# Business Acceleration Program as City Sowers: Assessing Public Funding's Impact on New Ventures in an Emerging Economy

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**Abstract**

**Purpose –** A debate on whether new ventures should be supported with public funding is taking place. Adopting a position on this discussion requires rigorous assessments of implemented programs. However, the few existing efforts have mostly focused on regional cases in developed countries. To fill this gap, our paper seeks to measure the effects of a regional acceleration program in a developing country (Medellin, Colombia).

**Design/methodology/approach –** The economic notion of capabilities is utilized to frame the analysis of firm characteristics and productivity, which are hypothesized to be heterogeneous within the program. To test these relationships, propensity score matching is utilized in a sample of 60 treatment and 16,994 control firms.

**Findings –** We find that treated firms had higher revenue than propensity-score-matched controls on average, confirming a positive impact on growth measures. However, such financial growth is mostly observed in service firms rather than other economic sectors.

**Originality –** These findings tip the balance in favour of the literature suggesting supportive programs for high-growth firms as opposed to everyday entrepreneurship. This is an insight, especially under the context of an emerging economy, which has scarce funding to support entrepreneurship.

**Research limitations/implications –** Further evaluations, with a longer period and using more outcome variables, are suggested in the context of similar publicly funded programs in developing countries.

**Keywords:** Entrepreneurship; impact evaluation; public funds; acceleration programs; developing countries.

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## 1. Introduction

Entrepreneurship policy is a matter of concern for governmental policies due to the importance of entrepreneurial activity for creating economic growth, employment opportunities, and total factor productivity (Cho and Honorati, 2014; Urbano and Aparicio, 2016). According to Audretsch *et al.* (2020), these policy initiatives for incentivizing entrepreneurship often mean programs, schemes, or plans implemented to support the establishment or development of entrepreneurial activity, with a special focus on mitigating any constraints faced by entrepreneurs in the initial stages of the venture creation.

Some literature has suggested that the support should apply to all kinds of entrepreneurial initiatives as inclusive programs (e.g., acceleration) would help regions in one way or another (Welter *et al.*, 2017). However, when it comes to public funding, the debate on whether some ventures should be supported and not others is still open (Acs *et al.*, 2016). For example, although entrepreneurial activity is desirable in an economy, entrepreneurship policy is sometimes related to undesired outcomes (Mandelman and Montes-Rojas, 2009). Encouraging the creation of average startups can result in low-productivity firms that create few jobs and little wealth (Shane, 2009). This introduces different challenges when implementing a policy. On the one hand, policymakers should focus on detecting and fostering high-growth and innovative startups with high survival rates (Colombelli *et al.*, 2016; Lall *et al.*, 2020), which are prone to generate creative destruction (Clarysse et al., 2015). On the other hand, this process of selecting high-growth startups is difficult to carry out for two main reasons. First, there is a well-known scarcity of these kinds of companies in developing economies as compared with developed countries (Charoontham and Amornpetchkul, 2024; Eslava *et al.*, 2019; Puente *et al.*, 2019). Second, even under optimal conditions, the government probably can't make decisions regarding variables that even venture capital professionals find difficult to predict (Coad *et al.*, 2014; Kantis *et al.*, 2020).

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Given entrepreneurship's growing popularity, its importance, and the (public) budget devoted to supporting entrepreneurship policies, it is relevant to measure the effects of accelerator programs on the ventures that are accepted to participate in these programs. From a taxpayer point of view, it is relevant to know whether funding an acceleration program - intended to allocate entrepreneurial resources efficiently, alleviate the constraints on growth, and build startups' dynamic capabilities- is a good use of public money.

In this scenario, and as part of an entrepreneurship ecosystem (Wurth *et al.*, 2021), public–private partnership enables private business accelerators to manage public funding and develop an important role in entrepreneurship policy. These accelerators focus on stimulating startup activity in their focal region, selecting high-growth firms, and alleviating the constraints on their growth using different strategies, such as working space, money, mentoring, and/or guidance (Clarysse *et al.*, 2015).

Since the inception of the first accelerator program more than 15 years ago, with the origin of Y Combinator (Bone *et al.*, 2017), numerous similar programs have appeared worldwide, being established and financed by a variety of organizations. According to Crișan *et al.* (2021), business accelerators, in contrast to other popular programs such as incubators, are shorter in the length of the program, are more focused on encouraging business development, and usually aim to attract companies in a startup phase (in comparison to incubators that are mainly focused on idea stage).

Given its appeal and the widespread adoption of these programs worldwide, several impact evaluations have been done to understand the effects in treated firms. Studies have identified positive effects on capital raised and firm survival (Gonzalez-Uribe and Leatherbee, 2017; Hallen *et al.*, 2020; Venâncio and Jorge, 2021), employment and valuation (Gonzalez-Uribe and Leatherbee, 2017), and revenue (Autio and Rannikko, 2016; González-Uribe and Reyes, 2020). The evaluations by Gonzalez-Uribe *et al.* (2017, 2020) provided a relevant benchmark for our research, given the similar settings in terms of both geographical context (i.e., a developing country) and business accelerator archetype context (i.e., ecosystem accelerators). Furthermore, Polo García-Ochoa (2020) concludes that certain business acceleration programs, depending on their design and methodology, effectively enhance startups' dynamic capabilities, although not all dynamic capabilities equally influence the performance of startups.

However, there is a gap in this literature as only a few studies exist that have evaluated the impact of business accelerators. To some extent, this limitation can be explained by the different settings of each program depending on the location and the ability to provide startup companies with high-quality guidance (Bone *et al.*, 2017; Fairlie, 2021; Kantis *et al.*, 2021). These specific settings can affect the results either positively or negatively on the regional level. Another important shortcoming is that fewer studies on impact evaluation have devoted attention to supportive programs in developing countries (Kantis *et al.*, 2020; Srhoj, Lapinski, *et al.*, 2021), which is necessary for promoting the discussion around the topic as well as achieving better public policy design.

Some other studies have used the same quasi-experimental settings, particularly propensity score matching, to evaluate different entrepreneurship programs executed with public funds. Regarding revenue, three of these studies have found positive effects of the program (Autio and Rannikko, 2016; Fairlie, 2021; Nyikos *et al.*, 2020; Oh *et al.*, 2009). The remaining studies have reported either negative or non-significant effects (Dvouletý *et al.*, 2018; Efobi and Orkoh, 2018; Norrman and Bager-Sjögren, 2011; Srhoj, Lapinski, *et al.*, 2021; Srhoj, Škrinjarić, *et al.*, 2021). Being aware of the limitations in the existing literature, this research aims to answer the following question: Does a publicly funded accelerator program really help new ventures to grow?

Hence, this paper seeks to measure the effects of a local acceleration program from Medellin, Colombia. The acceleration program is operated by Créame, which is a private, nonprofit institution founded in 1996 in Medellin by several public and private partners that were seeking an effective way to promote entrepreneurial activity in a city devastated by violence, poverty, and social exclusion. The program operates as a traditional accelerator with a sponsorship scheme provided by the Mayor's Office of Medellin and is focused solely on high-impact ventures, which are chosen in each acceleration cohort using a highly selective approach that results in a score above or below the cutoff to enrol in the program. This approach takes into consideration several variables, such as the business sector, sales, company age, and technological processes used for companies' operations. The program is 24 weeks long and has different modules, with a special focus on financial training and assessment.

To answer our research question, we use the economic notion of dynamic capabilities related to firms (Sutton, 2012; Teece, 2018). Within this framework, we empirically analyze a sample of 60 treatment and 16,994 control ventures, which come from Créame's database as well as the Chamber of Commerce of Medellin. Using propensity score matching, we find that the acceleration program has helped the treated firms to achieve better financial growth than ventures with similar characteristics that were operating outside of the program.

Based on our findings, we provide three contributions to the literature. First, we acknowledge the prior efforts in empirically evaluating entrepreneurship-supportive programs (for a thorough review, see Dvouletý *et al.*, 2021). However, entrepreneurship research still needs evidence on the real benefits (or losses) stemming from initiatives funded with public budgets. Our study sheds some light in that direction. Second, entrepreneurship policies have opened a debate amongst scholars in which the selection criteria have been reduced to characteristics such as innovation and technological basis, which ultimately result in high-growth entrepreneurship (cf. Shane, 2009), versus everyday entrepreneurship (cf. Welter *et al.*,

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2017). We suggest that picking "winners" or "losers" before the treatment would necessarily lead to positive results. Finally, we support policymakers by emphasizing the selection criteria in a region such as Medellin, which is part of a developing country. This entails a discussion about the quality of policy design, including implementation and assessment, with clear information for impact evaluation.

After this introduction, the paper continues as follows. Section 2 offers the contextual and theoretical background. Section 3 explains the data and empirical strategy. Section 4 describes the main findings, whereas Section 5 highlights the study's contributions, as well as limitations and future research lines. Finally, Section 6 concludes the study.

## 2. Setting of the study and conceptual framework

### 2.1. Medellin as an entrepreneurial region in Colombia

According to Baumol (1990), Medellin has shown different entrepreneurial dynamics. A long tradition of productive entrepreneurship has existed in the region thanks to the creation and expansion of firms in industries such as food, concrete, insurance, banking, and public services (Álvarez Morales, 2005; Mejía, 2011). However, the city has also faced different social issues derived from illegal activities and a lack of institutional stability (Ciro *et al.*, 2024). As a result, Medellin has suffered the destruction of capital at the firm level and poverty, violence, and inequality at the societal level. After fighting against drug dealers and guerrillas, Medellin has been able to rebuild the city and achieve social inclusion.

Entrepreneurship and innovation have been key in this process. Since the mid-2000s, Medellin has been well known in the Latin American spectrum for making public–private partnerships and investments to foster innovation and high-impact entrepreneurship, taking advantage of its dynamic ecosystem (Alcaldía de Medellín and Créame Incubadora de Empresas, 2018). In 2018, Medellin was the city with the highest research and development

(R&D) budget in Colombia, measured as a percentage of the GDP (Observatorio Colombiano de Ciencia y Tecnología, 2020).

Some of the policies applied in the city have focused on tackling issues such as informality, unemployment, territorial competitiveness, and productive development. Responding to the interesting ecosystem development throughout the 21st century, a complete economic development public policy was implemented in 2017, with a special core of solving the problem of the high company mortality rate and the low birth rate of innovative companies (García and Jaramillo, 2018). This policy was focused on grouping and coordinating some key ecosystem private actors that were initially functioning at a distance from each other, such as the Chamber of Commerce.

Thanks to this and previous dynamics, different private and public organizations have joined the ecosystem. For example, programs such as "Parque E," the first university incubator in the city, was born from an alliance between the Mayor's Office of Medellin and the University of Antioquia, the most important public university in the region. Other relevant entrepreneurship programs in the city include "Cedezo" (with a special focus on everyday entrepreneurship), "Seed Capital" (public grants for high-impact entrepreneurship), and the accelerator program, funded by the Mayor's Office and executed by Créame (Alcaldía de Medellín and Créame Incubadora de Empresas, 2018).

In general, the city has shown positive numbers in terms of entrepreneurial activity as it is one of the Colombian cities with the highest level of entrepreneurship (Gómez *et al.*, 2011). Despite all these initiatives, the city has not estimated the net benefits or losses coming from (accelerator) programs as part of public policy. It is thus important to evaluate the first and only business acceleration program that is offered publicly with the sponsorship of the Mayor's Office. It is also crucial to discover the role that the acceleration program has played in recent years, both in the city's ecosystem and in the companies that have passed through the program.

Although the same environment surrounds these companies, firm characteristics and capabilities create heterogeneity, making the selection of those ventures that can benefit from acceleration programs difficult. This is one of the major issues when it comes to entrepreneurship policies due to the possibility of wasting public funds (Acs *et al.*, 2016).

In the context of developing countries, the case of Medellín is crucial to understand if the public policy efforts to foster entrepreneurship are producing the desired outcomes; hence, the case of Medellín is relevant to other developing countries to understand if publicly-funded acceleration programs can enhance startups' dynamic capabilities and through which specific mechanisms, improving industrial growth policy replicability and formulation (Efobi and Orkoh, 2018). Likewise, this study also demonstrates the limits of business acceleration programs focused on building dynamic capabilities as public policy instruments, especially when considering the gaps existing for effectively generating high-growth dynamics within a firm.

**2.2. Conceptual framework**

To see the effect of public-funded programs on ventures' growth, this research utilizes the economic notion of capabilities related to firms (Sutton, 2012), which considers a set of elements, such as the know-how or working practices of the group of individuals inside companies, that help firms to achieve higher productivity. Sutton (2012, pp. 11–12) recognized that the concept of capabilities has attracted wide attention among management scholars thanks to theoretical developments such as the resource-based view (Barney, 1991), the knowledge-based view (Grant, 1996), and dynamic capabilities (Teece *et al.*, 1997), which focus on deeper attributes as inputs for firm growth.

These dynamic capabilities enable firms to create intangible assets that carry superior business results in the long term, and as such, firms with strong dynamic capabilities are idiosyncratically entrepreneurial (Teece, 2007). Therefore, firm capabilities and more specifically, dynamic capabilities are relevant for explaining productivity differences among firms (Syverson, 2011). Accordingly, firm capabilities constitute resources that could be misallocated, hence reducing intra-firm productivity and total factor productivity (Hopenhayn, 2014; Hsieh and Klenow, 2009).

As Butler *et al.* (2016) stated, the misallocation of entrepreneurial resources (i.e., when the entrepreneurial talent is not being fully exploited in firms) and economic resources, in general, could account for most productivity differences across countries (Eslava *et al.*, 2019). One of the main consequences of this misallocation of resources is the creation of firms with low productivity that do not account for the majority of job creation or aggregate wealth production.

This misallocation of entrepreneurial capacity is deeply related to the occupational decisions faced by entrepreneurs: subsistence entrepreneurship (or self-employment) exists as a direct alternative to wage employment with fixed salaries, particularly in developing economies (Cho *et al.*, 2016; Shane, 2009). In developed countries, where wages and wealth are higher on average, the opportunity costs of starting a company are higher (Cho and Honorati, 2014). This occupational decision is also constrained by several variables that must be taken into consideration, such as a lack of managerial skills, which can reduce the expected profits and survival probability of any new venture (Bloom *et al.*, 2011; Mano *et al.*, 2012).

For entrepreneurship policy design and accelerator programs focusing on high-growth ventures, it is fundamental to create highly selective processes to pick the companies that have the greatest potential to achieve an economic impact (productivity, employment, and economic growth) (Autio and Rannikko, 2016). After completing this stage, the main challenge remaining is to make efforts to alleviate the constraints on the building of dynamic capabilities (González-Uribe and Reyes, 2020) for which the management team plays a key role, therefore allowing differentiation across firms (Teece, 2018). Furthermore, Polo García-Ochoa (2020) concludes that certain business acceleration programs, depending on their design and methodology, effectively enhance startups' dynamic capabilities, although not all dynamic capabilities equally influence the performance of startups. A literature analysis can help us understand the mechanisms behind the relationship between supportive programs, and dynamic capabilities that enhance startups' performance.

There is a growing literature that aims to explore the possible outcomes of using public resources to encourage new venture creation, firm growth, and desirable results. In this sense, several authors have stated that fostering entrepreneurial activity is not a great way to use taxpayers' money unless the public policy is focused on high-growth and high-impact ventures with huge potential for job creation (Åstebro, 2017; Shane, 2009). Different authors have emphasized the role of public policy in solving a clearly identified market failure related to constraints and firm capabilities (Acs *et al.*, 2016; Åstebro, 2017; Lerner, 2012; McKenzie and Woodruff, 2014; Shane, 2009).

Business accelerators are now important figures worldwide focusing on alleviating firms' growth constraints, providing services through a highly selective, cohort-based program of limited duration (Clarysse *et al.*, 2015; Shankar and Shepherd, 2019). The first accelerator program can be traced back to 2005 with the origin of Y Combinator, which focused on digital startups and was funded mainly by venture capitalists based in San Francisco (Bone *et al.*, 2017). Since the inception of the first accelerator program 15 years ago, numerous similar programs have appeared worldwide, being established and financed by a variety of organizations. According to Crișan *et al.* (2021), business accelerators, in contrast to other popular programs such as incubators, are shorter in the length of the program, are more focused on encouraging business development, and usually aim to attract companies in a startup phase (in comparison to incubators that are mainly focused on idea stage).

Despite the differences between programs and settings specific to countries or sectors, there are four main mechanisms by which accelerators support entrepreneurship and innovation (Crișan *et al.*, 2021), namely i) the validation of ideas and products, ii) the provision of product development, iii) support to increase market access and growth, and iv) support for innovation. Crișan *et al.* (2021) concluded that acceleration platforms play a key role in developing an entrepreneurial climate and are perceived as vehicles that could revitalize industries and regions.

According to Clarysse *et al.* (2015), there are three accelerator archetypes based on their strategic focus: first, the investor-led accelerator, for companies seeking to maximize their venture capital investment attractiveness; second, the matchmaker accelerator, set up by corporations interested in providing stakeholders with services; and, third, the ecosystem accelerator, with government agencies as the main stakeholder and the objective of stimulating startup activity in a certain ecosystem. Moreover, this ecosystem accelerator archetype is the one in which is possible to find the conjunction between accelerator programs and entrepreneurship policy.

Given the importance of a well-justified entrepreneurship policy, there is also a limited but growing body of literature evaluating and measuring the effects of singular entrepreneurship programs all around the world. Although there are important results to consider, studies are highly dependent on local specific patterns, related to both firm dynamics and ecosystem dynamics (Kantis *et al.*, 2021; Venâncio and Jorge, 2021). For example, regarding public grants for SMEs, Cowling and Dvouletý (2023) and Dvouletý *et al.* (2021) found positive effects of grants on firm survival, employment, and tangible assets as well as mixed results about labor productivity and total factor productivity.

In the impact evaluation literature specifically focusing on entrepreneurship policy, it is possible to find that the most common outcome variables used in the empirical models are profits and/or revenue, number of employers, and survival (Dvouletý *et al.*, 2021; Kantis *et al.*, 2021). However, Audretsch *et al.* (2020) found that the results of these (and other) variables are highly skewed and that only a small group of outlier firms is driving the contributions in most evaluated policies.

It is relevant for public policy interventions to identify clearly the ventures that are moving the impact needle, using highly selective mechanisms to exclude the firms with a lower probability of success (Autio and Rannikko, 2016). One of these mechanisms to find high-impact, high-growth ventures is the propensity for process and product innovation of a given firm (Colombelli *et al.*, 2016).

In the high-impact entrepreneurship policy scenario, two impact evaluation studies have shown positive results for employment and firm survival but mixed or inconclusive results concerning sales and revenue (Butler *et al.*, 2016; Giraudo *et al.*, 2019). Nonetheless, McKenzie (2017) found positive results for employment, survival probability, sales, and profitability using similar settings that included business training and government grants.

Regarding more specific public policy interventions focusing solely on business training, it is possible to identify different results. Bruhn *et al.* (2013) found positive effects on return on assets, total factor productivity, and employment in SMEs. For business training in survival entrepreneurship settings, de Mel *et al.* (2014) identified significant impacts on profitability. Lastly, for business training focused on social entrepreneurship, Åstebro and Hoos (2021) reported positive significant effects on entrepreneurial activities up to three years after the intervention.

For impact evaluations of business accelerator programs, studies have identified positive effects on capital raised and firm survival (Gonzalez-Uribe and Leatherbee, 2017; Hallen *et al.*, 2020; Venâncio and Jorge, 2021), employment and valuation (Gonzalez-Uribe and Leatherbee, 2017), and revenue (Autio and Rannikko, 2016; González-Uribe and Reyes, 2020). The evaluations by Gonzalez-Uribe *et al.* (2017, 2020) provided a relevant benchmark for our research, given the similar settings in terms of both geographical context (i.e., a developing country) and business accelerator archetype context (i.e., ecosystem accelerators).

Some studies have used matching methods to evaluate different entrepreneurship programs executed with public funds. Due to the variety of programs, each study has focused on particular outcome variables, sales or revenue being common to all matching studies. Regarding sales, three of these studies have found positive effects of the program (Autio and Rannikko, 2016; Fairlie, 2021; Nyikos *et al.*, 2020; Oh *et al.*, 2009). The remaining studies have reported either negative or non-significant effects (Dvouletý *et al.*, 2018; Efobi and Orkoh, 2018; Norrman and Bager-Sjögren, 2011; Srhoj, Lapinski, *et al.*, 2021; Srhoj, Škrinjarić, *et al.*, 2021). For a summary of the literature on the impact evaluation of public entrepreneurship programs, see Table 1.

***-- Table 1 here --***

Finally, from a regional perspective, it is important to point out that entrepreneurial activity in Colombia depends on survival entrepreneurship. According to Bosma *et al.* (2020), 90% of the total early-stage entrepreneurial activity (TEA) in Colombia is carried out by people looking for a living instead of exploiting a market opportunity with high-growth potential. This fact alone reinforces our initial question about the capacity of any organization, managing public funds, to pick high-impact entrepreneurs efficiently and help them grow. Although the fraction of the TEA that belongs to innovative entrepreneurship is low, Aparicio, Urbano, and Gómez (2016) found that this type of entrepreneurship could contribute to sustainable economic growth for Colombia, when projected by 2032, and even reach a growth peak of 6.7%.

To reach that peak of economic activity, government decisions, such as ceasing to promote the indiscriminate creation of low-productive firms, should be considered (Consejo Privado de Competitividad, 2020). Any high-impact entrepreneurship program in Colombia, whether regional or national, should have a design that prioritizes the evaluation of results so that evaluators can determine the effectiveness of the executed public policy (Consejo Privado de Competitividad, 2020). This is one of the largest gaps in Colombian entrepreneurship studies. There is only one previous well-known impact evaluation of a business accelerator in Cali, Colombia, led by González-Uribe and Reyes (2020), which highlighted the need to conduct an impact evaluation of entrepreneurship policy. Responding to this call, as well as the divergent results of entrepreneurship-supportive programs' evaluation, we ask whether the acceleration initiative implemented in Medellin has been helpful in terms of firm growth. The next section explains the methodology that we utilized for the impact evaluation.

## 3. Methodology

### 3.1. The main method used to assess the acceleration program

For this research, the matching methodology was adopted to measure the effects of the acceleration program. According to Rosenbaum and Rubin (1983), the choice of the right measure of a program's effect relies on the construction of a proper counterfactual (i.e., the results obtained in the treatment group in the absence of the program). Therefore, it is not possible to estimate the impact of the acceleration program just by comparing the group of supported firms and the group of non-supported firms since their previous characteristics were already different. Thus, an appropriate impact measure of a program in a non-experimental setting would be to compare the performances of two firms with the same previous characteristics, assuming that one of them participated in the acceleration program and the other did not. The problem lies in finding a comparison group (i.e., a control group) that holds these similar characteristics.

This research uses matching techniques (Rosenbaum and Rubin, 1983), which facilitate the construction of a control group by matching similar firms based on the propensity score of firms that were not supported by the program. Matching techniques estimate a maximum likelihood model of the conditional probability of participating in the acceleration program (usually a logit or probit regression to ensure that the fitted values stay between 0 and 1), and then these propensity score values are used to compare treated and non-treated firms (Cunningham, 2021).

Matching methods require the fulfilment of two assumptions. The first is the conditional independence assumption (CIA), which means that conditioned on the observable characteristics (e.g., $X$ variables) of possible participating firms, the decision to participate in the acceleration program should be independent of the outcome measure (Oh *et al.*, 2009). In other words, conditional on $X$, the assignment of firms to the treatment group is as good as random (Cunningham, 2021). This assumption is written as follows:

$$(Y_1, Y_0) \perp D \mid X \tag{1}$$

where $\perp$ means independence, $Y_1$ is the outcome of the accelerated firm, and $Y_0$ denotes the outcome of the non-accelerated firm. $D$ is an indicator variable denoting participation in the acceleration program. $X$ is the set of variables being used for conditioning.

The second assumption is the common support assumption. It is written as follows:

$$0 < \Pr(D = 1 \mid X) < 1 \tag{2}$$

It means that, for any given probability, there must be firms in both the treatment group and the control group, and it requires that, for each value of $X$, there is a positive probability of being both treated and untreated. According to Rosenbaum and Rubin (1983), if these assumptions can be satisfied, it is theoretically possible to obtain an unbiased estimation of the effect of a program. The propensity score, in the context of this research, is the probability of firms participating in the acceleration program conditional on their observable characteristics. This can be expressed mathematically as follows:

$$\text{Propensity score} = p(X) = \Pr(D = 1 \mid X) \tag{3}$$

Given propensity score equation (3) and assuming equations (1) and (2), all biases due to observable variables can be removed by conditioning on the propensity score (Rosenbaum and Rubin, 1983). That is, $D$ and $X$ are independent of one another conditioned on the propensity score, which can be represented through the following equation:

$$(Y_1, Y_0) \perp D \mid p(X) \tag{4}$$

Based on (4), the policy effect can be defined for a population of units denoted by $i$ as the average effect of treatment on the treated (ATT):

$$ATT = E_{p(X_i)}\{E(Y_{1i}|D_i = 1, p(X_i)) - E(Y_{0i}|D_i = 0, p(X_i))\ |D_i = 1)\} \tag{5}$$

The ATT is the difference in the average results of the firms that are supported and not supported by the acceleration program, the group of not-supported firms being previously formed by matching units based on the propensity score.

### 3.2. Data and empirical approach

The first step for this research consisted of contacting Créame's acceleration program and gathering information from the 77 companies accelerated from 2018 to 2020. The acceleration program has operated since 2012, but information on the cohorts before 2018 is incomplete or difficult to access. Being selected for the program depends on different factors analyzed by a committee of internal and external judges with experience in entrepreneurship, company building, and consultancy. The variables considered for evaluation have changed slightly since 2012, but the focus remains on the following ones: sector, founding team experience, previous sales and sales growth, company age, business model, and company's use of technology.

The data for the pre-treatment period were obtained from Créame's inscription databases. Post-treatment information was obtained from a survey that must be completed by the entrepreneurs annually to follow up on the acceleration program's descriptive results. This survey gathers information regarding main revenue results for the last full year, number of employees, whether or not the company has received investments, whether or not the company is exporting product/services. The survey is sent directly through Créame's official e-mail and there is a team within the acceleration program in charge of doing weekly follow-ups to the entrepreneurs to complete the survey, and this team is also in charge of requiring evidence for the main revenue results (i.e., requesting audited financial statements).

With this survey, it was possible to gather information from three cohorts of the acceleration program and their results regarding the outcome variables (number of employees and revenue) for the year 2021. Of the 77 companies that were part of the program in those three cohorts, it was possible to obtain information about 60 of them using the survey. It was not possible to gather information on the remaining companies in a timely manner for this research. Throughout the whole research process, the names of the companies remained confidential.

To build a solid control group, we had access to the official "Registro Único Empresarial," a company registry containing all the information on companies in the territories that make up the jurisdiction managed by the Chamber of Commerce of Medellin. Since all the treated companies were located in Medellin, the database for the control group consisted of 70,000 companies that were historically registered in Medellin until 2018 with their baseline characteristics and their 2021 revenue as an outcome variable; such database included firms with heterogeneous characteristics because the only filter was registry location. We proceeded to exclude companies that were not operating in the services, manufacturing, or commerce sectors since those were the sectors found in the treatment group. We also excluded large companies from the group, leaving only micro, small, and medium-sized enterprises. The remaining control group consisted of 44,537 observations; 39,777 (89%) of them were micro-businesses, 3,939 (8.8%) were small businesses, and 817 (2.2%) were medium-sized businesses.

To ensure that the outcome variable was similar in characteristics between the treatment and the control group, we also excluded companies in the control group that had zero revenue for the post-treatment period, which excluded more than 25,000 observations; this could be explained, among others, due to the economic effect of COVID-19 pandemic on the financial results of these companies. Lastly, some specific International Standard Industrial Classification (ISIC) codes were excluded from the control group sample because they were not present in the treatment group. After these exclusions, the remaining control group before starting the matching process contained 16,994 observations.

Based on the available information for the control group, the only outcome variable that it was possible to measure in this research was revenue. According to Autio and Rannikko (2016) and Dvouletý *et al.* (2018), this remains a key performance indicator to determine the impact of a particular entrepreneurship program, because it represents real commitments and willingness to pay by the customers and, as an outcome variable, it can also represent growth in employees. It was not possible to directly measure employment or other financial variables, which would also have been useful in determining the impact of the program, due to the lack of the required post-treatment-period information for the control group.

The controls for the model are the number of employees, company's age in years, founder/CEO gender, legal form, company size, and sector. Number of employees. The company age in years is pertinent to indicate possible differences in the total time available to the company to commercialize its solution, which also can have effects on the propensity to be part of the acceleration program (Autio and Rannikko, 2016). Founder/CEO gender was built using the name of the legal representative of each company and using a special algorithm called Genderize.io to determine the gender based on the name. Prior literature on the impact evaluation of entrepreneurship programs has used gender dummies due to the differences between ventures led by women and ones led by others (Butler *et al.*, 2016; Gonzalez-Uribe and Leatherbee, 2017; González-Uribe and Reyes, 2020; McKenzie, 2017).

Company size is also a selection criterion for the acceleration program, so it has a direct effect on the propensity of being selected for the program. Lastly, as the sector distribution shows in the Table 2, most of the treated firms operated in the services sector, specifically in professional and technical services, and information, technology, and communication services. The selected control variables have been relevant in other impact evaluations using matching techniques concerning entrepreneurship policy (Autio and Rannikko, 2016; Dvouletý, 2017; Dvouletý and Blažková, 2019). The list of variables is presented in Table 2.

***-- Table 2 here --***

## 4. Findings

### 4.1 Descriptive results

Table 3 reports the summary statistics of each variable, both for the control and for the treatment group. For the outcome variable (Ln Revenue), it was found that the difference in mean between the treatment and the control group before matching is 0.926. The interval of values is wider and has a higher standard deviation for the control group than for the treatment group. Regarding the continuous control variables (i.e., employees and company's age), the standard deviation for the control group is again higher than that of the treatment group, but the average values remain close to each other. The range of employees for the control group goes from 0 to 3,000, while it goes from 1 to 94 in the treatment group; this is also indicative of the differences we find in company size between the treatment and control groups. These differences between treatment and control groups don't represent a problem, since the matching approach will pair treated firms with similar untreated firms. Treated companies have more employees and are younger, on average, than companies in the control group.

***-- Table 3 here--***

Regarding the discrete control variables (i.e., founder/CEO gender, manufacturing, commerce, services, micro, small, legal form), the standard deviations of the treatment group are in most cases higher than those of the control group. On average, there are more treated companies that are run by female CEOs than control companies. In addition, the sector categories manufacturing and commerce are more present in the control group than in the treatment group. In contrast, more treated firms than control firms are involved in service activities (professional and scientific services and information, communication, and technology services). The strong presence of the service sector in the treated companies was expected given the initial evaluation criteria for entering the program. Heterogeneity between the control and the treatment group can be found in this initial comparison for different variables. The matching process is therefore necessary to diminish the potential bias found in this initial comparison and to calculate an accurate average treatment effect on the treated (Cunningham, 2021).

**4.2. Propensity score estimation**

As presented earlier, the propensity score mechanism (Rosenbaum and Rubin, 1983) has been used in non-experimental settings in different knowledge areas. The first step in the estimation is to calculate the propensity score of participation in the acceleration program. This is accomplished using a logistic regression model with the treatment variable (Accelerated) as the dependent variable. The result of this logistic regression is then used to match the control group and the treatment group using different mechanisms (kernel matching and nearest-neighbor matching); after this matching, the result of the ATT will be available as the difference between the two groups in the outcome variable.

The results of the estimation are shown in Table 4. For the logistics regression and posterior matching, we used a nested approach, resulting in three different models with different covariates. Model 1 only takes into consideration the main internal characteristics of the firm (employees, company's age, founder/CEO gender, and legal form). We included another set of variables in Model 2, which involves variables related to firm size (micro and small). Finally, Model 3 considers all the previous variables plus the economic sector (i.e., manufacturing, commerce, and services). With this strategy, we guaranteed that, by including the control variables model by model, the results would remain similar and robust. Except for the employees variable, which changes in direction and significance, the rest of the variables hold across the different models, both in direction and significance.

***-- Table 4 here--***

Regarding the results of the propensity score, it is apparent that some variables are not statistically significant in the estimation. However, in accordance with the previous literature on matching techniques (Angrist and Pischke, 2008; Caliendo and Kopeinig, 2005; Cunningham, 2021) and the most commonly used covariates found in impact evaluations of entrepreneurship programs and their empirical findings (Autio and Rannikko, 2016; Dvouletý, 2017; Dvouletý *et al.*, 2019; Efobi and Orkoh, 2018), we decided to include them to obtain a more precise estimation of the propensity score.

The propensity score estimation shows that a company's age, founder/CEO gender, sector, size, and legal structure are statistically significant in determining the probability of participation in treatment. Regarding the company's age, young firms are more likely to be treated than older firms, which is coherent with the particularities of the program. Concerning size, companies that are not micro businesses are more likely to be part of the acceleration program, which indicates a tendency of the program to opt for firms that have at least a minimum operational structure that relies on more than 9 employees. This characteristic is also consistent with the findings of average company size on the business acceleration literature (Polo García-Ochoa *et al.*, 2020).

Regarding sectors, it was found that companies focusing on services (i.e., information, communication, and technology as well as professional, technical, and scientific services) are more likely to be accelerated than those in other sectors. Furthermore, companies with a female founder/CEO are more likely to be treated than their counterparts with a male founder/CEO.

The common support region remaining after the propensity score estimation shows that, using Model 3's covariates, there is a positive probability of being treated up to 31.4% for both accelerated and non-accelerated firms. However, most of the probability distribution is concentrated on the left tail, indicating overall a low probability of treatment for both treated and control firms. Common support was also calculated using the covariates of Model 2 and Model 1, resulting in maximum probability values of 15.1% and 7.8%, respectively. This means that it is possible to find a wider common support region with Model 3, being the estimation that better fits the first assumption of matching techniques, as seen in the previous section. Following Caliendo and Kopeinig (2005), given the low probability of being treated and the concentration on the left side of the distribution, a trimming method for the common support region was applied, limiting the matching algorithm to the region above a 0.15% probability of being accelerated. This trimming caused different observations of both the treatment and the control group in each of the three models to be excluded for the matching process. For a detailed common support graphic of each model after the trimming procedure was applied, see Figure A1 in Appendix.

**4.3. Main results**

The next step was to execute the matching with different mechanisms, having already calculated the propensity score for each model. This estimation was based on the outcome variable revenue, which was expressed in the natural logarithm form to reduce the variance.

According to Oh *et al.* (2009), to obtain an accurate calculation of the ATT, in theory, one should match accelerated and non-accelerated firms precisely on the base of the propensity score. However, it is not possible to match precisely on the propensity score in practice, so it is fundamental to use alternative matching techniques. For this study, kernel matching and nearest-neighbor matching were used and compared as a robustness check. The estimations show the average difference between the treatment group (accelerated startups) and the control group (non-accelerated startups). It is important to bear in mind that the calculated impacts are local to the treatment group and cannot be expanded to a broader sample of startups. The results, using the previously explained nested approach, are shown in Table 5.

***-- Table 5 here --***

Overall, we found a positive difference between the treated and the control group across matching techniques and models used. For Models 1 and 2, we see a statistically significant difference between treated companies and control companies for every matching criterion used. Although this is positive, it is important to remember the narrower common support found with these two models.

Regarding Model 3, when using nearest-neighbor matching with the 10 nearest neighbors, a statistically significant difference at the 5% level between the treatment and the control group was found in the outcome variable. The significance of this difference increased at the 1% level when using the kernel matching technique. However, when using five nearest neighbors, the difference was smaller and not significant at any level. The number of observations analyzed and matched was 7,835 due to the common support region trimming performed in the previous section.

It is worth noting that, in kernel matching, every treated subject is matched with the weighted average of all the control subjects. This means less variance in the estimation but also an increased bias compared with a matching algorithm performed with a smaller group of control subjects. Hence, the common support assumption is especially important when using kernel matching to ensure that all the treatment and control subjects have a positive probability of participation in treatment (Caliendo and Kopeinig, 2005).

As an alternative, nearest-neighbor matching implies the opposite problem (i.e., less biased estimation but more variance) because every treated subject is matched with a specific number of closest neighbors from the control group (Cunningham, 2021). This also means an extra decision to make regarding the number of nearest neighbors to include in the estimation. In this case, since the available treatment group is quite small compared with the available control group sample, the probability of finding just one nearest neighbor to match remains small; hence, following Oh *et al.* (2009), we decided to proceed with the matching algorithm using five and 10 nearest neighbors, the latter being more appropriate than the former.

Overall, based on the estimates for Model 3, using nearest-neighbor matching with 10 nearest neighbors and using kernel matching, accelerated firms on average have higher revenue three years after treatment than non-accelerated firms. The direction of this impact remained the same, although not statistically significant, when discretionally using five nearest neighbors with the nearest-neighbor matching algorithm.

As an additional robustness check, we changed the variables representing firms' size as the classification varies according to Colombian criteria (*Decreto 1074 de 2015 Sector Comercio, Industria y Turismo*, 2015). In Colombia, this classification is not based on the number of employees but instead on the annual operational revenue and sector. The revenue limits for micro, small, and medium-sized enterprises can reach US$440,000, US$4,174,000, and US$20,920,000, respectively, depending on the sector[1] (Bancoldex, 2021). Table A1 in the Appendix shows the logit estimation following the previously mentioned nested approach and indicates that, although the size variables are not statistically significant, they follow similar directions and magnitudes, signalling that our model remains solid when this regulation takes place. Table A2 in the Appendix confirms that the ATT is also positive for treated companies. With these results, we are confident that our modelling approach was appropriate and reliable.

[1] Using the average exchange rate of 2021 (3,780 COP/USD).

## 5. Discussion

Motivated by the lack of studies assessing the impact of supportive entrepreneurship programs (particularly acceleration), which is more evident when considering developing countries, we measured the effects of a regional acceleration program in Medellin, Colombia. Using capability theory (Sutton, 2012), which framed our literature review, we explored previous works suggesting that different characteristics have been utilized when assessing the impact of entrepreneurship-supportive programs on outcome variables such as revenues. Through both logit regressions and propensity score matching, we identified the company age, founder/CEO gender, company size, and sector as important variables to match treated and non-treated firms. The average treatment effects technique revealed that Créame's acceleration program, sponsored by the Mayor's Office of Medellin, was successful in increasing the revenue of treated ventures.

Regarding company age, we found that there is a higher probability of being treated when the firm is younger, which is consistent with other impact evaluation studies, mainly Dvouletý (2017), Gónzalez-Uribe and Leatherbee (2017), and Hallen *et al.* (2020). This result might exemplify the focus on new ventures as part of the business policy and ecosystem dynamics. As for the founder/CEO gender, our results indicated that being a female CEO positively influences the probability of treatment. This means, among other aspects, that the acceleration program is paying special attention to tackling gender disparities issues in entrepreneurship. This is important given the recent literature showing that entrepreneurial decisions made by women are closely linked to social mobility issues (Aparicio *et al.*, 2022). The sector is also a determinant of participation in treatment. This is consistent with the majority of impact evaluations performed for entrepreneurship programs, in which service companies


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(specifically information and technology services) have been the ones both being chosen and achieving the best outcomes (Bone *et al.*, 2017; Yu, 2020).

Thanks to these findings, this non-experimental impact evaluation is filling a gap in the literature. We have offered new evidence about impact evaluation in a developing country, which was useful for bringing insights into the discussion about entrepreneurship policies (Acs *et al.*, 2016; Shane, 2009; Welter *et al.*, 2017) and making suggestions for policymakers when considering public funds to support entrepreneurship and new venture growth.

**5.1. Contributions to the literature about impact evaluation in entrepreneurship**

Acceleration programs are expanding worldwide precisely because, on the one hand, by analyzing firm-level effects, they focus on building firm capabilities and alleviating the growth constraints faced by entrepreneurs in young ventures (Clarysse *et al.*, 2015). As the literature review showed, several programs (both public and private led) are achieving this goal. To measure the effectiveness of a particular program, it was necessary to evaluate outcome variables that could demonstrate company growth, such as revenue, assets, profitability, or employment (Autio and Rannikko, 2016).

It can be said, then, that Créame's local acceleration program effectively alleviated the growth constraints and built the firm capabilities of young ventures when analyzed in the light of their revenue three years after the program. Arguably, the mentorship focus of the acceleration program could be a specific mechanism for reaching this outcome, given that mentorship is proven to have a positive impact on the firms' absorption capabilities (Polo García-Ochoa *et al.*, 2020); however, further details of the program should be assessed. Although this conclusion is positive, further variables should be considered to obtain a thorough and more precise result concerning the impact. In addition, given that the program aims to select mainly technology-based companies, and some of these companies are not focused on growing revenue metrics in their initial years of operation due to their focus on technology and product development, the selection of revenue as a performance indicator could be debatable (Dvouletý *et al.*, 2018).

On the other hand, regarding ecosystem- or city-level effects, some accelerators and other publicly funded entrepreneurship programs also focus on allocating entrepreneurial resources in an efficient way. That means, for instance, promoting the allocation of top entrepreneurial talent to high-impact, high-growth ventures and disincentivizing the creation of survival ventures created by low-skilled entrepreneurs (Shane, 2009). Given the kinds of companies selected for the program and the ATT results found in this research, we could say that the program is efficiently allocating entrepreneurial resources and "picking winners"—that is, selecting entrepreneurs with the potential to become gazelles and unicorns (Autio and Rannikko, 2016). However, for a thorough analysis of this specific resource allocation problem, further productivity variables and spillover information should be measured (Butler *et al.*, 2016; Eslava *et al.*, 2019).

**5.2. Contributions to the debate about entrepreneurship policy**

Despite the results and whether the program is picking winners efficiently, it remains important to frame this public policy approach using the current trends in the entrepreneurship literature. A group of scholars—mainly Acs *et al.* (2016), Åstebro (2017), Autio and Rannikko (2016), Cho and Honorati (2014), Lall *et al.* (2020), and Shane (2009)—has argued in favor of an entrepreneurship policy focused on picking winners and promoting the creation of opportunity ventures, that is, ventures created by highly talented entrepreneurs exploiting huge market opportunities. The reason is that these firms are the ones that are interested in innovation and end up pushing the results of different macroeconomic variables, such as employment, productivity, and economic growth. These scholars have also generally agreed that encouraging the creation of subsistence entrepreneurship—that is, companies that would not be created if a decent job opportunity was on the table for the founders—is, overall, bad for the economy due to the possible negative effects on formality, employment, productivity, and company size in the long term.

However, another group of scholars—mainly Aparicio *et al.* (2022), Arshed *et al.* (2014), Puente *et al.* (2019), and Welter *et al.* (2017)—has argued for a more thorough approach to entrepreneurship study that avoids classic dichotomies (i.e., opportunity vs. necessity). This group of authors considered it necessary to give a place to the bulk of ventures that does not fit the Silicon Valley rubric of being venture capital backed, innovative, and technology based and having high growth rates. Overall, these gazelles and unicorns only account for 1% of firms in developed economies and far fewer in developing economies.

Furthermore, the obsession with Silicon-Valley-type entrepreneurship could harm entrepreneurial ecosystems because it was born thanks to a series of intricate and almost irreplicable decisions that accumulated for more than 50 years (Audretsch, 2019) and, to date, there is no other country with a similar dynamic (Nicholas, 2019). Instead of trying to replicate these settings—which, according to Audretsch (2019), would be positive but not enough—developing countries could focus more on creating their own ecosystem versions after gaining a deep understanding of their economic activities, human capital potential, and institutional arrangements. Pahnke and Welter (2019) provide a good example of this possibility.

### 5.3. Public policy implications for a region in a developing country

In the local context of developing countries, it is important to address the previous perspectives. Although Colombia and Latin America are following a positive venture-backed entrepreneurship trend, with record-hitting venture capital rounds quarter by quarter since 2020 (Stanford, 2022) and innovative entrepreneurship that will gradually become more relevant to economic growth in Colombia (Aparicio, Urbano, and Audretsch, 2016), most firms will not receive funding from venture capital deals and will face two possible outcomes: either the founders will pass through the Valley of Death and start growing steadily and constantly with bootstrapping strategies or they will end up closing their business.

It has been proven that, despite all the efforts worldwide to have the best criteria for picking winning ventures, several problems and a great deal of randomness are still involved. For instance, a majority of venture capital funds in the US, decade by decade since 1970, have been unable to outperform public markets (Nicholas, 2019). It is therefore relevant that local business accelerators in developing countries—fundamentally ecosystem accelerators, as defined by Clarysse *et al.* (2015)—should have precise criteria for selecting the entrepreneurs who will enter different cohorts but without becoming obsessed by the dichotomic categories for classifying those participating firms. As mentioned earlier, whether a company will become a unicorn in the next five years or not, there is evidence enough to state that a good acceleration program will still alleviate the growth constraints, enhance startups' performance, and build firm capabilities (González-Uribe and Reyes, 2020), mainly driven by absorption, integration, and innovation capabilities (Polo García-Ochoa et al., 2020).

In the context of emerging economies, the case of this business acceleration program is relevant to other developing countries to gain more insights on how publicly-funded acceleration programs can enhance startups' dynamic capabilities and through which specific mechanisms, in order to design and implement policies for entrepreneurship and SMEs growth (Efobi and Orkoh, 2018). The results of this and other programs show that the design of the program, including the mentors that participate on the process, the extension, and the intrinsic characteristics of the companies being chosen, are crucial to generate the desire impact in the long term.

Regarding other design and policy recommendations, it is fundamental to narrow down participants into treatment for more specific economic activities, as well as maintaining the given eligibility criteria for at least three years, so the timeline can be long enough to explore


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possible design mistakes. Furthermore, the technology-based categorization will be useful only if there is a clear step-by-step guide for judges to avoid subjective selection (Gonzalez-Uribe and Leatherbee, 2017). Furthermore, the way in which information is gathered in the pre-treatment and post-treatment periods is the key to ensuring that more robust impact evaluations can be undertaken in the future (Consejo Privado de Competitividad, 2020), even with a different mechanism, such as regression discontinuity design (RDD), which exploits the mean differences in outcome variables between accelerated and non-accelerated firms close to the cutoff line. Lastly, for future research, it will be vital to gain access to more financial data from startups' balance sheets or profit and loss statements (Dvouletý *et al.*, 2018).

**5.4. Limitations and future research avenues**

While our study addresses a significant gap, it is essential to acknowledge its limitations. Firstly, the widespread impacts of the COVID-19 pandemic across various economic sectors present challenges in accurately measuring outcome variables during a period of economic recovery (Dvouletý *et al.*, 2021). Utilizing an average of two to three years for outcome variables, as recommended by Dvouletý *et al.* (2018), could mitigate distortions caused by this extraordinary circumstance. Acquiring recent data would be advantageous in overcoming this limitation, although data availability remains an issue. Therefore, we encourage organizations responsible for implementing and managing acceleration programs to contribute data to repositories for academic use.

Second, the bulk of treated startups is too heterogeneous due to the already-mentioned changes in the program's focus and eligibility criteria. This complicates an accurate comparison with a control group and, overall, lowers the probability of being treated when calculating the propensity score (Caliendo and Kopeinig, 2005). Third, to assess the multilevel effects of the acceleration program on the ecosystem, it will be necessary to expand the time span between the pre-treatment and the post-treatment period.

Fourth, this research has only assessed the case of Medellin, which is one Colombian city. Nonetheless, Medellin serves as a microcosm of Colombia's economic landscape for various reasons. Firstly, it stands as the country's second-largest city by population. Despite enduring significant violence during the 1990s, Medellin experienced substantial migration, not only from rural to urban areas (Hernández-García, 2013), but also attracted skilled human capital from other cities and countries (Muñoz-Mora *et al.*, 2022). Secondly, owing to its economic prowess, statistics from DANE (2024) indicate that Medellin contributed an average of 14.4% to Colombia's total GDP between 2005 and 2022. This contribution closely rivals that of Bogota, which averaged 25.6% over the same period. Consequently, policies and public strategies implemented in Medellin hold significant sway over the nation as a whole, given its status as an emerging economy. Future research, though, might include regional comparisons that shed light on the effectiveness of local policies. As cultural differences exist within the country, an institutional perspective (North, 1990) might serve to disentangle the role of institutions in shaping and implementing programs to encourage entrepreneurship and firm growth (Thornton *et al.*, 2011; Urbano *et al.*, 2019). Thanks to this kind of evidence, new insights could be brought into the discussion of context for entrepreneurship (Welter, 2011), in which not only time but also space, history, and institutions define individual behavior toward entrepreneurship.

Lastly, the treatment group was smaller than expected due to the lack of access to previous years of information. This increased the heterogeneity problem and decreased the statistical power of the results (Cunningham, 2021). However, some impact evaluations with small sample sizes can be found in the literature, especially when the sample consists of companies and not of individual economic agents. For instance, Bloom *et al.* (2011) used 14 treatment plants and 14 control plants, and Mano *et al.* (2014) consulted 47 participants for a business training program evaluation. In addition, for the proposed matching method, good estimation of the ATT relies on the construction of an accurate control group that can satisfy both common support and conditional independence assumptions (Cunningham, 2021).

## 6. Conclusions

In this study, we focused on the relationship between a local business acceleration program sponsored with public funds and firm performance (measured through revenue) in the region of Medellin, Colombia, which has been under-researched by entrepreneurship scholars. We used firm-level information obtained both from the program's operational team and through surveys and then compared it with a database of non-accelerated firms located in the same region. Contrasting the control group, accelerated firms showed significantly better revenue using nearest neighbor matching with 10 nearest neighbors and using kernel matching. Although more outcome variables need to be measured, this exploratory research shows that the acceleration program is efficiently alleviating growth constraints and building firm capabilities. Further measures with an extended time span and a focus on multi-level effects are needed to address the problem of efficient entrepreneurial resource allocation. From a broader public policy view, we suggest that the program should not be obsessed with picking winners but should instead keep improving its design to assess the common growth pains faced by new ventures more accurately.

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**-- Appendix (Figure A1 and Table A1) here --**

## Tables

*Table 1. Impact evaluations of entrepreneurship programs.*

| Author(s) | Title | Type of Program | Country | Evaluation Method | D. variable (Outcome) | Were expected results achieved? (Program Impact) |
|---|---|---|---|---|---|---|
| **1. González-Uribe and Reyes (2020)** | Identifying and Boosting "Gazelles": Evidence from Business Accelerators | Business Accelerator (Public) | Colombia | Instrumental variables | Revenue | Yes |
| **2. Gonzalez-Uribe and Leatherbee (2017)** | The Effects of Business Accelerators on Venture Performance: Evidence from Start-Up Chile | Business Accelerator (Public) | Chile | Regression discontinuity design (RDD) | Performance proxies (Capital raised, valuation, sales, # employees, survival). | Yes |
| **3. McKenzie (2017)** | Identifying and Spurring High-Growth Entrepreneurship: Experimental Evidence from a Business Plan Competition | Business Training Course (Public) | Nigeria | Randomized experiment | Survival; # Employees; Sales; Profits; Firm+10Workers; Firm+25Workers | Yes |
| **4. Hallen, Cohen, and Bingham (2020)** | Do accelerators work? If so, how? | Business Accelerator (Private) | USA | Logit; quasi-Regression discontinuity design (RDD)[2] | Survival; Capital raised; Web traffic (proxy of traction) | Yes (Survival) No (Traction) |
| **5. Butler, Galassi, and Ruffo (2016)** | Public funding for startups in Argentina: an impact evaluation | Grants (Public) | Argentina | Regression discontinuity design (RDD) | Survival; Revenue; Employment | Yes (Survival) No (Revenue) Yes (Employment) |
| **6. Autio and Rannikko (2015)** | Retaining winners: Can policy boost high-growth entrepreneurship? | Business Accelerator (Public) | Finland | Propensity score matching; Difference in differences | SalesGrowth: (log difference of sales between the year before and three subsequent years after the program). | Yes |
| **7. de Mel, McKenzie, and Woodruff (2014)** | Business training and female enterprise start-up, | Business Training | Sri Lanka | Randomized experiment. | Monthly profits; | Yes[3] |

[2] Due to the ventures not being classified with a specific score, the authors were able to use a method stylistically similar, but not the same, as the RDD approach.
[3] Business training was found useful only for second group: women entrepreneurs that were interested in starting a business and were not already the owners of one.

| | | | | | | |
|---|---|---|---|---|---|---|
| | growth, and dynamics: Experimental evidence from Sri Lanka | Course (Public) | | | Monthly sales; Capital stock | |
| **8. Bruhn, Karlan, and Schoar (2013)** | The Impact of Consulting Services on Small and Medium Enterprises: Evidence from a Randomized Trial in Mexico | Subsidized consulting services (Public) | México | Randomized experiment. | Profits; Return-on-assets; Productivity residual; Employment | No (Profits) Yes (ROA) Yes (TFP) Yes (Employment) |
| **9. Åstebro and Hoos (2021)** | Impact measurement based on repeated randomized control trials: The case of a training program to encourage social entrepreneurship | Business training (Public) | France | Randomized experiment. | Entrepreneurial activity; New venture creation | Yes |
| **10. Nkiyos, Béres, Laposa, and Zavecs, (2020)** | Do financial instruments or grants have a bigger effect on SMEs' access to finance? Evidence from Hungary | Grants (non-refundable) and financial instruments (refundable)[4] (Public) | Hungary | Propensity score matching; DiD | Employment; Sales; Productivity | Yes (Employment) Yes (Sales) No (Productivity) |
| **11. Oh, Lee, Heshmati, and Choi (2009)** | Evaluation of credit guarantee policy using propensity score matching | Credit guarantee (Public) | South Korea | Propensity score matching | Sales; Employment; Wage; Survival; TFP | Yes (Sales, Employment, Wage, Survival) No (TFP) |
| **12. Norrman and Bager-Sjögren (2011)** | Entrepreneurship policy to support new innovative ventures: Is it effective? | Business Accelerator and Grant (Public) | Sweden | Matching pair analysis | Sales; Assets; Employment | No |
| **13. Efobi and Orkoh (2018)** | Analysis of the impacts of entrepreneurship training on growth performance of firms: Quasi-experimental evidence from Nigeria | Business Training Course (Public) | Nigeria | Propensity score matching; DiD | Sales; Innovation; Employment | No (Sales) Yes (Innovation, Employment) |
| **14. Dvouletý (2017)** | Effects of Soft Loans and Credit Guarantees on Performance of Supported Firms: Evidence from the Czech Public Programme START | Soft loans and credit guarantees (Public) | Czech Republic | Propensity score matching | Sales; Return on Assets (ROA); Return on Equity (ROE); | No (Either negative effect or not statistically significant effect). |

[4] Financial instruments in this context refer to repayable tools such as loans, guarantees, equity and venture capital.

| | | | | | | |
|---|---|---|---|---|---|---|
| **15. Dvouletý, Longo, Blažková, Lukeš, and Andera (2018)** | Are publicly funded Czech incubators effective? The comparison of performance of supported and non-supported firms | Business incubation (Public) | Czech Republic | Propensity score matching | Sales; Asset turnover; Personnel costs; Total assets | No (Either negative effect or not statistically significant effect). |
| **16. Smith and Hannigan (2015)** | Swinging for the fences: How do top accelerators impact the trajectories of new ventures | Business accelerator (Private) | USA | Probit; Coarsened Exact matching (CEM). | TimeToExit; TimeToQuit; TimeforVCRound | Yes[5] |
| **17. Fairlie (2021)** | Evaluating entrepreneurship training: How important are field experiments for estimating impacts? | Business training (Public) | USA | Nearest-neighbor matching; Propensity-score matching | Business ownership; Monthly sales; Employment | Yes |
| **18. Venâncio, A. and Jorge (2021)** | The role of accelerator programs on the capital structure of start-ups | Business accelerator (Private) | World | Propensity-score matching | External Equity To Capital Ratio | Yes |
| **19. Srhoj, Lapinski and Walde (2021)** | Impact evaluation of business development grants on SME performance | Matching grant public program | Croatia | Maximum distance matching | Capital Stock; Bank Loans; Sales; Employment | Yes (Capital Stock, Bank Loans). No (Sales, Employment). |
| **20. Srhoj, Škrinjarić and Radas (2021)** | Small matching grants for women entrepreneurs: lessons from the past recession | Matching grant public program | Croatia | Propensity-score matching | Capital Stock; Bank Loans; Employment; Sales; TFP | Yes (Capital Stock, Bank Loans, Employment). No (Sales, TFP). |

Source: Authors own work.

*Table 2. Variables and definitions.*

| **Definition** | **Variable** |
|---|---|
| ***Treated variable*** | |
| Accelerated | Dummy variable equal to 1 indicating whether the company was part of the acceleration program; 0 otherwise. |
| ***Control (matching) variables*** | |
| Employees | Variable refers to the number of employees before treatment period (2018). |
| Company's age | Variable refers to the years of existence of the company since its original registration. |

[5] New ventures that went through a top accelerator were found to reach exit or first venture capital round faster than those who did not went through those accelerators.

| | |
|---|---|
| Founder/CEO gender | Variable refers to a dummy equal to 1 if startup had a female CEO. This variable was created estimating legal representatives' genre based on their names using a special algorithm called genderized.io. |
| Manufacturing | Variable refers to a dummy equal to 1 indicating if company belongs to any of the International Standard Industrial Classification (ISIC) codes referring to industrial or manufacturing activities; 0 otherwise. |
| Commerce | Variable refers to a dummy equal to 1 indicating if company belongs to any of the International Standard Industrial Classification (ISIC) codes referring to commercial activities; 0 otherwise. |
| Services | Variable refers to a dummy equal to 1 indicating if company belongs to any of the International Standard Industrial Classification (ISIC) codes referring to professional and technical services, or information, technology, and communication services; 0 otherwise. |
| Micro | Variable refers to a dummy equal to 1 indicating if company is a micro business according to international classification (1-9 employees); 0 otherwise. |
| Small | Variable refers to a dummy equal to 1 indicating if company is a micro business according to international classification (10-49 employees); 0 otherwise. |
| Legal form | Variable refers to a dummy equal to 1 indicating if company holds a joint-stock legal structure or otherwise; 0 otherwise. |
| ***Outcome Variable*** | |
| Ln Revenue | Variable refers to the natural logarithm of post-treatment period revenue. |

Source: Authors own work.

*Table 3. Descriptive statistics for controls and outcome variable across both treatment and control groups.*

| **Variable** | **Group** | **N** | **Mean** | **SD** | **Min** | **Max** |
|---|---|---|---|---|---|---|
| Ln Revenue (Outcome) | Control | 16994 | 19.505 | 2.255 | 0.693 | 25.017 |
| | Treatment | 60 | 20.431 | 0.926 | 18.325 | 22.810 |
| Employees | Control | 16994 | 13.006 | 45.224 | 0 | 3000 |
| | Treatment | 60 | 15.033 | 15.527 | 1 | 94 |
| Company's age | Control | 16994 | 9.183 | 8.954 | 0 | 65.600 |
| | Treatment | 60 | 6.218 | 4.116 | 0.700 | 15.300 |
| Founder/CEO gender | Control | 16869 | 0.279 | 0.449 | 0 | 1 |
| | Treatment | 60 | 0.433 | 0.500 | 0 | 1 |
| Manufacturing | Control | 16994 | 0.277 | 0.448 | 0 | 1 |
| | Treatment | 60 | 0.200 | 0.403 | 0 | 1 |
| Legal form | Control | 16994 | 0.888 | 0.315 | 0 | 1 |
| | Treatment | 60 | 0.817 | 0.390 | 0 | 1 |
| Micro | Control | 16994 | 0.738 | 0.440 | 0 | 1 |
| | Treatment | 60 | 0.433 | 0.500 | 0 | 1 |

| | | | | | | |
|---|---|---|---|---|---|---|
| Small | Control | 16994 | 0.209 | 0.406 | 0 | 1 |
| | Treatment | 60 | 0.517 | 0.504 | 0 | 1 |
| Commerce | Control | 16994 | 0.311 | 0.463 | 0 | 1 |
| | Treatment | 60 | 0.133 | 0.343 | 0 | 1 |
| Services | Control | 16994 | 0.236 | 0.424 | 0 | 1 |
| | Treatment | 60 | 0.533 | 0.503 | 0 | 1 |

Source: Authors own work.

*Table 4. Robust logistic regression used for the estimation of propensity score*

| **Variables** | **Model 1** | **Model 2** | **Model 3** |
|---|---|---|---|
| Employees | 0.001***<br>(0.000) | -0.007<br>(0.006) | -0.004<br>(0.006) |
| Company's Age | -0.115***<br>(0.039) | -0.142***<br>(0.039) | -0.139***<br>(0.037) |
| Founder/CEO gender | 0.635**<br>(0.261) | 0.700***<br>(0.262) | 0.613**<br>(0.262) |
| Legal form | -1.664***<br>(0.556) | -1.711***<br>(0.507) | -1.631***<br>(0.467) |
| Micro | | -1.820*<br>(1.009) | -1.947*<br>(0.999) |
| Small | | 0.0533<br>(0.882) | 0.136<br>(0.888) |
| Manufacturing | | | -0.065<br>(0.456) |
| Commerce | | | -0.404<br>(0.501) |
| Services | | | 1.376***<br>(0.397) |
| Constant | -3.614***<br>(0.688) | -2.207**<br>(1.095) | -2.679**<br>(1.080) |
| Observations | 16,929 | 16,929 | 16,929 |

*** *p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Dependent variable: dummy = 1 if company was treated.*

Source: Authors own work.

*Table 5. Estimated average treatment effect on the treated after matching.*

| **Outcome Variable** | **Matching criterion** | **Model 1** | **Model 2** | **Model 3** |
|---|---|---|---|---|
| Ln Revenue | Nearest Neighbor (5) | 0.563***<br>(0.177)<br>[3.180] | 0.327*<br>(0.192)<br>[1.710] | 0.054<br>(0.186)<br>[0.290] |
| Ln Revenue | Nearest Neighbor (10) | 0.915***<br>(0.156)<br>[5.860] | 0.573***<br>(0.177)<br>[3.250] | 0.397**<br>(0.170)<br>[2.340] |
| Ln Revenue | Kernel | 1.145***<br>(0.122)<br>[9.410] | 1.113***<br>(0.143)<br>[7.810] | 0.812***<br>(0.139)<br>[5.830] |
| Observations | | 14,069 | 11,330 | 7,835 |

**** p<0.01, ** p<0.05, * p<0.1. Standard error in parentheses; t-stat in square brackets.*

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Source: *Authors own work.*

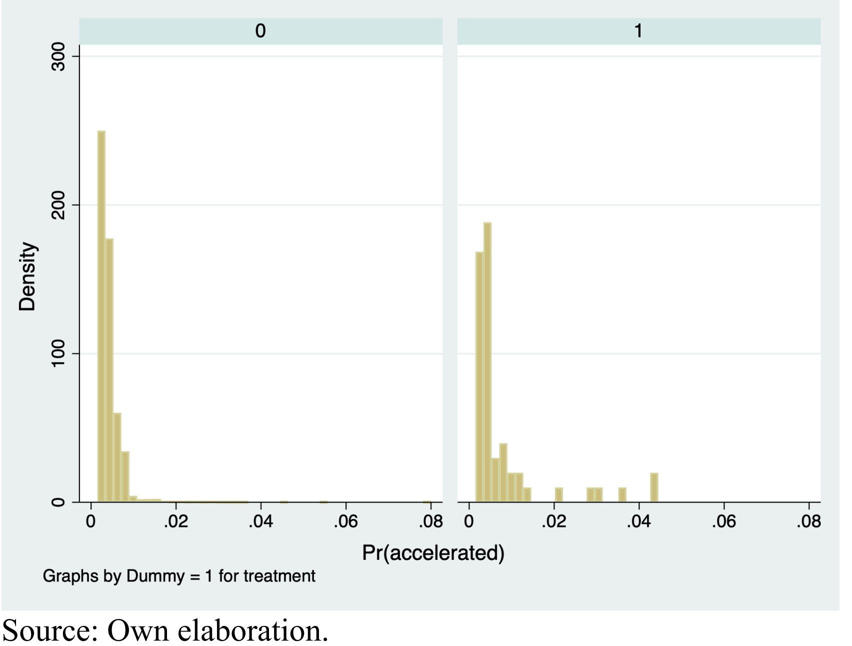
**Appendix**

***Figure A1. Common support region for three different models with probability values above 0.15%***

**A) Model 1**

Maximum probability: 7.8%

Observations after trimming: treatment 54 and control 14,069

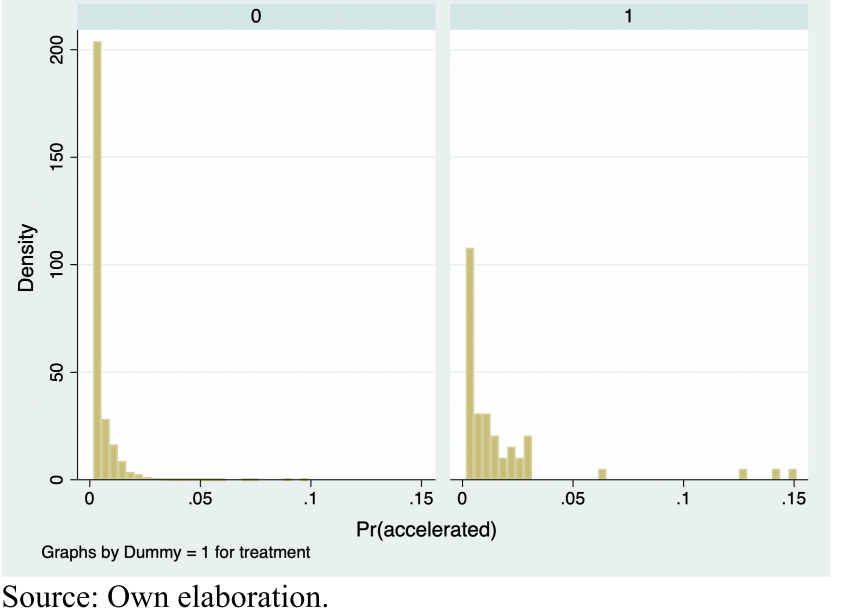



Source: Own elaboration.

**B) Model 2**

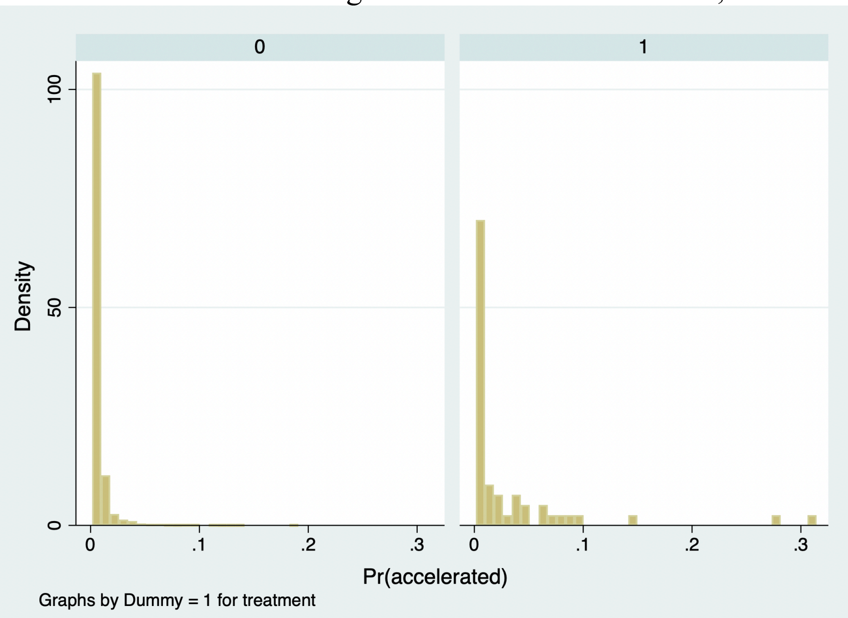
Maximum probability: 15.1%

Observations after trimming: treatment 52 and control 11,278




Source: Own elaboration.

**C) Model 3**
Maximum probability: 31.4%
Observations after trimming: treatment 52 and control 7,783




Source: Own elaboration.

*Table A1. Robust logistic regression used for the estimation of propensity score with local size parameter (Lp) variables*

| Variables | Model 1 | Model 2 | Model 3 |
|---|---|---|---|
| Employees | 0.001***<br>(0.000) | 0.001**<br>(0.000) | 0.001***<br>(0.000) |
| Company's Age | -0.115***<br>(0.039) | -0.120***<br>(0.039) | -0.117***<br>(0.038) |
| Founder/CEO gender | 0.635**<br>(0.261) | 0.646***<br>(0.261) | 0.597**<br>(0.262) |
| Legal form | -1.664***<br>(0.556) | -1.668***<br>(0.545) | -1.618***<br>(0.522) |
| Micro (Lp) | | -0.040<br>(0.788) | -0.275<br>(0.782) |
| Small (Lp) | | 0.304<br>(0.807) | 0.346<br>(0.782) |
| Manufacturing | | | -0.024<br>(0.458) |
| Commerce | | | -0.532<br>(0.506) |
| Services | | | 1.132***<br>(0.407) |
| Constant | -3.614***<br>(0.688) | -3.621***<br>(1.014) | -3.822**<br>(0.990) |
| Observations | 16,929 | 16,929 | 16,929 |

Source: Authors own work.

*Table A2. Estimated average treatment effect on the treated after matching using local size parameter variables*

| Outcome Variable | Matching criterion | Model 1 | Model 2 | Model 3 |
|---|---|---|---|---|
| Ln Revenue | Nearest Neighbor (5) | 0.563***<br>(0.177)<br>[3.180] | 0.500***<br>(0.175)<br>[2.860] | 1.321***<br>(0.217)<br>[6.080] |
| Ln Revenue | Nearest Neighbor (10) | 0.915***<br>(0.156)<br>[5.860] | 0.653***<br>(0.155)<br>[4.210] | 1.261***<br>(0.183)<br>[6.880] |
| Ln Revenue | Kernel | 1.145***<br>(0.122)<br>[9.410] | 1.114***<br>(0.122)<br>[9.160] | 1.112***<br>(0.127)<br>[8.840] |
| Observations | | 14,123 | 14,125 | 11,531 |

**** p<0.01, ** p<0.05, * p<0.1. Standard error in parentheses; t-stat in square brackets.*

Source: Authors own work.