Evaluating the impact of innovation incentives: Evidence from an unexpected shortage of funds

Guido de Blasio,[◊] Davide Fantino[•] and Guido Pellegrini[♦]

Abstract

To evaluate the effect of an R&D subsidy one needs to know what the subsidized firms would have done without the incentive. This paper studies an Italian programme of subsidies for the applied development of innovations, exploiting a discontinuity in programme financing due to an unexpected shortage of public money. To identify the effect of the programme, the study implements a regression discontinuity design and compares firms that applied before and after the shortage occurred. The results indicate that the programme was not effective in stimulating innovative investment.
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[◊] Bank of Italy, Structural Economic Analysis Department.
[•] Bank of Italy, Economic Outlook and Monetary Policy Department.
[♦] University of Rome "La Sapienza".

1. Introduction[1]

Stimulation of innovation activity is considered a very important task for policymakers.[2] The theoretical case for public intervention to stimulate R&D is clear. As the social returns from innovation are usually greater than the private ones, private firms allocate fewer resources to it than is required by the social optimum. Notwithstanding this sound rationale, past empirical literature shows that the evidence on the effectiveness of subsidies is highly controversial. This is partly due to the intrinsic difficulty of the evaluation exercise, which requires addressing the question of what would have happened without the subsidies.

What would the firm have spent on R&D had it not received the subsidy? On one hand, a financially *constrained* firm would have spent less money. Since such a firm cannot finance the whole project without the subsidy, which lowers the private share of financing, the project would not have been implemented.[3] On the other hand, a financially *unconstrained* firm would have spent a greater amount of private money had it not received the subsidy. As such a firm can finance the project from external or internal funds, the subsidy is in fact superfluous (it crowds out private R&D expenditures). Since the cost of the subsidy for the firm is substantially lower than that of alternative sources of financing, both constrained and unconstrained firms will ask for the subsidy. Given the difficulty of making a subsidy conditional on a firm's being constrained, there is no guarantee of the programme's effectiveness, which can only be evaluated ex post.

In theory, the best way to evaluate effectiveness is randomization: the agency in charge of the programme identifies a group of "potential beneficiaries" and randomly awards subsidies within this group, so the probability of receiving the subsidy is the same for of its members. In fact, however, randomization schemes are almost never implemented, and so to evaluate effectiveness researchers have to rely on identification strategies that aim at reproducing the fundamental feature of a random design as closely as possible (see the vast literature on programme evaluation; among others, Lee and Lemieux, 2010, Imbens and Rubin, forthcoming, Banerjee and Duflo, 2009, Blundell and Costa Dias, 2000).[4] As underscored by Duflo et al. (2007), exogenous events sometimes result in elements of randomization being introduced into programmes not originally designed as random schemes. This study elaborates on one of these circumstances.

We evaluate an Italian programme of subsidies for technological innovation (Fund for Technological Innovation; FTI or Fund, hereafter), which was *not* designed through a random scheme. The FTI allocated its

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[2] This is particularly true in Italy, where there is less private spending on R&D than in other advanced countries (in 2000, it amounted to 0.5 per cent of GDP).

[3] Compared with more traditional investments, there may be a greater likelihood that profitable R&D projects will not find adequate financing, owing to their higher variance of success probabilities. This could make their returns harder to identify, particularly for non-specialist external financiers such as commercial banks.

[4] Admittedly, it is not easy to understand why this is the case. For instance, Jaffe (2002, p. 28) writes. "I remain personally puzzled as to why it is okay to randomize when people's lives are the stake (drug trials), but not when research money is at the stake."

subsidies among applicant firms on the basis of a technical committee's selection of the most promising research proposals. At a certain point, however, the *financing* of the programme was unexpectedly interrupted by a shortage of public money. Firms were still allowed to apply for the subsidies, because it was hoped that the problems would soon be resolved. As things turned out, applications were suspended after ten months but it took five years to reinstate funding. Our strategy will basically compare firms that received the subsidies before the shortage with firms that applied in the subsequent 10-month period and were not assessed by the committee for five years. Thus, we compare firms that passed the assessment (treated group) with firms that did not go through the assessment process because of the suspension (control group). We argue that being in one group or the other was essentially a matter of luck, and that this was particularly true for firms that applied around the day financing was interrupted. The unexpected shortage of funds allows us to empirically implement a regression discontinuity design (RDD) that uses the date of the application as the *forcing variable* and the day the shortage occurred as the *cutoff*.

As for results, we find no evidence of the programme's effectiveness. The subsidized firms do not invest more in either tangible or intangible assets than the firms whose assessment was suspended. Therefore, in the experience of the FTI public funding simply substitutes for private funding. This result is highly robust to a number of sensitivity checks. While the effects of the programme on the sales, profitability and financial conditions of the firms are also negligible, we find a positive impact on the overall size of the balance sheet, which suggests that money saved on R&D was spent on alternative assets. Finally, the programme's effectiveness is not greater for firms that typically have a higher likelihood of being rationed by private lenders (for example, small and medium-sized enterprises or firms with high borrowing costs).

The rest of the paper is organized as follows. Section 2 examines the past literature on the topic. Section 3 describes the characteristics of the policy measure, Section 4 describes the data and sketches the methodology, Section 5 reports the results, and Section 6 concludes.

2. Literature review

The main reasons why private management of R&D may be socially inefficient are discussed by Arrow (1962): the external acquisition of knowledge is not always regulated by market mechanisms and agents cannot prevent observation and interaction from other agents, a phenomenon known as spillovers from knowledge in the literature; the social returns from innovation are therefore usually greater than the private ones and the resources allocated by agents to innovate are smaller than the socially optimal amount. Public subsidies therefore allow to reduce the gap between private and social returns.

Hall (2002) reviews the most important contributions about the effects of financial constraints on the financing of innovations. R&D investments are risky and subject to asymmetric information between firms and lenders; a higher interest rate than that equating demand and supply of credit can help lenders to discriminate between good and bad projects, at the social cost of a suboptimal overall credit amount. Public intervention through a concessional loan can loosen the financial constraints.

The empirical evidence about the efficacy of R&D subsidies has been widely discussed.Results are mixed. David, et al. (2000) examine the results of forty years of empirical studies and find that there is no conclusive evidence in favour of public support. In the analysis of the Small Business Innovation Research program in the U.S., Wallsten (2000) finds that public grants displace firm expenditures dollar for dollar. Lach (2002), on a panel of Israeli firms, shows that subsidies have been effective for small firms, while the policy had a negative effect on large firms. Gonzalez et al. (2005) in analysing Spanish data find that only a small subset of firms would not have undertaken R&D activity in the absence of the subsidy, while there is no evidence of crowding out among the innovation active firms. Gorg and Strobl (2007), using an Irish sample of firms, conclude that public subsidies replace private R&D expenditure when the award is substantial. Czarnitzki et al. (2007) find a positive effect of cooperation on the effectiveness of subsidies in a panel of firms from Germany and Finland.

In contrast with such a wide range of international empirical literature, very few studies examine the efficacy of Italian R&D policies, even if the number of interventions and the amount of public resources involved have been relevant in the last decades. Merito et al. (2008) evaluate the efficacy of the subsidies awarded in 2000 by the Special Fund for Applied Research of the Ministry of University and Research, introduced with the aim of supporting the research component of industrial R&D; they find that four years after the award of the subsidy, the policy had had little effect on number of employees, sales, productivity, labour costs and patent applications. Fantino and Cannone (2010) examine the efficacy of two European regional programs aiming at supporting innovative activity of small and medium firms in Piedmont and find limited effectiveness. Bronzini and Iachini (2010) when considering another regional program in Emilia Romagna find a positive effect only for small firms.

3. The Fund for Technological Innovation

Main features. – The Fund for Technological Innovation was set up in 2001 with the mission of "stimulating the applied development of innovations through subsidies to the R&D activity of firms." It functions along lines similar to those of other commercial research grant programmes, widely implemented all over the world. A high-level executive agency (in our case, the Ministry for Economic Development) is in charge, setting the rules of the game. Firms apply by submitting substantive R&D proposals.[5] A technical committee organized by the agency meets, reviews the proposals and provides a judgment on their merits. On the basis of this judgment, the Ministry decides whether to grant the subsidy. If a proposal is rejected, the Ministry explains the reasons adduced by the committee. There is no deadline for applications, which are evaluated one by one in chronological order of receipt.

The Fund focuses on applied innovations. While an R&D proposal may have both a research component and a development component, the Fund's area of responsibility only refers to the latter.[6] If the research

[5] The applications are preliminarily vetted by private banks, which have to express an opinion on the economic and financial soundness of the applicant firm and the project. This preliminary report is sent to the Ministry, along with the R&D proposal. The technical committee reviews both the proposal and the preliminary report to express its judgment. The bank assessment is quite uninformative. According to a Ministry official involved in implementing the FTI, the bank assessment is almost always highly favourable.

[6] According to OECD (2002), "research is experimental or theoretical work undertaken primarily to acquire new knowledge of the underlying foundation of phenomena and observable facts. Development is systematic work, drawing

costs are preponderant, the application is not handled by the Ministry for Economic Development but by the Ministry for Universities and Research.[7]

The overall amount of subsidy is equal to the upper bound allowed by the European Union (EU), which is 50 per cent for research costs and 25 per cent for development costs.[8] As the projects managed by the Ministry have development costs equal to at least a 50.1 per cent share of total costs, they receive a subsidy of between 25 and 37.5 per cent. The subsidy can be augmented up to an additional 25 per cent overall in the following cases: small and medium-sized enterprises,[9] firms located in underdeveloped areas (defined according to EU regional policy objectives 1 and 2), projects included in the objectives of the EU Research Framework Programmes, and projects carried out in cooperation with other firms or public research organizations. The stated cost of the project can include expenditures for labour, machinery, consulting, overheads and consumption costs, feasibility studies and research centre organization. The investment must begin between 12 months before and 6 months after the date of the application. The project must be completed between 18 and 48 months after the date of the application, although firms may request a 12-month extension.[10]

The unexpected shortage. – The Fund began operations on 27 October 2001 (see Figure 1) and ran smoothly through 17 March 2002. The next day the financing programme's was unexpectedly interrupted. The Ministry's no longer had current resources available, since the allocations were exhausted and the Treasury had not transferred funds to it. Nevertheless, the Ministry allowed firms to continue to apply for subsidies, in the hope that the financing problems could soon be resolved. But the public finance problems proved to be more severe and applications were suspended on 13 January 2003. Only at the end of 2007 was the Ministry in a position to reconsider the R&D proposals submitted to it between 18 March 2002 and 13 January 2003. At that time the applicant firms were notified that the Ministry was ready to start the committee's assessment process.

[Figure 1]

on existing knowledge gained from research and/or practical experience, which is directed to producing new materials, products or devices, to installing new processes, systems and services, or to improving substantially those already produced or installed".

[7] This ministry is in charge of the Fund for Support to Research. An evaluation of the effectiveness of subsidies for projects with a prevailing research component can be found in Merito et al. (2008).

[8] The subsidy is a combination of a concessionary loan covering 60 per cent of the subsidy at 1/5 the market rate of interest and a grant for the remainder. The loan amortization period cannot exceeded ten years, plus a grace period during the execution of the project.

[9] The Ministry uses the following criteria to identify small and medium-sized enterprises: the firm must have less than 250 employees, be independent and have total annual sales revenue of less than €40 million or total assets of less than €27 million in the last financial year.

[10] The subsidies are disbursed in tranches, the financial plan may include up to five installments. The payment is made within 60 days of the firm's certification of the expenses covered, but small and medium-sized enterprises may request upfront payment of the initial installments.

4. Data and methodology

Sources. – Our main data source is the Ministry for Economic Development's official archive of the FTI programme, which includes the 879 applications submitted until 17 March 2002 and funded and the 1,242 applications submitted after the cutoff date.[11] To ensure greater homogeneity we only consider manufacturing firms (around 80 per cent of the firms in the archive). The version of the archive released to us has records for: i) firms that applied between 27 October 2001 and 17 March 2002 and received the subsidy;[12] ii) all the firms that applied between 18 March 2002 and 13 January 2003, whose assessment was suspended. For each firm the archive includes the following information: name, address, tax number, amount of the planned R&D expenditure, amount of the subsidy (with a breakdown between grant and concessionary loan), the development component's share of total R&D expenditure. For the subsidized projects alone, it also makes available the project's starting date and completion date. We link the FTI archive with the Cerved datasets of financial statements to reconstruct an uninterrupted sample from 1999 to 2007.[13] In the linking procedure, firm identifier (tax number) misprints, the unavailability of balance-sheet data for the entire period, and standard data cleaning reduce the sample to 751 firms (329 of which subsidized).[14]

Outcome variable. – We do not have data on R&D expenditures. Thus, we have to use financial statement data to measure the outcome variable. An important issue is where R&D expenditures end up in the financial statements. An R&D project can include a number of items (labour and services costs, physical capital, intangible capital) that may be recorded in either the balance sheet or the profit and loss account, according to management's preference. Luckily for us, Italian accounting rules (see Pisoni et al., 2009) envisage that for the projects in which the development share of costs is preponderant, all the costs (even those with a single-year utility) have to be capitalized and stated in the balance sheet as tangible or intangible investments. Therefore, as our main outcome variable we select total investment (over initial capital) as recorded in the balance sheet.

Timing of investment. – We compare subsidized firms with firms that applied from 18 March 2002 on, when the assessment process was suspended. As explained above, FTI rules envisage that the project must start between 12 months before and 6 months after the application date and be completed between 18 and 48 month after from the application date (firms may request a one-year extension for completion). The Ministry's archive includes the actual timing of the subsidized investment. For the comparison, we need to figure out what the investment time profile of the suspended-assessment firms would have been had they decided to carry out the project anyway.

[11] The overall shares of applications from Northern, Central and South Italy are respectively 70, 25 and 5 per cent. The distribution is very similar for funded and not funded firms.

[12] Data on rejected firms were not released because of confidentiality. Italian privacy law envisages disclosure of the names of the firms that receive public money, but this does not apply to unsuccessful applicants. The Ministry feared that disclosing their names might be taken as signaling low quality of their investment prospects. As no assessment was conducted for the firms that applied between 18 March 2002 and 13 January 2003, the names of these firms were released without difficulty.

[13] The Cerved dataset does not include partnerships, but only companies; anyway, around 95 per cent of the firms of the initial sample of the Ministry were present in the Cerved dataset in at least one year and therefore this fact should not introduce any relevant bias in the composition of the final sample.

[14] To remove outliers, we dropped the firms in the 1st and the 99th percentile of the distribution of outcome variable. Firms involved in mergers and acquisitions during the period were also excluded.

[Figure 2]

If we had no data on the actual beginning and completion times, we could have inferred that subsidized firms could have started their project anytime between 27 October 2000 and 17 September 2002 and completed it between 27 April 2003 (for a project submitted on the earlier date and completed in 18 months) and 17 March 2007 (for a 48-month project submitted on the latest date and extended for additional 12 months; Figure 2). As we have data for the treated firms on the actual investment timing (Figure 3), we can check how closely the hypothetical timings approximate the actual timings. We find that the approximation is very close: all the subsidized firms began their projects by the end of 2002, while no project was initiated before 1 January 2001;[15] in the years 2003, 2004, 2005 an increasing share of firms completed the project. By the end of 2006 all firms had completed it.

Suppose now that the suspended-assessment firms carried out the project anyway. What should we have observed? For these firms the hypothetical investment time-line can be inferred in the same way as for the subsidized firms pretending we had no data. Accordingly, these firms would have started their project anytime between 18 March 2001 and 13 July 2003 and completed it anytime between 18 September 2003 (for a project submitted on the earliest date and completed in 18 months) and 13 January 2008 (for a 48-month project submitted on the latest date and then extended).

[Fig. 3]

As shown in Figure 2, there is a substantial overlap between the investment time pattern of the treated firms and that of the control firms, had they implemented the project anyway. For both, the projects should have been substantially completed by the end of 2007. By comparing cumulative investment over the period 2001-2007 between treated and suspended-assessment firms, we should therefore be able to detect whether the subsidy made some additional investment possible. Subsidized projects started before the counterfactual ones: at the end of 2006, when the bulk of the subsidized project had been completed, some counterfactual projects – if implemented – could have been in the process of being completed. In the empirical section we deal with the possibility that subsidized firms may have initiated their projects some months in advance of those in the control group by estimating the effect of the subsidies only for the firms that applied just a few days before or after the day when the unexpected shortage occurred.

Note, however, that the 2001-2007 comparison could be upwardly biased because of time substitution (see: Bronzini and de Blasio, 2006). This occurs if some subsidized firms moved up investment projects in order to take advantage of the incentives. As the suspended-assessment firms did not receive the incentive, they may have decided to implement their project according to the original timetable. The upward bias due to the potential of time substitution means that if we find a positive effect of the subsidies, our results must be deemed inconclusive.

[15] Although the Fund may consider projects already under way, only 5 per cent of the subsidized projects had been initiated before the application was submitted.

Missing data on rejected applicants. – Since we do not have data on firms that applied unsuccessfully before the day of the unexpected shortage, we will basically compare firms that received a positive assessment from the technical committee with firms that did not go through the committee's assessment.[16] As underscored by Lach (2002), government bureaucrats are under strong pressure to avoid the appearance of wasting public funds and therefore tend to fund projects with higher probabilities of success and clearly identifiable results. Indeed, it is very hard to believe that the committee's assessment was systematically biased in favour of projects of lower quality. Consequently, the R&D projects of the treated firms could be better than those of the control firms, which include both projects that would have been found valid and projects that would have been rejected. It could therefore be possible that unsubsidized lower-quality projects were not implemented. This implies that there are reasons to believe that our results may be upwardly biased. The upward bias due to the unavailability of data on rejected applicants means that if we find a positive effect of the subsidies, our results must be deemed inconclusive.

Regression discontinuity design. – To estimate whether the subsidies allowed investments to be made that otherwise would not have been undertaken, we implement a sharp regression discontinuity design (RDD). In this non-experimental setting the treatment is determined by whether an observed "forcing variable" exceeds a known cutoff point. We use the application date as the forcing variable and the day the shortage occurred as the cutoff. The main idea behind this research design (see Thistlewaite and Campbell, 1960; Angrist and Lavy, 1999; Black, 1999; and van der Klaauw, 2002) is that firms that applied just before the cutoff and those that applied just after it have a good degree of similarity. This strategy is deemed preferable to other non-experimental methods because (see: Lee, 2008) if the applicant firms are unable to precisely manipulate the forcing variable, the variation in treatment around the threshold is randomized as if the firms had been randomly drawn just below or just above the threshold. An implication of the local randomized result is that RDD can be tested like randomized experiments. If the variation in the treatment near the threshold is approximately randomized, then all "baseline covariates" – all the variables determined prior to the realization of the forcing variable – should have the same distribution just before and after the cutoff. To substantiate the empirical strategy, in the next section we show that it is extremely unlikely that applicants were able to manipulate the forcing variable and we present a test for the absence of discontinuity in baseline characteristics around the threshold.

5. Empirical evidence

Substantiating the identification strategy. – Lee and Lemieux (2010) explain that as long as firms exert some control over the forcing variable, albeit not a precise control, the conditions for the validity of an RDD are not violated. It is very hard to believe that our firms could have exerted precise control over the forcing variable. Some firms may have got wind of the possibility of an impact of financing problems on the functioning of the Fund, but the exact day on which the Ministry suspended the assessment was not known. Indeed, while the suspension covered applications received from 18 March onwards, it was not notified to the firms until 7 May. Furthermore, at the time of the notification the Ministry clearly conveyed the idea that the problems would

[16] According to a Ministry official involved in implementing the FTI, the percentage of unsuccessful firms was very low. The results provided in Tables 1 and 2 below lend support to this fact as the baseline covariates of the two groups of firms are balanced around the cutoff.

soon be resolved, since firms were still allowed to apply for the subsidies. Ten months passed before the Ministry closed this possibility. Very likely, firms realized at that point that their expectation of being assessed soon had been misplaced.[17] To corroborate the supposition that firms might have had (at most) an imprecise control over the forcing variable, Figure 4 illustrates the daily number of applications submitted by date. It is about 7 in the pre-shortage period and 4 in the post-shortage period and increases somewhat around the cutoff date (day 0), though remaining well balanced on the two sides. The only visible outlier (16 applications) refers to the first day for the submission of applications (5 November 2001). Note also that that the daily number of applications increased to some extent towards the end of 2002, before the possibility of applying was cut off.[18]

[Figure 4]

We also formally test for the presence of a density discontinuity at the threshold by performing a McCrary test. This test, which is based on kernel local linear regressions of the log of the density of the forcing variable run on both sides of the threshold separately (see McCrary, 2008), finds no discontinuity.[19,20]

A key implication of the RDD framework (see Lee and Lemieux, 2010) is that its validity can be tested by examining whether observed baseline covariates are locally balanced on either side of the cutoff. This evidence would substantiate the idea that the assignment of the treatment near the cutoff is approximately randomized. Table 1 presents for both treated and control firms means, standard deviations and mean differences between the two groups for a number of baseline covariates referring to 2000, the year before the programme was announced (the year before realization of the forcing variable). Following previous literature, the table displays the following variables: net overall investment (over 1999 capital), net intangible investment (over 1999 intangible capital), (log of) sales, (log of) assets, long-term debt (over assets), cash flow (over assets), average interest costs, ROA. Panel A considers the full sample of 751 firms. We find that the baseline covariates are roughly balanced on the two sides of the cutoff. However, average interest costs for treated firms are higher than for control firms. Panel B focuses on a sample of bandwidth (-90 days, +90 days) around the cutoff. As in Lalive (2008), the bandwidth is heuristically chosen to obtain a sample size equal to half of the full sample. Differences in covariates are again insignificant overall, with the sole exception of average interest costs. Smaller bandwidths (we tried $\pm$45-day and $\pm$30-day) deliver very similar results.

[Table 1]

[17] In a randomized setting, potential treated are notified shortly whether they are randomized in or out. In the FTI setting suspended-assessment firms realize they are not involved in the programme with a delay ranging from a few days (for firms that applied just before the suspension of applications) to 10 months (for firms that apply just after the shortage took place).

[18] This fact might suggest some sorting occurring before the option of applying expired. Perhaps, the possibility that the financing problems could have been more severe than initially envisaged permeated and this might have pushed some firms to rush to applying. As it will be shown in the empirical section, our estimates are robust to methods that exploit only the information in a limited neighborhood of days around the cutoff. Therefore, potential sorting at the end of the period in which firms were allowed to apply does not seem to impact on our estimates.

[19] The jump in density at the threshold was estimated by using three different optimal bandwidths (15.5, 31 and 62 days). The point estimates were 1.13 (standard error 1.62), 1.61 (2.70), and -1.04 (1.17), respectively.

[20] We are aware that a density test may have low explanatory power if manipulation occurs on both sides of the cutoff. However, there is no reason why firms would have been sorted after the cutoff.

As recognized by Lee and Lemieux (2010), with a large set of covariates some of the differences will be statistically significant by random chance. It is then useful to combine the multiple tests into a single test statistic to see if the data are consistent with random treatment around the cutoff. Table 2 presents the results we obtain with seemingly unrelated regressions (SURs) where each equation (with a function form as in equation (1) below) represents a different baseline covariate. A χ^2 test for discontinuity gaps in all the equations being zero is strongly supported by data for both the full sample and the $\pm$90-day bandwidth subsample[21].

[Table 2]

Results. – Figure 5 reports the cumulative (2001-2007) investment over initial capital by the application date. The evidence is based on the 329 subsidized firms (at the left of the threshold) and the 422 suspended-assessment firms (at the right of the threshold) of the full sample. We graph the mean of the outcome variable for each value of the discrete forcing variable (the application date). The figure superimposes the fit of a linear regression allowing for a discontinuity at the cutoff and linear trends in the forcing variable on both sides of the cutoff. From the figure it seems there is only a minor jump in the outcome variable at the cutoff. The jump would indicate that the investment activity of the financed firms is even lower than that of the control firms.

[Figure 5]

We turn now to more formal measures of the effect of the subsidy. As noted, RDD focuses on identifying the discontinuity in investments at the cutoff c. We start by using the following linear regression:

$$(1) \qquad I_i = \alpha_0 + \alpha_1 D_i + \beta_0 (X_i - c) + \beta_1 D_i (X_i - c) + \varepsilon_i$$

The parameter α_1 measures the average causal effect of the FTI subsidies on investment at the cutoff c. The parameters β_0 and β_1 capture the direct effects of the forcing variable X on the outcome I. The crucial issue in RDD estimation is the specification of the correlation between the outcome I and the forcing variable X. We propose two ways to assess whether the two-sided linear model specification (1) is appropriate. First, we parametrically evaluate the sensitivity of the results by augmenting the regression with quadratic and cubic terms in (X-c). Second, we move to non-parametric estimates (Pagan and Ullah, 1999) by running local linear regressions (Hahn et al., 2001) and estimating a triangular kernel (Fan and Gijbels, 1996). By relying only on outcomes from the firms that applied near the cutoff, the non-parametric results also provide robustness with respect to the circumstance that some subsidized firms started their project earlier than the control firms could have done.

Table 3 presents the regression results regarding the effect of the FTI subsidy on investment. Column (1) reports an estimate that compares average 2001-2007 cumulative investment on both sides of the cutoff.

[21] For these regressions, we also performed a robustness analysis similar to that reported in Table 3 below, with no modifications for our results.

The results indicate that investment by the subsidized firms is greater than that by the suspended-assessment firms (the estimate would amount to a 5 per cent annual difference)[22]. However, the estimate is not statistically significant. The second column reports the results from the basic model of equation (1). The estimated impact now turns out to be negative, but remains insignificant. Columns (3) and (4) report the results from the quadratic and cubic specifications, respectively. Again, there seems to be no effect of the FTI incentive. Column (5) describes local linear regression results, where model (1) is estimated over a $\pm$90-day bandwidth subsample. The results remain undisputed.[23] Column (6) reports estimation of a triangle kernel. As suggested by Fan and Gijbels (1996), for boundary estimation a triangular kernel is more efficient than the more standard rectangular kernel, as the former assigns more weight to the observations closer to the cutoff point. The $\pm$12-day bandwidth is chosen using the rule of thumb procedure proposed by Silverman (1986). The estimated impact of the treatment is negative and not significant. The results of this nonparametric estimation are also plotted in Figure 6.

Column (7) adds to the baseline specification of Column (2) a number of covariates. We include a set of dummy variables for the location of the firm (at the region level) and a set of two-digit industry dummies. The effect at the cutoff is basically zero. Column (8) provides the estimate of the impact by comparing cumulative investment over 2001-2006 instead of 2001-2007. This comparison is clearly biased in favour of finding a positive effect: at the end of 2006, when the bulk of the subsidized projects had been completed, some of the counterfactual projects could have been in the process of being completed. Again, we find a negative albeit insignificant impact.

[Table 3]

So far, the results show there is no increase in total investment as a consequence of receipt of the subsidy. Note, however, that the outcome variable – total investment – reflects not only the R&D expenditures for which the subsidy was granted or only requested, but also the additional investment that the firm has undertaken during the 2001-2007 period. For instance, suspended-application firms possibly chose to implement more traditional investments instead of unsubsidized R&D and this could explain why the overall effect is zero. To check for this possibility Column (9) uses as a dependent variable the ratio of intangible investment to intangible capital. In contrast with total investment, intangibles may be seen as more strictly related to R&D expenditures. As we fail to find any effect for intangible investment as well (the impact is negative albeit insignificant), we conclude that substitution between R&D investment and less innovative investment is not the reason behind our results.[24]

Table 4 presents the result we obtain by using alternative outcomes. For the sake of brevity, we only present the analogues to those of Table 2, Column (2). Consistently with the fact that firms get subsidies for projects they would have undertaken even without the subsidy, we fail to find in the considered period 2001-2007 any

[22] We examine the effect on the amount of general investments and therefore we cannot exclude the existence of differences in the qualitative composition of investments, in particular on their degree of innovativeness. Some hints regarding the composition come from considering the intangible component of investments in column (9) of the table; the results are confirmed.
[23] Estimates are insensitive to using smaller bandwidth.
[24] We also tried different specifications for the outcome variables (levels and growth rates of both overall and intangible investment). Results were very similar to those presented.

significant effect on sales, financial conditions of the firm (long-term debt over assets and cash flow over assets).[25] We also find no effect on the average interest rate charged by external financiers and on ROA. Finally, we find a positive impact on the overall size of the balance sheet, which suggests that money saved on R&D may have been capitalized in alternative (non-investment) assets.

[Table 4]

Table 5 displays the results obtained by splitting the sample along some potentially interesting dimensions. For instance, economic reasoning suggests that the effectiveness of subsidies should be greater for firms that are more likely to be rationed by private lenders, such as small enterprises or firms with high borrowing costs (see Guiso, 1998). Moreover, the rules determining the level of the subsidy imply that small firms are those with the highest intensity of subsidy. As a matter of fact, we fail to find signs of effectiveness even when we only consider these types of firm. Again, an argument made by practitioners is that R&D subsidies are usually wasteful, unless they can be targeted to firms already having a sufficient know-how in innovation activity. To check for this possibility, we estimate the impact of the programme only for the subsample of firms with high intangible balance sheet assets. The results do not support the practitioners' argument.

[Table 5]

6. Conclusion

Innovation is commonly invoked as one of the main engines of growth. Accordingly, policy for innovation at national and international level routinely highlight the role of public support for innovation.[26] Beyond public declarations and legitimate hopes, however, little is known about the effectiveness of public spending to foster private R&D. The reason is that to evaluate the effects of government-sponsored programmes it is necessary to address the intrinsically difficult counterfactual question of what would have happened without the subsidies. In principle, a ready-to-implement method to provide a decisive answer to the key counterfactual question is available: it is randomization. In practice, this method is almost never used, so researchers have long been struggling with identification strategies that aim at reproducing the fundamental feature of a random design as closely as possible. Exogenous events sometimes introduce some elements of randomization into programmes that were not originally designed as random schemes. This study elaborates on one of these circumstances: the unexpected shortage of funds that occurred with the Italian Fund for Technological Innovation. We compare firms that received the subsidies before the shortage with firms that applied after it and then saw assessment of their application suspended. Since being in one group or the other essentially a matter of luck, especially for firms that applied near the day when funds were cut off, we implement a regression discontinuity design.

Our results point to a simple conclusion: there is no evidence of effectiveness whatsoever. Compared with suspended-assessment firms, subsidized firms do not invest more in either tangible or intangible assets. Basically, subsidized firms get subsidies for projects that would have been undertaken even without the subsidy.

[25] We are not able to exclude that the real effects of the policy on some variables, in particular on sales, may require more time to be detected and therefore they may be delayed after 2007.
[26] For instance, R&D is one of the priorities of the European Union's Lisbon Strategy.

Tables and figures

Figure 1

Timeline of applications

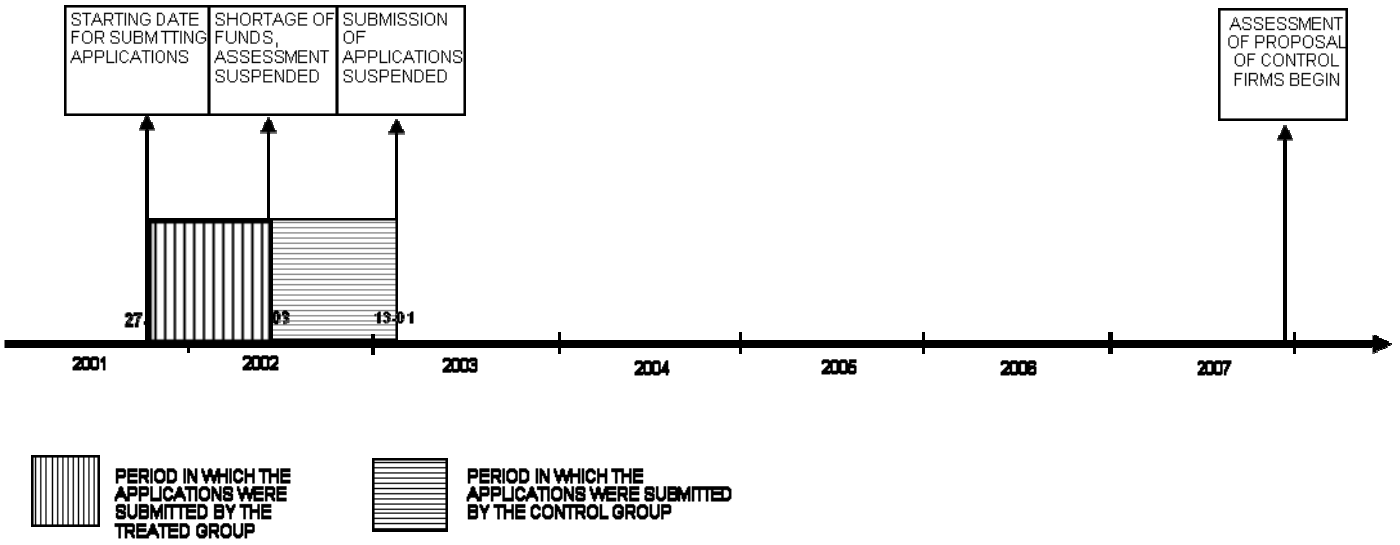


Figure 2

Hypothetical timeline of investment

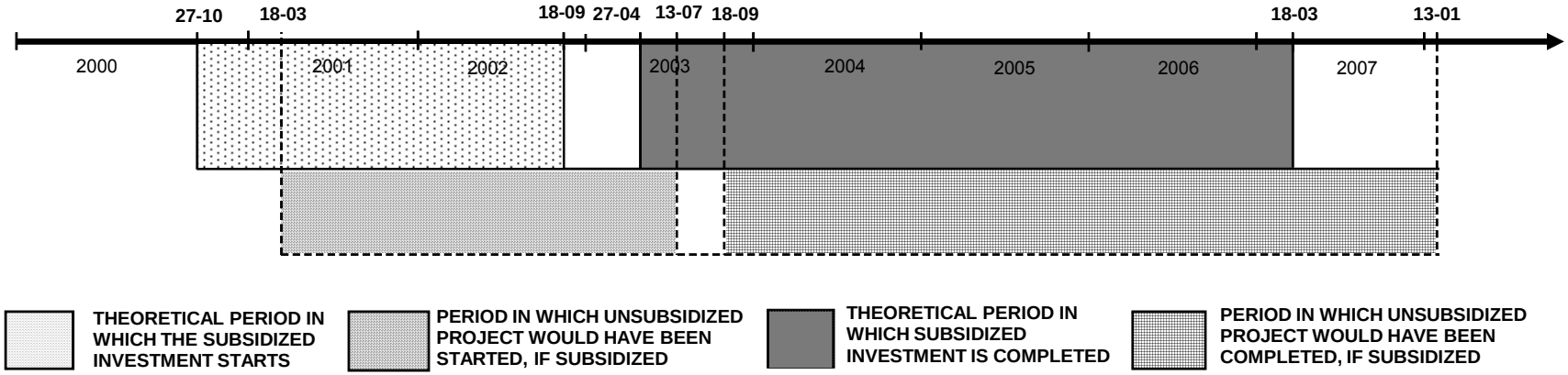
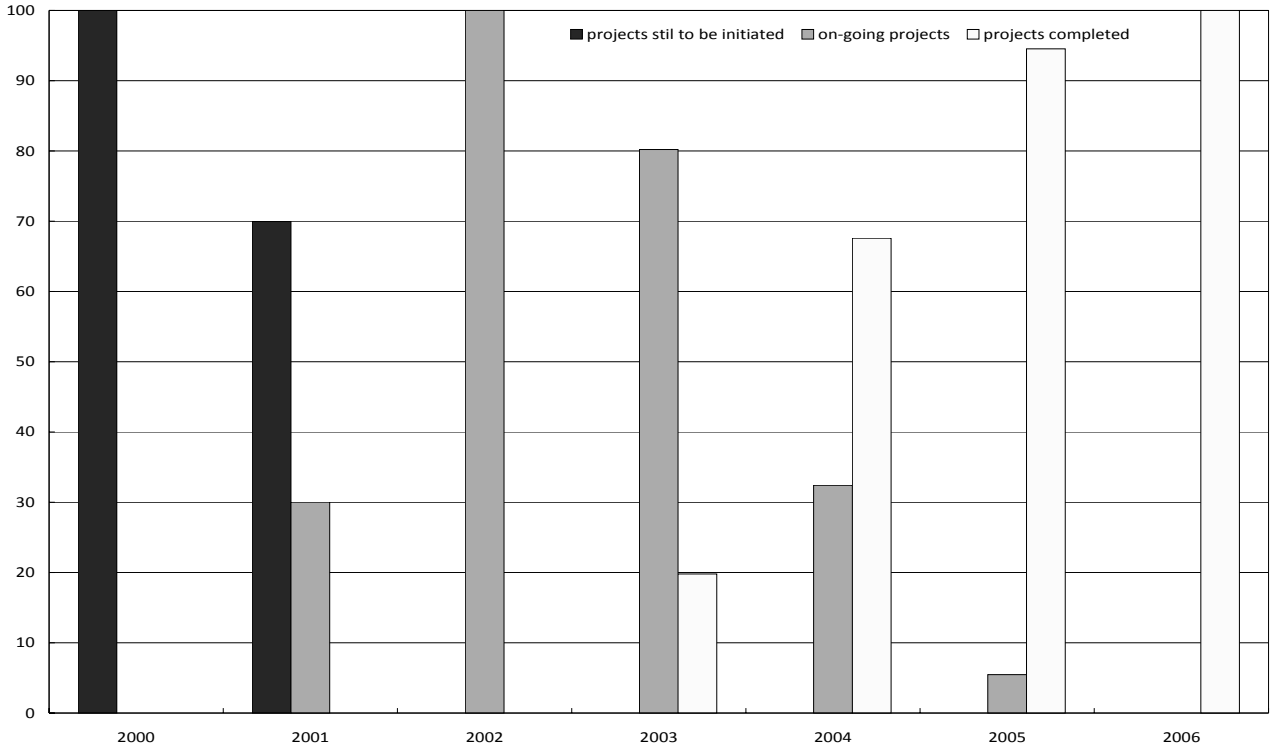


Figure 3

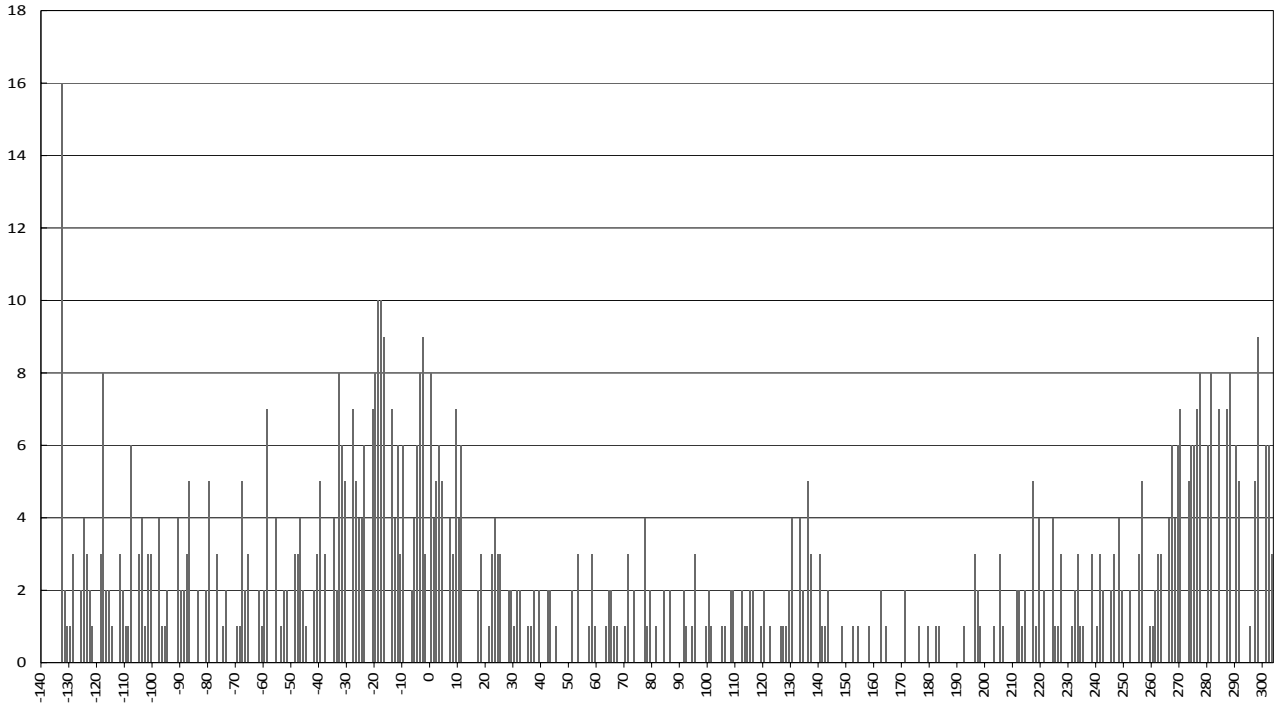
Actual timeline of subsidized investment



Source: Ministry for Economic Development.

Figure 4

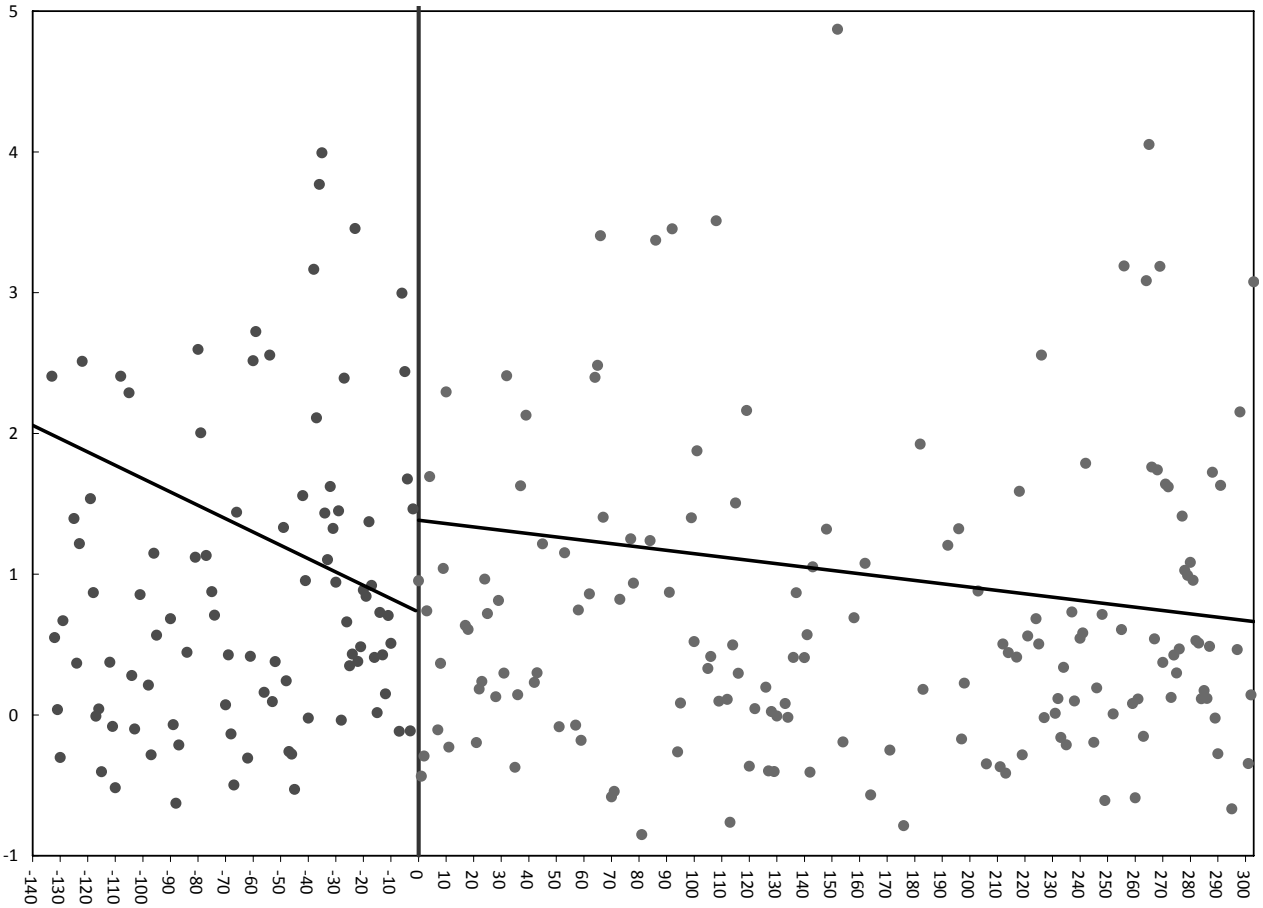
Frequency distribution of applications by date



Sources: Ministry for Economic Development and Cerved. Note: the x axis shows the number of days before or after the suspension (day 0) of assessment of the applications.

Figure 5

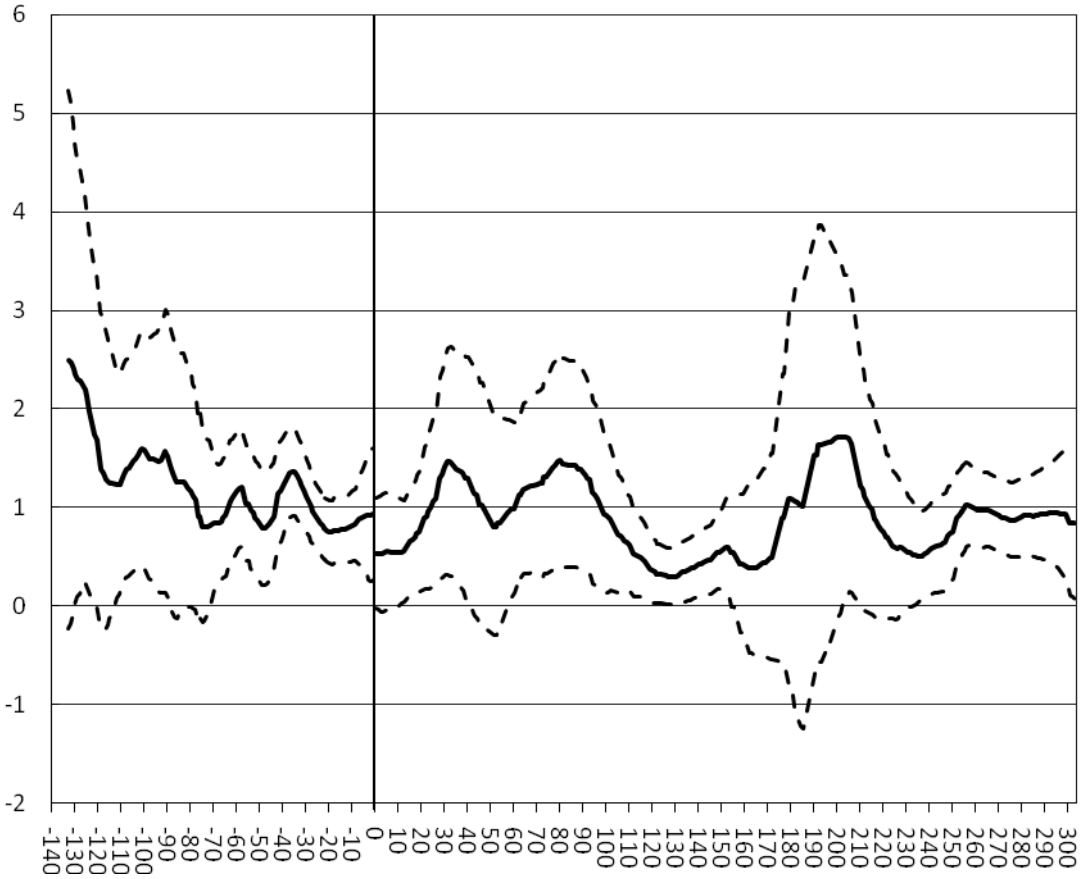
The effect of FTI subsidies on investment: parametric estimates



Sources: Ministry for Economic Development and Cerved.

Figure 6

The effect of FTI subsidies on investment: nonparametric estimates



Sources: Ministry for Economic Development and Cerved.

Table 1

Mean differences in the pre-treatment year

	Full sample				Sample restricted to 90 days around cutoff			
	mean treated	mean controls	mean difference	p value	mean treated	mean controls	mean difference	p value
Net overall investment over capital	9.5942	9.4847	0.1095	0.24	9.5717	9.4682	0.1036	0.46
Net intangible investment over intangible capital	9.5102	9.3927	0.1175	0.22	9.4948	9.3829	0.1119	0.44
log(Sales)	0.6477	0.4110	0.2367	0.37	0.7054	0.4503	0.2551	0.64
log(Assets)	0.7518	1.0336	-0.2818	0.28	0.7079	0.7840	-0.0761	0.81
Long-term debt over assets	0.1128	0.1047	0.0081	0.29	0.1149	0.1049	0.0100	0.39
Cash flow over assets	0.0892	0.0883	0.0009	0.84	0.0853	0.0892	-0.0038	0.56
Average debt cost	0.0383	0.0338	0.0044	0.05	0.0404	0.0330	0.0074	0.08
ROA	0.0819	0.0781	0.0038	0.46	0.0775	0.0774	0.0001	0.98
Number of observations	329	422			239	126		

Sources: Ministry for Economic Development and Cerved. Note: all variables refer to the year immediately before treatment (2000) except capital and intangible capital (which refer to 1999).

Table 2

SUR estimates of discontinuity of covariates

	(1) Full sample	(2) Sample restricted to 90 days around cutoff
Net overall investment over capital	0.1126	-0.0953
	(0.1531)	(0.2189)
Net intangible investment over intangible capital	0.1488	-0.0244
	(0.1579)	(0.2242)
log(Sales)	0.3547	0.7914
	(0.4343)	(0.8388)
log(Assets)	-0.1641	0.0262
	(0.4311)	(0.4920)
Long-term debt over assets	0.0165	0.0071
	(0.0128)	(0.0182)
Cash flow over assets	-0.0072	-0.0121
	(0.0072)	(0.0102)
Average debt cost	0.0083**	0.0099
	(0.0037)	(0.0064)
ROA	-0.0028	0.0016
	(0.0086)	(0.0117)
χ^2	7.63	6.67
p-value	0.47	0.57
Number of observations	751	365

Sources: Ministry for Economic Development and Cerved. Notes: all variables refer to the year immediately before treatment (2000) except capital and intangible capital (which refer to 1999). The standard errors, clustered by technological level of the sectors (OECD definition), are reported in brackets. The number of asterisks shows the statistical significance of the coefficient: * 90%; ** 95%; *** 99%.

Table 3

The effect of FTI subsidies on investment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment effect	0.3569	-0.2082	0.1463	0.2996	0.5482	-0.2458	-0.0759	-0.1115	-0.1946
	(0.3038)	(0.1901)	(0.9533)	(0.7554)	(0.8462)	(0.6482)	(0.1982)	(0.0901)	(1.3174)
Polynomial order	0	1	2	3	1		1	1	1
Bandwidth	∞	∞	∞	∞	90 days	12 days	∞	∞	∞
Control variables	no	no	no	no	no	no	yes	no	no
Dependent variable	Overall investments	Overall investments	Overall investments	Overall investments	Overall investments	Overall investments	Overall investments	Overall investments	Intangible investments
Period	2001-2007	2001-2007	2001-2007	2001-2007	2001-2007	2001-2007	2001-2007	2001-2006	2001-2007
R^2	0.003	0.010	0.012	0.013	0.017		0.052	0.011	0.007
Number of observations	751	751	751	751	365	751	751	751	751

Sources: Ministry for Economic Development and Cerved. Notes: all variables refer to the period shown except capital and intangible capital (which refer to 1999). The standard errors are reported in brackets. In column (6) standard errors are calculated by bootstrap; in the other columns they are clustered by technological level (OECD definition). The number of asterisks shows the statistical significance of the coefficient: * 90%; ** 95%; *** 99%.

Table 4

The effect of FTI subsidies on alternative outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	log(Sales)	log(Assets)	Long- term debt over assets	Cash flow over assets	Average debt cost	ROA
Treatment effect	0.0388	0.1200**	-0.0086	-0.0024	0.0021	-4.7925
	(0.0620)	(0.0603)	(0.0119)	(0.0034)	(0.0028)	(3.3395)
Polynomial order	1	1	1	1	1	1
Bandwidth	∞	∞	∞	∞	∞	∞
Control variables	no	no	No	No	no	no
Period	2001-2007	2001-2007	2001-2007	2001-2007	2001-2007	2001-2007
R^2	0.0020	0.0044	0.0108	0.0012	0.0037	0.0037
Number of observations	751	751	751	751	751	751

Source: Ministry for Economic Development and Cerved. Notes: all variables refer to the period shown except capital and intangible capital (which refer to 1999). The standard errors, clustered by technological level (OECD definition), are reported in brackets. The number of asterisks shows the statistical significance of the coefficient: * 90%; ** 95%; *** 99%.

Table 5

The effect of FTI subsidies: subsamples

	(1) Small and medium-sized firms	(2) High–capital-cost firms	(3) Intangible-asset-intensive firms
Treatment effect	-0.0720	-0.2008	-0.2649
	(0.1473)	(0.4125)	(0.4746)
Polynomial order	1	1	1
Bandwidth	∞	∞	∞
Control variables	no	no	no
Dependent variable	Overall investments	Overall investments	Overall investments
Period	2001-2007	2001-2007	2001-2007
R^2	0.0199	0.0007	0.0082
No. observations	533	368	386

Source: Ministry of Economic Development and Cerved. Balanced panel. Notes: all variables are referred to the shown period except capital and intangible capital (referred to 1999). The standard errors, clustered by technological level (OECD definition), are reported in brackets. The number of asterisks shows the statistical significance of the coefficient: * 90%; ** 95%; *** 99%.

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