position score.

As with the salary analysis, beyond looking at the most immediate changes in job characteristics across the incumbent and the next position, we are also interested in capturing long-term changes reflected in subsequent job transitions. To capture long-term changes, we define the variable *Position score change (ratio)all*, the difference in scores between the incumbent position and the average of all subsequent positions (divided by the incumbent position score). We also define variables using a fixed window over time for the analysis: *Position score change (ratio)_avg2*, which corresponds to the difference in scores between the incumbent position and the average of next positions held over the next 2 years (divided by the incumbent position score). We replace the variables to 0 for employees with no job transitions. We use the entire sample of incumbent employees for the analysis; employees with single job experiences are dropped from the regression (99=4601-4502). Results are the same if we use the O*NET subsample. We present results using both the TFIDF and cosine similarity score measures. To conserve space, Appendix 2 presents summary statistics for the managerial and entrepreneurial attributes variables.

Table 4 summarizes results from estimating equation (1) using these variables as outcome variables. They show evidence of higher propensity of entrepreneurial and managerial jobs after acceleration for treated employees relative to control employees. The positive difference begins with the first immediate job transition (column 3), remains for the rest of the job transitions until the end of the sample (column 7), and is also there if we consider any job transitions within a window of 2 years of acceleration (column 6). Some of the positive difference is driven by extensive margin effects: column 1 shows that treated employees are more likely to have a change in the score across positions. Results are similar across the TFIDF and cosine-similarity measures. In terms of economic magnitudes, the estimates in column 5 of both panels imply post-acceleration average increases in the propensity of holding entrepreneurial and managerial roles of close to 11% and 13%, respectively, for treated employees relative to control employees (based on the cosine-similarity measure). The economic magnitude is similar if we restrict the attention to the immediate next job after acceleration (column 2), or to consider the jobs within two years of acceleration (column 4).

1.6. Characterizing Jobs by Expected Average Salaries in O*NET sample

Tables 3 and 4 show significant changes in the skills, knowledge and abilities associated with subsequent jobs, as well as an increase in the probability of holding managerial and entrepreneurial

positions. These changes in subsequent jobs may translate into positive or negative changes in expected salaries. While managerial positions typically command a higher expected compensation, entrepreneurial positions may come at a discount. The shifts in skills, knowledge and abilities, have likewise ambiguous implications for compensation: some of the shifts are positive and some negative. To investigate the changes in expected salaries for subsequent positions, we turn to expected salary data.

We begin with a simple plot comparing the cross-job salary variation across the two groups illustrated in Figure 3. We organize the data around the acceleration timing but collapse observations to the employee-job position level. The figure plots the average wages for treatment and control groups around "acceleration event job positions," denoting the jobs after (before) acceleration for every employee with positive (negative) event number indicators. For a given position, only individuals with such a position estimate the average wage plotted in the figure, and the wage corresponds to that in ONET. Panel A in Figure 3 shows that conditional on having a job, treated and control employees have similar levels and trends in wages before acceleration. After acceleration, however, the figure shows a clear widening gap in the subsequent jobs' salaries across the two groups.

Table 5 shows that the widening gap in Panel A of Figure 3 is statistically different from zero. We present results from estimating equation (1) using the following outcome variables: *First position salary change dummy*, indicating whether the salary in the next position differs from the incumbent position salary; *Log first position salary change,* the log difference between the next and the incumbent salary; *Log all position salary change*, corresponding to the log differences between the incumbent salary and the average of all subsequent positions, to capture long-term career effects. The table shows positive wage changes after acceleration. In terms of economic magnitude, we find an average increase in expected annual salary after acceleration (on average 4 years) of approximately $12,000 dollars, which corresponds to roughly 14% of the sample mean.

In unreported regressions we show that positive wage results are robust to using use the whole ONET sample and replacing with zero the variables for employees with no job transitions or using the conditional sample of employees that hold different jobs after acceleration. In unreported regressions, we also show that the positive difference in wages begins with the first immediate job transition, remains for the rest of the job transitions until the end of the sample and is also there if we consider any job transitions within a window of 2 years of acceleration. Some of the positive differences are driven by extensive margin effects: treated employees are more likely to change salaries across positions and see a salary increase.

1.7. Long-Run Employability and Departure Rates in O*NET Sample

In this section, we examine whether the positive salary increases associated with acceleration come at the cost of reduced employability. We start by plotting the employment probability of incumbent employees in the treated and control groups at different time intervals following acceleration. Panel B in Figure 3 shows that employment probabilities remain similar across the two groups for up to three years after the acceleration event. However, by year four, a divergence emerges, with the probabilities taking different trajectories thereafter, and treated employees exhibiting higher employability probability, though economically very similar 94% versus 92%. This apparent similarity in overall employability, however, conceals notable differences in the frequency of job and company departures. Panel C in Figure 3 illustrates that treated employees are more likely to leave their incumbent positions than their control counterparts. Similarly, Panel D shows that treated employees are also more likely to leave their incumbent employers, although this difference is less pronounced compared to job departures. Together, these findings suggest that while treated employees exhibit higher mobility—leaving their positions and employers more frequently—this does not lead to large economically significant long-term differences in employability between treated and control groups.

We now turn to the regression analysis, which allows us to account for individual differences across employees and establish a comparison group within the matched sample of control companies. Panel A in Table 6 presents the results from estimating Equation (1) with long-term employability as the dependent variable. Column 1 reveals a positive and statistically significant—though economically modest—difference in long-term employability. Treated employees spend 2.3% more time employed, on average, compared to control employees after acceleration. In unreported regressions, we show there are no significant differences in employability within 1 to 4 years after acceleration; however, these differences become significant starting 5 years after acceleration. In unreported analysis we also show higher, economically meaningful differences in long-term employability for treated employees, once we condition the sample to employees leaving their incumbent roles or companies. They show that conditional on departing the incumbent position or company, treated employees have circa 10% higher employability rates. Column 2 shows economically meaningful differences between treatment and control groups in terms of the propensity to depart their incumbent companies. In unreported regressions, we show that the differences in departure rates are not explained with higher or faster failure rates of accelerated businesses, and that the speed at which departures occur does not vary across treated and control employees.

1.8. Long-term Career Average Expected Earnings in the O*NET Sample

We explore the implications on long-term expected career earnings after acceleration driven by the shifts in the characteristics of subsequent job roles and the higher departure rates from incumbent positions. We begin by re-organizing the data in acceleration-event time and plotting average annual salaries around acceleration. Panel E in Figure 3 plots the average wages for treatment and control groups around the "acceleration years," denoting the years after (before) acceleration for every

employee with positive (negative) event number indicators. For a given event year, only individuals employed that event year estimate the average wage plotted in the figure, and the wage corresponds to that in ONET. The plot shows that in the years prior to acceleration employees in accelerated and non-accelerated companies have similar expected wages, but a gap arises after acceleration.

Panel E in Figure 3 shows that treated and control employees are similarly compensated before acceleration, however after acceleration there is a decoupling of the two series, with accelerated employees exhibiting a positive change in the slope of their expected average salaries. Panel F replicates the event-time analysis around acceleration dates of Panel E but using earnings as dependent variable which replaces with zero the earnings of any period after incumbent employees depart their incumbent jobs and before they transition to a new position. Thus, any differences between treatment and control employees in the figure reflect potential differences in the types of job positions individuals access and the likelihood and duration of unemployment spells. The figure shows similar patterns for earnings. The figure shows that treated employees had slightly higher average annual earnings before acceleration than control employees but similarly increasing trends. After acceleration, however, the figure shows an apparent widening in the annual earnings gap between the two groups.

We then turn to the regression analysis where we can control for differences across individuals, fixe the comparison group to the matched group, and apply state-of-the-art stacked difference-in-difference techniques to mitigate any biases from potential heterogeneity of effects and sample composition. Figure 4 plots the estimated coefficients of Equation 2 for salaries and earnings and their log transformations; Table 7 summarizes the ATT estimates.

Figure 4 shows a significant increase in salaries after acceleration. These results demonstrate that the patterns in Panel E of Figure 3 are not driven by potential biases from simple event-study regressions around acceleration. Panel A2 shows that results and economic magnitude are robust to using the sample matched at the employee level that controls for time-invariant individual differences across employees in accelerated and matched non accelerated businesses.

Table 7 shows positive and statistically significant effects. Both average annual expected salary and earnings increase after acceleration relative to matched controls; 2.3% and 4.6% respectively. Figure 4 shows that the increases in expected salaries and earnings are immediate, with positive and statistically significant coefficients from the year of acceleration.

In summary, results from the cross-program analysis show robust evidence that employees of accelerated firms earn higher wages over time than their peers in non-accelerated firms, without facing increased employability risks. These wage gains stem from employees transitioning into managerial or entrepreneurial roles or joining larger companies. Consistent with a “soft skills dividend” from startup

acceleration, these wage increases align with shifts in skill demands: employees increasingly move into roles that emphasize soft skills while relying less on technical expertise.

In unreported regressions we show that the effects persist even for employees from failed startups, are stronger for more experienced workers and more mature firms at the time of acceleration, are not driven by workers moving to other accelerated firms and is there for programs providing financing support and those that offer non-monetary resources only.

2. The quasi-experimental analysis: the ValleE accelerator in Colombia[10]

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.[11] The accelerator is the brainchild of the Regional Network of Entrepreneurship in ValleE del Cauca (a private organization that aims to encourage entrepreneurship in the ValleE del Cauca region), and is operated by the city Chamber of Commerce, a private entity that has been delegated public duties such as the management of the Colombia business registry.[12] 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).[13] 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 in Section 3.2, 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.

[10] This section follows Gonzalez-Uribe and Reyes (2020).

[11] 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%).

[12] Chambers of Commerce oversee the private sector development policies in their region. They are key connecting actors that execute programs aimed at improving regional competitiveness.

[13] The top two impact objectives among ecosystem accelerators are employment generation (35%) and community development (30%). Source: The Entrepreneurship Database at Emory University.

Clarysse, Wright, and Van Hove, 2015).[14] 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 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 provided. The first type consists of bi-monthly meetings to discuss business strategy with high-level advisors assigned based on industry, which include renowned CEOs in the region, as well as managers at the Chamber of Commerce. Assigned advisors may provide introductions to potential clients or industry contacts, which are likely to be high impact, as the selected CEOs and Chamber of Commerce managers are well connected within the local ecosystem. The second type of mentoring sessions are handled by program coordinators who take a more hands-on approach: sessions are conducted weekly and are of varying duration. Coordinators are junior to advisors and focus on helping entrepreneurs throughout the day-to-day operations rather than designing avenues for growth. 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).

2.1. Accelerator Selection Process

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.[15] 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

[14] Circa 55% of the ecosystem accelerators in the Entrepreneurship Database at Emory University provide no seed capital. Source: https://www.galidata.org/accelerators/.

[15] The main reasons behind using judge panels (rather than individual judges) are to minimize the burden on individual judges (given their time constraints) and mitigate the chance that one judge determines the treatment status of any given project, as this could lead to unwanted biases such as judges favoring projects from their own industries, regions, or communities.

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. [16] 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.[17] 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. This average business and industry expertise of judges is not necessarily common among other business accelerator programs, where applications are managed by platforms that rely on a wider variety of less "hands-on" experienced judges such as academics (cf., González-Uribe and Leatherbee, 2018a).

Compliance with the selection rule was perfect: 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 (see Appendix 1), 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" 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). In Section 3.4, we discuss how we use this heterogeneity in scoring generosity across the randomly assigned judges to estimate the causal impact of participation in the accelerator on workers' careers.

2.2. Sample

[16] Entrepreneurs were never given their ranking or scores in order to avoid any negative psychological effects or create rivalry among participants.

[17] The characteristics of the final 135 projects differ slightly from the 120 businesses removed by the initial filter, which were more likely to have a female founder, have less educated founders, and refer to nonpecuniary benefits (e.g., being their own boss) as the main motivation behind their business.

ValleE provided us with all the application data, including application scores by each judge and final selection decisions, for the program's first cohort.[18] 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).

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 6 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).

[18] The judges' identities were not provided by ValleE for confidentiality reasons. For the purpose of our investigation, we were provided with anonymized information that includes judge identifiers in order to track different projects evaluated by the same judge.

As the ValleE sample, the PILA subsample is comparable to average applicant in the observational sample (see Table 1). It is also comparable to the average applicant of ecosystem accelerators worldwide, based on information from the Entrepreneurship Database (ED) program at Emory University.[19] The average ValleE applicant in the PILA sample is similarly sized (ED applicants have an average of 3.5 employees, a 43.2% likelihood of positive profits, and median [positive] revenue of $12,000 USD) but is more educated (47% of ED applicants have a bachelor's or master's degree), less likely to be female (29% of ED applicants are female), and has a more mature business (19% of ED applicants report positive revenues prior to application).[20] The PILA sample is also comparable to that used in prior work on ecosystem business accelerators: González-Uribe and Leatherbee (2018a) show that applicants to Start-up Chile, a renowned ecosystem accelerator sponsored by the Chilean government, are likely to be male (86%), have between two and three employees, and are predominantly from services industries such as E-commerce (18%). Finally, our sample is also similar to that in prior work on early-stage ventures. Haltiwanger, Jarmin, and Miranda (2013) document that 33% of young firms (less than a year old) in the U.S. have between one and four employees, and Puri and Zarutskie (2012) show that the distribution of VC-backed firms is concentrated in the services industry.

For the regression analysis, we organize the sample with observations at the employee month level. Each individual in the dataset is linked to a specific ValleE firm where it works for at least one month from January 2012 to June 2022. Note that this is not mechanical but rather a feature of the dataset: we see no employee transitions from one ValleE firm to another ValleE firm during the sample period. The variable *Acceleration* indicates whether the ValleE firm linked to the individual participated in the program. For each employee, we divide their career in formal labor market into three time periods: before working in the ValleE firm, while working at the ValleE firm, and after leaving the ValleE firm. Leaving the ValleE firm is an absorbing state for most workers in the sample: we exclude from the analysis the few employees that leave the ValleE firm and return during the sample period (less than 5%). For most of the regressions we consider only the last two time periods: during and after working in the ValleE firm. *PostE* indicates the time periods after employee leaves the ValleE firm. The variable *Post* indicates time periods after the acceleration program, so 2016-2022 inclusive.

We distinguish between two types of individuals in the sample. "Incumbent" employees are those that were working in their respective ValleE 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. Note that 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).

In the PILA sample, there are 16,143 employee-month non-missing observations covering 197 are incumbent employees and 31,238 employee-month non-missing observations covering 485 new

[19] This section follows Gonzalez-Uribe and Reyes (2020).
[20] See https://www.galidata.org/accelerators/.

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).

We define the following variables for the regression analysis: *Log (Wage)* is the logarithm transformation of the wage. This variable is conditional on formal employment and has a value of missing values if a given employee is not a formal employee in a given month. We also consider the variable *Log (Earnings)* that is equal to *Log (Wage)* except during formal unemployment spells where we replace the missing observation with 0. Note that we only replace with zeros formal unemployment spells: all observations with missing wage after the last observation of the individual in the data are not replaced with zero. Table 9 presents summary statistics of the outcome variables.

2.3. Empirical Strategy PILA sample

We begin the empirical analysis of the quasi-experimental sample incumbent workers to best mimic the analysis with the observational sample. The two main advantages of the quasiexperimental analysis is observing real compensation data from formal market job positions, and controlling for selection into the program.

We start 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 equation the following equation:

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

Where Y_{jt} is the outcome for employee j at time t, and X_{jt} captures several controls including age, gender, experience, and the square of experience. 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 variable $Post_t$ indicates the periods after acceleration (2016 onwards), which we only include interacted with $Acceleration_j$, as the month fixed effects absorb its variation. The coefficient of interest is ρ_1 capturing the average changes in employee outcomes between employees linked to a ValleE beneficiary and those linked to a rejected applicant, while they work in the ValleE business. In some estimations we restrict the sample to observations before the employee separates from the ValleE firm. We report heteroscedasticity robust standard errors to account for any

serial correlation over time for a given employee. Results from different estimates of this equation are summarized in Table 9.

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 $f(SG)$, where SG corresponds to the scoring generosity of the judges that scored the application of the ValleE company linked to the employee. The project's scoring generosity 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 estimating equation associated with equation (3) is:

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

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).

Using scoring generosity interacted with $Post_t$ 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 the accelerator for the individuals in ValleE firms whose participation is altered by scoring generosity. 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, respectively, mistakenly rejected/accepted due to the generosity/strictness of their judges. Figure 8 shows that in the ValleE sample, the frequency of type 1 and type 2 mistakes was similar. Instead, in the PILA sample, type 2 mistakes are very rare; most mistakes are type 1. 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) scoring generosity is associated with participation in the accelerator, (2) scoring generosity only impacts venture outcomes through the probability of participating in the accelerator

(i.e., the "exclusion restriction"), and (3) the impact of scoring generosity 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 B in Figure 9 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 B in Figure 9, we classify applicants into quartiles of scoring generosity, and estimate for each quartile the distribution of acceleration over adjusted scores.[21] 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 scoring generosity. Confirming this positive association, in columns (1) and (2) of Table 10, 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 look 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 Appendix. Any remaining concerns regarding the unintentional assignment of generous judges to high-quality firms are not consistent with the patterns shown in Panel A of Figure 9; 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 scoring generosity. Differences in the interaction between applicants and judges across applicants in different quartiles of scoring generosity 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

[21] Relative to a mean average score of 0.7, the breakpoints for the scoring generosity quartiles are -0.03, 0.001, and 0.05, and the max (min) scoring generosity 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.

unlikely (e.g., feelings of grandeur or depression). Finally, Gonzalez-Uribe and Reyes (2020) show evidence that scoring generosity also does not measure differences in predicting ability across judges Ultimately, however, the assumption that scoring generosity only systematically affects applicants' performance through acceleration is fundamentally untestable, and our estimates should be interpreted with this identification assumption in mind.

2.4. Analysis PILA Sample

In this section we summarize the results from the regression analysis using the PILA sample.

We begin by replicating the analysis of the cross-program sample using the PILA sample in Table 11. Columns 1 and 2 show that monthly wages increase on average 10.9% after acceleration and that these increases are linked to job transitions out of the ValleE applicant company. Consistent with the idea that the wage returns do not occur at the expense of higher unemployment risk, Columns 3-4 show that employees of accelerated companies are just as likely as those in non-accelerated companies to remain formally employed after acceleration, and if anything the time they spend formally employed after acceleration is longer, albeit statistically weak.

Columns 5-7 show compelling evidence of causal acceleration effects on incumbent workers' wages after acceleration: positive returns at equal risk than employees of non-accelerated companies. Column 5 shows that that monthly wages increase an average 13.6% after acceleration. This corresponds to an increase of $2.0M COP in annual wages ($500USD; economic magnitude). To place this economic figure into perspective within the Colombian context, the wage increase is approximately equivalent to two minimum monthly legal salaries in Colombia. Columns 6 and 7 show that instead there is no evidence of a causal effect on employability risk.

The IV estimates 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 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 (and for their sample period which spans 2018), 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 these firms do not formally employ workers.

We contrast the IV estimate with the naïve OLS estimates comparing average career outcomes across employees of ValleE participants and nonparticipants. A comparison between the coefficients in columns (1) and (5) and between columns (3) and (6) reveal a positive difference between the IV and

the OLS estimates. 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; table 10 shows compelling evidence of the strength of the instrument. Moreover, a comparison between the coefficients in columns (4) and (7) –i.e., the OLS and IV effects on percentage of months with formal employment after acceleration—shows a negative rather than a positive difference. Instead, the positive differences are instead 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 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 specifically correction is expected when comparing the OLS and the IV estimates.

3. Mechanisms of impact

Why are there such large and positive effects on workers' career paths caused by acceleration? The results in Section 3 suggest they cannot be entirely explained by a selection story where business accelerators select 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.

We are clear to note that we do not claim that the evidence in Section shows that all business accelerators will have positive impacts on their workers' career paths. There is large heterogeneity among business accelerators. However, several aspects about ValleE suggest that these results are not only confined to that accelerator. For starters, ValleE is very similar to the average ecosystem accelerator on many dimensions. For example, the location of the program outside the capital city of Colombia is a common trait among ecosystem accelerators. Roughly 38% of these programs are located in underdeveloped regions. 40% are in the United States, but outside Silicon Valley, Massachusetts, New York, or Washington D.C.; the rest are in Europe, but are typically not in the capital cities. In terms of services, those offered at ValleE are similar to the offerings of these programs worldwide (cf. Clarysse, Wright, and Van Hove, 2015). This is not to say that some differences between ValleE and other ecosystem accelerators do not exist. Perhaps the most distinguishing features of the program include its access to highly qualified staff, mentors, and judges. The external validity of our findings is likely confined to other ecosystem accelerators that attract young businesses with traction and have access to high-quality resources, including staff, mentors, and judges.

Given this context, what mechanisms can explain the treatment effects we observe of acceleration on workers' career paths? We consider three main types of mechanisms: soft skills acquisition, certification, validation and networking. We have no quasi-experimental variation to fully

rule out any of these mechanisms. However, in this section we argue that preponderance of evidence suggests the most consistent story is the soft skills dividend mechanism.

The first potential explanation for the treatment effect is a soft skills dividend mechanism whereby working in a company that experiences accelerated growth or demise through the business accelerator enables workers to build soft skills that are transferable across businesses thus enhancing employees' career prospects (Becker, 1962). Many researchers have noted how business accelerators help fill managerial and entrepreneurial skill gaps through business training, customized advice, networking and visibility (Gonzalez-Uribe and Hmaddi, 2020). Employees on the founding team can benefit directly from the training or indirectly from insights gained through close interactions with trained founders (Bender, Bloom, Card, Van Reenen, and Wolter, 2017). Even employees less directly involved with the founders could gain valuable experience from working in a rapidly expanding company or learning from its failures. This experience can set the right incentives for employees to invest in skills that are perceived important by the market. In related research, Agrawal and Tande (2016) identify positive spillover effects on workers' career paths resulting from acquiring transferable IT-related human capital after their employers go through a Private Equity transaction. The potential skill spillovers from business accelerators and training programs can be particularly strong in startups, where limited resources and smaller teams give employees the chance to take on larger responsibilities (Rosenbaum, 1979; Stewman and Konda, 1983). However, rather than necessarily becoming adept at any technology, as in the context of PE, it is likely that the employees working in accelerated businesses instead become particularly adept at softer skills like cross-functional skills, which are versatile abilities that enable people to work effectively with others from different areas of organizations. Startups where individuals take on more responsibilities and job functions are not necessarily well defined, provide an environment that helps workers develop versatile "jack-of-all-trades" skill profiles, which are highly valuable for managerial and entrepreneurial roles (Lazear, 2005; Rosenbaum, 1979; Stewman and Konda, 1983; Kotter, 1982; Troy et al., 2008). Sorenson, Dahl, Canales, and Burton (2021) describe working in a startup as akin to a rotation program.

Consistent with this mechanism, the analysis using the cross-program sample shows increases in soft skills requirements for the jobs of workers after their businesses are accelerated. Also consistent with this evidence are the results on higher likelihoods of holding managerial and entrepreneurial roles.

Further supporting the soft skills dividend mechanism, we conduct three additional tests using the ValleE data. First, we find that acceleration increases the likelihood of employees moving into soft skills intensive sectors, as inferred using O*NET data to classify industries. Second, we show that acceleration facilitates transitions across industries, suggesting employees gain broadly transferable skills. Third, we find stronger effects for employees who remain until the closure of the startup rather than leaving before that, indicating that the skill effects compound with experience and revealing no negative signaling effects from remaining in an ultimately failed startup.

Alternative explanations—such as certification, validation, and networking—do not fully account for the positive career effects we identify. While accelerator selection may enhance employees' market value by acting as a quality signal, similar to models where external credentials improve job prospects (Spence, 1974), this mechanism alone does not explain the full picture. Given the selectivity of accelerators, acceptance into a program may boost labor market outcomes. However, this cannot account for why employees from failed firms still benefit, as poor business performance should, in theory, send a negative signal to employers. Moreover, the effects are stronger for employees who remain until the startup closes rather than those who leave earlier, further challenging a pure certification explanation.

Additionally, we find stronger career gains for employees from lower-potential firms, as measured by internal program rankings. Since these rankings are not publicly available, a certification mechanism would predict similar career benefits across all employees, regardless of firm quality—contrary to our findings. We find even stronger effects for employees with prior experience in medium and large firms—individuals who likely already benefit from the certification value of that experience in the eyes of potential employers, and thus could if anything benefit less from any potential certification by the program.

The validation mechanism—where acceleration boosts employees' confidence, effort, or productivity, akin to models where entrepreneurs learn about their abilities through experience (Jovanovic, 1982)—is also inconsistent with our findings. If validation were the primary driver, career benefits would likely be weaker for employees of failed startups, as failure would undermine any positive selection signal. This issue is particularly relevant in the small teams common among accelerator participants, where it is difficult to separate business failure from individual performance. Furthermore, in additional analyses, we show that the effects are stronger for more experienced employees, who are less likely to be influenced by program validation, having already established career trajectories.

Networking is also unlikely to be the main driver. If accelerator participation primarily benefited employees through expanded professional connections (Rajkumar et al., 2022), we would expect them to move into jobs within the accelerator's network. However, we find no evidence of this—treated employees do not disproportionately transition into roles at ValleE peer firms. In fact, there are no such instances in the sample. Moreover, employees who go through the program are more likely to work in Bogotá rather than Cali, where ValleE is based. If networking were the key mechanism, we would expect stronger career effects within the accelerator's immediate ecosystem, which is not the case.

Together, these findings strongly support the soft skills dividend as the primary mechanism. While other factors may contribute, they do not fully explain the observed career benefits. This raises a key question: does the skills dividend arise from the intensified startup experience accelerators provide,

or from accelerator training itself? While both likely play a role, several pieces of evidence suggest the startup experience is the dominant factor.

First, the results hold even when excluding founders, who receive direct accelerator training, while employees do not participate in structured mentoring or group sessions. Second, the findings remain robust even after removing small-team firms, where knowledge spillovers from founders to employees are most pronounced. Third, we show that employees from high-potential firms mistakenly left out of the program (Type 1 errors) still experience better career outcomes than comparable rejected applicants. This suggests that working in a high-growth startup—regardless of acceleration—improves career prospects.

This does not mean acceleration training is irrelevant. Our findings suggest that accelerators shape career trajectories in distinct ways. While employees in both accelerated and non-accelerated high-growth firms see wage gains compared to similar employees from rejected companies, their career paths diverge. Employees from accelerated firms are more likely to transition into knowledge-intensive sectors and smaller firms, while those from non-accelerated high-growth firms tend to follow more conventional paths into medium and large businesses. This indicates that, beyond the startup experience, accelerators may cultivate unique skills or preferences that influence long-term career choices.

5 Conclusion

We provide the first systematic analysis of the effects of business accelerators on startup employees' careers using two approaches. The first is a cross-program analysis that employs novel text-based methods to examine the types of job positions held by workers in a large sample of accelerator participants and control firms across the Americas. The second approach is a quasi-experimental-level analysis that leverages quasi-experimental variation in participation in the Colombian ValleE accelerator, using official employer-employee linked wage data from Colombia's payroll tax and social security contribution registry.

We find that employees of accelerated firms experience sustained wage growth without increased employability risks. These gains come from transitions into managerial, entrepreneurial, and leadership roles that require strong soft skills—behavioral and social traits that enable effective collaboration. Notably, these benefits persist even for employees of failed startups, indicating that acceleration fosters long-term career development regardless of firm success. Together, these results suggest that startup employees gain a soft skills dividend from acceleration, even when it accelerates firm closure, highlighting the role of learning in career advancement.

Alternative explanations such as certification, validation, and networking cannot fully account for the observed career effects. While accelerators may signal quality to employers, this alone does not explain why employees from failed firms still benefit. Similarly, if networking were the primary driver, we would expect stronger career effects within an accelerator’s immediate ecosystem, yet employees move to diverse locations and industries.

Our findings point to the intensified startup experience as the key driver of the soft skills dividend. Employees gain exposure to fast-paced decision-making, problem-solving, and cross-functional collaboration—highly valued skills in the labor market. This effect holds even when excluding founders and remains robust across different firm sizes and industries. Moreover, career pathways differ between employees of accelerated and non-accelerated high-growth firms: those in accelerated firms are more likely to transition into knowledge-intensive sectors and smaller firms, while their non-accelerated counterparts move into traditional corporate roles in medium and large businesses.

The evidence contributes to research on entrepreneur support and training programs, which have become common solutions to the challenges startups face, particularly gaps in managerial and entrepreneurial skills. As startups play a crucial role in job creation, these programs are seen as key potential drivers of economic recovery and inclusive growth, improving workers' living standards (Bone et al., 2019). However, despite the focus on entrepreneurship as a job creator, there is limited research on the long-term effects of startup employment, especially for employees of accelerator-backed firms, due to data constraints.

Our work combining different approaches to address the traditional data challenges in this area provides novel insights on business accelerators' impacts, increasing our understanding of this programs as startup ecosystem builders (Gonzalez-Uribe and Hmaddi, 2020).

References

Aizer, A., Doyle, Jr., J., 2013. Juvenile incarceration, human capital, and future crime: Evidence from randomly-assigned judges. NBER Working Paper No. 19102.

Ahmad, N., Petersen, D.R., 2007. High-growth enterprises and gazelles – Preliminary and summary sensitivity analysis. OECD-FORA, Paris.

Autio, E., Arenius, P., Wallenius, H., 2000. Economic impact of gazelle firms in Finland. Working Papers Series 2000:3, Helsinki University of Technology, Institute of Strategy and International Business, Helsinki.

Autor, D., Houseman, S., 2010. Do temporary-help jobs improve labor market outcomes for low-skilled workers? Evidence from "Work First." American Economic Journal: Applied Economics 2, 96-128.

Bertrand, M., Schoar, A., 2003. Managing with style: The effect of managers on firm policies. Quarterly Journal of Economics 118, 1169-1208.

Birch, D.L., Haggerty, A., Parsons, W., 1995. Who's Creating Jobs? Cognetics Inc, Boston.

Birch, D.L., Medoff, J., 1994. Gazelles. In: Solmon, L.C., Levenson, A.R. (Eds), Labor Markets, Employment Policy and Job Creation, Westview Press, Boulder and London, pp. 159-167.

Bloom, N., Eifert, B., Mahajan, A., McKenzie, D., Roberts, J., 2013. Does management matter? Evidence from India. Quarterly Journal of Economics 128, 1-51.

Bloom, N., van Reenen, J., 2010. Why do management practices differ across firms and countries? Journal of Economic Perspectives 24, 203-24.

Bone, J., Allen, O., and Haley, C., 2017. Business incubator and accelerators: The national picture, Department for Business, Energy and Industrial Strategy, Nesta, BEIS Research Paper Number 7.

Bruhn, M., Karlan, D., Schoar, A., 2010. What capital is missing in developing countries? The American Economic Review 100, 629-33.

Calderon, G., Cunha, J.M., De Giorgi, G., 2013. Business literacy and development: Evidence from a randomized controlled trial in rural Mexico. NBER Working Paper No. 19740.

Card, D., 2001. Estimating the return to schooling: Progress on some persistent econometric problems. Econometrica 69, 1127-60.

Clarysse, B., Wright, M., Van Hove, J., 2015. A look inside accelerators: Building businesses. Research Paper, Nesta, London, UK.

Chang, T., Schoar, A., 2013. Judge specific differences in chapter 11 and firm outcomes. MIT Working Paper.

Cho, T., 2019, Truning Alphas into betas: Arbitrage and Endogenous Risk, Journal of Financial Economics, *Forthcoming*

Cohen, S.G., Hochberg, Y.V., 2014. Accelerating startups: The seed accelerator phenomenon. Working Paper.

Deschryvere, M., 2008. High-growth firms and job creation in Finland. Discussion Paper No. 1144, Research Institute of the Finnish Economy (ETLA), Helsinki.

Di Tella, R., Schargrodsky, E., 2013. Criminal recidivism after prison and electronic monitoring. Journal of Political Economy 121, 28-73.

Dobbie, W., Goldin, J., Yang, C.S., 2018. The effects of pre-trial detention on conviction, future crime, and employment: Evidence from randomly assigned judge. American Economic Review 108, 201-240.

Doyle, J., 2007. Child protection and child outcomes: Measuring the effects of foster care. American Economic Review 97, 1583-1610.

Doyle, J., 2008. Child protection and adult crime: Using investigator assignment to estimate causal effects of foster care. Journal of Political Economy 116, 746-770.

de Mel, S., McKenzie, D., Woodruff, C., 2008. Returns to capital in microenterprises: Evidence from a field experiment. The Quarterly Journal of Economics 123, 1329-72.

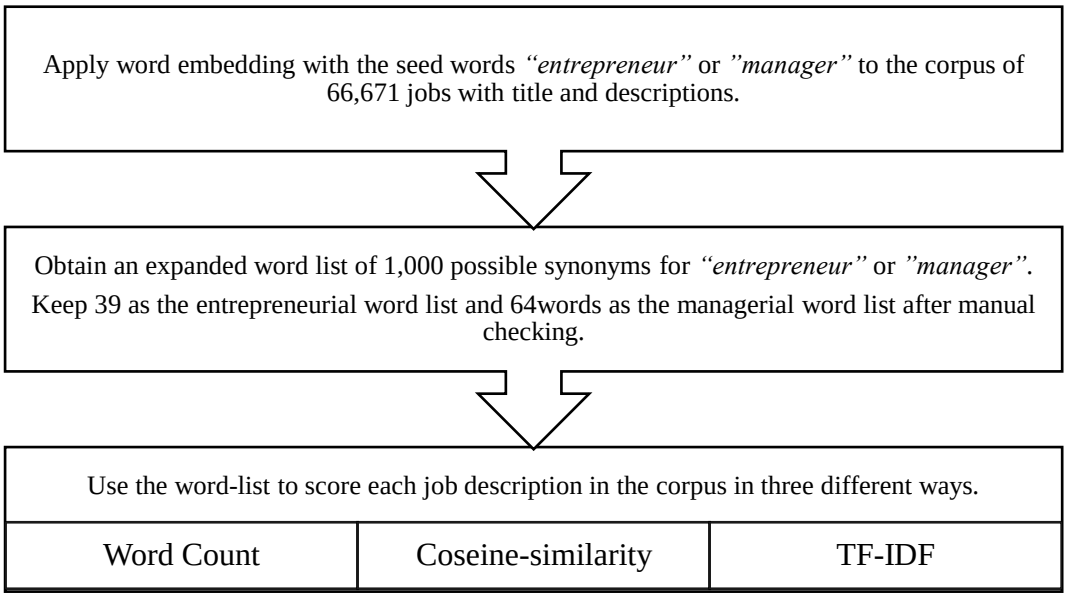
———, 2014. Business training and female enterprise start-up, growth, and dynamics: Experimental evidence from Sri Lanka. Journal of Development Economics 106, 199–210.

Dobbie, W., Song, J., 2015. Debt relief and debtor outcomes: Measuring the effects of consumer bankruptcy protection. American Economic Review 105, 1272-1311.

Drexler, A., Fischer, G., Schoar, A., 2014. Keeping it simple: Financial literacy and rules of thumb. American Economic Journal: Applied Economics 6, 1-31.
Ewens, M., Rhodes-Kropf, M., 2015. Is a VC partnership greater than the sum of its partners? The Journal of Finance 70, 1081-1113.
Fafchamps, M., Woodruff, C.M., 2016. Identifying gazelles: expert panels vs. surveys as a means to identify firms with rapid growth potential. Policy Research Working Paper, World Bank, WPS 7647.
Fan, J., Gijbels, I., 1996. Local Polynomial Modelling and Its Applications. Chapman and Hall, London.
Fee, C. E., Hadlock, C.J., Pierce, J.R., 2013. Managers with and without style: Evidence using exogenous variation. The Review of Financial Studies 26, 567-601.
Fehder, D. C., Hochberg, Y.V., 2014. Accelerators and the regional supply of venture capital investment. Working paper.
French, E., Song, J.E., 2011. The effect of disability insurance receipt on labor supply. Federal Reserve Bank of Chicago Working Paper WP-2009-05.
Goldfarb, B., Kirsch, D., Miller, D.A., 2007. Was there too little entry during the Dot Com Era? Journal of Financial Economics 86, 100-144.
González-Uribe, J., Leatherbee, M., 2018a. The effects of business accelerators on venture performance: Evidence from Start-Up Chile. Review of Financial Studies 31, 1566-1603.
González-Uribe, J. Leatherbee, M., 2018b. Selection issues. In: Wright, M. (Ed.), Accelerators. Imperial College Business School and Israel Drori, VU, Amsterdam.
González-Uribe, J., Zhongchen, H., Koudjis, P., 2019. Corporate accelerators. Working paper.
Goñi, E.A.G., Reyes, S., 2019. On the role of resource reallocation and growth acceleration of productive public programs. Inter-American Development Bank, working paper
Hall, R.E., Woodward, S.E., 2010. The burden of the nondiversifiable risk of entrepreneurship. American Economic Review 100, 1163-94.
Haltiwanger, J.C., Jarmin, R.S., Miranda, J., 2013. Who creates jobs? Small versus large versus young. The Review of Economics and Statistics XCV, 347-61.
Heckman, J., Ichimura, H., Todd, P.E., 1997. Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme. Review of Economic Studies 64, 605-654.
Henrekson, M., Johansson, D., 2008. Gazelles as job creators—A survey and interpretation of the evidence. IFN Working Paper No. 733.
Imbens, G.W., Angrist, J.D., 1994. Identification and estimation of local average treatment effects. Econometrica 62, 467-75.
Imbens, G.W., Rosenbaum, P., 2005. Randomization inference with an instrumental variable. Journal of the Royal Statistical Society Series A 168, 109-126.
Kahneman, D., Klein, G., 2009. Conditions for intuitive expertise: A failure to disagree. American Psychologist 64, 515-26.
Karlan, D., Valdivia, M. 2011. Teaching entrepreneurship: Impact of business training on microfinance clients and institutions. The Review of Economics and Statistics 93, 510-527.
King, E.M., Behrman, J.R., 2009. Timing and duration of exposure in evaluations of social programs. The World Bank Research Observer 24, 55-82, https://doi.org/10.1093/wbro/lkn009
Kirchhoff, B.A., 1994. Entrepreneurship and Dynamic Capitalism. Praeger, Westport.
Kling, J.R.., 2006. Incarceration length, employment, and earnings. American Economic Review 96, 863-876.
Klinger, B., Schundeln, M., 2011. Can entrepreneurial activity be taught? Quasi-experimental evidence from central America. World Development 39, 1592-1610.
Lafortune, J., Riutort, J., Tessada, J., 2018. Role models or individual consulting: The impact of personalizing micro-entrepreneurship training. American Economic Journal: Applied Economics 10, 222-45.
Lee, Daniel and Floyd, Eric and Hochberg, Yael V. and Fehder, Daniel C. and Fehder, Daniel C. and Bailey-Rihawi, Esther, LEARNING TO QUIT? A MULTI-YEAR FIELD EXPERIMENT WITH INNOVATION DRIVEN ENTREPRENEURS* (June 10, 2024). Available at SSRN: https://ssrn.com/abstract=4865251 or http://dx.doi.org/10.2139/ssrn.4865251
Landström, H., 2005. Pioneers in Entrepreneurship And Small Business Research. Springer, Berlin.

Lerner, J., Malmendier, U., 2013. With a little help from my (random) friends: Success and failure in post-business school entrepreneurship. Review of Financial Studies, Society for Financial Studies 26, 2411-2452.
Mano, Y., Akoten, J., Yoshino, Y., Sonobe, T., 2011. Teaching KAIZEN to small business owners: an experiment in a metalworking cluster in Nairobi. Working paper.
Maestas, N., Mullen, K., Strand, A., 2013. Does disability insurance receipt discourage work? Using examiner assignment to estimate causal effects of SSDI receipt. American Economic Review 103, 1797-1829.
McKenzie, D., 2017. Identifying and spurring high-growth entrepreneurship: Experimental evidence from a business plan competition. American Economic Review 107, 2278-2307.
McKenzie, D.J., Sansone, D., 2017. Man vs. machine in predicting successful entrepreneurs: evidence from a business plan competition in Nigeria. Policy Research Working Paper Series 8271, The World Bank.
McKenzie, D., Woodruff, C., 2008. Experimental evidence on returns to capital and access to finance in Mexico. The World Bank Economic Review 22, 457-82.
———, 2014. What are we learning from business training and entrepreneurship evaluations around the developing world? World Bank Research Observer 29, 48-82.
Nanda, R., 2006. Financing high-potential entrepreneurship. IZA World of Labor 2016, 252.
Puri, M., Zarutskie, R., 2012. On the life cycle dynamics of venture capital and non venture-capital financed firms. The Journal of Finance 67, 2247-93.
Rosenbaum, P.R., 2002. Observational Studies, 2nd Edition. Springer, New York, doi:10.1007/978-1-4757-3692-2.
Rosenbaum, P.R., 2010. Design of Observational Studies. Springer-Verlag, New York.
Shanteau, J., 1992. Competence in experts: The role of task characteristics. Organizational Behavior and Human Decision Processes 53, 252–62.
Smith, J., Todd, P., 2005. Does matching overcome LaLonde's critique of nonexperimental estimators? Journal of Econometrics 125, 305-353.
Stock, J., Yogo, M., 2005. Testing for weak instruments in linear IV regression. In: Andrews, D.W.K. (Ed.), Identification and Inference for Econometric Models. Cambridge University Press, New York, pp. 80-108.
Wooldridge, J.M., 2002. Econometric Analysis of Cross Section and Panel Data. MIT Press, Cambridge, MA.
Young, A., 2018. Consistency without inference: Instrumental variables in practical application. Working paper, LSE.
Xie, Y., Brand, J., Jann, B., 2012. Estimating heterogeneous treatment effects with observational data. Sociological Methodology 42, 314-347.

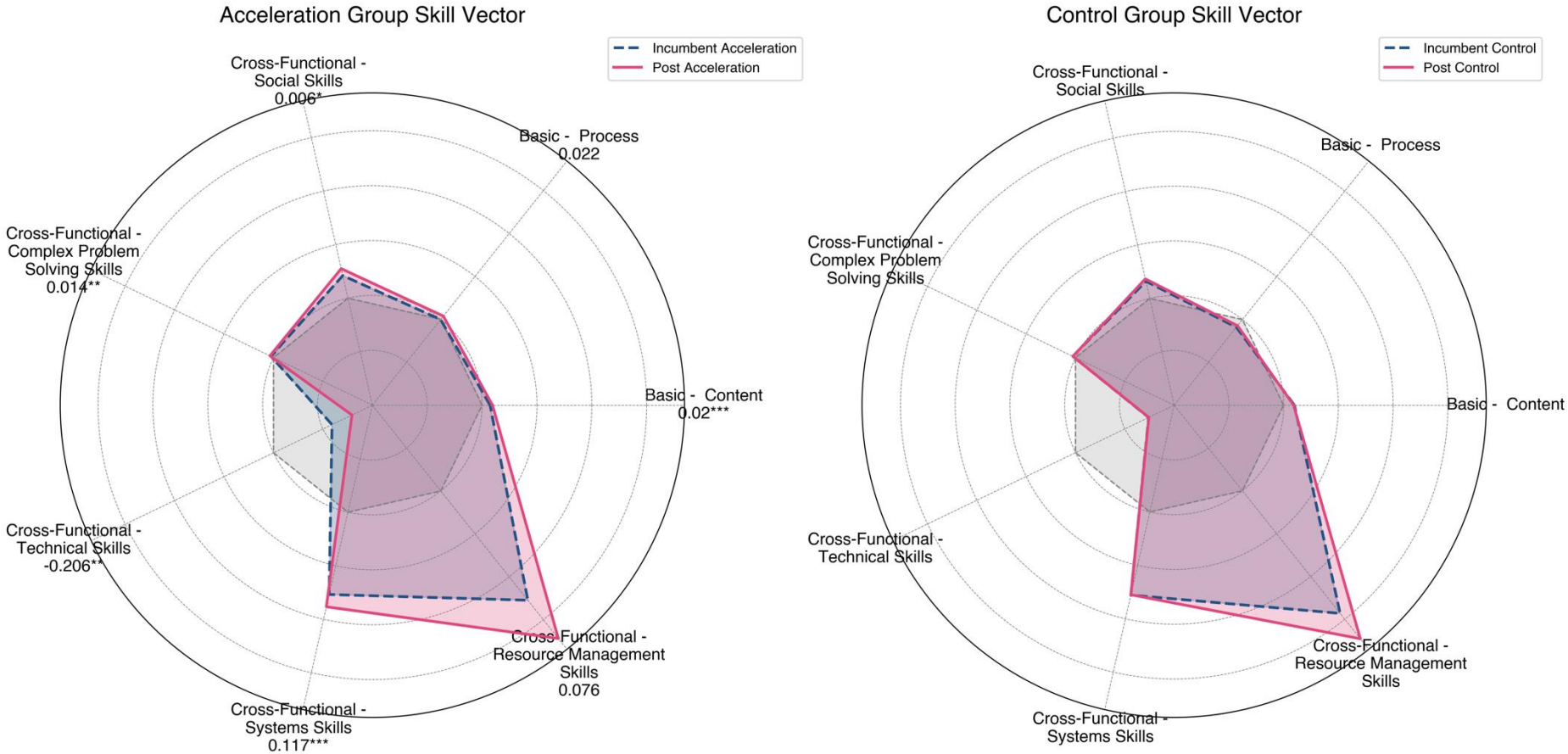
Figure 1. Flow Chart of the Neural-Network Algorithm



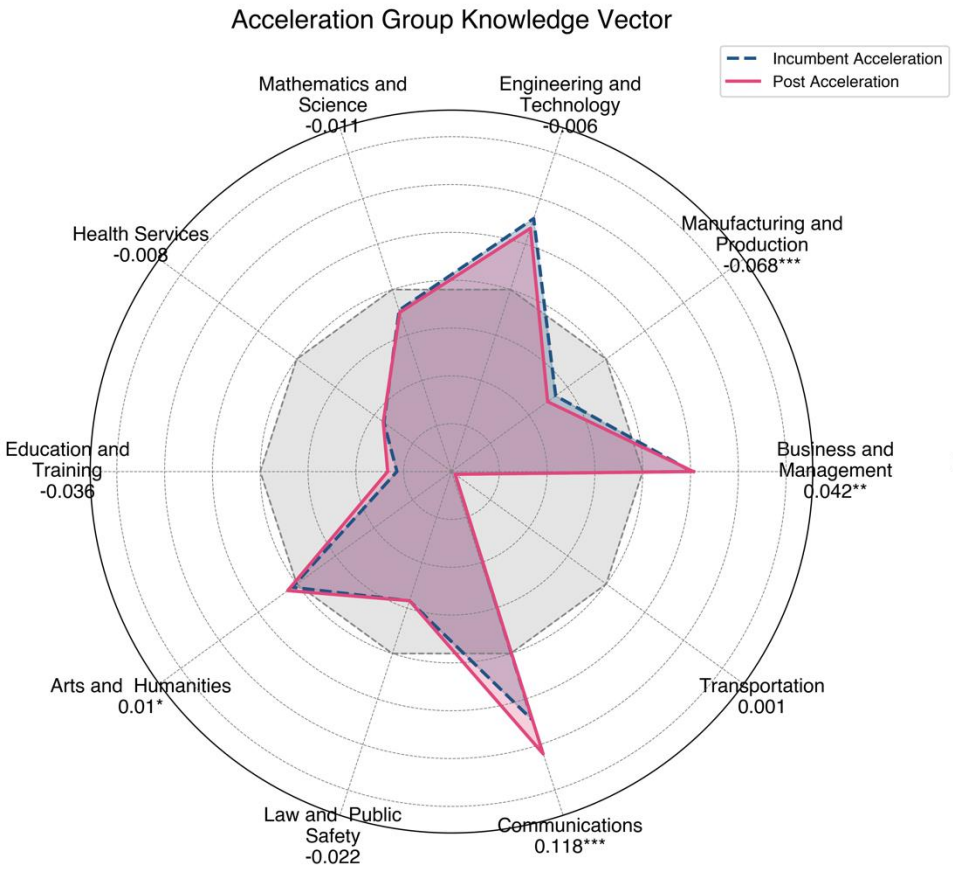
The figure presents the flow chart of the Neural-Network model we use to characterize jobs as entrepreneurial and/or managerial.

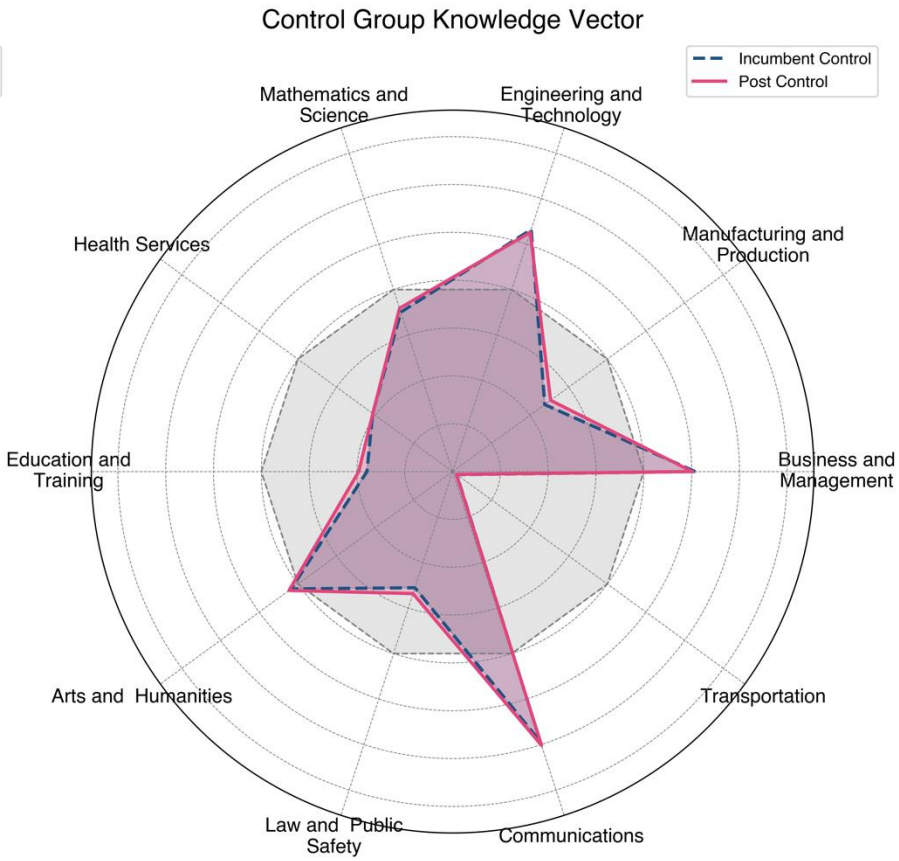
Figure 2. Skills, Knowledge and Abilities of jobs during and after acceleration

Panel A. Skills Radar Charts

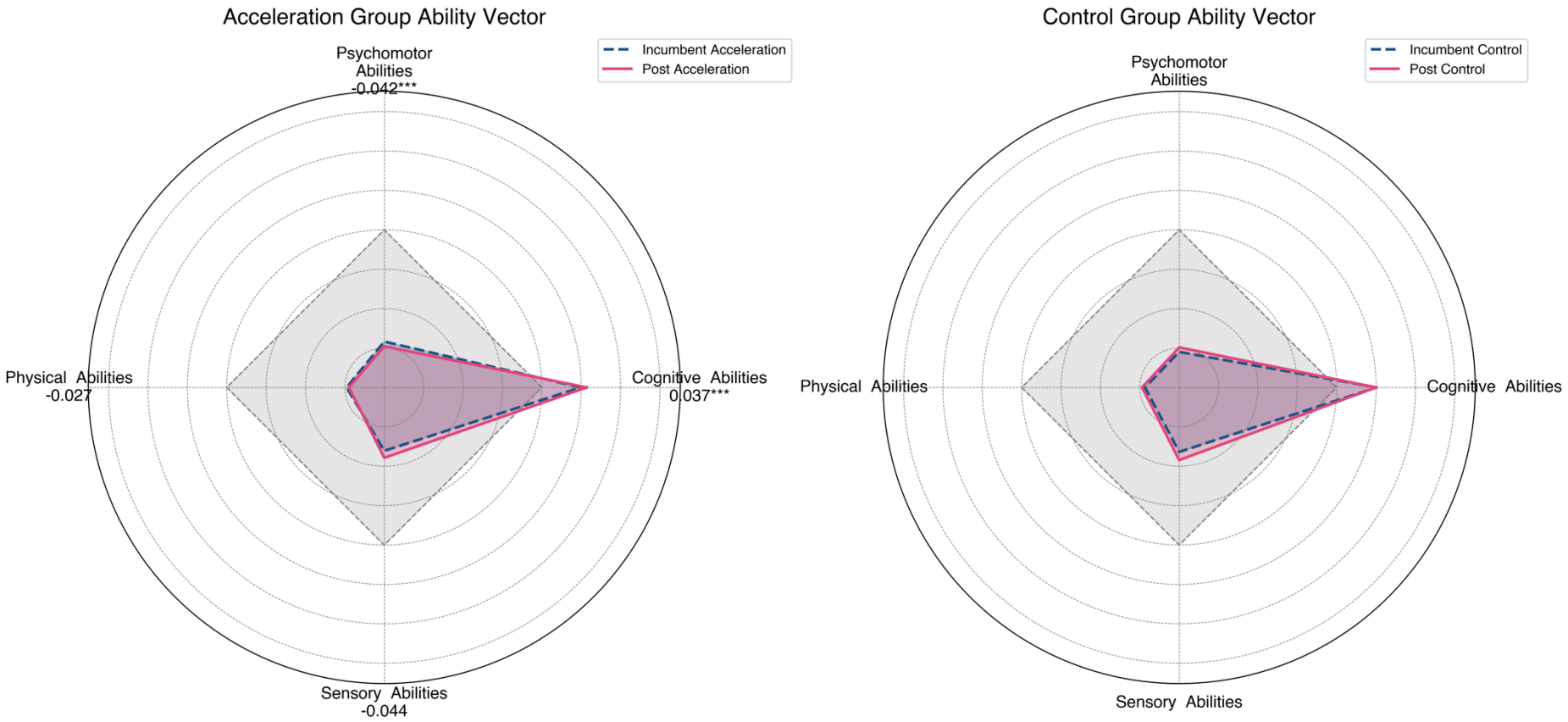


Panel B. Knowledge Radar Charts





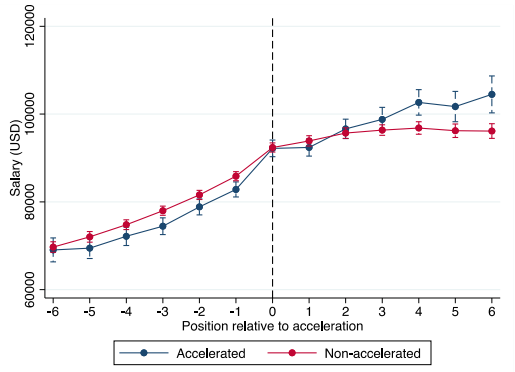
Panel C. Abilities Radar Charts



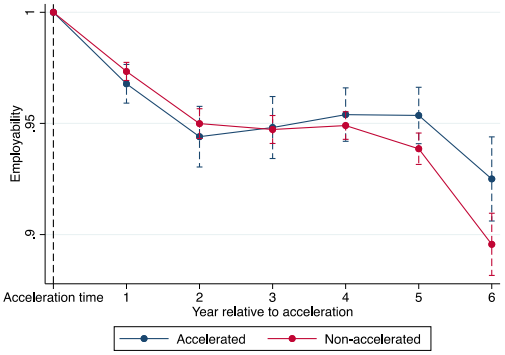
The figure shows radar charts for the job requirements in terms of skills (Panel A), knowledge (Panel B) and ability (Panel C). Each panel compares radar charts for treated employees (left) to control employees (right). For each group of employees, we plot the radar chart during and post treatment. The figures are based on 30,406 employees with a new job after acceleration.

Figure 3. Salaries, Employability, Departures, and Earnings after Acceleration

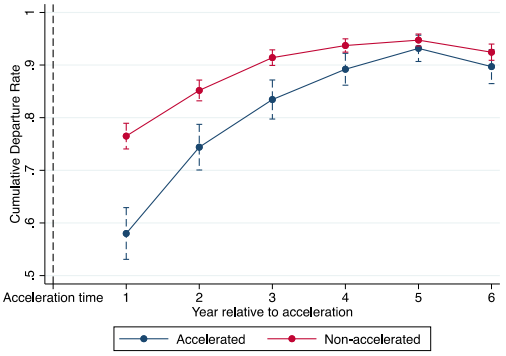
Panel A. Salaries in subsequent job positions after acceleration



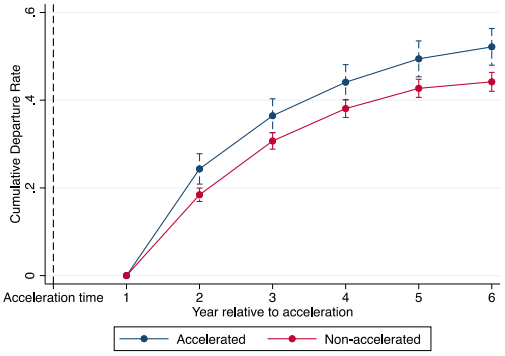
Panel B. Employability in the years after acceleration



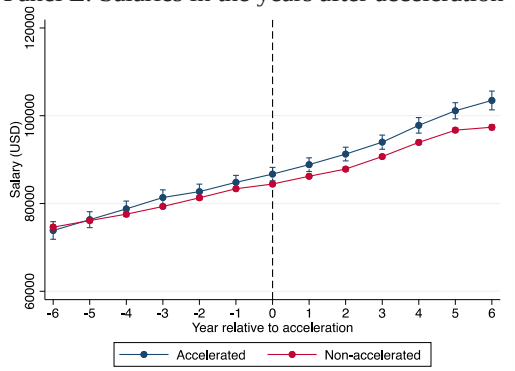
Panel C. Job departures in the years after acceleration



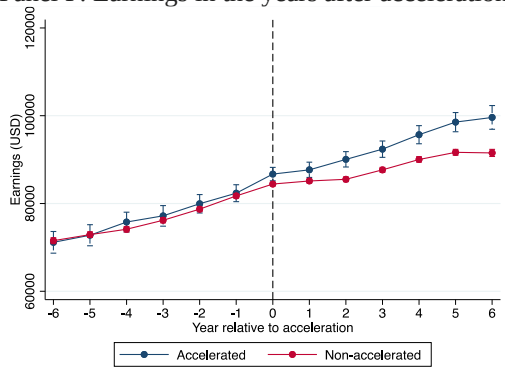
Panel D. Company departures in the years after acceleration



Panel E. Salaries in the years after acceleration



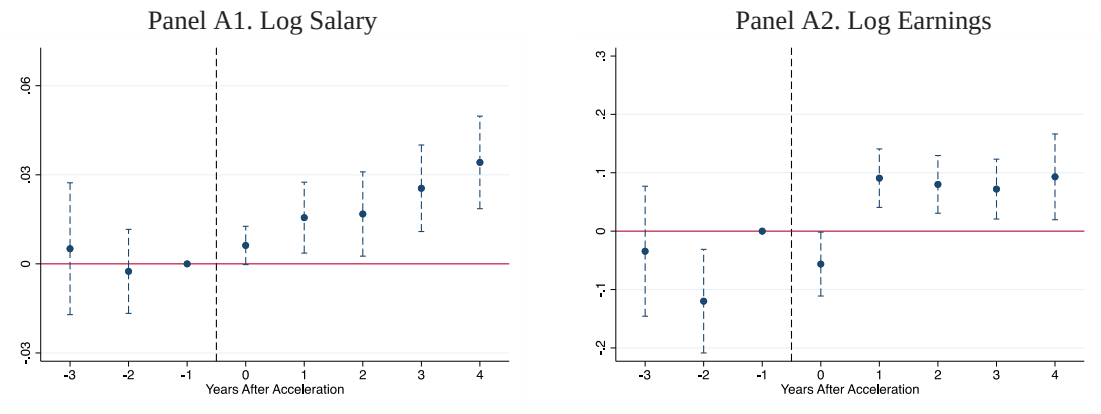
Panel F. Earnings in the years after acceleration



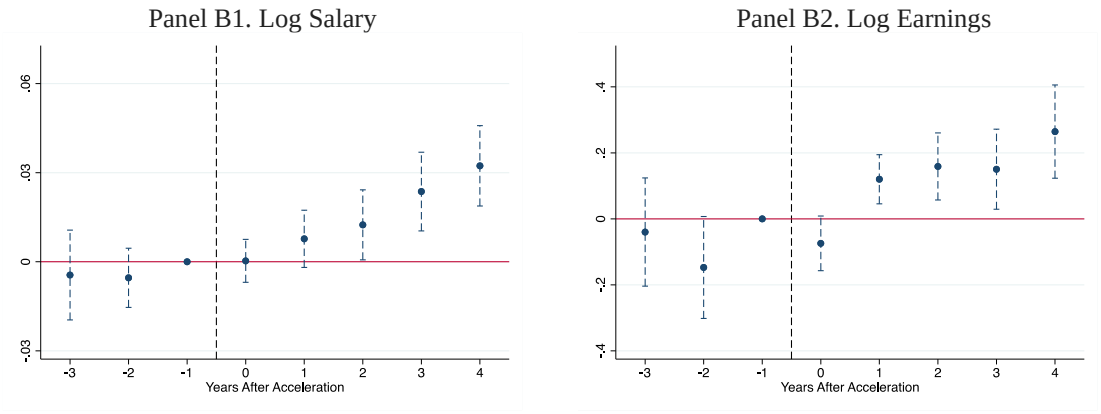
Panel A plots average salaries across positions held before, during and after acceleration. Panel B plots average employability for treatment and control groups around acceleration. Panels C and D plot the cumulative job and company departure rates after acceleration. Panel E plots average (across job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

Figure 4. Stacked Difference-in-Difference Expected Salaries and Earnings after Acceleration

Panel A. Sample matched by firm

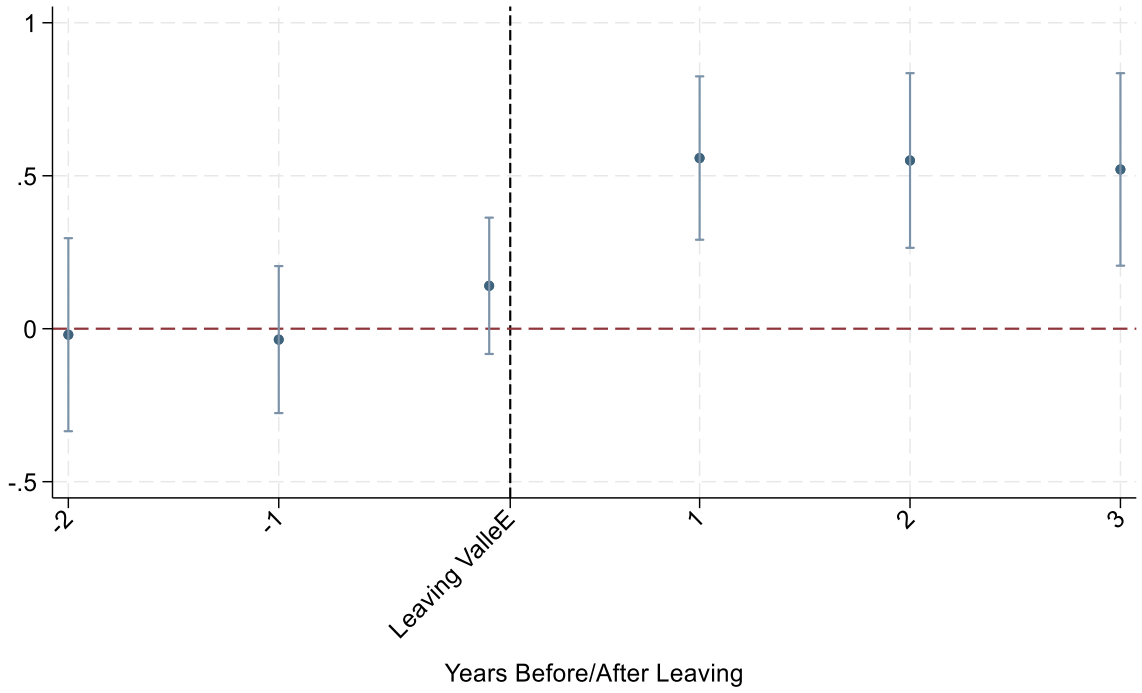


Panel B. Sample matched by individual employee



The figure plots the coefficient estimates from equation (2) using as outcome variable the variable indicated in the title.

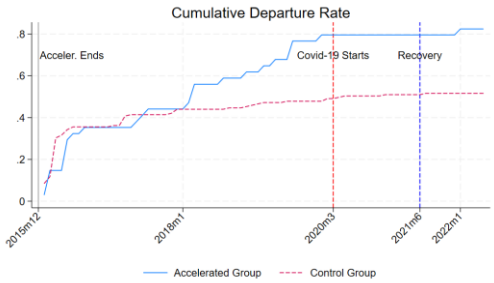
Figure 5. Salaries in Subsequent Job Positions after ValleE Acceleration



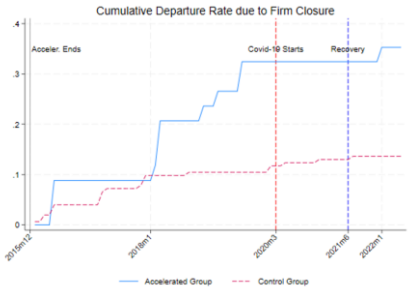
The figure plots the β_h estimates from equation (3). Standard errors are robust.

Figure 6. Departure Rates and Employability after ValleE Acceleration

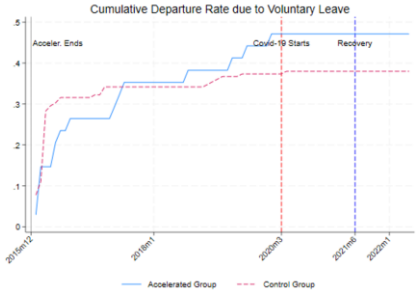
Panel A. Cumulative departure rate



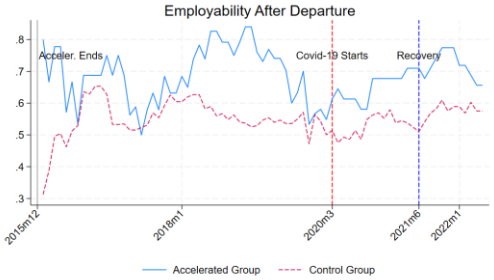
Panel B. Cumulative departure rate due to firm closure



Panel C. Cumulative departure rate not due to firm closure



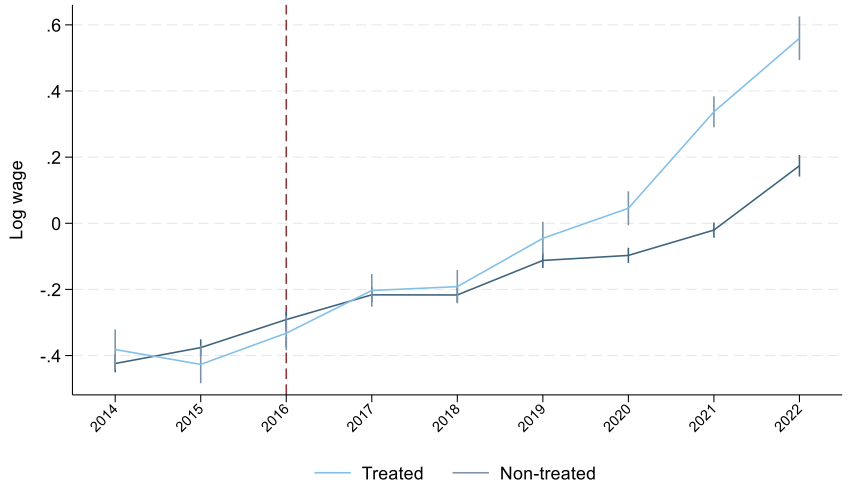
Panel D. Employability after leaving applicant firms



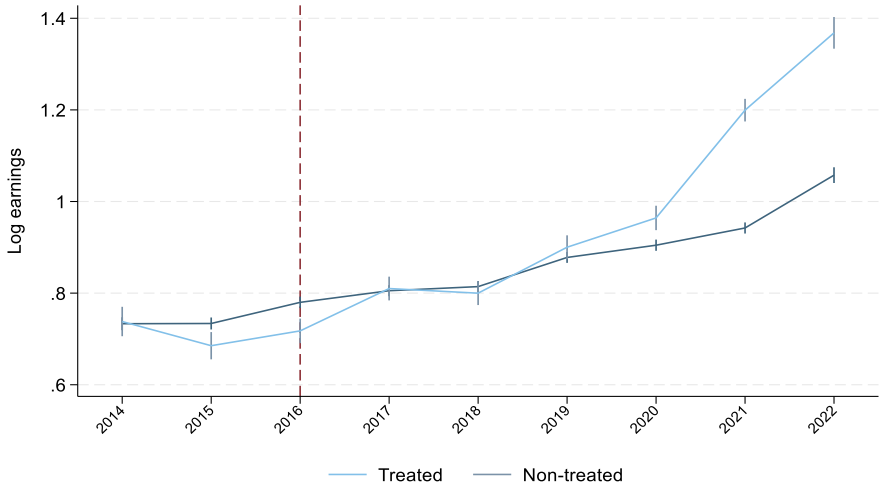
Panels A-C plot cumulative departure rates across employees of ValleE participants (accelerated group) and rejected firms (control group). Panel D depicts the fraction of employees employed after leaving the applicant firms.

Figure 7. Wages and Earnings After ValleE Acceleration

Panel A. Salaries in the years after acceleration



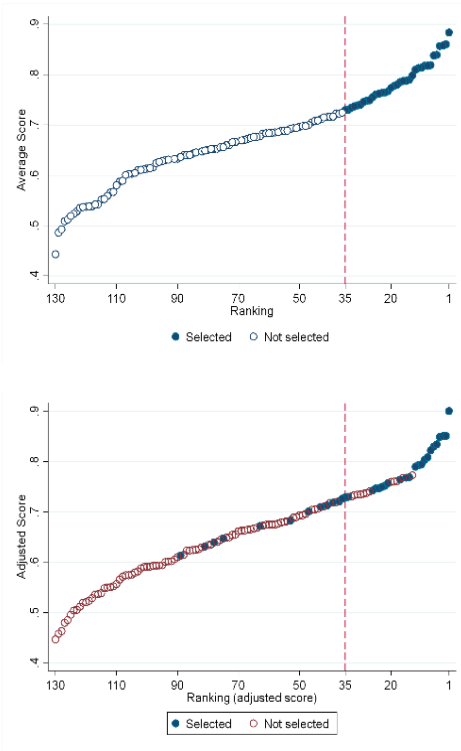
Panel B. Earnings in the years after acceleration



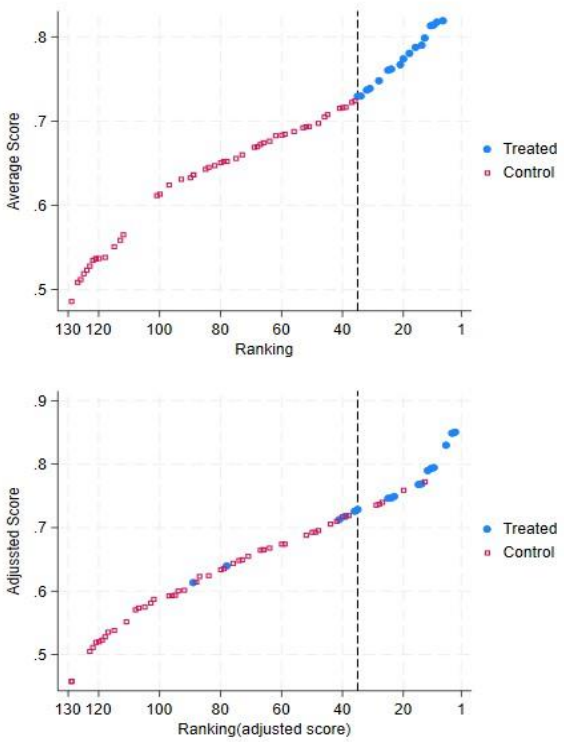
Panel A plots average (across job positions and employees) annual wages for employees of ValleE participants (Treated) and of the rejected applicants (Non-treated).

Figure 8. Distribution of Applicant Scores and Selection into the Accelerator

Panel A. ValleE Sample



Panel B. PILA Sample

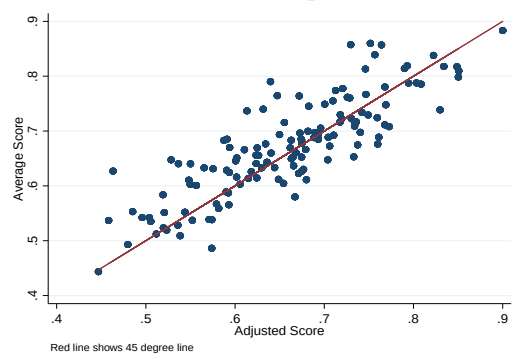


The top figure in each panel plots average scores against rankings based on the average score. The bottom figure in each panel plots adjusted scores against rankings based on the adjusted score, where adjusted scores correspond to the project's fixed effects when scores are regressed against project 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.

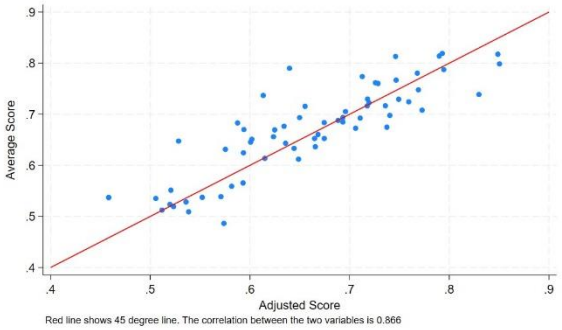
Figure 9. Acceleration Probability and Scoring Generosity

Panel A - Average Scores and Adjusted Scores

ValleE Sample

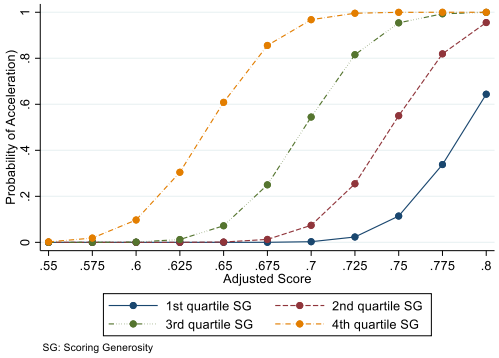


PILA Sample

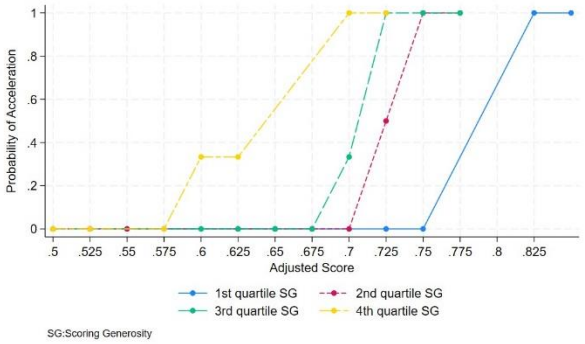


Panel B— Acceleration Probability and Generosity, by Quartiles of Adjusted Score

ValleE Sample



PILA Sample



Each plot in panel A 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). Each plot in panel B plots 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.

Table 1. Composition Cross-program

	N	mean	std.	min	P25	P50	P75	Max
Employee Level – ONET Sample								
Number of Positions	14210	7.512	3.956	1	5	7	10	16
Number of Companies Worked	14210	5.304	2.979	1	3	5	7	12
Avg. Number of Positions in Each Company	14210	1.522	0.644	1	1	1.286	1.75	3.333
Total Working Period (months)	14210	314.459	217.515	1	162	246	393	909
Avg. Tenure in Each Company (months)	14210	80.387	75.451	1	31.25	51.25	97	307
Avg. Term in Each Position (months)	14210	50.655	36.223	1	23.5	37.667	68.2	141.4
Number of Followers	14210	758.211	924.418	0	110	401.5	996	3471
Number of Connections	14210	321.501	194.396	0	119	410	500	500
Have Description (Dummy)	14210	0.52	0.402	0	0	0.6	0.917	1
Length of Description (# Words)	10266	328.102	228.129	0	143.573	287	467.95	834.857
Position Change Dummy after Acceleration	14210	0.623	0.485	0	0	1	1	1
Position Held after Acceleration	14210	1.824	1.934	0	0	1	3	6
Employer Change Dummy after Acceleration	14210	0.584	0.493	0	0	1	1	1
Number of Employers after Acceleration	14210	1.641	1.869	0	0	1	3	6
Employee Level – Full Sample								
Number of Positions	75061	6.798	3.812	1	4	6	9	16
Number of Companies Worked	75061	5.199	3.09	1	3	5	7	13
Avg. Number of Positions in Each Company	75061	1.387	0.522	1	1	1.2	1.5	3
Total Working Period (months)	75061	285.396	203.657	1	145	223	356	941
Avg. Tenure in Each Company (months)	75061	76.04	73.67	1	29.571	49	91.5	340
Avg. Term in Each Position (months)	75061	52.711	40.621	1	23.444	38.333	69.4	175
Number of Followers	75061	744.649	1034.371	0	94	359	903	4606
Number of Connections	75061	308.247	197.349	0	102	367	500	500
Have Description (Dummy)	75061	0.511	0.402	0	0	0.571	0.909	1
Length of Description (# Words)	53535	316.556	227.33	0	135.586	273.667	447.5	905.333
Position Change Dummy after Acceleration	75061	0.59	0.492	0	0	1	1	1
Position Held after Acceleration	75061	1.763	2.024	0	0	1	3	7
Employer Change Dummy after Acceleration	75061	0.553	0.497	0	0	1	1	1
Number of Employers after Acceleration	75061	1.597	1.961	0	0	1	3	7

The table presents the composition of the cross-program sample linked to O*NET and selected summary statistics of the variables extracted from the LinkedIn profiles. The observations are at the employee level. In the sample matched with ONET, there are a total of 14,210 unique LinkedIn profiles in the sample, corresponding to 1,057 employees of the 413 treated firms and 13,153 employees of the 1202 control companies.

Table 2. Summary statistics cross-program sample

Employee level								
Variable	N	Mean	SD	Min	p25	p50	p75	Max
Tenure in position (in months)	14210	16.484	13.926	1	5	12	24	45
Gender	14210	0.551	0.497	0	0	1	1	1
Age	14210	31.909	7.244	18	27	30	35	56
Education	14210	0.493	0.803	0	0	0	1	3
Subsequent positions salaries								
First position salary change dummy	14210	0.622	0.485	0	0	1	1	1
Log first position salary change	14210	0.043	0.197	-0.535	0	0	0.103	0.703
Log all position salary change	14210	0.078	0.188	-0.399	0	0	0.154	0.704
Employability								
Long run employability	14210	0.888	0.196	0.085	0.883	0.964	1	1
Change company after acceleration dummy	14210	0.584	0.493	0	0	1	1	1
Remain tenure at incumbent company (in months)	14210	50.074	39.26	2	15	42	77	151
NLP score								
Entrepreneurship								
Change word count ratio first position	14210	-0.065	0.333	-1	0	0	0	1.545
Change word count ratio all positions	14210	-0.062	0.3	-1	0	0	0	1.219
Change TF-IDF ratio first position	14210	-0.001	0.114	-0.435	0	0	0	0.437
Change TF-IDF ratio all positions	14210	0	0.101	-0.416	0	0	0	0.315
Change cosine-similarity ratio first position	14210	0.046	0.317	-0.574	-0.051	0	0.05	1.424
Change cosine-similarity ratio all positions	14210	0.053	0.289	-0.512	-0.031	0	0.08	1.27
Managerial								
Change word count ratio first position	14210	-0.039	0.659	-1	0	0	0	3.9
Change word count ratio all positions	14210	-0.001	0.672	-1	0	0	0	4.086
Change TF-IDF ratio first position	14210	-0.082	0.289	-1	0	0	0	0.414

Change TF-IDF ratio all positions	14210	-0.083	0.255	-1	0	0	0	0.371
Change cosine-similarity ratio first position	14210	0.066	0.321	-0.532	-0.016	0	0.074	1.547
Change cosine-similarity ratio all positions	14210	0.073	0.298	-0.46	-0.005	0	0.098	1.436

The table presents summary statistics for the variables in the cross-program sample. The observations are at the employee level. The sample corresponds to the sub-sample used for the O*NET analysis.

Table 3. Business acceleration and soft skills and technical skills required for subsequent jobs

Panel A: Sample matched by firm

	(1) Change soft skills all	(2) Change social skills all	(3) Change thinking skills all	(4) Change technical skills all
Acceleration	0.009***	0.007***	0.007***	-0.009***
	(2.76)	(2.78)	(2.78)	(-3.27)
Treated Group FE	Y	Y	Y	Y
Acceleration Year FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
N	14120	14120	14120	14120

Panel A: Sample matched by employee

	(1) Change soft skills all	(2) Change social skills all	(3) Change thinking skills all	(4) Change technical skills all
Acceleration	0.004**	0.005**	0.003**	-0.005***
	(2.21)	(1.98)	(2.28)	(-3.15)
Treated Group FE	Y	Y	Y	Y
Acceleration Year FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
N	14120	14120	14120	14120

The table shows results from simple regressions comparing characteristics of job requirements across treated and control employees. For every treated company, we fix the comparison of its employees' career trajectories to those of the employees in the matched control companies by including the Treated Group FE. We focus on variations in job requirements stemming from job transitions. Not all employees have job transitions, and the time between transitions varies. To avoid bias, we replace with zeros the variables for employees with no job transitions. Standard errors are clustered at the treated firm group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4. Business acceleration and frequency of subsequent managerial and entrepreneurial jobs

Panel A: Sample matched by firm

		Entrepreneurship roles		Managerial roles	
		(1)	(2)	(3)	(4)
		Position score change ratio	Position score change ratio_all	Position score change ratio	Position score change ratio_all
Word Count	Acceleration	0.016	0.020**	0.046*	0.097***
		(1.22)	(1.97)	(1.91)	(2.95)
TFIDF	Acceleration	0.015*	0.016**	0.021**	0.027***
		(1.85)	(2.14)	(2.44)	(2.62)
Cosine Similarity	Acceleration	0.020*	0.019	0.027**	0.036***
		(1.65)	(1.63)	(2.11)	(2.67)
Controls		Y	Y	Y	Y
Treated Group FE		Y	Y	Y	Y
Acceleration Year FE		Y	Y	Y	Y
N		14115	14115	14115	14115

The table shows results from simple regressions comparing outcome variables based on scores measuring propensity that jobs are entrepreneurial or managerial across treated and control employees. For every treated company, we fix the comparison of its employees' career trajectories to those of the employees in the matched control companies by including the Treated Group FE. We focus on salary variation stemming from job transitions. Not all employees have job transitions, and the time between transitions varies. To avoid bias, we replace with zeros the variables for employees with no job transitions. All regressions include controls for employee demographic characteristics including age, gender, education, and tenure before acceleration. Standard errors are clustered at the treated firm group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel B: Sample matched by individual employee

		Entrepreneurship roles		Managerial roles	
		(1)	(2)	(3)	(4)
		Position score change ratio	Position score change ratio_all	Position score change ratio	Position score change ratio_all
Word Count	Acceleration	0.016	0.019**	0.024	0.054**
		(1.54)	(2.17)	(1.17)	(2.39)
TFIDF	Acceleration	0.012*	0.015**	0.017*	0.028***
		(1.69)	(2.15)	(1.75)	(3.39)
Cosine Similarity	Acceleration	0.014	0.014	0.031***	0.041***
		(1.28)	(1.39)	(2.94)	(3.88)

Controls	Y	Y	Y	Y
Treated Individual FE	Y	Y	Y	Y
Acceleration Year FE	Y	Y	Y	Y
N	10896	10896	10896	10896

The table shows results from simple regressions comparing outcome variables based on scores measuring propensity that jobs are entrepreneurial or managerial across treated and control employees. For every treated employee, we fix the comparison of its employees' career trajectories to those of in the matched employees by including the Treated Individual FE. We focus on salary variation stemming from job transitions. Not all employees have job transitions, and the time between transitions varies. To avoid bias, we replace with zeros the variables for employees with no job transitions. All regressions include controls for employee demographic characteristics including age, gender, education, and tenure before acceleration. Standard errors are clustered at the treated individual group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5. Business acceleration and wages of subsequent jobs

	(1) First position salary change dummy	(2) Log first position salary change	(3) Log all position salary change
Acceleration	0.037**	0.062***	0.138***
	(2.46)	(12.16)	(18.33)
Sample mean (USD)		84822.44	81997.37
Economic effect (USD)		5425.44	12133.61
Treated Group FE	Y	Y	Y
Controls	Y	Y	Y
N	51063	51063	30403

The table shows results from simple regressions comparing salary-based outcome variables across treated and control employees. For every treated company, we fix the comparison of its employees' career trajectories to those of the employees in the matched control companies by including the Treated Group FE. Not all employees have job transitions, and the time between transitions varies. To avoid bias, we replace with zeros the variables for employees with no job transitions. All regressions include controls for employee demographic characteristics including age, gender and tenure in the incumbent job. Standard errors are clustered at the treated firm group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6. Business acceleration, long-run employability and departure rates

Panel A: Sample matched by firm

	(1) Long run employability	(2) Change company dummy	(3) Remain tenure at incumbent company
Acceleration	0.023***	0.046**	-1.711
	(3.37)	(2.50)	(-1.23)
Treated Group FE	Y	Y	Y
Acceleration Year FE	Y	Y	Y
Controls	Y	Y	Y
N	14115	14115	14115

The table shows results from simple regressions comparing salary-based outcome variables across treated and control employees. For every treated company, we fix the comparison of its employees' career trajectories to those of the employees in the matched control companies by including the Treated Group FE. Not all employees have job transitions, and the time between transitions varies. To avoid bias, we replace with zeros the variables for employees with no job transitions. All regressions include controls for employee demographic characteristics including age, gender, education, and tenure before acceleration. Standard errors are clustered at the treated firm group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel B: Sample matched by individual

	(1) Long run employability	(2) Change company dummy	(3) Remain tenure at incumbent company
Acceleration	0.020***	0.046***	-1.857
	(3.22)	(2.63)	(-1.37)
Treated Individual FE	Y	Y	Y
Acceleration Year FE	Y	Y	Y
Controls	Y	Y	Y
N	10896	10896	10896

The table shows results from simple regressions comparing salary-based outcome variables across treated and control employees. For every treated employee, we fix the comparison of its employees' career trajectories to those of in the matched employees by including the Treated Individual FE. Not all employees have job transitions, and the time between transitions varies. To avoid bias, we replace with zeros the variables for employees with no job transitions. All regressions include controls for employee demographic characteristics including age, gender, education, and tenure before acceleration. Standard errors are clustered at the treated individual group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7. Business acceleration and stacked differences-in-differences estimates of changes in salaries and earnings

Panel A: Sample matched by firm

	(1) Log Salary	(2) Log Earnings
Acceleration	0.023***	0.046**
	(2.89)	(2.30)
Cluster (Treatment Group)	Y	Y
Controls	Y	Y
Treated Group FE	Y	Y
Accelerated Year FE	Y	Y
N	42288	45723

The table shows results from estimating the trimmed aggregate ATT after trimming the sample and applying the corresponding weights following Wing, Freedman, and Hollingsworth (2024). As indicated in the bottom rows, the estimates control for employee demographic characteristics including age, gender, education, and tenure before acceleration. Standard errors are clustered at the treated firm group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel B: Sample matched by individual

	(1) Log Salary	(2) Log Earnings
Acceleration	0.019**	0.143**
	(2.22)	(2.33)
Cluster (Treatment Individual)	Y	Y
Controls	Y	Y
Treated Group FE	Y	Y
Accelerated Year FE	Y	Y
N	24768	30168

The table shows results from estimating the trimmed aggregate ATT after trimming the sample and applying the corresponding weights following Wing, Freedman, and Hollingsworth (2024). As indicated in the bottom rows, the estimates control for employee demographic characteristics including age, gender, education, and tenure before 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. 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					197					

The table presents the composition of the sample and selected summary statistics of the variables in the application forms. The ValleE sample includes all 135 applicants that were evaluated by judge panels. 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 9. Summary statistics PILA sample

Variables	*N*	*Mean*	*SD*	*Min*	*p25*	*p50*	*p75*	*Max*
			Incumbent Workers and Hew Hires					
Earnings	108,988	0.48	0.92	0.00	0.00	0.00	0.77	25.00
Log (Earnings)	108,988	0.29	0.40	0.00	0.00	0.00	0.57	3.26
Wage	47,381	1.10	1.13	0.02	0.62	0.83	1.12	25.00
Log (Wage)	47,381	-0.22	0.88	-4.17	-0.48	-0.19	0.11	3.22
			Incumbent Workers					
Earnings	31,085	0.57	1.06	0.00	0.00	0.15	0.80	16.49
Log (Earnings)	31,085	0.34	0.42	0.00	0.00	0.14	0.59	2.86
Wage	16,143	1.10	1.25	0.02	0.62	0.78	1.01	16.49
Log (Wage)	16,143	-0.23	0.84	-4.07	-0.48	-0.25	0.01	2.80
			New Hires					
Earnings	77,903	0.44	0.86	0.00	0.00	0.00	0.74	25.00
Log (Earnings)	77,903	0.27	0.39	0.00	0.00	0.00	0.55	3.26
Wage	31,238	1.10	1.07	0.02	0.62	0.83	1.18	25.00
Log (Wage)	31,238	-0.22	0.90	-4.17	-0.48	-0.19	0.17	3.22

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

Table 10. First-Stage: ValleE Selection and Scoring Generosity

	(1) Firm level ValleE sample	(2) Firm level PILA sample	(3) Employee level PILA sample	(4) Employee-month level PILA sample
	Acceleration	Acceleration	Acceleration	Acceleration×Post
SG	3.002***	2.638***	5.034***	
	(0.43)	(0.66)	(0.530)	
SG×Post				4.438***
				(0.0814)
N	135	66	195	48,548
R-sq	0.529	0.492	0.447	0.764
F-test excluded instruments	49.74	15.98	90.21	2970

The table shows results from the first stage regressions. Columns (1)-(2) regress Acceleration against SG, the scoring generosity of the applicants' judges. Standard errors are robust. Column (3) reports results from equation 3. Post is a variable that equals one after 2015. All regressions include employee fixed effects, month fixed effects and employee controls. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 11. Wages and Employability After ValleE Acceleration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Log Wage	Log Wage	Never in formal employment again	% of post months with formal employment	Log Wage	Never in formal employment again	% of post months with formal employment
	OLS	OLS	OLS	OLS	IV	IV	IV
Acceleration×Post	0.109***		-0.0671	0.127*	0.136***	0.00840	0.0388
	(0.0247)		(0.0564)	(0.0653)	(0.0399)	(0.163)	(0.143)
Acceleration×Post: At accelerated firm		-0.0580*					
		(0.0321)					
Acceleration×Post: After accelerated firm		0.144***					
		(0.0332)					
Constant	-0.356	-0.413	0.660**	0.355	0.509	0.727**	0.273
	(0.350)	(0.350)	(0.265)	(0.281)	(0.059)	(0.320)	(0.310)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employee & month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	48,548	48,548	195	193	48,548	162	195
R-squared	0.393	0.396	0.391	0.194	0.008	0.016	0.386

The table shows results estimating equation (3). Standard errors are heteroskedasticity robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix 1 - Word lists for entrepreneurial and managerial jobs

Table 1- Word list for entrepreneurial jobs

Word	Explanation
entrepreneur	Directly refers to someone who starts and runs their own business.
founder	Represents the originator of a business or venture, essential in entrepreneurial context.
ceo	CEO often implies leadership and responsibility in building and growing a business.
freelancer	Freelancers manage their own business-like work, reflecting entrepreneurial independence.
unicorn	A startup valued over a billion, an entrepreneurial success milestone.
pioneer	Leading innovation, a classic entrepreneurial quality.
bootstrap	Self-funding a business, an entrepreneurial strategy.
launch	Launch - Starting a business or product, a central entrepreneurial act.
startup	Startup is a new business venture, the essence of entrepreneurial spirit.
angel investor	An individual who funds early-stage startups, often with entrepreneurial experience.
innovator	Innovators bring new ideas to the market, a core entrepreneurial trait
founding	Means to establish a business, a foundational entrepreneurial action.
leader	Leaders inspire and drive business success, integral to entrepreneurial roles.
intrapreneur	Innovating within a company, reflecting entrepreneurial thinking.
freelancing	Self-employment through independent work, a form of entrepreneurship.
innovate	Creating new solutions, the essence of entrepreneurship.
emprendimiento	Spanish for entrepreneurship, directly tied to the concept.
cofound	Jointly found, often startups by entrepreneurial teams.
fundadora	Founder in Portuguese/Spanish, an entrepreneurial role.
incubation	Nurturing startups, a critical entrepreneurial process.
selfemployed	Working for oneself, a form of entrepreneurial independence.
franchise	Franchise refers to a business model where an individual operates under a larger brand, a form of entrepreneurial self-employment.
foundation	Foundation - Establishing a business, an entrepreneurial act.
cofundador	Co-founder in Portuguese/Spanish, a key entrepreneurial role.
cocreate	Collaborative innovation, common in entrepreneurial teams.
initiative	Proactive steps to start something, entrepreneurial in essence.
incubadora	Incubator in Portuguese/Spanish, nurturing startups.
creator	Creator is an innovator or founder, synonymous with entrepreneurial roles.
owner	Owner is the proprietor of a business, synonymous with entrepreneur or self-employed.
enterprise	Enterprise is a business undertaking, synonymous with entrepreneurial efforts.

leadership	Leadership is guiding a venture, a role entrepreneurs often take.
preincubation	Preincubation is an early stage of nurturing business ideas, a stepping stone in the entrepreneurial process.
self development	Self development is personal growth, a continuous process for entrepreneurs to enhance their skills and mindset.
self management	Self management is the ability to organize oneself, a vital skill for self-employed individuals and entrepreneurs.
partner	Suggests co-ownership or significant stake in a business.
contractor	Directly indicates self-employed or independent contractor status.
entrepreneurial	Directly related to entrepreneurship and starting new ventures.
business owner	Business owners run their own business, directly representing entrepreneurs
venture capitalist	Venture capitalists invest and start new businesses, linked to entrepreneurship
serial entrepreneur	Serial entrepreneurs start multiple ventures, a clear entrepreneurial role
startup leader	Startup leaders guide new businesses, an entrepreneurial position
business leader	Business leaders drive growth and success, typical of entrepreneurial roles
independent contractor	Independent contractors manage their own work, similar to entrepreneurs

Table 2- Word list for managerial jobs

Word	Explanation
manager	Directly refers to someone responsible for managing people or operations
organize	Organizing tasks and people is a core function of a manager
direct	Directing others is a fundamental aspect of managerial responsibilities
supervise	Supervising employees and projects is a key role for managers
oversee	Overseeing operations and tasks indicates a managerial position
control	Controlling processes and decisions is part of managing
administer	Administering operations is a common responsibility for managers
manage	Managing resources, teams, or projects reflects managerial expertise
ceo	Chief Executive Officer, the highest-ranking executive responsible for managing the entire organization.
chief	A high-ranking managerial role
chair	Leading meetings or committees, a managerial role.
coordination	Managers ensure all parts of the organization work together toward common goals.
administrative	Pertaining to the management and oversight of operational or organizational tasks.
executive	A high-level managerial role responsible for making strategic decisions.
president	President: The highest-level leader in an organization, responsible for strategic decision-making and overall management.
arrangement	Organizing and arranging resources is a core managerial function that ensures effective use of assets.
advisor	Advising leadership and providing strategic guidance are key functions in managerial positions.
cooperator	Cooperating with others in a managerial capacity involves team collaboration to achieve organizational goals.

coleader	Co-leading involves sharing management responsibilities and collaborating to drive organizational success.
collaborative	Collaborative management focuses on working together across teams to achieve common goals.
cfo	The Chief Financial Officer is a key member of the senior management team, handling financial planning, management, and risk control.
coordinate	Coordinating tasks, teams, or resources is a typical management function
regulate	Regulating policies or activities is often a management duty
lead	Leading teams or projects is a hallmark of managerial experience
guide	Guiding employees and decisions reflects managerial oversight
operate	Operating systems or departments is often required for managers
handle	Handling tasks and responsibilities is central to management
govern	Governing activities or teams indicates managerial authority
monitor	Monitoring progress and performance is typical for managers
command	Commanding resources or people is common in managerial roles
facilitate	Facilitating processes and meetings is a management responsibility
execute	Executing plans and strategies is a key function of a manager
delegate	Delegating tasks to others is a fundamental management skill
arrange	Arranging schedules, tasks, or resources is part of management duties
allocate	Allocating resources and time is a crucial responsibility for managers
assign	Assigning tasks to others is a core management task
strategize	Strategizing for business or projects is part of management duties
risk management	Managing risks is essential for business managers
operations management	Managing operations is a key management task

Appendix 2 – Summary statistics skills, knowledge and abilities

Employee level								
Variable	N	Mean	SD	Min	p25	p50	p75	Max
NLP analysis								
Entrepreneurial								
Pre-event avg TFIDF score	4601	0.0	0.0	0.0	0.0	0.0	0.0	0.2
Incumbent TFIDF score	4601	0.0	0.1	0.0	0.0	0.0	0.0	0.6
Change TFIDF score dummy	4601	0.1	0.3	0.0	0.0	0.0	0.0	1.0
Change TFIDF score	4601	0.0	0.1	-0.6	0.0	0.0	0.0	0.6
Change TFIDF score ratio	4601	0.0	0.2	-1.0	0.0	0.0	0.0	0.6
Change TFIDF score for average 2 years	4601	0.0	0.1	-1.0	0.0	0.0	0.0	0.6
Change TFIDF score for all future positions	4601	0.0	0.2	-1.0	0.0	0.0	0.0	1.1
Change TFIDF score ratio for average 2 years	4601	0.0	0.1	-1.0	0.0	0.0	0.0	0.6
Change TFIDF score ratio for all future positions	4601	0.0	0.2	-1.0	0.0	0.0	0.0	1.1
Pre-event avg cosine similarity score	4601	0.1	0.1	0.0	0.0	0.2	0.2	0.3
Incumbent cosine similarity score	4601	0.2	0.0	0.1	0.1	0.2	0.2	0.3
Change cosine similarity score dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Change cosine similarity score	4601	0.0	0.1	-0.1	0.0	0.0	0.0	0.2
Change cosine similarity score ratio	4601	0.1	0.4	-0.6	0.0	0.0	0.2	2.1
Change cosine similarity score for average 2 years	4601	0.0	0.1	-0.1	0.0	0.0	0.0	0.2
Change cosine similarity score for all future positions	4601	0.0	0.1	-0.1	0.0	0.0	0.0	0.2
Change cosine similarity score ratio for average 2 years	4601	0.1	0.4	-0.6	0.0	0.0	0.2	2.0
Change cosine similarity score ratio for all future positions	4601	0.1	0.4	-0.6	0.0	0.0	0.2	1.8
Managerial								
Pre-event avg TFIDF score	4601	0.0	0.1	0.0	0.0	0.0	0.0	0.4
Incumbent TFIDF score	4601	0.0	0.1	0.0	0.0	0.0	0.0	0.6
Change TFIDF score dummy	4601	0.1	0.3	0.0	0.0	0.0	0.0	1.0
Change TFIDF score	4601	0.0	0.1	0.0	0.0	0.0	0.0	0.6

Change TFIDF score ratio	4601	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Change TFIDF score for average 2 years	4601	0.0	0.1	0.0	0.0	0.0	0.0	0.6
Change TFIDF score for all future positions	4601	0.0	0.1	0.0	0.0	0.0	0.0	0.4
Change TFIDF score ratio for average 2 years	4601	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Change TFIDF score ratio for all future positions	4601	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Pre-event avg cosine similarity score	4601	0.2	0.1	0.0	0.0	0.2	0.2	0.4
Incumbent cosine similarity score	4601	0.2	0.1	0.1	0.1	0.2	0.2	0.3
Change cosine similarity score dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Change cosine similarity score	4601	0.0	0.1	-0.1	0.0	0.0	0.0	0.3
Change cosine similarity score ratio	4601	0.2	0.5	-0.6	0.0	0.0	0.2	2.2
Change cosine similarity score for average 2 years	4601	0.0	0.1	-0.1	0.0	0.0	0.0	0.3
Change cosine similarity score for all future positions	4601	0.0	0.1	-0.1	0.0	0.0	0.0	0.2
Change cosine similarity score ratio for average 2 years	4601	0.2	0.4	-0.5	0.0	0.0	0.2	2.0
Change cosine similarity score ratio for all future positions	4601	0.2	0.4	-0.5	0.0	0.0	0.3	1.8
Vector analysis								
Skills								
Basic content change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Basic content change for all future positions	4601	0.0	0.1	-1.0	0.0	0.0	0.0	0.6
Basic process change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Basic process change for all future positions	4601	0.0	0.1	-0.5	0.0	0.0	0.0	0.4
Cross-Functional - Social change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Cross-Functional - Social change for all future positions	4601	0.0	0.2	-0.7	0.0	0.0	0.0	0.7
Cross-Functional - Complex Problem Solving change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Cross-Functional - Complex Problem Solving change for all future positions	4601	0.0	0.1	-1.0	0.0	0.0	0.0	0.5
Cross-Functional - Technical change dummy	4601	0.2	0.4	0.0	0.0	0.0	0.0	1.0
Cross-Functional - Technical change for all future positions	4601	0.0	0.0	-0.1	0.0	0.0	0.0	0.1
Cross-Functional - Systems change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0

Cross-Functional - Systems change for all future positions	4601	0.0	0.2	-0.9	0.0	0.0	0.0	0.8
Cross-Functional - Resource Management change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Cross-Functional - Resource Management change for all future positions	4601	0.0	0.0	-0.2	0.0	0.0	0.0	0.5
Knowledges								
Business and management change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Business and management change for all future positions	4601	0.0	0.1	-1.0	0.0	0.0	0.0	0.7
Manufacturing and Production change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Manufacturing and Production change for all future positions	4601	0.0	0.1	-0.3	0.0	0.0	0.0	0.6
Engineering and Technology change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Engineering and Technology change for all future positions	4601	0.0	0.1	-0.6	0.0	0.0	0.0	0.5
Mathematics and Science change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Mathematics and Science change for all future positions	4601	0.0	0.1	-0.5	0.0	0.0	0.0	0.4
Health Services change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Health Services change for all future positions	4601	0.0	0.1	-0.6	0.0	0.0	0.0	0.8
Education and Training change dummy	4601	0.5	0.5	0.0	0.0	0.0	1.0	1.0
Education and Training change for all future positions	4601	0.0	0.1	-0.5	0.0	0.0	0.0	0.6
Arts and Humanities change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Arts and Humanities change for all future positions	4601	0.0	0.0	-0.4	0.0	0.0	0.0	0.4
Law and Public Safety change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Law and Public Safety change for all future positions	4601	0.0	0.1	-0.5	0.0	0.0	0.0	0.6
Communications change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Communications change for all future positions	4601	0.0	0.1	-0.2	0.0	0.0	0.0	0.4
Transportation change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Transportation change for all future positions	4601	0.0	0.0	0.0	0.0	0.0	0.0	1.0
Abilities								
Cognitive change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Cognitive change for all future positions	4601	0.0	0.1	-0.8	0.0	0.0	0.0	0.6

Sensory change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Sensory change for all future positions	4601	0.0	0.1	-0.4	0.0	0.0	0.0	0.5
Physical change dummy	4601	0.5	0.5	0.0	0.0	0.0	1.0	1.0
Physical change for all future positions	4601	0.0	0.0	-0.1	0.0	0.0	0.0	0.2
Psychomotor change dummy	4601	0.5	0.5	0.0	0.0	1.0	1.0	1.0
Psychomotor change for all future positions	4601	0.0	0.0	-0.1	0.0	0.0	0.0	0.3
Employability								
Long run employability	4601	0.9	0.2	0.1	0.9	1.0	1.0	1.0
Long-run employability after incumbent position	3269	0.8	0.3	0.0	0.7	0.9	1.0	1.0
Long-run employability after incumbent company	3245	0.8	0.3	0.0	0.7	0.9	1.0	1.0
Future 1 year employability	4601	1.0	0.1	0.2	1.0	1.0	1.0	1.0
Future 2 year employability	4601	0.9	0.2	0.2	1.0	1.0	1.0	1.0
Future 3 year employability	4601	0.9	0.2	0.2	1.0	1.0	1.0	1.0
Future 4 year employability	4601	0.9	0.2	0.1	1.0	1.0	1.0	1.0
Future 5 year employability	4601	0.9	0.2	0.1	0.9	1.0	1.0	1.0
Unemployment dummy	4601	0.1	0.3	0.0	0.0	0.0	0.0	1.0
Departure rates								
Change position dummy all	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Change position frequency all	4601	1.9	2.1	0.0	0.0	1.0	3.0	9.0
Change company dummy all	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
Change company frequency all	4601	1.8	2.0	0.0	0.0	1.0	3.0	9.0
First position transition speed	2913	158.3	281.6	0.0	0.0	31.0	184.0	1551.0
Avg position transition speed all	2174	675.2	455.8	0.0	365.0	564.5	852.0	2465.0
First company transition speed	2881	201.4	358.0	0.0	0.0	31.0	243.0	1918.0
Avg company transition speed all	2098	672.7	452.0	0.0	365.0	566.0	853.0	2465.0