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**by**

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# Intended and unintended effects of public incentives for innovation. Quasi-experimental evidence from Italy

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**Abstract:** *This paper provides an extensive empirical evaluation of a policy introduced in Italy at the end of 2012 to incentivize young innovative start-up firms. Using a Regression Discontinuity Design (RDD) we estimate the causal effects of the policy on the firms' share of intangible assets, turnover, number of employees, and number of partners. Our results indicate that two years after its implementation the policy was effective only in increasing the number of partners, thus attracting private investments, but failed, at least in the short run, in boosting innovation or increasing employment. It follows that the new investors generated by the policy might have been attracted only by the tax benefit and had little interest in innovation.*

**Keywords:** policy evaluation, regression discontinuity design, incentives to innovations
**JEL classification:** H32, L52, C21, O31

## 1. Introduction

Innovation is largely considered to be a key determinant of economic growth, and the sizable literature on investments in Research and Development (R&D) suggests large positive returns and even higher social returns (Hall et al., 2010). The fact that investments in innovation are considered to be public goods (e.g., Schumpeter, 1942; Arrow, 1972) motivates the need of publicly founded incentives for innovation (Bloom et al., 2013; Hall and Lerner, 2010). The empirical literature on publicly founded incentives for innovation has mostly focused on estimating the elasticities of R&D investments to taxes, finding mixed results in the short run. Some studies, such as Hall (1993), Hines Jr. (1994), Bloom et al. (2002), and Wilson (2009), do not find any short-run effect of tax incentive for R&D, while others (Rao, 2016; Thomson, 2015) find strong positive effects.

As pointed out by Thomson (2015), evidence based on firm-level data in a single country is likely to be biased. One issue is that differences in tax credits between firms are likely to be endogenous to the current level of R&D investment (Hall and Van Reenen, 2000). Moreover, and most importantly, R&D expenditure depends on firm-level characteristics that are likely to be correlated with the amount of tax credit received (Griffiths and Webster, 2010; Dagenais et al., 1997). Therefore, firm level analysis has not generated a consensus on the short-run effect of tax credits on R&D expenditure. One exception is Rao (2016), who uses an IV strategy to estimate the effect of R&D taxes on R&D expenditure. The proposed instrument assumes that there are no time varying firm specific characteristics that affect the evolution in R&D spending other than their tax credit. Cross-country studies exploit variation in policies among countries to estimate the effect of tax credits on aggregate R&D investment (Bloom et al., 2002; Guellec and Pottelsberghe, 2003; Falk, 2006, Thomson, 2015). Although those studies can control for time-constant confounders, they still rely on strong assumptions for identification.

In contrast to these studies, we are able to identify the effect of a substantial tax reduction for investing in innovative firms on innovation activity under minimal assumptions. More generally, the aim of this study is to provide a comprehensive counterfactual evaluation of a new policy introduced in Italy to incentivize young, innovative start-up firms. We exploit exogenous variation in eligibility in a (RDD) context to estimate the causal effects of the intervention on firms' share of intangible assets, turnover, number of employees, and number of partners. We found sizeable positive effects of the policy on the number of partners. This is in line with Finaldi Russo et al. (2016), who find a positive effect on external funding in a preliminary evaluation of the policy. Our result can be interpreted as evidence that the generous tax reduction for investors introduced as part of the policy has been successful in attracting private funds, even in the short run. Unfortunately, the increase in private investments does not seem to have generated the intended effects, at least in the short run. Indeed, we do not find any statistically significant effect on the share of intangible assets, the turnover, and the number of employees. This casts serious doubts on the overall effectiveness of the policy, as the new private investors might have merely used the firms to benefit from tax reduction. Our findings suggest that it might have been beneficial for private investors to link the tax benefit to actual investments in innovation activities.

The remainder of the paper is organized as follows. Section 2 provides institutional details about the policy. Section 3 describes the data set, while Section 4 discusses the identification strategy. The results, robustness checks, considerations about external validity, and a back of the envelope calculation of the costs faced by the Italian government are reported in Sections 5, 6, 7, and 8, respectively. Section 9 concludes.

## 2. The policy

In 2012, the Italian government introduced a series of incentives for young and newly created innovative enterprises with a high technological content.[1] The aim of the new legislation, as explicitly stated in the Italian Ministry of Economic Development's executive summary, was to "promote sustainable growth, technological advancement and to create favorable conditions for the development of a new business culture inclined towards innovation".[2]

Firms that fulfilled the eligibility criteria listed below had the possibility to register in a newly created special section of the Business Register as "innovative start-ups" if they:

(i) had been established for less than 48 months;[3]

(ii) had their headquarter in Italy or in any EU member State, but with at least one production site branch in Italy;

(iii) did not distribute profits;

(iv) did not originate from a company merger, split-up, or selling-off;

(v) had an innovative character identified by at least one of the following criteria:
   a. at least 15% of annual costs being devoted to R&D,
   b. at least one third of the total workforce being PhD students/graduates or researchers, or alternatively two third of the total workforce holding a master's degree,
   c. being the owner, depositary or license holder of at least a patent, or owner of at least a registered software pertain to biotechnology or semiconductor or plant variety;

(vi) had as their exclusive or prevalent company object production the development and commercialization of innovative goods or services of high technological value;

(vii) had a yearly turnover not surpassing 5 million euros;

Being registered as innovative start-up gave access to a number of benefits:[4]

1) online free-of-charge setting-up procedure;
2) cuts to red tape and fees;
3) flexible corporate management;

[1] The incentives together with the new type of firms were introduced with the Decree Law 179/2012, successively converted into Law 221/2012.
[2] https://www.mise.gov.it/images/stories/documenti/Executive-Summary-of-Italy-s-Startup-Act-new-format-23_02_2017.pdf
[3] In 2015 this was increased to 60 months.
[4] For a detailed explanation of each of them we refer the interested reader to the official guide provided by the Italian Ministry of Economic Development: http://www.mise.gov.it/images/stories/documenti/Executive-Summary-of-Italy-s-Startup-Act-new-format-23_02_2017.pdf

4) extension of terms for covering losses;
5) exemption from regulations on dummy companies;
6) easier compensation for VAT credits;
7) tailor-made labor law;
8) flexible remuneration system;
9) remuneration through stock options and work for equity schemes;
10) tax credit for hiring highly qualified staff;
11) tax incentives for corporate and individual investments;
12) equity crowdfunding;
13) fast-track access to the Central Fund for Credit Guarantees to Small and Medium Enterprises (SME);
14) targeted support to internationalization from the Italian Trade Agency;
15) "fail-fast" procedure;
16) easy access to a second policy program referred to as "innovative SME";
17) subsidized financial scheme consisting in zero interest rate loans from Invitalia a public agency.

At the onset of the policy not all the listed benefits were in place. The fast track access to the Central Fund for Credit Guarantees (item 13) did not start until May 2013 (see Zecchini and Ventura, 2009, and Boschi et al., 2014, for an evaluation of this policy instrument), while the tax benefits for investors (item 11) started in January 2014. The subsidized financial scheme (item 17) was enacted in February 2015 and thus refers to a period not covered by our data. For this reason it is not part of the treatment. Nevertheless, we decided to mention this benefit for the sake of completeness.

## 3. The dataset

### *3.1 Data sources and final sample*

Our main data set is the *Archivio Statistico delle Imprese Attive* (Asia), which contains information on the universe of around 4.4 million firms registered in Italy and was provided by the Italian National Institute of Statistics (Istat). Information about "innovative firms" is publicly available at the Business Register's web page, including their ID code.[5] This allowed us to identify the treated firms in the main data set, which are all the one registered as "innovative" over the period 31st October 2012-31st December 2014. The non-treated firms are defined as those who meet all the eligibility criteria, except

[5] http://startup.registroimprese.it/isin/home

the one concerning their age (item 1 Section 2), where we do not place any restriction. Most of the eligibility criteria are simple binary indicators, and therefore selecting non-treated firms which fulfill these criteria is straightforward. As far as criteria (v) and (vi) are concerned, selecting non-treated firms that fulfill these two criteria is a bit more challenging and in the latter case requires (mild) additional assumptions. Criterion (vi) requires that the company objective is exclusively or prevalently devoted to production, development, and commercialization of innovative goods or services. It follows that we have selected eligible non-treated firms operating in the same sectors of economic activity as the treated firms, as can be observed by having the identical NACE (Rev. 2) codes and after having discarded few outliers (more details are available from the authors upon request). Whether firms are innovation-oriented, as required in (v), is not directly observable from the data.[6] Nevertheless, it has been well documented in the literature (see Marrocu et al., 2012 and the literature therein) that innovative firms are characterized by a high value of the share of intangible assets over total assets. Therefore, we selected eligible non-treated firms to reproduce a similar empirical distribution of the share of intangible assets as that of the treated firms (see the appendix for more details).

At the end of the selection process, we are left with 27,038 eligible (except for their age) firms, of which 3,115 were registered as innovative start-ups ( treated). To further ensure that the non-treated firms are comparable to the treated ones we used a pre-processing technique (King and Zeng, 2006; Ho et al., 2007). This technique consists in the estimation of a propensity score using only pre-treatment variables and discarding all units with an estimated propensity score outside the range [0.1,0.9] (Crump et al., 2009).[7] As pre-treatment information we used sectoral dummies, area dummies, belonging to a group of firms, their legal form, and a dummy for whether they were already born before the promulgation of the law. After the pre-processing we have 12,520 firms, of which 1,576 treated. The balancing properties is comparatively good as shown in Table 1.

Table 1. Balancing test

| Variable | Unmatc., Matched | Mean | | % bias | t-val | P-val |
|---|---|---|---|---|---|---|
| | | Treated | Control | | | |
| Sector2 | U | 0.05 | 0.28 | -63.90 | -27.80 | 0.00 |
| | M | 0.05 | 0.05 | 0.00 | 0.00 | 1.00 |
| Sector3 | U | 0.66 | 0.32 | 73.50 | 39.30 | 0.00 |
| | M | 0.66 | 0.66 | 0.00 | 0.00 | 1.00 |

[6] Unfortunately, employees' educational attainments are not reported in our data set and nor is it possible to retrieve this piece of information by merging workers' individual data set with firms' ID code. R&D expenses are largely misreported in balance sheet data. Finally, a complete patent data set allowing the merging of information from patent holder to firm level data is not available in Italy.

[7] Using more or less restrictive trimming rules, [0.15, 0.85] and [0.05, 0.95], respectively, does not substantially affect our results.

| | | | | | | |
|---|---|---|---|---|---|---|
| Sector4 | U | 0.10 | 0.22 | -31.70 | -15.10 | 0.00 |
| | M | 0.10 | 0.10 | 0.00 | 0.00 | 1.00 |
| North-East | U | 0.25 | 0.21 | 10.60 | 5.80 | 0.00 |
| | M | 0.25 | 0.25 | 0.00 | 0.00 | 1.00 |
| Center | U | 0.22 | 0.26 | -9.90 | -5.12 | 0.00 |
| | M | 0.22 | 0.22 | 0.00 | 0.00 | 1.00 |
| South | U | 0.22 | 0.19 | 7.20 | 3.90 | 0.00 |
| | M | 0.22 | 0.22 | 0.00 | 0.00 | 1.00 |
| Ingroup | U | 0.14 | 0.23 | -24.70 | -12.10 | 0.00 |
| | M | 0.14 | 0.14 | 0.00 | 0.00 | 1.00 |
| Llc | U | 0.92 | 0.83 | 26.50 | 12.57 | 0.00 |
| | M | 0.92 | 0.92 | 0.00 | 0.00 | 1.00 |
| w | U | 0.65 | 0.44 | 43.40 | 22.72 | 0.00 |
| | M | 0.65 | 0.65 | 0.00 | 0.00 | 1.00 |

*Notes: Sector2: Trade transport accommodation and food service activities; Sector3: Scientific, design, professional and technical activities; Sector4: Other services. Reference category: R&D activity, industry, construction. Ingroup=1 if the firm belongs to a group and 0 otherwise; Llc=1 for limited company and 0 otherwise. w=1 if the firm was born after the Law 221/2012, i.e., in 2013 and 0 otherwise. The standardized % bias is the percentage difference of the sample means in the treated and non-treated (full or matched) sub-samples as a percentage of the square root of the average of the sample variances in the treated and non-treated groups (Rosenbaum and Rubin, 1985).*

### *3.2 Descriptive statistics*

Table 2 reports some descriptive statistics of our outcome variables. The table is divided to show the pre-treatment period, namely 2012, and the post treatment period 2014. By construction, the sub-sample of non-treated firms has, on average, a higher value of the share of intangibles in 2012.[8] Interestingly, in the post-treatment period non-treated firms experienced a 7% decrease in the share of intangible. In contrast, the treated firms recorded a slight increase of 0.6%. A similar dynamic is observed in terms of the level of fixed assets, -2.5 vs. +2.7% for non-treated and treated firms, respectively, while the opposite applies in terms of number of employees, +6.70% vs -9.9%. As far as turnover is concerned, treated firms start out with a lower average value but experienced a greater increase in the subsequent two years (23.51% against 4.7% for the non-treated firms). Finally, as shown in the last line of Table 2, treated firms had on average a higher number of partners. This variable partly measures the propensity of firms to attract private investors. Moreover, given that most of the firms are SME and that SME's partners are primarily workers, it may also capture part of both physical and human capital investments in the firms. Unfortunately, this variable is available only for 2014.

Table 2. Descriptive statistics

| 2012 |
|---|

[8] We recall that in the sample of eligible firms we kept only non-treated firms having a share of intangible assets greater than the first quartile of the same variable of treated firms; see the appendix.

| Variable | Non-Treated | | Treated | |
|---|---|---|---|---|
| | Mean | Std. Dev. | Mean | Std. Dev. |
| Sh. intangible | 0.763 | 0.260 | 0.671 | 0.322 |
| Intan. assets | 39.32 | 78.98 | 60.43 | 99.30 |
| Fixed assets | 9.713 | 19.85 | 13.60 | 23.33 |
| Turnover | 259.3 | 391.4 | 125.5 | 255.6 |
| Employees | 2.045 | 2.738 | 1.600 | 2.120 |
| | | 2014 | | |
| Sh. intangible | 0.709 | 0.301 | 0.675 | 0.343 |
| Intan. assets | 32.97 | 70.60 | 71.76 | 99.93 |
| Fixed assets | 8.496 | 16.95 | 13.97 | 21.55 |
| Turnover | 271.4 | 400.8 | 155.0 | 298.3 |
| Employees | 2.182 | 2.744 | 1.441 | 2.199 |
| Partners | 2.404 | 1.205 | 3.600 | 1.689 |

*Note: turnover is measured in thousands of Euro.*

Table 3. Sectoral and geographic breakdown

| Variable | Non-Treated | Treated |
|---|---|---|
| | Mean | Mean |
| R&D, ind., constr. | 0.331 | 0.341 |
| Trade, transport, accomm, food serv | 0.038 | 0.011 |
| Scientific, design, prof. activity | 0.464 | 0.529 |
| North-West | 0.338 | 0.329 |
| North-East | 0.244 | 0.273 |
| Center | 0.248 | 0.211 |
| Llc | 0.884 | 0.905 |
| Ingroup | 0.204 | 0.190 |

Table 3 shows the differences between treated and non-treated firms in terms of their sector of activity, location, legal form, and group membership. Given that we used the pre-processing technique, those differences are, not surprisingly, very small.

## 4. Identification strategy

To identify and estimate the causal effects of the policy on the outcomes of interest we exploit the fact that only firms that were established a maximum of 48 months before the date of promulgation of the Decree Law, namely after the 19th October 2008, were eligible, while firms born before that date were not eligible. This provides us with a natural experiment. The idea is that at the cutoff eligibility is as good as random. As our running variable is the firm's age and the cutoff is four years before the policy was implemented, it is not possible for firms to manipulate their running variable to become eligible. However, from a theoretical point of view, eligibility might still not be random around the cutoff if the Italian government targeted some type of firms in choosing the 48-months cutoff. Nevertheless, this appears to be very unlikely. Even if the Italian government had a target in mind when designing the policy, eligibility depends not only on firms' age but also on several other criteria, among which age is probably the least functional for targeting firms. As we will discuss more formally later in this section, the fact that eligibility is randomly assigned is not enough to identify the causal effect of the policy in our data. We will need to further assume that eligibility *per se* does not have a direct effect on the outcome measures. In our setting this will amount to assuming that being marginally older or younger than forty-eight months does not have an impact on our outcomes. This assumption seems very reasonable.

We will now formalize our identification strategy, which is a fuzzy RDD. We will start by introducing some notation. Let us denote as $D$ the binary treatment variable; i.e., being registered as an innovative start-up at the end of 2012 (from the 19th of October), and/or in 2013 and/or in 2014. We denote the outcome by $Y$ and the two potential outcomes (e.g., Rubin, 1974) firm $i$ would obtain if treated and if non-treated by $Y_i^1$ and $Y_i^0$, respectively. Under the Stable Unit Treatment Value Assumption (SUTVA) for each firm we observe only one of the potential outcomes depending on the treatment status according to the following observational rule:

$$Y_i = Y_i^1 D_i + (1 - D_i) Y_i^0$$

Let us define by $S$ the running variable firms' age which is in our case measured in days and centered at the cutoff, and by $\bar{s}$ the cutoff which is the 19th of October 2008 normalized at zero in our application. We define an indicator, $Z$, which takes value 1 if $S \geq \bar{s}$, and $D_i^1$ and $D_i^0$ as the potential treatment status of firm $i$ if eligible and non-eligible, respectively. As it is not possible for firms to register as an innovate start-up if non-eligible, $D_i^0 = 0$ while $D_i^1$ can be different from 1, as many eligible firms did not register as innovative start-ups. As in Imbens and Angrist (1994), we can then define two type of firms, denoted by $T$, depending on the values that $D_i^1$ and $D_i^0$ take. The *never takers*, denoted by $n$, are firms that never register as innovative start-up regardless of whether they are eligible or not. The *compliers*, denoted by $c$, are firms that register as innovative start-ups if eligible, and they correspond to the treated firms in our setting. Notice that, in contrast from Imbens and Angrist (1994), neither *always taker* nor *defier* firms can exist, as it is not possible to register if not eligible.

The first assumption that we impose is that at the cutoff the probability of belonging to a given type does not depend on firms' eligibility status. Formally,

**Assumption 1**

$S$ is continuous in a neighborhood of $\bar{s}$,

$$\lim_{s \uparrow \bar{s}} Pr(T = t|S = s) = \lim_{s \downarrow \bar{s}} Pr(T = t|S = s) = \Pr(T = t), t = n, c.$$

This assumption is satisfied as soon as we believe that eligibility is random at the cutoff and that the running variable can be treated as continuous around the cutoff. This assumption ensures that the share of compliers in the population does not vary with respect to the treatment status.

Secondly, we will assume that being eligible does not have a direct effect on the mean potential outcomes, at least at the cutoff, and that the potential outcomes are mean independent of the running variable if we condition on firms' types.

**Assumption 2**

$$\lim_{s \uparrow \bar{s}} E(Y^{d,s}|T = t, S = s) = \lim_{s \downarrow \bar{s}} E(Y^{d,s}|T = t, S = s) = E(Y^{d}|T = t, S = \bar{s}) \,, t = n, c.$$

Under Assumptions 1 and 2 it is easy to show that we can identify the Average Treatment Effect on the Treated (ATT) at the cutoff, which we define as [9]

$$ATT_{\bar{s}} = E(Y_i^1 - Y_i^0|D = 1, S = \bar{s})$$

To see this, note that above the cutoff all the treated firms are compliers and all the non-treated are never takers. Below the cutoff every firm is non-treated and could either be a complier or a never taker. Using a very similar derivation[10] to the one in Imbens and Angrist (1994), it is very easy to show that under Assumptions 1 and 2

$$ATT_{\bar{s}} = \frac{\lim_{s \downarrow \bar{s}} E(Y_i|S = s) - \lim_{s \uparrow \bar{s}} E(Y_i|S = s)}{\lim_{s \downarrow \bar{s}} E(D|S = s) - \lim_{s \uparrow \bar{s}} E(D|S = s)}$$

Arguably, the easiest and probably the most used widely estimation method consists in choosing a bandwidth, denoted by $bw$, and running a simple two stage least squares (TSLS) regression in the sample of units that have a value of $S$ in the interval $[\bar{s} - bw, \bar{s} + bw]$ using $Z$ as an instrument. This is equivalent to a non-parametric local linear IV regression with uniform kernel.

Recent developments in the literature focus on correct inference, data-driven optimal bandwidth selection and how to include covariates to improve precision (see, e.g., Calonico et al., 2014a, 2014b, 2018a, 2017, 2018b). One of the most important recent advances, which we will adapt to our setting, concerns deriving bias-corrected confidence intervals (CIs) that are robust to "large" bandwidth choices. The bandwidth is chosen according to some optimality criterion based on either the

[9] Note that Assumption 1 and 2 are weaker than assuming that eligibility does not directly affect the potential outcomes and it is randomly assigned at the cutoff.
[10] As this result is well known in the literature, we do not report a formal proof which is available from the authors upon request.

minimization of the Mean Squared Error (MSE) or of the Coverage Error-Rate (CER). We will report our results using both approaches. The MSE optimal bandwidth is quite popular in empirical works since the seminal contribution of Imbens and Kalyanaraman (2012). However, this method does not necessarily give an optimal bandwidth when the goal is inference. Indeed, Calonico et al. (2018b) showed that a different, smaller bandwidth must be used when the goal is constructing optimal CIs. They also provide a new CER bandwidth selection procedure that leads to robust bias corrected CIs. For this reason, our benchmark will be to use the CER optimal selection criterion.

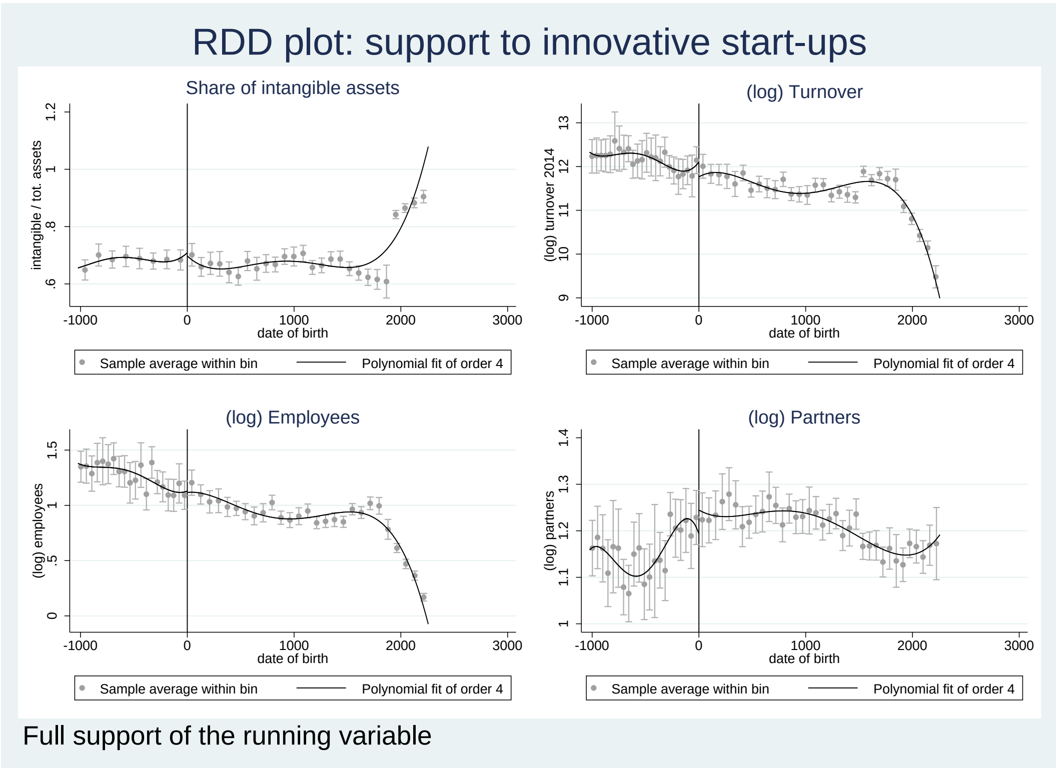
In general, it is often important to allow for an asymmetric bandwidth around the cutoff, as we are approximating two conditional expectations. In our specific case, due to the one-sided non-compliance problem that we are facing, it is crucial to allow for different bandwidths above and below the cutoff. Indeed, in our setting we have the feature of a sharp RDD on the left-hand side of the cutoff and that of a fuzzy RDD on the right-hand side. Thus, applying the bias-corrected estimators for fuzzy designs, as originally proposed in Calonico et al. (2014a), is not appropriate. In an analogous one-sided non-compliance setting, Battistin and Rettore (2008) use a standard RDD estimator showing results for different arbitrarily chosen bandwidths following the common practice back then. We provide a small methodological contribution by adapting the asymmetric CER optimal bandwidth to our one-sided non-compliance problem.

## 5. Results

We start by presenting the standard graphical exploratory analysis, which is essential in an RDD setting (see, e.g., Imbens and Lemieux, 2008; Lee and Lemieux, 2010). The idea is to plot polynomial approximation of the conditional expectation of the outcomes with respect to the running variable. This is done by dividing the support of the running variable into bins and computing local means of the outcome within those bins. The goal of this exercise is twofold. First, these types of plots can detect discontinuities, which must take place only at the cutoff; thus, they provide important information regarding the validity of the key identifying assumption of the RDD (i.e., the continuity of the conditional expectations at the cutoff). Any discontinuity in the conditional expectations away from the cutoff would cast doubts on the validity of the design. A discontinuity at the cutoff, on the other end, gives us an idea about the presence of a treatment effect. Second, these types of plots provide a tidy representation of the overall variability in the data. To avoid arbitrarily choosing the number of bins, Calonico et al. (2015) propose a data-driven procedure to select it, specifically tailored to the two aforementioned goals: detection of discontinuities and representation of variability. As far as the former goal is concerned, the optimal number of bins is obtained by minimizing the integrated mean square error (IMSE). With respect to the latter, Calonico et al. (2015) have developed a bin selector that employs more bins than the optimal number selected by the IMSE-minimization strategy.

Figure 1 reports the RDD plot for four outcomes of interest: the share of intangible assets, (the log of) turnover, (the log of) employment and (the log of) partners. The pictures are quite informative. On the one hand, they do not provide any evidence of potential discontinuities away from the cutoff in the underlying regression functions, thus acting as falsification tests and validating the key assumption of the design. On the other hand, the pictures show no jump in the conditional expectations of the outcome variables at the cutoff, for all the outcome variables, except for partners that present a visible positive jump.

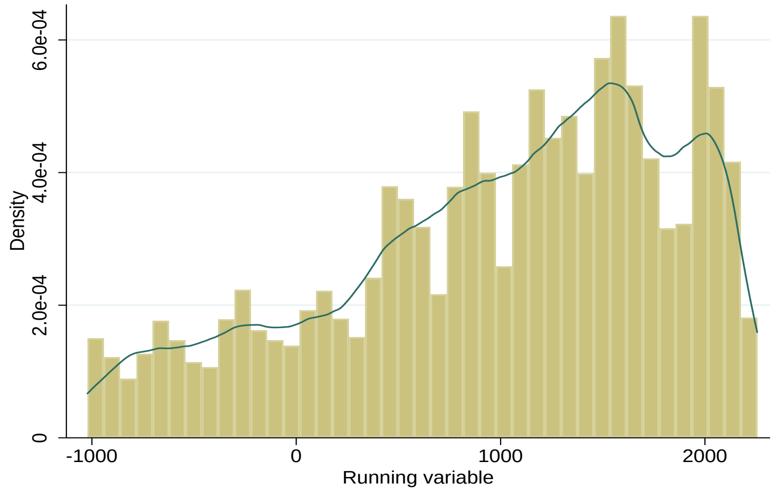
Figure 1. RDD Plot with evenly spaced number of bins using spacings estimators and CIs




*Notes: The figure plots smooth polynomial approximations of the conditional expectations of the outcome variable. The optimal number of bins is determined according to the evenly spaced optimal IMSE criterion. Vertical bars represent the CIs for each bin.*

Another simple graphical inspection for checking the continuity of the running variable is reported in Figure 2. Here we plot a kernel density estimate of the density of the running variable, which shows no holes in the entire support, once again supporting our assumption of continuity of the running variable.

Figure 2. Running variable kernel density plot




*Notes: The figure reports the density plot of the running variable centered at the cutoff. The Epanechnikov distribution has been used as a kernel.*

Table 4 reports the discontinuity in the probability of treatment (i.e. the first stage) which is around 5-7%. It is obtained by regressing the treatment variable D on Z in the sample obtained after selecting firms with running values within the range defined by the chosen bandwidths. In principle, the first stage should not be affected by the outcome variable used to select the two asymmetric bandwidths. However, the optimal bandwidths are different for the four outcomes we consider because of different missing values in the four outcomes. Thus, the estimated coefficients are slightly different.

Table 4. RDD – First Stage (CER optimal BW)

| | Share intangibles | Turnover | Employees | Partners |
|---|---|---|---|---|
| eligibility dummy (Z) | 0.054*** | 0.064*** | 0.071*** | 0.075*** |
| | (0.009) | (0.008) | (0.009) | (0.007) |
| Observations | 1,121 | 1,076 | 1,386 | 1,924 |
| Bandwidth | [-242.5; 250.8] | [-115; 335.2] | [-236.2; 300.1] | [-164.1; 489.7] |

*Notes: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. All the dependent variables are in logs except for the share of intangibles. The table reports the probability discontinuity (i.e. the regression of D on Z).*

Table 4 shows that only around 7% of eligible firms were treated. One possible explanation for the low take up rate could be that not all the benefits were active at the onset of the policy (see Section 2).

Before presenting our main specification results, we report in Table 5 the results of the RDD without covariates. All the coefficients are not statistically significant. It is interesting to notice that the sign of the estimated coefficients is only positive for the number of partners.

Table 5. RDD results without covariates – linear regression (CER optimal BW)

| | Share intangibles | Turnover | Employees | Partners |
|---|---|---|---|---|
| Innovative start-up dummy | -0.368 | -2.606 | -0.583 | 0.321 |
| | (0.313) | (1.949) | (0.613) | (0.265) |
| Observations | 1,121 | 1,076 | 1,386 | 1,924 |
| Bandwidth | [-242.5; 250.8] | [-115; 335.2] | [-236.2; 300.1] | [-164.1; 489.7] |

*Notes: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1, asymmetric CER optimal bandwidth selectors, below and above the cutoff. All the dependent variables are in logs except for the share of intangible assets.*

Following Calonico et al. (2017 and 2018b), we try to improve precision by including pre-treatment covariates. Before showing the results, we report the balancing test results in Table 6, which do not give any concern except for the variable *ingroup*, for which we reject the null of no jump at the cutoff at the 10% significance level. Although this is not strong evidence against balancing, we have taken a conservative stance by not including it in the conditioning set.[11]

Table 6. Balancing test at the cutoff.

| Variable | Effect | Robust p-val |
|---|---|---|
| sector1 | -.282 | .495 |
| sector3 | -.175 | .819 |
| sector4 | .355 | .207 |
| North-West | -.153 | .689 |
| North-East | .859 | .059 |
| Centre | -.329 | .368 |
| Llc | -.352 | .196 |
| Ingroup | -.655 | .077 |

*Notes: sector1: R&D activity, industry, construction; sector3: scientific, design professional and technical activities; Sector4: other services. Llc=1 for limited companies and 0 otherwise, Ingroup=1 if the firm belongs to a group and 0 otherwise. The table reports the balancing test according to the asymmetric CER optimal BW selectors, below and above the cutoff.*

Including covariates does not always improve precision. For example, if the covariates are irrelevant, they can even increase the length of the CIs. Thus, we have measured if and to what extent the inclusion of covariates shrinks the CI of the estimated treatment variable by working out the

[11] For the sake of robustness, the balancing test at the cutoff has been repeated using the optimal MSE bandwidths selection with identical results.

percentage change in the CI under the two situations, with and without covariates. We will refer to this measure as the "precision gain". Whenever it takes negative values it indicates a smaller CI under covariates inclusion. The results are shown in Table 7, which indicates substantial precision gains for all the outcome variables we consider.

Table 7. Precision gain

| Variables | Precision gain |
|---|---|
| *Share intang.* | -3.63 |
| *Turnover* | -4.46 |
| *Employees* | -2.27 |
| *Partners* | -11.1 |

*Notes: The table reports the percentage chance in the treatment variable CI when covariates are included in the RD estimation. The precision gain is worked out as:* $\left( {CI_w}\big/{CI_{w/o}} - 1\right)100$*, where:* $CI_w$ *is the CI of the treatment variable when covariates are included in the RD estimate and* $CI_{w/o}$ *is the CI of the treatment variable when covariates are not included. Negative values indicate a precision gain.*

Table 8 reports our main specification results.

Table 8. RDD with covariates - linear regression (CER optimal BW)

| | Share intang. | Turnover | Employees | Partners |
|---|---|---|---|---|
| Innovative start-up dummy | -0.403 | -0.944 | -0.236 | 0.450** |
| | (0.361) | (1.660) | (0.653) | (0.184) |
| Observations | 913 | 1,095 | 1,266 | 2,681 |
| Bandwidth | [-147.5; 252.3] | [-156.4; 304.5] | [-181.3; 312.9] | [-257.5; 591.6] |

*Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1, asymmetric CER optimal BW selectors, below and above the cutoff. Other controls: dummies for sector, area, legal status. All the dependent variables are in logs except for the share of intangible assets.*

The inclusion of the covariates does not change the magnitude of the effect compared to Table 5. However, the effect of the policy on the number of partners is now significant at the 5% confidence level. This is quite an interesting finding and it is fully consistent with the fact that there was a sizeable tax incentive for private investors in the new innovative start-ups (see benefit 11 in Section 2). Indeed, the benefit allows a deduction on personal income tax amounting to 19% or 20% of the amount invested, depending on whether the investment is made by individuals or legal entities, respectively.[12] The tax deduction applies to both direct and indirect investments. According to the Ministry of Economic Development's 2016 Annual Report, in 2014 our treated firms received incentivized

[12] Since 2017 the benefit has been increased to 30%.

investments by individuals and companies for a total of approximately 42 million of euros.[13] This shows that this benefit, although active only since January 2014, was immediately used by innovative start-ups. Most of the firms in our data are micro firms (see Table 2). Often in micro firms partners are workers, too. It follows that an increase in the number of partners by on average 1.57 heads per firm may represent a sizeable increase in the number of workers.[14] Most intuitively, the large positive effect that we find represents strong evidence that the policy has been successful at least in attracting private investors to highly innovative firms.

## 6. Robustness checks

Table 9 shows the results when MSE is used instead of CER to select the bandwidth.

Table 9. RDD - linear regression (MSE optimal BW)

| | Share intan. | Turnover | employees | Partners |
|---|---|---|---|---|
| Innovative start-up dummy | -0.203 | -3.032** | -1.569*** | 0.349* |
| | (0.182) | (1.327) | (0.443) | (0.182) |
| Observations | 1,886 | 2,097 | 2,577 | 3,541 |
| Bandwidth | [-389.4; 402.8] | [-185; 538.9] | [-381.9; 485.2] | [-264.1; 788.3] |

*Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; asymmetric MSE optimal BW selectors, below and above the cutoff. All the dependent variables are in logs except for the share of intangible assets.*

The optimal MSE criterion selects bandwidths 60% greater than the ones based on CER, with a remarkable increase in the number of observations ranging from 70% (share of intangibles) to 94% (turnover). We still find a positive and significant effect on the number of partners with a magnitude comparable to the one found in our main specification. We still find no impact on the share of intangible assets and a very small but negative effect on turnover, amounting to around 20 euros on average. Although the sign of the effect might seem puzzling, one must recall that among the eligibility requirements firms were asked not to distribute profits. This may act as a sort of disincentive to make profits, and hence to increase turnover. Furthermore, young innovative firms are more prone to invest, especially at the beginning of their entrepreneurial activity. This might also translate into a lower turnover, at least in the short run. The negative effect on the number of

[13] https://www.sviluppoeconomico.gov.it/images/stories/documenti/italian_startup_act_annual_report_to_parliament_2016.pdf , see Table 4.4.e.
[14] Exp(0.45)=1.57.

employees could be a consequence of the increase in the number of partners, who, as mentioned before, are very likely to be workers themselves.

As a further robustness check we have carried out falsification tests. The idea underlying these tests is to check whether a discontinuity is detectable in the outcome variables before the treatment occurred. Such a discontinuity can be interpreted as selection bias. So, we have rerun the RDD on the outcome variables that we are able to measure in 2012. The results are reported in Table 10 and show no evidence that there is selection bias. Adding covariates and using optimal MSE bandwidths gives results that are perfectly in line with the ones in Table 10.[15]

Table 10. RDD – Falsification tests, RDD estimates on outcome variables in 2012

| | Share intang. | Turnover | Employees |
|---|---|---|---|
| Innovative start-up dummy | -0.346 | -1.864 | 0.858 |
| | (0.306) | (2.110) | (0.918) |
| Observations | 1,171 | 886 | 1,108 |
| Bandwidth | [-245.8; 248.9] | [-136.5; 224.1] | [-178.9; 266.9] |

*Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; asymmetric CER optimal BW selectors, below and above the cutoff. All the dependent variables are in logs except for the share of intangible assets.*

Finally, we have checked for possible manipulation of the running variable. As we discussed in Section 4, firms have very little room for manipulation. Indeed, manipulation would occur if the firms were able to tamper with the Business Register's archive, which is a fairly unlikely event, to say the least. In Section 4 we also hinted at the very unlikely possibility that the government chose the cutoff to favor a group of firms. To rule out also this possibility we have carried out a McCrary (2008) test. The idea behind this test is that in the absence of systematic manipulation of the running variable the density of firms should be continuous near the cutoff. In our empirical application we have implemented a refined version of the test, computing automatic bandwidth selection and robust bias-corrected estimates of the test statistics to obtain valid statistical inference, as proposed by Cattaneo et al. (2018). Table 11 reports the t-test and the associated p-value.

Table 11. Manipulation test

| t | P-val |
|---|---|
| 1.402 | .161 |

*Notes: The table reports the t-statistic and the associated p-value of the McCrary (2008) test for manipulation, with robust bias-corrected estimation of the t as in Cattaneo et al. (2018). The bandwidth is selected as a combination of the MSE of difference and sum of densities criteria.*

[15] These results are available from the authors upon request.

*Uniform kernel function and jackknife standard error have been used.*

According to the evidence reported in Table 11, we are not able to reject the null of no manipulation.

## 7. External validity

One of the main drawbacks of RDD is that the results are valid only locally (i.e. at the cutoff) and are not necessarily informative for firms that are away from the cutoff. To increase confidence in the external validity of our results we follow the approach of Battistin and Rettore (2008). They provide a specification test for external validity of RDD estimates away from the cutoff, assuming that a Conditional Independence Assumption (CIA) holds beyond the cutoff. The testable implications are derived under a CIA, that is, they assume that

$$Y^0 \perp D|S,X,\ Var(D=1|S,X)>0,\ S\geq \bar{s}$$

Let us define the selection bias, *sb*, for treated and non-treated firms with $X=x$ and $S=s$ as

$$sb(s,x)=E[Y^0|D=1,s,x]-E[Y^0|D=0,s,x].$$

If the CIA assumption holds then $sb(\bar{s}^+,x)$ must be equal to zero. Since $sb(s,x)$ is identified in a right-neighborhood of $\bar{s}$, one can test

$$\mathrm{H_0}: E[Y(0)|\ s<\bar{s},D=0,X]=E[Y(0)|s\geq \bar{s},D=0,X]$$

As there exist only non-eligible firms on the left-hand side of the cutoff, the test compares the mean outcomes of non-treated firms at the cutoff. In practice, this amounts to estimating the following regression using only non-treated firms within the bandwidth:

$$Y_i=\alpha+\gamma \mathrm{s}_i+\lambda Z_i+\beta' X_i+\varepsilon_i$$

and testing that $\lambda=0$.

Table 12. Battistin and Rettore's test for external validity

| Trea | Share intang. | Turnover | Employees | Partners |
|---|---|---|---|---|
| Eligibility dummy (Z) | 0.005 | -0.133 | 0.104 | 0.006 |
| | (0.036) | (0.195) | (0.086) | (0.023) |
| Observations | 879 | 1,044 | 1,205 | 2,522 |
| Bandwidth | [-147.5; 252.3] | [-156.4; 304.5] | [-181.3; 312.9] | [-257.5; 591.6] |

*Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; asymmetric CER optimal BW selectors, below and above the cutoff. Other controls: Dummies for sector, area, legal status. All the dependent variables are in logs except for the share of intangible assets.*

Table 12 reports the results of the test for each outcome variable. Not only are we never able to reject the null, but also the estimated selection bias is always very small, irrespective of the outcome we use. This increases our confidence in the external validity of our results. Identical results are found when an optimal MSE bandwidth is used and when one includes the pre-treatment values of the outcome variables as covariates.

To further asses the external validity of our results we use the new approach proposed by Dong and Lewbel (2015). They show that the derivative of the treatment effect with respect to the running variable at the cutoff (TED), has implications for extrapolating the estimated Local Average Treatment Effect (LATE) away from the cutoff. Indeed, a large TED in a neighborhood of the cutoff should cast serious doubts on the external validity of the estimated LATE. Differently, if the identified TED is locally zero, the estimated LATE is more likely to have higher external validity. The sign of TED is also informative. Table 13 reports the estimated TEDs for various specifications.

Table 13. TED

| Outcome variable | With covariates | | Without covariates | |
|---|---|---|---|---|
| | TED | P-val | TED | P-val |
| Share intang. | -.229 | 0.921 | -.485 | 0.934 |
| Turnover | 1.842 | 0.806 | 1.368 | 0.790 |
| Employees | -.092 | 0.620 | -.362 | 0.762 |
| Partners | -.03 | 0.918 | -.038 | 0.786 |

*Note: The table reports the estimated TED and the associated robust P-value for each outcome; *** p<0.01, ** p<0.05, * p<0.1, asymmetric CER optimal BW selectors, below and above the cutoff. Other controls: Dummies for sector, area, legal status. All the dependent variables are in logs except for the share of intangible assets.*

The estimated TED is never significant in any specification and always very close to zero indicating that our results might have higher external validity.

## 8. The costs of attracting private investors

In this section we provide a back of the envelope calculation to quantify the cost for the Italian government in terms of lost revenues generated by the fiscal benefit for private investors.

First, we summarize all the relevant pieces of information:

a) Our estimates suggest exp(0.45)=1.57 partners per treated firm induced by the policy (Table 8).
b) We observe 3,115 treated firms, see Section 3.1.
c) This implies that the policy generated 1.57*3,115 = 4,891 new partners.

d) We observe 3.6 partners per treated firms on average (Table 2).

e) Thus, we have 3.6*3,115 = 11,214 partners in treated firms.

f) A total of 42 million euro of private investments in innovative start-ups was reported in 2014 (Section 6 in the Annual Ministry of Econ. Develop. Report, 2016).

g) The deduction on personal income tax (IRPEF) was 19% or 20% of the amount invested, depending on whether the investment is made by individuals or legal entities, respectively.

Based on these figures we estimate a total private investment generated by the policy of 18,318,360 euros. This estimate is given by the average investment per partner (42 million / 11,214 = 3,745.32) times the total number of partners created by the policy (item c above).

We first calculate an upper bound of the cost of the policy by taking into account the partners that would have invested even in the absence of the fiscal incentives. Indeed, by subtracting from the total number of partners in innovative start-ups (11,214) those created by the policy (4,891) we obtain that 6,323 partners were not created by the policy. In turn, this figure can be used to obtain an estimate of the total investment in the new type of firms not generated by the policy by multiplying it by the average investment per partner (i.e., 6,323*3,745.32 = 23,681,658 euros).

A tax deduction of α% means that only (100−α)% of any investment was taxable at the current personal income tax rate. The latter is progressive in Italy, and we will therefore estimate an upper bound by assuming that all the investments non generated by the policy would have been taxed at the highest tax rate. For completeness we report the tax income brackets in Table 13.

Table 13. Personal income tax rate in Italy in 2014

| Taxable income, euros | Tax rate |
|---|---|
| < 15,000 | 23% |
| 15,001 to 28,000 | 27% |
| 28,001 to 55,000 | 38% |
| 55,001 – 75,000 | 41% |
| > 75000 | 43% |

*Note: The table reports tax income brackets in force at 2014 in Italy.*

In the worst-case scenario the policy cost 23,681,658 *0.43*0.2=2,036,623 euros. Notice that this also assumes that all investments were made by legal entities which had the highest tax deduction (20%).

The estimated 18,318,360 euros of private investment generated by the policy gave rise to 18,318,360*0.8*0.43=6,301,515 euros of tax revenues. This implies that in the best-case scenario the policy generated 6,301,515−2,036,623=4,264,892 euros of tax revenues. In the worst-case scenario, private investors, both induced by the policy and not, could have invested in other activities taxed at the full tax rate. Thus, in this case the lost tax revenue for the Italian government can be estimated as being 42,000,000*.43*.2=3,612,000 euros. This simple calculation shows that the monetary costs of attracting new partners to the new firms has been either negative or arguably small. However, the opportunity cost of attracting the "wrong" type of investors, namely those interested only in the tax shield offered by the policy rather than in innovation, might still be substantial.

## 9. Conclusion

We provided an extensive evaluation of a policy implemented in Italy at the end of 2012 to favor young innovative start-ups. Our results suggest that the generous tax benefits for investing in this type of firms were able to attract a substantial number of new partners. Our results are in line with previous findings and robust to several falsification exercises.

Unfortunately, our findings also suggest that the increase in private investors did not translate into an increase in innovative activities as intended by the legislator. This casts serious doubts on the effectiveness of the policy overall as the new investors generated by the policy might just have used the tax benefit without investing in innovation. Our results indicate that a more effective policy would have been one that linked tax cuts to actual investments in innovation.

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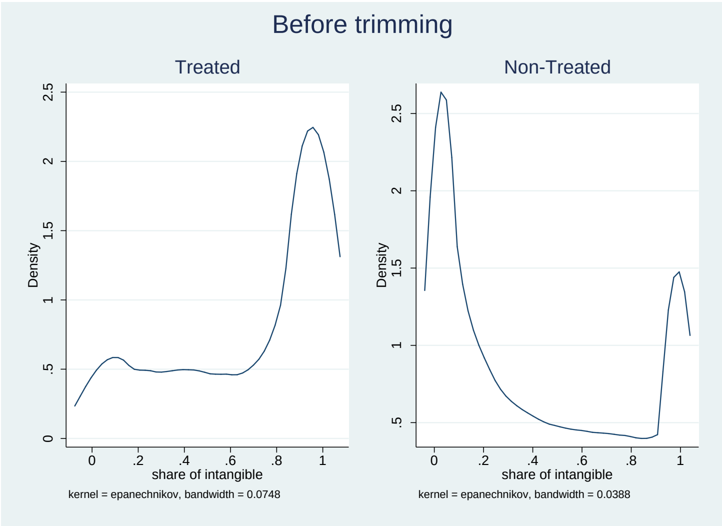
## Appendix

**Selecting non-treated firms based on their distribution of the share of intangible assets.**

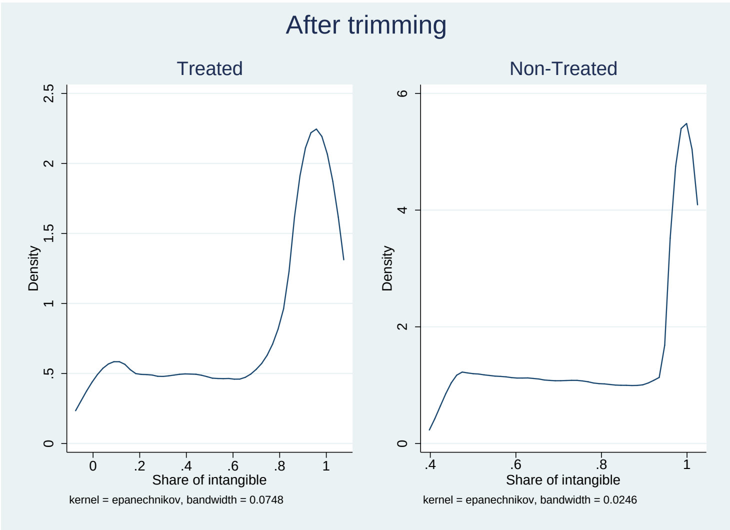
The upper panel of Figure 1A, Panel A, reports the empirical distribution of the share of intangible assets. The left-hand side of the panel reports the empirical distribution of the treated, while the right-hand side the one of the non-treated. Clearly there are relatively many treated firms experiencing high values of the indicator. Differently, non-treated firms show a bimodal distribution with relatively many firms having a low value of the indicator. Keeping only non-treated firms having a value of the share of intangible not less than the first quartile of the of the distribution of the treated firms, we obtain a new empirical distribution for the non-treated which looks very similar to the one the treated firms as shown in Panel B.

Figure 1A. Empirical distribution the share of intangible assets for the two sub-populations before and after the trimming.

A.




B.




*Notes: Panel A reports the empirical distribution of the share of intangible assets for the treated firms on the left-hand-side and for non-treated firms on the right-hand-side. Panel B reports the empirical distribution of the share of intangible assets for the treated firms on the left-hand-side and for non-treated firms after the trimming on the right-hand-side*