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THE EFFECT OF INVESTMENT TAX CREDIT:
EVIDENCE FROM AN ATYPICAL PROGRAMME IN ITALY

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**THE EFFECT OF INVESTMENT TAX CREDIT:
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Abstract

This paper examines how business investment responds to investment tax credit, as enacted by Italy's Law 388/2000. To assess whether the programme 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 eligible or that the amount of the bonus differs across eligible regions. Although the programme was fiscally unsustainable, and was therefore downsized well ahead of the expiry date, our findings suggest that it has been effective in stimulating investment.

JEL Classification: E22; H25.
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1 Introduction[1]

This paper evaluates the impact of investment tax credit (ITC) on business investment using a unique policy experiment provided by the Italian Government's ITC programme implemented through Law 388/2000. Like other ITC schemes, this measure reduces the cost to firms of acquiring capital without altering the returns from that capital. Unlike other ITC schemes, the tax credit programme we focus on is atypical, as the bonus envisaged is not restricted to profitable enterprises with tax liability. Indeed, the credit can be deducted from any outstanding payment due to central government (even social security contributions or tax paid by workers and temporarily held by the firm). The programme introduced with Law 388 has three additional features: (i) only some regions are eligible for it, as the majority of areas in the Centre and North of Italy are not entitled; (ii) the amount of tax credit differs across areas of eligibility, and the amount of the tax deduction envisaged decreases with the level of local development; and (iii) the financing of the scheme is characterized by a time discontinuity: having been enacted in December 2000, the programme was originally supposed to stay in place until December 2006; in 2003, however, owing 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 investigated the theory of the dynamic effects of ITC on the desired stock of capital (see, for instance, Abel, 1982, and Auerbach and Hassett, 1992). The main implication of this work is that, compared with a permanent investment tax credit, a measure known to be temporary gives firms a stronger incentive to invest while the credit is in effect. Empirical investigations have been less uncontroversial, however. For instance, Goolsbee (1998) presents evidence that an ITC programme pushed up the 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 expenditure.

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This paper assesses whether the tax credit programme made investments possible that otherwise would not have been made. This is not a simple task. The ITC is assigned on the basis of the firm's demand for the bonus, conditional on being located in an eligible region. It is difficult, therefore, to find a suitable control group; that is, a group of firms similar to the firms that receive the ITC in all respects except receipt of the bonus. In particular, two selection biases might apply: self-selection into the programme, perhaps by the most profitable firms, and non-random programme placement, as the programme favours disadvantaged areas.

The empirical strategy is designed to tackle the above selection biases. We start by estimating *eligibility*, that is the impact of having access to an ITC regime compared with not having it (van der Klaauw, 2007). To do so, we exploit the fact that under the scheme some areas are not entitled and we compare both subsidized and non-subsidized firms located in eligible areas with firms located in non-eligible areas. We find that compared with non-entitled firms, the additional investment triggered by programme exposure is economically large and highly statistically significant. This conclusion is robust to the way the comparison groups are selected and investment is measured. Moreover, the over-time impact of ITC follows the variations in the budget allocated to the programme: in 2003, when the budget is reduced, the estimated yearly effect slumps.

To make sure that our estimates are not driven by non-random programme placement, we implement an intuitive version of the regression discontinuity design (Campbell, 1969) by focusing on subsidized and eligible-non-subsidized firms located in areas very similar to the non-eligible areas. In addition, we study alternative experimental designs that focus only on firms located in eligible areas: (i) 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 programme; (ii) we exploit the fact that the amounts of tax credit differ across eligible regions and we compare firms receiving a relatively more generous fiscal bonus with firms receiving less liberal treatment (in these cases, the approach amounts to estimating *participation*, that is the impact of having received the ITC).

All the above empirical strategies point to the same conclusion: the ITC programme has been effective in stimulating investment. Moreover, as we show, 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. The effectiveness of the programme (its capacity to trigger additional investment) might have something to do with the fact that the ITC is not limited to profitable firms with tax liabilities: as those firms are less likely to be credit rationed they are also more prone to undertake the same amount of investment even without tax credit. However, the lessons to be learnt from this programme are greater than its effect on investment. The major drawback of the scheme is that the amount of fiscal resources needed is not under control (in the Italian experience, this lack of fiscal sustainability was the reason why the programme 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 Programme

This section explains the main features of the ITC programme enacted by Law 388. We focus on the aspects that are most relevant to our empirical analysis (more details can be found in Ministero delle Attività Produttive, 2002). The aim of the programme is to spur capital accumulation in Italy's lagging areas, as identified by the European Commission (see Article 87.3, points a) and c), of the European Treaty as amended in 1997 in Amsterdam).

This programme is atypical. 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 a firm's corporate tax charges, but also from VAT outstanding and social security contributions. Moreover, it can be subtracted from the amounts of income tax and social security contributions paid by workers and held temporarily by the firm (in Italy, the firm acts as withholding agent for these amounts on behalf of workers). In this respect, the programme is similar to an investment grant programme, as it provides firms with a direct government rebate of a certain fraction of investment expenditures (see Auerbach and Summers, 1979).

The programme envisages that firms investing in the South of Italy and few selected areas of the Centre and North are granted a tax exemption as a percentage of their annual net capital expenditure. Both manufacturing and service firms are eligible under the programme.[2] There are only minor restrictions as to the categories of investment goods covered. Basically, all tangible and intangible capital goods are included, with the only exception of advertising, goodwill and R&D expenditure, office furniture and vehicles for third-part transportation.

[2] Agricultural firms are also eligible. However, there are none in the dataset we use to estimate the impact of the programme (see below).

There are different areas of ITC intensity, as percentages of tax deductions vary by region (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[3] (the relatively most developed region in the South), where it amounts to 30%. For the few selected areas of the Centre and North that are eligible under the programme the fiscal bonus is equal to 18% of capital expenditure.[4]

The programme, which was enacted in December 2000 (see Article 8, Legge Finanziaria 388, December 2000), started in 2001 and was originally supposed to stay in place until December 2006; however, in August 2002 (see Law 178/ 2002) it became clear that the automatic character of the scheme was not compatible with the limits on the government's budget. An annual ceiling on the overall resources for the ITC was therefore imposed. To make sure that the amounts granted remained below the ceiling, it was decided that the requests should be subject to ex-ante approval by the tax office. Applications for the fiscal bonus were dealt with on a first-come-first-served basis, within the budgetary limits. The new rules began to be followed at the end of 2002. Their effect became evident in 2003, when the budget allocated to the ITC initiative was drastically reduced (to 650 million euros, from 1,700 million euros the year before; see Corte dei Conti, 2004).

The timing of the programme was surrounded by considerable uncertainty. The ITC was launched quite abruptly at the end of 2000, and the fact that it was not expected minimized the scope for firms to postpone investment in order to benefit from it. This helps us to identify the *true* share of additional investment triggered by the measure; that is, the share net of that due to postponement. As regards the supposed duration, the circumstance that the programme was downsized in August 2002, as explained above, reduces the potential bias deriving from firms bringing forward investment projects originally planned for the post-programme period, as these investments accelerate just before the known expiry date (Romer, 2001). Again, this facilitates identification, as it reasonably reduces the bias due to anticipation.

Finally, there is an important aspect to the programme from the viewpoint of the evaluation exercise. The estimation results we present below are based on the assumption that there are no other governmental programmes correlated with the allocation of the ITC programme. For instance, if the firms that receive the fiscal bonus also obtain other types of financial assistance, then our comparison will overestimate the effect of the programme. A feature of Law 388

[3] With Law 178/2002, the regional intensity of ITC for these regions was changed from 50% to 42.5%.

[4] Tax deductions for large firms are less generous (respectively, 50%, 35%, 20%, and 8% in Calabria, the remaining southern regions except Abruzzo, Abruzzo, and the eligible northern areas). Note, however, that no large firms are included in the dataset we use for estimation (see below).

minimizes the scope of this bias, as the ITC cannot be combined with other sources of public financing.[5] 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 programme, could in principle receive the grants envisaged under another incentive programme, Law 488. Fortunately, we are able to identify the firms that obtain assistance under Law 488, and they are excluded from the comparison groups.[6]

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, which covers the period 2001-2004 and provides the tax identification number (TIN) and location of each firm. There is no information about the timing of the receipt of the ITC, however: 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 the data with financial statement information taken 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 of the Cerved data is that there are frequent misprints of the TIN, which we use to link financial statements to 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[7] that received the ITC.[8] 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).[9]

[5] Law 178/2002 lifted the ban on combining the ITC with other incentive programmes, but only for the incentives envisaged under the so-called Tremonti-bis Law (Law 383/2001). This does not affect our results. Tremonti-bis incentives applies automatically to all firms, both eligible and non-eligible under the ITC scheme, and thus its effect is differentiated away in our diff-in-diffs framework (see below).

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

[7] Eligible areas are the regions of the South of Italy. However, a few selected areas in the Centre and North are also entitled to receive assistance under the programme. In our dataset we have data for 76 financed firms located in the Centre and North, which are used in the experiment in Table 6, below.

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

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

3.2 Empirical design

Empirically, we adopt a difference-in-differences framework (see, for example, Angrist and Krueger, 1999; Card, 1999; and Meyer, 1995) and try to find a control group that is as comparable as possible with the treatment group. If we can 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 will 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 located in 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 programme. In this specification, the coefficient of interest will 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 part of the policy-maker. 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 basis of the timing of the request and budget constraints). A subsidized firm is self-selected and cannot be compared with a non-subsidized firm without introducing the possibility of bias. Let us take a firm located in an entitled area that has not received the ITC. This firm has no incentive to invest notwithstanding the tax deduction. Thus, it self-selects out of the pool of participants, and the comparison of benefited firms versus non-benefited firms will be biased upwards. By the same token, a subsidized firm cannot be compared with a non-eligible firm, since we cannot be sure that the latter would have invested, and thus received the ITC, had it been located in an entitled area. In short, it is difficult to evaluate the effect of ITC on subsidized firms, since it is hard to disentangle the treatment effect from the self-selection bias. In this circumstances, a more promising approach (see van der Klaauw, 2007, and Angrist and Imbens, 1991) is to compare both subsidized and non-subsidized firms located in eligible areas to firms located in areas not deemed eligible. In this case, differences in outcomes reflect the presence of the programme 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 run 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 in firms located in entitled areas but not subsidized, compared with 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 having access to the ITC.

A key challenge is to find convincing control groups. Below, we select comparison groups by two different methods: *propensity score* and *exact matching*.[11] As argued by Winship and Sobel (2001), jointly considering these methods offers a way of assessing the robustness of the estimates.

As for the propensity score (Rosenbaum and Rubin, 1985), we use the Nearest Neighbour Matching as implemented by Becker and Ichino (2002).[12] Each treated firm is matched with the non-subsidized firm 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, from which firms receiving some other sources of aid (Law 488 grants) are removed. The propensity score is Logit-estimated using a set of firm-level covariates averaged over the pre-intervention period (1998-2000): we include a proxy for the firm size (sales), a measure of internal funds (cash flow as a percentage of

[10] In the programme evaluation literature there are many analogies 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 ours.

[11] Data processing was performed so as to guarantee the anonymity of the data and prevent the re-identification of firms.

[12] Matching is executed with replacement. Results differ only a little if matching without replacement is allowed instead. Similarly, results obtained 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).

assets), a measure of the interest rate (interest costs as a percentage of debt), a measure of leverage (debt as a percentage of assets), a proxy for gross profitability (gross operating margin, GOM, as a percentage of value added), and ROA. We also add a series of 3-digit industry dummies. Note, however, that control firms can belong to different industries from the treated firms.

We also rely on a different selection criterion: 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 as the subsidized firm. Note that this is a quite detailed industry level, which includes, for instance, cotton power-loom weaving and ceramic tile manufacture. Then, within each industry-level stratum we select two counterparts of each treated firm (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 covariates Investment/Capital, Sales, Cash Flow/Assets, Interest Cost/Debt, and ROA.[13] The control groups selected by exact matching have the nice property 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 also appear in the control group selected by exact matching (which includes 1,264 firms, 623 and 641 respectively). This feature enhances the robustness of our estimates, as we are contrasting subsidized firms with two quite different comparison samples.

We gauge the effect of eligibility by estimating equation (2) and averaging the effects of the programme for subsidized firms and eligible firms, compared with 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 to the measurement of investment (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 (cumulative over time) investment as a 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-

[13] This set of covariates is the one for the which the balancing properties are satisfied most nicely.

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 programme placement). Since eligible areas are the regions of the South of Italy while non-eligible areas include regions of the Centre and North, this could be a serious issue. As is well known, the South of Italy differs from the Centre and North in a number of respects. 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 programme that might drive 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 relating to non-random programme placement, focus on regions that can be deemed 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 always display 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).[14] As explained above, the average of the two coefficients can be interpreted as the causal effect of programme eligibility. We can therefore gauge the magnitude of having access to the ITC programme as follows. Descriptive statistics show that during the post-intervention period, investment as a percentage of the 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 suggest, however, that the additional investment caused by programme eligibility is much reduced, amounting to 44% of the post-intervention investment activity of the non-eligible firms. Column 2

[14] 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).

shows that when the region time-varying controls are included, the estimated effect of programme eligibility decreases to 38%.[15] 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, in this case too most of the pre-intervention observables are fairly similar across groups. Only a few covariates (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 programme eligibility remains positive and highly significant. When evaluated over capital (Panel A), the additional investment prompted by the existence of the programme amounts to 112% of the investment of the non-eligible firms (Column 1). It also survives the inclusion of the regional time-varying controls (Column 2).[16] 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 received the fiscal bonus. They represent a very small percentage of the corporations eligible under the programme. For instance, in 2001 and 2002 the Cerved dataset includes 59,980 southern corporations that have neither received the ITC nor any other form of aid (28,060 of them record positive investment). 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 unity (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. We can also calculate the average effect of

[15] 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.

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

ITC eligibility for the sub-sample of firms that record positive investment. This effect is equal to 12% of the investment of the non-eligible counterparts. [17]

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 programme 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 sharper surge in investment in the two initial years of the programme. Operationally, we estimate the impact of the programme for each single year of the post-intervention period. In this case, we run the following year-by-year version of equation (2):

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

where *YEAR* denotes time dummies, and *YEARPOST* is a series of dummies for each of the years after the introduction of the programme. The coefficients of interest in equation (3) are $a_{6,t}$ and $a_{7,t}$. Since the impact is evaluated over time, we 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 with firms located in non-entitled areas. Again, for each year the average of the two coefficients captures 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 give us a chance to discuss the role of time substitution in our results (Abel ,1982; Adda and Cooper, 2000; Auerbach and Hines, 1988). First, to take advantage of the ITC, firms may have postponed investment projects originally planned for the pre-intervention

[17] Investing firms in eligible areas might decide not to claim the bonus (see Knittel, 2005). On the one hand, this can be put down to a lack of knowledge, as there are virtually no costs involved in the claiming procedure. All an entitled firm needs to do to get the bonus is to complete an additional line in an application form (form F24), which has to be filled out monthly anyway. On the other hand, for the firms that apply for the fiscal bonus the tax authorities may carry out more thorough controls against tax evasion. Therefore, a firm might decide not to claim for the bonus in an attempt to skip the inspections.

period. As argued in Section 2, this is fairly unlikely as there was no expectation of the launch of the ITC programme. In any case, in this circumstance there would have been less investment by the treated firms compared with the comparison firms before the start of the programme. 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.[18] Second, firms may also have brought forward investment projects originally planned for the post-programme period. Again, as explained in Section 2, the bias arising from anticipating investments should reasonably be attenuated by the fact that the programme was abruptly downsized in 2003; that is, three years before the known expiry date. Indeed, standard dynamic models of investment behaviour predict that investments which are brought forward should boom prior to the known date of expiry of the law. In any case, to detect evidence of time substitution we turn to the data. In the anticipation scenario we should observe that the increased investment activity comes at the expense 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.[19] The fact that in 2003 and 2004 (Table 3) the investment of the treated group is most of the time lower than that of the non-eligible counterparts (and the effects are also statistically significant) signals that a moderate degree of time substitution cannot be ruled out. Clearly, since the net effects over time estimated in Tables 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 inter-temporal 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 Olley and Pakes, 1996). Suppose that the effect of the ITC is to keep alive a marginal firm. In this scenario, marginal firms in the control groups go out of business because 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 problem we

[18] 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.

[19] According the estimates provided in Bronzini and de Blasio (2006) for the other main Italian investment incentives programme (Law 488), the timing of the slowdown is approximately from one to two years after the end of the programme.

construct an unbalanced panel, for the which we do not require the financial accounts to be available over the entire period. We start by picking treated firms that have a minimum of two pre-intervention and two post-intervention adjacent sets of financial-statement data.[20] We are able to find 993 such firms, compared with the 634 firms in the balanced panel. Then, firms in the control groups are selected by the exact matching procedure explained above, in which, however, control firms are also required to share the same years of balance-sheet availability as the treated firms. 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 also 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 programme took place. Results from this experiment (Table 5) are also similar to those presented up to now (Table A4 provides the sample statistics) .

3.3 Alternative experimental designs for non-random programme placement

So far, we have tackled the non-random programme placement issue using region fixed effects and region time-varying controls. Clearly, even with these controls one cannot be sure 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 drive the apparent effect of the ITC. Below, we adopt three straightforward strategies to alleviate this concern. First, the impact of programme eligibility is estimated for the few selected areas of the Centre and North of Italy covered by the programme (jointly with the southern region most similar to the northern ones).

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

For this sample, southern unobserved trends are reasonably absent or at least drastically reduced. Second, we try to approximate a control group of southern eligible firms for which the self-selection problem is arguably diminished, using rejected applicants from another investment incentives programme. 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 our approach amounts to directly estimating 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 programme 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 the Centre and North of Italy[21] and in the most advanced southern region (Abruzzo).[22] Correspondingly, the *ELEG* group includes (76) firms similar to the treated ones located in the same areas,[23] while the non-eligible firms include (75) firms located in the areas of the Centre and North that are different from those few areas deemed eligible. This experiment represents an intuitive version of the regression discontinuity design (Campbell, 1969), as firms with very close characteristics as regards their local area are differently exposed to treatment. Table 6 describes the results and Table A5 presents the sample statistics. Overall, our previous findings remain confirmed: the estimated impact of programme eligibility is positive and highly significant, irrespective of how investment is measured

The impact of participation in the ITC programme, 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 firms' propensity to invest: comparison firms should display before the treatment the same willingness to invest as ITC-recipient firms. As argued above, because of the automatic award scheme envisaged for the fiscal credit, this comparison group is apparently not available. We try to approximate this comparison

[21] The possibility of including firms located in selected areas of the Centre and North of Italy in the ITC programme is envisaged under the Article 87.3.c of the 1997 Amsterdam Treaty.

[22] This is also formally recognized at the EU level. For instance, while southern regions currently 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.

[23] This was accomplished by exact matching.

group by turning to another programme of investment incentives, Law 488. In contrast with Law 388, this scheme allows firms willing to invest to receive a grant. Crucially, under this programme 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.[24] Second, for this programme we have natural candidates for the comparison group: rejected firms (see Bronzini and de Blasio, 2006).

The two programmes are not immediately comparable as Law 488 covers only manufacturing and construction firms and the respective areas of eligibility of the two programmes do not overlap completely. This requires some adjustments to the treatment group. Among the ITC-recipients we select only those that in principle could have applied for either programme (basically, manufacturing firms in Law 488 eligible areas). We end up with 354 treated firms. As for the untreated group, we take the rejected applicants for Law 488 grants after 2000. Note that in principle a rejected applicant might resort to ITC in the years after the application for Law 488 funding 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 A6, balancing properties are less convincing than previous cases. Some of the differences in observables between the two groups are not zero.[25] For instance, Law 488 rejected applicants record higher pre-treatment investment and interest costs and lower debt. With these caveats, we show in Table 7 the estimated interaction coefficients for the specification of equation (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

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

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

includes the *TREAT* and *ELEG* groups described in Table A2). Treated firms can be split into 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 firms located in the remaining southern regions, for the which the envisaged ITC is equal to 50%. Accordingly, eligible non-subsidized firms are those located in the same area as 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- introduction of the programme in subsidized firms versus firms located in entitled areas but non-subsidized in high and low ITC intensity regions, compared with firms in medium ITC intensity regions.

Table 8 shows the results. The evidence is again in favour 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 subsidizes 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 programme (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, firms' debt may also increase. Furthermore, changes in borrowing may bring about modifications in the interest rate paid by the firm. For instance, if the credit supply curve is negatively sloped, increases in debt should go hand in hand with a reduction in the cost of borrowing.

To make a first attempt at tackling these issues, in Table 9 we present results in which 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 studying 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, labour 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 programme. Again, note that our window of data availability extends only to 2004 financial statements. Thus, side-effects of the programme that materialize after that date are not captured.

4 Conclusions

This paper examines the effect on investment expenditure of the tax credit enacted by Italy's Law 388. The programme envisages that the ITC is assigned automatically on the basis of the firm's demand for the fiscal bonus. This implies that subsidized firms are self-selected and cannot be meaningfully compared with firms that do not request the ITC. To assess whether the programme made investment possible that otherwise would not have been made, the paper exploits a number of discontinuities of the scheme envisaged by the law. For instance, the fact that some areas in the Centre and North of Italy are not entitled allows us to estimate the impact of programme 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 differ across eligible regions entitles us to compare firms receiving a relatively more generous fiscal bonus with firms receiving less liberal treatment. Our results suggest that the programme has been effective in stimulating investment. This conclusion is robust to a variety of 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 in order, however. First, the ITC implemented by Law 388 differs substantially for the other ITC programmes implemented elsewhere in the world, mainly because it is not limited to profitable enterprises with tax liabilities. To be sure, the programme is similar to an investment grant programme, as it provides firms with a direct government rebate of a certain fraction of investment expenditure. The fact that the ITC programme is not biased in favour of the most profitable firms, which most likely would have invested more even without subsidies, might be a reason for 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 experience of Italy in this respect is highly 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 putting similar incentive programmes into action.

Tables and figures

Chart 1

ITALIAN REGIONS BY ITC ELIGIBILITY AND INTENSITY

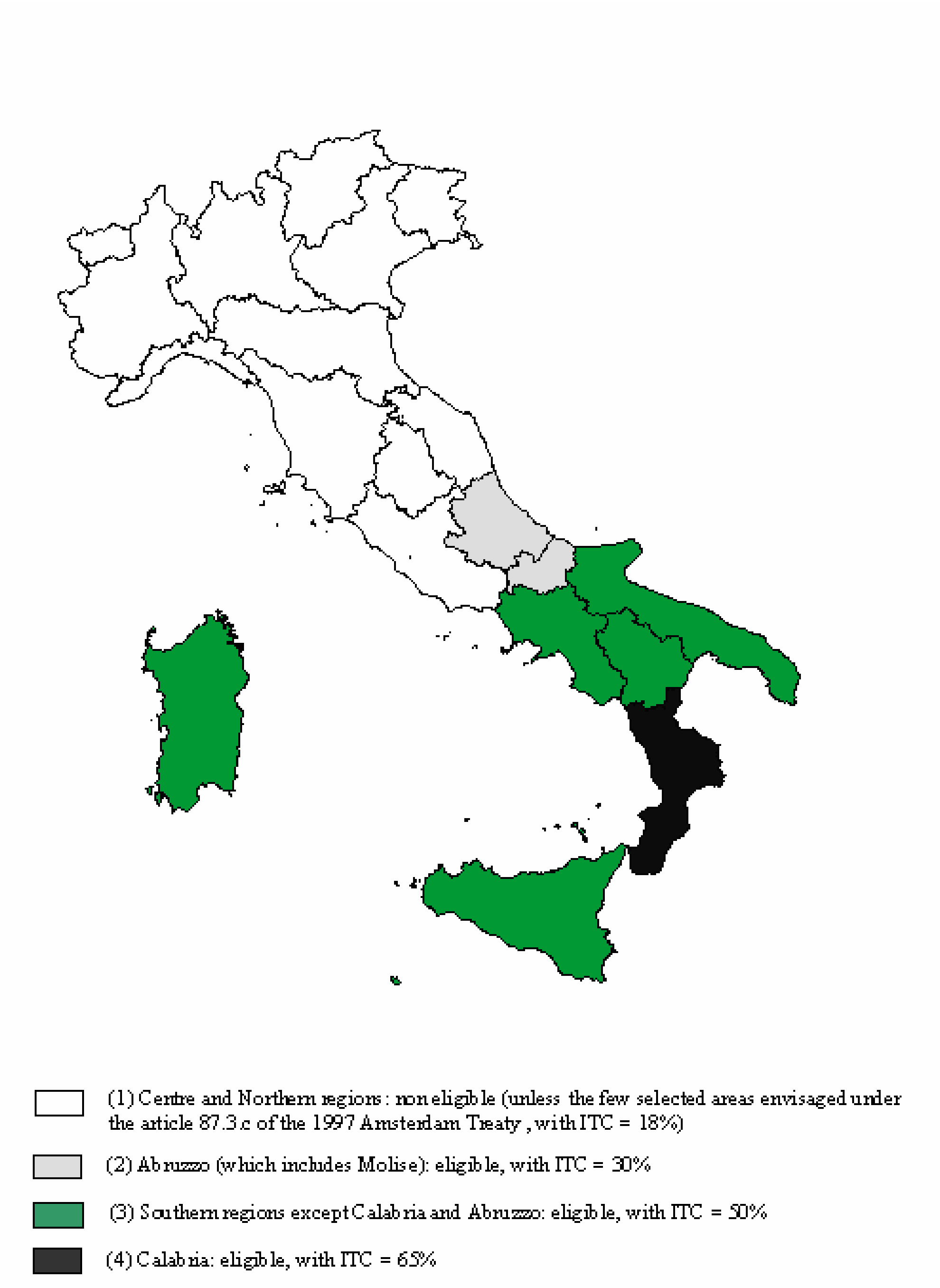

Table 1
EFFECT OF PROGRAMME ELIGIBILITY ON INVESTMENT
COMPARISON GROUPS SELECTED BY *PROPENSITY SCORE*

	(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 neighbour matching). The non-eligible sample includes 633 firms selected by propensity score (nearest neighbour matching). See Table A.1 and equation (2) for further details.

Table 2
EFFECT OF PROGRAMME 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 PROGRAMME 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 PROGRAMME 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 PROGRAMME ELIGIBILITY ON INVESTMENT
FIRMS WITH THE SAME PATTERN OF PRE-INTERVENTION INVESTMENT GROWTH RATE

	(1)	(2)
	Panel A. Dependent variable: I/K	
POST × TREAT	1.1310	1.0986
	*(0.0071)****	*(0.0503)****
	*[0.0106]****	*[0.0425]****
POST × ELEG	0.4685	0.4309
	*(0.0099)****	*(0.0432)****
	*[0.0099]****	*[0.0360]****
	Panel B. Dependent variable: I/S	
POST × TREAT	0.0178	0.1055
	*(0.0014)****	*(0.0039)****
	*[0.0016]****	*[0.0042]****
POST × ELEG	0.0416	0.0399
	*(0.0005)****	*(0.0032)****
	*[0.0008]****	*[0.0053]****
	Panel C. Dependent variable: I/A	
POST × TREAT	0.1327	0.1321
	*(0.0016)****	*(0.0034)****
	*[0.0018]****	*[0.0023]****
POST × ELEG	0.0451	0.0449
	*(0.0010)****	*(0.0020)****
	*[0.0013]****	*[0.0029]****
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 484 firms. The eligible sample includes 474 firms selected by exact matching. The non-eligible sample includes 483 firms selected by exact matching. See Table A.4 and equation (2) for further details.

Table 6
EFFECT OF PROGRAMME ELIGIBILITY ON INVESTMENT
COMPARISON GROUPS LOCATED IN CENTRE AND 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.5 and equation (2) for further details.

Table 7

EFFECT OF THE PROGRAMME 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.6 and equation (1) for further details.

Table 8
EFFECT OF THE PROGRAMME 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 9
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: labour 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 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 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 3

	Mean and standard deviation			Mean differences		
	TREAT	ELEG	NELE	TREAT vs ELEG	TREAT vs NELE	ELEG vs NELE
Investments/capital	0.9852	0.9151	1.0596	0.0701	-0.0743	-0.1444
	(2.4810)	*(2.4684)*	*(2.8533)*	*0.1396)*	*(0.1499)*	*(0.1508)*
Investment/sales	0.0787	0.0746	0.0651	0.0041	0.0137	0.0095
	(0.2894)	*(0.3122)*	*(0.2333)*	*(0.0169)*	*(0.0147)*	*(0.0155)*
Investment/assets	0.0883	0.070	0.0744	0.0181	0.0139	-0.0042
	(0.3021)	*(0.2703)*	*(0.2331)*	*(0.0161)*	*(0.0151)*	*(0.0142)*
Sales	2349.50	1868.79	2374.22	480.70	-24.716	-505.42
	(5087.09)	*(425.59)*	*(6259.63)*	*(265.10)**	*(319.84)*	*(303.22)**
Cash flow/assets	0.0799	0.0813	0.0808	-0.0013	-0.0008	0.0004
	(0.0804)	*(0.0932)*	*(0.0830)*	*(0.0049)*	*(0.0045)*	*(0.0049)*
Interest cost/debt	0.0265	0.0262	0.0258	0.0013	0.0007	0.0003
	(0.0245)	*(0.0248)*	*(0.0213)*	*(0.0013)*	*(0.0012)*	*(0.0013)*
GOM/value added	0.3371	0.3749	0.4622	-0.0377	-0.1250	-0.0872
	(1.2420)	*(1.3712)*	*(0.5534)*	*(0.0737)*	*(0.0540)***	*(0.0588)*
Debt/assets	0.7445	0.7363	0.7241	0.0082	0.0204	0.0122
	(0.2146)	*(0.2474)*	*(0.2063)*	*(0.0130)*	*(0.0118)**	*(0.0128)*
ROA	0.0221	0.0216	0.0252	0.000	-0.0031	-0.0036
	(0.0729)	*(0.0802)*	*(0.0726)*	*(0.0043)*	*(0.0041)*	*(0.0043)*

Notes: The TREAT sample includes 638 firms. The ELEG sample includes 620 firms selected by propensity score (nearest neighbour matching). The NELE sample includes 633 firms selected by propensity score (nearest neighbour 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 3

	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 4

	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 neighbour matching). The NELE sample includes 988 firms selected by propensity score (nearest neighbour 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 TABLES 5

	Mean and standard deviation			Mean differences		
	TREAT	ELEG	NELE	TREAT vs ELEG	TREAT vs NELE	ELEG vs NELE
Investments/capital	0.7373	0.5628	0.6989	0.1745	0.0384	-0.1360
	(2.0227)	*(1.7728)*	*(2.0365)*	*(0.1229)*	*(0.1305)*	*(0.1235)*
Investment/sales	0.0444	0.0224	0.0300	0.0220	0.0143	-0.0076
	(0.1643)	*(0.1417)*	*(0.1265)*	*(0.0099)***	*(0.0094)*	*(0.0086)*
Investment/assets	0.0585	0.0318	0.0502	0.0267	0.0083	-0.0184
	(0.1951)	*(0.1360)*	*(0.1776)*	*(0.0108)***	*(0.0120)*	*(0.0102)**
Sales	2703.03	2462.65	2744.90	240.37	-41.87	-282.24
	(5722.82)	*(5196.27)*	*(5720.58)*	*(353.38)*	*(367.99)*	*(353.47)*
Cash flow/assets	0.0806	0.0745	0.0789	0.0060	0.0016	-0.0043
	(0.0821)	*(0.0736)*	*(0.0796)*	*(0.0050)*	*(0.0052)*	*(0.0049)*
Interest cost/debt	0.3493	0.0293	0.0311	-0.0011	-0.0029	-0.0018
	(0.0258)	*(0.0249)*	*(0.0262)*	*(0.0016)*	*(0.0016)**	*(0.0016)*
Gross operating margin	0.3493	0.4178	0.4879	-0.0685	-0.1386	-0.0700
	(1.3887)	*(0.5482)*	*(1.2047)*	*(0.0684)*	*(0.0836)**	*(0.0606)*
Debt/assets	0.7269	0.7343	0.7283	-0.0073	-0.0013	0.0060
	(0.2217)	*(0.2167)*	*(0.2200)*	*(0.0142)*	*(0.0142)*	*(0.0141)*
ROA	0.0248	0.0214	0.0205	0.0033	0.0043	0.0009
	(0.0746)	*(0.0577)*	*(0.0655)*	*(0.0043)*	*(0.0045)*	*(0.0039)*

Notes: The TREAT sample includes 484 firms. The ELEG sample includes 474 firms selected by exact matching. The NELE sample includes 483 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 A5

DESCRIPTIVE STATISTICS AND MEAN DIFFERENCES FOR THE EXPERIMENT OF TABLE 6

	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 A6

DESCRIPTIVE STATISTICS AND MEAN DIFFERENCES FOR THE EXPERIMENT OF TABLE 7

	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.

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