# Corporate Taxes and Firms' Operating Cost Behavior

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ABSTRACT

Using a panel of European private firms, we show that corporate taxes amplify the responsiveness of operating costs to sales, consistent with conforming tax planning in the operational business. We also find evidence of a tax-driven cost stickiness: The tax-induced change in operating cost is stronger for increases in sales than for decreases in sales. Further, we show that the effect of corporate taxes on cost behavior varies in the cross-section: Cost behavior of firms with losses, firms with access to nonconforming tax planning, or firms with the ability to pass on taxes to stakeholders is less sensitive to corporate taxes. Altogether, our results suggest that taxes can affect reported pre-tax cost behavior and contribute to cost stickiness.

**Keywords:** Corporate taxes, tax planning, cost structure, asymmetric cost behavior, tax avoidance, sticky cost

**Data Availability:** Data are available from sources identified in the paper.

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## 1 Introduction

We analyze the effect of corporate tax rates on operating cost behavior. Operating cost responsiveness is at the core of management accounting research (see Anderson and Widener 2007 for a review). A large stream of literature has analyzed the phenomenon that costs respond less to decreases than to increases in sales (sticky cost phenomenon, Noreen and Soderstrom 1997, Anderson et al. 2003, see Banker and Byzalov 2014 for a literature review). We identify corporate taxes as a significant determinant of operating cost responsiveness and cost stickiness.

Taxes incentivize firms to reduce their taxable income through tax planning activities. This is linked to operating cost because tax planning can affect both book and taxable income through conforming tax planning (see Badertscher et al. 2019). The scope of such tax planning is wide covering intertemporal profit shifting (e.g., inventory valuation), contracts with owner-managers that affect EBIT (via management compensation), or real reactions to taxes (e.g. labor-leisure decisions of owner-managers). Since such tax planning is about reducing book income and taxable income through sales or reported costs, it is inevitably linked to the cost behavior research and it one channel how corporate taxes affect cost behavior.

We combine tax and management accounting literature by examining the role of corporate taxation in firms' pre-tax cost behavior. To illustrate our predictions, we set up a simple model where firm maximize profits and respond to corporate taxes via tax planning. Under two reasonable assumptions—increasing marginal cost of tax planning and economies of scale[1]—the model shows that higher corporate tax rates lead to higher marginal cost following sales increases. Intuitively, higher corporate tax rates increase the benefits of tax planning and as long as marginal costs do not increase similarly, firms have an incentive to increase operational costs when sales increase. Our simple model further allows us to illustrate the potential asymmetry in the cost response to corporate taxes when sales decline vis-à-vis when sales increase. We can show in the model that the corporate tax rate effect on operating cost via tax planning is asymmetric between decreases and increases in sales as long as there are economies of scale of tax planning. This results in asymmetric responses of operating costs to corporate taxes and, thereby, corporate taxes can contribute to cost stickiness.

To summarize our predictions, we expect that corporate taxes set incentives for tax planning which have observable effects on operating cost behavior. Second, we expect the

[1] The assumption of increasing marginal cost of tax avoidance directly follows Hines and Rice (1994), Huizinga and Laeven (2008), or De Simone et al. (2017). The argument of economies of scale captures that larger firms have more sophisticated tax departments and internal staff that enables these firms to respond quicker and in more sophisticated ways (Barrios and Gallemore 2019, Zwick 2020). Rego (2003), Dyreng et al. (2008), Badertscher et al. (2013), or Saunders-Scott (2015) find evidence for economies of scale to tax planning.

corporate tax effect on operating cost responsiveness to be smaller for decreases than for increases in sales under realistic assumptions (e.g., economies of scale of tax planning). We also note that there are two forces that work against our predictions, namely if the incidence of the corporate tax falls on stakeholders such as consumers or employees (e.g., Suárez Serrato and Zidar 2016; Fuest et al. 2018, Jacob et al. 2020) or if there are implicit taxes (Jennings et al. 2012, Markle et al. 2020). Ultimately, it is an empirical question whether (1) corporate taxes affect cost behavior and (2) whether corporate taxes contribute to cost stickiness.

Our empirical strategy is based on the response of reported operating costs to changes in reported firm sales. Intuitively, consider two firms with similar sales changes but different corporate tax rates. When sales increase, the firm facing a higher tax rate has greater incentives to report higher incremental costs than the firm facing a lower corporate tax rate. Hence, our identification approach requires variation in corporate tax rates and idiosyncratic changes in firm sales. We use a sample of over 470,000 domestically operating private corporations with unconsolidated financial statement data across 36 European countries, obtained from Amadeus (see, also, De Simone 2016; De Simone et al. 2017; Bethmann et al. 2018).

There are several key advantages of using private corporations across countries as our primary sample. First, cross-country data provide sufficient variation in corporate tax rates. Second, using unconsolidated data, we can proxy more precisely for a firm's marginal tax rate via the statutory corporate tax rate than in a sample of publicly traded firms. Using consolidated data on multinationals would not enable us to use the exact statutory tax rate faced by a firm because of cross-border operations. Third, multinationals are known to shift profits into low-tax countries (OECD, 2015), introducing measurement error to sales and operating costs. Importantly, this error is affected by corporate tax rates. A focus on domestic-only firms mitigates concerns that our results are driven by international profit shifting. Fourth, using private corporations with typically closely held ownership reduces capital market pressure to use income-increasing financial accounting choices (Klassen 1997; Badertscher et al. 2019). Hence, our sample firms are expected to use conforming tax planning, one channel through which corporate taxes can affect cost behavior. We also acknowledge that our analysis of domestically oriented, private corporations might not generalize to publicly traded firms. Still, our setting allows us to draw conclusions on an economically significant sector of economies around the world. Private firms employ about two-thirds (half) of all employees in the European Union (OECD) and they experience less tax audits and have more leeway in tax compliance (e.g. Beck et al. 2014, Bachas and Jensen 2019). It is thus important to understand the consequences of corporate taxes and tax planning for these firms.

In first-difference panel regressions following Anderson et al. (2003) and Banker et al. (2013), we find robust evidence supporting our predictions. The responsiveness of operating costs to increases in sales is positively associated with tax rates. For higher tax rates, a sales increase is associated with a higher increase in operating costs than in a low-tax country. For example, for a tax rate of 20%, a sales increase of 17.8% of total assets (the median sales increase for the subsample of firms with increasing sales) results in an increase in earnings before interest and taxes (EBIT) of 2.35%. For a tax rate of 30% (e.g., the tax rate in Germany), the increase in EBIT is only 1.95%, which is a reduction in pre-tax profitability of 16.8%. With respect to the potential asymmetry of the corporate tax effect, we find that the effect of tax rates on marginal operating costs for decreases in sales is statistically different from the one for sales increases. The overall effect in case of sales decreases is often insignificant. This finding is, however, consistent with the argument that corporate taxes have an asymmetric effect on cost behavior following sales increases versus decreases. If sales increase, firms increase operating costs due to tax incentives, but when sales decline, taxes incentivize firms to stick to their level of operational cost. This way, corporate taxes contribute to overall cost stickiness.

We subject our empirical tests to a battery of robustness and sensitivity tests. We briefly outline some of them here. First, we find evidence supporting the parallel trends assumption as we do not find any empirical evidence for the anticipation of tax rate changes. Second, we show that the results on the relation of taxes and cost stickiness are robust to using the Weiss (2010) approach, a "pure changes" approach, or a hierarchical model. Third, we show that our results cannot be solely explained by intertemporal income shifting around tax rate changes.

Finally, we examine several cross-sectional differences in the effect of taxes on cost behavior. First, we exploit the difference between loss-making and profitable firms. The former have very few incentives for tax planning. Hence, corporate taxes should have a weaker effect on operating cost responsiveness for loss firms than for profitable firms. Our empirical results support this notion. Second, we exploit differences between standalone firms and firms belonging to domestic or multinational groups. The latter have access to cross-border profit shifting, that is, nonconforming tax planning, while the former (standalone firms) are more likely to use tax planning via operating cost. We find corporate taxes to have a larger impact on cost responsiveness for standalone firms than for firms belonging to groups. However, we still find evidence of a corporate tax effect on cost behavior for multinational firms.

The third cross-sectional test examines differences in the ability of firms to pass on corporate taxes to stakeholders (tax incidence; e.g., Suárez Serrato and Zidar 2016; Fuest et al. 2018, Jacob et al. 2020). While our model implies that higher corporate taxes result in higher marginal operating costs (or lower sales), tax incidence falling on stakeholders works against this prediction. If firms can pass on the tax burden to other stakeholders, they have less incentive to avoid taxes as shown by Dyreng et al. (2020). For firms with more market power and, thus, the ability to pass on taxes to stakeholders, we expect corporate taxes to have a smaller impact on operating cost than for firms with less market power. We exploit cross-sectional differences in firms' market power and find empirical support for this prediction. Still, we find that for firms with high market power corporate taxes affect cost behavior.

The final cross-sectional test examines the role of implicit taxes (Jennings et al. 2012, Markle et al. 2020). Implicit taxes can alter the cost structure of firms, for example, because higher corporate taxes reduce prices of assets, and thereby leading to lower costs (e.g., via lower depreciation) irrespective of tax planning. To assess the extent to which implicit taxes drive our results, we sort industries into those with high versus low potential of being subject to implicit taxes using the asset redeployability measure by Kim and Kung (2017). Our results suggest that tax planning, but not implicit taxes are driving the effect of corporate taxes on operating cost behavior. One potential explanation for this finding is that for private firms, the employed assets are firm-specific. Hence, implicit taxes may not arise in our setting in a way that they would affect the estimates of the effect of corporate tax on cost behavior in our sample.

Taken together, our paper contributes to several strands of the literature. First, by analyzing the effect of tax rates on cost behavior, it adds to the literature on the consequences of tax planning activities (for reviews see, e.g., Hanlon and Heitzman 2010; Wilde and Wilson 2018). Second, as tax rates set the incentives for tax planning, we answer the call by Shust and Weiss (2014) to explore how reporting incentives affect cost behavior. We also contribute to the vast literature on the sticky cost phenomenon (e.g., see Anderson et al., 2003 or the review by Banker and Byzalov, 2014). We provide evidence that corporate tax rates and the resulting incentives for tax planning shape (reported) cost behavior and amplify asymmetric cost behavior. In light of our results, taxes and tax planning adjustment costs contribute to the explanation of cost stickiness. Third, we add to the literature on conforming tax planning (Badertscher et al. 2019). We show that there is an important asymmetry: when firm sales increase (decrease), tax planning expands (does not change as much). Hence, our paper is related but distinct from Eichfelder et al. (2019) who examine average levels of conforming tax avoidance but do not analyze how conforming tax planning reacts to sales changes and whether it is asymmetric.

## 2 Hypotheses, Empirical Strategy, and Data

### *2.1 Cost Behavior and Cost Stickiness*

The analysis of cost behavior, its determinants, and its consequences is at the focus of management accounting research (for reviews, see Anderson and Widener 2007; Krishnan 2015; Banker et al. 2018). One important area within this field is cost asymmetry. The theory of sticky cost argues that costs respond differently to decreases than to increases in sales (see Banker and Byzalov 2014). Following Anderson et al. (2003), most empirical approaches test this assumption in a first difference approach that regresses changes in cost on changes in sales. By using a dummy for sales decreases between two subsequent years ($SDEC_{it}$=1 if $Sales_{it}$<$Sales_{it\text{-}1}$), a potential asymmetry of the cost structure is accounted for:

$$\Delta OpCost_{it} = \alpha + \beta\,\Delta Sales_{it} + \gamma\,SDEC_{it} \times \Delta Sales_{it} + \varepsilon_{it} \qquad (1)$$

The coefficient $\beta$ indicates cost responsiveness for increases in sales. If sales have decreased ($SDEC_{it}$ = 1), management may drive down cost at the same rate as cost would increase for increases in sales ($\gamma$=0). If management keeps unused resources in expectance of a future sales recovery, costs will be sticky for decreases in sales ($\gamma < 0$). The higher the resource adjustment costs for reducing and/or building up resources are and the more optimistic the management is about future sales, the higher the absolute value of the coefficient estimate ($|\gamma|$). Sticky costs, which emerge from management decisions on resource adjustments, are within the continuum between full fixed cost (prohibitively high resource adjustment cost) and full variable cost (no resource adjustment costs, mechanical adjustments). Banker and Byzalov (2014) review the rich empirical evidence on cost stickiness. However, the role of tax rate incentives for cost behavior has not been discussed yet.[2]

### *2.2 Corporate Taxes, Tax Planning, and Cost Behavior*

We combine the literature on taxes and on cost behavior by examining the role of corporate taxation in firms' cost behavior. To illustrate the effects and predictions, we set up a simple static model of taxes and cost behavior where we allow for tax planning. In general, taxes set incentives for firms to reduce their tax bill through tax planning activities, e.g., by reducing book income. We use a simple model where a firm has sales *S* and "true" cost *C* which are a

[2] A paper closely related to ours is that of Xu and Zheng (2018), who argue that cash tax savings from tax avoidance alleviate managers' concerns about adjustment costs. They document a significantly negative association between tax avoidance and cost stickiness. We see serious endogeneity issues in this approach, since cost responsiveness and cash effective tax rates are both affected by activity (and potentially several other confounders, such as earnings management). In contrast, we argue that the causality is different: tax planning strategies directly affect reported sales and/or operating costs as a function of the corporate tax rate. In contrast to Xu and Zheng's setting, we exploit exogenous variations in corporate tax rates that affect tax avoidance incentives and cost stickiness.

function of sales ($C = C(S)$ with $dC/dS > 0$, $dC^2/d^2S \geq 0$). The firm can reduce its tax base via tax planning by inflating its tax deductible cost by the fraction $x$ (with $x \geq 0$), so that $(1+x)\cdot C$ is deductible. Hence, the variable $x$ describes tax planning. The scope of this kind of tax planning is wide and is assumed to be flexibly adjusted even in the absence of major shifts in the firm such as M&As or large asset purchases.[3] It covers intertemporal profit shifting (e.g., inventory valuation) as well as shifting of income between owners and the firms (e.g., shifting book income to shareholders via deductible compensation payments). Other examples are tax-driven contracts with suppliers, employees, or customers that affect EBIT. In addition, for closely held companies in which owners actively participate, owners' reduction of labor input or effort due to taxes (labor/leisure tradeoff, Berg and Thoresen 2019) can be qualified as real tax planning (e.g. Meghir and Phillips 2010). Tunneling and the expropriation of minority shareholders that affect EBIT could also be considered as tax planning, because tunneling decreases taxes paid and profits.[4]

However, tax planning is costly. We follow prior literature (e.g., Hines and Rice 1994; Huizinga and Laeven 2008; De Simone et al. 2017) and assume increasing marginal cost of tax planning. This is captured by the parameter $b$ with $b > 0$. Further, we assume that there are economies of scale in tax planning, as indicated by the parameter $e$. Intuitively, larger firms have more sophisticated tax departments and internal staff that enables these firms to respond quicker and in more sophisticated ways (Barrios and Gallemore 2019, Zwick 2020). Rego (2003) and Dyreng et al. (2008) find evidence for economies of scale to tax planning ($e > 0$). Scale economies to tax planning are further discussed in Badertscher et al. (2013) and Saunders-Scott (2015). Taken together, we follow prior literature and assume that the cost of reducing the tax base increase with the amount of tax planning $x$ (progressive cost of tax planning $b > 0$), but we allow for potential scale economies to tax planning ($e \geq 0$). Combined, we model the cost function of tax planning as

$$\frac{a}{1+b}\cdot x^{1+b}\cdot C^{1-e}$$

with $a>0$, $b>0$, $0\leq e<1$. $a/(1+b)$ sets the "baseline level" of tax planning cost, whereas $b$ determines its progressivity. The higher $b$, the more progressive are the cost of tax planning.

[3] The understanding that tax planning is a continuous process is consistent with prior literature that managers are continuously evaluating tax avoidance opportunities. More than 84 percent of surveyed managers consider the taxes paid as important (Graham et al. 2014) for managerial decisions.

[4] Throughout the paper, we refer to legal tax planning activities. We note that tax evasion would also show up as an inflation of reported cost (or a concealment of reported sales). Economically, the incentives to evade taxes are similar because individuals weigh the benefits (evaded tax) against the costs (potential penalties and non-pecuniary costs such as morals). Tax evasion is unfortunately unobservable in our data but has the same economic implications as legal tax planning.

The parameter $e$ captures the scale economies to tax planning. To illustrate, if firm A is twice as large as firm B and has twice the amount of firm B's cost, it would face less than twice the tax planning cost of firm B to increase deductible cost by the same fraction $x$. For the sake of simplicity, we follow the literature (e.g., Huizinga and Laeven 2008) in assuming that the cost of tax planning is not tax deductible. The after-tax profit $\Pi_\tau$ is

$$\Pi_\tau = S - C - \tau \cdot \left(S - (1+x)\cdot C\right) - \frac{a}{1+b} \cdot x^{1+b} \cdot C^{1-e} \tag{2}$$

The firm now maximizes profits by choosing the optimal level of tax planning $x$:

$$\frac{d\Pi_\tau}{dx} = \tau \cdot C - a \cdot x^b \cdot C^{1-e} = 0$$

$$\Rightarrow x = \left(\frac{\tau \cdot C^e}{a}\right)^{\frac{1}{b}}$$

The model gives the expected predictions for $x$: For profitable firms, the optimal fraction of cost inflation $x$ increases with increasing tax rates ($\tau$) and with increasing opportunities ($C$), and it decreases with increasing tax planning cost ($a$ and $b$). We omit the derivatives. To examine cost behavior, we are interested in how the deductible cost $(1+x)\cdot C$ react at the margin if sales increase. The marginal deductible cost for sales changes, considering tax planning, are

$$\frac{d(1+x)\cdot C}{dS} = \frac{dC}{dS} \cdot \left(1 + \underbrace{\left[\left(1+\frac{e}{b}\right)\cdot\left(\frac{\tau}{a}\right)^{\frac{1}{b}} \cdot C^{\frac{e}{b}}\right]}_{\substack{\text{Tax planning effect} \\ \text{on observed marginal cost}}}\right) \tag{3}$$

From the model, we can directly deduct the first hypothesis: As the tax planning effect on observed marginal cost strictly increases with the tax rate, we hypothesize that the responsiveness of operating cost to changes in sales increases with the corporate tax rate. Intuitively, if the marginal cost of tax planning is unaffected by the tax rate, the marginal benefits of tax planning exceed marginal costs of tax planning when the tax rate increases. Even if the marginal cost would be partly positively correlated with the tax increase (e.g., risk of tax audits may increase with the tax rate or if tax advisory services become costlier), some tax planning cost components would still be unrelated to the tax rate, so that a tax rate increase would still drive the marginal benefits of tax planning above the marginal costs of tax planning. Consequently, the incentive for additional tax planning in reaction to an increase in sales will increase with the tax rate.

Tax planning can manifest in increasing reported additional cost (as in our model) and/or attenuating reported additional sales. Both will show up in a steeper relation between reported additional cost and sales. In other words, if sales increase, the resulting increase in reported operating costs is expected to be higher for higher corporate tax rates. Moreover, several countries use progressive corporate tax rates, which increases the marginal tax rate applicable to the firm (as in the Scholes-Wolfson framework). For increases in sales, a progressive tax intensifies the incentive for tax planning to the extent that the tax rate on the additional tax base increases. Altogether, corporate tax rates alter incentives for tax planning and, thus, drive cost behavior. For higher tax rates, the responsiveness of operating costs to sales changes increases. This leads us to our first hypothesis.

**H1:** When sales increase, the responsiveness of operating costs to sales changes increases with the corporate tax rate.

In the next step, we examine a potential asymmetry of the tax effect on cost behavior. We analyze how the interaction of the tax rate and a change in sales affect the optimal level of tax planning. We compare an increase in sales (Δ, with 0<Δ<Π) with an equal decrease in sales (-Δ). The new tax planning optimum $\overline{x}$ $(\underline{x})$ for an increase (decrease) in sales becomes:

$$\overline{x}\cdot C(S+\Delta)=\left(\frac{\tau}{a}\cdot C(S+\Delta)^{b+e}\right)^{\frac{1}{b}}$$

$$\underline{x}\cdot C(S-\Delta)=\left(\frac{\tau}{a}\cdot C(S-\Delta)^{b+e}\right)^{\frac{1}{b}}$$

We define $A$ as the asymmetry between the absolute change in tax avoidance (change in tax base reduction via tax planning) for increases and decreases in sales:

$$\begin{aligned} A &= \left|\begin{array}{l}\text{Increase in tax base reduction}\\ \text{for increase in sales}\end{array}\right| - \left|\begin{array}{l}\text{Decrease in tax base reduction}\\ \text{for decrease in sales}\end{array}\right| \\ &= \overline{x}\cdot C(S+\Delta)-x\cdot C(S) \qquad -\left(x\cdot C(S)-\underline{x}\cdot C(S-\Delta)\right) \\ &= \left(\frac{\tau}{a}\right)^{\frac{1}{b}}\cdot\left[\left(C(S+\Delta)^{1+\frac{e}{b}}+C(S-\Delta)^{1+\frac{e}{b}}\right)-2\cdot C(S)^{1+\frac{e}{b}}\right] \end{aligned} \tag{4}$$

We start with interpreting equation (4) for symmetric cost ($C((S+\Delta)+C(S-\Delta)=2\cdot C(S))$. As long as scale economies to tax planning are not negative ($e\geq 0$), the function $C(S)^{1+\frac{e}{b}}$ is concave, so that the tax avoidance reaction is asymmetric ($A>0$). The asymmetry increases with the tax rate $\tau$.[5] In case operating cost are sticky ($C((S+\Delta)+C(S-\Delta) > 2\cdot C(S))$[6], the asymmetry increases and the tax effect on the asymmetry increases. Intuitively, our model assumes that scale economies in tax planning make tax planning adjustments for increases in sales relatively cheap, leading to stronger adjustments for increases compared to decreases in sales. Taken together, we argue that the tax rate effect on operating cost responsiveness via tax planning is asymmetric between decreases and increases in sales, resulting in asymmetric responses of operating costs to corporate taxes. This leads us to our second hypothesis.

**H2:** As tax planning is adjusted asymmetrically for decreases and increases in sales, the tax rate affects the responsiveness of operating costs to decreases in sales less strongly than to increases in sales.

Theoretically, a parameter of e<0 indicating diseconomies of scale to tax planning is possible, even if it is inconsistent with prior empirical literature. One potential reason is that tax audit probabilities may increase in profits. That would mean that if firm A is twice as large as firm B and has twice the amount of firm B's profits, it would face more than twice the tax planning cost to avoid the same fraction $x$ of its profits. In this case, the change in tax planning (tax base change) for decreases in profits would be larger than for increases in profits.

At least two other effects amplify the asymmetry A. To keep things simple, we only explain them verbally. First, if marginal tax rates are higher in case of sales increases than in case of sales decreases, then the incentives for tax planning will be lower for sales decreases than for sales increases. To illustrate this, consider the marginal tax rates in the Scholes-Wolfson framework. In case of sales decreases, firms may face losses and thus have a lower marginal tax rates. This reduces the marginal incentive of tax planning. In contrast, in case of sales increases, firms may face higher marginal tax rates, thereby increasing the incentive to avoid taxes.

Second, it might be that there are adjustment cost of changing the tax planning level, which are not explicitly included in the model through a specific model parameter. The literature on sticky cost made the case that the combination of adjustment cost, deliberate management choice on resource adjustment, and increasing long-term trends in expected sales gives rise to asymmetric cost behavior (e.g. Banker and Byzalov 2014 in the non-tax context). Adjustments in tax planning are costly as they require the adjustment of tax planning resources, tax advisory,

[5] In addition, tax rate changes induce an additional temporary asymmetry in the reform year (tax reform effect). In Section 3.3.6, we control for the tax reform effect.

[6] Cost stickiness is not the same as convexity in the cost function, as cost stickiness depends on the level of the lagged sales (Banker and Byzalov 2014). However, in our static model both phenomena show up in the same way.

rewriting of contracts, or renegotiations with suppliers, employees, or customers. Kim et al. (2019) show that converging toward the tax avoidance optimum takes firms several years. They conclude that there are nontrivial costs of adjusting tax avoidance activities. Cost of tax planning adjustments, interacted with expected long-term sales increases, are an alternative explanation for asymmetric effects of taxes on cost behavior. However, in the context of our model, small (and potentially temporary) reductions in tax savings following a sales decrease may not suffice to justify adjustments in tax planning. In this way, cost of tax planning adjustments could amplify the asymmetry shown in the model.

Finally, we note that an asymmetric effect of taxes on cost behavior as predicted in H2 can contribute to the cost stickiness phenomenon (e.g., Anderson et al. 2003). Firms adjust their costs asymmetrically when avoiding taxes in a profit-maximizing way. This tax effect on cost stickiness differs from the usual cost stickiness discussed in the management accounting literature (e.g., Banker and Byzalov 2014). Cost stickiness has its roots in asymmetric management reactions to decreases versus increases in sales driven by declining sales. In contrast, the tax effect on cost stickiness is driven by increasing sales, since we argue that firms' tax planning has greater effects on operating costs for increasing sales than for declining sales.

### *2.3 Tax Incidence & Implicit Taxes*

There is empirical evidence that the incidence of a corporate tax is not fully borne by shareholders but partly shifted to other employees (e.g., Suárez Serrato and Zidar 2016; Fuest et al. 2018) or consumers (Jacob et al. 2020). Tax incidence works against the predictions outlined above. H1 predicts that higher taxes will result in higher marginal operating cost (or lower sales). In contrast, tax incidence postulates that part of the tax burden is shifted to suppliers, employees, or customers. Shifting tax burden to suppliers or employees will manifest in lower operating cost responsiveness for a given increase in sales. Likewise, shifting tax burden to customers will increase sales revenues for given operating cost. As we are interested in how corporate tax rates affects cost behavior, we ultimately measure the net effect of both channels, tax planning and tax incidence.

Further, operating cost behavior might be affected by corporate taxes via implicit taxes (Jennings et al. 2012, Markle et al. 2020). The implicit tax theory suggests that country-level taxes are positively associated with firm-level pre-tax profits. In other words, the higher the country-level tax rate, the higher the sales or the lower the cost of firms operating in this country. In our model, in case of high implicit taxes, firms face lower operating costs C for given sales when corporate taxes increase. This tax-induced decrease in operating cost works, as tax incidence, in the opposite direction of our predictions derived in the simple model.

Taken together, tax incidence and implicit taxes yield predictions in the opposite direction of our two main hypotheses H1 and H2. Ultimately, it is an empirical question whether corporate taxes affect cost behavior. In our cross-sectional tests (see Sections 4.3 and 4.4), we aim to examine differences in the ability of firms to pass on corporate taxes to stakeholders as well as to assess the importance of implicit taxes in private firms' operating cost behavior.

### *2.4 Empirical Strategy*

To test our hypotheses, we examine the effect of corporate tax rates on operating cost responsiveness and its asymmetry in case of increases versus decreases in sales. Our empirical strategy is based on the cost behavior and, in particular, cost stickiness literature. Following prior literature, we use a first-differences approach. We regress the change in operating costs ($\Delta OpCost_{it}$) on the change in sales ($\Delta Sales_{it}$), the tax rate, and the interactions of the change in sales with the tax rate and three control variables (change in GDP, unemployment, asset intensity). We include a dummy for sales decreases ($SDEC_{it}$) and interact the difference in effect for sales decreases with the tax rate (see, e.g., Banker et al., 2013). We interact the tax rate with the change in sales because we are interested in how the tax rate affects the relation between changes in sales and changes in operating cost (and not how the tax rate affects changes in operating cost). We further interact the control variables with the change in sales because we want to rule out that they are confounders of the aforementioned relation. This yields our baseline specification:

$$\begin{aligned}\Delta OpCost_{it} = \alpha &+ (\beta_1 + \boldsymbol{\beta_2}\,\tau_{ct} + \beta_3\,\Delta GDP_{ct} + \beta_4\,Unemp_{ct} + \beta_5\,AInt_{it})\,\Delta Sales_{it} \\ &+ (\gamma_1 + \boldsymbol{\gamma_2}\,\tau_{ct} + \gamma_3\,\Delta GDP_{ct} + \gamma_4\,Unemp_{ct} + \gamma_5\,AInt_{it})\,SDEC_{it} \times \Delta Sales_{it} \\ &+ \chi\,\tau_{ct} + \delta_t + \lambda_c + \varepsilon_{it}\end{aligned} \quad (5)$$

where $\Delta OpCost_{it}$ ($\Delta Sales_{it}$) is the change in operating costs (in sales) for firm $i$ located in country $c$ in year $t$, scaled by lagged total assets, $TA_{it-1}$. The variable $\tau_{ct}$ is the statutory corporate tax rate for country $c$ in year $t$. The dummy variable $SDEC_{it}$ takes on the value of one if and only if the sales in year $t$ are lower than in year $t$ - 1. Following prior literature (e.g., Anderson et al., 2003, Banker et al., 2013, Kama and Weiss, 2013, Cannon, 2014), we do not include $SDEC_{it}$, but only its interactions with $\Delta Sales_{it}$ as well as with $SDEC_{it} \times \Delta Sales_{it}$.[7] However, as we are agnostic whether there is a direct effect of the tax rate on changes in operating cost (independent

[7] We estimate two local piecewise cost functions to the left and right, respectively, of $SDEC_{it}$. Following the literature on cost stickiness, we do not include a main effect for $SDEC_{it}$, since this would allow a jump discontinuity between these two pieces of the cost function and we do not have a theory for such a jump discontinuity. For the same reason, we do not include the interaction $SDEC_{it} \times \tau_{ct}$. As a robustness test, we add the main effects $SDEC_{it}$ and $SDEC_{it} \times \tau_{ct}$ (Section 3.3.2). The results are close to our reported results.

of the changes in sales) that has to be controlled for, we include a main effect for the tax rate ($\chi$). Our expectation is that, for higher tax rates, firm operating costs respond more strongly to changes in sales. That is, we expect that $\beta_2 > 0$ (H1). The coefficient $\gamma_2$ tests H2. We expect that $\gamma_2 < 0$ because, as predicted by H2, tax planning is expected to respond asymmetrically to sales decreases and increases, reflecting an asymmetric effect of corporate taxes on operating cost.

We include country-level variables such as GDP growth and unemployment because managerial expectations for future sales have been shown to affect firm-level cost behavior (Banker and Byzalov 2014), and these proxies for managerial expectations may be associated with tax rates. As we are interested in how taxes change the relation between changes in sales and changes in cost, we control for the interaction of changes in sales and these two variables (GDP growth ($\Delta GDP_{ct}$) and unemployment ($Unemp_{ct}$)). In addition, following earlier literature (Anderson et al. 2003), we control for asset intensity ($AInt_{it}$).[8] As we use the control variables in interactions, we standardize all control variables to have a mean of zero in order to ease the interpretation of the results.

Since we are interested in the effect of the tax rate (and not of tax rate changes) on operating cost responsiveness, we use the level and not the first difference of corporate tax rates. The level of the tax rate could capture country effects (as taxes are higher in larger countries) as well as year effects (decreasing tax rates due to tax competition). We thus include year fixed effects $\delta_t$ and country fixed effects $\lambda_c$ that capture time trends and country-level trends. In a robustness test, we further restrict the identification to a changes interaction (changes in sales × changes in tax rates. We do not use firm fixed effects because the first-difference approach already controls for unobserved firm level heterogeneity.[9] We cluster standard errors $\varepsilon_{it}$ at the country–industry level because we suspect similarities in cost functions among industries and because we exploit country-level variation (tax rates) as well as firm-/industry-level variation (changes in sales).[10]

### *2.5 Scaling*

In order to be comparable across firms, changes in cost and sales have to be scaled. Banker and Byzalov (2014) propose several methods of scaling changes in sales and operating costs.

[8] We define $AInt_{it}$ as $-Sales_{it}/(TotalAssets_{it-1}+1)$. We add 1 in the denominator because, albeit we dropped firms with average total assets below 25 T€, in some firm years we observe total assets close to 0.

[9] As a robustness test, we replace the country fixed effects by firm fixed effects, which then cover unobserved firm-level time trends. Alternatively, we interact the year fixed effects with the positive and negative changes in sales to capture time trends in the marginal cost. In both specifications, the results hold qualitatively and are close to our reported results.

[10] Our results hold qualitatively if we cluster at the country or industry level (results not tabulated). Additionally, we cluster standard errors along both dimensions (country and industry, results not tabulated).

They recommend a log changes approach ($\Delta\ln X_{it} = \ln X_{it} - \ln X_{it-1}$). Alternatively, they suggest scaling by size. Anderson et al. (2003) report that their results are qualitatively similar to the log change specification for all their models when they estimate them with linear specifications. Shust and Weiss (2014) employ a linear model as well as a log change model. Linear models are used by e.g. Banker et al. (2016) and Hoffmann et al. (2019).

Our main reason to prefer a linear model with size variables (lagged assets or sales) as scaling variable to the log changes approach is that estimating the log changes specification is affected by the levels of the lagged cost/sales ratio (Balakrishnan et al. 2014). Intuitively, the log changes approach measures how the percentage change in costs is associated with a percentage change in sales. For a €100 change in sales, this association depends on the € change in costs as well as on the pre-change level of sales and cost. If, for example, the level of costs is very low, then even a small absolute change in cost can show up in a large percentage change. As our hypotheses imply that taxes—through their effect on marginal cost—will also affect the cost-sales ratio, applying the change log specification in our setting implies that we effectively control for a common outcome, resulting in bad control bias (Angrist and Pischke 2009).

In addition, we perform a Davidson-MacKinnon (1981) J-test. For our sample, the test rejects the hypothesis that scaling changes with lagged total assets or lagged sales is nested in the log changes model. Moreover, the adjusted $R^2$ of the model that scales changes with total assets is considerably higher relative to the log changes model. We thus use lagged total assets ($TA_{it-1}$) as the scaling variable for sales and costs in our preferred specification. In alternative specifications, we use also lagged sales as a scaling variable and the (potentially biased) approach using changes in the logarithmic values of sales and costs (log change approach).

### *2.6 Data*

We start with all active corporations in Amadeus with available accounts during 2006–2016. These data comprise listed and unlisted corporations. We exclude banks and insurance companies (NACE, Rev. 2 codes 64–66), since they display different cost structures and are subject to different tax regulations. We use three steps to arrive at our baseline sample to ensure that our sample firms are standalone corporations subject to the domestic statutory corporate tax rate and without opportunities to shift profits across countries. These firms closely reflect our theory. First, we use accounts from companies with unconsolidated accounts only (Amadeus account type U1). With this step, we also drop nearly every publicly traded firm. If a company also publishes consolidated accounts, it controls a group and we drop these firms. The tax incentives of such a group are more difficult to measure, since different group entities could be subject to different tax rates. In addition, unconsolidated accounts are usually characterized by higher book–tax conformity, that is, they are closer to the corporate tax base than consolidated accounts. Second, we discard dependent firms (with a Bureau van Dijk Independence Indicator of C, D, or U) if their global ultimate owner is an industrial company. We assume that these firms have incentives and opportunities to shift profits across countries. Third, we exclude firms that have affiliates (i.e., subsidiaries) in other countries (and were not already discarded in step 1). We further exclude firms that are missing or negative operating cost or sales data for the full period or missing their industry classification. Furthermore, we exclude micro firms (average total assets below €25,000).

Banker and Byzalov (2014) argue and demonstrate that outlier treatment is especially important in empirically measuring cost structures.[11] We acknowledge that the cost stickiness literature has not provided generally accepted guidelines for these steps. On the one hand, we do not want outliers that are most likely not tax driven to affect our results. On the other hand, truncation and winsorization are subjective. In a first step, following Banker and Byzalov (2014), we thus drop observations with sales below EBIT, with a cost-to-sales ratio (OpCost/Sales) larger than 1,000% or below 10%. Banker and Byzalov (2014) drop the lower and upper 1% of the $\Delta\ln(OpCost)$ and $\Delta\ln(Sales)$ distributions. We use different scalars ($\Delta OpCost_t/TA_{t\text{-}1}$ and $\Delta OpCost_t/Sales_{t\text{-}1}$) as well as a log specification ($\Delta\ln(OpCost_t)$). We see winsorizing as helpful in using the three different specifications in the same data. Thus, we drop only the lower and upper 0.5% of the $\Delta OpCost_{it}/TA_{it\text{-}1}$ and $\Delta Sales_{it}/TA_{it\text{-}1}$ distributions and then winsorize all firm-level change variables at the 0.5% and 99.5% levels. Furthermore, if the change in sales is close to zero, even small changes in operating cost can have a large and in our view misleading effect on the estimation. For this reason, we discard observations where the absolute value of $\Delta OpCost$ is more than 500% of the absolute value of $\Delta Sales$.[12] In the Online Appendix, we show that alternative outlier treatments do not affect our main inferences (see Table A.1). Furthermore, since tax incentives have been shown to differ between profitable firms and loss firms (e.g., Maydew 1997; De Simone et al. 2017; Hopland et al. 2018), we exclude observations with negative earnings before taxes ($EBT_{t\text{-}1} < 0$) from our baseline sample. In our cross-sectional tests, we compare our sample with the excluded loss firm–years.

After these screens, we end up with 3,384,989 firm–year observations from 472,445 firms over the period 2006–2016. The firms are located in 36 different countries. To address concerns that some countries (France, Italy, Spain) are overrepresented in Amadeus, we demonstrate in

[11] We drop one exceptional extreme outlier observation: a small Italian firm with total assets of approximately €1 million that has EBIT of -€340,033,946,000 (given an EBT of €29) in 2008. In 2009, EBIT for this firm is €52,081.
[12] Contrary to Weiss (2010), we do not discard all observations with ΔOpCost/ΔSales<0 in our main test. When we discard this subsample (about 10% of observations), our inferences remain nearly unchanged.

our robustness section that our results hold if we subsequently remove different countries from the sample or collapse the data so that our inferences are based on country–year-level data. All data are in thousands of euros. We adjust all data for inflation using each country's Consumer Price Index and deflate to 2006 values.

Our data do not contain operating costs directly. Instead, for the typical firm, Amadeus provides different data, depending on whether the firm uses the cost of goods sold method (sales – cost of goods sold) or the total cost method (sales +/– changes in inventory + company-produced additions to plant and equipment – total cost of production). For the total cost method, Amadeus defines turnover as net sales + other operating revenues +/– stock variations (changes in inventory; see the Appendix). One possible empirical approach would be to regress changes in "full operating costs" (operating costs of turnover) on changes in turnover. Although this approach would more closely measure production activities (output) than using sales as the independent variable, it has several downsides. First, the results could depend on which valuation rules are applied to stock variations. The discretion in the valuation of inventories is associated with tax incentives (Badertscher et al. 2019), which would potentially bias our results. Second, the results are only available for firms that prepare their income statements according to the total cost method. For firms that prepare their income statements according to the cost of sales method, turnover is not available. Third, the data from Amadeus do not allow other operating revenues to be separated from stock variations, both of which are included in turnover. For these reasons, we rely on sales and on the operating cost of sales. We measure operating cost as $OpCost_{it} = Sales_{it} - EBIT_{it}$. We define a sales decrease dummy $SDEC_{it}$ that equals one if $Sales_{it} < Sales_{it\text{-}1}$, and zero otherwise. The Appendix presents the Amadeus variables used to define $OpCost_{it}$.

We also compile a panel of statutory corporate tax rates and data on country-level control variables (GDP growth, unemployment). The tax rate variables are obtained from the OECD, the World Bank, yearly tax guides published by Ernst & Young, KPMG, and Spengel et al. (2016). Control variables are from the European Commission, the OECD, and the World Bank.

Table 1 reports descriptive statistics. In about half of the firm–years, we observe sales decreases. The main explanation for this high number is that, as mentioned above, we deflate all data with country-individual inflation rates so that we compare deflated sales with last-year sales. In other words, following the literature on cost stickiness, we define sales decreases as decreases in real sales. Further, the firms in our sample are relatively small, with total assets of €8.233 million, sales of €7.808 million, and earnings before taxes (EBT) of €0.471 million on average. This is not surprising, since we eliminated firms with group accounts and multinational enterprises (MNEs, i.e., firms with foreign subsidiaries or foreign industrial owners) and these statistics reflect the samples of other papers using Amadeus data (e.g., Bethmann et al. 2018). Consistent with prior research on unlisted firms, the distributions of total assets, sales, operating cost, EBIT, and EBT are strongly skewed. We also note that the majority of firms (80.8%) use the total cost method for their profit and loss statements.

Table 2 shows that the panel is unbalanced (with an average of 7.1 observations per firm). Amadeus' coverage varies strongly among countries. Again, both results are consistent with prior literature but, as mentioned above and shown in robustness tests below, the difference in coverage does not drive our results. Importantly, Table 2 documents 77 tax rate changes for our sample period, with an average corporate tax rate of 27% (Table 1). Since our identification exploits tax rate changes, there seems to be sufficient variation in our sample.

# 3 Results

### *3.1 Preliminary Analysis: Is There Cost Stickiness in Our Data?*

In order to benchmark cost behavior in our sample relative to prior literature, we first run equation (1) without tax variables to measure the slope of cost changes for positive and negative sales changes. We control for size, changes in GDP, unemployment levels, inflation, year fixed effects, and country fixed effects. Standard errors are clustered on country-industry level where industry is defined as the 3-digit NACE code. **Table 3** presents the regression results from this non-tax model. In specification (1) and (2), we regress size-scaled changes in operating cost on size-scaled changes in sales, separated by sales increases and sales decreases using a dummy for sales decreases *SDEC*. We scale with $TA_{it-1}$ in specification (1) and with $Sales_{it-1}$ in specification (2). Specification (3) employs changes in the natural logarithm of costs and sales, respectively. In our sample, specifications (1) and (2) exhibit a small positive coefficient on the interaction term $SDEC_{it} \times \Delta Sales_{it}$, whereas the coefficient estimate in specification (3) is small negative, but statistically insignificant. We cautiously interpret this result as additional evidence for Balakrishnan et al. (2014) who show that general cost stickiness estimates may depend on the empirical specification. We further note that cost responsiveness is quantitatively higher and less sticky than in other samples (e.g., Banker and Byzalov 2014).[13] The difference may be due to our firms being smaller, domestic, under less capital market pressure, and often closely held by a single owner or a family. We conclude that our large sample of European private

[13] However, analyzing clinics, Balakrishnan et al. (2004) find stickiness for staffed hours and cost only if the clinic's resources are strained, but not when when the clinic is operating at "normal" utilization levels. In Weiss (2010), nearly half of the observations display anti-sticky cost behavior. Abudy and Shust (2019) find that family firms exhibit anti-sticky SG&A cost behavior on average.

firms is not directly comparable to public U.S. firms that have often been analyzed in cost behavior research.

### *3.2 Tax Rates and Cost Behavior*

We now turn to our main analysis of equation (1) and the role of corporate taxes. The results are reported in Table 4. We use changes in operating cost and in sales that are scaled by $TA_{it\text{-}1}$ in specification (1) and by $Sales_{it\text{-}1}$ in specification (2). Specification (3) employs logarithmic changes. In our preferred specification (1), consistent with H1, we find a positive coefficient estimate for the interaction term $\tau_{ct} \times \Delta Sales_{it}$. This result suggests that, as tax rates increase, reported operating costs respond more strongly to a given increase in reported sales. This finding is consistent with increases in sales providing the opportunity and tax rates setting the incentive for additional tax planning.

The coefficient on $\tau_{ct} \times SDEC_{it} \times \Delta Sales_{it}$ is negative, supporting H2. Higher tax rates are associated with greater differences between cost responsiveness for decreases versus increases in sales. The negative sign shows that for sales decreases the effect of taxes on cost responsiveness is weaker than for sales increases. Both coefficient estimates are significant at the 1% level. The overall tax effect on the slope for sales decreases is $\beta_2+\gamma_2$, the sum of the coefficient estimates for $\tau_{ct} \times \Delta Sales_{it}$ and $\tau_{ct} \times SDEC_{it} \times \Delta Sales_{it}$. The sum of the coefficient estimates amounts to 0.014 but is not jointly significant (p= 0.688, not tabulated). This result indicates that, in the case of sales decreases, there is no tax effect on operating cost responsiveness.

We continue to find empirical support for both hypotheses when using lagged sales to scale current costs and sales. Specification (2) displays a positive coefficient estimate for the interaction term $\tau_{ct} \times \Delta Sales_{it}$ and a negative coefficient estimate on $\tau_{ct} \times SDEC_{it} \times \Delta Sales_{it}$. The estimate for the overall tax effect on the slope for sales decreases ($\tau_{ct} \times \Delta Sales_{it}$ + $\tau_{ct} \times SDEC_{it} \times \Delta Sales_{it}$) is 0.0564; the joint coefficient estimate is again insignificant (p = 0.963, not tabulated). Collectively, columns (1) and (2) of Table 4 support H1 and H2.

To provide an easily understandable, visual interpretation of the economic magnitudes of our results, we plot the conditional sample means for EBIT change ($\Delta Sales - \Delta OpCost$, scaled by $TA_{it\text{-}1}$) on the sales change (scaled by $TA_{it\text{-}1}$) using the coefficient estimates from specification (1) in Figure 1, Panel A. For sales increases (x-axis values above zero), a higher tax rate is associated with higher operating cost responsiveness, which results in lower EBIT (y-axis). For sales decreases (x-axis values below zero), the tax rate has no significant effect on EBIT. These results are consistent with H1 and H2: For increases in sales, firms have an incentive to engage in tax planning to attenuate additional taxable income. This incentive increases with the tax rate. For sales decreases, the effect is much smaller and nearly invisible. The difference between increases and decreases in sales reflects the asymmetry in the tax effect on operating cost.

The magnitude of this stickiness depends on the level of the corporate tax rate. In economic terms, the effects are also reasonable. Suppose a firm's sales increase by 17.8% of total assets (which is the median sales increase in our subsample of firms with increasing sales). For a tax rate of 20% (30%), operating cost will on average increase by 15.4% (15.8%) of total assets.[14] The residual, pre-tax earnings (EBIT), will increase by 2.35% (1.95%) of total assets.[15] Compared to a tax rate of 20%, the 30% tax rate is associated with a reduction in profitability growth of (2.35% - 1.95%)/2.35% = 16.8% (due to an increase in operating costs of (15.8% - 15.4%)/15.4% = 2.57%).[16] For decreases in sales, there is no significant corresponding difference in profitability across tax rate levels.

If we scale the change in sales and in operating cost by lagged sales, the picture looks quite similar to that for increases in sales (see, Panel B, Figure 1). For decreases in sales, higher tax rates are associated with lower cost decreases (leading to, ceteris paribus, higher EBIT). This finding would be consistent with some reduction in tax planning for decreases in sales (albeit less than the increase for increases in sales). Despite the differences across scales—lagged sales versus lagged total assets—the results support our two hypotheses.

Finally, in specification (3) of Table 4, we regress the change in the logarithm of operating costs on the change in the logarithm of sales. The coefficient estimate for the interaction term $\tau_{ct} \times \Delta Sales_{it}$ is positive and highly significant. However, the coefficient estimate on $\tau_{ct} \times SDEC_{it} \times \Delta Sales_{it}$ is very small (albeit negative, as expected) but not significant. We interpret this finding as additional support for H1, whereas the overall picture for H2 is somewhat less clear. Given a potential bias in the log–log specification (see section 2.5), we put more weight on the results in specifications (1) and (2). In these specifications that are not subject to the Balakrishnan et al. (2014) critique, we find strong support for H1 and H2.

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[14] For a tax rate of 20%, we get: 0.1775×(0.823+0.2×0.223) = 15.4%. For a tax rate of 30%, we obtain: 0.1775×(0.823+0.3×0.223) = 15.8%. For the coefficients employed, see Table 4 Panel A, specification (1).

[15] Tax rate of 20%: 17.75% –15.4% = 2.35%. Tax rate of 30%: 17.75% –15.8% = 1.95%.

[16] These tests also rule out the alternative explanation that taxes insure corporations against negative outcomes. The seminal paper of Domar and Musgrave (1944) shows that under a full loss offset, an investor will increase his or her risk exposure (the share of the risky investment in the portfolio) to compensate for the reduction in yield. Taxes reduce positive as well as negative outcomes proportionally and work as insurance. For a firm, a production strategy with higher fixed costs (or higher resource adjustment costs) and lower marginal costs will be riskier, since adjustments to unforeseen sales decreases will be more difficult. Therefore, taxes could incentivize firms to incur higher fixed costs and lower marginal costs. Our results are inconsistent with this explanation as one would obtain a *negative* association of tax rates and operating cost responsiveness (i.e., lower marginal costs).

### 3.3 *Robustness and Sensitivity Tests*

In this section, we present a battery of robustness and sensitivity tests for our two hypotheses. These tests deal with the parallel trends assumption underlying our empirical approach (Section 3.3.1), potential omitted control variables and alternative explanations (Section 3.3.2), alternative measures of cost stickiness (Section 3.3.3), alternative specifications (Section 3.3.4), the sensitivity to the sample composition (Section 3.3.5), and intertemporal profit shifting as an alternative explanation for our findings (Section 3.3.6).

#### 3.3.1 Parallel Trends Assumption

One potential concern about our approach is that the tax rates are not randomly set.[17] Tax rates and operating costs behavior can depend on unobserved economic conditions. To mitigate concerns about confounding events and differences in trends prior to tax changes, we investigate whether the common trends assumption is violated. We therefore augment equation (1) and substitute the current tax rate with three leads corporate tax rates (see, also, Heider and Ljungqvist 2015). We scale the sales change and the change in operating cost with $TA_{it\text{-}1}$. Figure 2, Panel A (Panel B) plots the coefficient estimates for $\Delta Sales_{it}$ ($SDEC_{it} \times \Delta Sales_{it}$) interacted with the tax rate one, two, and three years, respectively, before the tax rate change.[18] We find that none of the interactions of $\Delta Sales_{it}$ with any of the three lead tax rates is significant. For all three coefficient estimates, we find that the 95% confidence bounds overlap with zero (Panel A, Figure 2). The same is true for interactions of lead tax rates with $SDEC_{it} \times \Delta Sales_{it}$. None of the interactions is significant at the 5% level. We interpret these results as confirmation for the common trends assumption.

#### 3.3.2 Additional Controls, Alternative Explanations, and the Underlying Assumptions

In the second step, we examine whether our results are sensitive to adding different sets of controls. First, the vast majority of the cost stickiness literature does not control for the main effect of the SDEC dummy (e.g. Andersen et al., 2003, Banker et al. 2013, Kama and Weiss, 2013, Cannon 2014). Banker et al. (2018) is a recent exception. Conceptually, controlling for SDEC would allow the cost function to "jump" at zero sales changes. While we do not have a theory for this jump, forcing the data into a specification without a stand-alone SDEC may lead into a misspecification. For this reason, we have added SDEC and the interaction of SDEC with

[17] We also run a randomization test where we randomly attribute to each country–year a tax rate from a different country–year cell (see, also Guenther and Young (2000), Bertrand et al. (2004), Kotakori and Poutvaara (2011), Hanlon et al. (2013) or Jacob et al. (2019) for randomization tests or pseudo-event analyses), We then use this random tax rate and rerun equation (1). We display the resulting coefficient estimates density function in the Online Appendix (Figure A.1). They are distributed evenly around 0.

[18] We obtain similar results when we scale by lagged sales.

τ to the estimation equation. Results are reported in columns (1) to (3) in the Table 5. Our results are qualitatively similar to our baseline findings.

Second, we address concerns about using firm-level sales. In using sales as a proxy for activity, we apply the common empirical approach in management accounting cost research. This approach raises two concerns. First, sales are a noisy proxy for activity (Weiss 2010; Banker and Byzalov 2014; Cannon 2014). Second, sales are endogenous. Sales as well as operating costs could be affected by industry-level demand and price trends. To address these issues, we control for country–industry–year sales (excluding the firm in question). It can be argued that country–industry–year sales proxy for demand and are likely exogenous to the single firm. We estimate industry demand based on all firms in our sample (including dependent firms and firms with foreign subsidiaries) at the 3-digit NACE code.[19] We then include changes in country–industry–year sales as a control for industry-level trends in our regressions. We interact changes in country–industry–year sales with the sales decrease dummy $SDEC_{it}$ to cover different effects of industry-level trends on operating costs for firms with decreasing sales compared to firms with increasing sales. The results in columns (4) to (6) in Table 5 show that the findings related to $\tau_{ct} \times \Delta Sales_{it}$ as well as to $\tau_{ct} \times SDEC_{it} \times \Delta Sales_{it}$ are virtually the same when we compare the coefficients to those in Table 4.

Third, we address concerns about a potential alternative explanation. Banker et al. (2013) identify employment protection strictness as a driver for cost stickiness. Since employment protection laws (EPL) can plausibly drive firms' tax avoidance decisions (De Vito et al., 2019), it is possible that EPL are a confounder in our analysis. Following prior literature (see, e.g., De Vito et al. 2019), we use a time-varying index of employment law strictness, provided by the ILO on a country-year level. One drawback of this analysis is that the coverage in time is limited, and some countries (e.g. Poland, Croatia) are not covered at all. Therefore, we lose about one third of our observations when we employ EPL as an additional interacted control variable. In this robustness test (see, Table A.4 in the Online Appendix), our results remain qualitatively unchanged. We continue to find a strong effect of taxes on cost behavior (H1) and evidence of the asymmetry (H2).

Finally, we test two important underlying assumptions. We implicitly assume that increases in sales are positively associated with increases in taxable income. Using book pretax income as a proxy for taxable income, we find a positive relation between changes in sales and

[19] We drop country–industries in which any firm has a market share above 50%, since the other firms' industry sales would be strongly driven by the firm in question, creating endogeneity issues (e.g., if the firm in question gains market shares at the expense of the other firms). We also discard firms with $2 \times sales < operating\ cost$.

changes in taxable income, even when controlling for firm and country controls as well as fixed effects (country and year fixed effects or firm and year fixed effects to allow for firm-specific trends). Second, we more directly test for tax planning by replacing operating costs with tax expense. We scale tax expense by sales (*Tax-to-Sales*) to relate tax planning to the actual output (i.e., sales). We find that, consistent with tax planning (at least partly) explaining the relation between corporate taxes and cost behavior, increases in corporate taxes increase tax planning if sales increase (lower *Tax-to-Sales*) but that there is an asymmetry when sales decline (no effect of corporate taxes on *Tax-to-Sales*). These results are consistent with our main findings. One major caveat is that financial reporting of the tax item differs across countries and that this variable is less populated. Results are reported in Table A.5 of the Online Appendix.

#### 3.3.3 Alternative Measure for Cost Stickiness

We next examine whether our result on the role of taxes in cost stickiness (H2( is robust to using an alternative proxy for cost stickiness. Specifically, we test whether our results hold if we use the Weiss (2010) measure of cost stickiness, denoted *STICKY*. *STICKY* is defined as the difference in cost function slopes between upward and downward changes in sales. Technically, it is measured as the difference between the rate of cost decrease for the most recent period with decreasing sales ($\underline{x}$) out of the last four periods and the corresponding rate of cost increase for the most recent period with increasing sales ($\overline{x}$) out of the last four periods:

$$STICKY_{i,t} = ln\left(\frac{\Delta OpCost}{\Delta Sales}\right)_{i,\underline{x}} - ln\left(\frac{\Delta OpCost}{\Delta Sales}\right)_{i,\overline{x}}, \underline{x}, \overline{x} \in \{t,...,t-3\} \quad (6)$$

As Weiss (2010), we discard observations with ΔOpCost/ΔSales<0. Weiss (2010) defines quarters as periods. As we do not have quarterly data but annual data. Hence, we define years as periods. We demand positive EBT in each year of the measurement period of four years. The *STICKY* measure in our sample has a mean of -0.015 and a median of -0.007 which indicates less stickiness than in the Weiss (2010) sample. We then regress *STICKY* on the corporate tax rate and year fixed effects. As lower values of *STICKY* indicate more stickiness, we expect a negative association between *STICKY* and the tax rate. Table 6 displays the results. In the baseline regression (1) without any restriction or controls, we lose 59% of our observations due to the long time period necessary for estimating *STICKY* on the firm level. In specification (2), we restrict the measurement of *STICKY* to firms that did not experience a tax rate change in the last four years. We lose 85% of our observations. In specification (3), we winsorize the *STICKY* at the 1% and the 99% levels. In specification (4), we add country fixed effects. In specification (5), we account for measuring *STICKY* repeatedly at the firm level. We drop all observations for which we have $STICKY_{i,t-1}$ or $STICKY_{i,t-2}$. All results show that *STICKY* is negatively (i.e. cost stickiness is positively) associated with the tax rate.

3.3.4 Alternative specifications

In this section, we present three alternative specifications. First, we use a model that restricts the identification to the interaction of changes in sales with tax rate changes. Second, we employ a hierarchical model that estimates average cost responsiveness on the country-year level without taxes (first stage) and then regresses these estimates on the tax rate (second stage). Third, we test various cutoffs for sales decreases.

One concern is that unobserved country-specific variation affects changes in operating costs as well as tax rate *levels*. We thus derive a pure first-difference model that only exploits the interaction of variations in sales with changes in tax rates (Giesselmann and Schmidt-Catran 2018). To incorporate the path dependency from H2 in a fixed effects model that exploits deviation from the *average* and not from last year's values is possible but complex (see Allison 2019). As a parsimonious but equivalent alternative, we use a simple fixed effects model defined in levels as a starting point. If we—for a start—assume that taxes have a symmetric effect on operating cost responsiveness, an appropriate fixed effects model would be

$$OpCost_{it} = \alpha + (\beta_1 + \beta_2\ \tau_{ct} + \beta_3\ \Delta GDP_{ct} + \beta_4\ Unemp_{ct}) \times Sales_{it} + \chi_1\ \tau_{ct} + \gamma_i + \varepsilon_{it} \qquad (7)$$

The firm fixed effects $\gamma_i$ absorb unobserved time-invariant variation at the country, industry, and firm levels. This model only exploits variations in tax rates and/or sales; the level of $\tau_{ct} \times Sales_{it}$ does not contribute to the identification of $\beta_2$. First-differencing this model (see Wooldridge (2009), p. 457; Baltagi (2013), p. 17) yields

$$OpCost_{it} - OpCost_{it\text{-}1} = (\alpha - \alpha) + \beta_1 \, (Sales_{it} - Sales_{it\text{-}1}) \tag{8}$$
$$+ \boldsymbol{\beta_2} \, (\tau_{ct} \times Sales_{it} - \tau_{ct\text{-}1} \times Sales_{it\text{-}1})$$
$$+ \beta_3 \, (\Delta GDP_{ct} \times Sales_{it} - \Delta GDP_{ct\text{-}1} \times Sales_{it\text{-}1})$$
$$+ \beta_4 \, (Unemp_{ct} \times Sales_{it} - Unemp_{ct\text{-}1} \times Sales_{it\text{-}1})$$
$$+ \chi \, (\tau_{ct} - \tau_{ct\text{-}1}) + (\gamma_i - \gamma_i) + (\varepsilon_{it} - \varepsilon_{it\text{-}1})$$

$$\Delta OpCost_{it} = \beta_1 \, \Delta Sales_{it} + \boldsymbol{\beta_2} \, (\tau_{ct} \times Sales_{it} - \tau_{ct\text{-}1} \times Sales_{it\text{-}1})$$
$$+ \beta_3 \, (\Delta GDP_{ct} \times Sales_{it} - \Delta GDP_{ct\text{-}1} \times Sales_{it\text{-}1})$$
$$+ \beta_4 \, (Unemp_{ct} \times Sales_{it} - Unemp_{ct\text{-}1} \times Sales_{it\text{-}1})$$
$$+ \chi \, \Delta \tau_{ct} + (\varepsilon_{it} - \varepsilon_{it\text{-}1})$$

The resulting model thus already accounts for firm fixed effects (Allison 2019). However, $\boldsymbol{\beta_2}$ is identified by sole changes in tax rates, sole changes in sales, and simultaneous variation in sales and tax rates (Giesselmann and Schmidt-Catran 2018). As we are interested in the *interaction* of changes in tax rates and in sales, we decompose $\beta_3$ into three terms, of which the last is the term of interest: $(\tau_{ct} \times Sales_{it} - \tau_{ct\text{-}1} \times Sales_{it\text{-}1}) = \tau_{ct\text{-}1} \times \Delta Sales_i + \Delta \tau_c \times Sales_{it\text{-}1}$ $\boldsymbol{+ \Delta \tau_c \times \Delta Sales_i}$. Now, we can add the sales decrease asymmetry to this model by including the sales decrease indicator:

$$\Delta OpCost_{it} = \beta_1 \, \Delta Sales_{it} \tag{9}$$
$$+ \beta_{2a} \, \tau_{ct\text{-}1} \times \Delta Sales_i + \beta_{2b} \, \Delta \tau_c \times Sales_{it\text{-}1} + \boldsymbol{\beta_{2c} \, \Delta \tau_c \times \Delta Sales_i}$$
$$+ \beta_3 \, (\Delta GDP_{ct} \times Sales_{it} - \Delta GDP_{ct\text{-}1} \times Sales_{it\text{-}1})$$
$$+ \beta_4 \, (Unemp_{ct} \times Sales_{it} - Unemp_{ct\text{-}1} \times Sales_{it\text{-}1})$$
$$+ \gamma_0 \, SDEC_{it} + \gamma_1 \, SDEC_{it} \times \Delta Sales_{it}$$
$$+ \gamma_{2a} \, SDEC_{it} \times \tau_{ct\text{-}1} \times \Delta Sales_i + \gamma_{2b} \, SDEC_{it} \times \Delta \tau_c \times Sales_{it\text{-}1}$$
$$\boldsymbol{+ \gamma_{2c} \, SDEC_{it} \times \Delta \tau_c \times \Delta Sales_i}$$
$$+ \gamma_3 \, SDEC_{it} \times (\Delta GDP_{ct} \times Sales_{it} - \Delta GDP_{ct\text{-}1} \times Sales_{it\text{-}1})$$
$$+ \gamma_4 \, SDEC_{it} \times (Unemp_{ct} \times Sales_{it} - Unemp_{ct\text{-}1} \times Sales_{it\text{-}1})$$
$$+ \chi \, \Delta \tau_{ct} + (\varepsilon_{it} - \varepsilon_{it\text{-}1})$$

Equation (4) is thus a first-difference model that exploits the interaction of changes in sales with changes in tax rates to test H1 and H2. As prior literature (e.g., Heider and Ljungqvist 2015, Ljungqvist et al. 2018), we refrain from adding firm fixed effects to this specification. The results of the estimation of this model are shown in Table 7. The results qualitatively confirm our basic results. The coefficient $\beta_{2c}$ for the interaction of tax changes and sales increases is positive and significant (p-value < 5%) in each specification. The additional coefficient for the interaction of tax changes and decreases in sales is negative in each specification and highly significant in two out of three specifications. The overall effect for sales decreases ($\beta_{2c}+\gamma_{2c}$) does not show a clear picture (positive and significant in (1), negative insignificant in (2), negative and significant (p-value < 10%) in (3)). Overall, we find that (1) tax rates drive operational costs and (2) that this effect is asymmetric resulting in sticky costs.

Another issue is that firm panel data exhibit firm-level autocorrelation. As an alternative to clustering standard errors, Bertrand et al. (2004) recommend collapsing the data to a more aggregate level (the level at which the treatment is defined) and to analyze collapsed data. We thus collapse the observations into country–year cells and employ a hierarchical model (see Guenther 2018). First, for each country–year cell, we regress changes in operating costs on changes in sales $\Delta Sales_{it}$, $SDEC_{it} \times \Delta Sales_{it}$ (each scaled by $TA_{it-1}$) and on firm size (logarithm of $TA_{it-1}$). By estimating the coefficients separately for each country–year cell, the constants in these regressions absorb any country–year specific characteristics. Due to the reduced number of observations per country and year, we use robust regressions in this step. We fit the efficient high–breakdown point MM estimator (Yohai 1987), which results in 357 coefficient estimates for $\Delta Sales_{it}$ and 353 estimates for $SDEC_{it} \times \Delta Sales_{it}$ (36 countries × 10 years = 360 – 7 missing estimates because of missing data, mostly for Cyprus). In the second step of the hierarchical model, we regress the coefficient estimates for $\Delta Sales$ and for $SDEC \times \Delta Sales$ on the statutory tax rates of country $c$ in year $t$ ($\tau_{ct}$). Since the reliability of estimates varies greatly between country–year cells, we choose a weighted least squares (WLS) approach with the inverted standard errors of the first-step regression as weights. Graphical illustrations of the respective coefficient estimate (y-axis) and the corporate tax rate (x-axis) are presented in Figure 3, Panels A and B. All specifications show a robust pattern: the tax rate is positively related to operating costs for sales increases, as indicated by the higher country–year-specific coefficients on $\Delta Sales_{it}$. This finding is consistent with H1. The tax rate also increases cost stickiness, as indicated by the negative slope for the tax rate when using the $SDEC_{it} \times \Delta Sales_{it}$ coefficients as dependent variables, supporting H2. These results are robust to the inclusion of various controls and fixed effects as reported in Table A.2 in the Appendix.

We now test various cutoffs for decreases in sales. The literature on sticky costs compares firm–years with sales increases to firm–years with sales decreases. However, the cutoff point for the definition of $SDEC_{it}$ at exactly zero sales changes ($\Delta Sales_{it} = 0$) could seem arbitrary. We thus test whether our results are sensitive to changing the cutoff point by estimating our main specification for alternative cutoff points of sales change ranging from -25% to +25% relative to total assets. Since an inappropriate cutoff would allocate firms with heterogeneous reactions to one group, it would bias our coefficient estimates toward zero. In other words, if the coefficient estimates for alternative cutoff points are much larger in absolute terms than the coefficient estimate for the standard cutoff point (0% sales change), then a misspecification is likely. Panels A and B, Figure A.3 of the Appendix plot the resulting coefficients on $\tau \times \Delta Sales$ and $\tau \times SDEC \times \Delta Sales$, respectively. The largest coefficient estimates are around the interval [-5%, +5%], which corroborates the choice of 0% as the cutoff point for sales changes. This cutoff choice also follows prior literature and appears to also extend to the tax setting.

### 3.3.5 Addressing Concerns about Representativeness

Another potential concern about our approach is that some countries have substantially more observations than other countries. Due to different financial reporting requirements for private firms, the coverage of firms in Amadeus differs strongly across countries. To evaluate whether our findings are driven by a single country, we repeat our main regression (Table 4 Panel A, specification (1)) for a sample from which we exclude each of the seven countries with the highest numbers of observations, one at a time (Figure A.2 of the Appendix). The coefficient estimates for cost responsiveness to increased sales ($\tau \times \Delta Sales$) range between 0.1932 and 0.236 and are significant at the 1% level in each of the seven regressions. The coefficient estimates for the asymmetry in the tax effect on cost behavior ($\tau \times SDEC \times \Delta Sales$) are in a range between -0.172 and -0.220 and are significant at the 1% level in each of the seven regressions. We conclude that no single country drives our results.

### 3.3.6 Alternative Explanation: Intertemporal Shifting around Tax Reform Years

In the final step of our sensitivity analyses, we examine whether our results hold generally or whether they are simply driven by one-time intertemporal profit shifting incentives around tax rate changes. Firms have incentives to shift sales and costs around tax reform years so that their profits are shifted from high- to low-tax years (e.g., Guenther 1994; Andries et al. 2017; Dobbins et al. 2018). This profit shifting incentive could drive our results. In the year immediately before (after) a tax cut, firms could manipulate operating costs upward (downward) and sales downward (upward) to shift profits to a low-tax year. The reverse can be expected for tax increases. Indeed, Haga et al. (2019) have recently shown that cost behavior around corporate tax cuts is affected by intertemporal tax shifting considerations. We thus exclude the year immediately before (pre-reform year) and after (post-reform year) each tax rate change to establish that our results are stable and not only driven by intertemporal profit shifting. We also only look at pre- and post-reform years separately. Table A.3 of the Appendix compares the baseline estimation (specification (1)), the estimation without years immediately preceding or following a tax rate change, and the estimation for tax reform years only. Altogether, the results are robust. The coefficient estimates for increases in sales (H1) and stickiness in cost behavior (H2) are slightly larger for reform years. Overall, although intertemporal profit shifting could amplify the effect, it does not appear to explain our findings.

# 4 Cross-Sectional Analyses

In this section, we examine several cross-sectional differences in the effect of taxes on cost behavior. Evidence of a stronger effect of corporate taxes on cost behavior than expected in theory would further corroborate our causal interpretation of the results. We conduct three cross-sectional analysis. The first cross-sectional analysis aims to compare differences in tax avoidance incentives by contrasting loss-making and profitable firms, as well as firms with low and high tax expenses. Second, we compare standalone firms and firms belonging to a domestic or multinational group to examine differences in the importance of corporate taxes in firms' cost behavior. Third, we examine cross-firm differences in the ability to pass on taxes to stakeholders. As we measure the tax effect of corporate taxes net of tax incidence, we should observe higher tax incidence for firms with high ability to pass on taxes. In addition, firms that can pass on taxes to their stakeholders have lower incentives to avoid taxes (Dyreng et al. 2020), reducing the potential effect of taxes on cost behavior. Finally, we examine the role of implicit taxes (Jennings et al. 2012, Markle et al. 2020) in firms' cost behavior.

## *4.1 Tax Avoidance Incentives: Profitable Firms versus Loss Firms*

In our main regressions, we exclude loss observations, that is, firm–years with $EBT_{t-1} < 0$. We now re-include these observations and examine whether firms operate differently in these years relative to profitable years. Finding a difference in the tax effect on cost behavior between loss-making and profitable firm–years would further corroborate our interpretation of our baseline findings as supportive of H1 and H2. We predict that, in loss years, firms have very little incentive to engage in tax planning. Hence, we should find corporate taxes to have a significantly lower effect on cost behavior for loss firms than for profitable firms.

To test this prediction, we rerun equation (3) for loss observations (excluded from the main sample) and for profitable firm–years (main sample). The results are reported in Table 8. Column (1) presents the results for profitable firm–years and Column (2) presents the results for loss years. For both sets of firms, we find evidence that higher corporate taxes increase cost sensitivity to sales increases. The positive coefficients on $\tau_{ct} \times \Delta Sales_{it}$ are consistent with H1. Further, we continue to find evidence of an asymmetric tax effect, as indicated by the negative $\tau_{ct} \times SDEC_{it} \times \Delta Sales_{it}$ coefficients in both columns, supporting H2. As expected, we find a weaker tax effect on operating cost responsiveness, as in the sample of loss-making firms. Overall, the tax effect on cost responsiveness for sales increases for loss-making firm years is about half the effect for the sample of profitable firm–years. These differences are significant at the 1% level. We interpret these findings as consistent with loss firms having lower incentives to engage in tax planning, resulting in a smaller effect of corporate tax rates.

We note that using EBT is an imperfect way to proxy for loss firms (see also, Bethmann et al. 2018) but, due to the unavailability of tax return data in a cross-country panel, we need to assume an overlap between financial and taxable income. We believe that this is a reasonable assumption given that we use unconsolidated financial accounting statements of private firms. To test whether the measurement error from proxying loss firms by negative EBT is severe, we triangulate our results by using tax expenses. Firms with tax loss carryforwards, current tax losses, or tax-exempt operations for other reasons will have relatively low tax expenses (scaled by total assets). We assume that these firms have fewer incentives for tax planning. The results are reported in Columns (3) and (4) of Table 8 and support our predictions.

### *4.2 Standalone Firms versus Groups and Multinational Firms*

Second, we exploit differences in the effect of corporate taxes on cost behavior between standalone firms and firms belonging to domestic or multinational groups. Firms in larger groups have access to international tax planning strategies, whereas standalone firms are more likely to only use conforming tax planning. Further, multinational firms and group firms have potentially higher non-tax costs of conforming tax planning than standalone firms. Hence, we should find that corporate taxes have a larger impact on cost behavior among standalone firms than among firms belonging to groups who can also exploit more nonconforming tax planning tools. Using ownership information from Amadeus, we expand the sample by adding firms belonging to domestic or multinational groups. We then split this extended sample into standalone firms and group firms and estimate equation (3) separately for these two groups.

Columns (5) and (6) of Table 8 present regression results for the two groups respectively. We find empirical support for H1 and H2 in both groups. Consistent with our prediction, we find evidence that the effect of corporate taxes on the responsiveness of operating costs to increases in sales (H1) is greater for standalone firms than for firms that are part of domestic or multinational groups. The difference is significant at the 1% level. This finding is also consistent with tax planning in the operational business and nonconforming tax planning (as well as other forms of conforming tax planning) being substitutes. Admittedly, the interesting question whether different tax planning channels are substitutes is beyond the scope of our paper. However, we find that the asymmetry in the corporate tax effect on cost behavior does not differ across partitions.

### *4.3 Tax Incidence: Market Power*

The next cross-sectional analysis examines differences in the ability of firms to pass on corporate taxes to stakeholders. Prior literature shows that part of the corporate tax burden is shifted from shareholders to other stakeholders (e.g., Suárez Serrato and Zidar 2016; Fuest et al. 2018, Jacob et al. 2020). If firms can pass on the tax burden to other stakeholders because of their market power relative to their stakeholders, then firms will have fewer incentives to avoid taxes (Dyreng et al. 2020). Hence, they ultimately face a lower effective tax burden. As discussed in section 2.3, tax incidence falling on stakeholders works in the opposite direction of our tax-planning-based explanation. To test this empirically, we exploit cross-sectional differences in firm market power. For each firm, we calculate its market share in the respective country–industry–year combination. We sort firms into those with market shares above the median (*High Market Share*) and below the median (*Low Market Share*) and re-estimate equation (1). The argument is that a firm with a high market share can exert its market power to pass on taxes to its stakeholders such as customers, suppliers, and employees (see, e.g., the theoretical model in Dyreng et al. 2020).

We present the results in Columns (1) and (2) of Table 9. We find that, for firms with a lower market share, corporate taxes increase the responsiveness of operating costs to increases in sales (H1), as well as stickiness of operating tax planning (H2), as indicated by the positive coefficient on $\tau_{ct} \times \Delta Sales_{it}$ and the negative coefficient on $\tau_{ct} \times SDEC_{it} \times \Delta Sales_{it}$, respectively. For firms with high market shares, we also obtain a significant positive coefficient on $\tau_{ct} \times \Delta Sales_{it}$ and a significant negative coefficient on $\tau_{ct} \times SDEC_{it} \times \Delta Sales_{it}$. As discussed and predicted in Section 2.3, the magnitude of the coefficient estimates for increases in sales ($\tau_{ct} \times \Delta Sales_{it}$) is smaller for firms with higher market shares. That is, these firms experience a smaller effect of corporate taxes in cost behavior. F-Tests indicate that the coefficient estimates for increases in sales ($\tau_{ct} \times \Delta Sales_{it}$) are statistically different from each other at the 10% level (p-value = 5.2%). These results suggest that, as firms bear more of the corporate tax burden, they are more likely to engage tax planning (see, also, Dyreng et al. 2020) giving rise to the effect of corporate taxes on firms' cost behavior.

### *4.4 Implicit Taxes: Exploiting Differences in Asset Redeployability*

The final cross-sectional test examines the role of implicit taxes (Jennings et al. 2012, Markle et al. 2020). Implicit tax theory suggests a positive association between country-level tax rates and firm-level pre-tax returns, i.e., lower cost per unit of sales. In other words, the higher the country-level tax rate, the higher the sales or the lower the cost of a firm operating in this country. In our test, we focus on the cost channel and apply it to firm assets. Here, implicit tax theory suggests a negative association between country-level tax rates and the prices of assets with given revenues in this country, thereby reducing the costs (e.g., via lower depreciation).

To assess the extent to which implicit taxes drive our results, we sort industries into those with high versus low potential of being subject to implicit taxes due to a lack of a firm-specific measure. Specifically, we use the asset redeployability measure by Kim and Kung (2017). If assets can be more easily sold on secondary markets, implicit tax theory suggests that asset prices reflect implicit taxes. If, however, assets cannot be easily sold to other firms (low asset redeployability), implicit taxes will not be transmitted through the cost channel.[20] Using the Kim and Kung (2017) industry-level measure of asset redeployability, we split firms at the median asset redeployability into firm operating in industries with low versus high redeployability. Our results (Table 9, Columns (3) and (4)) indicate that the effect of taxes (1) on operating cost and (2) on cost stickiness is significant and similar across partitions with high versus low redeployability. These results suggest that implicit taxes are unlikely to drive the effect of taxes on cost behavior. One potential explanation for this finding is that our sample comprises private, unlisted corporations. If the assets employed by the respective firm are very firm-specific, implicit taxes may not affect asset prices and, hence, implicit taxes are not affecting the estimates in our sample.

# 5 Conclusion

This paper examines how firms' reported operating cost responsiveness to changes in sales is affected by corporate taxes. Using a large panel of European standalone firms and several tax rate changes, we find robust evidence that the responsiveness of reported operating costs to increases in reported sales is positively associated with tax rates. This finding is consistent with taxes affecting financial reporting decisions via tax planning. We also find robust evidence of an asymmetry in the operating cost response to corporate taxes: taxes affect firms' operating cost responsiveness stronger for increases in sales (that are associated with increases in the tax base) than for decreases in sales. We subject this result to a battery of robustness tests. We show that corporate taxes affect operating cost responsiveness and contribute to cost stickiness.

[20] Alternatively, implicit taxes might also transmit through the sales channel. In both cases, we would expect lower cost relative to sales for higher taxes.

Our paper has limitations in terms of generalizability given the focus on private firms. Our results might thus not be extrapolated to large listed firms. Our results still have implications for the literature, because we show that, absent cross-border profit shifting incentives, taxes affect firms' cost behavior. Our results could have implications for policymakers, since private firms contribute a very significant proportion to overall economic activities in OECD countries. Our results suggest that corporate taxes affect not only investment decisions and capital structure decisions, as shown in prior literature (e.g., Djankov et al. 2010; Heider and Ljungqvist 2015; Giroud and Rauh 2019), but also operating cost behavior and cost stickiness in firms. Our results thus also relate to the literature on the sticky cost phenomenon (see, e.g., Anderson et al., 2003, or the review by Banker and Byzalov, 2014). We show that corporate taxes affect cost behavior and cost stickiness. Hence, taxes could contribute to the explanation of cost stickiness. Tax planning appears to be one channel through which corporate taxes contribute to the sticky cost phenomenon.

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## Appendix

### Variable Definitions

| Firm-Level Variables | |
|---|---|
| $\Delta OpCost_{it}$ | Change in operating costs for firm *i* in year *t*, equal to $OpCost_{it} - OpCost_{it-1}$, scaled by either $TA_{t-1}$ (main specification) or $Sales_{t-1}$ (alternative specification) |
| $\Delta Sales_{it}$ | Change in sales for firm *i* in year *t*, equal to $Sales_{it} - Sales_{it-1}$, scaled by either $TA_{t-1}$ (main specification) or $Sales_{t-1}$ (alternative specification) |
| $\Delta EBIT_{it}$ | Change in EBIT for firm *i* in year *t*, equal to $EBIT_{t} - EBIT_{it-1}$, scaled by either $TA_{t-1}$ (main specification) or $Sales_{t-1}$ (alternative specification) |
| $SDEC_{it}$ | Sales decrease dummy for firm *i* in year *t*, equal to 1 if $Sales_{it} < Sales_{it-1}$, 0 otherwise |
| $TA_{it-1}$ | Total assets of firm *i* in year *t* - 1 |
| $AInt_{it}$ | Asset intensity, $= - Sales_{it}/(TA_{it-1}+1)$, standardized |

| Country-Level Variables | |
|---|---|
| $\tau_{ct}$ | Corporate tax rate in country *c* and year *t*, including surcharges and local taxes |
| $Inflation_{ct}$ | Inflation (consumer price change) in country *c* and year *t* |
| $\Delta GDP_{ct}$ | GDP growth in country *c* and year *t*, standardized |
| $Unemployment_{ct}$ | Unemployment in country *c* and year *t*, standardized |
| $EPLex_{ct}$ | Employment protection legislation index (IFO), values for blue collar workers and firms with more than 15 employees |

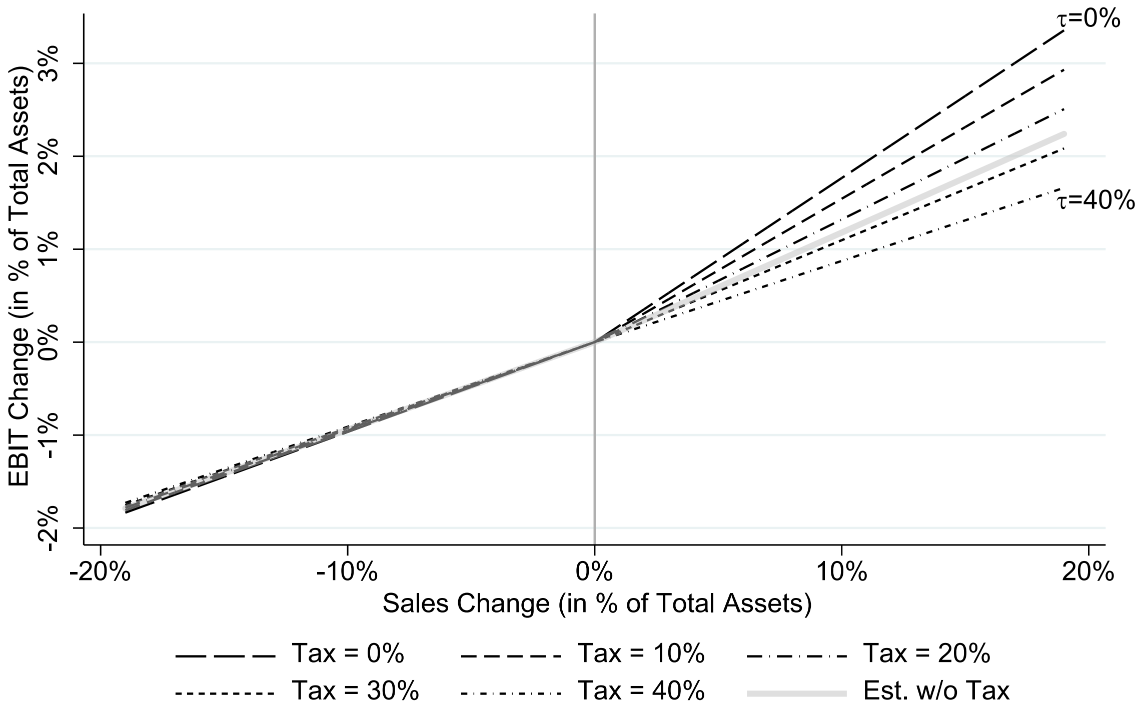
### Amadeus Data Sources

In this table, × stands for data available from Amadeus.

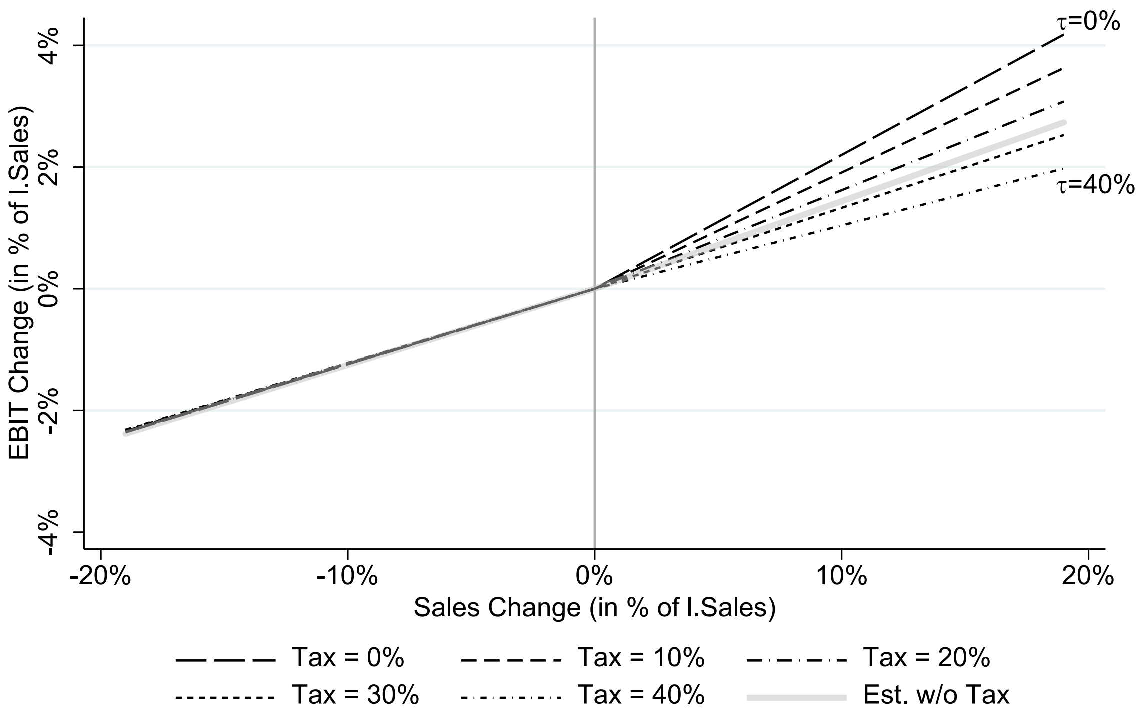
| Position | Definition | Firms that use the cost of goods sold method | Firms that use the total cost method |
|---|---|---|---|
| Sales | Net sales | × | × |
| Turnover | Net sales + other operating revenues + stock variations | | × |
| EBIT | All operating revenues - all operating expenses | × | × |

**Figure 1: Sample Average Sales Change vs. EBIT Change**

**Panel A: Sales Change vs. EBIT Change (% of Total Assets)**




**Panel B: Sample Average Sales Change vs. EBIT Change (% of Sales)**

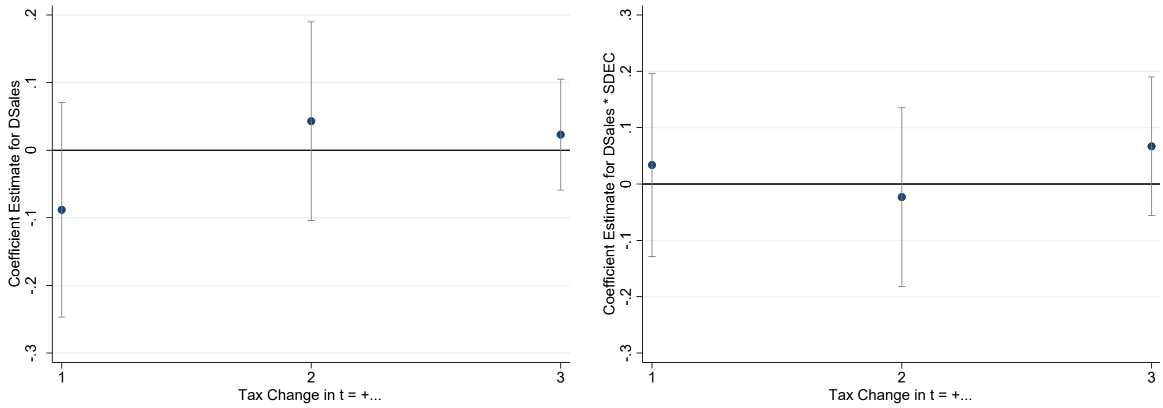


This figure plots the sales change (x-axis) and the associated average EBIT change (y-axis) for sales increases (sector right of zero sales changes) and sales decreases (sector left of zero sales changes). The variable $EBIT_{it}$ is $Sales_{it} - OpCost_{it}$. The slopes of the solid lines are calculated based on the regression $\Delta EBIT_{it} = \alpha + (\beta_1 + \beta_2\ \tau_{ct} + \beta_3\ \Delta GDP_{ct} + \beta_4\ Unemp_{ct} + \beta_5\ AInt_{it})\ \Delta Sales_{it} + (\gamma_1 + \gamma_2\ \tau_{ct} + \gamma_3\ \Delta GDP_{ct} + \gamma_4\ Unemp_{ct} + \gamma_5\ AInt_{it})\ SDEC_{it} \times \Delta Sales_{it} + \chi\ \tau_{ct} + \delta_t + \lambda_c + \varepsilon_{it}$. For sales increases, the slope is $\beta_1 + \beta_2\ \tau_{ct}$. For sales decreases, the slope is $\beta_1 + \beta_2\ \tau_{ct} + \gamma_1 + \gamma_2\ \tau_{ct}$. The slope of the grey continuous line (estimation without taxes) is calculated based on the regression $\Delta EBIT_{it} = \alpha + (\beta_1 + \beta_2\ \Delta GDP_{ct} + \beta_3\ Unemp_{ct} + \beta_4\ AInt_{it})\ \Delta Sales_{it} + (\gamma_1 + \gamma_2\ \Delta GDP_{ct} + \gamma_3\ Unemp_{ct} + \gamma_4\ AInt_{it})\ SDEC_{it} \times \Delta Sales_{it} + \delta_t + \lambda_c + \varepsilon_{it}$. For sales increases, the slope is $\beta_1$ (the control variables are standardized). For sales decreases, the slope is $\beta_1 + \gamma_1$. In Panel A, $\Delta EBIT_{it}$ and $\Delta Sales_{it}$ in the regression and the x- and y-axes in the figure are scaled by lagged total assets ($TA_{it-1}$). In Panel B, $\Delta EBIT_{it}$ and $\Delta Sales_{it}$ in the regression and the x- and y-axes in the figure are scaled by lagged sales ($Sales_{it-1}$).

## Figure 2: Pseudo Reforms: Coefficient Estimates for Future Tax Changes

Panel A: $\tau_{ct+x} \times \Delta Sales_{it}$ (Conforming Tax Planning)

Panel B: $\tau_{ct+x} \times SDEC_{it} \times \Delta Sales_{it}$ (Asymmetry in Conforming Tax Planning)

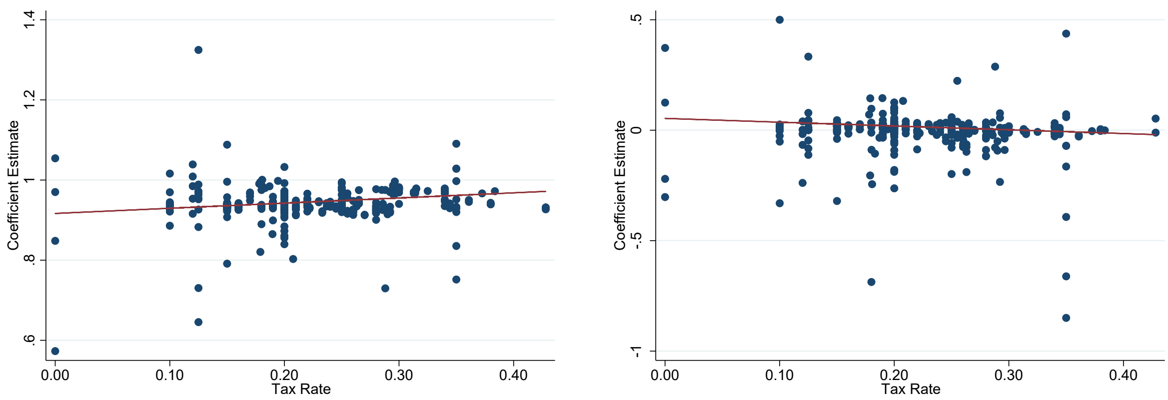



This figure shows the coefficient estimates for future tax changes. We calculate the coefficient estimates with the regression $\Delta OpCost_{it} = \alpha + (\beta_1 + \boldsymbol{\beta_2}\, \tau_{ct+x} + \beta_3\, \Delta GDP_{ct} + \beta_4\, Unemp_{ct} + \beta_5\, AInt_{it})\, \Delta Sales_{it} + (\gamma_1 + \boldsymbol{\gamma_2}\, \tau_{ct+x} + \gamma_3\, \Delta GDP_{ct} + \gamma_4\, Unemp_{ct} + \gamma_5\, AInt_{it})\, SDEC_{it} \times \Delta Sales_{it} + \chi\, \tau_{ct+x} + \delta_t + \lambda_c + \varepsilon_{it}$ with $x = 1, 2, 3$. We scale the sales change and the change in operating costs by $TA_{it\text{-}1}$. Panel A (Panel B) plots $\beta_2$ ($\gamma_2$), the coefficient estimate for $\Delta Sales_{it}$ ($SDEC_{it} \times \Delta Sales_{it}$) interacted with lead-terms of the corporate tax rate.

## Figure 3: Country–Year Cell Analysis

Panel A: Tax Rates versus Coefficient Estimates for $\Delta Sales_{it}$

Panel B: Tax Rates versus Coefficient Estimates for $SDEC_{it} \times \Delta Sales_{it}$ (Cost Stickiness)




This figure displays the associations between the corporate tax rate in a country–year (x-axis) and the coefficient estimates for sales increases (Panel A) and for cost stickiness as the difference between the slope for sales increases and decreases (Panel B) (y-axis). The coefficient estimates are obtained from the first-stage regression $\Delta OpCost_{it} = \alpha_{ct} + \beta_{1ct}\, \Delta Sales_{it} + \beta_{2ct}\, SDEC_{it} \times \Delta Sales_{it} + \beta_{3ct}\, \ln(TA_{it\text{-}1}) + \varepsilon_{ict}$ (with $\Delta OpCost$ and $\Delta Sales$ scaled by $TA_{it\text{-}1}$). We run this regression for each country and each year. To mitigate outlier effects due to low numbers of observations per country and year, we use robust regressions (efficient high–breakdown point MM estimator; see Yohai 1987). In Panel B, we suppress the country–year coefficient estimate for Malta in 2010 (-2.409936, N = 24). We consider this data point an outlier that would obfuscate the figure. Since we use WLS and weigh country–year coefficients with the inverse variance, the effect of this outlier in the regressions is negligible.

## Table 1: Summary Statistics

This table presents descriptive statistics of our main variables over 2006–2016. The variables are defined in the Appendix. All currency values are deflated with the 2006 values and are in thousand EUR.

| **Variables** | **N** | **Mean** | **Standard Deviation** | **p25** | **p50** | **p75** |
|---|---|---|---|---|---|---|
| *TA* | 3,384,989 | 8,233 | 178,969 | 613 | 1,490 | 3,731 |
| *Sales* | 3,384,989 | 7,808 | 102,299 | 845 | 1,755 | 4,193 |
| *SDEC* | 3,384,989 | 0.49 | 0.50 | 0.00 | 0.00 | 1.00 |
| *OpCost* | 3,384,989 | 7,340 | 98,167 | 766 | 1,634 | 3,924 |
| *EBIT* | 3,384,989 | 468 | 8,980 | 22 | 80 | 240 |
| *EBT* | 3,384,722 | 472 | 13,157 | 13 | 63 | 215 |
| $\tau_{ct}$ | 3,384,989 | 0.27 | 0.07 | 0.20 | 0.30 | 0.31 |
| $\Delta GDP$ | 3,384,989 | 0.72 | 3.22 | -1.00 | 1.00 | 2.40 |
| *Inflation* | 3,384,989 | 2.93 | 4.23 | 0.51 | 1.83 | 3.35 |
| *Unemployment* | 3,384,989 | 10.00 | 5.11 | 6.70 | 8.40 | 11.70 |
| *EPLex indicator* | 2,223,813 | 0.45 | 0.10 | 0.38 | 0.45 | 0.49 |

**Table 2: Firms, Observations, Corporate Tax Rates, and Tax Rate Changes by Country**

| Country Code | Country | # Firms | # Observations | Avg. $\tau_c$ | # $\tau_c$ changes ($\lvert\Delta\rvert \geq 0.5\%$) |
|---|---|---|---|---|---|
| AT | Austria | 1,219 | 6,434 | 0.250 | 0 |
| BA | Bosnia/Herzegovina | 1,661 | 11,928 | 0.275 | 6 |
| BE | Belgium | 3,556 | 23,344 | 0.342 | 1 |
| BG | Bulgaria | 12,179 | 86,371 | 0.105 | 1 |
| CH | Switzerland | 97 | 779 | 0.188 | 2 |
| CY | Cyprus | 7 | 45 | 0.109 | 1 |
| CZ | Czechia | 11,920 | 88,042 | 0.202 | 3 |
| DE | Germany | 5,112 | 33,799 | 0.312 | 1 |
| DK | Denmark | 4,646 | 9,740 | 0.247 | 3 |
| EE | Estonia | 2,722 | 19,154 | 0.211 | 3 |
| ES | Spain | 55,777 | 415,303 | 0.300 | 4 |
| FI | Finland | 6,402 | 44,011 | 0.241 | 2 |
| FR | France | 64,358 | 491,613 | 0.357 | 3 |
| GB | United Kingdom | 13,477 | 86,330 | 0.253 | 6 |
| GR | Greece | 5,556 | 35,539 | 0.253 | 5 |
| HR | Croatia | 2,518 | 19,780 | 0.200 | 0 |
| HU | Hungary | 502 | 3,727 | 0.191 | 2 |
| IE | Ireland | 157 | 936 | 0.125 | 0 |
| IS | Iceland | 157 | 796 | 0.185 | 3 |
| IT | Italy | 111,534 | 820,781 | 0.324 | 1 |
| LT | Lithuania | 1,831 | 12,673 | 0.161 | 4 |
| LU | Luxembourg | 256 | 1,215 | 0.291 | 1 |
| LV | Latvia | 4,557 | 25,107 | 0.150 | 0 |
| MD | Moldova | 183 | 976 | 0.082 | 2 |
| MT | Malta | 65 | 323 | 0.350 | 0 |
| NL | Netherlands | 456 | 2,527 | 0.256 | 2 |
| NO | Norway | 22,049 | 152,187 | 0.275 | 2 |
| PL | Poland | 9,897 | 77,164 | 0.190 | 0 |
| PT | Portugal | 21,272 | 163,191 | 0.287 | 4 |
| RO | Romania | 18,820 | 152,180 | 0.160 | 0 |
| RS | Serbia | 5,119 | 38,878 | 0.118 | 1 |
| RU | Russian Federation | 54,436 | 355,896 | 0.211 | 1 |
| SE | Sweden | 8,195 | 51,213 | 0.252 | 2 |
| SI | Slovenia | 2,487 | 20,397 | 0.197 | 6 |
| SK | Slovakia | 4,724 | 36,032 | 0.202 | 2 |
| TR | Turkey | 138 | 1,120 | 0.200 | 0 |
| UA | Ukraine | 14,403 | 95,458 | 0.222 | 3 |
| Sum | | 472,445 | 3,384,989 | | 77 |

**Table 3: Estimation Without Taxes: Cost Elasticity and Cost Stickiness**

This table presents the results of regressions of changes in operating costs on changes in sales, separated by sales increases and the additional effect of the sales decrease dummy $SDEC_{it}$ for sales decreases. In specification (1), the changes in operating costs and in sales are scaled by $TA_{it-1}$. In specification (2), the changes in operating costs and in sales are scaled by lagged sales. Specification (3) reports the results for a regression of changes in the logarithm of operating costs ($\Delta\ln(OpCost_{it})$) on changes in the logarithm of sales ($\Delta\ln(Sales_{it})$). As controls, we include firm size ($\ln(TA_{it-1})$), GDP growth, unemployment, and inflation into each regression. We report robust t-statistics with standard errors clustered at the country–industry level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | (1) | (2) | | (3) |
|---|---|---|---|---|
| $\Delta Sales$, $\Delta OpCost$ scaled by | $TotalAssets_{it-1}$ | $Sales_{it-1}$ | Scale | Log |
| VARIABLES | $\Delta OpCost$ | $\Delta OpCost$ | VARIABLES | $\Delta\ln(OpCost)$ |
| $\Delta Sales$ | 0.882*** | 0.856*** | $\Delta ln(Sales)$ | 0.938*** |
| | (204.2) | (106.7) | | (110.6) |
| $SDEC \times \Delta Sales$ | 0.0238*** | 0.0185*** | $SDEC \times \Delta ln(Sales)$ | -0.0031 |
| | (6.307) | (2.687) | | (-0.411) |
| $\Delta Sales \times \Delta GDP$ | -0.0053*** | -0.0097*** | $\Delta ln(Sales) \times \Delta GDP$ | -0.0033 |
| | (-2.999) | (-2.976) | | (-0.885) |
| $\Delta Sales \times Unemp$ | 0.0080*** | 0.0072* | $\Delta ln(Sales) \times Unemp$ | 0.0007 |
| | (4.119) | (1.735) | | (0.152) |
| $\Delta Sales \times AInt$ | -0.0113*** | -0.0301*** | $\Delta ln(Sales) \times AInt$ | -0.0167*** |
| | (-10.83) | (-8.085) | | (-3.754) |
| $SDEC \times \Delta Sales \times \Delta GDP$ | 0.0001 | -0.0109*** | $SDEC \times \Delta ln(Sales) \times \Delta GDP$ | -0.0157*** |
| | (0.0397) | (-3.410) | | (-3.870) |
| $SDEC \times \Delta Sales \times Unemp$ | -0.0051** | -0.0131** | $SDEC \times \Delta ln(Sales) \times Unemp$ | -0.0121** |
| | (-2.393) | (-2.427) | | (-2.382) |
| $SDEC \times \Delta Sales \times AInt$ | -0.0080*** | -0.107*** | $SDEC \times \Delta Ln(Sales) \times AInt$ | -0.102*** |
| | (-7.989) | (-8.991) | | (-9.538) |
| Observations | 3,384,989 | 3,384,989 | | 3,384,989 |
| Adjusted R-squared | 0.956 | 0.865 | | 0.826 |
| Year FE | YES | YES | | YES |
| Country FE | YES | YES | | YES |

## Table 4: Tax Effect on Marginal Costs and Cost Stickiness

This table presents the results of regressions of changes in operating costs on changes in sales (separated for sales increases and the additional effect of the sales decrease dummy $SDEC_{it}$ for sales decreases) and on the statutory tax rate $\tau$. In specification (1), the changes in operating costs and in sales are scaled by $TA_{it\text{-}1}$. In specification (2), the changes in operating costs and in sales are scaled by lagged sales. Specification (3) reports the results for a regression of changes in the logarithm of operating costs ($\Delta\ln(OpCost_{it})$) on changes in the logarithm of sales ($\Delta\ln(Sales_{it})$). As interacted controls, we include GDP growth, unemployment, and asset intensity (all standardized) into each regression. We report robust t-statistics with standard errors clustered at the country–industry level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | (1) | (2) | | (3) |
|---|---|---|---|---|
| *ΔSales, ΔOpCost* scaled by | $TotalAssets_{it\text{-}1}$ | $Sales_{it\text{-}1}$ | Scale | Log |
| VARIABLES | ΔOpCost | ΔOpCost | VARIABLES | Δln(OpCost) |
| τ | -0.00804 | -0.0146 | τ | 0.0286* |
| | (-0.633) | (-0.975) | | (1.669) |
| *ΔSales* | 0.823*** | 0.780*** | *Δln(Sales)* | 0.876*** |
| | (90.41) | (56.57) | | (80.73) |
| *SDEC × ΔSales* | 0.0800*** | 0.0957*** | *SDEC × Δln(Sales)* | 0.0342 |
| | (9.710) | (4.593) | | (1.195) |
| ***τ × ΔSales*** | **0.223*** ** | **0.290*** ** | ***τ × Δln(Sales)*** | **0.233*** ** |
| | (7.641) | (4.959) | | (4.245) |
| ***SDEC × τ × ΔSales*** | **-0.209*** ** | **-0.284*** ** | ***SDEC × τ × Δln(Sales)*** | **-0.132** |
| | (-7.790) | (-3.835) | | (-1.257) |
| *ΔSales × ΔGDP* | -0.00200** | -0.00525** | *Δln(Sales) × ΔGDP* | 0.000884 |
| | (-1.971) | (-2.208) | | (0.360) |
| *ΔSales × Unemp* | 0.00750*** | 0.00605 | *Δln(Sales) × Unemp* | -0.000176 |
| | (4.264) | (1.490) | | (-0.0422) |
| *ΔSales × AInt* | -0.0124*** | -0.0318*** | *Δln(Sales) × AInt* | -0.0179*** |
| | (-12.05) | (-9.842) | | (-4.648) |
| *SDEC × ΔSales × ΔGDP* | -0.00317*** | -0.0157*** | *SDEC × Δln(Sales) × ΔGDP* | -0.0201*** |
| | (-3.164) | (-6.070) | | (-6.236) |
| *SDEC × ΔSales × Unemp* | -0.00513*** | -0.0131** | *SDEC × Δln(Sales) × Unemp* | -0.0130** |
| | (-2.674) | (-2.502) | | (-2.527) |
| *SDEC × ΔSales × AInt* | -0.00680*** | -0.107*** | *SDEC × ΔLn(Sales) × AInt* | -0.103*** |
| | (-7.195) | (-9.042) | | (-9.617) |
| Observations | 3,384,989 | 3,384,989 | | 3,384,989 |
| Adjusted R-squared | 0.956 | 0.866 | | 0.826 |
| Year FE | YES | YES | | YES |
| Country FE | YES | YES | | YES |

## Table 5: Tax Effect on Marginal Costs and Cost Stickiness, Additional Controls

This table presents the results of regressions of changes in operating costs on changes in sales (separated for sales increases and the additional effect of the sales decrease dummy $SDEC_{it}$ for sales decreases) and on the statutory tax rate $\tau$. In specifications (1) and (4) the changes in operating costs and in sales are scaled by $TA_{it\text{-}1}$. In specifications (2) and (5) the changes in operating costs and in sales are scaled by lagged sales. Specifications (3) and (6) report the results for a regression of changes in the logarithm of operating costs ($\Delta\ln(OpCost_{it})$) on changes in the logarithm of sales ($\Delta\ln(Sales_{it})$). In specifications (1) to (3), we include *SDEC* as well as the interaction of *SDEC* with $\tau$ in the regression. Finally, in specifications (4) to (6), we control for country-industry-year sales in the 3-digit NACE industry (excluding the own firm's sales). As controls in all tests, we include firm size ($\ln(TA_{it\text{-}1})$), GDP growth, unemployment, and inflation into each regression. We report robust t-statistics with standard errors clustered at the country–industry level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | **Standalone SDEC** | | | **Control for Industry–Year Sales** | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ***τ × ΔSales*** | **0.198*** ** | **0.288*** ** | | **0.225*** ** | **0.276*** ** | |
| | (7.209) | (4.611) | | (7.595) | (4.543) | |
| ***SDEC × τ × ΔSales*** | **-0.193*** ** | **-0.274*** ** | | **-0.219*** ** | **-0.227*** ** | |
| | (-7.944) | (-2.931) | | (-7.911) | (-4.272) | |
| ***τ × Δ*ln(*Sales*)** | | | **0.273*** ** | | | **0.172*** ** |
| | | | (3.480) | | | (4.173) |
| ***SDEC × τ × Δ*ln(*Sales*)** | | | **-0.110** | | | **-0.0497** |
| | | | (-1.050) | | | (-0.565) |
| *SDEC* | 0.0408*** | 0.0287*** | 0.0196*** | | | |
| | (11.09) | (4.685) | (2.690) | | | |
| *SDEC × τ* | -0.0726*** | -0.0258 | 0.0189 | | | |
| | (-5.675) | (-0.850) | (0.485) | | | |
| Scaling Variable | TA | Sales | – | TA | Sales | – |
| Controls | YES | YES | YES | YES | YES | YES |
| Interactions Controls | NO | NO | NO | NO | NO | NO |
| Industry Sales Controls | NO | NO | NO | YES | YES | YES |
| Year FE | YES | YES | YES | YES | YES | YES |
| Country FE | YES | YES | YES | YES | YES | YES |
| Observations | | | | 3,150,368 | 3,150,368 | 3,150,368 |
| Adjusted R-squared | | | | 0.959 | 0.908 | 0.869 |

## Table 6: Taxes and Cost Stickiness using the Weiss (2010) Measure

This table presents the results of regressions of the STICKY parameter (Weiss 2010) on the statutory corporate tax rate $\tau$. The STICKY parameter is estimated on firm level as the difference in cost function slopes between upward and downward activity adjustments. We estimate STICKY using the preceding four years: $STICKY_{i,t} = \ln(\Delta OpCost/\Delta Sales)_{i,\underline{x}} - \ln(\Delta OpCost/\Delta Sales)_{i,\overline{x}}$, where $\underline{x}$, $\overline{x} \in \{t, ..., t\text{-}3)$ and $\underline{x}$ ($\dot{x}$) is the most recent of the last four years with a decrease (increase) in sales. The lower STICKY, the higher the cost stickiness. Depending on the specification, we lose large and different numbers of observations. In specification (1), we report the baseline results. In specification (2), we demand that the tax rate stays unchanged during the preceding four years (the period that STICKY relies on). In specification (3), we winsorize STICKY on the 1% level. In specification (4), we employ country fixed effects. In specification (5), in order to mitigate the issue of multiple consecutive STICKY observations per firm, we drop STICKY$_{i,t}$ if we employ STICKY$_{i,t\text{-}1}$ and/or STICKY$_{i,t\text{-}2}$. We report robust t-statistics with standard errors clustered at the country–industry level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | STICKY | STICKY | STICKY | STICKY | STICKY |
| τ | -0.145*** | -0.0380** | -0.137*** | -0.189*** | -0.158*** |
| | (-13.00) | (-2.118) | (-13.66) | (-3.345) | (-8.914) |
| Observations | 1,372,028 | 516,039 | 1,372,028 | 1,372,028 | 590,796 |
| Adjusted R-squared | 0.002 | 0.002 | 0.002 | 0.003 | 0.002 |
| Constant tax rates in {t, …, t-3} | NO | YES | NO | NO | NO |
| STICKY winsorized at 1% and 99% | NO | NO | YES | NO | NO |
| Country FE | NO | NO | NO | YES | NO |
| Year FE | YES | YES | YES | YES | YES |
| Multiple consecutive obs. per firm | YES | YES | YES | YES | NO |

## Table 7: First-Difference Model Restricted to the Combined Variation of Tax Rates and Sales

This table presents the results of regressions of changes in operating costs on changes in sales (separated for sales increases and the additional effect of the sales decrease dummy $SDEC_{it}$ for sales decreases) and on changes in the statutory corporate tax rate $\tau$. In specification (1), the changes in operating costs and in sales are scaled by $TA_{it-1}$. In specification (2), the changes in operating cost and in sales are scaled by lagged sales. Specification (3) reports the results for a regression of the change in the logarithm of operating costs ($\Delta\ln(OpCost_{it})$) on the change in the logarithm of sales ($\Delta\ln(Sales_{it})$). We estimate the model $\Delta OpCost_{it} = \beta_1 \Delta Sales_{it} + \beta_{2a}\, \tau_{ct-1} \times \Delta Sales_i + \beta_{2b}\, \Delta\tau_c \times Sales_{it-1}$ $\boldsymbol{+ \beta_{2c}\, \Delta\tau_c \times \Delta Sales_i}$ $+ \beta_3 (\Delta GDP_{ct} \times Sales_{it} - \Delta GDP_{ct-1} \times Sales_{it-1}) + \beta_4 (Unemp_{ct} \times Sales_{it} - Unemp_{ct-1} \times Sales_{it-1}) + \gamma_0\, SDEC_{it} + \gamma_1\, SDEC_{it} \times \Delta Sales_{it}$ $+ \gamma_{2a}\, SDEC_{it} \times \tau_{ct-1} \times \Delta Sales_i + \gamma_{2b}\, SDEC_{it} \times \Delta\tau_c \times Sales_{it-1}$ $\boldsymbol{+ \gamma_{2c}\, SDEC_{it} \times \Delta\tau_c \times \Delta Sales_i}$ $+ \gamma_3\, SDEC_{it} \times (\Delta GDP_{ct} \times Sales_{it} - \Delta GDP_{ct-1} \times Sales_{it-1})$ $+ \gamma_4\, SDEC_{it} \times (Unemp_{ct} \times Sales_{it} - Unemp_{ct-1} \times Sales_{it-1}) + \chi\, \Delta\tau_{ct} + (\varepsilon_{it} - \varepsilon_{it-1})$. We report robust t-statistics with standard errors clustered at the country–industry level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. In Specification (2), $Sales_{t-1} \times \Delta\tau$ is omitted because $Sales_{t-1} \times \Delta\tau$, scaled by lagged sales, and $\Delta\tau$ are perfectly collinear. The number of observations is smaller than in the baseline sample because scaling $Sales_{t-1}$ with $TotalAssets_{t-2}$ results in the loss of another year.

| | (1) | (2) | | (3) |
|---|---|---|---|---|
| *ΔSales, ΔOpCost* scaled by | *TotalAssets*$_{it-1}$ | *Sales*$_{it-1}$ | Scale | Log |
| VARIABLES | ΔOpCost | ΔOpCost | VARIABLES | Δln(OpCost) |
| $\Delta\tau$ | 0.0331** | -0.0138 | $\Delta\tau$ | -0.706*** |
| | (2.079) | (-0.420) | | (-4.954) |
| *ΔSales* | 0.869*** | 0.817*** | *Δln(Sales)* | 0.939*** |
| | (98.10) | (51.94) | | (84.94) |
| *SDEC × ΔSales* | 0.0341*** | 0.0169 | *SDEC × Δln(Sales)* | -0.0540** |
| | (4.361) | (0.762) | | (-2.482) |
| *ΔSales* × $\tau_{t-1}$ | 0.189*** | 0.273*** | *Δln(Sales)* × $\tau_{t-1}$ | 0.167*** |
| | (6.026) | (4.621) | | (3.098) |
| $Sales_{t-1} \times \Delta\tau$ | -0.0573*** | - | $ln(Sales_{t-1}) \times \Delta\tau$ | 0.0955*** |
| | (-3.987) | | | (5.064) |
| ***ΔSales × Δτ*** | **0.268***** | **0.462***** | ***Δln(Sales) × Δτ*** | **0.159**** |
| | (3.284) | (3.165) | | (2.012) |
| *SDEC × ΔSales* × $\tau_{t-1}$ | -0.0746*** | -0.259*** | *SDEC × Δln(Sales)* × $\tau_{t-1}$ | -0.159* |
| | (-2.858) | (-2.992) | | (-1.770) |
| *SDEC* × $Sales_{t-1} \times \Delta\tau$ | 0.0678*** | -0.0564 | *SDEC* × $ln(Sales_{t-1}) \times \Delta\tau$ | -0.00884** |
| | (3.679) | (-1.138) | | (-2.039) |
| ***SDEC × ΔSales × Δτ*** | **-0.0534** | **-0.696***** | ***SDEC × Δln(Sales) × Δτ*** | **-0.412***** |
| | (-0.517) | (-3.350) | | (-3.068) |
| Observations | 2,841,880 | 2,841,880 | | 3,258,545 |
| Adjusted R-squared | 0.957 | 0.887 | | 0.851 |
| Interacted Controls | YES | YES | | YES |
| Cluster | Country–Ind | Country–Ind | | Country–Ind |

## Table 8: Heterogeneity Analysis—Profit versus Loss Firms, High- versus Low-Tax Firms, Standalone versus MNEs

This table presents the results of regressions of changes in operating costs on changes in sales (separated for sales increases and the additional effect of the sales decrease dummy $SDEC_{it}$ for sales decreases) and on the statutory corporate tax rate $\tau$. The changes in operating costs and in sales are scaled by $TA_{it\text{-}1}$. In specifications (1) and (2), we compare profitable firms ($EBT_{it} \geq 0$) and loss firms ($EBT_{it} < 0$). In specifications (3) and (4), we compare high-tax firm–years ($tax\ expense_{it}/TA_{it\text{-}1}$ > median ($tax\ expense_t/TA_{t\text{-}1}$ for all firms in country $c$ and year $t$)) with low-tax firms ($tax\ expense_{it}/TA_{it\text{-}1}$ < median ($tax\ expense_t/TA_{t\text{-}1}$ for all firms in country $c$ and year $t$)). In specifications (5) and (6), we compare standalone firms (our baseline sample) with firms that own a foreign subsidiary or are themselves owned by a foreign industry firm. In each comparison, we test whether the coefficient estimates of interest are significantly different. In each regression, we include interacted controls (GDP growth, unemployment, and asset intensity). We report robust t-statistics with standard errors clustered at the country–industry level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| Subsample | (1) Profitable Firms | (2) Loss Firms | (3) High-Tax Firms | (4) Low-Tax Firms | (5) Standalone Firms | (6) Groups/ MNEs |
|---|---|---|---|---|---|---|
| VARIABLES | $\Delta OpCost$ | $\Delta OpCost$ | $\Delta OpCost$ | $\Delta OpCost$ | $\Delta OpCost$ | $\Delta OpCost$ |
| ***$\tau \times \Delta Sales$*** | **0.2228*** ** | **0.1147*** ** | **0.3051*** ** | **0.0715*** | **0.2228*** ** | **0.1436*** ** |
| | (7.641) | (2.924) | (9.373) | (1.669) | (7.641) | (5.321) |
| ***$SDEC \times \tau \times \Delta Sales$*** | **-0.2089*** ** | **-0.2018*** ** | **-0.2277*** ** | **-0.0146** | **-0.2089*** ** | **-0.2147*** ** |
| | (-7.790) | (-4.675) | (-7.344) | (-0.503) | (-7.790) | (-7.894) |
| Observations | 3,384,989 | 386,859 | 1,352,870 | 1,353,249 | 3,384,989 | 1,787,910 |
| | 3,771,848 | | 2,706,119 | | 5,172,899 | |
| Controls | YES | YES | YES | YES | YES | YES |
| Year FE | YES | YES | YES | YES | YES | YES |
| Country FE | YES | YES | YES | YES | YES | YES |
| Tests of significant differences (Prob > $chi^2$) | | | | | | |
| $\tau \times \Delta Sales$ | 0.0052*** | | 0.0000*** | | 0.0052*** | |
| $SDEC \times \tau \times \Delta Sales$ | 0.8733 | | 0.0000*** | | 0.8272 | |
| $\tau \times \Delta Sales + SDEC \times \tau \times \Delta Sales$ | 0.0023*** | | 0.4067 | | 0.0217** | |

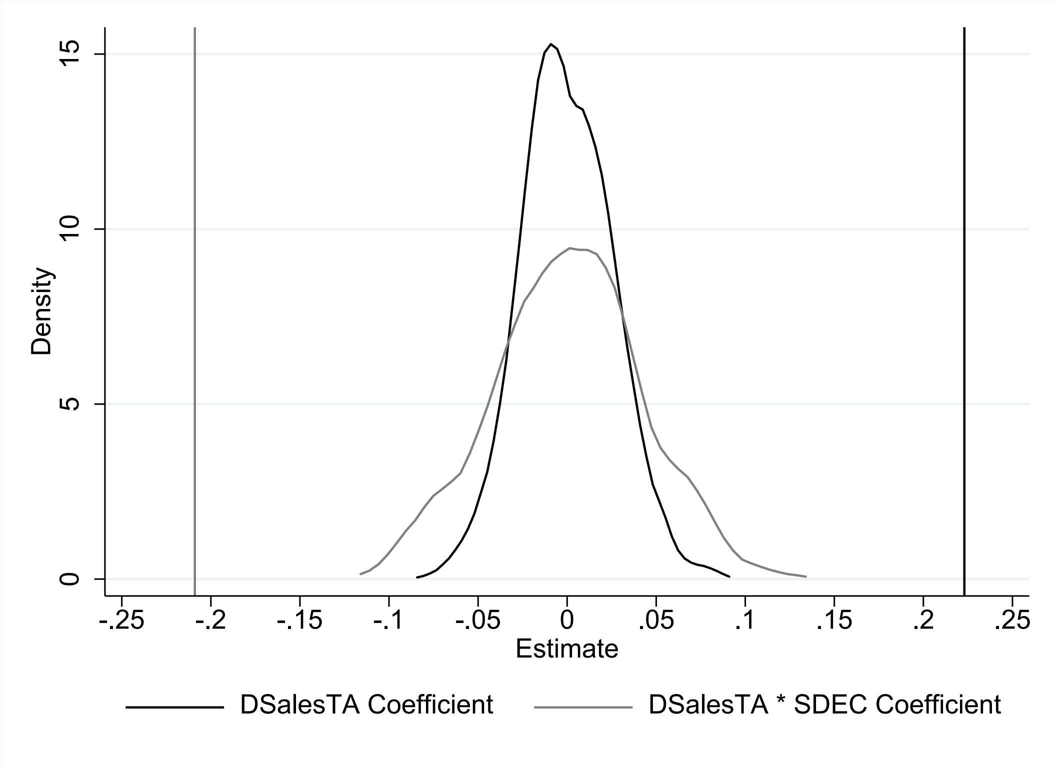
## Table 9: Heterogeneity Analysis—Role of Tax Incidence and Implicit Taxes

This table presents the results of regressions of changes in operating costs on changes in sales (separated for sales increases and the additional effect of the sales decrease dummy $SDEC_{it}$ for sales decreases) and on the statutory corporate tax rate $\tau$. The changes in operating costs and in sales are scaled by $TA_{it\text{-}1}$. In specifications (1) and (2), we use our industry sales calculation to calculate firm $i$'s market share, and we split the sample at the country–year median into firms with low and high market shares. In specifications (3) and (4), we split the sample at the country-level median of industry-level asset redeployability. We use a firm's SIC code to merge the Kim and Kung (2017) industry-level measure of asset redeployability. For each industry, we use the average over our sample period. In each comparison, we also test whether the coefficient estimates of interest are significantly different. In each regression, we include interacted controls (GDP growth, unemployment, and asset intensity). We report robust t-statistics with standard errors clustered at the country–industry level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *Subsample* | Low Market Share | High Market Share | Low Redeployability | High Redeployability |
| VARIABLES | $\Delta OpCost$ | $\Delta OpCost$ | $\Delta OpCost$ | $\Delta OpCost$ |
| ***$\tau \times \Delta Sales$*** | 0.2478*** | 0.1991*** | 0.2910*** | 0.2151*** |
| | (6.882) | (7.174) | (6.404) | (6.159) |
| ***$SDEC \times \tau \times \Delta Sales$*** | -0.2207*** | -0.2024*** | -0.2176*** | -0.1627*** |
| | (-6.288) | (-8.402) | (-5.826) | (-4.876) |
| Observations | 1,675,330 | 1,687,650 | 1,629,932 | 1,629,932 |
| | 3,362,980 | | 3,259,864 | |
| Controls | YES | YES | YES | YES |
| Year FE | YES | YES | YES | YES |
| Country FE | YES | YES | YES | YES |
| Tests of significant differences (Prob > chi$^2$) | | | | |
| $\tau \times \Delta Sales$ | 0.0522* | | 0.3898 | |
| $SDEC \times \tau \times \Delta Sales$ | 0.5290 | | 0.4812 | |
| $\tau \times \Delta Sales + SDEC \times \tau \times \Delta Sales$ | 0.3461 | | 0.7062 | |

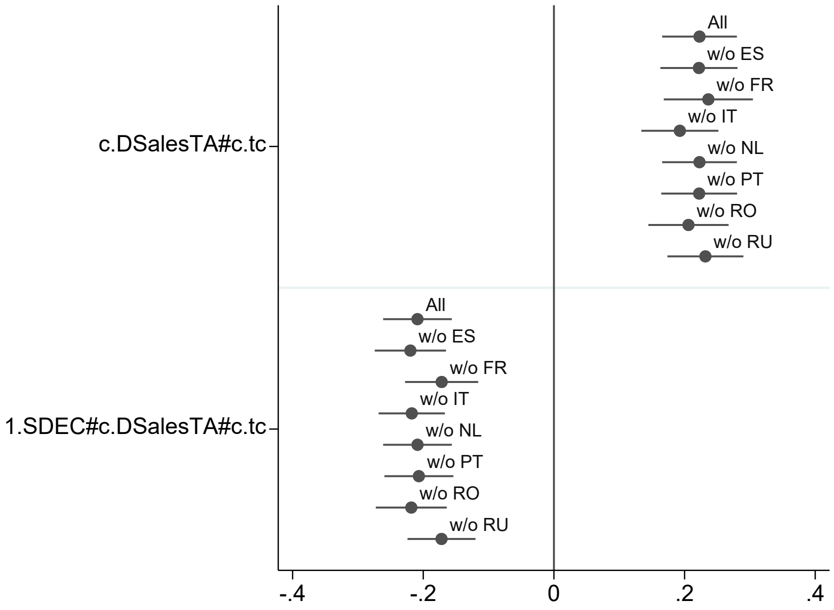
## Online Appendix

### Figure A.1: Coefficient Estimates for Randomly Assigned Tax Rates (1,000 Simulations)




This figure displays the coefficient estimates density function for $\tau \times \Delta Sales_{it}$ and for $\tau \times SDEC_{it} \times \Delta Sales_{it}$ from 1,000 simulations based on the regression $\Delta OpCost_{it} = \alpha + (\beta_1 + \boldsymbol{\beta_2}\, \tilde{\boldsymbol{\tau}}_{\boldsymbol{ct}} + \beta_3\, \Delta GDP_{ct} + \beta_4\, Unemp_{ct} + \beta_5\, AInt_{it})\, \Delta Sales_{it} + (\gamma_1 + \boldsymbol{\gamma_2}\, \tilde{\boldsymbol{\tau}}_{\boldsymbol{ct}} + \gamma_3\, \Delta GDP_{ct} + \gamma_4\, Unemp_{ct} + \gamma_5\, AInt_{it})\, SDEC_{it} \times \Delta Sales_{it} + \chi\, \tilde{\tau}_{ct} + \delta_t + \lambda_c + \varepsilon_{it}$, where $\tilde{\tau}_{ct}$ is a tax rate from a different country–year cell that we randomly attribute to the country–year cell *ct*. We scale the sales change and the change in operating cost by $TA_{it\text{-}1}$. We rerun the simulation 1,000 times. For a comparison, the two estimates from the main regression are marked with vertical lines.

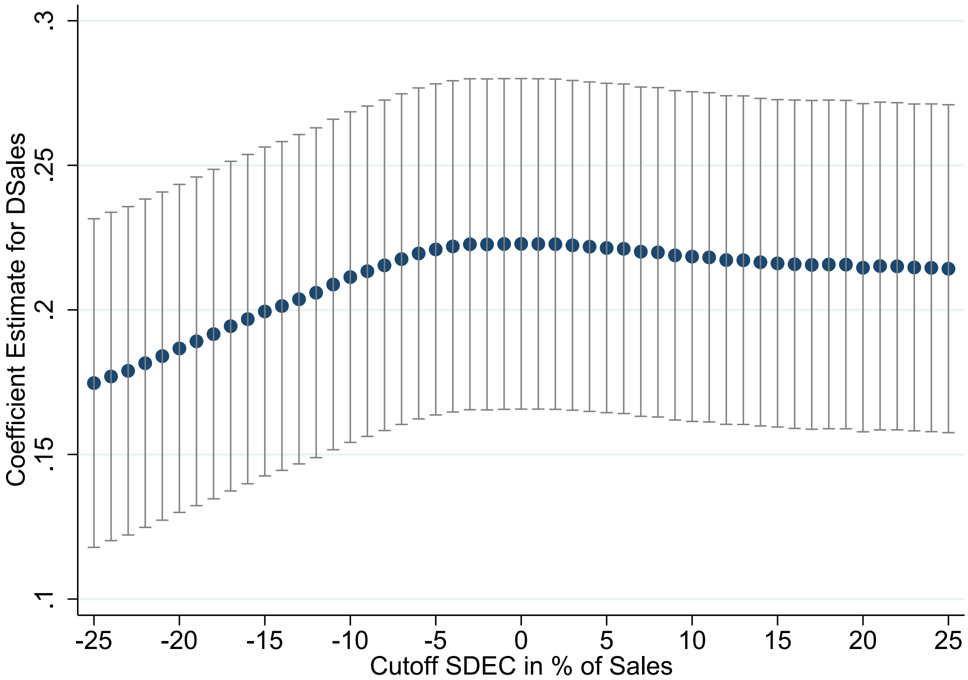
**Figure A.2: Exclusion of the Countries with the Highest Numbers of Observations**



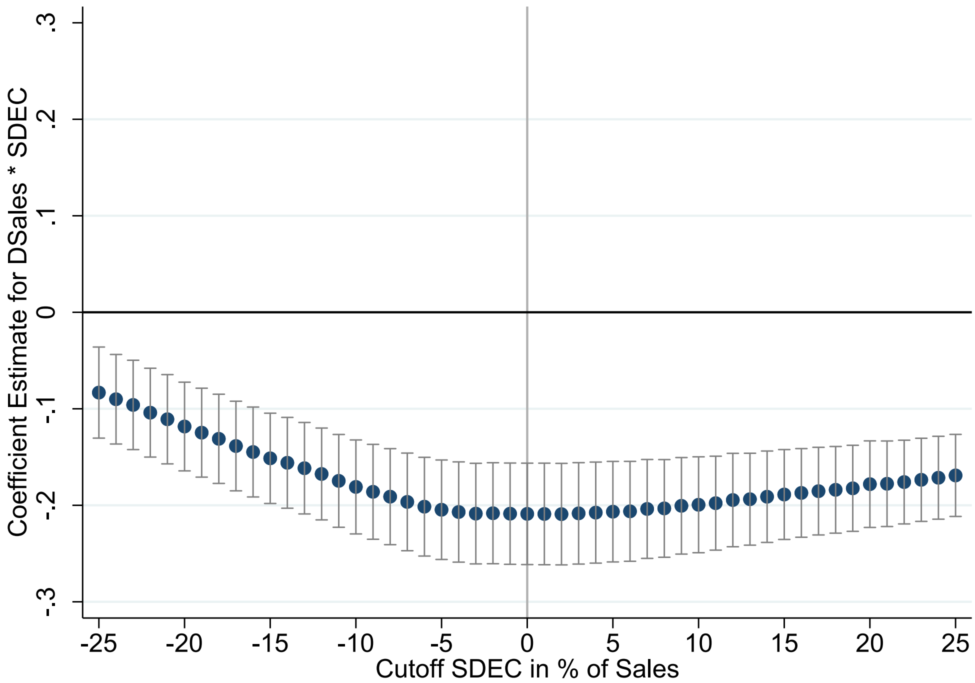

This figure displays the effect of excluding each of the seven countries with the highest numbers of observations. The figure depicts the coefficient estimates $\beta_2$ (top) and $\gamma_2$ (bottom) and the 95% confidence intervals from the regression $\Delta OpCost_{it} = \alpha + (\beta_1 + \boldsymbol{\beta_2\ \tau_{ct}} + \beta_3\ \Delta GDP_{ct} + \beta_4\ Unemp_{ct} + \beta_5\ AInt_{it})\ \Delta Sales_{it} + (\gamma_1 + \boldsymbol{\gamma_2\ \tau_{ct}} + \gamma_3\ \Delta GDP_{ct} + \gamma_4\ Unemp_{ct} + \gamma_5\ AInt_{it})\ SDEC_{it} \times \Delta Sales_{it} + \chi\ \tau_{ct} + \delta_t + \lambda_c + \varepsilon_{it}$. $\Delta OpCost$ and $\Delta Sales$ are scaled by $TA_{it\text{-}1}$.

**Figure A.3: Alternative Cutoff Points for $SDEC_{it}$**

**Panel A: Coefficient Estimates for $\tau_{ct} \times \Delta Sales_{it}$**




**Panel B: Coefficient Estimates for Cost Stickiness ($\tau_{ct} \times SDEC_{it} \times \Delta Sales_{it}$)**




This figure displays the effect of setting alternative cutoff points for the sales decrease dummy *SDEC*. In the main analysis, we follow the literature and define $SDEC_{it}$ as equal to one if $Sales_{it} < Sales_{it-1}$, and zero otherwise. In this analysis, we set $SDEC_{it}$ to cutoff values from -25% ($SDEC_{it}$ equal to one if $Sales_{it} < (1 - 0.25)Sales_{it-1}$, and zero otherwise) to +25% ($SDEC_{it}$ equal to one if $Sales_{it} < (1 + 0.25)Sales_{it-1}$, and zero otherwise) in steps of 1%. The figure displays the coefficient estimates $\beta_2$ (Panel A) and $\gamma_2$ (Panel B) and the 95% confidence intervals from the regression $\Delta OpCost_{it} = \alpha + (\beta_1 + \boldsymbol{\beta_2}\ \boldsymbol{\tau_{ct}} + \beta_3\ \Delta GDP_{ct} + \beta_4\ Unemp_{ct} + \beta_5\ AInt_{it})\Delta Sales_{it} + (\gamma_1 + \boldsymbol{\gamma_2}\ \boldsymbol{\tau_{ct}} + \gamma_3\ \Delta GDP_{ct} + \gamma_4\ Unemp_{ct} + \gamma_5\ AInt_{it})\ SDEC_{it} \times \Delta Sales_{it} + \chi\ \tau_{ct} + \delta_t + \lambda_c + \varepsilon_{it}$ (with $\Delta OpCost$ and $\Delta Sales$ scaled by $TA_{it-1}$).

## Table A.1: Different Outlier Criteria

This table compares different criteria for the exclusion of outliers. In Column (1), we drop observations with sales less than or equal to EBIT, with OpCost/Sales > 1,000% or < 10%, or with abs($\Delta OpCost/\Delta Sales$)>5. We further drop the lower and upper 0.5% of the $\Delta OpCost_{it}/TA_{it-1}$ and $\Delta Sales_{it}/TA_{it-1}$ distributions. Then we winsorize all firm-level change variables at the 0.5% and 99.5% levels. In Column (2), we drop observations with sales than or equal to EBIT, with OpCost/Sales > 1,000% or < 10%, or with abs($\Delta OpCost/\Delta Sales$)>5. We further drop the lower and upper 1% of the $\Delta OpCost_{it}/TA_{it-1}$ and $\Delta Sales_{it}/TA_{it-1}$ distributions. Then we winsorize all firm-level change variables at the 1% and 99% levels. In Column (3), we drop observations with sales than or equal to EBIT, with OpCost/Sales > 1,000% or < 10%, or with abs($\Delta OpCost/\Delta Sales$)>5. We further drop the lower and upper 0.5% of the $\Delta OpCost_{it}/TA_{it-1}$ and the $\Delta Sales_{it}/TA_{it-1}$ distributions. We do not winsorize. In Column (4), we drop observations with sales than or equal to EBIT, with OpCost/Sales > 1,000% or < 10%, or with abs($\Delta OpCost/\Delta Sales$)>5. We do not truncate the tails of the $\Delta OpCost_{it}/TA_{it-1}$ or $\Delta Sales_{it}/TA_{it-1}$ distributions. We winsorize all firm-level change variables at the 0.5% and 99.5% levels. In Columns (5) and (6), we do not correct for outliers. In Column (7), we only winsorize all firm *level* variables at the 1% and 99% levels.

| VARIABLES | (1)<br>$\Delta OpCost$ | (2)<br>$\Delta OpCost$ | (3)<br>$\Delta OpCost$ | (4)<br>$\Delta OpCost$ | (5)<br>$\Delta OpCost$ | (6)<br>$\Delta\ln(OpCost)$ | (7)<br>$\Delta\ln(OpCost)$ |
|---|---|---|---|---|---|---|---|
| $\tau \times \Delta Sales$ | **0.223***** | **0.241***** | **0.222***** | **0.185***** | **0.302** | | |
| | (7.641) | (7.881) | (6.343) | (7.002) | (0.250) | | |
| $SDEC \times \tau \times \Delta Sales$ | **-0.209***** | **-0.195***** | **-0.207***** | **-0.220***** | **3.160** | | |
| | (-7.790) | (-7.328) | (-7.732) | (-8.471) | (0.939) | | |
| $\tau \times \Delta ln(Sales)$ | | | | | | **0.202** | **0.274**** |
| | | | | | | (1.589) | (2.552) |
| $SDEC \times \tau \times \Delta ln(Sales)$ | | | | | | **-0.122** | **-0.102** |
| | | | | | | (-1.184) | (-1.105) |
| Observations | 3,384,989 | 3,334,373 | 3,384,989 | 3,415,139 | 3,487,292 | 3,487,446 | 3,487,446 |
| Adjusted R-squared | 0.956 | 0.947 | 0.956 | 0.969 | 0.997 | 0.795 | 0.822 |
| Controls | YES | YES | YES | YES | YES | YES | YES |
| Year FE | YES | YES | YES | YES | YES | YES | YES |
| Country FE | YES | YES | YES | YES | YES | YES | YES |
| Truncation of extreme outliers | YES | YES | YES | YES | NO | NO | NO |
| Truncation of sales and/or operating costs Distribution | 0.5% $\Delta Sales$, 0.5% $\Delta OpCost$ | 1% $\Delta Sales$, 1% $\Delta OpCost$ | 0.5% $\Delta Sales$, 0.5% $\Delta OpCost$ | NO | NO | NO | NO |
| Winsorizing | 0.5% Changes | 1% Changes | NO | 0.5% Changes | NO | NO | 1% Levels |

## Table A.2: Corporate Taxes, Marginal Cost, and Cost Stickiness (Hierarchical Country–Year Cell Analysis)

This table presents the results of the second-stage regressions of first-stage country–year-level coefficient estimates (for the responsiveness of operating costs to sales changes) on the country–year-specific corporate tax rate. We separately report the coefficient estimates for sales increases and the additional effect for sales decreases (sales decrease dummy *SDEC*). The first stage is estimated by the regression $\Delta OpCost_{it} = \alpha + \beta\ \Delta Sales_{it} + \gamma\ SDEC_{it} \times \Delta Sales_{it} + \ln(TA_{it\text{-}1}) + \varepsilon_{it}$. The changes in operating costs and in sales are scaled by $TA_{it\text{-}1}$. We estimate this regression for every country and every year, using robust regression (MM). The second stage is estimated by the WLS regression $\beta_{ct} = \chi + \delta\ \tau_{ct} + \lambda_{ct}\ Controls_{ct} + \varepsilon_{ct}$ (specifications (1) to (4)) or $\gamma_{ct} = \chi + \delta\ \tau_{ct} + \lambda_{ct}\ Controls_{ct} + \varepsilon_{ct}$ (specifications (5) to (8)). The following country-level controls are included: $\Delta GDP_{ct}$, $Unemployment_{ct}$, $Inflation_{ct}$. We weigh the country–year observations with the inverse standard error. In specifications (1) to (3) and (5) to (7), we report robust t-statistics with standard errors clustered at the country level in parentheses. In specifications (4) and (8), we only exploit within variation and include country fixed effects in the second-stage regressions. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| VARIABLES | (1)<br>ΔSales > 0 | (2)<br>ΔSales > 0 | (3)<br>ΔSales > 0 | (4)<br>ΔSales > 0 | (5)<br>SDEC × ΔSales | (6)<br>SDEC × ΔSales | (7)<br>SDEC × ΔSales | (8)<br>SDEC × ΔSales |
|---|---|---|---|---|---|---|---|---|
| $\tau$ | **0.1286*** ** | **0.0920*** ** | **0.0886*** ** | **0.0629** ** | **-0.1714*** ** | **-0.1491*** ** | **-0.1535*** ** | **-0.1047* ** |
| | (5.546) | (4.384) | (4.757) | (2.238) | (-9.964) | (-8.163) | (-8.502) | (-1.884) |
| Constant | | -0.0005 | -0.0010** | 0.0001 | | -0.0000 | 0.0002 | -0.0005* |
| | | (-1.272) | (-2.021) | (0.713) | | (-0.012) | (0.367) | (-1.666) |
| Observations | 357 | 357 | 357 | 357 | 353 | 353 | 353 | 353 |
| Adjusted $R^2$ | 0.184 | 0.449 | 0.487 | 0.848 | 0.286 | 0.331 | 0.367 | 0.524 |
| Controls | NO | YES | YES | YES | NO | YES | YES | YES |
| Year FE | NO | NO | YES | YES | NO | NO | YES | YES |
| Country FE | NO | NO | NO | YES | NO | NO | NO | YES |

**Table A.3: Intertemporal Profit Shifting, the Sample Split into Immediate Pre-/Post-Reform Years and Other Years**

This table presents the results of regressions of changes in operating costs on changes in sales (separated for sales increases and the additional effect of the sales decrease dummy $SDEC_{it}$ for sales decreases) and on the statutory corporate tax rate $\tau$. The changes in operating costs and in sales are scaled by lagged total assets ($TA_{it-1}$). The controls are interacted with the change in sales as in the baseline regression of Table 4. In specification (1), we report the baseline results from Table 4, column (1) (full sample). We split the sample into non-reform years and immediate pre- and post-reform years. If the corporate tax rate is changed between year $t$ and year $t + 1$, then we consider year $t$ as the immediate pre-reform year and year $t + 1$ as the immediate post-reform year. In specification (2), we leave out immediate pre-/post-reform years. In specification (3), we include only immediate pre- and post-reform years. We report robust t-statistics with standard errors clustered at the country–industry level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| VARIABLES | (1)<br>$\Delta OpCost$ | (2)<br>$\Delta OpCost$ | (3)<br>$\Delta OpCost$ |
|---|---|---|---|
| **$\tau \times \Delta Sales$** | **0.223*** ** | **0.206*** ** | **0.246*** ** |
| | (7.641) | (5.973) | (7.711) |
| **$SDEC \times \tau \times \Delta Sales$** | **-0.209*** ** | **-0.170*** ** | **-0.227*** ** |
| | (-7.790) | (-6.265) | (-5.102) |
| Observations | 3,384,989 | 1,940,330 | 1,444,659 |
| Adjusted R-squared | 0.956 | 0.956 | 0.957 |
| Controls | YES | YES | YES |
| Year FE | YES | YES | YES |
| Country FE | YES | YES | YES |
| Immediate pre-reform years | **ALL YEARS** | **NO** | **ONLY** |
| Immediate post-reform years | **ALL YEARS** | **NO** | **ONLY** |

## Table A.4: Employment Protection Strictness as a Potential Confounder

This table presents the results of the following regression: $\Delta OpCost_{it} = \alpha + (\beta_1 + \beta_2\ \tau_{ct} + \beta_3\ EPLex_{ct} + \beta_4\ \Delta GDP_{ct} + \beta_5\ Unemp_{ct} + \beta_6\ AInt_{it})\ \Delta Sales_{it} + (\gamma_1 + \gamma_2\ \tau_{ct} + \gamma_3\ EPLex_{ct} + \gamma_4\ \Delta GDP_{ct} + \gamma_5\ Unemp_{ct} + \gamma_6\ AInt_{it})\ SDEC_{it} \times \Delta Sales_{it} + \chi\ \tau_{ct} + \delta_t + \lambda_c + \varepsilon_{it}$. $\Delta Sales$ and $\Delta OpCost$ are scaled by lagged total assets. EPLex is an summary country-year indicator that is compiled by the ILO. The higher the indicator, the higher the employment protection strictness. For some countries, there are different summary indicators for blue-collar and for white-collar workers or for firms with not more or more than 10 or 15 workers. We used the summary indicators for blue-collar workers and firms with more than 15 employees. We report robust t-statistics with standard errors clustered at the country–industry level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| VARIABLES | (1)<br>$\Delta$*OpCost* |
|---|---|
| τ | 0.0234 |
| | (0.934) |
| $\Delta$*Sales* | 0.809*** |
| | (47.87) |
| *SDEC* × $\Delta$*Sales* | 0.0341** |
| | (2.577) |
| **τ × ΔSales** | **0.226*** ** |
| | (5.938) |
| ***SDEC* × τ × ΔSales** | **-0.193*** ** |
| | (-7.359) |
| *EPLex* × $\Delta$*Sales* | 0.0305 |
| | (1.288) |
| *SDEC* × *EPLex* × $\Delta$*Sales* | 0.0839*** |
| | (3.859) |
| Observations | 2,223,813 |
| Adjusted R-squared | 0.955 |
| Interacted Controls | YES |
| Year FE | YES |
| Country FE | YES |
| Clustering | Country-Ind |

## Table A.5: Testing the Assumptions

This table presents the results of regressions of changes in pretax income scaled by total assets on changes in sales (columns (1) and (2)). In column 3, we use the change in the ratio of tax expense to sales (*Tax-to-Sales*) as dependent variable. The changes in sales are scaled by lagged total assets ($TA_{it-1}$). In columns (1) and (2), we control for size and country-level control variables. Column (3) includes all control variables and interactions as in our main model. We report robust t-statistics with standard errors clustered firm level (columns 1 and 2) and at the country–industry level (column (3)) in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

| VARIABLES | (1)<br>Δ*Pretax Income* | (2)<br>Δ*Pretax Income* | (3)<br>Δ*Tax-to-Sales* |
|---|---|---|---|
| ***ΔSales*** | **0.0513*****| **0.0558***** | **0.0022**** |
| | (288.57) | (278.15) | (1.97) |
| ***τ × ΔSales*** | | | **-0.0185***** |
| | | | (-2.98) |
| ***SDEC × τ × ΔSales*** | | | **0.0218***** |
| | | | (2.26) |
| Observations | 4,128,543 | 4,128,543 | 2,860,955 |
| Adjusted R-squared | 0.0789 | 0.021 | 0.0157 |
| Controls | YES | YES | YES |
| Year FE | YES | YES | YES |
| Country FE | YES | NO | YES |
| Firm FE | NO | YES | NO |