The Italian Startup Act: A microeconometric program evaluation

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Abstract

This paper analyses the impact of the Italian Startup Act which entered into force in December 2012. This public policy provides benefits, such as tax incentives, public loan guarantees, and a more flexible labor law, for firms registered as "innovative startups" in Italy. This legislation has been implemented by the Italian government to increase innovativeness of small and young enterprises by facilitating improved access to (external) capital and (high-skilled) labor. Consequently, the goal of our evaluation is to assess the impact of the policy on equity, debt and employment. Using various conditional difference-in-difference models, we find that the Italian startup policy has met its primary objectives. The econometric results strongly suggest that firms operating under this program are more successful in obtaining equity and debt capital and they also hire more employees because of the program participation.

Keywords: start up, innovation policy, firm subsidies, small firms

JEL-Codes: M13, O38

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1 Introduction

Small and young companies are often seen as the engine of innovation and growth but unfortunately these companies are also known to be the most financially constrained (Himmelberg & Peterson, 1994; Schneider & Veugelers, 2009). This argument is especially pertinent for newly founded, innovative firms (Carpenter & Petersen, 2002). Capital market imperfections in financing Research and Development (R&D) investments are usually put forward as a theoretical justification for public support to private R&D (Hall, 2002). There is a general tendency to consider R&D investments are riskier than other investments with negative consequences both for financing, as investors discount uncertainty, and for debt financing, since collateralization becomes problematic due to sunk costs and intangibles (Hall et al., 2016). Moreover, the problems of contract incompleteness and information asymmetry between firm and investors are exacerbated in the case of R&D financing (Hall & Lerner, 2010). As a consequence, innovative firms rely more on their own internal finance, when available. Market failures in innovation can be particularly severe in countries that lack well-functioning capital markets for innovative startups (Myers & Majluf, 1984).

Italy, especially in the aftermath of the 2008-2009 financial crisis followed by the economic recession and the sovereign debt crisis, can be considered as one of those countries where the functioning of the financial markets for innovative startups became highly debatable, at the very least. The recognition that the crisis might have hit innovative, small and young firms more severely than other companies called for policy actions, especially for disadvantaged but potentially highly important companies for technological advancement and economic growth (cf. OECD, 2009; OECD, 2014; and Bergner et al., 2017).

As a response to the crisis, Italy passed the Decree Law 179/2012 transformed into Law 221/2012, which can be seen as an active high-tech startup policy. This policy scheme is a composite measure made of a set of complementary interventions aimed at unleashing the growth potential of innovative young and small firms. Among other features, it combines investment tax benefits, public loan guarantees and a more flexible labor legislation as benefits for the program participants.

The purpose of this paper is to take a first look at the effects of this newly designed, and in the context of science and technology policy, innovative program to incent startup activity and to enhance the growth potential of innovative companies. We apply state-of-the-art econometric techniques to estimate treatment effects of the policy on relevant target performance measures at the firm-level. We mainly rely on difference-in-difference (DiD) regressions with adequate control group designs, but also address possible self-selection mechanisms and attrition.

The remainder of this paper is organized as follows: Section 2 introduces the data, Section 3 presents the empirical strategy, Section 4 shows results, and Section 5 concludes.

1.1 Conceptual Background

As already experimented across the world, industrial policies to be effective must target a specific population of firms. Targeted firms can be selected according to multiple criteria such as age, size, location, industry, R&D intensity, etc.

The Italian law 221/2012, referred as Startup Act, is a remarkable example of the recent evolution of targeted industrial and innovative policy. Similar initiatives to support high-tech startup have been recently introduced in other countries such as: India (Companies Act 2013), Latvia (2016), Austria (startup program 2017) and Belgium (2017).

The crucial role of young and small firms in job creation is widely supported (Davis et al., 1996 and Criscuolo et al., 2014). Empirical evidence generally confirms firm size and age to be negatively correlated with rates of job creation and firm growth (Birch, 1981; Buldyrev et al., 2020; Harhoff et al., 1998; Buldyrev et al., 2007; Headd & Kirchhoff, 2009; Haltiwanger et al., 2013). It has been found that firm births account for a significant share of net job creation since firms do not grow much after an initial high growth period (Armington & Odle, 1982; Kirchhoff & Phillips, 1988; Audretsch & Mahmood, 1994; Broersma & Gautier, 1997; Voulgaris et al., 2005; Lotti, 2007). More importantly, it is noteworthy that not *all* small firms grow faster than larger firms but only some *small and young* firms: the so-called "gazelles" (Delmar et al., 2003; Acs & Mueller, 2008).

The contribution to innovation of the small business sector is another argument brought up in the literature and policy debate to support small and medium enterprises (SMEs), even tough, as it is well known, there is no linear, monotonic relationship between firm size and innovativeness (see among others Acs & Audretsch, 1988; Symeonidis, 1996; Freel, 2005; Hausman, 2005; Lee & Sung, 2005; Laforet & Tann, 2006; Baregheh et al., 2016). More compelling is the argument that financial constraints are particularly pronounced for SMEs: retrieving information on SMEs is more expensive, their securities are less frequently traded, and their financial statements do not have to be audited. The lack of assets to pledge as collateral is another problem of startups, particularly innovative newly founded firms centered around R&D activities. Information asymmetries between insiders and external potential investors and stakeholders are magnified by the overlap of ownership and management in most of the young and small firms. The theory thus suggests asymmetric information to induce an adverse selection, about debt financing. Empirical evidence indeed confirms that the problems above cause an insufficient provision of capital to young, innovative and small firms (Audretsch & Lehmann, 2004; Freel, 2007; Stucki, 2013; Duarte et al. 2016; Bergner et al., 2017).

This is the main rationale of Law 221/2012 targeting the group of innovative, high-growth and young small and micro firms in Italy, since they are the ones experiencing the highest demand for capital and featuring specific characteristics complicating the acquisition of funds, especially during recessions (Gompers & Lerner, 2001; Audretsch & Lehmann, 2004; North et al., 2013).

This policy is meant to contribute to filling the gap between Italy and other OECD countries regarding high-tech startups and high skilled labor force. Italy is well-known to be the country with the most considerable fraction of micro (< 10 employees) and small firms (< 50 employees) among OECD countries. Also, small firms in Italy account for the most relevant share of total employment among OECD countries, well above 60% of total employment (Criscuolo et al., 2014). By taking a closer look at the age composition of small business, we notice that in Italy more than one-half of small companies are older than five years. Startups (i.e. firms aged less than 5 years) represent a minority of small businesses in OECD countries, but only Finland has a lower share of young firms than Italy (Criscuolo et al., 2014).[1]

Against this background, Law 221/2012 has been prompted by the Italian government to stimulate the new and young innovative startups employing high skilled personel thanks to targeted incentives to new enterpreneurial ventures.

[1] Also Japan's share of young companies is lower than in Italy, but Japanese data are only available at the establishment level, thus no direct comparison is possible.

1.2 The Italian policy for innovative startups: rational and potential impact

The primary goal of the Italian Law 221/2012 is "[...] to create favorable conditions for the establishment and the development of innovative enterprises to contribute significantly to economic growth and employment, especially youth employment." (Italian Ministry of Economic Development, 2014). The Law 221/2012 includes a set of support measures as listed in the “Restart, Italia!” report by the Minister of Economic Development.

The eligible enterprises are small newly incorporated companies headquartered in Italy, which have been operational for less than 5 years and with a yearly turnover lower than 5 million Euros. According to the Law, innovative startups must develop and commercialize innovative products or services of high technological value, and they should fulfill at least one of the following criteria as reported in MiSE (2016)[2]:

- at least 15% of the company’s expenses can be attributed to (R&D) activities;
- at least 1/3 of the employees are PhD students, the holders of a PhD or researchers; alternatively, 2/3 of the total workforce must hold a Master’s degree;
- the enterprise is the holder, depositary or licensee of a registered patent or software (intellectual property).

As only a small group of young and upcoming enterprises accounts for the bulk of net job creation, Law 221/2012 targets incentives more specifically to those firms.

In a nutshell, Law 221/2012 is meant to unleash financial constraints to help high growth potential firms to create new jobs in young innovative enterprises. Indeed, firms that meet all the

[2] Other minor requirements are: limited company, headquarter in Italy or headquarter in the EU with at least an operational branch in Italy, no listed in stock market, do not distribute profits, not be created by a merger or split-up.

criteria set by Law 221/2012 can register free of charge at a special register of “innovative startups” and are entitled to the benefits of the new legislative framework. This aspect of the policy is particularly important to evaluate the impact of the new legislation, since it rules out any risk of contamination of the treated group of firms: only registered firms get access to the benefits of the policy, with no exception. The main benefits for innovative startups can be divided into three categories: (a) tax incentives for equity investments; (b) a simplified procedure to get credit guarantees on bank loans; and (c) tailored made labor rules to subscribe fixed-term contracts which last up to four years. Investors in innovative startups get a 30% tax credit as individuals and fiscal deduction as legal entities (as of 2016). As for credit guarantees, it covers up to 80% of the bank loans and up to a maximum of 2.5 m EUR, and it is provided through a Government Fund called “Fondo Centrale di Garanzia.” When firms are no more eligible for the benefits of the policy, they exit the “innovative startup” register, and special treatments immediately stop. A report is published every year by the Italian Ministry of Industry, providing an in-depth analysis of the evolution of the policy, its impact and cost (see for instance MiSE, 2015).

Since the main interventions are on equity investments, access to bank loans and employment, we will focus on whether this new policy has spurred equity collection, bank loans and creation of new jobs by startup firms, conditional upon survival.

2 Data and descriptive statistics

To evaluate the impacts of the program, we merge the participant data as published by the Ministry of Economic Development for the years 2013 to 2015 with firm-level (accounting) data from the AIDA database of Bureau van Dijk for the years 2008 to 2015.

As the policy program is focused on startup companies, we restrict our sample to firms with similar characteristics of the treated ones. Namely, we analyze companies which were at most 5 years old in 2013 or later[3], and with revenues below 5 million EUR in at least one observed year.

Moreover, we omit firms from highly regulated industries or industries with a high share of publicly owned firms, such as agriculture (NACE rev. 2 A industries), quarrying and mining (NACE rev. 2 B industries), utilities and waste management industries (NACE rev. 2 D and E industries), as well as financial, bank, real estate, insurance industries[4]. Furthermore, we apply some outlier cleaning to the data to avoid that spurious results are due to potentially erroneous entries in the AIDA database. Accordingly, we remove from our sample all small firms with equity greater than 100,000 EUR, bank debts more than 500,000 EUR.

Our initial sample consists of 89,834 Italian young, small enterprises including 1,569 program participants. As we observe firms for multiple years, the resulting unbalanced panel contains 338,289 firm-year observations.

Table 1 – Variable description

VARIABLES	**Mean**	**Min**	**Max**
Equity (EUR, thousands)	17.343	0	100
Bank debts (EUR, thousands)	25.774	0	500
Employment	2.717	1	241
Patent dummy	0.006	0	1
R&D dummy	0.024	0	1
Intangible dummy	0.687	0	1
Age	2.382	0	7
Survival	0.976	0	1

[3] This fact implies that in the sample some firms are at most 7 years old.

[4] It is worth noticing that only 1% of program participants are active in these sectors, according to a report of Italian Chambers of Commerce, 4th quarter 2015.

In our sample the majority (almost of the 50%) of treated firms belong to only 2 sectors: “Computer programming, consultancy and related activities” and “Scientific R&D” (see Table 2). Conversely, these two industries combined account for less than 4% of the untreated companies. Table 2 shows the distribution of firms across industries in some more detail.

By looking at the geographic composition of our sample (Table 3), we notice that about one-fourth of the innovative startups are located in the two largest urban areas of Milan and Rome. Treated companies are mostly located in the northern part of the country.

Table 2 – The main industries in which treated firms are active

NACE 2	Untreated		Treated	
	Frequency	**Percent**	**Frequency**	**Percent**
62-Computer programming, consultancy and related activities	2,808	3.18	510	32.50
72-Scientific research and development	408	0.46	260	16.57
63-Information service activities	1,960	2.22	122	7.78
71-Architectural and engineering activities; technical testing and analysis	2,056	2.33	75	4.78
26- Manufacture of computer, electronic and optical products	338	0.38	68	4.33
74-Other professional, scientific and technical activities	2,780	3.15	65	4.14
28- Manufacture of machinery and equipment n.e.c.	815	0.92	63	4.02
70-Activities of head offices; management consultancy activities	4,483	5.08	52	3.31
27-Manufacture of electrical equipment	490	0.56	33	2.10
73-Advertising and market research	1,456	1.65	30	1.91
Other industries	70671	80.07	291	18.59

As preliminary evidence, Table 4 shows an increase in all the main variables of interest after treatment. The growth is particularly pronounced for debts and the number of employees that more than doubled after treatment.

Table 3 - The location of treated firms, top 10 NUT-3 regions

NUTS-3 Italian regions	Untreated		Treated	
	Frequency	**Percent**	**Frequency**	**Percent**
Milano (North Central Italy)	9,000	10.20	264	16.83
Roma (North Central Italy)	14,238	16.13	153	9.75
Torino (North Central Italy)	2,602	2.95	105	6.69
Napoli (Southern Italy)	5,175	5.86	58	3.70
Bologna (North Central Italy)	1,524	1.73	53	3.38
Trento (North Central Italy)	591	0.67	51	3.25
Firenze (North Central Italy)	1,530	1.73	40	2.55
Bari (Southern Italy)	2,032	2.30	38	2.42
Modena (North Central Italy)	996	1.13	37	2.36
Padova (North Central Italy)	1,270	1.44	35	2.23
TOTAL in North Central Italy	59,313	67.20	1,255	79.99
TOTAL in Southern Italy	28,952	32.80	314	20.01

Table 4 – Summary statistics for treated firms, before and after treatment

	Before treatment			After 1st year of treatment		
VARIABLES Obs. 819 before tr. / 1,473 after 1st y tr.	**Mean**	**Min**	**Max**	**Mean**	**Min**	**Max**
Equity	17.612	0	100	19.364	0	100
Debts	10.427	0	378	26.090	0	462
Employment	1.418	1	18	2.039	1	26

As common in treatment effects studies that utilize panel data, we can compare the before and after treatment values of outcome variables between the program participants and the control group as a descriptive preview on the difference-in-differences estimates that are presented in the subsequent econometric section.

Typically, the difference-in-difference estimates are visualized by simply plotting the average values of the dependent variable before and after the treatment for the program participants and the control group in form of event-study graphs. In our situation, however, the reality of the data is more complex than the graphs that are usually presented in textbooks, because:

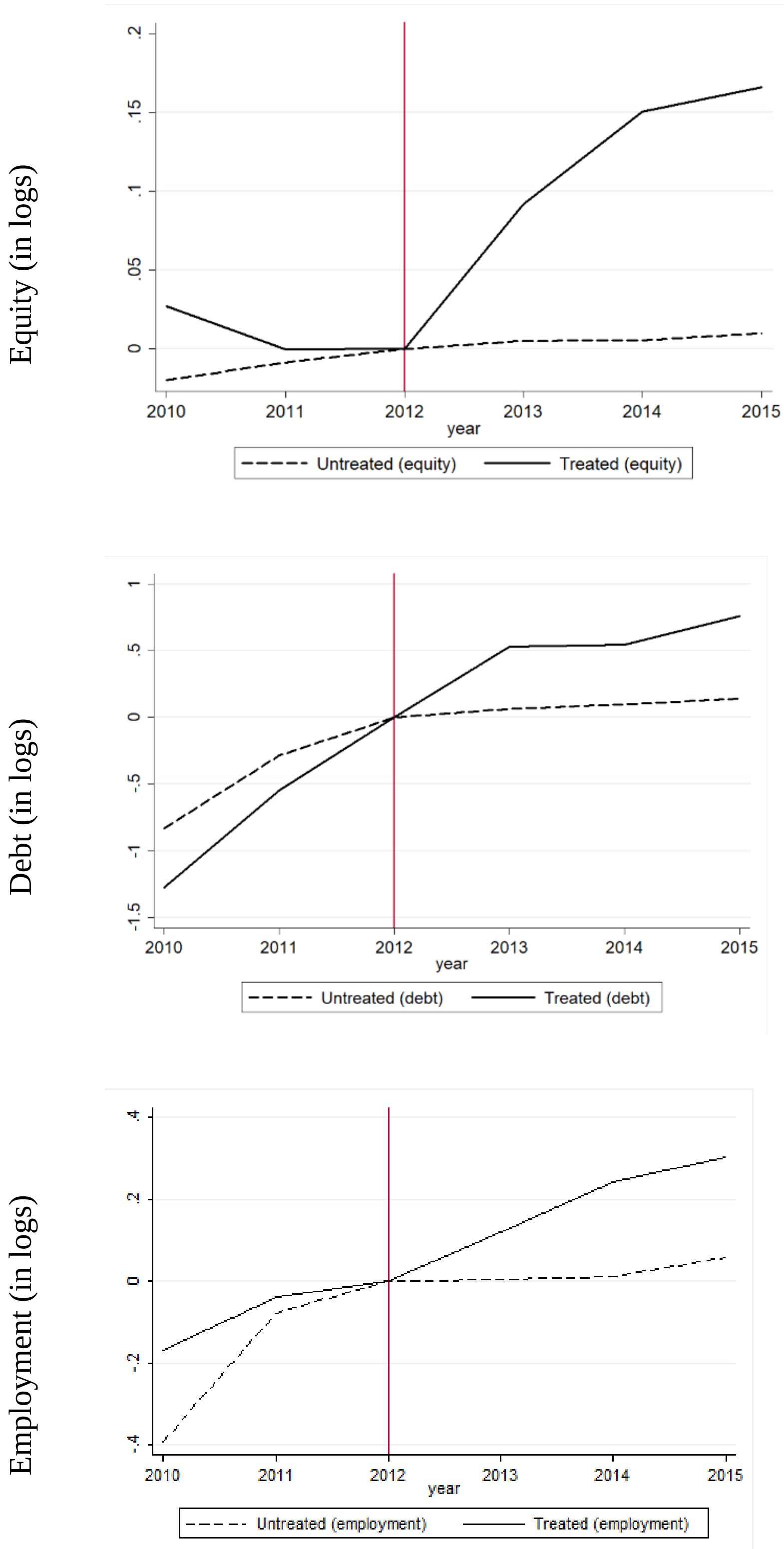
- instead of a law change that happens at a single point in time, our program participation is a *staggered treatment*, i.e. firms may start to enter the program from December 2012 onwards, but can also enter at any later stage in time. This implies that the treatment status does not change in a single point in time but can change in years 2013, 2014 or 2015 of our panel;
- our panel is by construction *unbalanced* as firms can enter the program only until they are maximally five years old. Therefore, we only consider eligible companies in the first place, i.e. firms that were founded between 2008 and 2014. Therefore, any average of an outcome variable that we would compute will not be formed by the same companies in every year. Therefore, the average might vary in a not meaningful way due to entry.
- the panel becomes unbalanced due to firm exits (*attrition*) and average values of the dependent variable might therefore be misleading as described above;
- we cannot exactly time the control group accurately in comparison to the treatment group because of the staggered treatments and the various entry and exit years.

While the subsequent regressions can be specified to account for these complexities, a simple event-study graph cannot encompass all these difficulties. Therefore, we only show some graphs for the sake of illustration that simplify the real data situation. Figure 1 therefore shows only parts of the data that is subsequently used in the regressions. In Figure 1, only data of the foundation cohort 2010 is shown, i.e. we have two pre-treatment periods. Furthermore, the data is limited to companies for which we observe at least five years of data until 2015. This means we exclude early exits that make the panel unbalanced. We plot the average of logs of the dependent variables equity, debt and employment and rescale the mean to be zero in 2012 for each time series in order to visualize the pretreatment trends and treatment trends as good as possible.

The graph shows a high effect in the treatment phase. The treated companies show a growth of equity of about 17% between the end of 2012 and 2015, whereas the control group has a relatively flat trend only growing about 2%. In the pre-treatment period, the two groups show quite similar trends between 2011 and 2012 but the equity drops by about 3% in the treatment group between 2010 and 2011, whereas it increases by about 2.5% in the control group during the same years. Thus, it is questionable whether the common trend assumption required for valid difference-in-difference regressions hold in the data. This will be tested in the subsequent econometric specification for the whole sample by including a pre-treatment dummy variable in the regressions. When the common trend assumption turns out to be violated, we apply conditional difference-in-differences models where we match the treated firms with comparable control firms.

The same econometric methods will be applied to debt and employment. The raw data of the 2010 firm-foundation cohort shows that the treated companies experienced higher growth in both the treatment period and the pre-treatment phase. Therefore, an estimate of the treatment effect without further adjustments might be misleading as the growth in debt was already higher before the Start-up Act was effective. With respect to employment, the pre-treatment trends are similar between 2011 and 2012 but the employment in untreated firms grew more between 2010 and 2011. In the treatment period, the employment of treated firms grows by about 30% but only about 5% in the control group. Again, more sophisticated econometric specifications are needed for deriving a convincingly estimated treatment effect.

Figure 1 –Untreated vs. Treated Firms



Note: The graph shows the data the foundation cohort 2010; only firms with at least five years of data. All firm-level data were within-demeaned, and then averaged by treatment and control group, respectively.

3 Empirical strategy

For the identification of policy effects, we mainly rely on (conditional) difference-in-difference (DiD) regressions (see e.g. Heckman et al., 1997, 1998, Angrist & Pischke, 2009, 2015). We compare equity, debt and employment of participating companies before and after the Startup Act of December 2012. To do that we must first identify a comparable set of non-participiping firms.

The DiD estimator is usually applied to situation where a policy affects a subpopulation of companies, e.g. all small and young firms in an economy. In that case, the firms cannot self-select into treatment. It is exogenously determined which firms are in the treatment group and which firms are in the control group. In our set-up, the firms can self-select into the treatment. This bears some potential bias in our estimation strategy, as the firms may have different participation probabilities. For instance, there might be some firms that expect less benefits from the program than others and therefore do not select into the program. These firms may not have a growth interest in the first place and are therefore not a good control group. In order to address the self-selection problem, we also conduct so-called conditional DiD estimations where we try to adjust the control group such that it has a similar participation likelihood as the treated firms. In that case, one would assume that the firms are either treated or not only because of purely random shocks. In practice, it means that we narrow the control group to become as similar as possible to the treatment group.

First, we consider all potentially eligible firms by size and age criteria. As tests will show, this control group does not fulfill the common trend assumption required for valid DiD estimates in all cases. Therefore, we construct more accurate control groups. As we cannot observe all other eligibility criteria comprehensively[5], we make use of propensity score matching techniques (cf. e.g. Heckman et al. 1997, 1998) to approximate eligibility and also the participation probability to the largest extent possible. As our econometric results will show, the matched control groups conform to the common trend assumption.

In our main analyses, we select our control group according to PSM. In more detail, the probit regression used to build the propensity score considers the presence of R&D expenditures measured by an R&D dummy variable, and also the presence of intangible assets and patent applications measured by two further dummy variables.[6] In addition, we consider the geographical location and the firms' industry differentiated by 12 sector dummies. As the common trend assumption was not fulfilled in all DiD regressions even after matching, we also considered lagged values before the treatment period of the dependent variables as matching criteria.

As further problem specific to panel data is attrition. Attrition leads to an unbalanced panel structure due to firm exits. If firm exits are disproportional between program participants and the control group, bias may be induced in the DiD estimates. We therefore explicitely model attrition by estimating survival regressions for each year as suggested e.g. in the textbook of Wooldridge (2010). We use the predictions of the survival regressions to compute annual, inverse mills ratios that we include as an additional regressor in the panel DiD models.

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[5] Among others, the eligibility criteria involve owning a patent or unique software or having an exclusive license, or an R&D intensity above 15% in terms of revenues, or at least 1/3rd of employees with Ph.D. degree or 2/3rds with master degree etc. (cf. subsection 1.2 for details).

[6] In case a firm is classified as an applicant, we set the patent dummy equal to 1 for the application year and all subsequent years. It is zero otherwise.

As discussed in the literature, the standard errors in DiD applications might be biased because of autocorrelation and the so-called Moulton bias. We address this concern by clustering the standard errors at a higher level (province level) than the observational unit, as recommended in the literature (see the discussion in Bertrand et al., 2004, or Angrist & Pischke, 2009).

Our first DiD specification implemented as fixed effects panel regression is:

$$y_{it} = \gamma_1 \cdot treatment_{it} + \gamma_2 \cdot post_{it} + \gamma_3 \cdot before_{it} + \beta X_t + \alpha_i + \varepsilon_{it}$$

with $i= 1 \ldots N$ (firms) and t=2008…2015 (years). (1)

To estimate the impact of the policy we consider different dependent variables (y_{it}): the natural logarithms of equity in thousands of Euros, bank loans in thousands of Euros, and the number of employees. Given our goal to evaluate the policy, our principal independent variable is represented by the treatment status ($treatment_{it}$). We add a before treatment dummy ($before_{it}$) to test whether the hypothesis of common trend assumption holds. It has value 1 the year before the treatment, otherwise it is 0. Moreover, the post-treatment dummy (called $post_{it}$) is present to avoid that formerly treated firms are considered as never-treated ones in the post-treatment phase and to estimateif the policy effects continue after the treatment period. The post-treatment dummy takes the value 1 once the firm drops out of the program because it became too large, too old, or it loses some mandatory requirements for an innovative startup (this is recorded in the administrative program data). Finally, we insert a full set of time dummies (X_{it}) to control for macro-economics shocks that might affect all firms.

In addition, we propose extended specifications of our base model. As in the case of relevant attrition effects, it could happen that program participants are more or less likely to survive than non-treated firms. On the one hand, treated firms may be able to make riskier investment because of improved access to equity and loans. Failures of risky investment projects may increase the probability of bankruptcy and thus exit (relative to the control group). On the other hand, the improved access to capital may also allow the companies to implement their business plans appropriately which might not have been possible without the program participation. As a result, firms with well implemented business plans might also survive longer. In order to account for attrition, we follow Wooldridge (2010: chapter 19) and estimated a series of probit regression on an indicator variable for survival. We estimate a cross-section probit model for each year *t* separately (always with the sample that was alive in *t-1*). From these probit models, we obtain the linear predictions and we calculate the inverse Mills ratio which is then included in the DiD regression as term accounting for attrition.

$$y_{it} = \gamma_1 \cdot treatment_{it} + \gamma_2 \cdot post_{it} + \gamma_3 \cdot before_{it} + \delta \cdot mills_{it} + \beta X_t + \alpha_i + \varepsilon_{it} \quad (2)$$

with *i*= 1… *N* (firms) and *t*= 2008…2015 (years).

Finally, we re-estimate eq. (2) with matched samples constructed by PSM techniques.

4 Results

4.1 Baseline model

In this Section, we show our baseline findings on the effects of the Startup Act. Since this law provides direct incentives on collecting equity, receiving bank loans and hiring people, we study the effects on these three variables.

As Table 5 shows we find positive treatment effects on all three dependent variables. The equity grows about 16% in the treated firms as response to the policy. The debt increases by about 76% and employment grows by about 18%.

The post-treatment dummy is also positive and significant in all cases. For instance, in the regression on equity it takes the value of about 11%. This would imply that the firms first manage to acquire 16% more equity as response to the policy (i.e. 16% more than they would have had if the policy would not have been introduced). Once the firm is no longer eligible to operate under the Italian start-up Act, e.g. because it became too large or too old, investors lose their tax benefits and as a consequence they could withdraw their equity. The post treatment coefficient of 10.8, however, shows that the equity remains higher than in the period before treatment. A test does not reject that the post treatment marginal effect of 10.8 is equal to the marginal effect of the treatment dummy which is 15.8. We thus conclude that we do not find a significant withdrawal of equity after the firm has to exit the Start-up Act program. The post treatment effects for debt and employment yield similar interpretations.

The test on common trends as indicated by the "before treatment" dummy variable is rejected in all cases, however. Thus, we conclude that these results may be affected by some bias. In order to remedy this situation, we consider further, more sophisticated estimation techniques.

Table 5 – The impact of the policy of equity, bank loans, employment: DiD regressions

	(1)	(2)	(3)
VARIABLES	Ln(equity)	Ln(debt)	Ln(employment)
Treatment	0.158***	0.758***	0.182***
	(0.019)	(0.068)	(0.024)
Post-treatment	0.108**	0.599***	0.206***
	(0.044)	(0.206)	(0.058)
Before treatment	0.025*	0.220***	0.047**
	(0.014)	(0.073)	(0.019)
Constant	2.502***	-0.448***	0.466***
	(0.005)	(0.059)	(0.017)
Firm Fixed Effects	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes
Observations	338,289	338,289	338,289
Number of firms	89,834	89,834	89,834

Clustered standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

4.2 DiD models accounting for attrition

In this subsection, the DiD models account for attrition in the panel. Therefore, we estimated survival equations as suggested by Wooldridge (2010: chapter 19). These survival regressions are estimated for each year separately based on covariates of the preceding year. In the appendix, we present a pooled cross-sectional regression for all years to save some space. The annual versions of this regression are used to compute yearly mills ratios that are then used as an additional regressor in the DiD models to account for attrition.

Table 6 – The impact of the policy of equity, bank loans, employment: DiD regressions considering attrition

VARIABLES	(1) Ln(equity)	(2) Ln(debt)	(3) Ln(employment)
Treatment	0.153***	0.697***	0.162***
	(0.019)	(0.071)	(0.024)
Post-treatment	0.108**	0.596***	0.205***
	(0.044)	(0.204)	(0.055)
Before treatment	0.022	0.178**	0.033*
	(0.015)	(0.074)	(0.019)
Mills ratio	-0.279***	-3.701***	-1.221***
	(0.022)	(0.128)	(0.055)
Constant	2.499***	-0.498***	0.449***
	(0.005)	(0.061)	(0.017)
Firm Fixed Effects	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes
Observations	338,289	338,289	338,289
Number of firms	89,834	89,834	89,834

Clustered standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 6 shows the DiD results for the specification accounting for attrition. The mills ratio is negative and significant in all cases. In terms of the treatment effects, the coefficients reduce slightly. In other words, without correcting for attrition, we overestimated the effects in the initial DiD regressions. Adding the mills ratio also reduces the coefficients and statistical significance of the “before treatment” dummy which tests the common trend assumption. However, the common trend is still rejected in the regression of debt and also weakly in the model on employment. Therefore, we turn to the matched control group below.

4.3 DiD models accounting for attrition using matched control groups

In this subsection, we apply a Propensity Score Matching technique to control for the selection into treatment by firms. Specifically, our PSM considers intangible assets, a dummy for positive R&D expenses, a patent dummy, as well as sets of dummy variables for the foundation years, the sector and the location of the firm.

The patent dummy, the R&D dummy and the intangible assets are used to approximate the program's eligibility criteria to the best possible extent. Intangible assets may be seen as a proxy of the presence of innovation activities. R&D expenditures are explicitly mentioned as an eligibility criterion since it is required to have and R&D intensity of at least 15%. Being a patent applicant is linked with the criteria that required to be holder, depositary or licensee of at least one industrial property. Unfortunately, we cannot observe the exact R&D intensity, nor unique software or other licenses and we have no information on the qualification structure of the firm's personnel. Even though this data may be available for the participant companies, it is not available for the control group that never applied for the program.

Finally, to refine the control group even further we also added lagged values of the outcome variables in pre-treatment periods in order to obtain common trends (if necessary)[7].

The PSM is implemented as nearest neighbor matching with one nearest neighbor for each treated firm.

When using the Propensity Score Matching, we obtain that the estimated coefficient of the pre-treatment dummy is statistically insignificant in all models, i.e. the common trend

[7] In this baseline, we consider only firms founded before 2013 because of at least one year of pre-treatment is observed. Indeed, many firms established in 2013 and onwards born directly as "innovative start-ups".

assumptions are not violated. Furthermore, Table 7 shows that the policy has a positive and significant impact on all three outcome variables: equity, bank loans, and employment.

Even though the estimated treatment effects are highly statistically significant and positive, they are of moderate economic significance. One has to keep in mind that the program participants are very young start-up companies. They thus have very small factor endowments: on average, the treated companies had before the policy program existed or they participated an equity endowment of € 17,612, average debt of € 10,427 and 1.4 employees.

Table 7 – The impact of the policy of equity, bank loans, employment: DiD regressions with matched control groups.

	(1)	(2)	(3)
VARIABLES	Ln(equity)	Ln(debt)	Ln(employment)
Treatment	0.105***	0.415***	0.117***
	(0.022)	(0.098)	(0.029)
Post-treatment	0.043	0.218	0.100*
	(0.070)	(0.236)	(0.053)
Before-treatment	0.007	0.125	0.014
	(0.018)	(0.083)	(0.021)
Mills	-0.542***	-3.211***	-0.458*
	(0.163)	(0.916)	(0.256)
Constant	2.712***	-0.997**	0.509***
	(0.053)	(0.446)	(0.086)
Fixed Effects	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes
Observations	6,251	6,416	6,349
Number of firms	1,432	1,469	1,455

Clustered standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

For equity, the estimated treatment effect amounts to a growth of 11.1% [=exp(0.105)-1]. In terms of real effects this implies that the equity grows as a result of the Italian Start-up Act from € 17,612 to € 19,549, or in other words an increase of € 1,937. The average debt increased by 51.4%, i.e. it changed from € 10,427 to € 15,790, and the average employment increased by about 1/5th of an employee (from 1.4 employees to 1.6 employees).

From the program implementation in December 2012 until the end of 2015, 5,145 firms had signed up for the program. In total, the program thus created about 926 more jobs in innovative start-up companies, and injected almost € 38 million of capital into these firms (in terms of equity and debt).

The DiD models also contain the post-treatment dummy. This would in principle allow to investigate whether the treatment effect is durable after the participants can no longer operate under the Start-up Act. However, the post-treatment dummy becomes insignificant in our matched samples. Given these imprecise estimates tests never reject that the post-treatment effects is equal to the treatment effect. However, more research with more data after the program exit seems warranted to verify these preliminary results on post treatment effects.

4.4 DiD models with heterogeneous treatment effects

As robustness tests, we also estimated annual treatment effects. It could be that the treatment effect evolves over time as potential investors are not yet familiar with the program shortly after its introduction and this behave more conservative in the beginning.

We create three dummy variables: *treatment2013*, *treatment2014*, *treatment2015* to see how the policy effects unfolded over the years. As we can observe in Table 8, the treatment effects intensify year by year. In the case of debts and employment, the treatment effect in 2013 was insignificant. However, this growing trend may be due to the typical time lag needed to observe the actual impact of a new policy. For instance, in our case, firms need some months to collect equity, receive loans from banks or hiring people and the final outcome may not realize immediately.

Table 8 - DID with attrition and matched samples: annual treatment effects

	(1)	(2)	(3)
VARIABLES	Ln(equity)	Ln(debt)	Ln(employment)
Treatment2013	0.075***	0.098	0.031
	(0.023)	(0.119)	(0.026)
Treatment2014	0.099***	0.281**	0.084***
	(0.025)	(0.108)	(0.032)
Treatment2015	0.149***	0.911***	0.256***
	(0.031)	(0.127)	(0.041)
Post-treatment	0.052	0.297	0.124**
	(0.070)	(0.237)	(0.050)
Before treatment	0.005	0.094	0.006
	(0.018)	(0.083)	(0.021)
Mills ratio	-0.717***	-5.295***	-1.005***
	(0.164)	(0.963)	(0.273)
Constant	2.708***	-0.996**	0.514***
	(0.053)	(0.450)	(0.088)
Fixed Effects	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes
Observations	6,251	6,416	6,349
Number of firms	1,432	1,469	1,455

Clustered standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Finally, in Table 9 we analyze the impact of the policy in the Northern and Southern regions of Italy. This historical gap between the more developed Northern area and the Southern part of Italy has been widening after the Great Recession. Specifically, the Northern part has a developed economy. Conversely, the Southern part is a depressed region which often meets severe difficulties in growth, innovation and employment because it has an undeveloped infrastructure system, weak institutions and a fragile industrial base. To analyze the impact of the Startup Act in the two macro-areas, we repeat the treatment analysis with a dummy variable for firms located in the Southern part of the country (called '*Mezzogiorno*', in Italian language). While the results' table suggests that the treatment effects vary across regions, tests on differences in coefficients between the North and the South do not yield any statistical result. We thus conclude that the policy works in both the Northern and the Southern regions of Italy.

Table 8 - DID with attrition and matched samples: Northern and Southern Italy

VARIABLES	(1) Ln(equity)	(2) Ln(debt)	(3) Ln(employment)
Treatment North	0.087***	0.426***	0.109***
	(0.026)	(0.100)	(0.028)
Treatment South	0.192***	0.362**	0.157**
	(0.069)	(0.146)	(0.070)
Post-treatment	0.040	0.220	0.099*
	(0.070)	(0.236)	(0.053)
Before treatment	0.007	0.125	0.014
	(0.018)	(0.084)	(0.021)
Mills ratio	-0.536***	-3.214***	-0.455*
	(0.160)	(0.917)	(0.256)
Constant	2.712***	-0.997**	0.509***
	(0.053)	(0.446)	(0.087)
Fixed Effects	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes
Observations	6,251	6,416	6,349
Number of firms	1,432	1,469	1,455

Clustered standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

5 Conclusions

In our analysis we documented that the effect of Italian Startup Act (Law 221/2012) is positive along multiple dimensions by easing firms' access to equity and debt capital. Specifically, tax benefits for new equity investors alleviate the problem of shortage in risk capital, since the estimated treatment effect is positive and statistically significant. The Startup Act also contributes to access to bank loans by small and young enterprises. Following our results, we find that innovative startups have higher debt as a response to the program participation. We interpret this finding as better access to debt capital because of the public loan guarantees.

Cost-Benefit-Analysis

In total, our results suggest that the program had injected almost € 34 million in terms of equity and debt capital into Italian innovative start-up companies between the end of 2012 and 2015. In addition, the program created more than 900 additional jobs because of more flexible labor market regulations for firms operating under the Italian Start-up Act.

These benefits of the policy can be contrasted with the associated cost, i.e. forgone income tax revenues for the government as well as default loans for which the government had made guarantees towards the creditors:

- In 2013, the total costs due to lower taxes is € 5.9 million (audited value) and € 0.5 in terms of loss for guaranteed loans (estimated value by the government).
- In 2014, the total costs due to lower taxes is € 10.2 million (audited value) and 0.5 million in terms of loss for guaranteed loans (estimated value).
- For 2015 we do not have data for total cost, but inferring from new created start-up companies we could expect a total cost in terms of forgone taxes and loss due to guaranteed loans of € 11 / 12 million.

This roughly corresponds to a total cost of € 29 million which would almost neutralize the capital injection of € 38 million in the total businesses. The policy thus mostly channels money that would have used for other purposes into small and young innovative companies. As this, however, promises some further options for economic growth in the future, it still seems to be a policy with potential future benefits. In addition, more than 900 jobs were created in young innovative companies which would otherwise not have existed.

If one thus interprets the € 29 million cost for the government as a direct subsidy for the business sector, one could calculate that the creation of one job (for about 5 years at least) did cost the Italian taxpayer about € 32,000. This seems a justifiable amount for governmental job creation. Our results also contribute to a better understanding of the impact of similar startup policies which have been recently implemented in several countries around the world such as Belgium, India, and Latvia.

For future investigations, a number of questions seem highly interesting: in terms of the Italian start-up Act, it would be interesting to investigate with more recent data how durable the estimated treatment effects are. Even though we estimated post-treatment effects, our results were somewhat inconclusive. This possibly owes to a limited number of post-treatment observations. With more recent data and thus more years elapsed after program participation, more reliable post-treatment effects could be estimated.

In addition, it would be worthwhile to explore to what extent the increased factor inputs in terms of capital and labor yield positive output effects in terms of sales or productivity growth. At time of writing this paper, the program was too recent to investigate output effects.

Finally, it would be interesting to compare the policy design of the Italian start-up Act to other international programs that also aim at (innovative) start-ups and to compare the effects of different policy designs.

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Appendix

Probit model on program participation

In order to select a matched control group, we employ a probit model on treatment status. We estimated cross-sectional models for each program cohort where the covariates stem from the last year before the firms may have entered the program. In particular, we consider an R&D dummy, a dummy for the presence of intangible assets, a dummy for the presence of at least one patent, the firm's foundation year and its squared value to control for non-linearities, as well as sets of sector dummies and regional dummies. Moreover, we added pre-treatment values of the outcome variables, i.e. the logarithms of equity, debt and employment in levels as of 2012 the year before the policy was launched). Thus we match on the lagged levels of the outcome variables next to other exogenous covariates that determine the subsequent treatment probability. We also experimented with pre-treatment trends of the outcome variables (not shown in table). We find that the selection criteria such as R&D, intangible assets and the presence of at least one patent have the expected positive signs on future treatment probability and that these variables are also statistically significant (see column 3). The levels of debt and equity also show a positive sign in the regressions, but employment has a negative sign. This is to be expected as larger firms are more likely to not qualify for the start-up act because of the participation threshold in terms of firm size.

Table A.1 – Probit models on treatment status

VARIABLES	(2)	(3)	(4)
Ln(debt)	0.026***	0.034***	0.017***
	(0.005)	(0.005)	(0.005)
Ln(employment)		-0.246***	-0.254***
		(0.018)	(0.019)
Ln(equity)		0.157***	0.078***
		(0.010)	(0.011)
R&D (dummy)			0.586***
			(0.031)
Intangible (dummy)			0.540***
			(0.029)
Patent (dummy)			1.263***
			(0.050)
Age	-0.032	-0.063***	-0.088***
	(0.020)	(0.020)	(0.021)
Age^2	-0.049***	-0.047***	-0.039***
	(0.004)	(0.004)	(0.004)
Constant	-2.730***	-2.842***	-3.140***
	(0.089)	(0.094)	(0.100)
Year dummies	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes
Observations	185,206	185,206	185,206
Pseudo R-squared	0.310	0.323	0.372
Loglikelihood	-10708	-10497	-9744

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Survival effects and refining the control group

As explained in the main body of the text, we control for attrition in the sample. The estimates of the program treatment effects might be biased if treated firms live significantly longer or die significantly earlier than the control group. In order to control for that, we estimate a set of survival equations. In order to construct a mills ratio as outlined in Wooldridge (2010: chapter 19), we estimate the survival models for each cross-section separately. In order to show the main regression results but save space, we print below regression results for the full sample using pooled cross-sectional data though. In the first 2 regressions, it turns out that the treated firms are more likely to survive. This effect of the treatment status disappears, however, once we control for debt, equity and employment. This means the survival is not influenced by some (unobserved) program effect but by the fact that firms have better access to equity, employees and debt capital; partly through their program participation.

Table A.2 – Probit models on firm survival

VARIABLES	(1)	(2)	(3)	(4)	(5)
Treatment	0.195***	0.194***	0.063	0.080	0.014
	(0.049)	(0.049)	(0.051)	(0.051)	(0.052)
Post-Treatment		-0.155	-0.122	-0.097	-0.158
		(0.180)	(0.180)	(0.182)	(0.184)
Ln(debt)			0.052***	0.039***	0.030***
			(0.003)	(0.003)	(0.003)
Ln(employment)				0.203***	0.179***
				(0.009)	(0.009)
Ln(equity)				0.030***	-0.002
				(0.006)	(0.007)
R&D (dummy)					-0.016
					(0.031)
Intangible (dummy)					0.307***
					(0.011)
Patent (dummy)					-0.075
					(0.057)
Age			-0.333***	-0.350***	-0.322***
			(0.011)	(0.011)	(0.011)
Age^2			0.037***	0.039***	0.039***
			(0.002)	(0.002)	(0.002)
Constant	1.765***	1.765***	2.276***	2.009***	1.861***
	(0.041)	(0.041)	(0.045)	(0.047)	(0.048)
Year dummies	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes
Observations	335,661	335,661	335,661	335,661	335,661
Pseudo R-squared	0.0603	0.0603	0.0837	0.0916	0.102
Loglikelihood	-35473	-35472	-34588	-34291	-33897

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Correlation among main variables

As shown in the Table A.3, the correlation among different variables is usually low; the highest one (in absolute value) is 0.29 between Age and the R&D dummy which does not raise an concern about multicollinearity.

Table A.3 – Correlation among main variables

	Equity	Bank debts	Employment	Patent (dummy)	R&D (dummy)	Intangible (dummy)
Equity	1					
Bank debts	0.14	1				
Employment	0.04	0.11	1			
Patent (dummy)	0.02	0.001	-0.01	1		
R&D (dummy)	0.07	0.06	0.05	0.02	1	
Intangible (dummy)	0.03	0.06	0.04	0.03	0.11	1
Age	0.09	0.13	0.07	0.02	-0.29	0.06