Supporting innovative entrepreneurship: an evaluation of the Italian "Start-up Act"

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Abstract. The role of innovative start-ups in contributing to aggregate economic dynamism has attracted increased attention in recent years. While this has translated into several public policies explicitly targeting them, there is little evidence on their effectiveness. This paper provides a comprehensive evaluation of the "Start-up Act", a policy intervention aimed at supporting innovative start-ups in Italy. We construct a unique database encompassing detailed information on firm balance-sheets, employment, firm demographics, patents and bank-firm relationships for all Italian start-ups. We use conditional difference-in-differences and instrumental variable strategies to evaluate the impact of the "Start-up Act" on firm performance. Results show that the policy induces a significant increase in several firm outcomes whereas no effect is detected in patenting propensity and survival chances. We also document that the policy alleviates financial frictions characterizing innovative start-ups through the provision of tax credits for equity and a public guarantee scheme which, respectively, trigger an increase in the probability of receiving VC and accessing bank credit.

Keywords Start-ups · Entrepreneurship policy · Policy Evaluation · Firm performance
JEL M13 · L25 · L53 · D04

1 Introduction

Start-ups are key to economic growth: they contribute disproportionally to input accumulation (Audretsch et al., 2006; Haltiwanger et al., 2013; Criscuolo et al., 2014), productivity growth (Haltiwanger et al., 2016; Dumont et al., 2016; Alon et al., 2018). Moreover, they indirectly impose competitive pressure on incumbent firms, thus forcing them to innovate in order to survive (Aghion et al., 2009).

Several frictions however may impair the contribution of start-ups to growth. Red tape and bureaucratic costs may reduce birth rates of new firms and affect their selection at entry (Ciccone and Papaioannou, 2007; Fernández, 2014; Amici et al., 2016). Start-ups may have difficulties accessing credit markets, because of a lack of past information on firm performance and modest endowments of collateralizable capital (Colombo and Grilli, 2007). They may also suffer from the underdevelopment of alternative markets for external finance (Inderst and Müller, 2004).

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Governments can intervene to curb these frictions and boost new firms' contributions to growth; however, designing effective and efficient policy tools is particularly difficult. Indeed, the average performance of start-ups masks a large heterogeneity among them: only a small share of firms are truly "transformational", i.e. their goal is to generate an increasing flow of profits and jobs in the future via innovations in products, processes, and marketing (Schoar, 2010). These businesses are ultimately risky and large experimentation is usually accompanied by high failure rates (Hyytinen et al., 2015; Fernandes and Paunov, 2015). At the same time, the vast majority of start-ups are ultimately not innovative (Shane, 2009; Hurst and Pugsley, 2011; Nightingale and Coad, 2014; Colombelli et al., 2016): they are created to run small, subsistence businesses.[1] Policy-makers face a trade-off: on the one hand, one may want to provide large horizontal support to as many start-ups as possible, to let "one hundred flowers bloom" to foster experimentation; on the other hand, given the limited public resources available, one may like to limit this support to innovative firms with high growth potential.

In 2012, the Italian government introduced a new policy framework, the "Start-up Act", which tries to strike a balance between these different objectives. The "Start-up Act" defined a set of eligibility criteria based on observable firm characteristics such as age, R&D expenditure, employment skills, and intellectual property, to identify firms that are ex-ante more likely to be innovative. All start-ups that meet these criteria may freely register to the program and access a large set of benefits, ranging from reduced red tape costs, tax incentives for equity investments, credit guarantee schemes, flexible labour laws and so on. By the end of 2019, over 10,000 firms have been registered into the program. The Italian "Start-up Act" has been considered a "best practice" in Europe, according to the Start-up Nation Scoreboard (European Digital Forum, 2016). While the extremely diverse policy tools included in the Act were meant to foster the build-up of an ecosystem for innovative startups in Italy, the policy stands out for supporting start-ups' post-entry performance by facilitating access to both equity and debt capital markets through tax incentives and a credit guarantee scheme, respectively. This represents a unique feature of the policy with the ultimate goal to tackle financial frictions, which are recognized as the single most important factor in preventing the growth of innovative start-ups (Hall, 2010; Kerr and Nanda, 2015).

In this paper we evaluate the effects of this policy. For this purpose, we construct a unique database that combines, for all start-ups born in Italy between 2005 and 2014, firm balance-sheets, demographic data, wages and employees, patent applications and credit information from the Credit Register. Our empirical strategy addresses the bias stemming from firms' self-selection into the program. Firms registering as innovative start-ups can be systematically different from those that do not. The decision to register might be related to both observable and unobservable characteristics of the firms that are correlated with their subsequent performance. First, we rely on matching techniques to identify untreated start-ups with similar pre-treatment observable characteristics to those that join the policy. Then, to control for unobservable characteristics that are fixed overtime, we use a difference-in-differences estimator (DID) on the matched sample. Finally, there may be time-varying unobservable shocks that are correlated with policy participation and with firm activities, such as a positive demand shock or a new business idea. To address this issue, we develop an instrumental variable strategy that exploits the heterogeneous diffusion of the policy at the geographical level in the aftermath of its introduction. This was due to a specific

[1] A series of studies have documented that most new firms display a low propensity to introduce innovations and little or no growth during the first five years of their activity (Stam and Wennberg, 2009; Hurst and Pugsley, 2011; Decker et al., 2014). Yet, a small share of new firms, often engaging in innovative activities, is able to experience very fast expansion (Stam and Wennberg, 2009; Santoleri, 2019).

provision of the program that required local Chambers of Commerce (CC) to deliberate on whether the start-up official mission statement was “innovation oriented”.

We find that the policy induces a substantial increase in firm assets, mainly driven by fixed capital. In particular, we document a considerable rise in intangibles, though not driven by patenting, an essential input for the innovative process of startups which is usually hampered by financial frictions. Higher intangible capital is accompanied by larger value added and a positive (albeit statistically weaker) effect on revenues and employment. Concerning the financial side, we do find a positive impact on firm equity and a stronger effect on indebtedness, so that average leverage increases. Conversely, the “Start-up Act” does not seem to affect the probability of survival of the firm.

We then move to study the effects of the policy in terms of accessing external financing. In particular, we exploit confidential Credit Register data on credit applications and find that the “Start-up Act” raises the probability that the application is accepted by banks and increase the amount of credit granted conditional on being accepted. Furthermore, leveraging data from the universe of venture capital deals in Italy, we find that the policy triggers an increase in the probability of receiving venture capital. These results suggest that the policy has successfully alleviated the financial frictions characterizing innovative start-ups. Moreover, our findings underscore the value of providing a full and comprehensive set of measures that ease both debit and equity financing, in order to let start-ups choose the financing instruments that are more appropriated for their expansion strategy.

Entrepreneurship policies have traditionally targeted firms based on their size, following the idea that small businesses contribute to net job creation the most. However, recent evidence has highlighted that it is actually young firms (Haltiwanger et al., 2013), and more prominently innovative young firms (Shane, 2009), that make a decisive contribution to aggregate economic dynamism, and that firm size *per se* is much less relevant once one conditions on age. Against this backdrop, greater attention has been devoted to (innovative) start-ups in both the academic and policy debate. This has resulted in governments implementing national Startup Acts with the explicit aim of offering support to innovative start-ups in several countries (Audretsch et al., 2019).[2] Because of the lack of case-studies, so far scholars have only been able to provide very few assessments of these new initiatives promoting innovative start-ups. Exceptions include the work by Autio and Rannikko (2016) on the Finnish case, and two contemporaneous works on the Italian policy by Finaldi Russo et al. (2016) and Giraudo et al. (2019). While the latter focuses on the interrelationships between the instruments supporting the access to external finance provided by the “Start-up Act”, the works by Autio and Rannikko (2016) and Finaldi Russo et al. (2016) are more related to ours: they evaluate the impact of the policies on some firm outcomes. However, they solely implement a propensity-score matching, which assumes that policy participation is random conditional on observable characteristics. We show that such an assumption is unlikely to hold, improving on the identification strategy by developing a PSM-DID with instrumental variable approach. We also improve by exploiting a more comprehensive set of information, which allow us to provide a broader and clearer picture of the effects of the policy.

The remainder of the paper is structured as follows. Section 2 describes the policy implemented by the Italian government since 2012. Section 3 presents the data whereas Section 4 discusses the empirical strategy to assess the effects of the “Start-up Act” on the performance of innovative start-ups. Section 5

[2] In the EU, 12 countries have introduced regulatory frameworks or given special status to startups by 2016. Recent examples are the NIY program in Finland and the Start-up Plans of Belgium and France (i.e. “Jeunes entreprises innovantes”). For a review of recent policy initiatives focuses on innovative start-ups see European Digital Forum (2016). An updated compendium of international public policies is the Startup Nations Atlas of Policies, available at https://www.genglobal.org/startup-nations/snap.

presents the main results of our estimation exercise, and Section 6 provides a series of robustness checks. Section 7 presents some policy implications of our findings and concludes.

2 Institutional framework

Italy has experienced sluggish productivity growth since the mid 1990s (Bugamelli et al., 2018). One of the factors behind this modest performance is the lack of business dynamism characterizing the Italian economy. Differently from other OECD countries, young firms in Italy grow less, for a shorter period of time while exit rates are generally flat over the age distribution (Criscuolo et al., 2014; Manaresi, 2015). As a result, the "up-or-out" dynamics that models of firm dynamics identify as a crucial ingredient for productivity growth is particularly subdued in Italy. Against this backdrop, the Italian government introduced a policy framework for innovative entrepreneurship - nicknamed "Start-up Act" - in October 2012, which continues to this day. The extensive regulatory framework aims to create a more favourable environment for small innovative start-ups for the first five years of activity through a number of complementary instruments. This policy stands out in the international comparison for the comprehensiveness of the bundle of activated policy instruments: these include measures that cut red tape and facilitate entry and exit to the market; tax incentives; tailor made labour laws; support to flexible remuneration schemes; incentives for equity crowdfunding; etc.[3]

One of the unique features of the "Start-up Act" framework is the simultaneous provision of policy instruments addressing the access to external finance. Financial frictions in both credit and equity capital markets have been identified as the single most severe constraint to small and young innovative firms' performance by a vast literature (Hall, 2010; Hall and Lerner, 2010; Kerr and Nanda, 2015; Revest and Sapio, 2012). To this end, the policy encompasses two measures to support innovative start-ups' demand for capital: i) an incentive for venture capitalists and external investors who invest in the equity of innovative start-ups; ii) fast and free of charge access to a public guarantee scheme for debt financing.[4] The incentives to equity financing are of paramount importance for two reasons. The first is that prior literature has documented the beneficial effects of venture capital financing on firm performance (Kortum and Lerner, 2000; Hellmann and Puri, 2000; Bertoni et al., 2011; Puri and Zarutskie, 2012). The second is related to the severe underdevelopment of the Italian venture capital industry in comparison to other OECD countries (Revest and Sapio, 2012; Bertoni et al., 2015; Menon et al., 2018; Bronzini et al., 2019). Yet, exclusively providing incentives to equity financing might miss out on those start-ups with innovative and growth potential that do not choose to seek equity or simply cannot obtain it.[5] The simultaneous

[3] A complete list of policy tools are reported on the website of the Italian Ministry of Economic Development, `www.mise.gov.it`.

[4] Note that these measures aimed at facilitating access to debt and equity financing are the most popular and used policy instruments of the "Start-up Act" as documented by a survey conducted on beneficiary firms (MISE-Istat, 2018).

[5] A large literature has documented that venture capitalists finance a very small amount of firms (Mulcahy, 2013; Gornall and Strebulaev, 2015) and that venture capital deals are heavily concentrated at the geographical level (Sorenson and Stuart, 2001; Cumming and Dai, 2010). This represents a substantial barrier since entrepreneurs tend to locate their businesses in their home regions (Michelacci and Silva, 2007). Catalini et al. (2019) show that, controlling for start-ups early growth potential, firms that do not obtain venture capital are actually very similar to those that do. Furthermore, start-ups might also opt for debt financing to, *inter alia*, retain equity ownership (Cumming and Groh, 2018).

provision of both debt and equity incentives is precisely aimed at encompassing different financing needs and choices of innovative start-ups.[6]

The "Start-up Act" defines a set of eligibility criteria to identify start-ups that are expected to be (or become) innovative firms and who may benefit from policy support: the company should be operational for less than five years, be headquartered in Italy, have an annual turnover lower than five million euros, not be the result of a branch split or merger from a previous company, have a mission statement explicitly related to innovation, be a limited company and not publicly listed, and should not have distributed profits. Furthermore, firms need to fulfil at least one of the following three criteria: at least 15 per cent of R&D expenditure ratio; 1/3 of employees are PhD students or graduates or researchers and/or 2/3 hold a Master's degree; be the holder, depository or licensee of a patent, or owner/author of registered software. By posing stringent limits on company size and age, the policy maker narrows eligibility to firms that are also expected to be in need of support, i.e., to be confronted with a number of possible market failures.

At the time of the analysis (April 2017), there were 7,044 active firms registered into the program. The average firm has three employees, an output of 123,131 euro, share capital worth of 52,528 euro and are 112 days old at the time of entry into the policy.[7]

Since the start of the policy, there has been a constant and steep increase in the number of entrants participating in the Italian "Start-up Act". Participation started off relatively slowly and picked up considerably from 2014. Gradual uptake at the beginning can be partially explained by the lack of awareness of the policy amongst eligible firms throughout the country. For example, according the results of a survey conducted by the Ministry of Economic Development and the National Statistical Institute, one of the main sources of information about the policy was the firm's accountant (MISE-Istat, 2018). Another factor that contributed to the slow uptake in the first years is the heterogeneous interpretation of the requirement that the mission statement should be innovation-oriented, which we exploit for identification, as described in Section 3.

One of the commendable aspects of the policy is the large variety of data collected on firm participants. In fact, an essential prerequisite to access the policy incentives and benefits is to register into the special section of the business register, maintained by the Italian Chambers of Commerce, on behalf of the Ministry of Economic Development.

Given the number and variety of different policy instruments, there is no clear method to calculate precisely the total cost of the policy. However, it is possible to obtain an approximate estimation based on the costs of the main instruments, most of which take the form of foregone tax revenues (more details are available in MISE (2017) and Menon et al. (2018)). A back-of-envelope calculation gives an aggregate cost of around 30 million euro for the period 2012-17 for the approximately 9,000 start-ups that have ever been registered into the policy, which corresponds to around 3,300 euro for each start-up.[8]

3 Data

In order to analyse the impact of the Italian "Start-up Act", the paper uses a unique and rich dataset that combines different sources of information. Some are maintained or collected by MISE to comply with

[6] Vacca (2013) shows that Italian young innovative firms receiving private equity and venture capital are usually also financed by banks. Instead Giraudo et al. (2019), who examine firms registered to the "Start-up Act", find that start-ups invested by VC tend to be remarkably different from those that recur to debt financing.

[7] These calculations are based on a snapshot of the Start-up Business Registry in May 2017, which covers the period from October 2012 to April 2017.

[8] This excludes the resources of the public guarantee scheme. Also, Smart&Start Italia and Invitalia Ventures matching fund which where not included in the original 2012 "Start-up Act".

their policy monitoring duties; we match them with micro-level data coming from both administrative and commercial sources, as well as the Bank of Italy and the European Patent Office (EPO). Overall, the resulting database allows us to assess the impact of the policy on many potential outcome variables, covering a multitude of different dimensions of start-up operations and growth patterns.

Despite the richness of the dataset, several factors of complexity arise. The first one is the definition and the measurement of start-up *success* within such a short time-span. While available evidence suggests that most unsuccessful start-ups tend to fail within the third year of activity (Calvino et al., 2016), a successful start-up may require more time to thrive and create value. Furthermore, this value may take many different forms, which are not equal and easy to measure. Some start-ups may grow in employment and value added, or become very productive: these phenomena can be captured by balance-sheet data, observed for instance with data on acquisitions. However, other start-ups may create value under the form of social mobility opportunities and inclusiveness, of increased competition and consumers' welfare in the market, of disruptive innovations in fields that are important for the society as a whole, like health or climate change mitigation. While these forms of value may have some repercussions on balance-sheet data, the full effect may be hard to measure accurately.

A further limitation is the time-span of firm-level data (especially balance sheets) after the implementation of the policy: the Italian "Start-up Act" was fully implemented in the first half of 2013 and since typically new firms do not publish a balance-sheet for their first year of activity, only the first three balance-sheets can be observed for the oldest participants, with the last observation being the fiscal year 2016. This leaves one with a three-year window that, while should be sufficient to detect some important indicators of success, is inevitably noisier and less precise than a longer time horizon.

The data source that allows us to identify participant firms is the special section of the Business Registry dedicated to innovative start-ups (henceforth "start-up registry"). The registry is updated weekly, allowing an on-time monitoring of the policy; this is one of the best practices of the policy that should be commended. Among the 7,044 active firms registered in the program on April 2017, 42 per cent of them were operating in information and communication services, 25 per cent in professional and scientific activities and 18 per cent in manufacturing. The variables of interest provided by the start-up registry include the eligibility criteria that qualified firms to enter into the policy and their age at entry, in addition to a number of other firm characteristics. Financial and current account statements for the universe of Italian limited liability companies are available from the Cerved database, administered by Cervedgroup ltd. It contains detailed accounting data for around 700 thousands firms per year over the period 2005-2016.

To assess the impact of the "Start-up Act" on firms' access to external finance, we use data from the Italian Venture Capital and Private Equity Association (AIFI) and the Bank of Italy. The former is used to examine the effects on venture capital financing whereas the latter is employed to study access to bank credit. Data from AIFI encompass the universe of VC deals in Italy during the time span 2004-2018.[9] The Central Credit Register is an information system operated by the Bank of Italy that collects the data supplied by banks and financial companies on the credit they grant to their customers. The database has information about quantities, prices and loan applications pertaining bank-firm relationships. This data reports the quantity of all credits granted by banks operating in Italy to borrowers for which the overall exposure to the banking system is above 30,000 euros (this amount includes both credit granted and the value of guarantees provided to the borrower). Finally, data on loan applications are used to analyse the probability that a firm registered in the program receives new credit from a bank. For supervisory

[9] In more detail, these data include all seed and early stage VC investments. Data are available through Venture Capital Monitor by AIFI at http://www.privateequitymonitor.it/rapporti_vem.php.

purposes, every time a new client submits a loan application to a bank, the latter requests information on this borrower and the query is recorded in the Credit register. By checking whether the bank eventually granted any credit to the applicant in the same or in the following quarter, one is able to classify a loan application as accepted or rejected.[10]

Finally, we access micro-level data collected by the Italian Chambers of Commerce to obtain detailed and reliable information on firm demographics for all businesses operating in Italy, e.g. information on when and where firms are established, whether they are truly *de-novo* firms or they are created out of a firm divestment, whether they effectively exit the market or they are acquired by other firms. We use this information to identify all start-ups born during the period 2005-2015 and check their survival over this time period. We exploit data from the National Social Insurance Institute (INPS) to obtain the number of employees and average wages for all Italian firms on a yearly basis over the period of analysis. Finally, we use PATSTAT to complement the above sources of information with patent applications filed to the EPO by Italian firms up to 2017.[11]

3.1 Sample selection and descriptive statistics

Our empirical strategy, outlined in the next section, requires treated firms to have at least one pre-treatment period in order to perform the matching procedure and at least two post-treatment periods in order to estimate treatment effects. Hence, we select only those firms observed for three consecutive years and that benefit from the policy for a minimum of two years. Since our balance-sheet data cover the period up until 2016, this implies that the final sample is restricted to those cohorts of firm born during the period 2009-2014 and that could have joined the policy since late 2012 up to 2015.[12] The number of registered start-ups up to 2015 is 3,560 whereas, by applying the above-mentioned criteria, the sample is restricted to 571 firms[13]. After dropping firms with missing variables in key balance-sheet covariates (i.e. revenues, total assets, fixed assets, and equity), the sample reduces to 328 treated firms.

Concerning those firms that could be considered as potential controls (354,694 firms), we make sure that they could meet the eligibility criteria of the policy in terms of both size and age: we discard those firms featuring minimum revenues higher than 5 million euros and keep only firms born during the period 2009-2014 as for our treated sample. By considering only those with three consecutive observations and discarding firms missing variables, we end up with a sample of 67,897 firms.

In order to verify the representativeness of our smaller sample of the initial universe of registered innovative start-ups firms, we compare a series of firm-level characteristics. Table A1 provides a comparison of our sample of treated start-ups with the set of firms that joined the policy until the end of 2015. Data on sales and equity, as well as the eligibility criteria and workforce composition, are collected by the Ministry of Economic Development and typically refer to the year in which the firms registered to the policy. The main difference that can be appreciated between the sample used in this paper and the full population of treated firms relate to the cohort of birth. In fact, since we need one pre-treatment year, our sample is mainly composed by firms that register in the early phase of the policy (i.e. during

[10] See Jiménez et al. (2012) for a seminal application of this methodology to Spanish Credit Register data.

[11] In line with the innovation literature, we resort to patent applications, instead of patents granted. This is motivated by the time needed to complete the patent granting procedure which would likely exceed our short post-treatment period.

[12] However, note that firms usually deposit their first balance-sheet after their first year of activity (i.e. firms born in 2013 deposit their first balance sheet in 2014). Hence there are very few firms observed for the cohort 2014.

[13] This is mainly due to the fact that, starting from 2014, most firms join the policy few months after their incorporation.

2013), therefore they belong to older cohorts with respect to the remaining registered start-ups. This also explains why the start-ups in our sample tend to have larger revenues and equity.

Table 1 reports summary statistics of observable firm characteristics for both treated and untreated firms. Treated firms are significantly smaller in terms of revenues, assets, and employees than untreated ones. These differences are consistent with past evidence showing that innovative firms pursue relatively more risky projects which take time before reaching the commercialisation phase (Gilbert et al., 2006).[14]

The higher propensity to innovate amongst the start-ups registered in the policy can be appreciated by their larger amount of intangible assets, reflecting higher expenditure on R&D and ownership of intellectual property such as patents.[15] Notably, around 10 per cent of treated firms are found to have filed at least one patent at the European Patent Office, while this share is practically zero among untreated firms. Additionally, a small share of treated firms (2 per cent) have received venture capital injections while, again, this share is almost zero for firms in the control group.

Finally, registered start-ups tend to be more concentrated in northern regions, whereas they are more likely to operate in manufacturing, ICT services, professional, scientific and technical services and, in particular, high-tech sectors.

4 Empirical strategy

We carry out a counterfactual evaluation exercise that compares the outcomes of targeted firms with the outcomes of a properly defined control group, in order to identify the *average treatment effect on the treated* (ATT) (Imbens and Rubin, 2015). The ATT corresponds to the difference in the outcome of the average treated firm with and without the policy, respectively. However, the identification of the ATT is challenging, as the counterfactual outcome - i.e. what would happen to the treated firms if they were never treated - is never observed, and firms that registered into the policy are likely to be systematically different from firms that did not, as the previous set of descriptive results has shown.

Part of these differences between treated and untreated firms are observable, e.g. from balance sheet variables. Assuming that their effect can be captured by a parametric formulation, these differences can be partialled-out by including the appropriate set of control variables in the model. It is nevertheless more likely that most of these differences are unobservable: for instance, data on R&D expenditures are not available for most firms, thus the eligibility criterion based on this measure cannot be discerned. Furthermore, treated firms may plausibly have a management that is better informed, have a more ambitious strategy, etc. In the econometric literature, these identification challenges are preferably addressed exploiting some discontinuities or quasi-experiments that introduce a degree of randomness in the probability of firms being treated. However, in the setting under scrutiny these are not readily available since the policy is relatively young and the number of treated firms is not very large, especially in the first years (2013 and 2014). A suitable empirical strategy should therefore control for selection bias as well as unobservable heterogeneity across treated and untreated firms. To this end, we adopt a conditional difference-in-differences (DID) approach (Heckman et al., 1998). This strategy combines matching methods with DID thus tackling selection on observables and unobservables at the same time. In particular,

[14] Hellmann and Puri (2000) show that early-stage innovative firms receiving venture capital do not have a sizable stream of revenues.

[15] Bronzini and Iachini (2014), who use the same balance-sheet data, show that intangible assets are correlated with firms' innovative capabilities. Indeed, intangible assets are mainly composed by R&D, patents, software and other intellectual property rights, licenses, trademarks whereas goodwill and start-up costs represents only a small share.

Table 1: Descriptive statistics of observable firm characteristics

	Untreated		Treated			Number of firms	
	Mean	SD	Mean	SD	Diff.	Untreated	Treated
Firm characteristics							
Revenues (1,000 euro)	806.41	1814.67	240.26	425.72	566.15*	67897	328
Value Added (1,000 euro)	200.48	901.01	60.54	220.26	139.94*	67533	325
Employees	6.14	17.53	3.28	4.00	2.87*	49576	170
Assets (1,000 euro)	873.26	6881.57	437.33	979.76	435.93*	67897	328
Fixed K (1,000 euro)	347.16	4759.97	192.73	807.22	154.43*	67897	328
Intangibles (1,000 euro)	67.11	531.36	101.88	207.76	-34.77*	67897	328
Patent (d)	0.00	0.05	0.10	0.31	-0.10*	67897	328
Leverage (%)	0.79	0.21	0.67	0.24	0.11*	67897	328
Equity (1,000 euro)	158.09	2075.43	150.46	539.51	7.63	67897	328
VC (d)	0.00	0.01	0.02	0.13	-0.02*	67897	328
Birth cohort							
Cohort 2009	0.19	0.39	0.14	0.35	0.05*	67897	328
Cohort 2010	0.23	0.42	0.23	0.42	-0.00	67897	328
Cohort 2011	0.25	0.43	0.35	0.48	-0.10*	67897	328
Cohort 2012	0.20	0.40	0.17	0.38	0.03	67897	328
Cohort 2013	0.13	0.33	0.10	0.30	0.03	67897	328
Cohort 2014	0.01	0.08	0.01	0.08	0.00	67897	328
Geographic area							
North West	0.29	0.45	0.35	0.48	-0.06*	67897	328
North East	0.20	0.40	0.33	0.47	-0.13*	67897	328
Center	0.24	0.43	0.21	0.41	0.03	67897	328
South	0.20	0.40	0.07	0.25	0.13*	67897	328
Islands	0.07	0.26	0.05	0.21	0.03*	67897	328
Sector							
Agriculture & Mining	0.00	0.00	0.00	0.00	0.00	67881	328
Manufacturing	0.22	0.41	0.27	0.44	-0.05	67881	328
Construction & Retail	0.49	0.50	0.08	0.28	0.41*	67881	328
Info & Communication Act.	0.06	0.24	0.30	0.46	-0.24*	67881	328
Professional & Scientific Act.	0.11	0.31	0.32	0.47	-0.22*	67881	328
Other services	0.12	0.33	0.03	0.17	0.09*	67881	328
High-tech	0.06	0.24	0.52	0.50	-0.46*	67897	328
Access to Credit							
Credit Application	0.38	0.02	0.55	0.04	-0.16*	233	61
Application Accepted	0.44	0.03	0.58	0.05	-0.14*	87	32
Credit Granted	12.13	0.09	12.19	0.19	-0.06	87	32

Notes: statistics computed using treated and control firms in our estimation sample. One observation per firm. The column "Diff" reports the difference between the means of treated and controls as well as the p-value of the t-test of equality of the two means. * $p < 0.05$.

propensity score matching (PSM) is used to create a control group among non-treated firms which are as similar as possible to treated firms with respect to observable characteristics in the year preceding the treatment (Caliendo and Kopeinig, 2008). Then we run DID regressions on the matched sample to test whether firms benefiting from the policy perform differently with respect to outcome variables, such as revenues, employment, and innovative capacity compared to other firms that did not registered as innovative start-ups. In the estimation we control for a broad set of fixed effects, including age and firm-level fixed effects, addressing potential omitted variable problems. While this procedure addresses differences in observables and unobservables that are time-invariant, a potential concern might stem from time-varying unobservables simultaneously influencing firms selection into treatment and outcomes. To this end, we make use of an instrumental variable strategy that exploits regional variation in treatment probabilities.

This section outlines the details behind our empirical approach. First, we describe the method to construct the counterfactual control sample by the PSM method. We then turn to the DID methodology to control for the potential confounding variables. Finally, we provide a description of the IV strategy.

4.1 Propensity score matching

The goal of the PSM is to pair beneficiary firms (treated group) with otherwise observationally identical firms that were not benefiting from the policy (control group). This requires to restrict the sample. First, treated firms included in the sample need to be observed for at least one year *before* entering the policy, in order to perform the balancing on pre-treatment variables. This implies that our sample is mainly composed by firms that were already active before the policy was enacted (i.e. late 2012). Second, we need to have a sufficient series of years for which we have non-missing data: currently, we restrict our focus to firms that provide at least three consecutive years of data. As a result of these restrictions, we end up by selecting firms born on or before 2014. We end up with a sample of 328 treated firms which are observed for three consecutive years, namely, a pre-treatment year and two post-treatment years.

We first estimate a logit model where we explain the probability of becoming an innovative start-up using a vector of variables that are likely to influence selection into treatment in the year preceding the registration to the "Start-up Act". The logit model takes the form:

$$\Pr\left(Treat_{it} = 1 | X_{it}\right) = \Phi\left(\beta_0 + \beta_1 X_{it-1} + \mu\right) \tag{1}$$

where Φ is the cumulative normal distribution, variable $Treat$ is a dummy determining if year t is the first year of treatment for firm i or not, matrix X contain a set of lagged firm-specific controls and matrix μ contains a set of fixed effects including cohort, industry, high-tech, geographic area and time. The choice of the time-varying explanatory variables intends to reflect both the differences detected in the descriptive statistics presented above as well as the eligibility criteria of the policy that are related to the innovative profile of the firms (i.e. R&D and patents). We do so by including variables capturing firms' innovative efforts: intangible assets (in logs) and the number of patent applications. We also add revenues (in logs) to reflect the fact that registered firms tend to be on average smaller than non-registering firms. Finally, we include a dummy variable capturing whether a start-up has obtained any venture capital financing in the period leading up to the treatment. The inclusion of pre-treatment patenting activity and venture capital backing is particularly important in our empirical strategy. A large body of literature has in fact documented that early-stage patents as well as venture capital funding provide a boost to firm performance and they are predictors of exceptional growth performance (Gompers and Lerner, 2001; Balasubramanian and Sivadasan, 2011; Kerr and Nanda, 2015; Farre-Mensa et al., 2017; Guzman and Stern, 2017). Additionally, patents can provide a certification effect that signals start-up quality to external investors (Long, 2002; Hsu and Ziedonis, 2013; Haeussler et al., 2014; Lahr and Mina, 2016) while venture capital can act as 'quality stamp' for follow-on financing stages (Megginson and Weiss, 1991; Hellmann and Puri, 2000; Hellmann et al., 2008). To accurately prevent these effects to be wrongly attributed to the policy because of misspecification of the propensity score, we do not just add these controls as covariates in the model, rather we match *exactly* on patenting and venture capital between treated and control firms.

The model is estimated out of a sample which includes the 328 treated firms plus all non-treated firms that have at least three consecutive years of non-missing observations. Results are reported in Tables 2. In line with descriptive statistics, and consistently with the eligibility criteria of the policy, start-ups with higher intangibility, with patents and in high-tech sectors display higher likelihood of registering. Furthermore, firms with lower revenues feature higher probability of registering, possibly suggesting that, due to their more innovative nature, it might take them more time to develop and commercialise their products and build a customer base (Gilbert et al., 2006). Additionally, start-ups that already obtained venture capital financing tend to display a considerably higher probability of joining the policy. Finally, it is worth to highlight that almost half of the explanatory power of the model is due to sectoral and high-tech dummies. This means that the likelihood of selecting into the policy stems in large part from heterogeneity across sectors. Firms in high-tech sectors, manufacturing, information and communication, and professional, scientific and technical activities tend to have a higher likelihood of registering as innovative start-up.

Table 2: Logit results

	(1) Treat$_t$	(2) Treat$_t$	(3) Treat$_t$	(4) Treat$_t$
Revenues$_{t-1}$	-0.761***	-0.712***	-0.734***	-0.686***
	(0.039)	(0.040)	(0.040)	(0.043)
Intangibles$_{t-1}$	0.279***	0.329***	0.299***	0.318***
	(0.032)	(0.032)	(0.032)	(0.033)
Patents$_{t-1}$	2.509***	2.625***	2.518***	1.443***
	(0.197)	(0.205)	(0.205)	(0.232)
VC$_{t-1}$	4.916***	4.980***	5.158***	4.339***
	(0.437)	(0.470)	(0.473)	(0.515)
Cohort 2010		0.263	0.246	0.398**
		(0.192)	(0.192)	(0.196)
Cohort 2011		0.737***	0.721***	0.812***
		(0.182)	(0.183)	(0.186)
Cohort 2012		1.248***	1.259***	1.346***
		(0.227)	(0.228)	(0.233)
Cohort 2013		1.954***	1.938***	2.053***
		(0.287)	(0.287)	(0.293)
Cohort 2014		2.106***	2.078***	2.174***
		(0.753)	(0.754)	(0.771)
North West			0.842***	0.496*
			(0.291)	(0.293)
North East			1.190***	0.898***
			(0.292)	(0.294)
Center			0.491	0.239
			(0.300)	(0.302)
South			-0.433	-0.487
			(0.348)	(0.351)
High-tech (d)				2.306***
				(0.169)
Constant	-2.712***	-3.003***	-3.434***	-5.948***
	(0.191)	(0.255)	(0.362)	(0.792)
Year FE	Yes	Yes	Yes	Yes
Sector FE	No	No	No	Yes
N	121043	121043	121043	121043
Pseudo-R^2	0.132	0.161	0.177	0.300
Log-likelihood	-1967.073	-1902.267	-1865.736	-1586.347
AIC	3944.146	3828.533	3763.471	3230.695

Notes: logit results obtained by estimating equation (1). The specification reported in column 4 is the one used to build the control group via PSM. Probabilistic matching is performed using lagged balance-sheet variables, patents, geographic area, sectoral and high-tech dummies. Exact matching is performed on year, cohort, VC and patents. Reference categories for cohorts, geographical areas and sectors are, respectively, cohort 2009, Islands. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

We exploit the propensity score estimated from the logit model to create a control group using a nearest neighbour matching algorithm (one neighbour with replacement), so that firms in the control group have similar characteristics to the treated group after matching.[16] As already mentioned exact matching is performed on pre-treatment patents and venture capital funding. Additionally, exact matching is employed on both the first year of treatment and cohort dummies in order to compare firms undergoing the same firm life-cycle dynamics as well as macroeconomic trends. Matching treated firms using an exact matching on the first year of treatment as well as cohort of birth is essential for two reasons: i) to prevent the PSM algorithm from matching a treated firm with either itself or considering beneficiary firms after treatment as controls; ii) to properly specify a pre- and post-treatment period for firms in the control

[16] We opted for this approach since it yields the best covariate balancing. However, estimation results obtained by employing Mahalanobis distance matching are qualitatively similar. See Section 6 for a more detailed discussion.

group since the timing of the potential treatment is not defined in their case. The latter condition is needed in order to properly estimate the difference-in-differences models described in the following section.

Finally, we impose a common support condition to satisfy the overlap assumption, dropping firms in the treatment group whose propensity score is higher than the maximum or lower than the minimum score among firms in the control group.

After matching, the matched treated and control groups should display similar characteristics in terms of observables during the pre-treatment period. In order to assess whether this is the case, we perform a balancing test, based on the comparison of the standardised differences of means and variances between treated and control firms. Prior research considers 0.20 as a reasonable threshold for acceptable mean standardised biases (Rubin, 2001; Stuart, 2010). From the results of the standardised differences before and after matching, displayed in Table 3, we can appreciate a considerable reduction in standardised differences thanks to the matching algorithm. All variables feature standardised mean differences close to 0 with the only exception of the high-tech dummy which still lies well below the 0.20 threshold. Summing up, the matching procedures successfully reduces imbalances between treated and untreated group.

Table 3: Standardised mean differences in raw and matched data

	Raw			Matched		
Means	Treated	Untreated	StdDif	Treated	Untreated	StdDif
Revenues_{t-1}	4.597	5.847	-0.936	4.611	4.527	0.063
Intangibles_{t-1}	3.054	2.439	0.332	2.974	2.883	0.049
Patents_{t-1}	0.086	0.002	0.434	0.071	0.071	0.000
VC (d)	0.037	0.000	0.273	0.003	0.003	0.000
North West	0.348	0.281	0.144	0.335	0.288	0.103
North East	0.329	0.198	0.300	0.339	0.332	0.015
Center	0.210	0.249	-0.091	0.214	0.240	-0.061
South	0.067	0.199	-0.397	0.070	0.093	-0.067
Islands	0.046	0.073	-0.115	0.042	0.048	-0.027
Agriculture	0.000	0.018	-0.193	0.000	0.000	0.000
Mining and quarrying	0.000	0.001	-0.039	0.000	0.000	0.000
Manufacturing	0.259	0.159	0.248	0.262	0.256	0.016
Electricity and gas	0.006	0.033	-0.195	0.006	0.003	0.023
Water supply	0.003	0.006	-0.049	0.003	0.000	0.047
Construction	0.006	0.118	-0.476	0.006	0.003	0.014
Wholesale and retail	0.058	0.259	-0.573	0.058	0.070	-0.036
Transportation	0.009	0.049	-0.238	0.010	0.003	0.038
Accommodation and food	0.000	0.085	-0.432	0.000	0.000	0.000
Information and communication	0.302	0.049	0.703	0.284	0.284	0.000
Financial and insurance services	0.003	0.009	-0.081	0.003	0.000	0.041
Real estate	0.000	0.013	-0.164	0.000	0.000	0.000
Professional, scientific activities	0.311	0.079	0.610	0.323	0.339	-0.042
Administrative services	0.021	0.045	-0.133	0.022	0.019	0.018
Education	0.009	0.012	-0.026	0.010	0.016	-0.063
Health and social services	0.006	0.023	-0.142	0.006	0.000	0.053
Arts and entertainment	0.006	0.040	-0.228	0.006	0.006	0.000
High-tech	0.521	0.055	1.200	0.508	0.460	0.123
Cohort 2009	0.140	0.337	-0.475	0.141	0.141	0.000
Cohort 2010	0.235	0.293	-0.132	0.236	0.236	0.000
Cohort 2011	0.348	0.193	0.352	0.345	0.345	0.000
Cohort 2012	0.171	0.119	0.146	0.166	0.166	0.000
Cohort 2013	0.101	0.054	0.174	0.105	0.105	0.000
Cohort 2014	0.006	0.003	0.050	0.006	0.006	0.000
2010	0.000	0.005	-0.099	0.000	0.000	0.000
2011	0.000	0.096	-0.462	0.000	0.000	0.000
2012	0.000	0.179	-0.660	0.000	0.000	0.000
2013	0.561	0.229	0.721	0.562	0.562	0.000
2014	0.253	0.269	-0.036	0.243	0.243	0.000
2015	0.186	0.222	-0.090	0.195	0.195	0.000

Notes: the table reports standardized mean differences for raw and matched samples. The control group is obtained via Propensity Score Nearest Neighbour Matching (1 neighbour with replacement) based on the logit regression reported in Table 2 in column 4.

4.2 Difference-in-differences

Treated and non-treated firms might still differ with regard to unobservable confounders which (i) are not perfectly correlated with observables, and (ii) are correlated with observables which are unbalanced between the treated and non-treated firms. In order to control for any residual differences between financed and rejected firms in unobservable firm characteristics that are fixed over time, we use the matched treated and control groups and exploit the longitudinal nature of our data to run a difference-in-differences estimation. By doing so, the effect of the "Start-up Act" is represented by the change in the difference in firms' outcome between recipient and non-recipient firms before and after the policy. Operationally, the estimation is based on the following model:

$$Y_{it} = \alpha + \beta_1 Post_{it} + \beta_2 (Treat_i \times Post_{it}) + X_{it} + \eta_i + \psi_{rt} + \tau_t + \epsilon_{it} \tag{2}$$

where Y_{it} is a given outcome variable observed for firm i at time t, $Post_{it}$ is a dummy defining the post-treatment period, $Treat_i$ is a dummy equal to one if the firm is registered to the policy, η_i are firm fixed-effects.[17] The vector X_{it} includes firm age and age squared (in logs) along with a set of province-level controls (GDP per capita, unemployment rate and population) in both levels and growth rates to account for heterogeneity in local economic conditions.[18] Furthermore, ψ_{rt} and τ_t indicate macro area-time and time fixed effects which absorb any shock in a particular macro region during a given year and macroeconomic factors, respectively. The coefficient of interest is β_2 which represents the ATT. Standard errors are clustered at firm level to account for the possibility of correlations across observations of the same firm in different years.

These absorb any shock to demand or to technology, which happens in a particular sector, in a particular geographic area during a given year.

We further assess the magnitude of the treatment impact for the two post-treatment years by estimating the extended DID specification:

$$\begin{aligned} Y_{it} = \alpha + \gamma_1 Post_{i,t=1} + \gamma_2 Post_{i,t=2} + \gamma_3 (Treat_i \times Post_{i,t=1}) + \\ \gamma_4 (Treat_i \times Post_{i,t=2}) + X_{it} + \eta_i + \psi_{rt} + \tau_t + \epsilon_{it} \end{aligned} \tag{3}$$

where γ_3 and γ_4 correspond to the ATT for years $t = 1$, and $t = 2$, respectively.[19]

4.3 Instrumental variable

The validity of the baseline empirical strategy combining PSM and DID rests on the conditional-independence assumption which requires that, once we condition on observables and fixed effects, participation in the policy is "as good as random". In our setting, this assumption can be violated if idiosyncratic exogenous shocks at the firm-level drive both the sudden increase in performance and the likelihood for a company to register into the policy. While the direction of the shock is not clear *a priori*, it is plausibly positive: differently from the start-ups in our control group, treated start-ups might enroll into the "Start-up Act"

[17] The standalone time-invariant variable $Treat_i$ is absorbed by the firm-specific fixed effects. Conversely, $Post_{it}$ is identified since firms register into the policy in different years.

[18] Data are drawn from Eurostat (Regional Statistics) available at https://ec.europa.eu/eurostat/web/regions/data/database.

[19] We perform a series of additional tests to ensure the robustness of our DID models into several directions. For instance, we cluster standard errors at a more aggregated level (e.g. province-level), we augment the baseline specification with a full set of sector-regions-year fixed effects to account for systematic differences across time within sectors and geographical areas. These are discussed in more detail in Section6.

due to an available investment project or idea, a change in growth orientation, or simply because they are better informed about the policy. We therefore develop an IV strategy based on geographical variation in the treatment probabilities (Brown and Earle, 2017; Caliendo and Tübbicke, 2019) to reach consistent estimates.

The IV approach exploits the spotty diffusion of the policy across the 110 Italian provinces (equivalent to the Eurostat "NUTS3" territorial classification). Figure 1 indeed shows that the share of firms that obtained the "innovative start-ups" status over the total number of firms that appear to be eligible based on age and size criteria is highly variables across the Italian territory, even within regions (the administrative spatial unit above the province in Italy). For instance, while northern regions are generally characterized by a higher share on average if compared with southern ones, there is also a notable within-region heterogeneity that is detected in both northern (e.g. Lombardy and Tuscany) and southern regions (e.g. Calabria and Sardinia). This is surprising, as provinces in Italy have limited political autonomy and their borders typically have very little economic meaning. Also, the differences do not seem to reflect a urban/rural divide, as provinces hosting the largest urban agglomerations (Rome, Milan, Turin, Naples) show different values among them, and do not stand out compared to other provinces.

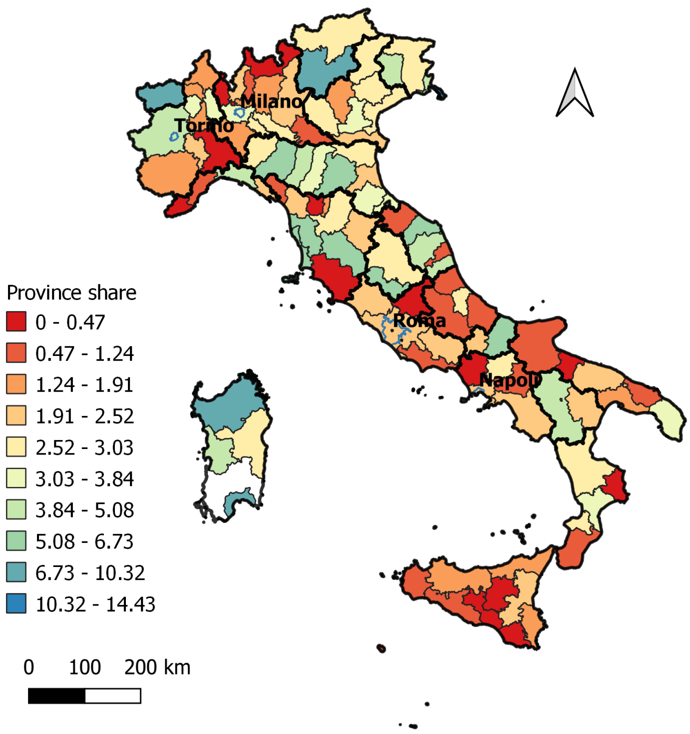

Fig. 1: Share of treated firms by province

Notes: the map depicts the share of treated firms across Italian provinces in 2013. This is computed as the ratio of the number of treated firms over all firms below 5 years of age and 5 million revenues in each province. Bold lines represent regional borders.

Discussions with practitioners and experts revealed that this variability is at least partially due to a specific provision of the program, which requires local Chambers of Commerce (CC) to deliberate on

whether the start-up official mission statement, registered at incorporation, is "innovation oriented". CC territorial competence fully overlaps provinces' areas. Given its discretionary nature, this requirement has been interpreted rather differently by the boards of the CC during the first years of policy. Reflecting this, on February 14th 2017, an official clarification document ("circolare") of the Italian Ministry of Economic Development asserts that the requirement related to the mission statement is the one that attracted most requests for clarifications.[20] The same document provides clarifications on the interpretation of that provision aimed at ensuring more homogeneity in the judgement across the Italian territory. Therefore, for the policy period under scrutiny in this paper (2013-16), the heterogeneous approach of local CC boards in deciding whether a mission statement is innovation-oriented may be used to build a valid instrumental variable, i.e., a variable that is correlated with the endogenous treatment, but which has no independent effect on the dependent variables (conditional firm fixed effects that absorb all time-invariant province characteristics and additional time-variant controls). While we do not observe the actual mission statements and we cannot therefore assess the degree of selectivity of CC boards, we can use the share of registered start-ups at the province (CC) level as a valid proxy. As discussed above, this variable does indeed show a fair amount of variability across provinces.
We then compute the share of treated firms over all firms below 5 years of age and 5 million revenues in each province in 2013 ($Share_p$), which is arguably the year when arbitrary choices by local Chamber of Commerce were more common, and use it interacted with $Post_t$ as an instrument for $Treat_i \times Post_{it}$ in (2).

To test the validity of our instrument and to increase the efficiency of the IV, we exploit the approach developed by Lewbel (2012), which generates additional instrumental variables from the error structure of the first-stage. To get an intuition of this methodology, consider the initial first-stage equation:

$$(Treat_i \times Post_{it}) = \alpha + \gamma_1 Post_{it} + \gamma_2 (Share_p \times Post_{it}) + X_{it} + \eta_i + \psi_{rt} + \tau_t + \epsilon_2 \tag{4}$$

Lewbel (2012) shows that, under the assumptions that the error term in the first stage is heteroskedastic and that a subset Z_{it} of controls X_{it} are uncorrelated with the product of the error terms in the first and second stage, one can construct additional instruments of the form $(Z_{it} - \bar{Z}_i t)\epsilon_2$ to be included in the first-stage equation. The first assumption is directly testable with a Breusch-Pagan test, and in our case the null of homoskedasticity is largely rejected ($p < 0.00001$); the second assumption can be test with a Hansen-type test, as the the model is over-identified: in our case it safely fails to reject the null that overidentifying restrictions are valid.

The Lewbel estimator readily applies to the case when when the endogenous variable is binary (Lewbel, 2018) and it has been recently applied in similar settings by Heim et al. (2017), Comin et al. (2019), and Czarnitzki et al. (2020). As stressed by Lewbel (2012, 2018), this approach is particularly appropriate when other instruments with a weaker first stage statistics are available, as is the case here. Moreover, and most importantly in our setting, the instruments created allow to perform an over-identification test on the exclusion restriction imposed to the share of treated firms in 2013 (in particular, with an Hayashi C test).

[20] Ministero dello Sviluppo Economico, Circolare N. 3696/C, 14 February 2017, available on-line (in Italian) at https://www.mise.gov.it/images/stories/normativa/Circolare-startup-e-PMI-innovative-14-02-2017.pdf.

As for the DID estimation, we also investigate the effects of the policy separately for the first two treatment periods, instrumenting $(Treat_i \times Post_{i,t=1})$ and $(Treat_i \times Post_{i,t=2})$ with $(Share_i \times Post_{i,t=1})$ and $(Share_i \times Post_{i,t=2})$.

A potential flaw in the validity of the instrumental variable is that the decisions of the CC boards may in part reflect local economic conditions. To take this into account, as in the DID specifications we include the following time-variant controls at province-level: unemployment rate, total population, and GDP per capita. These are inserted in the models in log terms and annual log differences to take into account the heterogeneity in local economic conditions across provinces in both levels and trends. In addition, the inclusion of firm fixed effects in all specifications implies that all time-invariant province characteristics over the six-year period are controlled for (as the few firms that change province during the period of analysis are excluded from the sample).

Aside from providing statistical evidence of its validity through Hansen-type tests, in Section 6 we also test the robustness of our IV strategy in several additional directions: we provide a falsification test using data from years preceding the introduction of the policy, we augment the empirical specification with additional control variables to test for stability of our estimates, and we exploit alternative definitions of the instrument.

5 Results

5.1 The effects of the policy on firm activity

This section displays the results obtained following the empirical strategy outlined in the previous section. We start by reporting the baseline DID and subsequently move to the IV estimates.

Panel A of Table 4 shows the results of equations (2) and (3) for various dependent variables without using the instrumental variables. The first two estimated effects are related to output: the policy induces a statistically significant increase in employment (15 per cent) and value added (28 per cent). The impact on fixed capital is large, positive, and significant, ultimately driving the positive impact on assets (39 and 20 per cent, respectively). The remaining rows present the treatment effects across the first two year of the policy. They show that the overall impact is driven by the effects accrued during the second year since registering to the start-up registry. This is especially the case for revenues, for which we observe a non statistically significant effect during the first year while the effect is large and statistically different from zero during the second year (20 per cent increase).

Panel B of Table 4 reports estimation results concerning the effects of the policy on firms' financial structure and innovative behaviour. Concerning the latter dimension, the rise in fixed capital mainly stem from intangible assets (66 per cent increase), indicating that firms tend to allocate more resources to innovative activities.

We also find that the policy improves the probability of applying for a patent to the EPO by 2 percentage points in the three years after joining the policy. With respect to the financial structure, we find a positive effects on the bank debt over assets, and a positive effect on equity which is consistent with the increase in total financial assets. Finally, we report the effects on firm failure on the subsequent three years since joining the policy. We find that the "Start-up Act" does not have any statistically significant effect on firm failure.

Results from the two-stage least square (2SLS) estimations using the IV strategy described in Section 4 broadly confirm the baseline findings. Yet, the IV estimates suggest an overall smaller effect of the "Start-up Act" with coefficients that are generally of lower magnitude with respect to the baseline estimates.

Table 4: DID estimates

Panel A	Revenues	Employees	Assets	Fixed K	Value Added
Treat x Post	0.077	0.152***	0.198***	0.385***	0.275***
	(0.070)	(0.054)	(0.041)	(0.066)	(0.081)
Treat x Post$_{t=1}$	-0.044	0.113**	0.120***	0.282***	0.161**
	(0.066)	(0.053)	(0.037)	(0.061)	(0.081)
Treat x Post$_{t=2}$	0.200**	0.205***	0.276***	0.490***	0.395***
	(0.085)	(0.065)	(0.051)	(0.077)	(0.094)
Post	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
N	1836	913	1836	1836	1569
Panel B	Leverage	Equity	Intangibles	Patent	Failure
Treat x Post	0.020**	0.177***	0.661***	0.020**	0.004
	(0.010)	(0.057)	(0.085)	(0.008)	(0.013)
Treat x Post$_{t=1}$	0.009	0.111**	0.489***	0.020**	0.004
	(0.010)	(0.053)	(0.079)	(0.008)	(0.013)
Treat x Post$_{t=2}$	0.031**	0.245***	0.835***		
	(0.013)	(0.073)	(0.102)		
Post	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
N	1836	1836	1836	1224	1224

Notes: in each panel the first row reports average treatment effects obtained via equation (2). The second and third rows report heterogeneous treatment effects across the first two years obtained via equation (3). Continuous dependent variables are in log form and are winsorized at 2% on both sides of the distribution to mitigate the influence of outliers. Patent is a dummy variable indicating whether firms apply for patents in the three years after treatment. Failure indicates whether a firm fails in the three years after treatment. Models include firm, geographic area-time and time fixed effects, (log) age and age squared, province-level population, unemployment, GDP per capita, in both levels and annual growth rates. Standard errors clustered at the firm-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

This indicates the presence of a positive shock, that is, start-ups registering the the “Start-up Act” tend to have unobserved prospects that are relatively better than those of control start-ups. In more detail, we observe that the coefficient on patenting preserves the positive sign but decreases by roughly one half and loses statistically significance. A similar pattern emerges for employment and leverage, yet a positive and statistically significant effect is still detectable after the first year of the registration into the policy. Table 5 displays the results for our set of firm-level performance measures along with first stage F-statistic and Hansen J test for over-identifying restrictions. The first stage F-statistic is always considerably above the critical lower bound suggested by Stock et al. (2002) whereas the Hansen J does not reject the over-identifying restrictions except for intangibles.

5.2 Does the policy alleviate financial frictions?

The positive effects of the “Start-up Act” reported in the previous section imply that some kind of market friction is impeding innovative start-ups to exploit their growth opportunities, absent the policy. An extensive literature has documented that financial frictions are particularly problematic for small and young innovative firms (Hall and Lerner, 2010; Kerr and Nanda, 2015). This is one of the reasons why governments support these type of firms, that is, to help financially vulnerable firms accessing external resources to carry out their investment projects they otherwise would not be able to pursue. The “Start-up Act” encompasses two instruments explicitly aimed at easing access to external finance: (i) a fiscal incentive for venture capitalists and outside investors who invest in equity capital and (ii) privileged access to a public scheme bank loan program. The former entails a tax incentive to private investors who invest in start-ups amounting to 30 per cent, with maximum limit of 1.8 million euros. The latter allows innovative newborn firms a fast-track, simplified and free-of-charge access to the Guarantee Fund

Table 5: IV estimates on firm size and activity measures

Panel A	Revenues	Employees	Assets	Fixed K	Value Added
Treat x Post	0.046	0.096	0.163***	0.365***	0.261***
	(0.075)	(0.061)	(0.040)	(0.074)	(0.095)
Treat x Post$_{t=1}$	-0.056	0.083	0.142***	0.260***	0.243**
	(0.057)	(0.072)	(0.043)	(0.071)	(0.103)
Treat x Post$_{t=2}$	0.179**	0.196***	0.263***	0.460***	0.427***
	(0.077)	(0.060)	(0.055)	(0.088)	(0.106)
1st stage F-stat	537.228	158.636	537.228	537.228	384.847
Hansen (p-value)	0.132	0.209	0.637	0.268	0.121
N	1836	975	1836	1836	1598
Panel B	Leverage	Equity	Intangibles	Patent	Failure
Treat x Post	0.014	0.132**	0.626***	0.009	0.002
	(0.009)	(0.067)	(0.081)	(0.006)	(0.010)
Treat x Post$_{t=1}$	-0.003	0.182***	0.490***		
	(0.012)	(0.070)	(0.080)		
Treat x Post$_{t=2}$	0.027*	0.278***	0.870***		
	(0.014)	(0.094)	(0.116)		
1st stage F-stat	537.228	537.228	537.228	94.794	94.794
Hansen (p-value)	0.811	0.572	0.059	0.576	0.431
N	1836	1836	1836	1224	1224

Notes: in each panel the first row reports the IV estimates. The second and third rows report the IV estimates across the first two years of the policy. Models include firm and geographic area-time and time fixed effects, the post dummy, (log) age and age squared, province-level population, unemployment, GDP per capita, in both levels and annual growth rates. Standard errors clustered at the province-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

(Fondo di Garanzia), a State Fund that facilitates access to credit through guarantees on bank loans. The guarantee covers up to 80 per cent of the bank loans granted to innovative start-ups, up to a maximum of 2.5 million euro, and it is provided through a simplified fast-track procedure.[21]

While venture capital investors are generally considered to better match the needs of innovative startups if compared to other financial operators (Lerner, 1995; Gompers, 1995; Hellmann and Puri, 2002; Denis, 2004), the share of firms financed by venture capital funds is considerably small even where equity industries are well-developed (Mulcahy, 2013; Gornall and Strebulaev, 2015). In line with this, Robb and Robinson (2014) show that external bank finance is an important source of startup capital in the United States, even for high-potential startups that might be engaged in innovation and who don't have any collateral to pledge.[22] The importance of bank debt for innovative startups is even more pronounced in bank-based systems like Italy inducing this type of firms to rely on bank debt as the primary source of external financing (Colombo and Grilli, 2007; Rodriguez-Palenzuela et al., 2013).

We exploit the Credit Register data to study whether the policy eases the access to credit markets by innovative start-ups. We focus on firms who ever appeared on the register over the period of analysis (leaving us with around 40% of the initial sample). We start by studying whether the policy increases the probability that firms apply for a loan. Indeed, firms may have stronger incentive to apply for a loan, foreseeing a higher probability of being accepted thanks to the public guarantee. In line with this

[21] Fast-track refers to the fact that their files are given priority over those concerning other companies. Unlike other companies, the SME Guarantee Fund does not evaluate any balance sheet or business plan submitted by the concerned start-up, i.e. the guarantee is provided automatically, based on the merit of credit evaluation carried out by the lending bank.

[22] Other studies consistent with the importance of the credit channel are Chava et al. (2013) and Cornaggia et al. (2015) who show that banking deregulations in the US had a particularly beneficial effect on innovative small private firms.Cole and Sokolyk (2018) report that start-ups using debt are significantly more likely to survive and achieve higher revenue if compared with all-equity start-ups.

conjecture, the IV result of Table 6 shows that the policy induces a 13 percent higher probability of applying for credit.

Second, we study the probability that the first loan application made by the firm is accepted by the bank. Focusing on the first application ever made by the startups during the period of analysis is particularly relevant for at least two reasons. It represents the moment in which information frictions between banks and firms are stronger, as the firm lacks any credit history. Moreover, according to the regulation of the Credit Register, the result of a credit application remains common knowledge among banks for up to six months: the result of the first application may have significant spillover effects on subsequent applications by the firm to other banks (Albertazzi et al., 2014). These spillover effects may also generate severe identification issues (due to endogenous selection of applicants) in studying subsequent applications. Results show a significant effect on the probability of acceptance, which increases by more than 36% as a result of the policy. Conversely the quantity of credit granted, conditional on having an application accepted, does not increase: the positive effect identified by the simpler PSM-DID model is found to be spurious once we implement the IV strategy.

Table 6: Effect of the policy on access to credit

	Prob. of Applying		Prob. of Acceptance		(Log) Quantity of Credit	
	DID	IV	DID	IV	DID	IV
Treat x Post	0.031	0.132**	0.310**	0.366***	0.483***	-0.168
	(0.098)	(0.062)	(0.089)	(0.151)	(0.170)	(0.209)
1st stage F-stat		161.233		35.732		74.001
Hansen (p-value)		0.275		0.408		0.382
N	731	731	215	215	215	215

Notes: Estimation results using DID and IV estimations. All regressions include firm, area-time fixed effects, the post dummy, (log) age and age squared, province-level population, unemployment, GDP per capita, in both levels and annual growth rates. Standard errors clustered at the firm-level for the DID models and at the province-level for the IV. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

We next examine the impact of the second instrument aimed at favouring access to external finance, namely, the fiscal incentives for venture capitalists who invest in the equity of innovative start-ups. A large literature has in fact documented that venture capital is beneficial to the performance of early stage innovative entrepreneurs (Kortum and Lerner, 2000; Hellmann and Puri, 2000; Bertoni et al., 2011; Puri and Zarutskie, 2012; Bronzini et al., 2019). To this end, we estimate the effect of the "Start-up Act" on venture capital financing using AIFI data on the universe of VC deals in Italy. In more detail, the dependent variable employed is a (time-invariant) dummy indicating with 1 a start-up experiences a VC injection by 2018, and 0 otherwise. Table 7 reports the results of the DID model (column 1) and the IV model (column 2) and indicate that the policy increases the likelihood of obtaining VC by 3.1-3.5 percentage points. This represents a substantial effect in economic terms given that the average probability of VC financing for treated firms before the policy was 2%.

To sum up, incentives to both debt and equity represent effective policy tools facilitating access to bank and equity finance for a segment of the private sector usually characterized by difficulties in securing external resources.

6 Robustness

This section presents an alternative identification strategy along with a series of sensitivity tests of our baseline model. We first discuss a falsification test of our an IV approach and then the robustness tests

Table 7: Effect of the policy on VC financing

	Prob. of VC financing	
	DID	IV
Treat x Post	0.035***	0.031**
	(0.010)	(0.013)
1st stage F-stat		57.019
Hansen (p-value)		0.287
N	1244	1224

Notes: VC is a dummy variable indicating whether a firm receives venture capital financing by 2018. Standard errors clustered at the firm-level for the DID estimation and at the province-level for the IV. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

for both the matching strategy as well as for alternative specifications of the baseline and IV models.

6.1 Falsification

We run a falsification test to corroborate the validity of the exclusion restrictions for the exogenous instrument based on province-level shares of treated companies. The falsification test consists in regressing the full set of outcome variables on the instrumental variable using a sample that predates the implementation of the policy. The specification of these regressions are identical to those employed to obtain the baseline estimates, and therefore mirrors the second-stage estimates of the 2SLS. The intuition underlying this exercise is the following: if the instrumental variable is affecting the outcome only via the endogenous treatment, then it should be uncorrelated with the outcomes before the treatment was designed. The falsification regressions show that the coefficients on the instrumental variable are never significant, supporting the assumption that is correctly excluded from the second stage of the 2SLS estimates.

The first step of the falsification test is the construction of the dataset. We use the same sources and variables that we used for the main sample, but instead of extracting data for the time period 2009-2016, we extract data for the period 2006-2011. We then restrict the sample to eligible firms, following exactly the same procedure used for the main sample, and we randomly allocate treatment to 328 firms, replicating the same timing of entry into treatment observed in the main sample, but with a five-year shift: entry into treatment is takes place in 2013-2014-2015 in the main sample, and in 2008-2009-2010 in the falsification sample. The proportion of firms entering into treatment in the first, second, or third year, respectively, is identical in the two samples.

The second step consists in replicating the PSM. This procedure is straightforward as it mirrors the approach used for the main sample. Unsurprisingly, given that treatment is random, the matching delivers on average fully balanced sub-samples and the ATT is equal to zero. The falsification sample is restricted to treatment and control groups resulting from the matching.

The third step consists in regressing each outcome variables used in the baseline estimates on the instrumental variable described in Section 4. These regressions contain the same set of control variables and fixed effects of the baseline estimates, and apply the same econometric methods.

Steps one to three are repeated 100 times, and at each time the t-test on the significance of the coefficient on the instrumental variable is stored. We therefore collect the full distribution of coefficients under different random allocations of the treatment variable in step one. Applying a 5% confidence interval, we consider the IV strategy validated by the falsification test if the null hypothesis is rejected

Table 8: Falsification test - summary of Monte-Carlo iterations

Dependent variable	Share of *p-value* < 0.05	Mean *p-value*
Revenues	0.05	0.48
Employment	0.03	0.52
Assets	0.00	0.52
Equity	0.04	0.53
Value added	0.02	0.43
Leverage	0.04	0.52
Net worth	0.04	0.54
Intangibles	0.04	0.52

Notes: For each dependent variable, the table reports the average value of the t-test and of the p-value across 100 iterations of the falsification tests.

with a 5% confidence interval at least 95% of times. The results, reported in Table 8, show that indeed none of the coefficients is statistically significant more than 5 per cent of times.

6.2 Alternative matching and specifications

King and Nielsen (2019) argue that PSM can be subject to a series of shortcomings compared with other matching techniques. We therefore employ Mahalanobis distance matching (MDM) to build an alternative matched sample. Table A2 displays balancing tests while estimation results are reported in Table A3. Balancing tests show good properties and results do not show particular variations if compared with the models estimated using the control group built via PSM.

We also test the robustness of our results by augmenting our specifications with additional control variables. In the baseline models we control for several covariates capturing local economic conditions in both levels and growth rates. To further account for differences across provinces, we consider controls addressing potential heterogeneity in business dynamics in innovative sectors. Indeed, it might well be that provinces with similar economic conditions feature different industrial structures with a more or less pervasive presence of innovative start-ups. To this end, we employ province-level data from Eurostat which refer to the three sectors whit the highest shares of registered start-ups, namely, manufacturing, ICT, and professional and scientific activities.[23] In more detail, the variables are: the number of new enterprises, the number of active firms, the share of 3 years' old firms over the business population, the average size of new enterprises, and the number of high-growth firms. Results of these augmented specifications are reported in Table A5. While estimates tend to display similar patterns, we observe that the positive effect on employment and leverage gains statistical significance at the 10% in the IV models. Overall, these results provide an assessment that is largely consistent with the one stemming from our baseline models.

The instrument used in the main specification is the share of registered innovative start-ups over the number of active firms younger than 5 years old and 5 million revenues at the province-level. One potential shortcoming of such shares is that the denominator captures firms that are indeed young and small but operating in sectors with a low innovation propensity and, hence, lower chances to feature start-ups with the required eligibility criteria. We test the robustness of the IV estimates by employing an alternative definition of such shares. We calculate the shares by using a different denominator: the number of active firms in those sectors featuring the highest shares of registered start-ups. These are manufacturing, ICT, and professional and scientific activities and together they account for roughly 90%

[23] Province-level (NUTS3) data are drawn from Eurostat (Business Demography Statistics), and are accessible at https://ec.europa.eu/eurostat/web/structural-business-statistics/entrepreneurship/business-demography.

of all registered start-ups (see Table 1). Opting for this alternative definition of the shares does not imply any meaningful change in estimation results (see Table A7).

7 Conclusion

The paper provides a comprehensive evaluation on the effects of the Italian "Start-up Act". The extensive regulatory framework, implemented at the end of 2012, aims to create a more favourable environment for small innovative start-ups. The examination of the "Start-up Act" is a useful exercise to inform policies for innovative entrepreneurship across the globe, particularly since the support to innovative start-ups is a policy priority in many countries. In fact, during the last decades, a number of countries have been adopting national Start-up frameworks though evidence on these initiatives remains scarce (Audretsch et al., 2019).

The analysis shows that the policy triggers an overall positive effect on firm performance, especially concerning the input side. Indeed, we find that treated start-ups exhibit a substantial increase in terms of total assets, driven by fixed assets, and in particular intangible assets which are an essential input of the innovative process and are usually suffer from financial frictions. Furthermore, the results indicate positive effects in terms of value added and employment and (albeit statistically weaker) effect on employment and revenues. Conversely, we do not find any effect on patenting propensity nor survival chances.

The paper also documents that the policy has been effective in curbing the financial frictions characterizing innovative start-ups. On one hand, thanks to the provision of a public guarantee scheme, their access to bank debt improves, as measured by the higher probability of loan application acceptance and by the larger quantity of credit received upon acceptance. These firms also obtain a lower interest rate on credit. On the other, the presence of equity incentives, triggers an increase in the probability of receiving venture capital injections.

These results show the importance of providing complementary measures to support the access to external finance for innovative start-ups, in order to let them select themselves into the financing instruments that are more appropriated for their expansion strategy.

While the empirical analysis finds positive effects from the use of guaranteed loans, it is nevertheless important to strike the right balance between equity and debt financing. While start-ups appear to benefit from the public guarantee fund for bank loans, the economic literature suggests that equity financing is more suited to high-growth and high-risk innovative start-ups. The provision of debt guarantees should therefore be closely monitored and evaluated, not only because it employs a substantial amount of public capital, but also in order to avoid the risk that easier access to credit, relative to equity, might induce high potential start-ups to opt for a slower growth path, based on debt financing rather than on equity injections.

The incentives for equity financing also deserves further discussion. While their provision seems effective in stimulating venture capital injections, it is nonetheless insufficient to kick-start a thriving venture capital ecosystem in Italy where this industry is still lagging behind those of other OECD countries. Therefore, it could be useful to assess the need for further public investments in VC by exploring a bolder commitment and sponsorship of existing and newly-established government-backed matching funds and funds-of-funds. Recent policy developments appear to go in such direction.[24]

Finally, it is worth highlighting a series of interesting aspects that are not addressed in the present analysis and that future research could focus on. The first is the long-term effects of the "Start-up Act"

[24] https://www.mise.gov.it/index.php/en/202-news-english/2039363-the-national-innovation-fund-unveiled.

on the economic performance of beneficiary firms. The second one is the potential impact that the policy has on lowering barriers to entry thus stimulating the establishment of innovative start-ups that would have not been born without the policy. Assessing the presence of the latter effects would be of great importance although empirically challenging.

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8 Online Appendix

Table A1: Registered start-ups vs registered start-ups in the sample

	(1) All start-ups	(2) Matched start-ups
Sales classes		
0-100,000	0.42	0.25
100,001-250,000	0.27	0.30
250,001-500,000	0.15	0.19
500,001+	0.16	0.27
Equity classes		
0-100,000	0.89	0.83
100,001-250,000	0.06	0.09
250,001-500,000	0.03	0.04
500,001+	0.02	0.04
Birth cohort		
Cohort 2009	0.05	0.14
Cohort 2010	0.08	0.23
Cohort 2011	0.13	0.34
Cohort 2012	0.18	0.17
Cohort 2013	0.27	0.10
Cohort 2014	0.29	0.01
Registration		
Year 2013	0.42	0.55
Year 2014	0.39	0.26
Year 2015	0.19	0.19
Industry		
Agriculture	0.00	0.00
Retail	0.05	0.05
Manufacturing	0.22	0.28
Services	0.72	0.67
Tourism	0.00	0.00
Geographic area		
North West	0.34	0.35
North East	0.27	0.33
Center	0.21	0.21
South	0.13	0.07
Islands	0.05	0.05
Elegibility Criteria		
R&D expenditures	0.64	0.63
Human Capital	0.31	0.34
Intellectual Property	0.24	0.24
Labour force composition		
Women majority	0.14	0.13
Youth majority	0.23	0.23
Foreign majority	0.02	0.02
N	3560	328

Notes: Column 1 reports summary statistics for all start-ups registered on or before 2016 and born up to 2015. Column 2 refers to start-ups registered on or before 2016 and born up to 2015 contained in our final sample. The latter refers to all start-ups with non-missing values for revenues in Cerved. Elegibility criteria do not add to 1 since firms can report to have more than one. Labour force composition does not add to 1 since firms can report to have none.

Table A2: Standardised mean differences in raw and matched data - MDM

	Raw			Matched		
Means	Treated	Untreated	StdDif	Treated	Untreated	StdDif
Revenues$_{t-1}$	4.597	5.847	-0.936	4.610	4.858	-0.186
Intangibles$_{t-1}$	3.054	2.439	0.332	2.984	3.012	-0.015
Patents$_{t-1}$	0.086	0.002	0.434	0.070	0.070	0.000
VC$_{t-1}$	0.037	0.000	0.273	0.006	0.006	0.000
North West	0.348	0.281	0.144	0.341	0.354	-0.027
North East	0.329	0.198	0.300	0.338	0.325	0.029
Center	0.210	0.249	-0.091	0.217	0.223	-0.015
South	0.067	0.199	-0.397	0.064	0.057	0.019
Islands	0.046	0.073	-0.115	0.041	0.041	0.000
Agriculture	0.000	0.018	-0.193	0.000	0.000	0.000
Mining and quarrying	0.000	0.001	-0.039	0.000	0.000	0.000
Manufacturing	0.259	0.159	0.248	0.264	0.271	-0.016
Electricity and gas	0.006	0.033	-0.195	0.006	0.006	0.000
Water supply	0.003	0.006	-0.049	0.003	0.003	0.000
Construction	0.006	0.118	-0.476	0.006	0.006	0.000
Wholesale and retail	0.058	0.259	-0.573	0.057	0.057	0.000
Transportation	0.009	0.049	-0.238	0.010	0.010	0.000
Accommodation and food	0.000	0.085	-0.432	0.000	0.000	0.000
Information and communication	0.302	0.049	0.703	0.283	0.283	0.000
Financial and insurance services	0.003	0.009	-0.081	0.003	0.006	-0.040
Real estate	0.000	0.013	-0.164	0.000	0.000	0.000
Professional, scientific activities	0.311	0.079	0.610	0.322	0.312	0.025
Administrative services	0.021	0.045	-0.133	0.022	0.022	0.000
Education	0.009	0.012	-0.026	0.010	0.010	0.000
Health and social services	0.006	0.023	-0.142	0.006	0.006	0.000
Arts and entertainment	0.006	0.040	-0.228	0.006	0.006	0.000
High-tech	0.521	0.055	1.200	0.506	0.497	0.025
Cohort 2009	0.140	0.337	-0.475	0.143	0.143	0.000
Cohort 2010	0.235	0.293	-0.132	0.236	0.236	0.000
Cohort 2011	0.348	0.193	0.352	0.344	0.344	0.000
Cohort 2012	0.171	0.119	0.146	0.166	0.166	0.000
Cohort 2013	0.101	0.054	0.174	0.105	0.105	0.000
Cohort 2014	0.006	0.003	0.050	0.006	0.006	0.000
2010	0.000	0.005	-0.099	0.000	0.000	0.000
2011	0.000	0.096	-0.462	0.000	0.000	0.000
2012	0.000	0.179	-0.660	0.000	0.000	0.000
2013	0.561	0.229	0.721	0.564	0.564	0.000
2014	0.253	0.269	-0.036	0.242	0.242	0.000
2015	0.186	0.222	-0.090	0.194	0.194	0.000

Notes: the table reports standardized mean differences for raw and matched samples. The control group is obtained via one-to-one Mahalanobis Distance Matching (1 neighbour with replacement).

Table A3: DID estimates based on MDM control group

Panel A	Revenues	Employees	Assets	Fixed_K	Value_Added
Treat x Post	0.108	0.176***	0.189***	0.455***	0.201**
	(0.070)	(0.049)	(0.039)	(0.061)	(0.084)
Treat x Post$_{t=1}$	0.029	0.131***	0.122***	0.359***	0.127
	(0.067)	(0.048)	(0.035)	(0.055)	(0.084)
Treat x Post$_{t=2}$	0.188**	0.235***	0.256***	0.551***	0.278***
	(0.083)	(0.061)	(0.049)	(0.075)	(0.097)
Post	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
N	1842	954	1842	1842	1591
Panel B	Leverage	Equity	Intangibles	Patent	Failure
Treat x Post	0.013	0.127**	0.596***	0.019**	-0.006
	(0.010)	(0.059)	(0.083)	(0.008)	(0.014)
Treat x Post$_{t=1}$	0.009	0.090	0.449***		
	(0.010)	(0.058)	(0.076)		
Treat x Post$_{t=2}$	0.017	0.165**	0.744***		
	(0.013)	(0.071)	(0.102)		
Post	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
N	1842	1842	1842	1228	1228

Notes: Estimation results using the MDM control group. Standard errors clustered at the firm-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: IV estimates based on MDM control group

Panel A	Revenues	Employees	Assets	Fixed_K	Value_Added
Treat x Post	0.110*	0.184***	0.149***	0.419***	0.188**
	(0.060)	(0.052)	(0.038)	(0.051)	(0.095)
Treat x Post$_{t=1}$	-0.011	0.120**	0.131***	0.307***	0.160
	(0.071)	(0.048)	(0.042)	(0.062)	(0.104)
Treat x Post$_{t=2}$	0.134*	0.239***	0.243***	0.547***	0.261***
	(0.074)	(0.056)	(0.045)	(0.075)	(0.100)
1st stage F-stat	469.584	131.227	469.584	469.584	506.294
Hansen (p-value)	0.046	0.298	0.731	0.324	0.489
N	1842	1010	1842	1842	1625
Panel B	Leverage	Equity	Intangibles	Patent	Failure
Treat x Post	0.018*	0.094	0.570***	0.007	-0.007
	(0.009)	(0.075)	(0.076)	(0.007)	(0.011)
Treat x Post$_{t=1}$	-0.005	0.189**	0.494***		
	(0.012)	(0.083)	(0.092)		
Treat x Post$_{t=2}$	0.012	0.191**	0.807***		
	(0.013)	(0.088)	(0.114)		
1st stage F-stat	469.584	469.584	469.584	106.843	106.843
Hansen (p-value)	0.527	0.501	0.149	0.581	0.476
N	1842	1842	1842	1228	1228

Notes: Estimation results using the MDM control group. Standard errors clustered at the firm-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: DID estimates using augmented specification

Panel A	Revenues	Employees	Assets	Fixed_K	Value_Added
Treat x Post	0.070	0.162***	0.194***	0.381***	0.272***
	(0.069)	(0.053)	(0.041)	(0.066)	(0.081)
Treat x Post$_{t=1}$	-0.053	0.121**	0.116***	0.279***	0.164**
	(0.065)	(0.053)	(0.037)	(0.062)	(0.081)
Treat x Post$_{t=2}$	0.195**	0.220***	0.272***	0.484***	0.387***
	(0.085)	(0.064)	(0.051)	(0.077)	(0.093)
Post	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
N	1836	913	1836	1836	1569
Panel B	Leverage	Equity	Intangibles	Patent	Failure
Treat x Post	0.020**	0.176***	0.655***	0.019**	0.001
	(0.010)	(0.057)	(0.084)	(0.008)	(0.013)
Treat x Post$_{t=1}$	0.010	0.108**	0.485***		
	(0.010)	(0.053)	(0.079)		
Treat x Post$_{t=2}$	0.030**	0.245***	0.827***		
	(0.013)	(0.072)	(0.102)		
Post	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
N	1836	1836	1836	1224	1224

Notes: Estimation results using additional province-level control variables. These additional covariates refer to firms in manufacturing, ICT, and professional and scientific services at the province-level. These are the number of new enterprises, the number of active firms, the share of 3 years' old firms over the business population, the average size of new enterprises, and the number of high-growth firms. Standard errors clustered at the firm-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: IV estimates using augmented specification

Panel A	Revenues	Employees	Assets	Fixed_K	Value_Added
Treat x Post	0.046	0.111*	0.178***	0.381***	0.268***
	(0.076)	(0.059)	(0.042)	(0.076)	(0.100)
Treat x Post$_{t=1}$	-0.079	0.121	0.145***	0.233***	0.240**
	(0.060)	(0.074)	(0.044)	(0.078)	(0.118)
Treat x Post$_{t=2}$	0.163**	0.220***	0.243***	0.446***	0.385***
	(0.081)	(0.062)	(0.051)	(0.085)	(0.115)
1st stage F-stat	708.888	300.230	708.888	708.888	641.332
Hansen (p-value)	0.217	0.389	0.588	0.120	0.087
N	1836	975	1836	1836	1598
Panel B	Leverage	Equity	Intangibles	Patent	Failure
Treat x Post	0.017*	0.153**	0.651***	0.009*	0.001
	(0.009)	(0.063)	(0.081)	(0.005)	(0.010)
Treat x Post$_{t=1}$	-0.002	0.203***	0.485***		
	(0.013)	(0.072)	(0.089)		
Treat x Post$_{t=2}$	0.028**	0.266***	0.839***		
	(0.014)	(0.088)	(0.111)		
1st stage F-stat	708.888	708.888	708.888	56.829	56.829
Hansen (p-value)	0.444	0.478	0.006	0.678	0.723
N	1836	1836	1836	1224	1224

Notes: Estimation results using additional province-level control variables. These additional covariates refer to firms in manufacturing, ICT, and professional and scientific services at the province-level. These are the number of new enterprises, the number of active firms, the share of 3 years' old firms over the business population, the average size of new enterprises, and the number of high-growth firms. Standard errors clustered at the firm-level. Standard errors clustered at the firm-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A7: IV estimates using alternative instrument

Panel A	Revenues	Employees	Assets	Fixed_K	Value_Added
Treat x Post	0.052	0.088	0.172***	0.373***	0.262***
	(0.075)	(0.062)	(0.042)	(0.073)	(0.099)
Treat x Post$_{t=1}$	-0.052	0.082	0.144***	0.261***	0.243**
	(0.057)	(0.073)	(0.043)	(0.071)	(0.104)
Treat x Post$_{t=2}$	0.181**	0.193***	0.267***	0.464***	0.426***
	(0.077)	(0.061)	(0.055)	(0.088)	(0.108)
1st stage F-stat	548.044	153.189	548.044	548.044	417.968
Hansen (p-value)	0.118	0.285	0.564	0.213	0.106
N	1836	975	1836	1836	1598
Panel B	Leverage	Equity	Intangibles	Patent	Failure
Treat x Post	0.015*	0.141**	0.634***	0.009	0.002
	(0.008)	(0.066)	(0.080)	(0.006)	(0.010)
Treat x Post$_{t=1}$	-0.003	0.181***	0.488***		
	(0.012)	(0.069)	(0.079)		
Treat x Post$_{t=2}$	0.027*	0.280***	0.872***		
	(0.014)	(0.094)	(0.117)		
1st stage F-stat	548.044	548.044	548.044	85.401	85.401
Hansen (p-value)	0.779	0.582	0.058	0.577	0.403
N	1836	1836	1836	1224	1224

Notes: Estimation results using an alternative instrument. This is computed by dividing the number of registered start-ups over the number of firms in manufacturing, ICT, and professional and scientific activities in each province in 2013. Standard errors clustered at the province-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.