

# 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 on innovation by beneficiary firms. In order to verify whether the subsidies enabled firms to increase patenting activity, we exploit the mechanism used to allot the funds. Since only projects that scored above a certain threshold received the subsidy, we use a sharp regression discontinuity design to compare the number of patent applications, and the probability of submitting one, of subsidized firms with those of unsubsidized firms close to the cut-off. We find that the program had a significant impact on the number of patents, more markedly in the case of smaller firms. Our results show that the program was also successful in increasing the probability of applying for a patent, but only in the case of smaller firms.

**JEL Classification:** R0, H2, L10.

**Keywords:** research and development, investment incentives, regression discontinuity design, patents.

## Contents

1. Introduction.....	5
2. The program to support R&D.....	7
3. Outcome variables and data.....	9
3.1 Patents as a proxy for innovation .....	9
3.2 Data.....	12
4. Empirical strategy.....	13
5. Results.....	16
5.1 Descriptive statistics and graphical analysis .....	16
5.2 Baseline econometric results .....	18
5.3 Results for small and large firms .....	20
6. Robustness .....	21
7. Conclusions.....	25
References .....	27
Tables and figures.....	31

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## 1. Introduction<sup>1</sup>

The need to subsidize private innovative activity is based on solid theoretical arguments that date back at least 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 research and invention ends up being below the socially optimal level (for a formal theoretical discussion of R&D output as a public good and the role of subsidies, see Spence, 1984).

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

Most of the papers assess whether R&D incentives have additional effects on firms' *innovation input*, e.g. on investment in R&D, tangible assets or employment.<sup>2</sup> By contrast, micro-econometric studies of the impact of subsidies on firms' *innovation output* are relatively scant. Among them, Branstetter and Sakakibara (2002) pointed out that public-sponsored research consortia increased the patenting activity of Japanese firms participating

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<sup>2</sup> See for example: 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), Takalo et al. (2013), de Blasio et al. (2011), and Bronzini and Iachini (2011). For excellent reviews of earlier

in a consortium. Bérubé and Mohnen (2009) found that Canadian firms benefiting from R&D tax credits and R&D grants are more innovative, in terms of new products, than firms that take advantage of R&D tax credits alone. More recently, Czarnitzki et al. (2011) found a positive effect of R&D tax credits in Canada on the number of new products introduced by recipient firms, whereas Cappelen et al. (2012) did not find that a similar policy in Norway had any impact on patenting activity and the introduction of new products by beneficiary enterprises.

This paper contributes to this stream of research. We evaluate the impact of a placed-based policy for innovation implemented in Emilia-Romagna, a region of northern Italy, on the recipient firms' patenting activity. Emilia-Romagna is a significant case study for our purposes: it is Italy's third-largest industrial region, accounts for more than 10 per cent of Italian patents, and boasts the highest patent intensity among the Italian regions.<sup>3</sup>

We contribute to the existing literature in several respects. Firstly, we adopt a quasi-random identification strategy. Since recipient and non-recipient firms are inherently different, a central issue in the program evaluation literature is to adopt a strategy that allows the researcher to correctly identify the effect of the policy. In our case we exploit the mechanism by which the funds were assigned. The program supported firm innovation with grants that reduced the costs of innovative activities. It established that only eligible projects that scored above a certain level on an assessment by a technical committee would be subsidized. In order to estimate the effect of the policy, we compare the patenting activity of subsidized and unsubsidized firms close to the threshold score, using a sharp regression discontinuity design (Hahn et al., 2001; Lee and Lemieux, 2010). This strategy allows us to draw inferences regarding the policy's impact using the quasi-randomness of the assignment of the subsidy around the cut-off, which makes recipient firms and non-recipient firms around the threshold comparable.

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studies, see David et al. (2000), Klette et al. (2000) and, on the impact of fiscal incentives, Hall and Van Reenen (2000).

<sup>3</sup> For the period 1995-2009 Emilia-Romagna registered an average of more than 160 patents per year per million inhabitants, more than the double the Italian average (Istat, Indicators for development policies <http://www.istat.it/it/archivio/16777>, June 2013).

Furthermore, we evaluate a place-based policy that is managed at the local level. The regional dimension allows us to further reduce the unobserved heterogeneity among enterprises by comparing firms located in the same area and thus more similar than those participating in nationwide programs. On the other hand, our assessment allows us to shed further light on the impact of place-based policies managed by regional government, which have been little studied to date even though they absorb a large share of total public transfers to the private sector.<sup>4</sup>

Overall we find that the program enhanced the number of patent applications submitted by recipient firms, especially for smaller ones. Our results suggest that the program also succeeded in increasing the probability of patenting, but only for small enterprises.

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

## **2. The program to support R&D**

The objective of the policy 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 office in Emilia-Romagna and are willing to implement innovative projects in the region are

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<sup>4</sup> In Italy, for example, 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 place-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 and Viladecans-Marsal and Arauzo-Carod (2012) for Spain, while the effects of place-based policies in the United States are examined by Moretti and Wilson (2013). On the effects of regional incentives for firms' innovation activities in Italy, see Gabriele et al. (2007), Corsino et al. (2012), and Fantino and Cannone (2013), who assess the policies implemented in Trentino Alto Adige and Piedmont, respectively. Bronzini and Iachini (2011) evaluate the effects of Emilia-Romagna's program on different outcome variables (innovation inputs), using different samples of firms and econometric models.

eligible. Firms are not allowed to receive other types of public subsidies for the same project.<sup>5</sup>

The program, financed and managed by the regional government, 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 such outlays as the cost of machinery, equipment and software, purchase and registration of patents and licenses, employment of researchers, use of laboratories, contracts with research centers, consulting and feasibility studies and, finally, the external costs for the creation of prototypes. In order to be eligible for a grant, the overall costs of a project must be between €150,000 and €250,000.<sup>6</sup>

The grants are assigned after assessment of the proposals by a committee of independent experts appointed by the regional government. For the evaluation process the committee may ask for the opinion of independent evaluators. The committee examines the projects and assigns a score for each of the following profiles: a) technological and scientific (max. 45 points); b) financial and economic (max. 20 points); c) managerial (max. 20 points); and d) regional impact (max. 15 points).<sup>7</sup> Only projects that get a satisfactory score for each profile and a total score of at least 75 out of 100 receive grants.<sup>8</sup>

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,

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<sup>5</sup> See the “Regional Program for Industrial Research, Innovation and Technological Transfer” of the Emilia-Romagna region launched in 2003, which puts into effect the Regional Law 7/2002, Art. 4 (see *Bollettino Ufficiale della Regione* no. 64 of 14 May 2002 and Regional Executive Resolution no. 2038 of 20 October 2003).

<sup>6</sup> 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 is completed.

<sup>7</sup> Point (a) includes the degree of innovation of the project and the adequacy of the technical and scientific resources provided; point (b) the congruence between the project’s financial plan and its objectives ; point (c) past experience in similar projects or the level of managerial competence; and point (d) regional priorities specified in regional law, e.g. projects involving universities and the hiring of new skilled personnel.

<sup>8</sup> For the evaluation process, both the committee and the independent evaluators must comply with the general 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.

respectively. Overall, a total of 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.

### **3. Outcome variables and data**

#### *3.1 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 patenting, 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. Notice that with the first variable we evaluate the impact of the program jointly on the intensive and extensive margin of firm patenting, i.e. on the number of patents of firms that have already started patent activity and on that of new patenting firms, while with the dummy variable we focus only on the second effect (extensive margin).<sup>9</sup>

The number of patents and patent probability 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. In addition, as a robustness check we also run the regression shifting the starting year forward by one year.

Measuring innovation output on the basis of firms' patent applications has pros and cons and deserves 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 firms can use to appropriate returns from their invention or to protect innovation, as keep the secrecy or exploit the 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

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<sup>9</sup> We are unable to focus only on the intensive margin because the sub-sample of firms with patent activity is too small to run the RD 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).

revenues by 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 the invention. 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, *ceteris paribus*, from country to country, over time or across sectors. Cohen et al. (2002), for example, explain the difference in patent propensity between Japanese and US businesses by the fact that US firms perceive patents as a less effective means of protecting property rights than do Japanese firms. 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 the 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.<sup>10</sup> 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, in which firms belong to the same region and the timeframe is relatively short.

Griliches (1990) suggests interpreting patenting activity as an indicator of the increase of economically valuable knowledge and hence a good way to measure 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

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<sup>10</sup> In one of the leading international survey on firms' innovation (the Community Innovation Survey) products and processes are considered new and firms innovative if the firm produced goods and services or adopted processes that are new for the firm but not for the market. Instead, by using patents we are certain to capture important innovations for the market.



Cloodt (2003) conclude that patents are a good indicator to capture innovative performance at firm level.

All in all, we believe that patenting activity is a suitable measure of innovation output that can be used in a satisfactory way in our empirical exercise. The restricted time period examined and the homogeneity of the firms and branches analyzed help to minimize the drawbacks. In addition, this is also a rather standard choice in the econometric literature on innovation.<sup>11</sup>

Because the costs of patenting are among reimbursable outlays under the public program, an 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 otherwise have not carried out. However, we think our exercise captures this effect only marginally, since the costs of filing patent applications with the EPO are low compared with the admissible costs of the proposals. 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 €1,800 to €5,000 (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.<sup>12</sup> 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 75<sup>th</sup> 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.

Consequently, we reject the idea that the costs for patenting are an issue for firms and that, by using patent application as outcome variable, we are capturing the policy's effect on patenting rather than on invention activity. In conclusion, we believe that our exercise shows

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<sup>11</sup> 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 research productivity of participating firms; and Moretti and Wilson (2013) to evaluate the effect of place-based policies on innovation output.

<sup>12</sup> This depends on the EPO's procedure. Before a patent is granted the main costs regard EPO search and examination fees and EPO renewal fees. Once it is 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.

whether the policy played an essential role in enabling firms to carry research and development investment needed to produce innovations.

### 3.2 Data

The analysis is based on three different dataset. First, we use the dataset provided by the Emilia-Romagna region, which give us information on firms participating in the program, e.g. name, score, investment planned, grants assigned, subsidies revoked and renunciations. We pool together the data of the two tenders concluded in 2004 and 2005.<sup>13</sup> Overall 1,246 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 did not receive a 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 plausibly 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 firms involved in renunciations and revocations and firms not subsidized in the first tender but subsidized in the second, for a total of 233 firms. The number of remaining firms is 612.

Second, we use the PATSTAT dataset, which provides information on applications filed and patents registered at the European Patent Office (EPO). More in detail, in order to get the number of patent applications of the firms that participated to the program, we referred to the recent works by Marin (2012) and Lotti and Marin (2013) for the number of patent applications filed from 1977 to 2011 by Italian firms registered in the AIDA (TOP) dataset sourced by Bureau van Dijk (AIDA provides financial statement information for the majority of Italian corporations).<sup>14</sup> Marin uses a very accurate procedure to match the

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<sup>13</sup> We did this to increase sample size. However, if scores are not totally 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: although 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 to patent was not significant in the sample for the second tender. The results of this exercise are not reported but available upon request.

<sup>14</sup> We refer the reader to Marin (2012) and Lotti and Marin (2013) for more details. Here it is worth recalling that due to delays in the publication of EPO data (eighteen months since 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 6,493

PATSTAT and AIDA datasets, enabling him to match more than 80 per cent of the patent applications submitted by Italian companies to the EPO during the observation period. However, in the Marin data set we failed to find information on 75 of the 612 firms of our complete sample; presumably the missing ones were small or non-limited-liability businesses not included in the AIDA dataset used by Marin (2012). Therefore, our next step was to recover information on patent applications by those 75 firms directly from PATSTAT, using their name and address.<sup>15</sup>

The third source set is the Cerved dataset on company account variables, which allows us to compare the characteristics of treated and untreated firms and carry out some robustness exercises.

#### **4. Empirical strategy**

To identify the impact of the program on firms' innovations, we exploit the scored-based fund assignment mechanism. We apply a sharp regression discontinuity (RD) 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). 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 (see Lee and Lemieux, 2010, for a thorough discussion of RD design in economics).

If the treatment, i.e. the subsidies, depends on whether a (forcing) variable exceeds a known threshold, this strategy relies on a general assumption: the agents must not be able to precisely control the forcing variable (Lee 2008). In such 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 such assumption holds, because it is highly unlikely that firms participating in the program can perfectly control the score.

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EPO applicants and 40,112 EPO applications.

<sup>15</sup> We used version 201204, released in January 2013.

In order to test for the discontinuity at the cut-off point several econometric models have been proposed (see, among others, Imbens and Lemieux, 2008, and Lee and Lemieux, 2010). In this paper we resort to a parametric model, although in the robustness section we also carry out non-parametric estimates.

Since the number of patents is a discrete count variable, when we estimate the effect of the policy on patent number by firm, we estimate parametric models suitable for count data as usual in the empirical literature on innovation (Hausman et al., 1984; Cincera, 1997).<sup>16</sup> We use the Poisson model because as long as the conditional mean is correctly specified and the model is estimated in the pseudo-ML form, it is robust to distributional misspecification: it is always consistent (Gourieroux et al., 1984; Santos Silva and Tenreyro, 2006). However, in the Poisson model the conditional mean is assumed to be equal to the conditional variance,  $E(y_i|x_i)=\text{Var}(y_i|x_i)$ . This assumption can be wrong because real data are often overdispersed, i.e.  $\text{Var}(y_i|x_i)>E(y_i|x_i)$ , and overdispersion leads to deflated standard errors and inflated t-statistics (Cameron and Trivedi, 2005, p. 670). To account for overdispersion in our data we also estimate the negative binomial model, a generalization of the Poisson distribution with an additional parameter  $\alpha$ , allowing the variance to exceed the mean. A commonly used function for the variance is  $\text{Var}(y_i|x_i)=\mu_i+\alpha\mu_i^2$ ; where  $\mu_i$  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 case of rejection, we would prefer the negative binomial model for fitting the data.<sup>17</sup>

For the probability to patent, i.e. when we use a dummy variable as the outcome variable if the firm has applied for a patent, we estimate a logit model.

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

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<sup>16</sup> 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).

<sup>17</sup> 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. In addition, this form preserves consistency, provided that the conditional mean is well-specified (Cameron and Trivedi, 2005, p. 677).

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

where  $F(.)$  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.  $Y_i$  is the outcome variable;  $T_i$  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 ( $\gamma_p$  and  $\gamma'_p$ ) are allowed to be different on the opposite side of the cut-off to allow for heterogeneity of the function across the threshold;  $\varepsilon_i$  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).

Equation (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 40 per cent. We also use two other threshold values (60 per cent and 30 per cent), with similar results (they are not shown but available on request).

Outcome variables are calculated on the patent applications submitted by each firm after the program. The treatment period starts 1 year (Period 1) or 2 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.<sup>18</sup> We sum the applications by firm over the time-span considered. In terms of number of patents, our sample of 612 firms has the following profile. Period 1 includes 142 firms with at least 1 patent registered between 2005 and 2011 for the firms belonging to the first tender and between 2006 and 2011 for those of the second tender (Table 1a). This is the main dataset for our experiment. Period 2 includes 126 firms with at least 1 patent registered from 2006 to 2011 for the firms belonging to the first tender and from 2007 to 2011 for those of the second. Pre-treatment (5 years) includes 127 firms with at

least 1 patent registered in 2000-2004 for the firms belonging to the first tender and in 2001-2005 for those of the second. Pre-treatment (6 years) includes 135 firms with at least 1 patent registered in 1999-2004 for the firms belonging to the first tender and in 2000-2005 for those of the second.

Figure 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 variable will be satisfactorily accounted for by the negative binomial model.<sup>19</sup>

## 5. Results

### 5.1 Descriptive statistics and graphical analysis

Table 1b reports the distribution of firms by sector. Since 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 notice 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 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. Notice 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 the treatment should not differ significantly just below and just above the cut-off. Accordingly,

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<sup>18</sup> 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.

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 sales, valued added, assets 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. Table 2b shows that before the program treated firms have a higher average number of patent applications per firm and a higher probability to 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.

Figure 2 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.<sup>20</sup> This drop in the density function might affect the estimates around the cut-off. Thus, as a robustness check we also estimated the model excluding the firms that received a score of 74 from our sample; the results turned out to be practically identical. They are not shown but available upon request.

Before showing the econometric results, we carried out a graphical analysis of the outcome variables as a function of the score. We plotted the number of patent applications and the probability of patenting (share of patenting firms) after the program averaged by

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<sup>19</sup> In the robustness section we use also other count models suggested by the literature for the large amount of zeros.

<sup>20</sup> We do not interpret this drop as signaling that firms just below the threshold were able to manipulate their score. Rather, we believe that the commission of experts avoided assigning a score just below the threshold for understandable reasons: such a score might have been perceived as particularly annoying by dismissed firms and left more room for appeals against the decision. This evidence shows that the commission enjoyed a degree of discretion in assigning the score, a characteristic of the assessment that does not invalidate our design.

score together with two interpolation lines: linear and quadratic (Figure 3). The graphs give visual evidence of a discontinuity, which is stronger in the quadratic case.

## 5.2 *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  $\beta$  of model (1) estimated by OLS, the Poisson model and the negative binomial model. For the probability to 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 the periods end in 2011.

The results are shown in Table 3. From OLS estimates we find a positive effect of the subsidies for the whole sample and for those closer to the cut-off and with both the post-program periods considered (first three columns of the table), although (robust) standard errors should be interpreted with caution as the normality assumption for the residuals can be violated. The coefficients turn out to be positive and statistically significant in all the estimates, also with the Poisson and negative binomial models. In most of the cases the AIC suggests that the best model is the quadratic one. Notice 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. Figure 4 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 the dummy 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).<sup>21</sup> 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 1 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). In relative terms the effect of the treatment is about 1.4 times the average for untreated firms.

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 widow 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 jump 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.<sup>22</sup> 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).

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<sup>21</sup> 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  $\exp(x'\beta)\beta_j$ . 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:  $ME=E(y|x,d=1)-E(y|x,d=0)$ . 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 score accordingly. For instance, when  $t=0$ , the variable  $score\_t=score*t=0$ , while  $score\_t(1-t)=score*t=score$ . Then, the regressors different from zero, at their average value, are equal to  $avg(score)$  or  $avg(score^2)$  in the quadratic specification. See Long and Freese (2005, p.425), for details.

<sup>22</sup> Also in Table 3 the coefficients in 40 per cent window are much higher than those in the 50 per cent one for the same reason. When we consider the zero-degree polynomial in 40% sample, estimated coefficients are quite similar in both subsamples and much smaller (1.142\*\* in Period 1 and 1.097\* in Period 2, the same in Poisson model and negative binomial model, as they are the same model in the case of 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.

The marginal effect of the treatment on the probability of patenting is about 0.12, meaning that the probability to 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.<sup>23</sup>

### 5.3 Results for small and large firms

It has been remarked that it can be harder for small or young firms to finance innovative activity owing to more acute problems of adverse selection (Hall and Lerner 2009). Some empirical evidence supports this argument, showing that incentives have been more effective in increasing R&D investment when they were disbursed to smaller firms (Lach 2002; Gonzalez et al. 2005; Bronzini and Iachini 2011). 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:

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

where the firms' size dummies are interacted with the treatment dummy and the score; Size<sup>k</sup> is equal to 1 (Small) if sales are below the median and to zero otherwise (Large). Notice 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  $\beta_k$  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 size is available.

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<sup>23</sup> This result could be interpreted as a lower-bound effect of the policy because some firms that did not receive the regional subsidies might have applied for (and obtained) non-regional incentives later. However, given the lengthy national procedures for obtaining any incentives in Italy and the relatively short after-policy period examined, it is unlikely that the estimates suffer from a substantial negative bias due to this effect.

The results are shown in Table 5. The effect on the number of patents 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 patents by 0.28, almost twice the mean for small untreated firms (0.15); large firms increase it 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 effective previously found is due to small firms, while 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 as great as the average probability for untreated firms. For large firms the marginal effect is very close to zero.

## 6. Robustness

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

*Econometric model.* – 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 different factors from those that prompt firms that already patent to increase the number of patents (Lotti and Schivardi 2005 for Italian evidence). The literature resorts to 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.<sup>24</sup>

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<sup>24</sup> 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

*Falsification tests.* – 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 to verify 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 firm 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. Figure 5 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.

*Difference-in-differences.* – The availability of data about patents in the pre-program period allows us to assess by a 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. Given the high persistence of firms' innovation activity (see e.g. Antonelli et al. 2012) it might be informative also to look at the change of the difference in

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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.

patent applications between treated and non-treated firms after the policy. Thus, we also run the following difference-in-differences estimation over the samples near to the threshold:

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

where  $y$  is the outcome variable;  $t=1,2$ , where 1 is the pre-program period (5-year time span ending the year of the tender) and 2 is 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. The coefficient of interest is  $\beta_3$ , which multiplies the two dummies and which is equal to 1 for those observations in the treatment group in the treatment period.  $F()$  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.<sup>25</sup> The results displayed in Table 7 include a logit DID and a Poisson DID.<sup>26</sup> We also add the OLS estimates for purposes of comparison, although OLS estimates in the presence of non-normal residuals might provide biased standard errors. The exercise is carried out only over the 50 and 40 per cent samples closer to the cut-off point, because in these samples the treated and untreated firms that 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 in both models (OLS and Poisson). However, the interaction term is positive, but not statistically significant, in the logit model. The results on the number of patents and probability of patenting, read jointly, suggest that the effect of the policy is positive and significant on both the intensive and extensive margin taken together, but it is weaker, though positive, on the extensive margin taken alone.

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<sup>25</sup> 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.

*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-observables 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, manufacture 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 upon request) are remarkably similar to the baseline ones. Next, we introduce some firm covariates into the regression in order 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 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. The results of these exercises are not shown but are available on request.<sup>27</sup>

*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 first date of filling the application (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 get results (available on request) not substantially different from the baseline ones.

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<sup>26</sup> In this simple specification without other exogenous regressors, negative binomial model and Poisson model coincide.

<sup>27</sup> Notice that this exercise is run over the 557 firms for which balance sheet information are available (see section 5.3). Here, “Services” stands for Trade, Transport and Hotels, whereas “Advanced services” includes Real estate, renting, ICT, research and development and business services.

*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 by a non-parametric kernel. Table 8 shows triangular kernel estimates using different 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 large 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.

## **7. Conclusions**

This paper evaluates the impact of an R&D subsidy program in Emilia-Romagna, a region of northern Italy, on innovation activities of recipient firms. Unlike most of the literature, we look at the effect of R&D incentives on innovation output rather than on innovation input, 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 the firm's probability to apply for a patent, although this effect, taken alone, 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.

The analysis has several advantages but also some limits that are worth mentioning. On the one hand, the local dimension of the policy allowed us to minimize the confounding factors and identify more precisely the policy's effect on participant firms in one of the more innovative Italian region. But the regional dimension of the program could also circumscribe the external validity of our results. Although the region is highly representative of the Italian economy, it is nevertheless possible that what we have found for it might be only partially valid for other regions or the whole country.

Even with these caveats, the paper provides some useful indications. The first derives from the negative correlation between the effectiveness of the policy and firm size. The

smaller the firm, the greater was the impact of the policy on the intensity and probability of patenting. Some suggest that this negative relationship may be due to financial frictions that affect smaller innovative firms more strongly (Bronzini and Iachini, 2011), 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, would certainly deserve deeper analyses. As regards Italy, another policy implication comes from the fact that regional programs seemed to be more effective than national programs. 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 overall more effective. 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.



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## Tables and figures

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

Note: 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 200-2004 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.

Table 1b

### DISTRIBUTION OF FIRMS BY 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 manuf. 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
<b>All firms</b>	<b>379</b>	<b>178</b>	<b>100.0</b>	<b>100.0</b>

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

Table 2a

**PRE-ASSIGNMENT STATISTICS: FINANCIAL STATEMENT SHEET ITEMS**

Sample	All				50% window				40% window			
	Untreated	Treated	Diff.	Diff. (t-stat)	Untreated	Treated	Diff.	Diff. (t-stat)	Untreated	Treated	Diff.	Diff. (t-stat)
Sales	13236	49527	10111	2.74***	14331	24442	10111	0.76	11188	19482	8294	0.63
Value added	3319	11883	8565	2.79***	3616	6523	2907	1.32	3345	5136	1791	1.12
Assets	13745	51075	37330	2.67***	15205	32365	17160	1.00	11635	21308	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	14500	11421	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% cut-off neighborhood sample treated firms number 195, untreated 90; in the 40% cut-off neighborhood sample treated firms number 160, untreated 68. \*, \*\*, \*\*\*: significant at 10%, 5% and 1% respectively.

Table 2b

**PRE-ASSIGNMENT STATISTICS: PATENT APPLICATIONS**  
(regressions on the treatment dummy)

Sample	All			50% window			40% window		
	Untreated	Treated	Diff.	Untreated	Treated	Diff.	Untreated	Treated	Diff.
Average 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
Frequency 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% cut-off neighborhood sample treated firms number 211, untreated 98; in the 40% 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 to apply for a patent is regressed on a dummy treatment. \*, \*\*,\*\*\*: significant at 10%, 5% and 1% respectively.

Table 3

**BASELINE RESULTS: EFFECT OF THE PROGRAM ON PATENTS (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
<i>Period 1</i>												
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.	<i>0.545</i>	<i>0.350</i>	<i>0.413</i>	<i>1.085</i>	<i>2.422</i>	<i>4.243</i>	<i>0.723</i>	<i>0.462</i>	<i>1.821</i>	<i>0.219</i>	<i>0.251</i>	<i>0.229</i>
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***			
<i>Period 2</i>												
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.	<i>0.447</i>	<i>0.277</i>	<i>0.318</i>	<i>1.176</i>	<i>3.283</i>	<i>0.230</i>	<i>0.813</i>	<i>0.509</i>	<i>0.704</i>	<i>0.252</i>	<i>0.338</i>	<i>0.362</i>
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  $\beta$  of model (1) using different outcome variables. 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. Robust standard errors clustered by score in italics. The meaning of parameter alpha is explained in Section 4. \*, \*\*, \*\*\*: significant at 10%, 5% and 1% respectively.





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.	<i>0.704</i>	<i>1.102</i>	<i>0.697</i>	<i>0.221</i>	<i>0.479</i>	<i>0.255</i>
Order pol. (min AIC)	2	2	2	0	0	0
Marginal effect	0.801	0.281	1.542	0.109	0.116	0.002
<i>Average number of patent applications (untreated firms)</i>	<i>0.674</i>	<i>0.154</i>	<i>1.250</i>			
<i>Freq. of firms with strictly positive patent applications (untreated firms)</i>				<i>0.162</i>	<i>0.054</i>	<i>0.308</i>

The table shows the estimates of the coefficient  $\beta$  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 italics. \*, \*\*,\*\*\*: significant at 10%, 5% and 1% respectively.

Table 6

**ROBUSTNESS: NO JUMPS AT THE CUT-OFF OVER THE PRE-TREATMENT PERIODS**

Dep. Var.	No. of patent applications			No. of patent applications			Dummy (patent applications >0)		
	Poisson			Negative binomial			Logit		
Model									
Sample	All	50% window	40% window	All	50% window	40% window	All	50% window	40% window
<i>Period A</i>									
Coeff.	0.127	3.646	3.130	0.498	-0.183	0.306	0.748	0.515*	-1.274
s.e.	<i>0.864</i>	<i>2.366</i>	<i>3.977</i>	<i>0.615</i>	<i>1.165</i>	<i>0.352</i>	<i>0.526</i>	<i>0.272</i>	<i>0.801</i>
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***			
<i>Period B</i>									
Coeff.	0.214	3.783	3.736	0.581	0.637*	0.362	0.337	0.600**	-1.013
s.e.	<i>0.869</i>	<i>2.364</i>	<i>4.124</i>	<i>0.624</i>	<i>0.376</i>	<i>0.361</i>	<i>0.3052</i>	<i>0.258</i>	<i>0.793</i>
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  $\beta$  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. \*, \*\*, \*\*\*: significant at 10%, 5% and 1% respectively.

Table 7

**ROBUSTNESS: DIFFERENCE-IN-DIFFERENCE ESTIMATION**

Dep. Var.	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.	<i>0.271</i>	<i>0.332</i>	<i>0.286</i>	<i>0.322</i>	<i>0.299</i>	<i>0.336</i>
Obs	618	492	618	492	618	492

The table shows the estimates of the coefficient  $\beta_3$  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. \*, \*\*,\*\*\*: significant at 10%, 5% and 1% respectively.

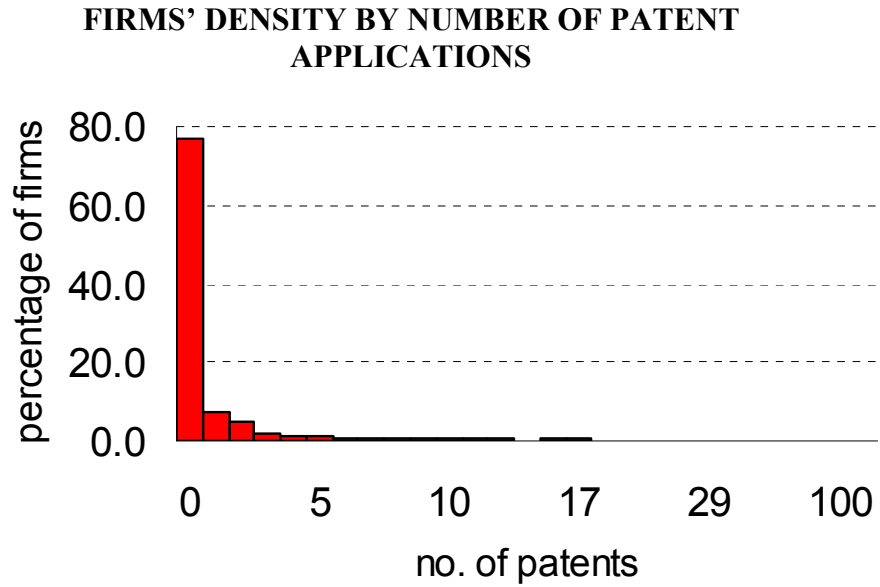
Table 8

**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***	1.011***	1.026**	0.109***	0.103***	0.110***
	<i>0.593</i>	<i>0.365</i>	<i>0.436</i>	<i>0.032</i>	<i>0.038</i>	<i>0.036</i>
1	0.821	1.473*	1.895**	0.072*	0.196***	0.216***
	<i>0.644</i>	<i>0.828</i>	<i>0.878</i>	<i>0.042</i>	<i>0.058</i>	<i>0.066</i>
2	1.106	3.090*	3.933***	0.123**	0.284	0.273**
	<i>1.317</i>	<i>1.659</i>	<i>1.179</i>	<i>0.058</i>	<i>0.208</i>	<i>0.109</i>

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. Robust standard errors clustered by score in italics. \*, \*\*,\*\*\*: significant at 10%, 5% and 1% respectively.

Figure 1



Notes: Counts in the treatment period (Period 1).

Figure 2

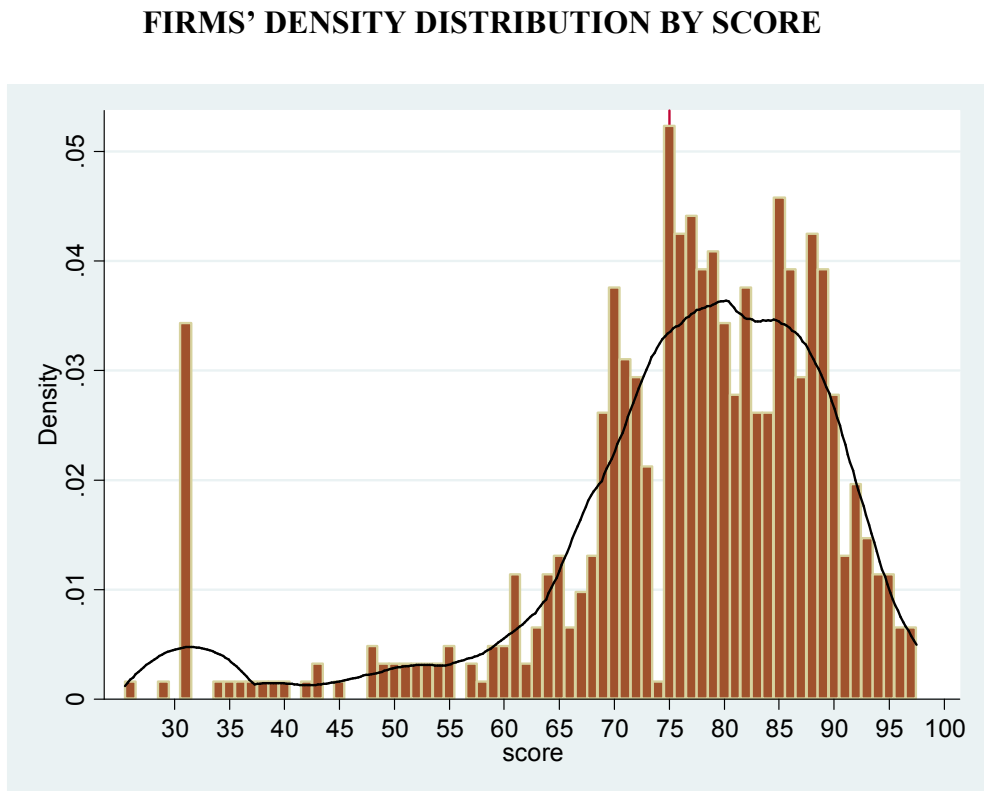
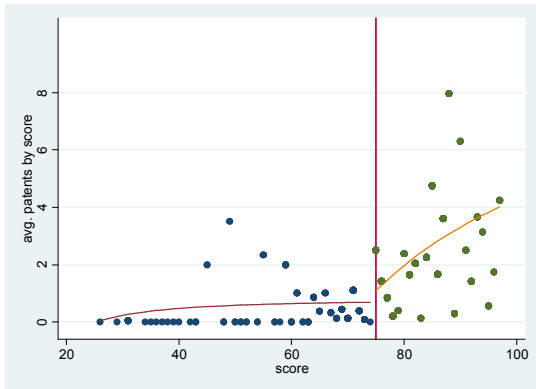


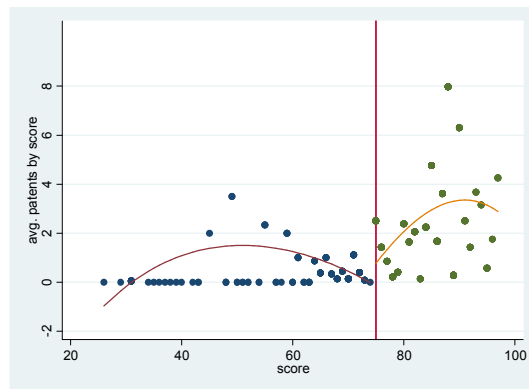
Figure 3

### NUMBER OF PATENT APPLICATIONS BY SCORE TREATMENT PERIOD (1)

*Linear interpolation*

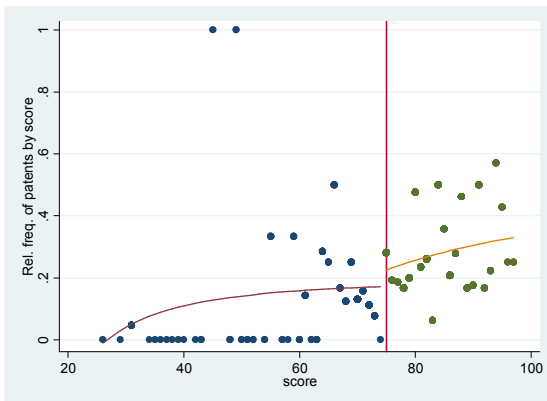


*Quadratic interpolation*

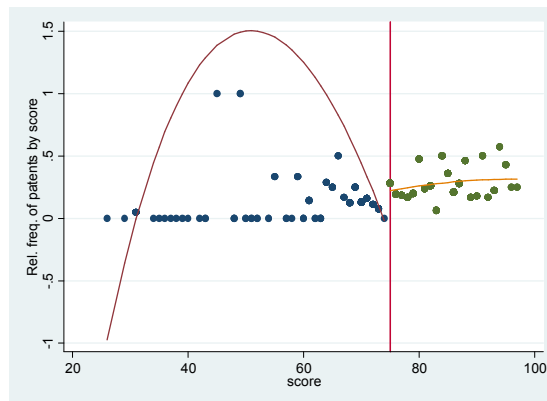


### PROBABILITY OF APPLYING FOR A PATENT BY SCORE TREATMENT PERIOD

*Linear interpolation*



*Quadratic interpolation*

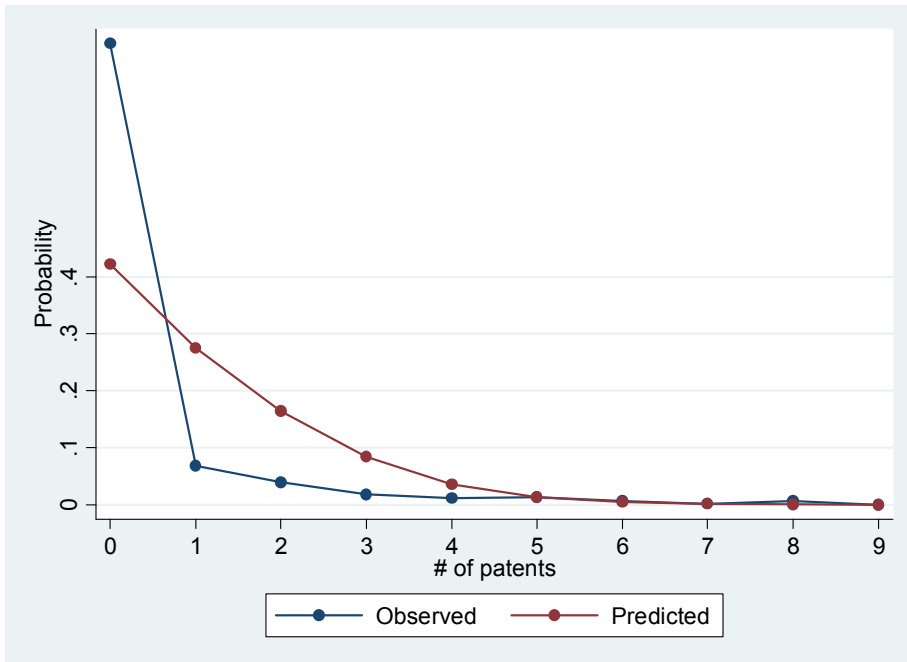


Notes: Based on counts in the treatment period (Period 1). (1) In order to make the graphs comparable, the y-axis scale is the same across the two panels. As a result, the two highest values in the first panel are not included in the graph. The interpolation curve is still worked out on the basis of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

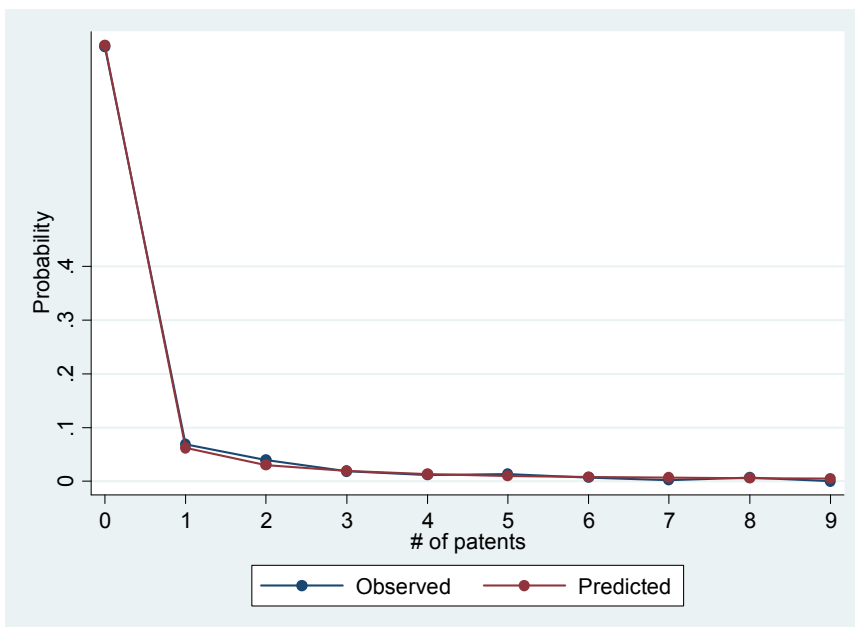
Figure 4

**FITTED PROBABILITY:  
POISSON & NEGATIVE BINOMIAL**

*Poisson*



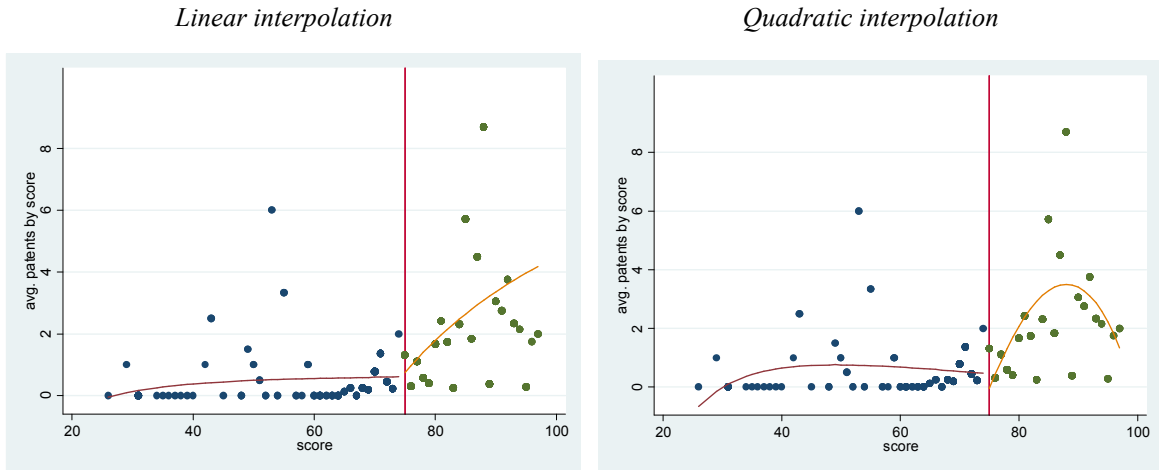
*Negative binomial*



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

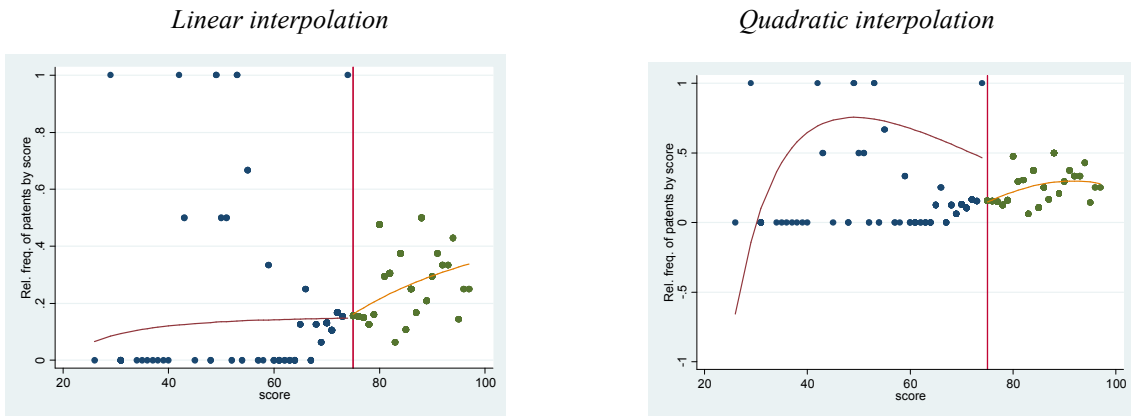
Figure 5

### NUMBER OF PATENT APPLICATIONS BY SCORE PRE-TREATMENT PERIOD



Notes: Based on counts in the 5-year length pre-treatment period.

### PROBABILITY OF APPLYING FOR A PATENT BY SCORE PRE-TREATMENT PERIOD



Notes: Based on counts in the 5-year length pre-treatment period.