Public funding for startups in Argentina: an impact evaluation

Inés Butler
IERAL

Gabriela Galassi
European University Institute

Hernán Ruffo[1]
UTDT

October 21, 2015

[1]We would like to thank Pablo Brassiolo and Pablo Sanguinetti for their comments. We also gratefully acknowledge the work of Guadalupe González and the research assistance of Augusto Terracini and Martín Clause on previous versions of this paper. A thank you is also owed to Gustavo Svarzman and Analía Avella from the Subsecretaría de Desarrollo Económico del Gobierno de la Ciudad de Buenos Aires for providing data and further insight into the program. The authors acknowledge support from the CAF program on "Productivity and Misallocation of Entrepreneurial Talent in Latin America." The opinions expressed are those of the authors and not those of CAF. Hernán Ruffo gratefully acknowledges the financial of FONCyT (PICT/2588-2010). Postal address: Universidad Torcuato Di Tella, Av. Figueroa Alcorta 7350, (C1428BCW), Buenos Aires, Argentina. E-mail: hruffo@utdt.edu.

Abstract

A broad literature has found that the misallocation of entrepreneurial talent has strong effects on productivity. To investigate whether the government can improve entrepreneurial activity, we analyze a policy aimed at promoting innovative startups through the provision of funding and technical assistance to potential entrepreneurs in Buenos Aires, Argentina. We conduct a survey and use regression discontinuity methods to identify the effects of the policy. We find significant effects on enterprise creation and survival as well as on employment. Overall, we show that small-scale public policy can help entrepreneurs overcome a wide variety of barriers to firm entry and improve the allocation of their entrepreneurial talent.

Keywords: Innovation; Entry; Exit; Cash grants; Impact evaluation; Argentina

JEL classification: C31, O31, H25

1 Introduction

The misallocation of entrepreneurial talent and distortions to firm entry are the main driving forces behind productivity differences across countries (Hopenhayn 2014). This misallocation problem arises when entrepreneurial talent is not fully exploited into firms. As a consequence, few or low-quality firms are created.

The problem of the misallocation of entrepreneurial talent is related to the occupational choice problem of a potential entrepreneur. Consider an individual faced with the occupational choice between being a wage earner and being an entrepreneur. The decision of this agent depends on the income and costs of each alternative, including the costs of and barriers to firm entry. Examples of these include (i) entry costs and regulations (Djankov et al. 2002; Feldman 2014), (ii) the opportunity cost related to not working for a wage (as in Lucas (1978) and many others), (iii) financial frictions (Banerjee and Newman 1993; Buera, Kaboski, and Shin 2011), and (iv) the lack of specific abilities (von Graevenitz et al. 2010; Kim et al. 2006) or limitations to managerial abilities and information that reduce expected profits, as shown in Bloom et al. (2013).

These obstacles, which interact to prevent the creation of firms and distort the decisions of truly able entrepreneurs, can affect not only the quantity of new firms but also their quality. For example, under financial constraints, if wealth is in the hands of less able entrepreneurs, then new firms would be of lower quality (Erosa and Allub 2012; Banerjee and Newman 1993). Additionally, new firms could be oriented to less capital-intensive and less productive sectors because of these constraints, as in Buera, Kaboski, and Shin (2011), or continue to operate informally because of the higher costs of entry in the formal sector (D'Erasmo and Moscoso Boedo 2012). These barriers can also lower survival rates (Midrigan and Xu 2014). Young firms are typically constrained by the amount able to be borrowed, while reduced information and managerial skills could also affect survival probability (Mano, Iddrisu, Yoshino, and Sonobe 2012).

Previous studies suggest that the misallocation of entrepreneurial talent through these channels is crucial for quantifying productivity differences between countries. Indeed, it has been reported that correcting this mismatch can improve the total factor productivity of less developed countries by as much as 25% (Hopenhayn 2014).

In this context, the government plays an important role in reducing costs and barriers as well as in improving the allocation of entrepreneurial talent. One natural intervention would be to improve institutions and reduce financial frictions. Other interventions that are gaining importance include new programs that promote entrepreneurship. Several recent government interventions, including "Venture Capital Limited Partnership" in Australia, "Yozma" in Israel, "Startup Chile," and "Innpulsa Colombia," have been justified as ways in which to help entrepreneurs overcome these hurdles and encourage startups, with the final objective being economy-stimulating innovation. Nevertheless, this type of public policy, if badly designed, could generate another source of misallocation if it favors more inefficient firms (Buera et al. 2013) or introduce distortions to firm growth (Restuc-

cia and Rogerson 2008; Guner et al. 2008). Indicators of these unintended effects would be the relatively poor performance of supported firms in the form of lower productivity, slower growth, and lower survival after intervention.

In this paper, we analyze a government program that aims to promote new innovative firms. This program, *"Buenos Aires Emprende" (BAE)*, provides funding (about US$ 16,000 per new firm) and technical assistance to entrepreneurs of startup companies. In particular, technical assistance is provided by specialized entrepreneurial support institutions, including a mentorship that can last up to a year. These two instruments are expected to lower barriers to firm entry, including opportunity and fixed costs, and address financial constraints and the lack of information or managerial skills.

To identify the impact of this policy, we exploit the particularities of the selection process. Each application to the program receives a score according to the applicant's entrepreneurial ability and potential enterprise's economic viability; those that are above a cutoff are selected as beneficiaries. This process allows us to use regression discontinuity methods, modeling the score as a continuous variable and approval as depending on a deterministic rule that defines selection into treatment. This element is essential as it allows us to estimate treatment effects by eliminating selection bias.

We conducted a survey among applicants, including both beneficiaries and non-beneficiaries, and collected information about firm creation, survival, and other outcomes including employment, sales, and net earnings. By analyzing these data, we found that the policy has a strong and significant impact on business creation and survival as well as on employment.

An intervention such as the one studied herein could have a null effect if innovative entrepreneurs were to realize their ideas anyway, implying high deadweight losses. This is not the case for BAE. The fact that many non-beneficiaries did not start their projects implies that barriers to entry affect these individuals. The high proportion of startups with beneficiaries suggests that diverse barriers were effectively lowered by the provision of such a simple intervention.

Additionally, the effects on survival and employment after the intervention suggest that the policy is not worsening the quality of the pool of created firms. In this sense, it is not distorting the allocation of entrepreneurial talent or artificially sustaining low-productivity companies. Thus, surprisingly, the presented findings suggest that limited public money can make a relevant difference in the creation of better new firms. Our results suggest that what constitutes a strong barrier to development in the literature can be addressed by the implementation of small-scale programs.

We argue that the policy has three important characteristics that contribute to these results: (i) it focuses on identifying entrepreneurial talent; (ii) it is limited in time, lowering the barriers of entrepreneurs but not generating any permanent distortion; and (iii) it combines both transfers and mentorship.

The empirical evaluation of entrepreneurship programs is methodologically challenging because of the difficulties in identifying a valid counterfactual scenario. These programs rarely randomize their assignment into treatment, and program beneficiaries are

substantially different from other entrepreneurs. For that reason, uncontrolled differences between beneficiaries and non-beneficiaries would be a biased measure of the impact of the program. Moreover, the follow-up of both beneficiaries and other non-beneficiaries is infrequent, limiting information on the control group. These difficulties have led to a paucity of studies rigorously evaluating entrepreneurship programs (Storey 1998; Parker 2009), particularly so in developing countries (López-Acevedo and Tan 2011). Our study overcomes these difficulties by using up-to-date impact evaluation methods that exploit the discontinuity of the treatment and gather follow-up information on both beneficiaries and non-beneficiaries.

Some examples in the impact evaluation literature find that financing and transfers for small and young firms can increase profits and employment (e.g., de Mel, McKenzie, and Woodruff 2008 for Sri Lanka, McKenzie and Woodruff 2008 for Mexico, and Blattman, Fiala, and Martinez 2014 and Fiala 2014 for Uganda). Evaluations of business training programs, on the contrary, are relatively inconclusive and scarce (see McKenzie and Woodruff 2012 for a survey). Nevertheless, the joint provision of training and loans does seem to be effective according to the meta-analysis presented by Cho and Honorati (2014) (see also Fiala 2014). However, this body of knowledge focuses on interventions provided to the self-employed or to groups with working difficulties rather than on innovative entrepreneurs as in our case. Furthermore, it frequently analyzes the effects on profits but rarely on the probability of firm creation.

Other investigations have found that financing entrepreneurs affects business creation. For example, Kerr, Lerner, and Schoar (2010) uses regression discontinuity analysis to identify the effect of the financing provided to entrepreneurs by "angel" investors; the forcing variable is the interest of individuals within the investor group in financing the project. The present study is different, however, because it analyzes public policy that provides both capital and educational support. This public/private distinction is important because government interventions are frequently seen as investing in low-return strategies or providing improper incentives (see Lerner 2012 for a discussion of related public policy lessons).

Our contribution is thus to provide solid evidence that public interventions can be effective at reducing barriers and helping potential entrepreneurs allocate their talent better. In light of the literature on misallocation, our results could therefore be read as evidence that the occupational choices of potential entrepreneurs can be influenced by limited but shrewd public interventions.

The following sections describe the program to be analyzed (Section 2), the survey conducted, and the methods used to analyze the data (Section 3). We summarize our findings in Section 4 and estimate the overall impact of the program as well as discuss our results in Section 5. In Section 6, we offer some concluding remarks.

2 The BAE Program

The government of the City of Buenos Aires has implemented several policies to encourage business creation. In general, these policies aim to increase the quantity and quality of new businesses in the city by providing assistance and financing. In particular, the BAE program, on which we focus here, aims to identify and support entrepreneurs with innovative ideas.

The program design is driven by the view that the most important determinant of a startup's success is the ability of the potential entrepreneur: a truly talented entrepreneur, within a properly supportive environment, will eventually succeed in creating an innovative firm, with a potentially great impact on productivity and job creation as well as positive externalities for other firms. Nevertheless, such natural entrepreneurial talent is not easy to identify *ex ante*.

The design of the BAE program seeks to manage this constraint. Full-scale implementation of the program began in 2008, with a new round beginning each calendar year. Each annual edition is structured in several consecutive stages:

- In the first stage, NGOs, universities, and institutions focused on entrepreneurship are selected. About 10 institutions are involved in each program edition, with the task of identifying potential beneficiaries, assisting them with business plan design, and guiding them through the application process. Each institution sponsors certain projects (generally around 10), which are submitted to BAE.

- In the second stage, the program evaluates the applicants and their projects. The selection process is based on assessing two dimensions, the sum of which determines the total score:

  - "Project viability," focused on analyzing the project itself, including diagnostics, projections, estimations, and internal consistency. (An application can score a maximum of 40 points on this dimension.)
  - "Entrepreneurial ability" is evaluated by BAE staff, with the help of psychologists and other specialized professionals, through in-depth interviews with applicants. These analyze past experience, leadership, commitment, in-depth knowledge of the project, and entrepreneurial attitude. (A maximum of 60 points are assigned to each project after this assessment.)[1]

- The program selects as beneficiaries all applicant projects obtaining a score of 55 points or above. The selected projects and their sponsors receive transfers from the program. The benefits include:

  - For beneficiaries, a cash transfer or grant up to AR$ 70,000 (in 2011, this was equivalent to US$ 16,000) or up to 40% of the initial project investment;

[1] See Appendix A for further description of the selection process.

– For the supporting institutions, a premium of AR$ 3,000 (US$ 750) for each selected project and AR$ 2,200 per month throughout the mentorship period (up to 12 months, during which the institution should assist the entrepreneur and provide external consulting services). In total, each institution can receive up to 35% of the amount transferred to the entrepreneur, representing about US$ 5,600 for the institution.

Table 1 summarizes the number of institutions, applications, and beneficiaries as well as total transfers provided.

Table 1: The BAE program

	2008	**2009**	**2010**	**2011**
Institutions	8	12	14	14
Applications (number of projects)	80	115	105	112
- beneficiaries	51	61	59	62
- non-beneficiaries	29	54	46	50
Applications in sample	7	15	25	61
- beneficiaries	7	11	14	36
- non-beneficiaries	0	4	11	25
Transfers, thousands of AR$	1 781	2 323	2 334	3 202

Source: Administrative data from BAE.

3 Data and Methods

Our analysis uses data provided by BAE as well as taken from a survey carried out specifically for this research. This survey complements the administrative information to focus on the performance of the firm. In this section, we briefly explain the characteristics of our survey and of the respondents. We also present the methods for identifying the effects of the program.

3.1 Survey of entrepreneurs

This paper is based on a survey distributed in 2012 to the over 400 entrepreneurs who participated in the program from 2008 to 2011, regardless of whether they were beneficiaries or not. We contacted entrepreneurs using the information provided by BAE, including name, e-mail addresses, and phone numbers. This information was recovered from the original application forms and project information records. The survey gathered information about the project (e.g., industry, initial capital), entrepreneur (e.g., age, education), and certain outcomes such as survival, sales, profits, and employment. It was answered by 108 firms/entrepreneurs, including 68 beneficiaries and 40 non-beneficiaries (see Table 1).[2]

[2]The response rate was lowered by the several monitoring surveys that BAE had previously implemented; the burden of replying progressively reduced participation and response rates. Appendix B provides details on the survey and questionnaire.

Table 2 presents some descriptive statistics for the sample. As a snapshot, entrepreneurs have a mean age of 36, 70% of them are men, 42% are college graduates, and 40% have a master's degree. Projects are diverse, but 33% are in IT-related fields, while 16% are in manufacturing. Almost 60% of beneficiaries report having some previous experience with startups, while 33% acknowledge some past failures. Importantly, there are no significant differences between beneficiaries and non-beneficiaries regarding these characteristics.

Table 2: Descriptive statistics of the surveyed entrepreneurs

	Total		Non-Beneficiaries		Beneficiaries		t-test for equal means
	Mean	Std	Mean	Std	Mean	Std	(p value)
Age	36.09	7.92	35.83	7.14	36.25	8.40	0.79
Men†	0.69	0.46	0.70	0.46	0.69	0.47	0.92
Edu. High S.†	0.09	0.29	0.13	0.33	0.07	0.26	0.38
Edu. Tertiary†	0.09	0.29	0.10	0.30	0.09	0.29	0.84
Edu. College†	0.42	0.50	0.40	0.50	0.43	0.50	0.79
Edu. Postgr.†	0.40	0.49	0.38	0.49	0.41	0.50	0.71
Sector IT†	0.43	0.50	0.40	0.50	0.44	0.50	0.68
Sector Manuf.†	0.19	0.39	0.17	0.38	0.19	0.40	0.84
Experience†	0.57	0.50	0.60	0.50	0.56	0.50	0.68
Failures†	0.33	0.47	0.38	0.49	0.31	0.47	0.49
N	108		40		68		.

Notes: The table shows the mean and standard deviations of the relevant variables, comparing beneficiaries and non-beneficiaries. The last column is the probability of equality of means using a t-test. Two-sample Wilcoxon and Kolmogorov–Smirnov tests of equality of distribution functions applied to the four education statuses and four sectors do not reject the null with probability values higher than 50%.
† Proportions.
Source: Survey of BAE program applicants.

An important issue is whether respondents represent only a particular subsection of the universe. We find that the data are consistent with a random sample of the entrepreneurs who participated in the program. This can be seen by the fact that the distribution of BAE scores for the sample aligns to that of the total universe (see Appendix B). Additionally, we compared the balance between the sample and the universe of applicants on several characteristics. We found no difference between those who answered the survey and those who did not in terms of the project characteristics (i.e., industry, size of project, amount of declared investment, and overall score). Moreover, all these variables combined do not explain the probability of response (a probit model for the probability of responding, based on these variables, yields a pseudo-R2 of less than 0.03, while a likelihood ratio test comparing a model without explanatory variables gives a p-value of 0.25).

3.2 Discontinuity methods

In order to identify the effects of the policy, we exploit the selection process, which scores each project (as explained in Section 2) and selects those applications with scores above

55.[3] Thus, selection into the program is non-random, and there could be systematic differences between the treated and the non-treated. Discontinuity methods exploit this deterministic discontinuity in the forcing variable to identify the impact of the treatment. Intuitively, the assumptions are that individuals near the cutoff value for the forcing variable are not essentially different and that the relationship between the forcing variable and outcomes is continuous. Thus, any differences in outcome between those observations above the cutoff and those below it are due to the treatment.[4]

Importantly, the scoring is carried out by BAE (not by the NGOs) and both evaluators and applicants are aware of the characteristics that projects and entrepreneurs should have. In this sense, the scoring procedure is no different from regression discontinuity approaches in which the running variable is the outcome of the test (and the treatment is a scholarship); see the seminal article from Thistlethwaite and Campbell (1960) as a main example.

Empirically, we identify the effects of the program by using the following general regression:

$$y_i = \alpha + \beta D_i + \delta Z_i + \gamma X_i + u_i \tag{1}$$

where y_i is an outcome variable (i.e., income, benefits, or employment) of the startup, D_i identifies selection into treatment (an indicator variable, taking the value of one if the observation corresponds to a beneficiary and zero otherwise), Z_i are variables defining a function for the score (quadratic in the main specification) and summarizing the continuous effect of the score on the examined outcome, and X_i are additional controls (a quadratic function of age as well as dummies for gender, completed education, industry, and year of the program edition). Following discontinuity methods, the coefficient β is the estimator for the policy's impact on the performance of the individuals near the cutoff value.

We also apply an analogous method for non-linear models, such as logit and duration models, for which the left-hand side variable must be accordingly reinterpreted (as a latent variable or a function of the hazard rate). In this, we follow the literature using discontinuity methods to analyze survival rates, for example of unemployment duration (Card et al. 2007; González-Rozada et al. 2011).

In addition, we identify the impact of BAE participation by estimating local linear regressions on both sides of the cutoff value. This method reduces the sensitivity of the results to the particular function of the score specified in the regression. We present the results of applying this method in Section 4.

It is important to emphasize the difference between global and local approaches. The global approach uses all the data available with equal weighting and is flexible enough to allow one to easily add variables as controls. The drawback is that if the functional form

[3] While the overall process remains similar in every program edition, both the scoring and the cutoff values change slightly. See Appendix A for details.

[4] This methodology has been applied by, among others, Thistlethwaite and Campbell (1960) to analyze the impact of merit awards on future academic outcomes, Hoxby (2000) to evaluate the impact of class size on educational outcome, and Pettersson-Lidbom (2008) to estimate the impact of political parties on fiscal policies. See van der Klaauw (2008) for a survey.

(e.g., quadratic) is incorrect, then the resulting estimator will be biased. For this reason, it is important to analyze the robustness of the results to different specifications for the relationship between the running variable and outcome (Lee and Lemieux 2010). We did this by estimating several polynomials (ordinary and Chebyshev) of different orders (linear, quadratic, and cubic) and analyzing the robustness of the results to these alternatives. We also restricted the sample to see whether the effects remain constant when these observations are removed. On the contrary, the strength of the local approach is that it avoids specifying a particular function for the relationship between the running variable and outcome. With finite samples, the local linear regression approach can be less efficient and can also lead to bias, such as if the underlying function is not linear near the threshold. It can also be sensitive to the bandwidth used. We manage this last issue by presenting the results for several bandwidths. Given that both local and global approaches have strengths and weaknesses, following Lee and Lemieux (2010) we use them as complements to check the robustness of the estimations (see also Imbens and Lemieux 2008; Imbens and Wooldridge 2009).

Regression discontinuity analysis relies on several important assumptions, some of which can be confirmed empirically. In Appendix D, we explain that covariates (such as education and age) do not jump at the cutoff value and that the mode of the distribution is close to this cutoff; we also discuss the potential problems of manipulating the running variable. Importantly, there is no evident way in which entrepreneurs could manipulate the test outcome. However, the manipulation of the running variable by program representatives could be a concern.

Nevertheless, it is highly implausible that selection into treatment could generate spurious effects through substantial manipulation. First, if the evaluators were to privately observe certain signals (e.g., the entrepreneur's network or charisma), they could introduce these observations into the scoring process (in fact these variables may be important in terms of indicating the future success of the startup). Thus, using the score as a control should eliminate such effects. Second, any manipulation of the scoring process that diverges from the objective of selecting the more capable applicants or those with better prospects would reduce the size of the effects. In other words, this type of manipulation would bias the estimates towards zero. Finally, it is important to emphasize that the applicants to and projects presented at BAE share certain similarities, even when comparing those with high and low scores. This is partly because the program is restricted to innovative projects and partly because, because of the NGOs' identification of potential beneficiaries, the beneficiaries are probably drawn from a rather homogeneous group.

Table 2 compares the characteristics of beneficiaries and non-beneficiaries, including age, gender, education, project sectors, experience as an entrepreneur, and any failures with previous startups. We find no substantial differences between them. We also run one-sample and two-sample tests on these characteristics and conclude that there are no differences in mean or distribution (the null hypothesis of equality is not rejected, and the probability of equality is higher than 50% for all tests; in the final column of the table

we present the probability of equal means for each variable). We analyze educational attainment more closely and find no relationship between it and scores using local linear regressions.

Additionally, project types do not seem to differ across the range of scores.[5] The fact that these observable characteristics do not differ is important as it reduces the relevance of focusing the analysis close to the cutoff value and allows for a more global estimation, such as the regressions described by equation (1); this increases the number of relevant observations.

4 Results

In this section, we focus on how the program affects firm creation and survival and then discuss its effects on employment, sales, and net income. We present three specifications for the right-hand side of equation (1). First, we control for any continuous effect of the score on the outcome by using a quadratic function of the score; we add only dummies for the year of the relevant BAE program. In a second specification, we add some covariates such as a quadratic function of age and dummies for gender, industry, and education level (tertiary, college, and postgraduate, with the base group being those who completed high school). Finally, a third specification restricts the sample to the observations with scores between 45 and 65 (i.e., closer to the cutoff value), and we consider only a quadratic function of the score and dummies for the program year. In all specifications, the effect of the program is identified by the estimated coefficient on the "treated" dichotomous variable D_i in equation (1). We also analyze alternative specifications, as presented in the appendix.[6] For the continuous variables (employment, sales, and income), we also apply local linear regressions.

4.1 Creation, survival, and duration

Creation Entrepreneurs must overcome a number of difficulties to successfully develop a business. Financial frictions are the most obvious (and perhaps most important) barriers, but a lack of information and high fixed startup costs may also be relevant. The first objective of the BAE program is to generate conditions to enable good ideas to be implemented and transformed into profitable firms. To analyze this effect, we compare

[5] As an example, consider the applicants to the 2011 edition. The application with the lowest score was a web application for medical records and appointment management. The application with the highest score was a web platform for sharing and managing audiovisual files. The projects close to the cutoff point (with scores around 55) were a webpage for selling office supplies (a beneficiary) and a webpage for managing and promoting student exchanges. These four ideas are not substantially different on the surface.

[6] In Appendix E, we present the results of these regressions. Each table refers to a different outcome; the first two columns of the tables refer to the simple differences between beneficiaries and non-beneficiaries with no additional controls (column 1) or simply with program year dummies (column 2). The following columns report the regression discontinuity estimations. We tried different functions of the score, such as quadratic (columns 3, 5, and 7), cubic (column 4), and a Chebyshev polynomial of degree 3 (column 6). Additionally, we considered a subsample of those with scores between 45 and 65 (column 7), and we included additional controls (column 5) comprising a quadratic function of age and dummies for gender, industry, and education level. The results presented in the main text correspond to columns 3, 5, and 7.

the business creation rate among beneficiaries with that of non-beneficiaries and find a strong difference between the two groups: 63% of non-beneficiaries started their projects, while this figure rises to 97% among beneficiaries.

Table 3: Probability of creation

	Non-beneficiaries	Beneficiaries	Total
Did not start	36.8	3.0	15.2
Started	63.2	97.0	84.8
Total	100.0	100.0	100.0

Notes: Average probability that the project started.
Source: Survey of BAE program applicants.

These differences are partially explained by observable characteristics and differences in project quality. To address this, we apply regression discontinuity methods, as discussed in Section 3. In particular, we run a logistic regression to control for the effect of the score; the results are presented in Table 4. We find a strong and significant impact of being a beneficiary on the likelihood of business creation. Specifically, the coefficient associated with the "treated" dummy is 2.1, significant at the 10% level. The marginal effects show that BAE increases the probability of business creation by 22 percentage points. The effect is qualitatively robust to including a set of control variables in the regression (see column 2), but the precision of the estimation decreases. When we restrict the analysis to the observations surrounding the cutoff (those with scores between 45 and 65), the effect is higher, with a significant marginal effect of 32%.

Table 4: Creation probability - Logit model

	(1) Quadratic	(2) Controls	(3) Subsample
Treated	2.10*	2.03	3.52*
	(1.26)	(1.34)	(2.04)
Marginal	0.22	0.20	0.32
	(0.02)	(0.02)	(0.03)
Number of Obs.	108	106	59

Notes: Logit estimations (standard errors) of the probability that a startup is created on the treatment dummy and a quadratic function of the score. In column (2), we include additional controls. In column (3), we restrict the sample to those observations with scores between 45 and 65. *, **, and *** indicate significance at the 10%, 5%, and 1% levels. The last rows reproduce the marginal effect (and standard errors) of the "treated" variable.
Source: Survey of BAE program applicants.

To summarize, the effect of being approved for BAE increases the likelihood of implementing the project in question by between 20 and 32 percentage points, as estimated via the marginal effects. This is a sizeable and strong effect: if the program had not been

in place, about 22% of beneficiaries' firms would not have been created. This result implies that almost 50 businesses formed as a result of the program across the four editions (during which 233 projects were approved).

The fact that almost 40% of non-beneficiaries did not start their businesses suggests that the program was effective at removing some relevant limitations for beneficiaries. This point also explains the impact of the program: without barriers to entry, firm creation could be equally high for treated and control groups, implying high deadweight losses. This is a first approximation of the program impact that is worth highlighting.

Survival A second important aspect of the program is that it aims to improve the survival of beneficiary businesses. New and innovative companies often have high exit rates, not necessarily due to flaws in the project itself but because of liquidity constraints, inadequate initial scale, or transitory technical and market problems. To consider how BAE impacted this, we focus on firm survival and find that the probability of continuing the project is higher among beneficiaries than non-beneficiaries: 92% versus 79%.

Table 5: Survival rate

	Non-beneficiaries	Beneficiaries	Total
Did not survive	20.8	7.7	11.2
Survived	79.2	92.3	88.8
Total	100.0	100.0	100.0

Notes: Average probability that the project is still in the market up to July 2012, given that it started.
Source: Survey of BAE program applicants.

To identify the effect of the program on firm survival, we performed a logistic regression with the dependent variable being an indicator of whether the firm was still in existence when the survey was taken. The results shown in Table 6 reveal that the probability of survival among beneficiaries is significantly higher than that among non-beneficiaries. In fact, the "treated" dummy that identifies an approved project (a BAE beneficiary) is significant at the 5% level, with an implied marginal effect above 33 percentage points. This result is robust to including additional controls (column 2 of Table 6) and becomes even stronger when we restrict the sample to the observations in the neighborhood of the cutoff (column 3).

Duration We delved deeper into this topic of firm survival by carrying out a duration analysis. This approach allowed us to address the fact that firms started in different periods and eventually exited the market after different durations of existence. It also allowed us to take into account the information provided by the right-censored project durations.

For this analysis, we defined firm duration (i.e., the age of the firm) as the number of months from its creation until the survey or until its exit from the market.[7]

[7]As a robustness check, we alternatively defined duration as the number of months since the start of

Table 6: Survival probability - Logistic regression

	(1) Quadratic	(2) Controls	(3) Subsample
Treated	4.16**	4.31*	8.27**
	(1.91)	(2.46)	(3.78)
Marginal	0.33	0.34	0.48
	(0.02)	(0.04)	(0.04)
Number of Obs.	90	83	52

Notes: Logit estimations (standard errors) of the probability of survival on the treatment dummy and a quadratic function of the score. See notes from Table 4.
Source: Survey of BAE program applicants.

We estimated the effect of the program by using a proportional hazard Cox model.[8] The results of these estimations, presented in Table 7, show that being a beneficiary reduces exit probability. In particular, the coefficient associated with the "treated" dummy is -2.36 when we control for a quadratic function of the score and -2.52 when we add additional covariates. The effect strengthens to -4.42 when the analysis is restricted to the neighborhood of the cutoff. We also used a parametric proportional hazard model specifying a Weibull distribution for the underlying hazard. As shown in panel B of Table 7, the effects are even stronger when using this model. In other words, the proportional hazard model confirms that beneficiaries have higher survival rates (i.e., long firm duration) than non-beneficiaries; specifically, the market exit hazard for beneficiaries is one-10th that of non-beneficiaries.

the relevant BAE edition. We also imposed censoring at different durations (e.g., at 48, 36, or 24 months). We found similar or stronger effects.

[8]This is a proportional hazard model in which hazard h at duration t is estimated through a partial likelihood of the form $\log h(t|X) = \alpha_t + \theta' X$ and where the underlying hazard, α_t, is unspecified and identified after estimating the coefficients θ. Note that the hazard refers to the probability that the firm ceases its operations at a given duration. Thus, a negative coefficient on the "treated" coefficient implies that being a beneficiary increases the survival rate (i.e., the firm's longevity).

Table 7: Exit rate - Duration models

	(1) Quadratic	(2) Controls	(3) Subsample
A) Cox			
Treated	-2.36*	-2.52	-4.42*
	(1.41)	(1.76)	(2.41)
B) Weibull			
Treated	-2.334*	-2.415	-4.476*
	(1.375)	(1.718)	(2.300)
Number of Obs.	88	88	51

Notes: Each cell is the result of a separate estimation through a proportional hazard model applied to the duration (age) of the firm. See notes from Table 4.
Source: Survey of BAE program applicants.

These results show that BAE has a strong impact on the rate of business creation and survival, with differences that are not only statistically significant but also important from an economic perspective. Indeed, without BAE, a non-negligible proportion of currently operating firms among beneficiaries would not have existed when our survey was conducted.

Additionally, the results suggest that the increase in firm creation was not at the expense of generating a pool of short-lived worse firms. In what follows, we analyze this issue further through certain performance indicators.

4.2 Employment, sales, and net income

Besides firm creation and survival, BAE aims to generate sustainable and competitive enterprises. To analyze whether it succeeds on this front, we concentrate on firm performance. In our survey, we capture employment figures, sales, and net income for the firm both during the year following application to BAE and in the final year represented in the dataset (2011). Before discussing the results, we must emphasize that these last variables are measured for a selection of projects: those that were created and, in the case of variables measured in 2011, those that survived until the time of the survey. Given that the selection process seems more severe for non-beneficiaries, it is plausible to expect milder differences between the two groups.

By comparing the means of these outcome indicators by group, we find that beneficiaries do have higher initial employment, sales, and net income as well as higher employment and income in 2011.

Table 8: Performance - Differences between beneficiaries and non-beneficiaries

	Initial	2011
Employment	0.29	0.22
Sales	0.39	-0.02
Net income	0.44	0.18

Source: Survey of BAE program applicants.

We now turn to assess the program impact by using regressions following the general specification of equation (1). As in previous exercises, we consider different specifications. In this section, we present the more relevant ones; Appendix E contains a detailed presentation of the results. The continuous variables are analyzed through local linear regressions. We present only the results for employment, as we found no other significant or robust effects in the regression analysis.

Table 9: Performance - Regressions

	(1) Employment	(2) Sales	(3) Net income
A) First year			
(a) Quadratic	1.49*	-0.371	-0.195
	(0.86)	(0.927)	(0.869)
	[87]	[54]	[42]
(b) Controls	2.36**	-0.013	0.157
	(0.92)	(1.196)	(1.131)
	[86]	[53]	[41]
(c) Subsample	2.63	0.444	1.022
	(1.65)	(1.760)	(1.089)
	[49]	[27]	[22]
B) 2011			
(a) Quadratic	3.07	-0.246	1.061
	(2.39)	(1.004)	(0.835)
	[78]	[50]	[40]
(b) Controls	2.73	-1.309	1.861
	(2.97)	(1.733)	(1.589)
	[76]	[49]	[39]
(c) Subsample	0.38	0.533	0.819
	(1.63)	(1.329)	(0.746)
	[45]	[26]	[20]

Notes: Each cell is the result of a separate OLS regression of the number of jobs (column 1), the log of deflated sales (column 2), and the log of deflated net income (column 3) on the treatment indicator variable and a quadratic function of the score. The coefficient presented in the table is the treatment indicator variable. The specification of line (a) adds a quadratic function of the score and year indicator variables; in line (b), we include additional controls, and in line (c) we restrict the sample to those observations with scores between 45 and 65. Standard errors are in parentheses and the number of observations is in square brackets. *, **, and *** indicate significance at the 10%, 5%, 1% levels.
Source: Survey of BAE program applicants.

Employment The first column of Table 9 shows the values (and standard deviations) for the coefficients associated with the "treated" indicator variable when we regress the number of jobs on a quadratic function of the score, add certain controls, and restrict the sample to those with scores between 45 and 65. We consider employment in the first year after BAE and employment in 2011 in panels A and B, respectively.

The results suggest that beneficiary firms have larger numbers of employees. In particular, initial employment is 1.5 more people among beneficiaries when we control for the quadratic function of the score. The impact grows to exceed 2.4 when we include additional controls and restrict the sample to that around the cutoff point. The impact of BAE on employment in 2011 is less clear, however: while the point estimates are larger in magnitude, they are not significantly different from zero when considering the standard deviations. Overall, the regression estimates indicate a difference in initial employment of about 2 jobs as well as an imprecise but positive effect on employment in 2011.

Table 4.2 shows the estimates of the impact of BAE on employment, as assessed through local linear regressions. According to these results, being a beneficiary increases employment by about 3 jobs for initial employment and 2.4 for employment in 2011. We also present results that, instead of using the optimal bandwidth, double it. For initial employment, the estimated impact becomes 2.4; for employment in 2011, it becomes 0.93 and turns insignificant. Figure 1 presents a plot of the local linear regression results.[9]

Overall, we find that beneficiaries tend to create more jobs. This is indirect evidence that firms created thanks to the assistance of BAE do not perform worse; on the contrary, they create more employment and survive longer.

Sales Column 2 of Table 9 presents the values (and standard deviations) of the coefficients on the "treated" indicator variable when the dependent variable is the log of the venture's inflation-adjusted sales value. We consider sales volume both during the first year of the startup and in 2011. We find no significant differences between the two groups when we control for a quadratic function of the score, consider additional controls, and examine a subsample.

Net income The impact on the net income of beneficiaries is also insignificant when we regress the logged inflation-adjusted net income on the score and other controls (see column 3 of Table 9).

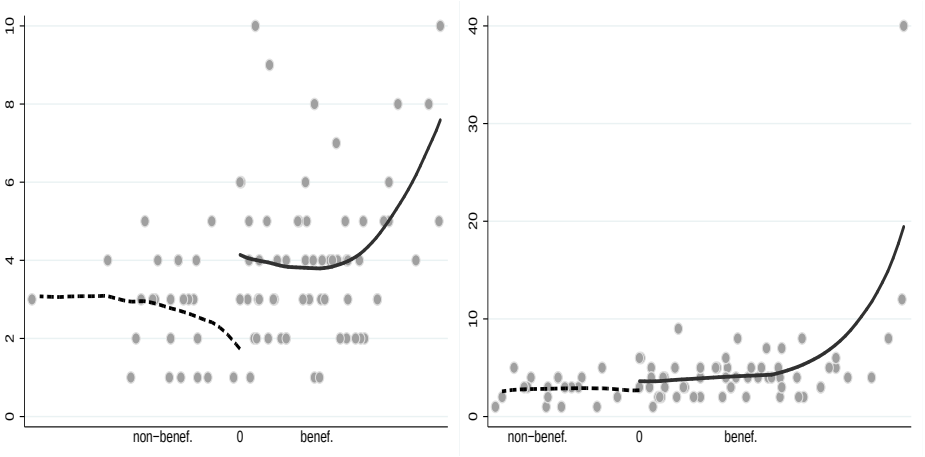
[9] A caveat to our exercise is that applications could represent more than one entrepreneur and the effects on employment could be driven by a preference among BAE selectors for larger groups of entrepreneurs. However, we find no correlation between the score and number of entrepreneurs listed on the application. Furthermore, when we compute initial employment figures minus the number of entrepreneurs listed on the application (i.e., non-founders), we find even higher and more significant effects, both in the regressions and in the local analysis. We thank an anonymous referee for pointing out this issue.

Table 10: Local regression discontinuity estimates

Variable	(1) Employment first year	(2) Employment 2011
Opt.Bandwidth	3.306***	2.421**
	(0.986)	(1.076)
2xOpt.Bandwidth	2.410**	0.929
	(1.139)	(1.292)

Notes: Local linear regression estimates (standard errors) have a triangle Kernel and bandwidth of 7 for initial employment and 6.5 for employment in 2011 (optimal bandwidth from Imbens and Kalyanaraman 2012). *, **, *** indicate significance at the 10%, 5%, and 1% levels.

Figure 1: Linear local regressions



(a) Employment - initial (b) Employment - 2011

Notes: Plot of the local linear regressions using twice the optimal bandwidth.

5 Assessment of BAE's Overall Impact and Discussion

In Section 4, we argued that the BAE program has had some important effects, notably improving the probability of business creation, the chances of the firm's survival, and the size of its staff. The impacts of BAE on other outcomes such as sales and net income are less clear, however; both sales and net income are highly dispersed, which can affect the robustness of their measurement and analysis.

This section provides a global assessment of the impact of BAE. To do so, we first select estimates of BAE's impact on each outcome (firm creation, survival, and employment). Second, we use these estimates to assess the total impact of the program.[10]

Table 11 presents our preferred estimates. We select a point estimate and standard deviation for each outcome variable from the results above, choosing the model specification that includes a quadratic function of the score as well as indicator variables for the program year. We opt for the Weibull duration model because this allows us to simulate baseline survival rates. We also add the effect of employment (in 2011). These coefficients indicate that BAE increases the likelihood of firm creation by 22 percentage points, that the survival of beneficiary firms would be about 43% at 24 months in the absence of the program, and that beneficiary firms create about 3 additional jobs. We assume no direct impact on sales or net income, but the existence of more firms implies larger aggregate sales and profits.

Table 11: Impact estimates

	Impact	St. Dev.
Creation	2.10	1.26
Hazard	-2.33	1.37
Employment	3.07	2.39

Notes: This table reproduces the results presented in previous tables used for the simulation.
Source: Survey of BAE program applicants.

To calculate the overall impact of the program, we simulate a counterfactual scenario by assuming that, if BAE had not been in place, some of the beneficiaries would not have created their firms, some of those created would have disappeared from the market, and those that survived would have created fewer jobs. We use the estimates shown in Table 11 to simulate this counterfactual scenario. To show the distribution of estimates, we perform this exercise 10,000 times with independent draws of impact values using the means and standard deviations in Table 11.[11]

Table 12 presents the results of these simulations. Overall, if BAE had not been in place from 2008 to 2011, 132 firms would not have existed in 2012 and about 1,000 jobs would have not been created.

Figure 2 shows the distribution of the estimates for employment. The median and mean measures do not diverge substantially, and the overall effect is significantly different from zero, ranging from 200 to almost 2,000 jobs.

[10] Throughout the paper, we assume that there is no displacement effect of BAE. In other words, the program is not diverting any other source of financing or harming the performance of other competing startups or firms. Given the small scale of the program, this "partial equilibrium" approach seems a reasonable assumption.

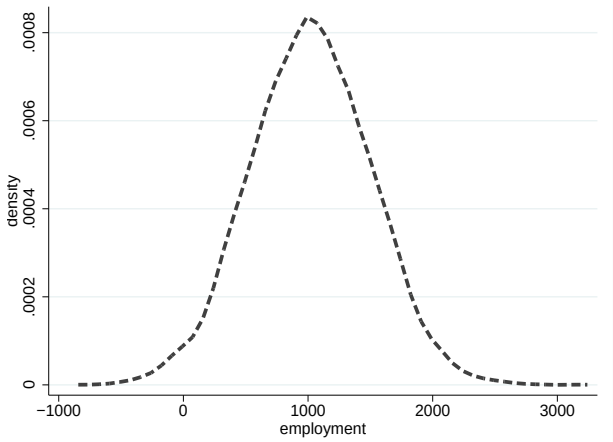
[11] Appendix C provides more details about this exercise.

Table 12: Simulation of overall impact

	Impact	St.D.
Firms	132.32	33.97
2008 edition	35.52	11.03
2009 edition	40.95	11.37
2010 edition	33.08	8.09
2011 edition	22.76	4.10
Employment	1020.76	482.01

Notes: Results from the simulations, as detailed in the main text.
Source: Survey of BAE program applicants.

Figure 2: Overall impact on employment



Notes: Overall impact on employment as of 2011. Results from the simulations, as detailed in the main text.
Source: Survey of BAE program applicants.

These estimates are modest in terms of sheer numbers. However, given the direct cost of BAE (in transfers), we can compute the cost of each additional job created by dividing the 2011 inflation-adjusted values of BAE's cost by its total impact on employment. This yields an estimate of less than US$ 4,000 for each created job.[12]

To put these results into context, it is important to compare them with those of previous studies. Parker (2009) reviews the literature on the evaluation of policies aimed at financing entrepreneurs and promoting innovation. Besides the methodological differences, two main estimates are comparable with our results: "additionality" and the cost per job of the program.

The first concept refers to the net increase in the number of firms created by a program, excluding the deadweight losses (i.e., those firms that would have been created in any case) and any displacement (i.e., firms that were replaced by those of beneficiaries). The estimates vary widely: between 15% and 85%. Generally, these proportions were constructed by asking beneficiaries whether they would have started their ventures with-

[12]The average formal monthly wage was about US$ 1,300 in 2011. Thus, the cost per job equates to about 3 months of wages.

out the policy—a very different method from ours. In any case, our estimates imply an "additionality" of about 50%, including both creation and survival. On the contrary, the estimates for job creation vary widely, but programs' cost-per-job figures are generally above $ 10,000 (Parker 2009)— much higher than ours of about $ 4,000.

The metrics of additionality and cost per job are important because they show that the program had a relevant impact on firm creation and survival at a reasonable cost. Our cost-per-job figure, though, does not include all the administrative costs of the program, but these seem to be low in any case. Additionally, the results could be amplified by factors external to BAE, such as beneficiaries receiving additional support from other programs; however, we found no difference in access to financial sources (besides BAE) among beneficiaries and non-beneficiaries.[13] Furthermore, as long as our regression discontinuity design holds, the score controls for the relevant *ex ante* differences between both groups. On the contrary, our measures are not the only potential positive consequences of the program. Other outcomes such as supply chain development, providing experience for entrepreneurs, knowledge spillovers, or institutional changes that better support entrepreneurial activity may also exist. While we lack information on possible impacts in other dimensions, we see the program effects shown in this paper as important not only by themselves but also as preconditions for other positive changes.

Given the impacts shown herein, it is important to emphasize the economic mechanism through which the program has such impacts. The main instruments of the program are transfers and mentorship. We have argued that these instruments are effective at reducing entry costs and barriers. We now provide additional evidence on this. The first such evidence comes from the survey responses of beneficiaries and non-beneficiaries. Beneficiaries generally agreed that receiving transfers and mentorship changed their decisions; non-beneficiaries cited the lack of BAE support as the reason that they did not launch their projects. Second, several pieces of evidence suggest that liquidity constraints exist for the entrepreneurs in our sample. When we asked those applicants who had not begun their projects the reason behind their decision, they noted a lack of financing as a main reason. We also found that only 45% of projects had access to financing (aside from BAE); moreover, this financing was mostly provided by family and friends (70% of those with financing). Thus, the transfers likely helped alleviate the financial constraints of the entrepreneurs. The non-refundable transfers also significantly changed the opportunity costs faced by these entrepreneurs. Most of the applicants are college graduates who could earn relatively high wages as employees; the program might thus have changed at the margin their decision to take chances as entrepreneurs. Besides firm creation, BAE was also shown to influence firm survival and job creation. Indeed, both transfers and mentorship might have helped improve firm management.

Overall, it is natural to think that the main role of BAE is to reduce barriers to firm entry given that support to beneficiaries is limited in time (i.e., one year or less). In this sense, BAE avoids introducing distortions and does not sustain low-productivity firms through permanent subsidies. The joint provision of transfers and mentorship seems to be another strength of the program, which affects not only the entry but also the early development of firms. Finally, the fact that BAE is focused on identifying entrepreneurial talents seems to be an additional benefit of the program to the extent that it helps avoid a commitment to particular sectors. In other words, BAE avoids the typical problem of "picking winners and saving losers."

[13]Only three projects, one in the treatment group and two in the control group, received additional support from other programs and these projects were not close to the cutoff of the running variable.

6 Conclusions

This paper analyzed a government program in Buenos Aires, Argentina, that assists potential entrepreneurs with their startups. Through a specially tailored survey, we collected information from both beneficiaries and non-beneficiaries. By examining these data using regression discontinuity methods, we identified the impact of the program on firm creation, survival, employment, sales, and net income. We found that BAE increases the likelihood of firm creation by about 22 percentage points; moreover, the project survival rate at 24 months from inception increases from 43% to 92%. The impact of the program on employment is also significant, with participating projects creating about three additional jobs. However, no significant impact was found on income and sales, which are much more volatile.

These estimates were then combined to assess the overall effect of the program through simulations. We found that about 1,000 jobs and 132 firms were generated by the program, with a cost-per-job ratio of US$ 4,000, lower than the estimates for other programs provided by previous authors.

Importantly, this assessment is based on high "additionality" (i.e., many beneficiaries would not have begun their projects without the support of BAE). Non-refundable transfers seem to be important for enabling entrepreneurial success, as they provide liquidity and reduce the opportunity costs faced by entrepreneurs. The role of mentorship could also be substantial in driving the impact of BAE. In any case, we cannot disentangle the relative importance of these two instruments by using our data.

In line with previous researchers (de Mel et al. 2008; Blattman et al. 2014), we found that small public transfers can have important effects on new small businesses. Unlike the findings of other studies, however, our results do not refer to poor entrepreneurs undertaking low-productivity activities with a high probability of being liquidity-constrained. On the contrary, we analyzed a program focused on innovative activities led by highly educated entrepreneurs.

In this sense, our results suggest that the government can help individuals with entrepreneurial talent overcome the costs of entry, financial constraints, and informational barriers. Given the high productivity gains of improving the allocation entrepreneurial talent that previous research has found, these achievements gain more importance.

In conclusion, our finding that small-scale public funding for startups can help transform promising ideas into businesses as well as improve firm survival and generate sustainable jobs in the medium term is good news for the myriad of new public programs at the national and local levels that intend to support innovative startups. The long-term impact of such programs may be even higher if some such startups truly and sustainably succeed.

References

Banerjee, A. V. and A. F. Newman (1993). Occupational Choice and the Process of Development. *Journal of Political Economy 101*(2), 274–98.

Blattman, C., N. Fiala, and S. Martinez (2014). Generating skilled self-employment in developing countries: Experimental evidence from uganda. *The Quarterly Journal of Economics 129 (2)*, 697–752.

Bloom, N., B. Eifert, A. Mahajan, D. McKenzie, and J. Roberts (2013). Does Management Matter? Evidence from India. *The Quarterly Journal of Economics 128*(1), 1–51.

Buera, F., B. Moll, and Y. Shin (2013). Well-Intended Policies. *Review of Economic Dynamics 16*(1), 216–230.

Buera, F. J., J. P. Kaboski, and Y. Shin (2011). Finance and Development: A Tale of Two Sectors. *American Economic Review 101*(5), 1964–2002.

Card, D., R. Chetty, and A. Weber (2007). Cash-On-Hand and Competing Models of Intertemporal Behavior: New Evidence from the Labor Market. *Quarterly Journal of Economics 122*(4), 1511–1560.

Cho, Y. and M. Honorati (2014). Entrepreneurship programs in developing countries: A meta regression analysis. *Labour Economics 28*(C), 110–130.

de Mel, S., D. McKenzie, and C. Woodruff (2008). Returns to capital in microenterprises: Evidence from a field experiment. *The Quarterly Journal of Economics 123*(4), 1329–1372.

D'Erasmo, P. N. and H. J. Moscoso Boedo (2012). Financial structure, informality and development. *Journal of Monetary Economics 59*(3), 286–302.

Djankov, S., R. L. Porta, F. Lopez-De-Silanes, and A. Shleifer (2002). The Regulation Of Entry. *The Quarterly Journal of Economics 117*(1), 1–37.

Erosa, A. and L. Allub (2012). Financial Frictions, Occupational Choice, and Economic Inequality. 2012 Meeting Papers 702, Society for Economic Dynamics.

Feldman, M. P. (2014). The character of innovative places: entrepreneurial strategy, economic development, and prosperity. *Small Business Economics 43*(1), 9–20.

Fiala, N. (2014). Stimulating microenterprise growth: Results from a loans, grants and training experiment in uganda. Mimeo.

González-Rozada, M., L. Ronconi, and H. Ruffo (2011). Protecting Workers Against Unemployment in Latin America and the Caribbean: Evidence from Argentina. Technical report, IDB. IDB Working Papers Series No. IDB-WP-268.

Guner, N., G. Ventura, and X. Yi (2008). Macroeconomic Implications of Size-Dependent Policies. *Review of Economic Dynamics 11*(4), 721–744.

Hopenhayn, H. A. (2014). Firms, misallocation, and aggregate productivity: A review. *Annual Review of Economics 6*(1), 735–770.

Hoxby, C. M. (2000). The effects of class size on student achievement: New evidence from population variation. *The Quarterly Journal of Economics 115*(4), 1239–1285.

Imbens, G. and K. Kalyanaraman (2012). Optimal Bandwidth Choice for the Regression Discontinuity Estimator. *Review of Economic Studies 79*(3), 933–959.

Imbens, G. W. and T. Lemieux (2008). Regression discontinuity designs: A guide to practice. *Journal of Econometrics 142*(2), 615–635.

Imbens, G. W. and J. M. Wooldridge (2009). Recent Developments in the Econometrics of Program Evaluation. *Journal of Economic Literature 47*(1), 5–86.

Kerr, W. R., J. Lerner, and A. Schoar (2010). The consequences of entrepreneurial finance: A regression discontinuity analysis. NBER Working Papers 15831, National Bureau of Economic Research, Inc.

Kim, P., H. Aldrich, and L. Keister (2006). Access (Not) Denied: The Impact of Financial, Human, and Cultural Capital on Entrepreneurial Entry in the United States. *Small Business Economics 27*(1), 5–22.

Lee, D. S. and T. Lemieux (2010). Regression Discontinuity Designs in Economics. *Journal of Economic Literature 48*(2), 281–355.

Lerner, J. (2012). *Boulevard of Broken Dreams: Why Public Efforts to Boost Entrepreneurship and Venture Capital Have Failed–and What to Do About It*. Princeton University Press.

López-Acevedo, G. and H. W. Tan (2011). *Impact Evaluation of Small and Medium Enterprise Programs in Latin America*. World Bank Publications.

Lucas, R. E. J. (1978). On the Size Distribution of Business Firms. *Bell Journal of Economics 9*(2), 508–523.

Mano, Y., A. Iddrisu, Y. Yoshino, and T. Sonobe (2012). How Can Micro and Small Enterprises in Sub-Saharan Africa Become More Productive? The Impacts of Experimental Basic Managerial Training. *World Development 40*(3), 458–468.

McKenzie, D. and C. Woodruff (2008). Experimental evidence on returns to capital and access to finance in mexico. *The World Bank Economic Review 22*(3), 457–482.

McKenzie, D. and C. Woodruff (2012). What are we learning from business training and entrepreneurship evaluations around the developing world ? Policy Research Working Paper Series 6202, The World Bank.

Midrigan, V. and D. Y. Xu (2014). Finance and Misallocation: Evidence from Plant-Level Data. *American Economic Review 104*(2), 422–58.

Parker, S. C. (2009). *The Economics of Entrepreneurship*. Number 9780521899604 in Cambridge Books. Cambridge University Press.

Pettersson-Lidbom, P. (2008). Do Parties Matter for Economic Outcomes? A Regression-Discontinuity Approach. *Journal of the European Economic Association 6*(5), 1037–1056.

Restuccia, D. and R. Rogerson (2008). Policy Distortions and Aggregate Productivity with Heterogeneous Plants. *Review of Economic Dynamics 11*(4), 707–720.

Storey, D. (1998). Six steps to heaven: Evaluating the impact of public policies to support small businesses in developed economies. Technical report, Centre for Small and Medium Sized Enterprises, University of Warwick.

Thistlethwaite, D. and D. Campbell (1960). Regression-discontinuity analysis: An alternative to the ex post facto experiment. *Journal of Educational Psychology 51*, 309–317.

van der Klaauw, W. (2008). Regression-discontinuity analysis: A survey of recent developments in economics. *LABOUR 22*(2), 219–245.

von Graevenitz, G., D. Harhoff, and R. Weber (2010). The effects of entrepreneurship education. *Journal of Economic Behavior & Organization 76*(1), 90–112.

Appendix - Supplementary Material

A Selection process

The BAE scoring process has been implemented since 2008, with some slight modifications in the subsequent editions. It concentrates on two main issues: entrepreneurial ability (addressing the skills of the entrepreneur) and project viability (appraising the project itself, independent of who will implement it). This assessment is based on a series of items, each of which assigned a given score.

The cutoff point slightly varied across the years (55 in 2008 and 2011, 60 in 2010, 54 in 2009). We normalized the cutoff to 55 to make these scores comparable. In the regressions, we added dummies for the year of the program to mitigate any impact of this procedure.

B Survey description

The survey was distributed between May and July 2012 to all individuals who applied to the BAE program using the contact information provided by the Subsecretaría de Desarrollo Económico del Gobierno de la Ciudad de Buenos Aires. Individuals were contacted by e-mail and phone. Below, we provide a translation of the questionnaire (originally in Spanish).

Survey

Here, we present the survey questions used for the analysis of the paper.

Age

What is your educational level? (completed) (Primary / Secondary / Tertiary / College / Postgraduate)

Before submitting your venture to BAE, did you have any previous experience as an entrepreneur or business partner? If so, with how many ventures? (I had no previous entrepreneurial experience / 1 venture / 2 / 3 / 4 / 5 or more)

If you had previous experience, did you face a business failure? (No / Yes)

In what year did you apply to the Buenos Aires Emprende program? (2008 / 2009 / 2010 / 2011)

Have you implemented the project presented to Buenos Aires Emprende? Yes / No

In what month and year did you start working on the project? (mm / yy) [Sample: started]

Briefly describe the project [Sample: project started]

Please choose the area that best defines the main entrepreneurial product (Gastronomy / Tourism / Software / Consulting / Web / Manufacturing / Retail / Editorial / Other) [Sample: project started]

From the time the results of Buenos Aires Emprende were published, were you working on the project for 12 months in a row? (Yes / No) [Sample: project started]

What was the amount of monthly sales, in pesos, in those 12 months? If you did not work for 12 months in a row, enter your usual sales. [Sample: project started]

What was the enterprise's monthly net income in the first 12 months? Net income is sales minus the costs of entrepreneurship (before taxes and before distributions to partners), on average per month. If you did not work for 12 months in a row, enter a typical monthly value. [Sample: project started]

How many hours on average per week did you work for the venture in the first 12 months? (51 hours or more / Between 41 and 50 hours / 40 hours / Between 31 and 40 hours / Between 21 and 30 hours / Between 11 and 20 hours / 10 hours or less / Other) [Sample: project started]

How many people worked for the venture at the end of the first 12 months? Please include employees, managers and partners, and yourself [Sample: project started]

Of those, how many worked part-time (fewer than 30 hours per week)? [Sample: project started]

Through what kind of society did your company transact? [Sample: project started]

Formal name of society [Sample: project started]

Did you obtain funding from third parties? (Yes / No) [Sample: project started]

Did you receive funds from the following sources? You can check more than one (Capital markets / Investment funds / Angel investors / Family and friends / Other) [Sample: project started]

Are you still continuing with the project currently? (Yes/No) [Sample: project started]

How much were your monthly sales in 2011? (Sales per month on average for the 12 months of the year, in pesos) [Sample: project started, editions from 2008–2010]

What was the enterprise's monthly net income in 2011? Net income is sales minus the costs of entrepreneurship (before taxes and before distributions to partners), on average per month, in pesos. [Sample: project started, editions from 2008–2010]

How many hours on average per week did you work for the venture in 2011? (51 hours or more / Between 41 and 50 hours / 40 hours / Between 31 and 40 hours / Between 21 and 30 hours / Between 11 and 20 hours / 10 hours or less / Other) [Sample: project started, editions from 2008–2010]

How many people worked for the venture at the end of 2011? Please include employees, managers and partners, and yourself [Sample: project started, editions from 2008–2010]

Of those, how many worked part-time (fewer than 30 hours per week)? [Sample: project started, editions from 2008–2010]

Did you start another project? (Yes / No) [Sample: did not start project]

Did you start another project? (Yes / No) [Sample: discontinued project]

In what month and year did you discontinue the project submitted to BAE? (mm / yy) [Sample: discontinued project & started another project]

Why did you change projects? [Sample: discontinued project & started another project]

Please choose the sector that best defines your new entrepreneurial venture (Gastronomy / Tourism / Software / Consulting / Online / Manufacturing / Retail / Editorial / Other) [Sample: discontinued project & started another project]

How much were your average monthly sales in 2011 (for the 12 months of the year, in pesos)? [Sample: discontinued project & started another project]

What was the enterprise's monthly net income in 2011? Net income is sales minus the costs of entrepreneurship (before taxes and before distributions to partners), on average per month, in pesos. [Sample: discontinued project & started another project]

How many hours on average per week did you work for the venture in 2011? (51 hours or more / Between 41 and 50 hours / 40 hours / Between 31 and 40 hours / Between 21 and 30 hours / Between 11 and 20 hours / 10 hours or less / Other) [Sample: discontinued project & started another project]

How many people worked for the venture at the end of 2011? Please include employees, managers and partners, and yourself [Sample: discontinued project & started another project]

Of those, how many worked part-time (fewer than 30 hours per week)? [Sample: discontinued project & started another project]

Did you obtain your venture's investment funding from third parties? (Yes / No) [Sample: discontinued project & started another project]

Did you receive funds from the following sources? You can check more than one (Capital markets / Investment funds / Angel investors / Family and friends / Other) [Sample: discontinued project & started another project]

In what month and year was the project that you submitted to Buenos Aires Emprende discontinued? (mm / yy) [Sample: discontinued project & did not start another project]

Why did you discontinue the project? [Sample: discontinued project & did not start another project]

Are you currently working? (Yes, full-time / Yes, part-time / No) [Sample: discontinued project & did not start another project]

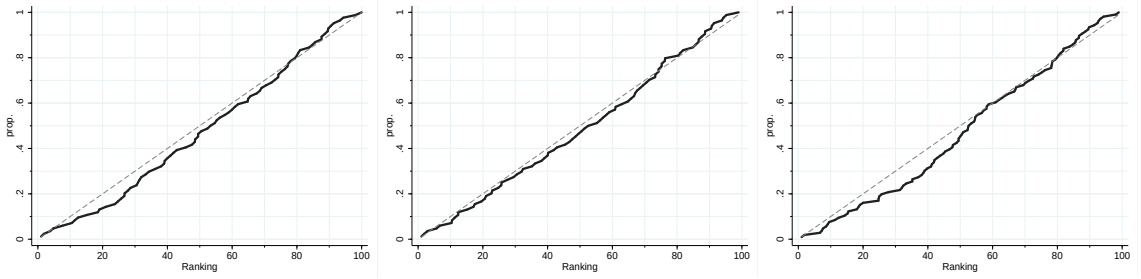
What is your monthly net income in pesos? Please enter the value in whole numbers (no periods or commas). [Sample: discontinued project & did not start another project]

Description of the sample

In this section, we present evidence that the survey respondents do not represent a particular selection of the universe of potential respondents. We have information of the total scores awarded to all applicants for all editions, and the disaggregation in the two components, entrepreneurial ability and project viability, for 2010 and 2011. By using the overall score for all editions and each component when possible, we compute the so-called "BAE ranking" by rescaling the variables to represent a uniform distribution between 1 (highest score for the edition) and 100 (lowest).

Figure B.1 shows the empirical cumulative distribution function of this variable in our sample. The graphs show the proportion of observations that fall below a given BAE ranking. For the whole universe, this graph would be a 45-degree line, as in a uniform distribution. Our sample is no different from such a distribution. From panel (c), we can say that, if anything, the sample is biased against the best projects: the proportion of respondents ranked by BAE in the first four deciles is about 30%. Nevertheless, this difference is not substantial.

Figure B.1: Empirical cumulative distribution function of the BAE ranking in the survey



(a) Entrepreneurial ability (b) Project viability (c) Overall score

Note: Panels (a) and (b) include respondents of 2010-2011 editions of BAE. Panel (c) includes all (2008-2011) editions.
Source: Survey of BAE program applicants.

C Overall assessment of impact

We performed a series of simulations to account for the overall impact of the program. To account for the difference in the number of firms, we proceed as follows. First, let F_i be the number of beneficiaries of edition i of BAE, c the impact of the program on the firm creation rate, and s_i the impact of the program on the survival probability of a firm that was created after edition i of BAE. Then, we compute the overall effect of the program on the number of firms by

$$I_i = cF_i + (1-c)s_iF_i$$

where the first term accounts for the number of firms that would not have been created if the program were not in place and the second term accounts for the fact that of those firms that would have been created regardless of the program, a proportion s_i of them would have exited the market if BAE was not in place.

To compute the impact on employment, we proceed by sampling our data. In particular, we first sample I_i firms from each edition of the program. We denote the mean employment in these firms by e_i. Second, we consider the impact of BAE on employment within the remaining firms. Let l be the impact of BAE on existing firms. We compute the total number of jobs created as

$$L_i = I_i(e_i + l) + (F_i - I_i)l$$

where the first term accounts for the employment within firms that would not be in place without BAE and the second term accounts for the fact that employment would have been lower in those firms that would have been created and would have survived without BAE.

Finally, the total number of firms and total employment are computed by summing across each edition, so that $I = \sum_i I_i$ and $L = \sum_i L_i$.

It is important to note that c, s_i and l are random variables. Thus, each simulation involves a draw from a distribution for each variable and the computation of I and L.

The computation of s_i demands a more detailed explanation. First, we estimate a Weibull model and then we perform a linear prediction of the model assuming that all observations are non-treated (the "treated" dummy was set to zero for all observations for the prediction). Let λ_0 be the mean of the exponential of that variable within the sample of beneficiaries. Then, we compute the impact of the program on survival at time t as

$$s(t) = \exp(-\lambda_0 exp(\beta)t^p) - \exp(-\lambda_0 t^p)$$

where p is the parameter of the Weibull distribution, the first term is the expected survival at duration t, including the effect of the program (β is the coefficient of the "treated" dummy in the proportional hazard duration model), and the second term is the expected survival without this effect. Finally, given that our data specify t in months, we compute $s_{08} = s(48)$, $s_{09} = s(36)$, $s_{10} = s(24)$, and $s_{11} = s(12)$.

D Regression discontinuity approach

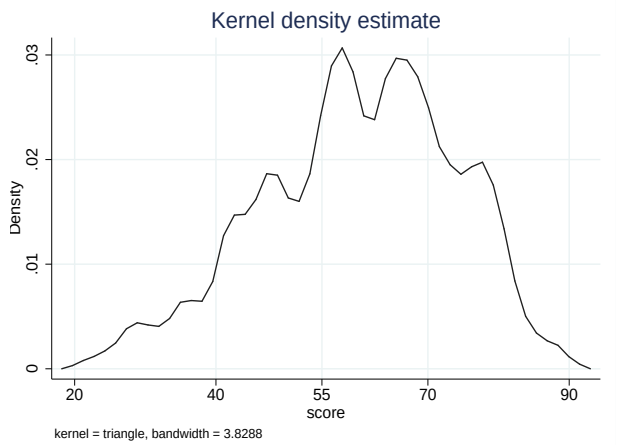
In this section, we verify the validity of the assumption underlying the discontinuity design. First, we analyze whether the covariates are continuous around the cutoff point.[14] To do so, we apply the methods described in Section 3: we analyze the continuity of age and

[14] Detailed results are available upon request.

education through local linear regressions and global regressions, finding no discontinuity. We then apply logit models to the gender and industry dummies and find no significant coefficients associated with the "treated" dummy. We also study the continuity of the other variables included in the applications such as the expected investment in the project, amount of subsidy applied for, and duration of the mentorship. We find no jump in these variables using either regressions or local methods. To check the joint significance of all variables, we also estimate both a probit and a logit model with the "treated" dummy as the dependent variable and independent variables including a quadratic of the score, age, and dummy variables for gender, education level, and project industry. We find that neither the joint test of significance nor the likelihood ratio test reject the null model that depends only on the score, with p-values above 0.9. In brief, there is no evidence that the covariates change on the two sides of the cutoff value of the running variable.

Finally, we present a Kernel density estimate of the distribution of the running variable. We find that the cutoff point is close to the mode of the distribution.

Figure D.2: Density of the running variable



Source: Administrative data from BAE.

E Tables

Table E.1: Logit startup

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	2.46***	2.36***	2.10*	2.02	2.03	2.75	3.52*
	(0.68)	(0.70)	(1.26)	(1.54)	(1.34)	(1.86)	(2.04)
2010 edition		-0.66	-0.68	-0.69	-0.41	-0.89	-16.47
		(1.23)	(1.25)	(1.26)	(1.31)	(1.23)	(4,227.64)
2011 edition		-0.52	-0.50	-0.50	-0.26	-0.41	-15.80
		(1.16)	(1.18)	(1.18)	(1.25)	(1.18)	(4,227.64)
Score			0.16	0.06	0.19		0.48
			(0.20)	(1.16)	(0.21)		(1.56)
Score squared			-0.00	0.00	-0.00		-0.01
			(0.00)	(0.02)	(0.00)		(0.01)
Score cubed				-0.00			
				(0.00)			
Tertiary					1.64		
					(1.42)		
College					-0.82		
					(1.01)		
Postgraduate					0.44		
					(0.76)		
Male					-0.61		
					(0.75)		
Age					0.08		
					(0.40)		
Age squared					-0.00		
					(0.01)		
Constant	0.62*	1.15	-3.32	-1.64	-6.05	-0.03	8.07
	(0.33)	(1.15)	(5.24)	(18.65)	(9.58)	(1.78)	(4,227.85)
N	108	108	106	106	106	104	59
Marginal	0.27	0.26	0.22	0.21	0.20	0.28	0.32
	(0.00)	(0.00)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)

Notes: Logit estimations (standard errors) of the probability that a startup is created on the treatment dummy and covariates. In columns (3) to (7), a polynomial of the score is included as a control. In column (6), a Chebyshev polynomial of degree 3 is used (coefficients are not reported). *, **, and *** indicate significance at the 10%, 5%, and 1% levels.

Table E.2: Logit survival

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	1.05	1.62*	4.16**	5.43**	4.31*	6.58**	8.27**
	(0.68)	(0.83)	(1.91)	(2.35)	(2.46)	(2.90)	(3.78)
2010 edition		0.05	-0.29	-0.44	-1.04	-0.59	-1.82
		(0.91)	(0.96)	(0.98)	(1.19)	(0.99)	(1.59)
2011 edition		2.26**	2.17*	2.07*	1.88	2.00*	2.33
		(1.09)	(1.14)	(1.15)	(1.23)	(1.14)	(1.79)
Score			-0.94	2.46	-0.86		-1.23
			(0.66)	(2.51)	(0.89)		(2.43)
Score squared			0.01	-0.06	0.01		0.01
			(0.01)	(0.05)	(0.01)		(0.02)
Score cubed				0.00			
				(0.00)			
Tertiary					-0.64		
					(1.49)		
College					0.00		
					(0.00)		
Postgraduate					1.00		
					(1.01)		
Male					0.34		
					(0.88)		
Age					-0.93		
					(1.15)		
Age squared					0.01		
					(0.02)		
Constant	1.44***	0.18	28.92	-30.48	44.47	-1.97	45.07
	(0.50)	(0.98)	(20.10)	(40.55)	(30.72)	(2.27)	(69.12)
N	91	91	90	90	83	89	52
Marginal	0.10	0.14	0.33	0.42	0.34	0.50	0.48
	(0.00)	(0.00)	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)

Notes: Logit estimations (standard errors) of the probability of survival on the treatment dummy and covariates. In columns (3) to (7), a polynomial of the score is included as a control. In column (6), a Chebyshev polynomial of degree 3 is used (coefficients are not reported). *, **, and *** indicate significance at the 10%, 5%, and 1% levels.

Table E.3: Cox proportional hazard model

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	-0.78	-0.76	-2.36*	-3.25**	-2.52	-4.02**	-4.42*
	(0.64)	(0.73)	(1.41)	(1.65)	(1.76)	(1.96)	(2.41)
2010 edition		0.60					
		(0.85)					
2011 edition		-0.70					
		(1.01)					
Score			0.69	-1.76	0.88		0.22
			(0.53)	(2.00)	(0.69)		(1.73)
Score squared			-0.01	0.04	-0.01		0.00
			(0.00)	(0.04)	(0.01)		(0.02)
Score cubed				-0.00			
				(0.00)			
Postgraduate					-0.66		
					(0.84)		
Male					0.16		
					(0.71)		
Age					0.91		
					(0.96)		
Age squared					-0.01		
					(0.01)		
N	89	89	88	88	88	87	51

Notes: Proportional hazard estimation (standard errors) of the duration of the firm on the treatment dummy and covariates. In columns (3) to (7), a polynomial of the score is included as a control. In column (6), a Chebyshev polynomial of degree 3 is used (coefficients are not reported). *, **, and *** indicate significance at the 10%, 5%, and 1% levels.

Table E.4: Weibull proportional hazard model

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	-0.83	-0.813	-2.334*	-3.174**	-2.415	-3.948**	-4.476*
	(0.63)	(0.735)	(1.375)	(1.596)	(1.718)	(1.894)	(2.300)
2010 edition		0.602					
		(0.845)					
2011 edition		-0.729					
		(1.013)					
Score			0.696	-1.693	0.846		-0.005
			(0.529)	(1.973)	(0.686)		(1.666)
Score squared			-0.005	0.039	-0.007		0.003
			(0.004)	(0.039)	(0.005)		(0.016)
Score cubed				-0.000			
				(0.000)			
Postgraduate					-0.582		
					(0.835)		
Male					0.150		
					(0.706)		
Age					0.931		
					(0.952)		
Age squared					-0.013		
					(0.013)		
Constant	-6.64***	-6.53***	-27.53*	14.15	-48.43**	-5.46**	-13.91
	(1.45)	(1.82)	(15.92)	(32.68)	(23.99)	(2.20)	(44.40)
ln p	0.46*	0.431	0.497*	0.494*	0.485*	0.514**	0.659**
	(0.26)	(0.274)	(0.259)	(0.260)	(0.263)	(0.260)	(0.335)
p	1.58	1.538	1.644	1.639	1.624	1.672	1.933
N	89	89	88	88	88	87	51

Notes: Proportional hazard estimation (standard errors) of the duration of the firm on the treatment dummy and covariates. A Weibull distribution is assumed. In columns (3) to (7), a polynomial of the score is included as a control. In column (6), a Chebyshev polynomial of degree 3 is used (coefficients are not reported). *, **, and *** indicate significance at the 10%, 5%, and 1% levels.

Table E.5: OLS on Initial employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	1.24***	1.17**	1.49*	2.96***	2.36**	2.82***	2.63
	(0.45)	(0.46)	(0.86)	(1.00)	(0.92)	(1.02)	(1.65)
2009 edition		-0.97	-0.52	-0.45	-0.77	-0.49	2.38
		(0.89)	(0.89)	(0.86)	(0.94)	(0.86)	(2.08)
2010 edition		-1.36	-0.99	-0.98	-1.75*	-0.98	1.27
		(0.84)	(0.87)	(0.84)	(0.93)	(0.84)	(2.06)
2011 edition		-0.63	-0.25	-0.32	-0.91	-0.32	2.13
		(0.78)	(0.78)	(0.76)	(0.91)	(0.76)	(2.00)
Score			-0.40**	1.73**	-0.44**		-0.60
			(0.19)	(0.84)	(0.20)		(1.34)
Score squared			0.00**	-0.04**	0.00**		0.00
			(0.00)	(0.02)	(0.00)		(0.01)
Score cubed				0.00**			
				(0.00)			
Tertiary					0.69		
					(0.74)		
College					1.25		
					(0.85)		
Postgraduate					0.12		
					(0.49)		
Male					0.92*		
					(0.49)		
Age					0.03		
					(0.20)		
Age squared					-0.00		
					(0.00)		
Additional controls	N	N	N	N	Y	N	N
Constant	2.84***	3.69***	14.11**	-21.02	15.82**	2.72**	18.67
	(0.38)	(0.85)	(5.69)	(14.52)	(7.30)	(1.04)	(38.27)
Rˆ2	0.08	0.12	0.19	0.26	0.36	0.26	0.17
N	88	88	87	87	86	86	49

Notes: OLS estimations (standard errors) of employment during the first year of the startup on the treatment dummy and covariates. Additional controls include industry of the project and firm's age. In columns (3) to (7), a polynomial of the score is included as a control. In column (6), a Chebyshev polynomial of degree 3 is used (coefficients are not reported). *, **, and *** indicate significance at the 10%, 5%, and 1% levels.

Table E.6: OLS, employment in 2011

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	1.91	1.22	3.07	3.30	2.73	3.30	0.38
	(1.15)	(1.13)	(2.39)	(2.10)	(2.97)	(2.10)	(1.63)
2009 edition		-6.21***	-4.78**	-5.13***	-6.29***	-5.13***	-1.89
		(2.06)	(1.88)	(1.66)	(2.06)	(1.66)	(1.87)
2010 edition		-6.70***	-4.62**	-5.22***	-6.68***	-5.22***	-0.75
		(1.96)	(1.87)	(1.65)	(2.04)	(1.65)	(1.86)
2011 edition		-6.21***	-4.75***	-4.89***	-6.80***	-4.89***	-0.83
		(1.73)	(1.60)	(1.41)	(1.89)	(1.41)	(1.77)
Score			-2.17***	13.74***	-1.66*		-0.59
			(0.73)	(3.50)	(0.92)		(1.28)
Score squared			0.02***	-0.25***	0.01**		0.01
			(0.01)	(0.06)	(0.01)		(0.01)
Score cubed				0.00***			
				(0.00)			
Tertiary					0.12		
					(1.63)		
College					3.53*		
					(1.89)		
Postgraduate					-0.36		
					(1.12)		
Male					2.07*		
					(1.13)		
Age					0.21		
					(0.42)		
Age squared					-0.00		
					(0.00)		
Add. controls	N	N	N	N	Y	N	N
Constant	2.95***	9.21***	68.85***	-240.4***	50.51*	4.05*	17.98
	(1.00)	(1.93)	(22.02)	(69.66)	(27.89)	(2.24)	(36.54)
R^2	0.03	0.19	0.37	0.52	0.52	0.52	0.16
N	79	79	78	78	76	78	45

Notes: OLS estimations (standard errors) of employment in 2011 on the treatment dummy and covariates. Additional controls include industry of the project and firm's age. In columns (3) to (7), a polynomial of the score is included as a control. In column (6), a Chebyshev polynomial of degree 3 is used (coefficients are not reported). *, **, and *** indicate significance at the 10%, 5%, and 1% levels.