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Raffaello Bronzini
Banca d'Italia
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Banca d'Italia
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Are Incentives for R&D Effective?
Evidence from a Regression Discontinuity Approach[†]

By Raffaello Bronzini and Eleonora Iachini*

This paper evaluates a unique R&D subsidy program implemented in northern Italy. Firms were invited to submit proposals for new projects and only those which scored above a certain threshold received the subsidy. We use a sharp regression discontinuity design to compare the investment spending of subsidized firms with that of unsubsidized firms. For the sample as a whole we find no significant increase in investment. This overall effect, however, masks substantial heterogeneity in the program's impact. We estimate that small enterprises increased their investments—by approximately the amount of the subsidy they received—whereas larger firms did not. (JEL G31, G38, L52, O33, O38, R32)

Public incentives for private research and development (R&D) are offered in most advanced countries through direct funding or tax relief. The amount of financial resources involved can be substantial. For instance, in the OECD countries (Organisation for Economic Co-operation and Development), direct government funding of business R&D, excluding tax incentives, amounts to 0.1 percent of gross domestic product (OECD 2008).

The economic rationale for R&D subsidies, which also explains their popularity, is based on a market failure argument. Since knowledge is a public good—i.e., it is nonrivalrous and nonexcludable—firms cannot internalize wholly 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 capable of increasing private R&D will improve 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).

Notwithstanding the popularity of R&D investment subsidies, the question of whether they actually work—i.e., increase firms' R&D activity—remains unsettled. Theory predicts that if a program subsidizes inframarginal projects, incentives will be ineffective because they will not trigger additional investment. In order to be successful a program must target marginal projects—those that would not be undertaken without the grants. On the empirical ground the impact of R&D subsidies has been widely studied, but previous analyses have yielded very mixed results. Of the 19 microeconometric studies surveyed by David, Hall, and Toole (2000), almost one-half found no effect. Examining the papers published in the last decade, we find a similar heterogeneity (see Table A1 in the Appendix for an overview).[1] The main challenge in empirical studies lies in 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 nonrecipient firms are likely to differ in both observed and unobserved ways that are correlated with the outcome of interest. In this context, the variable that captures subsidy recipients is endogenous, and models that fail to control adequately 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 only eligible projects that receive a certain score in an assessment by an independent technical committee will be 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, Todd, and van der Klaauw 2001). Compared with other methods employed in the program evaluation literature, this strategy has several important advantages. First, 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.

Second, the policy's local dimension allows us to remove much of the unobserved heterogeneity among enterprises, and compare recipient and nonrecipient 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 percent

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[1] Given that our paper is focused on incentives in the form of 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).

of Italian firms' R&D outlays and more than 10 percent of patents. Small- and medium-sized enterprises also play a key role in this area, as they do throughout Italy.

A third advantage is the program's size (overall, about €93 million have been granted) and its involvement of a large number of firms (1,246 applicant enterprises). In our baseline sample, each subsidized firm received an average of €182,000—one-fourth of the total investment made by each participating firm in 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.[2] To date, these policies have attracted scant attention from the evaluation literature, despite absorbing a relatively large share of total public transfers to the private sector.[3] In Italy, from 2000 to 2007 around €18 billion were granted to firms under these programs—one-fourth of total public funds assigned to private enterprises. It is crucial that the impact of these policies be assessed in order to gain a clearer picture 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 firm size we find that, unlike large firms, small enterprises increased their investment substantially, by on average the same amount of the grant received. Our findings are robust to multiple sensitivity checks.

The remainder of the paper is structured as follows. In Section I we discuss the theoretical issues and previous empirical literature. Section II illustrates the features of the program. Section III describes the empirical strategy and the data employed in the evaluation exercise. The main results are reported in Section IV, while the robustness exercises and the concluding remarks make up the final two sections.

I. Conceptual Framework and Empirical Evidence

In economic theory, public subsidies for R&D expenditure are expected to expand firms' R&D investment by reducing the cost of capital and increasing the expected investment profitability.[4] However, the effectiveness of the incentive cannot be taken for granted. Rather, it depends on which type of project is subsidized. In order to be effective the subsidy must trigger additional investment, therefore it must reward projects that without the grant would not be undertaken (*marginal projects*). These investments are those that if privately financed would be unprofitable, because the cost of the investment is larger than (or equal to) the expected return,

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

[3]In Italy, two exceptions are Bondonio (2007) and Gabriele, Zamarian, and Zaninotto (2007). However, they did not evaluate firms' R&D incentives.

[4]In the literature, the effect of R&D subsidies is usually examined within the standard neoclassical firm-level investment behavior. Within this framework, the R&D equilibrium expenditure is given by the firm profit-maximizing decision, and it is such that the (increasing) marginal cost of capital is equal to the (decreasing) marginal rate of return on the R&D investment (Howe and McFetridge 1976; David, Hall, and Toole 2000).

but that become profitable if financed with a (cost-free) public subsidy. On the other hand, if the policy subsidizes projects that would be profitable even without the incentive (*inframarginal projects*) the grant will be ineffective because recipient firms would have made the investment anyway, and the program will not activate new R&D spending. In these circumstances, firms will substitute completely public for privately financed R&D to take advantage of the lower cost of public funds, but will not expand R&D investment (Wallsten 2000). The incentive becomes a money transfer to firms without any positive effect on investment.

There are several considerations to bear in mind concerning the policy's effectiveness. First, all other things being equal, we can predict that the effectiveness of the grant is more likely to be associated with proposals submitted by firms that encounter greater difficulties in financing their projects. For example, enterprises which (owing to more acute problems of asymmetric information) face higher costs of capital or experience greater difficulties in accessing capital markets are more likely to find that their projects will be unprofitable if privately financed only.

Second, financing innovation is more difficult than financing tangible investment because of stronger informational asymmetries. These asymmetries may be amplified because R&D investments are riskier and less well understood by nonexpert 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. It could be more difficult to finance intangible investment also because financial intermediaries might prefer to fund projects related to tangible assets which, in turn, can be offered as collateral, rather than to intangible assets that are exclusively connected with future streams of profits (see Guiso 1998; Bond and Van Reenen 2007; Hall and Lerner 2009).

A third consideration on the effectiveness of the program concerns the strategic role played by the procedure for assigning funds, since only programs that subsidize marginal projects will activate additional investment. Even assuming that public institutions demonstrate an excellent ability in selecting the firms to subsidize, they may not have all the information necessary to distinguish between marginal and inframarginal projects. Therefore it is possible that, at least in part, funds will be given to inframarginal projects, thereby reducing the effectiveness of the subsidies. Furthermore, public institutions might be tempted to subsidize inframarginal projects to convince public opinion that the policy is not wasting resources given the higher probability of success of inframarginal investments (Wallsten 2000 and Lach 2002).

Up to now we have focused primarily on the direct effects of the subsidy on expenditure. However, several indirect effects of the policy might change the cost or revenue of the investment, 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 (Meuleman and De Maeseneire 2012). Moreover, thanks to the grants, firms may benefit from expanded or upgraded research facilities or from better-trained researchers, increasing the revenue of other current or future projects, and eventually increasing the marginal returns of the investment. These mechanisms can amplify the impact of the policy and induce firms to increase R&D outlays by

even more than the amount of the grants, generating a crowding-in effect of the public program. However, there might also be indirect effects acting in the opposite direction. For instance, 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 Goolsbee 1998 and, for a more extensive discussion of the indirect effects: David, Hall, and Toole 2000 and Lach 2002).

In the light of these considerations, it is evident that the effectiveness of a subsidy program is to a large extent an empirical question.

A. *Empirical Evidence*

There is a large empirical literature on the effects of R&D incentives. The main challenge that studies face in assessing the effectiveness of such policies is that subsidized firms are not randomly chosen. Rather, they differ from nonsubsidized 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. The endogeneity problem has been addressed in recent analyses mainly through matching methods or instrumental variable estimates. However, irrespective of the strategy adopted, the conclusions of earlier studies are mixed.

Surveying firm-level analyses conducted in the previous three decades, David, Hall, and Toole (2000) observe that almost one-half (9 out of 19) of the policies were not found to trigger additional investment while for the other half the opposite was true. More recent evidence is similarly inconclusive. In the case of the Small Business Innovation Research program in the United States, two studies reach opposite conclusions. Matching subsidized and unsubsidized firms by industry and size, Lerner (1999) finds that the policy increased the 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 nonsubsidized firms using difference-in-differences (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. González, Jaumandreu, and Pazó (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 predetermined (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. Combining the matching method with DID estimations, Görg and Strobl (2007) find that in Ireland only small grants had additional effects on private

R&D investment, while large grants crowded out private investment. Hussinger (2008) uses two-step selection models to show that in Germany public subsidies were effective in promoting firms' R&D investment. Finally, Jacob and Lefgren (2011) use a similar method to ours to estimate the impact of public grants on US researchers' output measured by the number of published articles and citations, and find a limited impact of public support. Meanwhile Takalo, Tanayama, and Toivanen (2013), using a structural model estimated on firm-level data from Finland, find positive general equilibrium effects of the subsidies on expected welfare—i.e., the expected benefits of the program net of its costs—although the expected effects of the incentives are highly heterogeneous.[5]

II. The Program

In 2003, the government of Emilia-Romagna inaugurated the "Regional Program for Industrial Research, Innovation and Technological Transfer," implementing Article 4 of Regional Law 7/2002, (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 to support industrial research by firms in the region and precompetitive 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). 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 percent of the costs of industrial research projects and 25 percent for precompetitive development projects; the 25 percent limit is extended by an additional 10 percent if applicants are small- or medium-sized enterprises. Eligible firms—including temporary associations or consortia—are those which have an operative main office and intend to implement the project in the region. Several types of outlay related to the eligible project can be subsidized: (i) costs for machinery and equipment; (ii) software; (iii) purchase and registration of patents and licenses; (iv) employment of researchers; (v) the use of laboratories; (vi) contracts with research centers; (vii) consulting; (viii) feasibility studies; and (ix) external costs for the realization of prototypes. The maximum grant per project is €250,000.[6] The duration of the investment is from 12 to 24 months, but it can be extended. Subsidies are transferred to the firms either after the completion of the project, or in two installments: the first at the completion of 50 percent of the project and the second on completion.

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[5] The empirical literature includes research by: 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 percent of participants; Branstetter and Sakakibara (2002), who show how public-sponsored research consortia increased the patenting activity of Japanese firms in a consortium; and Hujer and Radić (2005), who examine the impact of public subsidies on firms' innovation propensity in Germany, finding a positive impact only for Eastern Germany. For Italy, Merito, Giannangeli, and Bonaccorsi (2007) find that subsidies had no impact on the post-program employment levels, productivity, or sales of the subsidized firms with respect to matched untreated firms. See also the surveys by Klette, Møen, and Griliches (2000) and Hall and Van Reenen (2000).

[6] To be eligible, projects must be worth at least €150,000.

One important characteristic of the program is that firms cannot receive other types of public subsidy 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 committee of independent experts appointed by the regional government has assessed the projects. For the evaluation process the committee may request the assessment of independent evaluators. The committee examines the projects and assigns a score for each of the following aspects: (i) technological and scientific (max. 45 points); (ii) financial and economic (max. 20 points); (iii) managerial (max. 20 points); and (iv) regional impact (max. 15 points).[7] Only projects deemed sufficient in each category and which obtain a total score of at least 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.[8] Note that the program is designed in such a way that the likelihood of winning a subsidy is independent of the size of the requested grant.

To date, there have been two rounds of applications. The first deadline was in February 2004, the second in September 2004, and the evaluation process terminated in June 2004 and June 2005, respectively.[9] A total of about €93 million was granted overall, corresponding to 0.1 percent of regional GDP (the same ratio as that between assistance to private R&D and GDP in the national average). Total planned investment equaled €235.5 million. For the industrial firms in our sample used for the estimates, grants averaged €182,000, one-fourth of the total investment made by each participating firm in the two years after the program.

III. Empirical Strategy and Data

A. *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 nonsubsidized 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. To deal with the endogeneity issue, we exploit the funds' assignment mechanism. As described

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[7] Point (i) includes: the degree of innovation of the project and the adequacy of the technical and scientific resources provided; point (ii): the congruence between the project's financial plan and objectives; point (iii): past experience gained in similar projects or the level of managerial competence; point (iv): regional priorities indicated in the Regional Law such as projects involving universities and the hiring of new qualified personnel.

[8] 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.

[9] See the *Delibera della Giunta Regionale* No. 1205 (21 June 2004) and No. 1021 (27 June 2005).

above, the committee of experts assigned a score to each project and only those receiving a score equal to or above a given threshold were awarded grants (75 points out of 100). We apply a sharp regression discontinuity (RD) design comparing the performance of subsidized and nonsubsidized firms with scores 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); and van der Klaauw (2002).[10] This strategy is deemed preferable to other nonexperimental 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 cutoff 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) shows formally that if the treatment depends on whether a (forcing) variable exceeds a known threshold and agents cannot control precisely the forcing variable, the continuity assumption is satisfied since the variation in treatment around the cutoff is randomized, as if the agents had been randomly drawn just below or just above the cutoff. In this scenario, the impact of the program is identified by the discontinuity of the outcome variable at the cutoff point (Hahn, Todd, and van der Klaauw 2001).

RD design is suitable in contexts where the agents cannot manipulate the forcing variable (the score) perfectly. We believe that in our situation this strategy is appropriate, in that it is hard to argue that firms participating in the program can control their score completely. 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 which are far from the cutoff). The similarity of the two groups is a consequence of randomization and not vice versa (Lee 2008). It follows that 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 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 robustness exercises in Section V.

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

Several econometric models have been suggested to test for the discontinuity at the cutoff point (see, amongst others: Imbens and Lemieux 2008; Lee and Lemieux

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[10] Recent methodological reviews are provided by Lee and Lemieux (2010) and in the monographic number of the *Journal of Econometrics*, vol. 142(2), 2008.

2010). Here we use both parametric and nonparametric methods. First, we estimate up to a third-order polynomial model on the full sample:[11]

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

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 cutoff to allow for heterogeneity of the function across the threshold; and 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 cutoff point using two different sample windows. The wide window includes 50 percent of the baseline sample (firms with scores between 52 and 80); the narrow window includes 35 percent of the baseline sample (scores in the 66 to 78 range). The ranges have been chosen to (almost) balance the number of firms to the left and right of the threshold. Since the samples around the cutoff are relatively small (the former includes 171 firms, the latter 115) and higher-order polynomial models can be imprecisely estimated if the sample is small (see Lee and Lemieux 2010), for the local regressions around the cutoff we stop at the polynomial model of order two.

Third, we estimated the discontinuity using other nonparametric techniques, namely the Epanechnikov kernel regressions using two bandwidths, 30 and 15 points of the score (the results are presented and discussed in Section V).

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 cutoff point, corresponding to the unbiased estimate of the causal effect of the program. For the inference, however, a word of caution is called for. Since our forcing variable is discrete (the score can assume only integer values) random disturbances can be correlated within the group (not unlike 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 robust standard errors by the values of the score S (as suggested by Lee and Card 2008). In the kernel regressions standard errors are clustered and bootstrapped.

B. *Outcome Variables and Data*

Regarding the outcome variables, one natural candidate would be R&D investment. However, in our case the data on R&D spending are not available. Therefore, we adopt a different strategy. We build the analysis on balance-sheet data provided by Cerved group, which collects information on almost all Italian corporations.

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[11] Higher orders of polynomials were rejected by standard model selection criteria (Akaike Information Criterion and Schwarz Bayesian Criterion). Studies that adopt similar models include Card, Chetty, and Weber (2007) and Lalive (2008).

From the balance sheets we take as outcome variables those items that are associated with the expenditures reimbursable by the program listed in Section II. The rationale is that if the program enabled 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, compared with those of nonrecipient firms. More specifically, since the main reimbursable outlays are items that fall into the category of tangible or intangible assets, such as costs for machinery and equipment, software, patents, and licenses (see Section II), we take tangible and intangible investment as our first, and favored, outcome variables. In our study the examination of tangible investment is important also because in Italy a large share of firms' expenditure for innovation activity (about 40 percent) is covered by tangible assets (Istituto Nazionale di Statistica 2010). We employ net investment calculated from the balance-sheet data as annual differences in tangible or intangible assets net of amortization.

According to the Italian Civil Code, in the balance sheet intangible assets include the costs for intangible goods that have multiyear utility, namely: (i) start-up costs, (ii) R&D and advertising costs, (iii) costs of patents, software, and other intellectual property rights, (iv) licenses and trademarks, (v) goodwill (recorded only when it is acquired in a business acquisition), (vi) costs for ongoing intangible assets, and (vii) other intangible assets nonclassifiable with the previous ones. The Cerved dataset provides information on these items but only for some firms. Many firms (usually the smaller ones) can present a simplified version of the financial statement indicating the amount of the total intangible assets only (note that we are unable to distinguish the firms that present a simplified financial statement from the others). Moreover, data on each item (for the companies that provide them) is less precise than those on the overall intangible assets. With these caveats in mind, in order to shed some light on the actual content of intangible assets we examined their distribution across items for the five firms participating in the program that show the largest intangible assets (we imagine that for them the quality of data is relatively better). Of course, we consider this exercise to be merely an illustrative example. In the sample of five firms, R&D, patents, software and other intellectual property rights, licences, trademark, and ongoing intangible assets cover on average about 66 percent of the total intangible assets, while goodwill covers 22 percent, and the other intangible assets the remaining 11 percent (start-up costs are zero). We believe that since the vast majority of items included in intangible assets are expenditures eligible for the subsidy (e.g., R&D, patents, software, and licenses), to use intangible assets as an outcome variable in the analysis is reasonable. We are aware that among them there are also some items, such as goodwill, that have little connection with the program; however, they only make up a small share and so in our view they could only generate a second-order bias in our results.

Furthermore, given that other reimbursable outlays include those related to the employment of researchers, we use three additional variables: labor costs, level of employment, and wages. Thanks to the program, labor costs may increase because firms hire additional employees and/or because they replace low-skilled employees with high-skilled employees (researchers). Employment enables further light to be shed on the effect of the program on labor input. By using wages, calculated as

labor costs divided by the number of employees, we verify whether the program, by increasing the demand for labor, benefited mainly employees in terms of higher wages as shown by Goolsbee (1998).[12]

Finally, since other minor reimbursable costs refer to the services bought by the firms for R&D projects—such as costs for the use of laboratories, contracts with research centers, consulting, feasibility studies, and external costs for the realization of prototypes—we adopt service costs as our last outcome variable. On the whole this strategy permits us to also distinguish the effect on the different types of R&D expenditure.[13]

In short, we assessed the impact of the program on the following outcome variables: investment (total, tangible, and intangible), labor costs, employment, wages and service costs. All the variables are accumulated from the year of the assignment up to two years afterward (the expected period of the project's realization), to detect all R&D activity potentially related to the subsidized investment.[14] Moreover, except for employment and wages, the variables are scaled by pre-program sales. To prevent potential endogeneity issues due to the scale variable, sales and the other normalization variables refer to the first year before the assignment of the grant—the so-called pre-program period. Employment and wages are not scaled but in log. Finally, to avoid the results being driven by outliers—especially for investments that are highly volatile over time and uneven across firms—we trimmed the sample according to the fifth and ninety-fifth percentile of the distribution of $Total\,investment_i / Pre\text{-}program\,sales_i$ (trimming investment scaled by total assets does not change substantially the results either).[15]

The balance-sheet data have been combined with the dataset 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, planned investment, grants assigned, subsidies revoked, and renunciations.

To date, two invitations for applications have been concluded, in 2004 and 2005. We pool together the data from both. A total of 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 round because their projects were deemed insufficient in (at least) one aspect (among those listed in Section II). Note that the strategy is based on the test for discontinuity around the cutoff point, and plausibly omitted firms would have received a total score distant from the cutoff, thus we believe that their exclusion did not bias our results.

After linking information on participating firms provided by the Region with the balance-sheet dataset, and after cleaning the sample, we ended up with a full

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[12] Employment and wages are only available in our dataset for a subsample of firms and as a result regressions are run on samples other than those used for the other outcome variables.

[13] At the same time we acknowledge that the unavailability of a direct and reliable measure of R&D expenditure is a limit of our dataset.

[14] In our dataset, the duration of the projects is between 12 and 32 months depending on the firm (this information is provided by the Emilia-Romagna Region and is available in the dataset, though not for all enterprises). Because the funds are assigned in the month of June for both rounds, we expect that by including the year of the assignment and the following two years we are able to cover the whole period of the project's realization.

[15] In the robustness section we will show that the results are not sensitive to the type of trimming.

TABLE 1— DISTRIBUTION OF FIRMS BY SECTOR (*Percentages*)

Sector	All firms		Small firms		Large firms	
	Treated	Untreated	Treated	Untreated	Treated	Untreated
Food, beverages, and tobacco	7.1	6.4	5.0	5.1	8.9	4.5
Textiles, wearing apparel, leather products	1.6	2.0	2.5	1.7	0.7	4.5
Paper, printing, and publishing	1.2	1.1	2.5	1.7	0.0	0.0
Chemical products	11.0	10.4	10.9	5.1	11.1	13.6
Nonmetallic mineral products	3.9	3.9	1.7	5.1	5.9	2.3
Basic metal industries	7.9	9.0	9.2	10.2	6.7	13.6
Machinery and equipment	57.5	57.1	59.7	62.7	55.6	47.7
Transport equipment	6.3	5.3	3.4	3.4	8.9	2.3
Other manufacturing industries, wood, and wood furniture	1.6	2.8	3.4	1.7	0.0	11.4
Construction	2.0	2.0	1.7	3.4	2.2	0.0
Total industrial firms	100.0	100.0	100.0	100.0	100.0	100.0

sample of 357 industrial firms (254 treated and 103 untreated) and 111 service firms (of which 61 were treated).[16] The sample covers the vast majority of the grants. In the sample used for the estimates, the recipient firms received 66 percent of the total funds granted; if we include outliers of the trimmed tails we reach 94 percent.[17] However, it is worthwhile emphasizing that since start-ups and very small enterprises are underrepresented in our data, our findings might not apply to these categories of firm. Finally, given that the remarkable heterogeneity of 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.

Table 1 reports the distribution of firms by sector. We observe that there is a large concentration of firms in just two sectors: machinery and chemicals alone absorb two-thirds of the firms' sample. Machinery is a sector of regional specialization, but also represents the main industrial sector in Italian industry as a whole. The concentration of firms in relatively few sectors reinforces our evaluation exercise, in that it allows us to compare homogeneous enterprises. Note that due to the exclusion of the nonscored applicant firms from the second round, treated firms are more than double the number of untreated ones, while the proportion of treated and untreated firms is pretty evenly balanced within each sector.

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[16] We were able to link 750 (90 percent) of the scored firms (499 subsidized and 251 unsubsidized) with the balance-sheet dataset. 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), three firms from the energy and mining sectors together with firms having sales or assets equal to zero and firms unsubsidized in the first round but subsidized in the second. As mentioned earlier we also excluded the fifth and ninety-fifth percentile of our key outcome variable (investment over pre-assignment sales). Our dataset does not seem affected by selection problems due to firms' survival. In the last year used for the analysis (2007), we find about 95 percent of the industrial firms present at the beginning (2004). Such a high survival rate, which turns out to be rather similar between large and small firms, is probably due to the relatively short period of time examined.

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

TABLE 2—PRE-ASSIGNMENT MEAN-DIFFERENCES BETWEEN UNTREATED AND TREATED FIRMS
(*Standard errors in brackets*)

Variable	Full sample	50 percent cutoff neighborhood sample (score 52–80)	35 percent cutoff neighborhood sample (score 66–78)
Sales	44,694**	4,116	8,179
	(20,442)	(7,561)	(10,119)
Value-added	10,070**	1,328	1,888
	(4,724)	(2,057)	(2,778)
Assets	39,153**	5,692	7,792
	(17,576)	(7,792)	(10,415)
Return on assets	0.889	0.504	1.415
	(1.179)	(1.421)	(1.351)
Own capital/debts	−0.054	−0.212*	−0.232
	(0.082)	(0.115)	(0.152)
Gross operating margin/sales	0.011	0.001	−0.003
	(0.009)	(0.013)	(0.013)
Cash flow/sales	0.019**	0.010	0.012
	(0.008)	(0.011)	(0.013)
Financial costs/debts	−0.005	−0.006	−0.007
	(0.004)	(0.008)	(0.011)
Labor costs/sales	−0.009	0.003	−0.016
	(0.010)	(0.014)	(0.019)
Service costs/sales	−0.012	0.015	0.027
	(0.014)	(0.017)	(0.021)
Total investment/sales	0.003	0.009	0.024
	(0.009)	(0.015)	(0.019)
Tangible investment/sales	0.013	0.020	0.033
	(0.008)	(0.016)	(0.020)
Intangible investment/sales	−0.010	−0.011	−0.009
	(0.006)	(0.008)	(0.012)

Notes: Only manufacturing and construction firms. All the mean-differences refer to the first pre-assignment year (2003 for the first round and 2004 for the second). In the full sample 254 firms are treated, and 103 are untreated. In the 50 percent cutoff neighborhood sample there are 90 treated and 81 untreated firms; in the 35 percent cut-off neighborhood sample there are 57 treated and 58 untreated firms. Investments are calculated as the difference between (tangible and intangible) assets in two consecutive years.
   ***Significant at the 1 percent level.
   **Significant at the 5 percent level.
    *Significant at the 10 percent level.

Table 2 shows the mean differences between untreated and treated firms of several observables in the year before the assignment of funds (see Table A2 in the Appendix for the values of the means). We note that treated firms are substantially larger than untreated firms, as shown by mean differences of sales, valued-added, and assets. A significant, and potentially worrying, difference also arises for firms' self-financing capabilities, measured by cash flow over sales. However, when we restrict the sample to around the cutoff 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. Differences between the two groups are now remarkably smaller and never statistically significant.[18]

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[18] In the online Appendix (Table B1) we report the main descriptive statistics for the variables and samples used in the regressions.
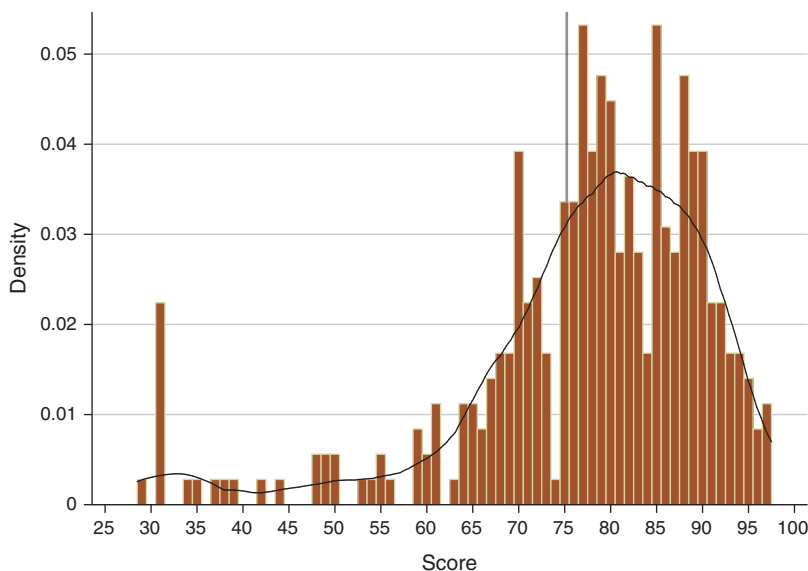
FIGURE 1. FIRMS' DENSITY DISTRIBUTION BY SCORE

In Figure 1 the density function of the sample by score is shown. The density is higher on the right-hand side of the threshold because of the cited exclusion of non-scored untreated firms in the second round and increases substantially around the cutoff point. We observe, however, that just below the cutoff (score $= 74$) it 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 committee of experts avoided assigning a score just below the threshold for understandable reasons. A similar outcome could have been perceived as particularly annoying by unsuccessful applicant firms and would potentially have left more room for appeals against the decision. If anything, this evidence shows that the committee enjoys a certain degree of discretion in assigning the score, a characteristic of the assessment that does not invalidate our design.

IV. Results

A. *Baseline Results*

We first present the estimations of the coefficient β of model (1) using total, tangible, and intangible investment scaled by pre-program sales as outcome variables. Since we do not observe privately financed investment separately from that financed by public incentive, we shall now briefly discuss how to interpret the results. A coefficient β equal to zero would signal complete crowding-out of private investment by public grants: firms reduced private expenditure by the amount of the subsidies received 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 privately 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 triggered privately financed investment—we have to compare the change in total investment with the grants.

Before showing the econometric results let us present the scatterplot of the (averaged by score) outcome variables against the score (Figure 2). As expected, the figure shows rather dispersed points, given that investment is usually greatly uneven across firms. The interpolation lines appear almost flat, showing a weak dependence of the overall outcome on the score. As a matter of fact, no remarkable jumps of the outcome variables 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 for total, tangible, and intangible investment 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 one-third 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 discontinuity of labor and service costs, using these as additional outcome variables. Furthermore, we change the scale variable for investment using capital and total assets (calculated in the pre-program year) to check the sensitiveness of our previous findings on investment. Both are taken from firms' balance sheets. Capital is defined as the sum of tangible and intangible assets (fixed assets). Total assets are the sum of fixed, current, and other assets. 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 the model's specifications. The previous results do not even appear affected by the variable used to scale investment, although 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 and wages for a subsample of firms that reported information on employment (263 out of 357).[19] The former aims to ascertain the effect of the policy on firms' employment; the latter to verify whether the benefits of the program went mainly to employees through higher wages, as shown for the United States by Goolsbee (1998). Table B2 in the online Appendix displays the results (in the local regressions we use only the wide-window because of the narrower sample size). Overall it seems that neither the level of employment nor that of wages changed thanks to the program: the coefficients are almost never statistically significant.

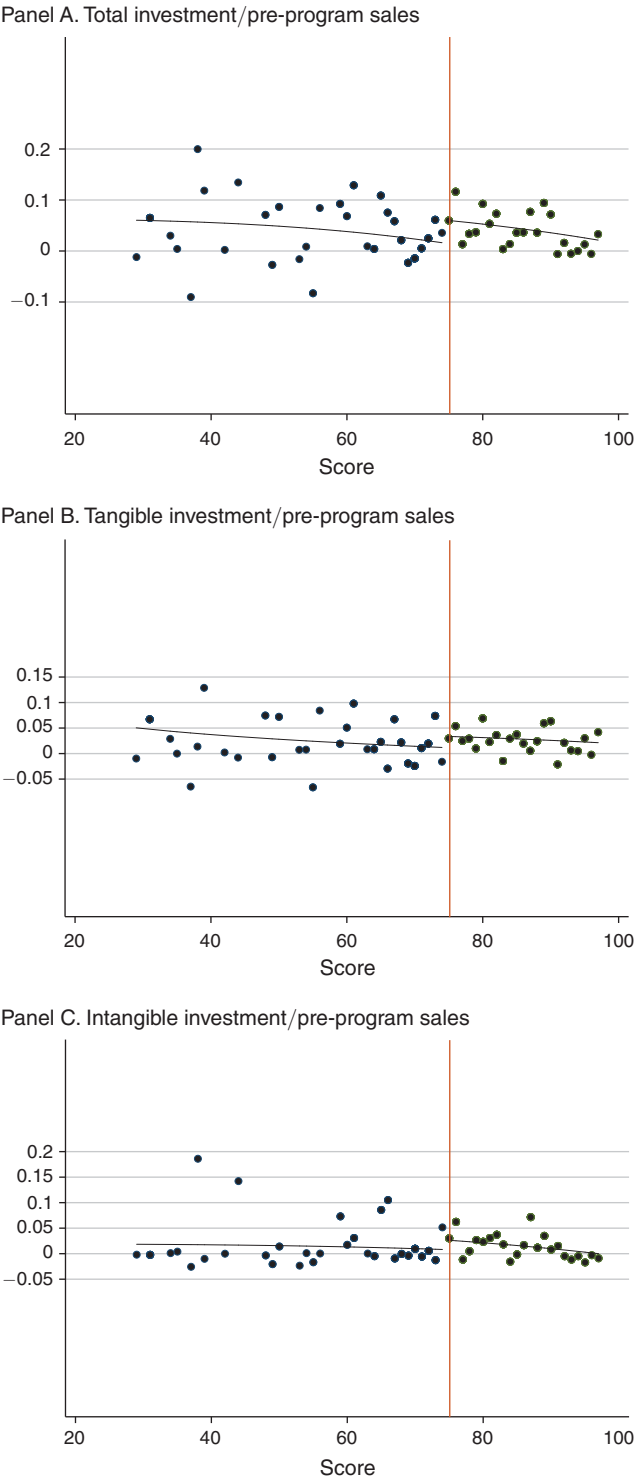[19] Wages are calculated dividing labor costs by employment.

Panel A. Total investment/pre-program sales

Panel B. Tangible investment/pre-program sales

Panel C. Intangible investment/pre-program sales

Figure 2. Full Sample

TABLE 3—BASELINE RESULTS, EFFECT OF THE PROGRAM ON INVESTMENT

Order of polynomial	Total investment/ pre-program sales		Tangible investment/ pre-program sales		Intangible investment/ pre-program sales	
	β	AIC	β	AIC	β	AIC
Panel A. Full sample						
0	0.012 (0.013)	−599.1	0.008 (0.010)	−710.7	0.004 (0.007)	−979.5
1	0.040* (0.020)	−598.8	0.024 (0.015)	−708.6	0.015 (0.012)	−978.5
2	0.045 (0.030)	−595.9	0.021 (0.022)	−704.6	0.024 (0.018)	−978.0
3	0.064 (0.041)	−592.8	0.025 (0.034)	−700.7	0.039 (0.024)	−975.5
Panel B. Local estimates: wide-window sample						
0	0.026 (0.019)	−277.1	0.019 (0.013)	−353.7	0.007 (0.011)	−463.3
1	0.041 (0.034)	−273.8	0.016 (0.022)	−350.0	0.024 (0.020)	−460.8
2	0.110** (0.051)	−274.7	0.036 (0.039)	−347.5	0.073*** (0.024)	−462.6
Panel C. Local estimates: narrow-window sample						
0	0.033 (0.022)	−200.3	0.022 (0.014)	−266.8	0.010 (0.016)	−305.6
1	0.068 (0.040)	−198.8	0.009 (0.034)	−263.5	0.058* (0.027)	−307.1
2	−0.079** (0.035)	−199.8	−0.078 (0.062)	−262.6	−0.000 (0.042)	−305.2
Mean (SD) 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. AIC is the Akaike Information Criterion. Investments are accumulated over the first three years after the assignment (including that of the assignment); sales refer to the pre-assignment year. The polynomial of order 0 is difference in mean between treated and untreated. All the samples have been trimmed according to the fifth and ninety-fifth percentile of the distribution of the Total investment/Pre-program sales ratio (calculated over the full sample). Robust standard errors clustered by score are in parentheses. The number of observations (firms) is 357 in panel A, 171 in panel B, and 115 in panel C.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

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.

B. *Results by Firm Size*

We found no evidence of the effectiveness of public subsidies. It is possible, however, that even if overall crowding-out occurred, for firms that experience more difficultly in internally or externally financing their projects (e.g., because they face higher costs of capital) the subsidies created additional investment. In the literature on capital market imperfection, it has been argued that small firms in

Table 4—Baseline Results: Effect of the Program on Other Outcome Variables

Order of polynomial	Total investment/ pre-program capital		Total investment/ pre-program total assets		Labor costs/ pre-program sales		Service costs/ pre-program sales	
	β	AIC	β	AIC	β	AIC	β	AIC
Panel A. Full sample								
0	0.192	1,199.5	0.019	−518.9	−0.051	244.2	−0.091	546.2
	(0.135)		(0.014)		(0.052)		(0.055)	
1	0.470*	1,197.6	0.044**	−517.3	−0.055	247.4	0.032	547.1
	(0.236)		(0.020)		(0.076)		(0.086)	
2	0.658**	1,200.6	0.062**	−516.6	−0.15	248.9	−0.008	550.7
	(0.314)		(0.029)		(0.104)		(0.126)	
3	1.083***	1,202.9	0.101**	−514.3	−0.398**	246.5	−0.079	554.3
	(0.341)		(0.039)		(0.175)		(0.171)	
Panel B. Local estimates: wide-window sample								
0	0.429*	640.4	0.032	−233.6	−0.005	131.9	−0.007	266.8
	(0.215)		(0.020)		(0.071)		(0.076)	
1	0.562	644.1	0.049	−231.4	−0.302***	121.8	−0.077	270.2
	(0.412)		(0.033)		(0.097)		(0.147)	
2	1.504***	644.4	0.153***	−234.8	−0.147	122.8	0.197	271.8
	(0.318)		(0.045)		(0.135)		(0.172)	
Panel C. Local estimates: narrow-window sample								
0	0.335	428.1	0.035	−193.5	−0.093	90.9	−0.025	198.9
	(0.272)		(0.021)		(0.065)		(0.106)	
1	1.288***	428.7	0.104**	−193.4	−0.275*	93.0	0.064	202.5
	(0.378)		(0.035)		(0.142)		(0.167)	
2	1.329**	430.9	−0.049	−195.7	0.172	92.7	0.166	206.3
	(0.535)		(0.030)		(0.119)		(0.216)	
Mean (SD) 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. AIC is the Akaike Information Criterion. Investments are accumulated over the first three years after the assignment (including that of the assignment); sales, capital, and total assets refer to the pre-assignment year. The polynomial of order zero is the difference in mean between treated and untreated. All the samples have been trimmed according to the fifth and ninety-fifth percentile of the distribution of the Total investment/Pre-program sales ratio (calculated over the whole sample). Robust standard errors clustered by score are in parentheses. The number of observations (firms) is 357 in panel A, 171 in panel B, and 115 in panel C.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

particular experience difficulties in accessing capital markets. This is because, first, information asymmetries are more marked for small enterprises given that they are less visible, usually younger, and the capabilities of their management less well known. Moreover, small firms often lack sufficient collateral. Finally, their production is normally less diversified and, as a result, 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.[20]

[20] Empirically, the negative relationship between access to capital markets and firm size was supported by Gertler and Gilchrist (1994); Gilchrist and Himmelberg (1995); and Beck, Demirgüç-Kunt, and Maksimovic

If liquidity constraints are amplified for innovative investment because of a high degree of risk and strong informational asymmetries associated with innovative activities, and small firms have less or more costly 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 González, Jaumandreu, and Pazó 2005).

To test for a heterogeneous causal effect of the program across firm size we estimated the following model, where the firm-size dummies are interacted with the treatment dummy and the score:

$$(2) \quad Y_i = (1 - T_i)\sum_{k=1}^{2} \alpha_k \, Size_i^k + T_i \sum_{k=1}^{2} \beta_k \, Size_i^k$$

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

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*).[21] The model allows for heterogeneous parameters between small and large firms across the threshold through the interaction of the dummy treatment and size. In equation (2), the parameter β_k is the estimate of the causal effect of the program for firms of size k.

Note that in our sample there are substantial differences in firm size. According to the information provided by the Emilia-Romagna Region—available for 249 out of 357 firms—before the program began in 2003, large firms were about five times bigger than small firms: the median large firm had 132 employees (mean = 382.1) whereas the median small firm had only 26 (mean = 27.5). This marked variability in size is helpful for the evaluation exercise to capture the potential heterogeneity of the treatment effect across firms.

Before showing the results by firm size, we verify whether treated and untreated firms are also similar within the two subsamples of large and small enterprises. In Table 1 we displayed the distribution of firms by size, sector, and treatment. In Table B3 (in the online Appendix) we report the 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 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 cutoff in terms of several observables. This evidence supports the implementation of our strategy also for each firm's subsample.

Figures 3 and 4 outline the investment by sales against the score for the two groups. Again, what emerges from the figures is the independence of the investment from the score. The effect appears null for large firms but positive and rather substantial for small ones.

(2005), among others, although other studies have questioned it (see, for example: Guiso 1998; and Audretsch and Elston 2002).

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

Panel A. Total investment/pre-program sales

Panel B. Tangible investment/pre-program sales

Panel C. Intangible investment/pre-program sales
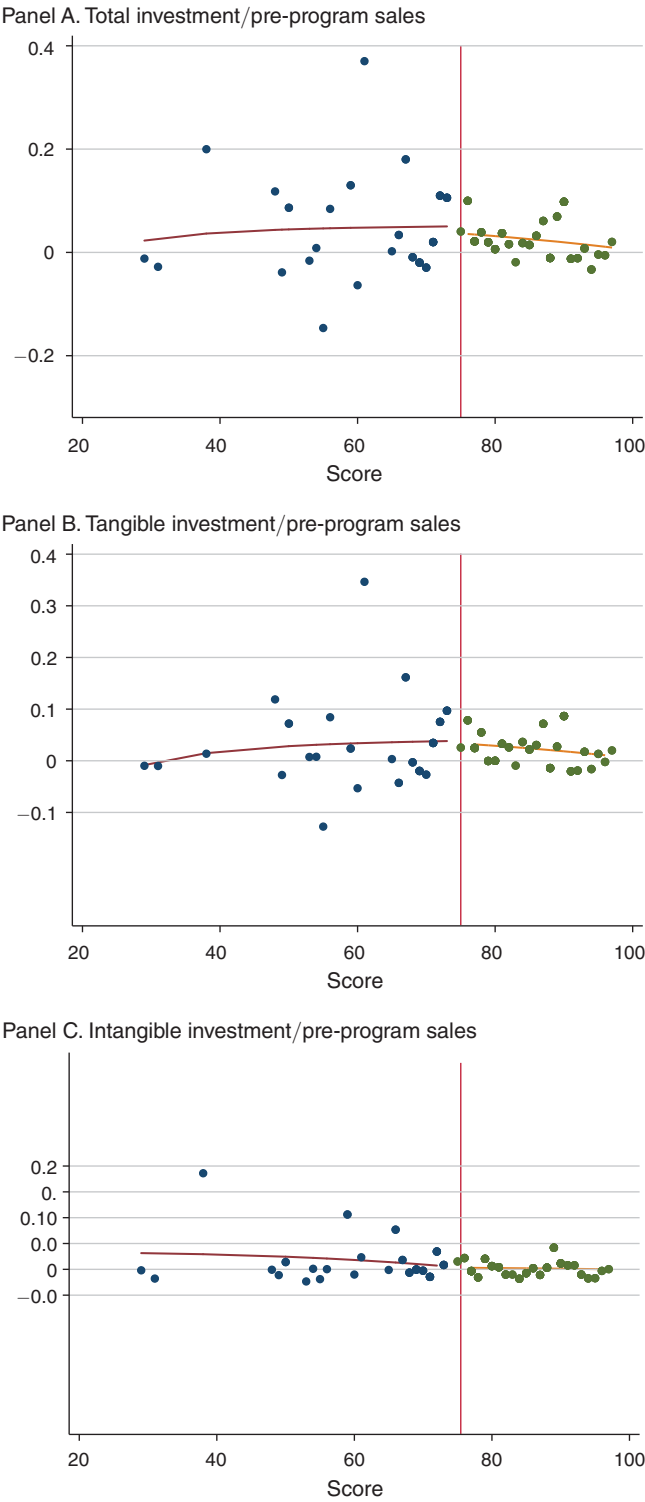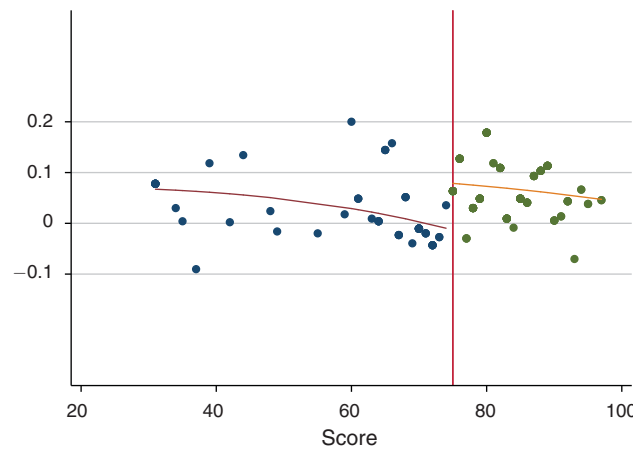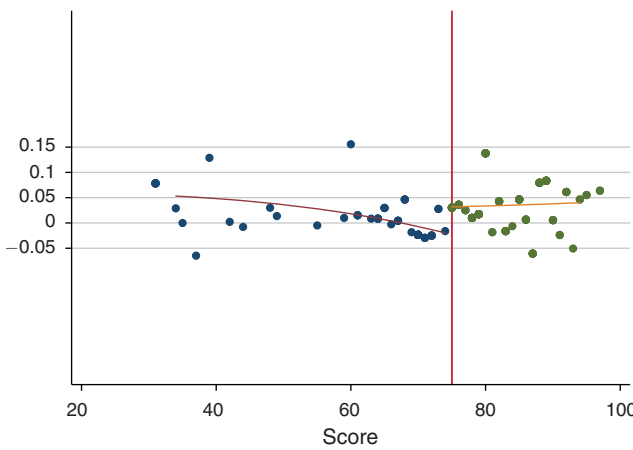

FIGURE 3. LARGE FIRMS

Panel A. Total investment/pre-program sales

Panel B. Tangible investment/pre-program sales

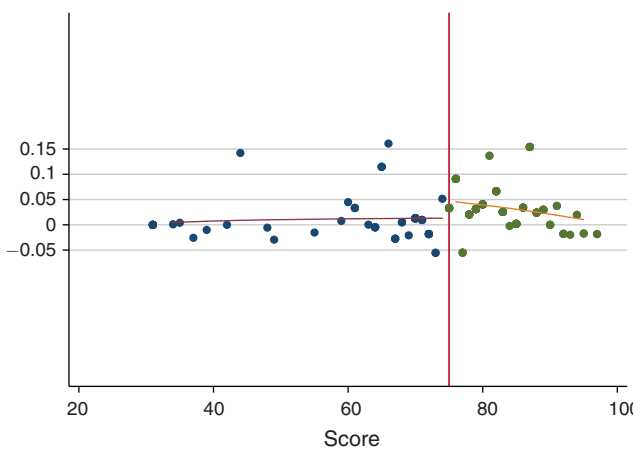Panel C. Intangible investment/pre-program sales

FIGURE 4. SMALL FIRMS

TABLE 5—EFFECT OF THE PROGRAM ON INVESTMENT BY FIRM SIZE

| | Model (2) | | | | | |
| | Total investment/pre-program sales | | | Tangible investment/pre-program sales | | |
Order of polynomial	Small	Large	AIC	Small	Large	AIC
Panel A. Full sample						
0	0.045**	−0.021	−607.2	0.022	−0.009	−709.2
	(0.018)	(0.020)		(0.015)	(0.017)	
1	0.080***	−0.012	−603.6	0.045**	−0.008	−706.9
	(0.026)	(0.028)		(0.017)	(0.023)	
2	0.099***	−0.010	−597.2	0.053***	−0.010	−699.2
	(0.029)	(0.041)		(0.019)	(0.033)	
3	0.149***	−0.030	−594.6	0.081***	−0.031	−694.5
	(0.037)	(0.063)		(0.024)	(0.051)	
Panel B. Local estimates: wide-window sample						
0	0.064**	−0.014	−279.5	0.042*	−0.006	−353.6
	(0.028)	(0.023)		(0.021)	(0.020)	
1	0.089**	0.008	−277.2	0.041*	0.011	−352.9
	(0.033)	(0.040)		(0.021)	(0.030)	
2	0.178***	0.031	−279.1	0.084***	−0.011	−353.9
	(0.052)	(0.080)		(0.027)	(0.063)	
Panel C. Local estimates: narrow-window sample						
0	0.066**	−0.002	−200.5	0.035***	0.007	−268.2
	(0.025)	(0.028)		(0.011)	(0.023)	
1	0.142***	−0.013	−198.8	0.066**	−0.045	−266.2
	(0.043)	(0.061)		(0.021)	(0.048)	
2	0.053	−0.228**	−201.9	0.015	−0.163**	−266.4
	(0.046)	(0.080)		(0.037)	(0.070)	
Mean (SD) for untreated firms	0.022	0.047		0.012	0.033	
(full sample)	(0.104)	(0.112)		(0.076)	(0.094)	

(*Continued*)

In Table 5 we show the results of the estimates of model (2) for total, tangible, and intangible 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 (panels B and C). Only in the smallest sample when we used the quadratic model, the parameter turns out to be statistically nonsignificant, arguably because of the loss of efficiency. By contrast, for large firms we find mainly negative but nonstatistically 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 proved mostly complementary.

For small firms the effect of the program seems nothing short of 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 percent of its standard deviation. Even if this

TABLE 5—EFFECT OF THE PROGRAM ON INVESTMENT BY FIRM SIZE (*Continued*)

| | Model (2) | | | Model (3) | | |
| | Intangible investment/ pre-program sales | | | Total investment | | |
Order of polynomial	Small	Large	AIC	Small	Large	t-test of $\beta_{small}=1$
Panel A. Full sample						
0	0.022*	−0.012	−992.4	0.972*	0.442	0.05
	(0.011)	(0.008)		(0.518)	(9.973)	
1	0.035**	−0.003	−988.1	1.720**	−6.811	1.05
	(0.017)	(0.011)		(0.687)	(13.154)	
2	0.045*	−0.000	−985.1	1.108	−5.575	0.14
	(0.023)	(0.015)		(0.785)	(16.569)	
3	0.068**	0.001	−979.4	1.274	−3.395	0.34
	(0.031)	(0.032)		(0.804)	(21.208)	
Panel B. Local estimates: wide-window sample						
0	0.022	−0.007	−465.0	1.826**	3.487	1.02
	(0.015)	(0.009)		(0.808)	(7.458)	
1	0.048*	−0.003	−459.0	1.810*	13.671	0.92
	(0.025)	(0.015)		(0.883)	(14.494)	
2	0.093**	0.041	−457.1	2.615	18.364	0.99
	(0.039)	(0.030)		(1.639)	(20.013)	
Panel C. Local estimates: narrow-window sample						
0	0.031	−0.009	−306.2	1.369*	8.492	0.56
	(0.020)	(0.010)		(0.656)	(8.084)	
1	0.076*	0.032	−304.1	2.289*	22.324	1.16
	(0.041)	(0.025)		(1.110)	(21.309)	
2	0.037	−0.064	−303.6	−1.331	19.860	1.32
	(0.070)	(0.038)		(1.761)	(27.049)	
Mean (SD) for untreated firms (full sample)	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. Investments are accumulated over the first three 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 fifth and ninety-fifth percentile of the distribution of the Total investment/pre-program sales ratio (calculated over the full sample). Robust standard errors clustered by score are in parentheses. The number of observations (firms) is 357 in panel A, 171 in panel B, and 115 in panel C.

   ***Significant at the 1 percent level.
    **Significant at the 5 percent level.
     *Significant at the 10 percent level.

seems like an exceptional increase, we have to take into account that the grants were substantial (averaging €173,000 for the small firms) especially when compared to the investment of untreated firms (€107,000 on average).

We replicated the estimates with labor and service costs as outcome variables. Table B4 in the online Appendix shows neither cost changed because of the program, either for small firms or for large ones. Finally, we verified the effects of the incentives also on the (log of) employment and wages. The results are displayed in Table B2 (in the online Appendix). Although they appear to be sensitive to the model used, overall we can conclude that not even employment or wages changed

thanks to the program; the results for employment are slightly more favorable for small firms, whereas the opposite occurs for wages.[22]

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

$$(3) \quad INV_i = (1 - T_i) \sum_{k=1}^{2} \alpha_k Size_i^k + GRANT_i \sum_{k=1}^{2} \beta_k Size_i^k$$

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

A coefficient β_k positive and smaller (larger) than 1 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 1 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. For small firms we find a parameter very close to 1 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 1 is largely accepted by *t*-tests (calculated with robust standard errors clustered by score) in all models. Therefore, it seems that small firms increased their total investment outlays after, and owing to, the program by exactly the same amount as the grant received. On the other hand, grants to larger firms completely displaced private expenditure.[23]

C. *Why Was the Program Effective for Smaller Firms?*[24]

We found that the program proved effective for small firms but not for large ones. The conclusion is in line with previous econometric analyses (Lach 2002; González, Jaumandreu, and Pazó 2005) and consistent with the available survey-based evidence.[25]

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[22] We extended the analysis by investigating how firms in the services sector reacted to the subsidies, thus we reestimated model (1) and (2) only for participating firms belonging to the services sector (by now excluded from the analysis). 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 firm size we find that the impact is again positive and mostly significant for small firms, while it is negative and only rarely statistically significant for large ones. The results are shown in Table B5 of the online Appendix.

[23] We verified whether the policy affected other measures of firm performance such as productivity (value-added/employment), profitability (ROA), value-added, and cost of debt (financial costs/debts). The results indicate that the incentives did not change the economic performance of small or large firms. One possible reason for this is that the length of the time period examined after the policy could be too short to observe some changes (two years for the second round and three years for the first round). For the sake of brevity we have chosen not to show the results of these exercises, but they are available upon request.

[24] This section was created thanks to the insightful comments and suggestions of an anonymous referee, who we would like to thank. One might argue that finding a significant impact of the policy for small firms is more likely simply because for them the subsidy is larger relative to the firm size. However, in our case this argument is not empirically grounded. We tested this claim by estimating the impact of the policy on large firms after assuming that their investments responded to the subsidy in the same way as for small firms (i.e., large firms' investments increase by the same amount of the grant), finding a positive and also statistically significant effect of the policy.

[25] For example, a recent survey conducted by the Bank of Italy on a representative sample of Italian firms with at least 50 employees shows that smaller industrial enterprises are more dependent on public incentives than large

One possible reason for the heterogeneous effect of the subsidy between small and large firms might be linked to a specific characteristic of the program. Indeed, for certain projects the program assigned a larger grant to smaller- and medium-sized firms (10 percent more) than for large firms. To test for this hypothesis we investigate the role played by the ratio of the grant to the cost of the subsidized project, i.e., what we call the coverage ratio. It is possible that for firms with a high coverage ratio that subsidies could be more effective than for those with a small coverage ratio. If the firms with higher coverage ratios are the smaller ones, this may help to explain why we found the program effective for them. Note that the exercise is also interesting per se, to explore if the amount of the grant affects the impact of the subsidy.[26] To test for the role of the coverage ratio we estimate the following model:

$$(4) \quad Y_i = (1 - T_i) \sum_{j=1}^{2} \alpha_j Intens_i^j + T_i \sum_{j=1}^{2} \beta_j Intens_i^j$$

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

where $Intens^1 = High$ and $Intens^2 = Low$ denote the intensity of the grant, i.e., the coverage ratio; $High$ (Low) is a dummy variable equal to 1 if the grant/cost of the project ratio of firm 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 coverage ratio. 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 twenty-fifth and seventy-fifth percentiles are equal to 0.38 and 0.43, respectively). As a consequence, firms above and below the median turn out to be mostly homogeneous in terms of the coverage ratio. The latter is greater for small firms than for large ones, but the gap is very narrow: the mean is equal to 0.41 for small and 0.40 for large firms (median values are very close to the means). Therefore, at first glance it seems that the coverage ratio could have only played a marginal role.

The estimates of model (4) are reported in the first columns of Table 6. We find that the coefficients are almost always positive for firms with both low- and high-grant intensity, 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 six out of ten models estimated on total investment the null of equality of the coefficients for a low- and high-grant coverage ratio is accepted by a standard F-test. As a consequence, on the whole we are inclined to believe that the

firms. Almost two-thirds of smaller firms receiving any type of R&D grants (with 50–100 employees) answered that they would have made less or zero R&D investment without public incentives, while the same share drops to around one-third for firms with more than 200 employees. See Bank of Italy (2011).

[26] 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 increase was not significant. For the same program, Adorno, Bernini, and Pellegrini (2007) showed that the level of the subsidies had a nonlinear impact on investment: up to a certain level subsidies grew along with investment, but after a certain point the relation reversed.

TABLE 6—EFFECT ON DIFFERENT CATEGORIES OF FIRMS, OUTCOME VARIABLE:
TOTAL INVESTMENT/PRE-PROGRAM SALES

Firm's coverage ratio [1]			Age [2]			Financial vulnerability [2]		
Order of polynomial [AIC]	Low	High	Order of polynomial [AIC]	Younger	Older	Order of polynomial [AIC]	Fragile	Healthy
Panel A. Full sample								
0	0.012	0.012	0	0.021	0.004	0	0.014	0.012
[−597.1]	(0.014)	(0.013)	[−489.4]	(0.017)	(0.019)	[−532.4]	(0.016)	(0.020)
1	0.046**	0.033	1	0.051**	0.031	1	0.064***	0.010
[−595.4]	(0.021)	(0.022)	[−485.9]	(0.024)	(0.031)	[−532.4]	(0.024)	(0.027)
2	0.061**	0.029	2	0.077**	−0.014	2	0.067*	0.004
[−591.3]	(0.028)	(0.033)	[−482.8]	(0.029)	(0.046)	[−530.1]	(0.037)	(0.047)
3	0.076*	0.052	3	0.112***	0.017	3	0.065	0.062
[−586.2]	(0.039)	(0.044)	[−476.5]	(0.037)	(0.075)	[−525.4]	(0.053)	(0.070)
Panel B. Local estimates: wide-window sample								
0	0.036	0.015	0	0.044**	0.015	0	0.043*	0.004
[−275.9]	(0.021)	(0.019)	[−227.9]	(0.021)	(0.029)	[−258.2]	(0.023)	(0.028)
1	0.042	0.038	1	0.067**	0.007	1	0.052	0.013
[−270.9]	(0.034)	(0.037)	[−221.7]	(0.032)	(0.055)	[−253.8]	(0.044)	(0.051)
2	0.139**	0.090*	2	0.117***	0.067	2	0.131*	0.020
[−272.9]	(0.050)	(0.050)	[−219.1]	(0.037)	(0.097)	[−256.2]	(0.064)	(0.064)
Panel C. Local estimates: narrow-window sample								
0	0.043**	0.021	0	0.049*	0.018	0	0.048	−0.004
[−199.0]	(0.019)	(0.026)	[−160.7]	(0.023)	(0.032)	[−191.9]	(0.030)	(0.019)
1	0.074**	0.059	1	0.076*	0.004	1	0.084	−0.029
[−197.6]	(0.033)	(0.047)	[−156.6]	(0.036)	(0.069)	[−189.9]	(0.053)	(0.046)
2	−0.053	−0.092**	2	0.003	−0.174**	2	−0.035	−0.216***
[−201.4]	(0.036)	(0.036)	[−156.2]	(0.040)	(0.069)	[−190.1]	(0.045)	(0.086)

Notes:
[1] The columns show the estimates of coefficients β_j of equation (4). Coverage ratio = Grant/Cost of subsidized project. High (Low) identifies firms that are above (below) the median of the distribution of the coverage ratio. The number of observations (firms) is 357 in panel A, 171 in panel B, and 115 in panel C. See, also, the notes to Tables 3 and 5.

[2] The columns show the estimates of coefficients β_j of equation (3) with different sample splits (age and financial vulnerability). In the sample split by age, the number of observations (firms) is 300 in panel A, 147 in panel B, and 96 in panel C; in the sample split by financial vulnerability the number of observations (firms) is 322 in panel A, 159 in panel B, and 108 in panel C.

AIC is Akaike Information Criterion. Investments are accumulated over the first three 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 fifth and ninety-fifth percentile of the distribution of the Total investment/Pre-program sales ratio (calculated over the full sample). Robust standard errors clustered by score are in parentheses. For more details see the text.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

intensity of the grant did not play a significant role in the program examined, nor does it explain the effectiveness of subsidies for small firms.

Another reason underlying the more important role of subsidies for small firms could be that they may have more difficulty accessing credit markets and face higher capital costs. An established body of literature concludes that firm size and the cost of finance are negatively correlated. For instance, by estimating a structural model Hennessy and Whited (2007) find that for small enterprises the cost of external funds

is substantially higher than for large ones. In their comprehensive study on financing innovation, Hall and Lerner (2009) conclude that there is clear evidence that small firms in R&D intensive industries face higher costs of capital than their larger competitors. If small innovative firms are subject to stronger information asymmetries and experience more intense financial frictions, their projects may run a greater risk of being unprofitable when privately financed (*marginal projects*), and the financial channel could be the reason why the program was effective only for them.

In the remainder of this section we attempt to shed more light on this issue. We verify whether the subsidies have been more effective for other types of firm that we would expect to experience stronger financial frictions, namely younger or more financially vulnerable firms. If we find that the incentive had a stronger impact on the investment of these enterprises, we interpret our results as a sign of the importance of the financial channel.

First of all, we break down the sample according to firm age. Along with size, age is considered a key factor in access to and the cost of financing. Younger firms are more exposed to information problems because the reputation of the company and the management is less well established. Moreover, young firms are perceived as being riskier than mature enterprises because bankruptcies are more frequent for them. This can aggravate asymmetric information and adverse selection problems thereby deepening financial frictions. In addition, young firms have had less time in which to accumulate earlier profits to finance investment with internal funds, and to develop strong relationships with banks that might mitigate problems of asymmetric information. Empirical support of this view comes, for example, from Brown, Fazzari, and Petersen (2009) who found that financial frictions affected R&D investment of younger firms in the United States, but did not affect those of more mature enterprises.

In our sample, information on firm age (year of establishment) is drawn by balance-sheet data and is available only for 300 out of 357 firms in our regression sample. To conduct the exercise we split the sample between *younger* and *older* firms according to the median foundation year (1987). In Table 6 we present the results of the estimation of model (2) where dummies for size are substituted by dummies for age defined as follows: *younger* = 1 if the foundation year is above the median and equal to zero otherwise; *older* = 1 if the foundation year is below the median and zero otherwise. Of course, size and age are positively correlated, but in our sample a significant share of large firms (35 percent) falls in the sample of younger firms. Thus, the exercise is not a mere replication of the previous one on size.

Our econometric results are in line with our expectations. There is strong evidence that for younger firms the program was effective whereas for older ones it was not. The coefficient for young firms is positive and almost always statistically significant, whereas for older firms the size of the impact is much smaller and never statistically significant.

In the second exercise we break down the sample according to firms' financial vulnerability. We measure the vulnerability by using the amount of short-term credit actually drawn by enterprises from banks, relative to the short-term credit granted by banks. Short-term credits include mainly banking current account overdrafts together with other short-term banking loans. Banks charge firms that utilize

short-time lines of credit a penalty interest rate: consequently, we envisage that only firms facing financial tensions, and which are therefore more financially vulnerable, are willing to use them. The source of data is the Italian Central Register, held by the Bank of Italy, which gathers detailed information on bank loans granted above the threshold of €75,000. Short-term bank credit is calculated the year before the program was launched. The econometric model is analogous to model (2). We split the sample between *financially fragile* firms—those that made use of this line of credit—and *financially healthier* firms—those that did not. Out of 357 firms in our baseline sample, we find the information for 322 enterprises, of which 89 are defined as financially fragile.[27]

The results are displayed in the last columns of Table 6. The coefficients are usually positive and much larger for fragile than for healthy enterprises. They are also statistically significant in the models preferred by the AIC in the full and wide-window sample. However, overall the parameters are less precisely estimated and sometimes statistically insignificant.[28]

On the whole, the empirical evidence suggests that the incentives were more effective for firms that in principle might be more exposed to financial frictions, but the results of the last exercises are less clear-cut than those for size or age. Given the overall outcomes we are inclined to believe that asymmetric information and adverse selection problems in the financial market, affecting access to capital markets and the cost of capital, have played a significant role in explaining the heterogeneous effect of the policy across firms. Yet, we cannot exclude that other factors might have contributed, and that the financial channel could not be the only explanation.

V. Robustness

In this 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 models (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 two-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), returns on assets (ROA), and total assets (measures of size). All variables refer to the pre-treatment period. The results shown in the online Appendix (Table B6) are remarkably similar

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[27] The results are similar if we split the sample according to the median value of the ratio between the amount of short-time credit drawn and the short-time credit granted. We believe that this measure is well-suited to capture the financial vulnerability of a firm even though it cannot be considered—as the others used in the literature—totally problem-free, e.g., because it treats (long-term) solvency problems and (short-term) liquidity problems similarly.

[28] Similar results are obtained using as index of financial vulnerability the credit *z*-score (a synthetic measure of the risk of corporate failure calculated mainly on balance sheet data).

to the baseline ones. The coefficients turn out to be close in magnitude to those previously estimated and highly statistically significant for small firms.

As a supplementary robustness exercise we use, as further outcome variables, nonnormalized investment and the log of investment. The results shown in Table B7 in the online Appendix confirm the baseline outcomes. Next we use as further normalization the pre-program capital and replicate the estimates of model (2) by dividing the sample into small and large firms. Also in this case the findings are largely analogous to those obtained with the benchmark outcome variable: the effect is limited to small firms (see Table B8 in the online Appendix). The main results also hold if we winsorize the sample (at 5 percent of the investment over sales distribution) or trim the sample at 2 percent (instead of trimming at 5 percent). Given the lower precision of the estimates, however, trimming at 2 percent produces coefficients that are less statistically significant but similar in size. For the sake of brevity the results are not shown but available upon request.

We further verify whether our main results depend on the estimation methods. To this end, we run kernel regressions of models (1) and (2) using the Epanechnikov kernel, several polynomials, and different bandwidths: 30 and 15 points of the score (below and above the threshold). The results shown in Tables B9 and B10 in the online 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 obtain similar findings.

RD identification strategy relies on the continuity assumption, which requires that potential outcome should be smooth around the cutoff 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 cutoff. If we do not observe jumps it is plausible that the outcome variable would also have been continuous without the treatment. The exercise is run using the following observables that could, in principle, be correlated with investment: profitability (ROA), net assets over debts, 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 find extremely few significant discontinuities (see Table B11 in the online Appendix).

Another indirect way to test for the continuity assumption is to verify whether the outcome variable before the program was smooth across the cutoff. 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 investment in the period before the program as our outcome variable. Note 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 B12 (panel 1) in the online Appendix and Figure 5 show that before the program there were no jumps in investment of small firms.

Finally, we check whether there are discontinuities of investment at score values other than the cutoff point. If the jump of the function is unique at the point that

Panel A. Total investment/pre-program sales

Panel B. Tangible investment/pre-program sales
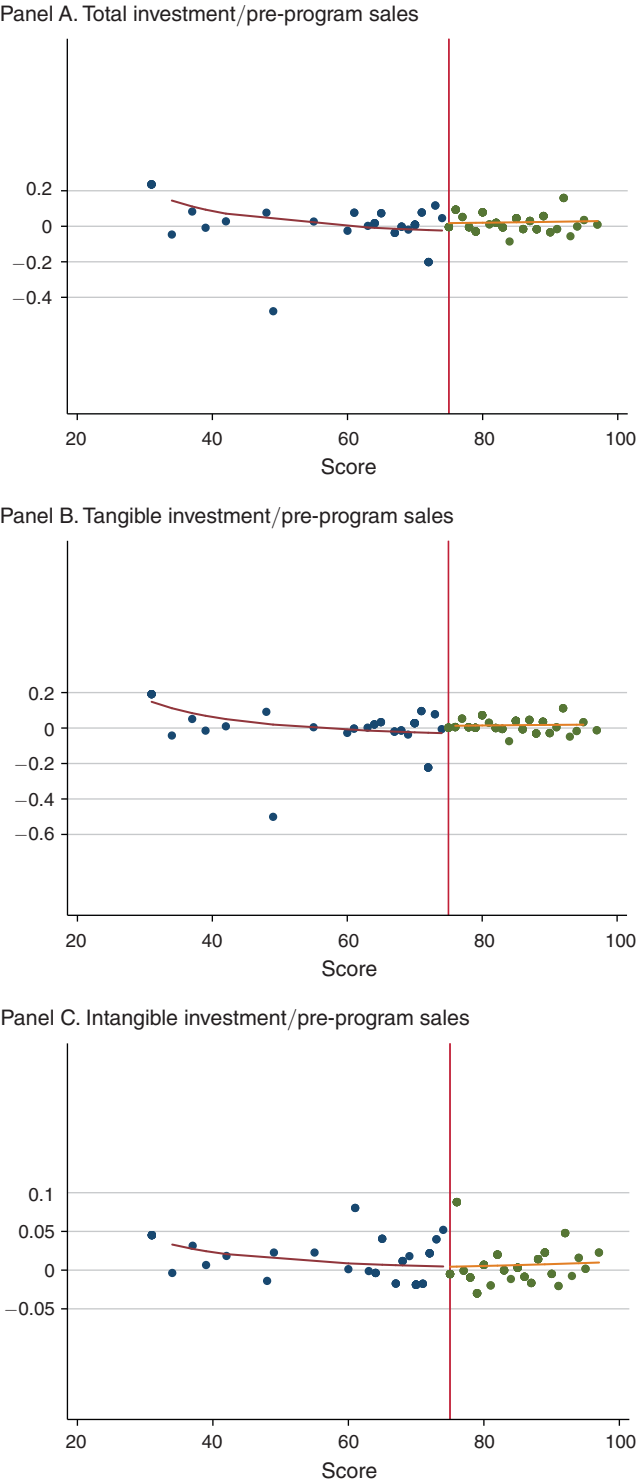
Panel C. Intangible investment/pre-program sales

FIGURE 5. SMALL FIRMS: PRE-PROGRAM INVESTMENT

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 (2010). We estimate the baseline model (2) adding a complete set of score dummies variables interacted with the small-firm 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 hypothesis, we can conclude that there are no other jumps of the investment: the only one is at the threshold. Panel 2 of Table B12 (in the online Appendix) reports the values of the F-test of this exercise. From the table it is evident that no other discontinuities are detected.

VI. 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 in a region of northern Italy. Using a sharp regression discontinuity strategy we find that on the whole 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 on average matched the subsidy received. Overall, our results are in line with those of Lach (2002); González, Jaumandreu, and Pazó (2005); and recently by Criscuolo et al. (2012), who assessed a major program that supported business investment in the United Kingdom. All of them found positive effects of the incentives mainly for small firms.

We argue that the effect of the program is greater on small firms because they are more exposed to financial frictions. To test for this hypothesis we verify whether the impact of the program also proved stronger for other categories of firm that in principle might have less or more costly access to capital market, because of stronger asymmetric information and adverse selection problems, namely younger and more financially vulnerable firms. The overall evidence suggests that the financial channel plays a substantial role in explaining the heterogeneous effect of the policy across firms; however, the results cannot allow us to exclude that other factors might have contributed. As a result, we believe that more work should be done to better understand all the forces driving the heterogeneous effect of subsidies.

We analyzed the direct effects of the policy on the main target variables. Of course, there are other interesting issues that we did not address but that nonetheless merit attention. One is the long-term effect of the grants on the economic performance of recipient firms. Then there are the indirect effects of the program. Of these, 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 investigate whether spillovers are localized. To understand these effects would be highly rewarding, albeit empirically challenging.

APPENDIX

FIGURE A1. MAP OF ITALY WITH THE AREA COVERED BY THE POLICY SHADED

TABLE A1—PAPERS ON FIRMS 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 for smaller grants only
Merito et al. (2007)	Italy	Employment, sales, productivity	Matching	No effect
Hussinger (2008)	Germany	Investment	Two-step selection models	Positive effect
Takalo et al. (2013)	Finland	Welfare	Structural model	Positive effect

Note: The table reports the published articles that examined the effect of firms' subsidies for R&D; those evaluating the impact of tax incentives are not included.

TABLE A2—PRE-ASSIGNMENT MEANS (*Standard deviation in brackets*)

Variable	Full sample		50 percent cutoff neighborhood sample (score 52–80)		35 percent cutoff neighborhood sample (score 66–78)	
	Untreated	Treated	Untreated	Treated	Untreated	Treated
Sales	21,269 (37,035)	65,963 (20,5961)	23,023 (39,068)	27,013 (57,067)	22,356 (38,963)	30,535 (66,293)
Value-added	5,534 (9,435)	15,605 (47,530)	5,980 (10,108)	7,308 (15,833)	6,165 (10,196)	8,054 (18,492)
Assets	20,510 (39,202)	59,664 (176,488)	21,033 (35,458)	26,726 (61,530)	21,848 (36,427)	29,640 (70,305)
Return on assets	6.38 (9.87)	7.27 (10.18)	6.25 (10.41)	6.75 (8.12)	4.92 (8.85)	6.34 (5.10)
Own capital/Debts	0.530 (0.911)	0.467 (0.604)	0.586 (0.994)	0.374 (0.428)	0.613 (1.081)	0.380 (0.394)
Gross operating margin/Sales	0.084 (0.096)	0.095 (0.077)	0.087 (0.101)	0.088 (0.069)	0.085 (0.092)	0.082 (0.051)
Cash flow/Sales	0.059 (0.077)	0.078 (0.076)	0.062 (0.081)	0.072 (0.062)	0.059 (0.083)	0.072 (0.055)
Financial costs/Debts	0.029 (0.065)	0.024 (0.016)	0.031 (0.073)	0.025 (0.017)	0.032 (0.086)	0.026 (0.019)
Labor costs/Sales	0.208 (0.101)	0.199 (0.087)	0.208 (0.104)	0.211 (0.088)	0.222 (0.109)	0.205 (0.095)
Service costs/Sales	0.287 (0.133)	0.275 (0.116)	0.273 (0.121)	0.288 (0.107)	0.264 (0.116)	0.292 (0.108)
Total investment/Sales	0.004 (0.107)	0.008 (0.070)	−0.002 (0.117)	0.007 (0.076)	−0.006 (0.126)	0.018 (0.075)
Tangible investment/Sales	−0.009 (0.117)	0.004 (0.050)	−0.015 (0.128)	0.004 (0.062)	−0.020 (0.144)	0.013 (0.058)
Intangible investment/Sales	0.013 (0.064)	0.004 (0.046)	0.014 (0.074)	0.002 (0.034)	0.014 (0.084)	0.005 (0.039)

Notes: Only manufacturing and construction firms. All the variables refer to the first pre-assignment year (2003 for the first round and 2004 for the second). In the complete sample 254 firms are treated, and 103 are untreated. In the 50 percent cutoff neighborhood sample there are 90 treated and 81 untreated firms; in the 35 percent cutoff neighborhood sample there are 57 treated and 58 untreated firms. Investments are calculated as the difference between (tangible and intangible) assets in two consecutive years.

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