Public funding for startups in Argentina: an impact evaluation

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Abstract

We evaluate the impact of a policy aimed at promoting innovative startups in Buenos Aires City. Each year the program selects about 50 out of 100 applicants and provides them with funds and technical assistance. We conducted a special survey to evaluate the impact of this policy. To identify the effects we use regression discontinuity methods exploiting the characteristics of the selection into the program. We estimate a significant impact regarding the rates of creation and survival, and the labor demand. An overall appraisal shows that the cost of each of the additional one thousand jobs was about $4,000.

1 Introduction

Business creation is crucial for economic development. This is the conclusion of a wide literature on firm dynamics. For example, new businesses have higher growth rates and, even though they start being relatively small, they represent an important share of job creation. At the same time, a considerable proportion of new businesses are tilted towards new markets and innovative sectors, which helps to increase the diversity in economic activity. Crucially, new businesses foster productivity growth.[1]

However, business creation is a complex process, in which multiple factors constrain the possibilities for good ideas to develop into commercial projects, for the projects to become startups and for the firms to grow and formalize, becoming competitive units.

In this context, it is important to analyze the actual effects of policies targeted at entrepreneurship and business creation. To what extent can public policies improve the chances for good ideas and projects to become real businesses? Are these policies resulting in high deadweight losses or they do genuinely generate new firms? Do policies help businesses to overcome their main obstacles for obtaining profitability? It is important to address these questions in order to provide valid intervention alternatives for local and national governments that intend to promote the creation of innovative firms.

Aiming at addressing this topic, we evaluate the program *"Buenos Aires Emprende"*. This policy supports entrepreneurs with their startups, providing them with funds and technical assistance. Transfers can constitute as much as a 40% of initial investment. Technical assistance is provided by specialized institutions in entrepreneurship, and it involves a tutorship that can last up to one year. To identify the impact of the program we exploit the particularities of the selection process: each application receives a score according to the entrepreneurial ability and to its economic viability; those that are above a cutoff are selected as beneficiaries. This process allows us to use regression discontinuity methods, in which the score is a continuous variable and the approval depends on a deterministic rule that defines selection into treatment.

To perform the impact evaluation, we conducted a survey among the pool of applicants, including both beneficiaries and non-beneficiaries. We collected information about firm creation, survival and other outcomes, including sales, net earnings and employment. The results show that the effects on the business creation and survival are strong and significant, as well as the impact on employment.

[1] See Foster, Haltiwanger, and Krizan (2002), Scarpetta, Hemmings, Tressel, and Woo (2002) and Bartelsman, Scarpetta, and Schivardi (2003) for measures of the importance of new firms on productivity growth and employment. For a more structural analysis see Kortum (1997), Klette and Kortum (2004) and Lentz and Mortensen (2008) among others.

This study aims at contributing to the empirical literature on impact evaluation of entrepreneurship programs. Despite its relevance, the impact evaluation is not a widespread practice regarding this kind of interventions. In most cases, it restricts to monitoring the implementation, the participation rate and the degree of satisfaction with the services provided (Storey 1998). This lack of empirical studies is even more noticeable in developing countries (López-Acevedo and Tan 2011).

Our paper relates indirectly to to those papers of the development literature that analyze the impact of providing capital to small businesses. An example is the study of de Mel, McKenzie, and Woodruff (2008), which presents an experiment conducted in Sri Lanka.

We also relate with those papers that analyze the effect of venture capital. For example, Kerr, Lerner, and Schoar (2010), use a regression discontinuity analysis to identify the effect of the financing provided to entrepreneurships by "angel" investors groups, where the forcing variable is the interest of individuals of the group on financing the project.

The paper follows with a description of the program that we want to evaluate (Section 2), the survey that we conducted and the methods that we implemented (Section 3). We summarize our findings in Section 4 and in Section 5 we provide an estimate of the overall impact of the program. In Section 6 we offer some concluding remarks.

2 The program Buenos Aires Emprende

The government of Buenos Aires City (GCBA) has implemented several policies to encourage business creation. From a general perspective, these policies aim at increasing the quantity and quality of new businesses in the city, by providing assistance and financing. In particular, the program Buenos Aires Emprende (BAE), in which we focus, aims at identifying and promoting entrepreneurs with innovative ideas.[2]

Behind the design of the program is the idea that the most important determinant of success of a startup is the ability of the potential entrepreneur: a really talented entrepreneur, under proper conditions, would eventually succeed in creating an innovative firm, with potentially huge impact in productivity, job creation and externalities over other firms. Nevertheless, this talent is not easy to identify *ex-ante*. There are many different tasks at which an entrepreneur should excel to

[2]Within the policies of the GCBA, the most outstanding programs include Desarrollo Emprendedor, which aims at generating a greater spreading of the practice and the entrepreneurship culture; the program BAITEC, which is dedicated to the incubation of technology-based ventures; the program INCUBA, focused on projects of the design, tourism and culture industries; and the program Buenos Aires Emprende (BAE).

overcome multiple obstacles and many personal characteristics (related to risk aversion, confidence, etc.) that are required to face them. Thus, there is no obvious test of entrepreneurial ability and no simple way to identify talent. For this reason, the design of the program is oriented to this objective. First, it uses independent NGO's, generally universities with MBA programs (see Table 1) to promote the program and to identify possible talented entrepreneurs. Second, part of the selection process is based on an in depth interview with psychologists and other specialized professionals that help the team of BAE to evaluate entrepreneurial ability of each applicant to the program. Third, the focus of the program is not on promoting a particular activity or sector but on innovative ideas of potentially able entrepreneurs. It is important to emphasize that the government program holds all the decisions: selects NGO's, evaluates projects and entrepreneurs, selects them, provides the benefits and implements the monitoring process.

Table 1: Institutions and number of sponsored projects (2010 and 2011)

Type	**Name**	**Projects sponsored**
Universities		*111*
	Universidad de Buenos Aires	25
	ACES-IAE	20
	CEMA	17
	UDESA	14
	UAI	11
	EAN	9
	UTN-BA	8
	ESEADE	6
	FGP-UTN	1
NGO		*106*
	EMPREAR	28
	FUNDES	22
	IECYT	18
	ENDEAVOR	14
	POLO-IT	9
	CAI-UB	3
	CPCE	3
	BAIREXPORT	9

Notes: The table lists all the institutions (classified in "universities" and "NGO's") implied in BAE in the editions 2010 and 2011 of the program. Each institution identified potential entrepreneurs and guided a number of projects through the application procedure.
Source: Survey to applicants to BAE program.

The full-scale implementation of the program dates to 2008. Each edition, beginning each calendar year, is structured in several consecutive stages:

- In the first stage, NGO's, universities and institutions specialized on entrepreneurship are selected. There are about 10 institutions involved in each edition of the program, and their role is to identify potential beneficiaries, to assist them in the design of the business plan and to guide them through the application process. Each institution sponsors these projects (generally close to 10), which are submitted to BAE.

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

  - "project viability", focused in the analysis of the project itself, diagnostic, projections, estimations, internal consistency, among other issues. (An application can have a maximum of 40 points in this dimension.)
  - "entrepreneurial ability" is evaluated through an in-depth interview with the applicants, analyzing past experience, leadership, commitment and in depth knowledge of the project, as well as entrepreneurial attitude. (A maximum of 60 points are assigned to each project after this dimension.)[3]

- The program selects as beneficiaries all the presented projects that reached the score of 55 points or more. The selected projects and their sponsors receive the transfers of the program. The benefits include:

  - a cash transfer or grant for the beneficiaries up to a AR$ 70,000 (in 2011, this was equivalent to US$ 16,000), or up to 40% of the initial investment of the project;
  - the institutions receive a premium of AR$ 3,000 (US$ 750) for each selected project, and AR$ 2,200 monthly during the tutorship 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 transfer to the entrepreneur, which represents about a total of US$ 5,600 to the institution.

Table 2 summarizes the number of institutions, applications and beneficiaries, as well as total transfers provided.

[3]As an example, 12 points of the score of entrepreneurial ability were given after considering the past experience of the entrepreneur. If the entrepreneur "has a relevant experience in similar businesses that would be useful" for implementing the project, the entrepreneur gets the maximum 12 points; if the entrepreneur "does not have any experience as entrepreneur or in the business" then the entrepreneur gets no points on this. See Appendix A for further description of the selection process.

Table 2: The program Buenos Aires Emprende

	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
Transfer, th. of nominal AR$	1 781	2 323	2 334	3 202

Source: Administrative data from BAE.

3 Data and methods

The impact evaluation uses data provided by BAE and from a specific survey carried on for this research. This survey complements administrative information and focuses on the performance of the firm. In this section we will briefly explain the characteristics of our survey and of the respondents. We will also present the methods for identifying the effect of the program.

3.1 Survey to entrepreneurs

This study is based on a specific survey distributed in 2012 to the more than 400 entrepreneurs that participated in the program from 2008 to 2011, regardless of whether they have been beneficiaries or not. We contacted entrepreneurs using the information provided by BAE, including name, e-mail and phone numbers of the entrepreneurs. This information were recovered from the original application forms and projects. The survey gathers information about the project (industry, initial capital), the entrepreneur (age, education), and some outcomes such as survival, sales, profits and employment. It was answered by 108 firms/entrepreneurs, including 68 beneficiaries and 40 non-beneficiaries (see Table 3).[4]

Table 3: Survey - Beneficiaries and non-beneficiaries in the sample

	2008	2009	2010	2011	Total
Beneficiaries	7	11	14	36	68
Non-beneficiaries	0	4	11	25	40
Total	7	15	25	61	108

Source: Survey to participants of BAE program.

The Table 4 shows some descriptive statistics of the sample. In a nutshell, entrepreneurs have a mean of 36 years of age, 70% of them are men, 40% are college

[4]Importantly, the response rate is affected by the several monitoring surveys that BAE implemented, which progressively reduce participation and response. In Appendix B we provide more details about the survey and the questionnaire.

graduates and 40% have a master's degree. Projects are diverse but a 33% of them are in IT related technologies while 16% are in manufacturing.

Table 4: Descriptive statistics of 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
Women	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. College	0.09	0.29	0.10	0.30	0.09	0.29	0.84
Edu. Posgr.	0.42	0.50	0.40	0.50	0.43	0.50	0.79
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		.

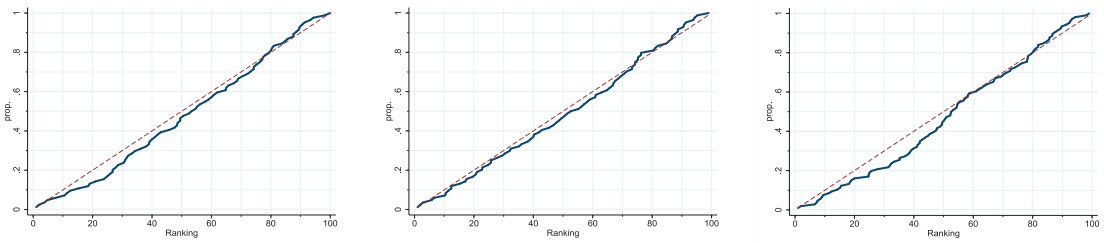
Notes: The table shows mean and standard deviations of relevant variables, comparing beneficiaries and non-beneficiaries. The last column is the probability of equality of means using t-test. Two-sample Wilcoxon and Kolmogorov-Smirnov tests of equality of distribution functions applied to four education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

An important issue is whether respondents are a particular selection of the universe. We find that the data is consistent with a random sample of the entrepreneurs that participated in the program. To show this we have generated a variable that represents the ranking that BAE awarded to each project within participants of each edition of the program. This variable has been rescaled to represent a uniform distribution between 1 (the highest score of the edition) to 100 (the worst score of the edition). The variable was generated for all projects presented to BAE (including those that did not answer our survey) in each year and for each of the dimensions: (i) the ability as an entrepreneur and (ii) the viability of the project. We call this variable "BAE's ranking". It is important to note that we only have information about each of the two components of score for years 2010 and 2011. We also computed the overall ranking (taking into account overall score as the sum of the two dimensions). This last includes the all four editions of the program.

Figure 1 shows the empirical cumulative distribution function in our sample of this variable. Thus the graphs show the proportion of the observations that has been classified below a given level in the BAE's ranking. For the universe this graph would be a 45 degree line as in a uniform distribution. Our sample is not different from that distribution. From panel (c) it can be said that, if something, the sample is biased against the best projects: the proportion of respondents ranked by BAE

in the first 4 deciles is about 30%. Nevertheless, the difference is not substantial.

Figure 1: Empirical cumulative distribution function of BAE's 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 to participants of BAE and program data.

We also analyzed the balance on other variables of those in the sample compared to the universe of applicants. We found that there is no difference between those who answered the survey and those who did not according to the characteristics of the project, meaning that industry, size of the project (amount of declared investment) and overall score. Moreover, all these variables combined do not explain the probability of response (a probit model of the probability of response on these variables generate a pseudo R2 of less than 0.03 and a likelihood ratio test comparing the model without explanatory variables gives a p-value of 0.25).

3.2 Discontinuity methods

In order to evaluate the policy, we exploit the selection process, that scores each project (as explained in Section 2) and selects those applications with a score higher than 55.[5] Thus, the selection into the program is nonrandom, and there could be systematic differences between the treated and the non-treated. Discontinuity methods exploit this deterministic discontinuity of the forcing variable to identify the impact of the treatment. Intuitively, the assumption is that individuals near the cutoff value of the forcing variable are not essentially different and the relation between the forcing variable and the outcomes is continuous and, thus, any differences in outcome from observations above the cut off and below it is due to the treatment.[6]

[5]While the whole process is similar in every edition, both the scoring and cutoff values slightly change. See Appendix A for details.

[6]This methodology has been applied by Thistlethwaite and Campbell (1960), to analyze the impact of merit awards on future academic outcomes; by Angrist and Lavy (1999) to evaluate

Importantly, the evaluation is done by BAE (and not by NGO's) and both evaluators and applicants are aware about the characteristics that projects and entrepreneurs should have. In this sense, the procedure of scoring is no different from those regression discontinuities 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.

Throughout the paper we will choose to identify the effect of the program using the following regression:

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

where y_i is an outcome variable (income, benefits, employment, etc) of the startup, D_i identifies selection into treatment (a dummy variable, that takes the value one if the observation corresponds to a beneficiary and zero otherwise), Z_i are variables that define a function of the score (quadratic in the main specification) and summarize the continuous effect of the score on the outcome, and X_i are additional controls (a quadratic function of age, and dummies for gender, completed education, industry and year of edition of the program). Following discontinuity methods, the coefficient β is the estimator of the impact of the policy on the performance for the individuals around the cutoff value.

We also apply an analogous method for non-linear models, such as logit and duration models, where left hand side variable should be reinterpreted accordingly (as a latent variable or a function of the hazard rate). In this, we relate to the literature that uses discontinuity methods for analyzing survival rates, for example of the duration of unemployment, as in Card et al. 2007, Schmieder et al. 2009, González-Rozada et al. 2011.

In addition, we identify the impact of BAE by estimating local linear regressions on both sides of the cutoff value. This method reduce the sensitivity of results to the particular function of the score specified in the regression. We present the results of this method in Section 4 and we include a graphical analysis in the Appendix D.1.

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

the impact of the classroom size on educational outcome (see also Hoxby (2000)); and Pettersson-Lidbom (2008) to estimate the impact of political parties on fiscal policy among other applications. See van der Klaauw (2008) for a survey.

Nevertheless, it is important to emphasize that it is implausible that selection into treatment were to generate spurious effects through substantial manipulation. First, if the evaluators were to privately observe some signals (for example the connections or charisma of entrepreneurs) they can introduce these observations into the scoring process (in fact these variables are potentially important for the future success of the startup). Thus, using the score as a control would eliminate these type of effects.

Second, any manipulation of the scoring process that diverge from the objective of selecting those more able or with better prospects would reduce the estimates of the effects. In other words, this type of manipulation would bias estimates towards zero.

Finally, it is important to emphasize the fact that applicants and projects presented at BAE are similar, even comparing those with the high score and with low score. This is partly because of the characteristics of the program which is restricted to innovative projects and because of the ONG identification of potential beneficiaries is probably drawn from a rather homogeneous group. In Table 4 we compare the characteristics of beneficiaries and non-beneficiaries, including age, gender, education and sectors of the project, as well as experience as an entrepreneur and the record of failures in previous startups. We find no substantial difference between them. We also run one-sample and two-sample tests on these characteristics and conclude that there is no difference in mean or distribution (null hypothesis of equality is not rejected and probability of null is higher than 50% in all tests; in the last column of the Table we present the probability of the null of mean tests for each variable). We analyzed education more closely and found no relationship with score using local linear regressions. Additionally, projects does not seem so different across all the support of the score.[7] The fact that these observable characteristics are not different is important in the sense that reduce the relevance of focusing the analysis close to the cutoff value and allows for a more global estimation such as regressions described by equation (1), increasing the number of relevant observations.

4 Results

In this section we will present the main results of our study. We will focus on the effects of the program on creation and survival of the firm. We will then discuss the

[7] As an example, consider the applicants to the 2011 edition. The application with the lowest score was a web development for medical recording and appointment management; the application with the highest score was a web platform for sharing and management of audiovisual files; those projects close to the cutoff point (with scores close to 55) were a web page for selling office supplies (beneficiary) and a web page for managing and promoting student exchange. All these four ideas are not substantially different between each other at first sight.

effects on employment, sales and net income.

We present in here three specifications of 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 and we add only the identification dummies for the year of edition of the program. In a second specification we add to these some covariates such as a quadratic function of age, and dummies for gender, industry and education. Finally, a third specification restricts the sample to those observations whose score is between 45 and 65, that is to say, closer to the cutoff value, and we consider only a quadratic function of the score and the dummies of year of edition. Of course, in all the specifications the effect of the program is identified by the coefficient associated to the "Treated" dichotomous variable, D_i in equation (1). We also analyzed alternative specifications that we present in the Appendix.[8]

For continuous variables (employment, sales and income) we apply the local linear regression approach. We present these results in this Section and in Appendix D.1.

4.1 Creation, survival and duration

Creation There are a number of difficulties that entrepreneurs must overcome to successfully develop a business. Financial frictions build the most obvious and perhaps important barriers, but also lack of information and high fixed start-up costs could be relevant. The first objective of the program is to generate the conditions so that good ideas are implemented and become profitable firms. To analyze this effect, we compared the business creation rate of beneficiaries to that of non-beneficiaries. We found a strong difference between these two groups: 70% of non-beneficiaries started their projects, while this figure goes up to 97% among beneficiaries.

These differences are in part explained by observables and by the different quality of projects. To address this problem we implement the regression discontinuity methods as presented in Section 3. In particular, we run a logistic regression controlling for the effect of the score. In Table 6 we present the main results. We find a very strong and significant impact of being a beneficiary. Specifically, the coefficient associated with the dummy "Treated" is 2.1, being significant at 10%. The marginal

[8]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 simple differences between beneficiaries and non-beneficiaries with no additional controls (column 1) or just with year of edition dummies (column 2). The following columns implement 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). Other controls that we considered include a quadratic function of age, and dummies for gender, industry and education (tertiary, college, postgraduate, being the base group those with completed High School). The results presented in the main text correspond to columns 3, 5 and 7.

Table 5: 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

Source: Survey to applicants to BAE program.

effects show that the "treatment" increases 22% the probability of business creation. The effect is robust to including a set of controls in the regression (see column 2) but we must aknowledge that the significance level decrease. When we restrict the analysis to the observations in the neighborhood of the cutoff (those with a score between 45 and 65) the effect is higher, with a significant marginal effect of 32%.

Table 6: Creation rate - 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

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 score between 45 and 65. A */**/*** next to coefficient indicates significance at 10/5/1% level. The last rows reproduce the marginal effect (and standard errors) of the "Treated" variable.
Source: Survey to applicants to BAE program.

To sum up, the effect of being approved increases the likelihood of implementing the project between 20% and 33%, as estimated marginal effects. This is a very important and strong effect: if the program had not been in place about 22% of the beneficiary's firms would not have been created. This result implies almost 50 businesses generated by the program in the four editions (233 projects were approved). This is a first approximation of the impact of the program that is worth highlighting.

Survival A second important aspect of the program is that it aims to improve the survival of beneficiary businesses. In particular, new and innovative companies can have a high exit rate, not necessarily by flaws of the project itself, but because

of liquidity constraints, inadequate initial scale, or transitory technical and market problems. To address this issue we focus on the probability of firms' survival. We find that the probability of continuing the project is higher within the group of beneficiaries compared to the non-beneficiaries: 79% among the latter and 92% in the former group.

Table 7: 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

Source: Survey to applicants to BAE program.

To identify the effect of the program we performed a logistic regression on the variable that identifies that the firm was still in place at the moment of the survey. The results shown in Table 8 reveal that the probability of survival among beneficiaries is significantly higher compared to non-beneficiaries. In fact, the "Treated" dummy that identifies an approved project (beneficiary of BAE) is significant at 5% probability and the implied marginal effect is above 33%. This result is robust to including additional controls (column 3 of Table 8) and even stronger when we restrict the sample to those observations in the neighborhood of the cutoff (column 3).

Table 8: Survival rate - 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

Notes: Logit estimations (standard errors) of the probability of survival on treatment dummy and a quadratic function of the score. See notes from Table 6.
Source: Survey to applicants to BAE program.

Duration To go a step further, we perform a duration analysis. These methods allow to deal with the fact that firms started in different periods, that eventually exit the market at different durations and to take into account the information provided by right censored durations.

For this analysis, we defined the duration (age of the firm) as the number of months from its creation until the survey or until its exit from the market.[9]

We estimate the effect of the program using a proportional hazard Cox model.[10] The results of these estimations, presented in Table 9, show that being beneficiary reduces the exit probability. In particular, the coefficient associated to the "Treated" dummy is -2.37 when we control for a quadratic function of the score and -2.43 when we add more covariates. The effect goes to -4.42 when the analysis is restricted to the neighbourhood 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 9 the effects are even stronger with this model.

In other words, the proportional hazard model confirms that beneficiaries have higher survival rates in duration compared to non-beneficiaries. These coefficients imply that the hazard of beneficiaries is one tenth of that of non-beneficiaries.

Table 9: 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)

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

To conclude, these results show that BAE has attained 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: without the BAE, a relevant proportion of firms currently in business would not have existed at the moment of our survey.

[9] As a robustness check, we alternatively defined duration as the number of months since BAE's edition. We also imposed censoring at different durations (for example, at 48, 36 or 24 months). We always found similar or stronger effects.

[10] This is a proportional hazard model, in which the 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 is identified after the estimation of coefficients θ. Notice that the hazard refers to the probability that the firm cease its operations at a given duration. Thus, a negative coefficient in the "Treated" dummy implies that being beneficiary increases survival rate.

4.2 Employment, sales and net income

Besides creation and survival, the program aims at generating sustainable and competitive firms. To analyze this issue we concentrate on the performance of the firms. In our survey we reviewed the employment, sales and net income of the firm, both during the first year after the application to BAE and in the last year (2011). Before going to the results we have to emphasize that these last variables were measured on a selection of projects: those that were created and, in the case of variables measured in 2011, that survived until the moment of the survey. Given that the selection process seems to be more severe for the non-beneficiaries, it is plausible to expect milder differences between the two groups.

A first rough approach is to compare the mean by group. We find that beneficiaries do have higher initial employment, sales and net income and that they also have higher employment and income in 2011.

Table 10: 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 to applicants to BAE program.

We now turn to asses the impact of the program using the type of regressions as presented in equation (1). As in previous exercises, we considered different specifications. We present in this section the more relevant ones and refer to Appendix E for a detailed presentation of results.

Furthermore, these continuous variables have been analyzed through local linear regressions. We leave to the Appendix D.1 the presentation of graphs and while we briefly comment the tables in this Section.

Employment In the first column of Table 11 we show the values (and standard deviations) of the coefficient associated to the "treated" dummy when we regress the number of jobs on a quadratic function of the score, when we add some controls and when we restrict the sample to those with a score between 45 to 65. We consider both employment in the first year after BAE and employment in 2011, in panel A and B respectively.

These results suggest that beneficiaries have larger firms in terms of employment. In particular, initial employment is 1.5 higher for beneficiaries when we control for the quadratic function of the score. The impact is higher and above 2.4 when we

Table 11: 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)
(b) Controls	2.36**	-0.013	0.157
	(0.92)	(1.196)	(1.131)
(c) Subsample	2.63	0.444	1.022
	(1.65)	(1.760)	(1.089)
B) 2011			
(a) Quadratic	3.07	-0.246	1.061
	(2.39)	(1.004)	(0.835)
(b) Controls	2.73	-1.309	1.861
	(2.97)	(1.733)	(1.589)
(c) Subsample	0.38	0.533	0.819
	(1.63)	(1.329)	(0.746)

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 dummy and a quadratic function of the score.
Source: Survey to applicants to BAE program.

include additional controls and when we restrict the sample around the cutoff point. The impact of BAE on employment in 2011 is less precise. While point estimates are higher, they are not significantly different from zero when considering standard deviations.

On the whole, estimates through regressions would indicate a difference of initial employment of about 2 jobs, and a more imprecise but positive effect for employment in 2011.

In Table 4.2 we show the estimates of the impact of BAE on the employment through local linear regressions. According to this, being a beneficiary increases employment in about 3 jobs for initial employment and 2.4 for employment in 2011. We also present the results when instead of using the optimal bandwidth we double it. For initial employment, the estimated impact goes down to 2.4, while for employment at 2011 goes down to 0.93 and fails to be significant.

Sales In column 2 of Table 11 we show the values (and standard deviations) of the coefficient associated to the "treated" dummy when we regress the the logged deflated sales of ventures. We consider both sales during the first year of the startup and sales in 2011. We find that there seem to be no significant differences between

Table 12: Local RD estimates

Variable	(1) Employment first year	(2) Sales first year	(3) Income first year	(4) Employment 2011	(5) Sales 2011	(6) Income 2011
Opt.Bandwidth	3.306***	0.294	1.639***	2.421**	-5.494***	-1.355**
	(0.986)	(0.900)	(0.479)	(1.076)	(0.963)	(0.555)
2xOpt.Bandwidth	2.410**	1.388	2.295***	0.929	-1.246	1.178
	(1.139)	(0.958)	(0.536)	(1.292)	(1.798)	(0.934)

Notes: Local linear regressions estimates (standard errors) using optimal bandwidth from Imbens and Kalyanaraman (2009); in the case of sales and income in 2011 we increased the bandwith 30%. A */**/*** next to coefficient indicates significance at 10/5/1% level.

the two groups. This is the case when we control for a quadratic function of the score, when we consider additional controls and when we consider a subsample. Also, estimates of local linear regressions show that the impact of the program on sales is not significant. (We have to acknowledge that the estimate of the impact of 2011 sales provides a negative and significant value, but this result is not robust to changing the bandwidth.)

Net income Net income of beneficiaries fails also to be significant when we regress logged deflated net income on the score and other controls (see column 3 of Table 11). Nevertheless, local linear regressions suggest a positive and significant effect of the program on initial net labor income of entrepreneurs between 1.8 and 2. There seem to be no robust effect on net income in 2011 (we found that estimates change with the bandwidth).

5 Assessment of overall impact and discussion

In Section 4 we have argued that the program BAE has had some important effects, improving the probability of business creation, its chances of survival and the size of the firm in terms of employment. The impact of BAE on other outcomes, such as sales and net income, are less clear, though. Both sales and net income are highly dispersed, what can affect the robustness of any measurement.

In this Section we will try to provide a global assessment of the impact of BAE. For that purpose, we first choose an estimate of the impact of BAE on each of the outcomes (creation, survival and employment). Secondly, we will use these estimates to simulate the total impact of the program.[11]

[11] We are assuming throughout the paper that there is no displacement effect of the BAE, in the sense that the program is not diverting any other source of financing or reducing the performance of other competing start-ups or firms. Given the low scale of BAE program this "partial equilibrium" analysis seems to us a reasonable assumption.

In Table 13 we show our preferred estimates. In particular, we picked a point estimate and a standard deviation from results above, choosing the specification of the model in which we use a quadratic function of the score and dummies for year of edition of BAE. We selected the Weibull duration model because it allows us to simulate a baseline survival rates. We also add the effect of employment (in 2011). These coefficients choices imply that BAE has an impact on creation of 22%, that the survival for beneficiaries would be about 43% at 24 months in absence of the program and that beneficiaries create about 3 additional jobs. We assume no direct impact on sales or net income, but of course higher number of firms imply larger aggregate sales and profits.

Table 13: Impact Estimates

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

Notes: This table reproduces results presented in previous tables and that are used for simulation.
Source: Survey to applicants to BAE program.

We now turn to explain our method to compute the overall impact of the program. The main objective is to simulate a counterfactual by assuming that, if BAE had not been in place, some of the beneficiaries would not have created their firms, of those created some would have disappeared from market, and even those that survived would have created less jobs. We use the estimates shown in Table 13 to compute this counterfactual scenario. To explicitly show the distribution of estimates we perform this exercise 10 thousand times with independent draws of impact values using mean and standard deviations from Table 13.[12]

In Table 14 we present the results of these simulations. On the whole, if BAE was not in place from 2008 to 2011, 132 firms would have not existed in 2012, and about one thousand jobs would have not been created.

In Figure 2 we show the distribution of the estimates of employment. Clearly, median and mean measures do not diverge substantially and the overall effect is significantly different from zero, ranging from 200 to almost 2 thousand jobs.

These estimates are modest in terms of absolute values. Nevertheless, given the direct cost of the BAE in transfers we can compute the cost of each additional job created by dividing 2011 inflated values of BAE by total impact on employment.

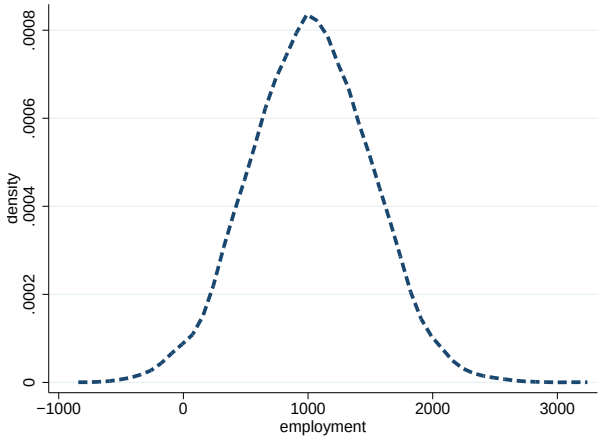
[12]In Appendix C we provide more details about this exercise.

Table 14: 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

Source: Survey to applicants to BAE program. Results from simulations as detailed in main text.

Figure 2: Overall impact over employment



Source: Survey to applicants to BAE program. Overall impact over employment. Results from simulations.

This gives less than US$ 4 thousand by each created job.[13]

In Parker (2009) there is an extensive review the literature on the evaluation of policies aimed at financing entrepreneurs and to promote innovation. Besides the methodological differences, there are two main estimates that are comparable to our results: "additionality" and the cost per job of the program.

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

[13]The average formal wage was about 1300 in 2011. Thus, cost-per-job is about 3 months of wages.

their ventures without the policy, a very different method compared to ours.

On the other hand, the estimates on job creation vary widely but cost-per-job figures are generally above $ 10,000.

In our case, the estimates imply an "additionality" of about 50% including both creation and survival and a cost-per-job of about $ 4,000.

Given these effects, it is important to understand which is the economic mechanism by which the program causes these impacts and what are the implications of this evaluation for program design.

We conjecture that the non-refundable transfer is the most important instrument by which the program generates effects on firm creation based on two observations: (i) direct qualitative responses of surveyed entrepreneurs and (ii) the relevance of monetary incentives and liquidity provision. First, some respondents directly pointed out that receiving these benefits changed their decision, and entrepreneurs that did not begun their projects pointed out that the reason was the lack of financing by BAE. Secondly, this type of transfer is important in a context of underdeveloped financial markets and financially constrained entrepreneurs. Lack of financing is the other reason why entrepreneurs answered that they could not begun their projects. We also found that only 45% of projects had access to financing (besides BAE) and this financing was mostly provided by family and friends (70% of those with financing). Moreover, the non-refundable transfer also changes in a relevant way the opportunity costs of entrepreneurs: it has to be taken into account that most of the applicants are college graduates that could earn relatively high wages as employees; the program could have changed in the margin their decision of taking a chance as entrepreneurs.

Besides firm creation, BAE also had an impact on survival and job creation. Transfers could have been important for these results also through higher initial investment and more effort by the entrepreneurs,[14] but also the tutorship could have helped to better manage the firm. The effect of each instrument cannot be clearly disentangled through our data.

Success for public programs is not determined solely by the instruments used but there are many aspects of the design and the context that should be taken into account. We want to emphasize some ot the characteristics of BAE that could have been relevant for its results. First, the program concentrates on identifying and promoting entrepreneurial talent and innovative ideas, and avoids just subsidizing firm creation or focusing on particular sectors or activities. Second, the link with NGO's could have helped the program in the identification of entrepreneurial talent,

[14]We found, for example, that 56% of entrepreneurs spent more than 40 hours a week in the project among beneficiaries compared to 37% of non-beneficiaries. Lower opportunity costs of leaving alternative sources of income could be behind this difference.

a highly difficult task. Finally, the selection process, combining project appraisal techniques with the evaluation of the entrepreneurs themselves in a flexible manner seems also very relevant in the results.

Finally, our evaluation suggests that the program is effective and should be upgraded in several ways. First, a larger scale might be important to increase overall impact on firm creation in the City. Second, the design should also consider promoting projects during second or third year of creation. A new process of selection, in this case based on actual results of the project, could improve the chances of selecting the good entrepreneurs with good ideas which, at the same time, are still struggling with the problems of a new firm and in a very risky phase of a project. Additionally, considering that the firm is already in place, BAE should also build a bridge to bank loans by providing itself a loan or by reducing collaterals for commercial loans.

6 Conclusions

This paper evaluates a government program that assist potential entrepreneurs with their startups. Through a specific survey we collected information of both beneficiaries and non-beneficiaries. We used regression discontinuity methods to identify the impact of the program on creation, survival, employment, sales and net income. We found that the program increased the probability of creation by about 22%, and survival rate of projects at 24 months raised from 43% to 92%. The impact of the program over labor is also significant, increasing the employment in about 3 jobs. Income and sales are much more volatile and no significant impact was found on these variables.

These estimates were then combined to assess the overall impact of the program through simulations. We found that about 1000 jobs and 132 firms were generated by the program. This gives a ratio of 4,000$ of cost-per-job, a figure that shows the low cost of this program compared to other estimates of the literature.

We conjecture that the non-refundable transfer is the main instrument behind the impact of BAE on firm creation, both by providing liquidity and by reducing opportunity costs for the entrepreneurs. The role of tutorship could be substantial in the impact of BAE on survival and employment, but we cannot disentangle its importance.

Finally, this paper suggests that low scale public funding for startups can be cost-effective in creating sustainable jobs in the medium term. Its long term impact could be even higher if some of these startups really succeed in the market. A more challenging issue is whether these type of policies can be scaled up so as to substantially modify the profile of new firms or to create innovative clusters.

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Appendix

A Selection process

The scoring process of BAE has been implemented since 2008 with some slight modifications through the subsequent editions. It concentrates on two main issues: entrepreneurial ability (addressing the skills of the entrepreneur), and the project viability (apprasing the project itself, independently on who would implement it). This assessment is based on a series of items evaluated, each of them with a given score. As an example, in the 2011 edition, 12 points of the score of entrepreneurial ability were given after considering the past experience of the entrepreneur. If the entrepreneur "has a relevant experience in similar businesses that would be useful" for implementing the project, the entrepreneur gets the maximum 12 points; if the entrepreneur "does not have any experience as entrepreneur or in the business" then the entrepreneur gets no points on this.

On the other hand, while the cutoff point was always similar (beneficiaries are those that have a score greater or equal to 55), it varied in some editions. In particular, in 2010 the criterion was greater or equal to 60 and in 2009 it was greater or equal to 54. To correct for this difference we simply substracted 5 to the score of 2010 and added 1 to the score of 2009 to make all the analysis consistent with a unique cutoff point for all editions, and simplify the exposition. In the regressions we added the dummies of the year of edition as a way to reduce any impact of this procedure.

B Survey description

The survey was distributed between May and July to all individuals that applied to the program "BUENOS AIRES EMPRENDE". The contact information of each applicant was provided by the "Subsecretara 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

Name and surname

Phone

E-mail

Age

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

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

If you had previous experience, have you faced a failure? (No / Yes)

In what year have you applied to the program BUENOS AIRES EMPRENDE? (2008 / 2009 / 2010 / 2011)

In your opinion, how do you rank yourself compared to other participants of Buenos Aires Emprende of that year in terms of your ABILITY AS ENTREPRENEUR? (Ranking from 1 to 100) To answer this question, assume that there were 100 submissions to BAE and rank yourself among those 100 entrepreneurs. As an example, choosing 1 would mean you consider yourself the most skilled entrepreneur among the 100 entrepreneurs.

In your opinion, how do you rank yourself compared to other participants of Buenos Aires Emprende of that year in terms of the VIABILITY OF YOUR PROJECT? (Ranking from 1 to 100) To answer this question, assume that there were 100 submissions to BAE and rank yourself among those 100 entrepreneurs. As an example, choosing 1 would mean your project was the best among the 100 submitted projects.

Have you implemented the project presented in BUENOS AIRES EMPRENDE? Yes / No

What month and year started with the project activity ? (mm / yy) [Sample: started]

Name of your venture [Sample: started]

Briefly describe the project [Sample: started]

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

From the time they were published the results of BUENOS AIRES EMPRENDE, were you working on the project 12 months in a row? (Yes / No) [Sample: started]

What was the amount of monthly sales in pesos in those 12 months ? Sales in pesos, average per month; if not worked twelve months in a row, take your usual sales. [Sample: started]

What was the amount of monthly net income of the enterprise in the first twelve months? Net income is sales minus costs of entrepreneurship (before taxes and before distribution to partners) on average per month. If you worked 12 months, take a typical value for you (average monthly net income in pesos). [Sample: started]

How many hours on average per week worked for the venture in the first twelve 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: started]

How many people worked in the venture at the end of the first twelve months? Please include employees, managers and partners, and yourself included (number of workers) [Sample: started]

Of those, how many did it part-time? (number of workers who worked less than 30 hours per week) [Sample: started]

Under what kind of society were held sales of your company? [Sample: started]

Formal name of society [Sample: started]

Did you get your venture investment funds for third parties? (Yes / No) [Sample: started]

Did you receive funds from the following sources? Can check more than one (Capital Markets / Investment funds / Angels Investors / Family and friends / Other) [Sample: started]

Do you continue with the project now? (Yes/No) [Sample: started]

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

What was the amount of monthly net income of the enterprise in 2011? Net income (sales less cost of the project before tax and before distribution to partners) per month on the average of the 12 months of the year, in pesos. [Sample: started, editions from 2008-2010]

How many hours worked on average per week for entrepreneurship 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: started, editions from 2008-2010]

How many employees working in late 2011 in the enterprise ? Please include employees, managers and partners, and yourself (number of workers) [Sample: started, editions from 2008-2010]

Of those, how many did it part-time? (number of workers who worked less than 30 hours per week) [Sample: started, editions from 2008-2010]

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

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

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

Why have you changed the project? [Sample: discontinued & started another project]

Please choose the sector that better defines your entrepreneurship (Gastronomy / Tourism / Software / Consulting / web / Manufacturing / Retail / Editorial / Other) [Sample: discontinued & started another project]

How much are your monthly sales in 2011? Sales per month on the average of the 12 months of the year, in pesos. [Sample: discontinued & started another project]

What was the amount of monthly net income of the enterprise in 2011? Net income (sales less cost of the firm before tax and before distribution to partners) per month on the average of the 12 months of the year, in pesos. [Sample: discontinued & started another project]

How many hours did you work on average per week for entrepreneurship 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 / Other) [Sample: discontinued & started another project]

How many employees working in late 2011 in the firm? Please include employees, managers and partners, and yourself included (number of workers) [Sample: discontinued & started another project]

Of those, how many did it part-time? (number of workers who worked less than 30 hours per week) [Sample: discontinued & started another project]

Did you get your venture investment funds for third parties? (No / Yes) [Sample: discontinued & started another project]

Did you receive funds from the following sources? Can choose more than one answer (Capital Markets / Investment funds / Angels Investors / Family and friends / Other) [Sample: discontinued & started another project]

In what month and year was the project submitted to BUENOS AIRES EMPRENDE discontinued? (mm / yy) [Sample: discontinued & did not start another

project]

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

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

> Consider the following information about new ventures before proceeding with the survey [Sample: Treated with information]
>
> The data on SURVIVAL is the probability that a venture continue its activity at the end of the year. (For example, 81% of startups continue at the end of the first year and 77% at the end of the second year.) The table provides information for different sectors (Manufacturing, Construction, Retail and Services).
>
> The data on NET INCOME shows the growth of net income deflated by CPI. (For example, deflated net income of entrepreneurs increased by 69% on average between the first and second year) The table provides information for different sectors.

	Survival		NET INCOME	
	1st year	2nd year	1st year	2nd year
All sectors	81%	77%	100	169
Manufacturing	83%	79%	100	187
Construction	75%	70%	100	158
Retail	82%	78%	100	167
Services	84%	81%	100	173

> After considering this information please answer the following questions.

How was the performance of your business? (compared with the average of other start-up presented in BUENOS AIRES EMPRENDE of the year of your application) (Exceptional (well above average) / Very strong (above average) / Average / Weak (below average) / Very weak (far below average)) [Sample: All]

How do you consider the potential growth of your business for the next year? (Compared with the average of other enterprises of BUENOS AIRES EMPRENDE of that year that continue on activity) (Exceptional (well above average) / Very strong (above average) / Average / Weak (below average) / Very weak (far below average)) [Sample: All]

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 all the beneficiaries of an edition i of BAE, c the impact of the program on creation rate, and s_i the impact of the program on 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 was not in place, and the second term accounts for the fact that those that would have been created in any case, a proportion s_i of them would have exited from the market if BAE was not in place.

On the other hand, for computing 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 on these firms by e_i. Secondly, we consider the impact of BAE on the employment on 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 at firms that would not be in place without BAE, and the second term account 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 employment are computed adding up 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 is a draw from a distribution for each variable and a computation of I and L.

The computation of s_i demands a more detailed explanation. First we estimated a Weibull model. We then performed a linear prediction of the model assuming that all observations were non-treated (the "Treated" dummy was imposed to zero for all observations for the prediction). Let λ_0 be the mean of the exponential of that variable in the sample of the 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, where the first term is the expected survival at duration t including the effect of the program (β is the coefficient of the dummy "Treated" in the proportional hazard duration model) and where the second term is the expected survival without this effect. Finally, given that in our data t are months, we computed $s_{08} = s(48)$, $s_{09} = s(36)$, $s_{10} = s(24)$ and

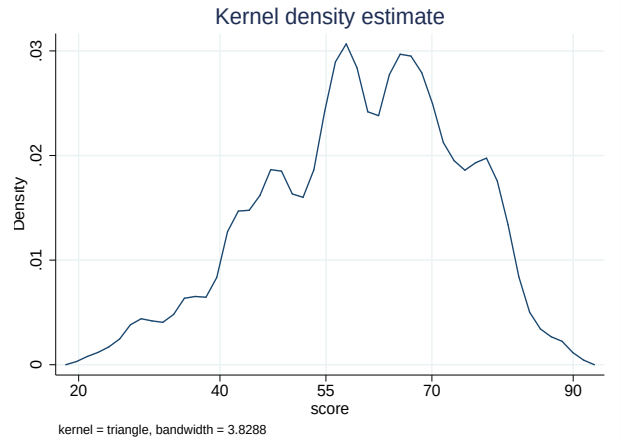
$s_{11} = s(12)$.

D Regression discontinuity

We analyzed whether covariates are continuous around the cutoff. For doing so we applyed the same methods as described in Section 3. In particular, we analyzed the continuity of age and education through local linear regressions and through regressions and found no discontinuity. We applied logit models to gender and industry dummies and found no significant coefficients associated to the variable "Treatment". We also studied the continuity of other variables from the applications, such as the expected investment of the project, the amount of the subsidy applied for and the duration of the tutorship. We found no jump in these variables using both regressions and local methods. To check the joint significance of all variables we also estimated both a probit and a logit model of the "Treatment" variable on age and dummy variables of gender, education level and industry of the project and found that both a joint test of significance and a likelihood ratio test never reject the null (in all the cases the probability is above 0.9). In brief, there is no evidence that covariates change at both 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.

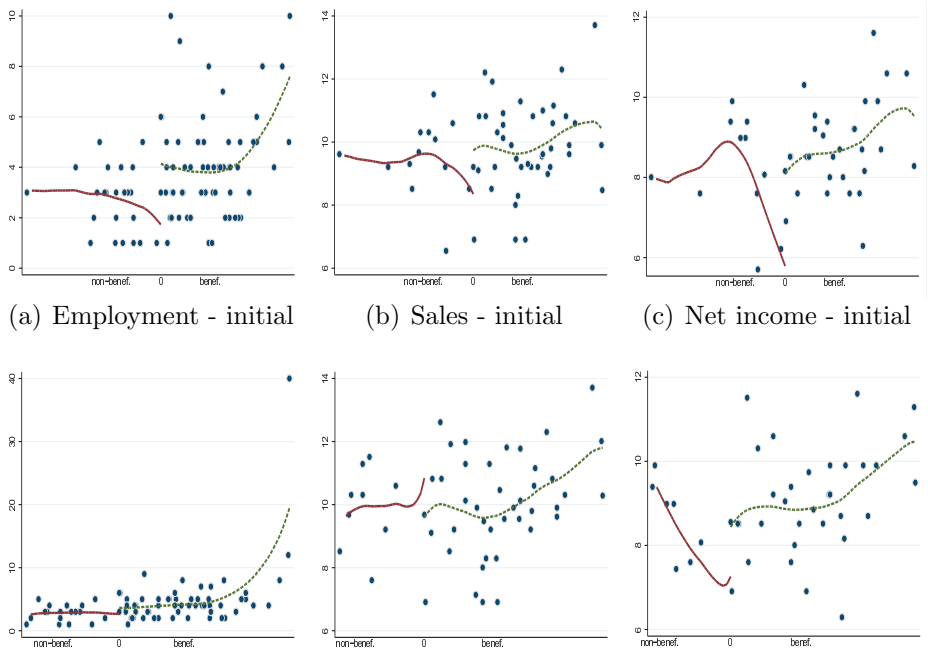
Figure D.1: Density of the running variable



Source: Administrative data from BAE.

D.1 Local linear regressions

Figure D.2: Linear local regressions



(a) Employment - initial (b) Sales - initial (c) Net income - initial

(d) Employment - 2011 (e) Sales - 2011 (f) Net income - 2011

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

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)		
Posgraduate					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 treatment dummy and covariates. In columns (3) to (7) a polynomial of the score is included as control. In column (6) a Chebyshev polynomial of degree 3 is used (coefficients are not reported). A */**/*** next to coefficient indicates significance at 10/5/1% level.

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)		
Posgraduate					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 treatment dummy and covariates. In columns (3) to (7) a polynomial of the score is included as control. In column (6) a Chebyshev polynomial of degree 3 is used (coefficients are not reported). A */**/*** next to coefficient indicates significance at 10/5/1% level.

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)			
Posgraduate					-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 duration of the firm on treatment dummy and covariates. In columns (3) to (7) a polynomial of the score is included as control. In column (6) a Chebyshev polynomial of degree 3 is used (coefficients are not reported). A */**/*** next to coefficient indicates significance at 10/5/1% level.

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)			
Posgraduate					-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 duration of the firm on treatment dummy and covariates. Weibull distribution is assumed. In columns (3) to (7) a polynomial of the score is included as control. In column (6) a Chebyshev polynomial of degree 3 is used (coefficients are not reported). A */**/*** next to coefficient indicates significance at 10/5/1% level.

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)		
Posgraduate					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)
R^2	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 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 control. In column (6) a Chebyshev polynomial of degree 3 is used (coefficients are not reported). A */**/*** next to coefficient indicates significance at 10/5/1% level.

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)		
Posgraduate					-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 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 control. In column (6) a Chebyshev polynomial of degree 3 is used (coefficients are not reported). A */**/*** next to coefficient indicates significance at 10/5/1% level.

Table E.7: OLS, Initial sales

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.29	0.27	-0.33	-0.05	-0.09	-0.16	0.79
	(0.43)	(0.45)	(0.87)	(1.08)	(1.11)	(1.13)	(1.80)
2009 edition		-0.52	-0.37	-0.36	-1.05	-0.40	-0.98
		(0.73)	(0.74)	(0.74)	(0.90)	(0.76)	(1.76)
2010 edition		0.13	0.53	0.52	0.46	0.55	0.11
		(0.70)	(0.76)	(0.77)	(0.93)	(0.78)	(1.75)
2011 edition		-0.09	0.14	0.12	0.50	0.11	-1.29
		(0.61)	(0.63)	(0.64)	(0.81)	(0.64)	(1.61)
Score			-0.04	0.30	-0.13		-0.54
			(0.17)	(0.76)	(0.19)		(1.44)
Score squared			0.00	-0.01	0.00		0.00
			(0.00)	(0.01)	(0.00)		(0.01)
Score cubed				0.00			
				(0.00)			
Tertiary					0.02		
					(0.78)		
College					-0.46		
					(0.88)		
Posgraduate					0.26		
					(0.61)		
Male					1.05*		
					(0.57)		
Age					0.33*		
					(0.19)		
Age squared					-0.00		
					(0.00)		
Additional controls	N	N	N	N	Y	N	N
Constant	9.57***	9.69***	9.76*	4.37	3.69	9.50***	26.41
	(0.37)	(0.70)	(4.92)	(12.83)	(7.37)	(0.97)	(41.15)
R^2	0.01	0.03	0.08	0.08	0.26	0.09	0.17
N	55	55	54	54	53	53	27

Notes: OLS estimations (standard errors) of the log of deflated sales during the first year of the startup on 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 control. In column (6) a Chebyshev polynomial of degree 3 is used (coefficients are not reported). A */**/*** next to coefficient indicates significance at 10/5/1% level.

Table E.8: OLS, Sales in 2011

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.07	-0.07	0.03	-0.50	0.31	-0.50	-2.49
	(0.55)	(0.56)	(1.52)	(1.61)	(2.03)	(1.61)	(2.55)
2009 edition		-0.94	-0.67	-0.73	-1.24	-0.73	-2.50
		(0.81)	(0.80)	(0.80)	(1.06)	(0.80)	(1.75)
2010 edition		-0.48	0.06	0.02	0.35	0.02	-0.45
		(0.82)	(0.84)	(0.85)	(1.07)	(0.85)	(1.81)
2011 edition		-0.97	-0.64	-0.65	-0.18	-0.65	-2.65
		(0.67)	(0.68)	(0.68)	(0.94)	(0.68)	(1.60)
Score			-0.40	2.09	-0.72		2.07
			(0.45)	(2.56)	(0.62)		(1.91)
Score squared			0.00	-0.04	0.01		-0.02
			(0.00)	(0.04)	(0.00)		(0.02)
Score cubed				0.00			
				(0.00)			
Tertiary					-0.30		
					(0.94)		
College					-0.77		
					(1.08)		
Posgraduate					-0.11		
					(0.69)		
Male					0.48		
					(0.70)		
Age					0.29		
					(0.21)		
Age squared					-0.00		
					(0.00)		
Additional controls	N	N	N	N	Y	N	N
Constant	9.96***	10.83***	21.44	-29.43	23.28	10.05***	-47.18
	(0.49)	(0.81)	(13.62)	(53.15)	(18.79)	(1.46)	(55.02)
R^2	0.00	0.05	0.15	0.17	0.29	0.17	0.33
N	51	51	50	50	49	50	26

Notes: OLS estimations (standard errors) of the log of deflated sales during 2011 on 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 control. In column (6) a Chebyshev polynomial of degree 3 is used (coefficients are not reported). A */**/*** next to coefficient indicates significance at 10/5/1% level.

Table E.9: OLS, initial net income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.44	0.16	-0.28	0.15	-0.08	0.26	1.20
	(0.41)	(0.45)	(0.83)	(1.03)	(1.05)	(1.08)	(1.23)
2009 edition		-1.19	-0.89	-0.79	-1.52	-0.73	-1.97
		(0.84)	(0.87)	(0.89)	(1.04)	(0.91)	(1.28)
2010 edition		-0.27	0.13	0.17	-0.24	0.13	-0.62
		(0.65)	(0.69)	(0.70)	(0.86)	(0.72)	(1.13)
2011 edition		0.06	0.32	0.32	0.30	0.33	-1.26
		(0.56)	(0.58)	(0.58)	(0.76)	(0.59)	(1.02)
Score			-0.08	0.41	-0.19		-1.74*
			(0.15)	(0.72)	(0.19)		(0.99)
Score squared			0.00	-0.01	0.00		0.02*
			(0.00)	(0.01)	(0.00)		(0.01)
Score cubed				0.00			
				(0.00)			
Score Ch0						0.00	
						(0.00)	
Tertiary					-0.68		
					(0.88)		
College					-0.70		
					(0.83)		
Posgraduate					-0.41		
					(0.62)		
Male					0.92		
					(0.54)		
Age					-0.10		
					(0.22)		
Age squared					0.00		
					(0.00)		
Additional controls	N	N	N	N	Y	N	N
Constant	8.31***	8.65***	9.90**	2.00	14.46*	8.76***	58.33*
	(0.35)	(0.66)	(4.33)	(11.90)	(7.40)	(0.94)	(28.10)
R^2	0.03	0.11	0.18	0.19	0.36	0.19	0.43
N	43	43	42	42	41	41	22

Notes: OLS estimations (standard errors) of the log of deflated net income during the first year of the startup on 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 control. In column (6) a Chebyshev polynomial of degree 3 is used (coefficients are not reported). A */**/*** next to coefficient indicates significance at 10/5/1% level.

Table E.10: OLS, Net income in 2011

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.30	0.19	1.32	1.56	2.84	1.56	-1.06
	(0.49)	(0.51)	(1.24)	(1.40)	(1.68)	(1.40)	(1.52)
2009 edition		-0.17	-0.45	-0.40	-0.99	-0.40	-2.46
		(0.89)	(0.84)	(0.85)	(1.08)	(0.85)	(1.41)
2010 edition		-0.06	0.35	0.35	0.30	0.35	-0.25
		(0.68)	(0.67)	(0.68)	(0.87)	(0.68)	(1.07)
2011 edition		-0.50	-0.27	-0.27	0.03	-0.27	-2.11**
		(0.56)	(0.54)	(0.55)	(0.75)	(0.55)	(0.94)
Score			-0.74*	-1.65	-1.48**		0.59
			(0.38)	(2.43)	(0.58)		(1.24)
Score squared			0.01**	0.02	0.01**		-0.00
			(0.00)	(0.04)	(0.00)		(0.01)
Score cubed				-0.00			
				(0.00)			
Tertiary					-1.43		
					(0.87)		
College					-2.06*		
					(1.00)		
Posgraduate					-0.84		
					(0.62)		
Male					0.94		
					(0.62)		
Age					0.08		
					(0.22)		
Age squared					-0.00		
					(0.00)		
Additional controls	N	N	N	N	Y	N	N
Constant	8.79***	9.19***	30.33**	49.30	50.11***	10.29***	-8.42
	(0.44)	(0.70)	(11.77)	(51.00)	(17.61)	(1.43)	(35.73)
R^2	0.01	0.04	0.23	0.24	0.43	0.24	0.57
N	41	41	40	40	39	40	20

Notes: OLS estimations (standard errors) of the log of deflated net income during 2011 on 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 control. In column (6) a Chebyshev polynomial of degree 3 is used (coefficients are not reported). A */**/*** next to coefficient indicates significance at 10/5/1% level.