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 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, greater for 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.

Keywords: research and development; investment incentives; regression discontinuity design; patents

JEL codes: R0; H2; L10.

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1. Introduction[1]

Most industrial countries have implemented policies to support private research and development (R&D) activity (OECD 2008). On the economic ground there are at least two market failures that justify such incentives. First, since knowledge is non-rival and non-excludable, firms cannot totally internalize the benefits of R&D investment, and the equilibrium level of private R&D spending ends up being below the social optimal level. In these circumstances the policy goal is to bring private R&D expenditure up to the social optimum. A second market failure stems from financial market imperfections. Firms willing to invest in R&D are more exposed to the typical adverse selection problems due to information asymmetries and find it more difficult to finance intangible investments than tangible investments, since the latter are harder to evaluate and intangible assets cannot be offered as collateral. This makes R&D investments more subject to credit constraints, especially if firms are small or young (Hall and Lerner 2009).

The empirical evidence on the effects of R&D policies is voluminous, but the findings are mixed. Most papers assess whether R&D incentives had additional effects on firm *innovation input*, e.g. on investment, employment or proxies of firm performance, such as productivity or sales.[2] By contrast, micro-econometric studies of the impact of subsidies on firm *innovation output* are relatively scant. Branstetter and Sakakibara (2002) show that public-sponsored research consortia increased the patenting activity of Japanese firms participating 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 taking advantage of R&D tax credits alone. More recently, Czarnitzki et

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[2] See for example: Lerner (1999), Busom (2000), Wallesten (2000), Lach (2002), Almus and Czarnitzki (2003), Gonzalez et al. (2005), Gorg and Strobl (2007), Merito (2007), Hussinger (2008), Clausen (2009), Takalo et al. (2010), de Blasio et al. (2011), Bronzini and Iachini (2014). For excellent reviews of earlier studies, see David et al. (2000), Klette et al. (2000) and, on the impact of fiscal incentives, Hall and Van Reenen (2000).

al. (2011) found a positive effect of R&D tax credits in Canada on the number of new products introduced by recipient firms, whereas Cappellen et al. (2012) did not find an impact of a similar policy in Norway on patenting and on the introduction of new products by a sample of beneficiary enterprises.

This paper contributes to this stream of research. We evaluate the impact of an innovation policy implemented in a region of northern Italy (Emilia-Romagna) 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.[3]

The policy supported firm innovative activity with grants that reduced the costs of a large set of tangible and intangible assets, including machinery and equipment, researchers' salaries, and the costs of registering and purchasing patents and licenses. We envisage at least two types of channel through which incentives may affect firms' patents. First, and more importantly, incentives may enable firms to carry out or conclude research activity (innovation input) necessary to produce innovations that would not have otherwise been undertaken. Secondly, incentives may also firms the financial resources they need to patent innovations that they had already produced but would not have patented without the grant.

The program establishes that only eligible projects that score above a certain level on an assessment by a technical committee are subsidized. In order to estimate the effect of the policy, we compare 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 policy 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.

The local dimension of the policy allows us to further reduce the unobserved heterogeneity among enterprises by comparing firms located in the same region and thus with a greater degree of similarity than those participating in nationwide programs. Our

[3] 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 (Istat, Indicators for development policies http://www.istat.it/it/archivio/16777, June 2013).

assessment also shed lights on the impact of place-based policies managed by regional government, little studied to date despite the fact that they absorb a large share of total public transfers to the private sector.[4]

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 has been also successful to increase the firm probability of patenting, but just 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 the data set used. We discuss 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

The government of Emilia-Romagna launched in 2003 the "Regional Program for Industrial Research, Innovation and Technological Transfer" putting into effect Regional Law no. 7/2002, art. 4 (see: *Bollettino Ufficiale della Regione* no. 64 of 14 May 2002 and *Delibera della Giunta Regionale* no. 2038 of 20 October 2003). The program aims at sustaining firms' industrial research and pre-competitive development – the activity necessary to convert the output of research into a plan, project or design for the realization of new products or processes or the improvement of existing ones – in the region. According to the program, the regional government subsidizes the R&D expenditure of eligible firms through grants that cover up to 50% of the costs for industrial research projects and 25% for pre-competitive development projects (the 25% limit is extended by an additional 10% if applicants are small or medium-sized enterprises). Eligible firms are those that have an operative main office and intend to implement the project in the region. The grants subsidized several types of outlays, such as the costs for machinery, equipment and software, the purchase and registration of patents and licenses, the employment of researchers, the use

[4] In Italy, for example, between 2006 and 2011 some €15 billion – almost 40 per cent of the total – went to firms under these programs. For a discussion of the theoretical rationale of place-based policies see Kline (2010).

of laboratories, the contracts with research centers, the consulting and feasibility studies and, finally, the external costs for the realization of prototypes. The maximum grant per project is 250,000 euros.[5]

One important characteristic of the program is that firms cannot receive other types of public subsidies for the same project. This helps the evaluating process given that the impact of the regional program cannot be confused with that of other public subsidies.

The grants are assigned after a process of assessment of the projects carried out by a committee of independent experts appointed by the Regional Government. For the evaluation process the committee may benefit from the assessment of independent evaluators. The committee examines the projects and assigns a score for each of the following elements, like technological and scientific (max. 45 points); financial and economic (max. 20 points); managerial (max. 20 points) and regional impact (max. 15 points).[6] Only projects assessed as sufficient in each profile, and that obtain a total score equal to or more than 75 points receive the grants (the maximum score is 100). For the evaluation process, both the committee and the independent evaluators must comply with the general principles for the evaluation of research specified by the Ministry of Education, University and Research of the Italian Government and the general principles of the European Commission.[7] Notice that according to the design of the program, the likelihood of winning a subsidy is independent from the size of the requested grant.

To date, two auctions have been implemented. The first application deadline was in February 2004, the second in September 2004, and the evaluation process terminated in June

[5] To be eligible projects must be worth at least 150,000 euros. The investment can last from 12 to 24 months. Subsidies are transferred to the firms either after the completion of the project, or in two installments, one at the completion of 50% of the project and the other once the project is completed.

[6] 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 financial plan and the objectives of the project; point (c): past experience collected in similar projects or the level of managerial competence; point (d): regional priorities indicated in the Regional Law such as projects involving universities and the hiring of new qualified personnel.

[7] See the *Linee guida per la valutazione della ricerca, Comitato di indirizzo per la valutazione della ricerca* – Ministry of Education, University and Research; and *Orientamenti concernenti le procedure di valutazione e di selezione delle proposte nell'ambito del VI Programma quadro per la ricerca e lo sviluppo tecnologico*, European Commission. More information on the evaluation process, procedures and principles are reported in the *Delibera della Giunta regionale* no. 2822/2003.

2004 and June 2005, respectively.[8] Overall, a total of about 93 million euros has been granted, corresponding to 0.1% of regional GDP (the same ratio as that between assistance to private R&D and GDP in the national average). Total planned investment equalled 235.5 million euros.

3. Outcome variables and data

We assess the effect of the policy on firm innovation output using two proxies for innovation. First, we utilize 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 has submitted at least one patent application after the policy and zero otherwise. Notice that while with the former variable the impact of the program is evaluated on both the extensive and intensive margin of firm patenting, i.e. on the number of patents of firms that have already started patent activity, and on the new patenting firms. With the dummy variable we focus only on the second effect (extensive margin).[9]

Number of patents and patent probability are measured over the post-program period: from 2005 to 2011 for firms participating to the first auction and from 2006 to 2011 for firms which participate to the second one. As robustness exercise we run the regression also shifting the starting year onward by one year.

The choice to measure innovation output by patents has pros and cons. On the one hand, it is well known that not all innovations are patented. There are several other informal mechanisms, as secrecy or lead time advantages, firms can use to appropriate returns from their invention. The choice to patent depends on a number of factors. For example, firms patent innovation to improve their goodwill reputation or to increase their bargaining power in the market, rather than protect their advances (Cohen et al., 2000). In many cases firms prefer not apply for a patent because they do not want to disclose the invention. A further limit is that the propensity to patent might differ, ceteris paribus, from country to country or

[8] See the *Delibera della Giunta Regionale* no. 1205 of 21 June 2004 and no. 1021 of 27 June 2005.

over time. Cohen *et al*. (2002) for example motivate the different patent propensity between Japan and US firms by the fact that US enterprises perceived patents as a less effective mechanism to protect the property rights than Japanese firms. In addition, the criteria that an innovation must satisfy to be patented (novelty, non-obviousness) change across countries and over time (Nagoaka et al. 2010).

On the other hand, patent is likely the hardest measure of innovation. Compared to other proxies usually collected through surveys, such as the number of new products or process introduced by the firms, they are less prone to personal or subjective considerations. Moreover, patent reflects also the quality of the innovation. To be patented an invention is accurately examined by experts that judge its novelty. On the contrary, reliable information on the quality of the innovation can be rarely gathered from other sources, especially if they are based on personal judgment.[10] Finally, some flaws of patents as a measure of innovation, as the weak comparability over time or across countries, do not apply to our exercise where firms belong to the same restricted regional area and the time windows is relatively short. All in all, even with some caveats we believe that patent propensity is a sound measure of innovation output that can be used in a satisfactory way in our empirical exercise.

We envisage two types of mechanisms through which the policy could boost patenting activity of firms. The first one is that it allowed them to carry out the innovation input activity, such as the research and development investment, necessary to produce innovations and that without the subsidies would have not been made. A second channel is more direct because the expenditures to register or purchase a patent are among the outlays that can be covered by the grant. Therefore firms can use such subsidies to patent innovations that were already produced or were under way. To distinguish among the two channels would be an interesting but challenging question that is beyond the scope of this paper.

[9] 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 only them.

[10] In one of the most used international survey on firms' innovation (the Community Innovation Survey) products and processes are considered new and firm innovative if a firm produced goods and services or adopted processes that are new for the firm but not for the market. On the contrary by using patents we are sure to capture important innovations for the market.

The analysis is based on three different dataset. First, we take advantage of the data set provided by the Emilia-Romagna Region that includes information on firms participating to the program, such as name, score, investment planned, grants assigned, subsidies revoked and renunciations. We pool together the data of the two auctions concluded in 2004 and 2005.[11] 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 deemed insufficient under at least one profile. Note that the strategy is based on the test for discontinuity around the cut-off point, and plausibly omitted firms would have received a total score distant from the cut-off, thus we believe that their exclusion did not bias our results. Finally, we also excluded firms involved in renunciations and revocations and firms unsubsidized in the first auction, but subsidized in the second, for a total of 233 firms. The number of remaining firms is 612.

Second, we use the PATSTAT data set that provides information on the applications submitted and patents registered at the European Patent Office (EPO). More in detail in order to obtain the number of patent applications of the firms that participated to the program, we referred to the recent works by Marin (2011, 2012) and Marin and Lotti (2013) who provided the number of patent applications submitted from 1977 to 2011 by Italian firms registered in the AIDA (TOP) data set sourced by the Bureau van Dijk (AIDA provides balance sheet information for the majority of Italian corporations).[12] Marin uses a very accurate procedure to carefully match PATSTAT and AIDA dataset. With this method he is able to match more than 80 per cent of the patent applications submitted by Italian companies to the EPO during the observed period. However, out of 612 firms of our

[11] We pooled auctions to increase sample size. However, if score is not totally exogenous to the total amount of funds allocated to the programs, scores in the second auction might be affected by this budget constraint. If present, this effect is likely to be negligible. As robustness exercise we re-estimate the model breaking down the two actions and over the two different samples. Results provided us with similar results: 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 in terms of number of patents. Yet, the effect on the probability to patent is not significant in the sample relative to the second auction.

[12] We refer the reader to Marin (2011) 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 data set (February 2012) for the period 2000-2011 contains 6493 EPO applicants and 40112 EPO applications.

complete sample, we did not find in the Marin data set information on 75 firms, plausibly because they are small or non-corporations firms which are not included in the AIDA data set used by Marin (2011). Therefore, we next have recovered information on patent applications on such 75 firms directly from PATSTAT by using their name and address. [13]

The third data set is provided by Cerved group on balance sheet variables utilized to compare the characteristics of treated and untreated firms and carry out some robustness exercises.

4. Empirical Strategy

In order to identify the impact of the program on firms' innovations we take advantage of the mechanism of the funds' assignment based on the score. We apply a sharp regression discontinuity (RD) design to compare the performance of subsidized and non-subsidized firms that have a score 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 comprehensive discussion of the 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 a case, the treatment around the threshold is as if it was 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 the strategy is suitable in our case because it is hard to argue that firms participating to the program can perfectly control the score.

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

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[13] We used 201204 version, released in January 2013.

Since the number of patents is a discrete count variable, when we estimate the effect of the policy on patents' 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), such as Poisson and negative binomial model.[14] We use the Poisson model because as long as the conditional mean is correctly specified and it 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, $E(y_i|x_i)= Var(y_i|x_i)$. This assumption can be inadequate because real data are often overdispersed, i.e. $Var(y_i|x_i)> E(y_i|x_i)$, 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 estimate also the negative binomial model, a generalization of the Poisson distribution with an additional parameter α allowing the variance to exceed the mean. A typical used function for the variance is $Var(y_i|x_i)= \mu_i+ \alpha\mu_i^2$; where μ_i is the conditional mean. If $\alpha=0$ the negative binomial model reduces to the Poisson model. Thus, by testing for $\alpha=0$ we verifies for overdispersion.[15]

As regards the probability to patent, i.e. when we use as outcome variable the dummy 1/0 if the firm has applied for a patent, we estimate a logit model.

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

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

[14] The classical linear model is not adapted, 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).

[15] This quadratic form is the most used in the literature among several possible functions (see Greene 2008), as it well-behaves 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).

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=1 if firm *i* is subsidized (all firms with $Score_i$ >=75) and T_i =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 the score *S* as suggested by Lee and Card (2008).

The equation (1) is also estimated "locally" around the cut-off point using two different sample windows. The wide-window includes 50% of the baseline sample; the narrow-window includes 40% of the baseline sample.

Outcome variables are calculated on patent applications submitted by each firm after the program. The treatment periods starts 1 year (Period 1) or 2 years (Period 2) after the grant is assigned, up to 2011; patents are attributed to the firms using as reference date the application year.[16] We sum up applications by firm over the time-span considered.[17]

Fig. 1 shows the distribution of patents by firms in Period 1. About 77 per cent of the firms present zero patents. Moreover, 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 satisfactory accounted for by the negative binomial model.[18]

[16] The problem of choosing the year to which a patent is attributed 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.

[17] In terms of number of patents, our sample of 612 firms presents as follows. Period 1 includes 142 firms with at least 1 patent registered between 2005 and 2011 for the firms belonging to the first auction and between 2006 and 2011 for those of the second auction. This is the main data set of our experiment. Period 2 includes 126 firms with at least 1 patent registered from 2006 to 2011 for the firms belonging to the first auction and from 2007 to 2011 for those of second auctions. Pre-treatment (5 years) includes 127 firms with at least 1 patent registered in 2000-2004 for the firms belonging to the first auction and in 2001-2005 for those of the second auction. Pre-treatment (6 years) includes 135 firms with at least 1 patent registered in 1999-2004 for the firms belonging to the first auction and in 2000-2005 for those of the second auction.

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

5. Results

In Table 1 is reported the distribution of firms by sector. Since information on sector is drawn from balance sheet data, the sample is a little smaller (557 firms) than our regression sample (612 firms). We notice that there is a large concentration of firms within just a few industries: machinery, electrical and optical equipment, chemical and knowledge-intensive business services. All together they absorb about 60 per cent of the firms' sample. The distribution by sector of treated firms is very similar to that of the untreated ones. However, we find a larger percentage of untreated firms in knowledge-intensive business services, whereas the reverse occurs for the group of coke and chemical products and food and beverages. Notice that treated firms are more numerous than untreated ones, because of the exclusion of the non-scored applicant firms from the second auction.

Table 2a shows the means of several balance sheet variables the year before the publication of the auctions for treated and untreated firms. The RD design relies on the assumption that near the cut-off the treatment is random, as a consequence firm covariates before the treatment should not differ remarkably just below and just above the cut-off. Accordingly, we compare the means of the main balance sheet items of our firms, above and below the cut-off, to carry out a first 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. The cost of debt is also smaller for the former than for the latter. On the contrary 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% 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 larger average number of patent applications by firm, and higher probability to patent, than untreated firms. However such differences drop dramatically, and are no longer statistically significant, when we restrict the sample around the cut-off point. Overall such evidences tend to support our empirical strategy.

The 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 cited exclusion of non-scored untreated firms in the second auction, but density increases substantially near the cut-off. We observe also that just at the score below the cut-off (score=74) the density is lower than at slightly more distant values. We do not interpret this drop as the signal 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. This record could have been perceived as particularly annoying by dismissed firms and potentially would have left more room for appeals against the decision. If any, this evidence shows that the commission enjoys a certain degree of discretion in assigning the score, a characteristic of the assessment that does not invalidate our design.[19]

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 score together with two interpolation lines: linear and quadratic (Fig. 3a-3b). Both graphs show a visual evidence of a discontinuity, which is stronger in the quadratic case.

We now move 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, Poisson and 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 Criterium (AIC), considering three samples around the cut-off: the whole sample, the 50% and 40%-sample windows. Moreover, we estimate the model over two post-program periods: period 1 starts one year after the program, and period 2 start two years after the program; both the periods terminate in 2011.

Results are displayed in Table 3. From OLS estimates it emerges a positive effect of the subsidies for the whole sample and those closer to the cut-off and with both the post program periods considered (first three columns of Table), although (robust) standard errors should be interpreted with caution as normality assumption of residuals can be violated. The coefficients turn out to be positive and statistically significant in all the estimates also with

[19] This drop in the density function might still affect the estimates around the cut-off. As a robustness check, we also kept out of the sample the only firm which received a score of 74; our results turned out to be practically identical.

Poisson and negative binomial model. In most of the cases AIC suggests that the best model is the quadratic one. Notice that the Poisson model is rejected in favour of the negative binomial: in the latter the estimates of the alpha parameter, remarkably larger than zero, reject the hypothesis of variance equal to the mean. Fig. 4 compares the predicted probability of different counts according either to the Poisson or the negative binomial model estimated on the baseline whole 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 we use as outcome variable the dummy for firms that have applied for a patent at least once in the post period program - the results are again positive and statistically significant (see last three columns of the Table 3).

Table 4 reports the marginal effect of treatment for the negative binomial and logit models of Table 3 (given the superiority of negative binomial model over the Poisson we did not compute the marginal effect for the latter).[20]

In the case of the number of patens, 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 the ability of patenting, we compare such effect with the average number of patents of untreated firms (0.61). Hence, in relative terms the effect of the treatment is about 1.4 times the average of untreated firms. As in Table 3, 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% sample windows. It is likely that in 40% sample there are too few firms to precisely estimate the impact of the policy.

[20] As it is well-known, in non-linear models the marginal effect of a change in a regressor is not equals to its coefficient. For the Poisson model (and negative binomial), where E(y|x)=exp(x'β), the marginal effect (ME) of a change in variable j is in general exp(x'β)β. Yet, for an indicator variable, derivatives are not appropriate, because the relevant change is when this variable changes 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 (2006), we compute the marginal effect of treatment as follows: We compute E(y|x=x0.,d=0), that is the expected value of the regression without treatment, where the interaction terms are equals to zero or equals to the average of score accordingly. For instance, when t=0, the variable score_t=score*t=0, while score_(1-t)= score*1=score. Then, the regressors different from zero, evaluated at their average value are equal to avg(score) or avg(score^2) in the quadratic specification. See Long and Freese (2006, p.425 for details).

These results are widely confirmed over the treatment period 2, when we start to count patents two years after the auctions. However, in such a case we found a relatively smaller although still remarkable impact of the policy (in relative term the marginal effect is near the unity).

The marginal effect of the treatment on the probability of patenting is about 0.12, meaning that the probability to patent increases on average around by 12 percentage points thanks to the grant; about 0.8 times the average of untreated firms. Such result is relatively stable across the samples used. Results in period 2 mirror period 1's ones.

It has been pointed out that because of stronger adverse selection problems, for small or young firms can be more difficult to finance innovative activity (Hall and Lerner 2009). Some empirical evidence support such argument showing that incentives have been more effective in increasing R&D investment if they were disbursed to smaller firms (Lach 2002; Gonzalez et al. 2005; Bronzini and Iachini 2014). This is question is relevant for the policy design given that to find heterogeneous effects across firms of different size has straightforward policy implications. In the remaining part of this section we verify such hypothesis by breaking down the sample by firm size and estimating the following equation:

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

where the firms' size dummies are interacted with the treatment dummy and the score; $Size^k$ =1 (Small) if sales are below the median and zero otherwise (Large). Notice that the model allows for heterogeneous parameters between small and large firms across the threshold through the interaction of the dummy treatment 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, out of 612 of the complete sample, for which information on size is available.

In Table 5 are shown the results. On the number of patents the effect turns out to be positive and statistically significant for both small and large firms. Interestingly enough, the impact is greater for small firms than for large ones. According to the estimated marginal effects, thanks to the program small firms increase by 0.28 the number of patents, almost twice the mean of small untreated firms (0.15); large firms increase the number of patents 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, whereas patent probability for large firms is unaffected by the policy. For small enterprises the estimated marginal effect of the grants is also remarkable, more than twice the average probability of 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 sensitiveness of our results.

Econometric model. - The large amount of zero patents in our data set would be dealt with other models for count data. Theory suggests that the excess zeros can be generated by a separate process from the count values. In fact, as regards patent activity, it is possible that to start patenting is determined by factors different from those pushing firms that already patent to increase the number of patents (Lotti and Schivardi 2005 for the Italian evidence). Literature recurs 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 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 (see Table A1 in the Appendix).[21] The standard Poisson model is rejected in favour of the correspondent zero-inflated according to Vuong's (1989) closeness test of the two models.

Falsification tests. - RD identification strategy relies on the continuity assumption, which requires that potential outcome should be smooth around the cut-off point in the

[21] Estimates are reported only for the whole sample; the closest-to-the cut-off samples turn out to be too small for the ML procedure to converge.

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 also the outcome variable would have 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. Table A2 shows that there is no evidence of discontinuities for any variables examined.

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 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 Poisson and negative binomial model), 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 auction. Figure 5a-5b and Table A3 show that before the program almost never there were 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% sample. However, such jumps vanish once we take into account the closest sample to the cut-off. We interpret these findings as further supporting evidence on the positive impact of the policy.

Difference-in-differences. - Data availability about patents in the pre-program period allows us to assess by a diff-in-diffs model whether patenting activity of recipient firms changed significantly after the policy, using non recipient firms near to the cut-off score as control group. Indeed, due to high persistence of innovation activity (see e.g. Antonelli et al. 2012) we might loose information by focusing on the level (number) of patents only in the post-treatment period, whereas it might be informative also too look at the change of the difference in patent applications between treated and non-treated firms after the policy. Thus, thanks to the availability of data on patents before the treatment we run also the following difference-in-differences estimation over the samples near to the threshold:

$$Y_{it} = F[\beta_0 + \beta_1 \mathrm{dTreat}_{\mathrm{i}} + \beta_2 \mathrm{dPeriod}_{\mathrm{t}} + \beta_3 \mathrm{dPeriod}_{\mathrm{t}} * \mathrm{dTreat}_{\mathrm{i}}] + \eta_{ti} \qquad (3)$$

where y is outcome variable; *dPeriod* is a dummy variable for the treatment period and *dTreat* is the dummy for the treated group; t=1,2, where 1 is the pre-program period (5 year time span, ending the year of the auction) and 2 is the post-program period (1 year after the grant is assigned). 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 treatment period. *F( )* is a 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 on patent.[22] Results displayed in Table A4 include a logit DID and a Poisson DID.[23] We also add the OLS estimates for comparison purposes 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% samples closer to the cut-off point, given that treated and untreated firms that fall in them are more similar.

There is evidence of a significant effect of the subsidies in terms of 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 patent 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, although positive on the extensive margin taken alone.

Covariates. - In principle, with the RD design you do not need to include firm covariates to obtain consistent estimates of the treatment effect, since around the threshold the treatment is as it was randomized. Yet, including some pre-treatment firm-observables

[22] 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) That is potentially a problem in a nonlinear DID, because β_3 , the parameter of interest in equation (3) is the coefficient of the interaction term dPeriod x dTreat, and variables are dummies. Likely, Puhani (2008) proves that the cross-differences in a nonlinear DID do not represent the treatment effect, while the coefficient of the interaction term still is. As a result, we can measure the treatment effect in the usual way, even if the model is nonlinear.

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

variables in model (1) can increase the precision of our estimates, moreover it can 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 e.g. 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 shown in Table A5 are remarkably similar to the baseline ones. Next, we introduce in the regression some firm covariates to check for any unbalances between treated and untreated firms, as previously done with the sectoral dummies. In particular we include those for which differences between recipient and non-recipient firms shown in Table 2 are larger (gross operative margins/sales, cash flow/sales, financial costs/debt; capital stock). Results of this exercise, reported in Table A6 are qualitatively comparable to the baseline ones. [24]

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 at EPO. However, in the PATSTAT dtaset it is also available the priority date, that is the first date of filling the application (usually to the applicant's domestic patent office), which is usually closer to the date of the invention. By counting the patents according to the priority year we obtain again results not remarkably different from the baseline ones (Table A6).

Kernel estimates. - Parametric models provide inconsistent estimates if the model is wrongly specified. To check for the robustness of the results obtained by the parametric non-linear model, we estimate the baseline model by a non-parametric kernel. Table A8 shows triangular kernel estimates using different bandwidths (50, 9 and 7 score points below and above the threshold). Results are again similar to the baseline ones. As regards the effect on the number of patents, in the large majority of cases the coefficients are statistically

[24] Notice that this exercise is run over the firms for which balance sheet information area available. Here, "Services" stands for Trade, Transport and Hotels, whereas "Advanced services" includes Real estate, renting, ICT, research and development and business services.

significant and of similar magnitude of the previous ones. Higher standard errors of non-parametric model make in a few cases the coefficients non-statistically significant.

7. Conclusions

This paper evaluates the impact of a unique R&D subsidy program in northern Italy on innovation of recipient firms. Unlike most of the literature, it focuses on the effect of R&D incentives on innovation output rather than on innovation input. We use patenting activity to measure firm innovation.

Comparing the number of patent applications and the probability to patent of subsidized and unsubsidized firms, using a regression discontinuity method, we find a positive impact of the program on the number of patents. The effect on the number of patents turns out to be significantly greater for smaller firms than for larger enterprises.

We also find that the program also has a positive impact on the probability of patenting, although this effect, taken alone, is weaker and limited to the smaller firms. The results are robust to a number of sensitivity exercises and falsification tests and are confirmed by the diff-in-diffs identification strategy.

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TABLES AND GRAPHS

Table 1

DISTRIBUTION OF FIRMS BY SECTOR

Sector	# 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, leather, 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, etc.	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.

Table 2a

PRE-ASSIGNMENT STATISTICS: BALANCE SHEET 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	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 flows/ 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 cost / 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 cost / 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
Intangibile 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 612 firms considered in the policy evaluation exercise. All the variables refer to the first pre-assignment year (2003 for the first auction 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 are 195, untreated 90; in the 40% cut-off neighborhood sample treated firms are 160, untreated 68. *, **,***: significant at 10%, 5% and 1% respectively.

Table 2b

PRE-ASSIGNMENT STATISTICS: PATENT APPLICATIONS

Sample	All				50% window				40% window			
	Untreated	Treated	Diff.	Diff. (t-stat)	Untreated	Treated	Diff.	Diff. (t-stat)	Untreated	Treated	Diff.	Diff. (t-stat)
Average # of patent applications by firm	0.517	2.296	-1.778	-2.069**	0.632	1.061	-0.428	-1.085	0.770	1.046	-0.276	-0.599
Frequency of firms with at least 1 patent	0.137	0.240	-0.103	-2.978***	0.132	0.203	-0.071	-1.511	0.148	0.203	-0.054	-1.001

Notes: The sample includes 612 firms. Variables refer to a 5-year pre-assignment period (2000-2004 for the first auction and 2001-2005 for the second). In the complete sample 415 firms are treated; 197 are untreated. In the 50% cut-off neighbourhood sample treated firms are 211, untreated 98; in the 40% cut-off neighborhood sample treated firms are 172, untreated 74. *, **,***: significant at 10%, 5% and 1% respectively.

Table 3

BASELINE RESULTS: TREATMENT PERIODS - EFFECT OF THE PROGRAM ON PATENTS

Var. Dip.	# of patents			# of patent applications			# 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. Patent applications are cumulated starting from 1 year after the assignment (for Period 1) or 2 years (Period 2) onward, trying to use all the data available, although for the last two years (2010 and 2011) 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 par. 4. *, **,***: significant at 10%, 5% and 1% respectively.

Table 4

BASELINE RESULTS: MARGINAL EFFECTS OF THE TREATMENT AND AVERAGES FOR UNTREATED FIRMS OVER THE TREATMENT PERIODS

Var. Dip.	# 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*	-	-	-
Freq. 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*	-	-	-
Freq. of firms with strictly positive patent applications (untreated firms)	-	-	-	*0.132*	*0.122*	*0.095*

Notes: Marginal effects are computed as differences between the expected value of estimated model for treated and untreated firms: E(y|x=x1.,d=1)-E(y|x=x0.,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 note in Table 3.

Table 5

BASELINE RESULTS BY FIRM SIZE

Dep. Variable	# 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*			
Freq. of firms with strictly positive patent applications (untreated firms)				*0.162*	*0.054*	*0.308*

The table shows the estimates of the coefficient β of model (1) based on the sample of 557 out of 612 firms for which balance sheet (CERVED) 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.

APPENDIX

Table A1

ZERO-INFLATED MODELS

Var. Dip.	# of patents	
Model	Zero-inflated Poisson	Zero-inflated NB
Coeff.	1.837	2.258***
s.e.	*1.461*	*0.723*
Order pol. min AIC	*2*	*2*
Vuong test	4.33***	0.95
Obs	612	612

The table shows the estimates of the coefficient β of model (1) when F() is a combination of a binary process and a Poisson/NB process. Robust standard errors clustered by firms. *, **,***: significant at 10%, 5% and 1% respectively.

Table A2

ROBUSTNESS I: DISCONTINUITY OF COVARIATES IN THE PRE-PROGRAM PERIOD

(OLS regressions)

Variable	Coeff.	s.e.	Order pol. min. AIC
Sales	-5538	*10124*	1
Value Added	-1524	*2590*	1
Assets	4373	*11140*	1
ROA	0.896	*1.062*	0
Leverage	-27.51	*20.62*	1
Gross operating margin / sales	0.052	*0.058*	0
Cash flows/ sales	-0.049	*0.047*	0
Financial cost / debt	-0.018	*0.0118*	0
Labor cost / sales	0.066	*0.057*	0
Total capital stock /sales	-0.121	*0.110*	0
Tangible investment / sales	-0.059	*0.044*	0
Intangible investment / sales	-0.054	*0.032*	0

Notes: The table shows the OLS estimates of the coefficient β of model (1) (in which the link function *F(.)* is the scalar 1) using different covariates, based on the sample of 557 out of 612 firms, for which balance sheet (CERVED) data are available. The period is one year before the program. 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 A3

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

Var. Dip.	# of patent applications			# 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*	*0.3052*	*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 belonging to the first auction and in 2001-2005 for those of the second auction (5-years period). Period B includes patents registered in 1999-2004 for the firms belonging to the first auction and in 2000-2005 for those of the second auction (6-years 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 A4

ROBUSTNESS VII: DIFFERENCE-IN-DIFFERENCE ESTIMATION

Var. Dip.	# of patent applications		# 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

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

Table A5

ROBUSTNESS III: IRRELEVANCE OF SECTOR DUMMIES

Dep. Variable	# of patent applications			Dummy (patent applications>0)		
Model	Baseline	Baseline + Sector Dummy1	Baseline + Sector Dummy2	Baseline	Baseline + Sector Dummy1	Baseline + Sector Dummy2
Coeff.	1.889***	2.050***	2.119***	0.691***	0.517**	0.543**
s.e.	*0.704*	*0.769*	*0.751*	*0.221*	*0.236*	*0.223*
Order pol. min AIC	2	2	2	0	0	0
Obs	557	557	557	557	557	557

The table shows the estimates of the coefficient β of model (1) based on the sample of 557 out of 612 firms, for which balance sheet (CERVED) data are available. Sector Dummy1 is based on 5 macrosectors (agriculture, industrial, construction, services, advanced services, others); Sector Dummy2 is a 16-sector dummy, as shown in Table 1. In the last two columns are reported the results of the estimates of model (2). Robust standard errors clustered by score in italics. *, **,***: significant at 10%, 5% and 1% respectively.

Table A6

ROBUSTNESS IV: IRRELEVANCE OF COVARIATES

Dep. Variable	# of patent applications		Dummy (patent applications>0)	
Model	(1) Baseline	(3)	(1) Baseline	(3)
Coeff.	1.971***	2.013***	0.698***	0.6046***
s.e.	*0.737*	*0.685*	*0.222*	*0.285*
Gross op. mar./ sales		X		X
Cash flows/ sales		X		X
Financial cost / debt		X		X
Total capital stock		X		X
Order pol. min AIC	2	2	0	0
Obs	545	545	545	545

The table shows the estimates of the coefficient β of model (1) based on the sample of 545 out of 612 firms, for which balance sheet (CERVED) data are available. Covariates included (X) are as shown in Table 2. Robust standard errors clustered by score in italics. *, **,***: significant at 10%, 5% and 1% respectively.

Table A7

ROBUSTNESS V: EFFECT OF THE PROGRAM ON PATENTS USING THE PRIORITY YEAR

Var. Dip.	# of patents			# of patents			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 1					
Coeff.	2.109*	7.207**	30.04***	2.051***	1.118**	30.31***	0.845***	0.645*	2.664**
s.e.	*1.13*	*3.072*	*0.193*	*0.785*	*0.469*	*0.425*	*0.252*	*0.371*	*1.102*
Order pol. min AIC	2	2	2	2	0	2	0	0	1
Obs	612	309	246	612	309	246	612	309	246
Alpha				11.12***	10.23***	7.897***			
				Period 2					
Coeff.	2.109*	7.207**	30.04***	2.051***	1.118**	30.31***	0.845**	0.645*	1.133***
s.e.	*1.30*	*3.072*	*0.193*	*0.785*	*0.497*	*0.425*	*0.252*	*0.371*	*0.402*
Order pol. min AIC	2	2	2	2	0	2	0	0	0
Obs	612	309	246	612	309	246	612	309	246
Alpha				11.12***	10.23***	7.897***			

Notes: The table shows the estimates of the coefficient β of model (1) using different outcome variables. Patents are accumulated starting from 1 year after the assignment (for Period 1) or 2 years (Period 2) onward, trying to use all the data available, although patents for 2010 and 2011 are largely incomplete. The polynomial of order 0 is the difference in mean between treated and untreated. Robust standard errors clustered by score initalics. *, **,***: significant at 10%, 5% and 1% respectively.

Table A8

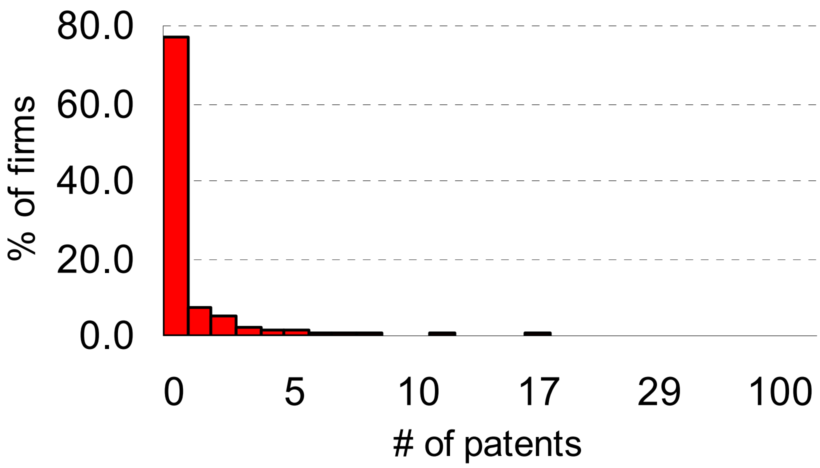
ROBUSTNESS VI: KERNEL ESTIMATIONS

Order of local polynomial	# 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***
	0.593	*0.365*	*0.436*	*0.032*	*0.038*	*0.036*
1	0.821	1.473*	1.895**	0.072*	0.196***	0.216***
	0.644	*0.828*	*0.878*	*0.042*	*0.058*	*0.066*
2	1.106	3.090*	3.933***	0.123**	0.284	0.273**
	1.317	*1.659*	*1.179*	*0.058*	*0.208*	*0.109*

We estimated the model using the triangular kernel combined with three different bandwidth for each sub-sample and various polynomials. A bandwidth of 50, 9 and 7 score points on each side of the cut-off spans respectively the full sample, 50% and 40% 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

FIRMS' DENSITY BY # OF PATENT APPLICATIONS



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

Figure 2

FIRMS' DENSITY DISTRIBUTION BY SCORE

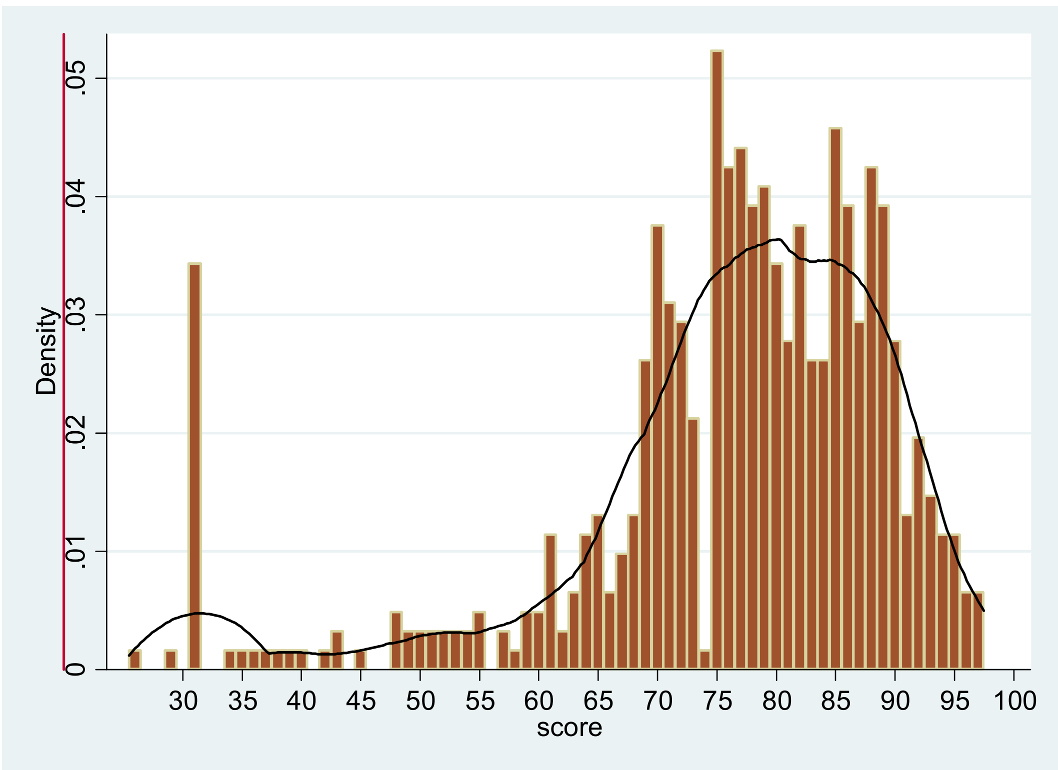
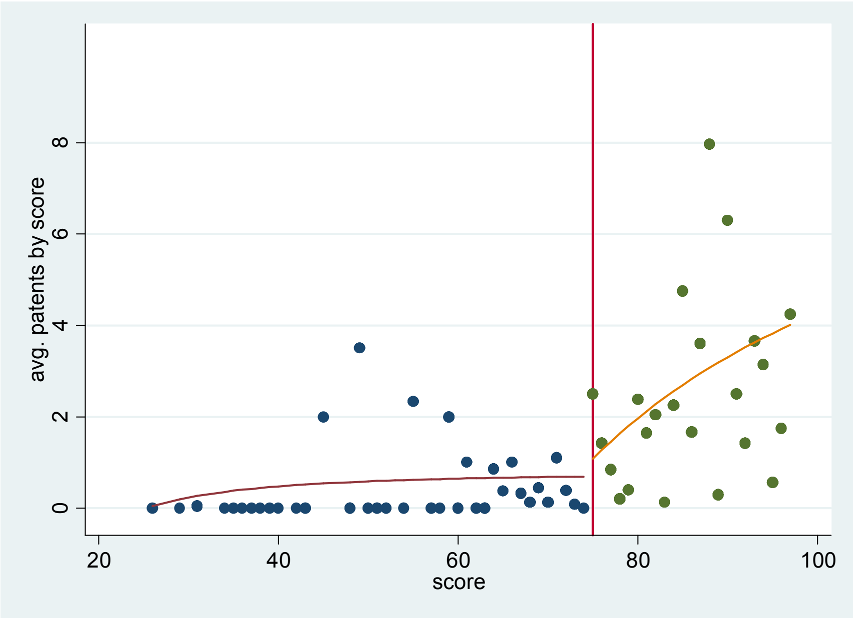


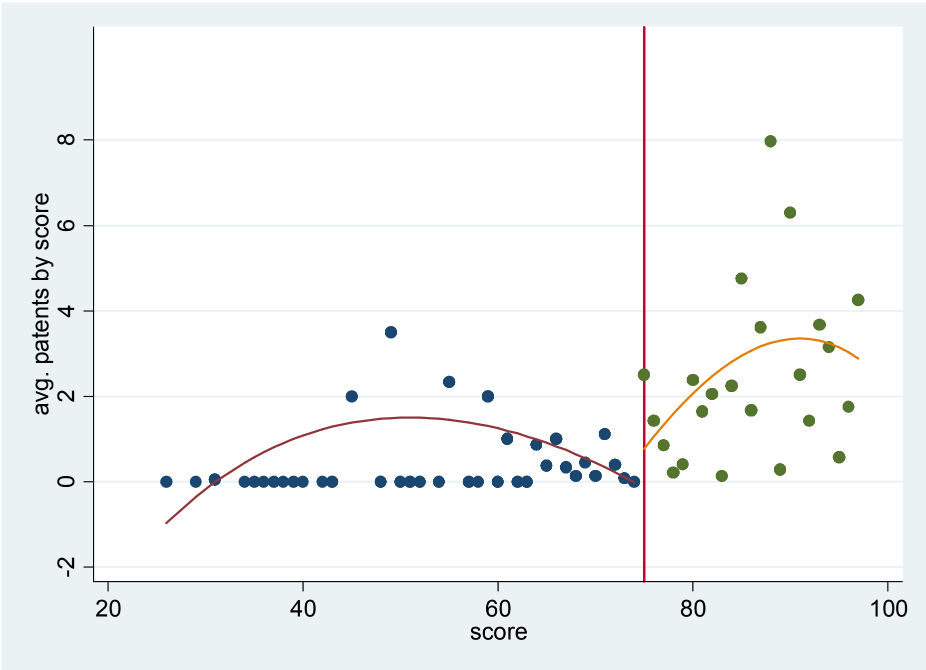
Figure 3a

NUMBER OF PATENT APPLICATIONS BY SCORE-TREATMENT PERIOD

Linear interpolation



Quadratic interpolation

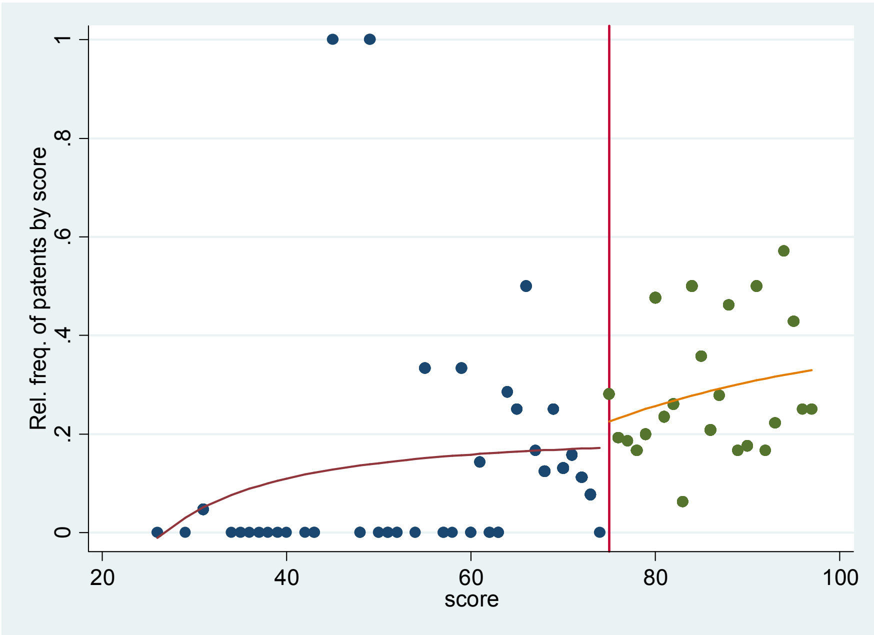


Notes: Based on counts in the treatment period (Period 1). In order to make graphs comparable, y-axis scale is the same across fig. 3a and 3b, 4a and 4b respectively. As a result, the two highest values in fig. 3a are not included in the graph. Interpolation curve is still worked out on the basis of the whole sample.

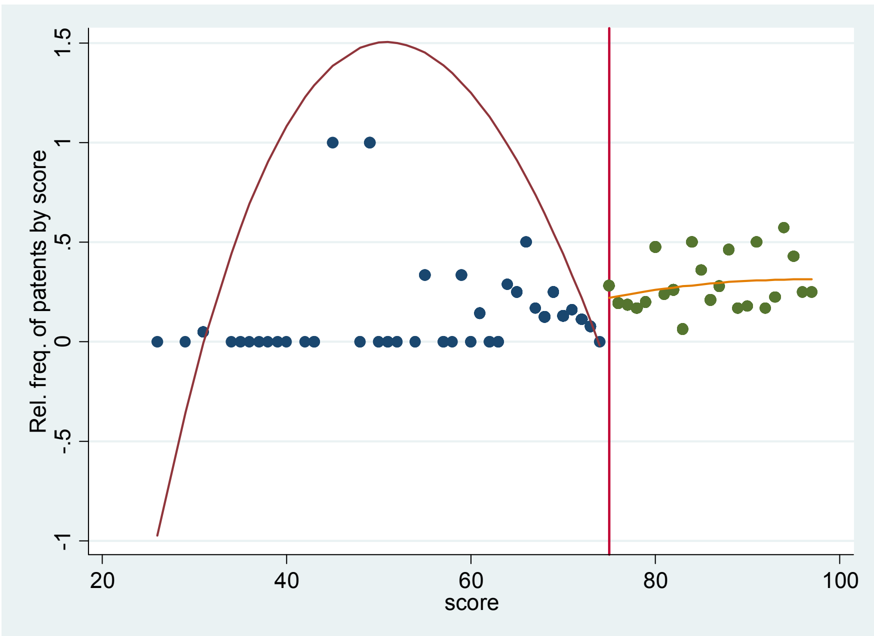
Figure 3b

PROBABILITY TO APPLY FOR PATENTING BY SCORE-TREATMENT PERIOD

Linear interpolation



Quadratic interpolation

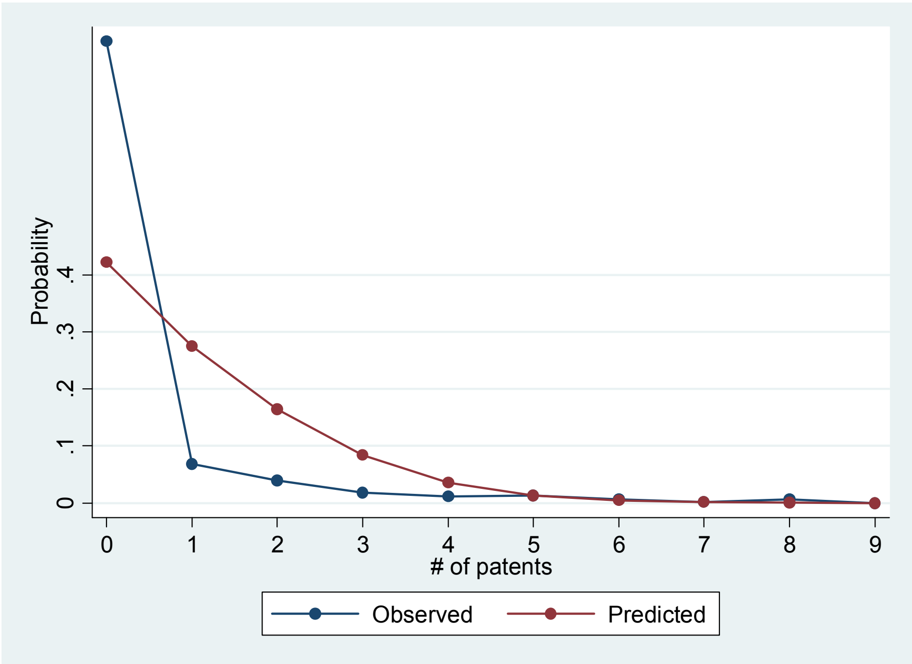


Notes: Based on counts in the treatment period (Period 1).

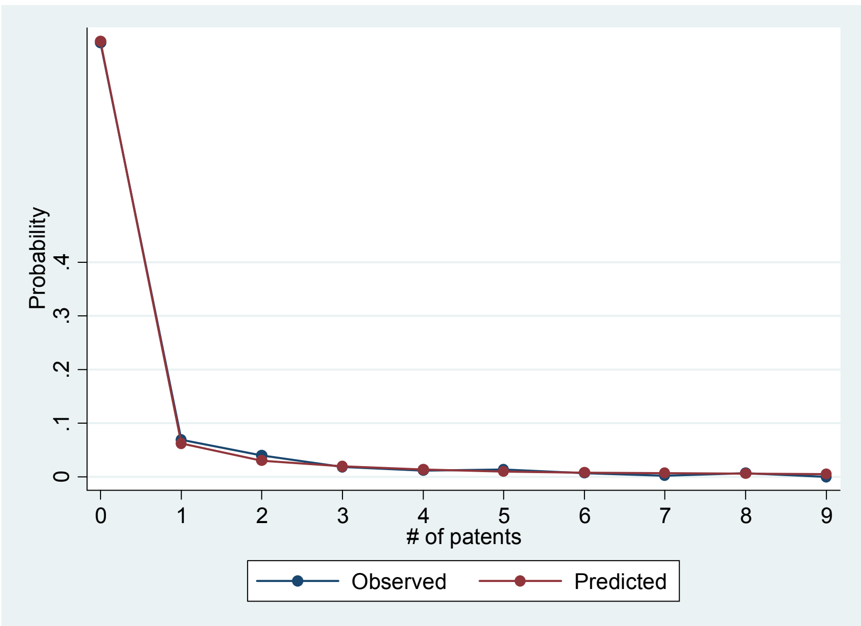
Figure 4

FITTED PROBABILITY:
POISSON & NEGATIVE BINOMIAL

Poisson



Negative binomial

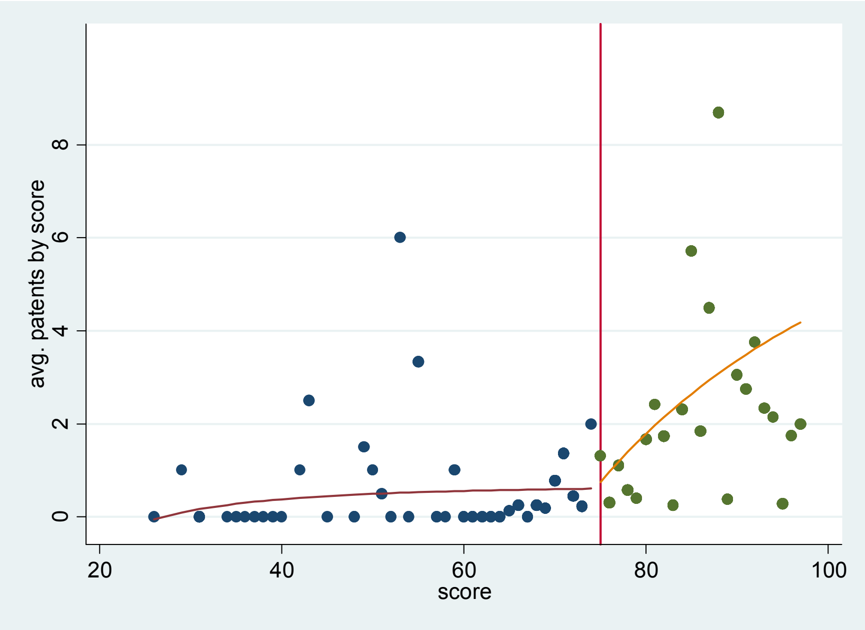


Notes: Predicted probability from estimations of poisson and negative binomial (whole sample; quadratic function).

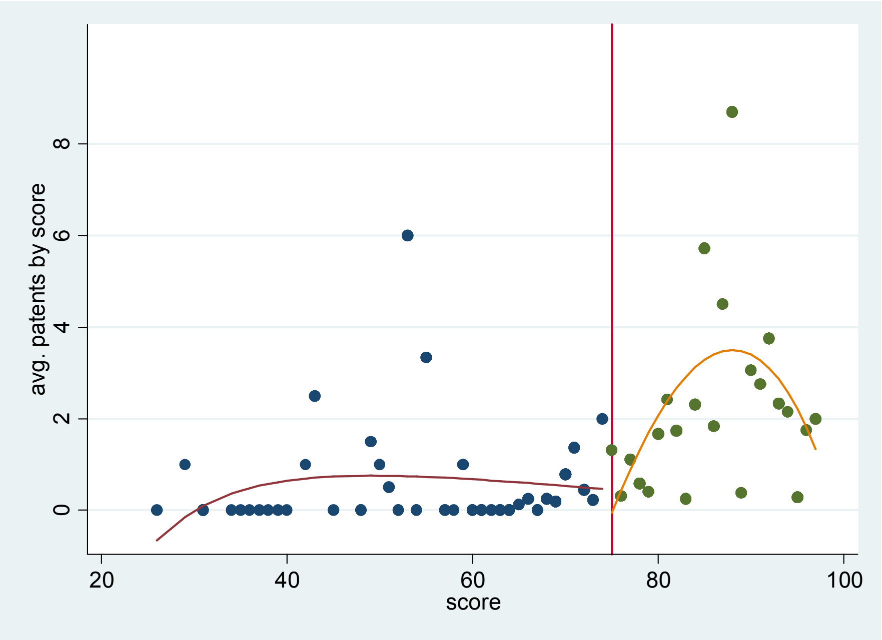
Figure 5a

NUMBER OF PATENT APPLICATIONS BY SCORE – PRE-TREATMENT PERIOD

Linear interpolation



Quadratic interpolation

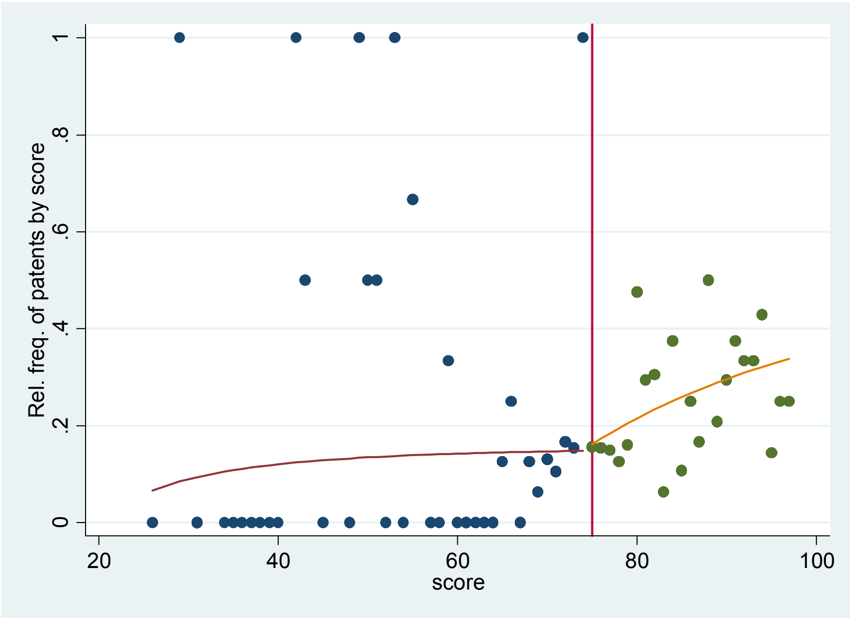


Notes: Based on counts in the 5-year length pre-treatment period.

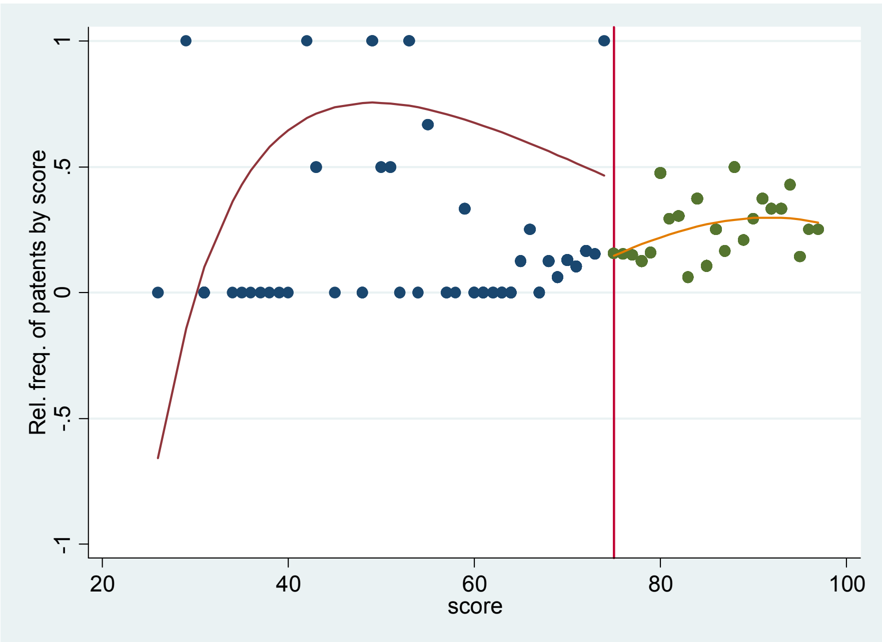
Figure 5b

PROBABILITY TO APPLY FOR PATENTING BY SCORE - PRE-TREATMENT PERIOD

Linear interpolation



Quadratic interpolation



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