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REVIEW

How effective are training and mentorship programs for entrepreneurs at promoting entrepreneurial activity? An impact evaluation

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Abstract

In this paper, we analyze the impact of a government-sponsored program aimed at promoting entrepreneurial activity in Uruguay. The C-Emprendedor program provides training and mentorship to potential entrepreneurs throughout the process of development of a business. We contribute to the empirical literature on the effects of different entrepreneurial programs, and provide information for policymakers, by conducting a rigorous evaluation of a program designed to foster entrepreneurial activity. Using regression discontinuity methods, we assess the impact of the program on actions taken to create a business, investment, business creation, and employment. We find significant, although non-robust, effects on employment and the probability to take actions aimed at creating a business. No effects were found on investment and the rate of business creation. Our research provides important insights for the better design of public policies aimed at developing entrepreneurship skills.

Keywords Training and mentorship programs for entrepreneurs · Entrepreneurial activity · Impact evaluation

JEL Classifications C31 · H25 · L26 · M53

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Introduction

The creation of new ventures, businesses or companies, is crucial for economic development (Baumol 2004; Zacharakis et al. 2000; Scarpetta et al. 2002). Scholars relate this causality to the effect of new businesses on productivity growth, and the driving force of economic development (Foster et al. 1998; Bartelsman et al. 2003; Butler et al. 2016). Hopenhayn (2014) suggests that both the suboptimal allocation of entrepreneurial talent and the distortions to the entry of firms in different markets undermine productivity.[1]

The existence of market failures (restrictions on access to credit, lack of an efficient intellectual protection system, information asymmetries, uncertainty about the production function, etc.) or coordination failures (between agents that support entrepreneurship activities, or between entrepreneurs with potential complementary capacities) act as obstacles for entrepreneurship.

In the presence of market failures, policymakers can intervene through a heterogeneous set of instruments, ranging from training and mentorship programs, promotion of coordination among entrepreneurs, technical advice, to financing of entrepreneurial activities at different stages of the business development process. We focus on the first type of interventions: training and mentorship programs for entrepreneurs. The question we want to answer in this paper is: What is the impact of training and mentorship programs for entrepreneurs on actions taken to create a business, investment activities, business creation, and employment? We answer this question using administrative and survey data from a government-sponsored program in Uruguay (C-Emprendedor), applying a regression discontinuity (RD) identification strategy.

Our contributions are threefold: first, we add empirical evidence about the effects of a program that offers only training and mentorship support, in contrast to most of the evaluated programs that also offer some funding. This adds to a literature that has yet to reach a consensus about the effects of different types of entrepreneurial support programs. Second, we conduct a rigorous evaluation of an entrepreneurship program, using regression discontinuity methods. Third, we contribute to the better design of policies aimed at supporting entrepreneurial activities in Uruguay. Based on our results, and considering the empirical literature, we provide policy recommendations to help increase the impact of such programs. Our contributions provide relevant information for policymakers. By comparing the effects of the program evaluated with similar instruments designed to support entrepreneurs, we contribute to the better allocation of funds to the fostering of entrepreneurial activities.

By assessing the impact of a training and mentorship program, our research contributes to the better design of public policies in Uruguay. Governments, international institutions, and NGOs recognize the importance of overcoming market failures to foster economic growth. However, most funds assigned to entrepreneurial programs are not allocated based on empirical evidence (Klinger and Schündeln

[1] Improving upon the misallocation of entrepreneurial talent could increase productivity in developing countries by up to 25%, according to this author.

2007). By estimating the effects of the C-Emprendedor program, we provide relevant information for policymakers. In addition, the instrument evaluated in our paper is among the few programs aimed at promoting entrepreneurial activity that does not provide any type of funding (prizes or the promise to get access to funds to beneficiaries) or infrastructure support. We, therefore, contribute to the literature on entrepreneurial programs by assessing the impact of a particular instrument, based purely on training and mentorship, on observable outcomes.

The institution in charge of this program (the Ministry of Industry, Energy and Mining; MIEM for its Spanish acronym) has been promoting entrepreneurship in Uruguay since 2006, based on a methodology of support to entrepreneurs developed by the United Nations Industrial Development Organization (UNIDO). The program supports entrepreneurs who have a business idea, newly created enterprises that are taking their first steps, or micro- and small companies that wish to start a new line of business. Beneficiaries of this program are mostly subsistence entrepreneurs with no clear innovative behavior, who are not targeted by other entrepreneurship support programs in Uruguay. C-Emprendedor provides training in managerial skills, technical assistance, and follow-up of the entrepreneurs until the implementation of their business ideas. The instrument assists the entrepreneur in her understanding of the complexities associated with running a small business, which helps to improve her entrepreneurial capacity, and eventually to contact with other entrepreneurs and entrepreneurial support institutions.

The impact evaluation of entrepreneurship programs presents numerous challenges, particularly so in developing countries (López Acevedo and Tan 2011). In general, problems to construct a valid counterfactual group arise mainly from two reasons: it is difficult to obtain reliable data about beneficiaries and non-beneficiaries of the programs; and selection processes into the program are rarely random, so that beneficiaries and non-beneficiaries groups differ in explanatory variables of entrepreneurial performance. We face both obstacles in our research. We overcome the first challenge using administrative data and a survey of both treated and non-treated individuals. To deal with the second obstacle, we exploit the score used to allocate individuals into the program and apply regression discontinuity methods. The RD analysis accounts for the endogeneity of receiving training and other omitted variable biases. The advantage of RD designs compared to other non-experimental analyses, such as those based on unconfoundedness, is their relatively high degree of internal validity. However, RD designs have been questioned for having only a limited degree of external validity (Imbens and Lemieux 2008).

We find significant, although non-robust, effects on employment and on the probability to take actions aimed at creating a business. Under the programs´ rationale, the mere fact of taking informed action to create a business out of an idea is considered an intermediate result.

The remaining of the paper is organized as follows. Section 2 reviews the literature on impact evaluation of this type of programs. Section 3 describes the C-Emprendedor program. Section 4 presents the data and discusses its limitations. Section 5 introduces the method for the identification of the impacts of the program. Section 6 presents the results, while Sect. 7 briefly discusses our findings in the context of the literature. Finally, Sect. 8 concludes.

Literature review

C-Emprendedor is an "organizational sponsorship" program as defined by Flynn (1993). Arguments in favor of these interventions include that sponsorship allows organizations to isolate themselves from the environment, engaging exclusively in formational and developmental activities. In addition, they facilitate relational connections and normative alignment, which are critical to the early survival of organizations (Barnett et al. 1994; Scillitoe and Chakrabarti 2010; Baum and Oliver 1991; Zimmerman and Zeitz 2002). This type of program can be thought of as attempts to mediate the relationship between new organizations and their environments, by creating a resource-munificent context through networking efforts, field-building efforts, and direct support (Amezcua et al. 2013).

Previous studies found conflicting results on the impact of "organizational sponsorship" programs (Cho and Honoratis 2014; Karlan et al. 2011). The sign and size of the impact depend mostly on outcomes of interest, target groups, and specific context of the programs. In a meta-analysis of various programs in developing countries, Cho and Honoratis (2014) suggest that the most relevant instruments to increase business performance are financing support (for women) and business training (for existing entrepreneurs). Additionally, providing a joint package of training and financing seems to be more effective for labor market activities than offering just one of these options (Chinen et al. 2018; Fiala 2013).

In light of the contrasting evidence, it seems relevant to deepen our understanding of the results of entrepreneurship support programs, to better assess the pertinence of funds disbursements associated with them. In particular, because the amount of public resources associated with the provision of direct support, networking, training, or a combination of them are different (Amezcua et al. 2013). The program under study allows us to focus on the impact of field-building efforts, neglecting the effects of cash transfers or infrastructure provisions. As far as we know, only a few studies focus on the consequences of pure business training and mentorship programs on entrepreneurship activities (Bruhn and Zia 2013; Drexler et al. 2014; De Mel et al. 2014; Premand et al. 2016).

The scarcity of pure business training programs is not the only reason behind the limitedness of reliable evidence of their impact on the economy. Scholars in the field of causal analysis often find it difficult to assess the impact of entrepreneurship programs. Indeed, program executing agencies usually prioritize reaching all potential candidates over the experimental design that would be needed for an ex-post evaluation. This is because randomly selecting treated and control individuals might have a high cost in terms of the program goals. However, while the random allocation of beneficiaries allows the construction of a counterfactual group that avoids identification bias, usually the non-experimental design of entrepreneurial support programs determines that their impact evaluation estimates can be biased. Under these circumstances, researchers apply impact evaluation techniques that minimize these biases. Some examples are the use of instrumental variables (Oosterbeek et al. 2010), difference in difference with matching techniques (Bonilla and Cancino 2011), and regression discontinuity analysis, being Butler et al. (2016) the closest to ours.

The regression discontinuity design is a quasi-experimental approach that was first described by Thistlethwaite and Campbell (1960) in the educational psychology literature, though it did not attract much attention in economics until the late 1990s when a growing number of studies relied on RD methods to estimate program effects.[2] Some scholars argue that the results from causal inferences from RD methods are potentially more credible than difference-in-differences or instrumental variables designs. In particular, Lee and Card (2008) prove that in the former, there is no need to assume that treatment variation is "as good as randomized"; instead, such randomized variation is a consequence of agents' inability to precisely control the assignment variable near the known cut-off (Lee and Lemieux 2010). Similarly, Hahn et al. (2001) formally recognized that the RD analysis requires mild assumptions compared to those needed for other non-experimental approaches. Finally, the regression discontinuity analysis does not rely upon matching to equate experimental and control groups. Hence, it avoids the difficulties of differential regression toward-the-mean effects, and incomplete matching due to failure to identify, and includes all relevant previous characteristics in the matching process.

A potential limitation of the RD analysis is its lack of external validity, meaning that the estimate of the treatment effect is only applicable to the sub-population of individuals near the discontinuity threshold. However, Lee and Lemieux (2010) suggest that, depending on the context, this may be an overly simplistic and pessimistic assessment. Instead, the RD estimate can be interpreted according to the authors as a weighted average treatment effect, where the weights are the relative ex ante probability that the value of an individual's score variable will be in the neighborhood of the threshold.

To our knowledge, there are only a few impact evaluations similar to ours in the use of discontinuity regression analysis to estimate the effect of entrepreneurship programs. Nevertheless, they all analyze programs that—unlike ours—give financial support to all or some of the beneficiaries: angel groups funding and consultancy services (Kerr et al. 2014), training and technical assistance with money grants (Butler et al. 2016; Klinger and Schundeln 2007).[3] The former finds positive effects on survival, employment, patenting, and financing. The latter argues that there is evidence of an increase in the probability of setting up a new business and the probability of survival, but no effects on sales or income.

Our study is different from previous work in that the main objective of entrepreneurs that postulate to the program is solely to get training and mentorship to start or expand an enterprise. One would expect that a program such as C-Emprendedor would have a smaller impact on the outcome variables (employment, business creation, investments, sales, etc.) than programs that also offer some sort of financing support.

[2] See Van Der Klaauw (2008), Imbens and Lemieux (2008) for recent surveys, and Cook (2008) for a historical perspective on RD designs.

[3] Argentina, El Salvador, Nicaragua, and Guatemala.

The C-Emprendedor program

Uruguay is far from being characterized as an entrepreneur's country. Uruguay´s score in the indicator of "Entrepreneurship as a good career choice" of the Global Entrepreneurship Monitor (GEM) falls in the three bottom scores among South American countries. The lack of entrepreneurial culture has been highlighted in several diagnostic studies (e.g., Aboal et al. 2016).

In this context, the C-Emprendedor initiative was created to promote entrepreneurial culture in the country. The general objective of the C-Emprendedor program is to support the creation and development of new ventures, mentoring entrepreneurs to transform their ideas into viable businesses. The lack of market knowledge can lead potential entrepreneurs to fail too soon for entering markets with an immature product, or in the wrong timing. This can lead the entrepreneur to quit a potentially good project. The lack of managerial skills and experience could also prevent individuals with good ideas to materialize them into a feasible project for a new product or service. The program goal is then to help individuals develop a feasible business. In the long run, the actions taken by C-Emprendedor would have an impact on the rate of entrepreneurship, higher survival rates of new ventures, and an increase in employment.

The institution in charge of this program[4] has been working to promote entrepreneurship in Uruguay since 2006, based on a methodology developed by the United Nations Industrial Development Organization (UNIDO) and implemented in several countries. This methodology was the origin of C-Emprendedor. This program seeks to promote the entrepreneurial culture and the development of ideas in commercial ventures. The basic idea is to select individuals based on their entrepreneurial talent and the potentiality of their projects. With this purpose, program officials interview individuals interested in participating in the program.

Participation in the program starts with an invitation to participate and the selection of those entrepreneurs/ideas that meet a certain profile. The selection is carried out by the Selection Committee, composed of the technical team of C-Emprendedor, local government agents, as well as representatives of business organizations. The order of preference among applicants is established according to a pre-defined evaluation criterion based on three dimensions: the entrepreneurial potential, the business idea, and the degree of progress. The program considers the total score obtained in the evaluation and in the interview (the maximum score is 18 points). Based on the above, the Committee resolves by consensus the beneficiaries of the program.

In cases in which the profile of the entrepreneur and/or ideas does not qualify as recipients of the integral program, they are recommended to participate in validation ideas workshops to deepen their analysis and validation in the market and to encourage the definition of a sustainable business model.

For selected entrepreneurs, the program provides training, technical assistance, and follow-up of the entrepreneur until the development of her business ideas. After

[4] Dinapyme, which is a department for SME support within the Ministry of Industry, Energy and Mining.

Goals	Logical framework				Long term objectivs
	Actions	Product (inmediat result)	Result	Impact	
Contribute to revert the low rate of entrepreneurship	Workshops to promote the entrepreneurial attitude / calls to present ideas	Assistance from a significant number of potential entrepreneurs	Critical mass of projects to support in later instances of the program	Contribute to the promotion of greater offer of entrepreneurial ideas	Increase the rate of entrepreneurship in the country Higher survival rate of ventures Increase employment
Decrease the restrictions that prevent good ideas from becoming commercial projects	Selection of entrepreneurs whose ideas have the potential to become successful companies (form evaluation + interview)	Potential entrepreneurs are selected to participate in the integral program. Entrepreneurs with immature ideas are derived to an ideas validation workshop	Entrepreneurs with high potential enter the comprehensive support program. Program resources focused on companies with potential		
Decrease the factors that restrict the realization of projects in new ventures	Training and support in setting up the action plan	Entrepreneurs with an action plan evaluated to carry out their entrepreneurship (defined business model and target audience). Draft of business plan	Entrepreneur takes actions to evaluate the market, adjust his business model if necessary, or give up if it is unfeasible after the analysis.		
	Advice on Business Planning	Entrepreneur makes a business plan	The business plan facilitates access to financing if necessary. The entrepreneur has an action guide to start his project	Entrepreneur takes actions to create a company.	
Decrease the restrictions that hinder the consolidation of enterprises in formal and competitive companies	Mentoring in action plan Accompaniment in search of financing / investors Accompaniment in practical difficulties	Companies implement training and mentoring apprenticeships	The entrepreneur makes the necessary investments to develop his business	The project takes shape in a formalized company Sales revenue is created Paid wages (generates employment)	

Fig. 1 Logical framework of the C-Emprendedor program

completing each stage of the program, the management team of C-Emprendedor coordinates a meeting with the entrepreneur to evaluate what was done in the previous stage and decide, eventually, to move to the next stage. Usually, the support lasts between 1 and 2 years. The entrepreneurs do not receive any type of subsidy and/or aid from the program during or after the treatment ends.

One of the program core assumptions is that for good projects not to fail when becoming ventures, entrepreneurs need to incorporate business management practices, planning, and market analysis, and to define a business model (draw a roadmap, set goals and deadlines, etc.). The program provides entrepreneurs with training to generate a business plan and skills to develop an action plan which includes marketing, legal, and financial strategies. Results of these actions include for entrepreneurs to check market conditions, adjust their business model if necessary, and acquire funding if needed. With the action guides, she can start taking action to create a company, which is the expected result of the program.

Even with a good planning and previous market analysis, the first months in the life of new ventures are challenging and it is very often than firms die before consolidating (between years 0 and 2). To decrease the impact of the obstacles that affect firms in this period, the program provides a group of specialists in charge of mentoring the new venture, providing accompaniment in the search of financing, or other practical difficulties. Making the necessary changes, taking informed decisions, making investments, etc., would supposedly increase the probability that the company survives this first period, increase sales revenue, hire personnel, and formalizes the Company (Fig. 1).

Data

We use data from two sources: administrative files provided by MIEM and data obtained from a survey created by our team. The survey was delivered to all individuals that ever signed up for the program (treated and non-treated). In both cases, data cover the period 2009–2015.

Table 1 Participants in successive stages of C-Emprendedor

Year of call	Postulated	Accepted into the validation ideas workshop (a)	Accepted into the full program (b)	Assisted in at least one stage of training and mentorship (c)	Assistance rate (b/c)
2009	128	48	50	48	96%
2010	296	92	87	78	90%
2011	332	124	108	89	82%
2012	327	101	102	87	85%
2013	405	134	131	122	93%
2014	489	139	155	138	89%
2015	448	118	91	71	78%
Total	2.420	756	724	633	89%

Source: Program´s administrative records

Administrative data provided by MIEM include sociodemographic information of all potential entrepreneurs that postulated to the program. This information was registered when application forms were filled by individuals at the time of signing up for the program. Sociodemographic information includes national ID number, name, gender, date of birth, highest formal education attainment, present occupational condition, previous experience as a business owner or employee, address, contact information (phone and/or email), and a brief description of the business idea.

A second administrative database contains the names and scores of both the entrepreneurial profile test (3 points) and the evaluation interview (composed of 5 sections, each one with a maximum of 3 points). This database also includes the decision taken by the evaluation committee (to be allowed into the program or not). Note that the maximum number of points one individual could earn was 18, and the cut-off to enter the program was arbitrarily set at 11.

The third database includes follow-up information about the performance of treated individuals only, during their transit through the different stages of the program. Follow-up information describes the step-by-step progress of individuals and if they eventually created a business (see Table 1). Based on this database, one can identify when an individual did not successfully complete a stage.

The administrative information contains sociodemographic and evaluation information about treated and non-treated individuals, and performance information about treated individuals only. This database includes 2,420 observations.

The second source of data is a survey sent to all individuals for whom we had a unique valid email account: 2305 individuals. Each questionnaire referred to the year that the individual signed up for the program, to be able to construct a baseline corresponding to information of every potential entrepreneur at the same point in time (whenever they applied for the program´s activities). This implied constructing seven different surveys, one for each sign-up year.

The survey contains a total of 65 questions, distributed in six parts: individual characteristics; actions taken to create a business; support from other

Table 2 Means of variables at the time of registration. (applicants and surveyed individuals)

Variable	Mean	
	All applicants (administrative data)	Surveyed individuals (survey data)
Women	53%	57%
Age	35	35
0–6 years of education	4%	3%
7–12 years of education	54%	47%
> 12 years of education	43%	50%
Non partof the labor force	11%	7%
Unemployed	8%	6%
Employed	82%	87%
Live in the capital of the country	57%	67%

Source: Program´s administrative records

entrepreneurship programs; characteristics of the business; personal traits and motivations; and characteristics of the program itself.

Out of 2,305 emails sent, we received 555 answers, a 24% response rate. This response rate is along the lines of the ones obtained by similar exercises conducted by Aboal et al. (2016) and Butler et al. (2016). After deleting 2 outliers, we merged survey information for 553 individuals (213 treated and 340 non-treated) into the administrative base to create our panel data. Table 2 shows only small differences in means of variables for the entire population of applicants and surveyed individuals.

Methodology

We claim that the RDD is a valid approach to evaluate the impact of the C-Emprendedor program in variables that measure the performance of entrepreneurs. This claim is based on the design of the C-Emprendedor program: participation in the program is partially determined by the score assigned to each potential participant (our forcing variable) and there is an exogenous cut-off point in the distribution of that score that is not subject to manipulation by potential participants.[5] The score works in practice as an important guide for the decision to be admitted into the program. Although a score of 11 is considered to be acceptable to receive the benefits of the C-Emprendedor training and mentorship, in practice, the limits are fuzzy around this cut-off value.

[5] The selection process evaluates six dimensions, using a standardized questionnaire and individual interviews with the potential entrepreneur. Each dimension receives a score between 0 and 3, so that the total score ranges from 0 to 18. To conclude the evaluation process, a selection committee composed of three representatives decides whether individuals will be admitted or not into the program.

It is evident that the decision to include an individual in the program is not random, and that there could be systematic differences between treated and non-treated individuals. The regression discontinuity method exploits the (almost) deterministic discontinuity in the forcing variable to identify the impact of the program. It relies on the fact that the admission into the program in an interval of the cut-off values can be considered random, since it is impossible to implement a criterion that offers no mistakes while admitting or not individuals into the program in that interval. In other words, treated and non-treated individuals whose scores belong to a relatively small interval centered in 11 are very similar, and their admittance into the program can be considered random. Consequently, differences in result variables between these two groups, under some assumptions, can be attributed to the program.

When implementing the RD analysis, it is important to determine the relationship between the forcing variable $\left(Z_i\right)$ and participation in the program. When there is a deterministic relationship, in such a way that participation in the program is fully explained by Z_i, the procedure for carrying out the evaluation is that of a "sharp" discontinuous regression. These cases are easily identifiable by means of a graphical analysis of the forcing variable against participation in the program. In such cases, a discontinuous jump is observed around the cut-off of the forcing variable (referred to as $\bar{Z}$), while the probability of participation is 0 and 1 on one side and the other of the threshold, respectively. This implies that all individuals who meet the allocation condition, and none of those who do not, receive the treatment.

There are cases, however, in which the value of the forcing variable not entirely explains the participation in the program. In such cases, we are dealing with a "fuzzy" RD design, as it is the example under study in this paper. The probability of participating in the program is closer to 1 to the right of $\bar{Z} = 11$, and closer to zero to the left of $\bar{Z} = 11$.

For the fuzzy RD design to be a valid strategy to estimate the impact of the treatment, two assumptions must be met. First, the assumption of local continuity requires that there are no large differences in terms of the characteristics that affect the outcome variable between individuals on either side of the threshold. Graphically, it is expected that when analyzing the relationship between some observable characteristics (e.g., educational level), we should not find evidence of large discontinuities around $\bar{Z}$. This shows that there is a balance in terms of the observable characteristics between groups on both sides of the threshold, so that the group to the left of the threshold constitutes a good counterfactual of the group to the right. Second, it is expected that both individuals and program administrators cannot manipulate the value of Z_i or the threshold $\bar{Z}$ in such a way as to affect the eligibility of potential beneficiaries. This can be noticed by looking at the density function of Z_i and finding some discontinuity in that density in the environment of $\bar{Z}$. We formally test the latter assumption and we provide evidence in favor of the former in the appendix.

When implementing the fuzzy RD design, it is possible to estimate the effect of the program either using a parametric or a non-parametric strategy. The first option involves making assumptions about the underlying relationship between the outcome variable, Y_i (for example employment) and the forcing variable, Z_i. In its simplest version, the parametric method assumes a linear relationship between Y_i and Z_i.However, it is also common to assume nonlinear relationships between the outcome and forcing variables. In general, within parametric specifications, it is usual to initially follow a linear approach and later move to nonlinear relationships, even including interactions between the treatment dummy D_i (which takes the value 1 if the individual is treated, and zero otherwise), and the forcing variable Z_i. Following Lee and Lemieux (2010), it is advisable to check the robustness of the results along this process.

The non-parametric approach (Hahn et al. 2001) is more general, as it does not impose assumptions about the underlying relationship between Y_i and Z_i, thereby reducing the potential bias induced by my misspecification.

According to our strategy (fuzzy RD design), we have a relevant instrument (being above or below $\bar{Z} = 11$) for the treatment variable $\left(D_i\right)$. Being above or below $\bar{Z}$, close to this threshold, is a fortuitous fact, so the instrument is exogenous.

The estimation consists of performing a non-parametric regression of Y_i on Z_i, and another of D_i on Z_i on each side of the threshold. Thereby, the average of the predicted values for both variables on one side and the other of the threshold is used to estimate the impact of the program, according to

$$\tau = \frac{\overline{\hat{Y}\left(\bar{Z}^{+}\right)} - \overline{\hat{Y}\left(\bar{Z}^{-}\right)}}{\overline{\widehat{Pr}\left(D = 1|\bar{Z}^{+}\right)} - \overline{\widehat{Pr}\left(D = 1|\bar{Z}^{-}\right)}}, \tag{1}$$

where τ is the impact of the program on outcome variable Y (i.e., employment), the numerator $\overline{\hat{Y}\left(\bar{Z}^{+}\right)} - \overline{\hat{Y}\left(\bar{Z}^{-}\right)}$ is the estimated difference on the outcome variable over each side of the threshold, and the denominator $\overline{\widehat{Pr}\left(D = 1|\bar{Z}^{+}\right)} - \overline{\widehat{Pr}\left(D = 1|\bar{Z}^{-}\right)}$ is the estimated difference in the probability of participation in the program over each side of the threshold.

The estimates presented below measure the impact on the beneficiaries of some instances of the complete program with respect to those who were not beneficiaries of any component of the full program or the idea validation workshops. This impact is measured in the second year after enrollment in the program.

Our strategy can be implemented within plausible assumptions, giving the method acceptable internal validity. However, there are some limitations regarding the external validity of the method. As it is the case when using instrumental variables, the fuzzy RD design estimator is of a local type. This means that the identified impact is attributable only to individuals who are around the threshold: those who have scored in an interval of 11 in the case of the C-Emprendedor program.

Results

Results presented in this section were estimated using the Stata rd routine proposed by Nichols (2011). This is a case of a fuzzy discontinuity regression, as the probability of entering the program does not jump from 0 to 1 at 11 (see Fig. 2). Since estimation results are sensitive to the bandwidth used (and therefore to the number of observations considered), we conducted estimations for many bandwidths. In general, fuzzy discontinuity regression exercises require considerable bandwidth, as

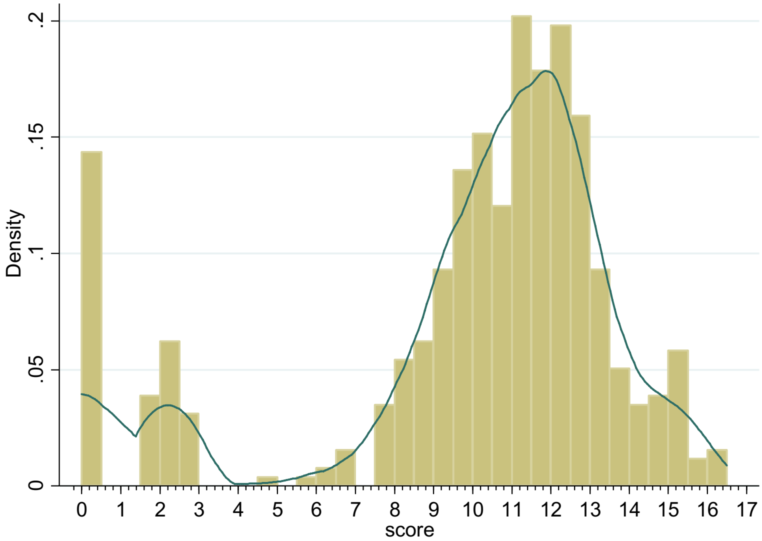


Fig. 2 Density of the Score (Continuous Line: Kernel Epanechnikov)

in our case. An increase in the bandwidth reduces the variance of the estimators (by considering more observations) at the cost of increasing potential biases (using more heterogeneous individuals).

We estimate a non-parametric local kernel regression at both sides of the cut-off (11 points) of the forcing variable. We use a polynomial of order 1. Estimations can be sensitive to the weights of the observations as we move further away from the cut-off point. We use Gaussian weights, or a Gaussian kernel, implying that observations closer to 11 receive a higher weight than those that are further away.

Optimal bandwidth is computed resorting to the cross-validation method proposed by Ludwig and Miller (2007). Noting that in fuzzy discontinuity regression exercises, we must perform two regressions, we report optimal bandwidths for both (one for the outcome variable, penultimate column in Tables 4, 5, 6, 7, and one for the probability of participation into the program, last column in Tables 4, 5, 6, 7). In general, our optimal bandwidth is 5.

In Fig. 2, we show the density function of the score variable. The identification strategy relies on the assumption that there is no jump in the density at the score 11. We have formally tested this hypothesis using the rd density command proposed by Cattaneo et al. (2018) and exhibit our results in Table 3. Results depend on the bandwidth and kernel function chosen. In 9 out of 15 alternative specifications, we cannot reject the null of continuity of the density function. In the Appendix, we show evidence supporting the assumption of continuity of exogenous variables prior to the intervention (see Graphs A.1 to A.11).

Figure 3 shows the probability of participation in the program. As we expect, there is a jump in the probability of participation at the cut-off. In the fuzzy RD design, the probability of receiving the treatment needs not to change from zero to

Table 3 Continuity of the score density function around the cut-off point

Bandwidth	Kernel function					
	Epanechnikov		Uniform		Triangular	
	T	p value	T	p value	T	p value
1	1.216	0.224	0.799	0.425	1.245	0.213
2	2.413	0.016	2.384	0.017	2.409	0.016
3	1.672	0.095	1.165	0.244	1.854	0.064
4	1.345	0.179	1.414	0.157	1.473	0.141
5	1.608	0.108	1.947	0.052	1.615	0.106

Jackknife standard errors; density estimation: unrestricted

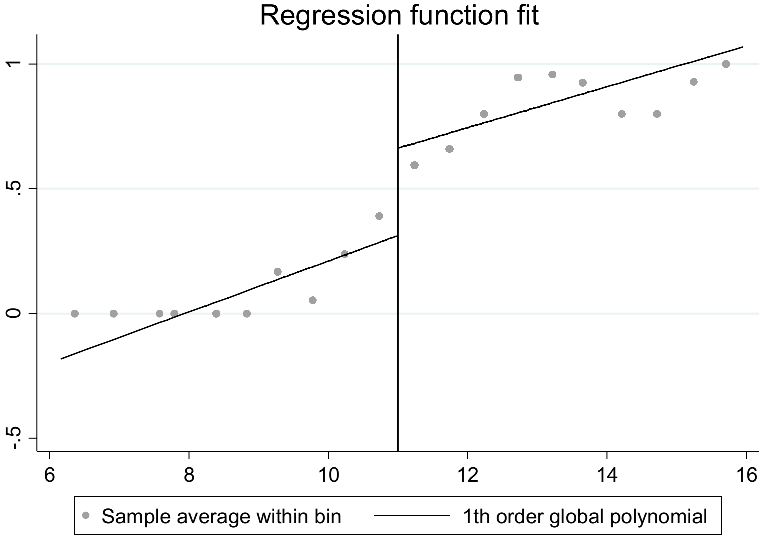


Fig. 3 Probability of participation

one at the threshold. Instead, the design allows for a smaller jump in the probability of treatment assignment at the threshold.

We estimate the impact on beneficiaries of the program (of any stage of it) with respect to those non-beneficiaries of any stage of the program or the ideas validation workshop. We measure the effects of the program 2 years after the individual signed up for the program. In this design, we interpret the ratio of the jump in the regression of the outcome on the covariate to the jump in the regression of the treatment indicator on the covariate as an average causal effect of the treatment (Imbens and Lemieux 2008), as shown in Eq. (1).

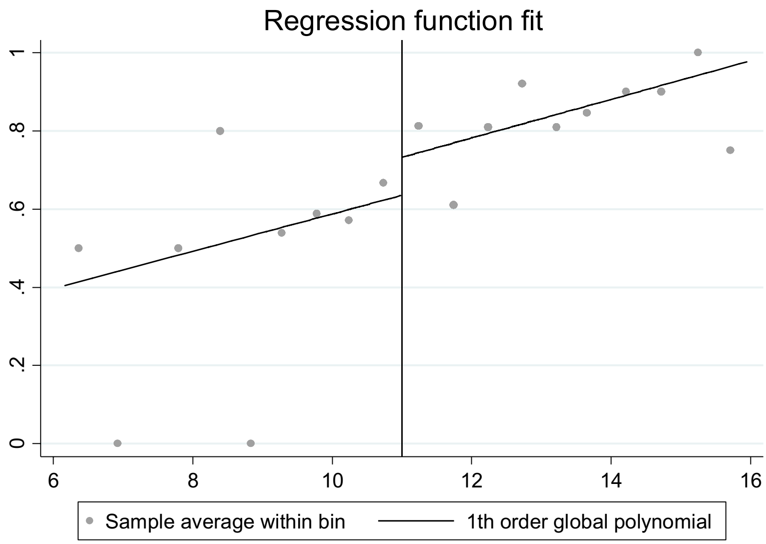


Fig. 4 Activities to startup a business up to year 2. Average of the interval, 10 intervals on each side of the cut-off point; polynomial regression of order 1. **Source**: own estimation

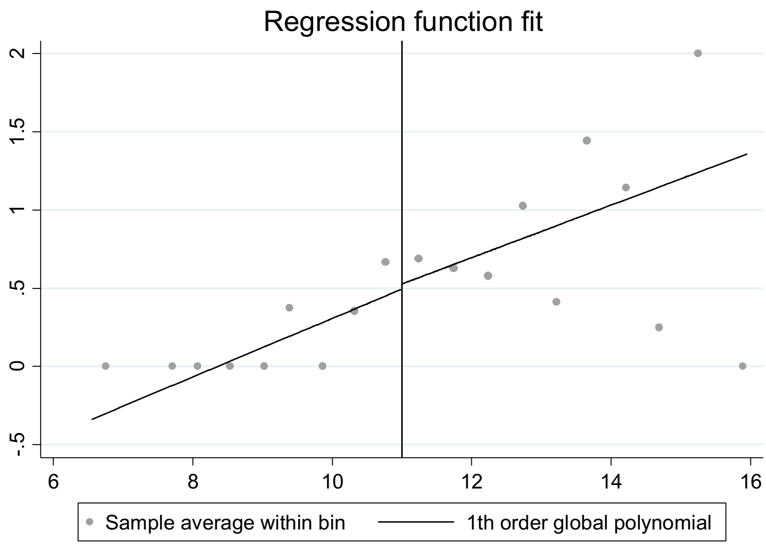
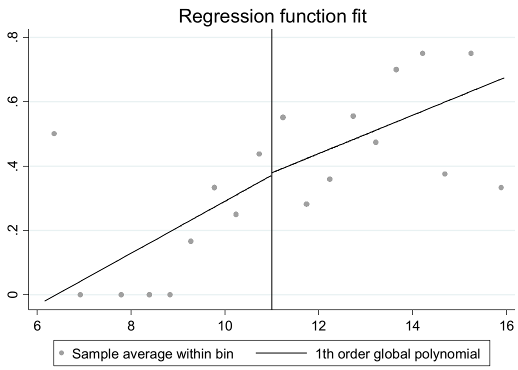


Fig. 5 Total employees in year 2. Note: average of the interval, 10 intervals on each side of the cut-off point; polynomial regression of order 1. **Source**: own estimation

Figures 4, 5, 6 show the fit of a first-order polynomial to our data on beneficiaries (to the right of the cut-off point) and non-beneficiaries (to the left of the cut-off point) of the C-Emprendedor program. Dots represent the average value of the



Note: average of the interval, 10 intervals on each side of the cutoff point; polynomial regression of order 1.

Fig. 6 Invested up to year 2. Average of the interval, 10 intervals on each side of the cut-off point; polynomial regression of order 1. **Source**: own estimation

Table 4 Impact of the program on the probability of carrying out activities to start up a business up to two years after the program

Bandwidth	Impact	SD	t-stat	Optimal bandwidth cross-validation criteria	
				Activities to startup	Probability
1	0.66	60.15	0.01	0.213	0.234
2	0.36	0.80	0.45	0.198	0.228
3	0.34	0.31	1.11	0.189	0.209
4	0.38	0.24	1.56	0.185	0.185
5	0.40	0.16	2.43	**0.182**	**0.174**

Number of observations between 107 and 307, depending on the bandwidth. Stata rd routine proposed by Nichols (2011). Gaussian weights, Bold identifies the lowest levels

Table 5 Impact of the program on employment in year 2

Bandwidth	Impact	SD	t-stat	Optimal bandwidth cross-validation criteria	
				Employment	Probability
1	−1,05	37,21	−0,03	1,402	0,234
2	−0,04	12,49	0	1,733	0,228
3	0,42	0,78	0,53	1,609	0,209
4	0,69	0,55	1,25	1,522	0,185
5	0,76	0,41	1,83	**1,685**	**0,174**

Number of observations between 107 and 307, depending on the bandwidth. Stata rd routine proposed by Nichols (2011). Gaussian weights, Bold identifies the lowest levels

Table 6 Impact of the program on the probability of carrying out investments in the first 2 years of the program

Bandwidth	Impact	SD	t-stat	Optimal bandwidth Cross-validation criteria	
				Invested	Probability
1	0.11	22.26	0.00	0.280	0.234
2	−0.21	2.49	−0.08	0.245	0.228
3	0.05	0.25	0.20	0.250	0.209
4	0.20	0.24	0.82	0.247	0.185
5	0.23	0.23	1.01	**0.245**	**0.174**

Number of observations between 107 and 307, depending on the bandwidth. Stata rd routine proposed by Nichols (2011). Gaussian weights, Bold identifies the lowest levels

Table 7 Impact of the program on the probability of not paying wages until year 2 (a proxy of not opening a business)

Bandwidth	Impact	SD	t-stat	Optimal bandwidth Cross-validation criteria	
				Did not paid wages	Probability
1	0.57	4.54	0.13	0.272	0.234
2	0.67	2.00	0.33	0.263	0.228
3	0.28	0.30	0.95	0.271	0.209
4	0.05	0.23	0.23	0.268	0.185
5	0.00	0.20	0.01	**0.262**	**0.174**

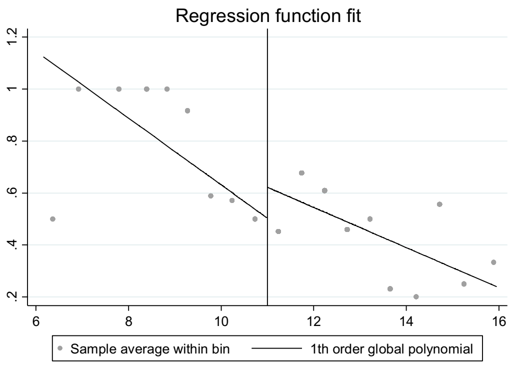
Number of observations between 107 and 307, depending on the bandwidth. Stata rd routine proposed by Nichols (2011). Gaussian weights, Bold identifies the lowest levels

outcome variable for each of the 10 bins used to either side of the cut-off.[6] The fitting of higher order polynomials did not significantly alter our results.[7] As Eq. (1) states, we estimate the effect of the program as the jump in the intercept of the linear functions (the numerator), properly scaled by the probability of participation in the program (the denominator). Intuitively, the denominator would be one in a sharp RD design, and we would only consider the difference in intercepts to measure the effect of the program. Since we face a fuzzy RD design, we must account for the probability of participation in the program not being fully explained by the value of the forcing variable. Tables 4, 5, 6, 7 show the estimates of Eq. (1) following the rd routine proposed by Nichols (2011). We find significant effects on the probability of actions undertaken to set up a business (Table 4) and on employment (Table 7).

Figure 4 shows the fit of linear functions to the probability that entrepreneurs conduct an activity that leads to startup a business up to 2 years after the program,

[6] We use the rdplot command for Stata proposed by Calonico et al. (2014) to create these graphs.

[7] Graphs and results from the fit of higher order polynomials are available upon request from the authors.



Note: average of the interval, 10 intervals on each side of the cutoff point; polynomial regression of order 1.

Fig. 7 Did not pay salaries up to year 2. Average of the interval, 10 intervals on each side of the cut-off point; polynomial regression of order 1. **Source**: own estimation

including the search for equipment, organizing a work team, looking for funding, etc. In Table 4, we report a significant effect of the program in this outcome variable. Participation in the program entails 40 percentage points more in the probability of actions undertaken to set up a business.

Figure 5 represents the fitting of a first-order polynomial to the total number of employees per business in year 2 after the treatment. We do not observe a large difference in intercepts in the graph. Nonetheless, the estimated effect of the C-Emprendedor is small but significant (see Table 5). Beneficiaries create on average 3 jobs more per every 4 entrepreneurs (the point estimate is 0.76) as compared to non-beneficiaries.

Figure 6 shows the linear fit of the probability of investing up to 2 years after the program. Casual observation suggests no difference in intercepts at both sides of the cut-off, which is consistent with the finding of a non-significant program effect in Table 6.

We choose a proxy variable to evaluate business creation: the probability of paying salaries up to year 2 after the program. We do not find a significant effect on the creation of a business. In fact, even though Fig. 7 shows a difference in intercepts of linear functions at each side of the cut-off point, once we take into consideration the effect of the denominator in Eq. (1), the effect of the program is not significant (see Table 7).

As a robustness check, we estimate the effects assuming Triangular weights (a Triangular kernel). In this case, we cannot reject the null of no effect for all four outcome variables (results are available upon request). Therefore, our results are not robust to alternative specifications of our estimation method.

Discussion

We find significant although non-robust effects on the actions to start a business and on employment, but no effects are identified on the other outcome variables. We conjecture that this result is driven by the program and its beneficiaries' characteristics. C-Emprendedor provides only mentorship and training support to beneficiaries that are mostly subsistence entrepreneurs. Our results are in line with some of the findings of the previous empirical literature on the effects of entrepreneurial training and mentorship programs.

In general, the evaluation of this type of program finds effects on outcome variables like business practices, investment activities (Bruhn and Zia 2013; De Mel et al. 2014), and the improvement of financial practices (Drexler et al. 2014). However, effects on sales, business creation, and profits are negligible. Premand et al. (2016) find a small increase in self-employment, but no effect on overall employment is identified.

Chakravarty et al. (2017) study the impact of an employment training program in Nepal. Authors find no effect on self-employment activities, while plans for starting a business in the future decrease strongly, when evaluating the impact of the program on subsistence entrepreneurs. However, authors find positive effects of training on self-employment among transformative entrepreneurs. Among the latter, training provision increases the likelihood of self-employment by 0.21, equivalently to a 50% increase in self-employment with respect to the baseline average.

Another rationale for our results could be related to the fact that training and mentorship support is not combined with financial support. Klinger and Schundeln (2007) separate the effect of pure training from that of mentorship and financing when evaluating an entrepreneurial program in Central America. Authors find that receiving business training increases the probability that applicants start a new business or expand an existing business, when this action is complemented with financial support.

In a similar vein, Fiala (2013), evaluating a training program implemented by the International Labor Organization (ILO) in Uganda in 2012 and directed to microenterprise owners, concludes that the joint provision of loans and training is the most effective combination for men in the short run.

Estimating the effects of a Chilean public financial and training program designed for microenterprises, Bonilla and Cancino (2011) find a positive and significant effect on the number of workers hired, and positive albeit not always significant effects on sales. We interpret these findings as evidence that the joint provision of training and financing is more conducive to significant results.

Butler et al. (2016) analyze the Buenos Aires Emprende (BAE) program in Argentina, a program in which beneficiaries must be sponsored by selected NGOs, Universities, and institutions focused on entrepreneurship support. Butler et al. (2016) find evidence of positive and significant effects of the BAE program on the probability of setting up a new business and the probability of survival but no effects on sales or income. This could be another dimension that explains our results, given the lack of sponsorship of beneficiaries in the C-Emprendedor program. There are at least two ways in which sponsorship could help beneficiaries: first, by lowering barriers to entry related to information asymmetries or managerial skills, and second because sponsor institutions might select stronger candidates.

Conclusions

In this paper, we provide empirical evidence based on a rigorous evaluation of the effects of a particular entrepreneurship program (only training and mentorship), and we contribute to the better design of public policies aimed at supporting entrepreneurial activities. By comparing the effects of C-Emprendedor with other programs designed to support entrepreneurs, we contribute to the debate about the optimal mix of instruments included in an entrepreneurship program.

C-Emprendedor is designed to provide some training in business practices and mentorship to either entrepreneur that have a business idea, or to micro- and small enterprises in the process of creating a new line of business. In all cases, these are not innovative entrepreneurs, who are more directly targeted by other programs in Uruguay.

In the baseline estimation, we find significant non-robust (to alternative estimations methods) effects on the probability of actions undertaken to set up a business and on employment. In addition, we find no significant effects on the creation of business or on the probability of investing.

We interpret these findings in light of the existing empirical literature results. We conclude that there are a few reasons, mostly related to the design of the C-Emprendedor program, as to why we find effects on employment and the actions to set up a business but not in other outcome variables. First, the C-Emprendedor program is an instrument that provides training and mentorship only. The empirical literature suggests that adding financial aid of any type that helps to overcome financial constraints is associated with higher business creation, investment, survival rates, etc. Second, C-Emprendedor supports mostly subsistence entrepreneurs. Third, beneficiaries of the C-Emprendedor program are not sponsored by institutions that provide support to entrepreneurs. A sponsor institution can help entrepreneurs overcome financial constraints and improve their managerial skills.

Given the little or no effects found on this paper, and the available international empirical evidence, one possible corollary is that training and mentorship programs seem to have an impact on attitudes towards entrepreneurship only. However, positive this can be for the entrepreneurial ecosystem; it does not seem to be enough in terms of significant economic impact.

As the empirical international evidence suggests, a more promising avenue could be the adoption of programs that combine both training and mentorship instruments with some sort of financial support (i.e., grants, prizes, funding, etc.) to have a relevant impact on firms' performance.

An alternative way of improving the design of training and mentorship programs is to reinforce the mentoring aspect by either extending the period of the mentorship or by helping the entrepreneurs to connect with other specialized entrepreneurial support institutions.

Finally, funds allocated to programs like C-Emprendedor could be better used by targeting a smaller number of entrepreneurs, to have a larger impact. We interpret the relative per entrepreneur small amount of funds allocated (around USD 1.400) as a possible shortcoming of the program.

Future research should address the short- and long-run differential effects of training and mentorship programs. Our study is a short-run evaluation, considering the impact up to 2 years after the program. Given that our methodology weighs more heavily on individuals that are closer to the cut-off point, it has limited external validity.

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Data availability The datasets generated during and/or analyzed during the current study are not publicly available, since they include information that can uniquely identify individuals (i.e., it contains national ID numbers), but are available from the corresponding author on reasonable request.

Code availability Code available upon request to corresponding author.

Declarations

Conflict of interest On behalf of all authors, the corresponding author states that there is no conflict of interest.

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