How Does Removing the Tax Benefits of Debt Affect Firms? Evidence from the 2017 US Tax Reform*

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Abstract

Despite extensive efforts, the impact of the tax benefits of debt on firm decisions is an open question. The 2017 US tax reform creates an opportunity to directly estimate the effects. The reform limits the tax advantage of debt for all firms except for small businesses with average sales below $25 million. I use the exception threshold in a regression discontinuity design and show that corporate debt declines nearly dollar for dollar as the present value of the tax benefits of debt shrinks, but equity financing is not affected. Treated firms decrease their investments and hiring, consistent with the rise in the cost of external financing. The effects are similar in public and private companies. Although the new law disproportionately reduces the tax benefits of debt in small firms, the evidence suggest that the estimates could be informative about the effects in large companies. Overall, I document a first-order role for tax incentives that affect the cost of capital in shaping corporate financial and real policies.

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"[The elimination of deductions for net interest] takes the tax code out of marginal business decisions, letting market forces more efficiently allocate investment where it is most productive. It also eliminates a tax-based incentive for businesses to increase their debt load beyond the amount dictated by normal business conditions."

U.S. House Committee on Ways and Means, The Tax Reform Blueprint, 2016

1 Introduction

Tax systems in almost all countries have historically subsidized firms' use of debt by making interest expenses deductible from taxable income. Critics argue that the tax-favored status of debt has created a corporate debt pileup, which intensifies economic recessions, and propose to remove the subsidies (e.g., OECD, 2013; IMF, 2016; U.S. House, 2016). This argument implicitly assumes that the tax incentives have led to a large increase in the use of debt. However, despite extensive efforts, the literature is inconclusive about the effect of the tax benefits of debt on corporate leverage. Existing estimates range from the tax benefits having no effect, to asymmetric effects, to first-order effects on the use of debt. This discrepancy is often attributed to the empirical challenges in identifying the effects (Fama and French, 2002; Graham, 2013). Also, a related unresolved debate evaluates whether tax incentives that change the cost of capital for firms affect their real outcomes (e.g., Yagan, 2015; Zwick and Mahon, 2017; Ohrn, 2018; Moon, 2022; Boissel and Matray, 2021). The main goal of this paper is to directly estimate the effects of the tax benefits of debt on firms' financial and real outcomes using the unique setting of the recent US tax reform, which overcomes the challenges in previous studies.

As part of a comprehensive tax reform in the US, the Tax Cuts and Jobs Act of 2017 (TCJA) limits firms' interest deductions to 30% of adjusted taxable income plus interest income for tax years beginning in 2018. This rule reduces the tax advantage of debt for "high-interest" firms, for which interest payments exceed the specified limit. However, to support small businesses, the Act makes an exception for firms with average annual sales below $25 million. This exception creates a discontinuity in the tax benefits of debt for firms on the two sides of the sales threshold.

I employ a regression discontinuity design (RDD), which compares high-interest firms in a narrow bandwidth on both sides of the threshold, and document a first-order impact from the reduction in the tax advantage of debt on policies of public and private US firms. Treated firms significantly decrease debt financing but do not change equity financing, compared to the control group. Also, the financing effects spill over to real outcomes through the cost of capital channel, as treated firms decrease hiring and investments relative to control firms.

The empirical setting provided by the TCJA has two special features that make it ideal for my purposes. First, The TCJA is unique in providing a shock to the *tax benefits* of debt and allowing for direct estimation of its impact. Previous studies exploit changes in corporate *tax rates* to overcome endogeneity issues. However, changes in tax rates affect corporate decisions in many ways, not just through the tax benefits of debt (Chen and Frank, 2021). For example, Ivanov, Pettit, and Whited (2021) show that tax hikes can depress debt financing because the increase in default probability outweighs the rise in the tax benefits of debt. Importantly, the RDD in this paper isolates the effects of the change in interest deductibility from other aspects of the tax reform—such as changes in tax rates—because those aspects affect firms on both sides of the threshold similarly.

Second, although limiting interest deductions had been discussed since the first tax reform proposal in 2016, the details and the exception rule emerged only in the final bill that was introduced two months before the new tax code went into effect. So, the discontinuity in the tax benefits of debt was likely exogenous and unexpected by firms.

The validity of the RDD hinges on the assumption that, absent the treatment, firms on the two sides of the assignment threshold would have similar policies. I show that, prior to the tax reform, no discontinuity is detected in key characteristics of firms on the two sides of the threshold. Also, it is unlikely that firms are able to manipulate their treatment status in the two years after the reform that is covered in my sample. This is because the $25 million exception rule depends on the average annual sales based on a three-year rolling window. Formal tests confirm this conjecture and find no discontinuity in the distribution of firms around the cutoff after the reform.

The first set of results from the RDD documents a strong treatment effect on firms' financing policies. Firms that lose the tax benefits of debt decrease their debt ratio by 37.2% (23.9%), according to the baseline (most conservative) estimate. This translates into an almost dollar-for-dollar relation between corporate debt and the present value of the tax benefits of debt. By construction, the estimates are based on a sample of small and high-leverage firms. For example, in the baseline sample of public firms, the average firm has around $46 million in capital stock and $38 million in long-term debt. I estimate that, because of the limitation on interest deductions, the average treated firm in this sample loses $13.50 million in the present value of the tax benefits of debt. In response, the most conservative estimate suggests that treated firms decrease long-term debt by an average $11.17 million compared to control firms. This means that per one dollar of reduction in the present value of the tax benefits of debt, firms decrease debt by \$0.83. However, the equity financing of treated firms is not affected, suggesting that firms do not simply replace debt with equity as the tax benefits of debt shrink. To validate these results, I show that the treatment effect on debt is stronger in firms with high profits and low non-debt tax shields, for which the debt tax shield is most valuable.

Given the reduction in total financing in treated firms, it is important to evaluate whether the changes in financing policies affect firms' real outcomes. The RDD estimates suggest that losing the tax benefits of debt indeed negatively affects firms' investments and hiring. My most conservative estimates suggest that, in the baseline sample, treated firms decrease their investment rate by 7.3% and their hiring rate by 18.4% relative to the control group. This translates into \$3.41 million less in investments and 31.28 fewer workers in an average treated firm in the baseline sample.

I also evaluate the treatment effects separately in a sample of private US firms. The effects are ex ante unclear because private companies typically face additional informational frictions that could affect their optimal financial policies and how they interact with tax incentives. However, I find that the estimated treatment effects on debt and equity decisions of private firms are similar in direction and size to those of public firms. Regarding the effects on real outcomes, the estimates suggest that treated firms decrease their hiring rate by 6.8% compared to the control group, while the negative effect on investments is not statistically significant among private firms. Given the importance of private businesses in the aggregate economy, these results are useful in evaluating the aggregate impact of the changes in the tax incentives.

I consider two potential explanations for the results. One hypothesis is that the results could be driven by changes in firm characteristics and not by the reduction in the tax benefits of debt. I rule out this possibility by showing that the treatment has no effect on key firm characteristics that are important for financial and real policies, such as market-to-book, profitability, and tangibility ratios.

In contrast, the main findings are consistent with the cost of capital channel, as in Glover, Gomes, and Yaron (2015). While limiting interest deductions reduces the incentive to issue debt and thus leads to lower leverage, it also increases the cost of external financing for treated firms. This increase in the cost of capital leads to a decline in new investments and hiring. Also, because the cost of equity is generally higher than the cost of debt, treated firms do not raise equity to replace the decline in debt, which is consistent with a decline in demand for total new financing. I confirm the cost of capital channel by studying the heterogeneity in treatment responses based on the firms' sensitivity to the cost of external capital, which is proxied by cash constraints, following Becker, Jacob, and Jacob (2013), Yagan (2015), and Moon (2022). I find that cash-constrained firms, which are more sensitive to the cost of capital because of their reliance on external financing, react more strongly to the treatment than cash-rich firms, supporting the cost of capital explanation.

Evaluating the sample of all public firms shows that the TCJA limitation on interest deductions disproportionately affects small firms. In fact, firms that belong to the bottom tercile of the size distribution make up a majority of firms for which the tax benefits of debt are reduced. However, it is important to go beyond small firms and evaluate whether the main findings could be generalized to large companies. Given the specific empirical strategy that is dictated by the details of the TCJA, it is not possible to conduct the same tests in a sample of large firms. However, to shed light on this issue, I compare the pre-reform magnitude of debt tax shields relative to taxable income across deciles of the firm size distribution in the sample of public US firms. Debt tax shields are calculated as interest payments multiplied by the firm-level marginal tax rate à la Graham (1996a,b). The tax shield-to-income ratio increases in firm size and is twice as large in the largest firms (top 30%) relative to the smallest firms (bottom 30%). This suggests that the tax benefits of debt are at least as important in the budget of large firms as they are for small firms, which is consistent with the findings of Nikolov, Schmid, and Steri (2021) and Ivanov, Pettit, and Whited (2021). So, the estimated treatment effects on small firms could be informative for the effects in large companies.

Finally, I conduct a number of robustness and falsification tests to ensure that the main results are not spurious. I show that the main results are robust to changes in RDD specifications, such as the bandwidth around the assignment threshold, the polynomial order, and the method for calculating standard errors. Moreover, to confirm that the main findings are not driven by the RDD framework, I show that the main results are also obtained in standard and dynamic difference-in-differences (DID) settings.

I also run two sets of placebo tests. The first test exploits the sample of "low-interest" firms in a narrow bandwidth around the $25 million cutoff. These firms are excluded from the baseline tests because the TCJA limitation on interest deductions is not binding for them. In both public and private firms samples, the placebo RDD finds no treatment effect in the low-interest sample, suggesting that the baseline results are likely not due to discontinuity in unobservables around the cutoff. The second placebo test shows that the treatment effects disappear when the RDD is estimated based on random pseudo-thresholds that are different from the actual $25 million threshold. This finding suggests that the baseline results are likely generated by a genuine discontinuity at the $25 million threshold created by the change in the tax benefits of debt.

Overall, the results show that tax incentives that affect the cost of capital for firms could have large effects on corporate policies. The findings are informative for policy discussions as more countries evaluate the removal of debt subsidies from the corporate tax system. The findings also contribute to the understanding of the main determinants of corporate financial policies. The lack of clear evidence of tax effects in the previous literature has led many prominent scholars to be skeptical of the trade-off theory of capital structure (e.g., Myers, 1984, 2003). My results document a first-order role for tax incentives in shaping corporate policies.

Outline. The remainder of this paper is organized as follows. Section 2 explains the paper's contribution to the related literature. Section 3 discusses the legal background of the TCJA and the critical details for the tests. Section 4 details the baseline RDD specification and construction of the main sample. Section 5 shows evidence to support the validity of the RDD. Section 6 provides the baseline results using the sample of public firms. Section 7 presents the results in the sample of private firms. Section 8 shows the robustness and falsification tests. Section 9 discusses the magnitude of the estimated elasticities compared to the existing literature, and Section 10 concludes.

2 Related Literature

The deduction of interest payments from taxable income has been a cornerstone of the trade-off theory of capital structure as one of the main benefits of corporate debt.[1] These tax benefits are estimated to be as large as 5% to 10% of firm value (Graham, 2000; Korteweg, 2010; Li, Whited, and Wu, 2016). Thus, many studies have attempted to infer whether the tax benefit of debt is a first-order determinant of firm policies. Short of an exogenous variation in the *tax benefits* of debt (i.e., the deductibility of interest expenses), previous studies have exploited the cross-sectional or times series variation in corporate *tax rates*.

Early cross-sectional studies find a positive relation between firms' marginal tax rates and the use of debt (MacKie-Mason, 1990; Graham, 1996a). However, the effect does not always appear as a first-order concern, even with later improvements in estimating the marginal tax rates (e.g., Graham, Lemmon, and Schallheim, 1998; Blouin, Core, and Guay, 2010).

[1] Graham (2013) provides an excellent review of the effect of taxes on firms' financial policies. Ai, Frank, and Sanati (2021) provide a review within the context of the trade-off theory.

Fama and French (1998, 2002) point out that cross-sectional examinations are vulnerable to potential endogeneity biases because the marginal tax rates could be correlated with firm policies and omitted variables. My estimates are based on an exogenous and unexpected shock to the tax advantage of debt, which addresses the endogeneity concerns. As Section 3 explains, details of the TCJA's limitation on interest deductions, and especially the exception rule for small firms, were not known until a couple of months before the tax reform became effective.

Some studies exploit changes in the corporate tax rates to address the endogeneity concerns, but the results are not always consistent. Givoly, Hayn, Ofer, and Sarig (1992) and van Binsbergen, Graham, and Yang (2010) use the US Tax Reform Act of 1986 and document a positive relation between a firm's effective tax rate and its leverage. Other studies find a similar relation using changes in tax rates in other countries (Rajan and Zingales, 1995; An, 2012; Fan, Titman, and Twite, 2012; Panier, Pérez-González, and Villanueva, 2013; Faccio and Xu, 2015, 2018; Doidge and Dyck, 2015). Also, Desai, Foley, and Hines Jr (2004) and Faulkender and Smith (2016) use specialized data on US multinational companies and show that higher tax rates are associated with greater use of debt by both foreign affiliates and parent companies, respectively.

On the other hand, some find evidence that is inconsistent with tax incentives being a critical driver of financing policy. Graham, Leary, and Roberts (2015) use a century of data on the US corporate capital structure and do not find a relation between taxes and the use of debt. Bargeron, Denis, and Lehn (2018) evaluate tax changes in the US in the early 1900s and find little evidence of taxes as a primary determinant of capital structure choices. Heider and Ljungqvist (2015) use staggered changes in corporate income tax rates across US states and document an asymmetric tax effect. Firms increase their leverage ratio in response to tax increases but do not respond to tax cuts. Perhaps more surprisingly, Ivanov, Pettit, and Whited (2021) also use changes in the US state-level corporate tax rates and document a negative relation between taxes and the use of debt in a sample of small firms.

Importantly, the analysis by Ivanov, Pettit, and Whited (2021) emphasizes the multifaceted effect of taxes on firms. In their model, a corporate tax hike increases both the probability of costly default and the tax benefits of debt. For small firms, the former effect dominates the latter, so tax hikes decrease leverage in small firms and vice versa. In fact, considering the multidimensional effect of taxes on firms offers an avenue for reconciling the conflicting results from studies that use the changes in tax rates to estimate the tax benefits of debt. The estimated effect of a tax change on debt policy could vary in different samples, simply because some aspects of the tax effects (other than interest deductions) are heterogeneous across firms (Gordon and Lee, 2001; Zwick and Mahon, 2017; Fleckenstein, Longstaff, and Strebulaev, 2020).

This evidence highlights the challenge in using the changes in tax rates to estimate the effects of the tax benefits of debt. My empirical strategy overcomes this challenge because it exploits an exogenous variation in the tax benefits of debt created by the TCJA, rather than variation in the corporate tax rates. This approach allows for a direct estimation of the impact of tax incentives on firm policies in isolation. Note that the TCJA is a comprehensive tax reform that changes corporate tax rates, depreciation schedules, and foreign tax provisions, among other features. However, all of these changes affect firms on both sides of the assignment threshold similarly, except for the new limitation on interest deductions, which is discontinuous at the threshold. So, the RDD provides a clean estimation of the effect of the tax benefits of debt.

In a related paper, Carrizosa, Gaertner, and Lynch (2020) use the TCJA's interest deduction limit to study the effect of the tax benefits of debt on capital structure. They perform a cross-sectional comparison of the leverage ratio between low-interest firms and high-interest firms, for which the deduction limit is binding. While this comparison could be informative, it likely does not represent a causal relation between the tax benefits of debt and firm policies. In fact, my analysis shows that, in a formal test that controls for relevant firm-level covariates and fixed effects, this strategy cannot detect the treatment effects, likely because the parallel trends assumption is violated (see Section 8.4 and Appendix F for details).

This paper also contributes to the debate about the degree to which various tax incentives affect corporate real outcomes. The estimated effect of various tax incentives on firms' investments vary across several studies, including Auerbach and Hassett (1992), Cummins, Hassett, and Hubbard (1996), Goolsbee (1998), Chirinko, Fazzari, and Meyer (1999), Desai and Goolsbee (2004), House and Shapiro (2008), Yagan (2015), Zwick and Mahon (2017), Ohrn (2018), Liu and Mao (2019), Maffini, Xing, and Devereux (2019), Moon (2022), and Boissel and Matray (2021). This paper is the first one to evaluate the impact of the tax benefits of debt—one of the most common tax incentives around the world—on real outcomes and document a first-order effect on firms' hiring and investments through its effect on the cost of capital. My findings are consistent with a growing literature that documents substantial firm responses to corporate tax incentives (e.g., Becker, Jacob, and Jacob, 2013; Giroud and Rauh, 2019; Akcigit, Grigsby, Nicholas, and Stantcheva, 2021).

Most of the evidence on the impact of taxes on firms is based on samples of large public firms.[2] The optimal policies of small public firms and private companies could be dominated by non-tax factors, such as information frictions, limited commitment, and cash constraints (Nikolov, Schmid, and Steri, 2021; Li, Whited, and Wu, 2016; Zwick and Mahon, 2017). So, the effect of tax incentives could be different from predictions of the trade-off theory. Nonetheless, I document a first-order impact from the tax advantage of debt on firm policies in both small public firms and small private firms that is consistent with the trade-off theory.

Finally, this paper is related to a broader literature studying the effect of taxes on firms' financial policies. Lin and Flannery (2013) and Cohn, Titman, and Twite (2020) study the effect of investor-level taxes on firm policies. Chen and Frank (2021) evaluate the tax effects in an equilibrium model with both corporate and personal taxes. Glover, Gomes, and Yaron (2015) use a dynamic model to evaluate the policy implications of removing the tax advantage of debt. They predict a substantial decrease in leverage and an increase in the cost of external financing, which are consistent with my results. On the other hand, Li, Whited, and Wu (2016) use a model to show that, in an economy with limited commitment, preserving financial flexibility is a primary concern for firms' debt policy, and changing the corporate tax rate affects optimal leverage only when tax rates are very low. Also, this paper relates to a large literature that studies alternative ways for firms to shield income from taxes (e.g., Graham, Lang, and Shackelford, 2004; Graham and Tucker, 2006). My results show that using interest deductions to shield income from taxes is a first-order concern for firms.

3 Legal Background

Before the TCJA, businesses could generally deduct interest expense from their taxable income. For many years, policymakers had been considering whether to eliminate or reduce interest deductions (Graham, 2013; OECD, 2013; IMF, 2016). This idea gained more attention after the 2008 financial crisis because presumably the tax-favored status of debt has encouraged too much debt financing, which could create or exacerbate economic downturns (Giroud and Mueller, 2017, 2021).

In the US, the first formal policy proposal to limit the tax advantage of debt was included in the June 2016 House of Representatives tax reform plan (U.S. House, 2016). As part of a comprehensive tax reform, the initial plan proposed the complete elimination of interest deductions for businesses to "equalize the tax treatment of different types of financing and reduce tax-induced distortions in investment financing decisions." All of the revised versions

[2] Exceptions are Ayers, Cloyd, and Robinson (2001) and Ivanov, Pettit, and Whited (2021) whose sample include small firms that are mostly privately held.

of the tax reform proposal in the following 18 months called for some type of limitation on interest deductibility but did not include specifics. Finally, the details appeared in the bill that was introduced in the House of Representatives on November 2, 2017. While the legislative process was very uncertain, the TCJA was eventually passed in Congress on narrow margins and was signed into law on December 22, 2017. The new tax code went into effect on January 1, 2018.

The TCJA imposes a significant limitation on the deduction for business interest expense, which is specified in Section 163(j) of the Internal Revenue Code. According to the new tax code, the amount of deductible business interest expense in a taxable year cannot exceed the sum of 1) 30% of the adjusted taxable income for the year, 2) the firm's interest income for the year, and 3) the firm's floor plan financing interest expense for the year. The amount of interest disallowed in a year is carried forward indefinitely but continues to be subject to the limitation in future years.

The adjusted taxable income is equal to earnings before interest, taxes, depreciation, and amortization (EBITDA) until 2021, and equal to earnings before interest and taxes (EBIT) for taxable years beginning after 2021. The floor plan financing interest expense is interest paid to finance the acquisition of motor vehicles held for sale or lease. This is relevant only for motor vehicle dealers, which are dropped from my sample (see details in the next section). So, in this paper, the interest deduction limit is determined by the first two conditions stated above.

Importantly, the TCJA limitation on interest deductions applies to all companies *except* for businesses with average annual sales below a threshold. This exception is put in place to support small businesses. As defined by "the gross receipts test" in Section 448(c) of the tax code, firms that have average annual sales of $25 million or less in the previous three years are exempt from the interest deduction limit. For tax year 2019 and subsequent years, the $25 million amount will be adjusted for inflation. This exception rule is the core of my empirical strategy.

In addition to the small business exception, certain trades or businesses are also exempt from the interest deduction limit. This includes regulated utility, real property, and farming businesses that elect to be excepted. As explained below, I exclude these businesses from the main sample to have a clean identification based solely on the sales threshold.

4 Methodology and Data

Methodology. The TCJA puts a limit on interest deductions rather than eliminating them altogether. So, firms that pay little interest are not affected by this aspect of the new tax code if the deduction limit does not bind. The law reduces the tax benefits of debt only if the firm's interest expense is greater than the Section 163(j) limitation, that is, if interest expense > (interest income + 0.3×EBITDA). I call this group, the "high-interest" firms.

I focus on high-interest firms and employ an RDD to estimate the impact of the tax deductibility of interest on firms' financial and real policies. The exogenous variation in interest deductions comes from the $25 million threshold on average annual sales. There is no limit on interest deductions for firms below the threshold, whereas interest deductions are capped by the Section 163(j) limitation for firms above the threshold. The RDD compares responses to the new tax code between the firms just below and just above the sales threshold.

Therefore, my main sample consists of high-interest firms with three-year average annual sales in a narrow bandwidth around the $25 million threshold. As Figure 1 shows, within this sample, the TCJA creates a clear discontinuity in the tax benefits of debt for firms on the two sides of the sales threshold. Firms with average sales just above the threshold are the treated group, and those just below the threshold are the control group. Later, I will use the sample of "low-interest" firms as a natural setting for a falsification test.

[Figure 1 around here]

I use a local polynomial regression to estimate the causal effects of limiting interest deductions. Because sales are measured in millions of dollars, I use the log of sales as the assignment variable. In particular, the baseline specification is

$$\Delta Y_f = \alpha + \beta \text{ Treated}_f + \sum_{m=1}^{2} \gamma_m^b (s_f - \bar{s})^m + \sum_{n=1}^{2} \gamma_n^a \text{ Treated}_f \times (s_f - \bar{s})^n + \Delta X_f + \varepsilon_f, \quad (1)$$

where the subscript f indexes firms. The left-hand-side variable ΔY_f is the change in a firm policy from before to after the TCJA enactment, that is, $\Delta Y_f = \bar{Y}_{f,\text{after}} - \bar{Y}_{f,\text{before}}$, where $\bar{Y}_f$ is the firm-level average of Y over the respective period. Note that given this data structure, each sample firm becomes one data point in the estimation. The variable Treated_f is a dummy that indicates whether a firm belongs to the treated group. The variable s_f is the log of the sample mean for three-year average annual sales, and $\bar{s}$ equals log(25), so $(s_f - \bar{s})$ is the distance to the sales threshold. Finally, ΔX_f is a vector of firm controls measuring the change in firm characteristics from before to after the change in the tax code.

In all regressions, standard errors are clustered at the three-digit SIC code to account for correlations within the industry.

The baseline model uses second-order polynomials and coverage error-rate (CER) optimal bandwidths, as given by Calonico, Cattaneo, and Titiunik (2014). The CER optimal bandwidths are shown to be less prone to biases than the mean squared error (MSE) optimal bandwidths as given by Imbens and Kalyanaraman (2012). However, I confirm that MSE bandwidths generate qualitatively similar results. In the robustness section, I show that all results are qualitatively similar when other polynomial orders and alternative bandwidths are used, as suggested by Lee and Lemieux (2010) and Roberts and Whited (2013).

Lastly, an important caveat applies to empirical studies that use policy reforms to make a causal inference. A growing literature shows that the estimated response to a shock could be biased when the policy reform is anticipated or expected to be transient (Hennessy and Strebulaev, 2020; Hennessy, Kasahara, and Strebulaev, 2020; Chemla and Hennessy, 2020; Borochin, Celik, Tian, and Whited, 2021). The identification strategy in this paper is based on the creation of a discontinuity in the tax benefits of debt. So, although there could have been expectations about the removal of interest deductions in the tax reform, it was virtually impossible for firms to anticipate the discontinuity cutoff. This is because the exception rule (i.e., the discontinuity) did not exist in the earlier reform proposals (e.g., U.S. House, 2016). Also, there was no major policy discussions about repealing or replacing the TCJA during the post-reform sample years (2018 and 2019). Therefore, it is unlikely that my estimates are severely biased due to expectations.

Data and Sample Construction. I conduct the estimations in two different samples. The first one is a sample of US publicly listed companies and the data come from Compustat. I describe this sample below and use it to produce the baseline results due to the richness of the available data. The second one is a sample of privately owned US firms, for which the data come from Mergent Intellect. I describe this sample in Section 7, where I confirm that the main results are consistent across the two samples.

Both samples cover the period 2016 to 2019, which is from two years before to two years after the TCJA's effective date. I choose 2016 as the beginning year to make the sample period symmetric around the enactment of the TCJA. Also, 2016 is the first year that balance sheet data for the sample of private firms are available. However, using the public firms data, I confirm that all results are qualitatively similar if the sample begins as far back as 2013. The samples end before 2020, in which many firms' financial and real decisions were affected in heterogeneous ways by the COVID-19 pandemic.[3]

The baseline sample consists of publicly traded firms that are headquartered in the United States. I require the firms to have the US dollar as their native currency code and drop observations if capital (i.e., nonfinancial assets) is below \$1 million. I drop businesses that are exempt from the Section 163(j) limitation on interest deductions, which include the regulated utility (SIC 4900–4999), real estate (SIC 6500-6599), and agriculture (SIC 0100-0999) sectors. I also drop motor vehicle dealers (SIC 5511-5521 and 5551-5599) to eliminate the effect of floor plan financing on the Section 163(j) limitation. Finally, as commonly done in corporate finance studies, I drop the financial (SIC 6000-6799) and public administration (SIC 9100–9999) sectors to keep the focus on industrial firms.

As mentioned above, the tests focus on high-interest firms, so in the baseline sample I only keep observations with interest expense > (0.3×EBITDA + interest income). Also, to create a symmetric sample, I only keep firms that have observations both before and after the TCJA's effective date and have the same treatment status throughout the sample period. All dollar values are converted to US\$(2018), and raw firm-level variables are winsorized at 1% on both tails to prevent potential biases from outliers and misrecorded data.

Sample Description. In each of the baseline tests, the CER optimal bandwidth around the RDD threshold determines the sample boundaries. However, to describe the characteristics of a typical firm in the tests, I use the bandwidth of 1.250 around the $\bar{s} = \log(25)$ threshold. This choice is in the middle of the range of optimal bandwidths for the main measures of financial leverage that are discussed later. This creates a sample of high-interest firms with the log of average annual sales in the range [1.969, 4.469], which is equivalent to average annual sales in the range [\$7.16m, \$87.27m].

Table 1 provides summary statistics for the firm-level variables in the baseline sample of public firms. Appendix A provides definitions and details on the construction of the variables. In this sample, the average firm has \$46.73 million in capital (i.e., nonfinancial assets) and average annual sales of \$33.99 million. The market-to-book ratio is 2.65 on average and 14.40% of firms' assets are tangible. The average firm in the sample does not make a profit, with a mean profitability ratio of -0.27 and a standard deviation of 0.28, which shows great heterogeneity along this dimension. Negative profitability, however, does not eliminate the tax advantage of debt because, under the new tax code, disallowed interest is carried forward indefinitely. It is worth mentioning that this profitability characteristic is not specific to the sample of small firms. In fact, a sample of all high-interest public firms has

[3]In fact, to subsidize firms during the pandemic, the Section 163(j) limitation was slightly relaxed for the 2020 fiscal year to cap the interest deductions at 50% (instead of 30%) of EBITDA plus interest income.

an average profitability of -0.26, and a sample of all public firms has an average profitability of -0.03 over the same period.

The main dependent variables are normalized by non-financial assets to prevent changes in cash from affecting the results via the denominator. However, the main results are robust to normalizing the variables by total assets. The average firm in the sample has a debt ratio of 0.44, which is higher than that in a typical Compustat firm. However, the higher financial leverage in this sample is fully expected given that it only includes high-interest firms, for which the interest deduction limit is binding. Finally, over the sample period, the average investment and hiring rates are 8.60% and -0.90%, respectively.

Table 1 also provides summary statistics for the treated and control subsamples. Not surprisingly, treated firms have more capital and sales than the control group. The mean differences between the two groups for all other firm characteristics are not statistically significant, except for the market-to-book ratio, which is larger for control firms and could be related to the size differences.

[Table 1 around here]

5 Validity of the RDD

The key identification assumption in an RDD is local continuity—that is, absent the treatment, policies of firms just below the threshold would be similar to those just above the threshold. This sections provides two sets of evidence that support the local continuity assumption in the baseline sample. Similar evidence for the sample of private firms is presented in Section 7.

5.1 Manipulation of the Assignment Variable

An important condition for the validity of an RDD is to confirm that subjects are unable to manipulate the assignment variable near the cutoff and, consequently, their assignment to treatment and control groups. In this case, it is unlikely that firms knew about the exceptions to the interest deduction limit long before the final bill was passed. This is because the exception rule did not exist in the earlier proposals (e.g., U.S. House, 2016). Moreover, the treatment assignment is based on the average annual sales in the previous three years, which makes it difficult to manipulate even if a firm knew about the exception rule in advance.

To support this argument, Figure 2 shows the post-TCJA distribution of high-interest firms in the baseline sample with respect to average sales around the $25 million threshold. The histogram does not show any bunching of observations on either side of the threshold. This evidence is not consistent with firms manipulating their sales to be exempted from the limitation on interest deductions.

[Figure 2 around here]

More formally, I use the procedure developed by McCrary (2008) to test the presence of a discontinuity in the density of average annual sales at $25 million. Figure 3 shows the result in the baseline sample. The solid line is the fitted density with a 95% confidence interval around it. The discontinuity estimate is 0.089, and the corresponding standard error is 0.237. Therefore, the test cannot reject the continuity of average sales around the cutoff at the conventional 5% level. This means that it is unlikely that firms manipulate their average sales in this sample.

[Figure 3 around here]

5.2 Discontinuity in Pre-TCJA Firm Characteristics

To further support the local continuity assumption, I use the baseline sample to examine whether firms on different sides of the threshold are similar prior to the tax reform. I consider debt and equity ratios, investment and hiring rates, as well as several characteristics that are known to affect firms' financial and real policies, including size, market-to-book, profitability, and tangibility. Predetermined characteristics are defined as the pre-TCJA average at the firm level.

First, I perform a visual analysis in Figure 4. Observations are put into non-overlapping bins based on the log of average sales over the sample period. In each graph, the circles show bin averages, and solid lines show polynomial fits of order two on each side of the threshold. Figures 4(a) to 4(c) do not show a significant discontinuity in pre-TCJA firm characteristics. Similarly, the pre-reform leverage ratio (Fig. 4(d)), investment rate (Fig. 4(e)), and hiring rate (Fig. 4(f)) seem to be similar on both sides of the cutoff.

[Figure 4 around here]

More formally, I test the pre-reform similarity of firms above and below the cutoff by replacing the outcome variable with each characteristic in the baseline RDD in Equation 1.

Table 2 shows the results. The coefficient β estimates the discontinuity in each characteristic at the cutoff. All estimates are statistically insignificant, suggesting that, prior to the tax reform, firms on both sides of the threshold have similar underlying characteristics as well as similar financial and real policies.

[Table 2 around here]

6 Effects of Interest Tax Deductions on Firm Policies

In this section, I use the baseline sample of public firms to investigate the effects of the reduction in the tax benefits of debt on firms' financial and real policies. Before discussing the formal estimates, Figure 5 provides a visual analysis. Firms are again put into bins based on the log of three-year rolling window average annual sales over the sample period. The circles show bin averages, and the solid lines show quadratic fits on each side of the $25 million average sales cutoff.

The figures show the change in each firm policy from before to after the tax reform. Figure 5(a) shows that firms just above the threshold (i.e., treated firms) have a smaller change in the debt ratio compared to firms just below the threshold (i.e., control firms). However, Figure 5(b) shows no difference in equity on the two sides of the threshold. Taking together, these figures suggest that firms use less debt financing when the tax benefit of debt is reduced, but equity financing is not affected. Figures 5(b) and 5(c) show that the change in investments and hiring in firms above the threshold are lower than those in firms below the threshold. These figures suggests that the tax benefit of debt also affects real policies through its effect on financing policies. I provide formal tests of these effects below and estimate the sensitivities of these policies with respect to the tax benefits of debt.

[Figure 5 around here]

6.1 Effects on Financing Policies

I start by using the baseline RDD (Equation 1) to estimate the impact of the limitation on interest deductions on firms' debt and equity financing in the baseline sample. Table 3 presents the results. The dependent variables are the changes in firm policies from before to after the tax reform. The baseline measure of financial leverage is the ratio of long-term debt to capital, mainly because short-term debt is often used for working capital needs and is unlikely to be altered in response to tax incentives. However, the results are robust to using the total debt ratio as shown in Appendix B. I use the optimal bandwidth for the debt ratio for all financing variables to keep the sample constant across the regressions. The optimal bandwidth, which determines the sample boundaries above and below the log(25) cutoff, is shown in the table along with the range for average sales in dollars for easier interpretation.

[Table 3 around here]

Baseline Estimates. Column 1 of Table 3 shows that, compared to the control group, treated firms reduce their debt ratio by 0.37 from before to after the tax reform. Importantly, however, column 3 shows that the treatment does not significantly affect firms' equity financing. Column 5 shows that the treatment causes a reduction in the overall external financing, which combines debt and equity financing. To interpret the magnitude of coefficients, it is critical to remember that the sample consists of highly levered firms, for which the interest deduction limit binds, with the average leverage ratio of 0.44. Overall, the results in Table 3 suggest that treated firms use significantly less debt financing than the control group, but their equity financing is not affected. This implies that firms do not simply substitute debt with equity as the tax benefits of debt shrink.

To further support the RDD estimates, I add relevant firm characteristics to the right-hand side of the regressions. These include the change in firms' size, market-to-book, profitability, and tangibility (Frank and Goyal, 2009). Columns 2, 4, and 6 show that adding the covariates increases the estimation precision but does not significantly change the estimates, as the coefficients are statistically indifferent from those in columns 1, 3, and 5, respectively. These results further suggest that the RDD's local continuity assumption is likely satisfied (Roberts and Whited, 2013). Finally, in the robustness section, I show that similar results are achieved with alternative bandwidths and polynomial orders.

Sensitivities to a Dollar of Tax Benefits. The results in Table 3 can be used to estimate the sensitivity of firms' debt policies to a dollar of tax benefits. The sensitivity is equal to the amount of debt reduction divided by the present value of lost tax benefits. Using the estimate in column 1 of Table 3, the average firm reduces debt by \$17.38 million ($= -0.372 \times \$46.73$m) in response to the treatment.[4] Considering various specifications that will be discussed in the robustness section, the most conservative estimate suggests that the average firm reduces debt by \$11.17 million ($= -0.239 \times \$46.73$m).[5]

[4] To make the sensitivity calculations, I assume that the change in debt ratio is driven by debt (i.e., the numerator) rather than capital in the denominator.

[5] The most conservative estimate is generated by an RDD model with second-order polynomials and a fixed bandwidth of 1.5.

To calculate the value of lost tax benefits, I assume that the firm's capital structure is stationary and interest expense remains constant in the future. The tax benefit of debt is equal to the amount of allowed interest deductions multiplied by the marginal tax rate. Assuming that the tax benefit is a perpetuity, I compute the present value using the average interest rate paid on the firm's long-term debt as the discount rate. So, the change in the present value of tax benefits is

$$\Delta PV_{\text{tax deductions}} = \frac{\tau_{after} \times \text{Allowed Interest}_{after}}{\bar{r}_{after}} - \frac{\tau_{before} \times \text{Allowed Interest}_{before}}{\bar{r}_{before}}, \quad (2)$$

where τ is the marginal statutory tax rate and $\bar{r}$ is the average discount rate in the respective periods. For the average treated firm, the present value of lost tax benefits is equal to \$13.50 million.

I use these estimates to compute the elasticity of debt with respect to the tax benefits of debt. According to the baseline estimate, for one dollar of lost tax benefits of debt, the average treated firm in the sample reduces debt by \$1.29 ($= \frac{\$17.38\text{m}}{\$13.50\text{m}}$). Based on the most conservative estimate, debt is reduced by \$0.83 ($= \frac{\$11.17\text{m}}{\$13.50\text{m}}$) per one dollar of lost tax benefits. In Section 9, I also estimate elasticities with respect to the cost of capital and compare the estimates with those in the existing literature.

Heterogeneous Treatment Effects. The value of debt tax shields varies depending on firms' profitability and the existence of non-debt tax shields. If the baseline results are indeed driven by the variation in the tax benefits of debt, the magnitude of the treatment effect should vary along these dimensions. This suggests two validation tests, which are presented in Table 4.

[Table 4 around here]

First, the effect of tax incentives on debt should vary with profits. Interest deductions are most valuable for firms that make large profits because they shield current year income from taxes. But less profitable (or loss-making) firms can only carry forward interest expenses to shield future profits, so they discount the tax benefits of debt. I sort firms based on average gross profits before the tax reform and create a dummy variable, *High-profit*, that equals 1 for top tercile firms and 0 otherwise. The profit sort is done within the narrow bandwidth around the sales cutoff to minimize the effect of firm size.[6] I interact the high-profit dummy

[6]These tests include interaction terms on the right-hand side of the regression, which affect the optimal bandwidth calculation. For consistency across tests, I use the baseline sample with the optimal bandwidth that was used in Table 3. Nonetheless, the results are qualitatively similar with alternative bandwidth choices.

with the treatment indicator and reestimate the RDD. The results are shown in the first two columns of Table 4. Column 1 shows the results for debt policy. The negative coefficient on the interaction term suggests that the treatment effect on high-profit firms is about twice as large as the effect on low-profit firms. Column 2 shows the effect on equity financing and suggests that high-profit firms increase their equity ratio by 3.7 percentage points, although it is not nearly enough to offset the much larger reduction in the debt ratio. Similarly, in the robustness section, I show that the treatment effects on financing and real policies are concentrated in firms with positive pre-reform profits.

Second, the impact of the tax benefits of debt also depends on whether the firm has any non-debt tax shields that could work as a substitute for interest deductions (Graham, Lang, and Shackelford, 2004; Graham and Tucker, 2006). Firms that have a large non-debt tax shield may be able to effectively shield their profits and not care about interest deductions. By contrast, the tax benefit of debt is most valuable for firms that have small or no non-debt shields. I define non-debt tax shields as the sum of "depreciation and amortization" and "investment tax credit" normalized by gross profits. I sort sample firms based on this measure and create a dummy variable, *Low non-debt shield*, that equals 1 for bottom tercile firms and 0 otherwise. The RDD is reestimated while the dummy variable is interacted with the treatment indicator. Columns 3 and 4 of Table 4 show the effects on debt and equity financing, respectively. The coefficients on the interaction term suggest that treated firms that have small non-debt shields in fact reduce debt financing more strongly than other treated firms. However, equity financing policy is not different across the two groups.

Overall, these patterns are consistent with the predictions and support that the baseline results are driven by the variation in the tax benefits of debt, which has a significant effect on corporate debt policy.

6.2 Effects on Investments and Hiring

The results so far establish that, in the baseline sample, firms decrease debt financing nearly dollar for dollar when facing a reduction in the tax benefits of debt. In addition, firms do not use more equity to replace the reduction in debt. Since total external financing is reduced, it is important to understand whether the changes in financing policies affect firms' real outcomes such as investment and hiring. These tests are motivated by studies that link firms' financing and real outcomes (e.g., Whited, 1992; Hennessy, 2004; Chava and Roberts, 2008).

I use the baseline RDD with optimal bandwidths to test the effect of the interest deduction limit on real outcomes. The dependent variables are the change in hiring and investment rates from before to after the tax reform. Table 5 shows the results. Column 1 and 3 suggest that treated firms decrease their hiring rate by 26.9% and investment rate by 12.1% compared to control firms. The average firm in the baseline sample has 170 employees and \$46.73 million in capital. The baseline coefficient estimates mean that the average firm hires 45.73 ($= -0.269 \times 170$) fewer workers and decreases investment by \$5.65 million ($= -0.121 \times \$46.73$m) in response to the treatment. The respective numbers from my most conservative estimate are 31.28 ($= -0.184 \times 170$) fewer workers and \$3.41 million ($= -0.073 \times \$46.73$m) less in investments.

[Table 5 around here]

I also estimate a version of the RDD that includes relevant firm covariates. These are the changes in market-to-book and profitability (i.e., cash flows), which are the common firm-level controls in empirical investment models. Columns 2 and 4 of Table 5 confirm the main results and show that the estimated treatment effects are statistically indistinguishable from those in columns 1 and 3, respectively, supporting the robustness of the RDD.

6.3 What Drives the Main Results?

Overall, the RDD results show that, in response to the limitation on interest deductions, firms use less debt financing but do not change equity financing. Also, there is a negative impact on firms' hiring and investments. To explain these findings, I first evaluate the hypothesis that the results are driven by changes in firm characteristics. One may argue that the tax reform somehow affects firms on the two sides of the sales threshold differently, such that key firm characteristics of the treated and control firms diverge after the reform. This, in turn, could create a divergence in optimal firm policies of the two groups.

I test this hypothesis by using the RDD to evaluate whether characteristics of the treated and control firms evolve differently around the tax reform. I use the baseline sample and focus on the key determinants of financial and real policies, including market-to-book, profitability, and tangibility ratios. I do not include firm size because it is directly affected by the investment policy. Table 6 shows the RDD estimations with optimal bandwidths. In all three regressions, the estimated treatment effect is statistically indistinguishable from zero. This means that there is no difference between the treated and control firms in the evolution of key firm characteristics from before to after the reform. One may expect that the market-to-book ratio be negatively affected for treated firms because of losing the tax benefits of debt. As Faccio and Xu (2018) show, however, the impact of tax changes on firm value is not as straightforward when firms adjust their leverage at the same time, because the changes in leverage affect equity value separately from the tax effects. Overall, these results, along with the evidence on pre-reform firm similarity in Section 5, suggest that the main findings are unlikely to be driven by differential trends in firm characteristics.

[Table 6 around here]

I interpret the main results through the cost of capital channel, similar to the economic mechanism discussed by Glover, Gomes, and Yaron (2015). This channel explains the effects on real outcomes as a spillover from the effects on financial policies. The reduction in the tax benefits of debt increases the cost of debt and, in turn, the overall cost of capital for treated firms. The rise in the cost of capital decreases the net present value of firms' prospective projects, which decreases investments, hiring, and demand for new financing. This explanation is also consistent with firms not replacing debt with equity. Note that raising equity does not help the firm because the cost of equity is generally higher than the cost of debt. So, raising equity would not reduce the cost of capital and increase the project's net present value. Therefore, even if the firm raised equity, it would not prevent the drop in real outcomes.

To test this mechanism, I study the heterogeneity in firm responses to the treatment based on the firms' sensitivity to the cost of capital, following Becker, Jacob, and Jacob (2013), Yagan (2015), and Moon (2022). Cash constraints are used as a proxy for sensitivity to the cost of external capital. Cash-constrained firms that rely on external financing are more sensitive to the cost of external capital than do cash-rich firms that use internal funds. Therefore, the cost of capital channel predicts that cash-constrained firms respond more aggressively to the reduction in the tax benefits of debt, which increases the cost of debt.

Identifying which firms are cash-constrained is difficult. For example, lagged sales, as used by Yagan (2015), directly determines whether a firm was treated in my setting. Instead, following Becker, Jacob, and Jacob (2013) and Moon (2022), I use pre-reform retained earnings scaled by capital as a proxy for cash-constraints. I sort firms based on this proxy and create a *Constrained* dummy variable that equals 1 for the bottom tercile of firms and 0 otherwise. The revised RDD interacts the *Constrained* dummy with the treatment indicator. Table 7 shows the results. The coefficients on the interaction terms in columns 1, 3, and 4 suggest that cash-constrained firms react more strongly to the treatment in reducing debt financing, hiring, and investments, respectively. Column 2 shows that cash-constrained firms are similar to the others in not using equity financing to replace the reduction in debt. These results are consistent with the cost of capital channel as explained above.

[Table 7 around here]

6.4 Implications Across the Firm Size Distribution

The results so far are based on a sample of small public firms with annual sales up to around \$100 million. This is, of course, by design and a result of the specific identification strategy based on small firms' exemption from the interest deduction limitation. Small firm results are informative and complement the literature, which mostly focuses on large public firms. Nonetheless, it is important to evaluate the implications for a broader range of firms.

First, I investigate the fraction of firms across different size groups that are directly affected by the limitation on interest deductions. I compare the fraction of high-interest firms across deciles of the firm size distribution in the sample of all public US firms before the tax reform (i.e., 2016 and 2017). As defined in Section 4, a firm is considered high-interest if interest expense > (interest income + 0.3×EBITDA). These are firms, for which the interest deduction limitation is binding, and that their tax benefit of debt is reduced after the reform.

Figure 6 shows the comparison. The fraction of firms with a binding constraint decreases in firm size. Around 68% of firms in the bottom three size deciles are affected by the limit on interest deductions, compared to only 17% of firms in the top three deciles. Also, a majority of firms that are impacted by the new Section 163(j) limitation and lose the tax benefits are relatively small firms that belong to the bottom three deciles of the size distribution.

[Figure 6 around here]

Second, I evaluate whether the estimated treatment effects in small firms are informative about the responses in large firms. Shedding more light on this issue is useful both to understand the effects of the current law on large firms that are impacted, and to understand the response to a potential future modification of the tax code that fully removes the tax benefits of debt. Such a change will affect the majority of large companies, which are not affected under the current law. To this end, I compare the importance of pre-TCJA tax benefits of debt for firms in different size groups using a sample of all public US firms in Compustat over 2016 and 2017.

Figure 7(a) shows the ratio of the tax benefits of debt to EBIT in deciles of the firm size distribution. The tax benefit of debt is equal to the total interest expense multiplied by the simulated firm-level marginal tax rate (before interest), which is given by Graham (1996a,b).[7] The figure shows that, for the largest firms in the top three deciles, the value of the debt tax shield equals 9.2% of income, which is twice as large as that in small firms in the bottom three deciles, for which the tax shield is valued at 4.6% of income. Furthermore, I note that large firms have a higher marginal tax rate than small firms. To separate the effect of interest deductions from the tax rates, Figure 7(b) plots the total interest expense as a share of EBIT for each decile and shows that the ratio is non-decreasing in size. In the top three deciles, interest deductions are equal to 34% of income, which is larger than the 31% figure in the bottom three deciles, although the difference is not statistically significant.

[Figure 7 around here]

Overall, the results in Figure 7 suggest that, on a relative basis, the tax benefits of debt in large firms are at least as large as those in small firms. This is consistent with the findings of Nikolov, Schmid, and Steri (2021) and Ivanov, Pettit, and Whited (2021). So, the estimated impact of the limitation of interest deductions in small firms is likely to be the lower bound of the impact in larger corporations.

7 Effects of Interest Tax Deductions on Private Firms

Despite the aggregate economic impact of private firms in the US economy, most of the empirical evidence on the relation between taxes and firm policies is based on samples of public companies. Most private firms face additional informational frictions in the capital markets, which could affect their optimal financing policies and how they interact with the tax incentives. So, the treatment effects on private firms are not necessarily similar to the effects on public firms. In this section, I use the RDD approach to investigate the impact of the reduction in the tax advantage of debt on private firms.

7.1 Sample Construction and Summary Statistics

The firm-level data for the sample of private US firms come from Mergent Intellect, which provides firms' financial data starting in 2016. Similar to the baseline case, I end the sample before 2020. The dataset includes millions of small businesses that lack balance sheet data, so I drop firms if sales are below $5 million in 2019, which is the last year in the sample. As

[7] I thank John Graham for sharing the data on marginal tax rates. For more information, see https://faculty.fuqua.duke.edu/~jgraham/taxform.html.

in the baseline sample, I also drop financial and public administration firms, and businesses that are exempt from the Section 163(j) limitation on interest deductions.[8]

The firms' interest expense is not observable in this sample. So, I define high-interest firms as those with $\frac{\text{Long-term debt}}{\text{EBITDA}} > 3$, which is roughly equivalent to the Section 163(j) limitation if private firms on average pay 10% interest on their loans. However, the results are not sensitive to this choice and results are qualitatively similar when this threshold is set to 2 or 4. Finally, to create a symmetric sample, I only keep firms that have observations both before and after the TCJA's effective date and have the same treatment status throughout the sample period. Raw firm-level variables are winsorized at 1% on both tails and all dollar values are converted to US$(2018).

Although the data set provides sales and employment data on roughly 470,000 firms with sales above \$5 million, the balance sheet data are sparsely populated. Therefore, to have a large enough sample of firms for reliable RDD estimations, I include all firms with three-year average annual sales below \$500 million. This creates a balanced panel with 252 firm-year observations over the four-year sample period. Some variables such as market-to-book, tangibility, and short-term debt are not available in this sample. However, all available variables are defined as in the baseline sample (as provided in Appendix A). The only exception is the asset growth rate, which replaces the investment rate because data on capital expenditures are not available. The asset growth rate is defined as the first difference in the log of capital (i.e., nonfinancial assets).

Table 8 provides the summary statistics. In this sample, the average firm has \$966.1 million in capital and average annual sales of \$168.6 million. These averages are larger than those in the baseline sample, however this is expected because this sample includes larger firms with up to \$500 million in average sales. The average firm has a mean profitability ratio of 0.03 with a standard deviation of 0.25, which, similar to the baseline sample, shows significant heterogeneity along this dimension. Finally, the average firm has a debt ratio of 0.35, an asset growth rate of 12.7%, and a hiring rate of 3.6% per year.

Table 8 also provides summary statistics for the treated and control subsamples. Similar to the baseline case, treated firms have more capital and sales than the control group, a pattern that is directly driven by the treatment definition. However, the mean differences between the two groups for all other firm characteristics are not statistically significant, despite the fact that the treated group includes much larger firms.

[Table 8 around here]

[8] See Section 4 for the list of SIC codes.

7.2 Validity of the RDD in the Private Firms' Sample

Validity of the RDD estimates hinges on the lack of firms' ability to change their treatment status by manipulating the assignment variable near the cutoff. As explained before, the timeline of the TCJA and the fact that the treatment assignment is based on the average annual sales in the previous three years, make manipulations unlikely during the sample period. Figure 8 shows the post-TCJA distribution of high-interest private firms around the $25 million sales threshold. Consistent with the no-manipulations conjecture, the histogram does not show any bunching of observations on either side of the cutoff.

[Figure 8 around here]

I also formally test the presence of a discontinuity in the density of average sales at the cutoff using the McCrary (2008) procedure. Figure 9 provides the result, which estimates a discontinuity of 0.825 at the cutoff with a standard error of 0.749. This means that the test cannot reject the continuity of average sales around the $25 million cutoff, supporting the absence of firm manipulations.

[Figure 9 around here]

7.3 Estimating the Treatment Effects on Private Firms

Next, I use the baseline RDD (Equation 1) to investigate the effects of the reduction in the tax advantage of debt on firm policies in the sample of private firms. Table 9 shows the results. Similar to the baseline tests, the dependent variables are the changes in firm policies from before to after the tax reform. However, to increase the sample size, all firms with three-year average annual sales below $500 million are used in the estimations, instead of using the optimal bandwidths around the threshold. Also, because of data availability, market-to-book and tangibility ratios are dropped from the set of firm controls.

The first two columns of Table 9 show the effects on firms' financing. Column 1 shows that treated firms reduce their debt ratio by 0.31 compared to the control group. This estimate is similar and comparable to the estimated 0.37 reduction in the debt ratio of treated public firms. Column 2 shows that the treatment does not have a significant effect on firms' equity financing. This means that, with respect to the financing policies, private firms' response to the changes in tax incentives is similar to the response of public firms in the baseline case.

I use the estimated coefficients to compute the sensitivity of firms' debt policy with respect to a dollar of tax benefits. The estimated coefficient implies that, in response to the treatment, the average firm in this sample decreases debt by \$295.62 million ($= -0.306 \times$ \$966.100m). Following a similar procedure as in the baseline case, I estimate that the present value of lost tax benefits is equal to \$174.34 million for the average treated firm in this sample. Therefore, per one dollar of lost tax benefits of debt, the average firm in this sample reduces debt by \$1.69 ($= \frac{\$295.62\text{m}}{\$174.34\text{m}}$). This estimate compares to the estimated elasticity of 1.29 for an average firm in the baseline public sample.

The last two columns of Table 9 show the impact on firms' real decisions. Column 3 shows that the hiring rate in treated private firms is reduced by 6.8% compared to control firms. This effect is smaller than the 26.9% (18.4%) reduction in the hiring rate of treated public firms in the baseline (most conservative) estimate in the previous section. Finally, Column 4 estimates the treatment effect on the asset growth rate, which is used as a proxy for investment rate in this sample because of data availability. The estimated coefficient has a negative sign but it is not statistically significant, which implies that the treatment does not affect private firms' investments. Although, another possibility is that the investment proxy is not measured precisely in the data given that the dependent variable, ΔAsset growth rate, has a standard deviation of 0.432 in this sample (see Table 8), which is much higher than the volatility of investment rate growth in most samples (including the baseline sample where the standard deviation of ΔInvestment rate is 0.150, see Table 1).

[Table 9 around here]

8 Robustness and Falsification Tests

8.1 Robustness of the RDD

I estimate the RDD with alternative specifications to ensure robustness of the main results. I show that the main findings are robust to using alternative bandwidths around the assignment threshold and alternative polynomial orders, as suggested by Lee and Lemieux (2010) and Roberts and Whited (2013).

Appendix C presents the results. Table C.3 uses the baseline sample and shows the estimations for an RDD with second-order polynomials and alternative bandwidths. Panels (a) and (b) use bandwidths of 1 and 1.5, respectively, around the log(25) threshold. Column 1 in both panels show that the estimated treatment effect on debt is negative and statistically significant. Column 2 in both panels show the lack of response in equity financing. Finally, columns 4 and 5 in both panels confirm the negative treatment effect on firms' hiring and investments. In panel (b), the coefficients are statistically significant, but in panel (a) the p-values are just above the conventional 10% threshold ($p = 0.11$ for investment and $p = 0.14$ for hiring), which is in part because of the smaller sample size due to a narrower bandwidth.

Table C.4 also uses the baseline sample and shows RDD estimations with alternative polynomial orders using optimal bandwidths. Panels (a) and (b) use first- and third-order polynomials, respectively. Again, in both panels, column 1 confirms the negative treatment effect on debt financing and column 2 shows equity financing is not affected. Columns 4 and 5 establish the negative effect on hiring and investment policies.

In the sample of private firms, I confirm that the main results are robust to using first- and third-order polynomials in the RDD. However, using smaller bandwidths in this sample shrinks the sample size and reduces the statistical significance of the estimates while the magnitudes of the coefficients remain similar to the results in Table 8.

Finally, untabulated results confirm that the main findings in both samples are robust to the nonparametric estimation of the standard errors given by Calonico, Cattaneo, and Titiunik (2014).

8.2 Positive versus Negative Profits

To confirm that the main results are indeed driven by the limitation on interest deductions, I study the heterogeneity in firm responses to the treatment based on their pre-reform profitability status. The cap on interest deductions causes an immediate cash flow shock to treated firms with pre-reform positive profits because the limitation increases their tax bill compared to the control group. However, loss making treated firms are not immediately impacted. The negative-profit firms are affected to the extent that they may need to carry forward the disallowed portion of the interest deductions for a longer time than under the previous law, so the treatment effect will be heavily discounted for them.

Table D.5 in Appendix D shows the RDD results for a sample split based on the sign of pre-TCJA gross profits. Columns 1 to 4 show that the financing and real decisions of negative-profit firms are not affected by the treatment. Columns 5 to 8 show that the treatment effects are concentrated in positive-profit firms. These findings are consistent with the fact that firms are more responsive to immediate cash flow shocks that are created by the tax code (Zwick and Mahon, 2017; Xu and Zwick, 2022).

8.3 Falsification Tests

I conduct two sets of placebo tests to rule out the possibility that the causal relations between the limitation on the tax benefits of debt and firm policies are spurious.

Using "Low-Interest" Firms. First, I focus on a group of firms to which the treatment does not apply and show that the RDD finds no estimated treatment effect in this sample. To achieve this, I use the sample of "low-interest" firms with average annual sales in a narrow bandwidth around the $25 million cutoff. In the baseline sample of public firms, these are firms with interest expense $\leq$ (interest income + $0.3\times$EBITDA), for which the Section 163(j) limitation on interest deductions does not bind. In the sample of private firms, these are firms with $\frac{\text{Long-term debt}}{\text{EBITDA}} \leq 3$ because data on interest expenses are not available. Figure 10 shows a graphical comparison between the main sample (solid area) and the placebo sample (hashed area). Firms with average sales above the $25 million sales cutoff are the pseudo-treated group, and those below the cutoff are the pseudo-control group.

[Figure 10 around here]

Table 10 shows the RDD estimates using the placebo samples. Panels (a) and (b) use the sample of public firms. Panel (a) shows the results with a 1.25 bandwidth around the log(25) cutoff, which is close to the optimal bandwidths used in the baseline tests. Panel (b) shows the results with the optimal bandwidths. Panel (c) uses the sample of private firms with a similar bandwidth as in the main tests due to the smaller sample size. All three panels show that no treatment effect is detected in the placebo samples on financing and real policies. These results reinforce the idea that the main results are not due to a coincidental discontinuity or a discontinuity in unobservables around the cutoff.

[Table 10 around here]

Randomizing the Exception Threshold. Second, I investigate whether the main results disappear if the exception threshold is set arbitrarily to a number other than the actual $25 million cutoff. I randomly choose a number between $100 million and $500 million as the pseudo-threshold and then estimate firm responses to the pseudo-treatment using the baseline RDD specification. This exercise is repeated 5,000 times and the distribution of pseudo-treatment effects are compared to the baseline results. The lower bound for the pseudo-thresholds is chosen to stay clear of the actual treatment effects, and the upper bound is chosen so that the results remain based on small and medium firms, although the results are robust to alternative boundaries.

I conduct this test separately in the samples of public firms and private firms. Figure 11 shows the histograms of the estimates for the sample of public firms. The vertical lines represent the baseline estimates of the treatment effects on debt from Table 3 (column 2) and on hiring and investments from Table 5 (columns 2 and 4). Figure 11 shows that the distributions of the placebo treatment effects are centered around zero and the estimated actual treatment effects are far from the placebo distributions. This finding suggests that the baseline results are likely generated by a genuine discontinuity in the tax benefits of debt at the $25 million threshold. The results in the sample of private firms are qualitatively similar and, for the sake of brevity, are not shown here.

[Figure 11 around here]

8.4 Alternative Setup: Difference-in-Differences

In this study, because the assignment to treatment and control groups is not random—but rather based on the firm's average sales—the RDD is the preferred identification strategy (Roberts and Whited, 2013). However, to confirm that the main findings are not driven by the RDD mechanics, I use the sample of public firms and conduct similar tests using a DID setting. Appendix E describes the details and presents the results. The *Treated* variable identifies high-interest firms with average sales above $25 million. The *After* variable identifies data realizations from after the TCJA's effective date on January 1, 2018. Table E.6 in Appendix E shows that the treatment effects estimated by the DID tests are consistent with the baseline RDD results.

In addition, the DID setup could be used to evaluate the dynamic treatment effects on firm policies. I estimate a revised version of the DID where the treated dummy is interacted with year indicators to measure the change in the outcome variables for affected firms relative to unaffected firms in each year over the sample period. Table E.7 in Appendix E shows the estimates. The results show that the policies of treated and control firms are indistinguishable before the reform, and that treated firms reduce debt financing, hiring, and investments after the TCJA went into effect in 2018.

Finally, one may consider using the interest deduction limit introduced in the TCJA as a basis for the identification strategy. In such a setting, the treated group would consist of all high-interest firms, that is, firms for which the Section 163(j) limitation binds, and the control group would include all low-interest firms. A cross-sectional comparison of the two groups may show that high-interest firms reduce debt financing compared to the other group after the tax reform (see, e.g., Carrizosa, Gaertner, and Lynch, 2020). However, for this empirical strategy, the parallel trends assumption is likely to be violated (i.e., treated (high-interest) firms are likely not comparable to control (low-interest) firms) for at least two reasons. First, a dissimilarity in characteristics, such as the cost of equity financing or future growth opportunities, could underlie the difference in the pre-TCJA level of debt across the two groups. These differences may cause distinct responses to the treatment. Second, even if the two groups have similar characteristics, high-interest firms are closer to their debt capacity. In response to the TCJA corporate tax cut, if a typical firm raises debt to finance new investments, the response could be muted for high-interest firms because of the binding borrowing constraint rather than the reduction in the tax benefits of debt. Overall, this argument shows the limits of the parallel trends assumption with this particular identification strategy. Of course, my baseline RDD strategy does not have these problems because both treated and control groups consist of only high-interest firms.

To complete this argument, I test this strategy in the sample of public firms and show the details in Appendix F. The results in Table F.8 show that a DID regression with relevant firm controls and fixed effects does not find a negative treatment effect on leverage, hiring, and investments. Based on the discussion above, this lack of results does not rule out a bona fide relation between the tax benefits of debt and firm policies, but is likely due to the violation of the parallel trends assumption.

9 Discussion of Magnitudes

The previous sections show that the implied debt financing, hiring, and investment elasticities with respect to the tax benefits of debt are economically and statistically significant. This section compares the estimates to those from the existing literature and interprets the magnitudes.[9] To make the results comparable to the estimates in other studies, I compute the implied debt and investment elasticities with respect to the user cost of capital. Following Hassett and Hubbard (2002), the user cost of capital per unit of invested capital is

$$UCC = (\rho + \delta - g)\frac{1 - \Gamma}{1 - \tau}, \tag{3}$$

where ρ is the investors' required rate of return, δ is the depreciation rate of capital, g is the capital gains rate of return, and τ is the corporate income tax rate. Γ represents the sum of

[9] I thank Eric Zwick for suggesting this analysis.

tax incentives per unit of invested capital and is defined as

$$\Gamma = \frac{\tau \times \text{Allowed Interest}}{\bar{r}} + \tau z + ITC. \tag{4}$$

In Equation 4, the first term is the present value of interest deductions, which represents the tax benefits of debt. z is the present value of future depreciation for a unit of capital, so the second term represents the depreciation tax credits. The third term represents the investment tax credits.

The elasticity of investment with respect to the user cost of capital is $\varepsilon_{I,UCC} = \frac{\Delta I}{K} \frac{1}{\Delta UCC}$. The first term ($\frac{\Delta I}{K}$) is equal to -0.121, which is the baseline estimate in column 3 of Table 5. To estimate the second term $\frac{1}{\Delta UCC}$, I set the investors' required rate of return to $\rho = 0.1$, the depreciation rate to its long term Compustat average of $\delta = 0.08$. I assume that invested capital is fully used and depreciated, so the capital gains return is set to $g = 0$. The pre-reform tax rate is set to $\tau = 0.35$, and the post-reform rate is $\tau = 0.21$. Note that the TCJA affects the income tax rate, depreciation tax credits, and investment tax credits similarly for the treated and controls groups in my setting. Therefore, the change in the user cost of capital (ΔUCC) is solely driven by the difference in the amount of allowed interest deductions.

Using these parameters, the baseline estimate of investment elasticity with respect to the user cost of capital is $\varepsilon_{I,UCC} = -2.52$, with a confidence interval of $[-4.54, -0.50]$. My most conservative estimate is $\varepsilon_{I,UCC} = -1.52$ with a confidence interval of $[-3.30, 0.26]$. For comparison, Hassett and Hubbard (2002) report the range of $[-1.0, -0.5]$ in the literature. However, Zwick and Mahon (2017) show that these estimates are based on the largest US public firms and that small firms respond more aggressively to the tax-induced changes in the cost of capital. They estimate an investment elasticity of -3.3 for the smallest size decile of US firms, whereas the estimate for an average Compustat firm is -0.8. Similarly, Moon (2022) estimates an investment elasticity of -1.84 in a sample of small firms. Overall, my estimates are in line with the estimates in the literature that are based on samples of small firms.

Finally, I take a similar approach to calculate the elasticity of debt with respect to the user cost of capital, that is $\varepsilon_{L,UCC} = \frac{\Delta L}{L} \frac{1}{\Delta UCC}$, where L is the debt-to-capital ratio. The numerator in the first term (ΔL) is equal to -0.372, which is the baseline estimate in column 1 of Table 3. I use the parameter values from above, and find the baseline estimate of $\varepsilon_{L,UCC} = -15.59$ with a confidence interval of $[-26.62, -4.56]$. This means that for a 1% increase in the user cost of capital, the debt ratio is reduced by 15.59%. My most conservative estimate is $\varepsilon_{L,UCC} = -10.02$ with a confidence interval of $[-17.95, -2.08]$. For comparison, de Mooij (2011) performs a meta-analysis, which implies an elasticity of -8.67.[10] Therefore, my estimates are larger than those in the literature. However, this could be driven by the fact that my sample consists of small firms, which respond more strongly to changes in the tax code (Zwick and Mahon, 2017), whereas the estimates considered by de Mooij (2011) are mostly based on samples of large public firms.

10 Conclusion

Almost all countries still allow firms to fully write off interest expenses against taxable income. This is despite policy proposals to remove such subsidies that allegedly encourage too much debt financing, thereby exacerbating economic downturns. The US is now an exception, as its tax reform in 2017 limits the amount of interest deductions. Empirically, however, it is an open question whether the tax benefits of debt are a primary determinant of corporate policies. The discrepancy in the estimates is often attributed to the empirical challenges in identifying the effects.

The recent US tax reform provides a unique opportunity to directly estimate the effect by creating a discontinuity in the tax advantage of debt across firms. I exploit this feature and document a first-order effect for the tax benefits of debt on firm decisions. Corporate debt declines nearly dollar for dollar with the decline in the present value of the tax benefits of debt. Moreover, because losing the tax advantage of debt raises the cost of external financing for firms, it has a first-order negative impact on real outcomes such as hiring and investment. Overall, my results document a primary role for tax incentives in shaping corporate policies, which contributes to policy discussions and to our understanding of the main determinants of firms' financial policies.

[10] de Mooij (2011) documents the elasticity of debt ratio with respect to income tax rate, which is estimated at 0.78 (see Table 4 in their paper). I use the model in Hassett and Hubbard (2002) to convert this estimate to the elasticity with respect to the user cost of capital. I use the parameters reported above and estimate that $\Delta\tau = 1\%$ is equivalent to $\Delta UCC = -0.09\%$, which implies an estimate of $\varepsilon_{L,UCC} = -8.67$ based on the results in de Mooij (2011).

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Figure 1: Construction of the treated and control groups

This figure shows the criteria for assignment to the treated and control groups. The baseline sample includes high-interest firms with average annual sales in a narrow bandwidth around the assignment threshold of $25 million.

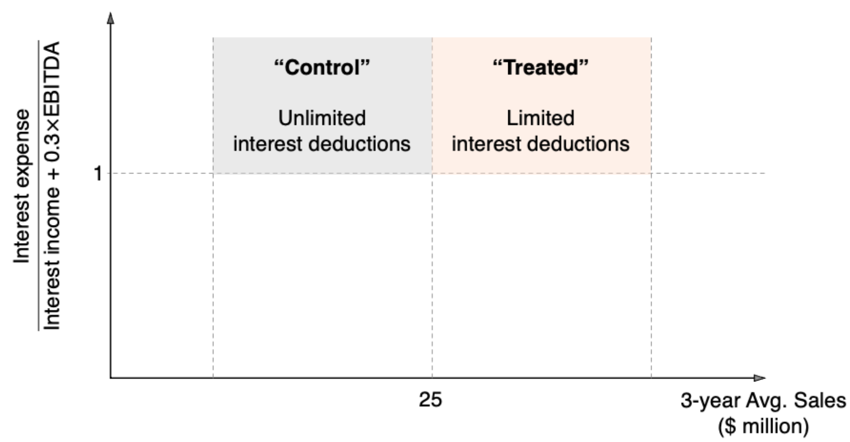


Figure 2: Sample distribution around the assignment threshold

This figure shows the distribution of firms with respect to average annual sales in the baseline sample of public firms. The goal is to evaluate irregularities around the assignment threshold of $25 million (dashed line). The histogram uses a sample of high-interest firms with average annual sales between $5m and $45m.

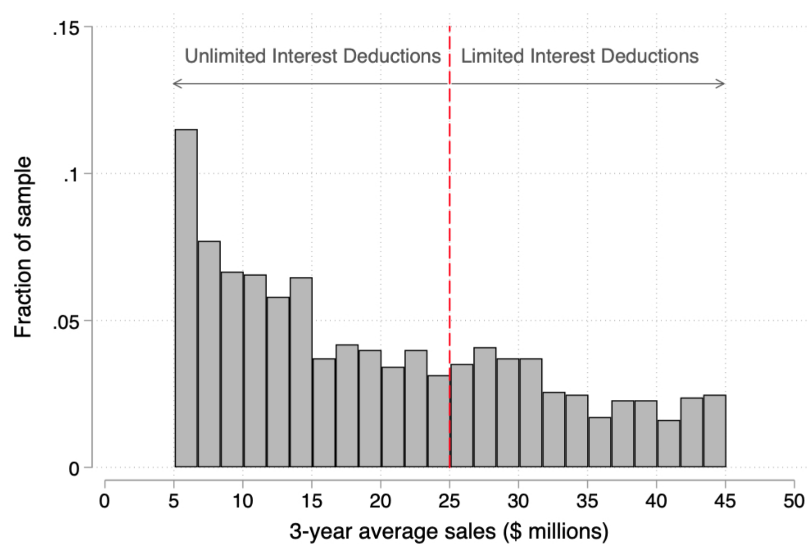


Figure 3: McCrary's test of discontinuity in the distribution of firms

This figure shows the results of the density test of McCrary (2008) in the baseline sample of public firms. The test examines the presence of a discontinuity at the threshold in the density of the forcing variable and uses the MSE optimal bandwidth of 7.991. The solid line is the fitted density with the 95% confidence interval around it. The discontinuity estimate is 0.089 and the corresponding standard error is 0.237.

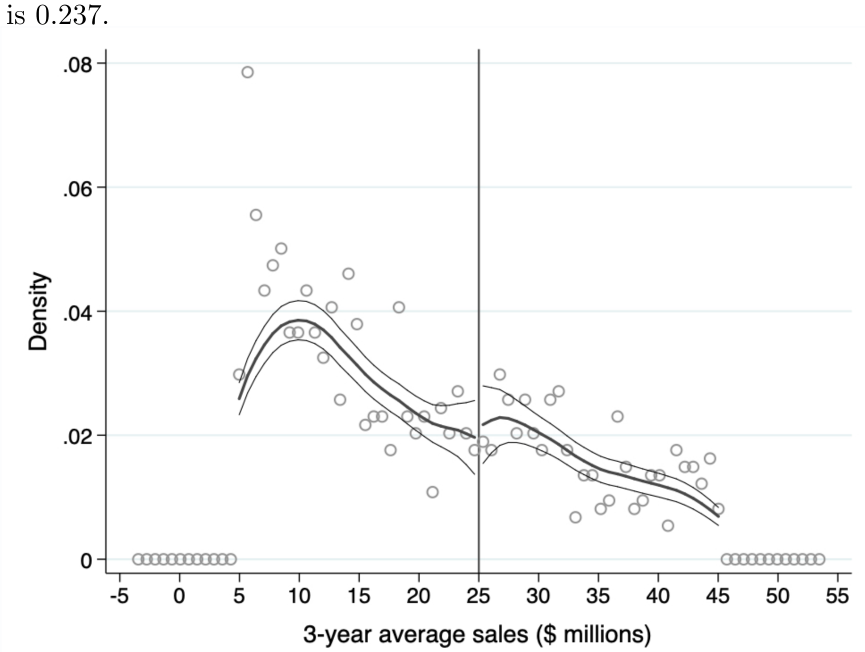
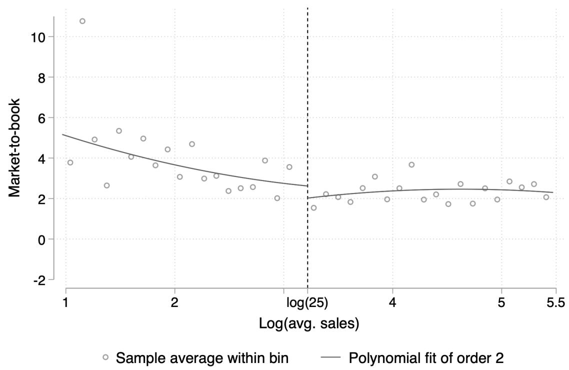


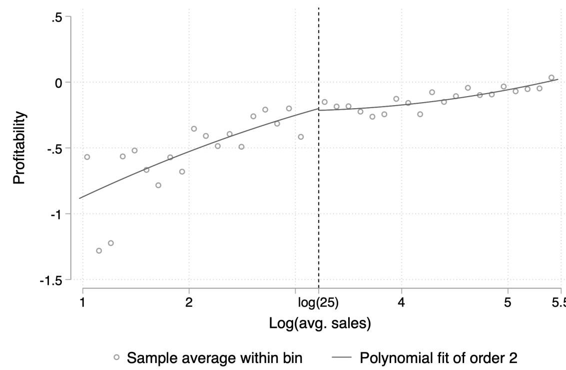
Figure 4: Evaluating discontinuity in pre-TCJA firm characteristics

These figures show regression discontinuity plots for pre-TCJA firm characteristics in the baseline sample of public firms. Observations are put into non-overlapping bins based on the log of average annual sales (horizontal axis) over the sample period. The circles show bin averages and solid lines show the polynomial fit of order 2 on each side of the log(25) cutoff. Appendix A provides variable definitions.

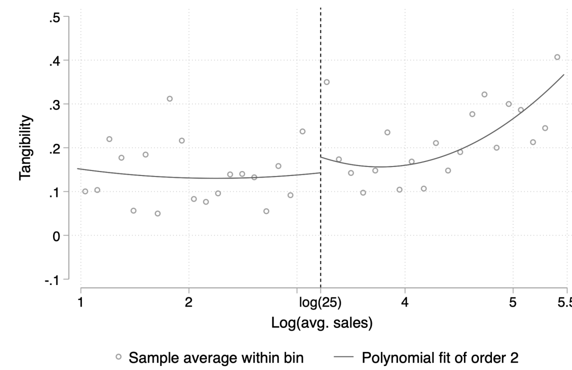
(a) Market-to-book ratio



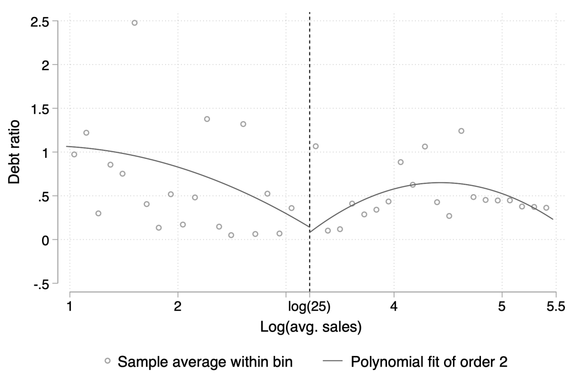
(b) Profitability ratio



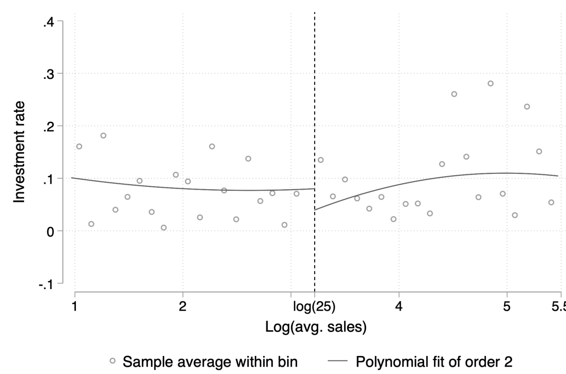
(c) Tangibility ratio



(d) Debt ratio



(e) Investment rate



(f) Hiring rate

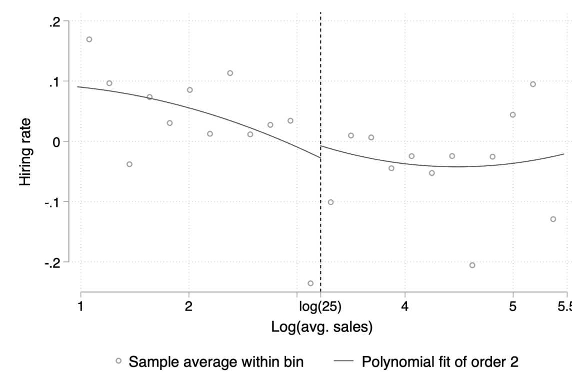
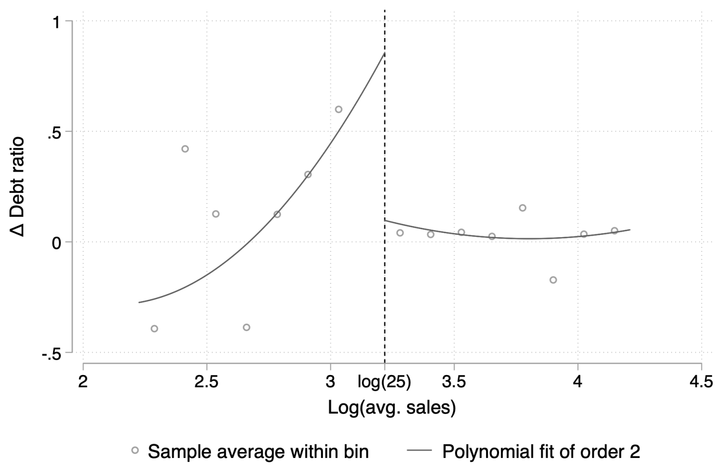


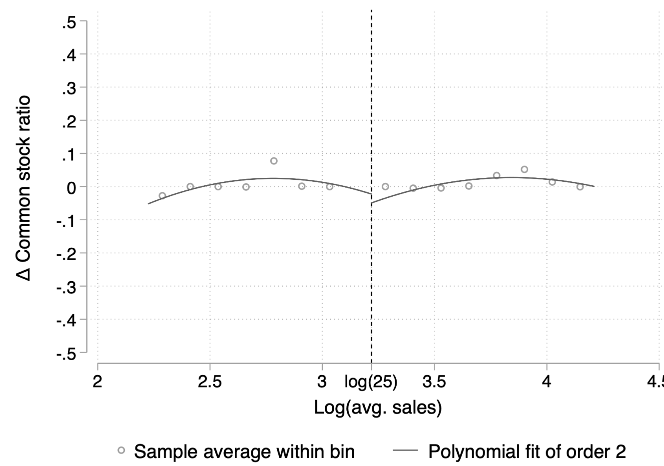
Figure 5: How does limiting interest deductions affect firm policies?

These figures show regression discontinuity plots for the leverage ratio (panel (a)), common stock ratio (panel (b)), investment rate (panel (c)), and hiring rate (panel (d)) in the baseline sample of public firms. The figures show the change in each firm policy from before to after the tax reform. Observations are put into non-overlapping bins based on the log of average annual sales (horizontal axis) over the sample period. The circles show bin averages and solid lines show the polynomial fit of order 2 on each side of the log(25) cutoff. Appendix A provides variable definitions.

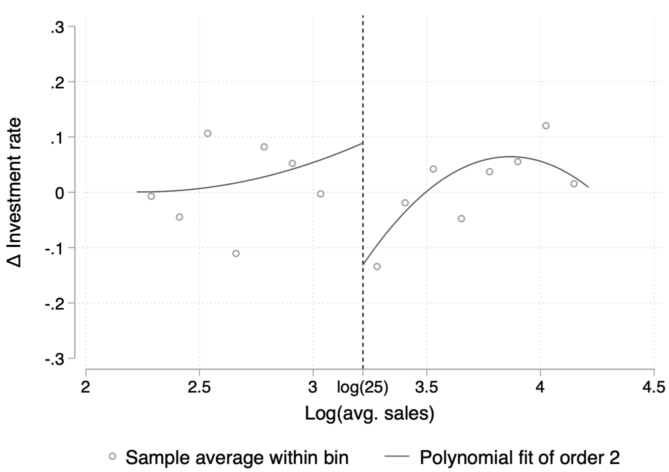
(a) The effect on the change in debt to assets ratio



(b) The effect on the change in common stock to assets ratio



(c) The effect on the change in investment rate



(d) The effect on the change in hiring rate

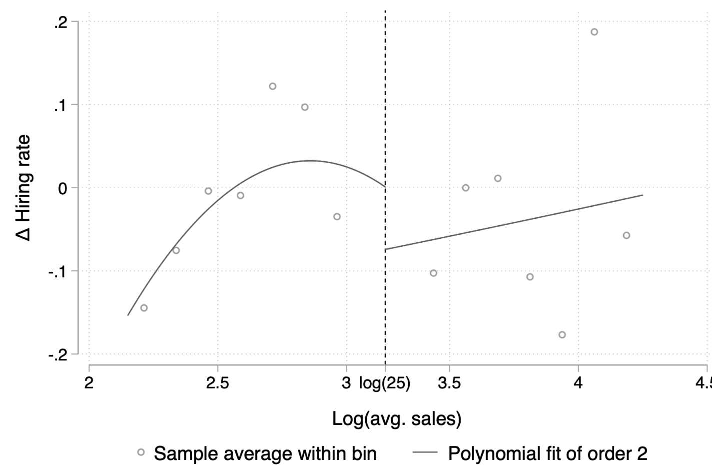


Figure 6: What fraction of firms are affected by the TCJA limit on interest deductions?

This graph shows the pre-TCJA fraction of high-interest firms across deciles of the firm size distribution using the sample of public US firms. A firm is considered high-interest if interest expense > (interest income + 0.3×EBITDA), which means that their interest deductions will be limited after the reform. For each decile, the graphs show the group mean and the 95% confidence interval.

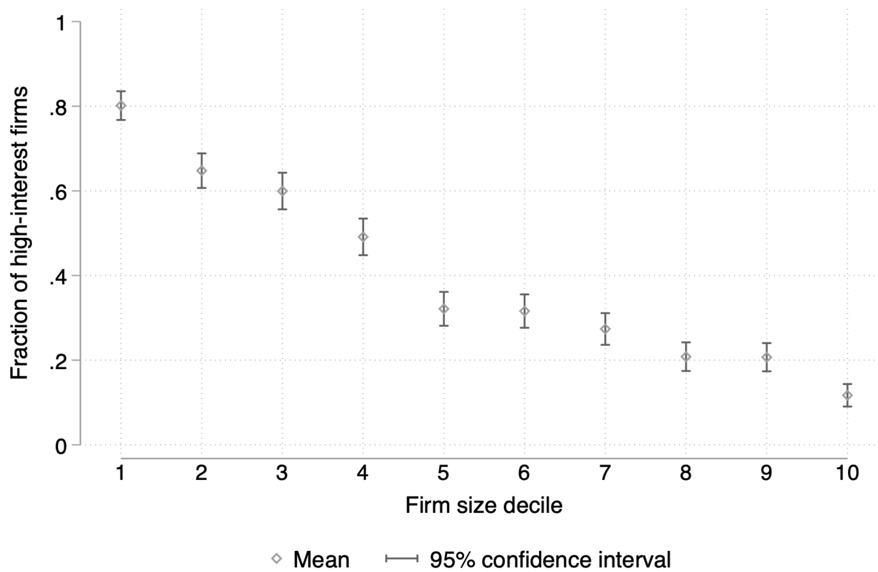
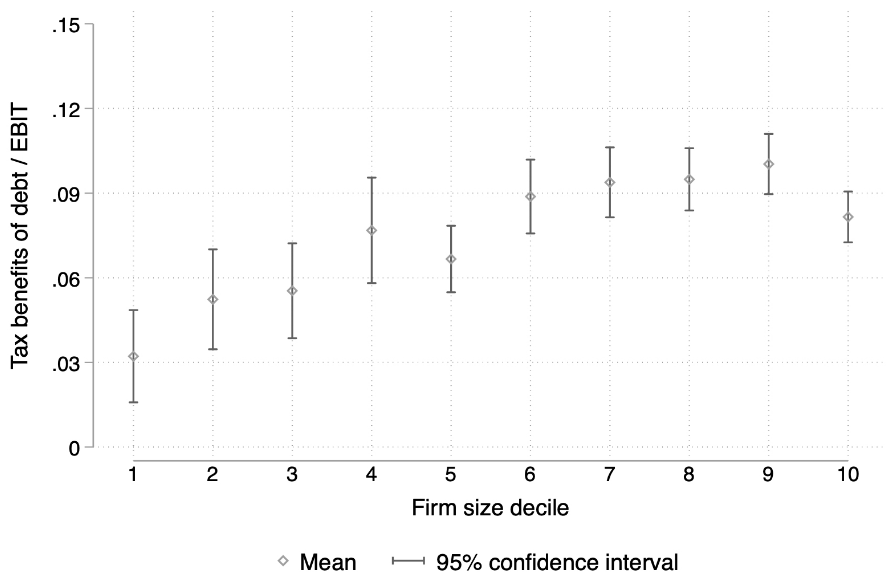


Figure 7: The pre-TCJA tax benefits of debt across size deciles of public firms

These graphs compare the relative size of the pre-TCJA tax advantage of debt for deciles of the firm size distribution using the sample of public US firms. Panel (a) plots the tax benefits of debt as a fraction of EBIT and panel (b) plots interest expenses as a fraction of EBIT. The tax benefit of debt is equal to total interest expense multiplied by the simulated firm-level marginal tax rate à la Graham (1996a,b). For each decile, the graphs show the group mean and the 95% confidence interval.

(a) Tax benefits of debt across size deciles



(b) Interest payments across size deciles

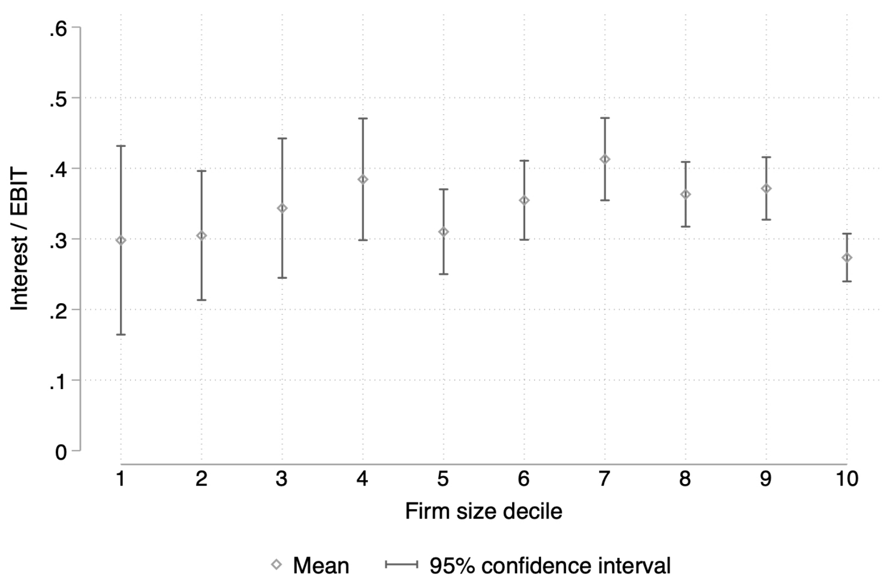


Figure 8: Private US firms: Sample distribution around the assignment threshold

This figure uses the sample of private US firms and shows the distribution of firms with respect to average annual sales. The goal is to evaluate irregularities around the assignment threshold of $25 million (dashed line). The histogram uses a sample of high-interest firms with average annual sales between $5m and $45m.

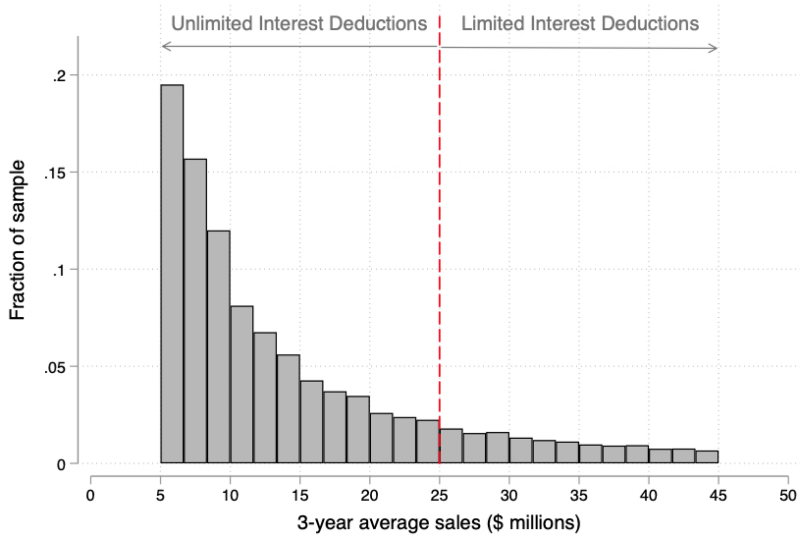


Figure 9: Private US firms: McCrary's test of discontinuity in the distribution of firms

This figure uses the sample of private firms and shows the results of the density test of McCrary (2008) that examines the presence of a discontinuity at the threshold in the density of the forcing variable. The test uses the MSE optimal bandwidth of 15.875. The solid line is the fitted density with the 95% confidence interval around it. The discontinuity estimate is 0.825 and the corresponding standard error is 0.749.

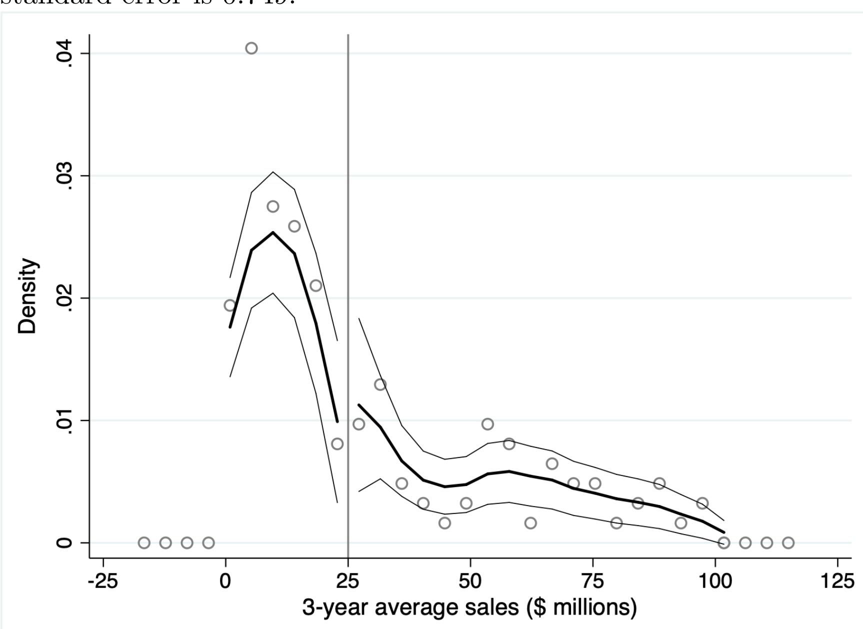


Figure 10: Placebo test #1: Using low-interest firms around the $25m threshold

This figure shows the criteria for assignment to the pseudo-treated and pseudo-control groups for the first falsification test. The placebo sample includes low-interest firms with average annual sales in a narrow bandwidth around the assignment threshold of $25 million. The low-interest firms are those with interest expense $\leq$ (interest income + 0.3$\times$EBITDA), which are not affected by the Section 163(j) limitation on interest deductions. For comparison, the figure shows both the baseline sample (solid area) and the placebo sample (hashed area). Table 10 shows the estimated treatment effect on the placebo sample.

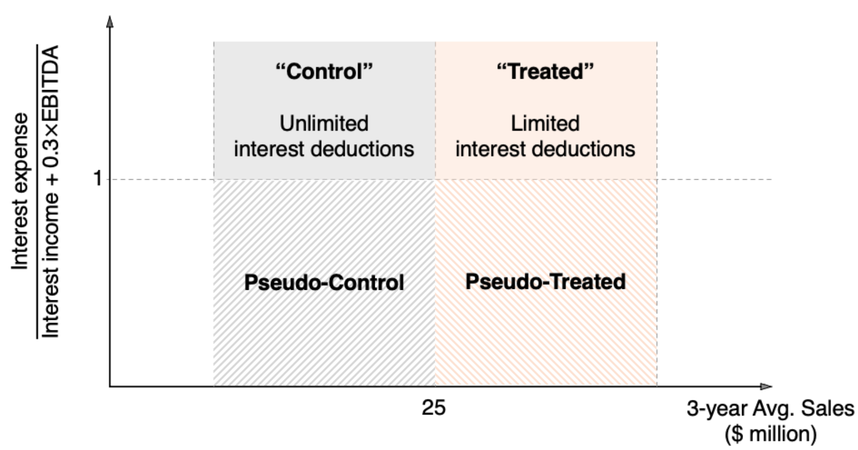
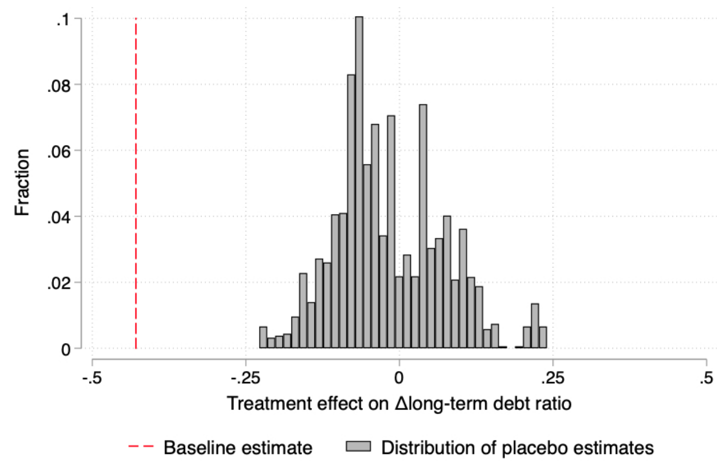


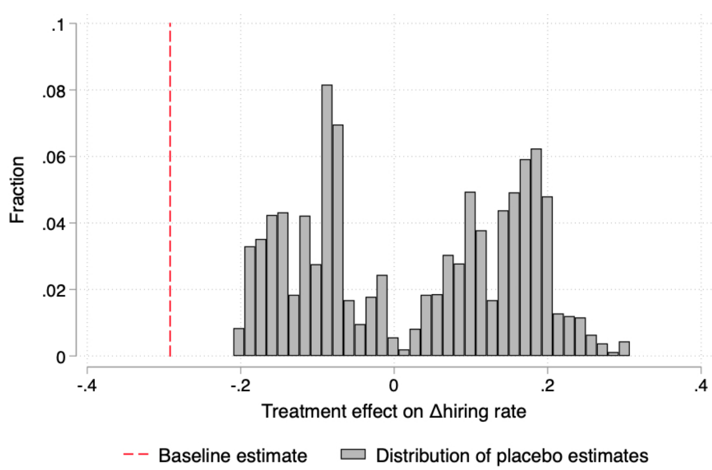
Figure 11: Placebo test #2: Randomizing the assignment threshold

These figures present the results of the second falsification test using the sample of public firms. I randomly choose a number between \$100 million and \$500 million as the pseudo-threshold and estimate firm responses to the pseudo-treatment using the baseline RDD specification. This exercise is repeated 5,000 times and the histograms of the estimated pseudo-treatment effects are reported. The vertical lines represent the estimated actual treatment effects from Tables 3 and 5.

(a) Debt ratio



(b) Hiring rate



(c) Investment rate

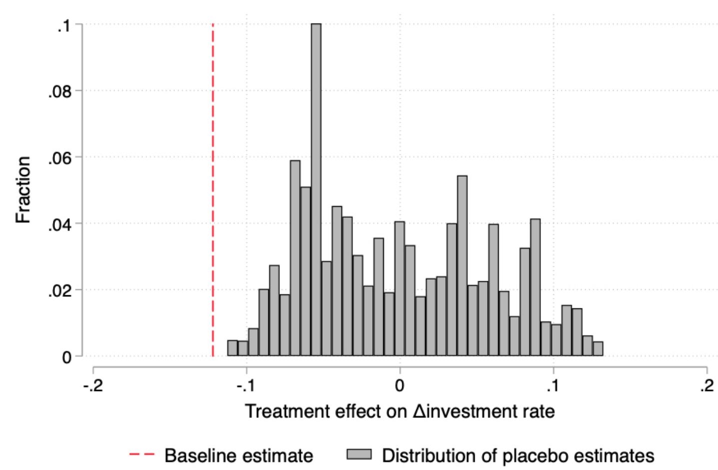


Table 1: Summary statistics

This table shows summary statistics for a sample of high-interest US public firms with three-year average annual sales between \$7.16m and \$87.27m. The lower and upper bounds are chosen to be close to the optimal bandwidths in the baseline tests. The sample covers a four-year period around the effective date of the TCJA, from 2016 to 2019. The top panel shows firm characteristics and the bottom panel shows the outcome variables that are used in the main RDD tests. The outcome variables are defined as the change in a firm policy from before to after the TCJA enactment, that is $\Delta Y_f = \bar{Y}_{f,\text{after}} - \bar{Y}_{f,\text{before}}$, where $\bar{Y}_f$ is the firm-level average of Y over the respective period. Real values are in millions of US\$(2018). Appendix A defines the variables. * $p < .1$; ** $p < .05$; *** $p < .01$.

Bandwidth around log(25)	1.250 (both sides)			1.250 (left side)			1.250 (right side)			
Range of avg. sales (\$ mil.)	[7.16, 87.27]			[7.16, 25]			[25, 87.27]			
	Obs.	Mean	SD	Obs.	Mean	SD	Obs.	Mean	SD	Difference
Firm characteristics:										
	Full sample			Control (pre-reform)			Treated (pre-reform)			
Capital (\$ mil.)	804	46.730	75.390	380	17.170	19.250	424	64.530	87.890	47.360***
3-year avg. sales (\$ mil.)	804	33.990	23.910	380	12.670	3.892	424	53.100	17.210	40.430***
Market-to-book	800	2.652	1.747	376	3.035	2.107	424	2.399	1.589	-0.636**
Profitability	804	-0.269	0.285	380	-0.249	0.355	424	-0.184	0.238	0.065
Tangibility	804	0.144	0.177	380	0.121	0.173	424	0.154	0.199	0.033
Debt ratio	804	0.444	0.684	380	0.555	1.774	424	0.497	0.945	-0.058
Common stock ratio	796	0.669	3.462	380	0.486	2.064	416	0.753	6.481	0.267
Investment rate	792	0.086	0.131	356	0.087	0.165	404	0.068	0.115	-0.019
Hiring rate	776	-0.009	0.225	368	0.025	0.294	392	-0.031	0.245	-0.056
Outcome variables:										
	Full sample			Control			Treated			
ΔDebt ratio	200	0.060	0.430	95	0.079	0.490	105	0.042	0.368	
ΔCommon stock ratio	199	0.017	0.070	95	0.029	0.089	104	0.007	0.045	
ΔInvestment rate	185	0.010	0.150	88	0.015	0.165	97	0.005	0.137	
ΔHiring rate	186	-0.018	0.332	90	-0.027	0.374	96	-0.011	0.289	
ΔSize	201	0.013	0.492	95	0.020	0.511	106	0.006	0.477	
ΔMarket-to-book	200	-0.160	1.382	94	-0.485	1.519	106	0.128	1.182	
ΔProfitability	201	0.012	0.210	95	0.013	0.243	106	0.010	0.175	
ΔTangibility	201	0.011	0.067	95	0.018	0.068	106	0.005	0.066	

Table 2: Testing discontinuities in pre-TCJA firm characteristics

$$Y_f = \alpha + \beta \text{ Treated}_f + \sum_{m=1}^{2} \gamma_m^b (s_f - \bar{s})^m + \sum_{n=1}^{2} \gamma_n^a \text{ Treated}_f \times (s_f - \bar{s})^n + \varepsilon_f$$

This table presents the test results for the null hypothesis that there is no systematic difference in the key firm characteristics before the TCJA, between firms in a narrow bandwidth on the two sides of the sales threshold ($\bar{s} = \log(25)$). I use the baseline sample of public firms and estimate the RDD regression above with the optimal bandwidths shown in the last column. Appendix A defines the variables. Standard errors are clustered at the industry level (SIC3). * $p < .1$; ** $p < .05$; *** $p < .01$.

Pre-TCJA characteristic	Coefficient (β)	Standard error	Optimal bandwidth
Size	0.182	0.257	2.932
Market-to-book	-1.351	1.078	2.0106
Profitability	-0.046	0.140	2.602
Tangibility	0.036	0.081	2.253
Debt ratio	0.180	0.194	2.338
Common stock ratio	0.282	1.163	1.635
Investment rate	0.001	0.038	2.113
Hiring rate	0.030	0.136	2.646

Table 3: How is capital structure affected by the limitation on interest deductions?

$$\Delta Y_f = \alpha + \beta \text{ Treated}_f + \sum_{m=1}^{2} \gamma_m^b (s_f - \bar{s})^m + \sum_{n=1}^{2} \gamma_n^a \text{ Treated}_f \times (s_f - \bar{s})^n + \Delta X_f + \varepsilon_f$$

This table presents the RDD estimates to evaluate the treatment effect on firms' financing decisions in the baseline sample of public firms. The estimates are from the RDD equation above using the optimal bandwidth that is shown at the bottom of the table. The effects on debt, common stock, and total external financing ratios are shown. The outcome variables are defined as the change in a firm policy from before to after the TCJA enactment, that is $\Delta Y_f = \bar{Y}_{f,\text{after}} - \bar{Y}_{f,\text{before}}$, where $\bar{Y}_f$ is the firm-level average of Y over the respective period. Appendix A defines the variables. Standard errors are clustered at the industry level (SIC3) and shown in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

	ΔDebt ratio		ΔCommon stock ratio		ΔExternal financing ratio	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.372**	-0.429**	-0.024	-0.018	-0.554	-0.645*
	(0.160)	(0.165)	(0.037)	(0.033)	(0.459)	(0.340)
ΔSize		0.111*		-0.000		0.478***
		(0.062)		(0.008)		(0.161)
ΔMarket-to-book		0.051*		0.008**		-0.016
		(0.029)		(0.004)		(0.033)
ΔProfitability		0.004		0.051**		-0.083
		(0.148)		(0.024)		(0.312)
ΔTangibility		1.215**		-0.075		0.270
		(0.474)		(0.098)		(0.887)
Const.	0.460***	0.501***	0.002	-0.008	0.229	0.407
	(0.144)	(0.168)	(0.032)	(0.033)	(0.337)	(0.291)
Polynomial terms	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	1.219					
Range of avg. sales ($ mil.)	[7.39, 84.61]					
N	195	194	194	193	193	192
adj. R^2	0.009	0.043	0.014	0.035	0.016	0.041

Table 4: Is the treatment effect stronger in firms for which the debt tax shield is more valuable?

This table uses the baseline sample of public firms and evaluates heterogeneous treatment effects in firms with a high perceived value of the debt tax shield by adding interaction terms to the baseline RDD. In columns 1 and 2, the treatment effect is interacted with a *High-profit* indicator. In columns 3 and 4, the treatment effect is interacted with a *Low-nondebt-shield* indicator. Appendix A defines the variables. Standard errors are clustered at the industry level (SIC3) and shown in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

	(1) ΔDebt ratio	(2) ΔCommon stock ratio	(3) ΔDebt ratio	(4) ΔCommon stock ratio
Treated	-0.321**	-0.029	-0.785**	-0.081
	(0.152)	(0.037)	(0.321)	(0.088)
Treated×High-profit	-0.333**	0.037**		
	(0.137)	(0.015)		
High-profit	0.316***	-0.043***		
	(0.112)	(0.016)		
Treated×Low-nondebt-shield			-0.182*	-0.021
			(0.102)	(0.024)
Low-nondebt-shield			0.145	0.022
			(0.101)	(0.026)
Polynomial terms	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Bandwidth around log(25)	1.219			
Range of avg. sales ($ mil.)	[7.39, 84.61]			
N	194	193	120	118
adj. R^2	0.053	0.039	0.101	0.030

Table 5: Real effects of the limitation on interest deductions

$$\Delta Y_f = \alpha + \beta \text{ Treated}_f + \sum_{m=1}^{2} \gamma_m^b (s_f - \bar{s})^m + \sum_{n=1}^{2} \gamma_n^a \text{ Treated}_f \times (s_f - \bar{s})^n + \Delta X_f + \varepsilon_f$$

This table presents the RDD estimates to evaluate the treatment effect on firms' real outcomes in the baseline sample of public firms. The estimates are from the RDD equation above using the optimal bandwidths that are shown at the bottom of each column. The effects on the hiring and investment rates are shown. The outcome variables are defined as the change in a firm policy from before to after the TCJA enactment, that is $\Delta Y_f = \bar{Y}_{f,\text{after}} - \bar{Y}_{f,\text{before}}$, where $\bar{Y}_f$ is the firm-level average of Y over the respective period. Appendix A defines the variables. Standard errors are clustered at the industry level (SIC3) and shown in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

	ΔHiring rate		ΔInvestment rate	
	(1)	(2)	(3)	(4)
Treated	-0.269**	-0.292***	-0.121**	-0.122**
	(0.112)	(0.092)	(0.059)	(0.060)
ΔMarket-to-book		0.013		-0.001
		(0.011)		(0.004)
ΔProfitability		0.321***		0.136***
		(0.107)		(0.035)
Const.	0.070	0.072	0.031	0.024
	(0.088)	(0.072)	(0.055)	(0.059)
Polynomial terms	Yes	Yes	Yes	Yes
Optimal bandwidth around log(25)	1.489		1.420	
Range of average sales ($ mil.)	[5.64, 110.83]		[6.04, 103.44]	
N	223	223	210	209
adj. R^2	-0.002	0.028	0.019	0.044

Table 6: Did changes in firm characteristics drive the changes in financial and real policies?

$$\Delta Y_f = \alpha + \beta \text{ Treated}_f + \sum_{m=1}^{2} \gamma_m^b (s_f - \bar{s})^m + \sum_{n=1}^{2} \gamma_n^a \text{ Treated}_f \times (s_f - \bar{s})^n + \varepsilon_f$$

This table presents the RDD estimates to evaluate the effect of the tax reform on key firm characteristics using the baseline sample of public firms. The estimates are from the RDD equation above using the optimal bandwidths that are shown for each column. The effects on market-to-book, profitability, and tangibility ratios are shown. The outcome variables are defined as the change in a firm policy from before to after the TCJA enactment, that is $\Delta Y_f = \bar{Y}_{f,\text{after}} - \bar{Y}_{f,\text{before}}$, where $\bar{Y}_f$ is the firm-level average of Y over the respective period. Appendix A defines the variables. Standard errors are clustered at the industry level (SIC3) and shown in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

	(1) ΔMarket-to-book	(2) ΔProfitability	(3) ΔTangibility
Treated	-0.569	0.164	0.023
	(0.631)	(0.125)	(0.043)
Const.	0.628	-0.068	-0.019
	(0.632)	(0.121)	(0.024)
Polynomial terms	Yes	Yes	Yes
Optimal bandwidth	1.420	1.661	1.641
Range of avg. sales ($ mil.)	[6.04, 103.44]	[4.75, 131.63]	[4.85, 129.02]
N	232	272	265
adj. R^2	0.053	-0.006	0.003

Table 7: The impact of cash constraints on the treatment effects

This table uses the baseline sample of public firms and evaluates the impact of firm-level cash constraints on the treatment effects. This is done by adding interaction terms to the baseline RDD using the optimal bandwidths that are shown for each column. Appendix A defines the variables. Standard errors are clustered at the industry level (SIC3) and shown in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

	(1) ΔDebt ratio	(2) ΔCommon stock ratio	(3) ΔHiring rate	(4) ΔInvestment rate
Treated × Constrained	-0.176**	0.031	-0.117*	-0.075***
	(0.075)	(0.026)	(0.059)	(0.028)
Treated	-0.400**	-0.026	-0.285***	-0.109*
	(0.168)	(0.033)	(0.092)	(0.061)
Constrained	0.093**	-0.030	0.052	-0.003
	(0.044)	(0.025)	(0.047)	(0.030)
Polynomial terms	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Bandwidth around log(25)	1.219	1.219	1.489	1.420
Range of avg. sales ($ mil.)	[7.39, 84.61]	[7.39, 84.61]	[5.64, 110.83]	[6.04, 103.44]
N	192	193	223	208
adj. R^2	0.046	0.042	0.025	0.057

Table 8: Summary statistics for the sample of private US firms

This table shows summary statistics for a sample of high-interest private US firms with three-year average annual sales below \$500m. The sample covers a four-year period around the effective date of the TCJA, from 2016 to 2019, and requires firms to have data available in all four years. The top panel shows firm characteristics and the bottom panel shows the outcome variables that are used in the RDD tests. The outcome variables are defined as the change in a firm policy from before to after the TCJA enactment, that is $\Delta Y_f = \bar{Y}_{f,\text{after}} - \bar{Y}_{f,\text{before}}$, where $\bar{Y}_f$ is the firm-level average of Y over the respective period. Real values are in millions of US\$(2018). * $p < .1$; ** $p < .05$; *** $p < .01$.

Range of avg. sales (\$ mil.)	[0, 500]			[0, 25]			[25, 500]			
	Obs.	Mean	SD	Obs.	Mean	SD	Obs.	Mean	SD	Difference
Firm characteristics:										
	Full sample			Control (pre-reform)			Treated (pre-reform)			
Capital (\$ mil.)	252	966.100	2755.0	64	274.700	564.1	176	1191.00	3189.0	916.300*
3-year avg. sales (\$ mil.)	252	168.600	142.400	64	9.038	5.100	176	215.000	117.600	205.962***
Profitability	252	0.028	0.248	64	0.018	0.633	176	0.046	0.246	0.028
Debt ratio	252	0.353	0.250	64	0.316	0.244	176	0.354	0.258	0.038
Common stock ratio	244	0.307	0.479	64	0.254	0.752	164	0.362	0.333	0.108
Asset growth rate	252	0.127	0.280	60	0.184	0.432	160	0.204	0.360	0.020
Hiring rate	252	0.036	0.051	64	0.033	0.065	164	0.061	0.095	0.028
Outcome variables:										
	Full sample			Control			Treated			
ΔDebt ratio	63	0.000	0.198	16	0.032	0.224	44	-0.011	0.182	
ΔCommon stock ratio	60	-0.008	0.380	16	-0.057	0.665	41	0.009	0.185	
ΔAsset growth rate	57	-0.136	0.432	15	-0.208	0.515	39	-0.125	0.401	
ΔHiring rate	56	-0.036	0.078	16	-0.015	0.074	40	-0.045	0.079	
ΔSize	63	0.223	0.772	16	0.266	1.076	44	0.218	0.644	
ΔProfitability	63	0.094	0.355	16	0.214	0.633	44	0.048	0.165	

Table 9: How does the limitation on interest deductions affect private firms?

This table presents the RDD estimates to evaluate the treatment effects on private firms. The RDD estimates are from a sample of private firms with average annual sales below \$500 million. The outcome variables are defined as the change in a firm policy from before to after the TCJA enactment, that is $\Delta Y_f = \bar{Y}_{f,\text{after}} - \bar{Y}_{f,\text{before}}$, where $\bar{Y}_f$ is the firm-level average of Y over the respective period. Standard errors are clustered at the industry level (SIC3) and shown in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

	(1) ΔDebt ratio	(2) ΔCommon stock ratio	(3) ΔHiring rate	(4) ΔAsset growth rate
Treated	-0.306**	0.228	-0.068*	-0.303
	(0.135)	(0.206)	(0.038)	(0.358)
ΔSize	0.071**	-0.001		
	(0.031)	(0.061)		
ΔProfitability	-0.129	0.155	-0.000	0.295*
	(0.082)	(0.208)	(0.018)	(0.151)
Const.	0.151*	-0.216	0.018	-0.170
	(0.085)	(0.167)	(0.022)	(0.206)
Polynomial terms	Yes	Yes	Yes	Yes
Range of avg. sales (\$ mil.)	[0, 500]			
N	60	72	73	67
adj. R^2	0.086	0.010	0.165	0.025

Table 10: Placebo test #1: Using low-interest firms around the $25 million threshold

This table estimates the treatment effects on two samples of firms that are actually not affected by the treatment to test whether there is a coincidental discontinuity or discontinuity in unobservables around the cutoff. The samples include low-interest firms with average annual sales in a narrow bandwidth around the threshold, as shown in Figure 10. Panel (a) uses the sample of public firms with a fixed 1.25 bandwidth. Panel (b) uses the sample of public firms with the optimal bandwidths. Panel (c) use the sample of private firms with average annual sales below $500 million. Columns in each panel show the effects on the debt ratio, common stock ratio, hiring rate, and investment rate. Appendix A defines the variables. Standard errors are clustered at the industry level (SIC3) and shown in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

(a) Public firms: Second order polynomial with a fixed 1.25 bandwidth

	(1) ΔDebt ratio	(2) ΔCommon stock ratio	(3) ΔHiring rate	(4) ΔInvestment rate
Pseudo-Treated	-0.061 (0.042)	-0.001 (0.004)	-0.215 (0.144)	-0.031 (0.079)
Polynomial terms	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Bandwidth around log(25)	1.250			
Range of avg. sales ($ mil.)	[7.16, 87.27]			
N	118	116	106	101
adj. R^2	0.269	0.067	0.002	-0.035

(b) Public firms: Second order polynomial with optimal bandwidths

	(1) ΔDebt ratio	(2) ΔCommon stock ratio	(3) ΔHiring rate	(4) ΔInvestment rate
Pseudo-Treated	-0.087 (0.061)	-0.006 (0.004)	-0.152 (0.259)	-0.224 (0.150)
Polynomial terms	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Optimal bandwidth	0.533	0.615	0.669	0.683
Range of avg. sales ($ mil.)	[14.6, 42.6]	[13.5, 46.2]	[12.8, 48.8]	[12.6, 49.5]
N	53	58	59	56
adj. R^2	0.126	0.025	0.010	0.022

Table 10: (continued)

(c) Private firms: Second order polynomial with a fixed bandwidth

	(1) ΔDebt ratio	(2) ΔCommon stock ratio	(3) ΔHiring rate	(4) ΔAsset growth rate
Treated	-0.116	-0.234	0.017	-0.034
	(0.212)	(0.140)	(0.045)	(0.092)
Polynomial terms	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Range of avg. sales ($ mil.)	[0, 500m]			
N	19	18	21	20
adj. R^2	0.044	-0.190	0.062	-0.031

A Variable Definitions

Table A.1: Variable definitions

This table shows the definitions of the variables and how each variable is constructed using Compustat items. For the ratio variables, if the numerator is a flow variable, such as capital expenditures, the denominator is lagged. If the numerator is a stock variable, such as debt, the denominator is contemporaneous. All results are robust to using all lagged or all contemporaneous denominators.

Variable	**Definition**	**Compustat items**
Capital	Nonfinancial assets=total assets − cash and short-term investments	$at - che$
Sales	Total receipts for sales completed during a year	$sale$
Size	Log of total assets	$\log(at)$
Market-to-book	(Market value of equity + total liabilities)/total assets	$\big[(prcc_f \times csho) + (at - ceq)\big]/at$
Profitability	Operating income before depreciation/total assets	$oibdp/at$
Tangibility	Property, plant, and equipment/total assets	$ppent/at$
Debt ratio	Total value of debt obligations due in more than one year/capital	$dltt/(at - che)$
Common stock ratio	Common stock/capital	$cstk/(at - che)$
External financing ratio	(Total value of long term debt obligations + common stock)/capital	$(dltt + cstk)/(at - che)$
Investment rate	(Capital expenditures + acquisitions − sales of capital)/lagged capital	$(capx_t + aqc_t - sppe_t)/(at - che)_{t-1}$
Hiring rate	First difference in the log of number of employees	$\log(emp_t) - \log(emp_{t-1})$
High-profit	An indicator variable that equals 1 for the top tercile of firms in a sort on pre-TCJA gross profits.	
Low-nondebt-shield	An indicator variable that equals 1 for the bottom tercile of firms in a sort on pre-TCJA non-debt tax shield.	
Non-debt tax shield	(depreciation and amortization + investment tax credit)/gross profits	$(dp + itci)/(gp * (gp > 0))$
Cash constrained	An indicator variable that equals 1 for the bottom tercile of firms in a sort on pre-TCJA retained earnings to capital ratio.	
Outcome variables:		
$\Delta \mathrm{Y}_f$	$\dfrac{\mathrm{Y}_{f,2018} + \mathrm{Y}_{f,2019}}{2} - \dfrac{\mathrm{Y}_{f,2016} + \mathrm{Y}_{f,2017}}{2} = \bar{\mathrm{Y}}_{f,\mathrm{after}} - \bar{\mathrm{Y}}_{f,\mathrm{before}}$	

B Treatment Effects on Total Debt Ratio

Table B.2: Treatment effects on total debt ratio

This table uses the baseline sample of public firms to evaluate the treatment effects on total debt ratio. Columns 1 to 3 show the RDD estimates with polynomials of order 2 with optimal, 1.0, and 1.5 bandwidths, respectively. Columns 4 and 5 show the results with polynomials of order 1 and 3, respectively, with optimal bandwidths. Appendix A defines the variables. Standard errors are clustered at the industry level (SIC3) and shown in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

	ΔTotal debt ratio				
	(1)	(2)	(3)	(4)	(5)
Treated	-0.661**	-0.655**	-0.447**	-0.333*	-1.110**
	(0.286)	(0.304)	(0.211)	(0.173)	(0.431)
ΔSize	0.033	0.079	0.045	0.137*	0.037
	(0.066)	(0.052)	(0.066)	(0.072)	(0.055)
ΔMarket-to-book	0.102***	0.116***	0.104***	0.118***	0.095***
	(0.025)	(0.026)	(0.028)	(0.030)	(0.018)
ΔProfitability	0.039	-0.017	-0.069	0.029	-0.109
	(0.102)	(0.169)	(0.083)	(0.312)	(0.125)
ΔTangibility	1.085	1.402**	1.121	1.450**	1.238**
	(0.752)	(0.541)	(0.675)	(0.565)	(0.533)
Const.	0.761***	0.802***	0.576***	0.388***	1.219***
	(0.236)	(0.292)	(0.178)	(0.123)	(0.370)
Polynomial terms	Yes	Yes	Yes	Yes	Yes
Polynomial order	2	2	2	1	3
Bandwidth around log(25)	1.310	1.000	1.500	0.914	1.699
Range of avg. sales ($ mil.)	[6.7, 92.6]	[8.4, 62.1]	[5.1, 102.4]	[10.0, 62.3]	[4.5, 136.7]
N	211	155	239	133	276
adj. R^2	0.071	0.083	0.067	0.093	0.082

C Robustness of the Regression Discontinuity Design

Table C.3: RDD robustness: Second order polynomials with alternative bandwidths

This table shows the RDD estimates of the treatment effects with alternative bandwidths. Panels (a) and (b) use fixed bandwidths of 1 and 1.5 around the log(25) cutoff. Columns 1 to 4 in each panel show the effects on the debt ratio, common stock ratio, hiring rate, and investment rate, respectively. Appendix A defines the variables. Standard errors are clustered at the industry level (SIC3) and shown in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

(a) Using a fixed bandwidth of 1

	(1) ΔDebt ratio	(2) ΔCommon stock ratio	(3) ΔHiring rate	(4) ΔInvestment rate
Treated	-0.299*	0.034	-0.310	-0.151
	(0.172)	(0.039)	(0.213)	(0.094)
Polynomial terms	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Bandwidth around log(25)	1.000			
Range of avg. sales ($ mil.)	[8.41, 62.12]			
N	156	156	145	146
adj. R^2	-0.018	0.063	-0.006	-0.005

(b) Using a fixed bandwidth of 1.5

	(1) ΔDebt ratio	(2) ΔCommon stock ratio	(3) ΔHiring rate	(4) ΔInvestment rate
Treated	-0.239**	-0.045	-0.259***	-0.104*
	(0.115)	(0.027)	(0.090)	(0.061)
Polynomial terms	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Bandwidth around log(25)	1.500			
Range of avg. sales ($ mil.)	[5.10, 102.41]			
N	241	238	226	217
adj. R^2	-0.012	0.005	0.022	0.038

Table C.4: RDD robustness: Alternative order of polynomials

This table shows the RDD estimates of the treatment effects with alternative polynomial orders. Panels (a) and (b) use linear and third-order polynomials, respectively. Columns 1 to 4 in each panel show the effects on the debt ratio, common stock ratio, hiring rate, and investment rate, respectively. Appendix A defines the variables. Standard errors are clustered at the industry level (SIC3) and shown in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

(a) First-order polynomials (linear)

	(1) ΔDebt ratio	(2) ΔCommon stock ratio	(3) ΔHiring rate	(4) ΔInvestment rate
Treated	-0.340*	-0.050*	-0.184*	-0.073
	(0.185)	(0.026)	(0.092)	(0.052)
Polynomial terms	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Optimal bandwidth	0.778	1.041	0.872	0.890
Range of avg. sales ($ mil.)	[10.49, 49.75]	[8.83, 70.81]	[10.45, 59.80]	[9.38, 55.64]
N	106	165	118	121
adj. R^2	0.023	0.032	-0.006	-0.010

(b) Third-order polynomials

	(1) ΔDebt ratio	(2) ΔCommon stock ratio	(3) ΔHiring rate	(4) ΔInvestment rate
Treated	-0.330*	-0.004	-0.252	-0.202**
	(0.180)	(0.025)	(0.166)	(0.081)
Polynomial terms	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Optimal bandwidth	1.769	1.989	1.876	1.663
Range of avg. sales ($ mil.)	[4.26, 146.64]	[3.42, 182.73]	[3.50, 149.16]	[4.33, 120.54]
N	297	336	303	247
adj. R^2	-0.013	0.019	0.031	0.030

D Profitable versus Unprofitable Firms

Table D.5: Are the treatment effects stronger for profitable firms?

This table uses the baseline sample of public firms and evaluates heterogeneous treatment effects based on firms' profitability. Columns 1–4 show the results for firms with negative pre-TCJA gross profits and columns 5–8 show them for firms with positive profits. Appendix A defines the variables. Standard errors are clustered at the SIC3 level and shown in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

	Pre-reform profit ≤ 0				Pre-reform profit > 0			
	ΔDebt ratio	ΔCommon stock ratio	ΔHiring rate	ΔInvestment rate	ΔDebt ratio	ΔCommon stock ratio	ΔHiring rate	ΔInvestment rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	-0.003	-0.241	-0.145	0.188	-0.457**	-0.003	-0.217*	-0.151**
	(0.089)	(0.172)	(0.418)	(0.288)	(0.222)	(0.031)	(0.120)	(0.064)
ΔMarket-to-book	0.176***	0.013***	0.016	-0.002	-0.003	0.007	0.005	-0.004
	(0.020)	(0.003)	(0.026)	(0.006)	(0.021)	(0.005)	(0.016)	(0.005)
ΔProfitability	0.628***	0.058***	0.438*	0.317**	-0.057	0.049	0.286**	0.067*
	(0.105)	(0.016)	(0.200)	(0.120)	(0.185)	(0.032)	(0.134)	(0.039)
ΔSize	0.115**	-0.045***			0.023	0.016		
	(0.034)	(0.009)			(0.043)	(0.016)		
ΔTangibility	1.263***	-0.138			0.886*	-0.008		
	(0.303)	(0.134)			(0.521)	(0.102)		
Polynomial terms	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Optimal bandwidth	1.219	1.219	1.489	1.420	1.219	1.219	1.489	1.420
N	42	42	55	46	152	151	168	163
adj. R^2	0.007	0.125	-0.046	0.114	0.030	0.069	0.019	0.036

E Difference-in-Differences Method

To confirm that the main findings are not driven by the RDD mechanics, I conduct the baseline tests in the following difference-in-differences (DID) specification:

$$Y_{ft} = \alpha_f + \alpha_t + \beta_1 \text{Treated}_{ft} \times \text{After}_{ft} + \beta_2 X_{ft} + \varepsilon_{ft}, \tag{5}$$

where subscripts f and t index firm and year, respectively. The intercepts α_f and α_t are firm and year fixed effects. The 'Treated' dummy takes the value of 1 for high-interest firms with average annual sales above \$25 million and 0 otherwise, similar to Figure 1. I use the baseline cleaned sample of public firms from 2016 to 2019. The 'After' dummy is equal to 1 for the last two years and 0 otherwise. Finally, X_{ft} is a vector of firm-level controls. This vector includes market-to-book, profitability, size, tangibility, and industry median of the dependent variable when Y_{ft} is related to debt and equity decisions, and only the first two variables when Y_{ft} is related to hiring and investments. The final sample includes all firm-year observations for firms with average annual sales between \$10 million and \$110 million. This range is an arbitrary choice to keep the treated and control groups similar in size and make the sample comparable to those in the baseline tests in Tables 3 and 5. Nonetheless, alternative lower and upper bounds lead to similar qualitative results.

Table E.6 presents the results. Columns 1 and 2 show the effect on corporate debt. The dependent variables are the debt to capital ratio and the one-year change in debt scaled by capital, respectively. The estimated negative coefficients on the interaction term confirms the negative treatment effect on debt financing. Columns 3 and 4 estimate the treatment effect on the common stock to capital ratio and the one-year change in common stock scaled by capital, respectively. The non-positive coefficients on the interaction term show that the treated firms do not increase equity to replace the reduction in debt. Finally, columns 5 and 6 estimate the impact on the real outcomes. These results confirm the negative treatment effect on hiring and investments, albeit the former estimate is not statistically significant in this setting. Overall, the treatment effects estimated by the DID tests are consistent with the baseline RDD results, confirming that the main findings do not depend on the choice of the methodology.

In addition, I evaluate the dynamic treatment effects on firm policies by estimating a revised version of the DID, where the treated dummy is interacted with year indicators. The coefficients on the interaction terms measure the change in the outcome variable for affected firms relative to unaffected firms in each year over the sample period. Table E.7 shows the results. Columns 1 and 2 show that the debt policy of treated and control firms are not different prior to the reform (2016 and 2017), but treated firms reduce debt financing after the reform (2018 and 2019). Columns 3 and 4 confirm that treated firms do not raise equity after the reform. Finally, in columns 5 and 6, the negative coefficients on the post-reform interaction terms show that hiring and investments of treated firms declines after the reform. Although the latter two coefficient are not estimated precisely in this test.

Table E.6: Estimated treatment effects using the difference-in-differences

This table presents the estimated treatment effects from the difference-in-differences test in Equation 5. Standard errors are clustered at the firm level and shown in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

	(1) Debt ratio	(2) ΔDebt ratio	(3) Common stock ratio	(4) ΔCommon stock ratio	(5) Hiring rate	(6) Investment rate
Treated×After	-0.156**	-0.176**	-0.078**	-0.004	-0.064	-0.053*
	(0.071)	(0.068)	(0.035)	(0.003)	(0.044)	(0.029)
Size_{t-1}	0.065	-0.073	-0.018	-0.002		
	(0.066)	(0.056)	(0.045)	(0.003)		
$\text{Market-to-book}_{t-1}$	-0.034	0.012	0.018	0.000	0.019**	0.019***
	(0.021)	(0.027)	(0.012)	(0.001)	(0.008)	(0.005)
$\text{Profitability}_{t-1}$	-0.087	-0.011	-0.058	0.000	0.149**	0.034
	(0.130)	(0.157)	(0.082)	(0.005)	(0.073)	(0.050)
Tangibility_{t-1}	0.246	-0.376	-0.160	-0.003		
	(0.402)	(0.268)	(0.139)	(0.011)		
Industry median	-0.054	1.958***	1.626*	-12.019		
of dependent var.	(0.320)	(0.660)	(0.890)	(18.388)		
Firm and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	988	986	989	989	927	876
adj. R^2	0.769	0.205	0.862	0.568	0.180	0.144

Table E.7: Estimated treatment effects by year

This table uses a difference-in-differences specification to show the treatment effects by year. Standard errors are clustered at the firm level and shown in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

	(1) Debt ratio	(2) ΔDebt ratio	(3) Common stock ratio	(4) ΔCommon stock ratio	(5) Hiring rate	(6) Investment rate
2016 * Treated	-0.087	-0.060	0.118**	0.005	-0.049	-0.024
	(0.082)	(0.085)	(0.052)	(0.004)	(0.060)	(0.046)
2017 * Treated	-0.023	-0.082	0.029	0.002	0.012	0.006
	(0.070)	(0.080)	(0.028)	(0.004)	(0.059)	(0.047)
2018 * Treated	-0.175**	-0.250***	-0.030	0.001	-0.053	-0.062
	(0.078)	(0.090)	(0.027)	(0.003)	(0.057)	(0.041)
2019 * Treated	-0.189*	-0.161*	-0.068*	-0.007	-0.094	-0.047
	(0.102)	(0.088)	(0.039)	(0.005)	(0.062)	(0.052)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	988	986	989	989	927	876
adj. R^2	0.768	0.204	0.863	0.569	0.178	0.140

F Identification with the Interest Deduction Limit

I consider using the TCJA's limitation on interest deductions as a basis for the identification strategy in a sample of *all* public firms with average sales above $25 million. In this setting, the treated group consists of all 'high-interest' firms—that is firms with interest expenses greater than the total of interest income and 30% of EBITDA. The control group includes the 'low-interest' firms. I test this strategy using a DID specification similar to Equation 5 (in Appendix E).

Table F.8 presents the results. The estimations do not show negative treatment effects on debt financing (columns 1 and 2) and real outcomes (columns 5 and 6). The effect on equity financing (columns 3 and 4) is virtually zero. Of course, these results highlight the problems with the identification strategy that are discussed in the text (see Section 8.4), rather than proving that firm policies are not affected by the tax benefits of debt.

Table F.8: Alternative treatment definition using the interest deduction limit

This table presents the estimated effects of an alternative treatment that is defined based on the interest deduction limit. Standard errors are clustered at the firm level and shown in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

	(1) Debt ratio	(2) ΔDebt ratio	(3) Common stock ratio	(4) ΔCommon stock ratio	(5) Hiring rate	(6) Investment rate
Treated×After	0.043***	0.008	-0.001	-0.000	-0.011	0.010
	(0.010)	(0.012)	(0.002)	(0.000)	(0.009)	(0.008)
Size_{t-1}	0.032**	-0.196***	-0.005**	-0.001***		
	(0.012)	(0.017)	(0.003)	(0.000)		
$\text{Market-to-book}_{t-1}$	0.008	0.033***	0.002	0.000	0.035***	0.034***
	(0.006)	(0.007)	(0.002)	(0.000)	(0.004)	(0.004)
$\text{Profitability}_{t-1}$	-0.125**	0.199***	-0.002	-0.000	0.225***	0.146***
	(0.052)	(0.062)	(0.010)	(0.001)	(0.040)	(0.033)
Tangibility_{t-1}	0.140**	0.149	0.002	-0.000		
	(0.059)	(0.093)	(0.010)	(0.001)		
Industry median of dependent var.	0.416***	2.071***	0.017	13.151***		
	(0.043)	(0.149)	(0.271)	(4.492)		
Firm and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	8038	8024	7817	7810	7767	5534
adj. R^2	0.795	0.191	0.939	0.525	0.223	0.249