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Abstract

Corporate taxation can have redistributive effects on income and wealth. We hypothesize and empirically establish such an effect working via bank credit. Using a unique sample of majority-owned firms that apply for credit, we show that after a decrease in corporate tax rates the relatively poor get easier access to credit. However, this policy also considerably increases loan amounts and decreases loan spreads for the relatively rich. Ultimately, reducing the corporate tax rate predominantly increases the future income and wealth of relatively rich business owners.

Keywords: Corporate taxes; Economic inequality; Bank credit; Credit score
JEL classification: G20; G21; H25; D63

1 Introduction

There is increasing consensus that reducing corporate tax rates contributes to the rising economic inequality observed in many Western economies. However, much less is known about the role of credit in this relation. Our study outlines a credit channel through which expansionary taxation policy contributes to rising income and wealth inequality.

Theoretically, a decrease in corporate tax rates leads to an increase in credit supply for two interrelated reasons. First, consistent with the debt restructuring theory (e.g., DeAngelo and Masulis, 1980; Stiglitz and Weiss, 1981), expansionary taxation policy decreases firms' and banks' propensity to use credit as a tax shield. Moreover, higher liquidity and after-tax profitability implies increased reliance on firms' internal funds. Second, an increase in after-tax profitability lowers firms' default risk and increases banks' incentives to increase their credit supply. Moreover, banks themselves may benefit from tax cuts, suggesting higher after-tax profits and associated expansionary lending opportunities.

The question then is which firms / business owners benefit and in what proportions. If banks relax their lending standards and predominantly expand credit to previously credit-constrained firms, or if they offer significantly lower lending rates to low-income business owners, then inequality will decrease because of the marginal increase in the income and wealth of previously credit-constrained / relatively poor business owners. This effect would be consistent with the extant literature on credit expansion and aggregate measures of inequality (e.g., Beck et al., 2010). In contrast, if changes in the credit supply occur predominantly via lower credit costs or higher loan amounts for relatively rich or existing borrowers, then inequality might increase because the bulk of the marginal benefit will not go to the credit constrained / relatively poor.

We empirically answer this question in three steps. In the first step, we estimate the probability that a bank originates a loan as a function of a change (decrease) in the corporate tax rate and the heterogeneous impact of that change due to an applicant's wealth or income at the time of the bank's credit decision. This model provides information on any asymmetric impact of the tax change on loan origination for the relatively poor versus the relatively rich.

In the second step, we examine whether and how the decrease in the corporate tax rate affects loan amounts and loan spreads for the relatively poor and the relatively rich. This step is important to uncover the effects on the intensive margin of approved loans and show how the terms of credit vary between the relatively poor and the relatively rich.

In the third step, we estimate the income or wealth of loan applicants three or five years after the loan application. These models show whether the overall relaxation of credit standards (identified in the first two steps) predominantly benefits (in terms of future income and wealth) the relatively rich versus the relatively poor.

We use a unique dataset of small firms with majority owners who apply for credit to a systemic North European bank between 2002 and 2010. Using data on such firms implies observing both relatively poor and relatively rich applicants, with the firms' financial soundness also reflecting the business owners' financial condition. This bank provides all types of corporate loans at a national and European level, and we show that it is representative across several aspects of lending behavior. We observe the business owner's income and wealth, along with several important traits (age, gender, family situation, etc.). The business owners / loan applicants have a continuing relationship with the bank, so that the bank also observes the future income and wealth of the applicants. Importantly, we also observe the applicant's credit score, which guides the bank's decision. That is, a positive credit score implies an accepted (granted) loan and a negative credit score implies a rejected loan application.

The bank's home country sharply decreased corporate tax rates in the 2000s, making our setting an almost ideal natural experiment to estimate our models. The key empirical identification challenge is that the expansionary fiscal policy might be endogenous to unobserved firm-specific and macroeconomic effects (instead, the bank is constant). Our model embodies two strategies to insulate from such bias.

First, we use a difference-in-differences-in-differences (triple differences) model, where the before-after effect of the treatment (the tax decrease) differs with the loan applicants' income or wealth (poorer versus richer applicants), as well as among loan applicants in the country experiencing the tax decrease versus those in neighboring countries that do not decrease taxes.

Second, we restore any deviation from randomness (for which deviation we already find very limited support) by controlling for applicant credit scores. This score encompasses all information that the bank knows about the applicant and creates a known cutoff point that perfectly predicts a bank's decision to originate the loan or reject it; there is a sharp discontinuity in the probability of loan origination at the zero-cutoff point.

Overall, our identification strategy survives in several placebo and robustness tests. Our results are also robust to the use of a Heckman regression that considers the probability that any firm in the countries examined (the bank's country and the neighboring ones) applies for a loan to our bank (sample selection of firms in our sample).

Our main results are as follows. After a decrease in the corporate tax rate, the treated relatively poor individuals are 4.6% more likely to get loans, according to our preferred specification. Thus, these applicants are less credit constrained after the policy intervention. However, the relatively rich treated individuals obtain significantly larger loans (approximately 7.2% larger) with lower spreads (approximately 7.7% lower).

The larger and more competitively priced loans contribute to a significantly larger increase in income and wealth for the richer business owners three or five years after the loan origination. Specifically, we find that the income of the relatively rich treated loan applicants is 7.3% larger three years after the loan application, and almost half of that effect is from the increase in loan amount and/or decrease in loan spread.

Placement in the literature

Our study mainly relates to two strands of literature, which we briefly review here without aiming to be exhaustive. The first focuses on the large theoretical and empirical literature (dated at least back to Graham, 1936) examining the effect of corporate taxation policy on economic inequality (for a recent review, see Faccio and Iacono, 2021). Among the most recent studies, Nallareddy et al. (2018) show that a 1% cut in corporate taxes raises the share of income accruing to the top 1% by 0.9, with this result largely due to top earners shifting income from labor to capital income to reduce their overall tax liabilities. Hines (2020) finds an opposite effect, suggesting that an increase in the corporate tax rate shrinks the corporate sector, leading several business owners to shift to the noncorporate sector (a reallocation effect). An increasing share of noncorporate businesses in the economy raises idiosyncratic risk (due to less diversification), thereby widening the distribution of income.

The second relevant strand of literature is on corporate taxation and bank lending. Recently, Ağca and Igan (2019) show that contractionary fiscal policy causes a significant increase in loan spreads. Heider and Ljungqvist (2015) examine debt and leverage specifications, suggesting that leverage responds positively to tax increases but not to tax cuts. This is in part because an increase in corporate taxation causes firms to readjust their leverage to benefit from the tax shield. Deli et al. (2022) show that loan spreads decrease by approximately eight basis points in response to a 1% tax cut, but they are insensitive to corporate tax increases (mainly because large firms buffer increases in loan spreads).

Compared with this literature, our study establishes a credit channel of taxation policy through which changes in corporate tax rates differentially affect credit (loan origination, loan amount, and loan spread) to the relatively poor versus the relatively rich.

2 Data and empirical models

2.1 Firms obtaining credit from a systemic bank

We use unique confidential data from loan applications at a North European bank. The bank is systemic according to the European Banking Authority (EBA), operates on a global scale, and provides all types of credit.[1] Using data from a single bank is common when detailed data are required (e.g., Iyer and Puri, 2012; Berg, 2018). In our setting, this practice has the additional advantage of holding the bank's supply-side behavior constant in the cross-section, allowing us to pinpoint the relevant mechanisms more easily.

We have information on all corporate loans from this bank during 2002-2020. However, we restrict our analysis to an event study in the window around the corporate tax decrease in the bank's country; thus, our panel covers 2003-2007 (also leaving out crisis-related events). Consistent with Delis et al. (2022), we use only loans to firms with a majority owner (individuals owning at least 50% of the firm) who is also the firm's manager. These firms are small and micro enterprises with

[1] This includes the EBA definitions for Global Systemically Important Institutions (G-SIIs) and the Other Systemically Important Institutions (O-SIIs). See https://eba.europa.eu/risk-analysis-and-data/global-systemically-important-institutions and https://eba.europa.eu/risk-analysis-and-data/other-systemically-important-institutions-o-siis-.

total assets of less than €10 million. The firms are headquartered in the bank's country or in European countries sharing borders with the bank's country. The loans are of all types, including working capital loans, real estate loans, startup loans, credit lines, etc.

The available information on these loans is unique in many respects. First, we have information about the business owners, including their incomes and wealth, age, gender, marital status and number of dependents, and completed level of education (from elementary school to MBA and Ph.D.). Second, we have information on firm characteristics, such as returns, sales, leverage, liquidity, R&D expenses, patents, ownership structure, region, and industry. Most important, we know the loan application date, the bank's decision (accepted for the full amount of the application, partially accepted, rejected), and the firm's credit score assigned by the bank. At the loan level, we also know the amount on the loan application and, for originated loans, the actual loan amount, the maturity (in months), the availability of collateral, and the type of covenants.

Last, our bank knows which firms are fully credit constrained in the sense that they cannot obtain credit from other regulated financial institutions (the bank obtains this information from the country's credit register).[2] Essentially, this provides information on whether the relationship between the bank and the firm is exclusive. In appendix section A.1, we highlight how representative our sample is across these dimensions.

2.2 Empirical models, identification, and variables

We provide thorough definitions for all variables in our empirical analysis in appendix table A1 and summary statistics in table 1.

Probability that a loan is granted

Our first empirical model takes the form:

$$Granted_{it} = a_0 + a_1 T_t + a_2 Y_{it} + a_3 D_i + a_4 T_t \times Y_{it} + a_5 T_t \times D_i + a_5 Y_{it} \times D_i + a_6 T_t \times Y_{it} \times D_i + a_7 CS_{it} + a_8 C_{it} + u_{it}. \tag{1}$$

[2] Of course, firms can borrow in the shadow banking system, but it will be at significantly inferior lending terms, further reinforcing our main findings.

In equation 1, *Granted* is a dummy that equals 1 if the bank grants the loan to applicant *i* in year *t*; it equals 0 if the bank rejects the applicant. *T* is a dummy taking the value 1 in the years before the tax decrease and the value 0 in the years after the tax decrease. *Y* is a dummy equal to 1 if the applicant is in the upper 25% of our sample's income distribution and equal to 0 if he/she is in the lower 25%. In several robustness tests, we use different cutoff points (e.g., 50%, 90%) or equivalent measures for wealth. *D* is a dummy equal to 1 if the firm is in the country experiencing the tax decrease and 0 otherwise. *CS* is the distance between the credit score's value and the cutoff point. We normalize this variable as the distance from the cutoff value 0, where for $CS \geq 0$ the bank originates the loan (i.e., *Granted* = 1) and for $CS < 0$ the bank rejects the loan application (i.e., *Granted* = 0). The vector *C* includes a set of controls.

[Insert Table 1 about here]

The coefficient of key interest in equation 1 is a_6, which shows the differential effect of the corporate tax rate on *Granted* for different levels of applicant income or wealth and for the treated versus untreated firms. We expect a_6 to be negative, consistent with the hypothesis that a tax decrease further increases the probability of loan origination for the relatively poor and treated applicants. For straightforward reasoning, we also expect a_1 to be positive; that is, a tax rate decrease increases the probability of loan origination in the general sample (as both firms and the bank will have higher after-tax profits, better cash flow, etc.).

There are two key issues allowing causal inference on a_6. First, given the inclusion of both treated firms from the bank's country (that issues the corporate tax decrease) and control firms from surrounding countries (that have not issued a corporate tax change), our model represents a DID specification.

Second, identification further tightens by observing the credit score, *CS*, that banks give to loan applicants at the time of their application. The reason is that the credit score creates a sharp discontinuity around the probability of granting the loan and fully explains the bank's credit decision on that basis. Any other observed variable, including *Y*, *T*, and *C*, should be in the bank's information set and thus in the credit score. Phrased differently, directly including other variables in equation 1 (and not indirectly via *CS*) implies extracting these variables from the effect of *CS*. Along that line, if we include all available variables in *C* (effectively extracting them from *CS*), the remainder effect of *CS* (the residual) is that of soft information; in contrast, the effect of the observed variables is that of hard information. In equation 1, we can also interact *CS* with *T* and *D* to uncover any nonlinear patterns.

Equally important, observing the credit score largely limits the possibility that the estimate a_6 is confounded by omitted-variable bias and thus implies that firms in the treated and control groups are directly comparable in the "eyes of the bank," thus restoring the notion of an almost ideal natural experiment. Given that the credit score creates a sharp discontinuity around the cutoff and this cutoff fully guides the bank's credit decision, the only possibility of an unobserved variable being correlated with both *Y* or *T* and *u* is that loan applicants systematically manipulate their credit scores (e.g., by providing false or omitted information to the bank). Appendix figure A1 provides a credit score manipulation test, as Cattaneo et al. (2018) proposes. The *p*-value of the robust (bias-corrected) statistic equals 0.314, rejecting the null hypothesis of manipulation.[3]

Loan amount and pricing

We next consider equivalent loan amount and spread specifications of the form:

$$Loan\ amount_{it} = b_0 + b_1 T_t + b_2 Y_{it} + b_3 D_i + b_4 T_t \times Y_{it} + b_5 T_t \times D_i + b_5 Y_{it} \times D_i + b_6 T_t \times Y_{it} \times D_i + b_7 CS_{it} + b_8 C_{it} + v_{it}. \quad (2)$$

$$Loan\ spread_{it} = c_0 + c_1 T_t + c_2 Y_{it} + c_3 D_i + c_4 T_t \times Y_{it} + c_5 T_t \times D_i + c_5 Y_{it} \times D_i + c_6 T_t \times Y_{it} \times D_i + c_7 CS_{it} + c_8 C_{it} + u_{it}. \quad (3)$$

We estimate equations 2 and 3 using only the accepted loan applications, for which information on the loan amount and loan spread is available. Essentially, these models allow a deeper look into

[3] A separate endogeneity problem is the sample selection bias, which we tackle below.

the lending terms of accepted applicants as a function of corporate tax rates, initial income (or wealth), and their interaction.

A positive and significant coefficient b_6 means that the impact of a tax decrease on the loan amount (reflected in a positive and significant b_1) is smaller for treated applicants (exposed to a tax decrease) who are relatively poor ($Y = 1$) at the time of the loan application. Such an effect pinpoints that most of the credit expansion after a tax decrease (identified via equation 1) goes to richer applicants (compared to the relatively poor). Similarly, a negative and significant coefficient c_6 pinpoints that a tax decrease has a larger effect on loan spreads for relatively poor applicants. Thus, equations 2 and 3 examine the intensive margin of the comparative benefit of credit expansion for accepted rich applicants versus accepted poor applicants in terms of loan volumes and loan spreads. Identifying causal inferences is precisely the same as in equation 1.

Future income and wealth

The natural extension is to analyze income and wealth distributional effects of the tax cut directly via the credit channel after the credit decision. Our empirical model is:

$$Income_{i,t+n} = d_0 + d_1 T_t + d_2 Y_{it} + d_3 D_i + d_4 T_t \times Y_{it} + d_5 T_t \times D_i + d_5 Y_{it} \times D_i + d_6 T_t \times Y_{it} \times D_i + d_7 CS_{it} + d_8 C_{it} + u_{it}, \quad (4)$$

where $Income_{i,t+n}$ is the income of individual *i* at *n* years after the loan application. In an alternative specification, we use $Wealth_{i,t+n}$ as the outcome variable. This model highlights how a decrease in corporate tax rate affects future income (wealth) for high-income (richer) and low-income (poorer) individuals at the time of their application. Thus, the model compares relatively poor and relatively rich individuals in the bank's country and neighboring countries, before and after the tax cut, and controlling for credit score CS. A positive d_6 suggests that a positive effect on future income (wealth) due to the corporate tax decrease is more potent for the treated high-income (richer) individuals.[4]

[4] Delis et al. (2022) present a similar model to identify the effect of the bank's credit decision on future income, without any reference to taxation policy.

To the extent that loan applicants cannot manipulate their credit scores (which we show is the case in figure A1 and in the prior relevant discussion), then controlling for credit score or the interaction among credit score, tax decrease, and current income is sufficient to identify the treatment effects (as it controls for any confounding effects in the treated and control firms). We provide several additional tests (besides the manipulation test) on the validity of this assumption in the discussion of our empirical results.

3 Empirical results

3.1 Loan origination

Table 2 reports our baseline results on equation 1 using *Income* as the key determinant of the relation between *Tax decrease* and *Granted*.[5] We begin in specification 1 without including control variables. In specification 2, we add only *Credit score*. In both specifications, we find positive coefficients on *Tax decrease*, *Income quartile*, and their interaction term. However, the estimates are smaller in specification 2, and the adjusted R-squared increases from 0.47 to 0.96. This increase comes from the positive and highly significant coefficient on *Credit score* and shows its effectiveness to explain the bank's credit decision and purify the effect of the tax decrease and income from unobserved confounding effects.

[Insert Table 2 about here]

Our main focus is on the triple term *Tax decrease* × *Income quartile* × *Issuing country*, with the negative coefficient showing that the impact of a tax cut on *Granted* is smaller (larger) for the treated relatively rich (poor) loan applicants. This implies that relatively poor loan applicants in a country experiencing a tax cut (treated applicants) see an increase in their probability of getting a loan compared to relatively poor but untreated applicants (in other countries) and relatively rich applicants. The probability of this increase is 4.6%.

[5] All specifications include year fixed effects, which somewhat increase the adjusted R-squared but do not affect our inferences.

In specification 3, we add several observed applicant and firm characteristics reported in appendix table A1.[6] Specifically, we include *Wealth*, *Age*, *Marital status*, *Dependents*, *Education*, *Firm size*, *Leverage*, *ROA*, *Cash*, and *Applications*, with most of these variables entering with highly statistically significant coefficients. Moreover, we control for the interaction term between *Tax decrease* and *Credit score*. This follows the econometrics literature on treating the variable defining the distance from the cutoff (*Credit score*), symmetrically with the treatment variable (*Tax decrease*) (e.g., Angrist and Pischke, 2009). In our case, the interaction term between *Tax decrease* and *Credit score* controls for the potential differential effect of *Tax decrease* on *Granted* due to any other variable rated by the bank. We find that our results remain unaffected, implying that extracting hard information from the effect of the credit score does not affect our inferences and only the coefficient on *Credit score* decreases (as expected).

In appendix section A.1, we establish that our sample is representative of the European population across three dimensions: bank characteristics (including loan acceptance rates), firm characteristics, and loan applicants, especially with respect to their exclusive relationship with the bank. In specifications 4 and 5 of table 2, we further limit the possibility of sample selection bias using Heckman's model. Specifically, we exclude issues of sample selection based on (i) firms' endogenous choice to apply for a loan in a given year and (ii) firms associated with our bank somehow not being representative of the country's firms.

For point (i), we estimate in specification 4 a two-stage model, where in the first stage we examine the probability that a firm applies for a loan in year *t* (dummy variable equal to 0 if there is no application). Thus, in the first stage we use the full firm-year panel (87,870 observations) and all control variables used in previous regressions, as well as the business owner's gender (dummy variable equal to 1 for male business owners and equal to 0 for female). Delis et al. (2022) show that male business owners are more likely to apply for business loans, but this does not directly affect the probability of the bank originating the loan (i.e., there is no evidence of gender discrimination by the bank).

[6] We also experiment with year, industry, and other fixed effects, and we find no significant impact on our inferences.

Our first-stage results are indeed consistent with the relevance condition in that male business owners are significantly more likely to apply for a loan: the coefficient estimate on *Gender* in the first stage equals 0.012 and is statistically significant at the 1% level. The second-stage results in specification 4 of table 3 are fully consistent with (if not economically stronger than) previous specifications. Intuitively, *Lambda* is statistically insignificant, denoting that our data are consistent with no selection.

Last, in specification 5, we examine the robustness of our results to the selection of firms associated with our bank somehow not being representative of the country's firms. To limit this possibility, we collect additional data from Orbis on the full sample of similarly-sized firms in the bank's country and add the new observations to the first stage of Heckman's two-stage model. Thus, our first-stage sample increases to 111,746 observations. This first-stage includes as controls only the firm-specific characteristics (the rest are not available in Orbis). Again, the second-stage results are fully consistent with the baseline, but *Lambda* is insignificant (consistent with no selection).

We next turn to equivalent specifications with wealth instead of income (results are in appendix table A2). The results are very similar. Considering specification 3, the coefficient on *Wealth quartile* shows that applicants in the upper 25% of the wealth distribution are 7.5% more likely to get accepted, *ceteris paribus*. The tax decrease moderates this average effect by increasing the probability of a positive credit decision for the treated low-wealth applicants by 4.4% compared to the untreated relatively low-wealth applicants and the relatively high-wealth applicants.

We examine several robustness tests on equation 1, the results of which are summarized in appendix table A3. We report the results from each specification on a different line of that table, whereas the columns represent results for income quartile and wealth quartile, respectively. In regressions 1 and 2, we perform falsification (placebo) tests by sliding the event date one or two years backward, respectively. In both specifications, the economic significance of the effect decreases substantially, and the statistical significance disappears. This is consistent with the identified effect being significant only after the actual tax cut and not in an earlier, artificially set year.

In the specifications of line 3, we drop the control group in our baseline specifications (observations from 2002 to 2004) and use the same firms in 2016-2018. This was a stable period in the euro area and without changes in the corporate tax rate, similar to the control period of 2002 to 2004. Our main results remain identical to our baseline.[7] Alternatively, in the specifications of line 4, we drop our treatment group (observations from 2008 to 2010) and use as a treatment group the same firms in 2016-2018 (used as a control group in the specifications of line 3) by artificially giving them a value of 1 for *Tax decrease*. Given that there was no change in the corporate tax rate during this period, we expect that the coefficient on the triple interaction term is insignificant. This is indeed what we find, with the relevant coefficient being very small economically and statistically insignificant.

In the specifications of lines 5 and 6, we tighten the number of observations to those in the 10% and 5% distance around the zero cutoff. This implies that we are looking at similar firms with considerable differences in their owners' income. We find that, if anything, our main effects are economically more relevant. Last, in specifications of lines 7 and 8, we change *Income quartile* and *Wealth quartile* with equivalent variables constructed using the median values and the 90^{th} to 10^{th} percentiles, respectively. Intuitively, we find that using the median (thus comparing applicants with more similar income and wealth levels), the economic significance of the results decreases (line 7). In contrast, using the 90^{th} to 10^{th} percentiles, increases the economic significance of our findings.

The results in this section suggest that a decrease in corporate tax rates positively affects access to credit among the relatively poor (assuming that the relatively rich enjoy access already). To examine the progressivity of the tax cut, we next turn to the respective effects on loan amount and loan pricing.

3.2 Loan amount and pricing

In this section, we estimate equations 2 and 3. In specification 1 of table 3, we show that the triple interaction term *Tax decrease* × *Income quartile* × *Issuing country* has a positive and statistically significant coefficient. Economically, the effect is substantial, implying that the relatively rich loan applicants in the issuing country take 7.2% larger loans in the post-treatment period (compared to

[7] We cannot use a control group from other European firms, because we lack information on their credit scores.

relatively rich loan applicants pretreatment, untreated applicants, and relatively poor applicants). The equivalent Heckman regression in column 2 reports that loans are 8.7% larger.

[Insert Table 3 about here]

In specifications 3 and 4 we report the results on the log of loan spread. The results indicate a considerably lower loan spread (approximately 2%) for loan applicants in the upper income quartile compared to loan applicants in the lower income quartile. Following the decrease in the corporate tax rate, the bank shaves off an additional 7.7% in loan spread for treated high-income applicants compared to untreated loan applicants and low-income applicants. These results are again conservative if we consider the Heckman regression in specification 4.

Symmetrically with section 3.1, we report placebo and robustness tests in appendix table A4. The placebo tests are consistent with the validity of the experiment, and all the results survive and even become more potent when considering observations closer to the credit score cutoff or when using income in the 90th to 10th percentile to generate the income variable. The results also survive when using wealth instead of income (last row of table A4).

The evidence in this section is consistent with our theoretical propositions. Lowering tax rates positively affects loan amounts for the relatively rich and negatively affects their respective loan spreads, consistent with the relatively rich being able to finance significantly larger projects at more competitive rates. This most probably buffers the results in section 3.1, which show increased access to bank credit among the poor after the tax cut. In the next section, we identify the effects on applicants' future income and wealth to infer effects on income and wealth inequality after a bank's decision to lend and the government's policy change. That analysis is important as a final outcome of the corporate tax credit channel and in light of the contradicting results of sections 3.1 and 3.2.

3.3 Future income and wealth

The unique advantage of our dataset is that it allows us to observe the future income and wealth of loan applicants because of relationship lending and loan monitoring. To this end, this section examines the differential effect of a tax decrease on future income and wealth for high-income applicants versus low-income applicants, controlling for credit score and the associated loan decision cutoff (estimation of equation 4).

We report the results on future income (three and five years after the loan decision) in table 4. The coefficient on *Income quartile* is positive and highly statistically significant, showing a Matthew effect irrespective of the treatment (the tendency of richer individuals to acquire more wealth). The interaction term *Tax decrease* × *Income quartile* × *Issuing country* is positive and highly statistically significant, suggesting that the treated and richer loan applicants have higher income three (column 1) and five (column 3) years after the loan application (compared to the untreated and poorer loan applicants) because of the tax decrease. The estimated effects of the treatment are 7.3% and 8.2%. These results hold in the Heckman regressions in columns 2 and 4, implying that selection bias does not drive our inferences (*Lambda* is also statistically insignificant).

In table 5, we repeat this analysis for future wealth and report similar results. The Matthew effect is still present and the triple interaction term again has a positive and significant coefficient. According to the results in column 1 (column 3), the tax decrease leads to a 6.5% (6.8%) increase in the future incomes of the richer treated applicants compared to the control groups. These results are slightly conservative compared to the Heckman regressions.

[Insert Tables 4 and 5 about here]

Symmetrically with the results of previous sections, we show in appendix table A5 that our results are robust to the use of several placebo tests, such as sliding the date of the tax decrease one and two years backward and observing statistically insignificant results (rows 1 and 2 of table A5), using observations from 10 years after the intervention as a control group and observing statistically significant results (row 3), and using the same forward observations as the treatment group and observing statistically insignificant results (row 4). Moreover, our results survive when using only observations of "very similar" applicants (rows 5 and 6) but generating income and wealth variables with less (more) unequal incomes increases the economic magnitude of the identified effect in row 7 (row 8).

As a final analysis, we examine whether the effect of the tax decrease indeed occurs (and to what extent) via the increase in loan amount / decrease in loan spreads identified in section 3.2. To this end, we obtain the partial prediction (fitted values) of specifications 1 and 3 of table 3 with respect to the tax decrease and use them as control variables in specification 1 of table 4. We find that the estimate on the triple term *Tax decrease* × *Income quartile* × *Issuing country* drops from 0.073 to 0.037, implying that 49.3% of the response in the forward income specification is due to the larger loan amount and lower loan spread given to the treated loan applicants. For the equivalent wealth specifications, we uncover an effect equal to 39.1%. Thus, a very significant portion of the increase in the future income and wealth of the relatively rich treated loan applicants is because of the larger loan amounts and more competitive loan spreads.

4 Conclusions

This study highlights a credit channel through which decreases in the corporate tax rate affect income and wealth inequality. We study triple differences model in which a bank provides credit to both relatively rich and relatively poor loan applicants (business owners) in two jurisdictions: one with a tax cut (treated group) and one without (control group). Our analysis restores any remainder bias via controlling for the loan applicants' credit score.

We find that after a tax cut, relatively poor loan applicants have increased access to credit. However, relatively rich applicants receive proportionately larger loans and lower loan spreads, which explain about half of the increase in the relative income (afterwards) of the treated relatively rich compared to the relative income of the untreated and the treated relatively poor. Similar results apply to wealth. Our analysis shows that the credit channel is an important mechanism through which a decrease in corporate tax rates disproportionally increases income and wealth of the relatively rich.

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Table 1. Summary statistics

The table reports the number of observations, mean, standard deviation, minimum, and maximum for the variables use in the empirical analysis. The variables are defined in Table 1, except from *Application probability*, which is obtained from the estimation of equation (1).

	Obs.	Mean	St. dev.	Min.	Max.
Apply	87,870	0.331	0.471	0	1
Granted	29,255	0.856	0.370	0	1
Credit score	87,870	0.652	0.604	-0.773	3.500
Income	87,870	10.81	0.407	9.734	12.55
Income quartile	87,870	0.500	0.500	0	1
Wealth	87,870	11.98	0.601	7.212	14.29
Wealth quartile	87,870	0.500	0.500	0	1
Tax decrease	87,870	0.150	0.366	0	1
Gender	87,870	0.810	0.362	0	1
Age	87,870	44.94	15.87	21	76
Marital status	87,870	0.589	0.463	0	1
Dependents	87,870	1.898	1.491	0	7
Education	87,870	2.714	1.009	0	5
Firm size	87,870	12.79	0.420	10.181	16.12
Leverage	87,870	0.201	0.120	0.126	0.817
ROA	87,870	0.080	0.100	-0.409	0.583
Cash	87,870	0.082	0.032	0.000	0.255
Applications	87,870	6.833	1.464	1	9
Default	87,870	0.017	0.098	0	1
Loan amount	29,255	3.509	1.988	0.686	11.41
Loan spread	25,038	340.7	246.1	33.45	985.7
Loan maturity	25,038	47.9	37.29	4	278
Loan provisions	25,038	0.407	0.451	0	1
Collateral	25,038	0.695	0.499	0	1
Application probability	87,870	0.259	0.027	0.140	0.611

Table 2. Tax decrease, income, and loan decisions

The table reports coefficient estimates and standard errors (clustered by firm) in parentheses from the estimation of equation 1. The dependent variable is the bank's loan decision (granted or denied loan), and all variables are defined in Table 1. The lower part of the table reports the number of observations, the adjusted R-squared, and whether control variables are used. All specifications are estimated with OLS, except from specifications 4 and 5, which are estimated with Heckman's two-stage model. For specifications 4 and 5, we also report the number of observations used in the first stage and the estimate on Lambda. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	1	2	3	4	5
	Granted	Granted	Granted	Granted	Granted
Income quartile	0.349***	0.105***	0.101***	0.108***	0.110***
	(0.030)	(0.028)	(0.027)	(0.034)	(0.034)
Tax decrease × Income quartile	0.076***	0.013	0.011	0.011	0.011
	(0.011)	(0.012)	(0.011)	(0.014)	(0.014)
Tax decrease × Issuing country	0.003	0.002	0.002	0.005	0.008
	(0.016)	(0.016)	(0.017)	(0.021)	(0.022)
Income quartile × Issuing country	-0.006	-0.004	-0.004	-0.006	-0.007
	(0.023)	(0.025)	(0.025)	(0.028)	(0.027)
Tax decrease × Income quartile × Issuing country	-0.067**	-0.046**	-0.046**	-0.055**	-0.056**
	(0.022)	(0.021)	(0.021)	(0.026)	(0.026)
Credit score		0.577***	0.429***	0.486***	0.453***
		(0.032)	(0.029)	(0.040)	(0.042)
Tax decrease × Credit score			0.123***	0.147***	0.139***
			(0.031)	(0.047)	(0.042)
Lambda				-0.178	-0.180
				(0.129)	(0.134)
Observations	29,255	29,255	29,255	29,255	29,255
Observations (first stage)				87,870	111,746
Adj. R-squared	0.47	0.96	0.96		
Control variables	No	No	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes

Table 3. Loan amount and loan pricing

The table reports coefficient estimates and standard errors (clustered by firm) in parentheses from the estimation of equations 2 (first two specifications) and 3 (latter two specifications). The dependent variable is given on the top of each specification, and all variables are defined in Table 1. The lower part of the table reports the number of observations, the adjusted R-squared, and whether control variables are used. Specifications 1 and 3 are estimated with OLS, and specifications 2 and 4 with Heckman's two-stage model. For the Heckman specifications we also report the number of observations used in the first stage and the estimate on Lambda. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	1 Loan amount	2 Loan amount	3 Loan spread	4 Loan spread
Income quartile	0.065**	0.073***	-0.020**	-0.023***
	(0.030)	(0.035)	(0.006)	(0.010)
Tax decrease × Income quartile	0.032**	0.034**	-0.016**	-0.025***
	(0.014)	(0.016)	(0.007)	(0.009)
Income quartile × Issuing country	-0.003	-0.005	0.001	0.001
	(0.016)	(0.021)	(0.003)	(0.005)
Tax decrease × Income quartile × Issuing country	0.072***	0.087***	-0.077***	-0.083***
	(0.018)	(0.023)	(0.017)	(0.021)
Credit score	0.196***	0.172***	-0.257***	-0.272***
	(0.024)	(0.033)	(0.046)	(0.054)
Tax decrease × Credit score	0.047***	0.051***	-0.071***	-0.079***
	(0.013)	(0.018)	(0.022)	(0.022)
Lambda		-0.217		0.170
		(0.141)		(0.197)
Observations	25,038	25,038	25,038	25,038
Observations (first stage)		111,746		111,746
Adj. R-squared	0.91		0.92	
Control variables	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes

Table 4. Future income

The table reports coefficient estimates and standard errors (clustered by firm) in parentheses from the estimation of equation 4. The dependent variable is applicants' future income, and all variables are defined in Table 1. The lower part of the table reports the number of observations, the adjusted R-squared, and the type of fixed effects used in each specification. Specifications 1 and 3 are estimated with OLS, and specifications 2 and 4 with Heckman's two-stage model. For the Heckman specifications we also report the number of observations used in the first stage and the estimate on Lambda. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	1 Income t+3	2 Income t+3	3 Income t+5	4 Income t+5
Income quartile	0.088***	0.090***	0.119***	0.131***
	(0.006)	(0.013)	(0.008)	(0.017)
Tax decrease × Income quartile	0.011	0.017	0.010	0.019
	(0.012)	(0.017)	(0.015)	(0.020)
Income quartile × Issuing country	-0.003	-0.003	-0.005	-0.006
	(0.012)	(0.015)	(0.013)	(0.017)
Tax decrease × Income quartile × Issuing country	0.073***	0.080***	0.082***	0.087***
	(0.027)	(0.030)	(0.029)	(0.033)
Credit score	0.372***	0.361***	0.350***	0.357***
	(0.071)	(0.081)	(0.064)	(0.072)
Tax decrease × Credit score	0.027	0.042	0.018	0.025
	(0.022)	(0.030)	(0.023)	(0.034)
Lambda		-0.247		-0.273
		(0.303)		(0.307)
Observations	25,038	25,038	25,038	25,038
Observations (first stage)		111,746		111,746
Adj. R-squared	0.90		0.86	
Control variables	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes

Table 5. Future wealth

The table reports coefficient estimates and standard errors (clustered by firm) in parentheses from the estimation of equation 4. The dependent variable is applicants' future wealth, and all variables are defined in Table 1. The lower part of the table reports the number of observations, the adjusted R-squared, and the type of fixed effects used in each specification. Specifications 1 and 3 are estimated with OLS, and specifications 2 and 4 with Heckman's two-stage model. For the Heckman specifications we also report the number of observations used in the first stage and the estimate on Lambda. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	1	2	3	4
	Wealth t+3	Wealth t+3	Wealth t+5	Wealth t+5
Wealth quartile	0.061***	0.067***	0.072***	0.078***
	(0.017)	(0.023)	(0.017)	(0.026)
Tax decrease × Wealth quartile	0.015	0.017	0.018	0.022
	(0.013)	(0.015)	(0.014)	(0.019)
Wealth quartile × Issuing country	-0.002	-0.003	-0.006	(-0.008)
	(0.010)	(0.012)	(0.011)	(0.012)
Tax decrease × Wealth quartile × Issuing country	0.065***	0.071***	0.068***	0.073***
	(0.020)	(0.024)	(0.020)	(0.026)
Credit score	0.344***	0.358***	0.355***	0.379
	(0.070)	(0.082)	(0.074)	(0.091)
Tax decrease × Credit score	0.033	0.030	0.037*	0.035
	(0.023)	(0.027)	(0.023)	(0.027)
Lambda		-0.211		-0.225
		(0.289)		(0.286)
Observations	25,038	25,038	25,038	25,038
Observations (first stage)		111,746		111,746
Adj. R-squared	0.84		0.82	
Control variables	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes

Appendix

This appendix, intended for online print only, includes a discussion of how representative our sample is (section A.1), as well as explicit variable definitions and additional robustness tests on our main results. Table A1 provides variable definitions. Table A2 reports results from equation 1 using *Wealth quartile* instead of *Income quartile*. Table A3 reports the results from robustness tests on the estimation of equation 1. Table A4 reports the results from robustness tests on the estimation of equations 2 (dependent variable is *Loan amount*) and 3 (dependent variable is *Loan spread*). Table A5 reports the results from robustness tests on the estimation of equation 4. Figure A1 reports results from the manipulation test (loan applicants being able to manipulate their credit score).

A.1 Sample representativeness

In this section, we provide information on how representative our sample is to show that the probability of having sample-selection bias is low (we additionally provide a formal approach to address this possibility in our empirical analysis). We consider sample representativeness across three dimensions: bank characteristics and loan acceptance rates, firm characteristics, and loan applicants, especially with regard to their exclusive relationship with the bank.

Data from Compustat on 32 other European systemic banks suggests that the annual averages of important bank characteristics, such as the ratio of liquid assets to total assets, the ratio of market to book value, and return on assets are at very similar levels and significantly correlated with the respective ratios of our bank over the years in our sample (correlation coefficients equal to 0.62, 0.67, and 0.75, respectively). Moreover, data from the Survey on Access to Finance of Enterprises shows that average annual rejection rate in the euro area is very strongly correlated with the equivalent from our bank (the correlation coefficient is 0.86). The acceptance rate of 84.2% in our sample is slightly lower than the equivalent reported in the Survey of Access to Finance of Enterprises (SAFE). However, SAFE additionally includes relatively safe medium-size firms. In a nutshell, our bank's business model is very similar to the European average (also see Delis et al., 2020).

Second, our sample of small firms closely mimics that of similarly-sized European firms. Appendix figure A1 plots the annual average leverage and profitability ratios of small and micro firms in Austria, Belgium, Denmark, France, Germany, and the Netherlands against the averages in our sample. The data for these countries are from Orbis (information is only available from 2013). The firms in our sample have a 1.1% lower leverage ratio and a 0.76% higher ROA. These very small differences are probably due to the fact that our bank is in one of the highest-income European countries and was not significantly affected by the economic downturn in 2010-2014. Still, the trends are very similar.

Third, it is common for small entrepreneurs to have an exclusive relationship with a bank, and our full sample suggests this is the case for 65% of the firms. This figure is fully consistent with previous studies on multiple or exclusive lending relationships. Berger and Schaeck (2011) document a 71% exclusive relationship between banks and SMEs in three European countries

(Germany, Italy, and the UK), but this is less often the case in the United States (Berger et al., 2014, document a 57% rate). It is hard to find more evidence on whether small entrepreneurs have one or more banking relationships in northern European countries. Farinha and Santos (2002) report similar statistics for Portugal (70% of firms with fewer than 10 employees have one bank relationship). More recently, Bonfim at al. (2018) report a mean value of two banks for small Portuguese firms, but the Portuguese banking sector is much less concentrated than in our bank's country. Essentially, the available evidence suggests that the percentage of exclusive relationships in our sample is comparable to the percentage in previous papers on relationship banking.

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Table A1. Data and variable definitions

Variable	Description
A. Dimension of the data	
Individuals	Loan applicants that are majority owners (own more than 50%) of a firm. These borrowers apply to the bank for one or more business loans during the period 2002-2018 and the loan is either originated (fully or at least 75% of the requested loan amounted) or rejected (bank advises against proceeding with the application, fully rejects, or only originates up to 25% of the requested loan amount). For monitoring reasons, the bank holds information on the applicants even outside the year of loan application.
Year	Our sample covers the period 2002-2021. Applications end in 2018 and we use three more year of firm financial ratios to examine future firm outcomes.
B. Variables	
Apply	A dummy variable equal to 1 if the individual applied for a loan in a given year and 0 otherwise.
Granted	A dummy variable equal to 1 if the loan is originated (Credit score>0) and 0 otherwise (Credit score<0).
Credit score	The credit score of the applicant, as calculated by the bank. There is a 0 cutoff: positive values indicate that the loan is granted, and negative values indicate that the loan is denied.
Income	The euro amount of individuals' total annual income (in log) in the year of the loan application and the two years before the application. For the missing years, we input the predicted value of the regression of the last available observation of income on the mean income by region, year, and industry.
Wealth	The euro amount of individuals' total wealth other than the assets of the firm and minus total debt (in log). The bank observes this in the year of the loan application and the two years before the application. For the missing years, we input the predicted value of the regression of the last available observation of wealth on the mean wealth by region, year, and industry.
Tax decrease	A dummy variable equal to 1 if there is a decrease in the corporate tax rate in a given year and 0 otherwise.
Gender	A dummy variable equal to 1 if the applicant is a male and 0 otherwise.
Age	The applicant's age.
Marital status	A dummy variable equal to 1 if the applicant is married and 0 otherwise.
Dependents	The number of the applicant's dependents.
Education	An ordinal variable ranging between 0 and 5 if the individual completed the following education. 0: No secondary; 1: Secondary; 2: Postsecondary, non-tertiary; 3: Tertiary; 4: MSc; 5: MBA or Ph.D.
Firm size	Total firm's assets (in log).
Leverage	The ratio of firm's total debt to total assets.
ROA	The ratio of firm's after-tax profits to total assets.
Cash	The ratio of cash holdings to total assets.
Applications	The number of applications to the same bank before the current loan application.
Default	A dummy variable equal to 1 if the firm defaults up to three years after the loan origination, and 0 otherwise.

Loan amount	Log of the loan facility amount in thousands of euros.
Loan spread	The difference between the loan rate and the LIBOR (log of basis points).
Loan maturity	Loan maturity in months.
Loan provisions	A dummy variable equal to 1 if the loan has performance-pricing provisions, and 0 otherwise.
Collateral	A dummy variable equal to 1 if the loan has collateral guarantees and 0 otherwise.

Table A2. Tax decrease, wealth, and loan decisions

The table reports coefficient estimates and standard errors (clustered by firm) in parentheses from the estimation of equation 1. The dependent variable is the bank's loan decision (granted or denied loan), and all variables are defined in Table 1. The lower part of the table reports the number of observations, the adjusted R-squared, and the type of fixed effects used in each specification. All specifications are estimated with OLS, except from specifications 4 and 5, which are estimated with Heckman's two-stage model. For specifications 4 and 5, we also report the number of observations used in the first stage and the estimate on Lambda. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	1	2	3	4	5
	Granted	Granted	Granted	Granted	Granted
Tax decrease	0.030**	0.022**	0.020**	0.026**	0.028**
	(0.012)	(0.011)	(0.010)	(0.013)	(0.013)
Wealth quartile	0.129***	0.079**	0.075**	0.082**	0.085***
	(0.040)	(0.038)	(0.036)	(0.041)	(0.040)
Tax decrease × Wealth	0.028**	0.019*	0.018*	0.027*	0.029*
quartile	(0.013)	(0.011)	(0.011)	(0.016)	(0.016)
Tax decrease × Issuing	0.002	0.002	0.003	0.005	0.006
country	(0.014)	(0.014)	(0.015)	(0.016)	(0.018)
Wealth quartile × Issuing	-0.005	-0.005	-0.005	-0.007	-0.007
country	(0.021)	(0.021)	(0.022)	(0.027)	(0.027)
Tax decrease × Wealth	-0.057***	-0.044**	-0.044***	-0.056**	-0.058**
quartile × Issuing country	(0.022)	(0.020)	(0.021)	(0.028)	(0.028)
Credit score		0.585***	0.478***	0.502***	0.517***
		(0.033)	(0.031)	(0.050)	(0.052)
Tax decrease × Credit			0.123***	0.147***	0.139***
score			(0.031)	(0.047)	(0.042)
Lambda				-0.176	-0.177
				(0.129)	(0.128)
Observations	29,255	29,255	29,255	29,255	29,255
Observations (first stage)				87,870	111,746
Adj. R-squared	0.42	0.96	0.96		
Control variables	No	No	No	Yes	Yes
Year fixed effects	No	No	No	Yes	Yes

Table A3. Robustness tests on the estimation of equation 1

The table reports coefficient estimates on the triple interaction term between *Income quartile* or *Wealth quartile*, *Tax decrease*, and *Issuing country*, as well as respective standard errors (clustered by firm) in parentheses. Each line-column represents the estimates from one regression and all specifications replicate those of specification 3 of Tables 2 and A2, respectively. All specifications are estimated with OLS. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	1 Income quartile	2 Wealth quartile
1. Slide the event date one year backward	-0.015	-0.010
	(0.011)	(0.010)
2. Slide the event date two years backward	-0.005	-0.002
	(0.011)	(0.009)
3. Use observations from the period 2016-2018 as a control group	-0.046**	-0.041**
	(0.021)	(0.020)
4. Use observations from the period 2016-2018 as a treatment group	0.003	-0.001
	(0.013)	(0.012)
5. Use only observations inside the 10% margin around the cutoff	-0.054**	-0.046**
	(0.020)	(0.020)
6. Use only observations inside the 5% margin around the cutoff	-0.051***	-0.049***
	(0.017)	(0.018)
7. Use the median income (wealth) to generate the income (wealth) variable	-0.040*	-0.035*
	(0.023)	(0.018)
8. Use the 90^{th} – 10^{th} income (wealth) centile to generate the income (wealth) variable	-0.109***	-0.076***
	(0.018)	(0.018)

Table A4. Robustness tests on the estimation of equations 2 and 3

The table reports coefficient estimates on the term *Tax decrease* × *Income quartile* × *Issuing country* and respective standard errors (clustered by firm) in parentheses. Each line-column represents the estimates from one regression and all specifications replicate those of specifications 1 and 3 of Table 3, respectively. All specifications are estimated with OLS. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	1 Equation 2	2 Equation 3
1. Slide the event date one year backward	0.014	-0.010
	(0.014)	(0.007)
2. Slide the event date two years backward	-0.003	0.001
	(0.014)	(0.006)
3. Use observations from the period 2016-2018 as a control group	0.075***	-0.074***
	(0.019)	(0.018)
4. Use observations from the period 2016-2018 as a treatment group	-0.001	-0.004
	(0.015)	(0.011)
5. Use only observations inside the 10% margin around the cutoff	0.072***	-0.069***
	(0.018)	(0.016)
6. Use only observations inside the 5% margin around the cutoff	0.081***	-0.088***
	(0.017)	(0.015)
7. Use the median income to generate the income variable	0.046***	-0.043**
	(0.016)	(0.017)
8. Use the $90^{th} – 10^{th}$ income centile to generate the income variable	0.102***	-0.094***
	(0.019)	(0.025)
9. Using wealth instead of income	0.045***	-0.057***
	(0.014)	(0.019)

Table A5. Robustness tests on the estimation of equation 4

The table reports coefficient estimates on the triple interaction term between *Income quartile* or *Wealth quartile*, *Tax decrease*, and *Issuing country*, as well as respective standard errors (clustered by firm) in parentheses. Each line-column represents the estimates from one regression and all specifications replicate those of specification 1 of Tables 4 and 5, respectively. All specifications are estimated with OLS. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	1	2
	Income t+3	Wealth t+3
1. Slide the event date one year backward	0.014	0.010
	(0.024)	(0.027)
2. Slide the event date two years backward	-0.003	0.001
	(0.024)	(0.026)
3. Use observations from the period 2016-2018 as a control group	0.071***	0.066***
	(0.023)	(0.025)
4. Use observations from the period 2016-2018 as a treatment group	-0.006	-0.003
	(0.025)	(0.021)
5. Use only observations inside the 10% margin around the cutoff	0.082***	0.062***
	(0.028)	(0.016)
6. Use only observations inside the 5% margin around the cutoff	0.091***	0.068***
	(0.023)	(0.025)
7. Use the median income to generate the income variable	0.036**	0.033**
	(0.016)	(0.017)
8. Use the $90^{th} – 10^{th}$ income centile to generate the income variable	0.092***	0.084***
	(0.022)	(0.020)

Figure A1. Manipulation test

The figure reports results from the manipulation testing procedure using the local polynomial density estimator proposed by Cattaneo et al. (2018). To perform this test, we rely on the local quadratic estimator with cubic bias-correction and triangular kernel. The p-value of the robust (bias-corrected) statistic equals 0.314, rejecting the null hypothesis of manipulation.