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

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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 threshold received the subsidy. Over 1,200 firms submitted proposals, with winners receiving an average subsidy of about 170,000 Euros. We use a sharp regression discontinuity design to compare investment spending of subsidized firms just above the cutoff score with spending by firms just below the cutoff. 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 impact of the program. 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.

Keywords: research and development; investment incentives; crowding-out; regression discontinuity design

JEL codes: R0; H2; L10.

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1. Introduction[1]

Public incentives for private research and development (R&D) are offered in most of advanced countries. Almost all OECD governments support business R&D by direct funding or tax relief; a support that is also substantial in its amount (OECD, 2008).[2] The economic rationale for R&D subsidies is based on a market failure argument. One justification is the existence of positive externalities. In these circumstances the social return to 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 is the presence of liquidity constraints. Such constraints are particularly important for intangible investments, which are subject to considerable uncertainty and information asymmetry (see e.g.: Bond and Van Reenen, 2007; Hall and Lerner, 2009).

In spite of the popularity of R&D investment subsidies, the question of whether these subsidies actually work – i.e., increase firms' R&D activity – is so far 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 public R&D subsidies has been widely studied but with very mixed results. Out of nineteen micro-econometric studies analyzed in the survey by David et al. (2000), one-half found no effect. By examining the studies published in the last decade we found a similar balance: only five, out of ten, show a positive role of the public incentives on R&D activity

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[2] In the OECD countries direct government funding of business R&D is the 0.1% of GDP (tax incentives are excluded).

(see Table A1 in the Appendix).[3] 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, subsidy receipt is endogenous, and comparisons 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). Such policy presents several key features to accurately examine the effectiveness of R&D incentives. 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 mechanism of funds' assignment. 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). With respect to other methods employed in the program evaluation literature this strategy has an important advantage. Under general assumptions – in our context firms must not have the capability to completely control their score – 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. To the best of our knowledge, this work represents the first attempt to evaluate the impact of R&D incentives through such method.

In addition, the local dimension of the policy 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 nation-wide 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 highly representative of the national industry: it is the third industrial

[3] Given that our paper is focused on incentives through grants we do not consider the literature examining the effects of tax incentives (on the literature on fiscal incentives see e.g.: Hall and Van Reenen 2000).

region of the country, covering 11% of Italian firms' R&D outlays and more than 10% of patents; the area is also characterized by the key role played by small and medium enterprises, as in the all country.

A third attracting feature is that the program was generous (the total amount of grants has been about 93 million euros) and involved a large number of firms (1,246 enterprises submitted a proposal). In our baseline sample, on average each subsidized firm received 182,000 euro; one fourth of the total investment made by each participating firms over the two years after the program. The size of the grants and the large participation are helpful to the evaluation exercise.

Finally, our assessment permits to shed some light on the effects of programs managed by local government. Such policies have so far attracted scant attention of the empirical literature, despite absorbing a non-marginal share of the total public transfers to private sector.[4] In Italy, from 2000 to 2007 around 18 billion euro were granted to firms owing to these programs--one fourth of total funds assigned to private enterprises. To know the impact of these policies is essential to have more awareness of the use of public resources.

We find that overall the program did not create additional investment. Our results do not reject the hypothesis that firms substituted public for privately financed R&D. This result is driven by large firms, though. When we estimated the impact of the program by firms' size we find that, unlike large firms, small ones substantially increased their investment on average by the same amount of the received grant. The heterogeneity of our results can be explained by the different degree of liquidity constraints between large and small firms, as shown by some financial indicators. We also find that for subsidized firms the amount of grant received in relation to the investment did not play a significant role. Our findings turn out to be robust to multiple sensitive checks.

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

[4] In the Italian experience two exceptions are represented by Gabriele et al., 2007 and Bondonio, 2007. They did not evaluate firms R&D incentives though.

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 cover 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 marginal cost is equal to marginal return. In that case, public policy is ineffective because the subsidy will not change the investment opportunity cost and there will be no increase in investment. In contrast, in an imperfect capital market firms face a supply of finance schedule that is initially horizontal, reflecting the availability of internal funds, and once the internal funds are exhausted it becomes upward sloping (see e.g. Bond 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, as moral hazard, increase with the amount of borrowed funds. Under these circumstances a public subsidy will shift the MC schedule to the right because it decreases the costs 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 firm's investment depends on the position of MR schedule. If the MR curve intersects the MC in its horizontal part (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 the case of MR_B), the grant will increase the optimal level of investment. Projects that before the program were unprofitable as 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).

Such framework applies to tangible as well as 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, could be amplified because R&D projects are riskier and less understood by non-expert agents than other kinds of investments, 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 that, 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 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. On this regards two considerations have to be outlined. First, even assuming an excellent ability of public institutions in choosing the projects to subsidize, they could 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 the public opinion that the policy is not wasting resources, in that infra-marginal investment have higher success probabilities and higher profitability than marginal ones (see Wallsten 2000; 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 further the private costs of capital. 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. There might be also indirect effects acting in the opposite direction, though. For example, if the supply of the R&D inputs is price inelastic, as for 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

From the theoretical framework it is evident how the effectiveness of the R&D policy is a question that requires an empirical answer. The challenge that empirical studies face is that subsidized firms are not randomly chosen. Rather, they differ from non-subsidized firms for important unobserved characteristics correlated with the outcome variable, so that treatment is endogenous. In the most recent analyses the endogeneity problem has been taken in serious consideration and addressed mainly through matching methods or instrumental variable estimates. However, independently from the strategy adopted, the conclusions of earlier studies are mixed.

David et al. (2000), surveying firm-level analyses conducted in the previous three decades, observe that in almost half (9 out of 19) the policies were not found to trigger additional investment while in the other half it was the contrary. In the case of Small Business Innovation Research program in U.S. two studies reach opposite conclusions. Lerner (1999), matching subsidized and unsubsidized firms by industry and size, finds that the policy increased sales and employment of subsidized firms; on the other side Wallsten (2000), using as instrument for the subsidy the amount of public funds available for each type of R&D investment in each year, shows that grants did not induce an increase of the employment and that public subsidy crowded out firm-financed R&D dollar for dollar. The evidence available for other countries is also mixed. In the case of Israel, Lach (2002) finds that grants created additional R&D investment for small firms but, since most part of the subsidies was given to large firms that did not show additional investment, the overall impact was null. He compared the performance of granted and non-granted firms using difference-in-difference (DID) estimator and controlling for several observables. Almus and Czarnitzki (2003) use matching strategy to study R&D subsidies in Eastern Germany, finding an overall positive and significant effect on investment. Gonzalez et al. (2005)

explore the case of Spain estimating simultaneously the probability to obtain 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. More recently, Gorg and Strobl (2007) combining matching method with DID estimation find in Ireland that only small grants had additional effects on private R&D investment, while large grants crowded-out private investment.[5]

3. The program

In 2003 the government of Emilia-Romagna implemented the "Regional Program for the Industrial Research, Innovation and Technological Transfer" putting into effect the Regional Law n. 7/2002, art. 4 (see: "Bollettino Ufficiale della Regione n. 64, 14 May 2002" and "Delibera della Giunta Regionale n. 2038, 20 October 2003"). The program aims at sustaining 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 - carried out by firms in the region (the geographic area covered by the policy is described by Figure A1 in Appendix). According to the program, the regional government subsidizes through grants the R&D expenditure of eligible firms. The grant may cover up to 50% of the costs for the industrial research projects and 25% for the pre-competitive development projects; the latter limit is extended by an additional 10% if applicants are small or medium enterprises. Eligible firms - including temporary associations or consortia- are those that have an operative main office and intend to realize the project in the region. Several types of costs, related to the eligible project, can be subsidized. Among them: costs of machinery and equipment, included those for software; costs of researchers; costs for the use of laboratories; contracts with research

[5] The empirical literature includes also: Busom (2000) who find that public fund induced more private expenditure in Spain, even if she cannot exclude that crowding-out occurred for the 30% of participants; Branstetter and Sakakibara (2002) who show how public-sponsored research consortia increased the patenting activity of Japanese firms that participate to the consortium; Hujer and Radic (2005) that examine the impact of the public subsidies on firms' innovation propensity in Germany finding a positive impact only for Eastern Germany. As regards Italy, Merito et al. (2007) show that subsidies had no impact on the post-program employment, 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).

centers (included those for consulting); feasibility studies; registration of patents; costs for licenses and prototypes (only for the pre-competitive development projects though), etc.[6] To be eligible projects must be worth at least 150,000 euro; the maximum grant per project is 250,000 euros. 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.

An 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 confounded with that of other public subsidies.

The grants are assigned after a process of assessment of the projects carried out by a committee of 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 aspects: 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).[7] Only projects assessed sufficient in each profile, and that obtain a total score equal to or larger 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 Department for Education, University and Research of the Italian Government and the general principles of the European Commission.[8]

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[6] The law does not specify how these costs 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 multi-year utility, they are usually booked among the tangible or intangible investments. The clarification is important because our evaluation is based on balance sheets data, given that direct information on firms' R&D expenditure are not available.

[7] Among the elements of point a) there are the innovative degree 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): the experience collected in the past for similar projects or the adequacy of the managerial competence; point d) regional priorities indicated in the Regional Law as the project that involve universities and hiring new qualified personnel.

[8] See: "Linee guida per la valutazione della ricerca", Comitato di indirizzo per la valutazione della ricerca – Ministero dell'Istruzione, dell'Università e della Ricerca; and "Orientamenti concernenti le procedure di valutazione e di selezione delle proposte nell'ambito del VI Programma quadro per la ricerca e lo sviluppo

Up to now two auctions have been implemented. In the first (second) the application deadline was in February 2004 (September 2004) and the evaluation process terminated in June 2004 (June 2005).[9] Overall, the total amount of funds granted has been about 93 million euro, corresponding to the 0.1% of the regional GDP (the same ratio between assistance to private R&D and GDP in the national average). The total planned investments have been equal to 235.5 million euro. For the industrial firms of our sample used for the estimates the grant has been on average equal to 182,000 euro, one fourth of the total investment made by each participating firms over 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 realized the same amount of R&D investment without the grants. A typical issue of the program evaluation literature is that subsidized and non-subsidized firms differ for unobserved characteristic correlated with the outcome. Therefore, the subsidy is endogenous and we have to adopt a strategy that addresses such endogeneity to correctly identify the effect of the program. We take advantage of the mechanism of the funds' assignment. As described above, the committee of experts assigned a score to each project and only those that received a score larger or equal to a given threshold were granted (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. Letting the outcome variable be a function of the score, the average treatment effect of the program is assessed by testing whether there is a 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)

tecnologico," European Commission. More information regarding the evaluation process, the procedures and the principles are reported in the "Delibera della Giunta regionale n. 2822/2003".

[9] See: Delibera della Giunta Regionale n. 1205, 21st June 2004 and n. 1021, 27th June 2005".

and van der Klaauw (2002).[10] 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 relies on the continuity assumption, which requires that firms in a neighborhood just below and just above the cut-off have the same potential outcome under identical funding experience. Even though there is no direct way to test 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 context, the impact of the program is identified by a discontinuity of the outcome variable at the cut-off point (Hahn et al. 2001).

RD design is suitable in the contexts were the agents cannot perfectly manipulate the forcing variable (the score). We believe that our situation is appropriate to adopt such identification strategy, in that it is hard to argue that firms participating in the program can completely control their score. In any case, the randomization assumption has testable implications. If 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). As Lee (2008) pointed out, similarity of the two groups is a consequence of randomization and not vice versa. Therefore, we can assess the validity of the design by verifying whether differences in treated and control firms' observables become negligible close to the cutoff point. Moreover, there are also indirect ways to test the validity of the crucial continuity assumption, as to check whether other covariates, or the outcome variable in absence of the program, are continuous across the threshold. We will carry out these tests in section 6.

Since in the RD method results can be sensitive to some arbitrary choices, as the functional form of the model or the interval around the cut-off point used in the local regressions, we use multiple functional forms and samples around the cutoff.

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[10] This literature is vast. For an overview see: Lee and Lemieux (2009); for recent applications see among others the monographic number of Journal of Econometrics, Vol. 142, 2008.

Several econometric models have been suggested to test for the discontinuity at the cut-off point (see e.g.: Imbens and Lemieux, 2008; Lee and Lemieux, 2009). Here we use a parametric approach estimating up to a third order polynomial model:[11]

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

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 γ_p are assumed different on the opposite side of the cut-off to allow for heterogeneity of the score function across the threshold; ε_i is the random error. We also test the mean difference between treated and untreated firms (in the Tables it is indicated as polynomial of order 0).

Equation (1) has been estimated on the full sample and locally around the cut off point using two different sample windows. The wide-window includes 50% of the baseline sample (firms with score between 52 and 80); the narrow-window includes 35% of baseline sample (score in range 66-78). The ranges have been chosen to (almost) balance the number of firms on the left and on the right side of the threshold.

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 caution is necessary. Since our forcing variable is discrete (it can assume only integer values) random disturbances can be correlated within group (similarly to the cases discussed by Moulton, 1990). In our situation the groups are represented by the firms that received the same score. In these circumstances standard errors could be downward biased and spurious statistical significance may occur. To correct for such bias we clustered the heteroskedasticity robust standard errors by the values of the score *S* (Lee and Card, 2008).

[11] The sample size advises against non-parametric models. Higher orders of polynomials were rejected by standard model selection criteria (Akaike information Criterion and Schwartz Bayesian Criterion). Some examples of studies that adopt similar model include Card et al. (2007) and Lalive (2008).

4.2 Data

For the evaluation exercise we combine two data sets. One provided by the Region Emilia-Romagna that includes a restrict number of information on participating firms: as name, score, investment programmed, grants assigned, subsidies revoked and renunciations. Besides, we utilize the CERVED data set which provides us with balance sheets data of (almost) all Italian corporations, from which we get the outcome variables and other covariates used in the empirical analysis.

Since we do not have direct information on the R&D expenditure of the firms, we use total, tangible and intangible investment as outcome variables. Investment has been scaled by sales so that our benchmark outcome variable is: $Y_i = Investment_i/Sales_i$. Investments are accumulated from the year of the assignment up to two years afterwards (the expected period of project's realization) to detect the whole R&D activity potentially related to the subsidized investment. Sales refer to the first pre-assignment year. To avoid results driven by outliers, we trimmed the sample according to the 5th and 95th percentile of the distribution of $Total\ investment_i/Sales_i$ (trimming also investment over assets does not change substantially the results). Some costs related to the eligible projects might not have been recorded among the intangible or tangible investment, e.g. those for hiring researchers or those for consulting and feasibility studies. Thus, we evaluated the effect of the program also using labor and services costs as outcome variables. As for investment, they have been accumulated over time and scaled by the pre-assignment sales; we expect that these costs increased thanks to the grants.

Up to now two auctions have been concluded, in 2004 and 2005. We pool together the data of the two auctions. Overall the number of participating firms is 1,246 (557 treated and 689 non-treated). Our empirical strategy is based on the score assigned to each firms, therefore we had to exclude 411 unsubsidized firms that did not receive a score in the second auction because they were insufficient in at least one profile. Notice 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 the exclusion does not bias our results.

After having linked information on participating firms provided by the Region with balance sheet data set, and 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] Given the remarkable heterogeneity between industrial and services firms, and within the service industry (that includes e.g. professional offices together with transports and real estates), that might produce large noise in our data, we focused on industrial firms (manufacturing and construction) and presented only the results of the baseline model for services.

In Table 1 the distribution of firms by sector is reported. We notice that there is a large concentration of the firms within only two sectors: machinery and chemical together absorb two third of the firms' sample. The first is the sector of the regional specialization, but represents also the main industrial sector for the entire Italian industry. The concentration of firms in a few sectors reinforces our evaluation exercise, in that permits to compare homogeneous firms. Moreover, because of the exclusion of the non-scored applicant firms to the second auction, treated firms are more than the double 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 overall remarkably larger than untreated firms, as shown by mean differences of sales, valued added and assets. A significant, and potentially worrying, difference arises also for the firms' self-financing capabilities, measured by cash flow over sales. However, when we restrict the sample around the cutoff, using both the wide and the 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.

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[12] We were able to link 750 of the scored firms (499 subsidized and 251 unsubsidized) with the balance sheet data set. Reasons of missing are that some applicants were not corporation, others were start up and presumably there were some misprints of the firms' identifiers. Then, we have excluded renunciations and revocations (114 firms), 3 firms of the energy and mining sectors together with firms that have sales or assets equal to 0 and unsubsidized firms in the first auction but subsidized in the second. As said, we excluded also the 5° and 95 percentile of our key outcome variable (investment over pre-assignment sales).

In Figure 2 the density function of the sample by score is shown. We notice that it is higher on the right 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 values slightly more distant. We do not interpret such 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. Such record could have been perceived 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 has 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 present first 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 ones, let us briefly discuss how to interpret the results. A coefficient β equals to zero would signal complete crowding-out of private expenditure by public grant: firms reduced private expenditure by the same amount of the subsidies and the investment turn 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 opposite even crowding in, occurred - that is if public subsidies have triggered private financed investment - we have to compare the change in total investment to 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 points rather dispersed, being the investment 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, from the figures remarkable jumps of the outcome variable at the threshold do not emerge; however, if any the impact seems somewhat positive.

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

It is possible that we were unable to detect any effect because firms did not book R&D expenditures among investment in balance sheet. For example, if the project has a short-term utility, R&D activity could have been booked in the current costs. To verify such eventuality we use labor and services costs as additional outcome variables. All are accumulated over time and scaled by pre-assignment sales. Moreover, we change scale variable using capital and total assets to check the sensitiveness of our previous findings. Results of these exercises are reported in Table 4. Labor costs almost always have a negative sign, but only rarely the coefficient is 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; however, in some models the coefficient now turns out to be 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 and we do not observe significant impact of the policy on other potentially affected variables.

5.2 Financial constraints and firms' size

So far we did not find 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 larger, 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 market there are the small ones. First of all because information asymmetries are strengthened for small enterprises, given that they are less visible, usually younger, and the capabilities of their management less known. Second, small firms often lack of sufficient collaterals. Third, they are less diversified and, as a consequence, their earnings may be more volatile. For all the previous reasons they are more dependent from the external finance and, at the same time, less able to rise funds from the capital market than large firms. 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), among others, even though questioned by some other studies (see e.g.: Guiso, 1998 and Audretsch and Elston, 2002).

If liquidity constraints are amplified for innovative investment and small firms have worse 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 such hypothesis (e.g. Lach 2002 and Gonzalez et al. 2005). In order to test for heterogeneous causal effect of the program across firms' size we estimated the following model:

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

where the firms' size dummies are interacted with the treatment dummy and the score; $Size_i^1=1$ if the value added of firm *i* is below the median (*Small*); $Size_i^2=1$ if the value added is above the median (*Large*).[13] 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*.

Figure 4 and 5 outline the investment by sale against the score for the two groups. The effect seems null for large firms but positive and rather substantial for small ones. In Table 5

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

we show the results of the estimates of model (2). 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; it remains still positive though. On the opposite, 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 impact of the program appears remarkable. If we take as 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 the 40% of its standard deviation. Even if it seems an exceptional increase, we have to take into account that the grants have been substantial (for the small firms on average equals to 173 thousands euro) especially if compared to the investment of the untreated firms (on average equal to 107 thousands euro). In order to more accurately measure the impact of the policy on small firms and to understand if partial crowding-out, or in contrast crowding-in, occurred, we re-estimated model (2) using as dependent variable the total investment and as key right hand side variable the grants paid to the firms instead of the treatment dummy variable T:

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

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 mean difference); in the linear model or higher order polynomials models the coefficient increase 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, likely because they possess sufficient internal financial resources and have better access to the credit market.

To verify such interpretation, we observe some indexes plausibly correlated to firms' internal and external financial capability (unfortunately direct information on financial constraints are not available). We calculate for industrial firms of our full sample: the own capital/debts ratio, that reflects the capability of the firms to provide collaterals; cash flows over sales and ROA, that show the ability of the firms to finance investment with internal funds; the financial costs over total debts as a proxy of interest rate paid by the firms for external funds. As reported in Table 6, all the indexes turn out to be 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 from, and plausibly have worse access to, the external finance than large firms.

6. Extensions and robustness checks

In this section we present some extensions of our previous model and the robustness exercises run to verify the validity of our empirical design and the sensitiveness of our results. First of all, we investigated how firms of the services sector reacted to the subsidies, thus we re-estimated model (1) and (2) only on participating firms that belong to the services sector. The results are presented in Table 7. Given that firms of services are less numerous we used only 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 and negative but only rarely statistically significant for large ones.

A topic relatively little studied in the evaluation literature, yet, which appears relevant to design effective policies, is the role played by the share of investment covered by the grant (coverage ratio). It is possible that for firms with high coverage ratio subsidies could be more effective than for those with small coverage ratio. For a program that supported tangible investment in Italy, Bronzini and de Blasio (2006) found that investment of firms with high coverage ratio increased significantly with respect to those of the untreated firms, while for those with 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 reverses.

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

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

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 resembles a normal with mean and median equals to 0.40. In our sample there is not much 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 coverage ratio. The estimates of model (4) are reported in Table 8. 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. Such differences however are often negligible and non-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 augment 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 more serious in the exercise with sample split, when the number of firms is reduced. The covariates introduced consists of 2-digits 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 9 are remarkable similar to the baseline ones. The coefficients turn out to be close in magnitude to those previously estimated and highly statistically significant for the small firms.

RD identification strategy relies on the continuity assumption, which requires that potential outcome should be smooth around the cut-off point in absence of the program. There is not a direct way to verify such 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 in absence of the treatment. The exercise is run using the following observables that could be in principle correlated with investment but pre-determined in the short-term: 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 10).

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 11 (section 1) and Figure 5 show that before the program there have been no jumps in investment.

We finally check whether there are discontinuities of function *Y(S)* at score values different from the cutoff 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 such dummies are jointly non-statistically different from zero. If we accept the null, we can conclude that there are not other jumps of the outcome variable: the unique one is that at the threshold. In Table 11 (section 2) are reported 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 regional program implemented recently in northern Italy. Using a sharp regression discontinuity strategy we find that overall grants did not have a positive effect on firms' investment. However, when we differentiate firms by size, we find that for small firms the grants substantially triggered 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. We argued that it is the different degree of financial constraints that can explain our findings. In our sample, several financial indexes show how smaller enterprises may have had worse access to credit market than larger ones. Overall, our results turn out to be similar to the conclusions reached by Lach (2002) and Gonzalez et al. (2005).

A policy implication of our analysis is that, since infra-marginal projects are more likely presented by small firms, only this category of firms should be the target of the policy. Alternatively, a more complex design should aim to subsidize, among the profitable projects, only those presented by firms that could be more likely subject to financial constraints.

We analyzed the direct effects of the policy on the main target variable. Of course, there are further interesting issues that we did not address and that would deserve attention. A first one is the long-term effect of the grants. If recipient large firms showed remarkably better performances than non-recipient ones in the long-term, the main conclusions on the effectiveness of the policy could be partially modified. Additional important issues are the indirect effects of the program. Among them, the presence of spillovers is one of the most relevant. The increase of R&D investment might produce positive spillovers across firms that, in terms of social welfare, could even offset the costs to have unsuccessfully financed larger enterprises. For regional programs an interesting question is also to know whether spillovers are localized. To understand such effects would be highly rewarding even though it is empirically challenging.

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Figure 1

INVESTMENT COSTS AND RETURNS IN AN IMPERFECT CAPITAL MARKET

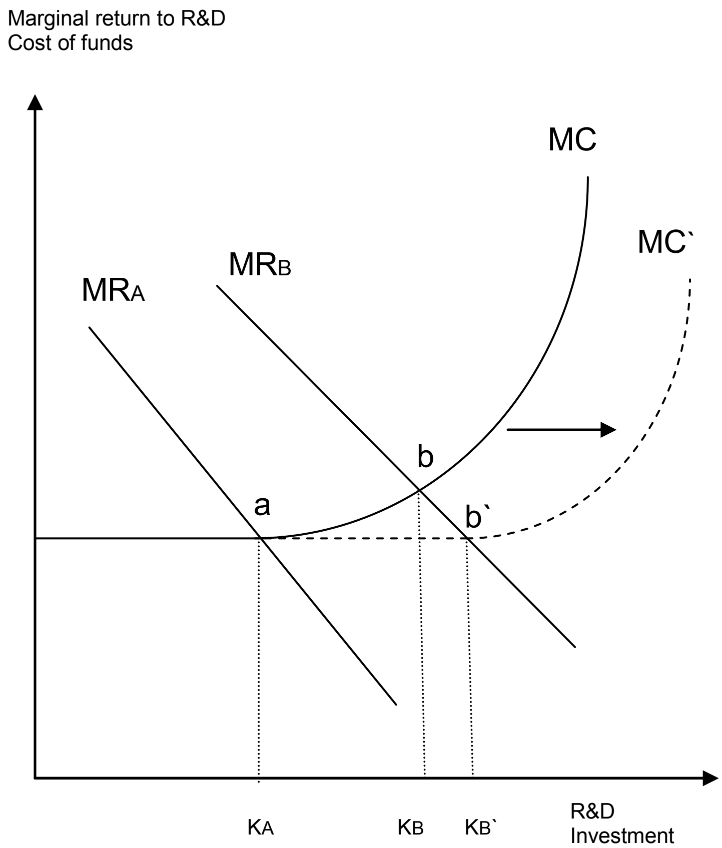


Figure 2

FIRMS DENSITY DISTRIBUTION BY SCORE

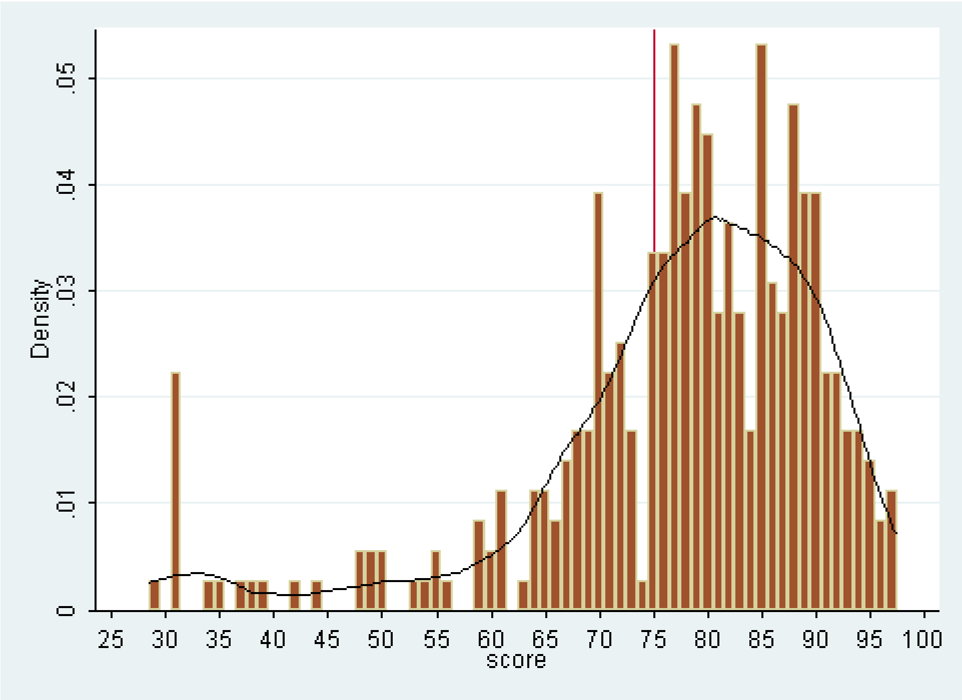
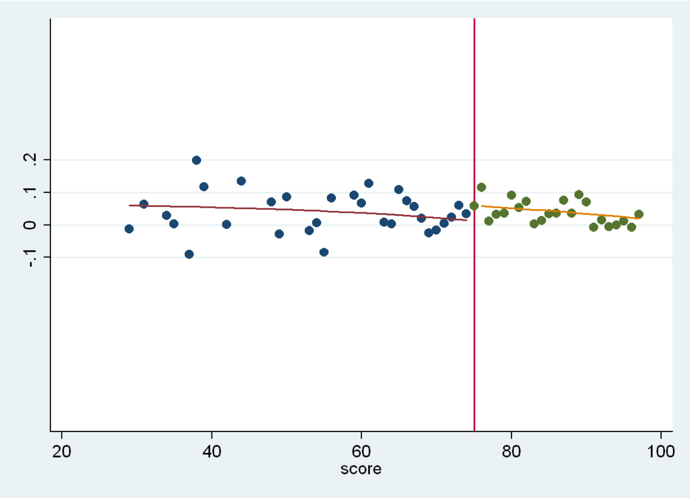
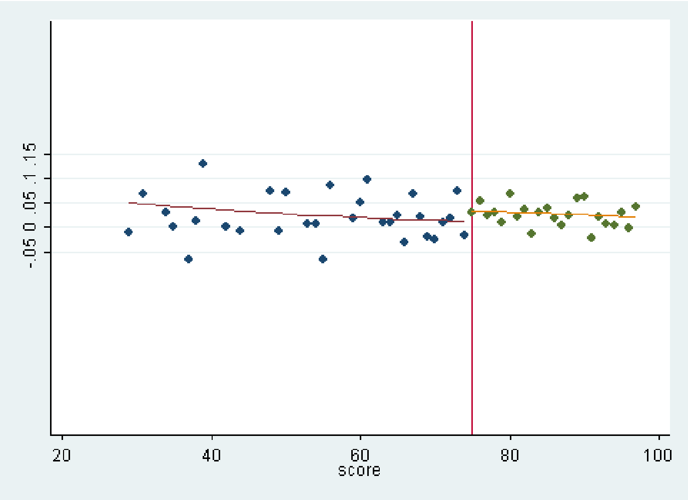


Figure 3

TOTAL INVESTMENT/SALES



TANGIBLE INVESTMENT/SALES



INTANGIBLE INVESTMENT/SALES

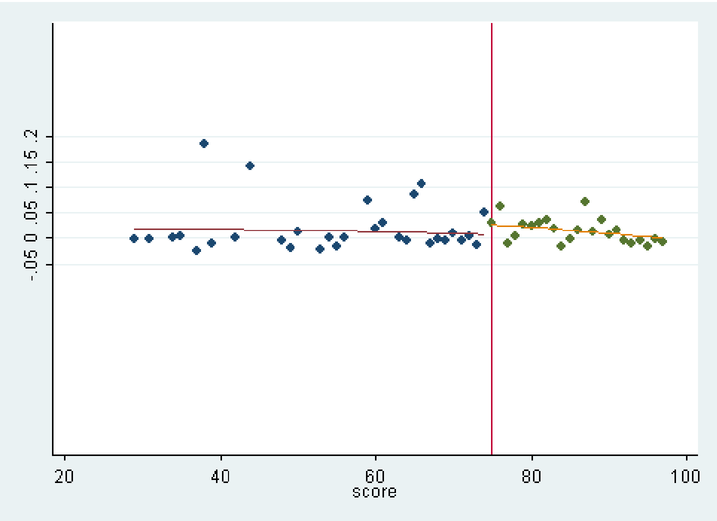
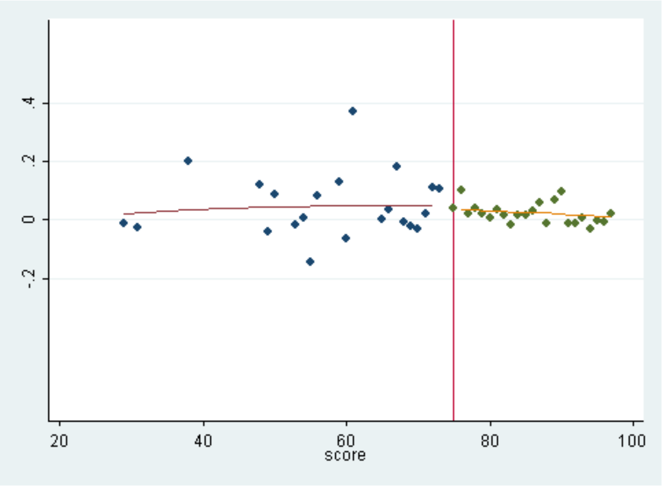


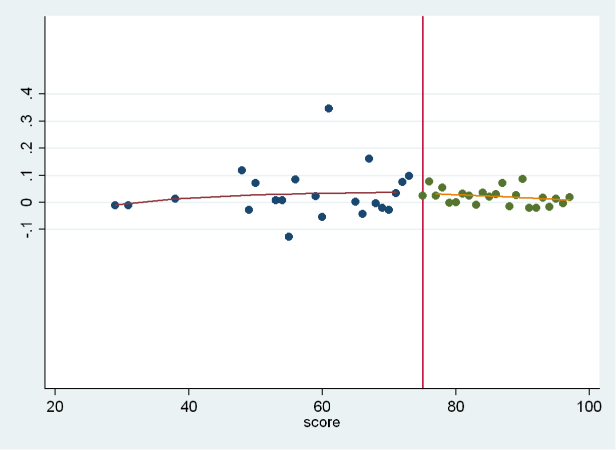
Figure 4

LARGE FIRMS

Total investment/Sales



Tangible investment/Sales



Intangible investment/Sales

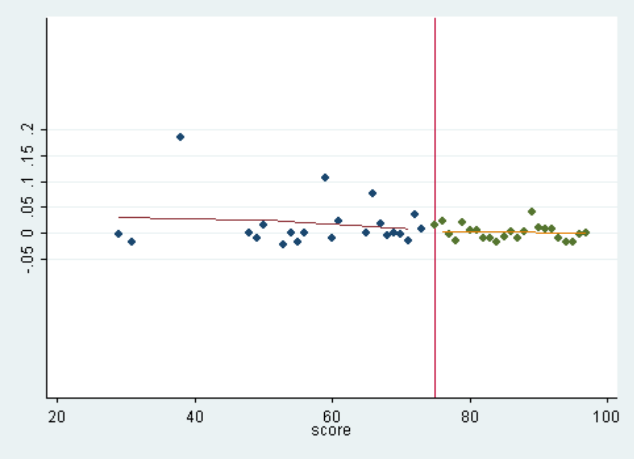
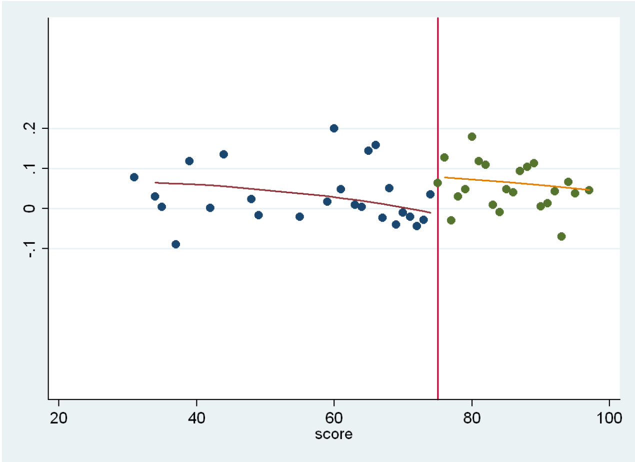


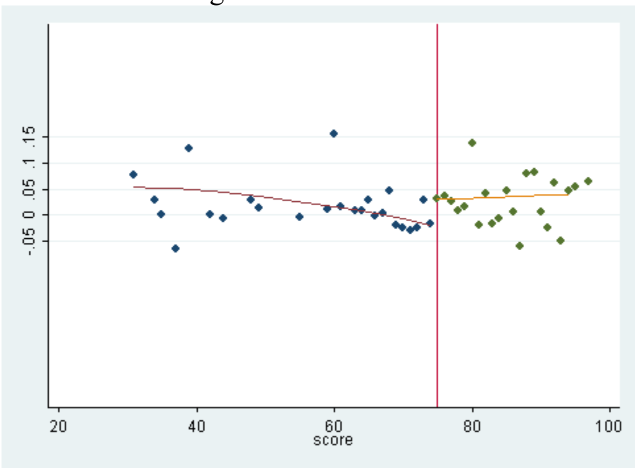
Figure 5

SMALL FIRMS

Total investment/Sales



Tangible investment/Sales



Intangible investment/Sales

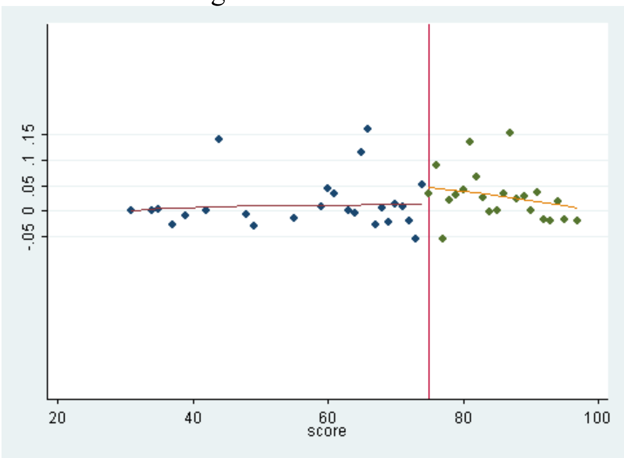
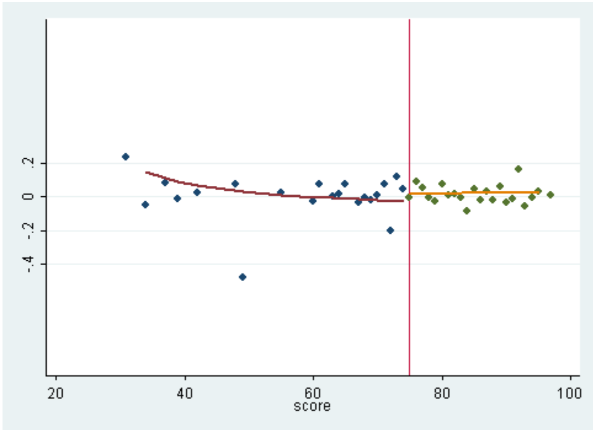


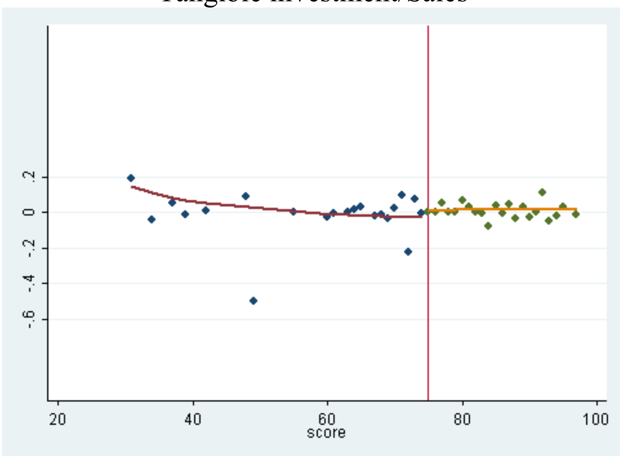
Figure 6

PRE-PROGRAM INVESTMENT – SMALL FIRMS

Total investment/Sales



Tangible investment/Sales



Intangible investment/Sales

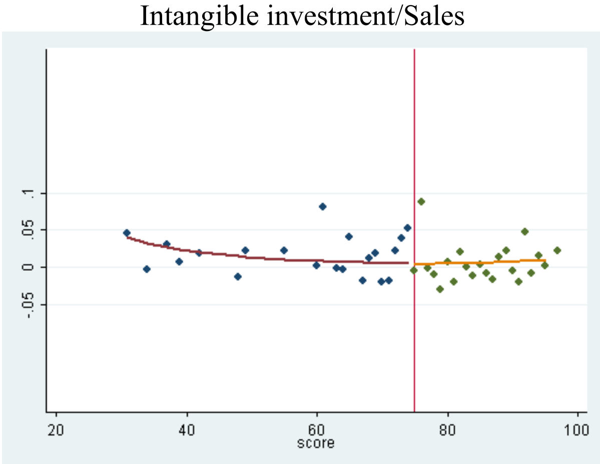


Table 1

DISTRIBUTION OF FIRMS BY SECTOR

	Number of firms		Percentages	
	Treated	Untreated	Treated	Untreated
Food, beverages and tobacco	18	5	7.1	6.4
Textile, wearing and apparel, leather products	4	3	1.6	2.0
Paper, printing and publishing	3	1	1.2	1.1
Chemicals 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)

	All			50% cutoff neighborhood sample (score 52-80)			35% cutoff 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 invest. /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 invest. /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 treated firms are 254; untreated firms are 103. In the 50% cutoff neighborhood sample treated firms are 90 untreated 81; in the 35% cut off neighborhood sample treated firms are 57 and untreated 58. Investments are calculated as the difference between the capital stock in two consecutive years.
*, **,***: significant at 10%, 5%, 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 (included that of the assignment); sales refer to the pre-assignment year. 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 is: 357 in panel A; 171 in panel B; 115 in panel C.
*, **,***: significant at 10%, 5%, 1% respectively.

Table 4

BASELINE RESULTS: EFFECT OF THE PROGRAM ON DIFFERENT 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 (included that of the assignment); sales and assets refer to the pre-assignment year. 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. Only industrial firms are considered.
*, **,***: significant at 10%, 5%, 1% respectively.

Table 5

EFFECT OF THE PROGRAM ON INVESTMENT BY FIRMS' SIZE

	Model (2)									Model (3)		
Order of polynomial	Total inv./Sales			Tangible inv./Sales			Intangible inv./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)	(0.07)		(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 means Akaike Information Criterion. Small [Large] firms are those falling in the first [second] half of the distribution of the value added. See also notes to Table 3.
*, **,***: significant at 10%, 5%, 1% respectively.

Table 6

FIRMS' FINANCIAL INDEXES

	Mean (st. dev.)		Mean diff. (st. errors)
	Small	Large	
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%, 1% respectively.

Table 7

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 Notes to Table 3 and 5. Number of observations are: 111 in panel A; 67 in panel B.
*, **,***: significant at 10%, 5%, 1% respectively.

Table 8

EFFECT OF THE PROGRAM ON INVESTMENT BY FIRMS' COVERAGE RATIO

	Total investment/ Sales			Tangible investment/ Sales			Intangible investment/ Sales		
Order of polynomial	Low	High	*AIC*	Low	High	*AIC*	Low	High	*AIC*
				A. Full sample					
0	0.012	0.012	*-597.1*	0.013	0.003	*-709.6*	-0.001	0.009	*-979.4*
	(0.014)	(0.013)		(0.012)	(0.011)		(0.007)	(0.009)	
1	0.046**	0.033	*-595.4*	0.033*	0.013	*-705.6*	0.013	0.019	*-977.2*
	(0.021)	(0.022)		(0.017)	(0.014)		(0.010)	(0.018)	
2	0.061**	0.029	*-591.3*	0.038	0.003	*-700.3*	0.022*	0.026	*-974.9*
	(0.028)	(.033)		(0.024)	(0.022)		(0.012)	(0.026)	
				B. Local estimates: Wide window sample					
0	0.036	0.015	*-275.9*	0.031*	0.006	*-353.6*	0.005	0.009	*-461.4*
	(0.021)	(0.019)		(0.016)	(0.011)		(0.009)	(0.016)	
1	0.042	0.038	*-270.9*	0.029	0.002	*-347.9*	0.013	0.035	*-457.7*
	(0.034)	(0.037)		(0.026)	(0.020)		(0.014)	(0.030)	
2	0.139**	0.090*	*-272.9*	0.074*	0.010	*-346.9*	0.064***	0.080**	*-459.1*
	(0.050)	(0.050)		(0.041)	(0.037)		(0.021)	(0.031)	
				C. Local estimates: Narrow window sample					
0	0.043**	0.021	*-199.0*	0.034**	0.008	*-266.5*	0.009	0.012	*-303.6*
	(0.019)	(0.026)		(0.015)	(0.013)		(0.010)	(0.024)	
1	0.074**	0.059	*-197.6*	0.031	-0.011	*-263.5*	0.043*	0.070*	*-306.1*
	(0.033)	(0.047)		(0.033)	(0.033)		(0.022)	(0.038)	
2	-0.053	-0.092**	*-201.4*	-0.043	-0.099	*-264.9*	-0.009	0.006	*-306.8*
	(0.036)	(0.036)		(0.063)	(0.063)		(0.042)	(0.045)	

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 notes to Table 3 and 5.

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*
	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 notes to Table 3 and 5.
*, **,***: significant at 10%, 5%, 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: Table shows the estimates of the coefficients β_k of model (2) using different outcome variables. See also notes to Table 3 and 5.

Table 11

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	0.010	0.012	0.003	-0.009	0.007
	(0.034)	(0.026)	(0.029)	(0.015)	(0.011)	(0.017)
1	0.042	-0.32	0.041	-0.004	0.001	-0.028
	(0.040)	(0.038)	(0.035)	(0.021)	(0.019)	(0.027)
2	0.002	-0.039	-0.011	-0.042	0.013	0.003
	(0.053)	(0.052)	(0.046)	(0.030)	(0.026)	(0.031)
			B. Local estimates: Wide window sample			
0	0.022	-0.011	0.028	-0.004	-0.006	-0.006
	(0.034)	(0.024)	(0.027)	(0.017)	(0.018)	(0.011)
1	0.019	-0.011	-0.005	-0.008	0.025	-0.003
	(0.058)	(0.043)	(0.047)	(0.030)	(0.035)	(0.019)
2	-0.006	0.011	-0.014	0.013	0.008	-0.002
	(0.076)	(0.059)	(0.060)	(0.043)	(0.036)	(0.034)
			C. Local estimates: Narrow window sample			
0	0.041	-0.008	0.026	-0.001	0.014	-0.006
	(0.042)	(0.030)	(0.036)	(0.023)	(0.024)	(0.013)
1	-0.024	-0.022	-0.019	-0.017	-0.004	-0.005
	(0.096)	(0.065)	(0.083)	(0.050)	(0.039)	(0.032)
2	-0.109*	0.056	-0.075	0.042	-0.033	0.014
	(0.059)	(0.059)	(0.048)	(0.040)	(0.020)	(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	1.11	1.27
	(0.28)	(0.30)	(0.12)
1	1.06	1.02	1.26
	(0.37)	(0.44)	(0.14)
2	1.07	1.01	1.22
	(0.36)	(0.45)	(0.17)

Notes: In the first section of the table are shown the estimates of the coefficients β_k of model (2) using investment of 2 years before the implementation of the program. In the second section of the Table are reported 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. Full sample has been used. P-value in brackets. See also notes to Table 3 and 5.

Appendix

Table A1

STUDIES ON R&D INCENTIVES PUBLISHED IN THE LAST DECADE *

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	Partial effect: only for smaller grants
Merito et al. (2007)	Italy	Employment, sales, productivity	Matching	No effect

* The Table reports the studies that examined the effect of direct subsidies for R&D; those evaluating the impact of tax incentives are not included.

Figure A1

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