THE IMPACT OF R&D SUBSIDIES ON FIRM INNOVATION[1]

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Abstract

This paper evaluates the impact of a unique R&D subsidy program implemented in northern Italy on innovation of recipient firms. Firms were invited to submit proposals for new projects and only those 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 to apply for patenting, of subsidized firms with those of unsubsidized firms.
We find a significant impact of the program on the number of firm patents that was larger for smaller firms. Our results show that the program was also successful to increase the probability to apply for patenting but only for smaller firms.

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

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1. Introduction

Industrial policies that support private Research and Development (R&D) activity are widespread among industrial countries (OECD 2008). On the economic ground these subsidies are justified by typical market failure arguments. Since knowledge has a public-good character (i.e., it is non-rival and non-excludable), firms cannot totally internalize the benefits of R&D investment, and the equilibrium level of private R&D outlays is below the social optimal level. In these circumstances the policy goal is to raise private R&D expenditure. A further 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. Especially small and young enterprises find more difficult to finance intangible investments than tangible ones because returns are more uncertain, intangible assets cannot be offered as collateral and the investment is more difficult to evaluate. As a result, R&D investments are more subject to credit constraints (Hall and Lerner 2009).

The empirical evidence on the effects of R&D policies is vast. Yet, the provided results are mixed. The majority of the papers assesses whether R&D incentives had additional effects on firm *innovation input*, as on investment or employment (Lerner 1999; Busom 2000; Wallesten 2000; Lach 2002; Almus and Czarnitzki 2003; Gonzalez et al. 2005; Gorg and Strobl 2007; Hussinger 2008; Clausen 2009; Bronzini and Iachini 2011; De Blasio et al. 2011) or on some proxies of firm performance like productivity or sales (Lerner 1999; Merito et al. 2007).[2] On the other contrary, microeconometric studies on the impact of the subsidies on firm *innovation output* are a few. 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 from R&D, grants are more innovative, in terms of new products introduced by subsidized firms, than enterprises taking advantage of only R&D tax credits. On the other hand, a couple of recent papers investigating the effect of R&D tax credits on innovation reached opposite conclusions. Czarnitzki et al. (2011) found

a positive effect of the effect of R&D tax credit on the number of new products introduced by the Canadian recipient firms. On the contrary, Cappellen et al. (2012) found no effect of a similar policy on patenting and on the introduction of new products for a sample of beneficiary Norwegian firms.

This paper contributes to this stream of research. We evaluate the impact of an innovation policy implemented in a region of northern Italy on patenting activity of the recipient firms. The region (Emilia-Romagna; in Figure 1 the area in green) is highly representative of the national economy, being the third largest industrial region of the country, covering more than 10% of Italian patents, and finally having by far the highest patent intensity among the Italian regions[3].

In the evaluation studies like ours the main challenge is to infer a causal effect of subsidies from comparisons between subsidized and unsubsidized firms. Since recipients firms are not randomly chosen, the variable capturing subsidy recipients is endogenous, and models that fail to adequately control for the endogeneity issue will be biased. The quoted literature usually addresses the endogeneity issue through matching methods or instrumental variable estimates. In this paper we evaluate the impact of the policy through a quasi-random strategy. The program envisages that, after the assessment of an independent technical committee, only eligible projects that receive a certain score are subsidized. 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). To the best of our knowledge, this work represents the first attempt to evaluate the impact of R&D incentives on innovation output using this method.

We take advantage of the local dimension of the policy that allows us to remove much of the unobserved heterogeneity among enterprises, and compare recipient and non-recipient firms that, being located in the same region, are more similar than those participating in nationwide programs. Furthermore, our assessment permits us to shed light on the effects of

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[2] For excellent reviews on earlier studies see David et al. (2000), Klette et al. (2000) and, on the impact of fiscal incentives, Hall and Van Reenen (2000).

[3] Measured as patents per million inhabitants, Emilia registered on average more than 160 patents, more than twice the Italian average (Context Indicators, Istat, 2012).

place-based policies managed by local government that have attracted scant attention from the program evaluation literature, despite absorbing a relatively large share of the total public transfers to the private sector. In Italy, for example, between 2000 and 2007 around 18 billion euros were granted to firms owing to these programs – one fourth of total public funds assigned to private enterprises.[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 the small enterprises.

The remainder of the paper is structured as follows. In the next section, we illustrate the features of the program. In section Section 3 we describe the outcome variables and the data set used. The empirical strategy is discussed in section 4, while the main results are shown in section 5. Some 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. The geographic area covered by the policy is described in Figure 1 in the Appendix. 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

[4] For a discussion of the theoretical rationale of place-based policies see Kline (2010). As regards Italy, in a companion paper (Bronzini and Iachini 2011) one of us investigated the effect of the same regional policy on firm investment.

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 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.

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[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*

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 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 to apply for 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. Number of patents and patent probability are measured over the post-program period.

The choice to measure innovation output by patents has pros and cons. On the one hand, it is well known that not all innovation 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 to improve their goodwill reputation or to increase their bargaining power in the market, more than to protect their innovation (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 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).

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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.

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. 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.

Our 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. 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. Next, we have also excluded firms involved in renunciations and revocations and firms unsubsidized in the first auction but subsidized in the second. The remaining firms are 618.

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, we referred to the recent work by Marin (2012) who matched the name of Italian firms in AIDA data set (Bureau van Dijk) collecting the majority of Italian corporations, with applicants at

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

the EPO in PATSTAT, and provided the number of patent applications submitted by each firm from1977 to 2011, together with the firm identification number.[9]

We matched firms applying for the subsidies found in AIDA (537) with the Marin's dataset on patents. If a firm was not in Marin's data set, we assumed that firm did not apply for any patent. On this last sample we carried out the econometric exercise.

We are aware that with this method we can wrongly assign zero patents to firms not found in the Marin-AIDA data set, but that nevertheless have applied for patents. This can happen for many reasons, e.g. because of errors in the identification number, or because AIDA does not include smallest enterprises and non-corporation. However, this bias in the data set becomes a concern for our identification strategy only if it affects firms around the cut-off in a systematic way, i.e. firms close to the threshold and falling into one side only of the cut-off. All in all, we do not believe that such condition is likely to occur in our sample. Moreover, Marin (2012) is able to match more than 80 per cent of the patent applications submitted by Italian companies to the EPO during the observed period, therefore, in our opinion the matching method, if any, should result only in a second order selection bias .

The third data set we use is that provided by Cerved group on balance sheet variables, used to compare the observables of treated and untreated firms and to carry out some robustness exercises.

4. Empirical Strategy

Our goal is to evaluate whether subsidized firms would not have made the same amount of patents without the grants. For the identification strategy, we take advantage of the funds' assignment mechanism. As described above, the committee of experts assigned a score to each project and only those receiving a score greater than or equal to a given

[9] Referring the reader to Marin's work for details, here it is worth recalling some limitations of the data set we used. The AIDA database does not contain the population of Italian firms, rather it provides information on larger firms, moreover there is a bias 'by construction' due to the exclusion of inactive firms after four years of inactivity. 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.

threshold were awarded grants (75 points out of 100). 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. By letting the outcome variable be a function of the score, the average treatment effect of the program is assessed through the estimated value of the discontinuity at the threshold.[10]

The strategy relies on the continuity assumption, which requires that firms in a neighborhood just below and just above the cut-off point have the same potential outcome in an identical funding experience. Even though there is no direct way of testing the validity of the continuity hypothesis, Lee (2008) formally shows that if the treatment depends on whether a (forcing) variable exceeds a known threshold and agents cannot precisely control the forcing variable, the continuity assumption is satisfied since the variation in treatment around the cut-off is randomized. In this circumstance, the impact of the program is identified by the discontinuity of the outcome variable at the cut-off point (Hahn et al. 2001).

We believe that in our situation this strategy is appropriate, in that it is hard to argue that firms participating in the program can perfectly control their score. In any event, if the treatment is random around the threshold, treated and untreated firms close to the threshold should be similar (Lee, 2008). Therefore, we can assess the validity of the design by verifying whether differences in treated and control firms' observables become negligible close to the cut-off point. We will present the results of this and further robustness test later on.

Several econometric models have been suggested to test for the discontinuity at the cut-off point (see amongst others: Imbens and Lemieux, 2008; Lee and Lemieux, 2010). Here we use a parametric method. We estimate up to a second order polynomial model on the full sample:

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

[10] In the last decade a growing number of empirical studies in economics have utilized the RD design. See Lee and Lemieux (2010) and the monographic number of the *Journal of Econometrics*, vol. 142(2), 2008.

where 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. We also test the mean difference between treated and untreated firms (polynomial of order 0).

The equation (1) has been also estimated through local regressions 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. Following the suggestion by Lee and Card (2008), being the score a discrete variable we clustered the heteroskedasticity robust standard errors by the value of the score *S*.

The OLS estimates of the model (1) with the number of patent applications as outcome variable is likely to be biased because of the large amount of zero patents in our data set (about 80% in our sample). In such circumstances we need to model discrete count data, and the benchmark is the Poisson model. In such a model the conditional mean equals the conditional variance $E(y_i|x_i)= Var(y_i|x_i)$[11], as a results it turns out to be inadequate because real data are often overdispersed, i.e. $Var(y_i|x_i)> E(y_i|x_i)$.[12] Therefore, when we use the number of patent applications as outcome variable we make use of two different models: the Poisson model and the negative binomial model that accounts for overdispersion and excess of zeros. A binomial distribution is 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 α=0 the negative binomial model reduces to the Poisson model. Thus, by testing for α=0 we verifies for overdispersion.[13]

[11] This is usual in the empirical literature on innovation and patents, see the seminal paper by Hausman et al. (1984) or Cincera (1997).

[12] Furthermore, sometimes excess of zeros are generated by a separate process from the count values and a mixed model is required, one for the zeros, one for the count values. See: Cameron and Trivedi (2005), chap. 20.

[13] 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; this is our case too. We also tried a zero-inflated binomial model, but the truncated binomial model for the counts did not converge. However, when we excluded some

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

We consider the total number of patent application submitted by each firms after the program. The treatment periods starts 1 year (Period 1) or 2 years (Period 2) after the grant is assigned, up to 2011. We counted the patents attributed to the firm in each year using as reference date the application date.[14] Then, we sum the patent applications for each firm over the time-span considered.[15]

Fig. 2b 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 1.9, while variance is about 97. These characteristics of the distribution of our outcome variable will be satisfactory accounted for by the negative binomial model.

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 (512 firms) than our regression sample (537 firms). We notice that there is a large concentration of firms within just three sectors: machinery, electrical and optical equipment and advanced services. All together they absorb 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 advanced services, whereas the reverse occurs for the group of coke and chemical

larger counts, the model provided estimates similar to those of the standard negative binomial, as confirmed by a formal test too.

[14] 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 this issue.

[15] In terms of number of patents, our sample of 537 firms presents as follows. Period 1 includes 121 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 107 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 109 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 (4 years) includes 98 firms with at least 1 patent registered in 2001-2004 for the firms belonging to the first auction and in 2002-2005 for those of the second auction.

products. 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 auctions for treated and untreated firms. The RD design relies on the assumption that near the cut-off the treatment is random, therefore firm covariates before the treatment should have the same distribution just above and just below the cut-off (Lee and Lemieux 2010, p.283). Accordingly, to test the validity of our method we first compare the means of the main balance sheet items of our firms, above and below the cut-off. On the whole sample, we notice that treated firms are substantially larger than untreated firms, as shown by mean differences of sales, valued added and assets. 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), propensity to investment and capital endowment. 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, like for the balance sheet variables. Overall such evidences support our empirical strategy

The Figure 2a displays the density function of the sample by score. We notice that it is higher on the right-hand side of the threshold because of the cited exclusion of non-scored untreated firms in the second auction, and that density increases substantially around the cut-off point. However, we observe 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.

Before showing the econometric results, we carried out a graphical analysis of the pattern of patents as a function of the score assigned. We plotted the number of patent applications and the probability of patenting (share of patenting firms) averaged by score together with two interpolation lines: linear and quadratic (Fig. 3a-3b).

As regards the number of patents, Fig. 3a shows a visual evidence of a discontinuity of the conditional mean, stronger in the quadratic case. The effect of the treatment is much smaller in terms of probability of patenting (Fig. 3b) emerging mainly in the quadratic specification.

The results of the econometric analysis are presented in Table 3. For the number of patent applications we show the estimations of coefficient β of model (1) estimated either by 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 1 year after the program, and period 2 start 2 years after the program; both the period terminate in 2011. Notice that data on patents provided by the EPO for 2011 are still incomplete.

For the number of patents our results show that the coefficients turn out to be positive and statistically significant in all the estimates, but in one case using a Poisson model. Notice that the Poisson model is rejected in favour of the negative binomial: the estimates of the alpha parameter, much larger than zero in the negative binomial, reject the hypothesis of variance equal to the mean. Moreover, in most of the cases AIC suggests that the best model is the quadratic one. Fig. 5 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. Only in one case in the treatment period 2 the coefficient turns out to be not statistically significant.

Table 4 reports the marginal effect of treatment for the models in Table 3.[16] Given the superiority of negative binomial model over the Poisson, we did not compute the marginal effect for the latter.

In the case of the number of patens, the marginal effect of treatment on the whole sample is about 0.95, meaning that the number of patents increases on average around by 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, reported in the right-hand side of Table 4. On the whole sample, such average is about 0.65, hence, in relative terms the effect of the treatment is about 1.3 times the average of untreated firms. As in Table 3, the marginal effects are stronger with the windows closer to the cut-off, becoming very large, and admittedly scanty plausible, in the 40% sample windows. It is likely that in this case there are too few firms to precisely estimate the impact of the policy. This result is 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 0.1, meaning that the probability to patent increases on average around by 10 percentage points thanks to the grant. Compared to the probability of patenting for the untreated firms, about 15 per cent, the effect of the treatment is 0.7 times the average of untreated firms. As for the negative binomial case, the marginal effects become very large in the 40% sample windows. Results in period 2 mirror period 1's ones.

[16] Unlike linear models, the coefficients cannot be interpreted as the marginal effects of treatment and they cannot be compared easily. As it is well-known, in a non-linear model the marginal effect of a change in a regressor is not equals to its coefficient. For the Poisson model (and negative binomial), where $E(y|x)=\exp(x'\beta)$, the marginal effect (ME) of a change in variable j is in general $\exp(x'\beta)\beta$. Yet, for an indicator variable, derivatives are not appropriate, because the relevant change is when this variable 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).

As a first robustness check of our results we carry out a falsification test over the pre-program period. If the jump in patents detected for treated firm is due to the grant, in absence of treatment we should not find any discontinuity in the outcome variable at the cut-off. Therefore, if we observe a smooth function before the program took place, it is plausible that the discontinuity after the program is due to the subsidy. 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) before the program, using two different pre-treatment periods: 5 years (period A) and 4 years (period B) before the program, both ending the year of the auction.

As in the baseline model, we singled out the best polynomial specification according to AIC criterion. Figure 4a-4b and Table 5 show that before the program there were no positive discontinuities of the functions around the cut-off. Only in the full sample there is some evidence of discontinuity in the probability of patenting. However, the jump vanishes once we take into account the samples closer to the cut-off or a different time period (period B).

6. Robustness

In this section we present further robustness exercises to test the validity of our empirical design and the sensitiveness of our results.

RD identification strategy relies on the continuity assumption, which requires that 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. Here, we verify whether the available firm observables are continuous at the cut-off in the year before the auction. If we do not observe jumps, it is plausible that also the outcome variable would have been continuous without 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 6 shows that there is no evidence of discontinuities.

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 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).

We introduced two different sets of sectoral dummies: either for each macro-sector (agriculture, manufacture and mining, construction, services, advanced services[17]) or for each of the 2-digit sectors presented in Table 1. The results shown in Table 7 are remarkably similar to the baseline ones. The best specification is again the negative binomial model. The coefficients turn out to be close in magnitude to those previously estimated, and highly statistically significant.

In addition, we test for a heterogeneous effect of the program across firms' size, as found by Lach (2002) and, for the same program under scrutiny but on different outcome variables, by Bronzini and Iachini (2011). Therefore, we estimated the models breaking down the sample by two classes of firm size, over the post program period using such new specification (the logit model is not reported but has been similarly changed):

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

where the firms' size dummies are interacted with the treatment dummy and the score, where $Size^1$ =1 if the value added of firm i is below the median and zero otherwise (Small); $Size^2$=1 if sales are above 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.

In last two columns of Table 7 we show the results of the estimates of model (2). On the number of patents the effect turns out to be positive and statistically significant in both cases. Interestingly enough, the impact is greater for small firm than for large ones.

[17] Here, "Services" stands for Trade, Transport and Hotels, whereas "Advanced services" includes Real estate, renting, research and business activities.

To properly assess the impact by firm size, we compare the estimated marginal effects to the average number of patents of small and large untreated firms (1.54). For small firms tanks to the program there is an increase by 0.34 patents, more than twice the mean of small untreated firms (0.15); for large firms the increase (1.74) is around 1.1 times the mean for large untreated ones (1.5).

As regards the probability of patenting, Table 7 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 quite remarkable, more than three times the average probability of untreated firms. For large firms the coefficient is almost zero and not statistically significant.

Finally, we carry three further robustness exercises. First, 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, and those potentially more correlated with the investment in research and development (gross operative margins/sales, cash flow/sales, financial costs/debt; capital stock). Results of this exercise are reported in Table 8; they are qualitatively comparable to the baseline ones. Second, we 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 literature, sometimes the priority date is preferred, that is the first date of filling the application (usually to the applicant's domestic patent office). This is closer to the date of the invention. By counting the patents according to the priority date (year) we obtain results very similar to the baseline ones (Table 9). Finally, we estimate the baseline model by triangular kernel using different bandwidths (50, 9 and 7 score points below and above the threshold). Results are again simlar to the baseline ones, even though in a few cases the coefficients are now not statistically significant because of higher standard errors.

7. Conclusions

This paper evaluates the impact of a unique R&D subsidy program implemented in northern Italy on innovation of recipient firms. Unlike most of the literature, this study

focuses on the effect of R&D incentives on innovation output rather than on innovation input. We use patenting activity to measure firm innovation.

By comparing the number of patent applications, and the probability to patent, of subsidized and unsubsidized firms by 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 larger for smaller firms than for larger enterprises. Our results show that also the probability of patenting has been positively affected by the program but only for smaller firms.

On the whole our results suggest that the program was effective on the intensive margin of patenting, i.e. to increase patenting activity of innovative firms, and to a lesser extent on the extensive one, i.e. to increase the number of patenting firms.

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Appendix

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	17	5	5,0	2,9
Textiles, wearing apparel, leather, wood products	4	5	1,2	2,9
Paper, printing and publishing	5	0	1,5	0,0
Coke, Chemical products, plastic	32	8	9,4	4,7
Non-metallic mineral products	12	4	3,5	2,4
Basic metal industries	22	12	6,4	7,1
Machinery and equipment	96	39	28,1	22,9
Electrical and optical equipment	52	22	15,2	12,9
Transport equipment	14	6	4,1	3,5
Other manufacturing industries, electricity, etc.	6	8	1,8	4,7
Construction	6	2	1,8	1,2
Trade, transport, financial services	16	14	4,7	8,2
Advanced services	56	43	16,4	25,3
Others	2	2	0,6	1,2
All firms	**342**	**170**	**100,0**	**100,0**

Notes: Based on CERVED data. The sample includes 512 out of 537 firms considered for the evaluation exercise.

Table 2a

PRE-ASSIGNMENT STATISTICS: BALANCE SHEET ITEMS

Sample	All				50% window				40% window			
Item	Untreated	Treated	Diff.	Diff. (t-stat)	Untreated	Treated	Diff.	Diff. (t-stat)	Untreated	Treated	Diff.	Diff. (t-stat)
Sales	13375	47016	8515	2,59***	14293	22807	8515	0,66	11521	16343	4822	0,37
Value Added	3402	11895	8494	2,63***	3715	6320	2605	1,12	3438	4572	1134	0,74
Assets	14059	50053	35993	2,47**	15451	30309	14858	0,81	11967	16646	4679	1,01
ROA	5,42	6,14	0,72	0,76	4,17	6,04	1,87	1,54	3,88	6,00	2,13	1,54
Leverage	13,00	27,65	14,65	0,64	8,35	6,43	-1,91	-0,31	8,38	5,36	-3,02	-0,39
Gross op. mar./ sales	0,05	0,11	0,06	0,76	0,00	0,17	0,17	1,27	-0,01	0,19	0,21	1,22
Cash flows/ sales	0,12	0,07	-0,05	-0,75	0,12	0,08	-0,05	-1,13	0,15	0,07	-0,08	-1,48
Financial cost / debt	0,04	0,02	-0,02	-2,03**	0,06	0,02	-0,04	-1.87*	0,03	0,03	0,00	-1,19
Labor cost / sales	0,23	0,31	0,07	0,82	0,24	0,32	0,07	0,51	0,25	0,33	0,08	0,44
Total capital stock	3117	14587	11470	2.04**	3667	8932	5264	0.65	2604	2821	217	0.28
Intangibile capital stock	710	3249	2539	1.32	809	2485	1676	0.61	520	521	1.30	0.01

Notes: Based on CERVED data. The sample includes 512 out of 537 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 342 firms are treated; 170 are untreated. In the 50% cut-off neighborhood sample treated firms are 173, untreated 86; in the 40% cut-off neighborhood sample treated firms are 141, untreated 66. *, **,***: 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	0.557	2.474	-1.917	-1.993**	0.667	1.067	-0.401	-0.937	0.792	1.034	-0.243	-0.495
Frequency of firms with at least 1 patent	0.147	0.232	-0.084	-2.303**	0.140	0.202	-0.0625	-1.267	0.153	0.200	-.0472	-0.842

Notes: Based on Marin (2012). The sample includes 537 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 354 firms are treated; 183 are untreated. In the 50% cut-off neighbourhood sample treated firms are 178, untreated 93; in the 40% cut-off neighborhood sample treated firms are 145, untreated 72. *, **,***: significant at 10%, 5% and 1% respectively.

Table 3

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

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 1					
Coeff.	2.079*	6.731***	19.03***	2.067***	1.223**	15.42***	0.679***	0.554*	0.808***
s.e.	*1.177*	*2.474*	*4.477*	*0.782*	*0.487*	*1.904*	*0.223*	*0.292*	*0.288*
Order pol. min AIC	2	2	2	2	0	2	0	0	0
Obs	537	271	217	537	271	217	537	271	217
Alpha				11.13***	10.54***	8.92***			
				Period 2					
Coeff.	2.028*	7.737**	30.54***	2.067***	1.153**	31.08***	0.645**	0.605	13.83***
s.e.	*1.61*	*3.319*	*0.206*	*0.866*	*0.533*	*0.689*	*0.256*	*0.378*	*5.26*
Order pol. min AIC	2	2	2	2	0	2	0	0	2
Obs	537	271	217	537	271	217	537	271	217
Alpha				12.123***	10.96***	8.92***			

Notes: The table shows the estimates of the coefficient β_1 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: TREATMENT PERIODS – MARGINAL EFFECTS OF THE TREATMENT AND AVERAGES FOR UNTREATED FIRMS

Var. Dip.	# of patent applications			Dummy (patent applications>0)			Averages for untreated firm			
Model	Negative binomial			Logit						
Sample	All	50% window	40% window	All	50% window	40% window		All	50% window	40% window
								Average number of patent applications		
				Period 1			Period 1	0.645	0.430	0.444
Marginal effect	0.941	1.031	30.37	0.110	0.085	0.116	Period 2	0.552	0.344	0.333
Order pol. min AIC	2	0	2	0	0	0				
				Period 2				*Share of firms with patent applications>0*		
Marginal effect	0.562	0.746	15.54	0.095	0.084	0.556	Period 1	0.153	0.150	0.125
Order pol. min AIC	2	0	2	0	0	2	Period 2	0.137	0.129	0.097

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

Table 5

ROBUSTNESS: ESTIMATES OF MODEL (1) 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.	-1.230	4.032	3.673	0.474	0.471	0.267	0.555**	0.445	-3.565*
s.e.	*1.065*	*2.385*	*4.078*	*0.724*	*0.386*	*0.415*	*0.267*	*0.288*	*2.139*
Order pol. min AIC	2	2	2	2	0	0	0	0	2
Obs	537	271	217	537	271	217	537	271	217
Alpha				12.72***	12.53***	12.48***			
				Period B					
Coeff.	-0.078	3.460	3.865	0.442	0.482	0.299	0.498	0.466	-3.012
s.e.	*1.040*	*2.683*	*4.637*	*0.709*	*0.415*	*0.450*	*0.262*	*0.299*	*2.303*
Order pol. min AIC	2	2	2	2	0	0	0	0	2
Obs	537	271	217	537	271	217	0.498	271	217
Alpha				13.74***	12.36***	12.76***			

Notes: The table shows the estimates of the coefficient β of model (1) using different outcome variables. 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-year period). 4-year period includes patents registered in 2001-2004 for the firms belonging to the first auction and in 2002-2005 for those of the second auction. 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 6

ROBUSTNESS II: DISCONTINUITY OF COVARIATES

(OLS regressions)

Variable	Coeff.	s.e.	Order pol. min. AIC
Sales	-6246	*11121*	1
Value Added	-1910	*2897*	1
Assets	1681	*12348*	1
ROA	.0715	*1.099*	0
Leverage	-29.29	*22.28*	1
Gross operating margin / sales	0.059	*0.064*	0
Cash flows/ sales	-0.091	*0.071*	2
Financial cost / debt	-0.030	*0.0241*	2
Labor cost / sales	0.075	*0.064*	0
Total capital stock /sales	-0.122	*0.117*	0
Tangible investment / sales	-0.062	*0.046*	0
Intangible investment / sales	-0.058	*0.035*	0

Notes: The table shows the estimates of the coefficient β of model (1) using different covariates. 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 III: SECTOR DUMMIES AND FIRM SIZE

Sample	Baseline	Baseline + Sector Dummy1	Baseline + Sector Dummy2	Baseline by firm size	
				Small firms	Large firms
	Negative binomial - # of patent applications				
Coeff.	1.884**	2.022***	2.125***	4.000***	1.533**
s.e.	*0.757*	*0.783*	*0.754*	*1.090*	*0738*
Order pol. min AIC	2	2	2	2	2
			Untreated firms average number of patent aplications	0.154	1.545
			Marginal effect	0.333	1.748
	Logit - Dummy (patent applications>0)				
Coeff.	0.594***	0.432*	0.495*	1.119**	0.065
s.e.	*0.218*	*0.229*	*0.214*	*0.464*	*0.270*
Order pol. min AIC	0	0	0	0	0
			freq. of firms with patent applications	0.058	0.333
			Marginal effect	0.207	0.035

In the first three columns the table shows the estimates of the coefficient $\beta_{.}$ of model (1) based on the sample of 512 out of 537 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. 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 8

ROBUSTNESS IV: IRRELEVANCE OF COVARIATES

Dep. Variable	# of patent applications			Dummy (patent applications>0)		
Model	(1) Baseline	(2)	(3)	(1) Baseline	(2)	(3)
Coeff.	1.884**	1.406*	2.001**	0.594***	0.583**	0.501**
s.e.	*0.757*	*0.775*	*0.699*	*0.218*	*0.228*	*0.230*
Gross op. mar./ sales		X	X		X	X
Cash flows/ sales		X	X		X	X
Financial cost / debt		X	X		X	X
Total capital stock			X			X
Order pol. min AIC	2	2	2	0	0	2
Obs	512	506	506	512	506	506

The table shows the estimates of the coefficient β of model (1) based on the sample of 512 out of 537 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 9

ROBUSTNESS IV: 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.	1.948	7.053**	26.77***	1.985**	1.118**	27.95***	0.692***	0.671*	14.02***
s.e.	*1.226*	*3.118*	*0.471*	*0.823*	*0.504*	*1.407*	*0.255*	*0.383*	*2.633*
Order pol. min AIC	2	2	2	2	0	2	0	0	2
Obs	537	271	217	537	271	217	537	271	217
Alpha				11.79***	10.42***	8.743***			
				Period 2					
Coeff.	1.938	7.558**	30.94***	2.039***	1.129**	31.24***	0.763**	0.565	18.17***
s.e.	*1.230*	*3.101*	*0.168*	*0.084*	*0.497*	*0.427*	*0.258*	*0.401*	*1.250*
Order pol. min AIC	2	2	2	2	0	2	0	0	2
Obs	537	271	217	537	271	217	537	271	217
Alpha				12.16***	10.81***	8.161***			

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 10

ROBUSTNESS V: 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	1.818***	1.164**	1.212**	0.096**	0.100**	0.106**
	0.649	*0.518*	*0.596*	*0.039*	*0.050*	*0.053*
1	0.993	2.038*	2.506**	0.069	0.223**	0.250**
	0.691	*1.116*	*1.186*	*0.064*	*0.106*	*0.127*
2	1.159	4.416**	4.4921**	0.123	0.335	0.343
	1.452	*2.134*	*2.032*	*0.090*	*0.231*	*0.209*

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

MAP OF ITALY WITH THE AREA COVERED BY THE POLICY IN GREEN

Figure 2a

FIRMS' DENSITY DISTRIBUTION BY SCORE

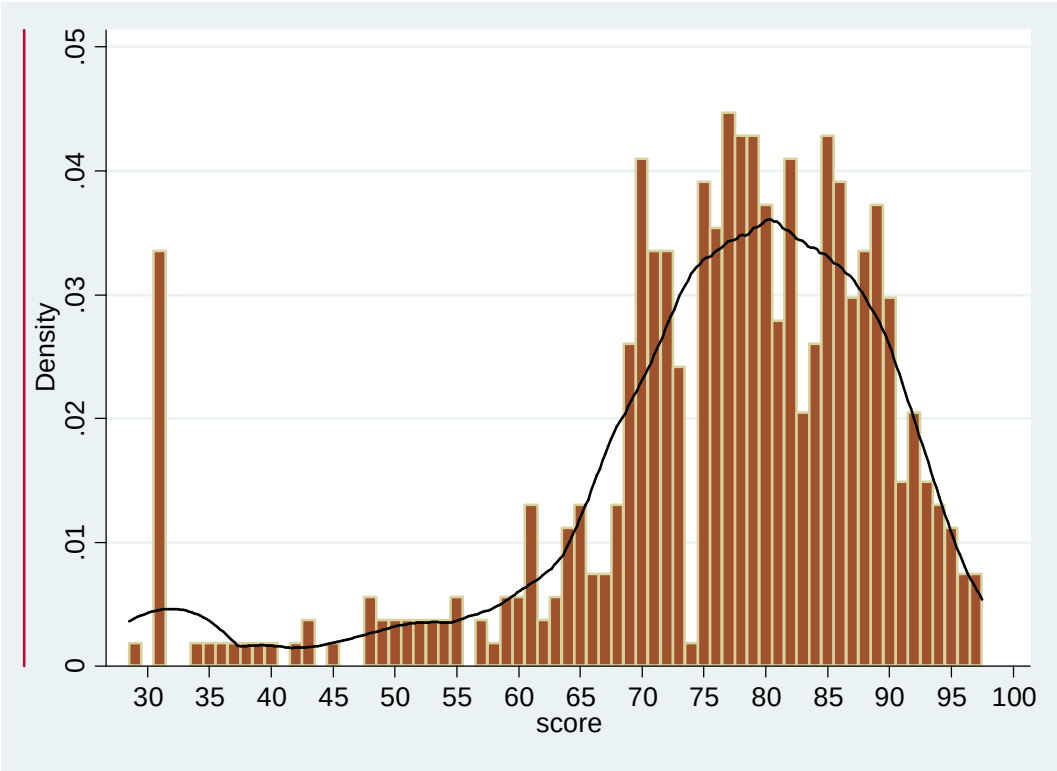
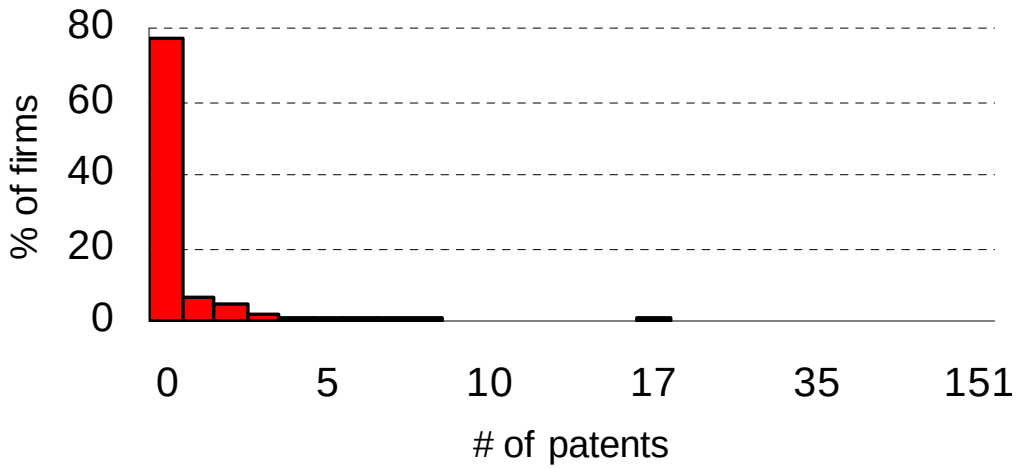


Figure 2b

FIRMS' DENSITY BY # OF PATENT APPLICATIONS

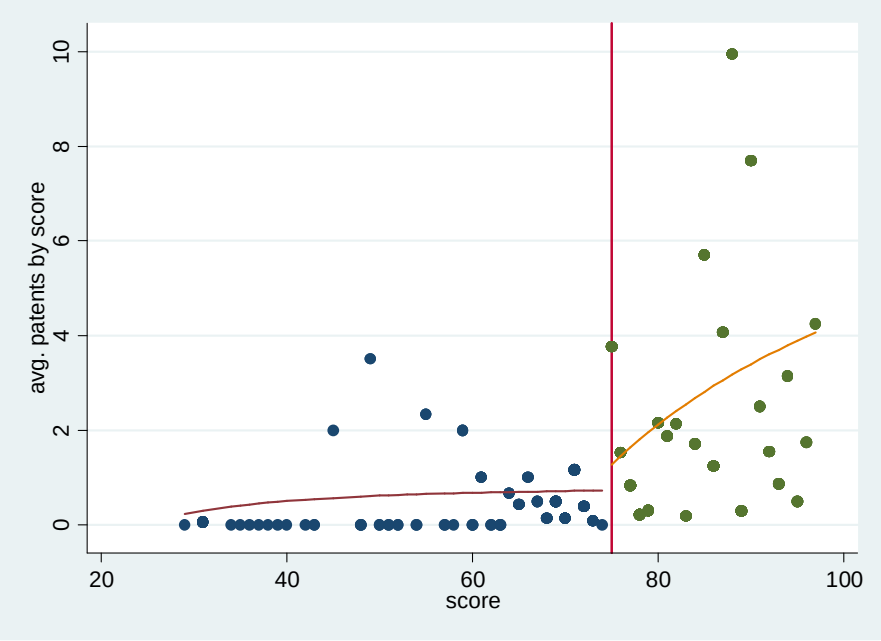


Notes: Counts in the treatment period (Period 1).

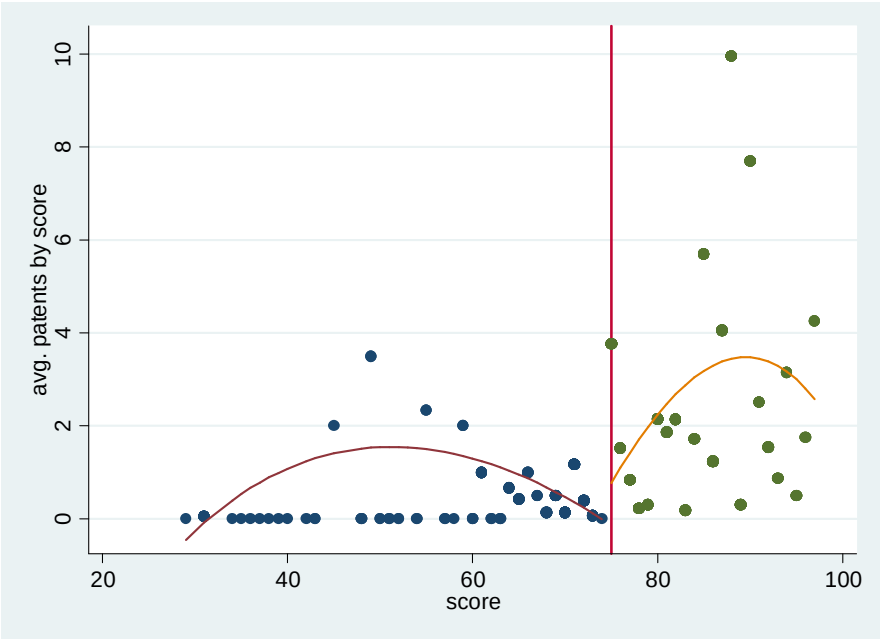
Figure 3a

NUMBER OF PATENT APPLICATIONS BY SCORE-TREATMENT PERIOD

Linear interpolation



Quadratic interpolation

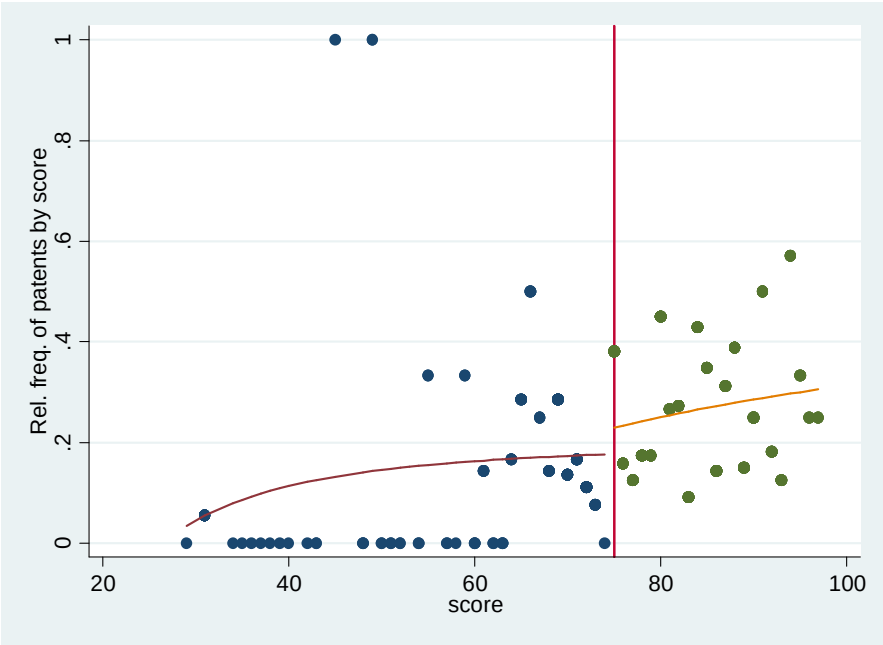


Notes: Based on counts in the treatment period (Period 1). In order to make graphs comparable, y-axis scale is the same across fig. 3a and 3b, 4a and 4b respectively. As a result, the two highest values in fig. 3a are not included in the graph. Interpolation curve is still worked out on the basis of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

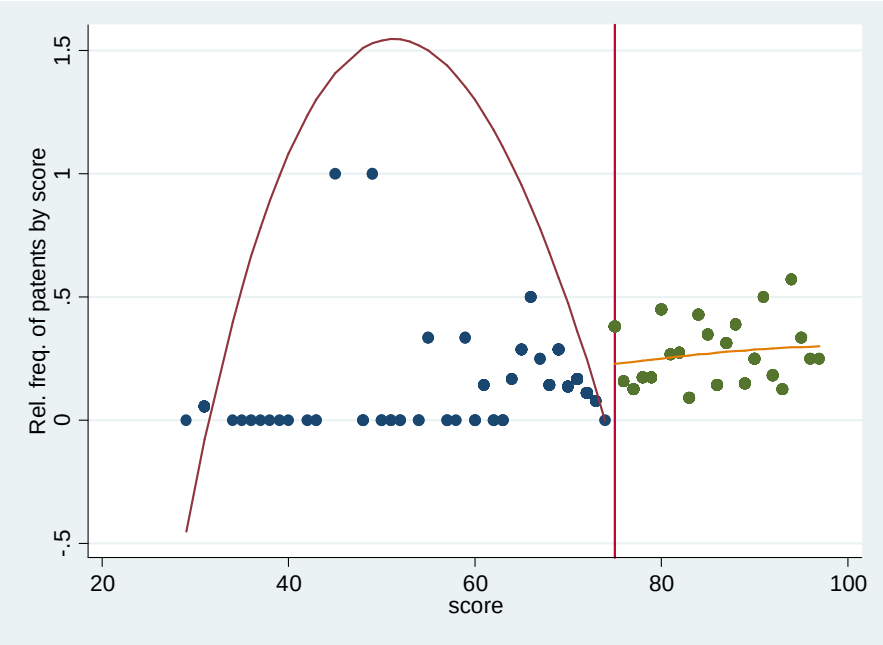
Figure 3b

PROBABILITY TO APPLY FOR PATENTING BY SCORE-TREATMENT PERIOD

Linear interpolation



Quadratic interpolation

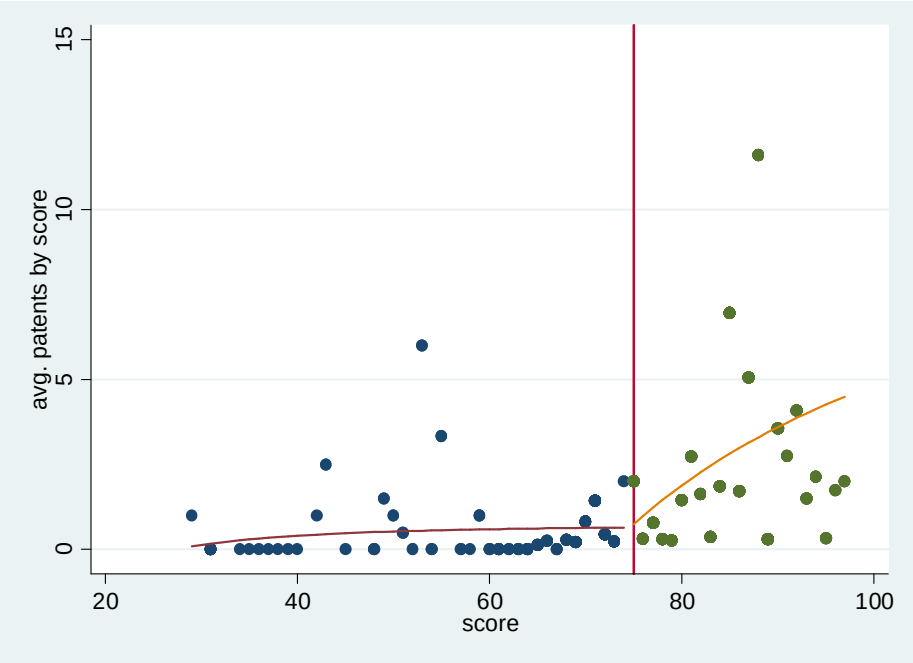


Notes: Based on counts in the treatment period (Period 1).

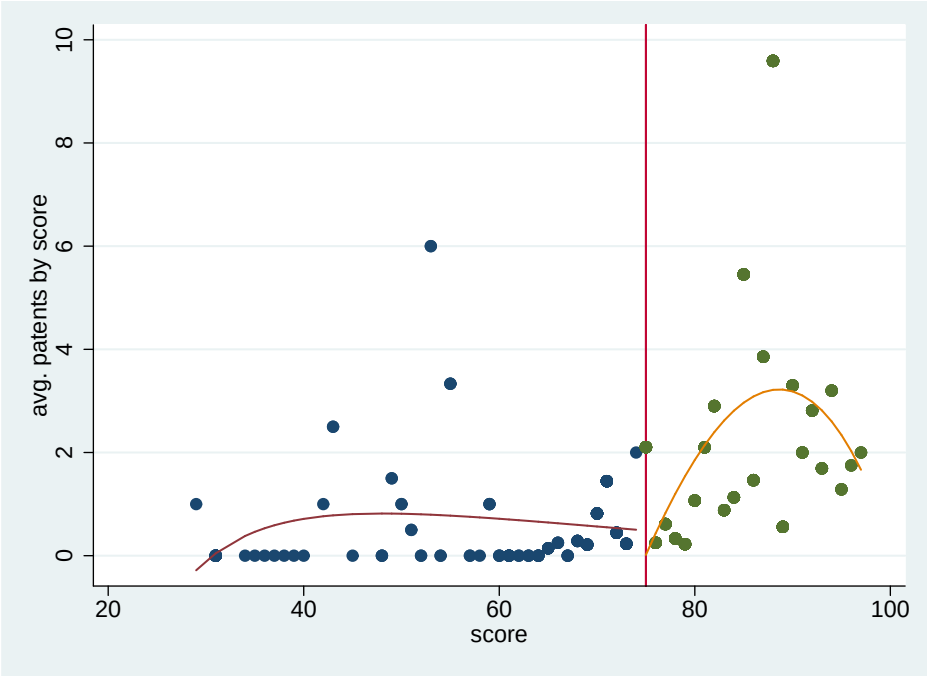
Figure 4a

NUMBER OF PATENT APPLICATIONS BY SCORE – PRE-TREATMENT PERIOD

Linear interpolation



Quadratic interpolation

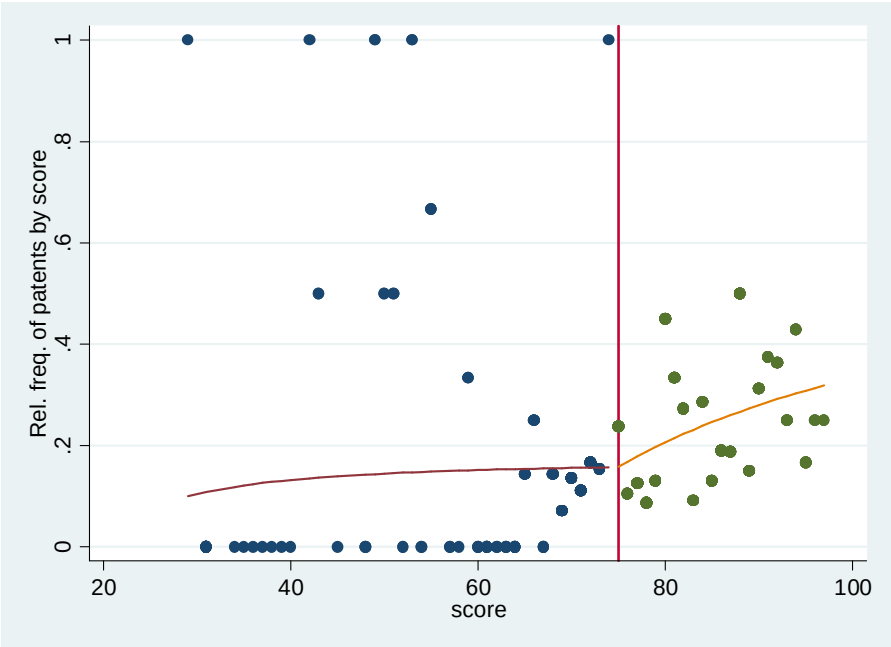


Notes: Based on counts in the 5-year length pre-treatment period.

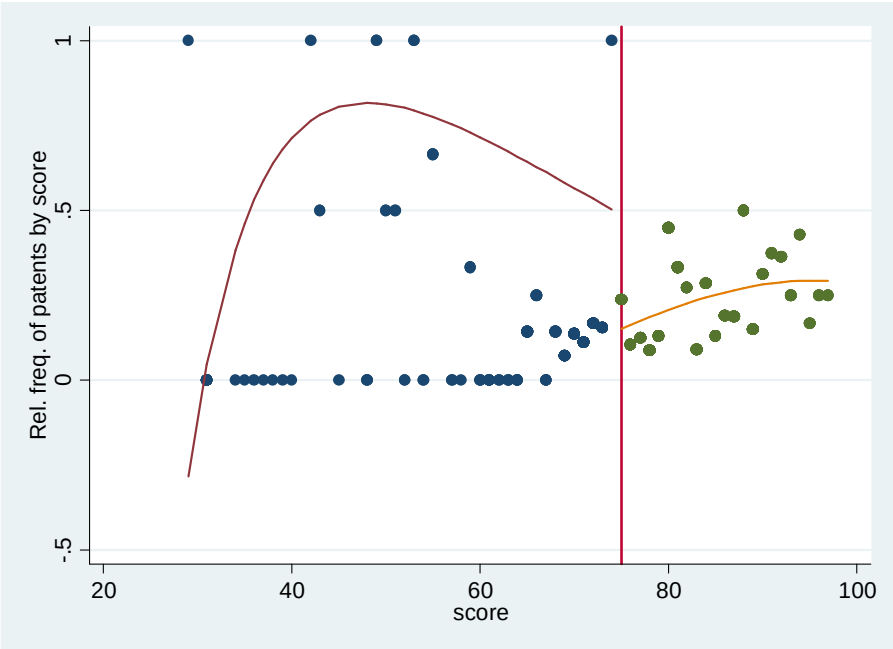
Figure 4b

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

Linear interpolation



Quadratic interpolation

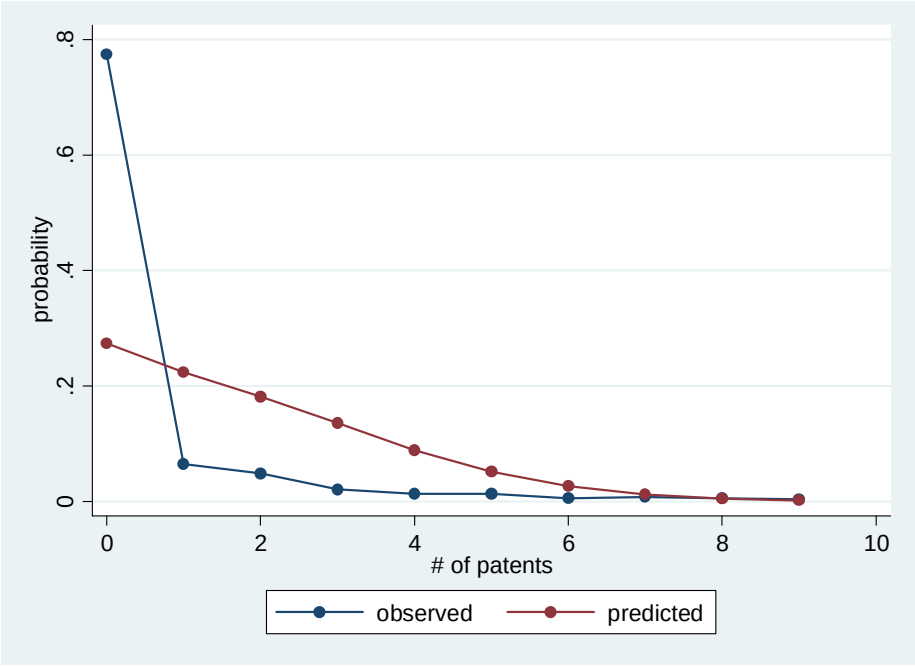


Notes: Based on counts in the 5-year length pre-treatment period.

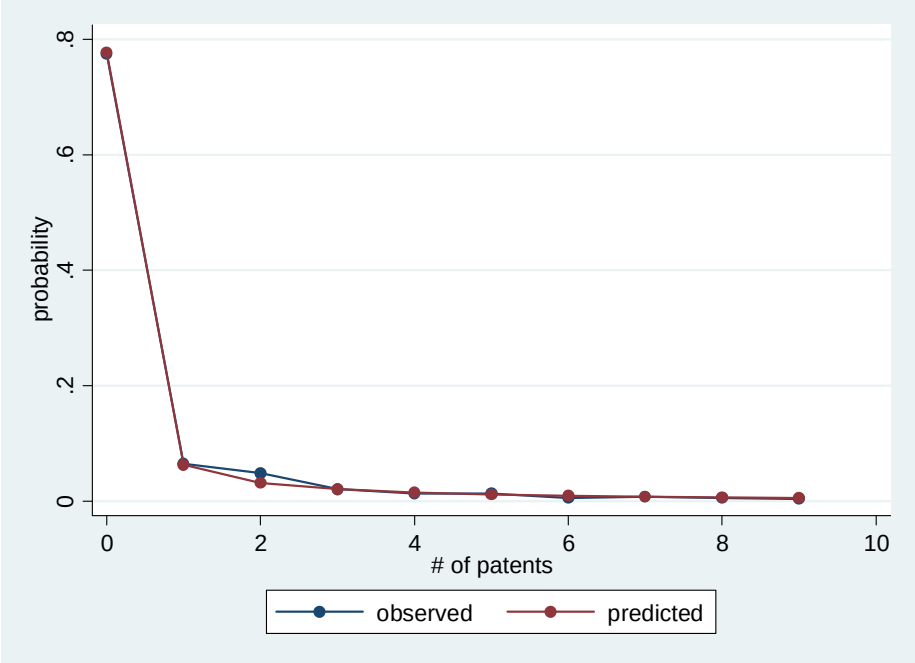
Figure 5

FITTED PROBABILITY:
POISSON & NEGATIVE BINOMIAL

Poisson



Negative binomial



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