Startup Employees' Career Paths: Evidence from Business Accelerators[1]

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This paper examines how working in high-growth young firms shapes workers' career trajectories. While employees of young firms often earn less in the long-term than those in established firms, high-growth young firms can offer skill development to workers that can translate into long-term positive effects on earnings. The challenge in capturing these potential positive effects is identifying high-young firms early on. Existing research often relies on hindsight by using the eventual success of startups to gauge career outcomes, which overlooks the learning potential in high-growth young firms that fail. This study takes a novel approach by using business accelerators, which identify and support high-growth young firms, as research laboratories. We conduct two types of analyses: a cross-program study of long-term job positions reported in LinkedIn among workers in accelerator-backed and non-accelerator-backed firms across the Americas, and a detailed analysis using administrative long-term wage data from participants in Colombia's ValleE accelerator. The findings from the cross-program approach show that within four years of acceleration, employees experience significant changes in the skills required for their new job roles relative to similar workers in matched control firms. There is a notable increase in cross-functional skills related to resource management, systems, and social interactions, alongside a decline in technical skills. Additionally, employees are more likely to take on managerial and entrepreneurial roles. These shifts result in higher expected wages, and do not come at the expense of lower employability, thus translating into overall average annual expected earnings increases of 5.3% in the three years after acceleration. The findings from the ValleE setting based on administrative wage data broadly confirm the patterns in wages and employability, and are robust to exploiting quasi-experimental variation in participation to the program. We interpret these findings as suggesting that business accelerators offer employees a unique intensified "startup experience," where the rapid growth or closure undergone during the program amplifies the typical learning opportunities found in high-growth young firms. Other complementary explanations for these career benefits after acceleration appear less plausible in this setting, including certification, validation, and networking effects.

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How does working at high-growth young firms shape employees' careers? In recent years, a major shift in economic research has highlighted the critical role that high-growth firms play in driving economic growth (Eslava, Haltiwanger and Pizon, 2022). Though few and facing high failure rates, high-growth young firms punch above their weight in creating jobs, fostering firm growth, and spurring innovation (Haltiwanger et al. 2016). Governments worldwide invest considerable resources to identify and support these companies, encouraging employees to join them in hopes of broader economic benefits (Goswami, Medvedev and Olafsen, 2019).

While the economic value of high-growth firms is widely recognized, surprisingly little is known about how working in these firms affects employees themselves. The effects are complex and not necessarily straightforward. On the one hand, employees may face significant challenges: greater uncertainty and unemployment risk, lower short-term pay, and benefits that, while valuable to the broader economy, do not always extend to the workers. Tensions between high-growth companies and their employees—highlighted by high-profile cases such as Amazon and Uber—underscore these risks[2]. On the other hand, these firms present unique opportunities for skill development, consistent with models of "on-the-job" training (Becker, 1962; Rosen, 1972). Employees often take on early responsibilities, gain exposure to diverse roles, and develop an entrepreneurial mindset. These experiences can position them for future success as managers or entrepreneurs, offering long-term career benefits. Even in cases of firm failure, employees may acquire transferable skills, such as "jack-of-all-trades" capabilities, that are highly valued in leadership and entrepreneurship (Rosenbaum, 1979; Stewman and Konda, 1983; Lazear, 2005).

One key reason we know so little about the effects of working in high-growth companies on employees is the significant challenges involved in accurately estimating these impacts. Much of the existing research relies on data from firms that eventually succeed, focusing on employees only after these companies have "made it" (Burton et al., 2018; Babina et al., 2019; Garcia-Trujillo, Gonzalez-Prieto, and Silva, 2024). This approach, however, neglects the high risk and failure rates associated with many high-growth young firms. Another common method compares jobs in new versus established companies (Sorenson, Dahl, Canales, and Burton, 2021). While useful, this method overlooks the considerable variation among new firms (Schoar, 2010; Pugsley and Hurst, 2011). Importantly, the skill-building opportunities colloquially attributed to high-growth young companies are relevant only to a small subset of new firms that achieve rapid growth. In contrast, most new businesses remain small and offer limited opportunities for meaningful skill development.

[2] See the following articles for a couple of examples mentioning worker related issues at Uber and Amazon: https://www.ft.com/content/9ef3a1c5-328c-460d-9261-33ea991cae62 and https://www.ft.com/content/afaf0d72-6003-484a-a62b-603e85dbb4f6.

In this paper, we address these challenges by using business accelerators as research laboratories to study how working at high-growth young firms impacts employee careers. Business accelerators are uniquely suited for this research because they specialize in identifying and supporting high-growth young firms. Unlike general small business support programs, accelerators employ rigorous selection processes to identify the most promising young companies and provide tailored services focused primarily on capability-building programs to help these businesses evolve (Gonzalez-Uribe and Hmaddi, 2021). While general training programs for entrepreneurs often yield modest results, accelerators stand out as effective (McKenzie et al., 2023). Evidence shows they excel at identifying high-potential young firms, fostering rapid growth, and facilitating the exit of less viable ventures (Gonzalez-Uribe and Reyes, 2021; Yu, 2016). However, despite their proven success in driving business growth, no study has yet explored the long-term career effects of accelerators on employees. Investigating these effects requires detailed, often hard-to-access data to track startup workers' employment trajectories, including job transitions and periods of unemployment.

Our study fills this critical gap by providing the first systematic evidence on the career paths of employees in high-growth young firms that participate in business accelerators. To achieve this, we begin by assembling the first-of-its kind comprehensive cross-program dataset of all workers in accelerator backed firms and control firms across accelerator programs in the Americas. This dataset is constructed using publicly available information from Crunchbase, a leading startup data aggregator, and LinkedIn, the primary professional networking platform for startup professionals in the Americas, including Latin America and the Caribbean (LAC). We use innovative text-based methods to analyze the long-term career trajectories of these workers. This analysis leverages job titles and descriptions from LinkedIn, combined with the ONET dataset from the U.S. Department of Labor, which characterizes job occupations based on skill requirements and average salaries.

Our main findings from the cross-program dataset reveal that within four years of acceleration, employees experience significant changes in the skills required for their post-acceleration jobs relative to similar workers in matched control firms. There is a notable increase in cross-functional skills related to resource management, systems, and social interactions, alongside a decline in technical skills. Additionally, employees are more likely to take on managerial and entrepreneurial roles. These shifts result in higher expected wages, and do not come at the expense of lower employability, thus translating into overall average annual 3-year earnings increases of 5.3%.

Next, we turn our focus to ValleE, a specific accelerator program in Colombia previously analyzed by Gonzalez-Uribe and Reyes (2021) to assess the impact of its services on participating high-growth young firms. Our current collaboration with ValleE brings a new perspective by examining the long-term career outcomes of employees. Leveraging detailed administrative wage data from

Colombia's payroll tax and social security contribution registry, we track long-term workers' career trajectories through 2022.

By focusing on ValleE, we can analyse a program with strong evidence of its ability to both identify and support high-growth young firms through capability building. This is important as accelerators vary widely in the services they offer and their level of effectiveness. Some provide cash in exchange for equity, potentially influencing career paths and salaries through mechanisms like alleviating financial constraints (e.g., startups increasing salaries given a cash influx because they tend to use back-loaded compensation packages, as shown by Howell, 2020). Notably, ValleE does not provide cash or take equity, ensuring a clean context for our analysis. Additionally, we have access to information on all applicants to the program, including those that were rejected, which allows a more robust basis for comparative analysis. The administrative data further allows us to measure formal wages comprehensively and identify periods of unemployment that workers might strategically omit from their LinkedIn profiles.[3] This detailed approach enhances our understanding of the long-term career impacts of acceleration, and thus of working in the high-growth young firms they target and support.

Our analysis of the ValleE dataset reveals that within seven years of acceleration, incumbent employees of participating firms see significant wage increases in their post-acceleration formal jobs compared to those at rejected applicant firms. Notably, these wage gains are not accompanied by an increased risk of prolonged long-term formal unemployment after acceleration. This combination of higher wages and sustained formal employability results in a permanent increase of nearly 10% in long-term annual average earnings and 20% in long-term annual average wages, starting five years after acceleration. Short-term earnings effects are slightly negative in the two years and remain flat for the following two. Supporting a skills-based explanation for these career outcomes, we also find that within seven years after acceleration, accelerated employees are more likely to be employed in knowledge-intensive sectors (as defined by the OECD classification; Galindo-Rueda, 2016), spend a greater proportion of their formal employment in these sectors, and are more likely to transition into managerial roles (proxied by being in the top 25% of the wage distribution).

In the final part of the paper, we investigate whether the observed patterns in workers' careers within the ValleE setting are purely the result of ValleE selecting high-growth companies that would have provided valuable career trajectories to workers regardless of program participation, or whether there is evidence of a causal relationship between ValleE participation and improvements in employee career outcomes. While this distinction is less critical for understanding the general career benefits of

[3] This approach also allows us to reduce any potential biases from employees in accelerated companies learning how to inflate their profiles in LinkedIn.

working in high-growth young businesses supported by accelerators—since, in either case, employees of accelerator-backed companies experience better career prospects—it holds significant implications for policy. Accelerator programs are often subsidized and understanding whether their impact stems from selection or causal mechanisms is essential for refining policy design and ensuring the efficient allocation of public funds.

We use an instrumental variables approach based on the methodology of Gonzalez-Uribe and Reyes (2021). This method takes advantage of the fact that participants were selected based on scores assigned by three randomly assigned, non-overlapping judges who independently evaluated their business plans. While the accelerator provided standardized scoring criteria, judges varied significantly in how they interpreted and applied these criteria. As a result, otherwise comparable applicants had different probabilities of being accepted into the program depending solely on the "scoring generosity" of the judges to whom they were randomly assigned. Notably, the program did not adjust for systematic differences in judges' scoring when making final selection decisions. This approach allows us to estimate the average effect of ValleE participation on employee career outcomes for companies whose acceptance—or rejection—was influenced by the generosity of the judges evaluating their applications.

We find evidence of positive causal effects from ValleE acceleration on employee career outcomes. Our findings suggest that business accelerators offer employees a unique, intensified "startup experience," where the rapid growth or closure experienced during the program amplifies the learning opportunities typically found in high-growth young firms. While the results on higher likelihood of employment in knowledge intensive sectors provides evidence consistent with this channel, we cannot cleanly rule out other complementary mechanisms that can also help explain the casual career benefits of acceleration, including potential certification, validation, or networking effects (Gonzalez-Uribe and Hmaddi, 2023). Accelerators may provide a form of certification that enhances workers' market value, aligning with models where employers struggle to distinguish between productive and unproductive workers, and employees benefit from public signals like their employer's acceleration participation (Spence, 1974). Alternatively, accelerators might offer validation that increases employees' incentives to invest in acquiring skills, consistent with models of "type revelation," where entrepreneurs learn their type through experience (Jovanovic, 1982). This validation could lead to higher skills and wages, but as a result of employees' behavioral responses rather than the accelerator directly fostering skill development. Finally, accelerators might enhance employees' networking opportunities by connecting them with other businesses in the cohort and program alumni. Such networks could improve employability and bargaining power, increasing employment returns even without significant changes in skill-building beyond socialization (Rajkumar, Saint-Jaques, Bojinov, Brynjolfsson, and Aral, 2022).

Our research contributes to the growing literature on the effects of high-growth young firm—often referred to as "startup"—employment on workers' earnings. While a substantial body of work has explored the consequences of founding a firm for entrepreneurs, far less attention has been given to the employees of startups. Most existing studies have focused on short-term wage patterns, finding that young firms, particularly small startups, tend to pay less than large, established companies (Troske, 1998; Audretsch et al., 2001; Brixy et al., 2007). Some of this wage disparity arises from sorting, as startups typically hire younger, less experienced, and less educated employees who would earn less regardless of their employer (Ouimet and Zarutskie, 2014; Sorenson et al., 2021). However, even after accounting for these differences, startups still pay less (Nystrom and Elvung, 2014; Ouimet and Zarutskie, 2014; Burton et al., 2018).

Few studies have examined the long-term consequences of startup employment, despite its importance in this context. Startup employees are not just compensated for their current performance with wages; their career advancement and future employment opportunities often hinge on their current performance (Holmstrom, 1982). Additionally, one key aspect about working for a high-growth startup is the potential learning grounds they offer to employees to acquire skills that are valued in the market. This learning option can compensate workers for the low wages and the lower liquidity that startups provide workers, given startup's tendency to use back-loaded contracts with employees, offering lower initial wages but potential long-term benefits (Howell and Brown, 2020). This raises the possibility that while startups pay less initially, the experience may lead to a long-term earnings premium both inside the firm but also through other jobs in subsequent companies.

One of the few papers examining long-term career effects is Sorenson et al. (2021), which finds that employees of young firms in Denmark earn about 17% less over the following decade compared to those hired by large, established firms. However, a critical gap in this literature is the lack of attention to the heterogeneity among young firms and the varying career impacts of working at firms with different growth ambitions and potential. Some exceptions include Burton et al. (2018), Babina et al. (2019), and Garcia-Trujillo, Gonzalez-Prieto, and Silva (2024), who document positive correlations between employment in successful high-growth young firms and employee career trajectories. By focusing on startups that succeeded, these studies capture effects for firms in the right tail of the ex-post growth distribution. However, it would be ideal to measure effects for startups in the right tail of the ex-ante growth distribution, as working in such firms could have significant career consequences even if they fail. We address this gap by using business accelerators—which specialize in identifying and supporting firms with high-growth potential—as a research laboratory to study the long-term career impacts of startup employment.

Our research also contributes to the literature on training entrepreneurs recently reviewed by McKenzie et al. (2023), specifically business accelerators (Gonzalez-Uribe and Hmaddi, 2022). By shifting the focus from the firm to the worker level, we estimate the impact of business accelerators on the long-term trajectories of individual worker's wages and employment. Our setting allows us to describe the overall impacts of these programs on employment and workers' reallocation. To the best of our knowledge, this is the first paper systematically measuring the effects of business accelerators on workers' career paths, which juxtaposes with the often-cited goal of these programs, which is to improve workers' livelihoods.

Our research also contributes to the broader finance and labor literature that examines the impact of corporate events on employees, such as 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). Within this context, our study is most closely related to research exploring the long-term career effects of private equity (PE). Existing studies highlight two opposing forces that we emphasize in our analysis: skill development and increased employability risk, though the specific skill and risk dimensions differ. For instance, Agrawal and Tambe (2016) find that employees of companies acquired by PE investors experience enhanced long-term employability and wage growth, which they attribute to the development of transferable, IT-complementary human capital. In contrast, we argue that employees in high-growth startups develop cross-functional skills oriented toward management, enabling transitions into managerial and entrepreneurial roles rather than purely technical ones. On the risk side, Antoni, Maug, and Obernberger (2018) show that employees of buyout targets often face earnings declines, consistent with increased human capital risk following PE transactions. However, our findings suggest that high-growth startup employment does not lead to significant increases in human capital risk, further distinguishing this employment pathway from the risks associated with PE acquisitions.

1. Training entrepreneurs and business accelerators

Most firms benefit from adopting a wide range of business and management practices.[4] 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

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

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 young firms 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., 2022). 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, 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. They aim to help entrepreneurs fill managerial and entrepreneurial skill gaps by providing business training, mentoring, and networking; some provide a co-working space. Some accelerators also fund their participating businesses and often take an equity stake in the portfolio company in return. Participants “graduate” at a public pitching event, commonly called a demo day.

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 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 identifying high-growth young firms in the population of new businesses and 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.

By using business accelerators as a research laboratory to explore how working on high-growth young firms shapes employee career trajectories, we also take a first step in examining the career paths of workers in participants of business accelerators. 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 our focus to the ValleE accelerator in Colombia to zoom in into a single program 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 story. We leverage administrative wage data to track wages and employability with precision. We summarize the sample and analysis in Section 3. Finally, in Section 4 we leverage quasi-experimental variation in selection to participate in ValleE to help tease the mechanisms behind the general patterns in worker's earnings after acceleration.

2. The cross-program approach: business accelerators in the Americas

To explore the startup workers' career paths in beneficiary firms of business accelerators in the Americas, we begin by assembling a dataset of companies that participated in these programs. Our data source is the online platform Crunchbase, which we accessed in June 2024.

2.1. 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 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.[5] 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

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

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. 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).

2.2. Sample

Our initial treatment sample includes 9,430 businesses sourced from Crunchbase that participated in business accelerators between January 2005 and December 2019. This timeframe ensures we can track career effects for at least four years after the acceleration program. To maintain consistency, we excluded companies that participated in small accelerators with fewer than 20 participants, as such programs may not represent typical acceleration models. Additionally, we restricted the age of firms at the time of participation to less than five years, ensuring that the treated companies are genuinely "startups." For each treated company, we identified 5–6 control startups based on the following matching criteria: (1) same country (if the company is in the US, same 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). To compile the employee data, we searched for the companies in our sample on LinkedIn. The final matched dataset includes 2,105 treated firms and 3,393 control firms. The reduction in the sample size is due to missing employer profiles on LinkedIn and invalid company LinkedIn URLs, particularly for firms outside the U.S. and Canada. The treatment firms participated in 4,249 accelerator programs (run by 408 unique accelerators) between January 2005 and December 2019. These firms collectively account for 365,314 unique LinkedIn profiles of individuals who have worked at these companies at any point. We track these individuals' career trajectories on LinkedIn from the earliest date reported on their profiles, spanning from May 1987 to October 2019. For most of the analysis, we focus on employees who were working at the company at the time of its first participation in a business accelerator program. We refer to this group as "incumbent employees." The dataset includes a total of 21,736 incumbent employees, comprising 6,721 employees from treatment companies and 15,015 from control firms. Table 1 provides an overview of the sample, including all treated and control employees.

Having assembled a dataset of career paths for 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.

*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 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 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, 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.

[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.

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.

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 162 unique treated startups (roughly 8% from the treatment group) that participated in 104 unique business accelerators, and their 530 unique control firms matches (control to treated ratio 3.27). These companies have 44,936 unique individual LinkedIn profiles, 7,934 of which correspond to treated employees who worked in beneficiary firms, and 37,002 of which correspond to control employees who worked in control companies. Of those, 4,345 correspond to incumbent employees; 356 in treated firms (2.20 employees on average per firm) and 3,989 (7.5 employees on average per firm) in control companies. We then complement this sample with all treated companies and their matches in the LAC region to better represent developed and developing countries in the sub-sample. The total number of companies in the LAC sub-sample is 98; there are 45 treatment firms and 53 control firms. The treatment firms participated in 45 unique business accelerators. Combined, these LAC companies have 7,367 unique LinkedIn profiles 6,863 correspond to treated employees who worked in beneficiary firms, and 504 correspond to the control employees who worked in control companies. Of those, 256 are incumbent employees, 171 treated employees (3.8 employees per firm), and 85 (1.6 employees per firm) are control ones. Table 1 provides an overview of the sample, including all treated and control employees.

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 use the "genism" library in Python to train the word2vec model. We set the dimension of word vectors to 300, define three words as neighbors if they are no farther apart than seven words in a sentence, and omit words that appear fewer than five times in the job positions and descriptions corpus.

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

After training, the model converts each of the 1,472,470 words in the LinkedIn job positions and description corpus to a 300-dimensional vector representing that word's meaning. Then, we compute the cosine similarity between any 2-word vectors to quantify their association.

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 word vector for "manager" and "entrepreneur".[8] We do not consider named entities that are recognized automatically by the Stanford CoreNLP package. We use ChatGPT 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 39-word and 64-word lists or dictionaries for entrepreneurial and managerial jobs, respectively.

With the dictionaries in hand, we classify jobs as managerial or entrepreneurial depending on how much a particular job title and description includes the words in the dictionaries. We use two different approaches to create a score. We denote the first by *CS*, which calculates the cosine similarity of all words in the job description with the words in the dictionaries. We denote the second by *TFIDF*, which corresponds to the product of two statistics: term frequency in a given job description (TF) and inverse document frequency (IDF) across all job descriptions. Intuitively, this method provides lower scores to words that appear with a high frequency across all documents.

2.3. 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.

[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.

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. Table 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 (June 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,

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

Where Y_j represents the 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, $\gamma_{\hat{\imath}}$. 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.

2.4. 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 summarized in Table 3.

Results in Table 3 show similar patterns to those in Figure 2. Panel A shows significant differences for both basic skills and cross-functional skills that facilitate performance of activities that occur across jobs. Panel A shows that basic content and process skills significantly increase by 3.89% and 11.83%, respectively. For cross-sectional skills, all but two (complex problem solving and resource management) of the cross-functional skills categories experience a significant shift relative to the control group. Systems and social cross functional skills increase by 13.80% and 9.77%, whereas technical cross-functional skills significantly decrease by 66.67%. With regards to knowledge, the significant positive changes in knowledge requirements are for the subcategories of Communications (34.17%), Business and Management (9.90%) and Law and Public Safety (11.69%). There are no changes in the categories Arts and Humanities and Transportation. All other knowledge categories see a negative and statistically significant change relative to the control group. Finally, Panel C shows no significant changes in the abilities required in the job positions across treated and control groups except for cognitive abilities, which increased by 8.06%. The patterns depicted in Panel D of Figure 2 show that the change is explained by an increase in the requirements of quantitative abilities and memory, both subcategories of cognitive abilities.

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

The shift 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).

2.6. Characterizing Jobs by Expected Average Wages 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. The figure 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.

Next, we define several variables comparing salary changes between job positions to use as outcome variables in regressions controlling for individual characteristics: *Incumbent position salary*, the salary of the incumbent position (job position when the treatment (control) firm is (not) accelerated); *First position salary change dummy*, indicating whether the salary in the next position differs from the incumbent position salary; *First position salary increase (decrease) dummy*, indicating whether the salary in the next position is higher (lower) than the incumbent position salary, *Log first position salary change*, the log difference between the next and the incumbent salary; and *First position salary change ratio*, the ratio change in salary between the incumbent position and the next position—calculated as the difference divided by the incumbent position salary. Beyond looking at the most immediate changes in salary across the incumbent and the next position, we are also interested in capturing long-term changes in salaries from subsequent job transitions. To capture long-term changes, we define the variables *Log all (last) position salary change*, corresponding to the log differences between the incumbent salary and the average of all (last) subsequent positions; and *All position salary change ratio*, the ratio change in salary between the incumbent position and the average of all (last) subsequent positions—calculated as the difference divided by the incumbent position salary. Finally, we note that for each employee we cover different time windows pre and post acceleration. This is so because companies participate at different times in acceleration programs and employees differ in terms of transitions across jobs and in and out of unemployment. To address this heterogeneity, we also define variables that use a fixed window over time for the analysis: *Position salary change ratio_2y*, which corresponds to the salary change between the incumbent position and the average of the positions held over the next two years (divided by the incumbent position salary). In some of the regressions, we use the whole ONET sample and replaced with zero the variables for employees with no job transitions (4,502 observations). In other regressions, we use the conditional sample of employees that hold different jobs after acceleration (2,808 observations). Table 2 presents summary statistics for the salary variables.

Results from estimating equation (1) using the different salary-based variables are summarized in Table 5. Panel A summarize results using variables focusing on the first subsequent job. Panel B summarizes results based on salary variables measured over the entire post-acceleration period, focusing on the last position during the sample period, or within a 2 year window from acceleration.

The results in Table 5 show evidence of higher expected salary increases across positions for treated employees relative to control employees. The positive difference begins with the first immediate job transition (Panel A); and they are economically similar if we estimate them using the entire sample (columns 4 and 5) or on the conditional sample of individuals with at least one subsequent job after acceleration. The positive difference remains for the rest of the job transitions until the end of the sample (Panel B, columns 1 and 2), and is also there if we consider any job transitions within a window of 2 years of acceleration (Panel B, columns 5 and 6). Some of the positive differences are driven by extensive margin effects: columns 1 and 2 in Panel A show that treated employees are more likely to change salaries across positions and see a salary increase. In terms of economic magnitude, the estimate in Panel B column 3 imply that the average increase in expected annual salary within 2 years after acceleration is approximately $7,000 dollars, which corresponds to roughly 8% of the sample mean (Panel B, column 6).

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

In this section, we examine whether the positive wage 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 3.9% more time employed, on average, compared to control employees after acceleration. The remaining columns in Panel A show no significant differences in employability within 1 to 4 years after acceleration; however, these differences become significant starting 5 years after acceleration. Columns 2 and 3 of Panel A focus on employability conditional on employees leaving their incumbent roles or companies. These results show higher, economically meaningful differences in long-term employability for treated employees, at 9%

and 9.2%, respectively. They show that conditional on departing the incumbent position or company, treated employees have circa 10% higher employability rates.

Panel B in Table 6 highlights economically meaningful differences between treatment and control groups in terms of job position changes and departure rates from companies, despite similar long-term employability. These differences are not necessarily associated with higher or faster failure rates of accelerated businesses. The last four columns of Panel B indicate that while the propensity to leave their incumbent jobs differs between treated and control employees, the speed at which departures occur does not.

2.8. 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 salaries around 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. Table 7 summarizes the ATT on salaries and earnings and their log transformations; Figure 4 plots the equation (2) estimated coefficients. Table 7 shows positive and statistically significant effects. Both average annual expected salary and earnings increase by $5K in the 3 years after acceleration (columns 2 and 6) relative to matched controls; which corresponds to a 6% and 5.3% percentage increase, respectively (columns 4 and 8). Figure 4 shows that the increases in expected salaries and earnings are immediate, with positive and statistically significant coefficients from the year of acceleration. The exception is log earnings—the plot shows a positive and statistically significant effect starting in year 3 after acceleration.

3. Zooming in on the ValleE accelerator in Colombia[10]

In this section, we zoom in the ValleE accelerator in Colombia. By focusing on ValleE, we can analyse a program with strong evidence of its ability to both identify and support high-growth young firms through capability building. This is important as accelerators vary widely in the services they offer and their level of effectiveness. Some provide cash in exchange for equity, potentially influencing career paths and salaries through mechanisms like alleviating financial constraints (e.g., startups increasing salaries given a cash influx because they tend to use back-loaded compensation packages, as shown by Howell, 2020). Notably, ValleE does not provide cash or take equity, ensuring a clean context for our analysis. The administrative data further allows us to measure formal wages comprehensively and identify periods of unemployment that workers might strategically omit from their LinkedIn profiles.[11] This detailed approach enhances our understanding of the long-term career impacts of acceleration, and thus of working in the high-growth young firms they target and support.

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.[12] 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.[13] 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).[14] 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

[10] This section follows Gonzalez-Uribe and Reyes (2020).
[11] This approach also allows us to reduce any potential biases from employees in accelerated companies learning how to inflate their profiles in LinkedIn.
[12] 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%).
[13] 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.
[14] The top two impact objectives among ecosystem accelerators are employment generation (35%) and community development (30%). Source: The Entrepreneurship Database at Emory University.

the scale of an existing young (0-3 years) business located in the region is in principle eligible for the program. However, the program focuses on high-growth entrepreneurs, and many applicants are *de facto* incompatible and thus rejected (as explained in more detail in Section 3.2).

Like traditional business accelerators, ValleE provides participants with a variety services, including standardized grouped business training, one-to-one customized advice, and increased visibility. It offers no cash as is common among the subset of ecosystem accelerators worldwide, cf. Clarysse, Wright, and Van Hove, 2015).[15] 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).

3.1. 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

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

individually score the application.[16] The total number of judges is 50, and thus judges only partially overlap across applicants. The judges evaluate the applications according to five criteria: (i) clarity of the business model proposal, (ii) innovation, (iii) scalability, (iv) potential profitability, and (v) entrepreneurial team. Finally, the staff at the accelerator makes the final decision by picking the top 35 applicants based on average scores. It is impossible for judges and applicants to manipulate the ranking process. Judges are unaware of the weight of each criteria in the final score; they independently score projects, are not aware of the identity of the other judges in the panel, and no judge sees all applications. Applicants do not know who their judges are, nor do they know their position in the ranking. [17] 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.[18] 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 8, Panel A). Gonzalez-Uribe and Reyes (2021) 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

[16] 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.
[17] Entrepreneurs were never given their ranking or scores in order to avoid any negative psychological effects or create rivalry among participants.
[18] 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.

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 (2021) 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.

3.2. Sample

ValleE provided us with all the application data, including application scores by each judge and final selection decisions, for the program's first cohort.[19] 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 (2021) 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

[19] 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.

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 47,381employee-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 8 compares the businesses in the ValleE sample the PILA sub-sample. The PILA sub-sample covers individuals in relatively more established businesses relative to those at the ideation stage (64% relative to 47%), and that are larger in terms of their revenues at the time of application (41.84 M COP relative to 25.80M). The relatively higher maturity and size of the businesses covered by the PILA sample aligns with the idea that businesses are more likely to employee formal employees as they become larger and are more mature, given the large costs associated with formal employment in Colombia (Bernal, Eslava, Melendez and Pinzon, 2017).

As the ValleE sample, the PILA subsample is comparable to average applicant in the cross-program 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.[20] 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).[21] 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

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

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 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, *Earnings* which is equal to *Wage* except during formal unemployment spells where we replace the missing observation with 0, and *log(Earnings)* which equals the inverse hyperbolic sine transformation of *Earnings*. Note that we only replace with zeros formal unemployment spells after we observe the individual for the first time in the data and until the individual is 69 years old. We also define *Long-Run Employability* as the fraction of time after acceleration and until the end of the sample (and before the employee turns 69) that we observe the employee hold a formal employment. Table 9 presents summary statistics of the main outcome variables.

3.3. Empirical Strategy and Results

We mimic the analysis of the cross-program sample in the empirical analysis of the ValleE sample. 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 mimicking the analysis we conducted for the cross-program sample.

First, we characterize the jobs that employees hold before and after their high-growth employer participates in ValleE or not. The characterization focuses on wages. Figure 5 plots the estimated β_h from the equation below comparing the wages of the new positions held by accelerated employees after they leave the accelerated company, and relative to control firms.

$$Log(Wage)_{jt} = \alpha + \sum_{h=-2}^{h=3} \beta_h (Acceleration_i \times D_h) + \sum_{h=-2}^{h=3} \mu_h \times D_h + X_{jt} + \vartheta_j + \mu_t + \varepsilon_{jt} \qquad (3)$$

Where j indexes individuals and t indexes months, X_{jt} is a vector of time-varying individual controls including age, experience, experience squared, tenure, tenure squared, share of potential experience in the formal labor market. We include individual and month fixed effects in the regression, and report heteroskedasticity robust standard errors. In this regression, the coefficients of interest are β_h capturing the average changes in formal labour market outcomes for any given employee who leaves the ValleE firm and relative to their average formal labour market outcomes while working in the ValleE firm. This comparison is relative to control workers that leave the rejected ValleE applicant to formally join another business. In some of the regressions, we also include new hires into the estimation to help estimate the role of controls and the effects of macroeconomic conditions (year effects) with a higher precision. In the tables we report only the coefficients and effects pertaining to incumbent employees.

Figure 5 shows evidence of positive transitions for employees of ValleE companies: conditional on leaving the company, accelerated employees see a relative increase in their wages. These results are consistent with the findings reported in Figure 3 and Table 5 for the analysis using the cross-program data showing immediate average higher salary increases across positions for treated employees relative to control employees.

The differences shown in Figure 5 are conditional on employees leaving the applicant firm after acceleration. These conditional differences in log wages reflect variations in the types of jobs individuals secure post-acceleration, as well as potential differences in departure rates, job-finding likelihood, and the speed of re-employment. These factors may vary between high-growth startups and other companies and could be influenced by acceleration as well. To further investigate, our second analysis examines differences in departure rates and long-term employability between employees of ValleE participants and those of rejected applicants.

We start by comparing cumulative departure rates between employees of accelerated and rejected applicants. Panel A of Figure 6 shows that until 2017, departure rates for employees of ValleE participants are similar to those of rejected applicants. However, two years after acceleration, employees of ValleE participants begin leaving their employers at a significantly higher rate, a trend that continues until the start of the pandemic, when the cumulative departure rate stabilizes at 80%. In contrast, the departure rate for employees of rejected applicants stagnates much earlier, around mid-2017, and rises only modestly throughout the rest of the sample period, reaching 40%. Panel B highlights that these differences in departure rates are largely driven by firm closures, which spike for ValleE participants around two years post-acceleration and continue to rise until the onset of the pandemic. Finally, Panel D compares the monthly formal employment probability of employees who leave their applicant firms. Employees from accelerated firms consistently demonstrate higher employability probabilities throughout the post-acceleration period compared to employees from rejected applicants.

Column 3 of Table 10 demonstrates that the higher employment likelihood shown in Figure 6 translates into a 17.6% increase in long-term employability for employees of accelerated companies compared to those of rejected applicants over the seven years following acceleration. This result holds even after accounting for individual differences between employees in accelerated and rejected firms. Additionally, Column 2 reveals that employees of accelerated companies are 16% less likely to never hold formal employment again after leaving their acceleration firm. However, as shown in Column 1, transition times between jobs are similar for employees of accelerated and rejected firms, conditional on finding a new job post-acceleration. Importantly, the results on higher employability and faster departure rates are consistent with the findings of the cross-program analysis of section 2.

In our third analysis, we integrate the first two approaches into a single regression that examines time-series average differences in wages and earnings between treated and control incumbent employees before and after acceleration. Figure 7 displays the unconditional average annual salaries and earnings of employees from ValleE participants and rejected applicants. The data show that wages and earnings were similar across the two groups before acceleration and remained comparable until 2017. However, starting three (four) years after acceleration, a clear inflection point emerges, with the two series diverging. Employees of ValleE participants experience a sustained increase in wages and earnings, which becomes progressively more pronounced through the end of the sample period.

Results in Table 11 confirm the patterns in Figure 7. The table presents results from estimating the following equation

$$Y_{jt} = \alpha + \rho_1 Acceleration_j \times Post_t + X_{jt} + \vartheta_j + \mu_t + \varepsilon_{jt} \qquad (4)$$

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 of the estimations we estimate a more flexible version of equation (4) allowing the effects post acceleration to differ across years in order to capture the differential effects over time. We report heteroskedasticity robust standard errors. In some of the regressions, we also include new hires into the estimation to help estimate the role of controls and the effects of macroeconomic conditions (year effects) with a higher precision.

The results in Column 1 of Table 11, combined with the departure patterns shown in Figure 6, suggest that the inflection point in wages and earnings observed in Figure 7 is linked to higher departure rates (and higher wages of at new positions) from the applicant companies by employees of ValleE participants. Supporting this interpretation, Column 3 reveals that the null average effects over the entire post-acceleration period, reported in Column 2, obscure significant variation in wages across job positions. While average wages for employees who remain at participating firms do not change after acceleration, Column 3 shows a substantial 8% increase in wages for employees after they leave the accelerated company. Column 4 shows the same patterns in earnings: null average effects over the entire post period, however masking substantial variation over time, and in particular positive, statistically significant, and permanent earnings increases of circa 10% starting 5 years after acceleration. The patterns in average annual salaries and earnings depicted in Figure 7 and Table 11, echo the results for the cross-program analysis.

Overall, the results from the detailed analysis of the ValleE accelerator align with the cross-program patterns discussed in Section 2. Employment in high-growth startups participating in accelerator programs is associated with positive long-term career effects, including higher wages and earnings over time. These outcomes are linked to transitions into higher-paying jobs without an increase in the frequency or duration of unemployment spells. employability Supporting the idea that these patterns in wages and employability stem from employees of high-growth startups acquiring market-valued skills, Table 12 shows that acceleration is associated with a higher likelihood of employees transitioning to jobs in knowledge-intensive sectors. Additionally, employees spend more time in these types of roles and are more likely to earn compensation in the top wage distribution, suggesting they move into managerial positions.

3.4. Impact mechanisms

Why are there such large and positive effects on workers' career paths after acceleration? One potential explanation is that business accelerators have the ability to select high-growth companies that provide skill-development opportunities for employees. Alternatively, it is possible that participation in the accelerator itself may positively affect workers' career paths. At least three main types of mechanisms are possible: further skill acquisition in the program, certification and window dressing.

In this section, we investigate whether the observed patterns in workers' careers within the ValleE setting are purely the result of ValleE selecting high-growth companies that would have provided valuable career trajectories to workers regardless of program participation, or whether there is evidence of a causal relationship between ValleE participation and improvements in employee career outcomes. While this distinction is less critical for understanding the general career benefits of working in high-growth young businesses supported by accelerators—since, in either case, employees of accelerator-backed companies experience better career prospects—it holds significant implications for policy. Accelerator programs are often subsidized and understanding whether their impact stems from selection or causal mechanisms is essential for refining policy design and ensuring the efficient allocation of public funds.

To identify the casual effects of acceleration, we follow Gonzalez-Uribe and Reyes (2021) 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 (2021) 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 (4) is:

$$(5) \qquad Acceleration_j \times Post_t = \alpha + \gamma_1 SG_j \times Post_t + X_{jt} + \vartheta_j + \mu_t + \varepsilon_{jt}$$

We present results using heteroskedasticity robust standard errors.

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 (2021). In unreported results, we verify that results are similar using the different SG measures as instrument. In some of the regressions, we also include new hires into the estimation to help estimate the role of controls and the effects of macroeconomic conditions (year effects) with a higher precision. In those cases, we expand the regression models and estimate separate effects for incumbents and new hires. We only report the corresponding coefficients for incumbent employees to ease exposition.

The IV estimate of ρ_1 captures the local average treatment 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. In the PILA sample, type 2 mistakes are slightly more frequent as are higher quality firms with relatively high adjusted scores. This is as expected given the predictive ability of judges, as shown by Gonzalez-Uribe and Reyes (2021). 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.

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 (2021) 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.[22]

[22] 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.

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) in Table 13, 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)-(4) of Table 13, we show results from running equation 5 in the conditional sample (all job spells) and the unconditional sample used for the earnings regressions (balanced panel). Standard errors are robust. We report the F-test 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 8; 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 unlikely (e.g., feelings of grandeur or depression). Finally, Gonzalez-Uribe and Reyes (2021) 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.

Table 11 presents the IV results for the main regressions looking at average changes in log wages and log earnings after acceleration. The evidence shows that the association between participation in the accelerator and positive career outcomes in the form of increased wages and earnings appear to not only be explained by a selection mechanism. The results in columns 4 and 6 show evidence the accelerator treatment effects on average wages and earning in the 7 years following acceleration.

We have no exogenous variation to cleanly distinguish the channels behind the treatment effects, but taken together with the highr likelihood of transitioning to working at a high skilled sector is strongly suggestive of skill story, where accelerator programs provide opportunities for employees to further develop capabilities valued in the market.

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. In the second approach, we zoom in to the Colombian ValleE accelerator, using official employer-employee linked wage data from Colombia's payroll tax and social security contribution registry.

Our findings show both short- and long-term positive effects on employee wages, even after they leave their jobs at the accelerated firm. These wage gains are linked to employees assuming new managerial and entrepreneurial roles or moving to larger companies. The wage differences cannot be fully explained by accelerators selecting firms with employees at pivotal career stages. Instead, evidence suggests that accelerator participation directly influences employees' career trajectories.

Several factors potentially contribute to this effect, including the businesses' acceleration providing opportunities to learn about new skills and develop them, as well as offering a form of certification that enhances market value. Consistent with the skill hypothesis, we observe significant shifts in the skills (cross-functional), knowledge (communications and business management), and abilities (cognitive) required in the career paths of workers in accelerated firms.

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).

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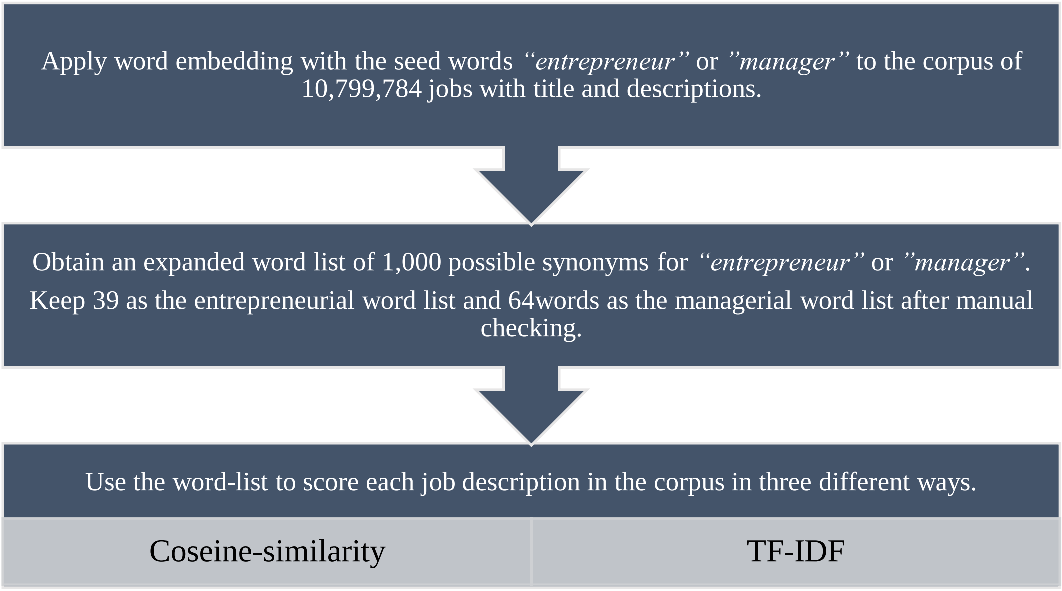
Figure 1. Flow Chart of the Neural-Network Algorithm

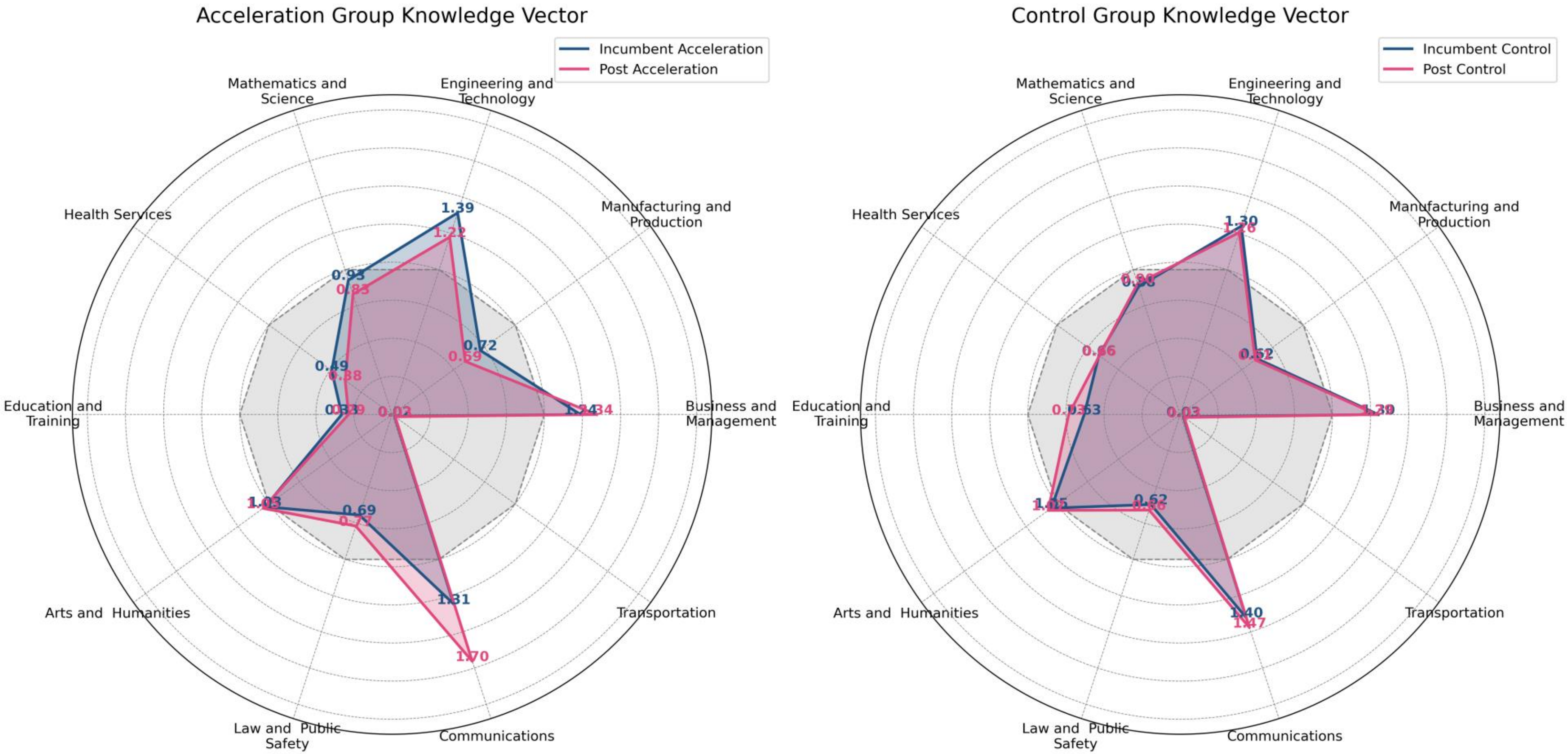


The figure presents the flow chart of the Neural-Network model we use to characterize jobs as entrepreneurial and/or managerial.

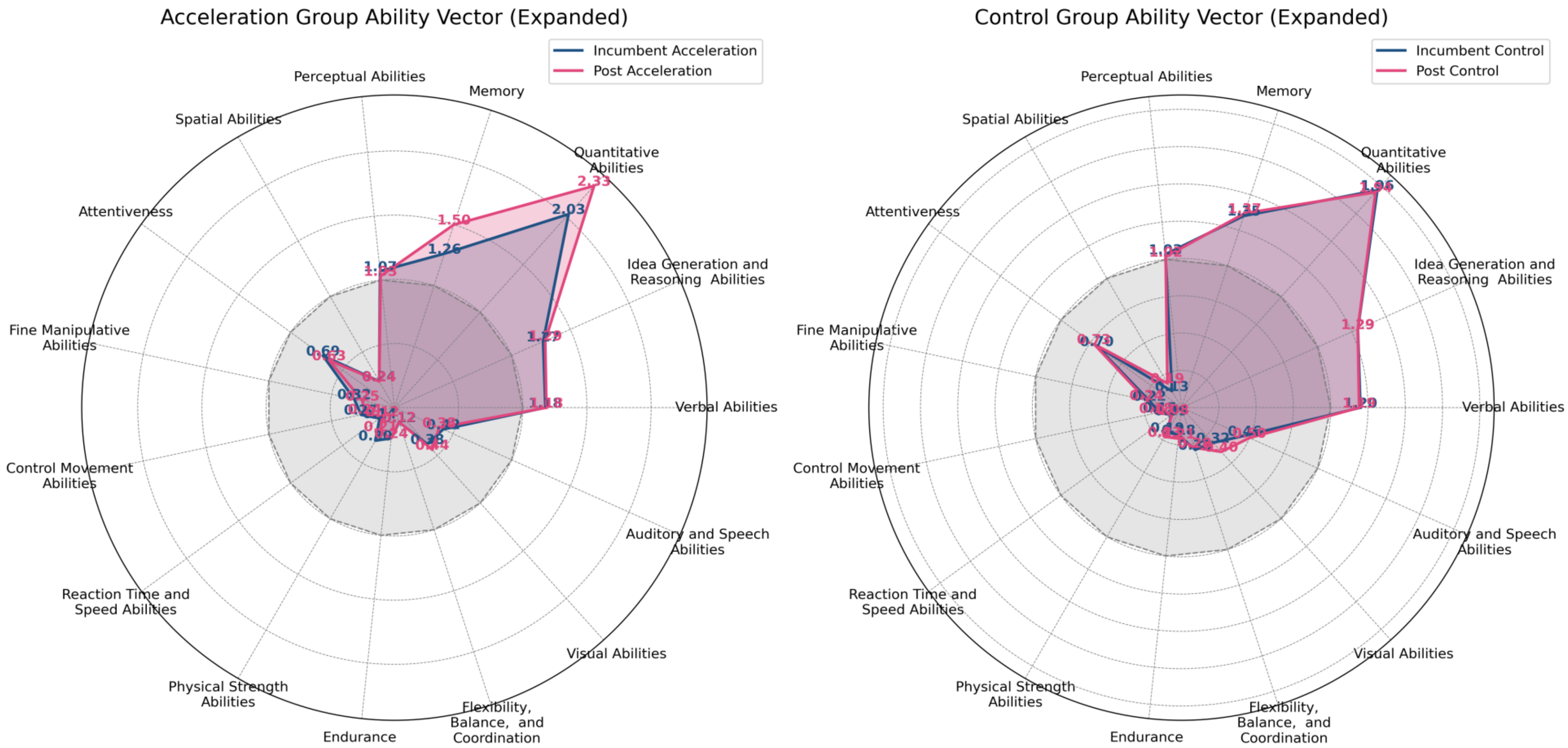
Figure 2. Skills, Knowledge and Abilities of jobs during and after acceleration

Panel A. Skills Radar Charts

Panel B. Knowledge Radar Charts



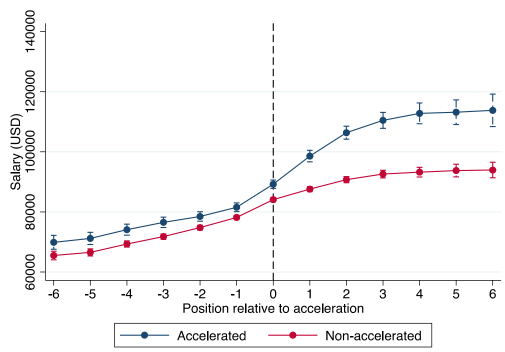
Panel C. Abilities Radar Charts by sub-sub-category



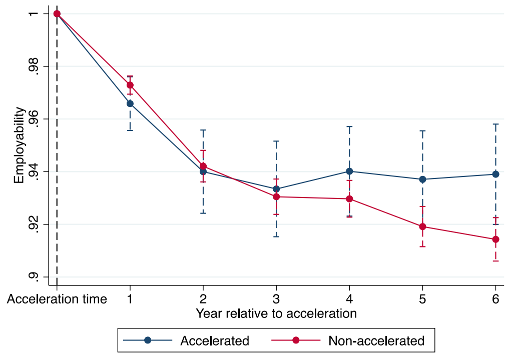
The figure shows radar charts for the job requirements in terms of skills (Panel A), knowledge (Panel B) and ability (Panel C). Each panel compares radar charts for treated employees (left) to control employees (right). For each group of employees, we plot the radar chart during and post treatment. The figures are based on 2,913 employees with a new job after acceleration.

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

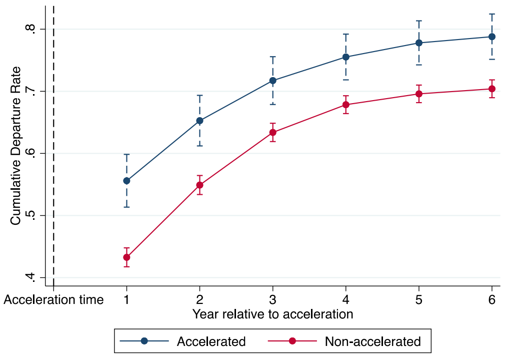
Panel A. Salaries in subsequent job positions after acceleration



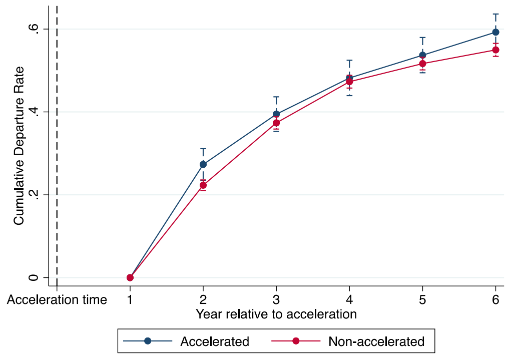
Panel B. Employability in the years after acceleration



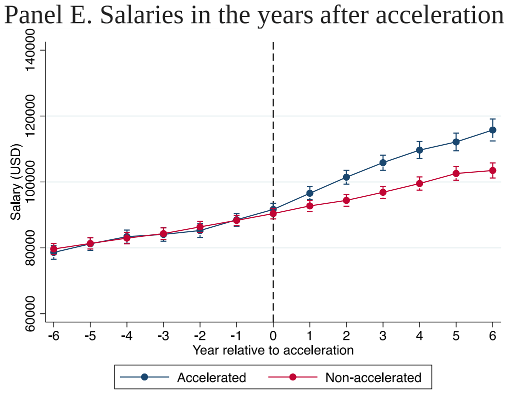
Panel C. Job departures in the years after acceleration



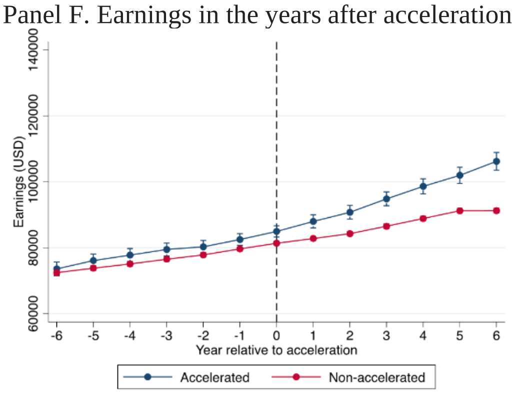
Panel D. Company departures in the years after acceleration



Panel E. Salaries in the years after acceleration

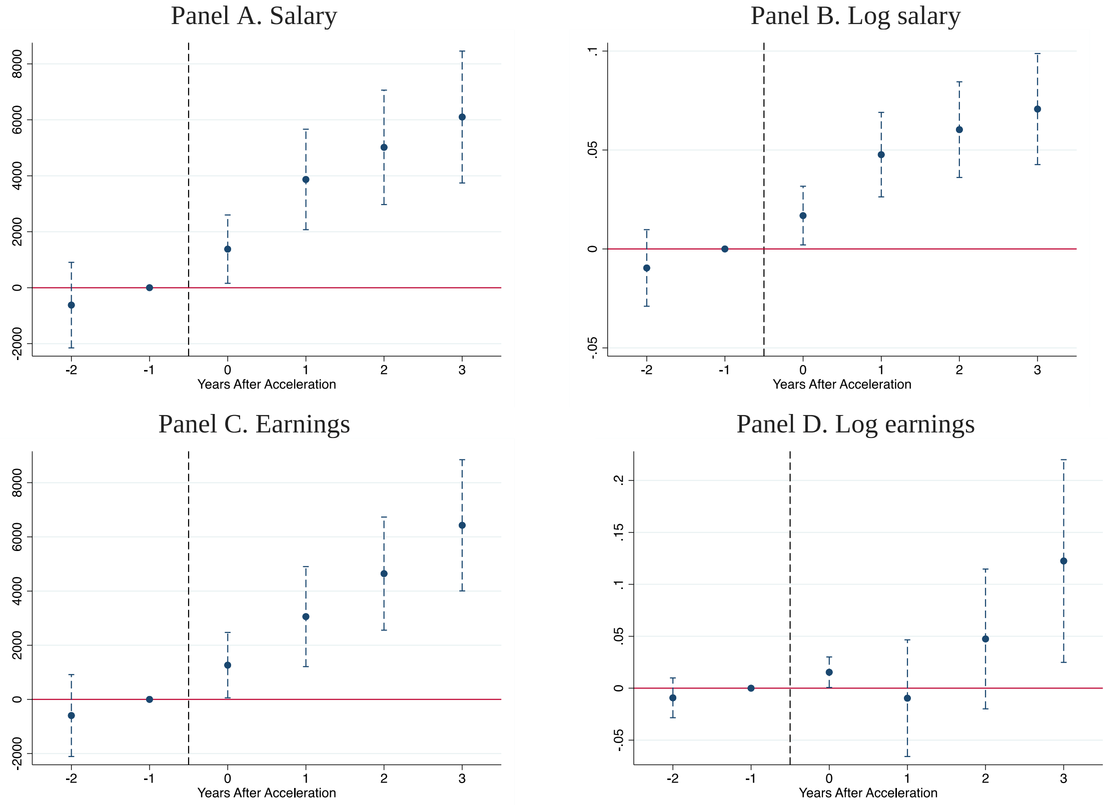


Panel F. Earnings in the years after acceleration



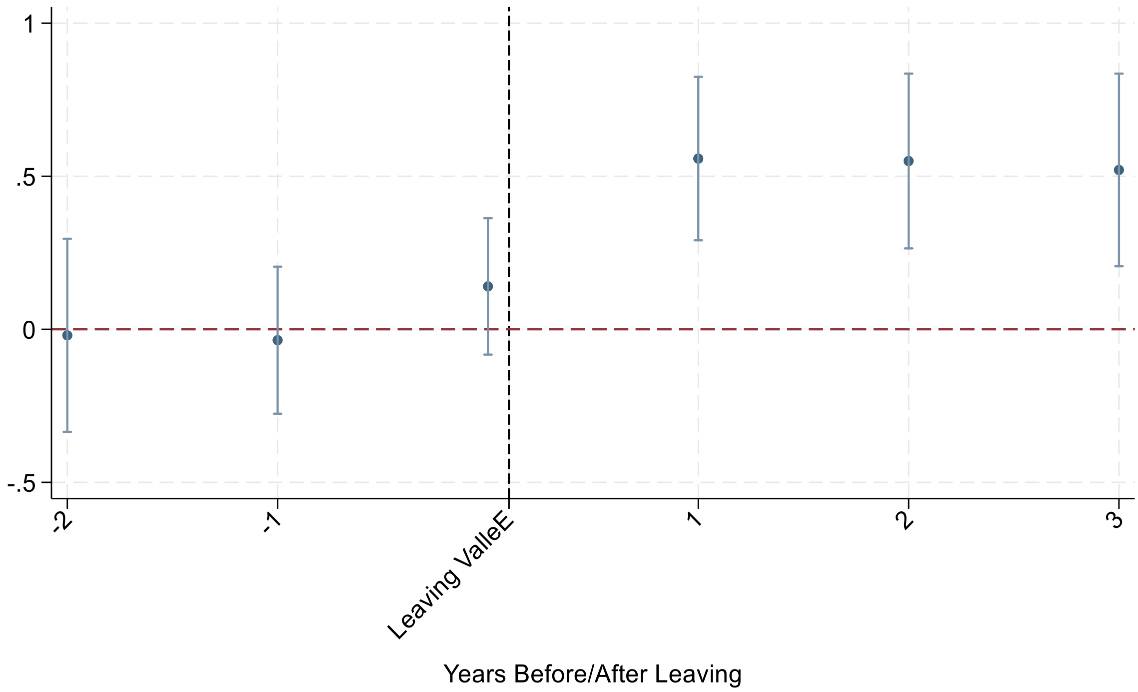
Panel A plots average salaries across positions held before, during and after acceleration. Panel B plots average employability for treatment and control groups around acceleration. Panels C and D plot the cumulative job and company departure rates after acceleration. Panel E plots average (across job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



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

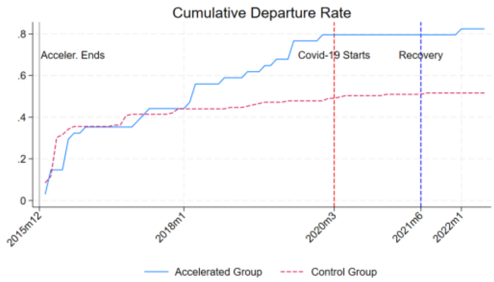
Figure 5. Salaries in Subsequent Job Positions after ValleE Acceleration



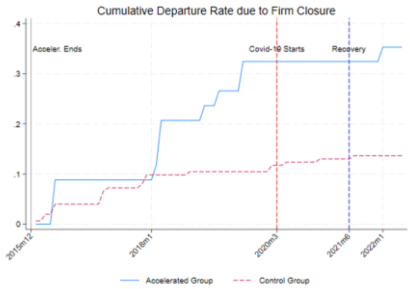
The figure plots the β_h estimates from equation (3). Standard errors are robust.

Figure 6. Departure Rates and Employability after ValleE Acceleration

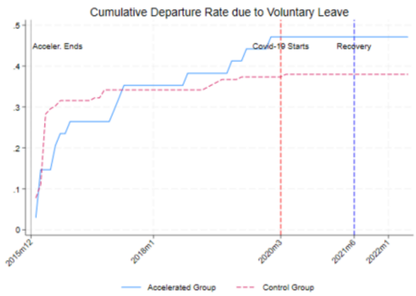
Panel A. Cumulative departure rate



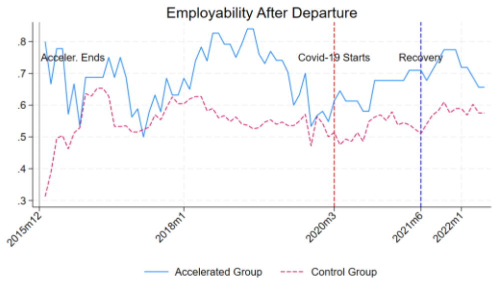
Panel B. Cumulative departure rate due to firm closure



Panel C. Cumulative departure rate not due to firm closure



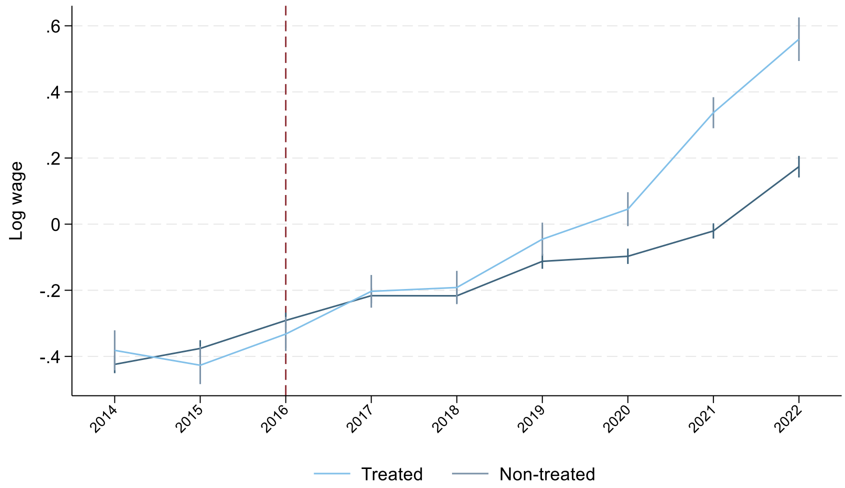
Panel D. Employability after leaving applicant firms



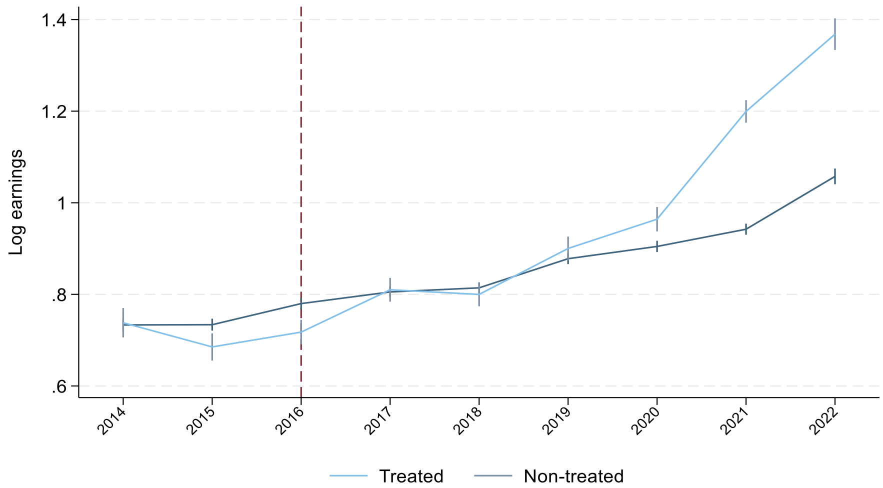
Panels A-C plot cumulative departure rates across employees of ValleE participants (accelerated group) and rejected firms (control group). Panel D depicts the fraction of employees employed after leaving the applicant firms.

Figure 7. Wages and Earnings After ValleE Acceleration

Panel A. Salaries in the years after acceleration



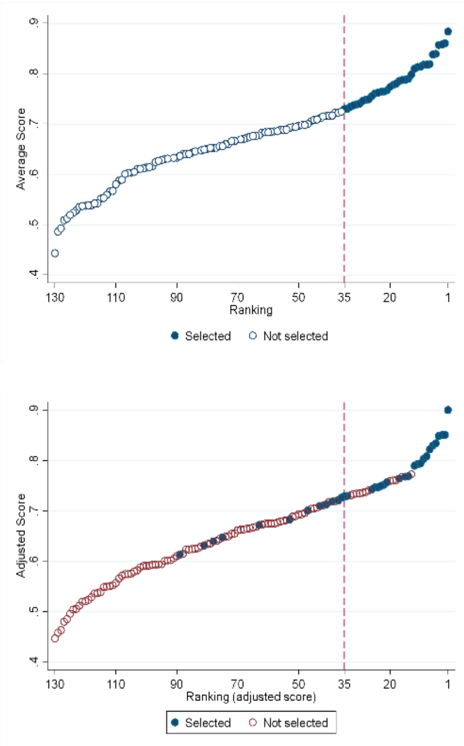
Panel B. Earnings in the years after acceleration



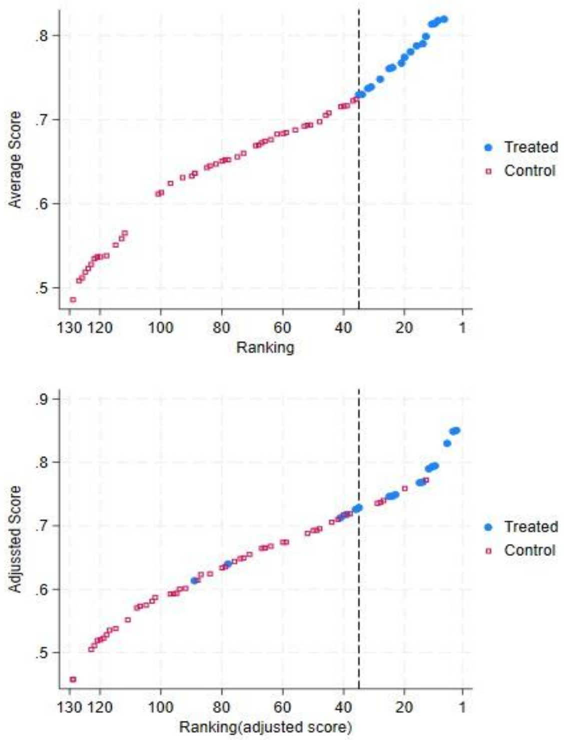
Panel A plots average (across job positions and employees) annual wages for employees of ValleE participants (Treated) and of the rejected applicants (Non-treated).

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

Panel A. ValleE Sample



Panel B. PILA Sample

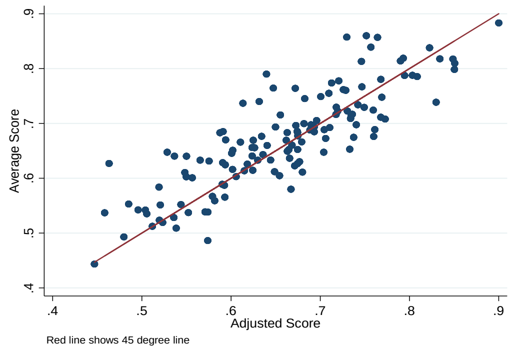


The top figure in each panel plots average scores against rankings based on the average score. The bottom figure in each panel plots adjusted scores against rankings based on the adjusted score, where adjusted scores correspond to the project's fixed effects when scores are regressed against project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

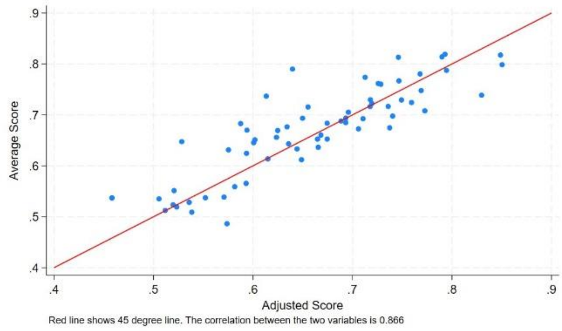
Figure 9. Acceleration Probability and Scoring Generosity

Panel A - Average Scores and Adjusted Scores

ValleE Sample

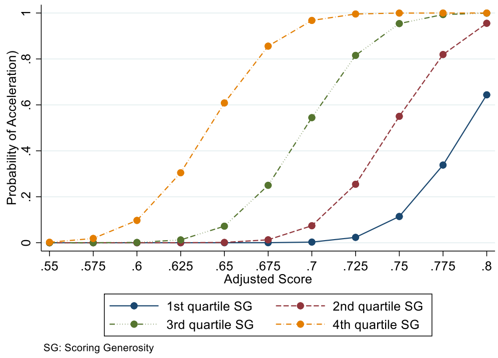


PILA Sample

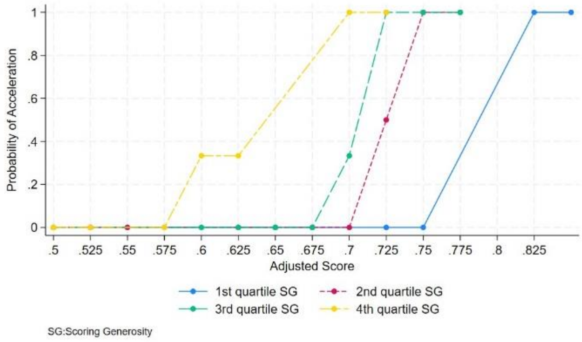


Panel B— Acceleration Probability and Generosity, by Quartiles of Adjusted Score

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

Employee Level – ONET Sample	N	mean	std.	min	P50	P75	Max
Number of Positions	4601	6.304	4.266	1	3	6	8
Number of Companies Worked	4601	5.563	3.704	1	3	5	7
Avg. Number of Positions in Each Company	4601	1.14	0.281	1	1	1	1.2
Total Working Period (months)	4601	265.219	210.408	3	152	219	312
Avg. Tenure in Each Company (months)	4601	64.227	57.319	3	29.3	46.5	77.5
Avg. Term in Each Position (months)	4601	58.855	55.014	3	25.667	41.2	72.25
Number of Followers	4601	599.401	2080.703	0	50	227	651
Number of Connections	4601	483.031	10430.27	0	62	248	500
Have Description (Dummy)	4601	0.5	0.406	0	0	0.5	0.917
Length of Description (# Words)	3212	377.925	313.557	0	151.087	312.938	516.792
Position Change Dummy after Acceleration	4601	0.633	0.482	0	0	1	1
Position Held after Acceleration	4601	1.871	2.064	0	0	1	3
Employer Change Dummy after Acceleration	4601	0.626	0.484	0	0	1	1
Number of Employers after Acceleration	4601	1.805	2.03	0	0	1	3
Employee Level – Full Sample							
Number of Positions	24227	6.708	5.404	1	4	6	9
Number of Companies Worked	24227	5.727	4.567	1	3	5	8
Avg. Number of Positions in Each Company	24227	1.227	0.527	1	1	1	1.25
Total Working Period (months)	24227	300.267	450.14	0	149	223	346
Avg. Tenure in Each Company (months)	24227	78.34	147.151	0	28.6	46.6	83
Avg. Term in Each Position (months)	24227	60.222	63.016	0	24.73	40.6	73
Number of Followers	24227	652.761	1725.286	0	56	269	714
Number of Connections	24227	321.818	4549.28	0	71	288	500
Have Description (Dummy)	24227	0.709	0.454	0	0	1	1
Length of Description (# Words)	17189	347.368	279.467	0	142	289.667	477.667
Position Change Dummy after Acceleration	24227	0.579	0.494	0	0	1	1
Position Held after Acceleration	24227	1.788	2.215	0	0	1	3
Employer Change Dummy after Acceleration	24227	0.572	0.495	0	0	1	1
Number of Employers after Acceleration	24227	1.734	2.182	0	0	1	3

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. There are a total of 4,601 unique LinkedIn profiles in the sample, corresponding to 527 employees of the 207 treated firms and 4,074 employees of the 615 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)	4601	24.6	32.3	1.0	5.0	14.0	28.0	179.0
Gender	4601	0.5	0.5	0.0	0.0	1.0	1.0	1.0
Age	4601	34.0	7.6	25.0	28.0	32.0	38.0	58.0
Education	4601	0.7	0.8	0.0	0.0	0.0	1.0	3.0
Subsequent positions salaries								
First position salary change dummy	4601	0.6	0.5	0.0	0.0	1.0	1.0	1.0
First salary increase dummy	4601	0.4	0.5	0.0	0.0	0.0	1.0	1.0
First salary decrease dummy	4601	0.2	0.4	0.0	0.0	0.0	0.0	1.0
Log first position salary change	4601	0.1	0.2	-0.5	0.0	0.0	0.1	0.7
First position salary change ratio	4601	0.1	0.2	-0.4	0.0	0.0	0.1	1.0
Log all position salary change	4601	0.1	0.2	-0.4	0.0	0.0	0.2	0.7
All position salary change ratio	4601	0.1	0.2	-0.3	0.0	0.0	0.2	1.1
Log last position salary change	4601	0.1	0.2	-0.5	0.0	0.0	0.3	0.9
Last position salary change ratio	4601	0.2	0.3	-0.4	0.0	0.0	0.3	1.5
Log position salary change of 2y	4601	0.0	0.1	-0.4	0.0	0.0	0.0	0.6
Position salary change ratio 2y	4601	0.0	0.2	-0.4	0.0	0.0	0.0	0.8
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

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 the knowledge skills and abilities required for subsequent jobs

Panel A – Skill sub-categories

		Acceleration		Sample Mean	Economic Effect	Treated Group FE	N	R-squared
	Dummy	0.011	(1.39)	0.745		Y	Y	2808
Basic content	Change	0.029***	(3.10)	0.745	3.89***	Y	Y	2808
	Dummy	0.011	(1.39)	0.262		Y	Y	2808
Basic process	Change	0.031***	(3.62)	0.262	11.83***	Y	Y	2808
	Dummy	0.011	(1.43)	0.399		Y	Y	2808
Cross-Functional - Social	Change	0.039*	(1.96)	0.399	9.77*	Y	Y	2808
	Dummy	0.011	(1.39)	0.888		Y	Y	2808
Cross-Functional - Complex Problem Solving	Change	0.003	(0.37)	0.888	0.34	Y	Y	2808
	Dummy	-0.033	(-0.60)	0.003		Y	Y	2808
Cross-Functional - Technical	Change	-0.002**	(-2.22)	0.003	-66.67**	Y	Y	2808
	Dummy	0.013	(1.58)	0.500		Y	Y	2808
Cross-Functional - Systems	Change	0.069***	(3.29)	0.500	13.80***	Y	Y	2808
	Dummy	0.089***	(4.94)	0.038		Y	Y	2808
Cross-Functional - Resource Management	Change	-0.001	(-0.14)	0.038	-2.63	Y	Y	2808

Panel B- Knowledge sub-categories

		Acceleration		Sample Mean	Economic Effect	Treated Group FE	N	R-squared
	Dummy	0.012	-1.58	0.697		Y	Y	2808
Business and management	Change	0.069***	-3.85	0.697	9.90***	Y	Y	2808
	Dummy	0.013	-1.58	0.1		Y	Y	2808
Manufacturing and Production	Change	-0.029***	(-3.74)	0.1	-29.00***	Y	Y	2808
	Dummy	0.011	-1.39	0.276		Y	Y	2808
Engineering and Technology	Change	-0.039***	(-3.12)	0.276	-14.13***	Y	Y	2808
	Dummy	0.011	-1.39	0.296		Y	Y	2808
Mathematics and Science	Change	-0.042***	(-3.96)	0.296	-14.19***	Y	Y	2808
	Dummy	0.025	-1.51	0.086		Y	Y	2808
Health Services	Change	-0.023***	(-2.75)	0.086	-26.74***	Y	Y	2808
	Dummy	0.027	-0.5	0.045		Y	Y	2808
Education and Training	Change	-0.009	(-1.45)	0.045	-20.00	Y	Y	2808
Arts and Humanities	Dummy	0.012	-1.57	0.391		Y	Y	2808

	Change	0.001	-0.14	0.391	0.26	Y	Y	2808
	Dummy	0.012	-1.57	0.154		Y	Y	2808
Law and Public Safety	Change	0.018**	-2	0.154	11.69**	Y	Y	2808
	Dummy	0.011	-1.39	0.12		Y	Y	2808
Communications	Change	0.041***	-7.58	0.12	34.17***	Y	Y	2808
	Dummy	0.015*	-1.78	0.001		Y	Y	2808
Transportation	Change	0.000	-1.48	0.001	0.00	Y	Y	2808

Panel C – Abilities categories

		Acceleration		Sample Mean	Economic Effect	Treated Group FE	N	R-squared
	Dummy	0.011	-1.39	0.744		Y	Y	2808
Cognitive	Change	0.060***	-5.06	0.744	8.06***	Y	Y	2808
	Dummy	0.021**	-2.31	0.05		Y	Y	2808
Sensory	Change	-0.005	(-0.85)	0.05	-10.00	Y	Y	2808
	Dummy	0.053	-1.08	0.006		Y	Y	2808
Physical	Change	0	(-0.11)	0.006	0.00	Y	Y	2808
	Dummy	0.052*	-1.77	0.009		Y	Y	2808
Psychomotor	Change	-0.002	(-1.40)	0.009	-22.22	Y	Y	2808

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: Entrepreneurial job positions

		(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Change score dummy	Score change for next position	Position score change ratio	Position score change avg2	Position score change_all	Position score change ratio_avg2	Position score change ratio_all
TFIDF	Acceleration	0.538***	0.003	0.111*	0.004*	0.004**	0.169***	0.161***
		(37.65)	(1.47)	(1.78)	(1.88)	(2.15)	(2.78)	(3.47)
	Sample mean		0.007		0.007	0.007		
	Economic effect (in %)		42.86		57.14	57.14		
Cosine Similarity	Acceleration	0.100***	0.012***	0.146***	0.013***	0.014***	0.152***	0.163***
		(6.85)	(6.84)	(6.13)	(7.25)	(8.28)	(6.63)	(7.27)
	Sample mean		0.122		0.122	0.122		
	Economic effect (in %)		9.84		10.66	11.48		
	Controls	Y	Y	Y	Y	Y	Y	Y
	Treated Group FE	Y	Y	Y	Y	Y	Y	Y
	N	23499	23499	23499	23499	23499	23499	23499

Panel B: Managerial job positions

		(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Change score dummy	Score change for next position	Position score change ratio	Position score change avg2	Position score change all	Position score change ratio_avg2	Position score change ratio_all
TFIDF	Acceleration	0.518***	0.009***	0.010***	0.009***	0.009***	0.010***	0.010***
		(36.53)	(3.15)	(3.06)	(3.14)	(3.27)	(3.09)	(3.15)
	Sample mean		0.013		0.013	0.013		
	Economic effect (in %)		69.23		69.23	69.23		
Cosine Similarity	Acceleration	0.100***	0.014***	0.159***	0.014***	0.016***	0.166***	0.181***
		(6.85)	(6.72)	(6.24)	(7.13)	(8.27)	(6.73)	(7.55)
	Sample mean		0.126		0.126	0.126		
	Economic effect (in %)		11.11		11.11	12.7		
	Controls	Y	Y	Y	Y	Y	Y	Y
	Treated Group FE	Y	Y	Y	Y	Y	Y	Y
	N	23499	23499	23499	23499	23499	23499	23499

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. Standard errors are clustered at the treated firm group level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5. Business acceleration and wages of subsequent jobs

Panel A – First subsequent position

	(1) First position salary change dummy	(2) First salary increase dummy	(3) First salary decrease dummy	(4) Log first position salary change	(5) First position salary change ratio	(6) Log first position salary change	(7) First position salary change ratio
Acceleration	0.103***	0.194***	-0.091***	0.114***	0.102***	0.161***	0.136***
	(2.65)	(4.77)	(-3.77)	(7.09)	(6.37)	(7.22)	(5.44)
Sample mean (USD)				86782.22	86782.22	84849.52	84849.53
Economic effect (USD)				10479.14	8851.79	14821.94	11539.54
Treated Group FE	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y
N	4502	4502	4502	4502	4502	2808	2808

Panel B – All subsequent positions, last position, and positions within a 2-year window after acceleration

	(1) Log all position salary change	(2) All position salary change ratio	(3) Log last position salary change	(4) Last position salary change ratio	(5) Log position salary change 2y	(6) Position salary change ratio 2y
Acceleration	0.161***	0.199***	0.167***	0.215***	0.104***	0.082***
	(7.22)	(6.81)	(6.70)	(6.06)	(2.73)	(2.61)
Sample mean (USD)	84849.52	84849.53	84849.52	84849.53	84689.2	84689.2
Economic effect (in %)	14821.94	16885.06	15421.76	18242.65	9281.97	6944.51
Treated Group FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
N	2808	2808	2808	2808	1761	1761

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. Panels C and D 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 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 – Long-run employability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Long run employability	Long-run employability after incumbent position	Long-run employability after incumbent company	Future 1 year employability	Future 2 year employability	Future 3 year employability	Future 4 year employability	Future 5 year employability
Acceleration	0.039***	0.090***	0.092***	0.001	0.003	0.012	0.019	0.026**
	(2.88)	(2.78)	(2.81)	(0.04)	(0.24)	(0.87)	(1.41)	(2.00)
Treated Group FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
N	4502	3162	3137	4502	4502	4502	4502	4502

Panel B – Departure rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Change position dummy all	Change position frequency all	Change company dummy all	Change company frequency all	First position transition speed	Avg position transition speed all	First company transition speed	Avg company transition speed all
Acceleration	0.103***	0.499***	0.100**	0.512***	-20.107	-8.847	-6.233	-29.393
	(2.63)	(2.76)	(2.41)	(2.80)	(-0.66)	(-0.23)	(-0.16)	(-0.75)
Treated Group FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
N	4502	4502	4502	4502	2808	2067	2775	1989

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. Panels C and D 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 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 7. Business acceleration and stacked differences-in-differences estimates of changes in salaries and earnings

	(1) Salary	(2) Salary	(3) Log Salary	(4) Log Salary	(5) Earnings	(6) Earnings	(7) Log Earnings	(8) Log Earnings
ATT	4996.607***	4994.519***	0.059***	0.060***	4766.009***	4758.946***	0.053*	0.053*
	(5.12)	(5.11)	(5.06)	(5.05)	(4.88)	(4.87)	(1.73)	(1.73)
Cluster (TreatmentGroup)	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	Y	N	Y	N	Y	N	Y
Fixed Effects (TreatmentGroup, AcceleYear)	Y	Y	Y	Y	Y	Y	Y	Y
N	27915	27915	27915	27915	28479	28479	28479	28479

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 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 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
ln(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
ln(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
ln(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
ln(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
ln(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
ln(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. Transition Speed, Formal Employment Likelihood and Long-Term Employability after ValleE

	(1) Transition time (months)	(2) No formal Employment	(3) Long-term Employability
Acceleration	-0.588	-0.156***	0.176***
	(1.633)	(0.0581)	(0.0636)
Constant	3.943	-0.130	0.710***
	(3.232)	(0.0927)	(0.100)
Controls	Y	Y	Y
Observations	162	195	193
R-squared	0.046	0.378	0.130

The table shows results from simple regressions comparing post acceleration outcomes between incumbent employees of ValelE participants and rejected applicants. In column 1, the outcome variable corresponds to the number of months the employee does not have a formal job (is not in the PILA sample) after leaving the ValleE firm. In column 2, the outcome variable is a dummy indicating the employees that never return to formal employment after leaving the ValleE firm. Finally, Standard errors are heteroskedasticity robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 11. Wages and Earnings After ValleE Acceleration

	(1) Log Wage	(2) Log Wage	(3) Log Wage	(4) Log Wage	(4) Log Earnings	(5) Log Earnings	(6) Log Earnings
	OLS	OLS	OLS	IV	OLS	OLS	IV
Treatment effect		0.0474		0.126**		-0.00570	0.204***
		(0.0303)		(0.0501)		(0.0147)	(0.0250)
Treatment effect * 2016	-0.0836*				-0.0465*		
	(0.0427)				(0.0254)		
Treatment effect * 2017	-0.128***				-0.0514*		
	(0.0468)				(0.0276)		
Treatment effect * 2018	-0.0913*				-0.0375		
	(0.0502)				(0.0260)		
Treatment effect * 2019	0.0132				0.000678		
	(0.0506)				(0.0249)		
Treatment effect * 2020	0.212***				0.0774***		
	(0.0502)				(0.0274)		
Treatment effect * 2021	0.279***				0.135***		
	(0.0431)				(0.0302)		
Treatment effect * 2022	0.177***				0.112**		
	(0.0609)				(0.0461)		
Treatment effect: At accelerated firm			-0.0253				
			(0.0514)				
Treatment effect: After accelerated firm			0.0859**				
			(0.0361)				
Sample	All job spells	All job spells	All job spells	All job spells	Balanced panel	Balanced panel	Balanced panel
Controls	Y	Y	Y	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y	Y	Y	Y
Observations	49,021	49,021	48,514	49,021	78,749	78,749	78,749
R-squared	0.399	0.401	0.408	0.012	0.600	0.550	0.244

The table shows results estimating equation (4). Standard errors are heteroskedasticity robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 12. Wages and Earnings After ValleE Acceleration

	(1)	(2)	(3)
		After leaving ValleE firm	
	Employed at knowledge intensive sector	Share of months at knowledge intensive sector	Being at top of wage distribution
Acceleration	0.171*	0.125**	0.178**
	(0.0906)	(0.0581)	(0.0868)
Constant	-0.181	-0.302**	0.644**
	(0.277)	(0.145)	(0.283)
Controls	Y	Y	Y
Observations	195	195	195
R-squared	0.065	0.102	0.115

The table shows results from simple regressions comparing post acceleration outcomes between incumbent employees of ValleE participants and rejected applicants. In column 1, the outcome variable corresponds to an indicator variable for having at least one job after acceleration in a company in a knowledge intensive sector. In column 2, the outcome variable is the share of months employed in a company of a knowledge intensive sector after leaving the ValleE firm. The outcome variable in column 3 indicates earning wages at the top of the wage distribution. Standard errors are heteroskedasticity robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 13. ValleE Selection and Scoring Generosity

	(1)	(2)	(3)	(4)
	Firm Level		Employee Level	
	ValleE sample	PILA sample	All job spells	Balanced panel
	Acceleration	Acceleration	Acceleration×Post	Acceleration×Post
SG	3.002***	2.638***		
	(0.43)	(0.66)		
SG*Post			5.202***	5.232***
			(0.122)	(0.094)
N	135	66	49,021	78,441
R-sq	0.529	0.492	0.45	0.45
F-test excluded instruments	49.74	15.98	575.40	803.63

The table shows results from the first stage regressions. Columns (1)-(2) regress Acceleration against SG, the scoring generosity of the applicants' judges. Columns (3)-(4) report results from equation 5. Post is a variable that equals one after 2015. All regressions include employee fixed effects, month fixed effects and employee controls. Al regressions include the corresponding level effects. We report only the interaction with SG to conserve space. Standard errors are 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
consultant	Consultants often run their own business or independent services, indicating entrepreneurship
innovator	Innovators bring new ideas to the market, a core entrepreneurial trait
contractor	Contractors operate independently, similar to entrepreneurs managing their own business
leader	Leaders inspire and drive business success, integral to entrepreneurial roles
director	Directors oversee operations and decisions, typical in entrepreneurial ventures
visionary	Visionaries have forward-thinking business ideas, key to entrepreneurship
investor	Investors fund and often start their own ventures, aligning with entrepreneurship
strategist	Strategists plan and execute business ventures, essential for entrepreneurship
executive	Executives manage and grow companies, an important entrepreneurial role
proprietor	Proprietors own their business, directly reflecting entrepreneurial activity
capitalist	Capitalists invest in and may start businesses, involving entrepreneurial decisions
developer	Developers create and innovate in business, linked to entrepreneurship
advisor	Advisors often provide entrepreneurial insights and run their own businesses
manager	Managers handle business operations, important for entrepreneurial success
owner	Owners of businesses are entrepreneurs by definition
consultant	Consultants run independent businesses, similar to entrepreneurship
chief	Chiefs hold leadership roles in companies, related to entrepreneurship
officer	Officers make decisions in companies, important in entrepreneurial contexts
organizer	Organizers coordinate business activities, a key entrepreneurial function
administrator	Administrators handle business operations, often in entrepreneurial settings
architect	Architects build business frameworks, aligned with entrepreneurship
builder	Builders construct new business opportunities, essential to entrepreneurship
producer	Producers create and manage business outputs, tied to entrepreneurial efforts
operator	Operators run businesses or ventures, a core entrepreneurial role
planner	Planners strategize business success, crucial for entrepreneurs
business owner	Business owners manage their own ventures, defining entrepreneurship
startup founder	Startup founders create new ventures, inherently entrepreneurial
self-employed	Self-employed individuals manage their own business, showing entrepreneurial traits

small business owner	Small business owners run their own business, directly representing entrepreneurs
company founder	Company founders create businesses, the essence of entrepreneurship
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
coordinate	Coordinating tasks, teams, or resources is a typical management function
plan	Managers are responsible for planning strategies and processes
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
run	Running day-to-day operations signifies management experience
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
manage	Managing resources, teams, or projects reflects managerial expertise
execute	Executing plans and strategies is a key function of a manager
decide	Making decisions is essential to managerial roles
delegate	Delegating tasks to others is a fundamental management skill
steer	Steering teams or projects toward success reflects management experience
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
conduct	Conducting meetings or evaluations is part of management roles
instruct	Instructing employees or teams shows management leadership

evaluate	Evaluating performance and strategies is common for managers
balance	Balancing resources, time, and priorities reflects management skill
maintain	Maintaining standards, operations, or teams is a management responsibility
improve	Improving processes or outcomes is a key management duty
optimize	Optimizing efficiency and productivity is a typical management task
prioritize	Prioritizing tasks and resources is essential in management roles
adjust	Adjusting strategies and plans is required in management positions
direct	Directing operations or people is a central role for managers
strategize	Strategizing for business or projects is part of management duties
mitigate	Mitigating risks or problems is key for managers
mobilize	Mobilizing teams or resources reflects a management role
empower	Empowering employees or teams is a management function
implement	Implementing plans and strategies is a key task for managers
control	Controlling operations or processes reflects management responsibilities
handle	Handling tasks or teams is central to management
oversee	Overseeing projects or teams indicates a managerial position
accomplish	Accomplishing goals or objectives reflects management experience
streamline	Streamlining processes or operations is a key management task
navigate	Navigating challenges and tasks is common for managers
troubleshoot	Troubleshooting problems is a key management skill
assess	Assessing performance or risks is a responsibility for managers
adapt	Adapting strategies and solutions is often needed in management
enforce	Enforcing policies or procedures is common for managers
balance	Balancing priorities, resources, or teams reflects management abilities
manage resources	Managing resources shows a direct management responsibility
project management	Managing projects is a key management role
stakeholder management	Managing stakeholders reflects management responsibilities
risk management	Managing risks is essential for business managers
financial management	Managing finances is crucial for financial or business managers
crisis management	Managing crises reflects management experience
conflict management	Managing conflicts is a common managerial responsibility
manage expectations	Managing expectations is key for successful management
operations management	Managing operations is a key management task
stress management	Managing stress and supporting teams is important in management
manage relationships	Managing relationships reflects leadership and managerial skills

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