NBER WORKING PAPER SERIES

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Working Paper 35134
http://www.nber.org/papers/w35134

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
April 2026

We thank James Karan, Manuel Kathan (discussant), John McConnell, and seminar participants at the 2025 RCF-ECGI Conference, Auburn University, Baruch College, Purdue University, and the University of Nevada, Las Vegas, for valuable comments. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 35134
April 2026
JEL No. G3, G32, G38, H25

ABSTRACT

We use establishment-level data to examine the relation between corporate taxes and financial distress. Using a border discontinuity design, we document that higher corporate income tax rates significantly increase financial distress, particularly for geographically concentrated firms, with sizable spillovers across establishments. We further investigate how taxes affect establishment-level financial distress by exploiting the interest limitation rule introduced by the 2017 Tax Cuts and Jobs Act. Using a difference-in-differences design, we find that affected firms experience a decline in financial distress. This occurs because the reduced tax advantage of debt induces firms to deleverage, reducing financial distress through capital structure adjustments.

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1 Introduction

How do corporate income taxes affect financial distress? In principle, higher corporate income tax rates can increase financial distress by reducing after-tax cash flows of firms (Ivanov et al., 2025) or, because interest expenses are typically deductible for the purpose of determining taxable income, by strengthening tax incentives to borrow (Faccio and Xu, 2015; Heider and Ljungqvist, 2015; Sanati and Beyhaghi, 2025; Carrizosa et al., 2023; Faccio and Xu, 2024; Heitzman and Hanlon, 2024).[1] Recent evidence in Ivanov et al. (2025), however, suggests that these channels are heterogeneous across firms. While tax cuts significantly reduce default risk for smaller private firms, operating through improved firm health and lower equilibrium credit spreads, bank-estimated default probabilities do not respond significantly to tax cuts for large private firms. Similarly, exploiting the limitation on interest deductibility in the Tax Cuts and Jobs Act (TCJA), Maydew et al. (2025) find no significant change in the creditworthiness of affected borrowers in the syndicated loan market.

This paper revisits this question using a unique combination of establishment- and firm-level data for U.S. publicly traded firms. Leveraging granular establishment-level data enables a precise analysis of the relation between state-level tax rate changes and financial distress. As state-level corporate taxation is primarily determined by the location of operations (Baker et al., 2020), ignoring firms' geographic footprint is likely to yield biased estimates. Since most publicly traded firms operate in multiple states and thus face heterogeneous local tax environments, focusing on each establishment's location rather than relying solely on headquarters location is essential. Importantly, given the financial and operational interdependence of establishments within the same firm, the establishment-level perspective also allows us to examine how fiscal shocks propagate across a firm's different establishments.

We employ an establishment-level measure of payment timeliness, the Paydex score, as our proxy for financial distress. This measure is developed and maintained by Dun & Bradstreet (D&B), a leading commercial credit reporting agency widely recognized as the primary provider of business credit information in the United States. As documented by Barrot and

[1] Comprehensive reviews of how tax benefits affect debt financing can be found in Graham and Leary (2011), Graham (2013) and Hanlon and Heitzman (2022).

Nanda (2020), Chava et al. (2023), and Leibovici and Wiczer (2024), this score 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 short-term default risk. Insurers, lenders, and lessors rely on these credit scores when setting insurance premiums and loan and lease terms (Becker, 2012; Bennett and Wang, 2021; Chava et al., 2023).

In our first empirical setting, granular establishment-level data enable us to compare establishments operating in the same industry but located in different states, before and after changes in state corporate income tax rates. Naturally, we recognize in our empirical analysis that the timing and magnitude of these tax rate changes may be tied to local economic conditions (Faccio and Xu, 2015; Heider and Ljungqvist, 2015) and changes in other local taxes (Baker et al., 2025), both of which can also affect a business's financial distress.

To address this endogeneity concern, we exploit staggered changes in state-level corporate income tax rates using a border discontinuity design (e.g., Dube et al., 2010; Mukherjee et al., 2017), while controlling for other local economic indicators and state-level taxes. Establishment-level data offer a unique advantage: they allow us to compare establishments in counties across state borders, where neighboring counties are likely exposed to similar economic conditions yet are subject to different state-level tax regulations. Furthermore, by pairing each border county with all counties across the state line and controlling for shared trends and contemporaneous changes in other taxes through the inclusion of county-pair times year fixed effects, we can precisely isolate the impact of corporate income tax rate changes on financial distress.

Using this empirical strategy, we document that higher corporate income tax rates *do* increase the financial distress of affected establishments of publicly traded firms. This evidence highlights the importance of focusing on establishment-level measures of financial distress and of properly controlling for local (rather than headquarters-level) economic conditions when estimating the effect of state-level changes in corporate income tax rates on the financial distress of large firms. When we do so, in contrast to Ivanov et al. (2025) and Maydew et al. (2025), we uncover evidence that taxes *do* influence financial distress even for large, publicly traded firms.

We further document that the impact of corporate income tax rate changes on financial distress depends on the location and combined tax exposure of all of a firm's establishments. More specifically, the effects are substantially larger for firms that are geographically concentrated in states experiencing a change in corporate income tax rates, suggesting that geographic diversification is valuable for mitigating the adverse impact of localized tax shocks on financial distress.

Importantly, we uncover significant within-firm spillover effects across establishments. Although non-legally independent establishments are not directly subject to state corporate income taxes,[2] we document that their Paydex score responds to tax changes affecting other establishments within the same firm, including establishments located in different states. This pattern is consistent with localized tax shocks exerting pressure on firms' internal capital markets and propagating distress across establishments. This evidence indicates that state-level tax policies can have firm-wide consequences that extend beyond directly affected locations.

Having established that changes in corporate income tax rates affect financial distress even for large publicly traded firms, we next investigate the channels through which this effect operates. To do so, we exploit the introduction of the Tax Cuts and Jobs Act (TCJA) in 2017 as a quasi-experimental setting. A key provision of this reform, contained in Section 163(j), limits the deductibility of interest payments to 30% of EBITDA, directly altering the tax incentives associated with debt financing.

The restriction on interest deductibility provides a natural environment to disentangle the two main theoretical channels linking taxation to financial distress: the cash-flow channel and the tax-shield channel. The cash-flow channel predicts that, *ceteris paribus*, affected firms will experience a relative decline in after-tax cash flows due to the loss of debt tax shields, leading to an increase in financial distress. In contrast, the tax-shield channel predicts that the reduced tax incentive to borrow will induce firms to lower their leverage, thereby reducing financial distress.

To identify which channel dominates, we combine a difference-in-differences design with propensity score matching and compare changes in Paydex scores between establishments

[2]These establishments do not file state corporate tax returns and do not directly remit corporate income taxes.

belonging to affected firms and those of similar, unaffected firms. We document that the establishments of firms affected by the interest limitation rule experience a significant relative reduction in financial distress, indicating that the deleveraging response dominates in net terms in our sample of publicly traded firms.

These findings are also supported by a difference-in-differences setting in which we compare firms *barely affected* by the limitation to those that are *barely unaffected*. By contrast, placebo tests using alternative deduction limits show no change in default risk for placebo-treated firms. An investigation of the dynamic effects of the TCJA further reinforces the causal interpretation of our findings. Specifically, in support of the parallel trends assumption, we document that establishments of treated and matched control firms exhibit parallel trends in financial distress prior to the TCJA, with a clear and persistent divergence emerging only immediately after the interest limitation rule was implemented.

Firm-level analyses provide further direct support for the tax-shield channel by showing that affected firms significantly reduce their leverage. Consistent with this adjustment, in the matched samples, affected firms also exhibit a significant reduction in net interest expense. Although taxes paid increase modestly in relative terms, the reduction in leverage and interest expense more than offsets the higher tax burden, explaining the decline in financial distress documented at the establishment level. 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.

We contribute to the literature in several ways. First, in a study closely related to ours, Ivanov et al. (2025) show that reductions in corporate income tax rates *do not* have a meaningful impact on the default risk of larger private firms, while lowering default risk among smaller, privately held firms. Similarly, Maydew et al. (2025) find no evidence that the TCJA's interest deductibility limitation affected borrowers' creditworthiness in the syndicated loan market. By contrast, earlier research documents that, even after accounting for default risk, both personal (Elton et al., 2001; Driessen, 2005; Liu et al., 2006, 2007) and corporate income taxes (Zhao et al., 2024; Deli et al., 2025) significantly affect the credit spreads of corporate

bonds and loans.

Our paper advances this literature by introducing an establishment-level measure of financial distress, which allows us to directly connect local tax rate changes to the affected units.[3] This approach avoids overlooking how differences in local tax policies affect financial distress across locations. Furthermore, we examine a specific policy, the interest deductibility limitation introduced by the TCJA, which enables us to disentangle the cash-flow and tax-shield channels through which corporate taxation can affect financial distress. Together, these features enable us to uncover richer and more nuanced evidence on the impact of taxation on financial distress than existing studies, and offer important insights into how corporate tax design influences both firm-level and systemic risk. Crucially, our findings show that taxes influence the financial distress of large firms as well.

Other research that primarily focuses on firms in the financial sector examines how geographic diversification shapes financial risk (e.g., Goetz et al., 2016; Chu et al., 2020; Doerr and Schaz, 2021).[4] We contribute to this literature by showing that the geographic concentration of non-financial firms can also significantly impact their financial distress, as geographically concentrated firms exhibit more pronounced responses to a localized tax shock.

By documenting significant spillover effects of corporate taxes on the financial distress of firms across establishments, our paper also contributes to the literature that shows that local shocks can have spillover effects through firms' internal networks (e.g., Giroud and Mueller, 2015; Giroud et al., 2024, 2026). However, when it comes to taxation, the existing evidence on spillover effects of tax reforms is mixed. For example, Akcigit et al. (2021) document that personal income taxation significantly impacts inventor mobility. This spatial reallocation of human capital carries meaningful consequences for regional innovation output: high-tax areas face diminished inventive activity, whereas low-tax regions enjoy positive externalities as incoming inventors deepen local knowledge stocks and stimulate further innovative activity.

[3] 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.

[4] Lewellen (1971) was among the first to document how the imperfect correlation of the cash flows of a conglomerate's business units can result in reduced financial distress.

Giroud and Rauh (2019) show that after a state increases corporate income taxes, affected firms reduce employment and the number of establishments in that state. They further document that about half of the lost economic activity in a tax rate-hiking state is reallocated to other states where the firm maintains establishments, providing evidence of cross-state spillovers in business activities. In contrast, Sapollnik and Swonder (2025) find no evidence of entrepreneurial spillovers across states following changes in corporate tax rates, and Fairlie et al. (2025) show that tax rate changes do not prompt startups to relocate across state lines. Extending the evidence of tax spillovers in Giroud and Mueller (2019), we show that state-level tax policy can affect financial stability across borders and undermine the fiscal sovereignty of neighboring states.

Last but not least, our study is also related to work that examines how firms adjust their capital structure in response to tax reforms. 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. This finding is further supported by evidence reported by Carrizosa et al. (2023), Heitzman and Hanlon (2024), and Sanati and Beyhaghi (2025) for U.S. firms. In contrast to these results, Ivanov et al. (2025) document that reductions in state-level corporate income tax rates lead firms to increase leverage, with effects that are particularly pronounced among smaller, privately held firms. Similarly, Goodman et al. (2024) document that the interest limitation rule did not affect the equity or debt issuance of large U.S. firms. We contribute to this literature by showing that an exogenous tax policy shock limiting the deductibility of interest expenses primarily affects the financial distress of publicly traded firms through the leverage channel.[5]

The remainder of the paper is organized as follows. Section 2 describes the data. Section

[5]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).

3 examines the impact of state-level corporate income tax rate changes on establishment-level financial distress. 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 financial distress. Section 5 concludes.

2 Data

To investigate our research questions, we collect data on corporate taxes as well as granular establishment- and firm-level financial data for U.S. publicly traded firms. Our data cover the period from 1990 to 2022, for which all required data sources are available.[6]

2.1 State taxes and local economic indicators.

Data on state taxes is from Baker et al. (2025). This dataset covers all major U.S. state-level taxes, including corporate income tax rates, as well as property, sales, and top marginal personal income tax rates. Throughout the paper, all tax rate variables (and their year-to-year changes) are expressed in percentage points. For example, a state corporate income tax rate of 7.3% is reported as 7.30, and a one-unit change in the variable corresponds to a one-percentage-point change in the underlying rate.

The primary variable of interest in our empirical analysis is the corporate income tax rate. Figure 1 illustrates corporate income tax rates in each state (identified by its Federal Information Processing System (FIPS) code) from 1990 to 2022. During this period, there is substantial variation in corporate income tax rates 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 imposing 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. During the period analyzed, some states changed their corporate tax rates only once, whereas most experienced

[6]Because our baseline specifications for the corporate income tax analysis are estimated in first differences, the effective estimation sample spans 1991–2022. Data for 1990 are retained solely to construct the first difference for 1991, which captures the change from 1990 to 1991.

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

[Insert Figure 1 about here]

We recognize that establishments located in different states are also likely to face different economic conditions. Therefore, we collect information on local economic indicators from the Bureau of Economic Analysis (BEA). Specifically, in our empirical analyses, we control for county-level population, income per capita, and unemployment insurance compensation per capita.

2.2 Counties along state borders.

The empirical strategy employed in the first part of this paper largely focuses on geographically adjacent treated and control counties located across state borders. By comparing establishments in bordering counties across state lines, we mitigate the risk that other unobserved local economic factors may give rise to our results (or lack thereof) (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 than counties located in the eastern U.S. states. This may reduce the likelihood that they share similar economic conditions. Therefore, to strengthen the credibility of our empirical approach, we follow Dube et al. (2016) and, in an additional test, further restrict our sample to county pairs whose centroids lie within 75 miles of each other.[7] Counties that satisfy this criterion are colored dark green in Figure 2.

[Insert Figure 2 about here]

[7] This threshold has been determined by Dube et al. (2016) using a data-driven procedure that minimizes the mean squared error of the estimator based on placebo treatments, thereby balancing the trade-off between improved comparability across geographically proximate counties and the loss of observations from a more restrictive sample

2.3 Establishment-level data.

Our establishment-level data come from the National Establishment Time Series (NETS) database. The data in NETS cover all U.S. establishments affiliated with both privately held and publicly traded firms. We restrict our analyses to establishments of publicly traded firms. We do so because publicly traded firms are predominantly organized as C-corporations and thus subject to corporate income taxes. We exclude privately held firms from our analyses because NETS does not report their legal form, and most are organized as pass-through entities not subject to corporate income taxes.[8]

From NETS, we collect each establishment's name, the name of the corporate entity it belongs to, and the entities' corresponding unique identifiers and locations. Importantly for our analyses, NETS also provides data on each establishment's Paydex score with annual frequency. This measure has 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 D&B by over 4,000 trade exchange participants in the United States. Lenders, insurance providers, and lessors 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 financial distress and plays a central role in our empirical analyses.

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 Paydex score of 70 corresponds to a payment delay of 15 days beyond terms. Importantly, this score has been shown to strongly predict business failure and financial distress (Barrot and Nanda, 2020; Chava et al., 2023; Leibovici and Wiczer, 2024).

In our analyses, we use information contained in NETS to further distinguish between

[8]See, for example, the information available at: `https://taxfoundation.org/research/all/federal/overview-pass-through-businesses-united-states/`.

legally independent and non-legally independent establishments. Legally independent establishments include those classified in NETS as (1) headquarters (defined as business locations that have branches or divisions reporting to them and are legally responsible for those branches or divisions), (2) incorporated subsidiaries (defined as incorporated entities that are more than 50 percent owned by another corporation), and (3) other standalone incorporated entities.[9] In contrast, non-legally independent establishments such as branches and divisions do not bear legal responsibility for corporate obligations. Those, for example, include secondary locations of a firm.

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), sales, total assets, total debt, cash holdings, dividends, intangible assets, total taxes paid, capital expenditures, return on assets (ROA), 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 affected firms. To perform our empirical analyses, we merge NETS with Compustat using a fuzzy matching procedure based on the names of the parent companies of the establishments and the names of firms in Compustat. This matching procedure is described in Appendix A.

2.5 Summary statistics.

In our first empirical setting, we restrict our sample to establishments that belong to publicly traded firms for which the Paydex score, state-level corporate income tax rates, and information on local economic indicators are available. We exclude establishments operating in the financial sector (NAICS code 52). The resulting sample includes 3,920,413 establishment-year observations, 473,409 unique establishments, and 15,278 unique firms, over the period from

[9] A standalone entity is the only location of a business and, as such, it has no branches or subsidiaries reporting to it.

1991 to 2022.

Summary statistics for the samples of legally independent and non-legally independent establishments are reported in Panels A and B of Table 1, respectively. Detailed definitions of all variables and their data sources are given in the Appendix Table B1. For both samples, our main variable of interest, the Paydex score, has a mean of 71, a median of 73, and a standard deviation of 9.15. Establishments in our two samples are subject to an average state-level corporate income tax rate of 6.82%, with a median of 7.30% and a standard deviation of 2.72%.

[Insert Table 1 about here]

3 Corporate income tax rates and financial distress

3.1 The taxation of establishments.

In the United States, corporate income taxes are imposed at both the federal and state levels. State-level corporate income tax liability is determined by where a firm conducts its operations, rather than by the location of its headquarters. Legally independent establishments, such as headquarters, subsidiaries, and other incorporated entities, bear state-level tax liability. These entities either remit taxes directly to the state(s) in which they operate or, when tax-sharing agreements are in place, remit tax payments to their parent company. By contrast, non-legally independent establishments are not themselves subject to corporate taxation. Nonetheless, because they are part of a legal entity subject to taxation, such establishments may still be affected by changes in corporate income taxes. For example, increases in a firm's overall tax burden may reduce the internal funds available for establishment-level operations and constrain internal capital reallocations. Even for non-legally independent establishments, these adjustments can impair cash flows, thereby influencing the establishments' Paydex score, despite the fact that non-legally independent establishments do not remit taxes directly.

The fact that state taxes have historically been determined based on the location of operations motivates the use of establishment-level data. State-level taxation relies on apportionment formulas. A state's share of a multistate firm's net income has traditionally been determined by the firm's weighted proportions of property, payroll, and sales within the state.[10] [11]

3.2 Baseline effects.

Since legally independent establishments (e.g., parent companies, incorporated subsidiaries, and standalone incorporated entities) bear tax liability, focusing on this set of establishments allows us to credibly estimate the effects of state-level corporate tax changes on financial distress, exploiting the fact that establishments in different states are subject to heterogeneous tax treatments. To estimate the impact of corporate income tax rates on the financial distress of legally independent establishments, we consider the following Equation (1).

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

In this specification, the outcome variable is the Paydex score of establishment i in year t. The key explanatory variable of interest is Corporate Income Tax Rate, the corporate income tax rate (in percentage points) in force in state s, the state in which establishment i is located, in year t.

To estimate the effect of corporate income taxes on the outcome variable, we include establishment fixed effects (η_i) and industry-by-year fixed effects ($\eta_{j,t}$). The inclusion of industry-by-year fixed effects allows us to account for time-varying factors, such as the costs of financial distress (Davydenko et al., 2012). Industry sectors are defined at the two-digit NAICS level. To mitigate concerns that our estimates may reflect confounding local economic conditions as opposed to state-level tax rate changes, we control for the change in (the

[10] Over time, some states have placed increasing weight on in-state sales, and several now use sales as the sole factor in their apportionment formulas.

[11] A few states have introduced taxes on gross receipts in place of, or in addition to, traditional corporate income taxes. Some states have also allowed local authorities (counties and municipalities) to impose additional local corporate income taxes. However, New York City alone accounts for almost 80% of all sub-state corporate income taxes levied in the United States.

natural logarithm of) county population, income per capita, and unemployment insurance compensation per capita, denoted $X_{c,t}$. In an additional specification, we add controls for changes in the state-level sales tax rates, property tax rates, and top personal income tax rates (Baker et al., 2025). 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 county within the border region with all the adjacent counties across state lines. As a result, an establishment may enter the regression sample multiple times, reflecting its inclusion in multiple matched county pairs across the state border. The county pair is indicated by the subscript p in the following Equation:

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

In this setting, to ensure a valid comparison of contiguous counties, we include 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 across state borders whose centroids are less than 75 miles apart. Those counties are, naturally, even more likely to be similar in any other observable and unobservable characteristics.

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

$$\begin{aligned} \Delta\text{Paydex}_{i,p,t} = \eta_{p,t} + \eta_{j,t} + \beta\, \Delta\text{Corporate Income Tax Rate}_{s,t} \\ + \Gamma \Delta X_{c,t} + \epsilon_{i,p,t} \end{aligned} \tag{3}$$

A key advantage of using a first-difference model in our setting is that many corporate income tax rate changes are small and evolve gradually over time rather than taking the form of large, discrete shocks. By differencing outcomes across consecutive 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. As highlighted by

Heider and Ljungqvist (2015), first differences have the additional benefit of accommodating the repeated shocks and reversals in tax changes observed in the data.

The regression results are presented in Panel A of Table 2. In Column (1), we present the results for the full sample and include controls for local economic indicators such as population, income per capita, and unemployment insurance per capita, as well as industry-by-year fixed effects. In the full sample, changes in corporate income tax rates are statistically significantly related to financial distress. In Column (2), we restrict the analysis to establishments located along state borders. Doing so further improves the comparability between treated and control groups.[12] In Column (3), we add controls for the contemporaneous changes in other types of taxes. The coefficient of interest is negative and remains statistically significant in all these specifications. In Column (4), we adopt a border discontinuity approach, matching counties across state lines and including county pair-by-year fixed effects. In Column (5), we further restrict the sample to counties whose centroids are within 75 miles of each other. In our preferred specification, shown in Column (5), we focus on county pairs with centroids less than 75 miles apart, and include county pair-by-year fixed effects.

Based on our preferred specification, we document that a one percentage point increase in the corporate income tax rate leads to a 0.112 reduction in the Paydex score. While the per-unit magnitude is modest, consistent with the typically small size of state-level rate adjustments, the estimate is precisely measured and contrasts with the null findings of Ivanov et al. (2025) for large private firms and Maydew et al. (2025) in the syndicated loan market. For comparison, Barrot and Nanda (2020) document a 0.50-point improvement in payment scores following the Quickpay reform, which accelerated federal contract payments by roughly 15 days, and Chava et al. (2023), using a similar border-discontinuity design, report a 0.53-point reduction in the Paydex score from a one-dollar increase in the federal minimum wage.

We also estimate Equation (3) by focusing on non-legally independent establishments.

[12] As argued by Favara and Imbs (2015), counties that are adjacent to one another and located along state borders are presumably more similar to one another, often belonging to the same metropolitan statistical area and sharing a high degree of social and economic integration, than counties drawn from state-level averages. Under the assumption that the relevant observed and unobserved control variables vary continuously around the border, restricting the sample to border counties by itself mitigates concerns about omitted local economic conditions, even before introducing pair-by-year fixed effects.

These establishments do not file state corporate tax returns and do not directly remit corporate income taxes. As such, they are not directly affected by changes in state corporate tax rates. Instead, any tax obligations arising from their operations are determined at the level of the parent company.

Consistent with this institutional distinction, Panel B of Table 2 provides, at best, only weak evidence that the Paydex score of non-legally independent establishments responds to state tax changes. The coefficients of interest are substantially smaller than those reported for legally independent establishments and are not statistically significant when we adopt the border discontinuity approach. The absence of a direct effect of corporate income tax changes on the Paydex score of establishments that are not directly subject to taxes serves as a natural placebo test, and reinforces the notion that only units bearing tax liability exhibit a measurable change in financial distress following a change in their state's corporate income tax rate. Overall, the results in this section provide evidence that, when accounting for the dispersed geographic footprint of large firms using establishment-level data, corporate income tax changes *do* affect the financial distress of large firms.

[Insert Table 2 about here]

3.3 The role of geographic diversification.

Having established that changes in corporate income tax rates affect the financial distress of establishments that are subject to corporate taxation, we next investigate whether this effect varies with firms' geographic footprint. This analysis is motivated by the idea that firms with geographically concentrated operations should be more exposed to localized tax shocks, as a larger share of their property, employees, and sales is tied to a single state. In contrast, firms with more geographically dispersed operations can partially absorb localized tax increases by reallocating resources across locations or by relying on cash flows generated in other states, thereby attenuating the impact of state-level tax changes on financial distress.

To test this hypothesis, we calculate the share of a firm's employees who work in the same

state as the establishment being analyzed. Next, we interact this variable with the change in the local corporate income tax rate, as formalized in Equation (4).

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

We report the results in Table 3. We find that the interaction term is consistently negative and statistically significant across the specifications focusing on counties located across state borders. This evidence further highlights the importance of controlling for local economic conditions using granular data. More specifically, according to the interaction coefficient, when the share of employees of the firm in the affected state is equal to one (that is, 100%), the Paydex score declines by 0.195 units following a one percentage point increase in the corporate income tax rate. This effect is nearly twice the magnitude of the average effect documented in Table 2.

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' financial distress. In contrast, firms with a more geographically dispersed structure are better positioned to buffer localized tax shocks because of diversification. 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]

3.4 Spillover effects

Although non-legally independent establishments are not directly subject to state corporate income taxes, establishments belonging to the same firm are financially interconnected through joint legal liability and internal capital markets. As a result, changes in the tax burden faced by one establishment can alter the firm's overall cash flows and financing capacity, potentially affecting the financial distress of other establishments belonging to the same legal entity, even when those establishments are located in different states and do not directly remit taxes. To investigate whether this is the case, we examine whether changes in state corporate income tax rates generate intra-firm spillover effects on establishment-level financial distress.[13]

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 establishment under consideration. 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 estimate the model focusing on establishments that are not legally independent and therefore are not expected to remit taxes directly. For these establishments, which bear no corporate income tax liability themselves, any changes in the Paydex score must reflect spillover effects. We report the estimation results in Panel A of Table 4.

While the coefficient of the spillover effect variable is always negative and statistically

[13]The Paydex score is computed at the D-U-N-S (establishment) level from trade experiences independently reported to D&B by that establishment's vendors and suppliers, with at least two distinct vendors and three trade lines required. It is not a weighted average of the parent firm's score, nor is it imputed from affiliated establishments. Any correlation across establishments within the same firm therefore reflects economic linkages, such as centralized accounts payable, shared financing, or common vendors, rather than the mechanical construction of the score.

significant, the direct impact of taxes on establishment-level financial distress is generally small and mostly not statistically significant. Based on the coefficient from our preferred specification, reported in the last column, a one-unit increase in our the spillover effects variable (that is, all other establishments of the same firm are exposed to a one-percentage-point increase in the corporate income tax rate, excluding the focal establishment) reduces the Paydex score by 0.253 units, indicating meaningful spillover effects across establishments within the same firm.

In a further test, we create a spillover measure based exclusively on the exposure (to the tax changes) experienced by the establishments located 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 0.201 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. To the best of our knowledge, we are the first to document that such spillover effects extend to financial distress and, thus, broader financial stability.

[Insert Table 4 about here]

4 The TCJA of 2017

Having established that changes in state-level corporate income tax rates significantly affect establishments' financial distress, we now turn to investigating the underlying channel driving this effect. For this purpose, we focus on Section 163(j) of the TCJA.

Two primary channels may explain the observed increase in financial distress associated with tax rate changes. First, ceteris paribus, higher tax rates reduce affected establishments'

cash flows, thereby increasing their financial distress. Second, ceteris paribus, higher tax rates increase the incentive to use more debt in the capital structure, which, in turn, increases financial distress. While both channels predict an increase in financial distress 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, Section 163(j) lowers cash flows and should thereby increase financial distress due to the partial loss of the debt tax shield. On the other hand, the provision also diminishes the tax shield benefit of debt, thereby reducing the incentive to rely on debt financing, which should decrease financial distress. With this trade-off in mind, this section examines which of these two channels dominates in explaining the change in financial distress, if any, among firms subject to the interest limitation rule. We also analyze firms' financial statements to assess whether the change in financial distress is accompanied by changes in leverage, interest expenses, or taxes paid.

4.1 Institutional framework.

The TCJA was a legislative priority of the first Trump administration. The stated goal of this initiative 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 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.[14]

We use information from Compustat to identify firms ex-ante likely to be affected by the interest deductibility limitation. To do so, we follow the methodology of Faccio and Xu (2024). Specifically, we define a firm as *treated* if, based on its pre-reform financials (averaged over the period 2015–2017)[15], 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, relative to unaffected firms, 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.

We then simulate the impact of the TCJA's interest-deductibility limitation by computing, for each firm, the change in non-deductible interest:

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

The first term represents the amount of interest that would have been deductible under the pre-TCJA regime, where firms could deduct interest payments up to the level of taxable income. The inner minimum ensures that deductible interest cannot exceed taxable earnings before interest deductions, proxied by EBIT, while the outer maximum restricts deductions

[14] The TCJA of 2017 introduced several other important changes to taxation in the U.S. First, it slashed the top statutory 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 deemed repatriation of certain non-previously taxed foreign earnings. Third, it replaced a progressive tax system with a flat tax.

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

to non-negative values when EBIT is negative. The second term reflects the amount of interest that remains deductible under the TCJA limitation, where the deduction, following the introduction of the interest limitation rule, is further constrained by the 30 percent of EBITDA cap. The difference between the two terms captures the portion of interest expense that becomes non-deductible due to the reform. The treatment indicator is then defined as:

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

4.2 A comparison between treated and untreated firms.

Using firms with non-missing data from Compustat, we identify 462 firms as treated and 5,095 firms as not treated. We remove firms whose average revenues are below $25 million during 2015-2017, as they are exempt from the interest limitation rule.[16] This restriction narrows the sample to companies that are at risk of treatment, improving the comparability between treated and control companies. As in the previous analyses, we exclude firms operating in the financial sector (NAICS code 52). Our final sample comprises 346 treated and 3,081 control firms. We winsorize all financial variables at the 1st and 99th percentiles to address potential reporting errors and outliers in the Compustat data.

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

As reported in Table 5, treated firms are significantly more leveraged, larger, and more

[16] One alternative approach is to follow Sanati and Beyhaghi (2025) and exploit this sales threshold using a regression discontinuity design (RDD). However, this identification strategy requires comparing firms with positive changes in non-deductible interest just below the threshold to those just above it. In our Compustat sample, this results in a very small and highly unbalanced sample, with only 26 firms below the $25 million threshold that would have been treated absent the exemption threshold.

profitable than control firms before the reform. Treated firms are also significantly less likely to pay dividends and exhibit substantially lower capital expenditures and cash holdings than untreated firms. (The definitions of all 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 by combining the difference-in-differences approach with a nearest-neighbor matching. Specifically, we match treated and control firms based on the firm characteristics listed in Table 5 using one-to-three and one-to-one nearest-neighbor matching with a caliper of 0.05, restricting the sample to 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 nearest-neighbor matching. Before matching, several key variables (such as the debt ratio) exhibit large imbalances, with standardized differences exceeding 50%. After matching, the standardized differences for all covariates are substantially reduced and close to zero, indicating a substantially improved balance between the treated and control groups. Furthermore, as shown in Table 6, there are no statistically significant differences between the two groups after matching in either the one-to-three (Panel A) or the one-to-one (Panel B) matched samples. This supports the validity of our matching procedure in creating a more comparable control group to estimate the effect of the TCJA on financial distress. Equally importantly, by restricting comparisons to truly similar firms, the matching procedure greatly mitigates the possibility that any documented change in financial distress may reflect the reduction in the Federal corporate income tax rate introduced by the TCJA.

[Insert Figure 3 about here]

[Insert Table 6 about here]

4.3 Difference-in-difference estimates.

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

$$\text{Paydex}_{i,t} = \eta_i + \eta_t + \beta \left(\text{Post}_t \times \text{Treated TCJA}_f\right) + \epsilon_{i,t} \tag{8}$$

Treated TCJA is an indicator equal to one if the establishment belongs to a treated firm f, as defined above, and zero otherwise. Post is an indicator equal to one for years after 2017, and zero otherwise. We include establishment fixed effects to account for time-invariant establishment characteristics and year fixed effects to absorb economy-wide shocks and aggregate time trends common to all establishments. In additional specifications, we gradually control for industry-by-year and county-by-year fixed effects to account for dynamic differences across industries and geographic areas. Finally, we restrict the sample to legally independent establishments and the years around the event, that is, 2014-2020. We cluster the standard errors at the firm level.

We report the results in Table 7. We find that establishments of treated firms, those affected by the TCJA interest deduction limitation, experience an increase in their Paydex scores, indicating a reduction in financial distress. In the full sample, the estimated coefficients reported in Panel A are positive across all specifications, ranging from 0.373 to 0.395, but are not statistically significant.

When we restrict the analysis to establishments belonging to firms with similar pre-reform characteristics identified using the one-to-three nearest-neighbor matching, the coefficients (reported in Panel B) become substantially larger and statistically significant. In particular, the coefficient is equal to 0.722 in Column (1) and remains highly significant when we gradually control for industry-by-year and county-by-year fixed effects. Finally, we obtain similar results when we further restrict the sample to a matched sample of firms constructed using one-to-one matching. In our last specification in Panel C, the coefficient remains statistically significant and is close to one in magnitude. These results indicate that financially compa-

rable treated establishments experience a meaningful relative reduction in financial distress following the reform.

[Insert Table 7 about here]

4.4 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:

$$\text{Paydex}_{i,t} = \sum_{\substack{\tau=2014 \\ \tau \neq 2017}}^{2020} \beta_\tau \left(\text{Treated TCJA}_f \times \text{Year}_\tau\right) + \eta_i + \eta_t + \epsilon_{i,t} \tag{9}$$

In Equation (9), we interact the treated variable with year indicators for the period 2014-2020, omitting 2017 (the final pre-treatment year), which is used as the reference year. The coefficients β_τ capture the difference in Paydex scores between treated and control establishments for each year relative to 2017.

The estimated β_τ coefficients and the relative standard errors are plotted in Figure 4. We report the results for the full sample of legally independent establishments and for the subsample of legally independent establishments belonging to matched firms, constructed using either the one-to-three nearest-neighbor matching or the one-to-one nearest-neighbor matching.

The results support the key identifying assumption of parallel trends: none of the coefficients for the pre-reform years (2014-2016) are statistically significant. In contrast, starting in 2018, the first year of implementation of the TCJA, we observe a positive, significant relative increase in the Paydex score of treated establishments. While this effect is not persistent in the full sample of establishments, it is long-lasting in the matched samples, with the magnitude of the annual effect increasing in subsequent years.

[Insert Figure 4 about here]

4.5 Threshold and placebo tests.

A potential concern with the previous results is that firms with higher interest-to-EBITDA ratios may differ from firms with lower ratios along dimensions correlated with financial distress, and that these pre-existing differences, rather than the interest limitation rule, may drive our results. We address this concern with two complementary tests that differ in whether the threshold is the actual policy cutoff or a placebo one.

First, we conduct a threshold test by restricting the sample to firms with an interest-to-EBITDA ratio between 15% and 45%, a ±15 percentage point window around the true 30% policy cutoff. Treated firms are those actually barely constrained by Section 163(j), and the control group consists of firms just below the threshold, i.e., barely unaffected. Panel A of Table 8 reports the results. We find a significant and economically meaningful decrease in financial distress for treated establishments. The effect ranges from 0.663 to 0.956 across specifications. Panel (a) of Figure 5 confirms clean pre-trends and a visible upward shift in the treatment effect beginning in 2018.

Second, we conduct a placebo test in which we restrict the sample to firms with an interest-to-EBITDA ratio below 30%. By construction, those firms that were never constrained by Section 163(j). We define a placebo-treated group as the set of firms with an interest-to-EBITDA ratio between 15% and 30%, with firms below 15% serving as the control group. If the level of the interest-to-EBITDA ratio alone explained our main findings, we should observe a significant treatment effect at this placebo threshold. Panel B of Table 8 displays the results. The placebo coefficients are economically small and statistically insignificant across all specifications. Panel (b) of Figure 5 shows a flat pattern with no divergence between placebo-treated and control groups in the pre- or post-reform periods.

Taken together, these two tests strongly corroborate the view that our previous results are driven by the 30% policy threshold introduced by Section 163(j), rather than by pre-existing differences correlated with the interest-to-EBITDA ratio. Equally importantly, by comparing firms barely affected by the limitation to those barely unaffected, the threshold test further mitigates the possibility that our results are driven by comparing firms differently affected by the Federal corporate income tax rate change introduced by the TCJA.

[Insert Figure 5 about here]

[Insert Table 8 about here]

4.6 Mechanisms.

To provide *direct* evidence about the debt tax-shield channel, we analyze, at the firm level, key adjustments to capital structure policy 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 data from Compustat and estimate the following Equation:

$$\text{Outcome}_{f,t} = \eta_f + \eta_{s,t} + \eta_{j,t} + \beta \left(\text{Post}_t \times \text{Treated TCJA}_f\right) + \epsilon_{f,t} \tag{10}$$

In this specification, *Outcome* is an outcome variable for firm f at time t. More specifically, we consider (1) the debt ratio, (2) the interest expense ratio, and (3) taxes paid scaled by total assets. η_f, $\eta_{s,t}$, and $\eta_{j,t}$ are firm, industry-by-year, and state-by-year fixed effects. Treated TCJA is an indicator equal to one if the firm is affected by the interest limitation rule. Standard errors continue to be clustered at the firm level. We report the results for both the full and matched samples.

The results in Table 9 indicate economically significant capital structure adjustments by treated firms following the TCJA. In Column (1), we show that the overall debt ratio declines by 5.8 percentage points. This reduction is statistically significant at the 1% level and economically sizeable, corresponding to an approximately 18.7% decrease relative to the sample mean of 0.31. We obtain similar results for the matched samples, with an average decline of 6.4 percentage points in the one-to-three matched sample and an even larger decline of 7.4 percentage points in the one-to-one matched sample. This result confirms that the reform triggered significant deleveraging and is consistent with recent evidence documented by Carrizosa et al. (2023), Faccio and Xu (2024), Heitzman and Hanlon (2024), and Sanati and Beyhaghi (2025). This 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.

In Column (2), we document a decrease in the interest expense ratio when using our matched samples. This change is consistent with the observed decline in leverage. In the matched samples, the effect is equal to 0.4 to 0.5 percentage points and is also economically meaningful, given average values of this variable of roughly 6–7%.

In Column (3), we show that taxes paid (scaled by total assets) increase modestly in relative terms in the full sample and in the one-to-three matched sample, with coefficients of 0.004 and 0.003, respectively, while the one-to-one matched estimate is positive but statistically insignificant. This pattern is consistent with the mechanics of the interest-deductibility limit: by reducing the deductible portion of interest expense, the TCJA may and, in fact, do raise treated firms' taxable income and therefore the cash taxes they remit.

[Insert Table 9 about here]

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. Further, although treated firms experience a modest increase in taxes paid relative to assets, the annual interest savings implied by their debt reduction more than offset this additional tax burden in the matched samples. This evidence suggests that capital-structure adjustments quantitatively dominate the direct tax-payment effect, reducing the financial distress of affected firms. Thus, in the context of the TCJA, the leverage response appears to dominate in net in our sample of publicly traded firms.

5 Conclusions

We study the impact of corporate income taxes on financial distress by leveraging a unique dataset that links an establishment-level financial distress measure to tax variables and firm-

level financial policies. This dual-level approach is essential because state-level corporate tax liabilities are determined based on the location of operations. By integrating granular data across these dimensions, we can precisely trace how tax changes at the local level propagate through different interconnected 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 corporate income tax rate changes 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 financial distress among large publicly traded firms. 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, in line with Ivanov et al. (2025), we show that this effect is greater when firms are geographically concentrated.

Importantly, we also document substantial spillover effects of corporate income taxes across establishments within the same firm, including those located in other states. Thus, we uncover a novel striking externality of local tax reforms: their effects ripple across state borders, affecting the financial stability of firms in other jurisdictions.

We further document *how* taxes shape establishment-level financial distress by analyzing the interest deductibility limitation introduced by the 2017 TCJA. Doing so allows us to determine whether the impact of tax rate changes on financial distress reflects lower cash flows or, rather, it reflects adjustments in corporate capital structure choices. In this setting, in support of the tax shield channel, we document a significant decrease in financial distress for affected establishments. At the firm level, we also find a relative reduction in leverage and, consequently, in interest expenses. Overall, the results indicate that, at least in the TCJA setting, the tax shield channel dominates the cash flow channel in explaining the impact of debt tax shields on financial distress for publicly traded firms.

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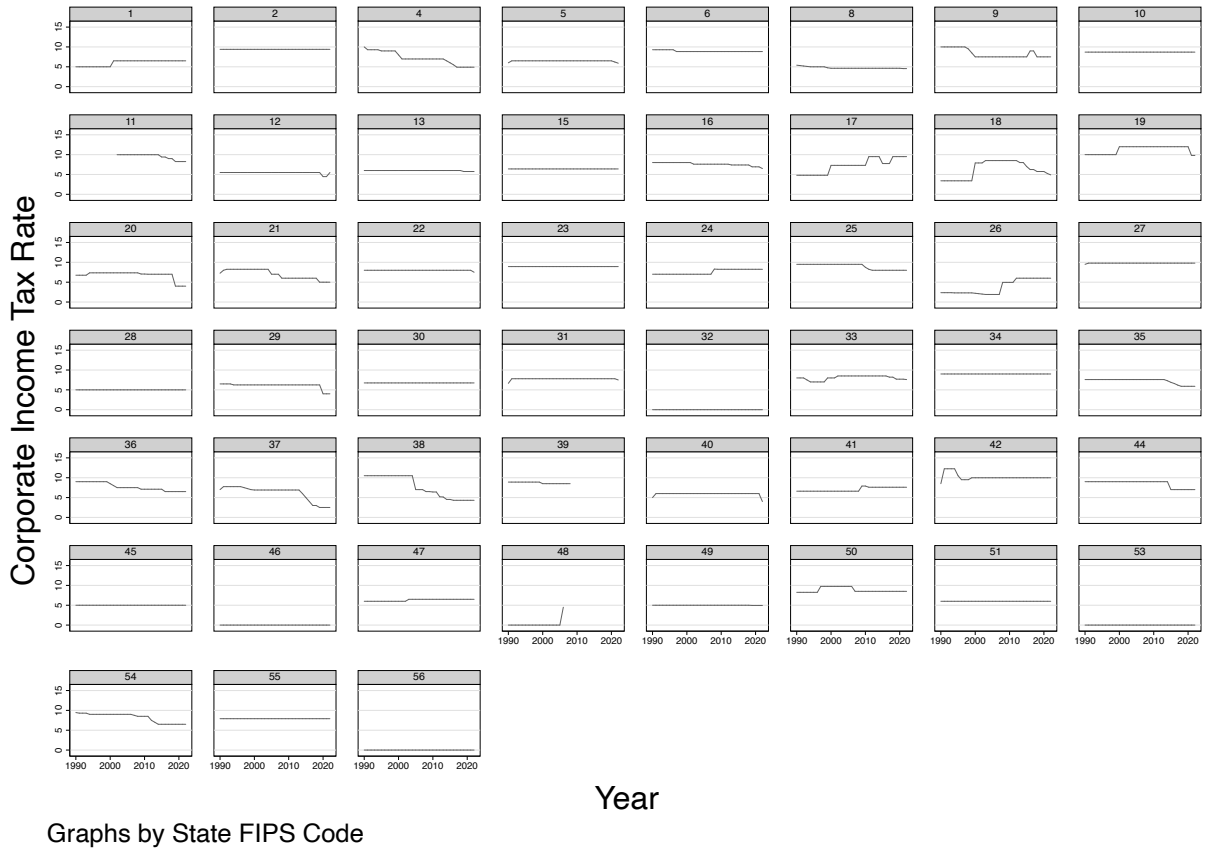
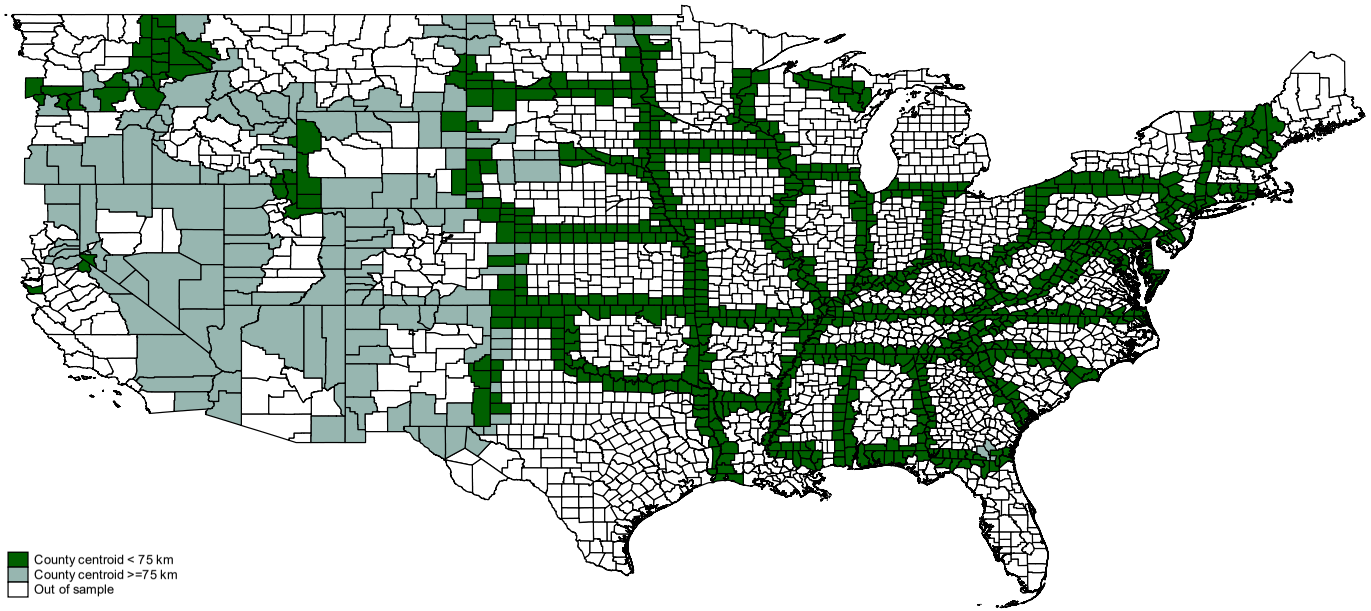
Zhao, H., H. Shen, H. Wang, and Y. Zhu (2024). State corporate tax changes and bond pricing: US evidence. *International Review of Financial Analysis 92*, 103057.

Figure 1: Corporate income tax rates by state



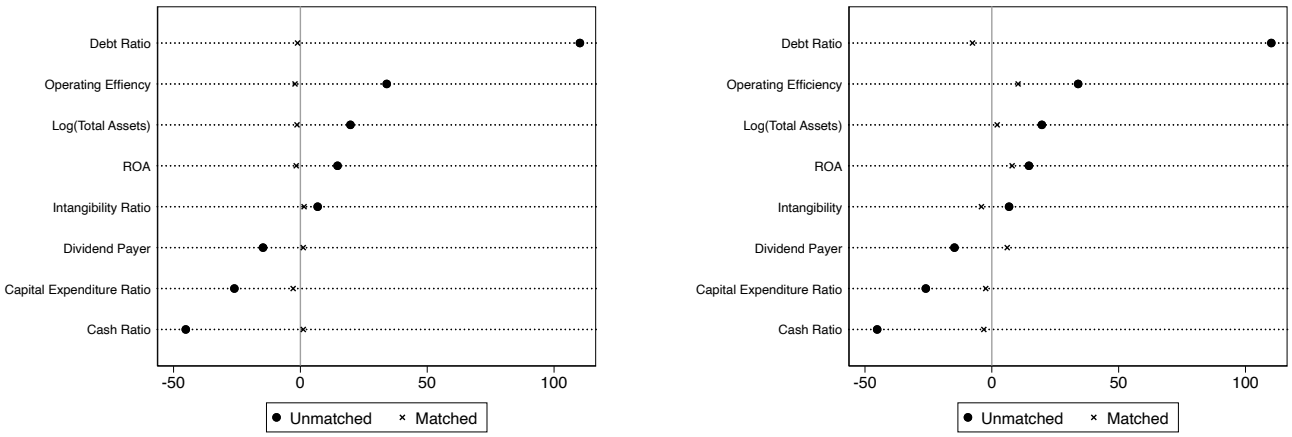
Notes: Figure 1 displays corporate income tax rates for each state over the period 1990-2022. States are identified by their FIPS codes. The data are from Baker et al. (2025).

Figure 2: Counties along state borders



Notes: Figure 2 shows all counties in the contiguous United States. Counties located along state borders are highlighted in green. Counties shown in dark green are adjacent counties whose centroids are less than 75 miles apart, indicating the closer geographic proximity used in our preferred specification.

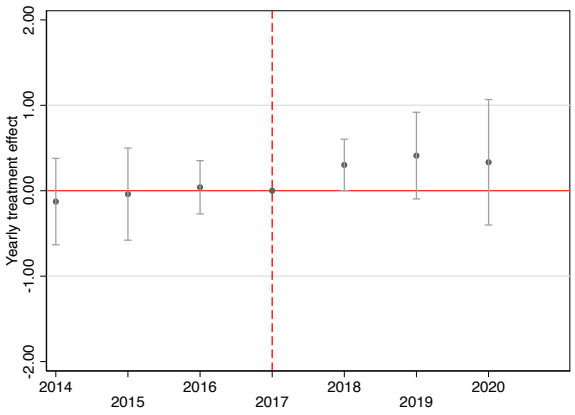
Figure 3: Matched characteristics



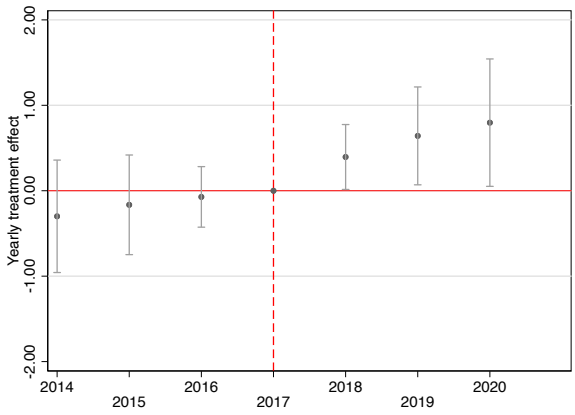
(a): One-to-three nearest-neighbor matching (b): One-to-one nearest-neighbor matching

Notes: Figure 3 displays the standardized mean differences, before and after matching, for the covariates used in the nearest-neighbor matching. Panel (a) reports results for the one-to-three matching; Panel (b) reports results for the one-to-one matching. Variables are ordered by the magnitude of pre-matching standardized differences (from largest to smallest). 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).

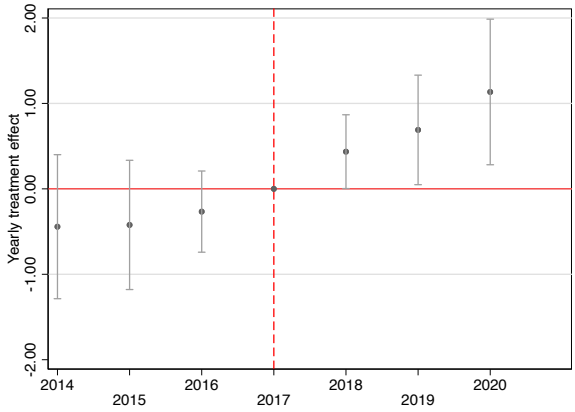
Figure 4: Yearly treatment effects coefficients



(a): Full sample



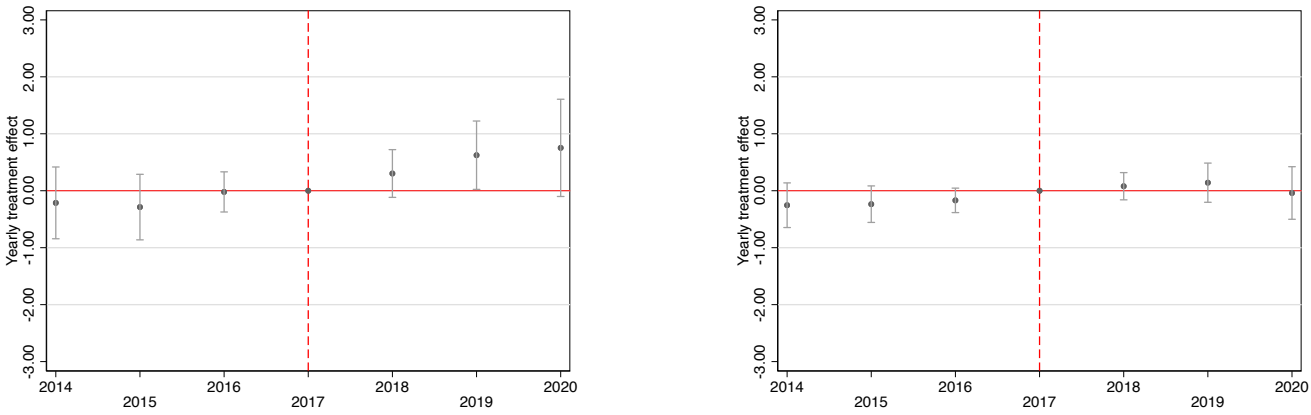
(b): Matched sample (1 to 3 matching)



(c): Matched sample (1 to 1 matching)

Notes: Figure 4 shows the yearly treatment effects from Equation (9) 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 year fixed effects. Plots display yearly point estimates with 95% confidence intervals based on standard errors clustered at the firm-level.

Figure 5: Threshold and placebo tests: Yearly treatment effects



(a): Threshold test

(b): Placebo test

Notes: This figure shows the yearly treatment effects from event-study specifications estimated on the sample of legally independent establishments. In Panel (a), the sample is restricted to firms with a pre-reform interest-to-EBITDA ratio between 15% and 45% (a ±15 percentage point window around the true 30% policy threshold). The treatment variable is an indicator equal to one if the firm is constrained by Section 163(j). In Panel (b), the sample is restricted to firms with a ratio between 0% and 30%, firms never constrained by Section 163(j). The placebo-treated group consists of firms with a ratio between 15% and 30%, with firms below 15% as the control group. The omitted year is 2017. All regressions include establishment and year fixed effects. Plots display yearly point estimates with 95% confidence intervals based on standard errors clustered at the firm level.

Table 1: Summary statistics by establishment type

Variable	Count	Mean	σ	p25	p50	p75
Panel A: Legally Independent Establishments						
Paydex	845,354	70.9998	9.1520	67.0000	73.0000	77.5000
Corporate Income Tax Rate	845,354	6.8234	2.7172	5.5000	7.3000	8.8400
Log(Population)	845,354	13.3253	1.3466	12.5732	13.4982	14.2100
Log(Income Per Capita)	845,354	10.5989	0.4473	10.2759	10.5765	10.8833
Log(Unemployment Insurance Per Capita)	845,354	4.8444	0.9478	4.2047	4.7536	5.3132
Sales Tax Rate	845,354	5.5205	1.4385	4.7500	6.0000	6.2500
Property Tax Rate	845,354	0.0531	0.4244	0.0000	0.0000	0.0000
Top Personal Income Tax Rate	845,354	5.6216	3.6150	3.0700	5.9500	7.8800
Share of Employees in the State	845,354	0.2737	0.3279	0.0341	0.1136	0.4035
Δ Paydex	845,354	-0.2780	6.3920	-2.5000	0.0000	2.0000
Δ Corporate Income Tax Rate	845,354	0.0044	0.4178	0.0000	0.0000	0.0000
Δ Log(Population)	845,354	0.0095	0.0134	0.0023	0.0080	0.0151
Δ Log(Income Per Capita)	845,354	0.0385	0.0350	0.0219	0.0395	0.0572
Δ Log(Unemployment Insurance Per Capita)	845,354	0.0114	0.7803	-0.2344	-0.0500	0.1401
Δ Top Personal Income Tax Rate	845,354	0.0304	0.4307	0.0000	0.0000	0.0000
Δ Sales Tax Rate	845,354	0.0201	0.2228	0.0000	0.0000	0.0000
Δ Property Tax Rate	845,354	0.0022	0.0955	0.0000	0.0000	0.0000
Panel B: Non-legally Independent Establishments						
Paydex	3,075,059	70.4747	10.0993	65.5000	73.0000	78.5000
Corporate Income Tax Rate	3,075,059	6.6393	2.6338	5.5000	6.9000	8.8400
Log(Population)	3,075,059	12.9439	1.4422	11.9889	13.1203	13.8271
Log(Income Per Capita)	3,075,059	10.5248	0.3923	10.2547	10.5245	10.7762
Log(Unemployment Insurance Per Capita)	3,075,059	4.8434	0.9569	4.1744	4.7622	5.3845
Sales Tax Rate	3,075,059	5.4583	1.4614	4.5000	6.0000	6.2500
Property Tax Rate	3,075,059	0.0790	0.5617	0.0000	0.0000	0.0000
Top Personal Income Tax Rate	3,075,059	5.4526	3.5115	3.0700	5.7500	7.7500
Share of Employees in the State	3,075,059	0.1058	0.1730	0.0167	0.0409	0.1076
Spillover Effects (Across States)	3,050,083	0.0013	0.1439	-0.0395	-0.0067	0.0050
Δ Paydex	3,075,059	-0.2765	7.0975	-3.0000	0.0000	2.5000
Δ Corporate Income Tax Rate	3,075,059	0.0025	0.4053	0.0000	0.0000	0.0000
Δ Log(Population)	3,075,059	0.0092	0.0137	0.0020	0.0079	0.0149
Δ Log(Income Per Capita)	3,075,059	0.0360	0.0346	0.0200	0.0375	0.0543
Δ Log(Unemployment Insurance Per Capita)	3,075,059	0.0040	0.7871	-0.2540	-0.0561	0.1335
Δ Top Personal Income Tax Rate	3,075,059	0.0187	0.3975	0.0000	0.0000	0.0000
Δ Sales Tax Rate	3,075,059	0.0219	0.2171	0.0000	0.0000	0.0000
Δ Property Tax Rate	3,075,059	0.0031	0.1131	0.0000	0.0000	0.0000

Notes: This table reports descriptive statistics separately for legally independent establishments (Panel A) and non-legally independent establishments (Panel B). The sample covers the period 1991–2022. Data for 1990 are used to construct the first difference for 1991. Variables with the prefix Δ denote year-to-year changes. The Corporate Income Tax Rate, Sales Tax Rate, Property Tax Rate, Top Personal Income Tax Rate, Spillover Effects, and Spillover Effects (Across States), as well as their year-to-year changes, are expressed in percentage points (i.e., raw rates multiplied by 100). See Table B1 for detailed variable definitions and data sources.

Table 2: State-level corporate income tax rates and financial distress

Dependent variable:	(1) Δ Paydex	(2) Δ Paydex	(3) Δ Paydex	(4) Δ Paydex	(5) Δ Paydex
Panel A: Legally Independent Establishments					
Δ Corporate Income Tax Rate	-0.041***	-0.081***	-0.079***	-0.109*	-0.112**
	(0.011)	(0.026)	(0.025)	(0.056)	(0.055)
Year × Industry FEs	Yes	Yes	Yes	Yes	Yes
Δ Economic Indicators	Yes	Yes	Yes	Yes	Yes
Δ Other Taxes			Yes	Yes	Yes
Year × County Pair FEs				Yes	Yes
Sample	All counties	Border counties	Border counties	Border counties	Centroids < 75 miles apart
Approach				County Pair Matching	County Pair Matching
Number of Observations	845,352	269,025	269,025	578,827	518,344
R-squared	0.004	0.007	0.007	0.040	0.040
Panel B: Non-legally Independent Establishments					
Δ Corporate Income Tax Rate	-0.020	-0.061**	-0.064***	-0.020	-0.019
	(0.022)	(0.026)	(0.023)	(0.045)	(0.047)
Year × Industry FEs	Yes	Yes	Yes	Yes	Yes
Δ Economic Indicators	Yes	Yes	Yes	Yes	Yes
Δ Other Taxes			Yes	Yes	Yes
Year × County Pair FEs				Yes	Yes
Sample	All counties	Border counties	Border counties	Border counties	Centroids < 75 miles apart
Approach				County Pair Matching	County Pair Matching
Number of Observations	3,075,058	980,642	980,642	2,136,340	1,836,165
R-squared	0.003	0.004	0.004	0.020	0.020

Notes: This table reports regression results for Equation (3). The outcome variable is Δ Paydex. Panel A focuses on legally independent establishments. Panel B focuses on non-legally independent establishments. Column (1) uses the full sample within the relevant establishment type and includes year times industry fixed effects and controls for local economic indicators; Column (2) restricts the analysis to establishments in counties located along state borders; Column (3) adds controls for other taxes; Column (4) includes year × county-pair fixed effects; Column (5) further restricts the sample to border-county pairs whose centroids are within 75 miles of each other. Standard errors clustered at the state level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: The role of geographic concentration

Dependent variable:	(1) Δ Paydex	(2) Δ Paydex	(3) Δ Paydex	(4) Δ Paydex	(5) Δ Paydex
Δ Corporate Income Tax Rate	-0.002	-0.018	-0.016	-0.063	-0.065
	(0.027)	(0.047)	(0.045)	(0.056)	(0.056)
Share of Employees in the State	-0.285***	-0.261***	-0.261***	-0.237***	-0.216***
	(0.037)	(0.047)	(0.046)	(0.044)	(0.057)
Δ Corporate Income Tax Rate × Share of Employees in the State	-0.141	-0.274**	-0.275**	-0.190*	-0.195*
	(0.089)	(0.130)	(0.128)	(0.096)	(0.098)
Year × Industry FEs	Yes	Yes	Yes	Yes	Yes
Δ Economic Indicators	Yes	Yes	Yes	Yes	Yes
Δ Other Taxes			Yes	Yes	Yes
Year × County Pair FEs				Yes	Yes
Sample	All counties	Border counties	Border counties	Border counties	Centroids < 75 miles apart
Approach				County Pair Matching	County Pair Matching
Number of Observations	845,352	269,025	269,025	578,827	518,344
R-squared	0.004	0.007	0.007	0.040	0.040

Notes: This table shows regression results for Equation (4). The outcome variable is Δ Paydex. Column (1) uses the full sample of legally independent establishments and includes year times industry fixed effects and controls for local economic indicators; Column (2) restricts the analysis to establishments in counties located along state borders. Column (3) adds controls for other taxes. Column (4) include pair-by-year fixed effects. Column (5) further restricts the analysis to establishments in counties located across state borders, whose centroids are within 75 miles of each other. Standard errors clustered at the state level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Spillover effects on establishment financial health

Dependent variable:	(1) Δ Paydex	(2) Δ Paydex	(3) Δ Paydex	(4) Δ Paydex	(5) Δ Paydex
Panel A: Spillover effects					
Δ Corporate Income Tax Rate	-0.009	-0.047*	-0.051**	-0.010	-0.012
	(0.023)	(0.028)	(0.025)	(0.045)	(0.047)
Spillover Effects	-0.203***	-0.320***	-0.320***	-0.317***	-0.253***
	(0.071)	(0.086)	(0.086)	(0.089)	(0.085)
Year × Industry FEs	Yes	Yes	Yes	Yes	Yes
Δ Economic Indicators	Yes	Yes	Yes	Yes	Yes
Δ Other Taxes			Yes	Yes	Yes
Year × County Pair FEs				Yes	Yes
Sample	All counties	Border counties	Border counties	Border counties	Centroids < 75 miles apart
Approach				County Pair Matching	County Pair Matching
Number of Observations	3,063,123	976,836	976,836	2,127,937	1,829,304
R-squared	0.003	0.004	0.004	0.020	0.020
Panel B: Spillover effects (across states)					
Δ Corporate Income Tax Rate	-0.032	-0.070***	-0.073***	-0.035	-0.032
	(0.024)	(0.026)	(0.022)	(0.048)	(0.050)
Spillover Effects (Across States)	-0.220**	-0.234**	-0.234**	-0.251**	-0.201*
	(0.092)	(0.106)	(0.106)	(0.099)	(0.103)
Year × Industry FEs	Yes	Yes	Yes	Yes	Yes
Δ Economic Indicators	Yes	Yes	Yes	Yes	Yes
Δ Other Taxes			Yes	Yes	Yes
Year × County Pair FEs				Yes	Yes
Sample	All counties	Border counties	Border counties	Border counties	Centroids < 75 miles apart
Approach				County Pair Matching	County Pair Matching
Number of Observations	3,050,081	973,385	973,385	2,120,625	1,823,960
R-squared	0.003	0.004	0.004	0.020	0.020

Notes: This table reports regression estimates of Equation (3) with Δ Paydex as the dependent variable. Panel A reports results using as an additional independent variable, the measure of spillover effects, computed as indicated in Equation (5), while Panel B uses spillover effects measured across states. Column (1) uses the full sample of non-legally independent establishments; Column (2) restricts the analysis to border counties; Column (3) adds controls for other taxes; Column (4) includes county-pair-by-year fixed effects; Column (5) further restricts the sample to border counties whose centroids are within 75 miles of each other. Standard errors are clustered at the state level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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	0.5768	0.2792	-0.2976	0.0144	0.000
Operating Efficiency	1.4626	1.2750	-0.1876	0.0273	0.000
Log(Total Assets)	7.6550	7.2782	-0.3768	0.1170	0.001
ROA	0.0587	0.0405	-0.0182	0.0092	0.049
Intangibility Ratio	0.2203	0.2043	-0.0160	0.0130	0.220
Dividend Payer	0.4290	0.5024	0.0734	0.0290	0.011
Capital Expenditure Ratio	0.0376	0.0507	0.0131	0.0031	0.000
Cash Ratio	0.0615	0.1113	0.0498	0.0074	0.000

Notes: This table reports pre-treatment differences in firm characteristics (in 2017) between treated and control firms. Differences are computed as Control minus Treated. p-values are based on two-sided t-tests.

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

Variables	Treated mean	Control mean	Mean diff.	Std. Err.	p-value
Panel A: One-to-three nearest-neighbor matching					
Debt Ratio	0.5768	0.5798	0.0031	0.0278	0.911
Operating Efficiency	1.4626	1.4744	0.0118	0.0492	0.812
Log(Total Assets)	7.6550	7.6806	0.0256	0.1422	0.859
ROA	0.0587	0.0607	0.0021	0.0089	0.818
Intangibility Ratio	0.2203	0.2169	-0.0034	0.0188	0.859
Dividend Payer	0.4290	0.4235	-0.0055	0.0396	0.886
Capital Expenditure Ratio	0.0376	0.0390	0.0015	0.0034	0.669
Cash Ratio	0.0615	0.0603	-0.0012	0.0069	0.855
Panel B: One-to-one nearest-neighbor matching					
Debt Ratio	0.5768	0.5974	0.0206	0.0278	0.459
Operating Efficiency	1.4626	1.4055	-0.0571	0.0480	0.235
Log(Total Assets)	7.6550	7.6146	-0.0404	0.1496	0.785
ROA	0.0587	0.0488	-0.0099	0.0100	0.320
Intangibility Ratio	0.2203	0.2299	0.0097	0.0193	0.616
Dividend Payer	0.4290	0.3988	-0.0302	0.0382	0.431
Capital Expenditure Ratio	0.0376	0.0388	0.0012	0.0034	0.716
Cash Ratio	0.0615	0.0650	0.0035	0.0074	0.641

Notes: This table reports post-matching differences in firm characteristics in 2017 between treated and matched control firms. Panel A reports results for the one-to-three nearest-neighbor matched sample; Panel B reports results for the one-to-one nearest-neighbor matched sample. Differences are computed as Control minus Treated. p-values are based on two-sided t-tests.

Table 7: TCJA: Establishment-level analyses

Dependent variable:	(1) Paydex	(2) Paydex	(3) Paydex
Panel A: Full Sample			
Post × Treated TCJA	0.373	0.389	0.395
	(0.298)	(0.280)	(0.283)
Number of Observations	143,712	143,711	139,921
R-squared	0.610	0.611	0.633
Panel B: 1 to 3 Matched Sample			
Post × Treated TCJA	0.722**	0.674**	0.711**
	(0.291)	(0.272)	(0.315)
Number of Observations	45,843	45,839	42,857
R-squared	0.625	0.627	0.659
Panel C: 1 to 1 Matched Sample			
Post × Treated TCJA	0.996***	0.864***	1.003***
	(0.319)	(0.301)	(0.319)
Number of Observations	29,062	29,061	26,641
R-squared	0.622	0.626	0.667
Fixed Effects Included in the Specifications Above:			
Establishment FEs	Yes	Yes	Yes
Year FEs	Yes	No	No
Year × Industry FEs		Yes	Yes
Year × County FEs			Yes

Notes: This table reports establishment-level estimates of Equation (8). Panel A uses the full sample of legally independent establishments. Panel B restricts the sample to establishments belonging to the matched sample of firms obtained using one-to-three nearest-neighbor matching, and Panel C restricts the sample to establishments belonging to the matched sample of firms obtained using one-to-one nearest-neighbor matching. Robust standard errors, clustered at the firm level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Threshold and placebo tests

Dependent variable:	(1) Paydex	(2) Paydex	(3) Paydex
Panel A: Threshold test			
Post × Treated TCJA	0.663**	0.848***	0.956***
	(0.301)	(0.290)	(0.318)
Number of Observations	47,368	47,365	44,325
R-squared	0.614	0.617	0.658
Panel B: Placebo test			
Post × Placebo Treated	0.218	0.084	0.031
	(0.151)	(0.157)	(0.171)
Number of Observations	119,400	119,400	115,757
R-squared	0.609	0.610	0.635
Fixed Effects Included in the Specifications Above:			
Establishment FEs	Yes	Yes	Yes
Year FEs	Yes	No	No
Year × Industry FEs		Yes	Yes
Year × County FEs			Yes

Notes: This table reports establishment-level difference-in-differences estimates for the sample of legally independent establishments. In Panel A, the sample is restricted to establishments belonging to firms with a pre-reform interest-to-EBITDA ratio between 15% and 45% (a ±15 percentage point window around the true 30% policy threshold). The treatment variable is an indicator equal to one if the firm is constrained by Section 163(j). In Panel B, the sample is restricted to establishments belonging to firms with a ratio between 0% and 30%, firms never constrained by Section 163(j). The placebo treatment is an indicator equal to one if the firm's ratio is between 15% and 30%, with firms below 15% serving as the control group. Robust standard errors, clustered at the firm level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: TCJA: Firm-level analyses

Dependent Variable:	(1) Debt Ratio	(2) Interest Expense Ratio	(3) Taxes Paid / Assets
Panel A: Full Sample			
Post × Treated TCJA	-0.058***	0.000	0.004***
	(0.012)	(0.003)	(0.001)
Number of Observations	19,320	17,672	17,434
R-squared	0.808	0.417	0.696
Panel B: 1 to 3 Matched Sample			
Post × Treated TCJA	-0.064***	-0.005**	0.003***
	(0.017)	(0.002)	(0.001)
Number of Observations	5,200	5,100	4,646
R-squared	0.781	0.617	0.741
Panel C: 1 to 1 Matched Sample			
Post × Treated TCJA	-0.074***	-0.004*	0.001
	(0.020)	(0.003)	(0.001)
Number of Observations	3,201	3,154	2,819
R-squared	0.797	0.656	0.748
Fixed Effects Included in the Specifications Above:			
Firm FEs	Yes	Yes	Yes
Year × Industry FEs	Yes	Yes	Yes
Year × State FEs	Yes	Yes	Yes

Notes: This table reports firm-level estimates of Equation (10). Panel A uses the full sample. Panel B restricts the sample to the matched sample of firms obtained using one-to-three nearest-neighbor matching, and Panel C restricts the sample to the matched sample of firms obtained using one-to-one nearest-neighbor matching. Column (3) uses total taxes paid scaled by total assets. Robust standard errors, clustered at the firm level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

This Appendix provides further details on the data and the results of the paper "Taxes and Financial Distress: 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 information on all the establishments in the United States. We restrict our attention to establishments of public firms because they are organized as C-corporations and therefore subject to corporate income taxation.

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 whitespace and common business 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 the 90% threshold, to ensure high matching quality. With this methodology, we successfully merge 937,232 of the establishments in NETS to firms in Compustat.

We use this matched sample to investigate the impact of the TCJA on financial distress. In our analysis, we focus on establishments that existed in 2017 and 2018, 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, expressed in percentage points (i.e., the raw rate multiplied by 100)	Baker et al. (2025)
Log(Population)	The natural logarithm of a county's population	BEA
Log(Income Per Capita)	The natural logarithm of a county's average income per capita	BEA
Log(Unemployment Insurance Per Capita)	The natural logarithm of a county's average unemployment insurance expenditure per capita	BEA
Sales Tax Rate	The state-level statutory rate imposed on retail sales of goods and services, expressed in percentage points (i.e., the raw rate multiplied by 100)	Baker et al. (2025)
Property Tax Rate	The effective rate of property tax rate levied at the state level, expressed in percentage points (i.e., the raw rate multiplied by 100)	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, expressed in percentage points (i.e., the raw rate multiplied by 100)	Baker et al. (2025)
Share of Employees in the State	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), expressed in percentage points (i.e., computed from rates multiplied by 100)	NETS

Table B1: Variable definitions (continued)

Spillover Effects (Across States)	The average tax exposure experienced by all other establishments within the same firm, excluding establishments in the specific state, expressed in percentage points (i.e., computed from rates multiplied by 100)	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, and zero otherwise	Compustat
	Panel B: Firm Level	
Debt Ratio	Ratio of total debt (short-term + long-term) to total assets: (dlc + dltt) / at	Compustat
Operating Efficiency	Ratio of sales to operating expenses: (sale / xopr)	Compustat
Log(Total Assets)	The natural logarithm of total assets: log(at)	Compustat
ROA	Ratio of operating income after depreciation to total assets: (oiadp/at)	Compustat
Intangibility Ratio	Ratio of intangible assets to total assets: (intan /at)	Compustat
Dividend Payer	Indicator variable equal to one if the firm distributes any dividend: dvc is greater than 0	Compustat
Capital Expenditure Ratio	Ratio of capital expenditures to total assets: (capex) / at	Compustat
Cash Ratio	Ratio of cash holdings to total assets: (ch / 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
Interest Expense Ratio	Ratio of interest expense to total debt: (xint / (dlc + dltt))	Compustat
Taxes Paid / Assets	Ratio of total taxes paid to total assets: (txpd / at)	Compustat