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Impact of incubation on innovative firms

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Abstract

This paper measures the impact of incubation on new and innovative Uruguayan firms' performance. Technological innovation has a fundamental role in explaining economic growth and broader economic development. With this in mind, the fact that new and innovative firms face larger difficulties when trying to validate their innovations becomes a policy concern. One of the answers given to this problem is incubation, which attempts to place this particular sort of companies in a "secure" environment until they are able to survive on their own. The evaluation was restricted to firms housed at a particular incubator called Ingenio, which is one of the largest and oldest operating in Uruguay. It was carried out using a unique panel of data gathered from the incubator and through a survey of current and former incubatees and of rejected candidates. In order to control for potential correlation between the outcome and firms' observed and unobserved traits a sharp regression discontinuity design was employed, exploiting the incubators selection process. Evidence showed timid support for the hypothesis that incubation has a positive impact on firms' sales and employment, while no impact was detected on their exports. One of the possible explanations for the small impacts detected is that small sample size may have biased the estimates downwards. Therefore it can be affirmed that, at the very least, incubation did not hamper these companies' performance.

Keywords: incubation, sharp regression discontinuity, impact evaluation

Resumen

En el presente trabajo se evalúa el impacto de la incubación en el desempeño de firmas uruguayas nuevas e innovadoras. La innovación tecnológica tiene un rol fundamental al momento de explicar tanto el crecimiento como el desarrollo económico. Teniendo esto en cuenta, el hecho de que firmas nuevas e innovadoras enfrenten mayores dificultades al intentar validar sus innovaciones pasa a ser una preocupación de política económica. Una de las respuestas que se ha dado a este problema es la incubación, que intenta localizar a estas empresas en un ambiente "seguro" hasta que sean capaces de sobrevivir por su cuenta. La evaluación se restringió a firmas situadas en una incubadora particular, Ingenio, que es una de las más grandes y más antiguas operando en Uruguay. Se llevó a cabo a través de un panel de datos recogidos directamente de la incubadora y a través de una encuesta a actuales y antiguos incubados así como también a candidatos a incubación rechazados por Ingenio. Para controlar por la posible correlación entre características inobservables de las firmas y su desempeño se empleó un diseño preciso ("sharp") de regresión discontinua, explotando el proceso de selección de la incubadora. Los datos muestran un tímido soporte a la hipótesis de que la incubación tiene un impacto positivo en la facturación y el empleo de las firmas, mientras que no se detectaron impactos en las exportaciones. Una de las posibles explicaciones para los pequeños impactos detectados es que el reducido tamaño de la muestra haya sesgado las estimaciones a la baja. Se puede afirmar que, como mínimo, la incubación no perjudicó el desempeño de estas empresas.

Palabras clave: incubación, regresión discontinua, evaluación de impacto

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1. Introduction

Technological innovation has had a fundamental role in explaining long-term growth since the works by Schumpeter (1934) and Solow (1957). In this context, the fact that new and innovative firms face larger difficulties when attempting to realize their innovations in the market becomes of great concern, since they are key players in economic development.

New and innovative firms may face two important challenges from the onset. The first one was deemed "the liability of newness" by Arthur Stinchcombe in 1965. The term was used to explain the higher rate of failure among newly-started firms and referred to the difficulties faced by those firms in obtaining the resources needed for survival. The liability arises because young firms have less of the legitimacy needed to gain support and trust from other market participants (Fergusson and Olofsson, 2004). This handicap may be particularly strong for highly innovative new companies, as they also have to validate new products or services. The second one may be referred to as the "liability of smallness" (Aldrich and Auster, 1986) and stems mainly from difficulties in raising capital (probably due to poor collateral) and from tax laws and government regulations that imply a larger burden for small firms. These liabilities stem from information asymmetries and produce sub-optimal market outcomes. Concretely, less resources (financing in particular) than the social optimum are directed towards new and innovative firms, thus rendering policy interventions theoretically justifiable.

Incubation might entail an “image benefit” for tenants by providing them with a “prestigious address” (Ferguson and Olofsson, 2004) which could in turn help them to overcome Stinchcombe’s “liability of newness” and Aldrich and Auster’s “liability of smallness”, improving their chances of survival and their subsequent performance.

In addition, founders and managers of technological start-ups may be less likely to have prior business experience and/or a management-oriented formation, as found in several studies using data from developed economies (Löftsten and Lindelöf, 2001; Westhead and Storey, 1994). This means that firms located in business incubators may benefit from support in business competences and therefore, exhibit a better performance than their off-incubator counterparts.

An incubator’s main goal can be stated as to significantly improve the probability of survival and the later performance of infant firms. In order to achieve that aim, incubators offer their tenants a range of services and resources, including a “prestigious address”, managing and marketing advice and a work space suitable for intense networking. In this way, incubators provide a “secure” environment where newly-started entrepreneurs can validate their ideas into marketable products and services (Cheng and Schaeffer, 2011). They are generally non-profit organizations and are often supported by public institutions, although they may be constituted as private or public entities.

In recent years, most Latin American countries have moved towards more advanced models of innovation policy. In these new set-ups, interactions between scientific and productive actors as well as public-private associations are taken into consideration, reinforcing the fact that new and innovative firms are increasingly the focus of the

intervention (Duhart, J. & Primi, A., 2012). Incubation sits at the core of this new approach, since it emphasizes the creation of innovation networks and experience-sharing as one of the channels through which it increases the survival probability and performance outlook of new and innovative firms. Notwithstanding this recent thrust, incubation in Latin America and particularly in Uruguay is still a fairly recent phenomenon, dating from end-90´s at earliest.

Although incubators may be called to play a relevant role in long-term growth and even when the fact that most are publicly funded is considered (thus implying an opportunity cost of public financing), rigorous evaluations of their impact on survival and performance of tenants is strikingly scarce in developed economies environments and extremely rare for emerging countries. In particular, no impact evaluations for incubators located in Uruguay have been written to date. The small number of studies is probably due to the fact that data is very hard to obtain as rigorous impact evaluations must necessarily rely on comparisons of incubated firms versus non-incubated ones.

This paper evaluates the impact of incubation on Uruguayan innovative firms housed at a particular incubator, Ingenio, which belongs to the Uruguayan Technological Laboratory (LATU for its Spanish acronym) and operates in its premises. Ingenio was created in 2001 from a combined effort of LATU and ORT Uruguay University with financial support from the Multilateral Investment Fund (MIF), the Inter-American Development Bank (IDB) and the World Bank (WB). Firms incubated are mainly linked to information and communications technology (ICT), software, electronics, creative industries, design, videogames, audio-visual, tourism and alternative energy. Current providers of funding are

the IDB and the National Agency for Research and Innovation (ANII for its Spanish acronym).

The evaluation focuses on the differential effect of incubation on the tenants' sales, exports and job creation performance and is based on a comparison between incubated firms and a control group of similar non-incubated firms. Data on their performance was directly requested to the entrepreneurs through a telephone and web-based survey. As a result, a unique and very rich data set was obtained and that is, in fact, one of the elements that sets this paper apart from the previous literature.

The impact evaluation intended presents strong selection biases. Firstly, Ingenio only admits applicants with good performance perspectives. Moreover, it is reasonable to assume that entrepreneurs who sign up for incubation processes may be more informed, more driven or have better contacts on average. Since a Selection Committee chooses which applicants are allowed to incubate after a first filter that selects which candidates are allowed and documents its decisions on every one of them, it is possible to compile an index based on how promising the projects were deemed by the Committee[1]. This index, in turn, enables the use of a regression discontinuity approach to treat for the potential endogeneity caused by selection.

This paper is organized as follows. Section 2 presents a brief literature review on impact evaluation of incubation and programmes aimed at fostering investment on R&D. Section 3 provides some information on incubation initiatives in Uruguay and particularly, on Ingenio. Section 4 discusses the evaluation strategy and data set. Section 5 presents the

[1] In order to be reviewed by the Selection Committee applicants first have to fill an online form which acts as a primary filter. The web form as a first selection stage was implemented in 2007.

main findings of the programme's impact evaluation. Section 6 concludes and identifies limitations and potential extensions of this research.

2. Literature review

As stated above, impact evaluations of incubators are fairly scarce, even more so when incubators operating in emerging economies are considered. Recent examples for developed economies include Ferguson & Olofsson (2004) for Sweden, Schwartz (2010) for Germany and Amezcua (2010) for the United States.

Ferguson & Olofsson (2004) attempted to measure the effects of Swedish science parks on new technology-based firms' survival and growth. Their main hypothesis was that firms housed in science parks showed better survival odds and growth perspectives than their off-park counterparts. A group of 30 incubated firms was compared with 36 similar, non-incubated companies for the period 1995-2002. In order to compile treatment and control groups, the authors considered technology-based companies located in two Swedish cities that participated in a survey in 1995 and used stratified sampling techniques to obtain similar sized groups. Ferguson & Olofsson argue that since both treatment and control-group individuals were drawn from the same sample (the mentioned survey), their method provides a better basis for comparison than matched-sampling (namely, propensity score matching).

Their assertion is based on the fact that matched-sampling implies a risk of differing sample bias, which renders impossible the task of separating treatment effects from differences related to the different sampling. The authors found that their initial assumption held. Nevertheless, since outcomes were compared directly, the study's results may reflect

biases stemming from variables that affect both the probability of treatment and the expected result of the treatment.

Schwartz (2010) measures the effect of incubation on long-term survival rates through a propensity-score matching methodology. The study used data from five incubators operating in Germany in 2006. A total of 371 firms graduated from these incubators were compared with 371 un-incubated ventures. Variables such as location, industry, age and legal form are used to match treated individuals with their controls. Results indicated that incubation did not increase the probability of firm survival in the long term and in fact, lowered it for three of the five incubators analyzed.

Amezcua (2010) also assessed the question of whether incubation helps new ventures survive and grow in the long-run using propensity-score matching techniques. Data available resulted in a matched sample of 18,426 incubated firms and 28,346 controls (all operating in the United States' territory). The variables used for matching were firms´ location, age, industry and gender of the entrepreneur. Sales and employment were the variables used to capture firm growth. Results showed that incubation lowers the probability of survival of new ventures but increases their growth outlook in the long-run. When the rates of survival are considered jointly with the growth perspectives, incubated firms reduce overall employment and sales when compared with their matched controls. This is due to the fact that gains in incubated firms´ size are outweighed by their higher hazard rates.

As can be seen from the examples above, the impacts of incubation on the survival and performance of firms operating in developed economies remain unclear. In addition, these

results can hardly be extrapolated to an emerging-market environment, since these have idiosyncratic traits (such as shallower financial markets and lower institutional quality) that can, *a priori*, make incubation impacts larger or smaller.

As already mentioned, incubation-impact evaluations for incubators operating in developing economies (and in particular in Latin America), are extremely rare. Studies assessing the effects of innovation-fostering programmes in the region have been mostly concentrated on interventions that provide funding for R&D.

A recent example is Alvarez, Crespi & Cuevas (2012), who analyze the effects of two Chilean public programmes aimed at supporting innovation, the National Productivity and Technological Development Fund (FONTEC for its Spanish acronym) and the Science and Technology Development Fund (FONDEF). FONTEC provides financing for innovation projects carried out by private companies through matching grants. It subsidizes a share of the total costs of the project ranging between 35% and 60%. In turn, FONDEF provides funding for R&D and technology projects organized jointly by universities, technology institutes and private firms. It also employs matching grants that cover a percentage of the total costs of the project.

The authors identified participants and non-participants in the programmes during the period 1995-2000 from a large panel of firms in the manufacturing industry. The impacts of the programmes were estimated using propensity-score matching and difference-in-difference methods. Propensity-score matching resulted in matched samples of 6,418 observations for evaluation of the FONTEC programme, 3,756 for FONDEF and 1,643 for both. Alvarez, Crespi & Cuevas concluded that the interventions have generally been

associated with increases in employment and productivity, although the effects are heterogeneous across programmes and indicators of firm performance.

When interpreting the results of impact evaluations where propensity-score matching techniques have been used, a strong caveat is in order. Propensity-score matching methods do not allow for selection in unobservable characteristics of the individuals. That is, belonging to the treatment or control group may depend on unobserved traits of the firms that can be correlated with the outcome. Therefore, strong assumptions regarding the distribution of unobserved variables must be made. In particular, propensity-score matching assumes that these unobserved characteristics are equally distributed in the treatment and control groups. If this key assumption does not hold, the resulting impact estimations present potentially fairly strong biases that may invalidate the conclusions obtained.

Finally, another example of impact evaluation of programmes aimed at fostering private R&D investment through grants (this time operating in an industrialized economy) is Bronzini & Iachini (2014), who employ a sharp regression discontinuity design. The authors evaluated the impact of a regional-based programme established in northern Italy. They conclude that public-financing for R&D investment projects did not have a positive effect on firms' R&D outlays. However, when differentiated by firm size, they find that programme did have a positive effect on small firms. They put forward the hypothesis that the impact is larger for small firms because they are more exposed to financial frictions and test it. Their data suggests that the financial channel is important when trying to explain the different impact of the programme across firms' size. Nonetheless, other elements cannot be discarded.

3. Incubation in Uruguay and Ingenio

The emergence of incubation in Uruguay is quite recent and is based mainly on initiatives originated in the public sector. Besides Ingenio, other relevant incubators include Cerro´s Technological and Industrial Park (sponsored by Montevideo's local government), Pando's Technological Pole (sponsored by the University of the Republic), Germinal (sponsored by Paysandu's local government and the Office of Planning and Budget) and Idear (sponsored by Maldonado's local government, the Ministry of Industry, Energy and Mining and the Office of Planning and Budget). Of the aforementioned institutions the oldest one is Cerro´s Technological and Industrial Park which was founded in 1997.

Ingenio was created in 2001 from a combined effort of LATU and ORT Uruguay University with financial support from the Multilateral Investment Fund (MIF), the Inter-American Development Bank (IDB) and the World Bank (WB). Firms incubated are mainly linked to information and communications technology (ICT), software, electronics, creative industries, design, videogames, audiovisual, tourism and alternative energy. Current providers of funding are the IDB and the National Agency for Research and Innovation (ANII for its Spanish acronym), although this particular incubator is mostly self-funded (a rare characteristic in the incubator population).

Besides physical incubation (where the tenant relocates to Ingenio's shared office space), a "remote" incubation arrangement is also offered. All incubatees (both physical and remote) are obligated to attend courses related to the firm's line of business as well as

general management topics. In addition, they have to disclose to the incubator´s authorities all information required, to prepare monthly reports detailing their performance and to participate in counselling meetings. The permanence of incubatees is evaluated based on their monthly performance regarding several key variables such as sales, exports, taxes paid, external investment and job creation. Evaluation criteria are fairly pragmatic as they are not based on predetermined benchmarks. Finally, Ingenio charges its tenants a monthly fee of about US$ 280 for physical incubation and US$ 140 for remote incubation and 3% of post-graduation earnings for three years.

About 23 firms can be simultaneously housed at Ingenio and 12 on average start incubation every year. The maximum incubation span is two years but the process can be terminated earlier either as a decision of the tenant or in agreement with Ingenio's management. Firms that successfully conclude the two-year incubation period are called "graduates", while those that interrupted the programme before graduation are called "egressed".

Candidates for incubation in Ingenio have to meet certain criteria in order to start the process. Firstly, they have to display outstanding growth perspectives based on genuine competitive advantages (sales growth of about 25% per year is frequently used as an informal benchmark) and must be new or recently started ventures. In addition, they must exhibit reasonable exporting possibilities, an innovative profile and have a potential for strong job creation. Candidates that do not meet these criteria are often directed to pre-incubation institutions. As of January 2007, postulants apply through a web form (see Annex) which is evaluated in order to determine if they are fit to pass to the second stage, the Selection Committee (integrated by institutional and business representatives as well as

graduates from Ingenio). This Committee then decides if the applicant should be incubated or not.

Postulations though the web form reached 604 in 2007-2013. Of those 604, 498 were rejected outright while 106 made it to the Selection Committee. Those 106 applicants that reached the Selection Committee stage are firms that look much more promising than the remaining 498 (notwithstanding that some of them were later rejected in the more thorough evaluation carried out by the Committee). It should be stressed that the first evaluation stage does in fact filter out the majority of applicants (498 out of 604 were rejected in the period 2007-2013), which renders the 106 remaining postulants a much more homogenous population.

Projects that were finally approved for incubation at the Selection Committee amounted to 64, rejected ones were 40 and two firms did not show up for evaluation. It should be noted then that those 64 firms approved for incubation at the Selection Committee constitute the group of "treated" individuals in the population for the period 2007-2013. Of those 64 approved firms, 22 are currently being incubated, 20 have egressed, 12 have graduated and 10 finally declined to be incubated (as of end-2013). Of the 22 companies in incubation as of end-2013, most belong to the ICT industry. Tables 1 and 2 and Figure 1 summarize these numbers.

Table 1. Candidates for incubation.

Period	Applicants that reached Selection-Committte stage				Applicants rejected in web-form stage	Total applicants through web form
	Approved	Rejected	Did not show up for evaluation	Total		
2001-2006	32	72	0	104	N/A	N/A
2007-2013	64	40	2	106	498	604

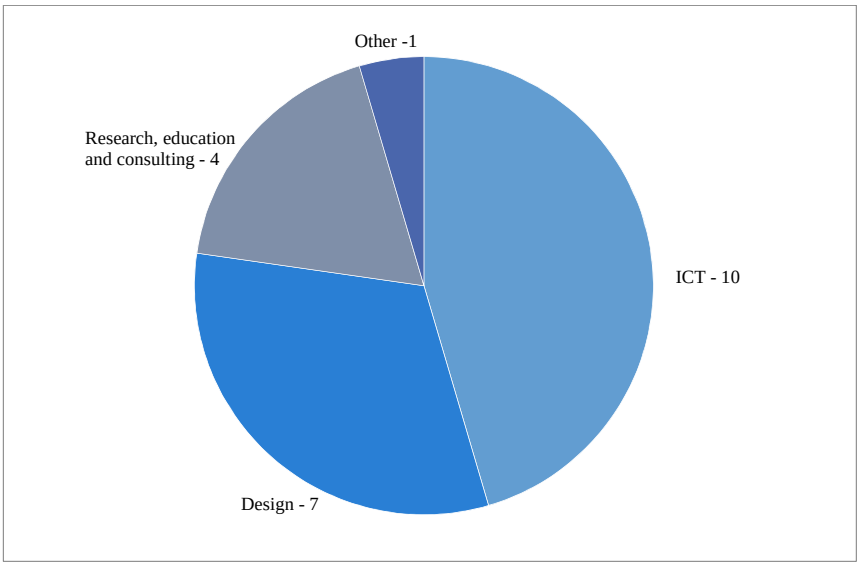
Note: "N/A" stands for "not available"

Table 2. Approved candidates (as of end-2013).

Period	Approved				
	Currently in incubation	Graduated	Egressed	Declined incubation*	**Total**
2007-2013	22	12	20	10	**64**

*Of which 4 are included in the sample

Figure 1. Main line of business of incubated firms - 2013 (total=22)



4. Evaluation strategy and data set

The question of whether and to what extent incubation in Ingenio significantly improves the performance of its tenants is addressed in this section and the following. The main hypothesis is that incubated firms should exhibit a better performance than similar off-incubator firms, using sales, employment and exports as performance indicators.

Despite the fact that the incubator was founded in 2001, only firms that made it to the Selection Committee stage from 2007 onwards were considered. This was due to the facts that the web postulation mechanism was put in place that year, the incubator changed management (and considerably altered some incubation practices) and that more systematic data-collection procedures were put in place around that time.

Moreover, the evaluation focuses on the population of firms that reached the Selection Committee for two reasons. Firstly, they are a much more homogenous group than the 604 projects submitted for evaluation. In fact, of 604 applicants in the 2007-2013 period, only 106 made it to the Selection Committee, which implies that the web formulary stage is an effective filter and that applicants that reached the second stage are indeed less heterogeneous. Therefore, it should be noted that this study assesses the impact of incubation on firms that looked promising *a priori*. The second reason is a practical one and it is the lack of contact information, which rendered impossible the task of surveying entrepreneurs whose projects did not reach the Selection Committee.

To carry out the evaluation a specific database was set up in cooperation with Ingenio's management, incubatees, former incubatees and rejected candidates. Firstly, a list of all candidates that made it through to the Selection Committee phase and the decision reached by the Committee was provided by Ingenio, along with documents detailing the Committee's decision and its justification. Based on that information, an index indicating the projects' quality (as judged by the Selecting Committee) was elaborated. The index´s values are summarized in Table 3.

Table 3. "Quality index".

Index value	Condition	Example
1	Project purports extremely good perspectives. Approved with no further questioning.	"Good growth potential. Very compromised entrepreneurs. Possible links with other incubatees. Difficult start shows candidates' entrepreneurial spirit. Feet on the ground"
2	Project was approved on a condicional basis, relevant changes were advised and/or warnings were issued.	"Good idea, has a potential market. High entry barriers. Can be solved with more "out of the box" thinking? Doubts regarding initial technological development. Should start with a simple initial version."
3	Project was rejected but nevertheless received positive comments or recommendations.	"Candidates lack entrepreneurial attitude. Interesting niche. Good system/interphase. Very committed to the project. Bad presentation. Has made good business deals. Recommendation: pre-incubate and try Ingenio again next year."
4	Project was rejected outright.	"Very green. Strong competition. Very volatile market."
5	Candidate did not show up for the Committee's evaluation.	-.-

Next, candidates that made it through to the Selection Committee stage in the period 2007-2013 were surveyed in order to gather information regarding sales, employment, exports, investment and previous entrepreneurial experience (see questionnaire in the Annex). The database obtained includes sales, employment, exports, external investment,

experience and industry data for 55 firms (a 52% answer rate) with similar distributions of approved and rejected candidates as well as of the "quality index" scores to that of the population.

Table 4. Distribution of applicants that reached Selection Committee Stage (as of end-2013).

Group	Applicants that reached Selection-Committte stage			
	Approved	Rejected	Did not show up for evaluation	Total
Population	60%	38%	2%	100%
Sample	56%	42%	2%	100%

Table 5. Distribution of approved candidates in the sample and in the population (as of end-2013).

Group	Approved (all figures in percentage of approved applicants)				
	Currently in incubation	Graduated	Egressed	Declined incubation	**Total**
Population	34%	19%	31%	16%	**100%**
Sample	29%	32%	26%	13%	**100%**

Table 6. Distribution of the "quality index" in the sample and in the population (as of end-2013).

"Quality" Index	Population		Sample	
	Individuals	**Percentage**	**Individuals**	**Percentage**
1	36	34%	11	20%
2	28	26%	20	36%
3	22	21%	16	29%
4	18	17%	7	13%
5	2	2%	1	2%

Note: two applicants with "quality index" value of 1 and two with "quality index" value of 2 finally declined incubation. Therefore, incubated firms in the sample are not 31 but 27 while non-incubated firms in the sample are not 24 but 28.

The sample consists of a panel of 27 firms that were incubated in the period 2007-2013 and 28 that were not incubated (although four of them were approved for incubation). Data corresponds to years 2005 through 2013. The tables below summarize the main characteristics of the treatment and control groups. The variable $experience_i$ refers to whether or not the firm's owner had previous entrepreneurial experience or not. It equals 1 when this was so and 0 otherwise. Therefore, figures reported are the share of firms headed by a person with experience.

Table 7. Means, difference in means tests and medians of key variables.

Variable	Means		P-value of two tailed t-test for the difference in means	Medians	
	On incubator	Off-incubator		On incubator	Off-incubator
Average Annual Sales (USD)	172,161.0 (82,676.3)	52,817.1 (37,581.1)	0.19	47,500.6	0
Average Annual Employment (number of full-time employees)	6.5 (2.2)	3.4 (1.4)	0.23	3.9	0
Average Annual Exports (USD)	27,450.7 (15,175.8)	9,238.2 (7,065.9)	0.28	0	0
Average External Annual Investment (USD)	12,571.9 (3,509.5)	15,348.0 (7,796.3)	0.75	4,371.3	337
Experience	0.37 (0.09)	0.39 (0.09)	0.87	0	0

Note: standard errors in parentheses

Table 7 shows that on-incubator firms boasted (on average) higher levels of sales, employment and exports, while they received less external investment (that is, financing provided by others than the firms' owners) and were less experienced. In addition, it is possible to conclude that the majority of firms that reached the Selection Committee stage were led by an inexperienced entrepreneur. Differences in means are not statistically significant for any of the variables considered (it should be noted that a strong heterogeneity in firms' performance is detected).

However, direct comparison of raw means is not enough to reach any robust conclusions regarding the impact of incubation. A common issue in programme impact evaluations is that treated and non-treated individuals (firms in this case) can differ in terms of unobserved characteristics correlated with the outcome variables. This means that the variable identifying treated firms would be endogenous, which would lead to invalid estimations if not accounted for. In order to control for this possible source of endogeneity, a regression discontinuity design was employed, exploiting Ingenio's selection process.

More precisely, whether or not a candidate firm that reached the Selection Committee is finally incubated depends on the Selection Committee's decision (although a few firms decline incubation after being accepted, see table 2). Therefore, the "forcing variable" (that is, the one that selects firms into and out of treatment) considered here is the "quality index" mentioned earlier. Values of 1 and 2 select the firm into treatment, while values of 3, 4 and 5 determine non-treatment.

The regression discontinuity design is preferred because, under rather general conditions, it can be demonstrated that it is equivalent to a randomized experiment. The identification strategy is based on the continuity assumption, which requires that firms at both sides of the cutoff point (the ones with "quality index" scores of 2 a 3 in this case) have the same potential outcome in an identical incubation experience. Although there is no direct way of testing the validity of the continuity assumption, Lee (2008) formally showed that if selection into treatment depends on whether a forcing variable that agent cannot completely control exceeds (or falls below) a determined threshold, the continuity assumption is consequently satisfied and the variation in treatment around the cutoff

resembles a randomized selection process. In this setup, the impact of the programme is identified by the discontinuity of the outcome variable at the cutoff.

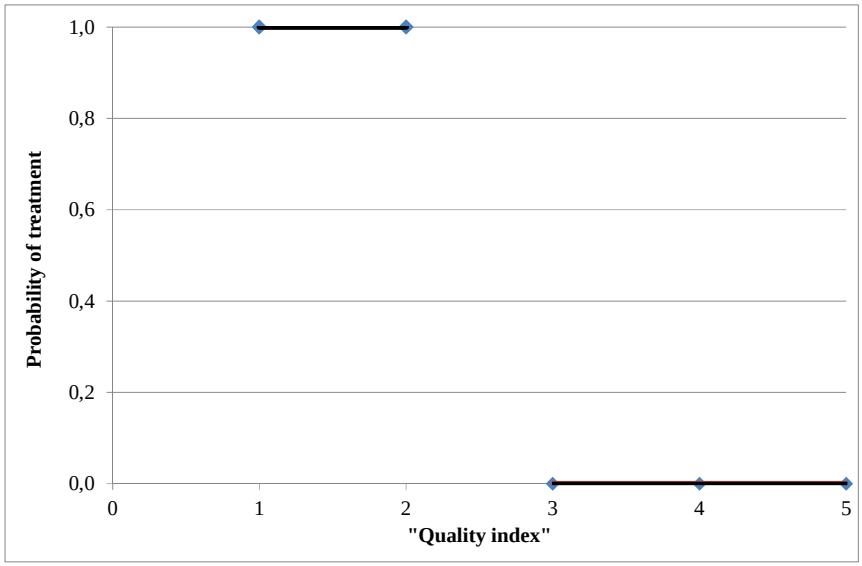
It can be reasonably argued that the results of the assessment by Ingenio's Selection Committee cannot be (completely) manipulated by the postulants. Therefore, the impact of incubation will be correctly addressed using the regression discontinuity design. Notwithstanding, the continuity assumption purports certain testable implications that are considered below.

Since the aim of this research is to assess the impact of incubation on the performance of incubated firms, the outcome variables considered are sales, employment and exports. The controls to be used are whether or not the entrepreneurs have any previous experience in managing a startup and the industries to which their current enterprise belong.

The current regression discontinuity design can be classified as belonging to the "sharp" kind, since probability of treatment jumps from 0 to 1 when the index changes from 3 to 2[2] (see Figure 2). The effect of treatment will be measured by estimating the jump in the outcome variable around the forcing variable's cut-off point. That is, estimating the effect of receiving treatment on sales, employment and exports for individuals with "quality" scores 2 and 3.

[2] It should be noted, though, that four individuals selected into incubation finally decided not to go through with the process. Two of them presented scores of 2 in the "quality index" (so they are close to cut-off point) and the remaining pair showed scores of 1.

Figure 2. Probability of treatment.



In order for this method to correctly identify the local average treatment effect (LATE), two key initial assumptions must hold. Firstly, if selection for incubation around the threshold is as random, treated and non-treated firms with “quality index” scores of 2 and 3 should be similar (this stems directly from the continuity assumption discussed above). More precisely, control variables must show a similar distribution in both treatment and control groups (the sample should be balanced). Since there are very few firms per each of the four sectors in which the sample is divided, to perform inference on the difference of firms per sector would not be correct. Therefore, in order to check the internal validity of the evaluation, a test was performed on whether the difference in experience between treated and non-treated was significant. That is, the percentage of firms headed by an “experienced” entrepreneur should be similar for both treatment and control groups, especially around the cutoff point.

Secondly, a more general assumption regarding all experiments with control groups ought to be tested. This is the “parallel trends” assumption and states that pre-incubation

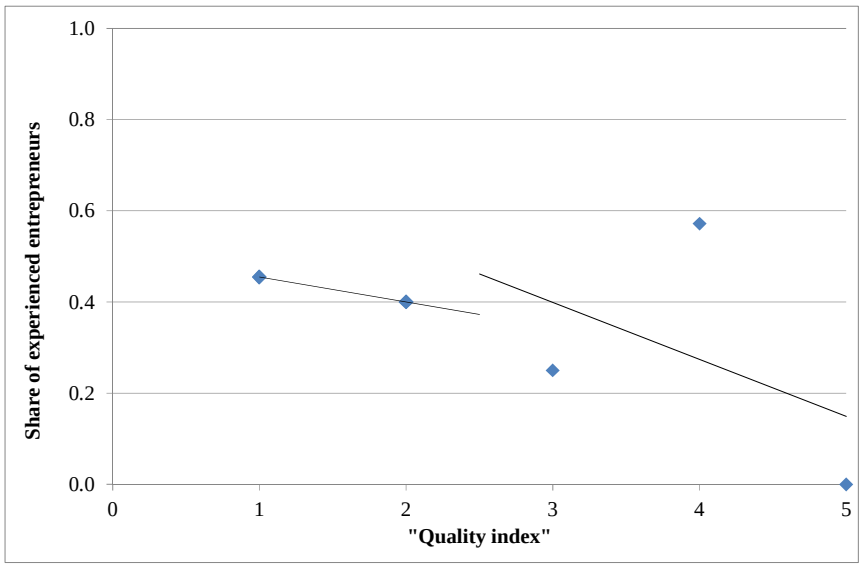
trends of the outcome variables for treated firms must follow a similar pattern to those of non-treated firms. Both assumptions held for the current sample, as shown below.

1) Continuity assumption

In the present sub-section the validity of the first assumption is assessed. Although it cannot be tested directly, it does have testable implications. The most important one states that there should not be a discrete jump in the share of experienced entrepreneurs at the cut-off.

As a first step in assessing the validity of this affirmation, the share of experienced entrepreneurs was plotted against the forcing variable (the "quality index") and trend lines were added to facilitate visualization. Graphical inspection does not reveal an important break in the tendency around the cut-off point.

Figure 3. Share of experienced entrepreneurs vs. "quality index".



Furthermore, tests for difference in means at both sides of the cut-off were performed using the whole sample and a trimmed sample that only includes individuals with "quality index" scores of 2 and 3 (which are the ones closer to the cut-off). Tables 8 and 9 show the t-tests. Since differences were statistically not significant, the conclusion is that differences in performance should be attributed to whether the firm received treatment or not.

Table 8. Test for the difference in means of "experience" for the whole sample.

Group ("incubated" = 0, "incubated "= 1)	Observations	Mean	Std. Err.	Std. Dev.	[95% Conf. Interval]	
0	28	0.39	0.09	0.50	0.20	0.59
1	27	0.37	0.09	0.49	0.18	0.57
Combined	55	0.38	0.07	0.49	0.25	0.51
diff		0.02	0.13		-0.25	0.29

Table 9 .Test for the difference in means of "experience" for individuals with "quality index" scores of 2 and 3.

Group ("incubated" = 0, "incubated "= 1)	Observations	Mean	Std. Err.	Std. Dev.	[95% Conf. Interval]	
0	18	0.28	0.11	0.46	0.05	0.51
1	18	0.39	0.12	0.50	0.14	0.64
Combined	36	0.33	0.08	0.48	0.17	0.50
diff	-	0.11	0.16		-0.44	0.22

2) Parallel trends test

Tables 10 and 11 show panel regressions that test the second assumption. A dummy variable ($treated_{ij}$) that takes the value 1 if firm i was being or had already been incubated at year j and zero otherwise was defined. Therefore, observations where $treated_{ij} = 0$ include firms that were never treated and firms that were ultimately incubated but had still not received treatment at year j.

Dependent variables $sales_{ij}$, $employment_{ij}$ and $exports_{ij}$ represent sales in U.S. dollars, employment in number of full-time employees and exports in U.S. dollars respectively for firm i at year j. Dummy variable $incubated_i$ equals 1 when the firm was incubated at some point in the period 2007-2013 and 0 otherwise. Regressions of outcome variables on *incubated* with and without controls for experience and industry were performed using only observations were $treated_{ij} = 0$.

The tests were carried out through standard random-effects OLS regressions and Tobit models that allow for truncation of outcome variables at zero to be taken into consideration[3]. In addition, all regressions were run with and without including a particularly extreme outlier that might have changed the conclusion. Tables 10 and 11 exhibit Tobit regressions of the outcome variables on $incubated_i$, controlling by entrepreneurs´ experience and firms´ industry. Table 10 includes the outlier while Table 11 does not (additional regressions reported in the Annex). Since *incubated* was not significant in any of the tests, the conclusion is that incubated firms' showed a similar performance prior to their incubation to that of firms that were never incubated.

In addition and in order to consider the small sample size, inference tests were performed using bootstrapped errors. When using this technique, random-effects OLS estimation including the outlier yields negative and significant coefficients for $incubated_i$, which would imply that firms that were finally treated were systematically outperformed before incubation. However, it should be noticed that OLS does not account for the fact that dependent variables are left-bounded and so, Tobit models seem more appropriate.

[3] A Hausman test was performed in order to determine if regressions should include a fixed-effects estimator or not. The test did not find a systematic difference between the random-effects and the fixed-effects estimator.

The presence of the outlier might be also biasing the results and in fact, when the estimation is done through Tobit regressions or when the outlier is excluded, coefficients on *incubated*$_{i,}$ are no longer significant[4-5].

Table 10. Parallel trends test, random-effects Tobit regression including outlier.

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
incubated	182,518	1.024	102,022
	(237,575)	(6.765)	(172,670)
experience	65,145	2.627	192,821
	(193,360)	(5.467)	(156,241)
design	-472,882**	-9.770*	-1.113e+06
	(223,637)	(5.832)	(7.103e+07)
rec	-214,310	-72.67	-1.067e+06
	(317,179)	(2,043)	(7.230e+07)
other sectors	50,686	-0.349	-868,485
	(345,591)	(9.657)	(1.077e+08)
Constant	-185,692	-1.582	-373,610**
	(167,051)	(4.708)	(181,899)
Observations	105	107	107
Number of id	34	35	35
ll	-738.6	-218.0	-199.2
p	0.348	0.569	0.901

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

[4] One of the firms did not disclose its data on sales but informed that they were positive. The firm obtained a "quality index" score of 1 but did not incubate. This firm was dropped from the sample when dependent variable was sales.
[5] All the regressions ran are reported in the Annex.

Table 11. Parallel trends test, random-effects Tobit excluding outlier.

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
incubated	52,118	1.032	49,320
	(37,092)	(2.581)	(50,803)
experience	-4,119	0.495	73,882
	(32,013)	(2.194)	(45,055)
design	-84,172**	-3.175	-418,421
	(36,195)	(2.280)	(3.840e+07)
rec	-58,004	-31.18	-424,916
	(52,110)	(5,048)	(7.163e+07)
other sectors	-8,409	0.201	-345,969
	(53,523)	(3.659)	(1.599e+08)
Constant	-8,564	0.324	-135,449**
	(26,843)	(1.859)	(53,639)
Observations	97	99	99
Number of id	33	34	34
ll	-535.8	-133.0	-18.66
p	0.226	0.783	0.717

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

It has been stated above that applicants who reached the second selection stage (i.e. the Selection Committee) are a much more homogenous group than the ones that applied for the first stage. In fact, they are such a homogenous group that the sample satisfies both identification assumptions without the need to trim it around the cut-off point. As a consequence, comparisons between treated and non-treated individuals using the whole sample should also be considered.

5. Results

Graphical inspection of plots of the means of outcome variables conditional on the forcing variable suggest a positive impact of incubation on firms' sales and employment, while it appears that incubation does not affect their capacity to export.

Figure 4. Average annual sales vs. "quality index".

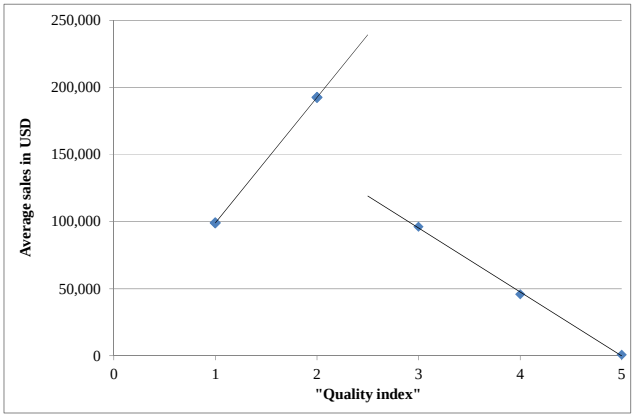


Figure 6. Average annual employment vs. "quality index".

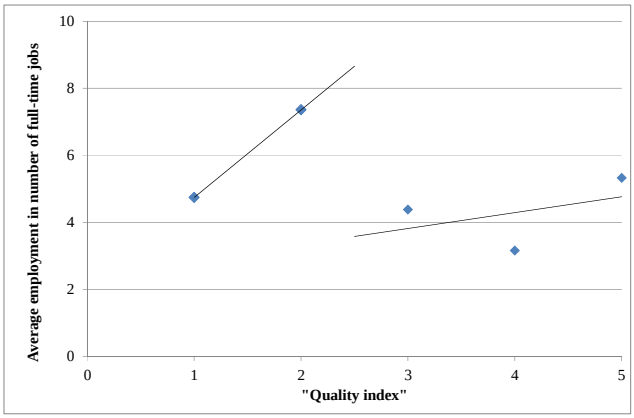
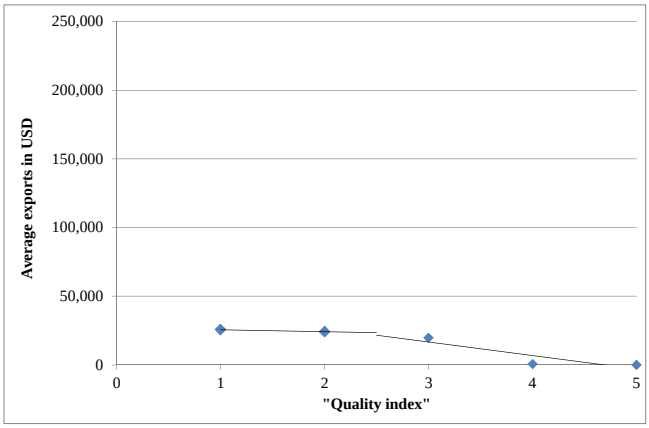


Figure 7. Average annual exports vs. "quality index".



To further assess the issue, a set of exploratory random-effects OLS and Tobit regressions with the whole sample was run in order to obtain additional preliminary evidence of a causal impact of incubation on firms´ performance[6-7]. Outcome variables $sales_{ij}$, $employment_{ij}$ and $exports_{ij}$ were regressed on $treated_{ij}$ and experience and industry controls were included. It has already been established that firms on both sides of the cut-off are similar, even when the whole sample is considered. In light of this, these estimations should not be disregarded. Tables 12 and 13 contain the results of the Tobit regressions for the three outcome variables. Estimations shown in Table 13 exclude the outlier mentioned earlier (additional random-effects OLS and Tobit regressions included in the Annex). Results using the whole sample point to a positive and significant effect of incubation on firm´s sales, employment and export levels.

[6] Again, the firm that did not disclose information on sales but stated that they were positive was not considered when dependent variable was sales.
[7] The Hausman test did not detect individual heterogeneity effects and so, random-effects estimators are more appropriate.

Table 12. Exploratory estimations, full-sample random-effects Tobit regression including outlier.

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	201,817**	4.927**	231,509**
	(83,914)	(2.249)	(91,284)
experience	109,595	2.487	280,496***
	(97,744)	(2.861)	(106,920)
design	-220,804*	-5.063	-279,452*
	(113,269)	(3.258)	(148,084)
rec	-127,219	-10.54**	-1.366e+06
	(161,511)	(5.268)	(5.030e+07)
other sectors	68,035	0.725	-911,790
	(240,720)	(6.787)	(5.930e+07)
Constant	-164,396*	-1.637	-488,823***
	(92,770)	(2.595)	(127,893)
Observations	186	188	188
Number of id	54	55	55
ll	-1617	-447.5	-590.6
p	0.0350	0.0397	0.0331

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

Table 13. Exploratory estimations, full-sample random-effects Tobit regression excluding outlier.

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	173,958***	4.093***	260,797***
	(42,706)	(1.159)	(92,494)
experience	39,838	0.772	243,037**
	(50,964)	(1.642)	(103,286)
design	-111,563*	-2.520	-245,166*
	(58,504)	(1.851)	(141,229)
rec	-82,218	-6.451**	-1.338e+06
	(83,236)	(2.966)	(7.171e+07)
other sectors	49,410	0.784	-858,127
	(122,143)	(3.725)	(6.377e+07)
Constant	-95,522**	-0.381	-499,702***
	(47,631)	(1.429)	(127,620)
Observations	178	180	180
Number of id	53	54	54
ll	-1433	-343.3	-504.6
p	0.000370	0.00218	0.0306

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

But to precisely address the issue of whether incubation does in fact have a significant effect on firms' performance, the sample has to be narrowed around the cut-off point. It has already been stated that firms are selected into treatment when the "quality index" (called the "forcing variable" in a regression discontinuity setting) takes a value of 1 or 2 and are selected out of it when the index equals 3, 4 or 5. Next, the results of the actual regression discontinuity are presented. Outcome variables $sales_{ij}$, $employment_{ij}$ and $exports_{ij}$ were regressed on $treated_{ij}$ and the control variables $experience_{i}$ and industry dummies using only observations that took the "quality index" values of 2 and 3. Since the variable $treated_{ij}$ equals one when the firm was being or had been incubated, its coefficient in the regression captures the actual LATE.

Put another way, it has already been demonstrated in the previous section that firms on both sides of the cut-off point are similar and they cannot control their forcing variable values completely, so assignment into treatment is "as random" (the continuity assumption holds), in particular for firms with "quality index" 2 and 3. It has also been shown that firms that were ultimately treated did not show a differential in performance prior to incubation. Therefore, dummy variable $treated_{ij}$ captures the possible difference in sales, employment or exports for firms that had been incubated or were undergoing incubation (the LATE).

Regressions of outcome variables on $treated_{ij}$ and the experience and industry controls using the trimmed sample show mixed results and are quite sensitive to the presence of a very extreme outlier and to the estimation method. Model sensitivity to this outlier is a direct consequence of small sample size but also of how much its values differ from the rest of the sample.

OLS results suggest a negative or insignificant effect in firms' performance if the outlier mentioned before is included in the sample. However, when it is removed the impact of incubation according to the OLS method is positive and significant for both sales and employment.

Given the large percentage of firms that show zero sales, employment or exports, Tobit models seem more appropriate. Tobit regressions detect no impact of incubation if the outlier is present, while they capture a positive and significant effect on sales and employment if it is excluded from the sample.

Finally and considering the small sample size, a bootstrapped estimation of both models may be in order, so the same regressions were run using 1,000 repetitions. When bootstrapped errors are used to carry out inference tests, OLS and Tobit estimations do not detect a significant effect of incubation on firms' performance measured as sales, employment or exports if controls are included. Table 14 summarizes the results of the regressions of the three outcome variables on $treated_{ij}$ [8]. The coefficients on display are the ones corresponding for the variable $treated_{ij.}$ The omitted sector in all regressions using industry dummies is ICT. Standard errors are presented below the coefficients between parentheses.

[8] All coefficients of all regressions reported in the Annex.

Table 14. Coefficient of *treated*$_{ij}$ in regressions using only firms around the cut-off point.

		Ordinary standard errors					
		Including outlier (N=36)			Excluding outlier (N=35)		
Controls		Including experience and industry controls	Including experience control	Not including controls	Including experience and industry controls	Including experience control	Not including controls
OLS	Sales	-105,753*	-65,165	-15,687	54,667**	65,124***	83,533***
		(63,609)	(60,040)	(63,294)	(24,660)	(22,605)	(22,198)
	Employment	-2.298	-1.328	-0.366	1.498*	1.753**	1.919**
		(1.965)	(1.838)	(1.897)	(0.854)	(0.799)	(0.785)
	Exports	-4,327	2,754	18,083	16,995	20,709	35,937**
		(20,450)	(19,077)	(20,042)	(20,306)	(18,417)	(18,103)
Tobit	Sales	68,460	159,779	193,760	125,444**	156,780***	167,746***
		(140,903)	(141,154)	(145,126)	(58,074)	(57,520)	(57,287)
	Employment	3.048	4.505	5.121	3.770**	4.164***	4.261***
		(3.790)	(3.699)	(3.779)	(1.609)	(1.571)	(1.557)
	Exports	8,071	86,890	242,045*	125,422	242,711**	314,843**
		(80,760)	(79,646)	(130,478)	(110,419)	(121,864)	(135,768)

		Bootstrapped standard errors					
		Including outlier (N=36)			Excluding outlier (N=35)		
Controls		Including experience and industry controls	Including experience control	Not including controls	Including experience and industry controls	Including experience control	Not including controls
OLS	Sales	-105,753	-65,165	-15,687	54,667	65,124	83,533*
		(72,520)	(60,719)	(49,753)	(47,015)	(41,537)	(44,173)
	Employment	-2.298	-1.328	-0.366	1.498	1.753	1.919
		(1.812)	(1.508)	(1.231)	(1.386)	(1.218)	(1.183)
	Exports	-4,327	2,754	18,083	16,995	20,709	35,937
		(34,847)	(29,515)	(31,738)	(51,798)	(47,258)	(49,629)
Tobit	Sales	68,460	159,779*	193,760**	125,444	156,780	167,746
		(101,346)	(89,018)	(87,107)	(130,152)	(126,592)	(124,295)
	Employment	3.048	4.505*	5.121**	3.770	4.164	4.261*
		(2.490)	(2.304)	(2.294)	(2.664)	(2.565)	(2.513)
	Exports	8,071	86,890	242,045	125,422	242,711	314,843
		(106,030)	(150,517)	(155,266)	(221,976)	(315,749)	(390,217)

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: industries are "rec" (research, education and consulting), design, ICT and "other sectors".
Omitted sector in all regressions using industry dummies is ICT.

With all evidence considered together, the conclusion can be drawn that data show only a timid support for the initial hypothesis when performance is measured through sales and employment, since the negative and significant coefficient detected by OLS regressions including the outlier can be disregarded due to the presence of the outlier and the fact that the method does not account for the large share of zeroes in the sample's dependent variables.

In light of the above, it can only be affirmed that incubation might induce slightly larger sales volumes and more rapid job creation on tenant firms, while no significant effects of incubation on incubatees' exports were detected. Meanwhile, what can be more categorically asserted is that incubation does not have a negative effect on the performance of tenants. One possible explanation for the limited impacts detected may be the small number of individuals, which may have reduced the sample´s power.

6. Concluding Remarks

This research measured the effects of incubation on the performance of new and innovative Uruguayan firms. The impact evaluation was narrowed to the largest and one of the oldest Uruguayan incubators, Ingenio, which is housed in the premises of the Uruguayan Technological Laboratory (LATU for its Spanish acronym). In order to carry out the evaluation, a database comprising of incubated firms and similar non-incubated companies was set up in collaboration with Ingenio's management, current and former incubatees and rejected candidates.

The incubator admits candidates through a two-stage selection process where the first stage is an online application form filled by potential incubatees and the second one a more thorough analysis carried out by Ingenio's Selection Committee. Most candidates are rejected in the first stage, so it is possible to conclude that the web form is an effective filter which only allows firms with similar (although not necessarily equal) potential to reach the Selection Committee. The treatment group consists of firms incubated in the period 2007-2013 and the control group are companies that were rejected in the Selection Committee stage in the same period.

With the collaboration of Ingenio's management, former and current incubatees and rejected candidates, a unique panel of data was set up. The data allowed for a regression discontinuity impact evaluation to be carried out. Information gathered included on and off incubator firms' sales, employment, exports and inward external investment as well as a measure of the experience of the entrepreneurs heading the company. In addition, the

incubator provided data related to the selection process. More precisely, decisions of the Selection Committee and their fundaments were disclosed, which made the construction of a "quality index" (as perceived by the incubator) possible. It was then possible to adopt a regression discontinuity approach using this "quality index" as a forcing variable that selected firms in and out of treatment. Both the parallel trends and selection in observables assumptions (which are key for this particular identification strategy) were tested and confirmed. Moreover, both assumptions held for the whole sample as well as for the sample trimmed around the cut-off point. This in turn implies that regressions using the whole sample should not be disregarded.

In order to assess the impact of incubation, exploratory OLS and Tobit regressions of outcome variables on a treatment dummy and experience and sector controls were performed using the whole sample (55 individuals). To further pinpoint the ATE on outcome variables sales, employment and exports, the same regressions were run using only observations immediately close to the cut-off point (36 individuals). Taking into account the small sample size, the regressions were run using bootstrapping techniques to carry out inference tests. Although inference tests performed in the standard way point to a significant and positive effect of incubation on tenants' sales and employment, coefficients were no longer significant after bootstrapping.

Therefore, it can be stated that evidence only exhibited a timid support for the initial hypothesis when performance is measured by firms' sales and employment. It should be noted that although the information gathered was quite complete, the database comprised of only 55 individuals. The reduced number of observations may have reduced the power of the sample to detect significant effects.

The most obvious limitation of this research is sample size and a natural extension would be to replicate it with a larger sample. Another possible extension would be to address how each of the services provided by the incubator ("prestigious address", counselling, networking, etc.) affected the performance of the incubatees. Finally, a relevant question is how important (given their existence) are the potential productivity spill-overs from the sort of enterprises incubated at Ingenio on their trading partners.

Annex

1) Ingenio's web postulation form

2) Web questionnaire used to survey information from firms

Si corresponde, indique en qué modalidad su empresa fue dada de alta de Ingenio (egreso o graduación). En caso contrario, marque "No corresponde" *

Si corresponde, indique en qué fecha su empresa fue dada de alta de Ingenio

dd/mm/aaaa

Facturación

Para los años en los que la empresa postulada aún no estaba operativa o no facturó, marque 0.

Facturación en 2005 (en US$) *
Indique la facturación total en dólares estadounidenses para el año 2005

Facturación en 2006 (en US$) *
Indique la facturación total en dólares estadounidenses para el año 2006

Facturación en 2007 (en US$) *
Indique la facturación total en dólares estadounidenses para el año 2007

Facturación en 2008 (en US$) *
Indique la facturación total en dólares estadounidenses para el año 2008

Facturación en 2009 (en US$) *
Indique la facturación total en dólares estadounidenses para el año 2009

Facturación en 2010 (en US$) *
Indique la facturación total en dólares estadounidenses para el año 2010

Facturación en 2011 (en US$) *
Indique la facturación total en dólares estadounidenses para el año 2011

Facturación en 2012 (en US$) *
Indique la facturación total en dólares estadounidenses para el año 2012

Facturación en 2013 (en US$) *
Indique la facturación total en dólares estadounidenses para el año 2013

Empleo

Refiere a puestos de trabajo a tiempo completo (40 hs. semanales). Para los años en los que la empresa postulada aún no estaba operativa o no había contratado personal a tiempo completo, marque 0.

Empleo en 2005 *
Puestos de trabajo de 40 hs. semanales en la empresa, en 2005

Empleo en 2006 *
Puestos de trabajo de 40 hs. semanales en la empresa, en 2006

Empleo en 2007 *
Puestos de trabajo de 40 hs. semanales en la empresa, en 2007

Empleo en 2008 *
Puestos de trabajo de 40 hs. semanales en la empresa, en 2008

Empleo en 2009 *
Puestos de trabajo de 40 hs. semanales en la empresa, en 2009

Empleo en 2010 *
Puestos de trabajo de 40 hs. semanales en la empresa, en 2010

Empleo en 2011 *
Puestos de trabajo de 40 hs. semanales en la empresa, en 2011

Empleo en 2012 *
Puestos de trabajo de 40 hs. semanales en la empresa, en 2012

Empleo en 2013 *
Puestos de trabajo de 40 hs. semanales en la empresa, en 2013

Exportaciones

Para los años en los que la empresa postulada aún no estaba operativa o no exportó, marque 0.

Exportaciones en 2005 (en US$) *

Exportaciones en 2006 (en US$) *

Exportaciones en 2007 (en US$) *

Exportaciones en 2008 (en US$) *

Exportaciones en 2009 (en US$) *

Exportaciones en 2010 (en US$) *

Exportaciones en 2011 (en US$) *

Exportaciones en 2012 (en US$) *

Exportaciones en 2013 (en US$) *

Inversión externa

Se entiende por "inversión externa" toda inversión de terceros. Incluye préstamos de amigos y/o familiares, capital ángel, otros tipos de inversión de cartera, financiamiento institucional (Fondo Emprender, Fundación Ricaldoni, etc) y fondos subsidiados (ANII, Uruguay XXI, etc). Para los años en que la empresa postulada aún no estaba operativa o no recibió inversión externa, marque 0.

Inversión externa en 2005 (en US$) *
Indique el monto total de inversión externa en 2005, en dólares estadounidenses.

Inversión externa en 2006 (en US$) *
Indique el monto total de inversión externa en 2006, en dólares estadounidenses.

Inversión externa en 2007 (en US$) *
Indique el monto total de inversión externa en 2007, en dólares estadounidenses.

Inversión externa en 2008 (en US$) *
Indique el monto total de inversión externa en 2008, en dólares estadounidenses.

Inversión externa en 2009 (en US$) *
Indique el monto total de inversión externa en 2009, en dólares estadounidenses.

Inversión externa en 2010 (en US$) *
Indique el monto total de inversión externa en 2010, en dólares estadounidenses.

Inversión externa en 2011 (en US$) *
Indique el monto total de inversión externa en 2011, en dólares estadounidenses.

Inversión externa en 2012 (en US$) *
Indique el monto total de inversión externa en 2012, en dólares estadounidenses.

Inversión externa en 2013 (en US$) *
Indique el monto total de inversión externa en 2013, en dólares estadounidenses.

Submit

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3) Parallel trends tests: random-effects OLS regression including outlier, bootstrapped errors (1000 repetitions)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
incubated	-92,829	-2.497*	-22,736***
	(60,121)	(1.456)	(8,811)
experience	154,457**	3.948***	31,083***
	(61,156)	(1.168)	(9,170)
design	-131,870***	-3.859***	-21,639***
	(42,747)	(0.919)	(6,526)
rec	-108,903***	-4.835***	-18,172***
	(40,775)	(0.961)	(6,673)
other sectors	-91,611***	-2.279**	-14,587***
	(30,747)	(1.108)	(4,530)
Constant	97,986***	4.047***	14,587***
	(30,703)	(0.693)	(4,530)
Observations	105	107	107
Number of id	34	35	35
ll	0.0737	1.85e-05	0.0251

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

3) Parallel trends tests: random-effects Tobit regression including outlier, bootstrapped errors (1000 repetitions)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
incubated	182,518	1.024	102,022
	(202,097)	(6.579)	(1.084e+06)
experience	65,145	2.627	192,821
	(95,850)	(2.407)	(399,334)
design	-472,882**	-9.770***	-1.113e+06
	(193,640)	(3.118)	(3.872e+06)
rec	-214,310	-72.67**	-1.067e+06
	(163,179)	(29.66)	(1.775e+07)
other sectors	50,686	-0.349	-868,485
	(434,968)	(16.30)	(3.518e+06)
Constant	-185,692*	-1.582	-373,610
	(101,973)	(2.183)	(407,832)
Observations	105	107	107
Number of id	34	35	35
ll	-738.6	-218.0	-199.2
p	0.158	0.00793	0.992

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

5) Parallel trends tests: random-effects OLS regression excluding outlier, bootstrapped errors (1000 repetitions)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
incubated	13,493	-0.225	-3,992
	(17,901)	(0.804)	(4,463)
experience	3,781	0.701	5,483
	(11,057)	(0.601)	(5,892)
design	-27,375***	-1.306***	-3,829
	(9,082)	(0.466)	(3,657)
rec	-27,630**	-2.500***	-3,218
	(10,761)	(0.463)	(3,999)
other sectors	-21,884**	-0.404	-2,583
	(8,572)	(0.937)	(2,587)
Constant	26,743***	2.223***	2,583
	(7,830)	(0.381)	(2,587)
Observations	97	99	99
Number of id	33	34	34
ll	0.0857	6.17e-07	0.932

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

6) Parallel trends tests: random-effects Tobit regression excluding outlier, bootstrapped errors (1000 repetitions)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
incubated	52,118	1.032	49,320
	(69,407)	(9.609)	(956,184)
experience	-4,119	0.495	73,882
	(21,384)	(1.179)	(176,205)
design	-84,172***	-3.175***	-418,421
	(25,652)	(1.204)	(1.002e+08)
rec	-58,004*	-31.18	-424,916
	(32,766)	(190.1)	(8.977e+07)
other sectors	-8,409	0.201	-345,969
	(82,341)	(7.032)	(8.376e+07)
Constant	-8,564	0.324	-135,449
	(16,000)	(0.948)	(278,240)
Observations	97	99	99
Number of id	33	34	34
ll	-535.8	-133.0	-18.66
p	0.0508	0.192	0.999

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

7) Exploratory estimations: full-sample random-effects OLS regression including outlier

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	12,402	0.810	22,287
	(42,807)	(1.332)	(15,274)
experience	111,300**	2.508*	27,739*
	(43,250)	(1.478)	(15,426)
design	-122,997***	-2.655	-35,676**
	(46,966)	(1.652)	(16,786)
rec	-124,480*	-3.975	-35,995
	(72,817)	(2.457)	(26,096)
other sectors	-98,549	-1.715	-21,163
	(146,636)	(3.986)	(52,584)
Constant	104,924**	3.496**	21,163
	(41,472)	(1.374)	(14,812)
Observations	186	188	188
Number of id	54	55	55
ll	0.00228	0.100	0.0162

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

8) Exploratory estimations: full-sample random-effects OLS regression excluding outlier

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	88,230***	2.115***	34,982**
	(22,822)	(0.733)	(14,594)
experience	30,597	0.845	14,494
	(23,803)	(0.936)	(14,763)
design	-46,192*	-1.226	-24,158
	(25,941)	(1.052)	(15,918)
rec	-66,496*	-2.655*	-26,872
	(39,314)	(1.500)	(24,445)
other sectors	-23,907	-0.309	-10,047
	(70,503)	(2.253)	(49,119)
Constant	29,979	2.113**	10,047
	(22,492)	(0.826)	(14,091)
Observations	178	180	180
Number of id	53	54	54
ll	5.39e-05	0.0127	0.0282

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

9) Exploratory estimations: full-sample random-effects OLS regression including outlier, bootstrapped errors (seed value 1, 1000 repetitions)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	12,402	0.810	22,287
	(36,664)	(0.765)	(15,666)
experience	111,300***	2.508***	27,739*
	(35,334)	(0.676)	(14,356)
design	-122,997***	-2.655***	-35,676***
	(29,824)	(0.566)	(8,450)
rec	-124,480***	-3.975***	-35,995***
	(26,542)	(0.587)	(9,263)
other sectors	-98,549***	-1.715	-21,163**
	(31,015)	(1.053)	(9,266)
Constant	104,924***	3.496***	21,163**
	(30,984)	(0.623)	(9,266)
Observations	186	188	188
Number of id	54	55	55
ll	1.38e-08	0	0.000622

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

10) Exploratory estimations: full-sample random-effects OLS regression excluding outlier, bootstrapped errors (1000 repetitions)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	88,230***	2.115***	34,982**
	(23,892)	(0.744)	(15,444)
experience	30,597*	0.845*	14,494
	(17,678)	(0.474)	(14,891)
design	-46,192***	-1.226***	-24,158***
	(12,924)	(0.420)	(7,850)
rec	-66,496***	-2.655***	-26,872***
	(14,341)	(0.413)	(9,522)
other sectors	-23,907	-0.309	-10,047
	(15,266)	(0.963)	(8,653)
Constant	29,979**	2.113***	10,047
	(14,914)	(0.513)	(8,653)
Observations	178	180	180
Number of id	53	54	54
ll	1.22e-08	1.00e-10	0.0181

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

11) Exploratory estimations: full sample random-effects Tobit regression including outlier, bootstrapped errors (1000 repetitions)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	201,817***	4.927***	231,509
	(54,408)	(1.400)	(309,739)
experience	109,595**	2.487**	280,496
	(47,396)	(1.047)	(782,456)
design	-220,804***	-5.063***	-279,452
	(54,990)	(1.267)	(633,098)
rec	-127,219	-10.54**	-1.366e+06
	(87,014)	(5.146)	(2.326e+06)
other sectors	68,035	0.725	-911,790
	(285,696)	(10.13)	(1.951e+06)
Constant	-164,396***	-1.637	-488,823
	(56,581)	(1.255)	(998,523)
Observations	186	188	188
Number of id	54	55	55
ll	-1617	-447.5	-590.6
p	7.34e-05	4.48e-06	0.720

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

12) Exploratory estimations: full sample random-effects Tobit regression excluding outlier, bootstrapped errors (1000 repetitions)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	173,958***	4.093***	260,797*
	(49,641)	(1.328)	(134,895)
experience	39,838	0.772	243,037*
	(28,107)	(0.709)	(136,483)
design	-111,563***	-2.520***	-245,166**
	(28,393)	(0.785)	(97,513)
rec	-82,218**	-6.451**	-1.338e+06
	(32,801)	(2.682)	(1.544e+07)
other sectors	49,410	0.784	-858,127
	(157,594)	(4.849)	(1.231e+07)
Constant	-95,522**	-0.381	-499,702**
	(40,479)	(0.975)	(233,627)
Observations	178	180	180
Number of id	53	54	54
ll	-1433	-343.3	-504.6
p	0.000106	7.21e-05	0.250

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

13) Results: trimmed-sample random-effects OLS regression including outlier, no controls

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	-15,687	-0.366	18,083
	(63,294)	(1.897)	(20,042)
Constant	113,402***	3.860***	20,843*
	(39,026)	(1.273)	(12,357)
Observations	121	121	121
Number of id	36	36	36
ll	0.804	0.847	0.367

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

14) Results: trimmed-sample random-effects OLS regression including outlier, experience control

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	-65,165	-1.328	2,754
	(60,040)	(1.838)	(19,077)
experience	272,678***	5.938***	84,478***
	(62,361)	(1.995)	(19,814)
Constant	44,323	2.371*	-557.7
	(39,640)	(1.319)	(12,595)
Observations	121	121	121
Number of id	36	36	36
ll	6.81e-05	0.0116	7.09e-05

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

15) Results: trimmed-sample random-effects OLS regression including outlier, experience and industry controls

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	-105,753*	-2.298	-4,327
	(63,609)	(1.965)	(20,450)
experience	229,592***	4.849**	76,227***
	(65,197)	(2.089)	(20,960)
design	-140,638*	-3.607	-25,766
	(74,102)	(2.434)	(23,823)
rec	-136,548	-4.631*	-32,564
	(85,029)	(2.758)	(27,336)
other sectors	-128,320	-3.952	-17,307
	(189,699)	(5.302)	(60,987)
Constant	136,653**	4.892**	17,307
	(58,496)	(1.911)	(18,806)
Observations	121	121	121
Number of id	36	36	36
ll	0.000198	0.0263	0.000847

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

16) Results: trimmed-sample random-effects OLS regression including outlier, no controls, bootstrapped errors (1,000 replications)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	-15,687	-0.366	18,083
	(49,753)	(1.231)	(31,738)
Constant	113,402***	3.860***	20,843
	(38,984)	(0.806)	(12,852)
Observations	121	121	121
Number of id	36	36	36
ll	0.753	0.766	0.569

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

17) Results: trimmed-sample random-effects OLS regression including outlier, experience control, bootstrapped errors (1,000 replications)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	-65,165	-1.328	2,754
	(60,719)	(1.508)	(29,515)
experience	272,678***	5.938***	84,478***
	(83,007)	(1.622)	(23,287)
Constant	44,323**	2.371***	-557.7
	(19,814)	(0.556)	(11,199)
Observations	121	121	121
Number of id	36	36	36
ll	0.000366	0.000139	0.00132

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

18) Results: trimmed-sample random-effects OLS regression including outlier, experience and industry controls, bootstrapped errors (1,000 replications)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	-105,753	-2.298	-4,327
	(72,520)	(1.812)	(34,847)
experience	229,592***	4.849***	76,227***
	(69,346)	(1.375)	(22,012)
design	-140,638***	-3.607***	-25,766*
	(49,976)	(1.173)	(15,058)
rec	-136,548***	-4.631***	-32,564**
	(44,236)	(1.062)	(12,878)
other sectors	-128,320***	-3.952***	-17,307
	(47,985)	(1.315)	(19,148)
Constant	136,653***	4.892***	17,307
	(47,549)	(1.165)	(19,148)
Observations	121	121	121
Number of id	36	36	36
ll	0.000486	6.99e-06	0.0146

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

19) Results: trimmed-sample random-effects Tobit regression including outlier, no controls

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	193,760	5.121	242,045*
	(145,126)	(3.779)	(130,478)
Constant	-265,086**	-4.418	-528,365***
	(115,570)	(3.009)	(153,700)
Observations	121	121	121
Number of id	36	36	36
ll	-973.2	-290.7	-331.4
p	0.182	0.175	0.0636

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

20) Results: trimmed-sample random-effects Tobit regression including outlier, experience control

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	159,779	4.505	86,890
	(141,154)	(3.699)	(79,646)
experience	213,895	4.995	390,338***
	(155,548)	(4.332)	(92,636)
Constant	-312,314***	-5.643*	-484,733***
	(120,556)	(3.184)	(107,585)
Observations	121	121	121
Number of id	36	36	36
ll	-972.3	-290.0	-320.8
p	0.163	0.206	8.05e-05

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

21) Results: trimmed-sample random-effects Tobit regression including outlier, experience and industry controls

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	68,460	3.048	8,071
	(140,903)	(3.790)	(80,760)
experience	122,372	2.717	331,084***
	(153,730)	(4.368)	(90,248)
design	-457,525**	-7.648	-1.488e+06
	(202,724)	(5.262)	(3.953e+08)
rec	-150,607	-12.22*	-1.511e+06
	(199,225)	(6.235)	(4.873e+08)
other sectors	-68,529	-3.815	-1.316e+06
	(357,674)	(9.937)	(2.965e+09)
Constant	-105,798	-0.422	-316,595***
	(143,475)	(3.980)	(95,510)
Observations	121	121	121
Number of id	36	36	36
ll	-969.6	-287.6	-322.4
p	0.122	0.155	0.0190

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

22) Results: trimmed-sample random-effects Tobit regression including outlier, no controls, bootstrapped errors (1,000 replications)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	193,760**	5.121**	242,045
	(87,107)	(2.294)	(155,266)
Constant	-265,086***	-4.418**	-528,365***
	(81,374)	(1.769)	(202,345)
Observations	121	121	121
Number of id	36	36	36
ll	-973.2	-290.7	-331.4
p	0.0261	0.0256	0.119

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

23) Results: trimmed-sample random-effects Tobit regression including outlier, experience control, bootstrapped errors (1,000 replications)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	159,779*	4.505*	86,890
	(89,018)	(2.304)	(150,517)
experience	213,895***	4.995***	390,338
	(80,568)	(1.701)	(280,192)
Constant	-312,314***	-5.643***	-484,733
	(89,014)	(1.811)	(473,449)
Observations	121	121	121
Number of id	36	36	36
ll	-972.3	-290.0	-320.8
p	0.00178	0.000513	0.369

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

24) Results: trimmed-sample random-effects Tobit regression including outlier, experience and industry controls, bootstrapped errors (1,000 replications)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	68,460	3.048	8,071
	(101,346)	(2.490)	(106,030)
experience	122,372	2.717*	331,084**
	(74,655)	(1.529)	(141,896)
design	-457,525***	-7.648***	-1.488e+06
	(141,947)	(2.338)	(2.226e+08)
rec	-150,607*	-12.22***	-1.511e+06
	(85,665)	(4.083)	(2.233e+08)
other sectors	-68,529	-3.815	-1.316e+06
	(1.052e+06)	(18.05)	(2.222e+08)
Constant	-105,798	-0.422	-316,595*
	(80,230)	(1.828)	(186,026)
Observations	121	121	121
Number of id	36	36	36
ll	-969.6	-287.6	-322.4
p	0.00483	0.000167	0.338

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

25) Results: trimmed-sample random-effects OLS regression excluding outlier, no controls

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	83,533***	1.919**	35,937**
	(22,198)	(0.785)	(18,103)
Constant	14,181	1.448***	2,989
	(14,163)	(0.559)	(11,550)
Observations	113	113	113
Number of id	35	35	35
ll	0.000168	0.0145	0.0471

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

26) Results: trimmed-sample random-effects OLS regression excluding outlier, experience control

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	65,124***	1.753**	20,709
	(22,605)	(0.799)	(18,417)
experience	68,029***	1.033	56,276***
	(24,891)	(0.937)	(20,279)
Constant	3,012	1.208**	-6,251
	(14,361)	(0.599)	(11,700)
Observations	113	113	113
Number of id	35	35	35
ll	1.33e-05	0.0273	0.00263

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

27) Results: trimmed-sample random-effects OLS regression excluding outlier, experience and industry controls

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	54,667**	1.498*	16,995
	(24,660)	(0.854)	(20,306)
experience	61,568**	0.856	53,895***
	(25,351)	(0.962)	(20,876)
design	-29,826	-0.797	-11,038
	(27,436)	(1.106)	(22,592)
rec	-51,677*	-2.278*	-21,283
	(31,021)	(1.225)	(25,544)
other sectors	-18,403	-1.103	-2,698
	(68,656)	(2.197)	(56,535)
Constant	26,736	1.991**	2,698
	(21,983)	(0.863)	(18,102)
Observations	113	113	113
Number of id	35	35	35
ll	0.000109	0.0563	0.0298

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

28) Results: trimmed-sample random-effects OLS regression excluding outlier, no controls, bootstrapped errors (1,000 replications)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	83,533*	1.919	35,937
	(44,173)	(1.183)	(49,629)
Constant	14,181	1.448**	2,989
	(17,825)	(0.566)	(22,510)
Observations	113	113	113
Number of id	35	35	35
ll	0.0586	0.105	0.469

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

29) Results: trimmed-sample random-effects OLS regression excluding outlier, experience control, bootstrapped errors (1,000 replications)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	65,124	1.753	20,709
	(41,537)	(1.218)	(47,258)
experience	68,029**	1.033	56,276**
	(29,141)	(0.691)	(26,197)
Constant	3,012	1.208**	-6,251
	(16,416)	(0.511)	(20,368)
Observations	113	113	113
Number of id	35	35	35
ll	0.0210	0.0375	0.0850

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

30) Results: trimmed-sample random-effects OLS regression excluding outlier, experience and industry controls, bootstrapped errors (1,000 replications)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	54,667	1.498	16,995
	(47,015)	(1.386)	(51,798)
experience	61,568**	0.856	53,895**
	(28,002)	(0.647)	(24,628)
design	-29,826	-0.797	-11,038
	(23,263)	(0.781)	(21,487)
rec	-51,677**	-2.278***	-21,283
	(20,313)	(0.539)	(16,481)
other sectors	-18,403	-1.103	-2,698
	(28,831)	(1.050)	(31,313)
Constant	26,736	1.991**	2,698
	(28,507)	(0.894)	(31,313)
Observations	113	113	113
Number of id	35	35	35
ll	0.00324	1.82e-05	0.372

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consultin
Omitted variable is ICT sectoral dummy.

31) Results: trimmed-sample random-effects Tobit regression excluding outlier,no controls

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	167,746***	4.261***	314,843**
	(57,287)	(1.557)	(135,768)
Constant	-134,616***	-1.956	-581,552***
	(47,610)	(1.306)	(165,373)
Observations	113	113	113
Number of id	35	35	35
ll	-806.4	-202.1	-243.7
p	0.00341	0.00619	0.0204

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

32) Results: trimmed-sample random-effects Tobit regression excluding outlier, experience control

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	156,780***	4.164***	242,711**
	(57,520)	(1.571)	(121,864)
experience	54,062	0.705	300,061**
	(64,500)	(1.966)	(127,779)
Constant	-144,179***	-2.116	-618,125***
	(49,359)	(1.387)	(168,711)
Observations	113	113	113
Number of id	35	35	35
ll	-806.1	-202.0	-240.7
p	0.00941	0.0219	0.00941

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

33) Results: trimmed-sample random-effects Tobit regression excluding outlier, experience and industry controls

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	125,444**	3.770**	125,422
	(58,074)	(1.609)	(110,419)
experience	25,552	0.0101	261,094**
	(63,471)	(1.949)	(102,396)
design	-159,832**	-2.348	-1.299e+06
	(80,691)	(2.258)	(1.393e+08)
rec	-68,745	-5.726**	-1.402e+06
	(79,749)	(2.725)	(1.582e+08)
other sectors	7,261	-1.045	-1.153e+06
	(141,998)	(4.202)	(5.528e+08)
Constant	-70,728	-0.208	-394,201***
	(58,625)	(1.720)	(119,671)
Observations	113	113	113
Number of id	35	35	35
ll	-803.9	-199.6	-237.0
p	0.0245	0.0370	0.0959

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

34) Results: trimmed-sample random-effects Tobit regression excluding outlier, bootstrapped errors (1,000 replications)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	167,746	4.261*	314,843
	(124,295)	(2.513)	(390,217)
Constant	-134,616	-1.956	-581,552
	(91,829)	(1.618)	(457,818)
Observations	113	113	113
Number of id	35	35	35
ll	-806.4	-202.1	-243.7
p	0.177	0.0900	0.420

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

35) Results: trimmed-sample random-effects Tobit regression excluding outlier, experience control, bootstrapped errors (1,000 replications)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	156,780	4.164	242,711
	(126,592)	(2.565)	(315,749)
experience	54,062	0.705	300,061
	(47,321)	(1.053)	(219,928)
Constant	-144,179	-2.116	-618,125
	(89,791)	(1.520)	(494,546)
Observations	113	113	113
Number of id	35	35	35
ll	-806.1	-202.0	-240.7
p	0.0993	0.0785	0.361

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

36) Results: trimmed-sample random-effects Tobit regression excluding outlier, experience and industry controls, bootstrapped errors (1,000 replications)

DEPENDENT VARIABLES	(1) sales	(2) employment	(3) exports
treated	125,444	3.770	125,422
	(130,152)	(2.664)	(221,976)
experience	25,552	0.0101	261,094
	(44,233)	(0.956)	(160,220)
design	-159,832**	-2.348*	-1.299e+06
	(77,698)	(1.382)	(4.502e+08)
rec	-68,745*	-5.726***	-1.402e+06
	(40,540)	(1.788)	(4.525e+08)
other sectors	7,261	-1.045	-1.153e+06
	(1.013e+06)	(16.92)	(4.395e+08)
Constant	-70,728	-0.208	-394,201
	(93,468)	(1.839)	(306,050)
Observations	113	113	113
Number of id	35	35	35
ll	-803.9	-199.6	-237.0
p	0.211	0.00244	0.746

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "rec" stands for research, education and consulting.
Omitted variable is ICT sectoral dummy.

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