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The Real and Financial Effects of Local Corporate Tax Increases: Evidence from Linked Firm–Bank Data

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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, inputs, 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, inputs, and employment and experience persistent declines in total factor productivity. Effects are strongest among small and young firms, where leverage rises in response to liquidity losses, while larger firms increase leverage in line with tax-shield incentives. 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.

JEL: E62; G38; H25; H32; H71

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

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1. Introduction

How do local corporate tax increases shape firms' access to financing and their productive performance? While corporate tax policy is a central tool of fiscal governance, credible causal evidence remains limited. National-level tax reforms often coincide with macroeconomic shifts, complicating inference. We address this challenge by exploiting a unique setting: Portuguese municipalities independently adjust a corporate surtax ("*derrama municipal*") on top of the national rate, creating staggered, plausibly exogenous variation in tax burdens.

We combine two administrative datasets of exceptional scope. First, balance sheet and profit-and-loss statements from the universe of Portuguese firms, filed annually with tax authorities, provide complete coverage of financial statements across the corporate sector. These data allow us to measure revenues, costs, employment, and investment with precision. Second, we link these records to the Bank of Portugal's Central Credit Registry, which tracks all bank loans to non-financial firms at monthly frequency, including exposures, collateral, and loan performance. This dataset enables us to characterize firms' financial conditions in detail, from leverage and liquidity to borrowing costs and non-performing loans.

Together, these sources provide a rare opportunity to examine how corporate tax shocks propagate across both financial and real margins of firm adjustment. Unlike most existing studies, which focus either on financing decisions or on investment and output, our analysis captures the full adjustment path: how tax increases alter firms' balance sheets and credit access, and how these financial strains translate into changes in sales, employment, and productivity.

Our empirical strategy employs local-projection difference-in-differences (LP-DiD) models (Dube *et al.*, 2025), which address timing heterogeneity and dynamic responses in staggered treatment settings. This design enables us to trace out the causal effects of surtax increases while ensuring credible comparisons across firms in treated and untreated municipalities. Moreover, the setting offers multiple tax rate changes from which to draw our conclusions, ensuring that the results do not hinge on a single episode—as is often the case in the literature that relies on single event-study designs.

The analysis yields three main results. First, local surtax increases impose immediate financing strain. Firms face higher implicit borrowing costs, curtail cash and fixed-asset holdings, and rely more heavily on debt. Liquidity positions weaken, both through lower cash holdings and by passing liquidity stress along the supply chain via trade credit. At the same time, we observe a marked rise in non-performing loans, indicating that higher taxes exacerbate financial fragility.

Second, these financial pressures spill over to real activity: firms cut back on sales, employment, and material inputs, and experience persistent reductions in total factor productivity. Third, effects are concentrated among small and young firms, who bear liquidity losses and higher funding costs, while larger, more established firms accommodate surtax shocks by increasing leverage. This divergence highlights two mechanisms behind rising leverage: among smaller and

younger firms, borrowing responds to liquidity squeezes, whereas among larger firms leverage reflects strategic tax-shield behavior.

As an extension, we also examine exit and relocation. Surtax hikes increase the probability of firm exit and relocation, yielding localized allocation shifts that do not raise aggregate output, suggesting inefficient reallocation.

These findings contribute to three strands of literature. They provide causal evidence on the corporate tax–financing–productivity channel, document real-economy consequences of local tax policy changes, and highlight firm-level heterogeneity and spatial reallocation dynamics. The results generalize to other fiscal-federal settings—such as Germany and Switzerland—where municipalities exercise tax-setting autonomy.

Our study relates most closely to recent work exploiting local variation in corporate taxation. For Germany, Fuest *et al.* (2018) show that higher local business taxes reduce wages, while Link *et al.* (2024) document downward revisions of investment plans and Lichter *et al.* (2025) analyze R&D spending responses.[1] These studies emphasize adjustment margins at the firm and worker levels but do not account for financing conditions or productivity effects. Moreover, the latter two rely on survey data to assess the impact of corporate taxation on their variables of interest, whereas the first draws on employee-level information at the establishment level. In contrast, our administrative datasets provide richer detail and broader coverage—spanning sectoral dimensions (e.g., Link *et al.* (2024) focus exclusively on manufacturing), financial variables, and heterogeneity across firms.

The paper also connects to research on capital structure and taxation. 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. In parallel, work on tax reforms that lower effective rates finds effects on investment and activity: for example, Zwick and Mahon (2017), Liu and Mao (2019), Maffini *et al.* (2019) and Moon (2022) show that investment and sales respond strongly when depreciation rules or incentives change. We provide additional evidence along these lines, employing a different identification strategy.

Our contributions are threefold. First, we unify perspectives that usually study either financing or real activity in isolation, by showing how tax hikes propagate through financing frictions into real outcomes, using administrative data that simultaneously capture balance sheets, loan conditions, and productivity. Second, we open up novel dimensions of analysis: we document how taxation affects firms' liquidity positions, trade credit adjustments, and the incidence of non-performing loans. These outcomes are rarely observed in prior work, yet they are central for understanding how fiscal policy interacts with financial stability. Third, we shed light on the mechanisms behind firms' rising leverage. For smaller and younger firms, leverage increases are closely tied to liquidity losses, consistent with borrowing to

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

offset cash shortfalls. For larger firms, by contrast, leverage rises without liquidity declines, suggesting strategic use of debt as a tax shield, in line with the literature on debt bias in corporate taxation. Overall, our evidence underscores complex and interconnected adjustments through both financial and real margins, with significant implications for fiscal design under decentralized settings.

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

This section describes the institutional setting and the firm-level data used in the analysis. We begin with the Portuguese corporate tax system and highlight the source of exogenous variation that underpins our empirical design: surtaxes set by municipalities. We then describe the administrative datasets on firm balance sheets and credit, outline our sample restrictions, and present descriptive statistics.

2.1. Corporate taxation and institutional setup

In Portugal, corporate taxation combines a national statutory code with a municipal surtax (*derrama municipal*).[2] The national code 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. This surtax provides sharp cross-sectional and temporal variation in effective tax rates.

Our identification strategy rests on the assumption that municipal surtax changes are exogenous from the perspective of individual firms. This assumption is plausible for two reasons. First, the vast majority of Portuguese firms are small and unlikely to exert lobbying power over local governments. Second, Lopes and Peralta (2018) show that surtax changes are mainly politically, not economically, driven. Additionally, our baseline analysis focuses on tax increases, which are unlikely to be driven by firm lobbying, further supporting the case for exogeneity.[3]

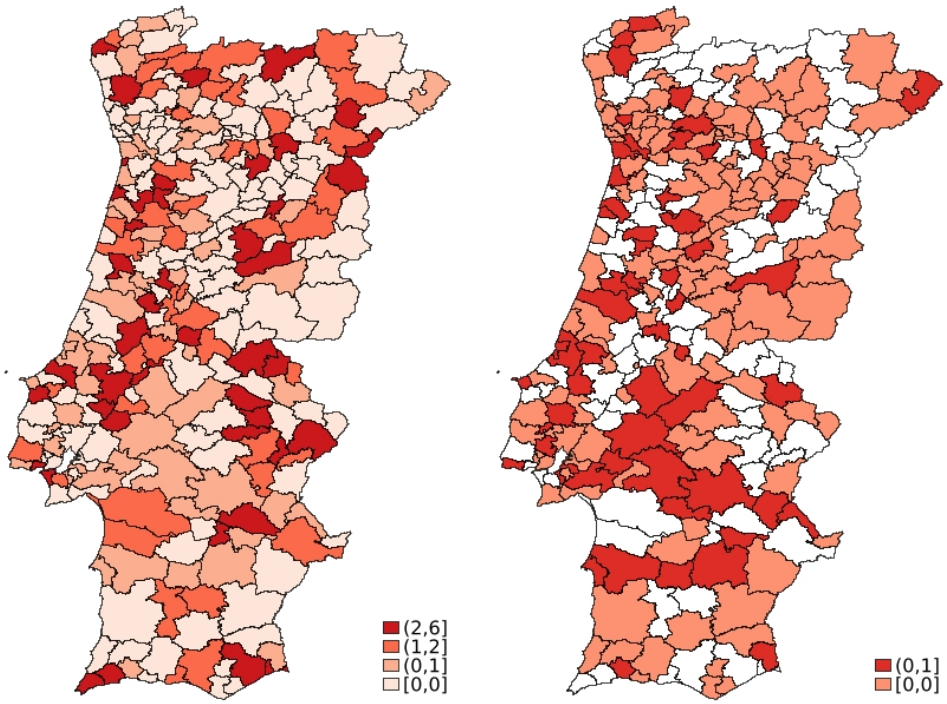
Between 2009 and 2021, the average municipal surtax was 0.89 percent, with a median of 1.2 percent. Of the 278 municipalities, 129 never changed their rate, and 68 kept it permanently at zero. Revenues from the surtax account for only 0.2 percent of local GVA, so changes are unlikely to be accompanied by major shifts in local fiscal policy.[4] For our baseline analysis, we restrict the sample to

2. We direct the reader to appendix A.1 for a detailed overview of the national tax system.

3. The focus on tax increases alone if further motivated by the concentration of surtax decreases in the Covid-period, aimed at helping firms cope with the pandemic (see Figure 2).

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

municipalities that either never changed their surtax or changed it only once during the sample. This 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.



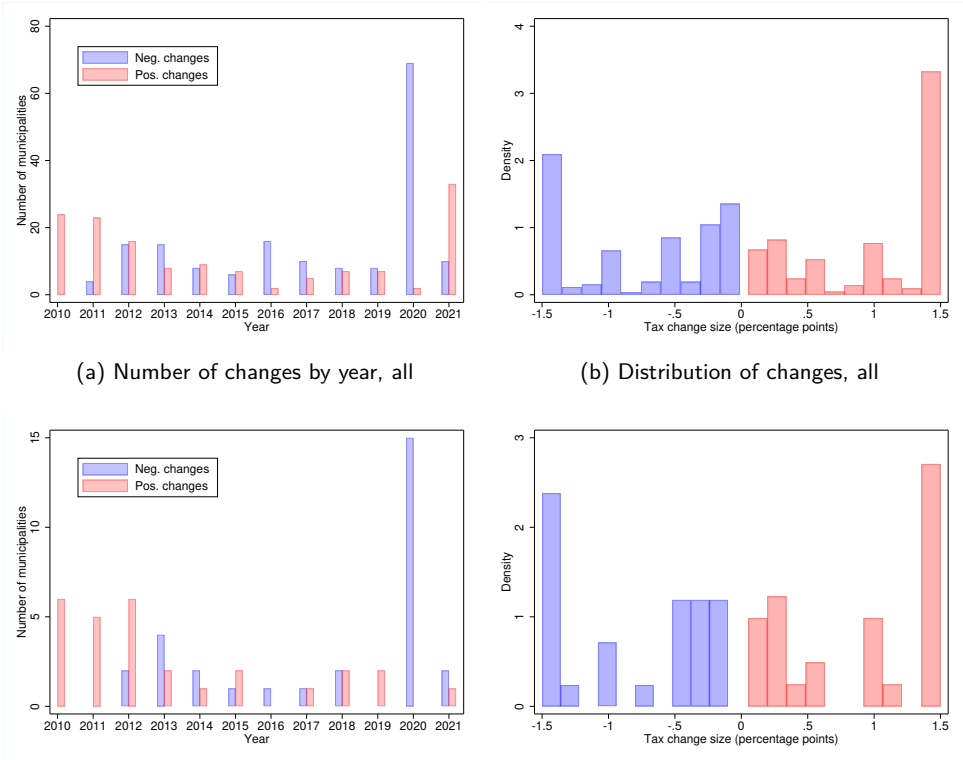
(a) All municipalities (b) Restricted sample

Figure 1: Number of corporate surtax changes by municipality

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 include only municipalities that implemented either no changes or only one change in their municipality corporate surtax rate (municipalities in white experienced more than one surtax change, and are therefore dropped from the sample).

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 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



(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

Figure 2: Temporal and size distributions of corporate surtax changes

Notes: This panel shows the time series of the number of municipalities implementing a surtax change and the histogram of such surtax changes. In the upper row of the panel, Figure 2a shows the number of municipalities changing the corporate surtax throughout the years, while Figure 2b shows the size of such changes, both for the overall sample. On the second row, we show the same graphs but for the restricted sample (see the text for more details on the restricted sample): Figure 2c shows the number of municipalities changing the surtax rate by year, while Figure 2d shows the distribution of the size of such changes. In all sub-figures, increases in the surtax are highlighted in red, while decreases are highlighted in blue.

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.[5]

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 Azores) are excluded since 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 in the year before a surtax increase. The two groups are similar across balance sheet, labor, tax, and credit measures, supporting the validity of our identification strategy.

	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

Table 1. Firm balance sheet and credit descriptive statistics

Notes: The sample consists of 2,086,873 firm-year observations between 2009 and 2021. Never treated are firms that operate in municipalities that during our sample never changed their corporate surtax. Pre-treatment columns are averages across firms on the period prior to being treated. Balance sheet data is winsorized at 1% and 99%. Effective tax rates (EFT) are restricted to be within 0% and 100%. Monetary variables are expressed in real terms.

5. See, for example, Bonfim *et al.* (2023), Gabriel (2024), and Bonfim *et al.* (2024) for other applications of these data.

Finally, we estimate firm-level total factor productivity (TFP) following Wooldridge (2009), assuming a Cobb–Douglas production function with sector-specific factor shares.[6] Results are robust to alternative estimators such as Levinsohn and Petrin (2003) and Ackerberg *et al.* (2015). On average, pre-treatment productivity growth is similar across treated and untreated firms, consistent with the identifying assumption of parallel trends.

3. Empirical Methodology

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 $\hat{\beta}^h$:

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

where $y_{i,t}$ is the outcome for firm i in year t and $X_{i,t}$ includes contemporaneous and lagged controls at the municipality and firm level.[7] The baseline specification controls for (i) log municipality-level gross value added (GVA), capturing local business cycles, (ii) firm-level effective tax rates, to account for firms in different brackets or enjoying tax incentives, and (iii) a lag of the dependent variable $y_{i,t-1}$. We also include $district \times year$ ($\alpha^h_{d,t}$) and $sector \times year$ ($\varphi^h_{s,t}$) fixed effects.[8] The operator Δ_h denotes long-differencing, $\Delta_h y_{i,t+h} = y_{i,t+h} - y_{i,t-1}$, which both cumulates IRFs over time and nets out firm fixed effects.

The key identification assumption is that treated and control firms follow parallel trends prior to treatment. To test this and check for anticipation, we include in the estimated IRFs the horizons $h = -3$ and $h = -2$.

A challenge in staggered-treatment settings is 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) controls must exclude firms that are still responding to past changes. Formally, we impose:

6. We detail the estimation procedure in appendix A.4.

7. 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.

8. Districts are a higher administrative level than municipalities; mainland Portugal has 18 districts, each containing on average 15 municipalities.

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

In practice, treated units are firms experiencing exactly one surtax increase during the sample. Controls are firms never treated or treated only in the distant past. In the baseline, we set $L = 6$, excluding from controls any firm in a municipality with a surtax change in the prior 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] Municipalities that lower their surtax are also excluded from the control group for the relevant L-year window.

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 start with balance-sheet responses. Figure 3 shows impulse responses for leverage and liquidity ratios, while Figure 4 decomposes the underlying drivers.

Leverage increases gradually following a tax increase. The impact response is about 0.46 percentage points and reaches 1.8 percentage points after four 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 4.2 percent on impact ($\approx$ €6,200 for the mean firm in our sample)[12], and remains elevated over the horizon (Figure 4b). Fixed assets fall more gradually, with a decline of 9.5 percent after four years (Figure 4a).

9. Results are robust to other windows, including $L = 12$, in which case controls consist only of never-treated firms (see Appendix C).

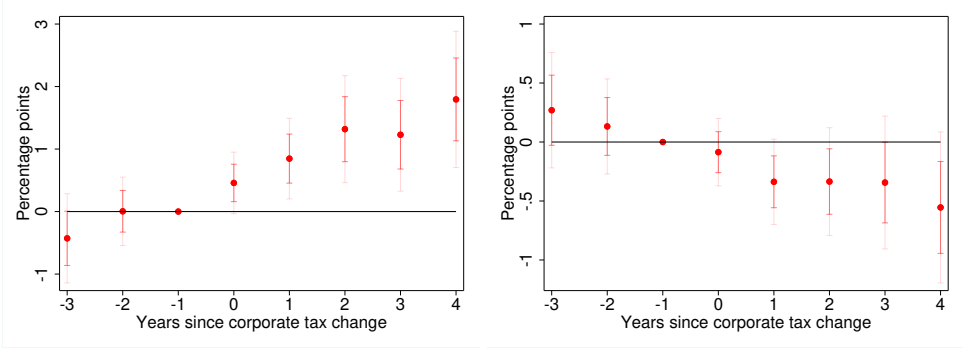
10. As noted by Dube *et al.* (2025), conditioning on future treatment can bias estimates if selection is endogenous. In our case, surtax changes are plausibly exogenous at the firm level.

11. Importantly, section B.1 of the appendix shows that, on average, a firm experiencing treatment observes and increase, at impact, of its effective tax rate, of around 0.84 percentage points.

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

Liquidity declines in parallel. The liquidity ratio falls by 0.3 percentage points one year after the shock and by 0.55 points after four years (Figure 3b). Cash and bank deposits fall sharply, by 4.3 percent on impact and 7.4 percent after four years (Figure 4c). These patterns are consistent with tax hikes tightening firms' liquidity positions, as highlighted in the Introduction.

Trade credit also adjusts, which is often associated with liquidity management decisions firm are forced to make. The ratio of payables to cost of goods sold rises, indicating that firms delay payments to suppliers (Figure 4d). By contrast, the ratio of receivables to sales is unchanged (Figure 4e), suggesting limited scope for firms to accelerate client payments. This asymmetry implies that liquidity shocks are passed upstream along supply chains (echoing previous findings such as Boissay and Gropp, 2013 and Amberg *et al.*, 2021).



(a) Leverage ratio (b) Liquidity ratio

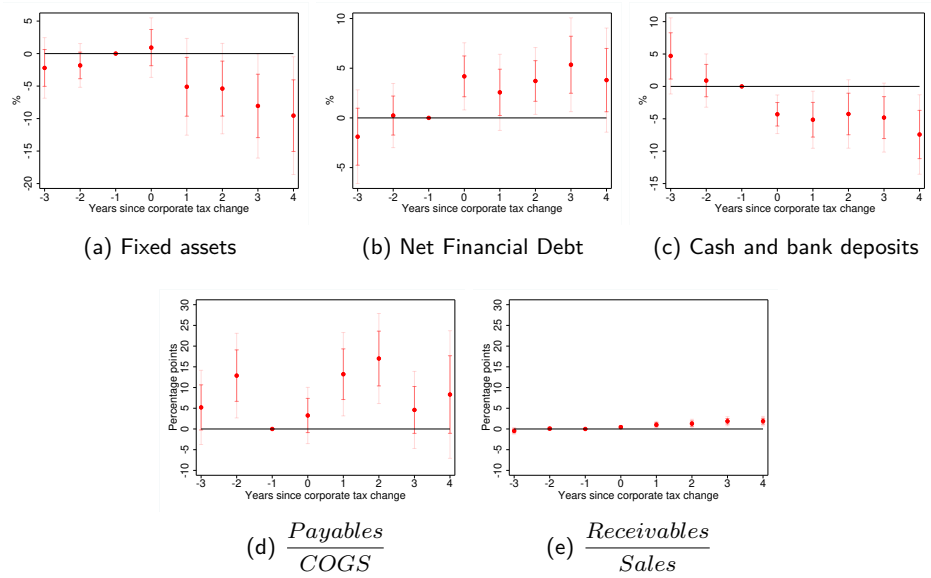
Figure 3: The effect of a corporate tax increase on leverage and liquidity

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.

Credit conditions. The increase in financial debt is accompanied by higher borrowing costs. Figure 5 shows that implicit interest rates rise by 0.16 percentage points on impact and 0.63 points after one year, stabilizing around 0.2 points above baseline thereafter.[13] Given that both debt volumes and borrowing costs rise, these patterns point to a strong credit demand channel, though a credit supply channel may also contribute as lower earnings reduce collateral.[14]

13. Implicit interest rates are proxied by the ratio between interest outlays and lagged financial debt.

14. See, e.g., earnings-based lending mechanisms discussed in Lian and Ma (2021); Ivashina *et al.* (2022); Caglio *et al.* (2021); Drechsel (2023); Gabriel (2024).



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

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

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

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.

Debt increases at both short and long maturities (Figures 6a and 6b). Short-term debt rises first, consistent with firms addressing liquidity shortfalls. Long-term debt rises more gradually.

Credit quality deteriorates. Non-performing loans (NPLs) increase both in value (as a share of total assets) and in incidence across firms (Figures 6c and 6d). Four years after the shock, the share of firms with an NPL is about 1.7 percentage points higher than baseline. This highlights that tax hikes exacerbate financial fragility, one of our novel findings emphasized in the Introduction.

4.2. Real outcomes

We next examine firms' activity. Figure 7 shows impulse responses for earnings, sales, inputs, labor, and productivity.

Earnings before taxes fall by 1.3 percentage points of assets one year after the shock and remain depressed thereafter (Figure 7a). Sales decline gradually, by 3.6 percent after four years (Figure 7b). On the input side, variable and external services costs decline by 1.3 and 0.9 percentage points of assets, respectively (Figures 7g and 7h). Labor inputs also contract: employment falls by 1.2 percent, hours worked

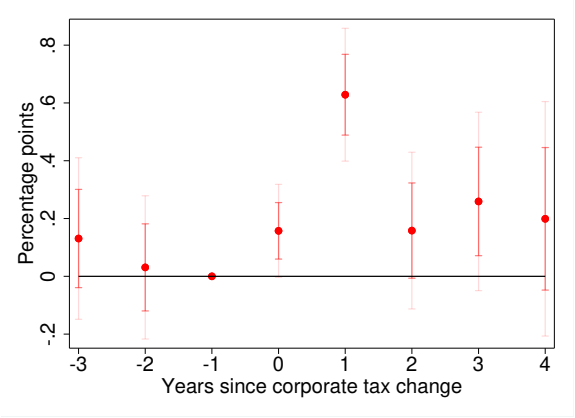


Figure 5: The effect of a corporate tax increase on implicit interest rates

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 $district \times year$ and $industry \times year$ fixed effects, as well as additional firm and municipality level controls. Robust standard errors are clustered at the municipality level.

by 2.2 percent, and average labor costs by about 1 percent (Figures 7c, 7d, and 7f).[15] By contrast, the number of R&D workers is unchanged, though the median firm employs none (Figure 7e).

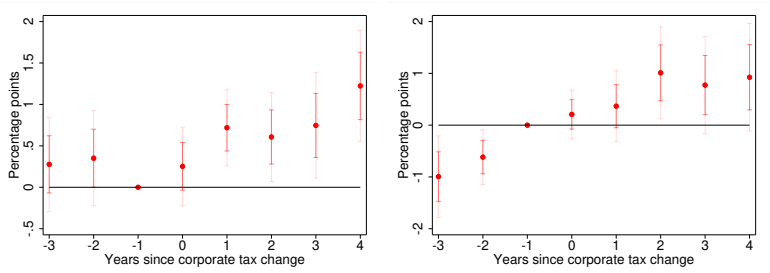
Total factor productivity declines persistently, by around 2.5 percent four years after the shock (Figure 7i). This productivity loss is consistent with the observed disinvestment in fixed assets and reductions in employment, underscoring that the effects of surtax increases extend beyond financial adjustments into firms' real capacity.

4.3. Heterogeneous impacts

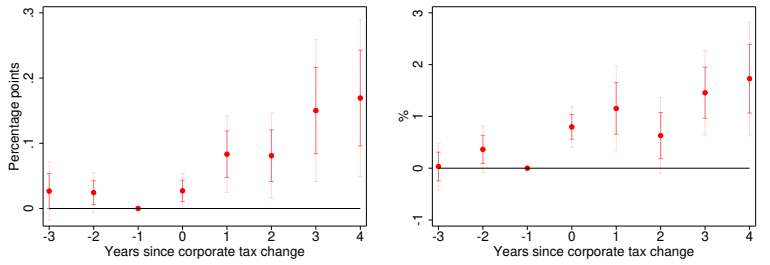
We next assess whether responses differ across firms with different characteristics. To do so, we re-estimate specification (1) under the same clean-control restrictions in equation (2), but separately by firm subgroup. This ensures that treated and control firms are always compared within the same category (e.g., treated small firms against untreated small firms), avoiding cross-group contamination. We focus on three dimensions: size, age, and profitability.

Table 2 reports results for leverage, liquidity, interest rates, and earnings at horizons $h = 1$ and $h = 3$. Small firms show the strongest responses: leverage

15. These magnitudes are in line with, e.g., Cloyne *et al.* (2025), that use a different identification strategy.



(a) Short term debt ($< 1y$) (b) Long term debt ($\geq 1y$)

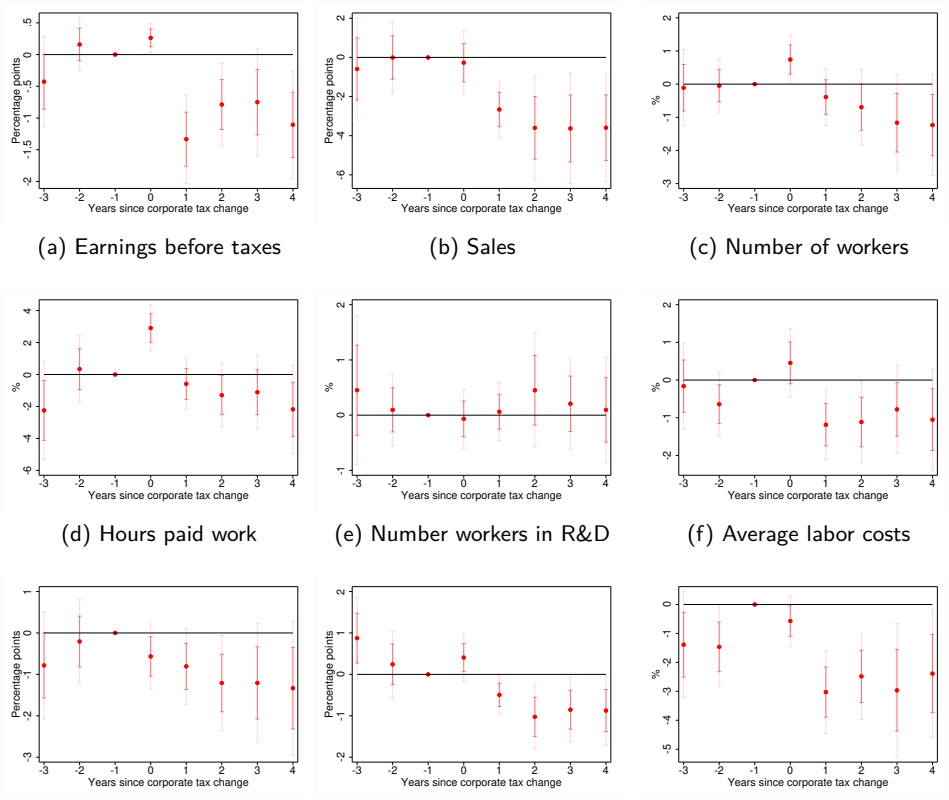


(c) Non-performing credit (d) Non-performing credit: extensive margin

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

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.

rises permanently, liquidity falls by 0.4–0.6 percentage points, and interest rates rise by 0.8 points one year after the shock. Earnings also fall sharply. Larger firms also increase leverage, but their liquidity does not decline significantly, and the rise in interest rates is more modest (0.4 points). This contrast is informative about mechanisms. 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). At the same time, liquidity-constrained firms may be forced to borrow more when retained earnings fall. Our results suggest that both channels operate: for small and young firms, leverage increases are clearly linked to liquidity losses, while for larger firms the absence of liquidity declines suggests strategic use of debt as a tax shield.



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

(d) Hours paid work (e) Number workers in R&D (f) Average labor costs

(g) Variable costs (h) External services (i) TFP

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

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.

Young firms also display more severe adjustments. Their leverage rises by 2 points after three years, liquidity falls by 0.8 points, and interest rates rise by 1.1 points in the short run. Older firms' leverage increases only in the short term and their liquidity remains stable. Earnings losses are much greater among young firms. This again points to a liquidity-driven mechanism among younger firms, in contrast to older firms where tax-shield behavior appears more relevant.

Profitability matters as well. Firms with positive earnings 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, heterogeneity results reinforce the view that surtax increases disproportionately harm firms that are small, young, or profitable—those with tighter financial constraints (Harju *et al.*, 2022). At the same time, larger and older firms adjust by using debt strategically to reduce their tax burden, consistent with tax-shield behavior.

	Leverage		**Liquidity**		**Int. Rate**		**EBT**	
	h=1	**h=3**	**h=1**	**h=3**	**h=1**	**h=3**	**h=1**	**h=3**
Small	0.78**	1.21***	-0.42**	-0.57*	0.76***	0.30*	-1.67***	-0.68*
	(0.42)	(0.61)	(0.23)	(0.40)	(0.19)	(0.25)	(0.51)	(0.54)
# Obs	461,614	310,403	461,614	310,403	226,916	143,998	461,614	310,403
Big	1.13**	1.13*	-0.10	0.32	0.40*	0.25*	-0.41	-0.77*
	(0.61)	(0.79)	(0.35)	(0.41)	(0.30)	(0.25)	(0.44)	(0.69)
# Obs	177,256	125,177	177,256	125,177	125,204	85,025	177,256	125,177
Wald test	0.64	0.93	0.45	0.12	0.32	0.88	0.06	0.92
Young	0.63	1.97***	-0.51**	-0.80***	1.11***	0.72**	-8.13***	-6.24*
	(0.63)	(0.64)	(0.31)	(0.34)	(0.41)	(0.43)	(2.56)	(4.09)
# Obs	189,740	140,926	189,740	140,926	50,282	37,724	155,358	110,947
Old	1.00***	0.37	-0.18	0.10	0.48***	0.13	-3.16*	1.80
	(0.35)	(0.61)	(0.21)	(0.37)	(0.14)	(0.20)	(2.57)	(3.22)
# Obs	449,107	294,640	449,107	294,640	301,806	191,279	394,380	248,459
Wald test	0.61	0.07	0.38	0.07	0.14	0.21	0.17	0.12
Profitable	0.83***	1.35***	-0.35*	-0.34	0.76***	0.20*	-5.44***	-2.12
	(0.41)	(0.54)	(0.23)	(0.36)	(0.15)	(0.20)	(1.76)	(2.58)
# Obs	589,291	400,383	589,291	400,383	318,826	206,396	511,692	333,779
Not Profitable	1.09	-0.65	-0.12	-0.58	-0.78***	0.67*	-2.60	-0.77
	(2.26)	(1.97)	(0.75)	(1.14)	(0.27)	(0.43)	(11.86)	(12.06)
# Obs	49,542	35,169	49,542	35,169	33,256	22,610	38,025	25,611
Wald test	0.91	0.33	0.77	0.84	0.00	0.33	0.81	0.91

Table 2. Heterogeneous effect of corporate taxation

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<0.32, p<0.10 and p<0.05, 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.

4.4. Relocation and exiting

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:

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

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

For relocation, we estimate:

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

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 a firm operates, as multi-establishment firms may find it easier to move headquarters.

	Exiting (at *t+1*)	**Relocating**
Tax increase	0.0310***	0.0724***
	(0.0115)	(0.0175)
× number of establishments	0.0014	0.0164***
	(0.0054)	(0.0079)
Controls	Yes	Yes
FE	Yes	Yes
Observations	652,857	714,049

Table 3. Probability of survival or relocating

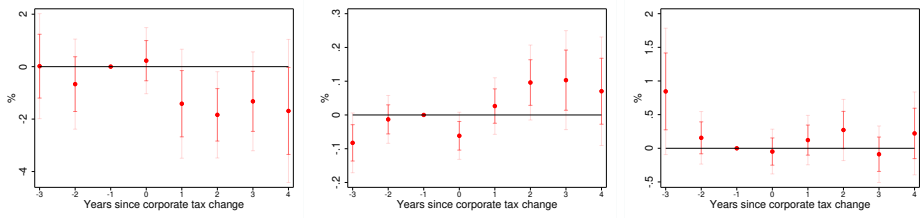
Notes: Estimates reflect the responses of the dependent variables to positive treatment, that is events in which there was an increase in corporate taxes in municipality m in year t. The dependent variables are dummy variables, taking the value of one if the firm is no longer in the panel dataset in period *t+1* or taking the value of one if they relocate the headquarters to another municipality in period t, respectively. *, **, and *** indicate statistical significance at p<0.32, p<0.10 and p<0.05, respectively. The estimation includes firm fixed effects, $district \times year$ and $industry \times year$ fixed effects, as well as additional municipality level controls. Robust standard errors are clustered at the municipality level.

Table 3 shows the results. A firm is around 3% more likely to exit the panel following a corporate tax hike. In addition, firms are also more likely to relocate to another municipality, with a positive coefficient around 7%. Also interestingly, this probability increases by around 1.6% for every additional establishment the firm has.

At the municipality level, we examine the aggregate effects on the number of firms and on neighboring areas. For this purpose, we estimate:

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

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 excluding m), and neighboring municipalities' aggregate GVA.



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

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

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 $district \times year$, as well as additional municipality level controls. Robust standard errors are clustered at the municipality level.

Figure 8a shows that the number of firms in treated municipalities declines by 1.4 percent one year after the shock and 1.7 percent after four years, equivalent to around 11–13 fewer firms per municipality. Neighboring municipalities see modest gains, about 0.1 percent (≈13 firms) at the peak (Figure 8b), consistent with relocation or reallocation of new firm births. However, Figure 8c shows no significant increase in aggregate GVA of neighbors, suggesting that reallocation is 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 non-performing loans. Real activity contracts, with lower earnings, sales, employment, and productivity. Effects are concentrated among small, young, and profitable firms, where leverage is driven by liquidity squeezes. Larger and older firms also increase leverage, but without liquidity declines, consistent with tax-shield behavior. Relocation partly offsets local losses but 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.

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.

6. Conclusion

This paper examined how local corporate tax increases affect firms' financial and real outcomes, using variation in municipal surtax rates in Portugal. By combining the universe of firm-level balance sheets with loan-level credit registry data, we provided evidence on both sides of firms' adjustments to taxation.

We find that surtax hikes tighten financing conditions: firms reduce liquidity, increase leverage, and face higher borrowing costs and default risk. Liquidity

pressures are visible not only in cash holdings but also in trade credit behavior, and loan-level data show clear increases in non-performing debt. These financial strains spill over into real outcomes, with lower sales, employment, and persistent declines in productivity. The effects are concentrated among small and young firms, while larger and older firms adjust mainly by shifting their capital structure. Relocation and exit rise, but such movements do not raise output in neighboring areas, suggesting inefficient reallocation.

Three contributions follow. First, we unify perspectives that typically study financing and real activity in isolation, showing how tax hikes propagate across both margins. Second, we introduce novel dimensions rarely observed in prior work: the impact of taxation on liquidity management, trade credit, and the incidence of non-performing loans. Third, we shed light on the mechanisms behind firms' rising leverage. For smaller and younger firms, leverage increases are clearly tied to liquidity losses, consistent with borrowing to offset cash shortfalls. For larger firms, by contrast, leverage rises without liquidity declines, consistent with strategic use of debt as a tax shield.

Overall, the findings suggest that even modest local surtax increases can generate meaningful financial and real distortions. In settings where municipalities hold tax autonomy—such as Germany and Switzerland—the results indicate that local tax policy has implications beyond revenue, shaping firms' balance sheets, credit quality, and productivity.

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Online Appendix

Appendix for Section 2

A.1. Portuguese corporate tax system

This appendix section briefly describes the Portuguese corporate income tax system. In terms of fiscal revenue, this tax is the third biggest source of revenue for the Portuguese government, behind VAT and personal tax income revenues. Corporate tax revenues are relatively small in percentage of GDP, averaging 3% between 2000 and 2020 (Braz *et al.*, 2022).

Corporate taxes apply to any firm resident or with a permanent establishment in the country, and the tax period coincides with the calendar year. The taxable income is calculated based on a set of rules put forward by the tax code. Initially, this set of rules is applied to do the transition for accounting profit or losses to taxable profits or losses. These include capital depreciation deductions, as well as interest expenditures.[16] Afterwards, taxable profits are adjusted from previous year losses, which could be carried forward for a period of 5 years since 2017 — with the exception of small and medium firms, which could carry forward losses for a period of 12 years. Before that year, all firms could carry forward losses for a period of 12 years. After applying the tax rates to taxable income, there are still tax incentives and deductions that are still applied as, for example, R&D investment incentives. There is still another component of the tax scheme — entitled autonomous taxation — that is levied over specific expenses considered not to be related to firms' activities, to avoid possible tax evasion schemes. Only after that is the total tax liability obtained.

In table A1 we show the overall tax rate levied in Portugal in the year of 2021 (the final year of our sample). The system also has tax brackets that introduce progressivity to the system, on top of the general rate. On top of general national rates and surtax rates, municipalities can also 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, excepting the 2012-2013 period, the average effective tax rates have been relatively stable across time (Braz *et al.*, 2022).

16. Since 2014, interest expenditure is deductible up to €1,000,000 or 30% of the EBITDA.

	Brackets	Rates
Statutory general 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%

Table A1. Corporate tax rates in Portugal

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

A.2. Descriptive statistics at the municipality level

This section presents some descriptive statistics of the municipalities. As mentioned in the paper, there are 278 municipalities in mainland Portugal. Municipalities are agglomerated in districts, with 18 districts in all territory. Table A2 reports these municipality statistics.

As it is clear from table A2, restricting the sample to municipalities that either changed once the corporate surtax rate or never changed does not alter the main statistics of the included municipalities, comparing to the case all municipalities are included. All the statistics of one group lie within confidence intervals of the other group.

	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

Table A2. Municipality descriptive statistics

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

Regarding the surtax changes, the bulk of it is concentrated in the extreme cases, that is either from a surtax of 0% to 1.5% or vice-versa (which is true both for the overall as well as for the restricted sample). Focusing on positive changes only, the mean change in corporate surtax for municipalities that only experienced one change is 0.88 percentage points. This means that the average increase in taxes of a treatment event is a sizable tax change, even though the municipality surtax is limited to a maximum of 1.5%, as mentioned before. Overall, 12,182 firms are treated throughout the considered period in our restricted sample.

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

This section presents some firm level descriptive statistics, for the overall and the restricted sample separately. Table A3 reports these values.

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

	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

Table A3. Firm balance sheet and credit descriptive statistics

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

The dataset also includes a variable indicating the yearly outlays paid to the government in corporate taxes, which allow us to calculate an effective corporate tax rate (ETR) paid by each firm in each year. Although there are some caveats in the Portuguese corporate tax system that make this measure not perfect — for example, we do not perfectly observe the tax base as the tax code defines it — it serves as a fairly good proxy for the ETR paid. In particular, we define ETR as paid corporate taxes over earning before taxes, and trim the estimates to be confined to values between 0% and 100%. For our sample, the mean and median effective tax rate are fairly close across the two groups, and in line with what is reported by Braz *et al.* (2022) for the Portuguese firms.

An additional important variable we use throughout our anaysis is the implicit interest rates firms pay. The loan level data we have does 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 interest outlays each firm pays in a given year. Implicit interest rates are then measured by the ratio between these interest outlays and lagged financial debt. Across firms in our sample, the average implicit interest rates is 4.29%. For treated firms in the pre-treatment year, average rates are 4.82%. For comparison, the average monthly interest rate on new loans below €1,000,000 for 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 for firm level total factor productivity (TFP).[17] The methodology assumes the following Cobb-Douglas production function:

$$y_{i,t} = \alpha + \beta_l l_{i,t} + \beta_k k_{i,t} + \omega_{i,t} + e_{i,t}, t = 1, \ldots, T, \qquad \text{(A.A.1)}$$

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

$$\omega_{i,t} = E\left(\omega_{i,t} \mid \omega_{i,t-1}\right) + \varepsilon_{it} = g\left(\omega_{i,t-1}\right) + \varepsilon_{i,t} \qquad \text{(A.A.2)}$$

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

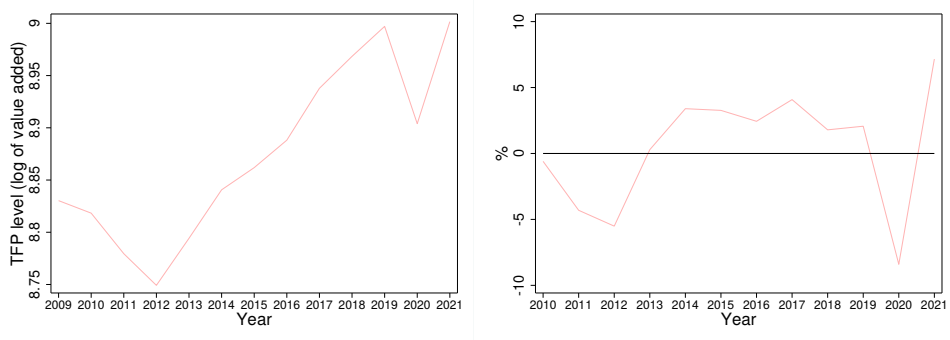
Estimating regression (A.A.1) by OLS leads to biased estimates, given simultaneity issues (firm's may be choosing inputs in response to productivity shocks) or selection bias (firm's that suffered productivity shocks may no longer be in the sample). Therefore, we perform a system Generalized Methods of Moments (GMM-SYS) estimation of (A.A.1). The GMM-SYS imposes the assumptions that inputs in lagged differences are uncorrelated with $\varepsilon_{i,t}$, allowing for a direct estimation of regression (A.A.1).[18] Variables are deflated at the 2-digit sector specific deflator, and estimation of factor shares (β_l and β_k) is done at the sector level.

Figure A1 plots the median evolution of firm level TFP and TFP growth rates across the sample years. It is also worth mentioning that we computed firm level TFP also following the approach of Ackerberg *et al.* (2015) and results did not significantly change.

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

17. There are 3 other different methodologies typically used by the literature to estimate firm level TFP, all of them semi-parametric approaches: Olley and Pakes (1996), Levinsohn and Petrin (2003) and Ackerberg *et al.* (2015). For a detailed discussion on the different methods see, for example, Bournakis and Mallick (2018) or Li and Su (2022).

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



(a) Median of TFP level

(b) Median of TFP growth

Figure A1: Median TFP and TFP growth rate

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

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 firms' pay. We estimate the following regression:

$$ETR_{i,t} = \mu_i + \alpha_{d,t} + \varphi_{s,t} + \beta \Delta D_{m,t} + \varphi X_{m,t} + \varepsilon_{i,t}, \tag{B.B.1}$$

where, relatively to the baseline specifications, we include a firm fixed effect, and controls $X_{m,t}$ only include contemporaneous and lagged values of municipality GVA. The coefficient of interest is presented in table B1.

We should note that it is not obvious that such policy change leads to a permanent increase in the effective tax rates, given all the previously describe adjustments firms go through. All those dynamic adjustments can in fact lead to lower effective tax rates. In any case, and reassuringly, at impact there is a positive impact on the effective tax rates — on average, a firm that operates in a municipality that increases the surtax rate, experiences an increase of 0.84 percentage points in its effective tax rate.

	Effective tax rate
Tax increase	0.8422***
	(0.3282)
Controls	Yes
FE	Yes
Observations	884,232

Table B1. Effective tax rate response to surtax increase

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<0.32, p<0.10 and p<0.05, respectively. The estimation includes firm fixed effects, $district \times year$ and $industry \times year$ fixed effects, as well as additional municipality level controls. Robust standard errors are clustered at the municipality level.

Appendix for Section 5

Table C1 shows the results of the robustness exercises explained in section 5 of the paper.

	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	0.86***	1.24***	-0.34*	-0.34*	0.62***	0.26*	-1.34***	-0.75*
	(0.39)	(0.55)	(0.22)	(0.34)	(0.14)	(0.19)	(0.42)	(0.51)
L=8	0.84***	1.22***	-0.34*	-0.35*	0.62***	0.27*	-1.33***	-0.74*
	(0.39)	(0.55)	(0.22)	(0.34)	(0.14)	(0.19)	(0.43)	(0.52)
L=12	0.83***	1.22***	-0.34*	-0.35*	0.62***	0.27*	-1.32***	-0.74*
	(0.39)	(0.55)	(0.22)	(0.34)	(0.14)	(0.19)	(0.43)	(0.52)
Zero rate control group	1.41***	1.86***	-0.51***	-0.55*	0.62***	0.28*	-2.19***	-1.76***
	(0.42)	(0.53)	(0.23)	(0.35)	(0.16)	(0.19)	(0.42)	(0.50)
#1 establishment only	0.99***	1.35***	-0.37**	-0.36*	0.53***	0.22*	-1.37***	-0.74*
	(0.40)	(0.55)	(0.22)	(0.35)	(0.14)	(0.21)	(0.43)	(0.52)

Table C1. Robustness extensions

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<0.32, p<0.10 and p<0.05, 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.

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