The Effect of Investment Tax Credit:
Evidence from an Atypical Program in Italy [•]

Raffaello Bronzini
Bank of Italy

Guido de Blasio[◊]
Bank of Italy

Guido Pellegrini
University of Bologna

Alessandro Scognamiglio
Bank of Italy

Abstract
This paper examines how business investment responds to investment tax credit, enacted by Italy's Law 388/2000. To assess whether the program made investments possible that otherwise would not have been made, it exploits some features of the tax credit scheme, such as the fact that some Italian regions are not deemed to be eligible or the amount of the bonus differs across eligible regions. While the program was fiscally unsustainable, and thus it was downsized well ahead of the expiration date, our findings suggest that it has been effective in stimulating investment.
JEL classification: E22; H25
Keywords: Investment incentives; State aid

[•] We are grateful to Simonetta Botarelli, Luigi Cannari, Massimo Omiccioli, and Alessandra Staderini for comments and suggestions, and Raffaela Bisceglia and Antonietta Mendolia for the editorial assistance. The views expressed are those of the authors and do not imply the responsibility of the respective Institutions.
[◊] Corresponding author: Via Nazionale, 91 (00184) Rome, Italy. Email: guido.deblasio@bancaditalia.it

1. Introduction

This paper evaluates the impact of investment tax credit (ITC) on business investment using a unique policy experiment provided by the ITC program carried out by the Italian Government trough Law 388/2000. As other ITC schemes, this measure reduces the cost of acquiring capital to firms without altering the returns from such capital. Unlike other ITC schemes, however, the bonus envisaged is not restricted to profitable enterprises with tax liability. Indeed, the credit can be deducted from any outstanding payment due to the central administration (even, social security contributions or tax paid by workers and temporarily held by the firm). The program introduced with Law 388 has some peculiar features, such as the fact that only some regions are deemed to be eligible, while the amount of tax credit differs across areas of eligibility. Enacted in December 2000, the program was originally supposed to be in place until December 2006; in 2003, however, due to public finance problems the budget allocated to the initiative was drastically reduced.

The role of ITC has been at the forefront of economic research for decades (see, for instance, Brown (1962) and Auerbach and Summers (1979)). Following Hall and Jorgenson (1967), a number of papers have theoretically investigated the dynamic effects of ITC on the desired stock of capital (see, for instance, Abel (1982) and Auerback and Hassett (1992)). The main implication of this work is that compared to a permanent investment tax credit, a measure that is known to be temporary gives firms a stronger incentive to invest while the credit is in effect. Empirical investigations have been, however, less uncontroversial. For instance, Goolsbee (1998) presents evidence that an ITC program pushed up prices of investment goods without sharp increases in real investment. Cohen and Cummis (2006) study the impact of temporary partial expensing and find that the measure was largely ineffective in boosting investment, while House and Shapiro (2006) show that the same measure had a discernable impact on capital expenditures.

By employing a difference-in-differences framework, this paper assesses whether the ITC program made investments possible that otherwise would not have been made. Since the ITC is assigned on the basis of the firm's demand for the fiscal bonus, the main challenge is to find a suitable control group; that is, a group of firms similar to the firms that receive the ITC in all respects except for the receipt of the ITC. To tackle this issue, the paper exploits a number of features of the scheme envisaged under the Law 388. First, we utilize the fact that some Italian areas in the Centre and North of Italy are not deemed to be entitled and estimate the impact of program eligibility by comparing both subsidized and non-subsidized firms located in eligible areas to firms located in non-eligible areas. Second, we make use of the circumstance that the budget allocated to the program was drastically reduced in 2003 and estimate yearly impacts of the program to verify whether the investment time-pattern follows the budget time-pattern. Third, we implement an intuitive version of the regression discontinuity design by focusing on subsidized and eligible-non-subsidized firms that are located in areas very similar to the non-eligible areas. Fourth, we take advantage of the ban on combining the ITC with other sources of public money and select a comparison group among the firms with rejected applications from an alternative investment incentives program. Fifth, we exploit the fact that the amounts of tax credit differs across eligible regions and compare firms receiving a relatively more generous fiscal bonus with firms receiving a less liberal treatment.

All the above empirical strategies point out to the same conclusion: the ITC program has been highly effective in stimulating investment. This conclusion is robust to the way the comparison groups are selected and investment is measured. Moreover, the investment boost attributable to the ITC is not driven by time substitution or counterbalanced by negative side-effects on factor efficiency and profitability, at least within the time window of data availability. However, the lessons from this program are wider than its effect on investment. The major drawback of the scheme is that the amount of fiscal resources needed are not under control. In the Italian experience, this lack of fiscal sustainability was the reason why the program was abruptly downsized.

The paper is structured as follows. We start in Section 2 with a description of Law 388. Section 3 describes the data, the methodology, and the empirical findings. Section 4 offers some concluding comments.

2. The Program

This section explains the main features of ITC program enacted by Law 388. We focus on the aspects more relevant for our empirical analysis (more details can be found in *Ministero delle Attività Produttive* (2002)). The aim of the program is to spur capital accumulation in Italy's lagging areas, as identified by the European Commission (see the article 87, paragraph 3, points a) and c), of the European Treaty as modified in 1997 in Amsterdam).

Unlike other ITC schemes, the bonus envisaged is not restricted to profitable enterprises with tax liability. Indeed, the credit can be deducted not only from firm's corporate tax charges, but also from the VAT outstanding and social security contributions. Moreover, it can be subtracted from the amounts of income tax and social security contributions paid by the workers and temporarily held by the firm (in Italy' case for these amounts the firm acts as withholding agent on behalf of the workers). In this respect, the program is similar to an investment grant program, as it provides a direct government rebate to firms of a certain fraction of investment expenditures (see: Auerbach and Summers (1979)).

The program envisages that firms investing in the South of Italy and few selected areas in the Center and North are granted a tax exception as percentage of their annual net capital expenditures. Both manufacturing and service firms are eligible under the program.[1] There are only minor restrictions as to the categories of investment goods covered. Basically, all tangible und intangible capital goods are included, with the only exception of advertising, goodwill and R&D expenditures, office furniture, and vehicles to be used for third-part transportation.

There are different areas of ITC intensity, as percentages of tax deductions vary by regions (see: Chart 1). The ITC is equal to 65% in Calabria (which is the relatively least-developed region) and to 50% in the remaining southern regions except Abruzzo (which represents the relatively most-developed region in the South), where it amounts to 30%. As for the few selected areas of the Centre and North eligible under the program, the fiscal bonus is equal to 18% of the capital expenses.[2]

Enacted in December 2000 (see *art. 8 Legge Finanziaria 388, dicembre 2000*), the program started in 2001 and was originally supposed to be in place until December 2006; in August 2002 (see *Legge n. 178, agosto 2002*), however, it became clear that the automatic character of the scheme was not compatible with the limits on the government's budget. Therefore, an annual ceiling on the overall resources for the ITC was imposed. To make sure that the amounts granted were kept below the ceiling, it was decided that the requests were subject to ex-ante approval by the Tax Office. The requests of fiscal bonus were served on a first-come-first-served basis, within the budgetary limits. The new rules started to be followed at the end of 2002. Their effect became evident in 2003 when the budget allocated to the ITC initiative was reduced to 500 million euros, from 2,000 million euros the year before (see, Corte dei Conti (2004)).

[1] Agricultural firms are also eligible. However, we do not have any of them in the dataset we use to estimate the impact of the program (see below).

[2] Tax deductions for large firms are less generous (respectively, 50%, 35%, 20%, and 8% in Calabria, remaining southern regions, Abruzzo, and northern eligible regions). Note, however, that do not have any large firm in the dataset we use for estimation (see below).

The timing of the program was surrounded by a considerable uncertainty. The program was launched quite abruptly at the end of 2000. The fact that it was not expected minimizes the scope for firms to postpone investment in order to benefit from the ITC, and therefore helps us to identify the share of additional investment triggered by the measure net of that due to postponement. Regarding the period in which the program was supposed to last, and as explained above, it was abruptly downsized in August 2002, with effect starting from 2003. This circumstance attenuates the potential bias deriving from firms bringing forward investment projects originally planned for the post-program period, as these investments accelerate just before the known expiration date (Romer (2001)). Again, this fact facilitates identification, as it reasonably reduces the bias due to anticipation.

Finally, for the purpose of the evaluation exercise an important aspect of the program has to be noted. The estimation results we present below are based on the assumption that there are no other governmental programs correlated with the allocation of the ITC program. If the firms that do not receive the fiscal bonus obtain other types of financial assistance, then our comparison will underestimate the effect of the program. A feature of the Law 388 minimizes the scope of this bias, as the ITC cannot be combined with other source of public financing. This implies that subsidized firms are not receiving extra subsidies in addition to the ITC. However, firms in the comparison groups, which do not participate in the ITC program, could in principle receive the grants envisaged under another incentives program, the Law 488. Fortunately, we are able to identify the firms that obtain the Law 488 assistance; therefore, they are excluded from the comparison groups.[3]

3. Data, Empirical Strategy, and Results

3.1 *Data*

To identify the firms that have received the ITC we use the official Law 388 dataset from the Ministry of Industry. It includes the firms that have received the tax credit during the period 2001-2004. For each of them, the dataset provides the fiscal code and the location. Note, however, there is no information about the timing of the receipt of the ITC: firms could have received it anytime between 2001 and 2004. Since this dataset lacks information on investment (as well as additional covariates and firm features) we augment our data with financial statement data taken

[3] We thank Sergio Gison and Salvatore Mignano from the Italian Ministry of Economic Development for providing us with the information on the recipients of the Law 488 assistance.

from the Cerved dataset. As Cerved data are available only for corporations, the paper concentrates on them (the Law 388 dataset includes 2,030 corporations). A drawback with the Cerved data is that there are frequent misprints as regard the fiscal code that we use to link financial statements with the Law 388 dataset. As a result, we are able to find uninterrupted Cerved financial statements from 1998 to 2004 for 634 firms located in eligible areas[5] that received the ITC.[6] This represents the treatment group (*TREAT*). In the estimations below, we have four post-intervention years (from 2001 to 2004) and two pre-intervention years (1999 and 2000).[7]

3.2 *Empirical design*

Empirically, we will be adopting a difference-in-differences framework (see, for example, Angrist and Krueger (1999), Card (1999), and Meyer (1995)) and trying to find a control group that is as comparable as possible with the treatment group. If we could find a group of firms similar to the firms that receive the ITC in all respects except for the receipt of the fiscal bonus, then we would estimate the equation:

$$(1) \qquad Y_{ijt} = a_1 + a_2 X_{ijt} + a_3\, TREAT_i + a_4\, POST_t + a_5\, (TREAT_i * POST_t) + \varepsilon_{ijt}$$

where Y_{ijt} is the outcome variable, investment of firm i in located in the region j in year t; X_{ijt} denotes a vector of firm-level and region-level characteristics; *TREAT* denotes a dummy variable indicating whether the firm has received the ITC; *POST* is a dummy variable equal to 1 for the period after the introduction of the program. In this specification, the coefficient of interest would be a_5, which picks up the impact of the ITC on the treated.

Finding a suitable comparison group is not straightforward. The tax credit introduced with Law 388 is an *automatic* measure, as there is no discretion involved on the policy maker side. In the entitled areas, all the investing firms requesting the benefit will receive it (unless, as explained in Section 2, after 2003 the benefit is refused on the base of timing of the request and budget

[5] Eligible areas are deemed to be the regions of the South of Italy. However, few selected areas of the Centre and North of Italy are also entitled to receive assistance under the program. In our dataset we have data for 76 financed firms located in the centre and north of Italy (these data are used in the experiment of Table 5, below).

[6] We select only firms with non-negative values for capital stocks, assets, and sales for each year, and trimmed the sample at the 1 and 99 percentiles of the distribution of investment over capital.

[7] We also make use of 1998 data for physical capital, assets and sales.

constraints). A subsidized firm is self-selected and cannot be compared with a non-subsidized firm without introducing the possibility of bias. Take a firm located in an entitled area that has not received the ITC. This firm had no incentive to invest notwithstanding the tax deduction. Thus, it self-selects out of the pool of participants, and comparison of benefited firms versus non-benefited firms will be biased upward. By the same token, a subsidized firm cannot be compared with a non-eligible firm, since we cannot rest assured that the latter would have invested, and thus received the ITC, had it been located in an entitled area.[9] In this circumstances, a more promising approach is to compare both subsidized and non-subsidized firms located in eligible areas to firms located in areas not deemed to be eligible. In this case, differences in outcomes reflect the presence of the program in the eligible areas. That is, they measure the impact of *eligibility* rather than *participation*.[10]

To estimate eligibility we contrast treated firms and eligible non-participating firms (*ELEG*) with non-eligible firms (*NELE*). We will be running the following specification:

$$(2) \qquad Y_{ijt} = a_1 + a_2 X_{ijt} + a_3\, TREAT_i + a_4\, ELEG_i + a_5\, POST_t + a_6\, (TREAT_i * POST_t) + a_7\, (ELEG_i * POST_t) + \varepsilon_{ijt}$$

The coefficients of interest in equation (2) are a_6 and a_7. They measure the change in investment after the introduction of the ITC, in subsidized firms and firms located in entitled areas but non subsidized, compared to firms located in non entitled areas. Under the hypothesis that the positive selection bias for the treated is offset by the negative selection bias of the eligible non-participating firms, the average between the two coefficients a_6 and a_7 will capture the effect of eligibility.

A key challenge is to find convincing control groups. Below, we select comparison groups by two different methods: *propensity score* and *exact matching*. As argued by Winship and Sobel (2001), their joint consideration offers a way to assess the robustness of the estimates.

[9] In more technical jargon, it is difficult to evaluate the effect of ITC on subsidized firms, since it is hard to disentangle the treatment effect from the selection bias.

[10] In the program evaluation literature there are many analogues to this exercise, such as estimating the economic impact of firms' exposure to road and rail networks rather than their usage. In the study of micro-credit, Morduch (1998) uses a framework similar to the ours.

As for the propensity score (Rosenbaum and Rubin (1985)), we use the Nearest Neighbor Matching as implemented by Becker and Ichino (2002).[11] Each treated firm is matched with the non-subsidized located in the same area of ITC intensity (see Section 2) and displaying the nearest propensity score. In addition, it is matched with the non-eligible firm displaying the nearest propensity score. Both control groups are derived from the population of Italian firms with uninterrupted Cerved financial statements over the period 1998-2004, where firms receiving some other sources of aid are removed. The propensity score is Logit-estimated by using a set of firm-level covariates averaged over the pre-intervention period (1998-2000): we include, in addition to investment over pre-dated capital, a proxy for the firm size (sales), a measure of internal funds (cash flow as percentage of assets), a measure of the interest rate (interest costs as percentage of debt), a measure of leverage (debt as percentage of assets), a proxy for gross profitability (Gross Operating Margin, GOM, as percentage of value added), and ROA. We also add a series of 3-digit industry dummies. Note, however, that control firms can belong to industries different from that of the treated.

We also rely on a different selection criteria: exact matching. In this case, we first impose treated and control firms to be in the same industry. In particular, both eligible non-subsidized and non eligible firms have to share the same 4-digit ATECO of the subsidized one. Note that this is a quite detailed industry level, which includes, for instance, cotton power-loom weaving or ceramic tile manufacture. Then, within each industry-level stratum we select for each treated firm its two counterparts (again, one located in the same area of ITC intensity, the other in a non-eligible area) by minimizing a loss function that has in argument the following covariates, Investment/Capital, Sales, Cash Flow/Assets, Interest Cost/Debt, and ROA.[12] The control groups selected by exact matching have the nice propriety that very detailed industry-level patterns are differentiated away.

The control groups selected by the alternative methods of propensity score and exact matching are basically disjoint. Among the 1,253 firms selected by propensity score (620 eligible non-subsidized firms and 633 non-eligible firms), only 22 of them appear also in the control group selected by exact matching (which includes 1,264 firms, 623 and 641 respectively). This feature enrich the robustness of our estimates, as we are contrasting subsidized firms with two entirely different comparison samples.

[11] Matching is executed with replacement. Results differ only little if matching without replacement is instead allowed. Similarly, results obtained by using alternative propensity score estimators, such as the radius matching and the kernel matching are qualitatively very similar to those presented in the text (see Dehejia and Wahba (2002)).

[12] As we checked, this set of covariates is the largest one for the which balancing properties are satisfied.

We gauge the effect of eligibility by estimating equation (2) and averaging the effects of the program for subsidized firms and eligible firms, compared to non-eligible firms. We start by using the comparison groups selected by propensity score. As shown by Table A1, which reports descriptive statistics of the three groups as well as their mean differences, the propensity score ensures a good balance, as most of the mean differences in firm observables are not significant.

Table 1 shows the results we obtain by estimating equation (2). To provide some robustness on the way investment is measured (see: Cummis et al (1994) and Lamont (1997)), we compute the dependent variable in a variety of different ways. In Panel A the dependent variable is (overtime cumulate of) investment as percentage of the capital stock at the beginning of the period; in Panel B investment is normalized by pre-dated sales; finally, in Panel C it is divided by lagged assets. The table shows the estimates for the coefficients on the two interactions. The specifications always include, in addition to the dummies *TREAT*, *ELEG*, and *POST*, also region fixed effects and firm-level covariates. As for the latter, which vary by firm and (post-intervention) year, we include the same variables used for the propensity score.

A major concern is that the estimates may reflect general differences across eligible and non-eligible areas (non-random program placement). Since eligible areas are the regions of the South of Italy while non-eligible areas include regions of the Centre and North of Italy, this could be a serious issue. As it is well known, the South of Italy differs from the Centre-North in a number of aspects. The South is generally poorer and less endowed with infrastructures. The South also has a lower quality of local institutions and less property-right protection. We try to tackle this issue by adopting a number of empirical strategies. First, we always include region fixed-effects in the estimates. This ensures that our findings are not driven by omitted fixed local characteristics. However, there could be omitted time-varying and region-specific effects correlated with the program that might be driving the apparent effect of the ITC on eligible firms. Therefore, we also include a number of time-varying controls defined at the regional level. In particular, we add the growth rate of GDP, Investment, and Employment. Later on, we implement a more straightforward strategy to alleviate the concerns related to non-random program placement and focus on regions that can be deemed to be similar and estimate the effect of the ITC within these regions.

Turning to the results, we find that both the interaction coefficient between *POST* and *TREAT* and that between *POST* and *ELEG* enter with a positive sign. Both terms display always high statistical significance, irrespective of the way the dependent variable is measured. The high statistical significance is also robust to how we specify the stratum of the clustering correction (Wooldridge (2002)).[13] As explained above, the average between the two coefficients can be interpreted as the causal effect of program eligibility. Therefore, we can gauge the magnitude of eligibility as follows. Descriptive statistics show that during the post-intervention period, the investment as percentage of capital of the treated, eligible non-participating and non-eligible groups is equal to 165%, 100%, and 99%, respectively. That means that the average (non causal) investment of the eligible firms amounts to 130%, that is about 1.3 times the investment carried out by non-eligible firms. Diff-in-diffs estimates in Column 1, Panel A of Table 1, however, suggest that the additional investment caused by program eligibility is much reduced, as it amounts to 44% percent of the post-intervention investment activity of the non-eligible firms. Column 2 shows that when the region time-varying controls are included, the estimated effect of program eligibility decreases to 38%.[14] Normalizing investment by sales and assets (Panel B and Panel C) delivers similar pictures. In these cases, the estimated impacts amount respectively to 65% and 60%, in the specification that allows for region time-varying covariates.

We then turn to exact matching. As shown in Table A2, also in this case most of the pre-intervention observables are quite similar across groups. Only few covariates (see, for instance, interest costs and gross margins) are not perfectly balanced, as their mean differences are not zero. Table 2 shows the results we obtain by estimating equation (2) for this sample. We find that the estimated effect of program eligibility remains positive and highly significant. When evaluated over capital (Panel A), the additional investment prompted by the existence of the program amounts (Column 1) to 112% of the investment of the non-eligible firms. It also survives to the inclusion of the regional time-varying controls (Column 2).[15] Moreover, the estimated magnitude of the effect obtained by using alternative dependent variables is in the same range. In the specification that allows for region time-varying controls, the impact is equal to 134% when investment is measured over sales (Panel B) and to 180% when investment is normalized by assets (Panel C).

The results of Table 1 and Table 2 can be used to guess estimate the effect of the ITC on the population of eligible firms. In the Law 388 dataset there are 1,970 southern corporations that have

---

[13] Since we compare differences in outcomes over two adjacent collapsed periods, the estimated standard errors are robust to potential serial correlation even in small samples (see Bertrand et al (2004)).

[14] For cost-benefit purposes, this increase should be considered borderline satisfactory, as the fiscal bonus received by the firms in our sample amounts to 30% of the pre-dated capital.

[15] These magnitudes are roughly comparable with those found in other studies (see, for example, House and Shapiro (2006).

received the fiscal bonus. They represent a very small percentage of the corporations eligible under the program. For instance, in 2001 and 2002 the Cerved dataset includes 59,980 southern corporations that have neither received the ITC nor other form of aid (28,060 of them display a positive investment).

First, to have an idea of the population-average effect of the ITC, we can weight the coefficient a_6 by the share of treated firms in the eligible population of firms (1,970/(59,980+1,970)=0,03), and the coefficient a_7 by its complement to unit (0,97). In this case, if we take, for instance, the estimates of Table 1, Panel A, Column 2, we calculate that the effect is equal to 9% of the investment of the non-eligible firms.

Second, investing firms in eligible areas might decide not to claim for the bonus.[16] On the one hand, this occurrence can be explained by a lack of knowledge as there are virtually no costs implied in the claiming procedure. For an entitled firm the only thing needed to get the bonus is to fill an additional line in an application form (so called form F24), which has to be filled out monthly anyway. On the other hand, for the firms that apply for the fiscal bonus the controls of the fiscal administration against tax evasion can be deeper. Therefore, a firm might decide not to claim for the bonus in an attempt to skip the inspections. We can calculate the average effect of the ITC eligibility for the sub-sample of firms which display a positive investment. This effect is equal to 12% of the investment of non-eligible counterparts.

As explained above, the timing of the receipt of the ITC for a single firm is not known. A firm in our dataset could have received it anytime between 2001 and 2004. However, we do know the timing of the aggregate amounts involved. In 2003 the budget allocated to the ITC program was drastically reduced. Therefore, the bulk of the financing occurred in 2001 and 2002. This is a piece of information that we can exploit. If the estimated investment pattern is truly driven by the ITC, we should observe a relatively more intense surge in investment in the two initial years of the program. Operationally, we estimate the impact of the program for each single year of the post-intervention period. In this case, we run the following year-by-year version of equation (2):

$$(3) \qquad Y_{ijt} = a_1 + a_2 X_{ijt} + a_3\, TREAT_i + a_4\, ELEG_i + \Sigma_t\, a_{5,t}\, YEAR_t + \Sigma_t\, a_{6,t}\, (TREAT_i * YEARPOST_t) + \Sigma_t\, a_{7,t}\, (ELEG_i * YEARPOST_t) + \varepsilon_{ijt}$$

where *YEAR* denotes time dummies, and *YEARPOST* is a series of dummies for each of the years after the introduction of the program. The coefficient of interest in equation (3) are $a_{6,t}$ and $a_{7,t}$.

[16] For instance, Knittel (2005) finds that for American firms there is evidence in this respect.

Since the impact is evaluated overtime, we will observe as many coefficients as the years of the post treatment period. They measure the yearly change in investment after the introduction of the ITC, in subsidized firms and firms located in entitled areas but non subsidized, compared to firms located in non entitled areas. For each year the average between the two coefficients will capture the effect of eligibility. Results are described in Table 3. They are very encouraging, as in 2001 and 2002 the coefficient on the interaction between *YEARPOST* and *TREAT* is almost always positive and significant. In the two remaining years, the coefficient is either negative or positive but with a smaller absolute value. This finding is robust to the method we employ to select the comparison groups and the way we specify the dependent variable.

The estimates of Table 3 gives us a chance to discuss the role of time substitution for our results (Abel (1982), Adda and Cooper (2000), Auerbach and Hines (1988)). First, to take advantage of the ITC, firms could have postponed investment projects originally planned for the pre-intervention period. As argued in Section 2, this is quite unlikely, as there was no expectation about the launch of the ITC program. In any case, under this circumstance there would have been a lower investment for the treated firms compared to the comparison firms before the start of the program. Since in our sample (see tables A1 and A2) pre-treatment investment for treated firms is undistinguishable from that of the untreated counterparts, this cannot be the reason behind our estimates. Moreover, we find similar results in an additional experiment (see below) in which we impose treatment and control groups to be comparable for a long time series of pre-treatment investment growth rates. Second, firms could have also brought forward investment projects originally planned for the post-program period. Again, as explained in Section 2, the bias arising from anticipating investments should reasonably be attenuated by the fact that the program was abruptly downsized in 2003 (three years before the known expiration date). Indeed, standard dynamic models of investment behavior predict that pulled-forward investments should boom prior to the known date of expiration of the law. In any case, to detect evidence of time substitution we turn to data. In the anticipation scenario we should observe that the higher investment activity comes at the expenses of future accumulation. Since the bulk of the treatment was provided in 2001 and 2002, the investment of the treated firms should have slowed down subsequently. According the estimates provided in Bronzini and de Blasio (2006) for the other main incentive investment program (Law 488), the timing of the slow down is approximately from one to two years after the end of the program. As shown in Table 3, we find some evidence that time substitution has happened, as in 2003 and 2004 the investment of the treated group is lower than that of the non-eligible counterparts (the effects are also statistically significant). Clearly, since the net overtime effects estimated in Table 1 and 2 are positive, the initial investment increase triggered by the ITC is higher than the subsequent decrease. Note that our data end in 2004. Therefore, to the extent that the drop in accumulation might have occurred after 2004, we would be unable to disentangle an intertemporal substitution pattern.

A potential issue with our balanced panel of uninterrupted balance sheets is survivorship bias. In particular, there could be a differential loss of financial-statement availability for treated and untreated firms (see: Pakes and Ericson (1998)). Suppose that the effect of the ITC is that of keeping alive a marginal firm. In this scenario, marginal firms in the control groups go out of business, as they remain unsubsidized. Therefore, the estimates from the balanced panel could be negatively biased, because the marginal unsubsidized firms, which are likely to display the lowest accumulation rates, are no longer included in the comparison groups. To tackle this issue we construct an unbalanced panel, for the which we do not require the availability of the financial accounts over the entire period. We start by picking treated firms that have as a minimum two pre-intervention and two post-intervention adjacent sets of financial-statement data.[17] We are able to find 993 of such firms, compared to the 634 firms in the balanced panel. Then, firms in the controls groups are selected by the exact matching procedure explained above, in which we also take care of balance-sheet availability. To the extent that unsubsidized firms go out of business after a first stage of the post-intervention period, the unbalanced panel would include such firms (see Table A3 for the comparison between firms belonging to the different samples). Since the results with the unbalanced panel, shown in Table 4, are very close to the previous findings, we are keen to conclude that survivorship bias is not relevant.

As highlighted by Blundell et al (2004), systematic pre-treatment differences in the level of the dependent variable across comparison groups are a lesser concern, since they can be controlled for by difference-in-differences methods. However, failure of the parallel trend assumption would invalidate our estimates. To provide some robustness in this respect, we also run an additional experiment in which treated and controls are selected on the basis that they share the same growth rate of investment over a long pre-intervention period (we take 1996 2000). In this case, the comparison groups mirror the time-series pattern of investment of the treated group before the program took place. Results from this experiment (not reported for the sake of brevity) are also similar to those presented up to now.

[17] This is required because investment is measured as the difference in capital stock between period t and period t-1.

3.3 *Alternative experimental designs for non-random program placement*

So far, we tackled the non-random program placement issue by using region fixed effects and region time-varying controls. Clearly, even with these controls one cannot rest assured that all the possible omitted determinants of investment are differentiated away. Eligible areas are the regions of the South of Italy (see: Chart 1). An unobserved shock in the Southern regions of the country between the pre-and the post-ITC periods might be driving the apparent effect of the ITC. Below, we adopt three straightforward strategies to alleviate this concern. First, the impact of program eligibility is estimated for the few selected areas of the Centre and North of Italy covered under the program (jointly with the southern region most similar to the northern ones). For this sample, southern unobserved trends are absent or drastically reduced. Second, we try to approximate a control group of southern eligible firms for which the selection problem is arguably diminished, by using rejected applicants from another investment incentives program. Third, we exploit the fact that the intensity of the treatment differs across eligible regions. Note that in these last two cases, a possible unobserved shock in the South is differentiated away, as we estimate within southern regions. Moreover, in these two experiments we can directly estimate the effect due to participation rather than the effect due to eligibility.

First, we focus on subsidized and eligible-non-subsidized firms that are located in areas very similar to that of the non-eligible firms. To be sure, we compare firms for which the non-random program placement issue is minimized, as they belong to areas that share the same degree of economic and social development. For this experiment, we run the specification of equation (2) where the *TREAT* group includes the few (76) subsidized firms located in centre and north of Italy[18] and in the most advanced southern region (Abruzzo).[19] Correspondingly, the *ELEG* group includes (76) firms similar to the treated ones located in the same areas,[20] while the non-eligible firms include (75) firms located in the areas of centre and north of Italy different from those few areas deemed eligible. This experiment represents a intuitive version of the regression discontinuity design (Campbell (1969)), as firms with very close characteristics as for their local area are differently exposed to treatment. Table 5 describes the results and Table A4 presents the sample statistics. Overall, our previous findings remain confirmed: the estimated impact of program eligibility remains positive and highly significant, irrespective of how investment is measured

[18] The possibility to include in the ITC program firms located in selected areas of the centre and north of Italy is envisaged under the article 87.3.c of the 1997 Amsterdam Treaty.

[19] This is also formally recognized at the EU level. For instance, while currently southern regions still belong to the areas designated as Objective 1 (regions suffering from general underdevelopment) for the purpose of EU Structural Funds, Abruzzo lost its Objective 1 status in 1996.

[20] This was accomplished by exact matching..

The impact of participation in the ITC program, rather than that of eligibility, could be estimated if we were able to find a suitable control group. This group should include firms similar to those receiving the ITC. To be sure, the similarity should hold for the firm propensity to invest: comparison firms should display before the treatment the same willingness to invest that ITC-recipient firms. As argued above, because of the automatic award scheme envisaged for the fiscal credit, this comparison group is not apparently available. We try to approximate this comparison group, by turning to another program of incentives to investment: the Law 488. In contrast to Law 388, this law allows firms willing to invest to receive a grant. Crucially, under this program the award scheme is not automatic. Instead, grants are assigned through competitive auctions according to predetermined criteria, such as the proportion of firms' equity invested in the project, the number of jobs involved and the proportion of assistance sought. Two features of these scheme are particularly useful for our purposes. First, this scheme is not available for ITC recipients, as a firm cannot combine the two sources of aid.[21] Second, for this program we have natural candidates for the comparison group: rejected application firms (see: Bronzini and de Blasio (2006)).

The two programs are not immediately comparable, as the Law 488 covers only manufacturing and constructions and the respective areas of eligibility of the two programs do not overlap completely. This required some adjustments in the treatment group. Among the ITC-recipients we select only those that in principle could have applied for either programs (basically, manufacturing firms in Law 488 eligible areas). We end up with 354 treated firms. As for the untreated group, we take the Law 488 rejected applicants for the actions that took place after 2000. Note that in principle a Law 488 rejected applicant might resort to ITC the years after its Law 488 application was rejected. Alternatively, it can re-apply for the grants. In both cases, since we are able to identify these firms, they are excluded from the pool of rejected applicants. By implementing these restrictions, we select a comparison group of 354 firms by exact matching. As shown in Table A5, balancing properties are less convincing than previous cases. Some of the differences in observables between the two groups are not zero.[22] For instance, Law 488 rejected applicants display higher pre-treatment investment and interest costs and lower debt. With these caveats, we show in Table 6 the estimated interaction coefficients for the specification of equation

[21] The ban on combining Law 388 and Law 488 is already binding at the time of the application for the Law 488. Firms applying for the Law 488 have to give up to other sources of public subsidies. Similarly, firms that request the ITC cannot apply for the grants.

[22] In contrast to previous experiments, in this case the pool of candidates for being in the control sample is much more limited.

(1). We find that the investment of the ITC recipients outperforms that of the Law 488 rejected applicants and that the effects are statistically significant.

Finally, we exploit the fact that Law 388 envisages different amounts of ITC for different regions of eligibility (see: Section 2). If the ITC stimulates additional investment, then we should find that the higher the intensity of the treatment the greater the impact. For this experiment we focus only on southern firms, both subsidized and eligible non-subsidized (the estimation sample includes the *TREAT* and *ELEG* groups described in Table A2). Treated firms can be split in three groups according to the regional intensity of the ITC. The HIGH group includes (38) firms located in Calabria, entitled to receive an ITC amounting to 65% of the investment outlay. The LOW group includes (27) firms located in Abruzzo, which are entitled to receive an ITC of 30%. The omitted group comprises of firms located in the remaining southern regions, for the which the envisaged ITC is equal to 50%. Accordingly, eligible non-subsidized firms are taken to be located in the same area than their financed counterparts. The equation we estimate is a straightforward differences-in-differences-in-differences specification, in equation (4) below:

$$
\begin{aligned}
(4) \qquad Y_{ijt} = {} & a_1 + a_2 X_{ijt} + a_3\, TREAT_i + a_4\, HIGH_j + a_5\, LOW_j + a_6 POST_t \\
& + a_7 (TREAT_i * HIGH_j) + a_8\, (TREATi * LOW_j) + a_9\, (TREAT_i * POST_t) \\
& + a_{10}\, (HIGH_j * POST_t) + a_{11}\, (LOW_j * POST_t) \\
& + a_{12}\, (TREAT_i * HIGH_j * POST_t) + a_{13}\, (TREAT_i * LOW_j * POST_t) + \varepsilon_{ijt}
\end{aligned}
$$

The coefficients of interest in equation (4) are the coefficients on the triple interaction terms, *TREAT * HIGH * POST* and *TREAT * LOW * POST*. These coefficients measure the change between pre and post the introduction of the program in subsidized firms versus firms located in entitled areas but non subsidized in high- and low-regional ITC intensity, compared to firms in medium ITC intensity regions.

Table 7 shows the results. The evidence is again in favor of the effectiveness of the ITC. We find that the interaction coefficients display the expected sign, as *TREAT* * LOW * POST* enters negatively, while *TREAT * HIGH * POST* display a positive sign. Given the small number of observations in the three groups, however, some interaction coefficients are imprecisely measured.

3.4 *Side-effects*

Beyond its effect on investment, the ITC could have indirect effects on firm performance. For instance, Alesina et al (2001) argue that subsidies may foster a culture of rent-seeking , and this, in turn jeopardizes future efficiency. In addition, since the fiscal bonus subsidies capital it may cause allocative inefficiencies by encouraging a non-optimal mix of factors. Finally, the degree of credit rationing may vary as a result of the program (see: Albareto et al (2007)). The ITC is a source of financing alternative to debt. However, to the extent that the bonus activates investment in excess of the subsidy and the extra investment is financed through borrowing, firm's debt may also increase. Furthermore, changes in borrowing may bring about modifications in the interest rate faced by the firm. For instance, if the credit supply curve is negatively sloped, increases in debt should come hand in hand with a reduction in the cost of borrowing.

To make a first cut to these issues, in Table 8 we present results where we apply the regression frameworks described above and use a variety of financial statement indicators as dependent variables. In these experiments the dummy POST takes on the value of 1 for the years 2003 and 2004. As most of the treatment occurred in 2001 and 2002, this basically amounts to study the effect of the ITC on firm performance from one to two years after the intervention. Regarding profitability (Panel A), we find that the return on assets for treated firms does not differ significantly from that of their non-eligible counterparts. At the same time, profitability decreases significantly for eligible non-subsidized firms. Our results also suggest that factor inefficiency (Panel B) is a concern of second order. Indeed, labor cost over value added decreases for treated firms, indicating that a factor mix biased toward capital could have been the result of the ITC. Yet, the negative effect is not statistically significant.. As for the debt dynamics (Panel C), we find that the ratio of debt over assets for subsidized firms decreases more than that for eligible non-subsidized counterparts. This supports the view that ITC substitutes external borrowing. Finally, we also find that the cost of borrowing (Panel D) increases. Our findings on debt and interest rate patterns support the identification assumption of the paper. As argued by Banerjee and Duflo (2004), if the degree of credit rationing or the interest rate decrease as a result of the availability of the fiscal credit, then our estimates will erroneously attribute the variation in investment allowed by the higher availability (or lower cost) of external financing to the effects of the program. Again, notice that our window of data availability extend only to 2004 financial statements. Thus, side-effects of the program that materialize after that date are not captured.

4. Conclusions

This paper examines the effect on investment expenditures of the tax credit enacted by Italy's Law 388. The program envisages the ITC be assigned automatically on the basis of the firm's demand for the fiscal bonus. This implies that subsidies firms are self-selected and cannot be meaningfully compared with firms that do not request the ITC. To assess whether the program made investments possible that otherwise would not have been made, the paper exploits a number of discontinuity of the scheme envisaged under the law. For instance, the fact that some Italian areas in the Centre and North of Italy are not deemed to be entitled allows us to estimate the impact of program eligibility by comparing both subsidized and non-subsidized firms located in eligible areas to firms located in non-eligible areas. Likewise, the fact that the amounts of tax credit differs across eligible regions entitles us to compare firms receiving a relatively more generous fiscal bonus with firms receiving a less liberal treatment. Our results suggest that the program has been effective in stimulating investment. This conclusion is robust to a variety of robustness tests. Moreover, we fail to find evidence that the investment boost attributable to the ITC is due to time substitution or counterbalanced by negative side-effects on factor efficiency and profitability.

Two remarks are, however, in order.

First, the ITC implemented by Law 388 differs substantially for the other ITC programs implemented elsewhere in the world, mainly because it is not limited to profitable enterprises with tax liabilities. To be sure, the program is similar to an investment grant program, as it provides a direct government rebate to firms of a certain fraction of investment expenditures. The fact that the ITC program is not biased in favor of the most profitable firms, which most likely would have invested more even without subsidies, might be a reason behind its effectiveness.

Second, the scheme implemented by Law 388 has the obvious drawback that the amount of budget resources needed is not under control. This is particularly relevant for countries with public finance problems. The Italian experience in this respect is relevant, as a ceiling was imposed and the funding was downsized after two years of implementation. This represents a key warning for the development agencies that are considering to put into action similar incentive programs.

References

Abel, A. (1982), "Dynamic Effects of permanent and Temporary Tax Policies in a q Model of Investment," *Journal of Monetary Economics* 9, 353-373.

Adda, J. and Russell C. (2000) "Balladurette and Juppette: A Discrete Analysis of Scrapping Subsidies," *Journal of Political Economy* 108(4), 778-806.

Albareto, G., Bronzini R. de Blasio G. and Rassu R. (2007), "Evidence of Credit Constraints from an Investment Incentives Program," MIMEO, Bank of Italy.

Alesina, A., Danninger S. and Rostagno M. (2001), "Redistribution Through Public Employment: The Case of Italy," *IMF Staff Papers*, 48(3), 447-473.

Angrist, J.D. and Krueger A. B. (1999), "Empirical Strategy in Labor Economics", in O. Ashenfelter and D. Card (eds), *Handbook of Labor Economics*, Vol. IIIA, 1277-1366.

Auerbach, A.J. and Hines J. R. (1988) "Investment Tax Incentives and Frequent Tax Reforms," *American Economic Review* 78(2), 211-216.

Auerbach, A.J. and Summers L. H. (1979), "The Investment Tax Credit: An Evaluation", NBER, 404.

Auerbach, A.J. and Hassett K. (1992), "Tax Policy and Business Fixed Investment in the United States", *Journal of Public Economics*, 47(2), 141-170.

Banerjee, A.V. and Duflo E. (2004) "Do Firms Want to Borrow More? Testing Credit Constraints Using a Direct Lending Program," MIMEO, MIT.

Becker S. and Ichino A. (2002), "Estimation of Average Treatment Effects Based on Propensity Scores", *The Stata Journal,* 2(4), 358-377.

Bertrand, M., Duflo E. and Mullainathan S. (2004), "How Much Should We Trust Difference-in-Differences Estimates?", Quarterly Journal of Economics, 119(1), 249-275.

Blundell, R., Costa Dias M., Meghir C. and Reenen J.V. (2004) "Evaluating the Employment Impact of a mandatory Job Search Program," *Journal of the European Economic Association* 2(4), 569-606.

Bronzini, R. and de Blasio G. (2006), "Evaluating the Impact of Investment Incentives: The Case of Italy's Law 488/1992", *Journal of Urban Economics*, 60(2), 327-349.

Brown E. C. (1962), "Tax Incentives for Investment", American Economic Review, 52(2), 335-345.

Campbell, D. T. (1969) "Reforms as Experiments," *American Psychologist* 24, 407-429

Card D. (1999), "The Causal Effect of Education on Earnings", in O. Ashenfelter and D. Card (eds), Handbook of Labor Economics, Vol. IIIA, 1801-1864.

Cohen D. and Cummis J. (2006), “A Retrospective Evaluation of the Effects of Temporary Partial Expensing”, Finance and Economics Discussion Series 2006-19, Federal Reserve Board, Washington D. C.

Corte dei Conti (2004), “Crediti d’imposta per gli investimenti nelle aree svantaggiate”, Roma.

Cummis, J. G., Hassett K. A., and Hubbard G. R. (1994) “A Reconsideration of Investment Behavior Using Tax Reforms as Natural Experiments,” *Brooking Papers on Economic Activity* 2, 1-74.

Dehejia R. H. and Wahba S. (2002), “Propensity Score-Matching Methods for Nonexperimental Causal Studies”, *Review of Economics and Statistics*, 84(1), 151-61.

Goolsbee, A. (1998) “Investment Tax Incentives, Prices, and the Supply of Capital Goods,” *Quarterly Journal of Economics* 113(1),121-148.

Hall, R. E. and Jorgenson D.W. (1967) “Tax Policy and Investment Behavior” *American Economic Review* 57, 391-414.

House C. F. and Shapiro M. D. (2006), “Temporary Investment Tax Incentives Theory with Evidence from Depreciation”, Univ. of Michigan, Mimeograph.

Knittel, M. (2005) “Taxpayer Responses to Partial Expensing: Do Investment Incentives Work as Intended?” U:S: Department of Treasury Working Paper.

Lamont, O. (1997) “Cash Flow and Investment; Evidence from Internal Capital Markets,” *Journal of Finance* 52(1), 83-109.

Ministero delle Attività produttive (2002), “Relazione sugli interventi di sostegno alle attività economiche e produttive. Indagine sul credito d’imposta per le aree svantaggiate”. Roma.

Meyer B. D. (1995), “Natural and Quasi-Experiments in Economics”, *Journal of Business and Economic Statistics*, 13(2), 151-161.

Morduch J. (1998), “Does Microfinance Really Help the Poor? New Evidence from Flagship Programs in Bangladesh”, Harvard University, Mimeograph.

Pakes, A. and Ericson R. (1998) “Empirical Implication of Alternative Models of Firm Dynamics*,”* *Journal of Economic Theory* 79(1), 1-45

Romer, D. (2001), Advanced Macroeconomics. 2nd Edition. McGraw Hill.

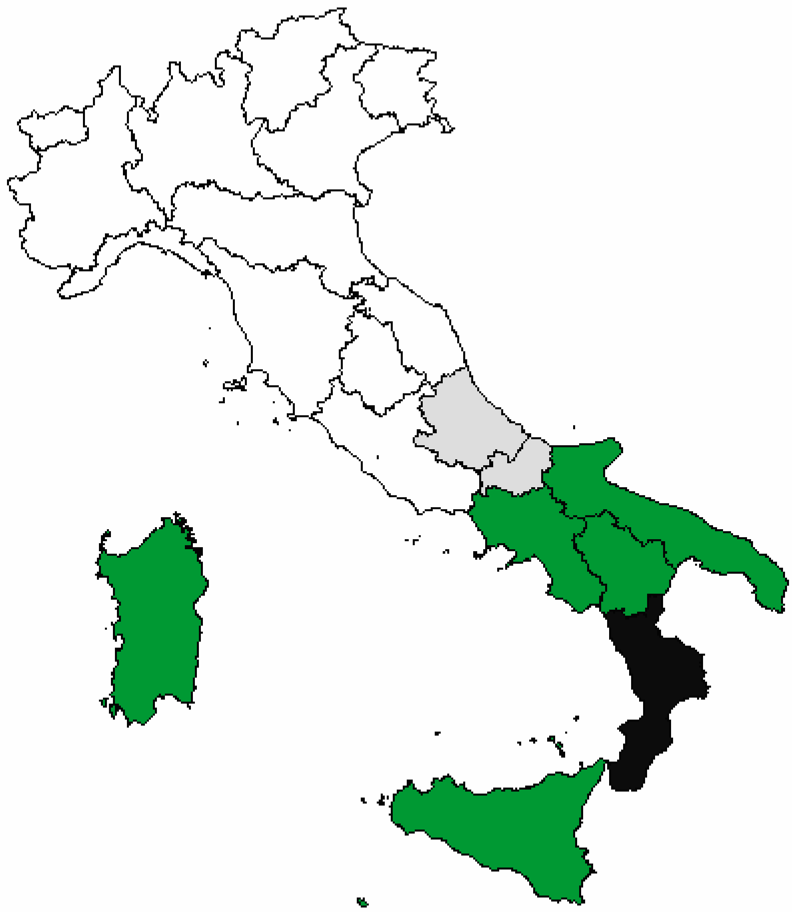
Rosenbaum P. R. and Rubin D. B. (1985), “Constructing a Control Group Using Multivariate Matched Sampling Methods that incorporate the Propensity Score”, *American Statistician*, 39, 33-38.

Winship, C. and Sobel, M. (2004), “Causal Inference in Sociological Studies” In M. Hardy (Ed.), The Handbook of Data Analysis, Thousand Oaks, CA:Sage

Wooldridge J. M. (2002), *Econometric analysis of Cross Section and Panel Data*, Cambridge M.A., MIT Press.

Chart 1

ITALIAN REGIONS BY ITC ELIGIBILITY AND INTENSITY



(1) Centre and Northern regions: non eligible (unless the few selected areas envisaged under the article 87.3.c of the 1997 Amsterdam Treaty, with ITC = 18%)

(2) Abruzzo (which includes Molise): eligible, with ITC = 30%

(3) Southern regions except Calabria and Abruzzo: eligible, with ITC = 50%

(4) Calabria: eligible, with ITC = 65%

Table 1

EFFECT OF PROGRAM ELIGIBILITY ON INVESTMENT

COMPARISON GROUPS SELECTED BY *PROPENSITY SCORE*

	(1)	(2)
	Panel A. Dependent Variable: I/K	
POST × TREAT	0.7294	0.6704
	*(0.0135)****	*(0.0660)***
	*[0.0141]****	*[0.0442]*
POST × ELEG	0.1578	0.0816
	*(0.0099)****	*(0.0578)****
	*[0.0116]****	*[0.0403]**
	Panel B. Dependent Variable: I/S	
POST × TREAT	0.0721	0.0689
	*(0.0011)****	*(0.0039)****
	*[0.0015]****	*[0.0029]****
POST × ELEG	0.0104	0.0058
	*(0.0011)***	*(0.0056)*
	*[0.0014]****	*[0.0038]*
	Panel C. Dependent Variable: I/A	
POST × TREAT	0.0826	0.0832
	*(0.0017)****	*(0.0086)***
	*(0.0018)****	*[0.0056]****
POST × ELEG	0.0156	0.0156
	*(0.0017)***	*(0.0085)*
	*(0.0015)****	*[0.0059]***
Region time-varying controls	NO	YES

Notes: All specifications include a dummy for TREAT, a dummy for ELEG, a dummy for POST, region fixed effects and firm time-varying controls. Robust standard errors clustered on treatment (eligibility, control) status are in parenthesis below coefficient estimates, and robust standard errors cluster on treatment (eligibility, control) status-post interactions are in square brackets below coefficient estimates. *** (**) [*] denotes significance at the 1% (5%) [10%] level. The treated sample includes 634 firms. The eligible sample includes 620 firms selected by propensity score (nearest neighbor matching). The non-eligible sample includes 633 firms selected by propensity score (nearest neighbor matching). See Table A.1 and equation (2) for further details.

Table 2

EFFECT OF PROGRAM ELIGIBILITY ON INVESTMENT

COMPARISON GROUPS SELECTED BY *EXACT MATCHING*

	(1)	(2)
	Panel A. Dependent Variable: I/K	
POST × TREAT	1.0557	1.0394
	*(0.0111)****	*(0.0372)****
	*[0.0098]****	*[0.0294]****
POST × ELEG	0.3568	0.3454
	*(0.0105)****	*(0.0273)***
	*[0.0083]****	*[0.0231]****
	Panel B. Dependent Variable: I/S	
POST × TREAT	0.0874	0.0838
	*(0.0012)****	*(0.0036)***
	*[0.0009]****	*[0.0026]****
POST × ELEG	0.0142	0.0100
	*(0.0007)***	*(0.0060)*
	*[0.0011]****	*[0.0044]**
	Panel C. Dependent Variable: I/A	
POST × TREAT	0.1259	0.1216
	*(0.0013)****	*(0.0011)****
	*[0.0011]****	*[0.0018]****
POST × ELEG	0.0356	0.0298
	*(0.0015)***	*(0.0032)***
	*[0.0015]****	*[0.0038]****
Region time-varying controls	NO	YES

Notes: All specifications include a dummy for TREAT, a dummy for ELEG, a dummy for POST, region fixed-effects and firm time-varying controls. Robust standard errors clustered on treatment (eligibility, control) status are in parenthesis below coefficient estimates, and robust standard errors cluster on treatment (eligibility, control) status-post interactions are in square brackets below coefficient estimates. *** (**) [*] denotes significance at the 1% (5%) [10%] level. The treated sample includes 634 firms. The eligible sample includes 623 firms selected by exact matching. The non-eligible sample includes 641 firms selected by exact matching. See Table A.2 and equation (2) for further details.

Table 3

YEAR-BY-YEAR EFFECTS OF PROGRAM ELIGIBILITY ON INVESTMENT

COMPARISON GROUPS SELECTED BY *PROPENSITY SCORE* AND *EXACT MATCHING*

	PROPENSITY SCORE				*EXACT MATCHING*			
	2001	2002	2003	2004	2001	2002	2003	2004
				Panel A. Dependent Variable: I/K				
POST × TREAT	0.8672	0.0503	-0.1565	-0.0291	1.0497	0.0078	-0.0615	-0.0735
	*(0.0304)****	*(0.0304)*	*(0.0304)***	*(0.0304)*	*(0.0140)****	*(0.0140)*	*(0.0140)***	*(0.0140)***
	*[0.0213]****	*[0.0213]**	*[0.0213]***	*[0.0213]*	*[0.0118]****	*[0.0118]*	*[0.0118]***	*[0.0118]***
POST × ELEG	0.0807	0.0565	0.0408	0.0131	0.0722	-0.0014	0.0499	0.1604
	(0.0291)	*(0.0291)*	*(0.0291)*	*(0.0291)*	*(0.0126)***	*(0.0126)*	*(0.0126)**	*(0.0126)***
	*[0.0212]***	*[0.0212]**	*[0.0212]*	*[0.0212]*	*[0.0099]****	*[0.0099]*	*[0.0099]***	*[0.0099]****
				Panel B. Dependent Variable: I/S				
POST × TREAT	0.0640	0.0064	-0.0103	-0.0100	0.0529	0.0013	0.0067	-0.00004
	*0.0008****	*0.0008***	*0.0008****	*0.0008****	*(0.0019)****	*(0.0019)*	*(0.0019)**	*(0.0019)*
	*0.0007****	*0.0007****	*0.0007****	*0.0007****	*[0.0012]****	*[0.0012]*	*[0.0012]***	*[0.0012]*
POST × ELEG	0.0025	-0.0086	-0.0117	0.007	-0.0021	-0.0200	0.0002	0.0156
	*0.0007**	*0.0007****	*0.0007****	*0.0007***	*(0.0026)*	*(0.0026)***	*(0.0026)*	*(0.0026)***
	*0.0006***	*0.0006****	*0.0006****	*0.0006****	*[0.0017]*	*[0.0017]****	*[0.0017]*	*[0.0017]****
				Panel C. Dependent Variable: I/A				
POST × TREAT	0.0697	0.0148	-0.0122	-0.0074	0.0681	0.0272	0.0060	-0.0030
	*0.0039****	*0.0039**	*0.0039**	*0.0039****	*(0.0010)****	*(0.0010)****	*(0.0010)***	*(0.0010)**
	*0.0027****	*0.0027****	*0.0027****	*0.0027***	*[0.0008]****	*[0.0008]****	*[0.0008]****	*[0.0008]***
POST × ELEG	0.0150	-0.0013	-0.0112	0.011	-0.0027	-0.0019	0.0039	0.0090
	*0.0037**	*0.0037*	*0.0037**	*0.0037**	*(0.0006)***	*(0.0006)*	*(0.0006)***	*(0.0006)***
	*0.0028****	*0.0028*	*0.0028***	*0.0028***	*[0.0011]**	*[0.0011]*	*[0.0011]***	*[0.0011]****

Notes : All specifications include a dummy for TREAT, a dummy for ELEG, time dummies, region fixed-effects, region time-varying controls and firm time-varying controls. Robust standard errors clustered on treatment (eligibility, control) status are in parenthesis below coefficient estimates, and robust standard errors cluster on treatment (eligibility, control) status-post interactions are in square brackets below coefficient estimates. *** (**) [*] denotes significance at the 1% (5%) [10%] level. The treated sample includes 634 firms. The eligible sample selected by propensity score (exact matching) includes 620 (623) firms. The non-eligible sample selected by propensity score (exact matching) includes 633 (641) firms.. See Table A.1, Table A2, and equation (3) for further details.

Table 4

EFFECT OF PROGRAM ELIGIBILITY ON INVESTMENT

UNBALANCED PANEL

	(1)	(2)
	Panel A. Dependent Variable: I/K	
POST × TREAT	0.6511	0.6868
	*(0.0225)****	*(0.0490)***
	*[0.0189]****	*[0.0404]****
POST × ELEG	-0.0353	0.0097
	*(0.0106)**	*(0.0503)*
	*[0.0106]***	*[0.0388]*
	Panel B. Dependent Variable: I/S	
POST × TREAT	0.0615	0.0531
	*(0.0029)***	*(0.0011)****
	*[0.0021]****	*[0.0027]****
POST × ELEG	0.0006	-0.0102
	(0.0007)	*(0.0022)***
	[0.0014]	*[0.0052]*
	Panel C. Dependent Variable: I/A	
POST × TREAT	0.0807	0.0823
	*(0.0029)***	*(0.0092)***
	*[0.0024]****	*[0.0063]****
POST × ELEG	-0.0057	-0.0039
	*(0.0007)***	*(0.0094)*
	*[0.0011]***	*[0.0078]*
Region time-varying controls	NO	YES

Notes: All specifications include a dummy for TREAT, a dummy for ELEG, a dummy for POST, region fixed-effects and firm time-varying controls. Robust standard errors clustered on treatment (eligibility, control) status are in parenthesis below coefficient estimates, and robust standard errors cluster on treatment (eligibility, control) status-post interactions are in square brackets below coefficient estimates. *** (**) [*] denotes significance at the 1% (5%) [10%] level. The treated sample includes 993 firms. The eligible sample includes 962 firms selected by exact matching. The non-eligible sample includes 988 firms selected by exact matching. See Table A.3 and equation (2) for further details.

Table 5

EFFECT OF PROGRAM ELIGIBILITY ON INVESTMENT

COMPARISON GROUPS LOCATED IN CENTRE-NORTH ITALY AND ABRUZZO

	Panel A. Dependent Variable: I/K
POST × TREAT	0.7101
	(0.2642)
	*[0.2015]****
POST × ELEG	-0.6119
	*(0.1363)***
	*[0.0877]****
	Panel B. Dependent Variable: I/S
POST × TREAT	0.1534
	*(0.0332)***
	*[0.0248]***
POST × ELEG	-0.0667
	*(00084)***
	*[0.0074]****
	Panel C. Dependent Variable: I/A
POST × TREAT	0.1784
	*(0.0361)***
	*[0.0275]****
POST × ELEG	-0.0318
	*(0.0093)**
	*[0.0073]***

Notes: All specifications include a dummy for TREAT, a dummy for POST, region fixed-effects, region time-varying controls, and firm time-varying controls. Robust standard errors clustered on treatment (control) status are in parenthesis below coefficient estimates, and robust standard errors cluster on treatment (control) status-post interactions are in square brackets below coefficient estimates. *** (**) [*] denotes significance at the 1% (5%) [10%] level. The treated sample includes 76 firms located in the Centre and North of Italy and Abruzzo. The eligible sample includes 76 firms located in the eligible areas of Centre and North of Italy and Abruzzo selected by exact matching. The control sample includes 75 non-eligible firms located in the non-eligible areas of the centre and north of Italy selected by exact matching. See Table A.4 and equation (2) for further details.

Table 6

EFFECT OF THE PROGRAM PARTICIPATION ON INVESTMENT

COMPARISON GROUP MADE UP OF LAW 488 REJECTED APPLICANTS

	Panel A. Dependent Variable: I/K
POST × TREAT	5.5747
	*(0.6265)**
	*[0.3654]****
	Panel B. Dependent Variable: I/S
POST × TREAT	1.4495
	(0.5872)
	*[0.4364]***
	Panel C. Dependent Variable: I/A
POST × TREAT	0.1392
	*(0.0056)***
	*[0.0038]****

Notes: All specifications include a dummy for TREAT, a dummy for POST, region fixed-effects, region time-varying controls, and firm time-varying controls. Robust standard errors clustered on treatment (control) status are in parenthesis below coefficient estimates, and robust standard errors cluster on treatment (control) status-post interactions are in square brackets below coefficient estimates. *** (**) [*] denotes significance at the 1% (5%) [10%] level. The treated sample includes 354 firms. The control sample includes 354 firms selected by exact matching among the Law 488 rejected applicants. See Table A.5 and equation (1) for further details.

Table 7

EFFECT OF THE PROGRAM PARTICIPATION ON INVESTMENT

REGIONAL INTENSITY OF TREATMENT

	Panel A. Dependent Variable: I/K
POST × TREAT × LOW	-0.1571
	(0.1283)
	[0.1077)
POST × TREAT × HIGH	1.1732
	*(0.0123)****
	*[0.0139]****
	Panel B. Dependent Variable: I/S
POST × TREAT × LOW	-0.0185
	(0.0095)
	*[0.0085]**
POST × TREAT × HIGH	0.1847
	*(0.0006)****
	*[0.0006]****
	Panel C. Dependent Variable: I/A
POST × TREAT × LOW	-0.0655
	*(0.0165)***
	*[0.0127]****
POST × TREAT × HIGH	0.1545
	*(0.0011)****
	*[0.0009]****

Notes: All specifications include a dummy for TREAT, a dummy for POST, dummies for the regional intensity of aid, interactions between the dummies for the regional intensity of aid and TREAT, interactions between the dummies for the regional intensity of aid and POST, interaction between TREAT and POST, region fixed-effects, region time-varying controls and firm time-varying controls. Robust standard errors clustered on treatment (control) status-post interactions are in parenthesis below coefficient estimates, and robust standard errors cluster on treatment (control) status-regional intensity of aid-post interactions are in square brackets below coefficient estimates. *** (**) [*] denotes significance at the 1% (5%) [10%] level. (1). The treated sample includes 634 firms. The control sample includes 623 eligible firms selected by exact matching. By propensity score we selected 638 treated firms and 620 eligible firms. See Table A.2 and equation (4) for further details.

Table 8

SIDE-EFFECTS ON FIRM PERFORMANCE

	Panel A. Dependent Variable: ROA
POST × TREAT	0.0025
	(0.0021)
	[0.0014]
POST × ELEG	-0.0246
	*(0.0022)****
	*[0.0014]*****
	Panel B. Dependent Variable: Labor cost/value added
POST × TREAT	-0.0419
	(0.1208)
	[0.0839]
POST × ELEG	0.3275
	(0.1291)
	*[0.0902]****
	Panel C. Dependent Variable: Debt/Assets
POST × TREAT	-0.0453
	*(0.0048)****
	*[0.0031]*****
POST × ELEG	-0.0180
	*(0.0049)**
	*[0.0031]****
	Panel D. Dependent Variable: Interest cost/Debt
POST × TREAT	0.0044
	*(0.0004)****
	*[0.0009]****
POST × ELEG	0.0014
	*(0.0004)**
	[0.0009]

Notes: All specifications include a dummy for Treat, a dummy for Post, region fixed-effects, region tim,e-varying controls, and firm time-varying controls. Robust standard errors clustered on treatment (control) status are in parenthesis below coefficient estimates, and robust standard errors clustered on treatment (control) status-post interactions are in square brackets below coefficient estimates. *** (**) [*] denotes significance at the 1% (5%) [10%] level. The treated sample includes 634 firms. The eligible sample includes 623 firms selected by exact matching. The non-eligible sample includes and 641 firms selected by exact matching.. See Table A.2 and equation (2) for further details.

Table A1

DESCRIPTIVE STATISTICS AND MEAN DIFFERENCES FOR THE EXPERIMENT OF TABLES 1 AND 4

	Mean and standard deviation			Mean differences		
	TREAT	ELEG	NELE	TREAT vs ELEG	TREAT vs NELE	ELEG vs NELE
Investments/Capital	1.3179	0.9733	1.0278	0.0701	-0.0743	-0.1444
	(2.8286)	*(2.6618)*	*(3.0564)*	*0.1396)*	*(0.1499)*	*(0.1508)*
Investment/Sales	0.1111	0.0762	0.0608	0.0041	0.0137	0.0095
	(0.3140)	*(0.3491)*	*(0.2827)*	*(0.0169)*	*(0.0147)*	*(0.0155)*
Investment/Assets	0.1334	0.0801	0.0775	0.0181	0.0139	-0.0042
	(0.3419)	*(0.3072)*	*(0.3289)*	*(0.0161)*	*(0.0151)*	*(0.0142)*
Sales	2841.05	2095.3	2644.2	480.70	-24.716	-505.42
	(7164.5)	*(4828.4)*	*(6733.2)*	*(265.10)**	*(319.84)*	*(303.22)**
Cash flow/Assets	0.0791	0.0691	0.0751	-0.0013	-0.0008	0.0004
	(0.0768)	*(0.0977)*	*(0.0801)*	*(0.0049)*	*(0.0045)*	*(0.0049)*
Interest cost/Debt	0.0262	0.0255	0.0259	0.0013	0.0007	0.0003
	(0.0223)	*(0.0250)*	*(0.0228)*	*(0.0013)*	*(0.0012)*	*(0.0013)*
GOM/Value added	0.3598	0.3385	0.4317	-0.0377	-0.1250	-0.0872
	(1.6772)	*(1.2301)*	*(1.7538)*	*(0.0737)*	*(0.0540)***	*(0.0588)*
Debt/Assets	0.7155	0.7323	0.7092	0.0082	0.0204	0.0122
	(0.2108)	*(0.2531)*	*(0.2168)*	*(0.0130)*	*(0.0118)**	*(0.0128)*
ROA	0.02171	0.0111	0.0200	0.000	-0.0031	-0.0036
	(0.0679)	*(0.0900)*	*(0.0721)*	*(0.0043)*	*(0.0041)*	*(0.0043)*

Notes: The TREAT sample includes 638 firms. The ELEG sample includes 620 firms selected by propensity score (nearest neighbor matching). The NELE sample includes 633 firms selected by propensity score (nearest neighbor matching). Standard deviations in parenthesis below means. Standard errors of the mean differences in square brackets. *** (**) [*] denotes significance at the 1% (5%) [10%] level.

Table A2

DESCRIPTIVE STATISTICS AND MEAN DIFFERENCES FOR THE EXPERIMENT OF TABLES 2 AND 4

	Mean and standard deviation			Mean differences		
	TREAT	ELEG	NELE	TREAT vs ELEG	TREAT vs NELE	ELEG vs NELE
Investment/Capital	1.0640	0.9895	0.9995	0.0745	0.0645	-0.0099
	(2.7281)	*(2.6050)*	*(2.5431)*	*(0.1505)*	*(0.1476)*	*(0.1448)*
Investment/Sales	0.0828	0.0754	0.0637	0.0074	0.0191	0.0116
	(0.2872)	*(0.2780)*	*(0.2663)*	*(0.0159)*	*(0.0155)*	*(0.0153)*
Investment/Assets	0.0915	0.0880	0.0769	0.0034	0.0145	0.0110
	(0.3024)	*(0.2758)*	*(0.2826)*	*(0.0163)*	*(0.0163)*	*(0.0157)*
Sales	2364.875	2197.518	2314.376	167.3564	50.4988	-116.8575
	(5100.268)	*(4638.192)*	*(4964.266)*	*(275.1108)*	*(281.8724)*	*(270.404)*
Cash flow/Assets	0.0791	0.0761	0.0789	0.0030	0.0001	-0.0028
	(0.0796)	*(0.0749)*	*(0.0817)*	*(0.0043)*	*(0.0045)*	*(0.0044)*
Interest cost/Debt	0.0265	0.0298	0.0298	-0.0032	-0.0033	-0.00007
	(0.0245)	*(0.0249)*	*(0.0257)*	*(0.0013)***	*(0.0014)***	*(0.0014)*
GOM/Value added	0.3372	0.4733	0.4391	-0.1360	-0.1018	0.0342
	(1.2457)	*(1.6444)*	*(0.7231)*	*(0.0822)**	*(0.0569)**	*(0.0711)*
Debt/Assets	0.7469	0.7536	0.7333	-0.0066	0.0135	0.0202
	(0.2127)	*(0.1989)*	*(0.2099)*	*(0.0116)*	*(0.0118)*	*(0.0115)**
ROA	0.0222	0.0202	0.0164	0.0020	0.0057	0.0037
	(0.0720)	*(0.0606)*	*(0.0685)*	*(0.0037)*	*(0.0039)*	*(0.0036)*

Notes: The TREAT sample includes 634 firms. The ELEG sample includes 623 firms selected by exact matching. The NELE sample includes 641 firms selected by exact matching. Standard deviations in parenthesis below means. Standard errors of the mean differences in square brackets. *** (**) [*] denotes significance at the 1% (5%) [10%] level.

Table A3

DESCRIPTIVE STATISTICS AND MEAN DIFFERENCES FOR THE EXPERIMENT OF TABLES 3

	Mean and standard deviation			Mean differences		
	TREAT	ELEG	NELE	TREAT vs ELEG	TREAT vs NELE	ELEG vs NELE
Investments/Capital	1.2236	1.0407	1.0199	0.1829	0.2037	0.0207
	(3.0564)	*(2.9534)*	*(2.8213)*	*(0.1359)*	*(0.1321)*	*(0.1307)*
Investment/Sales	0.1353	0.1032	0.0979	0.0320	0.0374	0.0053
	(0.4631)	*(0.4207)*	*(0.4234)*	*(0.0200)*	*(0.0199)**	*(0.0191)*
Investment/Assets	0.1218	0.0886	0.0691	0.0332	0.0526	0.0194
	(0.4344)	*(0.3650)*	*(0.2094)*	*(0.0181)**	*(0.0153)****	*(0.0134)*
Sales	2255.899	1860.85	2340.582	395.049	-84.683	-479.732
	(6304.773)	*(5351.643)*	*(5433.429)*	*(264.882)*	*(264.505)*	*(244.248)***
Cash flow/Assets	0.0817	0.0733	0.0731	0.0084	0.0085	0.0001
	(0.0825)	*(0.0732)*	*(0.0750)*	*(0.0035)***	*(0.0036)***	*(0.0033)*
Interest cost/Debt	0.0246	0.0238	0.0249	0.0007	-0.0003	-0.0010
	(0.0231)	*(0.0214)*	*(0.0213)*	*(0.0010)*	*(0.0010)*	*(0.0009)*
GOM/Value added	0.3977	0.4371	0.4055	-0.0394	-0.0077	0.0316
	(1.5192)	*(0.9489)*	*(1.9158)*	*(0.0574)*	*(0.0776)*	*(0.0687)*
Debt/Assets	0.7561	0.7511	0.7463	0.0050	0.0098	0.0048
	(0.2055)	*(0.2114)*	*(0.2113)*	*(0.0094)*	*(0.0093)*	*(0.0095)*
ROA	0.0249	0.0229	0.0223	0.0019	0.0026	0.0006
	(0.0708)	*(0.0603)*	*(0.0667)*	*(0.0029)*	*(0.0030)*	*(0.0028)*

Notes: The TREAT sample includes 993 firms. The ELEG sample includes 962 firms selected by propensity score (nearest neighbor matching). The NELE sample includes 988 firms selected by propensity score (nearest neighbor matching). Standard deviations in parenthesis below means. Standard errors of the mean differences in square brackets. *** (**) [*] denotes significance at the 1% (5%) [10%] level.

Table A4

DESCRIPTIVE STATISTICS AND MEAN DIFFERENCES FOR THE EXPERIMENT OF TABLE 5

	Mean and standard deviation			Mean differences		
	TREAT	ELEG	NELE	TREAT vs ELEG	TREAT vs NELE	ELEG vs NELE
Investment/Capital	1.7190	0.7905	1.1788	0.9284	0.5402	-0.3882
	(6.5861)	*(3.0625)*	*(3.3271)*	*(0.8331)*	*(0.8509)*	*(0.5202)*
Investment/Sales	0.0527	0.0664	0.1008	-0.0137	-0.0481	-0.0344
	(0.1463)	*(0.2807)*	*(0.2594)*	*(0.0363)*	*(0.0342)*	*(0.0440)*
Investment/Assets	0.0768	0.0864	0.1141	-0.0095	-0.0373	-0.0277
	(0.1914)	*(0.3642)*	*(0.3069)*	*(0.0472)*	*(0.0415)*	*0.0548*
Sales	17725.48	12311.36	15868.56	5414.118	1856.918	-3557.200
	(52188.42)	*(42564.25)*	*(45876.98)*	*(7724.99)*	*(8000.575)*	*(7200.641)*
Cash flow/Assets	0.0876	0.0825	0.0746	0.0050	0.0129	0.0078
	(0.0634)	*(0.0724)*	*(0.0757)*	*(0.0110)*	*(0.0113)*	*(0.0120)*
Interest cost/Debt	0.0336	0.0294	0.0301	0.0042	0.0035	-0.0007
	(0.0322)	*(0.0256)*	*(0.0186)*	*(0.0047)*	*(0.0043)*	*(0.0036)*
GOM/Value added	0.0279	0.4323	0.4900	-0.4043	-0.4620	-0.0577
	(2.9533)	*(0.2953)*	*(0.7973)*	*(0.3404)*	*0.3530*	*(0.0976)*
Debt/Assets	0.6950	0.7077	0.7257	-0.0126	-0.0306	-0.0179
	(0.1978)	*(0.2152)*	*(0.2402)*	*(0.0335)*	*(0.0357)*	*(0.0371)*
ROA	0.0190	0.0147	0.0124	0.0042	0.0065	0.0023
	(0.0417)	*(0.0533)*	*(0.0470)*	*(0.0077)*	*(0.0072)*	*(0.0081)*

Notes: The TREAT sample includes 76 firms located in the Centre and North of Italy and Abruzzo. The ELEG sample includes 76 firms located in the eligible areas of the Centre and North of Italy and Abruzzo selected by exact matching. The NELE sample includes 75 firms located in the non-eligible areas of the Centre and North of Italy selected by exact matching. Standard deviations in parenthesis below means. Standard errors of the mean differences in square brackets. *** (**) [*] denotes significance at the 1% (5%) [10%] level.

Table A5

DESCRIPTIVE STATISTICS AND MEAN DIFFERENCES FOR THE EXPERIMENT OF TABLE 6

	Mean and standard deviation		Mean differences
	TREAT	LAW 488 REJECTED FIRMS	
Investment/Capital	2.2097	4.6320	-2.4222
	(11.8534)	*(36.7959)*	*(2.0546)*
Investment/Sales	1.9835	3.1287	-1.1451
	(34.6561)	*(28.4265)*	*(2.3823)*
Investment/Assets	0.1092	0.2590	-0.1497
	(0.3426)	*(0.6895)*	*(0.0405)****
Sales	3361.718	2106.782	1254.797
	(14295.1)	*(1630.385)*	*(797.9789)*
Cash flow/Assets	0.0793	0.0764	0.0028
	(0.0723)	*(0.1073)*	*(0.0068)*
Interest cost/Debt	0.0247	0.0300	-0.0053
	(0.0201)	*(0.0207)*	*(0.0015)****
GOM/Value added	0.3247	0.4403	-0.1156
	(0.2469)	*(1.6787)*	*(0.0901)*
Debt/Assets	0.7337	0.6720	0.0616
	(0.2097)	*(0.2196)*	*(0.0161)****
ROA	0.0212	0.0147	0.0064
	(0.0594)	*0.1083*	*(0.0065)*

Notes: The TREAT sample includes 354 firms. The control sample includes 354 Law 488 rejected applicants. Standard deviations in parenthesis below means. Standard errors of the mean differences in square brackets. *** (**) [*] denotes significance at the 1% (5%) [10%] level.