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ARE INCENTIVES FOR R&D EFFECTIVE? EVIDENCE FROM A REGRESSION DISCONTINUITY APPROACH

by Raffaello Bronzini [*] and Eleonora Iachini [**]

Abstract

This paper contributes to the literature on the effectiveness of R&D incentives by evaluating a unique investment subsidy program implemented in northern Italy. 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 investment spending of subsidized firms just above the cut-off score with spending by firms just below the cut-off. For the sample as a whole we find no significant increase in investment as a result of the program. This overall effect, however, masks substantial heterogeneity in the program's impact. On average, we estimate that small enterprises increased their investments by about the amount of the subsidy they received from the program, whereas for larger firms the subsidies appear to have had no additional effect.

JEL classification: R0, H2, L10.
Keywords: research and development, investment incentives, crowding-out, regression discontinuity design.

Contents

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[*] Bank of Italy, Structural Economic Analysis Department. E-mail: raffaello.bronzini@bancaditalia.it

[**] Bank of Italy. Market and Payment System Oversight Department. E-mail: eleonora.iachini@bancaditalia.it

1. Introduction[1]

Public incentives for private research and development (R&D) are offered in most of the advanced countries through direct funding or tax relief. This support is also substantial in amount. In the OECD countries direct government funding of business R&D, excluding tax incentives, amounts to 0.1% of GDP (OECD, 2008). The economic rationale for R&D subsidies, which also justifies their popularity, is based on a market failure argument. One justification is that knowledge has a public-good character: it is non-rival and non-excludable. Firms cannot entirely internalize the effect of R&D activity and positive externalities arise. In these circumstances the social return on R&D spending is greater than the private return. As a consequence, the equilibrium private investment is lower than the optimal social level and subsidies able to increase private R&D will raise social welfare. Another justification for R&D incentives is the presence of liquidity constraints. These constraints are particularly important for intangible investments, which are subject to considerable uncertainty and information asymmetry (see, for example, Bond and Van Reenen, 2007; Hall and Lerner, 2009).

In spite of the popularity of R&D investment subsidies, the question of whether these incentives actually work – i.e. increase firms' R&D activity – remains unsettled. Theory predicts that if a program subsidizes marginal projects, incentives will be ineffective because they do not trigger additional investment. To be successful a program must target infra-marginal projects – those that would not occur without the grants. Empirically, the impact of R&D subsidies has been widely studied but previous analyses have yielded very mixed results. Out of nineteen micro-econometric studies surveyed by David et al. (2000), half found no effect. Examining the papers published in the last decade we found a similar balance: out of a total of eleven, just six confirm a positive role for public incentives on

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R&D activity (see Table A1 in the Appendix for an overview).[2] The main challenge in empirical studies arises from the difficulty of inferring a causal effect of subsidies from comparisons between subsidized and unsubsidized firms. Subsidy recipients are not randomly chosen, rather, recipient and non-recipient firms are likely to differ in both observed and unobserved ways that are correlated with the outcome of interest. In this context, the variable capturing subsidy recipients is endogenous, and models that fail to adequately control for this endogeneity will be biased.

This paper contributes to the existing literature on firms' R&D subsidies by studying a unique program recently implemented in a region of northern Italy (Emilia-Romagna). The policy has several key features enabling the effectiveness of R&D incentives to be carefully assessed. First, it allows us to address the endogeneity issue with a sharp quasi-experimental strategy. The program envisages that, after the assessment of an independent technical committee, only eligible projects that receive a certain score are subsidized. Our identification strategy takes advantage of the funds' assignment mechanism. We compare the investment of subsidized and unsubsidized firms close to the threshold score using a sharp regression discontinuity (RD) design (Hahn et al., 2001). Compared with other methods employed in the program evaluation literature this strategy has an important advantage. Under general assumptions – in our study firms must not have the capacity to control their score completely – the assignment of the subsidy around the threshold is as if it had been random, so that the method becomes equivalent to a random experiment (Lee, 2008). Since the assumption of the imperfect control of the score has several direct and indirect testable implications, the validity of the strategy is also verifiable.

In addition, the policy's local dimension allows us to remove much of the unobserved heterogeneity among enterprises, and compare recipient and non-recipient firms that are more similar than those participating in nationwide programs. In fact, to be eligible a firm must both be located and implement the investment in the same region. Meanwhile, we focus on a region that is highly representative of the national industry: it is the third largest industrial region of the country, covering 11% of Italian firms' R&D outlays and more than

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[2] Given that our paper is focused on incentives through grants we do not consider the studies examining the effects of tax incentives (for the literature on fiscal incentives see, for example, Hall and Van Reenen 2000).

10% of patents. Small and medium-sized enterprises also play a key role in this area, as they do throughout the country.

A third attractive feature is the program's generosity (in total, about 93 million euros have been granted) and its involvement of a large number of firms (1,246 enterprises submitted a proposal). In our baseline sample, each subsidized firm received an average of 182,000 euros; one fourth of the total investment made by each participating firm during the two years after the program. The size of the grants and the high participation rate are helpful for the evaluation exercise.

Finally, our assessment permits us to shed light on the effects of place-based policies managed by local government.[3] To date these policies have attracted scant attention from the evaluation literature, despite absorbing a relatively large share of the total public transfers to the private sector.[4] In Italy, from 2000 to 2007 around 18 billion euros were granted to firms owing to these programs – one fourth of total public funds assigned to private enterprises. It is crucial to know the impact of these policies in order to gain greater awareness of the use of public resources.

Overall we find that the program did not create additional investment. Our results do not reject the hypothesis that firms substituted public for privately financed R&D. This overall effect, however, masks substantial heterogeneity in the impact of the program. When we estimated the effect of the program by firms' size we find that, unlike large firms, small enterprises increased their investment substantially, by on average the same amount of the grant received. We also find that for subsidized firms the amount of the grant received in relation to the investment programmed did not play a significant role. Our findings are robust to multiple sensitivity checks.

The remainder of the paper is structured as follows. In the next section, we discuss the theoretical issues and previous empirical literature. In section 3, we illustrate the features of the program. Section 4 describes the empirical strategy and the data employed in the

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[3] For a discussion of the theoretical rationale of place-based policies see Kline (2010).

[4] In Italy two exceptions are Gabriele et al., 2007 and Bondonio, 2007. However, they did not evaluate firms' R&D incentives.

evaluation exercise. The main results are shown in section 5. Some extensions of the baseline model together with the robustness exercises and the concluding remarks make up the final two sections.

2. Theoretical background and empirical evidence

Let us first discuss the theoretical issues in a simple static partial equilibrium setting. In a perfect capital market, each firm faces a downward sloping marginal return on investment schedule (MR) and a horizontal marginal cost of capital schedule (MC) that reflects the opportunity cost of the investment. There is a perfectly elastic supply of capital so that internal and external funds are perfectly interchangeable. The profit maximizing level of investment is such that the marginal cost is equal to the marginal return. In this case, public policy is ineffective because the subsidy will not change the investment opportunity cost and there will be no increase in investment. By contrast, in an imperfect capital market firms face a finance supply schedule that is initially horizontal, reflecting the availability of internal funds, and once the internal funds are exhausted it becomes upward sloping (see, for example, Bond and Van Reenen, 2007). This is shown by the continuous MC curve in Figure 1. The reason why the curve of the supply of finance is increasing is that the degree of leverage raises the probability and the severity of financial distress. Moreover, problems generated by asymmetric information, such as moral hazard, increase with the amount of borrowed funds. In these circumstances a public subsidy will shift the MC schedule to the right because it decreases the cost of funds, allowing the firms to substitute public funds for more costly private funds. The after-program schedule is represented by the dashed MC schedule in Figure 1. The impact of the grant on the firm's investment depends on the position of the MR schedule. If the MR curve intersects the MC curve at a point where the latter is horizontal (such as MR_A in the figure) the policy will not affect the equilibrium investment. The firm will completely substitute public for privately financed R&D to take advantage of the cost difference, but it will not change the optimal level of investment corresponding to K_A*. As in perfect capital markets, the privately financed R&D investment will be completely crowded out by the granted R&D expenditure, given that the policy does not change the opportunity cost of investment. These are the *infra-marginal* projects (or firms). On the other hand, if the pre-program firm's equilibrium is in the upward-sloping part of the MC curve (as in the case of MR_B), the grant will increase the optimal level of investment. Projects that before the program were unprofitable when privately financed become profitable after the subsidy, and firm will expand the optimal level of R&D from K_B* to K_B*`. These are the *marginal* projects (or firms).

This framework applies to both tangible and intangible investments, like R&D outlays. However, it has been argued that in the case of intangible assets liquidity constraints may be exacerbated. Informational asymmetries, causing credit constraints, may be amplified because R&D projects are riskier and less well understood by non-expert agents than other kinds of investment, or because firms may be less willing to share information with intermediaries to prevent leaks of knowledge to competitors. Intangible investment could be more subject to credit rationing also because financial intermediaries might prefer to finance projects related to tangible assets which, in turn, can be offered as collateral, rather than to intangible assets that are related only to future streams of profits (see Guiso, 1998; Bond and Van Reenen, 2007; Hall and Lerner, 2009).

Concerning the effectiveness of a program, it is clear that the process of assignment of funds plays a critical role, since only programs that subsidize marginal projects will activate additional investment. In this regard two considerations ought to be made. First, even assuming that public institutions demonstrate excellent ability in choosing the projects to subsidize, they may not be perfectly able to discern between marginal and infra-marginal ones. Therefore it is likely that, at least in part, funds will be given to infra-marginal projects, reducing the effectiveness of the subsidies. Second, institutions might be induced to subsidize infra-marginal projects to convince public opinion that the policy is not wasting resources, insofar as infra-marginal investments have higher success probabilities and higher profitability than marginal ones (see Wallsten, 2000 and Lach, 2002).

Up to now we focused on direct effects. However, several indirect (general equilibrium) effects of the policy might shift the MC or the MR schedules, generating multiple potential outcomes. For example, the grant might convey information on the profitability of the project and reduce the information asymmetries that subsidized firms face, lowering the private costs of capital further. Moreover, thanks to the grants, firms may benefit from an expanded or upgraded stock of research facilities, or from better trained researchers, both increasing the revenue of other current or future projects, and eventually shifting to the right of the MR schedule. However, there might also be indirect effects acting in the opposite direction. For example, if the supply of the R&D inputs is price inelastic, as with the supply of researchers in tight local labor markets, and the program is sufficiently large, demand shift for inputs triggered by the public program might increase the costs, ultimately crowding out the subsidies (see David et al., 2000, and Lach, 2002 for a more extensive discussion on indirect effects).

2.1 Empirical evidence

The main challenge that empirical studies face in assessing the effectiveness of R&D policies is that subsidized firms are not randomly chosen. Rather, they differ from non-subsidized firms in terms of important unobserved characteristics correlated with the outcome variable, so that in the econometric model the variable that identifies subsidized firms is endogenous. In recent analyses the endogeneity problem has been addressed mainly through matching methods or instrumental variable estimates. However, independently of the strategy adopted, the conclusions of earlier studies are mixed.

Surveying firm-level analyses conducted in the previous three decades, David et al. (2000) observe that almost half (9 out of 19) of the policies were not found to trigger additional investment while for the other half it was the contrary. More recent evidence is similarly non-conclusive. In the case of the Small Business Innovation Research program in the U.S. two studies reach opposite conclusions. Matching subsidized and unsubsidized firms by industry and size, Lerner (1999) finds that the policy increased sales and employment of subsidized firms; by contrast Wallsten (2000), using the amount of public funds available for each type of R&D investment in each year as an instrument for the subsidy, shows that grants did not lead to an increase in employment and that the public subsidy crowded out firm-financed R&D dollar for dollar. The evidence available for other countries is also mixed. For Israel, Lach (2002) finds that grants created additional R&D investment for small firms but, since the greatest share of the subsidies was given to large firms that did not make additional investment, the overall impact was null. He compared the performance of subsidized and non-subsidized firms using difference-in-difference (DID) estimates and controlling for several observables. Almus and Czarnitzki (2003) use matching strategies to study R&D subsidies in Eastern Germany, finding an overall positive and significant effect on investment. Gonzalez et al. (2005) examine the effects of R&D policies in Spain, estimating simultaneously the probability of obtaining a subsidy, assuming a set of firms' observables as pre-determined (e.g. size, age, industry, location, capital growth), and the impact of the grant on investment. They find a positive, albeit very small, effect on private investment that turns out to be significantly larger for small firms. Gorg and Strobl (2007) combining the matching method with DID estimation find that in Ireland only small grants had additional effects on private R&D investment, while large grants crowded out private investment. Finally, Hussinger (2008) uses two-step selection models showing that in Germany public subsidies were effective in promoting firms' R&D investment.[5]

3. The program

In 2003 the government of Emilia-Romagna implemented 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 A1 in the Appendix. According to the program, the regional government subsidizes the R&D expenditure of eligible firms through grants. The grant may 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 – including temporary associations or consortia – are those that have an operative main office and intend to implement the project in the region. Several types of outlays, related to the

[5] The empirical literature includes also Busom (2000), who finds that public funds led to more private expenditure in Spain, even if she cannot exclude that crowding out occurred for 30% of participants; Branstetter and Sakakibara (2002), who show how public-sponsored research consortia increased the patenting activity of Japanese firms in a consortium; Hujer and Radic (2005), who examine the impact of public subsidies on firms' innovation propensity in Germany, finding a positive impact only for Eastern Germany. In Italy, Merito et al. (2007) show that subsidies had no impact on post-program employment levels, productivity or sales of the subsidized firms with respect to matched untreated ones. See also the surveys by Klette et al. (2000) and Hall and Van Reenen (2000).

eligible project, can be subsidized: a) costs for machinery and equipment; b) software; c) registration of patents; d) employment of researchers; e) the use of laboratories; f) contracts with research centers; g) consulting; h) feasibility studies; i) licenses and external costs for the realization of prototypes. To be eligible, projects must be worth at least 150,000 euros; the maximum grant per project is 250,000 euros. The investment can last from 12 to 24 months, but the period can be extended. 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.

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: a) technological and scientific (max. 45 points); b) financial and economic (max. 20 points); c) managerial (max. 20 points); d) 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]

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[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.

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. For the industrial firms in our sample used for the estimates grants averaged 182,000 euros, one fourth of the total investment made by each participating firm during the two years after the program.

4. Empirical strategy and data

4.1 Empirical strategy

Our goal is to evaluate whether subsidized firms would not have made the same amount of R&D outlays without the grants. A typical issue of the program evaluation literature is that subsidized and non-subsidized firms can differ in terms of unobserved characteristics correlated with the outcome. Therefore, the variable identifying recipient firms in the econometric models can be endogenous and we have to adopt a strategy that addresses this endogeneity to identify correctly the effect of the program. 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 threshold were awarded grants (75 points out of 100). We apply a sharp regression discontinuity (RD) design comparing the performance between 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.

In the last decade a growing number of empirical studies in economics have utilized the RD design, since the seminal contributions by Angrist and Lavy (1999), Black (1999)

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

and van der Klaauw (2002).[9] This strategy is deemed preferable to other non-experimental methods to control for the endogeneity of treatment because, under rather general conditions, it is possible to demonstrate that it is equivalent to a randomized experiment. The identification 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, as if the agents had been randomly drawn just below or just above the cut-off. In this scenario, the impact of the program is identified by the discontinuity of the outcome variable at the cut-off point (Hahn et al. 2001).

RD design is suitable in contexts where the agents cannot perfectly manipulate the forcing variable (the score). We believe that in our situation this strategy is appropriate, in that it is hard to argue that firms participating in the program can completely control their score. In any event, the randomization assumption has testable implications. If a subsidy is random around the threshold, treated and untreated firms close to the threshold will be similar (more than those distant from the cut-off). The similarity of the two groups is a consequence of randomization and not vice versa (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. Moreover, there are indirect ways of testing the validity of the crucial continuity assumption, by checking whether other covariates, or the outcome variable in the absence of the program, are continuous across the threshold. We will present the results of these tests in section 6.

Since under the RD method results can be sensitive to some arbitrary choices, such as the functional form or the interval around the cut-off point used in the local regressions, we use multiple functional forms and econometric models for robustness purposes.

[9] See Lee and Lemieux (2009) and the monographic number of the *Journal of Econometrics*, vol. 142(2), 2008.

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, 2009). Here we use both parametric and non-parametric methods. First, we estimate up to a third order polynomial model on the full sample:[10]

$$Y_i = \alpha + \beta T_i + (1 - T_i)\sum_{p=1}^{3} \gamma_p (S_i)^p + T_i \sum_{p=1}^{3} \gamma'_p (S_i)^p + \varepsilon_i \tag{1}$$

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).

Second, equation (1) has been estimated through local regressions around the cut-off point using two different sample windows. The wide-window includes 50% of the baseline sample (firms with scores between 52 and 80); the narrow-window includes 35% of the baseline sample (scores in the 66-78 range). The ranges have been chosen to (almost) balance the number of firms to the left and right side of the threshold. Third, we estimated the discontinuity using other non-parametric techniques, namely the Epanechnikov kernel regressions using two bandwidths, 30 and 15 points of the score (see section 6).

If model (1) is correctly specified, the OLS estimate of the parameter β measures the value of the discontinuity of function $Y(S_i)$ at the cut-off point, corresponding to the unbiased estimate of the causal effect of the program. For the inference, however, a word of caution is necessary. Since our forcing variable is discrete (the score can assume only integer values) random disturbances can be correlated within the group (similarly to the cases discussed by Moulton, 1990). In our study the groups are represented by firms that received the same score. In these circumstances standard errors could be downward biased and spurious statistical significance may occur. To correct for this bias we clustered the heteroskedasticity

[10] Higher orders of polynomials were rejected by standard model selection criteria (Akaike Information Criterion and Schwartz Bayesian Criterion). Studies that adopt similar models include Card et al. (2007) and Lalive (2008).

robust standard errors by the values of the score *S* (Lee and Card, 2008). In the kernel regressions standard errors are clustered and bootstrapped.

4.2 Outcome variables and data

Regarding outcome variables, one potential candidate is R&D investment. However, reliable data on R&D outlays are normally gathered directly from the firms through specific surveys and in our case are unavailable. Therefore, we adopt a different strategy. We build the analysis on balance-sheet data provided by the CERVED, which collects information on Italian corporations. From the balance sheets we take as outcome variables those items that are associated with R&D outlays reimbursable by the program and listed above in section 3. The rationale is that if the program allowed outlays that without the grant would not have been made, we should observe a significant increase in at least one of these items for the recipient firms after the program, relative to those of non-recipient firms. More specifically, since the main reimbursable outlays refer to tangible and intangible investment (see section 3; those labeled *a*, *b* and *c*), we take the investment as the first (and favored) outcome variable.[11] However, other reimbursable outlays are those related to the employment of researchers (point *d*). Thus, we use two additional outcome variables: labor costs and level of employment. Thanks to the program, labor costs may increase because firms hire additional employees and/or because they substitute high-skilled employees (researchers) for low-skilled employees. Employment - available in our data set only for a sub-sample of firms - enables further light to be shed on the effect of the program on labor input. Finally, since other minor costs listed in section 3 refer to the services bought by the firms for R&D projects (see points *e* to *i*), we take the services' costs as our last outcome variable. This strategy permits us to distinguish the effect on the different types of R&D expenditure.

To sum up, we assessed the impact of the program on the following outcome variables: investment (total, tangible and intangible), labor costs, level of employment and service costs. All the variables are accumulated from the year of the assignment up to two years

[11] The law does not specify how R&D outlays must be booked in the balance sheets. If the costs are related to the development of a specific product, productive process, or the application of innovations that have multiyear utility, they are usually booked among investments.

afterwards (the expected period of the project's realization), to detect the whole R&D activity potentially related to the subsidized investment. Moreover, except employment, they are scaled by the pre-program sales (first pre-assignment year). Employment is not scaled but in log. Finally, to avoid results being driven by outliers – especially for investment that are highly volatile over time and uneven across firms – we trimmed the sample according to the 5th and 95th percentile of the distribution of $Total\ investment_i/Sales_i$ (also trimming investment over assets does not substantially change the results).

The balance-sheet data have been combined with the data set provided by the Emilia-Romagna Region that includes a limited amount of information on participating firms, but that is nonetheless crucial for the evaluation exercise: such as name, score, investment planned, grants assigned, subsidies revoked and renunciations.

To date two auctions have been concluded, in 2004 and 2005. We pool together the data of the two auctions. 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 in the second auction 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.

Having linked information on participating firms provided by the Region with the balance-sheet data set, and having cleaned the sample, we ended up with a full sample of 357 industrial firms (254 treated and 103 untreated) and 111 services firms (of which 61 treated).[12] The sample covers the large majority of the grants. In the sample used for the estimates, overall recipient firms received 66% per cent of the total funds granted; if we include outliers of the trimmed tails the coverage ratio reaches 94%.[13] However, since start-

[12] We were able to link 750 of the scored firms (499 subsidized and 251 unsubsidized) with the balance sheet data set. Other applicants are missing because, for example, they were not corporations, were start-ups or because of misprints of the firms' identifying data. Next, we excluded firms involved in renunciations and revocations (114 firms), 3 firms from the energy and mining sectors together with firms that have sales or assets equal to 0 and firms unsubsidized in the first auction but subsidized in the second. As mentioned earlier we also excluded the 5° and 95° percentile of our key outcome variable (investment over pre-assignment sales).

[13] Renunciations and revocations, which cover only a minor part of the total grants, are excluded.

ups and very small enterprises are underrepresented in our data, we are aware that our findings could not apply to these categories of firms. Finally, given that the remarkable heterogeneity between industrial and service firms, and within the service industry (which includes, for example, professional offices, transport and real estate), might produce large noise in our data, we focused on industrial firms (manufacturing and construction) and present the results of the baseline model for services only as an extension.

In Table 1 the distribution of firms by sector is reported. We notice that there is a large concentration of firms within just two sectors: machinery and chemicals together absorb two thirds of the firms' sample. The former is a sector of regional specialization, but also represents the main industrial sector in Italian industry as a whole. The concentration of firms in a few sectors reinforces our evaluation exercise, in that it allows us to compare homogeneous firms. Moreover, because of the exclusion of the non-scored applicant firms from the second auction, treated firms are more than double the number of untreated ones, while the proportion of treated and untreated firms is pretty well balanced within each sector.

Table 2 shows the means of several observables in the year before the assignment of funds for treated and untreated firms. We notice that treated firms are substantially larger than untreated firms overall, as shown by mean differences of sales, valued added and assets. A significant, and potentially worrying, difference arises also for firms' self-financing capabilities, measured by cash flow over sales. However, when we restrict the sample to around the cut-off, using both the wide and narrow band described above, treated and untreated firms become more alike. In particular the improvement is notable for size and self-financing power. Now differences between the two groups are remarkably smaller and never statistically significant.

In Figure 2 the density function of the sample by score is shown. 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. We observe, however, 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.

5. Results

5.1 Baseline results

We first present the estimations of the coefficient β of model (1) using total, tangible and intangible investment scaled by sales as outcome variables. Since we do not observe private investment but total investment, let us briefly discuss how to interpret the results. A coefficient β equal to zero would signal complete crowding-out of private investment by public grant: firms reduced private expenditure by the same amount of the subsidies and the investment turned out to be unaffected by the program. On the other hand a positive coefficient would show that overall treated firms invested more than untreated firms, plausibly thanks to the program, and that total crowding-out did not occur. However, it is still possible that firms partially substituted public for private financed R&D outlays. In order to evaluate if partial crowding-out, or on the contrary even crowding-in, occurred – that is if public subsidies have triggered private financed investment – we have to compare the change in total investment with the grants.

Before showing the econometric results let us present the scatter plot of the (averaged by score) outcome variables against the score (Figure 3). As expected, the figure shows rather dispersed points, given that investment is usually greatly uneven across firms. Apparently, the interpolation lines are almost flat, showing a weak dependence of the overall outcome on the score. As matter of fact, no remarkable jumps of the outcome variable at the threshold emerge from the figures; however, if anything, the impact seems somewhat positive.

This perception is confirmed by the econometric estimates of the coefficient β shown in Table 3. Akaike Information Criterion (AIC) suggests a preference for more parsimonious models, namely simple mean differences, rather than a higher order of polynomials in all cases but one. The sign of the coefficient is almost always positive. Using the full sample as a benchmark, the jump turns out to be equal to about 1/3 of the mean of the outcome variable of the untreated firms. Due to the sample variance, however, the discontinuity is almost never statistically significant (the coefficient is weakly significant in just 4 out of 30 models). Local estimates generate similar results to those of the full sample.

It is possible that we were unable to detect any effect because, for example, firms had used the grants for hiring researchers or for consulting contracts. To check for this eventuality we test for the discontinuity of labor and services costs, using these as further outcome variables. Moreover, we change the scale variable for investment using capital and total assets to check the sensitiveness of our previous findings on investment. The results of these exercises are reported in Table 4. Labor costs almost always have a negative sign, but only rarely is the coefficient statistically significant. With regard to service costs, the discontinuity is never significant and the sign is not stable across model's specifications. The previous results do not even seem affected by the variable used to scale investment; even though in some models the coefficient now turns out to be statistically significant. Finally, we estimated the effect of the incentives on the (log of) employment on a sub-sample of firms that reported such information (263 out of 357). Table A4 in the appendix displays the results (see the first three columns; in the local regressions we use only the wide window because of the sample size). Overall it seems that the level of employment did not change thanks to the program: the coefficients are almost never statistically significant.

On the whole, the results show that the effectiveness of the program is questionable. We cannot reject the hypothesis of complete crowding-out of private investment and we do not observe any significant impact of the policy on the other variables potentially affected by the program.

5.2 Results by firms' size

So far we have not found evidence of effectiveness of the public subsidies. It is possible, however, that even if overall crowding-out occurred, for firms for whom the cost premium of external finance was greater, the subsidies created additional investment. In the literature on capital market imperfection, it has been argued that among the firms that may have worse access to capital markets are the small ones. First, because information asymmetries are strengthened for small enterprises, given that they are less visible, usually younger, and the capabilities of their management less well-known. Second, small firms often lack sufficient collateral. Third, their production is usually less diversified and, as a consequence, their earnings may be more volatile. For all these reasons they are more dependent on external finance and, at the same time, less able than larger firms to raise funds from the capital market. Empirically, the negative relationship between financial constraints and firms' size has been supported by Gertler and Gilchrist (1994), Gilchrist and Himmelberg (1995) and Beck et al. (2005), amongst others, although other studies have questioned it (see, for example: Guiso, 1998 and Audretsch and Elston, 2002).

If liquidity constraints are amplified for innovative investment and small firms have less access to financing, the effectiveness of innovation subsidies could be inversely related to the size of firms. Some of the previous empirical evidence tends to support this hypothesis (e.g. Lach, 2002 and Gonzalez et al., 2005). To test for a heterogeneous causal effect of the program across firms' size we estimated the following model, where the firms' size dummies are interacted with the treatment dummy and the score:

$$Y_i = (1-T_i)\sum_{k=1}^{2}\alpha_k Size_i^k + T_i\sum_{k=1}^{2}\beta_k Size_i^k + (1-T_i)\sum_{k=1}^{2}\sum_{p=1}^{3}\gamma_{kp} Size_i^k (S_i)^p + T_i\sum_{k=1}^{2}\sum_{p=1}^{3}\gamma'_{kp} Size_i^k (S_i)^p + \eta_i \qquad (2)$$

where $Size_i^1=1$ if the value added of firm *i* is below the median and zero otherwise (*Small*); $Size_i^2=1$ if the value added is above the median and zero otherwise (*Large*).[14] 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*.

Before showing the results by size of firms, let us verify whether treated and untreated firms are similar in the two sub-samples of large and small enterprises. In Tables A2 in the appendix we display the distribution of firms by size, sector and treatment. In Table A3 we report mean differences of various observables for treated and untreated firms by size. The tables show that in each category of firm there are no significant differences in the

[14] The results are not sensitive to the choice of the variable used to measure size. We have replicated the estimates using sales, instead of value added, obtaining results that are almost undistinguishable from those presented in the table.

distribution of treated and untreated firms across sectors. Moreover, in our sample small (large) treated firms are greatly similar to small (large) untreated ones around the cut-off in terms of several observables. This evidence supports the implementation of our strategy also for each firms' sub-sample.

Figures 4 and 5 outline the investment by sale against the score for the two groups. Again, from the figures emerge the independence of the investment from the score. The effect seems null for large firms but positive and rather substantial for small ones. In Table 5 we show the results of the estimates of model (2) on investment. For small firms the impact turns out to be positive and statistically significant. This result is robust to the choice of both functional form and sample: the discontinuity is positive and significant in the full sample (Panel A) and in the local regressions (Panel B and C). Only in the smallest sample when we used the quadratic model, the parameter turns out to be statically non-significant, arguably because of the loss of efficiency. By contrast, for large firms we find mainly negative but non-statistically significant coefficients.

Interestingly enough, the impact seems rather balanced between investment in tangible and intangible assets: the coefficients turn out to be rather similar among the two types of investment. Therefore, it seems that intangible and physical capital investment have been mostly complementary.

For small firms the effect of the program appears remarkable. If we take as our benchmark the estimates by the polynomial of order 0, as AIC suggests, in the full sample the increase in investment is twice the mean of investment of untreated firms, around 40% of its standard deviation. Even if it seems like an exceptional increase, we have to take into account that the grants have been substantial (for the small firms on average equal to 173 thousand euros) especially if compared to the investment of untreated firms (107 thousand euros on average).

We replicated the estimates using labor and services costs as outcome variables. Table 6 shows that both types of cost did not change because of the program, neither for small firms nor for large ones. Finally, we verified the effects of the incentives on the (log of) employment. The results are displayed in Table A4 in the appendix. While they seem sensitive to the model chosen and the estimation strategy used, overall we can conclude that not even employment changed thanks to the program; again the results are slightly more favorable for small firms.

In order to measure more accurately the impact of the policy on small firms' investment and to understand if partial crowding-out, or in contrast crowding-in, occurred, we re-estimated model (2), regressing total investment on the grants disbursed to the firms (instead of on the treatment dummy variable T):

$$INV_i = (1-T_i)\sum_{k=1}^{2}\alpha_k Size_i^k + GRANT_i\sum_{k=1}^{2}\beta_k Size_i^k + (1-T_i)\sum_{k=1}^{2}\sum_{p=1}^{3}\gamma_{kp} Size_i^k (S_i)^p + T_i\sum_{k=1}^{2}\sum_{p=1}^{3}\gamma'_{kp} Size_i^k (S_i)^p + \eta_i \quad (3)$$

A coefficient β_k positive and smaller (larger) than one would indicate that on average partial crowding-out (crowding-in) occurred; i.e. the change in the investment produced by the subsidy was smaller (larger) than the grant; a coefficient equal to one implies that the increase of the investment was equal to the subsidy received.

The estimations of β_k in model (3) are reported in Table 5 (last three columns). For small firms we find a parameter very close to one in the polynomial of order 0 (equivalent to the mean difference); in the linear model or higher order polynomial models the coefficient increases in magnitude. Yet, the hypothesis of β_{small} equal to one is largely accepted by t-tests (calculated with robust standard errors clustered by score) in all models. Therefore, it seems that small firms have increased their total investment outlays after, and owing to, the program exactly by the same amount of the grants received. On the other hand, grants to larger firms completely displaced private expenditure, probably because they possess sufficient internal financial resources and have better access to the credit market to finance their innovation outlays.

To verify this interpretation, we observe some indexes plausibly correlated to firms' internal and external financial capability. We calculate for industrial firms of our full sample the mean of: the own capital/debts ratio, reflecting the capability of the firms to provide collaterals; the cash flows over sales and ROA, showing the ability of the firms to finance investment with internal funds; the financial costs over total debts as a proxy of the interest rate paid by the firms for external funds. As reported in Table 7, all the indexes turn out to be on average worse for small firms. For cash flow and ROA the gap of small firms is also statistically significant. This evidence tends to supports the interpretation that the program was effective for small firms because they were more dependent on, and plausibly have more limited access to, external finance than larger firms.

6. Extensions and robustness checks

In this section we present some extensions of our previous model and the robustness exercises run to test the validity of our empirical design and the sensitiveness of our results. First of all, we investigated how firms in the services sector reacted to the subsidies, thus we re-estimated model (1) and (2) only for participating firms belonging to the services sector. The results are presented in Table 8. Given that service firms are less numerous we only used the wide-window sample for local regressions. Overall, the results obtained for industrial enterprises are confirmed for those of services. We do not find any positive effect of the policy on the whole sample, but when we split it by firms' size we find again that the impact is positive and mostly significant for small firms, while it is negative and only rarely statistically significant for large ones (labor and service costs did not change because of the program; results are not shown but available upon request).

A relatively little-studied topic in the evaluation literature, yet one which appears important for designing effective policies, is the role played by the share of investment covered by the grant (coverage ratio). It is possible that for firms with a high coverage ratio subsidies could be more effective than for those with a small coverage ratio. For a program that supported tangible investment in Italy, Bronzini and de Blasio (2006) found that investment by firms with a high coverage ratio increased significantly with respect to those of the untreated firms, while for those with a low coverage ratio the rise was not significant. For the same program, Adorno et al. (2007) showed that the coverage ratio of subsidies had a non-linear impact on investment: up to a certain point subsidies grew along with the coverage ratio, but after a certain point the relation reversed.

The exercise is based on the estimation of the following model:

$$Y_i = (1 - T_i)\sum_{j=1}^{2} \alpha_j Intens_i^j + T_i \sum_{j=1}^{2} \beta_j Intens_i^j + (1 - T_i)\sum_{j=1}^{2}\sum_{p=1}^{2} \gamma_{jp} Intens_i^j (S_i)^p + T_i \sum_{j=1}^{2}\sum_{p=1}^{2} \gamma'_{jp} Intens_i^j (S_i)^p + \nu_i \quad (4)$$

where $Intens^1$=*High* and $Intens^2$=*Low*. *High* (*Low*) is a dummy variable equal to 1 if the grant/investment ratio of firms *i* is higher (lower) than the median of the overall firms' distribution, and 0 otherwise.

Before describing the results, let us present some descriptive statistics of the grant intensity variable. Its distribution looks like a normal with mean and median equal to 0.40. In our sample there is little variation across firms: the standard deviation is 0.05; the 25th and 75th percentiles are equal, respectively, to 0.38 and 0.43. As a consequence, firms above and below the median turn out to be rather homogeneous in terms of the coverage ratio. The estimates of model (4) are reported in Table 9. We notice that the coefficients are almost always positive, for both low and high grant-intensity firms, but only sometimes statistically significant. Rather surprisingly, the coefficients of low grant-intensity firms turn out to be usually larger than those of high ones. These differences, however, are often negligible and not statistically significant. For example, in 5 out of 9 models estimated on total investment the null of equality of the coefficients for low and high grants coverage ratio is accepted by a standard F-test. On the whole, we are inclined to believe that the intensity of the grant did not play a significant role in the program examined.

In the remaining part of the section we present some robustness checks of our main findings, carried out on the sample of industrial firms. As a first check, we introduce pre-treatment firm-observables in model (1) and (2) to increase the precision of our estimates and correct for potential imbalances between treated and untreated firms that might be correlated with the outcome variable, for example differences in sectoral composition. This imbalance might be larger in the exercise with the sample split, when the number of firms is reduced. The covariates introduced consist of 2-digit sectoral dummies and some observables that in principle may be correlated with the investment: gross operative margin/sales (a measure of operative profitability), cash flows/sales (proxy of the self-financing capability), own capital/debts (measuring the leverage), financial costs/debts (proxy of the cost of borrowing), ROA and total assets (measures of size). All variables refer to the pre-treatment period. The results shown in Table 10 are remarkably similar to the baseline ones. The coefficients turn out to be close in magnitude to those previously estimated and highly statistically significant for small firms.

In addition, we verify whether our main results depend on the estimation methods. Therefore, we run kernel regressions of model (1) and (2) using the Epanechnikov kernel, several polynomials and different bandwidths: 30 and 15 points of the score (below and above the threshold). Results shown in tables A4 and A5 in the appendix confirm those previously obtained. The coefficients are significant only for the investment of small firms and very close in their magnitude to the earlier ones. By using the triangular kernel or different bandwidths we obtained similar findings.

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. A first one is to verify if some firms' covariates that in principle should not be affected by the treatment (at least in the short run) are continuous at the cut-off. 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 following observables that could, in principle, be correlated with investment: profitability (ROA), net assets over debts, the cash flow over sales and costs of debts (interest costs over debts). We replicated the estimates of model (2) using these covariates as outcomes. We almost never find significant discontinuities (Table 11).

Another indirect way to test for the continuity assumption is to verify whether the outcome variable before the program was smooth across the cut-off. If we observe a smooth function before the program took place, it is plausible that the jump we observe after the program is due to the subsidy. Therefore, we re-estimated model (2) using as outcome variable the investment in the period before the program. Notice that since in the baseline exercise we accumulated the investment over some years after the program, to make the robustness exercise as comparable to the baseline estimates as possible we accumulated the investment over the two years before the program. Table 12 (section 1) and Figure 5 show that before the program there were no jumps in investment.

Finally, we check whether there are discontinuities of investment at score values other than the cut-off point. If the jump of the function is unique, at the point that divides subsidized from unsubsidized firms, the evidence in favor of the causality effect of the program becomes more persuasive. We implement the following test suggested by Lee and Lemieux (2009). We estimate the baseline model (2) adding a complete set of score dummies variable interacted with the small dummy. Then we test the null hypothesis that the coefficients of these dummies are jointly not statistically different from zero. If we accept the null, we can conclude that there are no other jumps of the investment: the only one is at the threshold. Table 12 (section 2) reports the values of the F-test of this exercise. From the table it is evident that no other discontinuities are detected.

7. Conclusions

This paper contributes to the existing literature on the effects of incentives for firms' R&D investment. We evaluated the impact of a place-based program implemented recently in a region of northern Italy. Using a sharp regression discontinuity strategy we find that overall grants did not have a positive effect on firms' R&D outlays. However, when we differentiate firms by size, we find that for small firms the grants triggered substantial additional investment, while for large ones they did not. The change in investment of small firms has been on average equal to the subsidy received. Overall, our results are similar to the conclusions reached by Lach (2002) and Gonzalez et al. (2005). We argue that it could be the lower capability of smaller firms to raise funds on the capital market that can explain our findings. In our sample, several financial indexes show how smaller enterprises may have had more restricted access to the credit market than larger ones.

We analyzed the direct effects of the policy on the main target variables. Of course, there are further interesting issues that we did not address but that deserve attention. A first one is the long-term effect of the grants in terms of the economic performance of recipient firms. Other important issues are the indirect effects of the program. Among them, the presence of spillovers is one of the most significant. An increase in R&D investment might produce positive spillovers across firms that, in terms of social welfare, could even offset the cost of having unsuccessfully financed larger enterprises. For regional programs an interesting question is also to know whether spillovers are localized. To understand these effects would be highly rewarding, albeit empirically challenging.

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Tables and figures

Figure 1

INVESTMENT COSTS AND RETURNS IN AN IMPERFECT CAPITAL MARKET

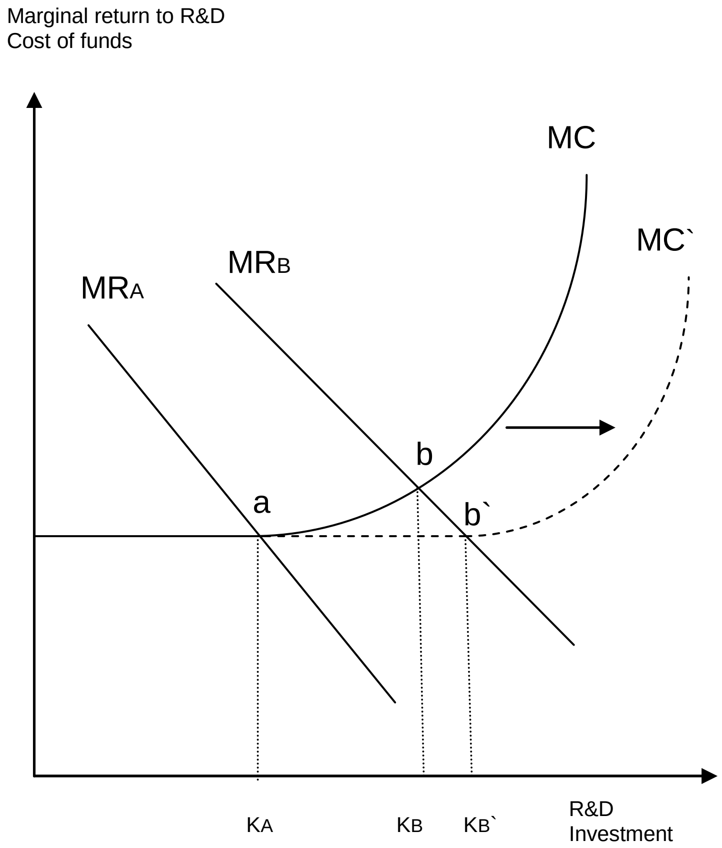


Figure 2

FIRMS' DENSITY DISTRIBUTION BY SCORE

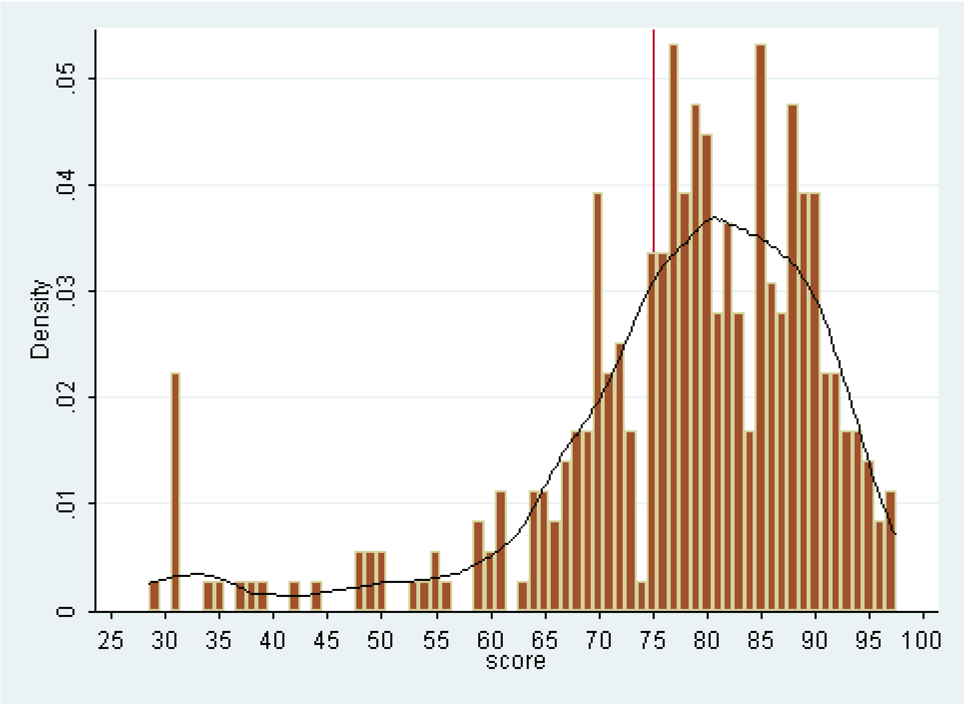
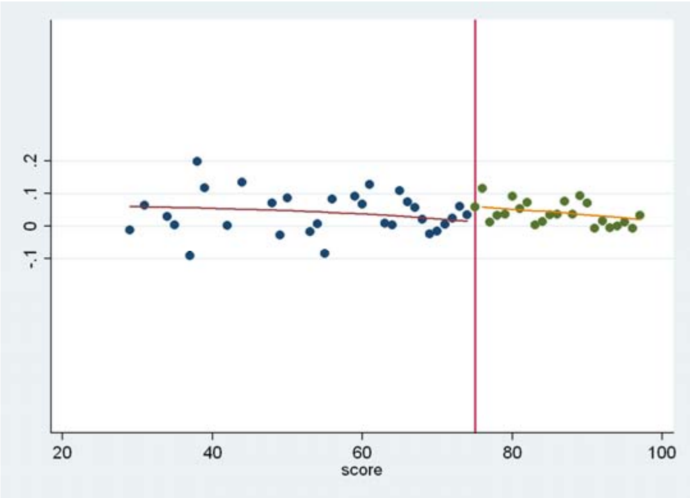
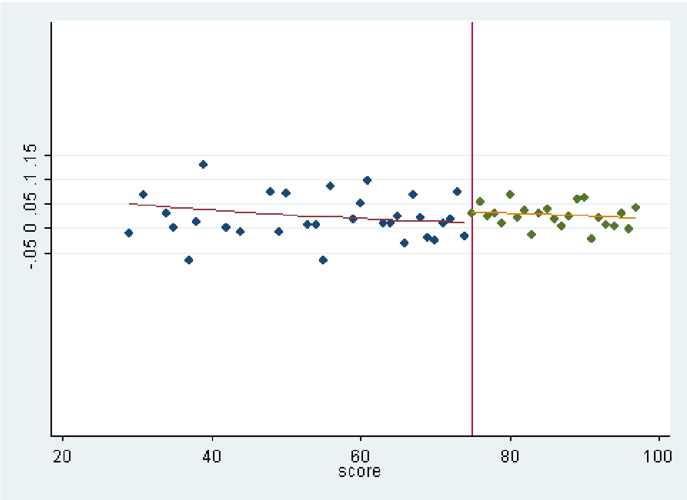


Figure 3

TOTAL INVESTMENT/SALES



TANGIBLE INVESTMENT/SALES



INTANGIBLE INVESTMENT/SALES

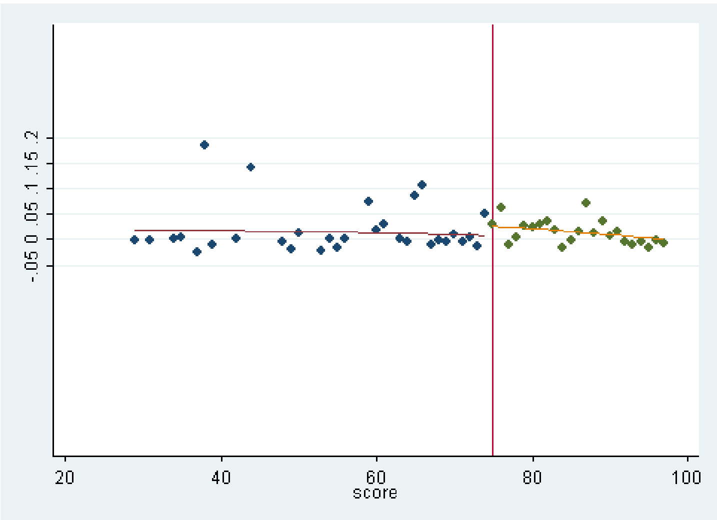
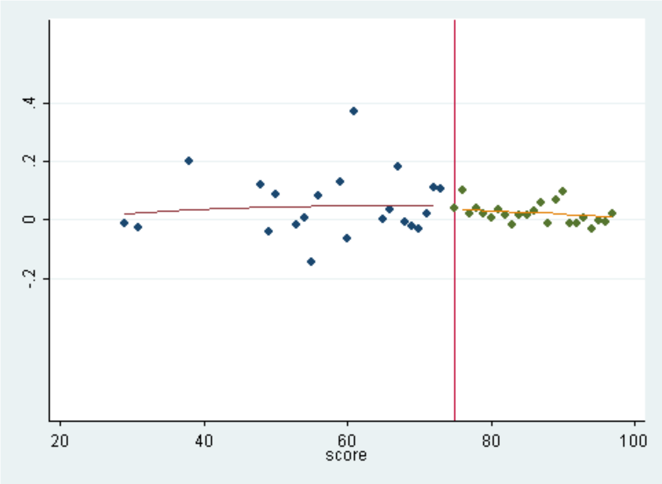


Figure 4

LARGE FIRMS

Total investment/Sales



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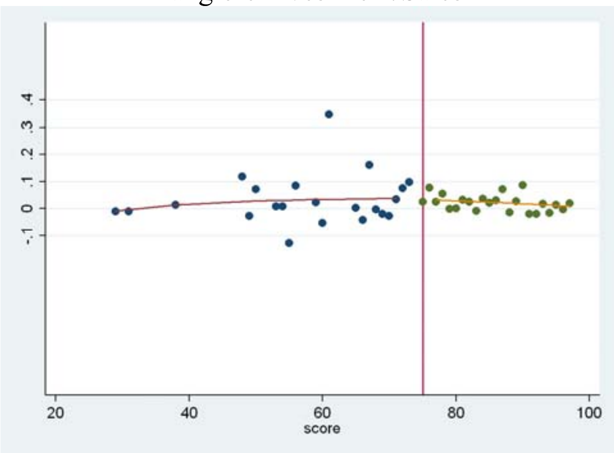


Intangible investment/Sales

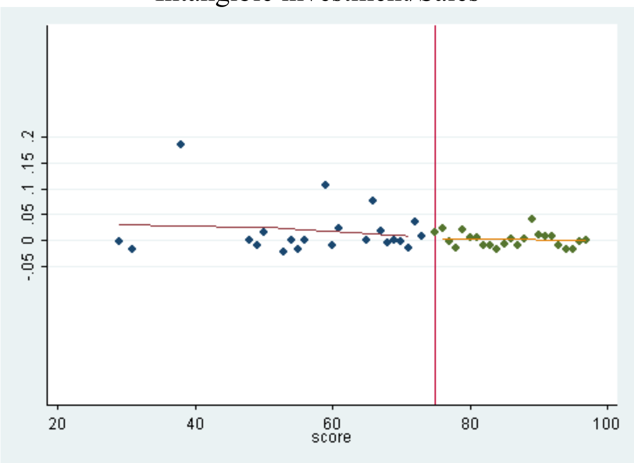
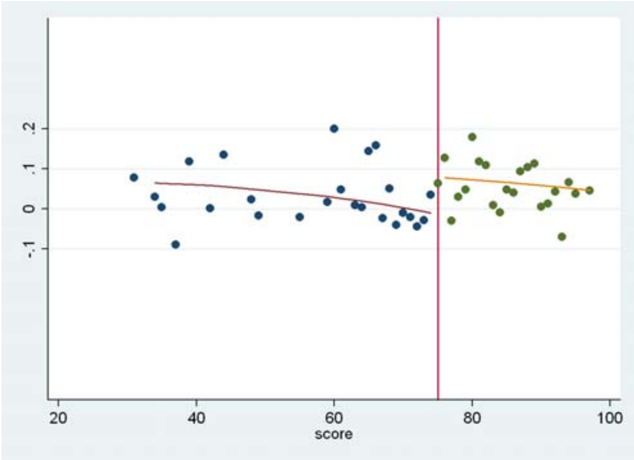


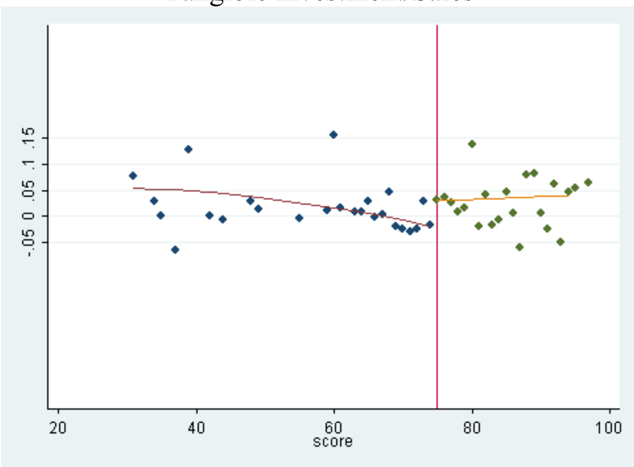
Figure 5

SMALL FIRMS

Total investment/Sales



Tangible investment/Sales



Intangible investment/Sales

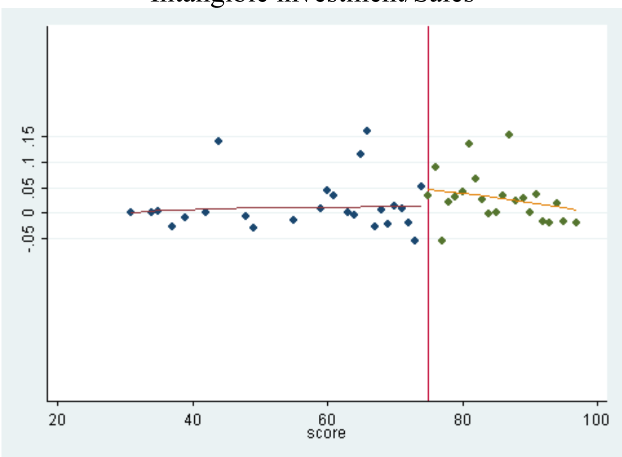
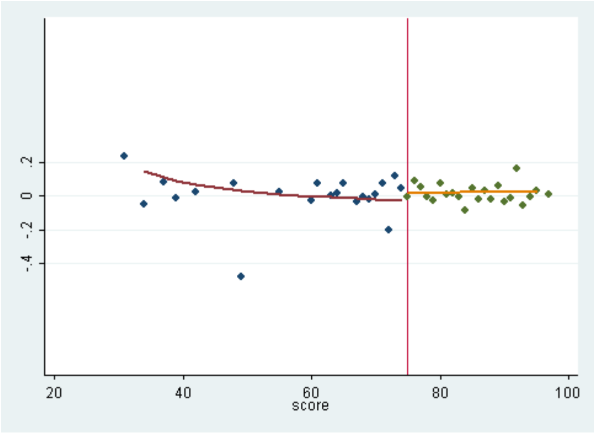


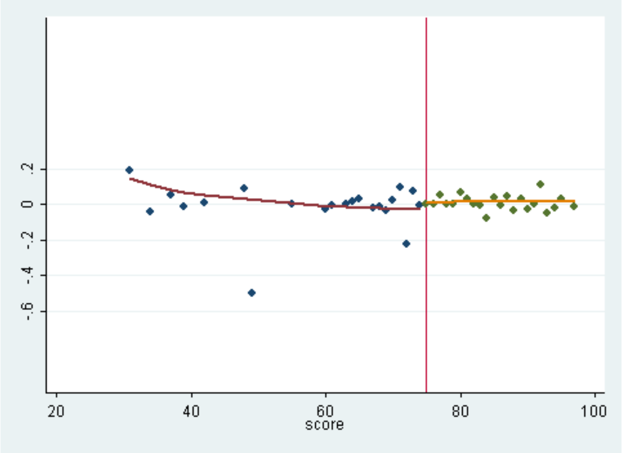
Figure 6

PRE-PROGRAM INVESTMENT – SMALL FIRMS

Total investment/Sales



Tangible investment/Sales



Intangible investment/Sales

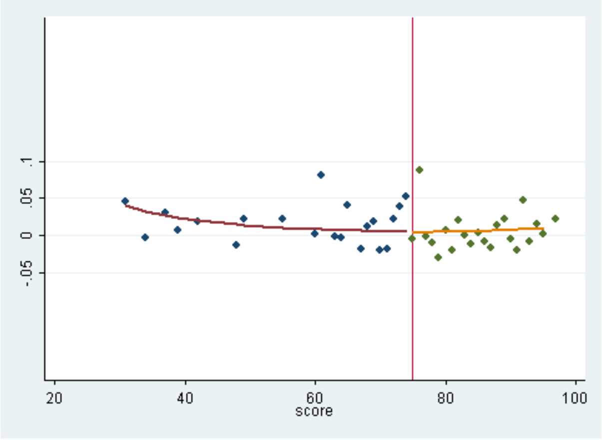


Table 1

DISTRIBUTION OF FIRMS BY SECTOR

Sector	Number of firms		Percentages	
	Treated	Untreated	Treated	Untreated
Food, beverages and tobacco	18	5	7.1	6.4
Textiles, wearing apparel, leather products	4	3	1.6	2.0
Paper, printing and publishing	3	1	1.2	1.1
Chemical products	28	9	11.0	10.4
Non-metallic mineral products	10	4	3.9	3.9
Basic metal industries	20	12	7.9	9.0
Machinery and equipment	146	58	57.5	57.1
Transport equipment	16	3	6.3	5.3
Other manufacturing industries, wood and wood furniture	4	6	1.6	2.8
Construction	5	2	2.0	2.0
Total industrial firms	254	103	100.0	100.0

Table 2

PRE-ASSIGNMENT MEANS (STANDARD DEVIATION) AND MEAN-DIFFERENCES (STANDARD ERRORS)

Variable	All			50% cut-off neighborhood sample (score 52-80)			35% cut-off neighborhood sample (score 66-78)		
	Untreated	Treated	Mean Diff.	Untreated	Treated	Mean Diff.	Untreated	Treated	Mean Diff.
Sales	21269	65963	44694**	23023	27013	4116	22356	30535	8179
	(37035)	(205961)	(20442)	(39068)	(57067)	(7561)	(38963)	(66293)	(10119)
Value added	5534	15605	10070**	5980	7308	1328	6165	8054	1888
	(9435)	(47530)	(4724)	(10108)	(15833)	(2057)	(10196)	(18492)	(2778)
Assets	20510	59664	39153**	21033	26726	5692	21848	29640	7792
	(39202)	(176488)	(17576)	(35458)	(61530)	(7792)	(36427)	(70305)	(10415)
ROA	6.38	7.27	0.889	6.25	6.75	0.504	4.92	6.34	1.415
	(9.87)	(10.18)	(1.179)	(10.41)	(8.12)	(1.421)	(8.85)	(5.10)	(1.351)
Own capital/Debts	0.530	0.467	-0.054	0.586	0.374	-0.212*	0.613	0.380	-0.232
	(0.911)	(0.604)	(0.082)	(0.994)	(0.428)	(0.115)	(1.081)	(0.394)	(0.152)
Gross operating margin/Sales	0.084	0.095	0.011	0.087	0.088	0.001	0.085	0.082	-0.003
	(0.096)	(0.077)	(0.009)	(0.101)	(0.069)	(0.013)	(0.092)	(0.051)	(0.013)
Cash flow/Sales	0.059	0.078	0.019**	0.062	0.072	0.010	0.059	0.072	0.012
	(0.077)	(0.076)	(0.008)	(0.081)	(0.062)	(0.011)	(0.083)	(0.055)	(0.013)
Financial costs/Debts	0.029	0.024	-0.005	0.031	0.025	-0.006	0.032	0.026	-0.007
	(0.065)	(0.016)	(0.004)	(0.073)	(0.017)	(0.008)	(0.086)	(0.019)	(0.011)
Labor costs/Sales	0.208	0.199	-0.009	0.208	0.211	0.003	0.222	0.205	-0.016
	(0.101)	(0.087)	(0.010)	(0.104)	(0.088)	(0.014)	(0.109)	(0.095)	(0.019)
Service costs/Sales	0.287	0.275	-0.012	0.273	0.288	0.015	0.264	0.292	0.027
	(0.133)	(0.116)	(0.014)	(0.121)	(0.107)	(0.017)	(0.116)	(0.108)	(0.021)
Total investment/ Sales	0.004	0.008	0.003	-0.002	0.007	0.009	-0.006	0.018	0.024
	(0.107)	(0.070)	(0.009)	(0.117)	(0.076)	(0.015)	(0.126)	(0.075)	(0.019)
Tangible investment /Sales	-0.009	0.004	0.013	-0.015	0.004	0.020	-0.020	0.013	0.033
	(0.117)	(0.050)	(0.008)	(0.128)	(0.062)	(0.016)	(0.144)	(0.058)	(0.020)
Intangible investment /Sales	0.013	0.004	-0.010	0.014	0.002	-0.011	0.014	0.005	-0.009
	(0.064)	(0.046)	(0.006)	(0.074)	(0.034)	(0.008)	(0.084)	(0.039)	(0.012)

Notes: only manufacturing and construction firms. All the variables refer to the first pre-assignment year (2003 for the first auction and 2004 for the second). In the complete sample 254 firms are treated; 103 are untreated. In the 50% cut-off neighborhood sample treated firms are 90, untreated 81; in the 35% cut-off neighborhood sample treated firms are 57, untreated 58. Investments are calculated as the difference between the capital stock in two consecutive years.

*, **,***: significant at 10%, 5% and 1% respectively.

Table 3

BASELINE RESULTS: EFFECT OF THE PROGRAM ON INVESTMENT

Order of polynomial	Total investment/ Sales	Tangible investment/ Sales	Intangible investment/ Sales
		A. Full sample	
0	0.012	0.008	0.004
	(0.013)	(0.010)	(0.007)
	[-599.1]	*[-710.7]*	*[-979.5]*
1	0.040*	0.024	0.015
	(0.020)	(0.015)	(0.012)
	[-598.8]	*[-708.6]*	*[-978.5]*
2	0.045	0.021	0.024
	(0.030)	(0.022)	(0.018)
	[-595.9]	*[-704.6]*	*[-978.0]*
3	0.064	0.025	0.039
	(0.041)	(0.034)	(0.024)
	[-592.8]	*[-700.7]*	*[-975.5]*
		B. Local estimates: Wide-window sample	
0	0.026	0.019	0.007
	(0.019)	(0.013)	(0.011)
	[-277.1]	*[-353.7]*	*[-463.3]*
1	0.041	0.016	0.024
	(0.034)	(0.022)	(0.020)
	[-273.8]	*[-350.0]*	*[-460.8]*
2	0.110*	0.0367	0.073***
	(0.051)	(0.039)	(0.024)
	[-274.7]	*[-347.5]*	*[-462.6]*
		C. Local estimates: Narrow-window sample	
0	0.033	0.022	0.010
	(0.022)	(0.014)	(0.016)
	[-200.3]	*[-266.8]*	*[-305.6]*
1	0.068	0.009	0.058*
	(0.040)	(0.034)	(0.027)
	[-198.8]	*[-263.5]*	*[-307.1]*
2	-0.079*	-0.078	-0.000
	(0.035)	(0.062)	(0.042)
	[-199.8]	*[-262.6]*	*[-305.2]*
Mean (st. dev.) for untreated firms - Full sample	0.033 (0.107)	0.021 (0.084)	0.012 (0.057)

Notes: The table shows the estimates of the coefficient β of model (1) for industrial firms. Investments are accumulated over the first 3 years after the assignment (including that of the assignment); sales refer to the pre-assignment year. The polynomial of order 0 is the difference in mean between treated and untreated. All the samples have been trimmed according to the 5th and 95th percentile of the distribution of the Total investment/Sales ratio (calculated over the full sample). Robust standard errors clustered by score in round brackets. Akaike Information Criterion in squared brackets. Number of observations: 357 in Panel A; 171 in Panel B; 115 in Panel C.
*, **,***: significant at 10%, 5% and 1% respectively.

Table 4

BASELINE RESULTS: EFFECT OF THE PROGRAM ON OTHER OUTCOME VARIABLES

Order of polynomial	Total investment/ Capital	Total investment/ Assets	Labor costs/ Sales	Service costs/ Sales
		A. Full sample		
0	0.192	0.019	-0.051	-0.091
	(0.135)	(0.014)	(0.052)	(0.055)
	[1199.5]	*[-518.9]*	*[244.2]*	*[546.2]*
1	0.470	0.044**	-0.055	0.032
	(0.236)	(0.020)	(0.076)	(0.086)
	[1197.6]	*[-517.3]*	*[247.4]*	*[547.1]*
2	0.658**	0.062**	-0.15	-0.008
	(0.314)	(0.029)	(0.104)	(0.126)
	[1200.6]	*[-516.6]*	*[248.9]*	*[550.7]*
3	1.083***	0.101**	-0.398**	-0.079
	(0.341)	(0.039)	(0.175)	(0.171)
	[1202.9]	*[-514.3]*	*[246.5]*	*[554.3]*
		B. Local estimates: Wide-window sample		
0	0.429*	0.032	-0.005	-0.007
	(0.215)	(0.020)	(0.071)	(0.076)
	[640.4]	*[-233.6]*	*[131.9]*	*[266.8]*
1	0.562	0.049	-0.302***	-0.077
	(0.412)	(0.033)	(0.097)	(0.147)
	[644.1]	*[-231.4]*	*[121.8]*	*[270.2]*
2	1.504***	0.153***	-0.147	0.197
	(0.318)	(0.045)	(0.135)	(0.172)
	[644.4]	*[-234.8]*	*[122.8]*	*[271.8]*
		C. Local estimates: Narrow window sample		
0	0.335	0.035	-0.093	-0.025
	(0.272)	(0.021)	(0.065)	(0.106)
	[428.1]	*[-193.5]*	*[90.9]*	*[198.9]*
1	1.288***	0.104**	-0.275*	0.064
	(0378)	(0.035)	(0.142)	(0.167)
	[428.7]	*[-193.4]*	*[93.0]*	*[202.5]*
2	1.329**	-0.049	0.172	0.166
	(0.535)	(0.030)	(0.119)	(0.216)
	[430.9]	*[-195.7]*	*[92.7]*	*[206.3]*
Mean (st. dev.) for untreated firms - Full sample	0.354 (1.124)	0.033 (0.114)	0.251 (0.156)	0.355 (0.209)

Notes: The table shows the estimates of the coefficient β of model (1) using different outcome variables. Investments are accumulated over the first 3 years after the assignment (including that of the assignment); sales and assets refer to the pre-assignment year. The polynomial of order 0 is the difference in mean between treated and untreated. All the samples have been trimmed according to the 5th and 95th percentile of the distribution of the Total investment/Sales ratio (calculated over the whole sample). Robust standard errors clustered by score in round brackets. Akaike Information Criterion in squared brackets. Number of observations: 357 in Panel A; 171 in Panel B; 115 in Panel C.
*, **,***: significant at 10%, 5% and 1% respectively.

Table 5

EFFECT OF THE PROGRAM ON INVESTMENT BY FIRMS' SIZE

Order of polynomial	Model (2)									Model (3)		
	Total investment/Sales			Tangible investment/Sales			Intangible investment/Sales			Total investment		
	Small	Large	*AIC*	Small	Large	*AIC*	Small	Large	*AIC*	Small	Large	*t-test of* $\beta_{small}=1$
	A. Full sample											
0	0.045**	-0.021	*-607.2*	0.022	-0.009	*-709.2*	0.022*	-0.012	*-992.4*	0.972*	0.442	*0.05*
	(0.018)	(0.020)		(0.015)	(0.017)		(0.011)	(0.008)		(0.518)	(9.973)	
1	0.080***	-0.012	*-603.6*	0.045**	-0.008	*-706.9*	0.035**	-0.003	*-988.1*	1.720**	-6.811	*1.05*
	(0.026)	(0.028)		(0.017)	(0.023)		(0.017)	(0.011)		(0.687)	(13.154)	
2	0.099***	-0.010	*-597.2*	0.053***	-0.010	*-699.2*	0.045*	-0.000	*-985.1*	1.108	-5.575	*0.14*
	(0.029)	(0.041)		(0.019)	(0.033)		(0.023)	(0.015)		(0.785)	(16.569)	
3	0.149***	-0.030	*-594.6*	0.081***	-0.031	*-694.5*	0.068**	0.001	*-979.4*	1.274	-3.395	*0.34*
	(0.037)	(0.063)		(0.024)	(0.051)		(0.031)	(0.032)		(0.804)	(21.208)	
	B. Local estimates: Wide-window sample											
0	0.064**	-0.014	*-279.5*	0.042*	-0.006	*-353.6*	0.022	-0.007	*-465.0*	1.826**	3.487	*1.02*
	(0.028)	(0.023)		(0.021)	(0.020)		(0.015)	(0.009)		(0.808)	(7.458)	
1	0.089**	0.008	*-277.2*	0.041*	0.011	*-352.9*	0.048*	-0.003	*-459.0*	1.810*	13.671	*0.92*
	(0.033)	(0.040)		(0.021)	(0.030)		(0.025)	(0.015)		(0.883)	(14.494)	
2	0.178***	0.031	*-279.1*	0.084***	-0.011	*-353.9*	0.093**	0.041	*-457.1*	2.615	18.364	*0.99*
	(0.052)	(0.080)		(0.027)	(0.063)		(0.039)	(0.030)		(1.639)	(20.013)	
	C. Local estimates: Narrow-window sample											
0	0.066**	-0.002	*-200.5*	0.035***	0.007	-268.2	0.031	-0.009	*-306.2*	1.369*	8.492	*0.56*
	(0.025)	(0.028)		(0.011)	(0.023)		(0.020)	(0.010)		(0.656)	(8.084)	
1	0.142***	-0.013	*-198.8*	0.066**	-0.045	-266.2	0.076*	0.032	*-304.1*	2.289*	22.324	*1.16*
	(0.043)	(0.061)		(0.021)	(0.048)		(0.041)	(0.025)		(1.110)	(21.309)	
2	0.053	-0.228**	*-201.9*	0.015	-0.163**	-266.4	0.037	-0.064	*-303.6*	-1.331	19.860	*1.32*
	(0.046)	(0.080)		(0.037)	(0.070)		(0.070)	(0.038)		(1.761)	(27.049)	
Mean (st. dev.) for untreated firms - Full sample	*0.022* *(0.104)*	*0.047* *(0.112)*		*0.012* *(0.076)*	*0.033* *(0.094)*		*0.010* *(0.058)*	*0.014* *(0.056)*				

Notes: The table shows the estimates of the coefficients β_k of model (2) and model (3). AIC is the Akaike Information Criterion. Small [Large] firms are those falling in the first [second] half of the distribution of the value added. See, also, the notes to Table 3.
*, **,***: significant at 10%, 5% and 1% respectively.

Table 6

EFFECT OF THE PROGRAM ON DIFFERENT OUTCOME VARIABLES BY FIRMS' SIZE

Order of polynomial	Labor costs/Sales			Service costs/Sales		
	Small	Large	*AIC*	Small	Large	*AIC*
			A. Full sample			
0	-0.001 (0.064)	-0.093 (0.086)	*242.4*	-0.069 (0.085)	-0.057 (0.089)	*527.9*
1	-0.068 (0.095)	-0.041 (0.138)	*248.5*	0.026 (0.137)	0.031 (0.136)	*533.5*
2	-0.069 (0.118)	-0.241 (0.171)	*249.9*	0.076 (0.181)	-0.079 (0.188)	*540.4*
3	-0.247 (0.156)	-0.625* (0.348)	*241.8*	0.220 (0.185)	-0.604* (0.313)	*541.1*
			B. Local estimates: Wide-window sample			
0	0.004 (0.096)	-0.010 (0.097)	*134.2*	-0.013 (0.116)	0.018 (0.091)	*256.6*
1	-0.262** (0.115)	-0.290* (0.155)	*125.8*	0.062 (0.195)	-0.201 (0.167)	*262.5*
2	-0.049 (0.149)	-0.206 (0.256)	*127.3*	0.246 (0.200)	-0.155 (0.275)	*267.2*
			C. Local estimates: Narrow- window sample			
0	-0.066 (0.102)	-0.121 (0.110)	*94.6*	0.021 (0.166)	-0.057 (0.109)	*194.1*
1	-0.215 (0.135)	-0.238 (0.245)	*96.2*	0.256 (0.288)	-0.179 (0.257)	*198.5*
2	0.340** (0.122)	-0.009 (0.354)	*93.4*	0.209 (0.342)	-0.226 (0.316)	*191.5*

Notes: The Table shows the estimates of the coefficient β_k of model (2) using labor and services costs scaled by the pre-assignment sales. Costs are accumulated over the first 3 years after the assignment (included that of the assignment). Robust standard errors clustered by score in round brackets. AIC: Akaike Information Criterion (AIC) Small [Large] firms are those falling in the first [second] half of the distribution of the value added. See, also, the notes to Table 5.
*, **,***: significant at 10%, 5%, 1% respectively.

Table 7

FIRMS' FINANCIAL INDEXES

Variable	Mean (st. dev.) Small	Mean (st. dev.) Large	Mean difference (st. errors)
Own capital/Debts	0.448 (0.783)	0.527 (0.499)	-0.079 (0.069)
Cash flow/Sales	0.050 (0.087)	0.077 (0.046)	-0.027*** (0.007)
ROA	5.269 (9.288)	7.797 (7.438)	-2.528*** (0.891)
Financial cost/Debts	0.022 (0.014)	0.020 (0.013)	0.002 (0.001)

*, **,***: significant at 10%, 5% and 1% respectively.

Table 8

RESULTS FOR SERVICES:
EFFECT OF THE PROGRAM ON TOTAL INVESTMENT/SALES

Order of polynomial	Model (1)		Model (2)		
	β	AIC	β - Small	β - Large	AIC
			A. Full sample		
0	0.032 (0.025)	*-86.5*	0.068* (0.036)	0.000 (0.036)	*-85.2*
1	-0.016 (0.036)	*-85.2*	0.048 (0.046)	-0.114 (0.032)	*-83.1*
2	0.036 (0.050)	*-82.9*	0.139*** (0.044)	-0.085 (0.054)	*-77.6*
3	0.034 (0.091)	*-80.6*	0.191* (0.099)	-0.165 (0.122)	*-72.5*
			B. Local estimates: Wide-window sample		
0	0.030 (0.032)	*-66.4*	0.074* (0.042)	-0.055* (0.031)	*-67.5*
1	-0.035 (0.040)	*-64.9*	0.052 (0.047)	-0.126*** (0.031)	*-62.8*
2	0.057 (0.074)	*-63.7*	0.224** (0.090)	-0.083 (0.087)	*-60.6*
Mean (st. dev.) for untreated firms - Full sample	0.030 (0.143)		0.029 (0.158)	0.031 (0.127)	

Notes: The table shows the estimates of the coefficient β of model (1) and (2) on service firms. See the notes to Tables 3 and 5. Number of observations: 111 in Panel A; 67 in Panel B.
*, **,***: significant at 10%, 5% and 1% respectively.

Table 9

EFFECT OF THE PROGRAM ON INVESTMENT BY FIRMS' COVERAGE RATIO

Order of polynomial	Total investment/ Sales			Tangible investment/ Sales			Intangible investment/ Sales		
	Low	High	*AIC*	Low	High	*AIC*	Low	High	*AIC*
				A. Full sample					
0	0.012 (0.014)	0.012 (0.013)	*-597.1*	0.013 (0.012)	0.003 (0.011)	*-709.6*	-0.001 (0.007)	0.009 (0.009)	*-979.4*
1	0.046** (0.021)	0.033 (0.022)	*-595.4*	0.033* (0.017)	0.013 (0.014)	*-705.6*	0.013 (0.010)	0.019 (0.018)	*-977.2*
2	0.061** (0.028)	0.029 (0.033)	*-591.3*	0.038 (0.024)	0.003 (0.022)	*-700.3*	0.022* (0.012)	0.026 (0.026)	*-974.9*
				B. Local estimates: Wide-window sample					
0	0.036 (0.021)	0.015 (0.019)	*-275.9*	0.031* (0.016)	0.006 (0.011)	*-353.6*	0.005 (0.009)	0.009 (0.016)	*-461.4*
1	0.042 (0.034)	0.038 (0.037)	*-270.9*	0.029 (0.026)	0.002 (0.020)	*-347.9*	0.013 (0.014)	0.035 (0.030)	*-457.7*
2	0.139** (0.050)	0.090* (0.050)	*-272.9*	0.074* (0.041)	0.010 (0.037)	*-346.9*	0.064*** (0.021)	0.080** (0.031)	*-459.1*
				C. Local estimates: Narrow-window sample					
0	0.043** (0.019)	0.021 (0.026)	*-199.0*	0.034** (0.015)	0.008 (0.013)	*-266.5*	0.009 (0.010)	0.012 (0.024)	*-303.6*
1	0.074** (0.033)	0.059 (0.047)	*-197.6*	0.031 (0.033)	-0.011 (0.033)	*-263.5*	0.043* (0.022)	0.070* (0.038)	*-306.1*
2	-0.053 (0.036)	-0.092** (0.036)	*-201.4*	-0.043 (0.063)	-0.099 (0.063)	*-264.9*	-0.009 (0.042)	0.006 (0.045)	*-306.8*

Notes: The table shows the estimates of coefficients β_j of model (4). Coverage ratio = Grant/Planned investment. High (Low) identifies firms that are above (below) the median of the distribution of the coverage ratio. See, also, the notes to Tables 3 and 5.

Table 10

ROBUSTNESS I: ESTIMATIONS WITH COVARIATES

Dependent variable: Total investment/Sales

Order of polynomial	Model (1) + covariates		Model (2) + covariates		
	β	AIC	β - Small	β - Large	AIC
			A. Full sample		
0	0.015 (0.012)	*-585.9*	0.041** (0.016)	-0.015 (0.018)	*-589.54*
1	0.036* (0.019)	*-584.2*	0.071*** (0.026)	-0.009 (0.025)	*-584.4*
2	0.038 (0.029)	*-581.9*	0.090*** (0.031)	-0.016 (0.038)	*-578.9*
3	0.064 (0.040)	*-579.2*	0.142*** (0.043)	-0.024 (0.061)	*-575.9*
			B. Local estimates: Wide-window sample		
0	0.021 (0.018)	*-267.1*	0.050* (0.025)	-0.013 (0.022)	-266.8
1	0.034 (0.037)	*-263.4*	0.084** (0.039)	-0.008 (0.004)	-264.1
2	0.101* (0.053)	*-263.8*	0.165*** (0.057)	0.042 (0.081)	-265.5
			C. Local estimates: Narrow-window sample		
0	0.035 (0.022)	*-189.1*	0.064** (0.028)	0.001 (0.026)	-193.2
1	0.062 (0.044)	*-190.1*	0.143** (0.059)	-0.011 (0.062)	-196.9
2	-0.066 (0.040)	*-193.8*	0.038 (0.049)	-0.186* (0.093)	-202.9

Notes: The table shows the estimates of the coefficient β of model (1) and (2) on industrial firms including as covariates 2-digit sector dummies, gross operative margin/value added, own capital/debts, ROA, cash flow/sales, total assets, financial costs/debts all referred to the pre-treatment period. See, also, the notes to tables 3 and 5.
*, **,***: significant at 10%, 5% and 1% respectively.

ROBUSTNESS II: DISCONTINUITY OF COVARIATES

Order of polynomial	ROA		Net worth assets/Debts		Cash flow/Sales		Interest costs/Debts	
	Small	Large	Small	Large	Small	Large	Small	Large
				A. Full sample				
0	0.139	0.317	0.042	0.018	0.015	0.006	-0.001	-0.001
	(1.575)	(1.288)	(0.109)	(0.087)	(0.018)	(0.008)	(0.003)	(0.002)
1	-1.777	-0.515	-0.223	0.035	-0.030	-0.004	-0.000	0.000
	(2.329)	(1.581)	(0.149)	(0.133)	(0.021)	(0.009)	(0.005)	(0.003)
2	-1.967	1.191	-0.387*	-0.132	-0.048	0.001	0.001	0.007
	(2.502)	(2.122)	(0.197)	(0.196)	(0.032)	(0.001)	(0.008)	(0.006)
				B. Local estimates: Wide-window sample				
0	-2.325	-0.635	-0.161	-0.046	-0.013	-0.002	0.001	0.001
	(1.872)	(1.196)	(0.111)	(0.098)	(0.014)	(0.008)	(0.004)	(0.003)
1	-0.494	1.172	-0.237	0.108	-0.032	0.005	-0.006	0.013
	(2.456)	(2.098)	(0.196)	(0.205)	(0.025)	(0.011)	(0.008)	(0.009)
2	3.592	1.513	-0.265	0.902***	0.006	0.000	-0.004	0.027
	(4.446)	(4.495)	(0.386)	(0.240)	(0.032)	(0.028)	(0.012)	(0.016)
				C. Local estimates: Narrow-window sample				
0	-1.357	0.596	-0.132	-0.020	-0.021	0.005	-0.002	0.003
	(1.192)	(1.084)	(0.138)	(0.123)	(0.017)	(0.008)	(0.006)	(0.004)
1	1.405	-1.349	-0.358	0.555**	-0.002	-0.024	-0.010	0.021
	(4.656)	(3.804)	(0.346)	(0.225)	(0.028)	(0.018)	(0.013)	(0.016)
2	-8.457	11.978	-0.065	1.606***	0.016	0.032	0.007	0.023
	(5.410)	(3.701)	(0.467)	(0.382)	(0.064)	(0.023)	(0.023)	(0.013)

Notes: The table shows the estimates of the coefficients β_k of model (2) using different outcome variables. See, also, the notes to Tables 3 and 5.

Table 12

ROBUSTNESS III: FURTHER CHECKS

1. Tests for discontinuity in the pre-program period

Order of polynomial	Total investment/Sales		Intangible investment/Sales		Tangible investment/Sales	
	Small	Large	Small	Large	Small	Large
			A. Full sample			
0	0.003 (0.034)	0.010 (0.026)	0.012 (0.029)	0.003 (0.015)	-0.009 (0.011)	0.007 (0.017)
1	0.042 (0.040)	-0.32 (0.038)	0.041 (0.035)	-0.004 (0.021)	0.001 (0.019)	-0.028 (0.027)
2	0.002 (0.053)	-0.039 (0.052)	-0.011 (0.046)	-0.042 (0.030)	0.013 (0.026)	0.003 (0.031)
			B. Local estimates: Wide-window sample			
0	0.022 (0.034)	-0.011 (0.024)	0.028 (0.027)	-0.004 (0.017)	-0.006 (0.018)	-0.006 (0.011)
1	0.019 (0.058)	-0.011 (0.043)	-0.005 (0.047)	-0.008 (0.030)	0.025 (0.035)	-0.003 (0.019)
2	-0.006 (0.076)	0.011 (0.059)	-0.014 (0.060)	0.013 (0.043)	0.008 (0.036)	-0.002 (0.034)
			C. Local estimates: Narrow-window sample			
0	0.041 (0.042)	-0.008 (0.030)	0.026 (0.036)	-0.001 (0.023)	0.014 (0.024)	-0.006 (0.013)
1	-0.024 (0.096)	-0.022 (0.065)	-0.019 (0.083)	-0.017 (0.050)	-0.004 (0.039)	-0.005 (0.032)
2	-0.109* (0.059)	0.056 (0.059)	-0.075 (0.048)	0.042 (0.040)	-0.033 (0.020)	0.014 (0.025)

2. F-Tests for discontinuities at different cut-off points

Order of polynomial	Total investment/Sales	Total investment/Capital	Total investment/Assets
0	1.12 (0.28)	1.11 (0.30)	1.27 (0.12)
1	1.06 (0.37)	1.02 (0.44)	1.26 (0.14)
2	1.07 (0.36)	1.01 (0.45)	1.22 (0.17)

Notes: The first section of the table shows the estimates of the coefficients β_k of model (2) using investment of 2 years before the implementation of the program. Robust standard errors clustered by score in round brackets. The second section shows the F- tests for the null hypothesis that a full set of score dummies interacted with the small-firms dummy included in the model (2) are equal to zero. The full sample has been used. P-value in round brackets. See, also, the notes to Tables 3 and 5.

Appendix

Figure A1

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

Table A1

PAPERS ON R&D INCENTIVES PUBLISHED IN THE LAST DECADE (1)

Articles	Country	Outcome variable	Methodology	Results
Lerner (1999)	United States	Employment, sales	Matching	Positive effect
Wallsten (2000)	United States	Employment, investment	Instrumental variables	No effect
Busom (2000)	Spain	Employment, investment	Structural model	Positive effect
Branstetter and Sakakibara (2002)	Japan	Innovation activity	Matching	Positive effect
Lach (2002)	Israel	Investment	Diff-in-diff with controls	No effect
Almus and Czarnitzki (2003)	Eastern Germany	Investment	Matching	Positive effect
Hujer and Radic (2005)	Germany	Innovation activity	Matching	No effect
Gonzalez et al. (2005)	Spain	Investment	Instrumental variables	Positive effect
Gorg and Strobl (2007)	Ireland	Investment	Matching	Positive effect just for smaller grants
Merito et al. (2007)	Italy	Employment, sales, productivity	Matching	No effect
Hussinger (2008)	Germany	Investment	Two-step selection models	Positive effect

(1) The table reports the studies that examined the effect of firms' subsidies for R&D; those evaluating the impact of tax incentives are not included.

Table A2

DISTRIBUTION OF FIRMS BY SECTOR AND SIZE: PERCENTAGES

Sector	Small		Large	
	Treated	Untreated	Treated	Untreated
Food, beverages and tobacco	5.0	5.1	8.9	4.5
Textile, wearing and apparel, leather products	2.5	1.7	0.7	4.5
Paper, printing and publishing	2.5	1.7	0.0	0.0
Chemicals products	10.9	5.1	11.1	13.6
Non-metallic mineral products	1.7	5.1	5.9	2.3
Basic metal industries	9.2	10.2	6.7	13.6
Machinery and equipment	59.7	62.7	55.6	47.7
Transport equipment	3.4	3.4	8.9	2.3
Other manufacturing industries, wood and wood furniture	3.4	1.7	0.0	11.4
Construction	1.7	3.4	2.2	0.0
Total industrial firms	100.0	100.0	100.0	100.0

Table A3

PRE-ASSIGNMENT MEAN-DIFFERENCES BY FIRMS' SIZE

Standard errors in brackets

Variables	Small Firms			Large firms		
	All	50% cut off sample	35% cut off sample	All	50% cut off sample	35% cut off sample
Sales	1547 (967)	2534 (1675)	3364 (2516)	74782* (41275)	3015 (14429)	10904 (18833)
Value added	279** (140)	378* (194)	392 (258)	16672* (9522)	1612 (3952)	2801 (5192)
Assets	654 (634)	1382 (951)	1392 (1371)	65424* (35288)	7686 (15092)	12096 (19549)
ROA	2.85 (1.96)	3.16 (2.18)	3.30 (2.14)	-1.36 (1.23)	-2.52 (1.72)	-0.59 (1.60)
Own capital/Debts	-0.017 (0.088)	-0.176* (0.104)	-0.137 (0.120)	-0.136 (0.143)	-0.268 (0.212)	-0.341 (0.281)
Gross operating margin/Sales	0.024 (0.015)	0.021 (0.019)	0.005 (0.022)	-0.005 (0.012)	-0.020 (0.017)	-0.012 (0.017)
Cash flow/Sales	0.025** (0.011)	0.023 (0.017)	0.022 (0.023)	0.008 (0.013)	-0.006 (0.013)	0.002 (0.012)
Financial costs/Debts	0.001 (0.002)	0.001 (0.003)	0.000 (0.003)	-0.014 (0.009)	-0.014 (0.016)	-0.014 (0.023)
Labor costs/Sales	-0.005 (0.015)	-0.012 (0.022)	-0.031 (0.030)	-0.008 (0.014)	0.021 (0.019)	-0.001 (0.023)
Service costs/Sales	-0.025 (0.020)	-0.007 (0.026)	0.007 (0.032)	0.0165 (0.018)	0.045** (0.019)	0.051** (0.024)
Total investment/ Sales	0.007 (0.014)	0.027 (0.025)	0.053 (0.034)	-0.004 (0.012)	-0.013 (0.015)	-0.007 (0.017)
Tangible investment/Sales	0.017 (0.013)	0.035 (0.022)	0.051 (0.032)	0.006 (0.012)	0.003 (0.020)	0.014 (0.025)
Intangible investment/Sales	-0.011** (0.005)	-0.008 (0.008)	0.003 (0.009)	-0.010 (0.012)	-0.016 (0.017)	-0.021 (0.022)
Number of firms	178	90	58	179	81	57

*, **,***: significant at 10%, 5%, 1% respectively.

Table A4

OUTCOME VARIABLE: LOG (EMPLOYMENT)

Order of polynomial	Baseline model (1)			Kernel regressions (2)		
	All firms	Small firms	Large firms	All firms	Small firms	Large firms
	A1. Full sample			B1. Bandwidth=30		
0	0.284*	0.226*	0.237*	0.253*	0.208*	0.243*
	(0.154)	(0.116)	(0.129)	(0.146)	(0.113)	(0.132)
1	-0.096	0.278	-0.011	-0.054	0.298	-0.021
	(0.228)	(0.167)	(0.182)	(0.264)	(0.394)	(0.214)
2	0.528*	0.808***	-0.158	0.523*	0.793	-0.165
	(0.277)	(0.212)	(0.265)	(0.303)	(0.765)	(0.332)
3	0.377	0.326	-0.152	0.344	0.319	-0.138
	(0.339)	(0.337)	(0.446)	(0.450)	(1.199)	(0.619)
	A2. Local estimates: Wide-window sample			B2. Bandwidth=15		
0	0.077	0.331**	-0.014	0.213	0.191*	0.245*
	(0.191)	(0.143)	(0.136)	(0.135)	(0.109)	(0.129)
1	0.407	0.725***	0.068	0.186	0.505	-0.058
	(0.318)	(0.184)	(0.349)	(0.251)	(0.393)	(0.220)
2	0.312	0.415	0.318	0.325	0.472	-0.267
	(0.315)	(0.3289	(0.570)	(0.411)	(0.760)	(0.478)

Notes: The Table reports the differences of the outcome variable between recipient and non-recipient firms estimated at the cut-off score (score=75). Employment is accumulated over the first 3 years after the assignment (including that of the assignment). Polynomial of order 0 is the difference in mean between treated and untreated. Small (large) firms are those with value added below (above) the median.

(1) In panel A1 observations are 263; in panel A2 they are 118.

(2) We estimated the model using the Epanechnikov kernel combined with two bandwidths (± 30 and ± 15 points around the cut-off) and various polynomials. In panel B1 observations are 263; in panel B2 firms are 271. Bootstrapped standard errors (100 replications) clustered by score in round brackets.

*, **,***: significant at 10%, 5%, 1% respectively.

Table A5

RESULTS OF KERNEL REGRESSIONS

Order of polynomial	All firms					Small firms					Large firms				
	Total investment	Tangible investment	Intangible investment	Labor costs	Service costs	Total investment	Tangible investment	Intangible investment	Labor costs	Service costs	Total investment	Tangible investment	Intangible investment	Labor costs	Service costs
								A. Bandwidth=30							
0	0.014	0.009	0.004	-0.051	-0.077	0.048***	0.026**	0.022**	-0.008	-0.061	-0.021	-0.010	-0.012	-0.086	-0.045
	(0.014)	(0.012)	(0.008)	(0.057)	(0.053)	(0.016)	(0.013)	(0.011)	(0.056)	(0.102)	(0.018)	(0.021)	(0.008)	(0.091)	(0.088)
1	0.041*	0.024	0.017	-0.059	0.029	0.081***	0.045**	0.035*	-0.067	0.026	-0.011	-0.007	-0.003	-0.058	0.025
	(0.022)	(0.016)	(0.013)	(0.089)	(0.089)	(0.030)	(0.022)	(0.021)	(0.095)	(0.150)	(0.031)	(0.027)	(0.012)	(0.153)	(0.151)
2	0.047	0.022	0.024	-0.173	-0.015	0.103***	0.057***	0.046	-0.082	0.092	-0.013	-0.011	-0.001	-0.274	-0.116
	(0.031)	(0.023)	(0.018)	(0.0143)	(0.149)	(0.042)	(0.019)	(0.030)	(0.175)	(0.261)	(0.047)	(0.032)	(0.017)	(0.234)	(0.231)
3	0.066	0.024	0.042	-0.375	-0.056	0.148	0.079	0.069	-0.236	0.211	-0.026	-0.033	0.007	-0.571*	-0.540
	(0.051)	(0.051)	(0.029)	(0.229)	(0.224)	(0.116)	(0.107)	(0.064)	(0.178)	(0.457)	(0.092)	(0.063)	(0.039)	(0.327)	(0.374)
								B. Bandwidth=15							
0	0.018	0.013	0.005	-0.051	-0.049	0.057***	0.034**	0.022*	-0.023	-0.047	-0.020	-0.109	-0.009	-0.070	-0.013
	(0.014)	(0.012)	(0.008)	(0.063)	(0.053)	(0.017)	(0.012)	(0.012)	(0.065)	(0.118)	(0.018)	(0.022)	(0.008)	(0.089)	(0.091)
1	0.047*	0.024	0.023*	-0.142	-0.009	0.102***	0.054**	0.048**	-0.087	0.081	-0.012	-0.007	-0.004	-0.192	-0.066
	(0.025)	(0.019)	(0.013)	(0.091)	(0.096)	(0.030)	(0.024)	(0.021)	(0.092)	(0.161)	(0.036)	(0.028)	(0.012)	(0.169)	(0.174)
2	0.058	0.020	0.038	-0.256	-0.010	0.135***	0.075***	0.060	-0.136	0.149	0.026	-0.039	0.013	-0.419	-0.284
	(0.043)	(0.032)	(0.023)	(0.158)	(0.153)	(0.043)	(0.019)	(0.038)	(0.183)	(0.339)	(0.077)	(0.049)	(0.027)	(0.282)	(0.303)
3	0.044	-0.010	0.055	-0.101	0.152	0.148	0.053	0.095	-0.024	0.292	-0.069	-0.073	0.004	-0.178	-0.228
	(0.076)	(0.059)	(0.034)	(0.233)	(0.225)	(0.126)	(0.113)	(0.081)	(0.225)	(0.715)	(0.168)	(0.129)	(0.056)	(0.516)	(0.587)

Notes: The Table reports the differences of the outcome variable between recipient and non-recipient firms estimated at the cut-off score (score=75). All the variables are accumulated over the first 3 years after the assignment (including that of the assignment) and scaled by sales in the pre-assignment year. We estimated the model using the Epanechnikov kernel combined with two bandwidths (30 and 15) and various polynomials. The full sample includes 341 firms in panel A and 271 in panel B. Bootstrapped standard errors (100 replications) clustered by score in round brackets. Polynomial of order 0 is the difference in mean between treated and untreated. Small (large) firms are those with value added below (above) the median.
*, **,***: significant at 10%, 5%, 1% respectively.

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