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The impact of R&D subsidies on firm innovation

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ABSTRACT

This paper evaluates the impact of an R&D subsidy program implemented in a region of northern Italy in the early 2000s on innovation by beneficiary firms. We use a regression discontinuity design strategy to assess the effect of the grants on the number of patent applications and the likelihood of submissions by subsidized firms. We find that the program had a significant impact on the number of patent applications, more markedly in the case of smaller firms. Our results also show that the program was successful in increasing the likelihood of applying for a patent, but only for smaller firms. Our estimates show that one additional patent application requires grants of between €206,000 and €310,000 to the firms.

1. Introduction

The need to subsidize private innovative activity is based on solid theoretical arguments dating back to Arrow (1962). According to economic theory, in the case of research and development (R&D) perfect competition is unable to maximize social welfare because the outputs of innovative activity are strongly affected by problems of non-appropriability, non-divisibility and uncertainty that prevent firms from totally internalizing the benefits of R&D investment. As a result, without public support the equilibrium level of private resources allocated to R&D ends up being below the socially optimal level (Spence, 1984).

To ensure an optimal allocation of resources for innovation, most industrial countries have public policies that support private R&D activity manly through subsidies or fiscal incentives. These policies aim to reduce the costs of the innovative outlays in order to stimulate investment in innovation. Although the empirical literature on the effects of such programs is already voluminous and growing fast, the results published to date are still mixed.

Most of the papers assess whether R&D incentives have additional effects on firms' *innovation inputs*, e.g. on investment in R&D, tangible assets or employment.[1] By contrast, micro-econometric studies of the impact of subsidies on firms' *innovation outputs* are relatively rare (see, for example, Branstetter and Sakakibara, 2002; Bérubé and Mohnen, 2009; Moretti and Wilson, 2014), although assessing the effects of public incentives on innovation outputs is crucial for at least two reasons. First, because innovation is probably the ultimate goal of most programs that support R&D activity. If the policy is able to increase firms' innovative capabilities, eventually it will also be able to raise firm competitiveness. Second, because the public program might affect innovation outputs even when keeping R&D spending or other innovation inputs constant. For example, it may encourage firms to undertake more radical projects, start R&D collaborations or improve R&D management (OECD, 2006). As a result, evaluating the effects only on innovation inputs provides a partial assessment of the impact of the incentives.

This paper contributes to this stream of research. We evaluate the impact on the recipient firms' patenting activity of a placed-based policy for innovation that subsidized private enterprises through grants. More specifically, we study the effect of a business R&D program implemented in the early 2000s in a region of northern Italy (Emilia-Romagna) on the likelihood of applying for

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[1] See, for example, the recent surveys by Köhler et al. (2012), Zúñiga-Vicente et al. (2104), and Becker (2014). On the econometric methods, see Cerulli (2010); for earlier reviews, see David et al. (2000), Klette et al. (2000) and Hall and Van Reenen (2000).

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a patent, and on the number of patent applications, of recipient firms, from a sample of 612 manufacturing and services enterprises participating in the program. The Emilia-Romagna region is an important case study for our purposes: it boasts the highest patent intensity among the Italian (Nuts 2) regions, accounting for more than 10 per cent of Italian patents.[2]

We contribute to the existing literature in several respects. First, we shed more light on the effects of R&D grants on the innovation outputs of firms. This issue is largely overlooked by the evaluation literature that instead mostly gauges the effects on innovation inputs. Second, we provide evidence of the program's effects on a large sample of small and medium-sized firms – those that in principle would need to be subsidized because of their greater relative exposure to financial constraints (Hall and Lerner, 2009), and that may carry out more innovative R&D activity (Akcigit and Kerr, 2010). Our third contribution is methodological. Since recipient and non-recipient firms are inherently different, a central issue in the program evaluation literature is the adoption of a strategy that allows the researcher to correctly identify the effect of the policy. In our case we use a sharp regression discontinuity design (Lee and Lemieux, 2010). The program established that only the projects scoring above a certain level on an assessment by a technical committee would be subsidized. In order to evaluate the policy's impact we thus compare the patenting activity of subsidized and unsubsidized firms close to the threshold score.[3] The regional dimension allows us to further reduce the unobserved heterogeneity among enterprises by comparing firms located in the same area and therefore more alike than those participating in nationwide programs. Moreover, our assessment allows us to shed further light on the impact of place-based policies managed by a regional government, which have been little studied to date (Zúñiga-Vicente et al., 2014).[4]

Overall we find that the program increased the number of patent applications submitted by recipient firms, especially smaller ones. The program also appears to have succeeded in increasing the probability of applying for a patent, but only for small enterprises. According to our results, one additional patent application made under the program requires grants of between €206,000 and €310,000 disbursed to the firms by the regional government (the administrative costs of the policy are excluded).

The rest of the paper is organized as follows. In the next section we discuss the theoretical background and the related empirical literature. In Section 3 we illustrate the features of the program. In Section 4 we describe the outcome variables and our dataset. We present the empirical strategy in Section 5 and set out the main results in Section 6. The robustness exercises and concluding remarks make up the final two sections.

2. R&D subsidies and innovation outputs: the theoretical and empirical framework

In theory R&D incentives to private firms are justified by two market failures. It is traditionally argued that the existence of technological spillovers in R&D activity are not taken into account when firms plan their R&D investment (Arrow, 1962). Because of positive spillovers, private investment falls short of the socially optimal level and public support aims to increase the level of R&D investment to bring it closer to the socially optimal equilibrium. Another argument is based on the capital market imperfections that hamper firms' ability to access financing on the markets. This market failure is due to informational asymmetries that are amplified in the case of R&D financing because innovative activity is very risky and difficult to evaluate. For these reasons, it is argued that in the case of R&D internal funds are largely preferred to external financing, and small or younger firms in particular might face financing constraints on their R&D activities (Hall and Lerner, 2009). The purpose of public incentives is therefore to provide firms with sufficient funds to implement innovative investment.

The most common forms of public support for private firms' innovation are subsidies or fiscal incentives. Both aim at increasing firms' investment in R&D by reducing the attendant cost, but while grants are assigned only after projects have been evaluated, tax incentives reduce the firms' tax burden automatically (usually according to the amount of the R&D expenditure realized) without any system of assessment. In this respect tax incentives are more neutral than subsidies because they enable firms to take advantage of fiscal subsidies irrespective of the kind of project undertaken.

Widespread public support for innovation has spawned a huge body of empirical papers that have assessed the effects of various types of incentives on business innovation inputs such as R&D expenditure, investment and employment, but the results are rather mixed. For example, in a recent survey, Zúñiga-Vicente et al. (2014) conclude that the effects are very heterogeneous across programs and studies; in another review Becker (2014) remarks that recent papers on tax credit mostly display positive results, especially for small firms that are likely to be more exposed to financial constraints.[5]

Despite such a large body of evidence on the effects on innovation inputs, very few papers have evaluated the effects of firm incentives on innovation outputs (Köhler et al., 2012). This scant attention may at first appear puzzling because innovation outputs probably represent the ultimate aim of public support for private R&D activities designed to boost firms' competitiveness. However, it could be justified by the approach favored by the evaluators, mainly based on the knowledge production function framework, where innovation is considered a function of a set of innovative inputs, such as R&D investment, the number of researchers, or human capital (Griliches, 1990). Following this approach, incentives are supposed to enhance innovation outputs if they affect R&D spending or other quantitative innovative inputs (such as the number of researchers). Therefore, to examine whether the policies raise such inputs it should be sufficient to assess the policy impact on innovation outputs as well. However, there are several ways in which public incentives might increase the level of innovation outputs without raising innovation inputs in quantitative terms. This may occur if the policy affects the choice of the innovative projects to start, keeping R&D spending constant.

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[2] For the period 1995–2009 Emilia-Romagna registered an average of more than 160 patents per year per million inhabitants, more than double the Italian average (see: Istat, Indicators for development policies http://www.istat.it/it/archivio/16777, June 2013).

[3] Jacob and Lefgren (2011) use a similar method to estimate the impact of public grants on U.S. researchers' output measured by the number of published articles and citations, and find a limited impact of public support.

[4] In Italy between 2006 and 2011 about €15 billion – almost 40 per cent of the total – was disbursed to firms under these programs. The literature on the impact of placed-based policies for innovation is growing, but it is still rather thin. For example, the effects of some place-based policies recently implemented in Europe to promote clusters of innovative activities are evaluated by Albert et al. (2002) for France, Dohse (2000) for Germany, Viladecans-Marsal and Arauzo-Carod (2012) for Spain, while the effects of placed-based policies in the United States are examined by Moretti and Wilson (2014). On the effects of regional incentives for firms' innovation in Italy, see Gabriele et al. (2007), Corsino et al. (2012), Fantino and Cannone (2013) and Bronzini and Iachini (2014). Of these only Moretti and Wilson (2014) study the effect on innovation outputs.

[5] This evaluation literature includes, among others, Lerner (1999), Busom (2000), Wallsten (2000), Lach (2002), Almus and Czarnitzki (2003), Gonzalez et al. (2005), Görg and Strobl (2007), Merito et al. (2007), Hussinger (2008), Clausen (2009), de Blasio et al. (2011), Link and Scott (2013), Takalo et al. (2013), Bronzini and Iachini (2014), Einiö (2014), and Moretti and Wilson (2014).

Public funding might lead recipient firms to make riskier choices, but also to take on more challenging and innovative projects in order to increase the likelihood of obtaining the incentive, or enabling firms to implement projects that would have been difficult to finance privately from the market. Another justification is that the public policy might shift the firm's innovative activity to different components of R&D investment. For instance, if the policy encourages firms to increase expenditure on research activity and decrease it on development (i.e. the activity necessary to convert the outputs of research into a plan or project for the realization of new products or processes), incentives might affect innovation outputs even if the level of overall spending does not change, because innovation is more dependent on research expenditure than on development expenditure (Griliches, 1986; Czarnitzki et al., 2009).

These arguments are related to the so-called behavioral additionality of the public support of business R&D (see, for example, OECD, 2006), i.e. policy-driven changes in how firms conduct their R&D activities. Such additionality occurs if the policy affects a firm's management of R&D activity. For instance, because it encourages firms to engage in more challenging research, such as starting new R&D collaborations with public research centers or universities, ultimately it could increase their propensity to cooperate, or to undertake changes in the organization of R&D activities that translate into higher efficiency. These considerations justify extending the evaluation analysis to innovation outputs.

The evaluation literature on innovation outputs is, however, rather small. Some papers examine the effects of fiscal incentives such as tax credits. Czarnitzki et al. (2011) found a positive effect of R&D tax credits in Canada over the period 1997–1999 on the number of new products, and the sale shares of new products, of the manufacturing recipient firms. They address the potential selection bias issue – i.e. the bias due to the inherent difference between recipient and non-recipient firms – by using a control group of firms that did not benefit from the tax credit similar to the recipient firms chosen by the matching method. In a later paper, using a two-step Heckman model, Cappelen et al. (2012) found that the tax incentives introduced in 2002 in Norway had no impact on patenting activity, or the introduction of new products for the market by beneficiary enterprises.

To the best of our knowledge, few papers study the impact of the grants on innovation outputs – and some of them assess the effects of the grants together with those of the tax incentives. Using a matching approach, Branstetter and Sakakibara (2002) pointed out that the public-sponsored research consortia that benefited from some government subsidies increased the patenting activity of Japanese firms participating in a consortium. More recently, adopting a similar approach Bérubé and Mohnen (2009) examine Canadian firms benefiting from R&D tax credits and R&D grants and find that these firms are more innovative, in terms of new products, than firms that take advantage of R&D tax credits only. Moretti and Wilson (2014) evaluate the effect of state-based incentives to biotechnology firms in the U.S., namely R&D tax credits and specific subsidies to biotech enterprises on several outcome variables at state level including patents. Using the variation of the amount of public funds within states and over time to identify their impact on the local economy, they found that the public programs had a limited effect on spurring state patenting.

In the innovation policy literature just how the nature of the incentives might influence their effects is an open issue. Compared to subsidies that are usually granted after selective procedures, automatic incentives like tax credits or other forms of fiscal incentive present some advantages for firms and public authorities such as simple implementation and low administrative costs. On the other hand, the reduction of the tax burden (usually proportional to the volume of firms' R&D activity) depends on, and can be constrained by, the actual amount of tax liabilities of the firms. In this respect the instrument is less suitable to financing start-ups, young or unprofitable firms that might not have enough tax liabilities to take advantage of the credit. Grants might also be preferable for firms that experience more difficulty accessing capital markets, because unlike with tax incentives firms do not have to finance their projects in advance.[6] Finally, fiscal incentives tend to lead to lower allocative distortions than subsidies, because they are more neutral in terms of the projects being backed but, on the other hand, they are less suitable when it comes to influencing the kind of R&D activity realized by the target firms. The literature that compares the effect of selective subsidies and fiscal incentives is still limited. Colombo et al. (2011) show that selective R&D subsidies had a positive impact on the productivity of a sample of high-tech Italian recipient firms whereas automatic incentives did not. But when or if the subsidies must be preferred to tax incentives is a question that deserves further attention and calls for more empirical evidence.

This paper contributes to this literature by shedding more light on the effects of the R&D grants on the firms' innovative capability. Our study complements and extends the analysis carried out by Bronzini and Iachini (2014) in three directions. First, we examine the effect of the policy on some proxies of innovation outputs instead of measures of innovation inputs; second, we consider a wider sample of firms participating in the program that includes very small firms and start-ups; and finally, we estimate the impact of the incentives using different econometric models though following a similar identification strategy. There are two particularly important characteristics of our firms' sample that makes our analysis especially helpful. First, our sample is mostly comprised of small and medium-sized firms whose median number of employees is 39 (168 on average).[7] This allows us to depict the effects of the grants on the type of firms that in principle need more help to finance their R&D expenditure (Hall and Lerner, 2009), and display a higher propensity to innovate.[8] Finally, the analysis includes firms in the services sector, usually neglected by previous program evaluation studies mainly focused on manufacturing (as argued by Köhler et al., 2012).

3. The program to support R&D

The objective of the public program under scrutiny is to support the innovative activities of private regional firms through grants that reduce the costs of their projects. All firms that have an operational base in Emilia-Romagna and are willing to implement innovative projects in the region are eligible; participants are barred from receiving other types of public subsidies for the same project.[9]

Financed and managed by the regional government, the program subsidizes the innovative activities of eligible firms through grants that cover up to 50 per cent of the costs for research projects and 25 per cent for pre-competitive development projects (for small and medium-sized firms the ceiling is 10 percentage points higher). The grants subsidize outlays such as the cost of

[6] Busom et al. (2014), for instance, demonstrate that the probability of using tax incentives decreases with financial constraints, and young firms in knowledge-intensive industries use less tax credits than grants.

[7] Data on employment are available only for a subsample of recipient firms in our full sample.

[8] Acs et al. (1994) show that small firms are more innovative in some industries thanks to knowledge spillovers; Cohen and Klepper (1996) and Akcigit and Kerr (2010) argue that small firms are more likely to undertake more innovative and radical R&D activity than larger enterprises.

[9] See the "Regional Program for Industrial Research, Innovation and Technological Transfer" of the Emilia-Romagna region launched in 2003, implementing the provisions of Article 4 of Regional Law 7/2002 (see *Bollettino Ufficiale della Regione* No. 64 of 14 May 2002 and Regional Executive Resolution No. 2038 of 20 October 2003).

machinery, equipment and software, the purchase and registration of patents and licenses, the employment of researchers, use of laboratories, contracts with research centers, consulting and feasibility studies and, finally, the external costs for the creation of prototypes.[10]

The grants are assigned following an assessment of the proposals by a committee of independent experts, and by specialized independent evaluators, appointed by the regional government. More specifically, the committee and the independent evaluators individually examine the projects and assign a separate score for each of the following profiles: (1) technological and scientific (max. 45 points); (2) financial and economic (max. 20 points); (3) managerial (max. 20 points); and (4) regional impact (max. 15 points).[11] For each project, profiles (1)–(3) were assessed independently by different evaluators specialized in the corresponding field, who assigned a specific score. These specialists were independent from the regional government and were chosen among professionals who were accredited by the Italian Ministry of Education, Universities and Research (MIUR) outside the Emilia-Romagna region. The committee, which was appointed by, but independent from, the regional government, assessed the fourth profile and assigned the related score. Afterwards, the committee calculated the overall score by adding up the scores previously assigned. Only projects that get a sufficient score for each profile, and a total score of at least 75 out of 100 receive grants. Both the criteria must be satisfied in order to receive the subsidies.[12]

We focus on two tenders. The first application deadline was in February 2004, the second in September 2004, and the evaluation process ended in June 2004 and June 2005 respectively. Overall, about €93 million, corresponding to 0.1 per cent of regional GDP, went to 415 firms. The total planned investment amounted to €235.5 million.

4. Outcome variables and data

4.1. Patents as a proxy for innovation

We assess the effect of the policy on firm innovation performance using two proxies for innovation output. First, we use the number of patent applications submitted by the firms to the European Patent Office (EPO). Second, to assess the effect of the policy on the probability of applying for a patent, we use a dummy variable equal to 1 if the firm submitted at least one patent application after the policy was implemented and zero otherwise. Note that with the first variable we evaluate the impact of the program on both the intensive and extensive margin of firm patenting, i.e. on the number of patent applications of firms and the probability to apply for a patent, while with the dummy variable we focus only on the second effect (extensive margin).[13]

The number of patent applications and the probability of applying for a patent are measured over the post-program period: from 2005 to 2011 for firms participating in the first tender and from 2006 to 2011 for firms participating in the second. Finally, as a robustness check we also run the regression shifting the starting year forward by one year.[14]

Measuring innovation outputs on the basis of firms' patent applications has pros and cons and merits a brief discussion. On the one hand, it is well known that not all innovations are patented or patentable. There are several other informal mechanisms that the firms can use to secure appropriate returns from their invention or to protect innovation, such as secrecy or the exploitation of lead time advantages. The choice to patent depends on a number of factors. For example, firms might wish to patent innovation to improve their goodwill reputation or to increase their bargaining power in the cross-licensing market to extract revenues from patented inventions (Cohen et al., 2000; Anand and Khanna, 2000). In many cases firms prefer not to apply for a patent because they do not want to disclose their inventions. Moreover, only inventions whose patent has an economic value above a certain minimal threshold are patented (Griliches, 1990, and for further discussion see OECD, 2009).

Furthermore, the propensity to patent might vary, other things being equal, from country to country, over time or across sectors. Cohen et al. (2002), for example, explain the difference in patent propensity between Japanese and U.S. businesses with reference to the fact that U.S. firms perceive patents as a less effective means of protecting property rights than Japanese firms do. In addition, the degree of patent enforceability and the criteria that an innovation must satisfy to be patented (novelty, non-obviousness) can also vary across countries and over time, and these differences might affect the propensity to patent (Nagaoka et al., 2010).

On the other hand, patents are probably the most definite measure of innovation. Compared with other proxies, usually measured through surveys, such as the number of new products or processes introduced by firms, they are less exposed to personal or subjective considerations. Moreover, patents also reflect the quality of an innovation. To be patented an invention is examined by experts who judge its novelty and utility. By contrast, reliable information on the quality of an innovation can rarely be gathered from other sources, especially if they are based on personal judgment.[15] Finally, a number of flaws of patents as a measure of innovation, such as poor comparability over time or across countries, do not apply to our exercise, because firms belong to the same region and the timeframe is relatively short.

In the literature on innovation, Griliches (1990) suggests interpreting patenting activity as an indicator of the increase of economically valuable knowledge and hence a good way of

[10] The investment can last from 12 to 24 months. Subsidies are transferred to the firms either after the completion of the project or else in two installments, one at the halfway mark and the other when the project has been completed. To qualify for a grant, the overall costs of a project must be between €150,000 and €250,000.

[11] Profile (1) includes the degree of innovation of the project and the adequacy of the technical and scientific resources provided; profile (2) the congruence between the project's financial plan and its objectives; profile (3) past experience in similar projects or the level of managerial competence; and profile (4) regional priorities specified in regional law, e.g. projects involving universities and the hiring of new skilled personnel.

[12] Sufficient scores correspond to 27 for profile (1); 12 for profiles (2) and (3); and 9 for profile (4). These passing scores correspond on different scales to a score of 6 out of a scale of 10 points. For the evaluation process, both the committee and the independent evaluators must comply with the guidelines set by the Italian Ministry of Education, University and Research and the general principles laid down by the European Commission. More information on the evaluation process, procedures and principles are reported in the *Delibera della Giunta regionale* No. 2822/2003 of the Emilia-Romagna region.

[13] We use patent applications, instead of the number of patents granted, because the patent granting procedure lasts some time and would have been completed only for a few applications over our post-program time window. We were unable to focus on the intensive margin exclusively because the sub-sample of firms with patenting activity is too small to run the R&D exercises using them only. Of 612 firms, 142 applied for at least one patent in the period starting one year after the assignment of the grants up to 2011 (126 in the period starting two years after).

[14] Since the post-program periods of the two rounds of applications have different lengths, we also estimated the model using as an outcome variable the number of patent applications per year over the post-program period, obtaining very similar results; they are not shown but available on request.

[15] In one of the leading international surveys on firms' innovation (the Community Innovation Survey) products and processes are deemed new and firms innovative if the firm produced goods and services or adopted processes that are new for the firm but not necessarily for the market. By using patents we are instead able to capture innovations for the market.

measuring inventive activity, even if only a (random) fraction of inventions is patented. OECD (2009) and Nagaoka et al. (2010), among others, argue that using patents as a proxy for invention is possible, but warn that researchers should be aware of the pros and cons. As regards enterprises, Hagedoorn and Cloodt (2003) conclude that patents are a good indicator of innovative performance at firm level. In the light of these considerations, we believe that patenting activity is a suitable measure of innovation output that can be used in a satisfactory way in our empirical exercise.[16]

Because the costs of patenting are among the reimbursable outlays under the public program, the objection that could be raised in our case is that the incentives might boost the propensity to patent previous inventions, rather than enable firms to engage in innovation-spurring R&D activity which they would not otherwise have carried out. However, we think our exercise might capture this effect only marginally, since the costs of filing patent applications with the EPO are low compared with the admissible costs of the projects. According to van Pottelsberghe (2009), in the first five years the cumulative costs of applying for a patent at the EPO increased on average from €1800 to €5000 (the increase is due to the search and examination activity carried out by the EPO). Only after a patent is granted do the costs increase significantly.[17] Considering that eligible projects must cost between €150,000 and €250,000 and that in our sample treated firms applied on average for 1.7 patents (the median and 75th percentile are zero) in the five pre-program years, patenting costs make up only a marginal part of the total costs of the proposal submitted to the regional government. Accordingly, we reject the idea that the costs for patenting are an issue for firms and that, by using patent applications as an outcome variable, we are capturing the policy's effect only on patenting activity rather than on invention activity.

4.2. Data

This analysis is based on three different datasets. First, we use the dataset provided by the Emilia-Romagna region, which gives us information on firms participating in the program, e.g. name, score obtained after the assessment, investment planned, grants assigned, subsidies revoked and renunciations. We pool the data of the two tenders concluded in 2004 and 2005.[18] A total of 1246 firms participated (557 treated and 689 untreated). Given that our empirical strategy is based on the score assigned to each firm, we had to exclude 411 unsubsidized firms that received no score because their projects were unsatisfactory in at least one respect. Note that the strategy is based on the test for discontinuity around the cut-off point, and it is likely that omitted firms would have received an overall score far from the cut-off, so we think their exclusion did not bias our results. Finally, we also excluded a total of 233 firms involved in renunciations and revocations and firms not subsidized in the first tender but subsidized in the second. The number of remaining firms is 612. Overall they are small and medium-sized enterprises with a median of 39 employees (168 on average).

Second, we use the PATSTAT dataset, which provides information on applications filed and patents registered at the European Patent Office (EPO). More specifically, in order to obtain the number of patent applications of the firms that participated in the program, we referred to the recent work by Lotti and Marin (2013) that provided the number of patent applications of Italian firms from the late 1970s to 2011.[19] They use a very accurate procedure to match the PATSTAT and AIDA datasets (the AIDA dataset sourced by Bureau van Dijk provides financial statement information for the majority of Italian corporations), enabling them to match more than 80 per cent of the patent applications submitted by Italian companies to the EPO during the observation period.[20]

The third source is the Cerved dataset that provides information on company financial statements, allowing us to compare the characteristics of treated and untreated firms and to carry out some robustness exercises. In one of these exercises we include some control variables in the R&D estimates, namely those with the highest differences between recipient and non-recipient firms, i.e. gross operating margins/sales, cash flow/sales, financial costs/debt and capital stock (see Section 7). All these covariates are drawn from the Cerved dataset.

5. Empirical strategy

In the program evaluation literature the main econometric issue is the selection bias. Since subsidy recipients are not randomly chosen, recipient and non-recipient firms differ substantially. In this circumstance, the estimates of the policy impact may be correlated with the unobservable characteristics of the recipient firms, and thus the assessment of the effects of the program could be biased. In order to deal with the selection bias the empirical literature mostly recurs to matching methods or instrumental variables. In this paper we employ a different strategy called regression discontinuity design (RDD).

To identify the impact of the program on firms' innovations, we exploit the scored-based fund assignment mechanism. We apply a sharp regression discontinuity design to compare the performance of subsidized and non-subsidized firms that have scores close to the threshold (the cut-off score is 75 points out of 100). The method is based on the intuition that around the cut-off the assignment of the grants occurs as if it had been random, so that the method becomes equivalent to a random experiment (Lee, 2008). In these circumstances, treated and untreated firms across the cut-off are supposed to be very similar, and after the program the different performance of recipient and non-recipient firms that fall around the threshold can be attributable to the subsidy (for a thorough

[16] This is also a rather standard choice in the econometric literature on innovation. For instance, Crepon et al. (1998) and Criscuolo et al. (2010) use patents as an indicator of innovation output to estimate a knowledge production function; Aghion et al. (2009) to assess the effect of firm entry on innovation performance of incumbent firms; Branstetter and Sakakibara (2002) to evaluate the role of Japanese government-sponsored research consortia in increasing the research productivity of participating firms; and Moretti and Wilson (2014) to evaluate the effect of place-based policies on innovation output.

[17] This depends on the EPO's procedure. Before a patent is granted the main costs are EPO search and examination fees and EPO renewal fees. Once granted, the applicants must pay translation costs and renewal fees to the national patent offices in the countries where the invention is protected, which are much higher, especially if they want to protect the invention in several countries.

[18] We did this to increase the sample size. However, if scores are not completely exogenous to the total amount of funds allocated to the programs, scores in the second tender might be affected by this budget constraint. If present, this effect is likely to be negligible. As a robustness exercise we re-estimated the model, breaking down the two tenders and over the two different samples. The results were similar: though with higher standard errors, the magnitude of the effect of the policy on the number of patents was close to the baseline and statistically significant in both samples. However, the effect on the probability of applying for a patent was not significant in the sample for the second tender. The results of this exercise are not reported but available on request.

[19] For more details, see Lotti and Marin (2013). It is worth recalling that due to delays in the publication of EPO data (18 months since the application or priority date, see OECD, 2009, p. 61), there is an underestimation for application counts in the last two years of coverage of the database. The latest version of Marin's dataset (February 2012) for the period 2000–2011 contains 6493 EPO applicants and 40,112 EPO applications.

[20] We failed to find information in their dataset on 75 of the 612 firms in our complete sample (presumably the missing ones were small or non-limited-liability businesses not included in the AIDA dataset). Thus, we recovered information on patent applications by those 75 firms directly from PATSTAT using their name and address. We used version 201,204, released in January 2013.

discussion of this methodology in economics, see Lee and Lemieux, 2010).[21]

More formally, by letting the outcome variable be a function of the score, the average treatment effect of the program is estimated by the value of the discontinuity at the threshold. If the treatment, i.e. the subsidies, depends on whether a (forcing) variable exceeds a known threshold, this strategy relies on the general assumption that the agents must not be able to control the forcing variable precisely (Lee, 2008). In that case, the treatment around the threshold is as if it were randomized, and the impact of the program is identified by the discontinuity of the outcome variable at the cut-off point (Hahn et al., 2001). We think that in our case this assumption holds, because it is highly unlikely that firms participating in the program can control their score perfectly. In any event, even if the randomization assumption cannot be directly tested, it can be indirectly verified by checking the similarity of the subsidized and unsubsidized firms around the cut-off or other indirect tests (some of them are carried out in the robustness section).

In order to test the discontinuity at the cut-off point, we use parametric econometric models and leave the estimates of non-parametric models to the robustness section (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). Since the number of patents is a discrete count variable, when we assess the effect of the policy on the number of patent applications by firm we estimate parametric models suitable for count data (Hausman et al., 1984; Cincera, 1997), namely Poisson and negative binomial models.[22] In the Poisson model the conditional mean is assumed to be equal to the conditional variance: $E(y_i|x_i) = \mathrm{Var}(y_i|x_i)$. With real data this assumption might be wrong because they are often overdispersed, i.e. $\mathrm{Var}(y_i|x_i) > E(y_i|x_i)$, and overdispersion leads to deflated standard errors and inflated t-statistics (Cameron and Trivedi, 2005, p. 670). To account for overdispersion we also estimate the negative binomial model, which is a generalization of the Poisson distribution with an additional parameter α that allows the variance to exceed the mean. We follow a common practice in the literature and assume the following function for the variance: $\mathrm{Var}(y_i|x_i) = \mu_i + \alpha\mu_i^2$; where Var(.) is the variance and μ_i is the conditional mean. When $\alpha = 0$ the negative binomial model reduces to the Poisson model, thus by testing for $\alpha = 0$ we test for the presence of overdispersion, and in the event of rejection we favor the negative binomial model for fitting the data.[23]

On the other hand, for the probability of applying for a patent, i.e. when we use as outcome variable a dummy equal to one if the firm has applied for a patent and zero otherwise, we use a standard logit model (with a probit model the results are similar).

Table 1a
Patent applications by period.

	Number of firms with at least one patent application	Number of firms without patent applications
Treatment periods		
Period 1	142	470
Period 2	126	486
Pre-treatment periods		
Period A	127	485
Period B	135	477

Notes: The sample includes 612 firms. Period 1 includes firms with patent applications between 2005 and 2011 for the first tender, and between 2006 and 2011 for those of the second tender. This is our main dataset. Period 2 includes firms with patent applications between 2006 and 2011 for the first tender, and between 2007 and 2011 for those of second tender. Pre-treatment Period A (5 years) includes firms with patent applications in 2000–2004 for the first tender and between 2001 and 2005 for those of the second. Pre-treatment Period B (6 years) includes firms with patent applications in 1999–2004 for the first tender and in 2000–2005 for those of the second.

More formally, given a general link function $F(.)$ and the outcome variable Y, we estimate the following parametric polynomial discontinuity regression models:

$$Y_i = F\left[\alpha + \beta T_i + (1 - T_i)\sum_{p=0}^{2}\gamma_p (S_i)^p + T_i \sum_{p=0}^{2}\gamma'_p (S_i)^p\right] + \varepsilon_i \qquad (1)$$

where $F(.)$ is an exponential link when the outcome variable is the number of patent applications, and a logit link when the dependent variable is a dummy equal to 1 for firms with at least one patent application. Y_i is the outcome variable (number of patent applications or dummy); T_i is equal to 1 if firm i is subsidized (all firms with $Score_i \geq 75$) and to 0 otherwise; $S_i = Score_i - 75$; the parameters of the score function (γ_p and γ'_p) are allowed to be different on the opposite side of the cut-off to allow for heterogeneity of the function across the threshold; ε_i is the random error. The polynomial order 0 is the mean difference between treated and untreated firms. Given that the score is a discrete variable, we clustered the heteroskedasticity-robust standard errors by the value of score S, as suggested by Lee and Card (2008).

Eq. (1) is also estimated locally around the cut-off point using two different sample windows. The wide window includes 50 per cent of the baseline sample, the narrow window includes 40 per cent. We also use two other threshold values (60 per cent and 30 per cent), for sensitiveness purposes, obtaining similar results (they are not shown but available on request).

The outcome variables are calculated on the patent applications submitted by each firm after the program. The treatment period starts one year (Period 1) or two years (Period 2) after the grant is assigned, up to 2011; patents are attributed to firms using the year of application as the reference date.[24] We sum the applications by firm over the time span considered. In terms of the number of patents, our sample of 612 firms has the following profile shown in Table 1a. Period 1 includes 142 firms with at least one patent application registered (between 2005 and 2011 for the firms belonging to the first tender and between 2006 and 2011 for those in the second tender). This is the baseline dataset for our exercise. Period 2 includes 126 firms with at least one patent application registered (from 2006 to 2011 for the firms belonging to the first tender and

[21] In the literature on R&D incentives Jacob and Lefgren (2011) and Bronzini and Iachini (2014) use a similar methodology. Notice that in principle also a fuzzy RDD could have been employed, because to receive the subsidies firms must have obtained a sufficient score in each profile and an overall score of at least 75 points. As a result firms might have been discarded even with an overall score of (or above) 75 (because insufficient in one profile). However, in practice projects assessed insufficient in one profile did not receive the overall score, therefore all the firms with at least 75 points were subsidized. In these circumstances the fuzzy RDD was not appropriate and the sharp RDD was the most suitable strategy.

[22] The classical linear model is not suitable, as the shape of the observation set does not correspond to a linear model the assumption of normality of the disturbances cannot be made and the prediction formulae give impossible values (Gourieroux et al., 1984).

[23] This quadratic form is the one most frequently used in the literature among several possible functions (see Greene, 2008), as it behaves well in many empirical applications, as well as in our case This form also ensures consistency, provided that the conditional mean is well specified (Cameron and Trivedi, 2005, p. 677).

[24] The problem of choosing the reference year is that every patent document includes several dates, reflecting the timing of the invention, the patenting process and the strategy of applicants (OECD, 2009, p.61). In Section 6 we will carry out some robustness checks on the choice of reference year.

Table 1b
Distribution of firms by sector.

Sector	Number of firms		Share (%)	
	Treated	Untreated	Treated	Untreated
Agriculture and fishing	1	0	0.3	0.0
Mining	1	0	0.3	0.0
Food, beverages and tobacco	18	5	4.7	2.8
Textiles, apparel, wood products	4	5	1.1	2.8
Paper, printing and publishing	5	1	1.3	0.6
Coke, chemical products, plastic	35	9	9.2	5.1
Non-metallic mineral products	12	4	3.2	2.2
Basic metal industries	22	13	5.8	7.3
Machinery and equipment	110	39	29.0	21.9
Electrical and optical equipment	56	23	14.8	12.9
Transport equipment	19	6	5.0	3.4
Other manufacturing industries	8	7	2.1	3.9
Construction	6	2	1.6	1.1
Trade, transport, financial services	17	12	4.5	6.7
Knowledge-intensive business services	24	17	6.3	9.6
Others	41	35	10.8	19.7
All firms	**379**	**178**	**100.0**	**100.0**

Notes: Based on Cerved data. The sample includes 557 out of 612 firms considered in the evaluation exercise.

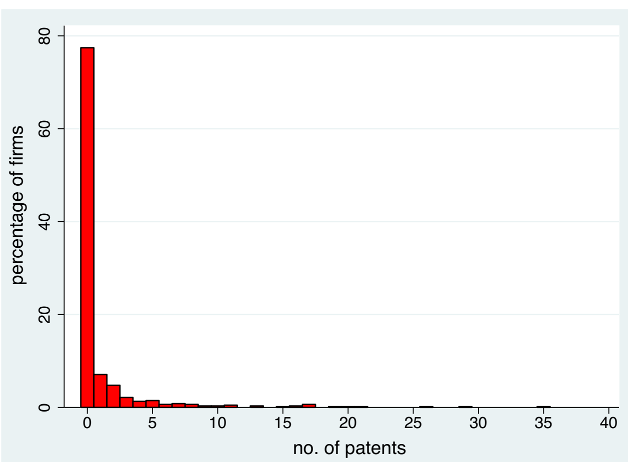

Fig. 1. Firms' density by number of patent applications. Notes: Counts in the treatment period (Period 1).

from 2007 to 2011 for those in the second). Pre-treatment period A (5 years) includes 127 firms with at least one patent application (registered in 2000–2004 for the firms belonging to the first tender and in 2001–2005 for those in the second). Pre-treatment period B (6 years) includes 135 firms with at least one patent application (registered in 1999–2004 for the firms belonging to the first tender and in 2000–2005 for those in the second). The length of the pre-program periods has been chosen to mirror that of the post-program periods.

Fig. 1 shows the distribution of patents by firm in Period 1. About 77 per cent of the firms have no patents. The average number of patents in this sample is 1.8, while variance is about 87. These characteristics of the distribution of our outcome variables will be satisfactorily accounted for by the negative binomial model.[25]

6. Results

6.1. Descriptive statistics and graphical analysis

Fig. 2 and Table 1b report the distribution of firms by sector. Since a firm's sectoral identification is based on financial statement data, the sample is a little smaller (557 firms) than in our regression sample (612). We note that there is a concentration of firms in just a few industries: machinery, electrical and optical equipment, chemicals and knowledge-intensive business services. Together they make up about 60 per cent of the sample. The distribution by sector of treated firms is very similar to that of untreated ones. However, we find a larger percentage of untreated firms in knowledge-intensive business services, whereas the opposite holds for firms in the coke and chemical products industries and the food and beverages sector. Note that treated firms are more numerous than untreated ones, because of the exclusion of the non-scored applicants from the second tender.

Table 2a shows the means of several financial statement variables the year before the publication of the tenders for treated and untreated firms. The RD design relies on the assumption that near the cut-off the treatment is random, so that firm covariates before

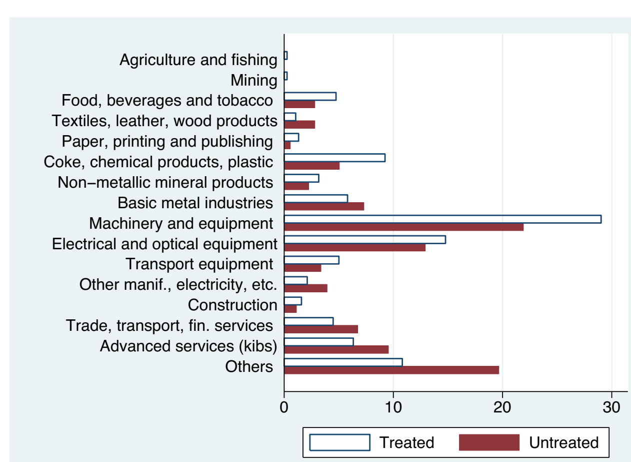

Fig. 2. Distribution of firms by sector (percentage change).

[25] In the robustness section we also use other count models suggested by the literature for the large amount of zeros.

Table 2a
Pre-assignment means and mean differences: financial statement items.

Sample	All				50% window				40% window			
Variables	Untreated	Treated	Diff.	Diff. (t-stat)	Untreated	Treated	Diff.	Diff. (t-stat)	Untreated	Treated	Diff.	Diff. (t-stat)
Sales	13,236	49,527	10,111	2.74***	14,331	24,442	10,111	0.76	11,188	19,482	8294	0.63
Value added	3319	11,883	8565	2.79***	3616	6523	2907	1.32	3345	5136	1791	1.12
Assets	13,745	51,075	37,330	2.67***	15,205	32,365	17,160	1.00	11,635	21,308	9673	1.55
ROA	5.12	6.02	0.90	0.96	3.91	5.87	1.96	1.68	3.60	5.60	2.00	1.52
Leverage	13.06	25.90	12.85	0.60	8.55	6.80	−1.75	−0.31	8.18	5.93	−2.25	−0.31
Gross op. mar./sales	0.05	0.11	0.05	0.72	0.01	0.16	0.15	1.22	−0.01	0.17	0.19	1.17
Cash flow/sales	0.12	0.07	−0.05	−0.77	0.11	0.07	−0.05	−1.12	0.14	0.06	−0.08	−1.46
Financial costs/debt	0.04	0.02	−0.02	−2.00**	0.06	0.03	−0.03	−1.88*	0.03	0.03	0.00	−0.96
Labor costs/sales	0.23	0.30	0.07	0.78	0.24	0.31	0.06	0.48	0.26	0.32	0.06	0.38
Total capital stock	3079	14,500	11,421	2.16**	3664	9117	5454	0.73	2529	3936	1408	0.87
Intangible capital stock	708	3369	2660	1.45	829	2849	2020	0.76	505	1244	739.29	0.69

Notes: Based on Cerved data. The sample includes 557 out of the 612 firms considered in the policy evaluation exercise. All the variables refer to the first pre-assignment year (2003 for the first tender and 2004 for the second). In the complete sample 379 firms are treated; 178 are untreated. In the 50 per cent cut-off neighborhood sample treated firms number 195, untreated 90; in the 40 per cent cut-off neighborhood sample treated firms number 160, untreated 68. *, **, and ***: significant at 10%, 5%, and 1%, respectively.

the treatment should not differ significantly just below and just above the cut-off. Accordingly, we compare the means of the main financial statement items of our firms, above and below the cut-off, to perform a preliminary validation of our strategy. On the whole sample, we notice that treated firms are substantially larger than untreated firms, as shown by mean differences of sale, valued added, asset and capital stock. Also, the cost of debt is lower for treated firms. By contrast, treated and untreated firms are similar in terms of self-financing capabilities (cash-flow over sales), profitability, leverage and labor costs. When we restrict the sample around the cut-off, using both the 50 and 40 per cent sample windows described above, treated and untreated firms become more alike. The improvement is notable for size variables. Around the cut-off score mean differences are not more statistically significant.[26] Table 2b shows that before the program treated firms have a higher average number of patent applications and a higher probability of applying for a patent than untreated firms. However, these differences diminish dramatically and are no longer statistically significant when we restrict the sample around the cut-off point. Overall, these findings support our empirical strategy.[27]

Fig. 3 displays the density function of the sample by score. We notice that it is lower on the left-hand side of the threshold because of the exclusion of non-scored untreated firms in the second tender, but density increases substantially near the cut-off.

We also observe that at the score just below the cut-off (score = 74) the density is lower than at slightly more distant values.[28] Moreover, there is a high number of treated firms (32) at the threshold score (75). This discontinuity could pose a problem to the identification strategy because it might suggest a non-random assignment of the grants just around the cut-off. Thus, as robustness checks we also estimated the model excluding the firms that received a score of 74 or 75 from our sample. In both the cases the results, not shown but available on request, turned out to be very similar to those reported below.[29]

Before showing the econometric results, we carried out a graphical analysis of the outcome variables as a function of the score. In

[26] Table 2a reports the main available balance sheet variables, which might be correlated with the innovative capabilities of the firms including intangible assets. According to the Italian Civil Code, intangible assets include the costs for non-tangible goods that have multi-year utility, namely: (a) start-up costs, (b) R&D and advertising costs, (c) costs of patents, software and other intellectual property rights, (d) licenses and trademarks, (e) goodwill (recorded only when it is acquired in a business acquisition), (f) costs for ongoing intangible assets, (g) other intangible assets non-classifiable with the previous ones. The Cerved dataset provides information on these items only for some firms. For the five firms participating in the program that show the largest intangible assets, R&D, patents, software and other intellectual property rights, licenses, trademarks, and ongoing intangible assets all together cover on average about 66 per cent of total intangible assets, while goodwill covers 22 percent and the other intangible assets the remaining 11 per cent (start-up costs are zero).

[27] Note that applicants represent a small sample of firms that differ from the universe of the regional firms in several respects, mainly in that they are willing to invest in highly innovative projects. For descriptive purposes, it is worthwhile verifying the main peculiarities of applicants with respect to the firms that did not apply but that were localized in the same region. In this exercise we use the balance sheet information and firms (applicants and non-applicants) that were present in the balance sheet dataset in the pre-program year. More specifically, we regress the probability of participating in the program on the variables shown in Table 1b, controlling for 16 sectoral dummies. The results (that for the sake of synthesis are not reported) show that applicants are larger firms (higher sales), with lower profitability (ROA) and less indebted (lower leverage) than non-applicant firms. The largest difference is in firm size: sales of applicant untreated firms are, on average, more than four times that of the overall sample of regional firms (those of applicant treated firms more than ten times larger). This evidence was to a certain degree expected because investment in innovation is mainly made by large firms.

[28] We believe that the committee of experts avoided assigning a score just below the threshold because such a score might have been perceived as particularly annoying by dismissed firms and left more room for appeals against the decision.

[29] This point was raised by an anonymous referee that we would like to thank.

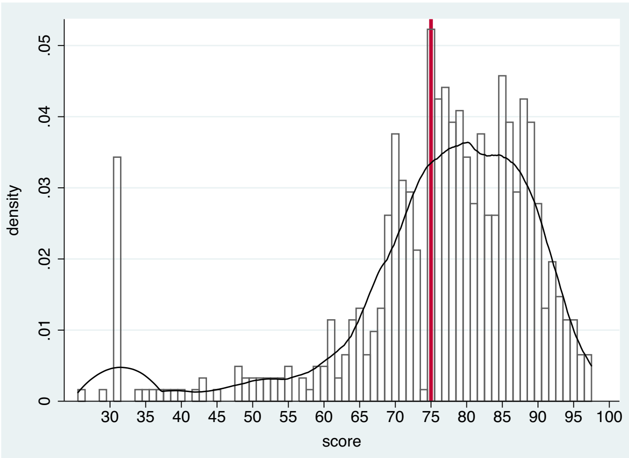

Fig. 3. Firms' density distribution by score.

Fig. 4, we plotted the number of patent applications and the probability of applying for a patent after the program, averaged by score together with two interpolation lines: linear and quadratic. The graphs give visual evidence of a positive discontinuity, which is stronger in the quadratic case. They suggest, therefore, a positive effect of the program on firms' innovative capabilities.

6.2. Baseline econometric results

We now move on to a more formal test for the discontinuity. For the number of patent applications, we show the estimations of coefficient β of model (1) estimated by OLS, the Poisson model and the negative binomial model. For the probability of applying for a patent we show the estimates of a logit model. We report the best specification chosen by the order of polynomial that provided the minimum Akaike Information Criterion (AIC), considering three samples around the cut-off: the whole sample, the 50 and the 40 per cent sample windows. Moreover, we estimate the model over two post-program periods: Period 1 starting one year after the program, and Period 2 starting two years after the program; both periods end in 2011.

The results are shown in Table 3. From the OLS estimates we find a positive effect of the subsidies for the whole sample and for those closer to the cut-off and for both the post-program periods considered (the first three columns of the table). The coefficients turn out to be positive and statistically significant in all the estimates, including with the Poisson and negative binomial models. In most cases the AIC suggests that the best model is the quadratic one. Note that the Poisson model is rejected in favor of the negative binomial: in the latter the estimates of the alpha parameter, substantially greater than zero, reject the hypothesis of variance equal to the mean. Fig. 5 compares the predicted probability of different counts according to either the Poisson model or the negative binomial model estimated on the complete baseline sample: the better performance of the negative binomial in fitting the data, especially the observed probability of zero counts, emerges clearly.

As regards the probability of patenting – that is, when the outcome variable is a dummy equal to one for firms that have applied for a patent at least once in the post-program period – the results are again positive and statistically significant (see the last three columns of Table 3).

Table 4 reports the marginal effect of treatment for the negative binomial and logit models of Table 3 (given the superiority of the negative binomial over the Poisson model, we did not compute the marginal effect for the latter).[30] In the case of the number of patents, the marginal effect of treatment on the whole sample is about 0.87, meaning that the number of patents increases on average by a little less than one for firms receiving the grant. In order to evaluate the magnitude of this improvement in patenting ability, we compare it with the average number of patents of untreated firms in the treatment period (0.61). Hence, in relative terms the effect of the treatment is about 1.4 times the average for untreated firms.

By using the estimated marginal effects, the grants disbursed and the number of firms subsidized, we are able to calculate the cost of one additional patent application induced by the program. Our estimates show that one additional patent application requires €206,000 of subsidies paid by the regional government, if we use the marginal effect estimated over Period 1 (0.867), and €310,000 if we use the smaller marginal effect estimated over Period 2 (0.577). Note that in this calculation, we consider only the direct cost of the grants but we exclude the (indirect) administrative costs of the policy.

The marginal effects are bigger with the windows closer to the cut-off, becoming very large and admittedly a little less plausible in the 40 per cent window sample. This is the result of the quadratic functional form selected by the AIC (instead of the zero degree chosen by AIC for the 50 per cent window sample). When we estimate the discontinuity around the cut-off using the zero-degree polynomial model (as done in the 50 per cent sample), the marginal effects are much smaller and very close to the value estimated with the 50 per cent sample: 0.922 in Period 1 and 0.647 in Period 2.[31] These results are widely confirmed over Period 2, when we start to count patents two years after the auctions. However, in this case we found a relatively smaller, though still significant, impact of the policy (in relative terms the marginal effect is nearly one).

The marginal effect of the treatment on the probability of patenting is about 0.12, meaning that the probability of applying for a patent increases on average by around 12 percentage points thanks to the grant; about 0.8 times the average for untreated firms. This result is relatively stable across the samples. The results in Period 2 mirror those in Period 1.

6.3. Results for small and large firms

It has been observed that it can be harder for small or young firms to finance innovative activity owing to more acute informational asymmetries and adverse selection (Hall and Lerner, 2009). Some empirical evidence supports this argument, showing that

[30] As it is well-known, in non-linear models the marginal effect of a change in a regressor is not equal to its coefficient. For the Poisson model (and the negative binomial), where $E(y|x) = \exp(x'\beta)$, the marginal effect (ME) of a change in variable j is in general $\exp(x'\beta)\beta$. Yet, for an indicator variable derivatives are not appropriate, because the relevant change is when this variable goes from 0 to 1. Then the ME is worked out as a finite-difference calculation: $ME = E(y|x, d = 1) - E(y|i, d = 0)$. Following Long and Freese (2005), we compute the marginal effect of treatment as follows: we compute $E(y|x = x_0, d = 0)$, the expected value of the regression without treatment, where the interaction terms are equal to zero or to the average of the score accordingly. Then, the regressors other than zero, at their average value, are equal to avg(score) or avg(score$\hat{2}$) in the quadratic specification. See Long and Freese (2005, p. 425), for details.

[31] For the same reason in Table 3, the coefficients in the 40 per cent window are much higher than those in the 50 per cent one. When we consider the zero-degree polynomial in the 40 per cent sample, the estimated coefficients are quite similar in both subsamples and much smaller (1.142** in period 1 and 1.097* in period 2, the same as in the Poisson model and negative binomial model, as they are the same model in the case of the zero-degree polynomial). AIC is no longer minimized, but it is still very close to the previous one. Moreover, the analysis of the distribution of patents by score allowed us to exclude that the higher coefficients in the 40 per cent sample are due to the presence of outliers around the threshold, because outliers (defined as firms with more than 20 patent applications), are outside these sub-samples.

Table 2b
Pre-assignment means and mean differences: patent applications (regressions on the treatment dummy).

Sample	All			50% window			40% window		
	Untreated	Treated	Diff.	Untreated	Treated	Diff.	Untreated	Treated	Diff.
No. of patent applications by firm (1)	0.517	2.296	−1.778***	0.632	1.061	−0.428	0.770	1.046	−0.276
Percentage of firms with at least 1 patent (2)	0.137	0.240	−0.103***	0.132	0.203	−0.071	0.148	0.203	−0.054

Notes: The sample includes 612 firms. Variables refer to a 5-year pre-assignment period (2000–2004 for the first tender and 2001–2005 for the second). In the complete sample 415 firms are treated; 197 are untreated. In the 50 per cent cut-off neighborhood sample treated firms number 211, untreated 98; in the 40 per cent cut-off neighborhood sample treated firms number 172, untreated 74. (1) The test of the mean differences and the relative standard errors are based on a negative binomial regression model where the number of patent applications are regressed on a dummy treatment (the negative binomial model is statistically preferred to a Poisson model); (2) The test of the mean differences and the relative standard errors are based on a logit model where the probability of applying for a patent is regressed on a dummy treatment. ***: significant at 1%.

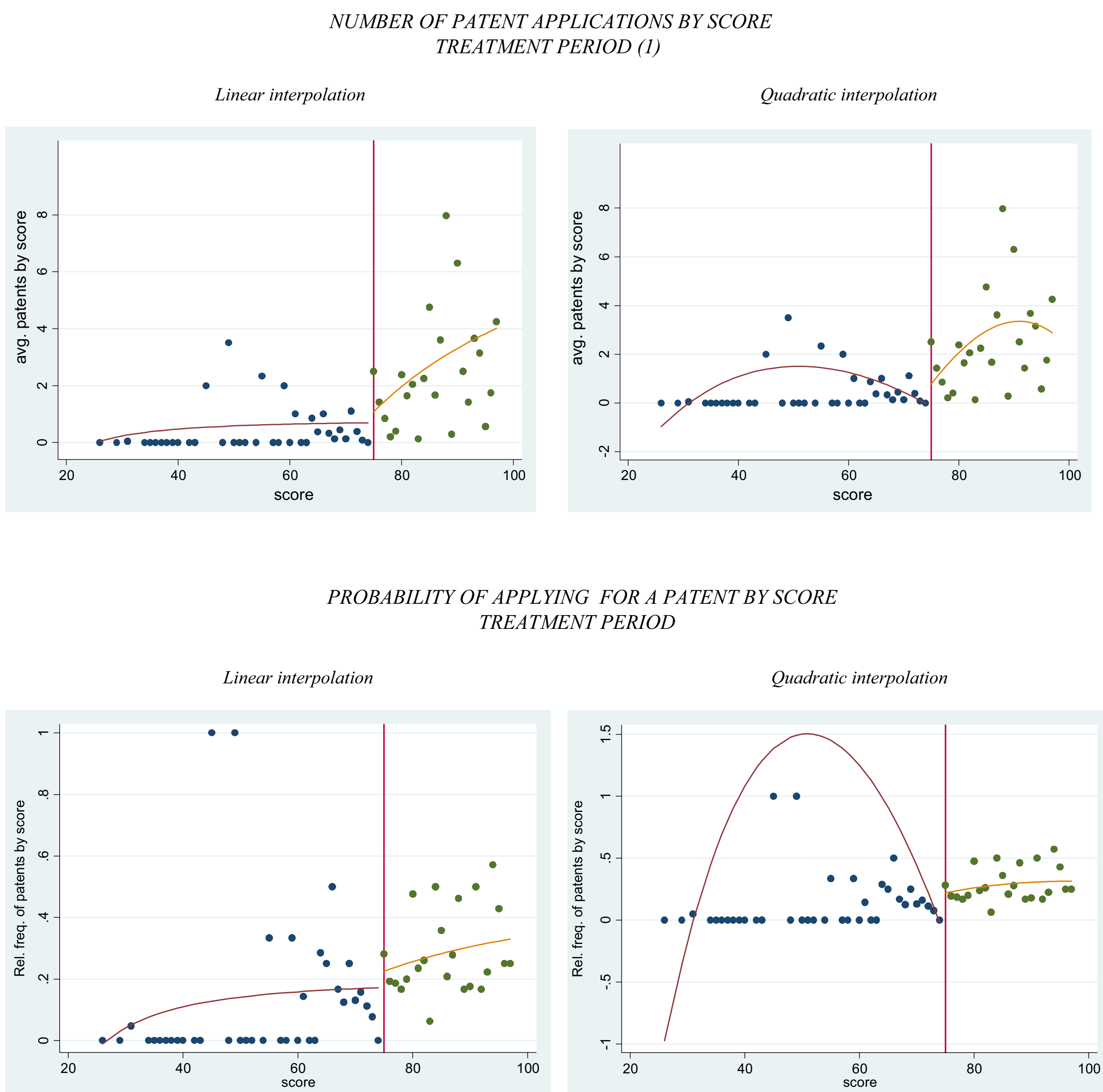

Fig. 4. Number of patent applications and probability of applying for a patent by score. Notes: Based on counts in the treatment period (Period 1). (1) In order to make the graphs comparable, the *y*-axis scale is the same across the two panels. As a result, the two highest values in the first panel are not included in the graph. The interpolation curve is still worked out on the basis of the whole sample.

Table 3
Baseline results: effect of the program on patent applications (treatment periods).

Dep. variable	No. of patent applications			No. of patent applications			No. of patent applications			Dummy (patent applications > 0)		
Model	OLS			Poisson			Negative binomial			Logit		
Sample	All	50% window	40% window	All	50% window	40% window	All	50% window	40% window	All	50% window	40% window
Period 1												
Coeff.	1.793***	0.928**	0.922**	2.127**	6.341***	18.51***	2.021***	1.186**	14.94***	0.773***	0.596**	0.847***
s.e.	(*0.545*)	(*0.350*)	(*0.413*)	(*1.085*)	(*2.422*)	(*4.243*)	(*0.723*)	(*0.462*)	(*1.821*)	(*0.219*)	(*0.251*)	(*0.229*)
Order pol. min AIC	0	0	0	2	2	2	2	0	2	0	0	0
Obs	612	309	246	612	309	246	612	309	246	612	309	246
Alpha							10.36***	10.47***	8.97***			
Period 2												
Coeff.	1.328***	0.678***	0.646*	2.128*	7.369**	29.62***	2.043**	1.124**	30.16***	0.736***	0.664**	1.032**
s.e.	(*0.447*)	(*0.277*)	(*0.318*)	(*1.176*)	(*3.283*)	(*0.230*)	(*0.813*)	(*0.509*)	(*0.704*)	(*0.252*)	(*0.338*)	(*0.362*)
Order pol. min AIC	0	0	0	2	2	2	2	0	2	0	0	0
Obs	612	309	246	612	309	246	612	309	246	612	309	246
Alpha							11.18***	10.61***	8.70***			

Notes: The table shows the estimates of the coefficient β of model (1) using different outcome variables, models and firm samples. Patent applications are cumulated starting from 1 year after the assignment (for Period 1) or 2 years (Period 2) onward, using all the data available, although for the last two years (2010 and 2011) the data are incomplete. The polynomial of order 0 is the difference in mean between treated and untreated firms. Robust standard errors clustered by score in italics. The meaning of parameter alpha is explained in Section 4. *, **, and ***: significant at 10%, 5%, and 1%, respectively.

Table 4
Baseline results: marginal effects of the treatment and averages for untreated firms (treatment periods).

Dependent variable	No. of patent applications			Dummy (patent applications > 0)		
Model	Negative binomial			Logit		
Sample	All	50% window	40% window	All	50% window	40% window
Period 1						
Marginal effect	0.867	0.928	11.58	0.125	0.089	0.122
Order pol. min AIC	2	0	2	0	0	0
Average number of patent applications (untreated firms)	0.609	0.408	0.432	–	–	–
Percentage of firms with strictly positive patent applications (untreated firms)	–	–	–	0.147	0.142	0.122
Period 2						
Marginal effect	0.577	0.678	5.792	0.109	0.091	0.132
Order pol. min AIC	2	0	2	0	0	0
Average number of patent applications (untreated firms)	0.517	0.326	0.324	–	–	–
Percentage of firms with strictly positive patent applications (untreated firms)	–	–	–	0.132	0.122	0.095

Notes: Marginal effects are computed as the differences between the expected values of the estimated model for treated and untreated firms: $E(y|x=x_1, d=1) - E(y|x=x_0, d=0)$. For the Poisson and the negative binomial models they measure the increase in the number of patents due to the treatment; for the logit model, the increase in the probability of patenting. See Section 5 and the notes to Table 3.

incentives have been more effective in increasing R&D investment when they were disbursed to smaller firms (see Lach, 2002 and Gonzalez et al., 2005). This question is relevant for policy design, given that finding heterogeneous effects across firms of different size has straightforward policy implications. In the remaining part of this section we verify this hypothesis by breaking down the sample by firm size and estimating the following equation:

$$Y_i = F\left[(1-T_i)\sum_{k=1}^{2}\alpha_k Size_i^k + T_i\sum_{k=1}^{2}\beta_k Size_i^k + (1-T_i)\sum_{k=1}^{2}\sum_{p=0}^{2}\gamma_{kp} Size_i^k (S_i)^p + T_i\sum_{k=1}^{2}\sum_{p=0}^{2}\gamma'_{kp} Size_i^k (S_i)^p\right] + \eta_i \qquad (2)$$

where the firms' size dummies are interacted with the treatment dummy and the score; $Size^k$ is equal to 1 if sales are below the median (Small firms) and equal to zero otherwise (Large firms). Note that the model allows for heterogeneous parameters between small and large firms across the threshold through the interaction of the treatment dummy and size. In model (2) the parameter β_k is the estimate of the causal effect of the program for firms of size k. The exercise is carried out on the 557 firms (of the complete sample of 612) for which information on sales is available.

The results are shown in Table 5. The effect on the number of patent applications turns out to be positive and statistically significant for small and large firms alike. Interestingly enough, the impact is greater for small firms. According to the estimated marginal effects, thanks to the program small firms increase the number of patent applications by 0.28, almost twice the mean for small untreated firms (0.15); large firms increase patent applications by 1.54, around 1.2 times the mean for large untreated ones (1.25).

As regards the probability of patenting, the right-hand side of Table 5 shows that the overall positive effect previously found is due to small firms, whereas patent probability for large firms is unaffected by the policy. For small enterprises the estimated marginal effect of the grants is also very substantial, more than twice the average probability for untreated firms. For large firms the marginal effect is very close to zero.

7. Robustness

In this section, we carry out several robustness exercises to test the validity of our empirical design and the sensitivity of our results.

Table 5
Baseline results by firm size (treatment period).

Dependent variable	No. of patent applications			Dummy (patent applications > 0)		
Model	Negative binomial			Logit		
Sample	All firms	Small firms	Large firms	All firms	Small firms	Large firms
Coeff.	1.889***	3.997***	1.433**	0.691***	1.114**	0.191
s.e.	(*0.704*)	(*1.102*)	(*0.697*)	(*0.221*)	(*0.479*)	(*0.255*)
Order pol. (min AIC)	2	2	2	0	0	0
Marginal effect	0.801	0.281	1.542	0.109	0.116	0.002
Average number of patent applications (untreated firms)	0.674	0.154	1.250			
Percentage of firms with strictly positive patent applications (untreated firms)				0.162	0.054	0.308

Notes: The table shows the estimates of the coefficient β of model (1) based on the sample of 557 out of 612 firms for which Cerved company account data are available. Firm size dummies are interacted with the treatment dummy and the score; a firm is small (large) if its sales are below (above) the median. (2). Robust standard errors clustered by score in brackets. ** and ***: significant at 5%, and 1%, respectively.

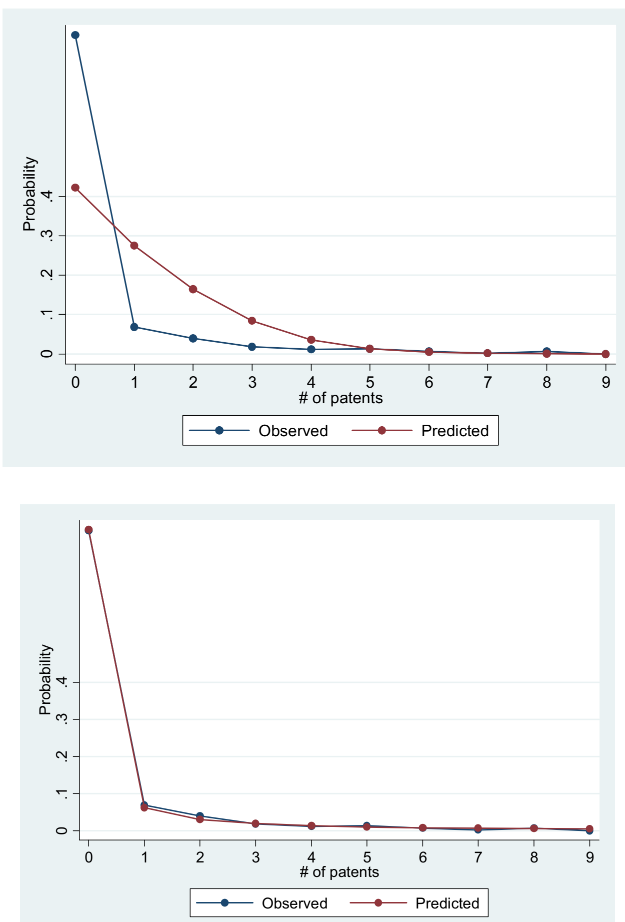

Fig. 5. Fitted probability: Poisson & negative binomial. Notes: Predicted probability from estimations of the Poisson and the negative binomial model (whole sample; quadratic function).

7.1. Econometric model

The theory suggests that the excess zeros may be generated by a separate process from the count values. In the case of patent activity, the decision to start patenting may be determined by other factors than those prompting firms that already patent to increase the number of applications (Lotti and Schivardi, 2005 for Italian evidence). The literature relies on two main models, the zero-inflated model and the hurdle model (Cameron and Trivedi, 2005, p.680). We estimated a zero-inflated Poisson and a negative binomial model, which supplement a count density (Poisson or negative binomial) with a binary process for zeros (logit). The estimated effect of subsidies is similar to that of the baseline model in Table 4. According to the results of Vuong's (1989) closeness test, the standard Poisson model is rejected in favor of the correspondent zero-inflated model, while the negative binomial model turns out to be statistically equivalent to the zero-inflated negative binomial specification.[32]

7.2. Falsification tests

The regression discontinuity identification strategy relies on the continuity assumption, which requires that the potential outcome should be smooth around the cut-off point in the absence of the program. There is no direct way of verifying this hypothesis. However, we can run some indirect tests. First of all, we verify whether the available firm observables are continuous at the cut-off before the program. If we do not observe jumps, it is plausible that the outcome variable would have also been continuous without the treatment. The exercise is run using the observables of Table 2 (some of them scaled by sales) as outcome variables, and estimating model (1) over the year before the treatment. As usual, we select the best specification which minimizes AIC. We found no discontinuities for any of the variables examined. The results, which are not shown, are available on request.

Another way to test for the continuity assumption is to verify whether the outcome variable before the program is smooth at the cut-off. If the jump in patents detected for treated firms is due to the grant, in the absence of treatment we should not find any discontinuity. To carry out this test, we re-estimated model (1) for the cumulated number of patent applications (by the Poisson model and the negative binomial models) and for the probability of patenting (logit model) over two different pre-treatment periods: 5 years (Period A) and 6 years (Period B) before the program, both ending the year of the tender. Fig. 6 and Table 6 show that before the program there were no positive discontinuities of the functions around the cut-off. There is some evidence of slightly significant discontinuity in the probability of patenting, but only when estimated over the 50 per cent sample, and the jump vanishes once we take into account the sample closest to the cut-off. We interpret these findings as further evidence of the positive impact of the policy.

[32] The coefficient estimated over the whole sample by the zero-inflated Poisson is 1.837 (robust standard error clustered by score = 1.461; quadratic interpolation) and the Vuong Test = 4.33**. The coefficient estimated over the whole sample by the zero-inflated negative binomial is 2.258 (robust standard error clustered by score = 0.723; quadratic interpolation) and the Vuong test = 0.95.

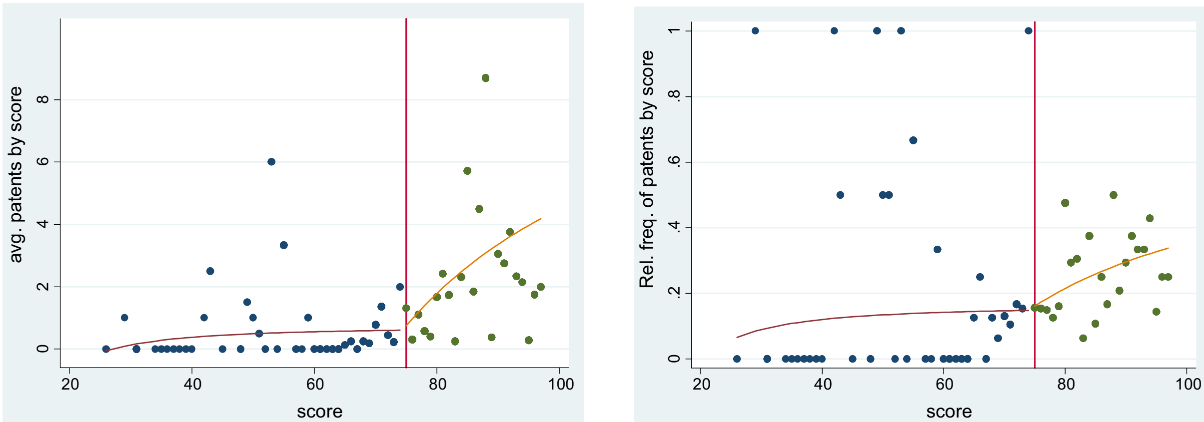

Fig. 6. Number of patent applications in the 5-year pre-treatment period on the left and probability of applying for a panel on the right; linear interpolation.

Table 6
Robustness: no jumps at the cut-off over the pre-treatment periods.

Dependent variable	No. of patent applications			No. of patent applications			Dummy (patent applications > 0)		
Model	Poisson			Negative binomial			Logit		
Sample	All	50% window	40% window	All	50% window	40% window	All	50% window	40% window
Period A									
Coeff.	0.127	3.646	3.130	0.498	−0.183	0.306	0.748	0.515*	−1.274
s.e.	(*0.864*)	(*2.366*)	(*3.977*)	(*0.615*)	(*1.165*)	(*0.352*)	(*0.526*)	(*0.272*)	(*0.801*)
Order pol. min AIC	2	2	2	2	1	0	2	0	1
Obs	612	309	246	612	309	246	612	309	246
Alpha				11.92***	12.30***	12.19***			
Period B									
Coeff.	0.214	3.783	3.736	0.581	0.637*	0.362	0.337	0.600**	−1.013
s.e.	(*0.869*)	(*2.364*)	(*4.124*)	(*0.624*)	(*0.376*)	(*0.361*)	(*0.305*)	(*0.258*)	(*0.793*)
Order pol. min AIC	2	2	2	2	0	0	1	0	1
Obs	612	309	246	612	309	246	612	309	246
Alpha				11.58***	12.50***	12.76***			

Notes: The table shows the estimates of the coefficient β of model (1) using different outcome variables. In Period A patent applications are cumulated in 2000–2004 for the firms of the first tender and in 2001–2005 for those of the second (5-year period). Period B includes patents registered in 1999–2004 for the firms of the first tender and in 2000–2005 for those of the second (6-year period). The polynomial of order 0 is the difference in mean between treated and untreated. Robust standard errors clustered by score in italics. *, **, and ***: significant at 10%, 5%, and 1%, respectively.

7.3. Difference-in-differences

The availability of data on patents in the pre-program period allows us to assess by a difference-in-differences (diff-in-diffs) model whether the patenting activity of recipient firms changed significantly after the policy, by using non-recipient firms near to the cut-off score as a control group. This is an informative exercise given that firms' innovation activity is highly persistent (see, for example, Antonelli et al., 2012) and diff-in-diffs estimates control for potential differences in the performances of treated and untreated firms before the program. Thus, we also run the following difference-in-differences estimation over the samples near the threshold:

$$Y_{it} = F[\beta_0 + \beta_1 dTreat_i + \beta_2 dPeriod_t + \beta_3(dPeriod_t \times dTreat_i)] + \eta_{ti} \quad (3)$$

where Y is the outcome variable; $t = 1, 2$ are, respectively, the pre-program period (5-year time span ending the year of the tender) and the post-program period (5-year time span after the tender); *dPeriod* is a dummy variable equal to 1 in the post-program period (treatment period) and zero otherwise; and *dTreat* is the dummy for the treated group of firms. The coefficient of interest is β_3, which multiplies the two dummies and which is equal to 1 for those observations in the treatment group in the post-program period (treatment time-span). *F*() is an exponential link when the outcome variable is the number of patents and a logit link when the dependent variable is a dummy equal to 1 for firms with at least one patent application.[33] The results displayed in Table 7 include a logit DID and a Poisson DID.[34] Although OLS estimates in the presence of non-normal residuals might provide biased standard errors, we also add the OLS estimates for illustrative purposes. The exercise is carried out over the 50 and 40 per cent samples closer to the cut-off point, because in these samples the treated and untreated firms they include are more similar.

There is evidence of a significant effect of the subsidies in terms of a higher number of patents in both samples and both models (OLS and Poisson). However, the interaction term is positive, but not statistically significant, in the logit model. Read in tandem, the results on the number of patents and probability of patenting suggest that the effect of the policy is positive and significant on both the

[33] When the model is nonlinear and the variables are dichotomous or limited, it is no longer true that the coefficient of the interaction term between two variables measures the effect of a change in both variables, because the real effect includes some cross-derivatives or differences (Ai and Norton, 2003). However, Puhani (2008) proves that the coefficient of the interaction term can still be interpreted as the treatment effect, even if the model is nonlinear.

[34] In this simple specification without other exogenous regressors, the negative binomial model and the Poisson model coincide.

Table 7
Robustness: difference-in-differences estimates.

Dependent variable	No. of patent applications		No. of patent applications		Dummy (patent applications > 0)	
Model	OLS		Poisson		Logit	
Sample	50% window	40% window	50% window	40% window	50% window	40% window
Coeff.	0.499*	0.646*	0.668**	0.835***	0.081	0.467
s.e.	(*0.271*)	(*0.332*)	(*0.286*)	(*0.322*)	(*0.299*)	(*0.336*)
Obs	618	492	618	492	618	492

Notes: The table shows the estimates of the coefficient β_3 of model (3), OLS linear DID, Poisson DID and Logit DID. Patents are cumulated starting from 1 year after the assignment or 5 years (Period 1). Robust standard errors clustered by firms. *, **, and ***: significant at 10%, 5%, and 1%, respectively.

Table 8
Robustness: Kernel estimations.

Order of local polynomial	No. of patent applications			Dummy (patent applications > 0)		
	Bandwidth (score points)			Bandwidth (score points)		
	50	9	7	50	9	7
0	1.679*** (*0.593*)	1.011*** (*0.365*)	1.026** (*0.436*)	0.109*** (*0.032*)	0.103*** (*0.038*)	0.110*** (*0.036*)
1	0.821 (*0.644*)	1.473* (*0.828*)	1.895** (*0.878*)	0.072* (*0.042*)	0.196*** (*0.058*)	0.216*** (*0.066*)
2	1.106 (*1.317*)	3.090* (*1.659*)	3.933*** (*1.179*)	0.123** (*0.058*)	0.284 (*0.208*)	0.273** (*0.109*)

Notes: We estimated the model using the triangular kernel combined with three different bandwidths for each sub-sample and various polynomials. Bandwidths of 50, 9 and 7 score points on each side of the cut-off spans respectively the full sample, 50 per cent and 40 per cent of the sample around the cut-off. Bootstrapped standard errors (100 replications) clustered by score in italics. Polynomial of order 0 is the difference in mean between treated and untreated. *, **, and ***: significant at 10%, 5%, and 1%, respectively.

intensive and extensive margin overall, but weaker, though positive, on the extensive margin only.

7.4. Covariates

In principle, with the RD design it is not necessary to include firm covariates to obtain consistent estimates of the treatment effect, since it is assumed that around the threshold the treatment is randomly assigned. Nevertheless, including some pre-treatment firm-observable variables in model (1) can increase the precision of our estimates, and it can also control for potential imbalances between treated and untreated firms that might be correlated with the outcome variable, e.g. for differences in sectoral composition. This is important because there is evidence that sectors differ in their propensity to patent (see, for example, Lotti and Schivardi, 2005).

First, we introduce two different sets of sectoral dummies: either for each macro-sector (agriculture, manufacturing and mining, construction, services, advanced business services) or for each of the 2-digit sectors presented in Table 1. The results (not shown but available on request) are remarkably similar to the baseline ones. Next, we introduce some firm covariates into the regression to check for any imbalances between treated and untreated firms, as previously done with the sectoral dummies. In particular, we include those for which the differences between recipient and non-recipient firms shown in Table 2 are the largest (gross operating margins/sales, cash flow/sales, financial costs/debt, capital stock). The results of this exercise are qualitatively comparable to those of the baseline (they are not shown but available upon request).[35]

[35] Note that this exercise is run over the 557 firms for which balance-sheet information is available (see Section 6.1). Here, "Services" stands for trade, transport and hotels, whereas "Advanced services" includes real estate, renting, ICT, research and development, and business services.

7.5. Changing patent reference year

We also check whether the date of application matters. Up to now, we have used the application date, i.e. the date on which the patent was filed with the EPO. However, the PATSTAT dataset also gives us the priority date, i.e. the date on which the application was first filed (usually with the applicant's domestic patent office), which is usually closer to the date of the invention. When patents are counted according to the priority year, we again obtain results (available on request) that are not substantially different from the baseline ones.

7.6. Kernel estimates

Parametric models provide inconsistent estimates if the model is misspecified. To check for the robustness of the results obtained with the parametric non-linear model, we estimate the baseline model using a non-parametric kernel. Table 8 shows triangular kernel estimates using different symmetric bandwidths (50, 9 and 7 score points below and above the threshold). The results are again similar to those of the baseline. As regards the effect on the number of patents, in the great majority of cases the coefficients are statistically significant and of similar magnitude to the previous ones. In a few cases the higher standard errors of the non-parametric model make the coefficients statistically non-significant.

8. Conclusions

This paper evaluates the impact of an R&D subsidy program in Emilia-Romagna, a region of northern Italy, on the innovation activities of recipient firms. Unlike most of the literature, we look at the effect of R&D grants on innovation outputs rather than on innovation inputs, measuring firm innovation by patenting activity.

Using a regression discontinuity method, we find a positive impact of the program on the number of patent applications of subsidized firms. The effect turns out to be significantly greater for smaller firms than for larger enterprises. We also find that the program has a positive impact on a firm's probability of applying for a patent, though this effect, by itself, is weaker on the whole than the previous one: in this respect the program proved to be effective only for smaller firms. Our results are robust to a number of sensitivity exercises and falsification tests and are also confirmed by a diff-in-diffs identification strategy.

According to our estimates, one additional patent application requires grants of between €206,000 and €310,000 disbursed to the firms by the regional government.

The analysis suggests some useful policy indications. The first stems from the negative correlation between the effectiveness of the policy and firm size. The smaller the firm, the greater the impact of the policy on the intensity and probability of patenting. Some suggest that this negative relationship may be due to financial frictions affecting smaller innovative firms more strongly (Bronzini and Iachini, 2014), but the reasons why programs are often more effective for smaller than for larger firms, as shown by Lach (2002) and Criscuolo et al. (2012), among others, certainly merit further analyses.

Another policy implication stems from the fact that the regional programs seemed to be more effective than national programs. As regards Italy, Merito et al. (2007) and de Blasio et al. (2011) found that two different programs implemented at national level had no impact, whereas Corsino et al. (2012), Fantino and Cannone (2013), and our paper find positive effects of similar R&D-support programs implemented at regional level. We can suggest a couple of possible explanations. First, regional programs are more closely aimed at smaller firms, for which public support proved to be more effective on the whole. Second, regional policy makers may well have a better knowledge of the local economic environment and of regional firms' activity, which could facilitate policy design and implementation. However, further research is needed in order to draw stronger conclusions on the supposed superiority of locally managed over national programs.

It is worth reminding that our research suffers from at least two limitations. Firstly, as discussed extensively in section four, we use patents as measure of innovation but we are aware that many important firm innovations are not patented. In addition, we did not consider explicitly the general equilibrium effects of the program. Although ours is a relatively standard approach in the program evaluation literature, it is also possible that the program affected firms outside our sample that we did not observe, for example through some spillover effects. These interesting issues are left for the future research.

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