EUROPEAN CENTRAL BANK
EUROSYSTEM

Working Paper Series

João B. Duarte, Afonso S. Moura

The real and financial effects of local corporate tax increases: evidence from linked firm–bank data

No 3309 / November 2026

Disclaimer: This paper should not be reported as representing the views of the European Central Bank (ECB). The views expressed are those of the authors and do not necessarily reflect those of the ECB.

Abstract

We study how local corporate tax increases affect firms' financing and real activity. In Portugal, municipalities independently set a surtax on corporate income, generating plausibly exogenous variation across space and time. We link the universe of corporate balance sheets and profit-and-loss statements to loan-level data from the Bank of Portugal's credit registry, allowing us to track firms' liquidity, leverage, borrowing costs, and credit quality alongside revenues, employment, and productivity. We estimate local-projection difference-in-differences models that address staggered treatment timing and dynamic responses. Surtax hikes immediately tighten financing conditions: liquidity falls, implicit interest rates rise, debt increases at shorter maturities, and non-performing loans become more prevalent. These strains spill over into real outcomes: firms reduce sales and employment and experience persistent declines in total factor productivity. Effects are strongest among small firms, where leverage rises in response to liquidity losses. We also find higher exit and relocation probabilities, but no gains in neighboring output, pointing to inefficient reallocation. Our results highlight how local tax policy can transmit through both financial and real margins, with implications for fiscal design in decentralized settings.

Keywords: Corporate taxation, local taxation, credit, business activity, firm relocation.

JEL Classification: E62; G38; H25; H32; H71

Non-technical summary

Assessing how corporate tax changes affect firms is difficult for two reasons. First, national tax reforms often coincide with broader economic changes, so it is hard to isolate the effect of taxation itself. Second, it is unclear whether taxes affect firms only by reducing after-tax returns or also by straining their finances through higher tax bills, amplifying the real response. Answering this requires data that capture both financial conditions and real activity, which is rarely available at the firm level.

We address both challenges using repeated changes in a corporate surtax set independently by Portuguese municipalities, which gives us variation in tax burdens within the same country that is plausibly unrelated to firms' circumstances. We link balance sheets and income statements for the universe of Portuguese firms to the Bank of Portugal's Central Credit Registry, so we observe liquidity, debt, borrowing costs, and credit quality alongside sales, employment, investment, and productivity.

We compare firms in municipalities that raised the surtax with firms in municipalities that did not, using a recent difference-in-differences method designed for staggered changes (Dube et al., 2025). Comparison firms come only from municipalities with no tax change shortly before or after. Several pieces of evidence support the assumption that treated and comparison municipalities would have followed similar paths otherwise: local output does not predict surtax increases, there are no diverging pre-trends in local employment or firm numbers, rates are largely decided before the year's conditions are known, and local tax decisions in Portugal follow political cycles more than economic ones. We also compare firms within the same district and year and within the same sector and year. If any bias remains because municipalities tend to raise taxes in good times, it would make the harmful effects look smaller than they truly are.

We find four main results. A typical tax increase raises the surtax by 0.88 percentage points, which lifts firms' effective tax rate by 0.76 points. First, firms' finances weaken

immediately. Net financial debt rises by about 7 percent, liquid funds fall as a share of assets, firms delay payments to suppliers, and borrowing becomes more expensive and shorter-term. Four years later, the share of firms with non-performing loans is about 1.4 points higher, indicating cash shortages and financial distress.

Second, real activity shrinks: employment falls by about 2 percent and productivity by around 2.5 percent after three years. Labor costs fall by 3 percent, more than employment, so pay per worker declines, suggesting workers bear part of the burden. Third, small firms are more affected, while larger firms show no significant financial response. Debt rises exactly where cash falls, pointing to borrowing to cover shortfalls rather than deliberate use of debt for tax advantages. Fourth, a surtax increase raises the probability that a firm exits by 2.2 percentage points and that it relocates by 7.7 points. Neighboring municipalities gain a few firms but no additional output, so mobility shifts activity without creating measurable gains elsewhere.

Our work contributes to three areas of research. The first uses local variation in corporate taxes to study effects on firms and workers, and also complements work on targeted tax measures such as deductions or investment incentives. Our advantage is that we observe financial and real outcomes together for the same firms. The second studies how taxation relates to debt and financial distress—we add evidence from a setting dominated by small, unlisted firms, including trade credit, debt maturity, and non-performing loans. The third shows that financing constraints have real effects, and we connect it to taxation by showing that tax increases create exactly this kind of financial pressure, eventually impacting real activity.

1 Introduction

Assessing how corporate tax changes affect firms poses two challenges. The first is identification. National-level tax reforms often coincide with broader macroeconomic changes, making it difficult to isolate the causal effect of taxation. The second is to determine whether corporate taxes affect firms only through the standard reduction in post-tax returns or whether meeting higher tax liabilities also strains their finances and amplifies the real response. Answering this question requires data that jointly capture firms' financial conditions and real activity—a combination seldom available at the firm level.

This paper addresses both challenges in a single setting. We exploit repeated changes in a corporate surtax ("*derrama municipal*") independently set by Portuguese municipalities, generating staggered, plausibly exogenous within-country variation in corporate tax burdens. We then link annual balance sheets and profit-and-loss statements covering the universe of Portuguese firms to the Bank of Portugal's Central Credit Registry. The combined data allow us to observe firms' liquidity, leverage, borrowing costs, debt maturity, and credit quality alongside sales, employment, investment, and productivity.

We estimate the dynamic effects of surtax increases using local-projection difference-in-differences (LP-DiD) models (Dube et al., 2025), which accommodate staggered and non-absorbing treatment. At each horizon, firms exposed to a surtax increase are compared with clean controls located in municipalities with no recent or subsequent tax change during the relevant treatment window. This restriction prevents firms that may still be adjusting to other tax changes from contaminating the control group. The estimates therefore combine information from multiple municipal tax increases and trace firms' responses over time without relying on a single reform episode.

A causal interpretation requires that, absent the surtax increase, outcomes in treated and control municipalities would have followed parallel trends. Several pieces of evi-

dence support this assumption. First, municipal GVA does not predict surtax increases contemporaneously or at one- and two-year horizons, while municipal employment and the number of active firms display no differential pre-treatment trends. Furthermore, the timing of tax decisions reinforces this evidence: since surtax changes are set at the end of the preceding year for implementation in the next, they are largely predetermined relative to that year's economic conditions, limiting the scope for reverse causality from contemporaneous local shocks. Existing evidence also indicates that Portuguese local tax decisions are driven primarily by political cycles rather than economic conditions (Lopes and Peralta, 2018). Second, district-by-year and sector-by-year fixed effects absorb regional and industry-specific shocks, so identification comes from comparisons within narrowly defined geographic-time and industry-time cells. Surtax revenues are also small relative to municipal budgets, limiting the scope for confounding changes in local public spending. Finally, we find no systematic pre-treatment differences in firm outcomes and assess cross-border spillovers directly through firm relocation and outcomes in neighboring municipalities. If any residual endogeneity remains because municipalities are more likely to raise taxes during favorable economic conditions, it would tend to attenuate the adverse effects we estimate.

The analysis yields four main findings. A positive treatment event raises the municipal surtax by an average of 0.88 percentage points. Since treated municipalities experience a single change, the higher statutory rate remains in place over subsequent years. At the firm level, the effective tax rate increases by approximately 0.76 percentage points on impact. First, surtax increases weaken firms' financial positions immediately. Net financial debt rises by about 7 percent, while the liquidity ratio falls by 0.3 percentage points on impact and by approximately 0.95 percentage points after four years. Firms also delay payments to suppliers, using trade credit to preserve liquidity. External finance becomes more costly: implicit interest rates rise by 0.47 percentage points after one year, and debt increases primarily at shorter maturities. Credit quality subsequently

deteriorates. Four years after the tax increase, the share of firms with non-performing loans is about 1.4 percentage points higher. The financial response therefore extends beyond a simple adjustment in capital structure to include liquidity stress and financial distress.

Second, real activity contracts following the tax increase. The sales-to-assets ratio falls by 2.1 percentage points after two years, employment declines by about 2 percent, and total factor productivity falls persistently, by approximately 2.5 percent after three years. Labor costs decline by 3 percent, more than employment, implying a reduction in labor costs per worker. This pattern is consistent with workers bearing part of the corporate tax burden through both employment and compensation adjustments.

Third, the effects are concentrated among small firms. These firms experience lower liquidity, higher borrowing costs, and a persistent increase in leverage, whereas larger firms show no statistically significant financial response. Increases in leverage therefore occur precisely among firms that experience liquidity losses, consistent with borrowing to cover cash shortfalls rather than strategic use of the tax shield. Taken together, the immediate financial response, the more gradual contraction in real activity, and the concentration of the effects among small firms are consistent with—but do not by themselves identify—a financial amplification mechanism.

Fourth, surtax increases raise the probability of firm exit by 2.2 percentage points and the probability of relocation by 7.7 percentage points. Although the number of firms rises slightly in neighboring municipalities, their aggregate GVA does not. Firm mobility therefore reallocates activity across municipalities without producing detectable gains in neighboring output.

Our findings contribute to three related strands of literature. The first exploits subnational variation in corporate taxation to identify effects on firms and workers. For Germany, Fuest, Peichl and Siegloch (2018) show that higher local business taxes reduce wages, Link et al. (2024) document downward revisions of investment plans, and

Lichter et al. (2025) analyze R&D spending responses.[1] Like these papers, we exploit broad-based subnational tax-setting autonomy. Relative to this work, our contribution lies in the outcomes we observe jointly. While existing studies typically isolate wages, investment, or R&D, the linked balance-sheet and credit-registry records allow us to trace financial and real adjustments within the same firms. We also document responses in productivity, firm survival, relocation, and neighboring output, providing a broader account of adjustment to local corporate tax changes.

Related work studies how reforms that lower effective tax rates affect investment and activity, typically through targeted deductions, depreciation rules, or investment incentives (House and Shapiro, 2008; Zwick and Mahon, 2017; Ohrn, 2018; Maffini, Xing and Devereux, 2019; Guceri and Albinowski, 2021; Xu and Zwick, 2024; Liu and Mao, 2019; Moon, 2022). Our employment results are consistent with the substantial responses documented by Garrett, Ohrn and Suárez Serrato (2020). Because these instruments change the tax treatment of particular investments or firms, their estimated effects need not carry over to changes in the overall corporate tax burden. A municipal surtax applies across the corporate tax base and therefore speaks more directly to this broader question. Moreover, this literature primarily identifies real-side responses, whereas observing financial and real outcomes jointly allows us to document the financing pressures that accompany the contraction in activity.

The second strand studies the relationship between corporate taxation, capital structure, and financial distress. Heider and Ljungqvist (2015) find that U.S. firms increase leverage following corporate tax hikes, while Sanati and Beyhaghi (2024) show that reducing the tax benefits of debt lowers firm indebtedness. Faccio and Manfredonia (2026) find that higher corporate tax rates increase financial distress, particularly among geographically concentrated firms, and show that the interest limitation introduced by the 2017 Tax Cuts and Jobs Act reduced distress by inducing deleveraging. Ivanov, Pettit

[1] In Switzerland, Krapf and Staubli (2024) study how municipal tax differences shape tax base elasticities.

and Whited (2025), by contrast, find that tax cuts increase leverage among small private firms and attribute the response to the productivity of capital rather than the tax shield motive. We contribute to this debate by studying a setting dominated by small, unlisted firms and by observing leverage and liquidity jointly. Leverage rises precisely among firms experiencing liquidity losses, pointing to borrowing that covers cash shortfalls rather than strategic use of the tax shield. We also provide evidence on trade credit, debt maturity, and non-performing loans—margins that are rarely observed in studies of corporate taxation but are central to understanding its implications for financial stability.

The third strand establishes that financing constraints have real effects. Exogenous reductions in internal funds lower corporate investment (Rauh, 2006), while disruptions to bank credit reduce employment and broader economic activity (Chodorow-Reich, 2014; Huber, 2018) and affect firm investment (Amiti and Weinstein, 2018). Melcangi (2026) shows that cash flow shocks from the 2010 revaluation of U.K. business rates move firm employment, especially among small and more leveraged firms. Evidence from Portugal also shows that targeted credit support raises investment and growth among small firms (Bonfim, Custódio and Raposo, 2023). We connect this literature to corporate taxation by showing that tax increases generate the types of financial pressure it identifies as consequential. We do not estimate the causal mediation from financial strain to real activity; rather, we document both empirical links within the same firms: financial conditions deteriorate after the tax shock, and real activity subsequently contracts, especially among small firms.

The remainder of the paper is organized as follows. Section 2 outlines the institutional background and data. Section 3 presents the empirical framework. Section 4 reports the results on financial conditions and real outcomes (including heterogeneity and relocation). Section 5 provides robustness checks. Section 6 concludes.

2 Institutional Background and Data

Corporate income in Portugal is taxed at a national rate plus a municipal surtax, set independently by each of the country's 278 mainland municipalities. This institutional feature—staggered, plausibly exogenous variation in local tax burdens—is the source of identification for our empirical design. This section describes the surtax mechanism in detail and then turns to the administrative data on firm balance sheets and credit, the sample restrictions we impose, and descriptive statistics.

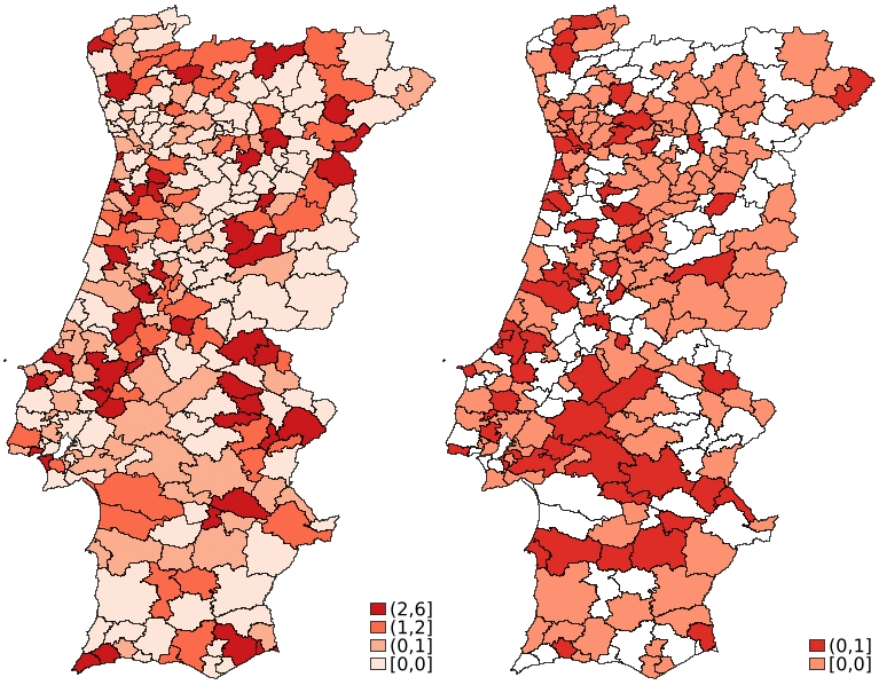
2.1 Corporate taxation and institutional setup

In Portugal, corporate taxation combines a national statutory code with a municipal surtax (*derrama municipal*). The national corporate tax code, explained in detail in section A.1 of the appendix, applies uniformly to all firms, while each of the 278 mainland municipalities sets an additional surtax between 0 and 1.5 percent of taxable profits. Municipal assemblies approve the surtax rate during the annual budget cycle, typically toward the end of the preceding calendar year, and the corresponding liability is subsequently settled through the corporate income tax return. In the empirical analysis, we date each change to the first fiscal year in which the new rate applies. The municipal decision therefore predates the annual firm outcomes assigned to the treatment year, mitigating mechanical reverse causality from contemporaneous firm performance to the tax rate. Municipalities choose only the statutory surtax rate: the tax base and liability rules are defined nationally and cannot be tailored to individual firms. This institutional structure provides sharp cross-sectional and temporal variation in effective tax rates.

Between 2009 and 2021, the average municipal surtax on the full sample was 0.88 percent, with a median of 1.2 percent. Of the 278 municipalities, 181 never changed their rate throughout our sample period, while 78 kept it permanently at zero. On average, around 3 percent of firms experience a surtax increase each year. Revenues

from the surtax account for only 0.2 percent of local GVA, so changes are unlikely to be accompanied by major shifts in local fiscal policy.[2] For our baseline analysis, we restrict the sample to municipalities that either never changed their surtax or changed it only once during the sample, which avoids multiple treatment episodes for the same firm and preserves comparability across treated and untreated groups. Figures 1a and 1b illustrate the distribution of surtax changes across municipalities before and after this restriction, while Figure 2 shows their timing and magnitude.

Figure 1: Number of corporate surtax changes by municipality

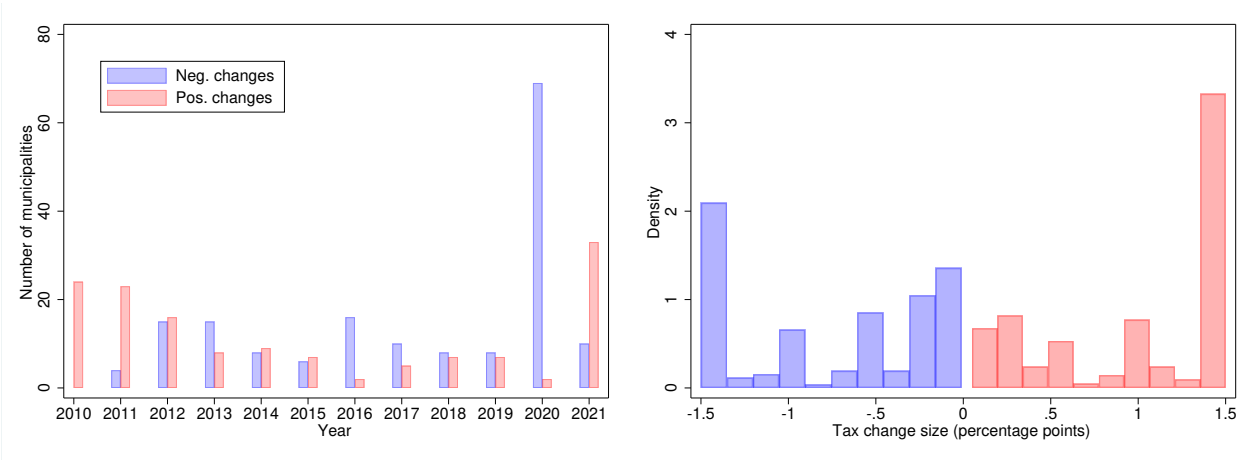


(a) All municipalities (b) Restricted sample

Notes: This figure shows cross-sectional differences in the number of corporate surtax changes implemented in Portuguese municipalities between 2009 and 2021. Figure 1a shows the total number of surtax changes experienced in each municipality; Figure 1b restricts the sample to municipalities that implemented either no changes or only one change in their municipal corporate surtax rate (municipalities in white experienced more than one surtax change and are therefore dropped from the sample).

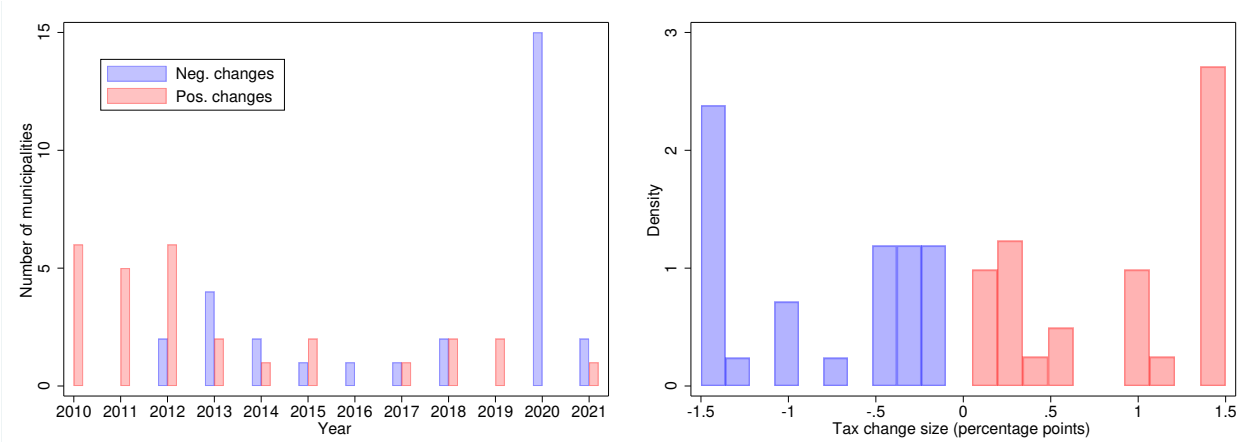
[2]In appendix A.2 we detail more descriptive statistics at the municipality level.

Figure 2: Temporal and size distributions of corporate surtax changes



(a) Number of changes by year, all

(b) Distribution of changes, all



(c) Number of changes by year, restricted sample

(d) Distribution of changes, restricted sample

Notes: This panel shows the time series of the number of municipalities implementing a surtax change and the distribution of the sizes of those changes. In the upper row, Figure 2a shows the number of municipalities changing the corporate surtax across years, while Figure 2b shows the size distribution of those changes, both for the full sample. The second row presents the same graphs for the restricted sample (see the text for further details): Figure 2c shows the number of municipalities changing the surtax rate by year, while Figure 2d shows the distribution of the sizes of those changes. In all subfigures, increases in the surtax are highlighted in red, while decreases are highlighted in blue.

2.2 Firm-level data

Our main source of firm-level data is the Simplified Corporate Information Survey (*Informação Empresarial Simplificada, IES*), filed annually with the tax authorities and compiled by the Bank of Portugal (Banco de Portugal Microdata Research Laboratory -

BPLIM, 2023). The IES covers the universe of Portuguese firms, including private non-financial enterprises of all sizes, and reports detailed balance-sheet and profit-and-loss variables. Compared with datasets such as Compustat, which cover only publicly listed firms, the IES captures the entire corporate sector and its heterogeneity, making it especially valuable in an SME-dominated economy.

We link the IES to the Central Credit Registry (CRC), a monthly loan-level database maintained by the Bank of Portugal, which records all outstanding loans to non-financial firms, including exposures, collateral, and loan performance (Banco de Portugal Microdata Research Laboratory - BPLIM, 2024). We aggregate the CRC to the firm-year level and merge it with the IES. The resulting dataset provides a unique opportunity to analyze both financial and real margins of adjustment, covering leverage, liquidity, trade credit, and non-performing loans alongside revenues, employment, and productivity.[3]

We restrict the sample to private non-financial firms with more than one employee and complete years of activity. Firms in the autonomous regions (Madeira and the Azores) are excluded because they follow distinct tax rules. Balance-sheet and profit-and-loss variables are winsorized at the 1st and 99th percentiles. Table 1 reports descriptive statistics for firms in never-treated municipalities and for those observed in the year before a surtax increase. The two groups are similar across balance-sheet, labor, tax, and credit measures.

Finally, we estimate firm-level total factor productivity (TFP) following Wooldridge (2009), assuming a Cobb–Douglas production function with sector-specific factor shares.[4] Results are robust to alternative estimators such as Levinsohn and Petrin (2003) and Ackerberg, Caves and Frazer (2015). On average, pre-treatment productivity growth is similar across treated and untreated firms, consistent with the identifying assumption of parallel trends.

[3] See, for example, Bonfim, Custódio and Raposo (2023), Gabriel (2024), and Bonfim et al. (2024) for other applications of these data.

[4] We detail the estimation procedure in Appendix A.4.

Table 1: Firm balance-sheet and credit descriptive statistics

	Never treated			Pre-treatment		
	Mean	**Std. Dev.**	**Median**	**Mean**	**Std. Dev.**	**Median**
Balance sheet						
Total assets (€K)	877.09	2711.05	160.27	723.88	2260.31	154.73
Leverage	10.47	58.59	0.00	11.32	51.34	4.47
Liquidity	19.35	22.95	9.76	17.80	21.98	8.56
Tangibility	24.01	26.06	13.95	26.46	26.26	17.64
ROA	-6.12	59.56	5.37	-3.13	52.03	5.53
Productivity						
TFP growth	-0.78	66.34	0.51	-2.82	64.64	-0.89
Labor						
# Employees	11.73	132.64	4.00	9.01	34.97	4.00
Taxes						
ETR	24.79	18.18	21.12	23.05	17.48	18.84
Credit						
Total (€K)	386.99	4520.23	27.98	386.76	5286.91	38.33
Non-performing (€K)	16.75	811.46	0.00	8.70	175.82	0.00

Notes: The sample consists of 2,086,873 firm-year observations between 2009 and 2021. Never-treated firms operate in municipalities that did not change their corporate surtax during our sample period. The pre-treatment columns report firm-level averages over the period before treatment. Balance-sheet data are winsorized at 1% and 99%. Effective tax rates (ETRs) are restricted to values between 0% and 100%. Monetary variables are expressed in real terms.

3 Empirical Methodology

3.1 Research design

We analyze the impact of corporate tax changes on firm-level outcomes by estimating local projections (LPs) (Jordà, 2005) in a panel data context. In particular, we extend the usual LP framework to a local-projection difference-in-differences (LP-DiD) approach, following the methodology proposed by Dube et al. (2025). We estimate the following model for each horizon $t + h$, and construct impulse response functions (IRFs) from the estimated coefficients $\hat{\beta}^h$:

$$\Delta_h y_{i,t+h} = \alpha^h_{d,t} + \phi^h_{s,t} + \beta^h \Delta D_{m,t} + \varphi^h X_{i,t} + \varepsilon_{i,t+h}, \tag{1}$$

where $y_{i,t}$ is the outcome for firm i in year t and $X_{i,t}$ includes controls at the municipality and firm levels.[5] The baseline specification controls for contemporaneous and three lagged values of log municipality-level gross value added (GVA), capturing local business cycles, and lagged firm-level effective tax rates (ETR), accounting for heterogeneity in firms' tax positions prior to treatment—in particular, differences in tax brackets and eligibility for tax incentives.[6] We also include *district* $\times$ *year* ($\alpha^h_{d,t}$) and *sector* $\times$ *year* ($\phi^h_{s,t}$) fixed effects.[7] The operator Δ_h denotes long-differencing, $\Delta_h y_{i,t+h} = y_{i,t+h} - y_{i,t-1}$, which cumulates IRFs over time. We should also emphasize that this transformation nets out firm fixed effects, since "*as a result of differencing, the LP specification does not include unit fixed effects*" (Dube et al., 2025, p. 743). Furthermore, parallel trends between treated and control firms, as well as possible anticipation effects, are tested by including horizons $h = -4$ to $h = -2$ in the estimated IRFs. Finally, IRFs are presented with robust standard errors clustered at the municipality level.[8]

A challenge in staggered-treatment settings is the contamination of controls by previously treated units. Following Dube et al. (2025), we impose clean-control restrictions for each horizon $t + h$. Specifically, we address two issues: (i) treatment is non-absorbing, as some municipalities adjust the surtax multiple times during the sample, and (ii) control groups, even if not treated contemporaneously, must exclude firms that are still responding to past treatment. Formally, we impose the following sample restrictions:

$$\begin{cases} \text{treatment:} & \left(D_{i,t+j} = 1 \text{ for } 0 \leq j \leq h\right) \text{ and } \left(D_{i,t-j} = 0 \text{ for } j \geq 1\right), \\ \text{clean control:} & D_{i,t-j} = 0 \text{ for } -h \leq j \leq L. \end{cases} \tag{2}$$

[5] The dependent variables enter in logs, so IRFs are reported as cumulative percentage changes. When the dependent variable is a ratio (e.g., leverage), it enters in levels, so IRFs are reported in percentage points.

[6] We use the one-period lagged value rather than the contemporaneous ETR to avoid a bad control problem: since the surtax directly affects firms' tax liabilities, contemporaneous ETR is itself an outcome of the treatment, and its inclusion could introduce post-treatment bias.

[7] Districts are a higher administrative level than municipalities; mainland Portugal has 18 districts, each containing an average of 15 municipalities.

[8] Results are robust to alternative clustering strategies, such as two-way clustering at the municipality and year levels.

In practice, treated units are firms that experience exactly one surtax increase during the sample period. Controls are firms that are never treated or were treated only in the distant past. In the baseline, we set $L = 6$, excluding from the control group any firm operating in a municipality with a surtax change in the previous six years.[9] We also exclude firms facing surtax changes in the post-treatment horizon ($t + 1$ to $t + h$).[10] Our baseline results focus on surtax increases, $\Delta D_{m,t} = \mathbf{1}(\Delta \text{surtax}_{m,t} > 0)$.[11] Firms operating in municipalities that lower their surtax rates are also excluded from the control group for the relevant L-year window.

3.2 Identification

Our identification strategy does not require municipal tax changes to be randomly assigned. It requires that, conditional on the clean-control restrictions, fixed effects, and observed controls, outcomes for firms in treated municipalities would have evolved along the same trajectory as outcomes for firms in untreated municipalities in the absence of the surtax change. This is a statement about the counterfactual—what would have happened to treated firms had the surtax not changed—and is therefore inherently untestable. In what follows, we discuss the main threats to this assumption and present institutional and empirical evidence supporting its plausibility.

Timing of tax decisions and endogeneity of treatment to local conditions. Municipal surtax rates are approved during the annual budget cycle, typically toward the end of the calendar year preceding their application. Our treatment indicator dates a change to the first fiscal year in which the new rate applies, rather than to the year in which it is ap-

[9] Results are robust to both relaxing the single-treatment restriction—allowing firms to experience multiple surtax increases while still imposing the clean-control requirement—and using alternative clean-control windows, including $L = 12$, in which case controls consist only of never-treated firms (see Appendix C).

[10] As noted by Dube et al. (2025), conditioning on future treatment can bias estimates if selection is endogenous. In our case, surtax changes are plausibly exogenous at the firm level.

[11] Importantly, Section B.1 of the Appendix shows that, on average, a treated firm experiences an increase of around 0.76 percentage points in its effective tax rate on impact.

proved. The tax decision is therefore predetermined relative to the annual firm outcomes assigned to the treatment year. This timing substantially limits direct reverse causality from contemporaneous firm performance to the surtax rate. It does not, by itself, rule out decisions based on forecasts of future local conditions; the tests of treatment predictability, local economic pre-trends, and firm-level pre-treatment dynamics discussed below address that remaining concern.

Building on the timing argument, we further examine whether municipal decisions to increase the corporate surtax are systematically correlated with both contemporaneous and lagged local economic conditions. We start by estimating the change in the probability that a municipality experiences a surtax increase in response to a one percent increase in municipal GVA. Table 2 reports the estimates for the full sample and for the restricted sample. Across specifications, the estimated coefficients are statistically indistinguishable from zero—both contemporaneously and at one- and two-year horizons, the latter accounting for the possibility of lagged policy responses to economic conditions. Overall, these results indicate that surtax increases are not correlated with contemporaneous or lagged local economic conditions, mitigating concerns that endogenous policy responses to local economic performance threaten our identification strategy. Section B.2 of the appendix complements this evidence by testing for pre-trends in municipal employment rates and the number of active firms, reaching similar conclusions.

Further evidence points in the same direction: existing research documents an important role for political ideology and strategic interaction across neighboring municipalities in the Portuguese municipal surtax setting (Lopes and Peralta, 2018). These determinants are distinct from short-run shocks to the performance of individual firms and complement our direct evidence that municipal GVA does not predict surtax increases. We do not interpret the political evidence as establishing random assignment; rather, it supports the view that rate changes reflect a broader municipal policy process and are not simply mechanical responses to contemporaneous firm outcomes.

Table 2: Probability of surtax increase following a local GVA increase

	All changes		
	Contemporaneous	1 year ahead	2 years ahead
Municipal GVA	0.0003	-0.0002	0.0007
	(0.0005)	(0.0005)	(0.0007)
District × year FE	Yes	Yes	Yes
Observations	2,780	2,502	2,224
	Restricted sample		
	Contemporaneous	1 year ahead	2 years ahead
Municipal GVA	-0.0000	-0.0000	-0.0005
	(0.0002)	(0.0002)	(0.0004)
District × year FE	Yes	Yes	Yes
Observations	1,660	1,494	1,328

Notes: The dependent variable is a dummy variable that takes the value of one if the municipality experienced a positive change in the corporate surtax in period t. *, **, and *** indicate statistical significance at p<0.1, p<0.05, and p<0.01, respectively. The estimation includes *district* × *year* fixed effects, as well as lagged municipal GVA as an additional control. Robust standard errors are clustered at the municipality level.

Important confounding government spending changes are also unlikely, since revenues from the municipal surtax are quantitatively small, amounting on average to approximately 0.2% of local gross value added and around 4.5% of total municipal tax revenues.[12] Their limited budgetary importance reduces the scope for large contemporaneous changes in municipal expenditure financed by the additional surtax revenue. This argument does not rule out all coincident municipal policies, but it makes a quantitatively important spending channel less likely. At the same time, the direct firm-level tax burden is meaningful: Section B.1 shows that the statutory change raises treated firms' effective tax rate by approximately 0.76 percentage points on impact.

Finally, our econometric specification includes *district* × *year* fixed effects, which ab-

[12]This last value refers to 2019 and comes from *Portal Mais Transparência*, a government initiative that publicly reports municipal accounts.

sorb time-varying shocks common to municipalities within the same district. Since mainland Portugal has 18 districts, each containing, on average, 15 municipalities, treated and control firms are compared within tightly defined geographic and temporal cells. This substantially reduces the risk that identification relies on comparisons between municipalities experiencing considerably different regional economic conditions. Likewise, *sector* $\times$ *year* fixed effects absorb nationwide industry-specific fluctuations, ensuring that the estimates do not compare firms exposed to different national sectoral cycles.

Firm influence over tax-setting. A separate concern is that firms may influence municipal tax-setting decisions through lobbying or political pressure. Several features limit this possibility. The municipality sets a single statutory rate that applies to all liable firms, while the tax base and liability rules are determined by national law. The vast majority of Portuguese firms are small, limiting their individual influence over municipal policy, and our baseline focuses on surtax increases, which firms would generally have little incentive to request.[13]

Spillovers (SUTVA). The stable unit treatment value assumption may be violated if firms relocate across municipal borders or if competitive spillovers operate across jurisdictions. Rather than assuming such responses away, Section 4 measures them directly through firm relocation and the evolution of firm counts and aggregate output in neighboring municipalities. We find a relocation response and a small increase in neighboring firm counts, but no detectable increase in neighboring aggregate output. These results do not establish the complete absence of spillovers; they make their observed spatial margins transparent and show that the contraction in treated municipalities is not fully offset by measurable output gains in neighboring areas.

[13]The focus on tax increases is further motivated by the concentration of surtax decreases during the Covid period, which were largely aimed at supporting firms through the pandemic (see Figure 2).

Supporting evidence from pre-treatment dynamics. Although the identifying assumption is about counterfactual post-treatment trends and cannot be directly verified, the behavior of outcomes prior to treatment provides useful diagnostic evidence. Our impulse response functions include pre-treatment horizons $h = -4$ and $h = -2$, which allow us to assess whether treated and control firms were already diverging before the surtax change. The absence of significant pre-treatment effects across our main outcomes is consistent with—though does not prove—the validity of the identifying assumption. It also alleviates concerns about anticipatory adjustments by firms in response to announced surtax changes.

Interpreting the combined evidence. None of the arguments above proves the identifying assumption in isolation. However, an omitted factor capable of explaining the full set of results would have to be a municipality-specific shock that coincides with the predetermined tax change, is not absorbed by district-by-year or sector-by-year fixed effects, is not reflected in contemporaneous or lagged municipal GVA or in pre-treatment employment and firm-count dynamics, and simultaneously generates an immediate rise in firms' effective tax rates and financial pressure followed by a more gradual contraction in real activity. It would also need to explain why the financial response is concentrated among small firms. The conjunction of the institutional timing, clean-control design, pre-treatment diagnostics, and internally consistent outcome dynamics therefore supports a causal interpretation, while making explicit that identification remains conditional on counterfactual parallel trends.

4 Results

This section presents the dynamic effects of municipal surtax increases on firm outcomes. We begin with financial conditions, focusing on leverage, liquidity, cash, trade credit, and credit quality. We then turn to real outcomes such as sales, earnings, inputs,

employment, and productivity. Next, we explore heterogeneity across firms of different sizes, ages, and profitability. Finally, we examine exit and relocation, and conclude with a short synthesis.

4.1 Financial conditions

We begin by examining firms' balance-sheet responses. Figure 3 presents impulse responses for leverage and liquidity ratios, while Figure 4 decomposes their underlying drivers.

Leverage increases gradually following a tax increase. The impact response is about 0.85 percentage points and reaches 1.5 percentage points after three years (Figure 3a), in line with results for the U.S. in Heider and Ljungqvist (2015). This increase is driven both by rising financial debt and by declining fixed assets. Net financial debt rises immediately, by about 7.0 percent on impact ($\approx$ €10,400 for the mean firm in our sample)[14], and remains elevated over the following couple of years (Figure 4b). Fixed assets fall more gradually, with a decline of around 11.5 percent after five years (Figure 4a).

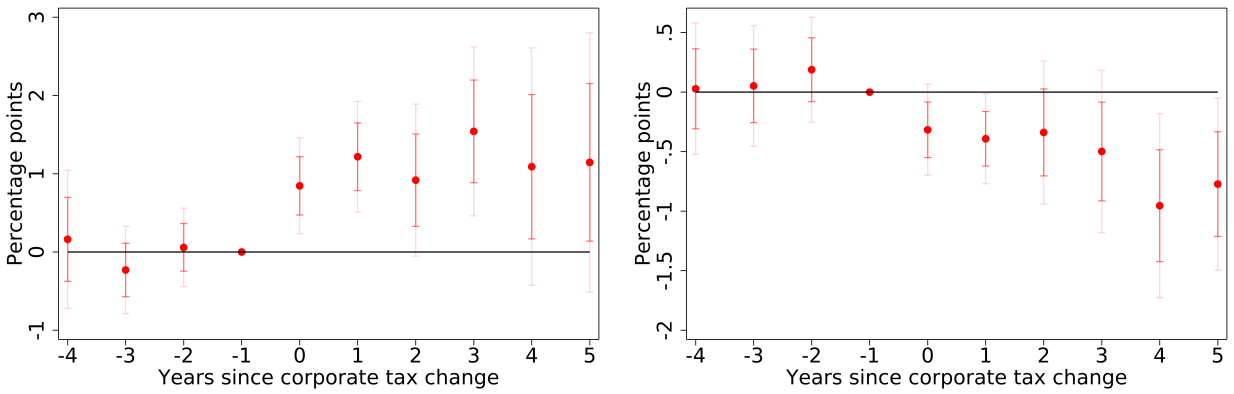
Liquidity declines in parallel. The liquidity ratio falls by 0.3 percentage points on impact and reaches a decline of 0.95 percentage points after four years (Figure 3b). Cash and bank deposits show a more delayed response, declining only modestly in the first year before falling more substantially over the medium term (Figure 4c). Overall, these patterns are consistent with tax hikes tightening firms' liquidity positions.

Trade credit also adjusts, reflecting liquidity-management decisions that firms are forced to make. The ratio of payables to cost of goods sold (COGS) rises, indicating that firms delay payments to suppliers (Figure 4d). By contrast, the ratio of receivables to sales is unchanged (Figure 4e), suggesting that—despite the incentive to accelerate client payments—firms have limited scope to do so. This asymmetry implies that liquidity shocks are passed upstream along supply chains (echoing previous findings such as

[14]The average level of net financial debt in our sample is €148,521.

Boissay and Gropp, 2013 and Amberg et al., 2021).

Figure 3: The effect of a corporate tax increase on leverage and liquidity



(a) Leverage ratio

(b) Liquidity ratio

Notes: The IRFs show the point estimates $\hat{\beta}^h$ of regression (1) reflecting the responses of the dependent variables to positive treatment, that is events in which there was an increase in corporate taxes in municipality m in year t. Point estimates for each horizon are presented in circles, together with 68% and 90% confidence bands (darker and lighter bands, respectively). The estimation includes *district* × *year* and *industry* × *year* fixed effects, as well as additional firm- and municipality-level controls. Robust standard errors are clustered at the municipality level.

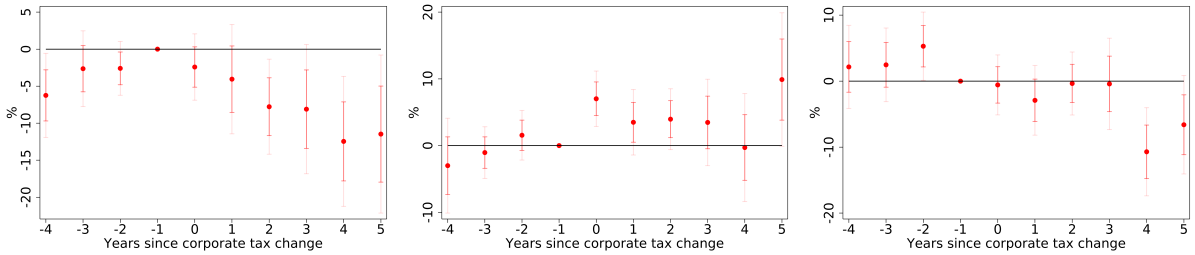
Credit conditions. The increase in financial debt is accompanied by higher borrowing costs and overall worse credit conditions. Figure 5 shows that implicit interest rates rise by 0.13 percentage points on impact and 0.47 points after one year.[15] Both demand and supply channels are plausible: firms may seek more external financing to compensate for the shortfall in liquidity, while a credit supply channel may also be present as lower earnings after tax erode collateral value.[16] However, the joint rise in both borrowing costs and debt volumes hints at a demand-driven shift, suggesting that the demand channel dominates.

Short- and long-term leverage both increase following the tax hike (Figures 6a and 6b). Short-term leverage rises more strongly, increasing on impact and reaching 1.6 percentage points after four years. Long-term leverage responds more gradually and less

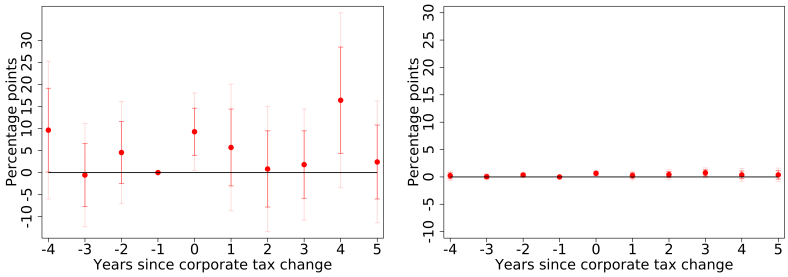
[15]Implicit interest rates are proxied by the ratio between interest outlays and lagged financial debt.

[16]See, e.g., earnings-based lending mechanisms discussed in Lian and Ma (2021); Ivashina, Laeven and Moral-Benito (2022); Caglio, Darst and Kalemli-Özcan (2021); Drechsel (2023); Gabriel (2024).

Figure 4: The effect of a corporate tax increase on capital structure and financing



(a) Fixed assets (b) Net Financial Debt (c) Cash and bank deposits



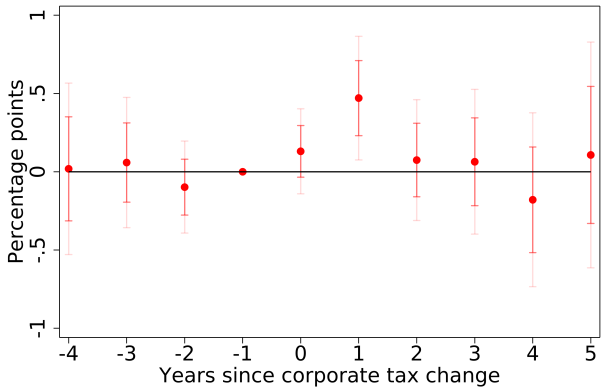
(d) $\frac{Payables}{COGS}$ (e) $\frac{Receivables}{Sales}$

Notes: The IRFs show the point estimates $\hat{\beta}^h$ of regression (1) reflecting the responses of the dependent variables to positive treatment, that is events in which there was an increase in corporate taxes in municipality m in year t. Point estimates for each horizon are presented in circles, together with 68% and 90% confidence bands (darker and lighter bands, respectively). The estimation includes $district \times year$ and $industry \times year$ fixed effects, as well as additional firm- and municipality-level controls. Robust standard errors are clustered at the municipality level.

strongly, remaining below 0.4 percentage points for the first two years before reaching 0.87 percentage points in year three. This pattern is consistent with firms first addressing liquidity shortfalls through short-term borrowing.

Overall, credit quality deteriorates. Non-performing loans (NPLs) increase both in value (as a share of total assets) and, especially, in incidence across firms (Figures 6c and 6d). Four years after the shock, the share of firms with an NPL is about 1.4 percentage points higher than at baseline. This highlights that tax hikes exacerbate financial fragility, one of our novel findings emphasized in the Introduction.

Figure 5: The effect of a corporate tax increase on implicit interest rates



Notes: The IRFs show the point estimates $\hat{\beta}^h$ of regression (1) reflecting the responses of the dependent variables to positive treatment, that is events in which there was an increase in corporate taxes in municipality m in year t. Point estimates for each horizon are presented in circles, together with 68% and 90% confidence bands (darker and lighter bands, respectively). The estimation includes *district* × *year* and *industry* × *year* fixed effects, as well as additional firm- and municipality-level controls. Robust standard errors are clustered at the municipality level.

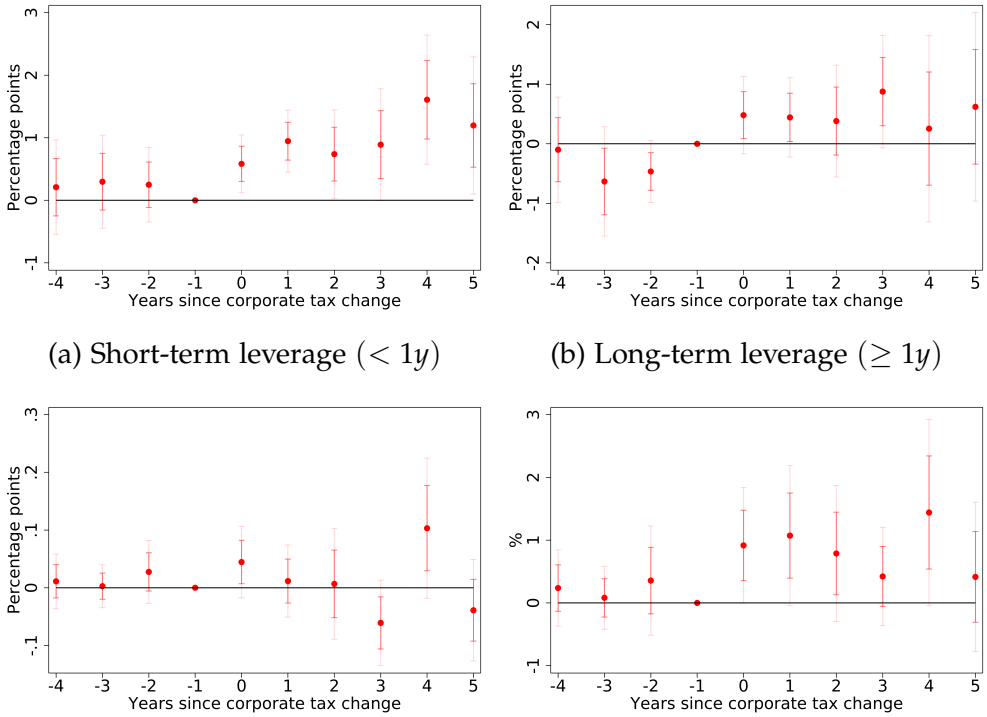
4.2 Real outcomes

We next examine firms' activity responses to the corporate tax change. Figure 7 shows impulse responses for earnings, sales, number of workers, labor costs, external services, and total factor productivity.

Earnings before taxes fall by 1.2 percentage points of assets one year after the shock (Figure 7a). Although less precisely estimated, sales over assets decline gradually, reaching -2.1 percentage points after two years (Figure 7b). On the input side, firms also cut back on labor and external services. Spending on external services falls by 1.3 percentage points of assets three years after the tax increase (Figure 7e). Employment declines gradually, reaching around -2 percent, while total labor costs fall by 3 percent (Figures 7c and 7d), consistent with Fuest, Peichl and Siegloch (2018) and Cloyne, Kurt and Surico (2025). Since labor costs decline by more than employment, the average cost per worker falls as well, suggesting the tax burden is passed onto workers not only through job losses but also through lower per-worker compensation.

Total factor productivity declines persistently, falling by around 2.5 percent three

Figure 6: The effect of a corporate tax increase on debt by maturity and non-performing credit



(a) Short-term leverage ($< 1y$)

(b) Long-term leverage ($\geq 1y$)

(c) Non-performing credit

(d) Non-performing credit: extensive margin

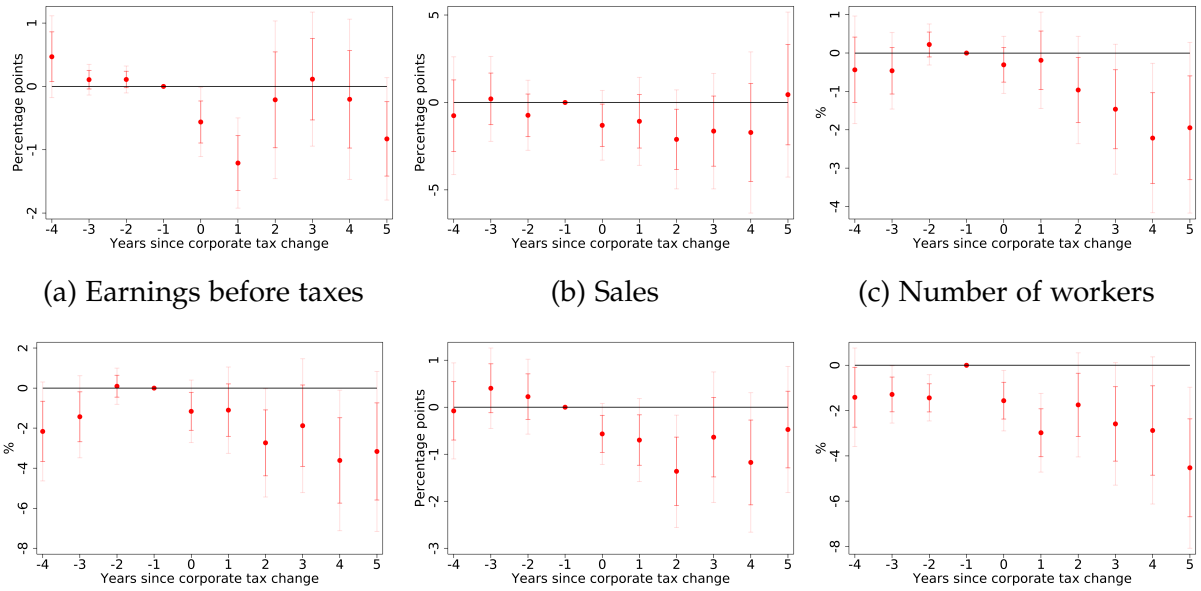
Notes: The IRFs show the point estimates $\hat{\beta}^h$ of regression (1) reflecting the responses of the dependent variables to positive treatment, that is events in which there was an increase in corporate taxes in municipality m in year t. Point estimates for each horizon are presented in circles, together with 68% and 90% confidence bands (darker and lighter bands, respectively). The estimation includes *district* $\times$ *year* and *industry* $\times$ *year* fixed effects, as well as additional firm- and municipality-level controls. Robust standard errors are clustered at the municipality level.

years after the shock and eventually reaching a sizable decline of 4.5 percent (Figure 7f). This productivity loss is consistent with the observed disinvestment in fixed assets and reductions in employment, underscoring that the effects of surtax increases extend beyond financial adjustments into firms' real capacity.

4.3 Heterogeneous impacts

We next assess whether responses differ across firms with different characteristics. To do so, we re-estimate specification (1) under the same clean-control restrictions in equa-

Figure 7: The effect of a corporate tax increase on firm dynamics



(a) Earnings before taxes (b) Sales (c) Number of workers

(d) Labor costs (e) External services (f) TFP

Notes: The IRFs show the point estimates $\hat{\beta}^h$ of regression (1) reflecting the responses of the dependent variables to positive treatment, that is events in which there was an increase in corporate taxes in municipality m in year t. Point estimates for each horizon are presented in circles, together with 68% and 90% confidence bands (darker and lighter bands, respectively). The estimation includes *district* × *year* and *industry* × *year* fixed effects, as well as additional firm- and municipality-level controls. Robust standard errors are clustered at the municipality level.

tion (2), but separately by firm subgroup. This ensures that treated and control firms are always compared within the same category (e.g., treated small firms against untreated small firms), avoiding cross-group contamination. We focus on three dimensions: size, age, and profitability.[17]

Table 3 reports results for leverage, liquidity, interest rates, and earnings at horizons $h = 1$ and $h = 3$. Small firms show the strongest responses, which are in most cases statistically different from the responses of larger firms: leverage rises permanently, liquidity falls by 0.7 to 1 percentage point, and interest rates rise by 0.4 points one year after the shock—although this increase is statistically significant only at the one-standard-

[17]Small firms are defined as firms that employ fewer than 10 workers and have an annual turnover or an annual balance-sheet total not exceeding €2 million; young firms are defined as firms younger than 10 years; profitable firms are defined as firms that have positive average earnings over the three years prior to the tax change.

deviation confidence level. Earnings also fall sharply. Larger firms, on the other hand, show no statistically significant response in leverage, liquidity, or interest rates. While the literature has long emphasized that corporate taxation creates incentives to increase debt usage because interest is deductible, lowering taxable profits (Heider and Ljungqvist, 2015; Zwick and Mahon, 2017; Sorensen, 2017), we find little evidence of this channel in our sample. Instead, the leverage increases documented above appear to be driven entirely by liquidity pressure: smaller firms borrow more when retained earnings fall, while larger firms—facing no significant liquidity losses—do not adjust their financial structure. This suggests that, at least in our setting, the liquidity channel dominates over the tax shield motive.

Splitting firms by age tells a different story. Young and old firms display broadly similar adjustment patterns, though responses tend to be more precisely estimated for older firms. This is likely connected to the composition of the Portuguese economy, which is dominated by small firms regardless of age—limiting the scope for age-based heterogeneity to emerge independently of size. Old firms increase their leverage by 1.8 points after three years, liquidity falls by 0.6 points, and interest rates rise by 0.5 points in the short run, although these effects are statistically indistinguishable from those for younger firms. These results suggest again that liquidity pressure is the dominant mechanism across firm age groups, with limited evidence of tax-shield behavior even among older firms.

Profitability, on the other hand, also matters. Firms that have positive average earnings over the three years before the reform exhibit large increases in leverage and interest rates and significant declines in earnings, whereas non-profitable firms show muted responses. This is consistent with the notion that firms actually paying taxes are most exposed to surtax changes.

Overall, the heterogeneity results reinforce the view that surtax increases disproportionately harm small firms, which face tighter financial constraints (Harju, Koivisto and

Matikka, 2022).

Table 3: Heterogeneous effects of corporate taxation

	Leverage		Liquidity		Int. Rate		EBT	
	h=1	**h=3**	**h=1**	**h=3**	**h=1**	**h=3**	**h=1**	**h=3**
Small	1.80***	2.14***	-0.73**	-1.04*	0.44	0.03	-1.85***	0.34
	(0.61)	(0.81)	(0.33)	(0.57)	(0.32)	(0.29)	(0.59)	(0.61)
# Obs	256,735	160,348	256,735	160,348	158,984	94,494	256,735	160,348
Big	-0.04	0.36	0.30	0.57	0.50	0.33	-0.01	-0.43
	(0.61)	(0.83)	(0.36)	(0.52)	(0.33)	(0.48)	(0.42)	(0.96)
# Obs	124,530	80,434	124,530	80,434	101,503	64,323	124,530	80,434
Wald test	0.03	0.12	0.04	0.03	0.90	0.58	0.01	0.50
Young	0.68	0.91	0.04	-0.23	0.42	0.45	-0.54	4.50
	(0.89)	(1.34)	(0.55)	(0.96)	(0.39)	(0.46)	(4.44)	(4.83)
# Obs	116,681	71,321	116,681	71,321	75,919	44,961	98,922	60,151
Old	1.39***	1.77**	-0.6**	-0.63*	0.51**	-0.06	-9.67**	-6.02
	(0.46)	(0.82)	(0.28)	(0.35)	(0.26)	(0.30)	(3.73)	(7.04)
# Obs	264,562	169,451	264,562	169,451	184,559	113,853	228,216	144,235
Wald test	0.48	0.59	0.30	0.70	0.85	0.35	0.12	0.22
Profitable	1.22***	1.54**	-0.39*	-0.50	0.47*	0.06	-6.45**	-2.65
	(0.43)	(0.66)	(0.23)	(0.41)	(0.24)	(0.28)	(2.60)	(5.42)
# Obs	381,277	240,801	381,277	240,801	260,517	158,843	327,176	204,419
Not Profitable	1.07	-1.02	-0.11	0.37	0.02	0.36**	0.26	15.24*
	(1.21)	(1.73)	(0.32)	(0.54)	(0.25)	(0.22)	(10.69)	(10.58)
# Obs	251,727	162,955	251,727	162,955	164,073	99,859	49,710	34,470
Wald test	0.91	0.17	0.48	0.20	0.20	0.40	0.54	0.13

Notes: The coefficients show the point estimates $\hat{\beta}^h$ for $h = 1$ and $h = 3$ of regression (1) reflecting the responses of the dependent variables to positive treatment, that is events in which there was an increase in corporate taxes in municipality m in year t. *, **, and *** indicate statistical significance at p<0.1, p<0.05, and p<0.01, respectively. The estimation includes *district* × *year* and *industry* × *year* fixed effects, as well as additional firm- and municipality-level controls. In the specification restricted to firms that are non-profitable prior to treatment, lagged effective corporate tax rates are excluded as controls because these firms usually do not pay corporate taxes, rendering the variable undefined. Robust standard errors are clustered at the municipality level.

4.4 Relocation and exit

Our empirical framework can also be adapted to study extensive-margin outcomes such as firm exit, relocation, and the number of firms operating in a municipality. These outcomes require modifications relative to the baseline LP–DiD model. At the firm level, exit and relocation are binary events, so we estimate linear probability models with firm fixed effects. At the municipality level, we aggregate outcomes and estimate local projections in the spirit of equation (1), but with municipality-level data.

For firm exit, we estimate:

$$Dies_{i,t+1} = \mu_i + \alpha_{d,t} + \phi_{s,t} + \beta \Delta D_{m,t} + \delta \Delta D_{m,t} \times \#establishments_{i,t} + \varphi X_{i,t} + \varepsilon_{i,t}, \quad (3)$$

where $Dies_{i,t+1}$ equals one if firm i is no longer in the panel in period $t+1$.

For relocation, we estimate:

$$Relocates_{i,t} = \mu_i + \alpha_{d,t} + \phi_{s,t} + \beta \Delta D_{m,t} + \delta \Delta D_{m,t} \times \#establishments_{i,t} + \varphi X_{i,t} + \varepsilon_{i,t}, \quad (4)$$

where $Relocates_{i,t}$ equals one if firm i changes its main address to another municipality in year t. Both specifications include firm fixed effects and interactions with the number of establishments operated by a firm, as multi-establishment firms may find it easier to move headquarters.

Table 4 shows the results. Tax hikes raise the probability of firm exit by 2.2 percentage points—a non-trivial margin relative to an average annual exit rate of around 10 percent in the sample—suggesting that the liquidity and profitability pressures documented above push some firms out of operation entirely. At the same time, firms are 7.7 percentage points more likely to relocate to a lower-tax municipality—a substantial effect relative to an average annual relocation rate of just 1.2 percent—pointing to tax avoidance as a parallel margin of adjustment. The relocation response is particularly pronounced

Table 4: Probability of exit or relocation

	Exit (at *t+1*)	**Relocation**
Tax increase	0.0220**	0.0766***
	(0.0097)	(0.0247)
× number of establishments	0.0052	0.0175*
	(0.0051)	(0.0092)
Controls	Yes	Yes
FE	Yes	Yes
Observations	385,456	431,374

Notes: Estimates reflect the responses of the dependent variables to positive treatment, that is events in which there was an increase in corporate taxes in municipality m in year t. The dependent variables are dummy variables that equal one if the firm is no longer in the panel dataset in period $t+1$ or if the firm relocates its headquarters to another municipality in period t, respectively. *, **, and *** indicate statistical significance at p<0.1, p<0.05, and p<0.01, respectively. The estimation includes firm fixed effects, *district* × *year* and *industry* × *year* fixed effects, as well as additional municipality-level controls. Robust standard errors are clustered at the municipality level.

among multi-establishment firms, whose probability of moving rises by an additional 1.8 percentage points per establishment, reflecting the lower organizational cost of reassigning headquarters when the firm already operates across multiple locations.

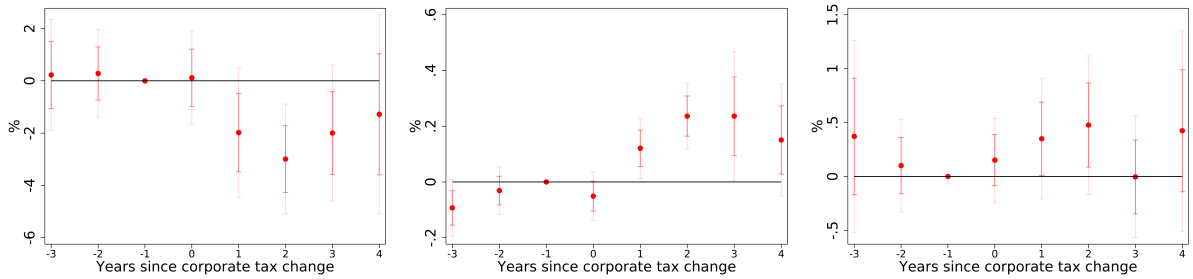
At the municipality level, we examine the aggregate effects on the number of firms and on neighboring areas. For this purpose, we estimate the same LP-DiD specification but at the municipality level:

$$\Delta_h y_{m,t+h} = \alpha^h_{d,t} + \beta^h \Delta D_{m,t} + \varphi^h X_{m,t} + \varepsilon_{m,t+h}, \tag{5}$$

where $y_{m,t}$ is measured at the municipality level. Outcomes include the number of firms operating in municipality m, the number of firms in neighboring municipalities (same district d, excluding municipality m), and neighboring municipalities' aggregate GVA.

Figure 8a shows that the number of firms in treated municipalities declines by 1.9 percent one year after the shock and remains around that value for the estimated horizon, equivalent to around 15 fewer firms per municipality. Neighboring municipalities see an

Figure 8: The effect of a corporate tax increase on firm relocation and production in neighboring municipalities



(a) No. firms – municipality (b) No. firms – neighbors (c) GVA – neighbors

Notes: The IRFs show the point estimates $\hat{\beta}^h$ of regression (5) reflecting the responses of the dependent variables to positive treatment, that is events in which there was an increase in corporate taxes in municipality m in year t. Point estimates for each horizon are presented in circles, together with 68% and 90% confidence bands (darker and lighter bands, respectively). The estimation includes *district* × *year* fixed effects, as well as additional municipality-level controls. Robust standard errors are clustered at the municipality level.

increase in the number of firms of around 0.2 percent at the peak (Figure 8b), consistent with relocation or reallocation of new firm births. However, Figure 8c shows only modest, mostly statistically insignificant, and short-lived increases in neighbors' aggregate GVA, suggesting that reallocation is somewhat inefficient: firms move to less productive areas or face productivity losses from relocation.

4.5 Taking stock

Our results reveal a consistent pattern. Financially, surtax increases reduce liquidity, raise leverage, and worsen credit quality, with higher interest rates and more nonperforming loans. Real activity contracts, with lower earnings, sales, employment, and productivity. Effects are concentrated among small and profitable firms, for which leverage is driven by liquidity squeezes. Firms are significantly more likely to exit and to relocate, especially if they already have more than one establishment. However, although relocation partly offsets local losses, it does not increase aggregate output.

Taken together, these findings confirm the introduction's emphasis: corporate surtax

increases transmit through both financial and real margins, with liquidity strains, rising non-performing loans, and the dual mechanisms behind leverage as central contributions.

5 Robustness

We complement our baseline results with a set of robustness checks. This section summarizes the main exercises and conclusions, while the full set of results is presented in Appendix C. Overall, all robustness checks confirm the validity of our empirical design. The main conclusions—that surtax hikes reduce liquidity, increase leverage and borrowing costs, worsen credit quality, and depress real outcomes—hold across specifications.

Size of clean-control window. Our baseline specification applies the clean-control restriction of Dube et al. (2025), excluding from the control group any firm located in a municipality that experienced a surtax change in the previous $L = 6$ years (equation (2)). This ensures that controls are not still adjusting to past treatments. Table C1 shows that results are virtually unchanged when varying L. Choosing $L = 8$ or $L = 12$ produces coefficients nearly identical to the baseline, and even $L = 4$ yields estimates very close to the main results. Thus, the findings are robust to alternative definitions of the clean-control window.

Zero-rate control group. In our baseline, control firms are drawn from municipalities that either never changed their surtax or had changes outside the L-year window. One concern is that such municipalities may still apply positive surtaxes. As a robustness check, we restrict the control group to municipalities that permanently set the surtax at zero. Results are similar to the baseline, with slightly larger magnitudes for leverage, liquidity, and earnings responses, and nearly identical effects for implicit interest rates

(see Table C1).

Dose of treatment. As an additional robustness check, we replace the binary treatment indicator with the continuous change in the surtax rate to exploit variation in the intensity of treatment across municipalities and over time. This specification allows us to assess whether firms subject to larger surtax increases experience proportionally larger effects. The results, reported in Table C1, are consistent with our baseline findings in both sign and statistical significance across all outcome variables. Importantly, the magnitudes are also broadly comparable to the baseline estimates. This is expected given that the average surtax increase in our sample is close to 1 percentage point—meaning that the continuous specification evaluated at the sample mean recovers an average treatment effect similar to that of the binary dummy.

Single-establishment firms. Multi-establishment firms may complicate identification, since they can operate across municipalities. Although the median firm in our dataset has only one establishment, we verify robustness by restricting the sample to single-establishment firms only. Table C1 shows that the results remain essentially unchanged.

Multiple treatments. As a further robustness check, we relax the single-treatment restriction imposed in the baseline and allow firms to experience multiple surtax increases over the sample period. The baseline restriction ensures a clean research design by focusing on firms with a single, well-defined treatment episode, but it comes at the cost of discarding observations from municipalities that enacted repeated surtax changes. When we include these observations, retaining the clean control window to ensure that control firms remain unexposed to recent tax changes, results are virtually unchanged (see Table C1). This suggests that the single-treatment restriction is not driving our findings, and that the estimated effects are representative of the broader population of firms exposed to surtax increases rather than an artifact of the particular subsample retained in

the baseline.

Placebo permutation test. As a further check on the validity of our identification strategy, we conduct a permutation test in which we randomly reassign the treatment indicator across municipality-year cells, respecting the share of treated observations in the data, and re-estimate the full model—including the construction of the clean control restrictions—on each of 2,000 permuted samples. The full permutation distributions are displayed in Figure C1 for different estimation horizons and variables. For horizons where the baseline estimates are statistically significant, the true coefficients lie well into the tails of the permutation distributions, confirming that these effects are unlikely to arise from random treatment assignment.

6 Conclusion

How do local corporate tax increases affect firms' financial conditions and real activity? We address this question using repeated changes in the corporate surtax independently set by Portuguese municipalities. The resulting variation across municipalities and over time, combined with local-projection difference-in-differences models that accommodate staggered and non-absorbing treatment, allows us to estimate firms' dynamic responses to tax increases. By linking the universe of corporate balance sheets and profit-and-loss statements to loan-level credit registry data, we observe financial and real adjustments within the same firms.

We find that a tax-increase event raises the municipal surtax rate by an average of 0.88 percentage points and the effective corporate tax rate by approximately 0.76 percentage points on impact. Firms' financial positions deteriorate immediately. Liquidity falls, net financial debt rises, and borrowing shifts toward shorter maturities. The implicit interest rate increases by 0.47 percentage points after one year, while the share of firms with non-performing loans is about 1.4 percentage points higher after four years. The response

therefore extends beyond a change in capital structure and includes both liquidity stress and a deterioration in credit quality.

These financial adjustments are followed by a contraction in real activity. The sales-to-assets ratio falls by 2.1 percentage points after two years, employment declines by around 2 percent, and total factor productivity falls persistently. A large literature shows that exogenous reductions in internal liquidity and bank credit reduce investment, employment, and economic activity (Rauh, 2006; Chodorow-Reich, 2014; Huber, 2018; Amiti and Weinstein, 2018). Our design does not separately identify this causal link. Nevertheless, the ordering of the responses and their concentration among small firms support the interpretation that financing pressures amplify the standard effect of lower post-tax returns. Small firms experience lower liquidity and higher leverage, whereas larger firms show no statistically significant financial response, suggesting that the increase in leverage reflects borrowing to cover cash shortfalls rather than a strategic response to the tax deductibility of interest. Total labor costs also decline by more than employment, implying lower labor costs per worker. This pattern is consistent with evidence that workers bear part of the corporate tax burden through lower wages (Fuest, Peichl and Siegloch, 2018).

Firms also adjust along the extensive margin. Surtax increases raise the probability of exit by 2.2 percentage points and the probability of relocation by 7.7 percentage points. Although the number of firms increases slightly in neighboring municipalities, their aggregate GVA does not. The decline in activity in treated municipalities therefore cannot be interpreted as a simple spatial redistribution that produces offsetting output gains elsewhere.

Our findings have implications for the design of corporate taxation in decentralized settings. Municipal tax changes affect not only firms' tax liabilities, but also their liquidity, borrowing costs, credit quality, employment, and productivity. Assessments of local corporate tax policy should therefore consider the possibility that financing constraints

reinforce the real effects of tax increases, particularly among small firms, alongside the revenue raised. Although the magnitude of the effects may depend on Portugal's institutional setting and its predominance of small, bank-dependent firms, the financial and real adjustment margins documented here are likely to be relevant in other systems where local governments set corporate taxes. The effects of nationwide tax changes may differ, however, because firms and production factors face different mobility constraints across countries than across municipalities.

References

Ackerberg, Daniel A, Kevin Caves, and Garth Frazer. 2015. "Identification properties of recent production function estimators." *Econometrica*, 83(6): 2411–2451.

Amberg, Niklas, Tor Jacobson, Erik Von Schedvin, and Robert Townsend. 2021. "Curbing shocks to corporate liquidity: The role of trade credit." *Journal of Political Economy*, 129(1): 182–242.

Amiti, Mary, and David E Weinstein. 2018. "How Much Do Idiosyncratic Bank Shocks Affect Investment? Evidence from Matched Bank-Firm Loan Data." *Journal of Political Economy*, 126(2): 525–587.

Banco de Portugal Microdata Research Laboratory - BPLIM. 2023. "Central Balance Sheet Harmonized Panel." *Banco de Portugal*, Version V1, extraction June 2023. DOI: 10.17900/CB.CBHP.Jun2023.V1.

Banco de Portugal Microdata Research Laboratory - BPLIM. 2024. "Harmonized Central Credit Responsibility Database." *Banco de Portugal - BPLIM*, Version V1, extraction June 2024. DOI: 10.17900/HCRC.Jun2024.V1.

Boissay, Frederic, and Reint Gropp. 2013. "Payment defaults and interfirm liquidity provision." *Review of Finance*, 17(6): 1853–1894.

Bonfim, Diana, Cláudia Custódio, and Clara Raposo. 2023. "Supporting small firms through recessions and recoveries." *Journal of Financial Economics*, 147(3): 658–688.

Bonfim, Diana, Miguel A. Ferreira, Francisco Queiró, and Sujiao (Emma) Zhao. 2024. "Fiscal policy and credit supply: The procurement channel." *American Economic Review (forthcoming)*.

Bournakis, Ioannis, and Sushanta Mallick. 2018. "TFP estimation at firm level: The fiscal aspect of productivity convergence in the UK." *Economic Modelling*, 70: 579–590.

Braz, Cláudia, Sónia Cabral, and Maria Manuel Campos. 2022. "A micro-level analysis of corporate income taxation in Portugal." *Banco de Portugal Revista de Estudos Económicos*, 8(1): 51–73.

Caglio, Cecilia R, R Matthew Darst, and Şebnem Kalemli-Özcan. 2021. "Collateral heterogeneity and monetary policy transmission: evidence from loans to SMEs and large firms." National Bureau of Economic Research.

Chodorow-Reich, Gabriel. 2014. "The Employment Effects of Credit Market Disruptions: Firm-Level Evidence from the 2008–9 Financial Crisis." *The Quarterly Journal of Economics*, 129(1): 1–59.

Cloyne, James, Ezgi Kurt, and Paolo Surico. 2025. "Who gains from corporate tax cuts?" *Journal of Monetary Economics*, 103722.

Drechsel, Thomas. 2023. "Earnings-based borrowing constraints and macroeconomic fluctuations." *American Economic Journal: Macroeconomics*, 15(2): 1–34.

Dube, Arindrajit, Daniele Girardi, Òscar Jordà, and Alan M Taylor. 2025. "A Local Projections Approach to Difference-in-Differences Event Studies." *Journal of Applied Econometrics*, forthcoming.

Faccio, Mara, and Stefano Manfredonia. 2026. "Taxes and Financial Distress: Evidence from Establishment-Level Data." *Available at SSRN 6623719*.

Fuest, Clemens, Andreas Peichl, and Sebastian Siegloch. 2018. "Do higher corporate taxes reduce wages? Micro evidence from Germany." *American Economic Review*, 108(2): 393–418.

Gabriel, Ricardo Duque. 2024. "The credit channel of public procurement." *Journal of Monetary Economics*, 103601.

Garrett, Daniel G, Eric Ohrn, and Juan Carlos Suárez Serrato. 2020. "Tax policy and local labor market behavior." *American Economic Review: Insights*, 2(1): 83–100.

Guceri, Irem, and Maciej Albinowski. 2021. "Investment responses to tax policy under uncertainty." *Journal of Financial Economics*, 141(3): 1147–1170.

Harju, Jarkko, Aliisa Koivisto, and Tuomas Matikka. 2022. "The effects of corporate taxes on small firms." *Journal of Public Economics*, 212: 104704.

Heider, Florian, and Alexander Ljungqvist. 2015. "As certain as debt and taxes: Estimating the tax sensitivity of leverage from state tax changes." *Journal of financial economics*, 118(3): 684–712.

House, Christopher L, and Matthew D Shapiro. 2008. "Temporary investment tax incentives: Theory with evidence from bonus depreciation." *American Economic Review*, 98(3): 737–768.

Huber, Kilian. 2018. "Disentangling the Effects of a Banking Crisis: Evidence from German Firms and Counties." *American Economic Review*, 108(3): 868–898.

Ivanov, Ivan T, Luke Pettit, and Toni M Whited. 2025. "Taxes depress corporate borrowing: Evidence from private firms." *Review of Economic Studies*, rdaf094.

Ivashina, Victoria, Luc Laeven, and Enrique Moral-Benito. 2022. "Loan types and the bank lending channel." *Journal of Monetary Economics*, 126: 171–187.

Jordà, Òscar. 2005. "Estimation and inference of impulse responses by local projections." *American economic review*, 95(1): 161–182.

Krapf, Matthias, and David Staubli. 2024. "Regional Variations in Corporate Tax Responsiveness: Evidence from Switzerland." *European economic review (forthcoming)*.

Levinsohn, James, and Amil Petrin. 2003. "Estimating production functions using inputs to control for unobservables." *The review of economic studies*, 70(2): 317–341.

Lian, Chen, and Yueran Ma. 2021. "Anatomy of corporate borrowing constraints." *The Quarterly Journal of Economics*, 136(1): 229–291.

Lichter, Andreas, Max Löffler, Ingo E Isphording, Thu-Van Nguyen, Felix Poege, and Sebastian Siegloch. 2025. "Profit taxation, R&D spending, and innovation." *American Economic Journal: Economic Policy*, 17(1): 432–463.

Link, Sebastian, Manuel Menkhoff, Andreas Peichl, and Paul Schüle. 2024. "Downward revision of investment decisions after corporate tax hikes." *American Economic Journal: Economic Policy*, 16(4): 194–222.

Liu, Yongzheng, and Jie Mao. 2019. "How do tax incentives affect investment and productivity? Firm-level evidence from China." *American Economic Journal: Economic Policy*, 11(3): 261–291.

Li, Xiang, and Dan Su. 2022. "Total factor productivity growth at the firm-level: The effects of capital account liberalization." *Journal of International Economics*, 139: 103676.

Lopes, Marta C., and Susana Peralta. 2018. "I like the way you move: tax competition in portuguese municipal corporate income tax." *Working Paper*.

Maffini, Giorgia, Jing Xing, and Michael P Devereux. 2019. "The impact of investment incentives: evidence from UK corporation tax returns." *American Economic Journal: Economic Policy*, 11(3): 361–389.

Melcangi, Davide. 2026. "The Marginal Propensity to Hire." *Review of Economic Studies*.

Moon, Terry S. 2022. "Capital gains taxes and real corporate investment: Evidence from Korea." *American Economic Review*, 112(8): 2669–2700.

Ohrn, Eric. 2018. "The effect of corporate taxation on investment and financial policy: Evidence from the DPAD." *American Economic Journal: Economic Policy*, 10(2): 272–301.

Olley, Steven, and Ariel Pakes. 1996. "The dynamics of productivity in the telecommunications equipment industry." *Econometrica*, 64(6): 1263–1297.

Rauh, Joshua D. 2006. "Investment and Financing Constraints: Evidence from the Funding of Corporate Pension Plans." *The Journal of Finance*, 61(1): 33–71.

Sanati, Ali, and Mehdi Beyhaghi. 2024. "How Does Removing the Tax Benefits of Debt Affect Firms? Evidence from the 2017 US Tax Reform." *Review of Financial Studies (forthcoming)*.

Sorensen, Peter Birch. 2017. "Taxation and the Optimal Financing of Firms." In *Handbook of Public Economics*. Vol. 5, , ed. Alan J. Auerbach, Raj Chetty, Martin Feldstein and Emmanuel Saez, 221–307. Elsevier.

Wooldridge, Jeffrey M. 2009. "On estimating firm-level production functions using proxy variables to control for unobservables." *Economics letters*, 104(3): 112–114.

Xu, Qiping, and Eric Zwick. 2024. "Tax policy and abnormal investment behavior." *The Review of Financial Studies*, 37(10): 2971–3023.

Zwick, Eric, and James Mahon. 2017. "Tax policy and heterogeneous investment behavior." *American Economic Review*, 107(1): 217–248.

Online Appendix

A Appendix for Section 2

A.1 Portuguese corporate tax system

This section briefly describes the Portuguese corporate income tax system. In terms of fiscal revenue, this tax is the third-largest source of revenue for the Portuguese government, behind VAT and personal income tax revenue. Corporate tax revenues are relatively small as a percentage of GDP, averaging 3% between 2000 and 2020 (Braz, Cabral and Campos, 2022).

Corporate taxes apply to any firm that is resident in Portugal or has a permanent establishment in the country, and the tax period coincides with the calendar year. Taxable income is calculated according to a set of rules established by the tax code. These rules first reconcile accounting profits or losses with taxable profits or losses and include deductions for capital depreciation and interest expenditure.[18] Taxable profits are then adjusted for losses from previous years, which could be carried forward for a period of five years from 2017 onward—except for small and medium-sized firms, which could carry losses forward for a period of 12 years. Before 2017, all firms could carry losses forward for a period of 12 years. After the tax rates are applied to taxable income, tax incentives and deductions, such as R&D investment incentives, are also applied. Another component of the tax system, known as autonomous taxation, is levied on specific expenses considered unrelated to firms' activities to prevent possible tax evasion schemes. The total tax liability is obtained only after these adjustments.

Table A1 shows the corporate tax rates levied in Portugal in 2021 (the final year of our sample). The system also includes tax brackets that introduce progressivity in

[18] Since 2014, interest expenditure has been deductible up to €1,000,000 or 30% of EBITDA.

addition to the general rate. In addition to the general national rate and national surtaxes, municipalities can charge a surtax between 0% and 1.5%, which we use for identification and to make causal claims in the body of the paper.

We should note that, despite some changes in tax rates and the tax code during our sample period, average effective tax rates have been relatively stable over time, except during the 2012–2013 period (Braz, Cabral and Campos, 2022).

Table A1: Corporate tax rates in Portugal

	Brackets	**Rates**
Statutory standard rates		21%
	For SMEs < €25K	17%
National surtaxes	€1.5 - €7.5 million	3%
	€7.5 - €35 million	5%
	> €35 million	7%
Municipality surtax		0% - 1.5%

Notes: For small and medium-sized firms (SMEs), the first €25,000 of taxable income is taxed at a 17% rate. This reduced rate was introduced in 2014 and had a threshold of €15,000 between 2016 and 2019. Taxable income exceeding this threshold is taxed at the general rate of 21%. The rate for each national surtax bracket is levied only on taxable income that exceeds the preceding bracket.

A.2 Descriptive statistics at the municipality level

This section presents descriptive statistics for the municipalities. As mentioned in the paper, there are 278 municipalities in mainland Portugal. Municipalities are grouped into 18 districts. Table A2 reports these municipal statistics.

As Table A2 shows, restricting the sample to municipalities that either changed the corporate surtax rate once or never changed it does not alter the main statistics of the included municipalities relative to the full sample. All the statistics for one group lie within the confidence intervals for the other group.

Table A2: Municipality descriptive statistics

	All			**Restricted sample**		
	Mean	**Std. Dev.**	**Median**	**Mean**	**Std. Dev.**	**Median**
GVA (€M)	305	1,343	66	351	1,576	71
Total population	35,646	58,355	14,865	38,384	61,013	15,719
Number of firms	706	1,557	250	777	1,773	256
Surtax rate	0.89	0.68	1.20	0.82	0.71	1.20
Surtax revenue (% GVA)	0.22	0.29	0.21	0.20	0.23	0.18

Notes: "All" indicates the descriptive statistics for all municipalities before the sample is restricted. "Restricted sample" shows the same descriptive statistics after we restrict the sample to municipalities that either never changed their surtax or experienced only one surtax change. Total population and surtax revenue are available only from 2011 onward. There are 278 municipalities in mainland Portugal and 30 on the Portuguese islands, which we do not include in the analysis.

Most surtax changes are concentrated at the extremes, namely changes from a surtax of 0% to 1.5% or vice versa, in both the full and restricted samples. Focusing on positive changes only, the mean corporate surtax change among municipalities that experienced only one change is 0.88 percentage points. This means that the average tax increase associated with a treatment event is sizable, even though the municipal surtax is limited to a maximum of 1.5%, as mentioned above. Overall, 12,182 firms are treated over the period considered in our restricted sample.

A.3 Descriptive statistics—firms' balance-sheet data

This section presents firm-level descriptive statistics for the full and restricted samples separately. Table A3 reports these values.

As Table A3 shows, restricting the sample to municipalities that either changed the corporate surtax rate once or never changed it does not alter the main statistics of the firms included in the exercises. All the statistics for one group lie within the confidence intervals for the other group.

Table A3: Firm balance-sheet and credit descriptive statistics

	All			Restricted sample		
	Mean	**Std. Dev.**	**Median**	**Mean**	**Std. Dev.**	**Median**
Balance sheet						
Total assets (€K)	935.00	2779.46	180.05	899.91	2733.42	169.00
Leverage	9.86	55.72	0.78	10.33	57.14	0.52
Liquidity	19.09	22.42	9.86	19.14	22.64	9.73
Tangibility	24.66	25.89	15.21	24.39	25.99	14.67
ROA	-3.22	53.99	5.94	-4.65	56.77	5.64
Productivity						
TFP growth	-0.17	63.62	0.76	-0.53	65.21	0.59
Labor						
Employees	12.17	117.29	4.00	11.70	119.89	4.00
Taxes						
ETR	24.90	18.02	21.29	24.87	18.13	21.21
Credit						
Total (€K)	391.28	4234.81	30.99	391.76	4383.87	29.60
Non-performing (€K)	15.81	832.50	0.00	16.60	787.47	0.00
Observations (firm-year)		2,552,447			2,086,873	

Notes: "All" indicates the descriptive statistics for all firms before the sample is restricted. "Restricted sample" shows the same descriptive statistics after we restrict the sample to firms that are either never treated or treated only once. Balance-sheet data are winsorized at 1% and 99%. Effective tax rates (ETRs) are restricted to values between 0% and 100%.

The dataset also includes a variable reporting annual corporate tax payments to the

government, which allows us to calculate the effective corporate tax rate (ETR) paid by each firm in each year. Although the Portuguese corporate tax system has some features that make this measure imperfect—for example, we do not perfectly observe the tax base as defined by the tax code—it serves as a reasonably good proxy for the ETR paid. In particular, we define the ETR as corporate taxes paid divided by earnings before taxes and restrict the estimates to values between 0% and 100%. For our sample, the mean and median effective tax rates are fairly similar across the two groups and in line with those reported by Braz, Cabral and Campos (2022) for Portuguese firms.

Another important variable used throughout our analysis is the implicit interest rate paid by firms. Our loan-level data do not contain interest rate information; therefore, we cannot directly estimate the impact of corporate taxation on loan-level interest rates. However, our balance-sheet dataset contains data on the interest outlays reported by each firm in a given year. Implicit interest rates are then measured by the ratio of these interest outlays to lagged financial debt. Across firms in our sample, the average implicit interest rate is 4.29%. For treated firms in the pre-treatment year, the average rate is 4.82%. For comparison, the average monthly interest rate on new loans below €1,000,000 to non-financial firms reported by the Bank of Portugal for our sample period is 4.44%.

A.4 Firm-level TFP estimates

In our baseline exercises, we follow Wooldridge (2009) and use parametric estimates of firm-level total factor productivity (TFP).[19] The methodology assumes the following Cobb–Douglas production function:

$$y_{i,t} = \alpha + \beta_l l_{i,t} + \beta_k k_{i,t} + \omega_{i,t} + e_{i,t}, t = 1, \ldots, T, \tag{6}$$

for firm i in period t. TFP is given by the unobserved productivity term that we seek to estimate—$\omega_{i,t}$—and $e_{i,t}$ is an idiosyncratic shock with a conditional mean independent of current and past inputs. $y_{i,t}$ represents firm i's output (sales plus the increase in inventories), $k_{i,t}$ represents capital (fixed tangible and intangible assets, plus inventories and biological assets), and $l_{i,t}$ represents labor input (the number of employees). All variables are included in logs. $\omega_{i,t}$ is assumed to follow a first-order Markov process:

$$\omega_{i,t} = E\left(\omega_{i,t} \mid \omega_{i,t-1}\right) + \epsilon_{it} = g\left(\omega_{i,t-1}\right) + \epsilon_{i,t} \tag{7}$$

where $\epsilon_{i,t}$ is a productivity shock, uncorrelated with $\omega_{i,t}$.

Estimating regression (6) by OLS leads to biased estimates because of simultaneity issues (firms may choose inputs in response to productivity shocks) and selection bias (firms that suffered productivity shocks may no longer be in the sample). Therefore, we use the system generalized method of moments (GMM-SYS) to estimate (6). The GMM-SYS estimator assumes that lagged differences of inputs are uncorrelated with $\epsilon_{i,t}$, allowing regression (6) to be estimated directly.[20] Variables are deflated using two-digit sector-specific deflators, and factor shares (β_l and β_k) are estimated at the sector level.

[19] Three other methodologies are commonly used in the literature to estimate firm-level TFP, all of which are semiparametric approaches: Olley and Pakes (1996), Levinsohn and Petrin (2003), and Ackerberg, Caves and Frazer (2015). For a detailed discussion of the different methods, see, for example, Bournakis and Mallick (2018) or Li and Su (2022).

[20] Besides capital and labor, contemporaneous and lagged values of intermediate inputs (variable costs, including external services) are also used as instruments in the estimation.

Figure A1 plots the median evolution of firm-level TFP and TFP growth rates over the sample years. We also computed firm-level TFP following the approach of Ackerberg, Caves and Frazer (2015), and the results did not change significantly.

Figure A1: Median TFP and TFP growth rate



(a) Median of TFP level

(b) Median of TFP growth

Notes: The figures plot the median values, by year, of firm-level TFP and TFP growth, measured following Wooldridge (2009).

The average and median TFP growth rates in the pre-treatment year for treated firms are slightly lower than the corresponding values for the never-treated group, which might reflect the fact that surtax increases are slightly more concentrated in the first years of the sample—given the global financial crisis, sovereign debt crisis, and subsequent austerity measures implemented in Portugal. However, given the considerable standard deviations in both groups, these values are not statistically different from each other.

B Appendix for Section 3

B.1 Effective tax rate increase following the tax change

This section confirms that our treatment induces an increase in the average effective tax rate paid by firms. We estimate the following regression:

$$ETR_{i,t} = \mu_i + \alpha_{d,t} + \phi_{s,t} + \beta \Delta D_{m,t} + \varphi X_{m,t} + \varepsilon_{i,t}, \tag{8}$$

where, relative to the baseline specifications, we include a firm fixed effect, and the controls $X_{m,t}$ include only contemporaneous municipal GVA and three of its lags. The coefficient of interest is presented in Table B1.

We should note that it is not obvious that such a policy change leads to a permanent increase in effective tax rates, given all the previously described adjustments made by firms. These dynamic adjustments can, in fact, lead to lower effective tax rates. Nevertheless, the effective tax rate rises on impact: on average, a firm operating in a municipality that increases the surtax rate experiences an increase of 0.76 percentage points in its effective tax rate.

Table B1: Effective tax rate response to surtax increase

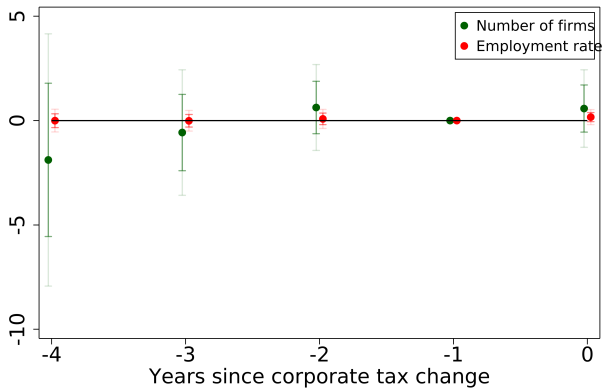
	Effective tax rate
Tax increase	0.7618**
	(0.3127)
Controls	Yes
FE	Yes
Observations	602,906

Notes: Estimates reflect the responses of the dependent variables to positive treatment, that is events in which there was an increase in corporate taxes in municipality m in year t. *, **, and *** indicate statistical significance at p<0.1, p<0.05, and p<0.01, respectively. The estimation includes firm fixed effects, *district* × *year* and *industry* × *year* fixed effects, as well as additional municipality-level controls. Robust standard errors are clustered at the municipality level.

B.2 Pre-trends in other local economic conditions

To complement the evidence in Table 2, we examine whether treated and control municipalities were on differential trends in local economic conditions prior to surtax increases. Rather than asking whether local conditions predict surtax changes, we approach the same concern from the opposite direction: we estimate our baseline event-study specification using the municipal employment rate and the number of active firms as dependent variables, and test whether pre-reform coefficients are significant. Figure B1 shows the results. The pre-reform coefficients are statistically indistinguishable from zero for both outcomes, indicating no differential pre-trends between treated and control municipalities. Taken together with the GVA evidence in the main text, these results provide reassurance that surtax increases are neither predicted by observable local conditions nor preceded by diverging economic trajectories, supporting the identifying assumption underlying our empirical design.

Figure B1: Pre-trends in the number of firms and employment rate at the municipality level



Notes: The IRFs show the point estimates $\hat{\beta}^h$ of regression (5) reflecting the responses of the dependent variables (the total number of firms and the municipal employment rate) to positive treatment, that is events in which there was an increase in corporate taxes in municipality m in year t. Point estimates for each horizon are presented in circles, together with 68% and 90% confidence bands (darker and lighter bands, respectively). The estimation includes *district* × *year* fixed effects, as well as additional municipality-level controls. Robust standard errors are clustered at the municipality level. Employment rates are defined as the share of non-self-employed workers in the total local population.

C Appendix for Section 5

Table C1 shows the results of most of the robustness exercises described in Section 5 of the paper.

Table C1: Robustness extensions

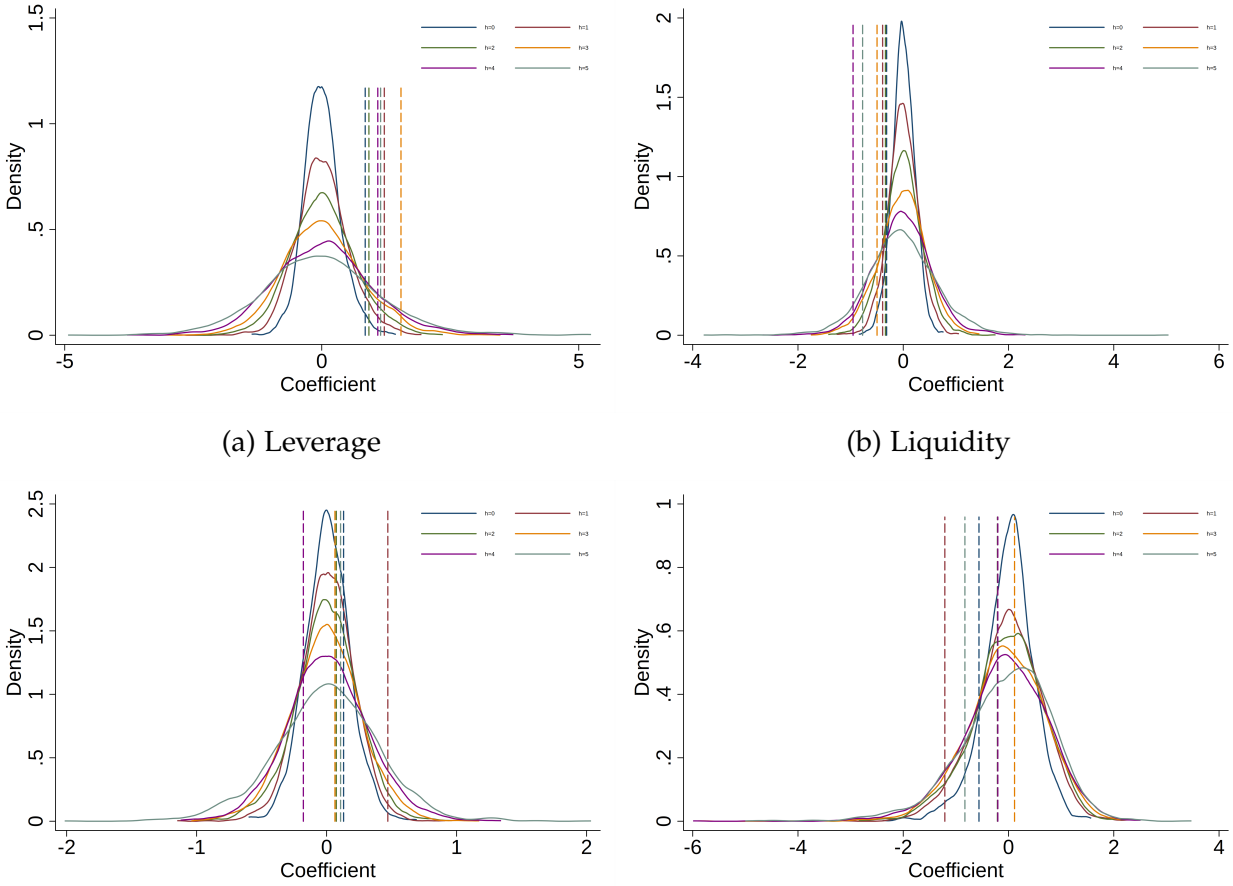
	Leverage		Liquidity		Int. Rate		EBT	
	h=1	**h=3**	**h=1**	**h=3**	**h=1**	**h=3**	**h=1**	**h=3**
Clean control size								
L=4	1.21***	1.52**	-0.41*	-0.49	0.45*	0.06	-1.20***	0.12
	(0.43)	(0.66)	(0.23)	(0.41)	(0.24)	(0.28)	(0.43)	(0.64)
#Obs	391,673	247,786	391,673	247,786	267,995	163,672	391,673	247,786
L=8	1.20***	1.50**	-0.38*	-0.48	0.47*	0.09	-1.20***	0.15
	(0.44)	(0.66)	(0.23)	(0.42)	(0.24)	(0.28)	(0.44)	(0.65)
#Obs	373,673	238,857	373,673	238,857	255,147	157,400	373,673	238,857
L=12	1.20***	1.50**	-0.38*	-0.48	0.47*	0.09	-1.19***	0.15
	(0.43)	(0.66)	(0.23)	(0.42)	(0.24)	(0.28)	(0.44)	(0.65)
#Obs	371,607	238,857	371,607	238,857	253,560	157,400	371,607	238,857
Zero rate control group	1.65***	2.17***	-0.43	-0.77**	0.55**	-0.01	-1.75***	-0.67
	(0.46)	(0.69)	(0.27)	(0.34)	(0.27)	(0.29)	(0.44)	(0.61)
#Obs	110,345	61,465	110,345	61,465	77,200	41,472	110,345	61,465
Dose of treatment	0.82*	0.45	-0.23	-0.24	0.27	-0.25	-0.93**	0.52
	(0.53)	(0.62)	(0.29)	(0.35)	(0.30)	(0.32)	(0.55)	(0.67)
#Obs	381,277	240,801	381,277	240,801	260,517	158,843	381,27	240,801
#1 establishment only	1.22***	1.51***	-0.37	-0.50	0.45***	0.06	-1.22***	0.19
	(0.43)	(0.72)	(0.25)	(0.43)	(0.26)	(0.31)	(0.46)	(0.68)
#Obs	359,454	226,044	359,454	226,044	243,492	147,596	359,454	226,044
Multiple treatments	0.83***	1.22***	-0.12	-0.48	0.40***	0.07	-0.89***	0.21
	(0.42)	(0.58)	(0.27)	(0.36)	(0.22)	(0.26)	(0.31)	(0.44)
#Obs	429,104	265,577	429,104	265,577	295,109	176,362	429,104	265,577

Notes: The coefficients show the point estimates $\hat{\beta}^h$ for $h = 1$ and $h = 3$ of regression (1) reflecting the responses of the dependent variables to positive treatment, that is events in which there was an increase in corporate taxes in municipality m in year t. *, **, and *** indicate statistical significance at p<0.1, p<0.05, and p<0.01, respectively. The estimation includes *district* × *year* and *industry* × *year* fixed effects, as well as additional firm- and municipality-level controls. Robust standard errors are clustered at the municipality level.

Figure C1 presents the results of the randomization inference exercise described in

Section 5. Each panel plots the kernel density of the estimated coefficients across 2,000 permutations of the treatment indicator, separately for each horizon, alongside vertical dashed lines indicating the true horizon-specific estimated coefficients. Under the null hypothesis of no effect, the permutation distribution should be centered around zero, and the true estimates should lie in the tails of the distribution. This is indeed the case for the horizons where the baseline estimates are statistically significant—the corresponding dashed lines fall clearly in the tails of the permutation distributions. For horizons where the baseline estimate is not statistically significant, the true coefficient lies closer to the center of the distribution, which is consistent with the absence of a detectable effect at those horizons.

Figure C1: Placebo permutation distributions



(a) Leverage

(b) Liquidity

(c) Interest rate

(d) EBT

Notes: Each distribution is based on 2,000 random permutations of the treatment indicator, assigned at the municipality-year level while preserving the share of treated observations in the data. For each permutation, the clean-control restrictions are reconstructed from the permuted treatment series using a window of $L = 6$ years, and the baseline specification is re-estimated at each horizon. Dashed vertical lines indicate the true estimated coefficient at each horizon.

Acknowledgements

The authors thank Diana Bonfim, João Pereira dos Santos, Katarzyna Bilicka, Miguel Oliveira, Ricardo Duque Gabriel, and seminar participants at Nova SBE, Banco de Portugal, Banco Central do Brasil, the Lubrafin 2025 meeting, and the Portuguese Economic Journal 2025 conference for their insightful comments and suggestions. Duarte gratefully acknowledges funding from Fundação para a Ciência e a Tecnologia (UID/00124/2025, UID/PRR/124/2025, Nova School of Business and Economics) and LISBOA2030 (DataLab2030 - LISBOA2030-FEDER-01314200). Moura gratefully acknowledges funding from Fundação para a Ciência e a Tecnologia (PD/BD/150429/2019).
The views expressed in this paper are those of the authors and do not necessarily reflect those of the European Central Bank, the Banco de Portugal, or the Eurosystem.

João B. Duarte
Nova School of Business and Economics, Carcavelos, Portugal; email: joao.duarte@novasbe.pt

Afonso S. Moura
European Central Bank, Frankfurt am Main, Germany; Banco de Portugal, Lisbon, Portugal; email: afonso.smoura@ecb.europa.eu

© European Central Bank, 2026

Postal address 60640 Frankfurt am Main, Germany
Telephone +49 69 1344 0
Website www.ecb.europa.eu

All rights reserved. Any reproduction, publication and reprint in the form of a different publication, whether printed or produced electronically, in whole or in part, is permitted only with the explicit written authorisation of the ECB or the authors.

This paper can be downloaded without charge from www.ecb.europa.eu, from the Social Science Research Network electronic library or from RePEc: Research Papers in Economics. Information on all of the papers published in the ECB Working Paper Series can be found on the ECB's website.

PDF ISBN 978-92-899-8070-8 ISSN 1725-2806 doi:10.2866/0958231 QB-01-26-253-EN-N