Taxes and Default Risk: Evidence From Establishment-Level Data

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Abstract

We use establishment-level data to examine the relation between corporate taxes and default risk. Exploiting corporate income tax rate changes across states and using a border discontinuity approach, we document that higher corporate income tax rates increase default risk. This effect is more pronounced for firms that are geographically concentrated. We also find substantial spillover effects across establishments within the same firm, including those located in other states. We further investigate *how* taxes influence establishment-level default risk by exploiting the interest limitation rule introduced by the 2017 Tax Cuts and Jobs Act. Using a difference-in-differences design, we find that the establishments of firms affected by this limitation experience a significant reduction in default risk. This occurs because the diminished tax benefits of debt incentivize firms to reduce their dependence on debt, thus mitigating default risk through the corresponding adjustments to their capital structure.

Keywords: Corporate Taxes; Default Risk.

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1 Introduction

How do corporate income taxes affect the default risk? Higher corporate income tax rates can increase the risk of default by reducing after-tax cash flows of firms (Ivanov et al., 2024), or by strengthening the tax incentive to use debt financing (Faccio and Xu, 2015; Heider and Ljungqvist, 2015; Sanati and Beyhaghi, 2025; Carrizosa et al., 2023; Faccio and Xu, 2024; Heitzman and Hanlon, 2024). However, firms can, and often do, employ aggressive non-debt tax-shielding strategies that can render the effect of taxes on default risk inconsequential. (See Hanlon and Heitzman (2010), Wilde and Wilson (2018), and Dyreng and Hanlon (2021) for reviews of corporate tax planning).

This paper investigates this question using a combination of establishment- and firm-level data on U.S. publicly traded firms. The use of granular establishment-level data allows us to conduct a more precise investigation of the relation between state-level tax rate changes and default risk than focusing on the location of firms' headquarters, as state-level corporate taxation is largely determined by production locations (Baker et al., 2020). Doing so is key as most publicly traded firms operate establishments in multiple states. Importantly, we can employ a default risk measure based on payment performance to suppliers and vendors at the establishment level, the Paydex score. As documented by Barrot and Nanda (2020), Chava et al. (2023), and Leibovici and Wiczer (2024), this measure strongly correlates with business closures and other indicators of financial distress. According to Becker (2012), it is widely used in the credit industry as a proxy for the risk of short-term default.

Granular data at the establishment level enables us to compare establishments that operate in the same industry but are located in different states, before and after a change in the corporate income tax rate. In this setting, we recognize that corporate tax changes are not exogenous since the timing and extent of these policies may be tied to the state's economic health (Faccio and Xu, 2015; Heider and Ljungqvist, 2015). Furthermore, changes in corporate income tax rates are often correlated with changes in other types of local and state taxes (Baker et al., 2025). Thus, an increase in the corporate income tax rate could be correlated with changes in the economic environment that can also affect a business's default risk.

To address this endogeneity problem, we leverage the staggered changes in state-level corporate income tax rates by combining them with a border discontinuity approach (e.g., Dube et al., 2010; Mukherjee et al., 2017), while additionally controlling for several additional state-level taxes. Specifically, we exploit our establishment-level database by selecting establishments located in counties along state borders. Neighboring counties along state lines share similar local economic conditions while being subject to different state-level tax policies. By matching each establishment in a county within the border region with all establishments in neighboring counties across state lines and controlling for shared trends between paired counties and other contemporaneous tax changes, we isolate the effects of tax policy variation on default risk.

Using this empirical strategy, we show that corporate taxes significantly increase the risk of default of affected establishments. More specifically, a one percentage point increase in the state-level corporate income tax rate reduces the Paydex score of an establishment in that state by 6 basis points, or 23% of its average annual change. We also show that the impact of corporate income taxes on default risk is influenced by the location and combined tax exposure of all of a firm's establishments. More specifically, we document that the effects are substantially larger for firms that are geographically concentrated in states experiencing a corporate income tax rate change, suggesting that geographic diversification is valuable in mitigating the adverse impact of localized tax shocks on default risk. Furthermore, we find significant spillover effects of corporate taxes across establishments, including exposure originating from establishments in other states. Therefore, state-level tax policies can have firm-wide implications beyond the directly affected locations, underscoring the interconnectedness of establishments within multi-state firms and the need to account for cross-location dynamics when evaluating the impact of tax changes on default risk (and, possibly, other outcomes).

Having established that taxes affect the risk of default, we then turn to investigating *how* they do so. For this purpose, we take advantage of the introduction of the Tax Cuts and Jobs Act (TCJA) in 2017 as a quasi-experimental setting. More specifically, at the time of its introduction, Section 163(j) limited the deductibility of interest payments to 30% of

EBITDA.

This setting is particularly attractive and unique because the limitation on the deductibility of interest payments introduced by this law allows us to disentangle the tax shield and cash flow channels. Specifically, the tax shield channel predicts that, due to the diminished incentive to use debt, affected firms reduce their leverage more than unaffected firms, thereby experiencing a greater relative decline in default risk. Conversely, the cash flow channel posits that affected firms experience a relative decline in after-tax cash flows and, thus, should experience a relative increase in default risk. To investigate which channel dominates in this setting, we compare the credit scores of establishments of firms affected by the interest deductibility limitation with those that are not, using a difference-in-differences approach.

We document that this policy has a substantial effect on the establishments of the affected parent companies. More specifically, we find that establishments belonging to parent firms affected by the interest limitation rule experience a significant relative reduction in default risk. Our baseline estimates indicate a yearly increase in the business credit score of approximately 0.5 units relative to the establishments of unaffected firms, or 257% of its average annual change. This result indicates that the tax shield channel appears to dominate the cash flow channel in our sample of publicly traded firms.

An investigation of the dynamic effects reinforces our findings: establishments of treated and untreated firms exhibit parallel trends in default risk before the tax reform, with a clear divergence emerging only afterward. This supports the parallel-trend assumption, which is key to the credibility of our difference-in-differences design. We also document that the effect persists over time. Further supporting a causal interpretation, we find that the relative reduction in default risk remains robust when we combine our difference-in-differences approach with propensity score matching based on several key firm characteristics, as well as when we further restrict the matched sample to firms near the threshold that triggers the interest deduction limitation. Therefore, although the stated goal of the TCJA was to stimulate economic growth by lowering the federal corporate income tax rate, our evidence reveals an important, perhaps unintended, consequence of the reform: improved financial stability.

Indeed, in direct support of the tax-shield channel, we document that affected parent

firms reduce their leverage ratio. For this reason, they do not experience a relative increase in the effective income tax rate, which would have been expected to increase absent a debt reduction. At the same time, we document that affected firms pay significantly lower interest expenses. We also find positive effects on cash holdings.

We contribute to the literature in several ways. First, previous literature shows that, after accounting for default risk, both personal (Elton et al., 2001; Driessen, 2005; Liu et al., 2007) and corporate income taxes (Zhao et al., 2024) significantly impact the credit spreads of corporate bond issuers. Other studies in this literature derive corporate default risk endogenously from structural models of firm value and capital structure, rather than inferring it from observed defaults (Leland and Toft, 1996; Ivanov et al., 2024). In this literature, the recent work by Ivanov et al. (2024) is the closest to ours. Ivanov et al. (2024) present compelling firm-level evidence that state-level corporate income tax cuts lead to a significant reduction in the default risk of private U.S. firms, with the effect being particularly pronounced among smaller private firms.

Our paper contributes to this literature by providing direct evidence that corporate taxes do significantly affect default risk. We further offer insights on the importance of considering a firm's overall geographic footprint when assessing this risk. We do so by proposing an establishment-level measure of default risk that allows us to closely link local tax rate changes to the affected establishments' default risk.[1]

Other papers in the literature examine how geographic diversification shapes financial risk. While previous literature mostly focuses on firms in the financial sector (e.g., Goetz et al., 2016; Chu et al., 2020; Doerr and Schaz, 2021), we contribute to this literature by showing that the geographic concentration of non-financial firms can significantly impact their default risk, as geographically concentrated firms exhibit more pronounced responses to a localized tax shock.

Second, by showing significant spillover effects of corporate taxes on the default risk of

[1] Market-based measures, such as bond ratings, face several limitations. First, these measures are only available at the firm level and therefore do not allow for precise consideration of local taxes. Second, they are typically available only for the subset of large firms with publicly traded bonds, limiting coverage and introducing sample selection bias. Third, they often incorporate noise from macroeconomic conditions, which may obscure the effects of state-level tax changes.

firms across establishments, our paper contributes to the literature that shows that local shocks can have spillover effects through firms' internal networks. Within this literature, Giroud and Mueller (2015) examine multi-establishment firms' internal capital markets. They show that when one plant in a firm experiences a positive shock to investment opportunities, financially constrained firms reallocate resources internally: investment and employment rise at the treated plant, but decline at the firm's other plants in a similar magnitude. Giroud and Mueller (2019) document that local economic shocks propagate across regions via firms' internal networks. More similar to our paper, Giroud and Rauh (2019) show how differences in state corporate taxes affect multi-state firms. C-corporations significantly cut employment and reduce the number of establishments in states after corporate tax increases, while pass-through firms (which are not subject to corporate income taxes) do not. About half of the lost economic activity in a tax-hiking state is reallocated to other states where the firm has establishments, evidencing spillover relocation of business across state lines.

Third, our study is also related to papers that examine adjustments in capital structure as a specific channel through which corporate taxes influence default risk. For example, Faccio and Xu (2015) exploit tax reforms across OECD countries and find that higher corporate income tax rates lead to increased leverage. Heider and Ljungqvist (2015) use staggered changes in U.S., state-level corporate income tax rates and find that firms increase their debt ratios when tax rates increase. Focusing on private firms, Faccio and Xu (2024) investigate how the introduction of limitations to the full deductibility of interest expenses affect capital structure decisions. Exploiting discontinuities around the thresholds that trigger these limitations, the paper shows that affected firms reduce leverage relative to their unaffected peers. In contrast to these results, Ivanov et al. (2024) document that state-level corporate income tax rate reductions lead firms to increase their leverage, a result that is particularly pronounced among small private firms.[2] We contribute to this literature by showing that an exogenous tax policy shock affects default risk through the leverage channel.

Last but not least, although previous research documents a correlation between debt and distress (e.g., Cathcart et al., 2020; Traczynski, 2017), recent evidence in Bhardwaj et al.

[2] Other studies of tax reforms include, but are not limited to, Doidge and Dyck (2015), Fleckenstein et al. (2020), Givoly et al. (1992), and Rajan and Zingales (1995).

(2025) shows that dividend recapitalizations, which lead to significant increases in firm debt, causally lead to a significant increase in financial distress. This finding is consistent with finance theories that predict that higher leverage increases the risk of bankruptcy despite its tax benefits (e.g., Leland, 1994; Fan and Sundaresan, 2000). However, identifying the causal effect of leverage on distress is challenging, as less risky firms tend to select higher debt ratios (Myers, 2001; Titman et al., 2005). We advance this literature by providing evidence that an exogenously driven incentive to reduce leverage results in a lower default risk.

The remainder of the paper is organized as follows. Section 2 describes the data. Section 3 examines the impact of state-level corporate income tax rate changes on establishment-level default risk. Section 4 investigates the effects of the interest limitation rule introduced by the TCJA and provides direct evidence on the channels through which taxation affects default risk. Section 5 concludes.

2 Data

To investigate our research questions, we collect data on corporate taxes as well as granular establishment- and firm-level data on U.S. public companies.

2.1 State taxes.

We collect data on state taxes from Baker et al. (2025). This dataset covers all major U.S. state-level taxes, including corporate income tax rates, as well as personal income, sales, and property taxes.

The primary variable of interest in our empirical analysis is the corporate income tax rate. Figure 1 illustrates these rates for each state (identified by its FIPS code). From 1991 to 2022, there is substantial variation both across and within states. In the cross-section, states exhibit considerable differences in corporate income tax rate levels, with some maintaining rates close to or equal to zero, while others impose tax rates exceeding 10%. In particular, Nevada (FIPS 32), South Dakota (FIPS 46), and Wyoming (FIPS 56) did not levy any corporate income tax during the period analyzed. Some states changed their corporate tax rates only

once, whereas most experienced small, gradual adjustments rather than large, discrete shifts. Overall, the figure underscores the rich variation in corporate income tax policy that forms the basis of our identification strategy.

[Insert Figure 1 about here]

2.2 Counties along state borders.

Our empirical strategy largely focuses on geographically adjacent treated and control counties located along state borders. By comparing establishments in bordering counties across state lines, we mitigate the risk that unobserved local economic factors may drive our results (Dube et al., 2010; Mukherjee et al., 2017). In addition, we acknowledge that counties located along state borders in the western U.S. states tend to be significantly larger, which may prevent them from sharing similar local economic conditions. Therefore, to strengthen the credibility of this approach, we follow Dube et al. (2010) and, in some tests, further restrict our sample to county pairs whose centroids lie within 75 km of each other. Counties that satisfy this criterion are colored dark green in Figure 2.

[Insert Figure 2 about here]

2.3 Establishment-level data.

Our source of establishment-level data is the National Establishment Time Series (NETS) database, which covers all U.S. establishments that belong to private and publicly traded firms from 1990 to 2022.

We restrict our attention to publicly traded companies because they are organized as C-corporations and, therefore, subject to corporate income taxes. In contrast, the tax status of private firms is unobservable in our data, as their legal form is not reported in NETS.

Most private firms are structured as pass-through entities, which are generally not subject to corporate income taxes.

The database includes the name of each establishment, the name of the corporate entity it belongs to, unique identifiers for both establishments and firms, as well as detailed information on the industry and location of each establishment. Importantly for our analysis, NETS also provides the Paydex score of each establishment. Paydex scores in NETS have been used, for example, by Barrot and Nanda (2020), Borisov et al. (2021), Akey and Appel (2021), and Chava et al. (2023).

The Paydex score is a dollar-weighted measure of how promptly an establishment pays its bills, derived from trade experiences reported to Dun & Bradstreet (D&B) by over 4,000 trade exchange participants in the United States. Lenders, insurance providers, and lessors allegedly use these credit scores to determine terms for credit, loans, lease payments, and insurance premiums (Becker, 2012; Bennett and Wang, 2021; Chava et al., 2023). A low Paydex score can lead to tighter financing conditions, higher financing costs, or reduced credit access. As such, the score is a forward-looking indicator of an establishment's default risk and plays a central role in our empirical analysis.

The Paydex score ranges from 0 to 100. A score of 80 indicates on-time payment, while scores above 80 signal early payments, and scores below 80 indicate payment delays. For example, a drop from 80 to 79 corresponds to a delay of about two days beyond terms. Importantly, this score has been shown to strongly predict business failure and financial distress (Wilson et al., 2000; Kallberg and Udell, 2003; Chava et al., 2023).

2.4 Firm-level data.

We obtain firm-level balance sheet and income statement data from Compustat. Specifically, we use firm-level data on interest expenses, earnings before interest and taxes (EBIT), earnings before interest, taxes, depreciation, and amortization (EBITDA), total assets, total debt, cash holdings, corporate income taxes, capital expenditures, return on assets (ROA), market capitalization, and the ratio of net interest expenses to EBITDA.

We use this information to identify firms affected by the interest limitation rule introduced

by the TCJA and to assess the impact of this limitation on the corporate financial policies of these affected firms. To perform this empirical analysis, we merge NETS with Compustat using a fuzzy matching procedure based on the names of the headquarters of the establishments and the names of NETS firms in Compustat. This matching procedure is described in detail in Appendix A.

2.5 Summary statistics.

In the first part of our analyses, we restrict our tests to establishments that belong to publicly traded firms for which both the Paydex score and the state-level corporate income tax rate are available. We exclude establishments operating in the financial sector (NAICS code 52). After constructing our final sample, all the variables used in our empirical analyses are winsorized at the 1st and 99th percentiles. Summary statistics are reported in Table 1. A detailed description of all variables and data sources is available in the Appendix Table B1.

The sample includes 2,995,866 establishment-year observations, 346,665 unique establishments, and 13,470 unique firms, over the period from 1991 to 2022. For what concerns our main variables of interest, the average Paydex score is 70.7, with a median of 73 and a standard deviation of 9.3, and the average state-level corporate income tax rate is 6.7%, with a median of 7.0% and a standard deviation of 2.7%.

[Insert Table 1 about here]

3 Corporate income tax rates and default risk

In the United States, corporate income taxes are levied at both the federal and state levels. While the firm's consolidated tax liabilities are determined at the headquarters level, the corporate income tax owed to a state depends on the location of the firm's establishments and their associated economic activity. This is because most U.S. states apply formulary apportionment rules that allocate a firm's nationwide income to each state based on the

distribution of factors such as sales, payroll, and property. Therefore, an establishment's location directly determines its exposure to a given state's corporate income tax regime.

This institutional feature motivates our use of establishment-level data. This approach allows us to credibly estimate the effects of state-level corporate tax changes on default risk, exploiting the fact that establishments in different states are subject to heterogeneous tax treatments. More specifically, we estimate the impact of corporate income tax rates on the default risk of establishments. To do so, we consider the following Equation (1).

$$\text{Paydex}_{i,t} = \eta_i + \eta_{j,t} + \beta \text{Corporate Income Tax Rate}_{s,t} + \Gamma X_{s,t} + \epsilon_{i,t} \tag{1}$$

In this specification, the outcome variable is the Paydex score of establishment i at time t. The key explanatory variable of interest is $Corporate\ Income\ Tax\ Rate_{s,t}$, the corporate income tax rate in force in state s in year t. We control for establishment (η_i) and industry-by-year ($\eta_{j,t}$) fixed effects. Industry is defined by the four-digit NAICS code. To mitigate the possibility that our results may be driven by other state-level tax changes, we follow Baker et al. (2025) and control for sales, property, and (both median and top) personal income tax rates ($X_{s,t}$). Finally, we cluster standard errors at the state level, which corresponds to the level at which treatment occurs (Bertrand et al., 2004; Abadie et al., 2023).

In a second test, we combine this approach with a border discontinuity design. More specifically, after focusing exclusively on the counties along a state border, following Dube et al. (2010), we match each establishment in a county within the border region with all establishments in neighboring counties across state lines. As a consequence, establishments may appear multiple times depending on the county pair they belong to. This is indicated by the subscript p in the following Equation:

$$\text{Paydex}_{i,p,t} = \eta_i + \eta_{j,t} + \eta_{p,t} + \beta \text{Corporate Income Tax Rate}_{s,t} + \Gamma X_{s,t} + \epsilon_{i,p,t} \tag{2}$$

In this setting, to ensure a valid comparison of contiguous counties, we incorporate pair-by-year fixed effects ($\eta_{p,t}$). These fixed effects capture shared local economic shocks and

trends. In additional specifications, we focus only on counties located along state borders whose centroids are less than 75 km apart, which are therefore even more likely to be similar in observable and unobservable characteristics (Dube et al., 2010).

Following Barrot and Nanda (2020), we collapse Equation (2) into the following Equation in first differences:

$$\Delta \text{Paydex}_{i,p,t} = \eta_{p,t} + \eta_{j,t} + \beta \Delta \text{Corporate Income Tax Rate}_{s,t} + \Gamma \Delta X_{s,t} + \epsilon_{i,p,t} \quad (3)$$

A key advantage of using a first-difference model in our setting is that many corporate tax changes are often small and evolve gradually over time rather than taking the form of large, discrete shocks. By differencing outcomes across adjacent years, we isolate within-establishment variation that aligns more directly with changes in local tax policy, while controlling for local economic trends through pair-by-year fixed effects. First differences also accommodate the repeated shocks and reversals in tax changes that we observe in the data (Heider and Ljungqvist, 2015).

The regression results are presented in Table 2. In Column (1), we present the results for the full sample. In Column (2), we restrict the analysis to establishments located on state borders to improve comparability between treated and control groups. In Columns (3) and (4), we gradually control for other contemporaneous changes in tax policies and industry-level aggregate trends. In Column (5), we adopt a border discontinuity approach, matching establishments across state lines and including county pair-by-year fixed effects. In Column (6), we further restrict the sample to counties whose centroids are less than 75 km apart.

In the full sample, corporate income tax rate changes are not statistically significantly related to default risk. However, the coefficient of interest is negative and statistically significant in all the specifications that focus on counties along border lines. This result underscores the fundamental need to adequately control local economic conditions. In our preferred specification, shown in the last Column, we focus on county pairs with centroids less than 75 km apart, and include county pair-by-year fixed effects. We document that a one percentage

point increase in the corporate income tax rate leads to a 0.060 reduction in the Paydex score. This effect is economically meaningful, representing 23% of the average annual change in the Paydex score (-0.257).

[Insert Table 2 about here]

3.1 Robustness tests.

We provide several pieces of evidence to support the robustness of our findings. In Appendix Table B2, we show that our results hold when we focus the analysis on the six years surrounding a state-level corporate income tax rate change. As reported in Appendix Table B3, our results are also robust in excluding states without corporate income taxes (Nevada, South Dakota, and Wyoming). As an additional test, we show that our coefficients are statistically significant when we alternatively cluster the standard errors at the firm, industry, or county level. We report these results in Appendix Table B4.

3.2 The role of geographic diversification.

We investigate whether the effect of corporate tax rates on default risk is heterogeneous depending on the geographic footprint of firms. This analysis is motivated by the presumption that firms with more geographically concentrated operations should be more exposed to localized tax shocks.

To test this hypothesis, we compute the share of employees of a firm that operate in the same state as the establishment under consideration, and interact this variable with Δ Corporate Income Tax Rate, as formalized in Equation (4).

$$\begin{aligned}\Delta\text{Paydex}_{i,p,t} = {} & \eta_{p,t} + \eta_{j,t} + \beta_1 \Delta\text{Corporate Income Tax Rate}_{s,t} + \beta_2 \text{Share of Employees}_{i,t-1} \\ & + \beta_3 \left(\text{Share of Employees}_{i,t-1} \times \Delta\text{Corporate Income Tax Rate}_{s,t}\right) \\ & + \Gamma \Delta X_{s,t} + \epsilon_{i,p,t}\end{aligned} \tag{4}$$

We report the results in Table 3. We find that the interaction term is negative and statistically significant across the specifications focusing on counties located along state borders. More specifically, according to the interaction coefficient, when the share of employees of the firm in the affected state is hypothetically equal to one (that is, 100%), the Paydex score declines by 0.379 units following a one percentage point increase in the corporate income tax rate.

This result indicates that geographic concentration amplifies a firm's financial vulnerability to localized tax shocks. When a large share of a firm's operations, and hence its tax exposure, is concentrated in a single state, even modest increases in local corporate income tax rates can have economically significant effects on the establishments' default risk. In contrast, firms with a more geographically dispersed structure are better positioned to buffer localized shocks through strategic tax planning. This finding highlights the importance of geographic diversification not only as a business strategy but also as a risk management tool in the presence of decentralized taxation.

[Insert Table 3 about here]

3.3 Spillover effects.

Because of the joint legal liability of establishments that belong to the same legal unit, changes in the tax burden of one establishment may affect the default risk of other establishments that belong to the same firm. We, therefore, investigate whether corporate income taxes indeed generate intra-firm spillover effects.

To test this hypothesis, we construct the following variable:

$$\text{Spillover Effects}_{i,f,t} = \frac{\sum_{k \in F_{f,t}, k \neq i} \Delta \text{Corporate Income Tax Rate}_{k,t}}{N_{f,t} - 1} \tag{5}$$

This leave-one-out spillover measure captures the average tax shock experienced by all other establishments of the same firm, excluding the focal establishment itself. More specifically, in Equation (5), $F_{f,t}$ denotes the set of establishments belonging to firm f in year t, and

k indexes each establishment in that set other than establishment i. The numerator sums the tax rate changes across all such establishments k, while the denominator $N_{f,t}$ - 1 reflects the number of other establishments in the firm.

We add this variable to Equation (3), and report the estimation results in Panel A of Table 4. According to the coefficient of our preferred specification, reported in the last column, we find that a one-unit increase in this variable, that is, all the establishments of a firm are exposed to one percentage increase in the corporate income tax rate, further decreases the Paydex score by 0.269 units, suggesting significant spillover effects across establishments of the same firm.

In a further test, we create a spillover measure exclusively based on exposure to tax changes of the establishments in other states. We report the results in Panel B of Table 4. We still find negative and significant effects of tax exposure of establishments in other states on the default probability of an establishment. More specifically, according to the coefficient of the specification reported in the last column, we find that a one-unit increase in this variable decreases the Paydex score by 0.228 units.

This cross-state exposure implies that state-level tax policy can have unintended (or, perhaps, intended) consequences beyond its jurisdiction, undermining the fiscal sovereignty of other states. These spillovers introduce a form of fiscal externality that is rarely considered in tax research.

[Insert Table 4 about here]

4 The Tax Cuts and Jobs Act of 2017

Having established that changes in state-level corporate income tax rates significantly affect establishments' default risk, we investigate the underlying mechanisms driving this effect. For this purpose, we focus on Section 163(j) of the Tax Cuts and Jobs Act (TCJA). Two primary channels may explain the observed increase in default risk associated with tax rate changes.

First, ceteris paribus, higher tax rates reduce establishments' cash flows, thereby weakening their financial health. Second, ceteris paribus, higher tax rates increase the incentive to use more debt in the capital structure, which, in turn, increases default risk. While both channels predict an increase in default risk in response to a higher corporate income tax rate, they diverge in their implications regarding the interest deductibility limitation introduced under Section 163(j).

This provision restricts the deductibility of interest payments for affected firms. On the one hand, by reducing allowed interest deductions, ceteris paribus, Section 163(j) lowers cash flows and, thereby, should increase default risk. On the other hand, the provision also diminishes the tax shield benefit of debt, thereby reducing the incentive to issue debt, which could decrease default risk. With this trade-off in mind, this section examines which of these two channels dominates in explaining the change in default risk, if any, among firms subject to the interest limitation rule. We also analyze firms' financial statements to provide direct evidence of any associated changes in financial policies.

4.1 Institutional framework.

The TCJA was a legislative priority of the first Trump administration. The goal was to simplify the tax system and promote economic growth. The law was introduced in the House of Representatives on November 2 and signed into law by President Donald Trump on December 22, 2017. Most of the corporate tax provisions in the TCJA took effect on January 1, 2018.

Importantly for our paper, the TCJA introduced a new limitation on the deductibility of business interest expenses under §163(j). Starting on January 1, 2018, firms could deduct net interest expenses only up to 30% of their adjusted taxable income (initially based on EBITDA; after 2021, based on EBIT). The Coronavirus Aid, Relief, and Economic Security (CARES) Act of 2020 retroactively increased the deduction limit from 30% to 50% for the taxable years beginning in 2019 and 2020, respectively.[3]

[3] The TCJA of 2017 introduced several other important changes to taxation in the U.S. First, it slashed the federal corporate income tax rate from 35% to 21%. Second, it replaced the worldwide taxation system with a modified territorial system. To transition into this new system, the TCJA imposed a one-time tax on the

4.2 Identifying firms affected by the interest deductibility limitation.

We use information from Compustat to identify firms affected by the interest deductibility limitation introduced by the TCJA. To do so, we follow the methodology of Faccio and Xu (2024). Starting in 2018, the TCJA limits the deductibility of interest expenses to 30% of EBITDA. Therefore, we define a firm as *treated* if, based on its pre-reform financials (averaged over the period 2015–2017)[4], the amount of interest expense that exceeds the new deductibility cap is strictly positive. This indicates that the firm is newly subject to the limitation and faces a higher tax burden post-reform.

More specifically, we define the following variables:

- $\text{EBITDA}_f^{\text{pre}}$ is the sum of EBITDA over 2015–2017 for firm f,
- $\text{EBIT}_f^{\text{pre}}$ is the sum of EBIT over 2015–2017 for firm f,
- $\text{Interest}_f^{\text{pre}}$ is the sum of interest expenses over 2015–2017 for firm f,
- $\text{Cap}_f = max(0, 0.30 \times \text{EBITDA}_f^{\text{pre}})$ is the maximum deductible interest under Section 163(j) of the TCJA.

Next, we compute the change in non-deductible interest as:

$$\begin{aligned}\Delta\text{Non-deductible Interest}_f = &\max\left(0, \min\left(\text{EBIT}_f^{\text{pre}}, \text{Interest}_f^{\text{pre}}\right)\right) \\ &- \max\left(0, \min\left(\text{EBIT}_f^{\text{pre}}, \text{Interest}_f^{\text{pre}}, \text{Cap}_f\right)\right) \end{aligned} \tag{6}$$

The treatment indicator is then defined as:

$$\text{Treated}_f = \begin{cases} 1 & \text{if } \Delta\text{Non-deductible Interest}_f > 0 \\ 0 & \text{otherwise} \end{cases} \tag{7}$$

deemed repatriation of certain non-previously taxed foreign earnings.

[4] We use three averages to account for noise in the accounting data.

4.3 A comparison between treated and untreated firms.

Using Compustat, we identify 462 firms as treated and 5,095 firms as not treated. After removing firms with less than an average of 25 million dollars in average revenues during the period 2015-2017, which are exempt from the interest limitation rule, we obtain a sample of 425 treated and 3,353 control firms.

A potential concern with this sample is that firms above and below the threshold imposed by the interest limitation rule may still differ in various observable and unobservable characteristics that could lead to divergent trends in default risk even in the absence of treatment. To investigate this point, Table 5 compares key financial characteristics between treated and untreated firms in 2017, using simple t-tests.

As reported in Table 5, the treated firms are significantly larger than the control firms before the reform. They are also more heavily leveraged and invest a lower share of assets in capital expenditures. They are less likely to pay dividends, although this difference is not statistically significant. In terms of valuation, treated firms exhibit significantly lower market-to-book ratios. Operating performance, as measured by return on assets, is also higher for treated firms. (The definitions of the variables are provided in Appendix Table B1.)

[Insert Table 5 about here]

Considering that firms above and below the threshold differ in several observable characteristics, relying on a simple difference-in-differences estimator may yield biased estimates (Heckman et al., 1997). We address this concern in two ways. First, we combine the difference-in-differences approach with propensity score matching. Specifically, within the same broad sector (two-digit NAICS code), we match treated and control firms based on the firm characteristics listed in Table 5 using a one-to-five nearest neighbor matching with a caliper of 0.05, retaining only observations within the region of common support.

Figure 3 confirms that, after matching, the treated and control groups are nearly identical across all the major firm characteristics. More specifically, it illustrates the standardized differences in covariates between treated and control firms, before and after propensity score matching. Before matching, several key variables (such as the debt ratio) exhibit large imbalances, with standardized differences exceeding 50%. Other variables, including firm size (log assets) and capital expenditures, also show notable pre-matching differences. After matching, the standardized differences for all covariates are substantially reduced and close to zero, indicating an improved balance between the treated and control groups. Furthermore, as shown in Table 6, there are no statistically significant differences between the two groups across any variable after the matching. This supports the validity of our matching procedure in creating a more comparable control group to estimate the effect of the TCJA on default risk.

[Insert Figure 3 about here]

[Insert Table 6 about here]

Second, we further restrict our matched sample to firms operating near the threshold set by Section §163(j) of the Tax Cuts and Jobs Act (TCJA). This approach aims to mitigate concerns that treated and control firms, even after propensity score matching, might still differ on unobservable characteristics not accounted for in the matching procedure. Specifically, we focus on firms whose interest-to-EBITDA ratios fall within a 20%-40% range – i.e., within ± 10 percentage points of the 30% deductibility cap.

4.4 Difference-in-differences.

We consider a difference-in-differences approach and estimate Equation (8):

$$\text{Paydex}_{i,t} = \eta_i + \theta_{s,t} + \theta_{j,t} + \beta \text{Post}_t \times \text{Treated TCJA}_f + \epsilon_{i,t} \tag{8}$$

In this setting, Treated TCJA is an indicator equal to 1 if the establishment belongs to a treated firm f, as defined above. Post is an indicator equal to 1 for years after 2017. We

include establishment fixed effects to account for establishment time-invariant characteristics and industry-by-year and state-by-year fixed effects to control for dynamic differences across geographic areas and industries (Davydenko et al., 2012).

For consistency with our previous analyses, we estimate the following first difference model, focusing on the event year of the TCJA:

$$\Delta\text{Paydex}_i = \theta_s + \theta_j + \text{Treated TCJA}_f + \epsilon_{i,t} \tag{9}$$

In Equation (9), Δ Paydex is the change in the Paydex score of an establishment i between 2018 and 2017. We focus on the two years immediately preceding and following the introduction of the TCJA to estimate the short-term effect of this policy on default risk. Furthermore, we do so to avoid contamination from the CARES Act, which retroactively changed the limitation rule in 2020. We also control for industry fixed effects (θ_j) and state fixed effects (θ_s). We cluster the standard errors at the firm level, which is the level at which the treatment takes place (Bertrand et al., 2004; Abadie et al., 2023).

In Table 7, we report the results for both the whole sample, as well as for the sample of establishments that belong to matched firms. We document that treated firms, those subject to the interest deduction limitation experience a reduction in default risk (Columns 1-4). When considering the full sample of establishments, the estimated coefficient of 0.415 in Column (2) implies that treated establishments experience an increase in their Paydex score of 0.415 units relative to control establishments in the year of the reform. The coefficient is even larger and equal to 0.574 in Column (4), when we consider the sample of firms with similar characteristics before the reform, which has been created using the propensity score matching. The effect is economically large, representing 223% of the average annual change in the Paydex score.

As discussed above, we further restrict our analysis to the sample of establishments of firms operating around the threshold triggering the limitation to the full deductibility of interest payments introduced by §163(j) of the TCJA. The estimates reported in the last two columns confirm the robustness of our results. Furthermore, the coefficient of interest is even

larger, and equal to 0.660, or 257% of the average annual change in the Paydex score. Overall, the results reported in this section support the notion that the tax shield channel dominates the effect of taxes on cash flows.

[Insert Table 7 about here]

4.5 Heterogeneity of the effects depending on financial constraints.

We explore potential heterogeneity in treatment effects. Specifically, we test whether the impact of the TCJA varies with the degree of financial constraints and the firm's size. Ivanov et al. (2024) document that the risk of default decreases with higher tax rates, but only for small, financially constrained, private firms. To examine whether this result is present in our sample of publicly traded firms, in addition to the natural logarithm of total assets as a proxy for firm size, we employ three commonly used measures of financial constraints: the Size–Age (SA) index of Hadlock and Pierce (2010), the KZ index of Kaplan and Zingales (1997), and the WW index of Whited and Wu (2006). Each variable is standardized. Therefore, the interaction with the treatment variable captures the effect of a one standard deviation increase in financial constraints or size for treated firms.

The results, reported in Table 8, show no statistically significant differential effects across any of the proxies. That is, the estimated treatment effects on Paydex scores do not systematically differ between more and less financially constrained firms. We similarly find no evidence of differential effects based on firm size. Thus, the results of Ivanov et al. (2024) do not extend to the sample of U.S. publicly traded firms.

[Insert Table 8 about here]

4.6 Parallel trends assumption and business score dynamics.

To assess the validity of the parallel trends assumption underlying our difference-in-differences strategy, we estimate the following event-study specification:

$$\text{Paydex}_{i,t} = \sum_{t=2014, t\neq 2017}^{t=2022} \beta_t \cdot \text{Treated TCJA}_f \times \text{Year}_t + \eta_i + \theta_{j,t} + \epsilon_{i,t} \tag{10}$$

In Equation (10), we interact the treated variable with year indicators for the period 2014–2022, omitting 2017 (the final pre-treatment year), which is used as the reference year. The specification includes establishment fixed effects (η_i) and industry-by-year fixed effects ($\theta_{s,t}$). The coefficients β_t capture the difference in Paydex scores between treated and control establishments for each year relative to 2017.

The estimated β_t coefficients and the relative standard errors are displayed in Figure 4. We report the results for the whole sample, for the sample of establishments of matched firms, and for the establishments of matched firms operating near the threshold set by §163(j) of the Tax Cuts and Jobs Act (TCJA). The results support the key identifying assumption of parallel trends: the coefficients for the years before the reform (2012–2016) are generally not statistically significant, except for the year 2014. In contrast, starting in 2018 (the first year of implementation of the TCJA), we observe a positive and statistically significant relative increase in the Paydex score of treated establishments. The effect is long-lasting, and the magnitude of the yearly effect grows in subsequent years. Furthermore, the coefficients become more precise as matching quality improves, suggesting that potential omitted variables are unlikely to bias our findings in any meaningful way.

[Insert Figure 4 about here]

To further test whether the effect of the reform is long-lasting, we estimate Equation (9) for the period 2012-2022, which spans five years before and after the tax event. To do so, we interact the treatment indicator with the variable Post, and include industry-by-year and state-by-year fixed effects. We report the results in Appendix Table B5. We still find positive

and significant effects across our different approaches. The coefficient of 0.253 in Column (6) indicates that affected firms experience an annual increase in the Paydex score of 0.239, or a total increase of 1.195 (0.239 $\times$ 5) over the five years that follow the introduction of the TCJA.

4.7 Mechanisms.

To better understand the mechanisms underlying our results, we analyze, at the firm level, key adjustments to financial policies following the TCJA. The reform reduces the tax advantage of debt financing and may lead firms to adjust their capital structure accordingly.

To investigate this possibility, we use information from Compustat and consider a database at the firm-year level. We next estimate the following Equation:

$$\text{OUTCOME}_{f,t} = \alpha_f + \gamma_{s,t} + \gamma_{j,t} + \beta \text{Post}_t \times \text{Treated TCJA}_f + \epsilon_{i,t} \tag{11}$$

In this specification, $OUTCOME$ is an alternative outcome variable of firm f at time t. More specifically, we consider the debt ratio, the effective income taxes, the interest expense ratio, and the cash holding ratio. α_f, $\gamma_{s,t}$, and $\gamma_{j,t}$ are firm, industry-by-year, and state-by-year fixed effects. Treated TCJA is still an indicator equal to one if the firm is treated. Standard errors are clustered at the firm level.

For consistency with the analyses reported in Table 8, we report the results using the following first differences transformation around the TCJA event, that is, the differences between 2017 and 2018.[5]

$$\Delta\text{OUTCOME}_{f,t} = \gamma_s + \gamma_j + \beta \text{Treated TCJA}_f + \epsilon_{i,t} \tag{12}$$

The results in Table 9 indicate economically significant adjustments by treated firms following the TCJA. In Column 1, the overall debt ratio declines by 1.6 percentage points. This reduction is statistically significant at the 10% level, suggesting that the reform triggered a wide deleveraging. This result is in line with recent evidence documented by Faccio and

[5] We report the summary statistics for this corporate-level sample in Appendix Table B6.

Xu (2024), Heitzman and Hanlon (2024), and Sanati and Beyhaghi (2025).

At the same time, in Column (2), we document a significant decrease in interest expenses, which is consistent with the observed decline in leverage. By lowering interest-bearing debt, firms mitigated the additional tax burden associated with the increase in non-deductible interest expenses. This behavioral response highlights how corporate financial policies adjust in response to tax-induced disincentives, reinforcing the view that tax reforms can have meaningful effects on corporate decisions.

At least in part because firms reduce their leverage in response to the interest deductibility limitation, in Column (3) we do not observe a significant change in their effective income tax rate. This is likely because the lower interest payments offset the effect of the new limitation, leaving the overall tax burden largely unchanged *vis-à-vis* unaffected firms. In other words, firms proactively adjusted their capital structure enough to avoid an increase in taxable income resulting from an increase in non-deductible interest expenses.

In parallel, treated firms increased their cash holdings by 0.1 percentage points (Column 4), a moderate increase that is not statistically significant. This likely reflects a precautionary response to the reform, consistent with affected firms seeking greater liquidity and reduced dependence on debt in a relatively more restrictive tax environment.

[Insert Table 9 about here]

To further test whether these effects are long-lasting, we estimate Equation (12), focusing on the period 2012 and 2022, that is, the five years before and after the event. Again, we interact the treatment indicator with the variable Post, including industry-by-year and state-by-year fixed effects. We report the results in Appendix Table B7. We find significant effects for all the different variables of interest. For example, the debt ratio drops annually by 0.014 percentage points, or a total of 0.070 percentage points (0.014×5) over the period 2018-2022.

Overall, the magnitude of the coefficients documented in this section implies that the TCJA induced meaningful shifts in firms' financing behavior, especially in reducing the leverage ratio. This adjustment is statistically significant and economically large. It is because of these changes that the default risk of affected firms decreases, rather than increasing, as

documented by Ivanov et al. (2024) for small private U.S. firms. Thus, in net terms, the tax incentive effect dominates the cash flow effect in our sample of publicly traded firms.

5 Conclusions

We study the impact of corporate income taxes on business default risk by leveraging a unique dataset that links establishment-level default risk to firm-level financial policies and tax variables. This dual-level approach is essential because state-level corporate tax liabilities are determined at the establishment level. By integrating detailed data across these dimensions, we can precisely trace how tax changes at the local level propagate through different business units and ultimately influence financial distress. This setting allows us to move beyond firm averages and quantify the heterogeneous and firm-wide effects of tax policy, including both localized shocks and intra-firm spillovers.

Exploiting the staggered implementation of tax policies across states and using a border discontinuity approach to account for differences in local economic conditions, we document that higher corporate income tax rates increase default risk. More importantly, we show that this effect strongly depends on the location of all establishments and their exposure to state-level corporate income taxes. More specifically, we show that this effect is greater when firms are geographically concentrated. We also document substantial spillover effects of corporate income taxes across establishments within the same firm, including those located in other states.

We further document *how* taxes shape establishment-level default risk by analyzing the limited interest deductibility provision introduced by the 2017 Tax Cuts and Jobs Act. Doing so allows us to determine whether the impact of tax rate changes on default risk documented in the first part of the paper reflects lower cash flows or, rather, reflects adjustments in firms' financial policies. In this setting, we document a significant decrease in default risk for affected firms. At the parent-firm level, we also find a relative reduction in leverage and, consequently, in interest expenses. Interestingly, we find no evidence of a relative increase in effective tax rates, despite the cash flow channel predicting precisely such an effect. The

results, rather, indicate that, in this setting, the tax shield channel dominates the cash flow channel.

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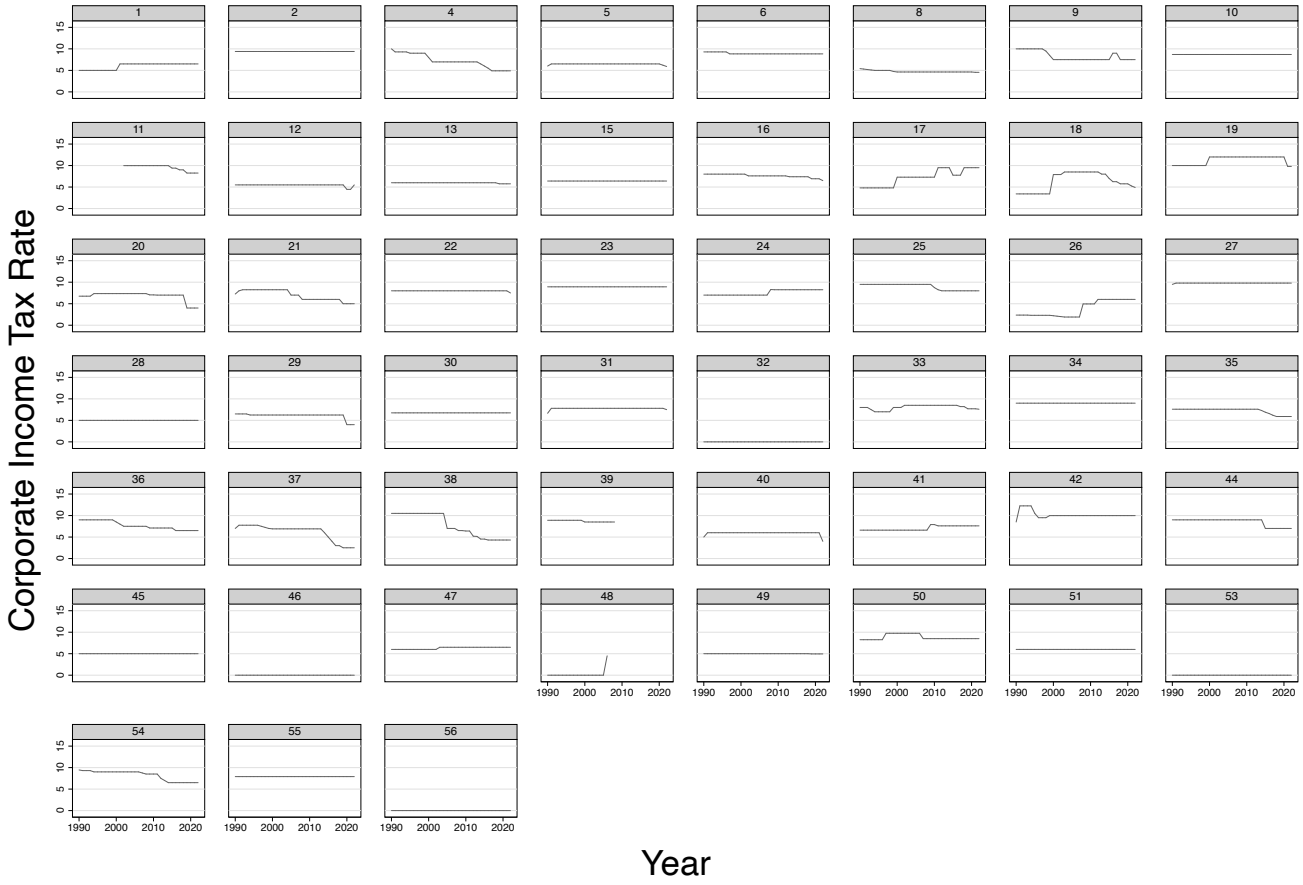
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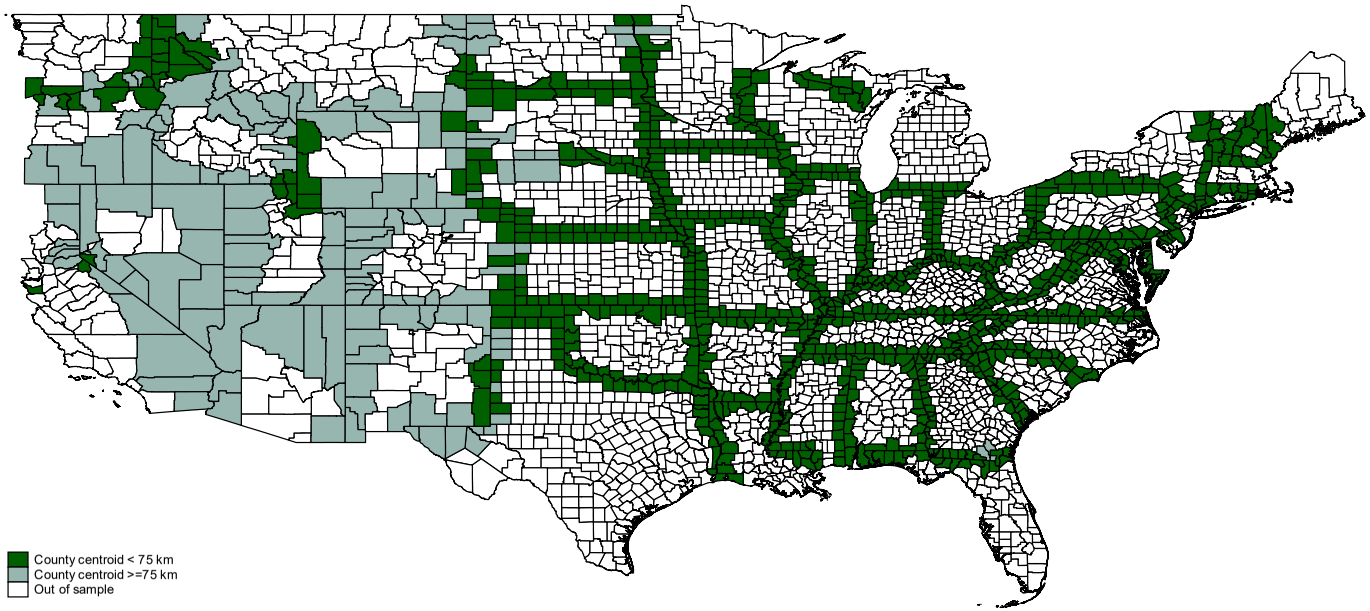
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Figure 1: Corporate income tax rates by state



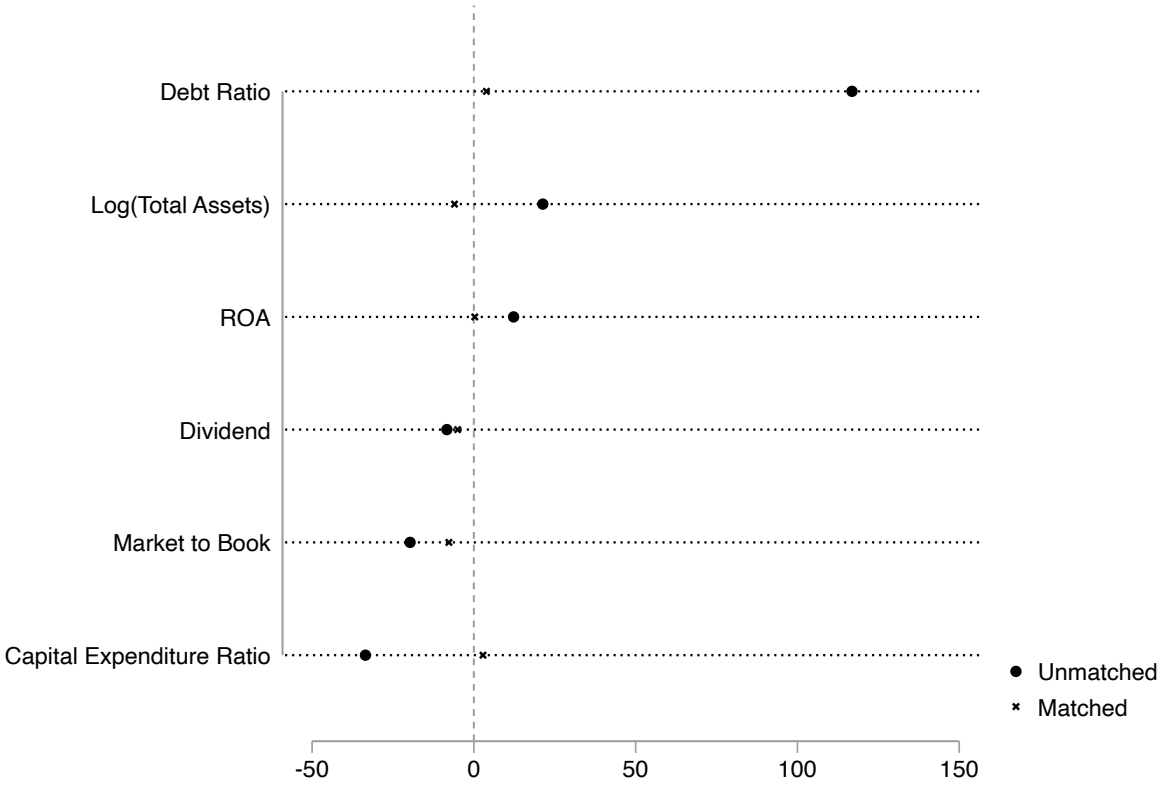
Notes: Figure 1 displays the corporate income tax rates for each state, identified by their FIPS code, over the period 1991–2022. The source of this information is Baker et al. (2025).

Figure 2: Counties along state borders



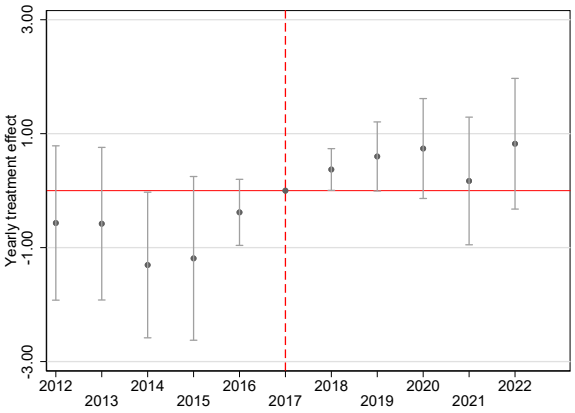
Notes: Figure 2 shows all the counties located along state borders, highlighted in dark and light green. Counties in dark green are those whose centroids are less than 75 km apart, indicating closer geographic proximity used in our preferred specification (Dube et al., 2010). Alaska and Hawaii are not included in the map because they do not share borders with other U.S. states.

Figure 3: Matched characteristics

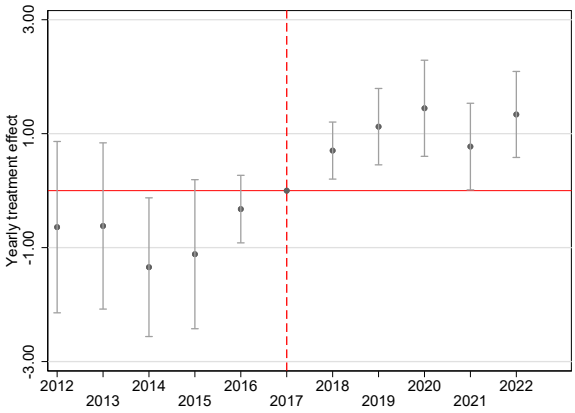


Notes: Figure 3 displays the standardized mean differences, before and after matching, for the covariates used in the propensity score matching. Variables are ordered by the magnitude of pre-matching standardized differences (from largest to smallest) to facilitate visual interpretation of the matching effectiveness. The horizontal axis shows the standardized differences in means between treated and control firms, while the vertical axis lists the covariates employed. Two sets of dots are shown for each variable: unmatched (pre-matching differences) and matched (post-matching differences).

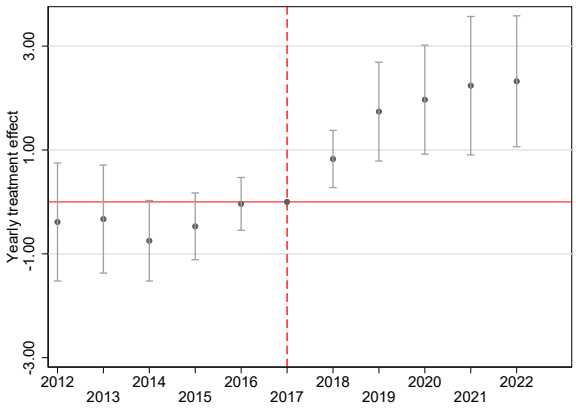
Figure 4: Yearly treatment effects coefficients



(a): Full sample



(b): Matched sample



(c): Threshold sample

Notes: Figure 4 shows the yearly treatment effects from Equation (10) across three different samples. The outcome variable is the Paydex score. The treatment variable is an indicator equal to one if the establishment is affected by the TCJA. All regressions include establishment and industry-by-year fixed effects. Plots display yearly point estimates with 95% confidence intervals based on firm-level clustered standard errors.

Table 1: Summary statistics - Establishment-level database

Variables	Count	Mean	σ	p25	p50	p75
Paydex	2,995,866	70.7483	9.2523	66.0000	73.0000	78.0000
Corporate Income Tax Rate	2,995,866	6.6645	2.6847	5.5000	6.9680	8.8400
Median Personal Income Tax Rate	2,995,866	3.7557	2.1690	2.8750	3.9450	5.2500
Top Personal Income Tax Rate	2,995,866	5.4901	3.4690	3.0700	5.7500	7.7500
Sales Tax Rate	2,995,866	5.4125	1.4383	4.5000	6.0000	6.2500
Property Tax Rate	2,995,866	0.0464	0.1799	0.0000	0.0000	0.0000
Share of Employees	2,995,866	0.1491	0.2350	0.0188	0.0509	0.1515
Spillover Effects	2,934,608	0.0020	0.1153	-0.0364	-0.0038	0.0024
Spillover Effects (Across States)	2,901,456	0.0010	0.1150	-0.0368	-0.0039	0.0015
Δ Paydex	2,995,866	-0.2567	6.2750	-3.0000	0.0000	2.5000
Δ Corporate Income Tax Rate	2,995,866	-0.0051	0.2565	0.0000	0.0000	0.0000
Δ Median Personal Income Tax Rate	2,933,183	0.0136	0.2168	0.0000	0.0000	0.0000
Δ Top Personal Income Tax Rate	2,995,866	0.0213	0.3346	0.0000	0.0000	0.0000
Δ Sales Tax Rate	2,995,866	0.0227	0.1887	0.0000	0.0000	0.0000
Δ Property Tax Rate	2,995,866	0.0001	0.0040	0.0000	0.0000	0.0000

Notes: This table reports descriptive statistics for the variables used in the establishment-level analyses. The analyses span the period from 1991 to 2022.

Table 2: Baseline results - State-level corporate income tax rates

Variable	(1) Δ Paydex	(2) Δ Paydex	(3) Δ Paydex	(4) Δ Paydex	(5) Δ Paydex	(6) Δ Paydex
Δ Corporate Income Tax Rate	-0.018	-0.063*	-0.065**	-0.060**	-0.069**	-0.060**
	(0.032)	(0.032)	(0.027)	(0.024)	(0.029)	(0.028)
Year FEs	Yes	Yes	Yes			
Controlling for other taxes			Yes	Yes	Yes	Yes
Year × Industry FEs				Yes	Yes	Yes
Year × County Pair FEs					Yes	Yes
Sample	All counties	Border counties	Border counties	Border counties	Border counties	Counties <75km apart
Observations	2,995,866	967,716	945,755	945,346	2,074,788	1,816,288
R-squared	0.002	0.002	0.002	0.017	0.020	0.021

Notes: This table shows regression results for Equation (3). The outcome variable is Δ Paydex. Column (1) uses the full sample; Column (2) restricts the analysis to establishments in counties located along state borders. Columns (3)-(4) add controls for other taxes and industry x year fixed effects. Columns (5) and (6) include pair-by-year fixed effects. Column (6) further restricts the analysis to establishments in counties located along state borders, whose centroids are within 75 km of each other. Standard errors clustered at the state level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See Table B1 for a detailed description of our variables and their data sources.

Table 3: Baseline results – The role of geographic concentration

Variable	(1) Δ Paydex	(2) Δ Paydex	(3) Δ Paydex	(4) Δ Paydex	(5) Δ Paydex	(6) Δ Paydex
Δ Corporate Income Tax Rate	-0.004	-0.005	-0.005	-0.023	-0.010	
	(0.036)	(0.046)	(0.041)	(0.037)	(0.040)	(0.037)
Share of Employees	-0.074***	-0.074	-0.078*	-0.107**	-0.123***	-0.103**
	(0.025)	(0.044)	(0.042)	(0.043)	(0.037)	(0.041)
Δ Corporate Income Tax Rate × Share Employees State	-0.167	-0.447***	-0.450***	-0.416***	-0.354***	-0.378***
	(0.104)	(0.127)	(0.125)	(0.115)	(0.101)	(0.097)
Year FEs	Yes	Yes	Yes			
Controlling for changes in other taxes			Yes	Yes	Yes	Yes
Year × Industry FEs				Yes	Yes	Yes
Year × County Pair FEs					Yes	Yes
Sample	All counties	Border counties	Border counties	Border counties	Border counties	Counties <75km apart
Observations	2,995,866	967,716	945,755	945,346	2,074,788	1,816,288
R-squared	0.002	0.002	0.002	0.017	0.020	0.021

Notes: This table shows regression results for Equation (4). The outcome variable is Δ Paydex. We interact Δ Corporate Income Tax Rate with Share of Employees to assess any heterogeneity based on geographic concentration. Column (1) uses the full sample; Column (2) restricts the analysis to establishments in counties located along state borders. Columns (3)-(4) add controls for other taxes and industry x year fixed effects. Columns (5) and (6) include pair-by-year fixed effects. Column (6) further restricts the analysis to establishments in counties located along state borders, whose centroids are within 75 km of each other. Standard errors clustered at the state level are reported in parentheses. Standard errors clustered at the state level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See Table B1 for a detailed description of our variables and their data sources.

Table 4: Baseline results – Spillover effects

Variable	(1) Δ Paydex	(2) Δ Paydex	(3) Δ Paydex	(4) Δ Paydex	(5) Δ Paydex	(6) Δ Paydex
Panel A: Spillover effects from all the establishments						
Δ Corporate Income Tax Rate	0.002	-0.043	-0.047*	-0.047*	-0.052*	-0.046
	(0.032)	(0.032)	(0.027)	(0.024)	(0.031)	(0.029)
Spillover Effects	-0.365***	-0.410***	-0.382***	-0.324***	-0.352***	-0.269**
	(0.079)	(0.095)	(0.095)	(0.102)	(0.113)	(0.116)
Year FEs	Yes	Yes	Yes			
Controlling for changes in other taxes			Yes	Yes	Yes	Yes
Year × Industry FEs				Yes	Yes	Yes
Year × County Pair FEs					Yes	Yes
Sample	All counties	Border counties	Border counties	Border counties	Border counties	Counties <75km apart
Observations	2,934,608	949,277	927,646	927,241	2,035,388	1,781,782
R-squared	0.002	0.002	0.002	0.018	0.020	0.021
Panel B: Spillover effects from other states						
Δ Corporate Income Tax Rate	-0.039	-0.077**	-0.078***	-0.072***	-0.082**	-0.064**
	(0.035)	(0.033)	(0.029)	(0.026)	(0.033)	(0.031)
Spillover Effects (Across States)	-0.365***	-0.361***	-0.338***	-0.298**	-0.356***	-0.230*
	(0.100)	(0.110)	(0.118)	(0.127)	(0.127)	(0.124)
Year FEs	Yes	Yes	Yes			
Controlling for changes in other taxes			Yes	Yes	Yes	Yes
Year × Industry FEs				Yes	Yes	Yes
Year × County Pair FEs					Yes	Yes
Sample	All counties	Border counties	Border counties	Border counties	Border counties	Counties <75km apart
Observations	2,901,456	940,292	918,972	918,562	2,016,344	1,766,678
R-squared	0.002	0.002	0.002	0.018	0.021	0.022

Notes: This table shows regression results for Equation (3) with Δ Paydex as the outcome. Panel A reports estimates using our baseline spillover effect measure, and Panel B reports estimates using spillover effects from other states. Columns differ by controls and sample restrictions. Standard errors are clustered at the state level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See Table B1 for detailed variable definitions and data sources.

Table 5: TCJA - Pre-Matching Differences in Means between Treated and Control Firms

Variables	Treated Mean	Control Mean	Mean Diff	Std. Err.	p-value
Debt Ratio	0.5694	0.2686	-0.3008	0.0127	0.000
Log(Total Assets)	7.7519	7.3251	-0.4268	0.113	0.000
ROA	0.0557	0.0415	-0.0142	0.0080	0.075
Dividend	0.4753	0.5170	0.0417	0.0271	0.123
Market to Book	1.6107	3.2616	1.6509	0.456	0.000
Capital Expenditure Ratio	0.0300	0.0460	0.0160	0.0028	0.000

Notes: This table compares financial characteristics of treated and control firms in 2017, the year before the TCJA became effective. Treated firms are those subject to the TCJA's interest deduction limitation, defined based on their 2015–2017 pre-reform financials. The sample excludes financial firms. All variables are constructed from Compustat. See Table B1 for a detailed description of our variables and their sources. Differences are computed as Control – Treated.

Table 6: TCJA - After-Matching Differences in Means between Treated and Control Firms

Variables	Treated Mean	Control Mean	Mean Diff	Std. Err.	p-value
Debt Ratio	0.5389	0.5289	-0.0100	0.0200	0.620
Log(Total Assets)	7.7473	7.8672	0.1199	0.144	0.408
ROA	0.0546	0.0542	-0.0004	0.0094	0.965
Dividend	0.4986	0.5237	0.0251	0.0375	0.504
Market to Book	1.8548	2.5010	0.6462	0.770	0.399
Capital Expenditure Ratio	0.0313	0.0299	-0.0014	0.0031	0.654

Notes: This table compares financial characteristics of treated and control firms in 2017, the year before the TCJA became effective. Treated firms are those subject to the TCJA's interest deduction limitation, defined based on their 2015–2017 pre-reform financials. The sample excludes financial firms. All variables are constructed from Compustat. See Table B1 for a detailed description of our variables and their sources. Differences are computed as Control – Treated.

Table 7: TCJA – Establishment-level analysis

Variable	(1) Δ Paydex	(2) Δ Paydex	(3) Δ Paydex	(4) Δ Paydex	(5) Δ Paydex	(6) Δ Paydex
Treated TCJA	0.390*	0.415**	0.599**	0.574**	0.661**	0.660**
	(0.212)	(0.203)	(0.279)	(0.225)	(0.266)	(0.259)
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
State FEs		Yes		Yes		Yes
Sample	All establishments	All establishments	PSM	PSM	Around threshold	Around threshold
Observations	73,024	73,024	28,436	28,436	7,389	7,389
R-squared	0.009	0.010	0.020	0.024	0.032	0.041

Notes: This table shows regression results for Equation (9). Columns (1)–(2) use the full sample, Columns (3)–(4) use a sample matched via propensity score matching (PSM), and Columns (5)–(6) restrict to establishments with interest-to-EBITDA ratios between 20% and 40%. Each pair of columns gradually adds industry and state fixed effects. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See Table B1 for a detailed description of our variables and their data sources.

Table 8: TCJA – Heterogeneity by Financial Constraints

Variables	(1) Δ Paydex	(2) Δ Paydex	(3) Δ Paydex	(4) Δ Paydex	(5) Δ Paydex	(6) Δ Paydex
Panel A: WW Index						
WW Index	0.033	0.037	0.112	0.096	0.011	-0.050
	(0.063)	(0.062)	(0.116)	(0.105)	(0.172)	(0.179)
Treated TCJA	0.340	0.357	0.484*	0.457*	0.751**	0.735**
	(0.248)	(0.238)	(0.283)	(0.243)	(0.346)	(0.355)
Treated TCJA × WW Index	0.031	0.037	0.028	0.053	-0.194	-0.108
	(0.195)	(0.195)	(0.226)	(0.222)	(0.337)	(0.363)
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
State FEs		Yes		Yes		Yes
Sample	All establishments	All establishments	PSM	PSM	Around threshold	Around threshold
Observations	72,990	72,990	28,436	28,436	7,389	7,389
R-squared	0.009	0.010	0.020	0.024	0.032	0.041
Panel B: SA Index						
SA Index	0.058	0.061	0.081	0.076	-0.128	-0.177
	(0.060)	(0.061)	(0.092)	(0.089)	(0.174)	(0.164)
Treated TCJA	0.414**	0.441**	0.631**	0.604***	0.794***	0.789***
	(0.202)	(0.192)	(0.256)	(0.206)	(0.294)	(0.285)
Treated TCJA × SA Index	-0.261	-0.271	-0.383	-0.331	-0.143	-0.097
	(0.192)	(0.185)	(0.255)	(0.213)	(0.274)	(0.264)
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
State FEs		Yes		Yes		Yes
Sample	All establishments	All establishments	PSM	PSM	Around threshold	Around threshold
Observations	73,024	73,024	28,436	28,436	7,389	7,389
R-squared	0.009	0.011	0.020	0.024	0.032	0.042

Notes: Each panel reports regression results of Δ Paydex on the interaction between the TCJA treatment and alternative measures of financial constraints and firm size. Robust standard errors, clustered at the firm level, are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. See Table B1 for a detailed description of our variables and their data sources.

Table 8: TCJA – Heterogeneity by Financial Constraints (continued)

Variables	(1) Δ Paydex	(2) Δ Paydex	(3) Δ Paydex	(4) Δ Paydex	(5) Δ Paydex	(6) Δ Paydex
Panel C: KZ Index						
KZ Index	0.043	0.035	0.221	0.233	0.169	0.146
	(0.078)	(0.079)	(0.157)	(0.157)	(0.173)	(0.191)
Treated TCJA	0.402*	0.424**	0.649**	0.604***	0.772**	0.728**
	(0.217)	(0.206)	(0.282)	(0.215)	(0.301)	(0.295)
Treated TCJA × KZ Index	0.082	0.094	0.004	0.006	-0.297	-0.301
	(0.177)	(0.178)	(0.218)	(0.217)	(0.225)	(0.223)
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
State FEs		Yes		Yes		Yes
Sample	All establishments	All establishments	PSM	PSM	Around threshold	Around threshold
Observations	69,378	69,378	26,103	26,103	6,754	6,754
R-squared	0.010	0.012	0.022	0.028	0.034	0.045
Panel D: Firm Size						
Log(Total Assets)	-0.054	-0.057	-0.140	-0.124	-0.065	-0.033
	(0.058)	(0.057)	(0.101)	(0.089)	(0.149)	(0.149)
Treated TCJA	0.372	0.395*	0.546**	0.523**	0.701**	0.699**
	(0.226)	(0.217)	(0.270)	(0.223)	(0.306)	(0.308)
Treated TCJA × Log(Total Assets)	0.039	0.038	0.120	0.085	0.211	0.158
	(0.186)	(0.183)	(0.227)	(0.208)	(0.312)	(0.334)
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes
State FEs		Yes		Yes		Yes
Sample	All establishments	All establishments	PSM	PSM	Around threshold	Around threshold
Observations	73,024	73,024	28,436	28,436	7,389	7,389
R-squared	0.009	0.010	0.020	0.024	0.032	0.041

Notes: Each panel reports regression results of Δ Paydex on the interaction between the TCJA treatment and alternative measures of financial constraints and firm size. Robust standard errors, clustered at the firm level, are reported in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. See Table B1 for a detailed description of our variables and their data sources.

Table 9: TCJA - Firm-level analysis

Variables	(1) Δ Debt Ratio	(2) Δ Interest Expense Ratio	(3) Δ Effective Tax Rate	(4) Δ Cash Ratio
Treated TCJA	-0.016***	-0.002***	-0.158	0.001
	(0.006)	(0.001)	(0.120)	(0.004)
Industry FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
Observations	3,108	3,094	3,117	3,102
R-squared	0.088	0.096	0.090	0.078

Notes: This table shows regression results for Equation (12). We use alternative outcome variables as reported in the second row. Robust standard errors, clustered at the firm level, are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. See Table B1 for a detailed description of our variables and their data sources.

Appendix

This Appendix provides further details on the data and the results of the paper "Taxes and Default Risk: Evidence from U.S. Establishment-Level Database". Appendix A describes the procedure used to merge the NETS database with Compustat. Appendix B presents additional tables.

Appendix A

Merging NETS with Compustat

The 2022 version of the National Establishment Time Series (NETS) database comprises 87,564,680 establishments. We restrict our attention to establishments of public firms because they are organized as C-corporations and therefore subject to corporate income taxation. Based on the information reported in the NETS database, we identify 1,024,177 establishments associated with publicly traded firms, including both headquarters and subsidiary branches.

The NETS database provides the legal business names of establishments and the names of the primary domestic firms within a hierarchical "Family Tree" of companies. We use fuzzy matching to match company names between NETS and Compustat. Before we do so, we convert all characters in a firm's name to lowercase and remove punctuation, extra spaces, and common suffixes (e.g., Inc., Corp., LLC) to reduce noise in the matching. We next compute string similarity scores between company names in the two datasets, and retain only matches with a similarity score above a 90% threshold, to ensure high matching quality. With this methodology, we successfully merge 777,371 of the establishments in NETS to firms in Compustat.

We use this matched sample to investigate the impact of the TCJA on default risk. In our analysis, we focus on establishments that existed in 2007 and 2008, and for which the Paydex score is available.

Appendix B

Table B1: Variable definitions

Variable name	Description	Source
	Panel A: Establishment Level	
Paydex	Average of the minimum and maximum Paydex scores for an establishment in a given year. The Paydex score, developed by Dun & Bradstreet, reflects payment timeliness and ranges from 0 (worst) to 100 (best)	NETS
Corporate Income Tax Rate	The top statutory state-level corporate income tax rate applied to business profits	Baker et al. (2025)
Medium Personal Income Tax Rate	The effective tax rate applied to a representative middle-income earner	Baker et al. (2025)
Top Personal Income Tax Rate	The top tax rate applied to the highest income bracket within a state's personal income tax system	Baker et al. (2025)
Sales Tax Rate	The state-level statutory rate imposed on retail sales of goods and services	Baker et al. (2025)
Property Tax Rate	The effective rate of property taxation levied at the state level	Baker et al. (2025)
Share of Employees	The share of a firm's total workforce employed in a given state-year	NETS
Spillover Effects	The average tax exposure experienced by all other establishments within the same firm, excluding the focal establishment itself, as reported in Equation (5)	NETS
Spillover Effects (Across States)	The average tax exposure experienced by all other establishments within the same firm, excluding establishments in the specific state	NETS
Treated TCJA	As reported in Equation (7), this variable equals one if a firm becomes newly subject to the TCJA's interest deductibility limitation, based on pre-reform (2015–2017) financials	Compustat

Table B1: Variable definitions (continued)

	Panel B: Firm Level	
Debt Ratio	Ratio of total debt (short-term + long-term) to total assets: (dlc + dltt) / at	Compustat
Log(Total Assets)	Natural logarithm of total assets: ln(at)	Compustat
ROA	Ratio of operating income after depreciation to total assets: (oiadp/at)	Compustat
Dividend	Indicator variable equal to one if the firm distributes any dividend: dvc is greater than 0	Compustat
Market to Book	Ratio of market capitalization divided by common equity:($prcc_f$ × csho)/cew	Compustat
Capital Expenditure Ratio	Ratio of capital expenditures to total assets: (capex) / at	Compustat
Treated TCJA	As reported in Equation (7), this variable equals one if a firm becomes newly subject to the TCJA's interest deductibility limitation, based on pre-reform (2015–2017) financials	Compustat
WW Index	Following Whited and Wu (2006), the WW index is defined as: (-0.091 × cf) - (0.062 × divpos)+(0.021 × tltd) - (0.044 × log(at)) + (0.102 × isg) - (0.035 × sg), where cf is the ratio of cash flow (item ib + item dp) to total assets (item at), divpos is an indicator variable equal to one if the firm pays cash dividends (item dvt) and zero otherwise, tltd is the ratio of long-term debt (item dltt) to total assets (item at), ISG is the firm's 3-digit SIC industry sales (item sale) growth, SG is firm sales (item sale) growth. A higher value of the index indicates more severe financial constraints	Compustat

Table B1: Variable definitions (continued)

SA Index	Following Hadlock and Pierce (2010), the SA index is defined as: (-0.737 × log(at))+(0.043 × log(at)^2) − (0.040× Age). Age is defined as the number of years the firm is present in Compustat. Size is winsorized at (the log of) $4.5 billion, and Age is winsorized at 37 years. A higher value of the index indicates more severe financial constraints	Compustat
KZ Index	Following Kaplan and Zingales (1997), the KZ index is defined as: -1.001×[(ib+dp)/ppegt] +0.282×[(at+MktValceq-txdb)/at]+3.139 ×[(dltt+dlc)/(dltt+dlc+at)]-39.367 ×[(dvc+dvp)/ppegt]-1.314×[CH/ppegt]. MktVal is defined as [csho×prcc]. MktVal and ppegt are lagged. A higher value of the index indicates more severe financial constraints	Compustat
Interest Expense Ratio	Ratio of interest expense to total assets: (xint / at)	Compustat
Effective Tax Rate	Ratio of total income taxes to pretax income: (txt / pi)	Compustat
Cash Ratio	Ratio of cash holdings to total assets: (che / at)	Compustat

Robustness tests. In Table B2, we show that our result hold when we focus our empirical analyses on the six years state-level corporate income tax rate change. As reported in Table B3, our results are robust to excluding states with no corporate income taxes (Giroud and Rauh, 2019). Furthermore, our coefficients are still statistically significant when we cluster the standard errors at different levels, that is firm, industry, and county. We show these results in Tables B4.

Table B2: Robustness – Focusing around a tax change period

Variable	(1) Δ Paydex	(2) Δ Paydex	(3) Δ Paydex	(4) Δ Paydex	(5) Δ Paydex	(6) Δ Paydex
Δ Corporate Income Tax Rate	-0.020	-0.085***	-0.063**	-0.057**	-0.067**	-0.058*
	(0.025)	(0.024)	(0.027)	(0.024)	(0.030)	(0.030)
Year FEs	Yes	Yes	Yes			
Controlling for changes in other taxes			Yes	Yes	Yes	Yes
Year × Industry FEs				Yes	Yes	Yes
Year × County Pair FEs					Yes	Yes
Sample	All counties	Border counties	Border counties	Border counties	Border counties	Counties <75km apart
Observations	1,466,968	504,426	492,875	492,358	1,003,751	934,937
R-squared	0.002	0.002	0.002	0.024	0.028	0.030

Notes: This table shows regression results for Equation (3), focusing on the 6-year window around a tax change. Columns add controls and sample restrictions progressively. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See Table B1 for a detailed description of our variables and their data sources.

Table B3: Robustness – Excluding states with no corporate income taxes

Variable	(1) Δ Paydex	(2) Δ Paydex	(3) Δ Paydex	(4) Δ Paydex	(5) Δ Paydex	(6) Δ Paydex
Δ Corporate Income Tax Rate	-0.017	-0.061*	-0.063**	-0.057**	-0.066**	-0.060**
	(0.033)	(0.033)	(0.028)	(0.024)	(0.029)	(0.028)
Year FEs	Yes	Yes	Yes			
Controlling for changes in other taxes			Yes	Yes	Yes	Yes
Year × Industry FEs				Yes	Yes	Yes
Year × County Pair FEs					Yes	Yes
Sample	All counties	Border counties	Border counties	Border counties	Border counties	Counties <75km apart
Observations	2,953,961	930,741	908,780	908,357	1,950,199	1,803,529
R-squared	0.002	0.002	0.002	0.018	0.020	0.021

Notes: This table reports regression results from Equation (3), excluding states without a corporate income tax. The outcome variable is Δ Paydex. Columns progressively add controls and restrict the sample. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See Table B1 for a detailed description of our variables and their data sources.

Table B4: Robustness – Alternative levels of clustering

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Clustering at the firm level						
Δ Corporate Income Tax Rate	-0.018	-0.063**	-0.065**	-0.060**	-0.069**	-0.060*
	(0.019)	(0.027)	(0.029)	(0.029)	(0.031)	(0.031)
Panel B: Clustering at the sector level						
Δ Corporate Income Tax Rate	-0.018	-0.063**	-0.065**	-0.060**	-0.069**	-0.060**
	(0.020)	(0.027)	(0.029)	(0.028)	(0.030)	(0.030)
Panel C: Clustering at the county level						
Δ Corporate Income Tax Rate	-0.018	-0.063**	-0.065**	-0.060**	-0.069**	-0.060*
	(0.017)	(0.029)	(0.028)	(0.028)	(0.031)	(0.031)
Year FEs	Yes	Yes	Yes			
Controlling for other taxes			Yes	Yes	Yes	Yes
Year × Industry FEs				Yes	Yes	Yes
Year × County Pair FEs					Yes	Yes
Sample	All counties	Border counties	Border counties	Border counties	Border counties	Counties <75km apart
Observations	2,995,866	967,716	945,755	945,346	2,074,788	1,816,288
R-squared	0.002	0.002	0.002	0.017	0.020	0.021

Notes: Each panel reports regression results from Equation (3) using Δ Paydex as the outcome variable and alternative ways to cluster the standard errors. Columns progressively add controls and sample restrictions. Standard errors are clustered at the level indicated in each panel. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See Table B1 for a detailed description of variables and sources.

Table B5: TCJA – Establishment-level analysis (Long-term effects)

Variable	(1) Δ Paydex	(2) Δ Paydex	(3) Δ Paydex	(4) Δ Paydex	(5) Δ Paydex	(6) Δ Paydex
Post × Treated TCJA	0.146	0.165*	0.278**	0.281***	0.215**	0.239**
	(0.093)	(0.089)	(0.110)	(0.093)	(0.106)	(0.105)
Industry × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FEs		Yes		Yes		Yes
Sample	All establishments	All establishments	PSM	PSM	Around threshold	Around threshold
Observations	826,181	826,179	317,218	317,217	78,398	78,394
R-squared	0.015	0.017	0.020	0.024	0.042	0.051

Notes: This table shows regression results for Equation (9). Columns (1)–(2) use the full sample, Columns (3)–(4) use a sample matched via propensity score matching (PSM), and Columns (5)–(6) restrict to establishments with interest-to-EBITDA ratios between 20% and 40%. Each pair of columns gradually adds industry times year and state times year fixed effects. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See Table B1 for a detailed description of our variables and their data sources.

Summary statistics — Firm-level data. We report the summary statistics for our firm-level data in Table B6. We focus on the period around the TCJA (2017-2018).

Table B6: Descriptive statistics — Firm-level data

Variables	N	mean	sd	p25	p50	p75
Debt Ratio	3,766	0.3113	0.2479	0.1262	0.2856	0.4361
Interest Expenses Ratio	3,743	0.0191	0.0207	0.0062	0.0140	0.0243
Effective Income Tax	3,775	0.1426	0.5186	0.0000	0.1710	0.2563
Cash Ratio	3,757	0.1354	0.1725	0.0235	0.0683	0.1697
Δ Debt Ratio	3,763	0.0091	0.0860	-0.0228	0	0.0298
Δ Interest Expenses Ratio	3,743	0.0012	0.0087	-0.0009	0.0003	0.0029
Δ Effective Income Tax	3,773	0.0217	1.4569	-0.2011	-0.0093	0.1102
Δ Cash Ratio	3,757	-0.0066	0.0652	-0.0255	-0.0017	0.0121

Notes: This table shows descriptive statistics of the variables used in our firm-level analyses. We focus on the period around the TCJA. See Table B1 for a detailed description of our variables and their sources.

Table B7: TCJA - Firm-level analysis (Long-term effects)

Variables	(1) Δ Debt Ratio	(2) Δ Interest Expenses Ratio	(3) Δ Effective Income Tax	(4) Δ Cash Ratio
Post × Treated TCJA	-0.014***	-0.001***	-0.021	0.003*
	(0.003)	(0.000)	(0.024)	(0.001)
Industry × Year FEs	Yes	Yes	Yes	Yes
State × Year FEs	Yes	Yes	Yes	Yes
Observations	31,425	31,048	31,551	31,314
R-squared	0.146	0.123	0.103	0.116

Notes: This table shows regression results for Equation (12). We further include industry times and state times year fixed effects. We use alternative outcome variables as reported in the second row. Robust standard errors, clustered at the firm level, are reported in parentheses. *p <0.10, **p <0.05, ***p <0.01. See Table B1 for a detailed description of our variables and their data sources.