

Tax Policy, Investment, and Firm Financing: Evidence from the U.S. Interest Limitation*

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Abstract

This paper studies the effects of the Tax Cuts and Jobs Act interest limitation on firms' investment and financing choices. Using U.S. tax data and three research designs, we find economically small and statistically insignificant effects on investment, leverage, debt issuance, and equity issuance. Our results are consistent with high-interest firms facing steeply upward sloping marginal cost of debt curves, and not using debt to finance marginal investment projects, such that limits on interest deductions are unlikely to substantially reduce investment or borrowing.

*This research was conducted while the authors were employees at the IRS or the U.S. Department of the Treasury. This work expresses the views of the authors themselves and does not necessarily reflect the views of the IRS or the U.S. Department of the Treasury. Any taxpayer data used in this research was kept in a secured IRS data repository, and all results have been reviewed to ensure that no confidential information is disclosed. We thank seminar and conference participants at the Office of Tax Analysis, Princeton, Maryland, MIT, Hoover, the 2023 NTA Meetings and the 2025 NBER Public Meetings for useful feedback. We also thank Eduard Boehm, Gabriel Chodorow-Reich, Mara Faccio, Michael Faulkender, Daniel Garrett, John Graham, Nathan Hendren, Kilian Huber, Henrik Kleven, Josh Rauh, Matthew Smith, Toni Whited, and Motohiro Yogo for helpful comments and suggestions. Richmond thanks his dissertation committee Ilyana Kuziemko, Richard Rogerson, and Owen Zidar for guidance and support. We declare that we have no relevant or material financial interests that relate to the research described in this paper. All errors are our own. Corresponding author: Jordan Richmond, Robert H. Smith School of Business, University of Maryland, jordanwr@umd.edu

1 Introduction

Most corporate tax codes around the world allow firms to deduct interest expense on their debt, providing a tax advantage for debt relative to equity financing. Proponents of interest deductions argue they lower the cost of capital and stimulate investment, while opponents counter that interest deductions narrow the tax base while encouraging high levels of borrowing that increase macroeconomic risk.¹ How much do interest deductions actually affect firms' investment and borrowing? Despite its importance, the question has proven difficult to answer because variation in firms' ability to deduct interest is rare.

In this paper, we study the economic impacts of limiting interest deductions using natural experiments and data from U.S. tax returns. The 2017 law known as the Tax Cuts and Jobs Act (TCJA) introduced an interest limitation for U.S. firms, capping interest deductions at 30% of earnings plus interest income and broadly limiting interest deductions for the first time in modern U.S. history.² However, the limitation only applies to firms with more than \$25 million in average receipts over the three previous years. Exploiting this variation, we use complementary regression discontinuity, event study, and triple difference research designs to evaluate the impacts of the limitation. Across these designs, we find that the limitation has small effects on investment, leverage, debt issuance, and equity issuance, well below what leading estimates in the literature would predict, even though it lowers the marginal tax benefit of debt by 45%.

Our first research design is a regression discontinuity (RD) that compares the investment and financing behavior of *ex-ante* high-interest firms that fall narrowly on each side of the \$25 million lagged receipts cutoff, classifying firms as high interest if from 2015-2017 their average interest exceeds their limitation. This design relies on an assumption that firms do

¹Concerns among policymakers and academics about debt overhang have risen in recent years as U.S. nonfinancial corporate debt reached an all time high as a percentage of GDP (Kaplan, 2019; Powell, 2019). Firms entering the 2008 financial crisis with higher leverage laid off more workers and reduced their investment by more after the crisis (Giroud and Mueller, 2017; Kalemli-Özcan et al., 2022).

²Prior to TCJA, the U.S. only limited interest deductions from intra-group lending to curb profit shifting. Less recently, interest deductions were broadly capped before the World War I excess profits tax. The U.S. began allowing unlimited corporate interest deductions as a temporary measure to mitigate the effects of the excess profits tax in 1918, and when the tax was repealed in 1921, Congress kept unlimited interest deductions as part of the corporate income tax without any explanation (Warren, 1974; Bank, 2014).

not manipulate their pre-TCJA receipts to fall below the \$25 million cutoff. The limitation was announced in November 2017, enacted in December 2017, and took effect in 2018, with no evidence of bunching at the cutoff. Using our RD to study effects through 2019, we find the limitation reduces interest deductions but does not have a statistically significant impact on investment, leverage, debt issuance, or equity issuance.

While the RD design delivers a clean comparison between similarly sized firms that do and do not lose interest deductions, it measures local effects of the limitation relying on a small number of firms. To assess the limitation’s effects on a larger set of firms that are more informative about aggregate impacts, we turn to our second design, an event study. Unrestricted by the narrow RD bandwidth, we compare outcomes for all *ex ante* big and high-interest firms that face the interest limitation to all small, high-interest firms that do not from 2013-2019. We find little evidence of differential pre-trends between the bigger and smaller comparison groups, and the larger sample yields additional precision relative to the RD, allowing us to rule out statistically or economically significant impacts of the limitation on investment, leverage, debt issuance, and equity issuance.

One concern with our event study design is that other tax changes included in the TCJA or large shocks like COVID-19 could differentially impact big and small firms, biasing our estimates. We address this concern with a triple difference design that compares big and small high-interest firms, netting out any differential trends between big and small low-interest firms that face the same TCJA changes and other possible contemporaneous size-varying shocks, but not the limitation. Using data through 2023, our triple difference estimates continue to rule out statistically and economically significant impacts of the limitation on investment, leverage, debt issuance, and equity issuance, aligning with our results from the shorter horizon RD and event study designs.

Our findings rejecting substantial responses to the interest limitation are not only consistent across research designs, but each individual design also exhibits a high degree of internal validity. We continue to find similar results when using different specifications (varying polynomials, bandwidths, controls, and fixed effects), sample frames (unbalanced, balanced, dropping largest firms), and outcome variables (stocks and flows, alternative scalings).

Our null borrowing estimates reject the joint hypothesis that the limitation substantially

reduces the marginal tax benefit of debt, and that borrowing is meaningfully responsive to marginal benefit changes. To distinguish between these two possibilities, we first confirm that the limitation lowers firms' marginal tax benefit of debt. Using pre-TCJA income and deduction dynamics to simulate interest, income, and the limitation forward, we calculate firm-specific marginal tax benefit of debt curves to measure how much each firm's expected tax bill falls if it deducts an additional dollar of interest (Graham, 1996, 2000). For a profitable, low-interest firm, the marginal tax benefit equals the statutory tax rate, but marginal benefits decline as interest rises because tax losses and binding limitations become more likely. Averaging firm-specific marginal tax benefit of debt curves with and without the limitation over big, high-interest firms shows that the limitation reduces the marginal tax benefit of debt by 45%. A tax benefit cut of this size, combined with our event study or triple difference estimates, implies a range of leverage tax rate elasticities from $[-0.05, 0.03]$, ruling out anything but a very inelastic response.

How can the marginal tax benefit of debt decline substantially, but result in at most small declines in borrowing? Consistent with trade-off theory, reductions in the marginal benefit of debt can lead to small declines in borrowing if firms facing the limitation are on steep regions of their marginal cost of debt curves.³ Indeed, affected firms likely face steep marginal cost curves because the limitation binds at high interest and debt levels, where costs escalate quickly. While a first dollar of debt yields little bankruptcy risk, additional borrowing for high-debt firms can substantially raise risk (Scott, 1976). High leverage firms rely more on debt with strong creditor controls, and face tighter loan covenants, with restrictions often binding at similar interest coverage ratio thresholds as the limitation (Kermani and Ma, 2023; Hanlon and Heitzman, 2024). This interpretation also aligns with prior evidence: Van Binsbergen et al. (2010) find steeply upward sloping marginal cost of debt curves among publicly-held firms with lower credit ratings and similarly high debt levels.

Turning to investment, neoclassical theory argues that an increase in the marginal cost of investment, or user cost of capital (UCC), reduces investment (Hall and Jorgenson, 1967).

³Our results do not explicitly rule out the possibility that firms do not trade off marginal costs and benefits while making financing choices, but this is difficult to square with the large literature confirming these incentives matter for capital structure choices (Kraus and Litzenberger, 1973; Jensen and Meckling, 1976; Scott, 1976; Graham, 1996, 2000; Booth et al., 2002; Hennessy and Whited, 2005; Van Binsbergen et al., 2010; Faccio and Xu, 2015; Heider and Ljungqvist, 2015; Fleckenstein et al., 2020; Ivanov et al., 2025).

Using common UCC parameterizations to scale our event study (triple-difference) estimates into investment rate elasticities with respect to the UCC yields a range of $[-0.34, 0.64]$ ($[-0.51, 0.63]$), ruling out the range of $[-2, -8]$ estimated from other tax policy variation by a factor of four (Chodorow-Reich, 2026). Thus, our null investment estimates reject a joint hypothesis that the interest limitation substantially increases firms' UCC and that investment responds to marginal costs as much as leading estimates predict.

Our evidence points to the failure of the first component of this joint hypothesis: the limitation appears to not increase the marginal cost of investment. For affected firms, our results suggest that additional borrowing is particularly costly, so debt is unlikely to be the marginal source of financing for new investment. Consistent with a pecking order (Myers and Majluf, 1984), firms should instead rely on other financing sources for incremental investment, allowing the limitation to reduce the tax benefit of debt without materially increasing the marginal cost of investment.⁴ This mechanism also highlights an important limitation of standard UCC parameterizations. Although the Hall and Jorgenson (1967) framework assumes financing from retained earnings, common parameterizations we use for comparison with other studies assume that the fraction of marginal investment financed with debt equals the average debt-financing share (Burnham and Carloni, 2022; Foertsch, 2022). If firms rely less on debt at the margin, these parameterizations overstate the limitation's effect on the UCC. Consistent with this interpretation, our event-study and triple-difference estimates cannot reject an investment rate UCC elasticity of -2 – at the low end of the range of prior estimates – when only 13% and 15%, respectively, of marginal investment is debt financed. These financing shares are in line with empirical and survey evidence that U.S. firms do not typically debt-finance marginal investment (Yagan, 2015; Sharpe and Suarez, 2021).

Steep marginal cost of debt curves and small debt-financing fractions for marginal investment projects are not the only possible explanations for our null investment and financing estimates. However, the magnitudes of our estimates, heterogeneity cuts, and additional tests do not support adjustment costs, irreversibility, sticky discount rates, high hurdle rates, debt

⁴There is little reason to expect investment to be less responsive to marginal cost changes in the post-TCJA period. Unlike the limitation, which only impacted the cost of debt-financed investment, other TCJA provisions that changed the marginal cost of all investment had substantial impacts on investment (Chodorow-Reich et al., 2026; Kennedy et al., 2026).

overhang, policy uncertainty, inattention, using average costs to make decisions, a leverage ratchet effect, or creditor evergreening as driving explanations for our null results.

While the similarity of our estimates across designs suggests responses do not vary by firm size, our results may not apply to lower interest firms or stricter limitations. We address this concern with two additional analyses. First, the earnings component of the limitation tightened from 30% of EBITDA to 30% of EBIT in 2022, drawing in more firms and disallowing more deductions. Although the latter portion of our triple difference time horizon already captured this additional loss of deductions for originally treated firms, we separately implement our research designs around the 2022 tightening to evaluate effects for lower interest firms and find no evidence of substantial declines in investment or leverage. Second, dividing firms into deciles by the extent to which pre-TCJA interest exceeds their limit, event study and triple difference estimates cannot reject zero investment or leverage impacts across any decile, and exhibit no evidence of trends across deciles. Thus, across the range of levered capital structures that lose interest deductions, both tests indicate that marginally tighter limitations are unlikely to cause substantial declines in investment or borrowing. Extrapolation to complete elimination of interest deductions, however, depends on the slope of marginal cost curves and propensities to debt-finance investment across wider ranges of the leverage distribution.

Three other papers study the U.S. interest limitation. Carrizosa et al. (2022) and Hanlon and Heitzman (2024) compare big, high-and low-interest firms in Compustat, while Sanati and Beyhaghi (2025) use an RD design on high-interest firms in Compustat and Y-14Q data. Carrizosa et al. (2022) and Sanati and Beyhaghi (2025) find large borrowing responses to the limitation. Implementing the Carrizosa et al. (2022) comparison in our data, we also estimate large borrowing declines, but show they appear to be driven by mean reversion that does not impact our estimates. Building on the same argument, Hanlon and Heitzman (2024) address reversion using past high- versus low-interest comparisons, an approach that depends on the stability of borrowing reversion over time, and find marginally significant declines that are broadly consistent with our estimates. Relative to these other data sources we possess a larger sample, can construct a more precise definition of the receipts running variable and location of the policy cutoff, and directly observe whether firms face the limitation.

This research also contributes to the literature testing theories of corporate capital structure by attempting to isolate variation in the marginal benefits or costs of debt. Past empirical tests using data on publicly-held firms and corporate tax rate variation across time, countries, and U.S. states have found a positive relationship between tax rates and leverage.⁵ One notable exception is Ivanov et al. (2025), who find a negative relationship between debt and U.S. state tax rates for smaller private firms, highlighting that tax rate changes alter the value of interest deductions, the after-tax value of all income, and firms’ distance to default. The interest limitation changes the marginal benefit of debt without changing the after-tax value of every dollar of income, providing a cleaner test of firm responses to changes in the tax benefit of debt without significant simultaneous changes to costs.

Finally, this research contributes to understanding how firms’ marginal financing sources determine the effects of tax policy. For example, the old and new views of dividend taxation make different predictions about how dividend taxes affect the cost of capital depending on how investment is financed (Auerbach, 1979; Poterba and Summers, 1985; Yagan, 2015; Moon, 2022). Trade-off theory implies that firms borrow until the marginal cost and benefit of debt are equal, with costs such as bankruptcy risk, creditor control rights, and binding loan covenants arising to address asymmetric information between borrowers and lenders. Absent such frictions, capital structure would not affect firms’ real economic choices (Modigliani and Miller, 1958). Our evidence suggests these same frictions that make financing choices relevant can also dampen firms’ responses to tax policy, because high marginal costs factor into investment and financing choices.

The rest of the paper proceeds as follows. Section 2 describes the interest limitation. Section 3 introduces the tax data and shows summary statistics. Section 4 presents RD, event study, and triple difference estimates of the impact of the interest limitation, section 5 discusses the robustness of these estimates, and section 6 explores heterogeneity. Section 7 quantifies the economic magnitudes of our results and discusses mechanisms. Section 8 assesses external validity and section 9 concludes.

⁵See MacKie-Mason (1990); Rajan and Zingales (1995); Graham (1996); Booth et al. (2002); Faccio and Xu (2015); Heider and Ljungqvist (2015); Fleckenstein et al. (2020). A related literature studies limitations on interest from multinationals’ inter-subsidiary lending that facilitates profit shifting (Desai et al., 2004; Blouin et al., 2014; Alberternst and Sureth-Sloane, 2016; Bilicka et al., 2022). In contrast to these narrowly tailored limitations, the U.S. interest limitation applies more broadly to domestic and international firms.

2 Tax Policy Background

2.1 The Interest Limitation

In December 2017, the U.S. passed a major tax reform referred to as the Tax Cuts and Jobs Act (TCJA) that introduced a limitation on interest deductions. The limitation stipulates that interest deductions cannot exceed 30% of a firm's adjusted taxable income (ATI) plus interest income. The TCJA defined ATI from 2018-2021 as net income before interest expense and interest income, depreciation, depletion and amortization, similar to the accounting concept of earnings before interest, taxes, depreciation, and amortization (EBITDA). From 2022 onwards, the law defined ATI as net income before interest expense and interest income, closer to the accounting concept of earnings before interest and taxes (EBIT).

The interest limitation does not apply to small business taxpayers whose average annual receipts over the previous three years are less than \$25 million.⁶ Therefore, among firms with net interest expense greater than 30% of ATI, firms with average lagged receipts exceeding \$25 million lose interest deductions, while firms with average lagged receipts below \$25 million do not. This comparison forms the basis of our identification strategies. We depict this variation in Figure 1. The x-axis measures average lagged receipts and the y-axis measures the ratio of interest expense to firms' limitation. Firms in quadrant A have interest above their limitations but are too small to face the interest limitation. Firms in quadrant B face the interest limitation. Firms in quadrants C and D are low interest and therefore do not face the interest limitation regardless of size.

The interest limitation lowers the marginal tax benefit of debt and raises the marginal cost of debt-financed investment by disallowing interest deductions. With interest rate r and tax rate τ , each incremental dollar of debt costs $1 + (1 - \tau)r$ when interest is deductible. The interest limitation lowers the marginal tax benefit of debt from τr (in quadrants A, C, and D in Figure 1) to 0 (in quadrant B) for each incremental dollar of debt. Neoclassical theory argues that firms set marginal benefits equal to marginal costs when making investment and financing choices, so our research designs focus on identifying sets of firms that face

⁶The \$25 million cutoff adjusts upwards in later years to account for inflation, which we account for when looking at the 2022 tightening, but for simplicity we refer to it as a \$25 million cutoff throughout.

the limitation, will not be eligible to take interest deductions on incremental borrowing, and therefore face lower marginal tax benefits of debt and higher marginal costs of debt-financed investment. In addition to impacting marginal costs and benefits, the limitation also disallows some inframarginal interest deductions, expanding the tax base and raising tax bills. Therefore, we also consider the potential liquidity impacts of the interest limitation.

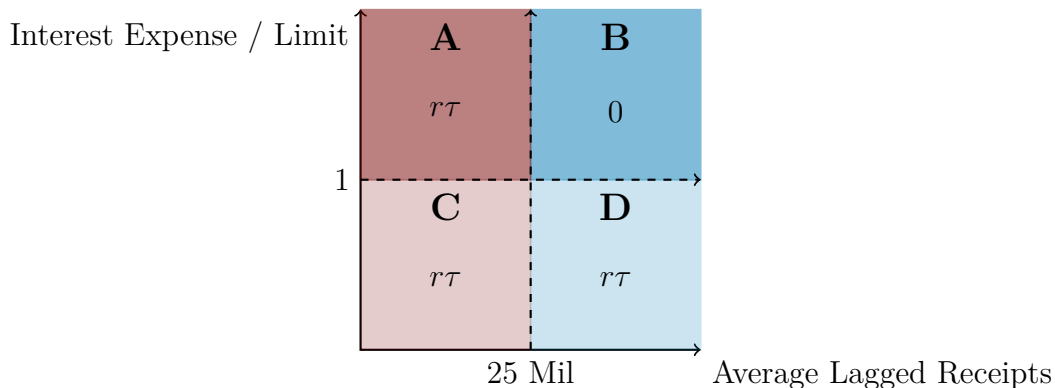


Figure 1: Marginal Tax Benefit of Borrowing

Notes: This figure displays the static marginal tax benefit of debt for firms with interest rate r and tax rate τ . The four quadrants represent large and small and high and low interest firms. The firms in quadrant B with average lagged receipts exceeding \$25 million and with interest expense above their limit lose interest deductions.

There are some exceptions to the general interest limitation rules. Small businesses that are deemed to be tax shelters still face the limitation.⁷ In addition, businesses with agriculture or real estate components can opt out of the limitation, but then must use a less generous depreciation method. In practice, only 16.5% of real estate and 3.4% of agriculture firms elect out of the limitation. Finally, floor plan financing is included in the limitation formula for auto dealers.

The interest limitation is applied at the entity level to C-corporations, S-corporations and partnerships. Excess business interest expense can be carried forward to future years.⁸ Firms that are closely related to each other are required to aggregate their receipts for the purpose of determining whether they are below or above the \$25 million threshold.

⁷The IRS deems firms tax shelters if the agency determines that a significant purpose of the business is to evade or avoid federal income tax, or if the business is an S-corporation or partnership where 35% or more of losses are allocated to limited partners or entrepreneurs.

⁸Interest limitation carryforwards do not drive our economically small estimates. We document carry-forward use rates and adjust our estimates to account for them in section 7 and Appendix F.

In March 2020, the Coronavirus Aid, Relief, and Economic Security (CARES) Act raised the share of ATI used to calculate the interest limitation from 30% to 50% for 2020, and applied this retroactively to C- and S-corporations for 2019 (and under more complex rules for partnerships). Given the timing of the CARES Act passage, this could have affected firms' decisions in 2020 but not 2019. The limitation reverted to 30% of ATI in 2021, and as legislated, ATI moved from an EBITDA to EBIT concept in 2022.⁹

2.2 Additional TCJA Policy Changes

The TCJA made many other changes to the individual and corporate tax codes. Auerbach (2018) and Barro and Furman (2018) provide detailed discussions of the legislative changes. Beyond the limitation, the TCJA modified investment incentives, eliminated loss carrybacks and limited loss carryforward deductions to 80% of taxable income, repealed the corporate alternative minimum tax and domestic production activities deduction, and modified the taxation of multinational firms' income. Rather than describe each change exhaustively, we briefly discuss the TCJA changes that are relevant for evaluating the validity of our empirical strategy, particularly the event study, due to differences in tax incentives faced by big and small firms before and after the reform or heterogeneous responses to such incentives.

The TCJA cut the corporate tax rate from 35 to 21%, cut the top individual tax rate from 39.6 to 37%, and introduced an additional deduction on some pass-through income that effectively lowered the tax rates of pass-through businesses by 2-7 percentage points (Goodman et al., 2025; Kennedy et al., 2026). Therefore, C-corporations, which tend to be larger, and pass-through businesses, which tend to be smaller, faced different pre-reform tax rates and different-sized tax rate changes.

The TCJA also expanded two investment incentives. Section 179 expensing (immediate deduction of a limited amount of investment expense) rose from \$500,000 to \$1 million, while bonus depreciation raised the share of investment immediately deductible from 50% to 100%. These changes could increase investment incentives for large firms more than small

⁹The One Big Beautiful Bill Act (passed in 2025) reverted the definition of ATI to EBIDTA for 2025 onward, but this change, and the release of legislative text signaling this change, occurred well beyond our sample period.

firms because section 179 only applies to the first \$1 million of investment, while bonus depreciation is not capped.

Finally, one change coincided with the \$25 million lagged receipts cutoff: the TCJA permitted businesses below the threshold to switch from accrual to cash accounting (i.e., recording revenue and expense items for tax purposes when cash changes hands rather than when commitments are made about the exchange of goods and services). This accounting change can provide short-term tax savings by deferring taxable income. While this effect would go in the same direction as the limitation, later RD analysis of low-interest firms indicates it is not confounding our investment and financing estimates.

3 Data on Firm Investment and Financing

3.1 Samples of U.S. Business Tax Returns

This paper primarily uses stratified random samples of U.S. business tax returns produced by the Internal Revenue Service’s (IRS) Statistics of Income (SOI) division. These samples are used by government agencies and researchers to construct aggregate statistics, and perform revenue estimation and policy analysis. Each year, SOI randomly samples business tax returns separately for C-corporations, S-corporations and partnerships using a sampling rate that is an increasing function of firm size.¹⁰ Once the sample is selected in each year, SOI manually edits many variables for accuracy and consistency.

3.2 Analysis Samples and Variable Definitions

Our main analysis sample is an unbalanced panel of C-corporations, S-corporations and partnerships, constructed by appending yearly SOI corporate and partnership files from 2013-2023. We restrict the sample to only include firms i) with positive income or positive

¹⁰C-corporations are stratified by total assets and net income, S-corporations are stratified by total assets and ordinary business income, and partnerships are stratified by total assets, industry, and an income measure including both ordinary business income and portfolio income (Decarlo and Shumofsky, 2015). For all business types, large businesses are sampled with probability one, while smaller businesses are sampled at lower rates. For example, in the 2013 sample, Form 1120 filers with at least \$50 million of assets or \$10 million of net income are sampled with probability one, as are Form 1120-S filers with at least \$50 million of assets or \$10 million of ordinary business income.

deductions, ii) with assets and one year lagged assets never below \$100,000, and iii) present in at least one year between 2015-2017 and at least one later year. We also drop financial and utility firms. This unbalanced panel has 588,324 firm-years, 69,751 unique firms, and 25,582 firms that appear in every year. We use this entire sample in our triple difference design, and subsets of the sample in our regression discontinuity and event study designs.

The SOI data include the information filed on business income tax returns necessary to construct the key variables for the analysis in this paper: assets, capital, investment, debt, and equity. We discuss the definitions of our key variables in this section, and provide additional definitions and specific tax form line item numbers in Appendix A. *Assets* represent the book value of all firm assets. *Capital* is the book value of all tangible capital assets. *Investment* equals the purchase price of all newly installed capital assets listed on Form 4562, a supplemental tax form filed to claim depreciation and expensing deductions.

To track firm financing responses to the interest limitation we measure both debt and equity. We use multiple measures of debt to align with past studies and focus on interest bearing liabilities whose use is most likely to be impacted by the limitation. *Debt* is the sum of all mortgages, notes, and bonds. Short-term debt is mortgages, notes, and bonds due in less than one year, and long-term debt is mortgages, notes, and bonds due in one year or more. We also track loans from shareholders, which are required by law to bear interest, and use the sum of short-term debt, long-term debt, and loans from shareholders as a broader measure of interest bearing liabilities. *Equity* is total paid-in capital which equals the sum of common stock, preferred stock, and additional paid-in capital. To measure debt and equity issuance, we use net concepts to measure whether firms are issuing more debt than they are retiring or more equity than they are buying back. *Debt issuance* is therefore $\text{debt}_t - \text{debt}_{t-1}$, while *equity issuance* is $\text{equity}_t - \text{equity}_{t-1}$. We also measure liquidity, or *cash*, as the sum of cash and all other liquid securities.

The interest limitation only applies to firms with average lagged receipts over the previous three years exceeding \$25 million, and the policy disallows interest deductions exceeding 30% of firm adjusted taxable income plus interest income. *Receipts* are gross receipts plus dividends, interest, rents, royalties, capital gains, and tax exempt interest. *Adjusted taxable income* is a tax-based EBITDA concept, all income minus all deductions, plus deductions

for interest, depreciation, depletion, and amortization, and minus interest income.

The corporate and partnership SOI samples include information from Form 8990 that allows us to directly observe which firms lose interest deductions. We measure *interest deductions* as the sum of all firm interest deductions, *interest disallowed* as the amount of interest disallowed on Form 8990, and *total interest* as interest deductions for firms without an 8990 and as reported on Form 8990 for firms that file an 8990. From total interest, we construct an *interest rate*, the ratio of total interest to all interest bearing liabilities.

To account for skew in the firm size distribution, we scale outcome variables and winsorize non-zero observations at the 5th and 95th percentiles. We scale investment by gross capital and financing variables by assets using a one year lag. For example, in year t , the *investment rate* is $\text{Investment}_t / \text{Capital}_{t-1}$, and *leverage* is $\text{Debt}_t / \text{Assets}_{t-1}$.

3.3 Summary Statistics

We summarize key variables in Table 1 in 2017 U.S. dollars. In 2018-2019, the average big, high-interest firm facing the interest limitation has \$21 million in interest disallowed (10% of total income). The median firm has \$3 million in interest disallowed (8% of total income).

We present additional descriptive statistics in Appendix B, including summary statistics for supplementary outcomes and balance tables comparing big versus small and high- versus low-interest firms. We define big firms as those with average 2015–2017 receipts above \$25 million and high-interest firms as those whose interest exceeds their limitation on average over 2015–2017. By construction, there are substantial size differences between big and small firms. Big, high-interest firms have similar leverage and debt issuance rates as smaller high-interest firms but hold less cash, payout less to shareholders, are older, and have higher investment rates. In 2017 in our data, big, high-interest firms do 16% of investment and hold 26% of debt, suggesting large changes in investment or borrowing for these firms could have macroeconomic implications, with 55% of investment done by private firms, emphasizing the importance of including private firms in the analysis. Appendix Figure B.1 plots the distribution of firms’ interest relative to their limitation, showing the limitation is high relative to average interest, but that there are still many big and small firms in our data with interest exceeding their limitations.

Table 1: Summary Statistics

	Mean	Std Dev	P10	P50	P90	Obs	Firms
<i>Scaling</i>							
Assets (Mil 2017 USD)	901.7	28,488.4	1.0	29.7	480.3	588,324	69,751
Tangible Capital (Mil 2017 USD)	173.5	2,384.5	0.0	5.0	129.3	588,324	69,751
<i>Tax and Interest</i>							
Total Interest / Lagged Assets	0.013	0.017	0.000	0.005	0.038	588,324	69,751
Interest Deductions / Lagged Assets	0.012	0.016	0.000	0.005	0.037	588,324	69,751
Interest Disallowed / Lagged Assets	0.003	0.017	0.000	0.000	0.000	312,136	69,751
Interest Rate	0.061	0.082	0.003	0.039	0.112	588,324	69,751
<i>Investment and Financing</i>							
Investment / Lagged Capital	0.087	0.125	0.000	0.038	0.248	538,965	64,407
Debt / Lagged Assets	0.245	0.325	0.000	0.076	0.763	588,324	69,751
Debt Issuance / Lagged Assets	0.004	0.106	-0.098	0.000	0.116	588,324	69,751
Equity Issuance / Lagged Assets	0.009	0.056	-0.030	-0.000	0.058	588,324	69,751

Notes: This table reports summary statistics for our entire unbalanced panel data set spanning 2013-2023. To preserve taxpayer anonymity, percentile statistics are reported as the means of the 11 observations nearest the true percentile. Interest disallowed statistics are reported only for the post-reform period when the variable is non-zero.

4 Investment and Financing Effects of the Limitation

4.1 Short-Run Effects

Regression Discontinuity Design: We begin by evaluating the short-run impacts of the interest limitation through 2019 using a regression discontinuity (RD) design. We compare high-interest firms just above and below the \$25 million lagged receipts threshold, estimating the causal effect of the limitation on firms just large enough to face the policy. The RD design delivers local estimates of the effect of the limitation, relying on an identification assumption that firms cannot precisely manipulate their past receipts (Lee, 2008; Lee and Lemieux, 2010). This assumption is unlikely to be violated because the TCJA text (including the specific parameters of the limitation) was not released until November 2017 or passed until December 2017.

To implement the RD, we restrict our SOI panel to years 2015-2019 and to firms with interest above their limitation averaging over 2015 to 2017. To obtain cleaner variation in the

applicability of the interest limitation in narrow bandwidths around the \$25 million lagged receipts cutoff, our RD sample excludes all firms that are parents or children in ownership links with $> 50\%$ stakes that may have to aggregate their receipts.¹¹

First, we provide a graphical description of the RD design by plotting the difference in raw means between 2018-2019 and 2015-2017 of important variables around the \$25 million cutoff. We use average receipts over 2015-2017 as the running variable to alleviate concerns about potential endogenous receipts responses after 2018. Figure 2 plots outcomes in evenly spaced \$2 million receipts bins within a \$12 million bandwidth around the cutoff. Panel (a) plots first stage outcomes, showing there is a clear extensive and intensive margin jump in interest disallowed around the \$25 million cutoff. Panels (b)-(d) display pre-post mean differences in the investment rate, leverage, and debt and equity issuance. There is no clear visual discontinuity for investment, leverage, or either issuance variable, suggesting no obvious changes in the investment or financing choices for firms just above the lagged receipts cutoff, despite their loss of interest deductions.

To complement our visual depictions of the RD design, we estimate the RD following

$$(1) \quad \Delta Y_i = \psi Big_i + f(z_i) + \varepsilon_i,$$

where ΔY_i is the pre-post mean difference in the investment rate, leverage, debt issuance, or equity issuance, z_i is average lagged receipts over 2015-2017, $Big_i = 1$ if $z_i > \$25$ million, and $f(z_i)$ are polynomials in the running variable separately on each side of the receipts threshold. Differencing the outcome increases precision and controls for different pre-reform outcome levels. Following Gelman and Imbens (2018) and Calonico et al. (2014), we use a triangular kernel, first-order polynomial, and \$12 million bandwidth, consistent with guidance to use first order polynomials when higher order coefficients are insignificant and with optimal bandwidths for our outcomes.

Panel A of Table 2 displays regression estimates of ψ from equation (1). Across all four

¹¹We show in Appendix C that our results are similar without this restriction. We describe our ownership link measure in Appendix A. The measure uses information from Form 851, Schedule K-1 of Forms 1120-S and 1065, and Schedules K and G of Form 1120. As such, our closely related flag may not capture all brother-sister and combined corporate groups that are required to aggregate receipts. Small firms deemed tax shelters by the IRS can also face the limitation, but we are unable to identify tax shelters in the data.

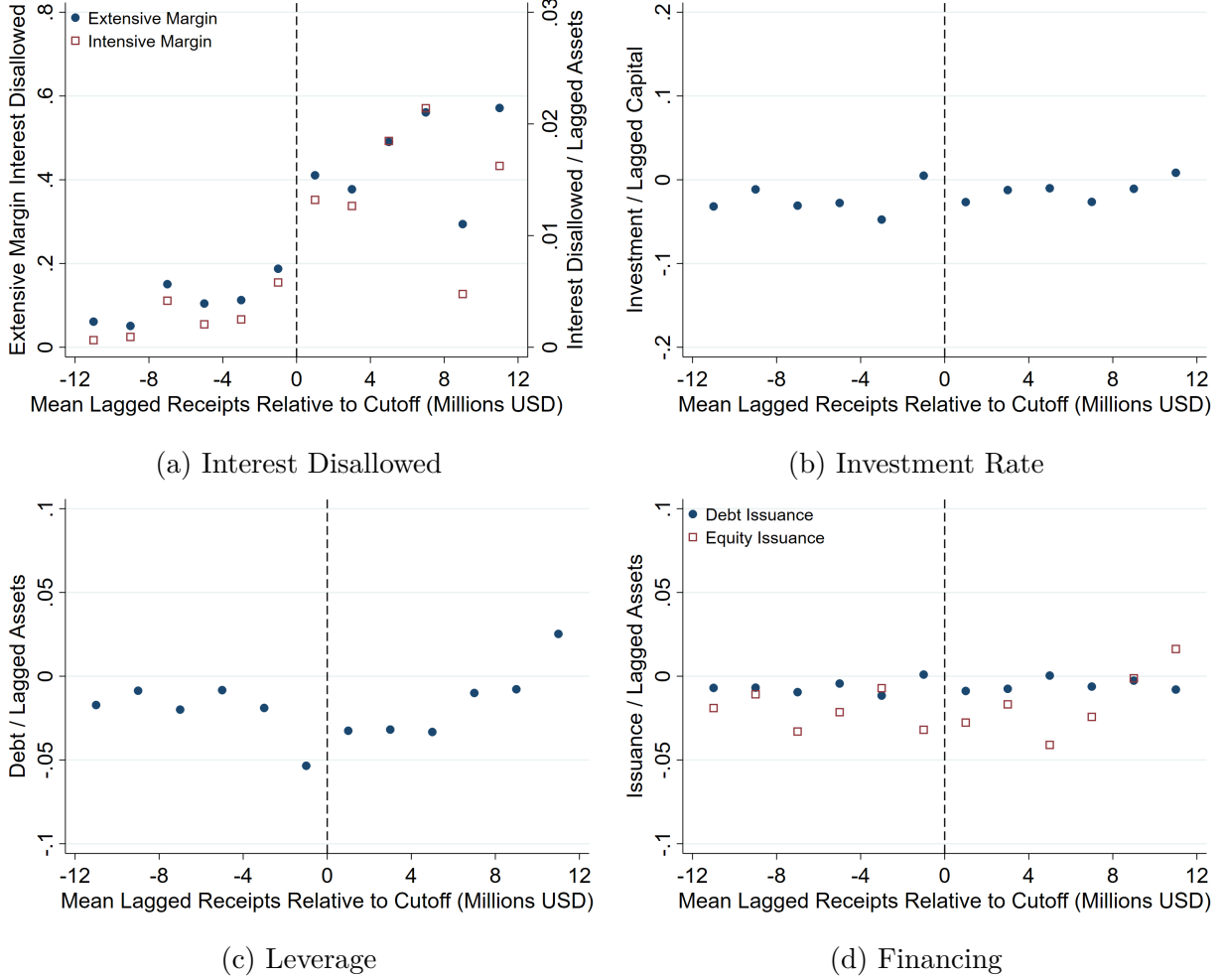


Figure 2: Regression Discontinuity Binned Scatter Plots

Notes: This figure plots pre-post mean differences in evenly spaced \$2 million receipts bins around the \$25 million cutoff using different variables in each panel. Panel (a) uses having any interest disallowed and interest disallowed scaled by lagged assets. Panel (b) uses investment scaled by lagged capital. Panel (c) uses debt scaled by lagged assets, and panel (d) uses debt and equity issuance scaled by lagged assets.

investment and financing variables, we cannot reject zero impacts of the interest limitation. The point estimate in column 1 suggests that investment rates are 0.9 percentage points lower for firms just above the receipts threshold compared to the firms just below the receipts threshold, but the bottom of the 95% confidence interval admits declines in the investment rate of 4.60 percentage points.

While the RD delivers well-identified local effects of losing interest deductions around the \$25 million lagged receipts threshold, the estimates are not consistently precise enough

to rule out economically significant effects, nor are they necessarily informative about the impacts of the limitation on all firms that lose interest deductions. To obtain more precise estimates of the impacts of the limitation on a broader set of firms that is more informative about aggregate policy impacts, we next turn to our event study design.

Event Study Design: Our event study design compares high-interest firms that face the interest limitation because their average lagged receipts are above the \$25 million threshold to high-interest firms that do not because their average lagged receipts fall below the threshold. Taking a broader sampling frame, we restrict the SOI panel to years 2013-2019 and to firms with interest above their limitation averaging over 2015 to 2017. Our event study specification is

$$(2) \quad Y_{it} = \sum_{e=2013, e \neq 2017}^{2019} \beta_e \mathbb{1}(t = e) \times Big_i + \delta_{jt} + \xi_i + \varepsilon_{it},$$

where Y_{it} is an outcome for firm i in year t , Big_i is a dummy variable equal to 1 if firm i has average receipts from 2015-2017 above \$25 million, ξ_i is a firm fixed effect, and δ_{jt} is a three digit NAICS industry j by year fixed effect. We include the latter group of fixed effects because while TCJA tax policy changes could differentially impact big and small firms, they are less likely to do so for high-interest firms in the same industry that are more likely to face the same marginal tax rates, and have similar cost structures and investment durations. We intentionally use a pre-reform measure of treatment status Big_i in equation (2) to avoid endogeneity between the treatment definition and post-reform behavior.

Figure 3, panel (a) displays estimates of the β_e coefficients from equation (2) using an indicator for any disallowed interest and interest disallowed scaled by lagged assets as outcome variables. The estimates confirm that our treatment definition identifies firms affected by the limitation. In the pre-reform period, by definition, zero firms have any interest disallowed. In 2018, the fraction of treatment relative to control firms with interest disallowed jumps to almost 40%, with disallowed interest increasing by 1.1% and 1.5% of lagged assets in 2018 and 2019, respectively. We discuss the imperfect persistence of treatment in Appendix C. While *ex ante* big, high-interest firms do not always have interest disallowed in the post-reform period, there is a substantial increase in the number of firms with interest disallowed

and the amount of interest disallowed for the treatment relative to the control group.

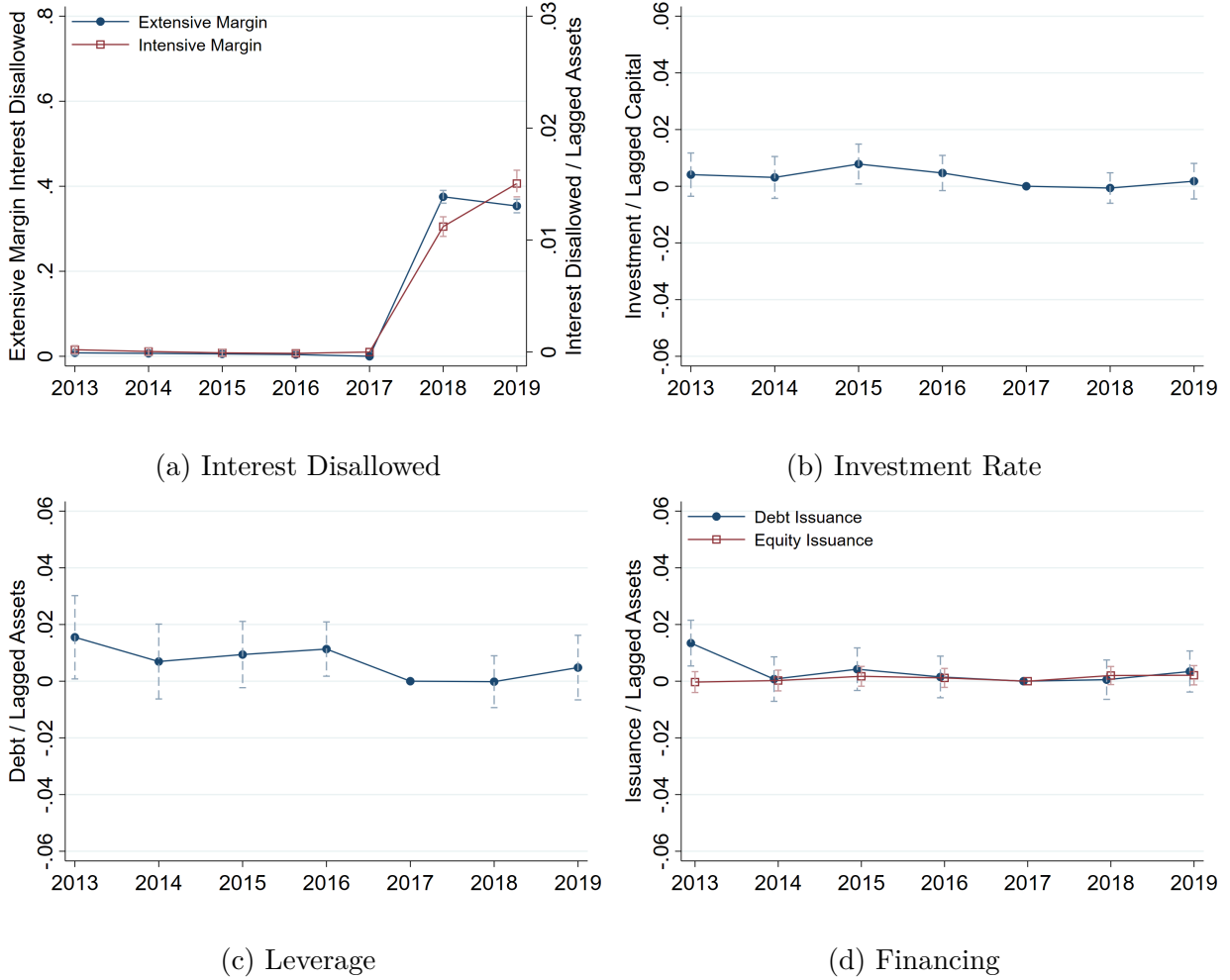


Figure 3: Event Study Estimates

Notes: This figure plots event study estimates of β_e from equation (2) using (intensive and extensive margin) interest disallowed scaled by lagged assets, investment scaled by lagged capital, debt scaled by lagged assets, and debt and equity issuance scaled by lagged assets as outcome variables. 95% confidence intervals are constructed from standard errors clustered at the firm level.

Figure 3, panels (b) - (d) display event study estimates of β_e from equation (2) using the investment rate, leverage, debt issuance, and equity issuance as outcome variables. Across outcomes, pre-reform coefficients are largely flat and do not reject zero (with the exception of perhaps a slight downward pre-trend for leverage), suggesting the outcomes of the treatment and control groups are likely to continue to evolve similarly in the absence of the policy. In the post-reform period, we find no evidence of statistically significant investment or financing responses to the interest limitation.

To quantify magnitudes, we re-estimate equation (2) replacing 2018 and 2019 indicators with a single post-reform indicator. The resulting β_{post} coefficients, displayed in Table 2, panel B, represent estimates of the average post-reform response for treatment relative to control firms. Column 1 indicates that investment increases by \$0.0015 per dollar of lagged tangible capital assets with a standard error of \$0.0026. Relative to a pre-reform mean value of \$0.12 per dollar of lagged capital, we estimate a precise null effect of the limitation on the investment rate.

Columns 2 through 4 of Table 2 suggest that the interest limitation had statistically insignificant and economically small effects on leverage, debt issuance, and equity issuance. Per dollar of lagged assets, point estimates (standard errors) are \$0.0020 (\$0.0045) for debt, \$0.0021 (\$0.0031) for debt issuance, and \$0.0026 (\$0.0014) for equity issuance, in each case implying small, insignificant increases. A violation of leverage parallel trends is unlikely to explain the absence of borrowing declines; extrapolating any pre-trends would, if anything, push estimates upward. These estimates are an order of magnitude more precise than the RD estimates in Table 2, panel A, further indicating that the limitation did not have statistically significant impacts on investment or financing choices and ruling out economically significant effect sizes. While these estimates do not rule out any decline in investment or substitution from debt to equity, our estimates bound the magnitude of any such effects to be small. For example, we can rule out debt issuance declines greater than 0.7% of assets and equity issuance increases greater than 0.5% of assets. We discuss the magnitude of these effects relative to the size of the shock in section 7.

Our event study design relies on a parallel trends assumption that the outcomes of the larger treatment and smaller control firms would have evolved similarly in the absence of the limitation. Visual inspection of parallel trends in Figure 3 shows that outcomes for treatment and control firms largely trended similarly before the reform, suggesting they would likely continue to do so in the absence of the reform. However, a key threat to our event study design is that non-interest limitation shocks could differentially impact larger and smaller firms, biasing our estimates. In particular, other TCJA tax policy changes could differentially impact the larger treatment and smaller control firms. For example, our null results could be explained by the limitation causing a decline in investment, but being offset by larger

firms disproportionately benefiting from TCJA tax rate cuts. However, we believe this is not a significant concern in our setting for three reasons. First, the industry-year fixed effects in equation (2) ensure we compare high-interest treatment and control firms with similar capital structures within the same industry that are more likely to face the same marginal tax rates. Second, we find no differential responses between big and small high-interest firms to a previous change in the tax rate. Third, placebo event study regressions comparing big to small low-interest firms reveal no differential responses to other TCJA reforms by firm size. We present raw mean outcome trends for our treatment and control groups along with these latter two checks and a battery of additional robustness tests in Appendix C.

4.2 Long-Run Effects

In principle, we could extend our event study design through 2023 to estimate longer-run impacts of the limitation. However, our placebo event study fails in 2020. Implementing our event study design through 2020 in Appendix C.1, we show that while big, high-interest firms exhibit substantial declines in leverage and debt issuance that are not present for smaller high-interest firms, big low-interest firms exhibit nearly identical declines relative to smaller low-interest firms. The timing and magnitude of these declines strongly suggest they are caused by COVID-19, and that we can account for the COVID-19 shock by differencing out outcome trends of big and small low-interest firms.

Building on this evidence, we study longer-run impacts of the interest limitation in our entire SOI panel using a triple difference design that compares big and small high-interest firms, netting out any differential trends between big and small low-interest firms. This design relies on a different assumption, that the difference in outcome trajectories between big and small high-interest firms would be the same as the difference in trajectories between big and small low-interest firms in the absence of the limitation. High- and low-interest firms both face other TCJA reforms, so other policy changes are unlikely to bias the triple difference estimates.

Table 2: Effects of Interest Limitation on Investment and Financing

Outcome	(1) Investment Rate	(2) Leverage	(3) Debt Issuance	(4) Equity Issuance
Panel A: Regression Discontinuity Estimates				
ψ	-0.0090 (0.0188)	0.0076 (0.0353)	-0.0094 (0.0224)	-0.0052 (0.0084)
Obs	715	734	734	734
Pre-Reform Mean	0.0930	0.5227	0.0179	0.0163
Panel B: Event Study Estimates				
β_{post}	0.0015 (0.0026)	0.0020 (0.0045)	0.0021 (0.0031)	0.0026 (0.0014)
Obs	79,150	84,737	84,737	84,737
Clusters	14,243	15,274	15,274	15,274
Pre-Reform Mean	0.1178	0.4704	0.0256	0.0169
Panel C: Triple Difference Estimates				
γ_{post}	0.0005 (0.0024)	0.0021 (0.0044)	0.0017 (0.0026)	-0.0014 (0.0012)
Obs	538,576	588,318	588,318	588,318
Clusters	64,024	69,751	69,751	69,751
Pre-Reform Mean	0.1177	0.4694	0.0255	0.0170

Notes: This table reports estimates from our regression discontinuity, event study, and triple difference research designs. In Panel A, we display estimates of ψ from equation (1). In Panel B, we display estimates of β_{post} from equation (2), replacing post-reform indicators and interactions with a single post-reform indicator and interaction. In Panel C, we display estimates of γ_{post} from equation (3), replacing post-reform indicators and interactions with a single post-reform indicator and interaction. The estimation samples vary across designs as described in the text. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for treatment firms in the sample for each design before 2018.

To implement the triple difference design, we estimate

$$(3) \quad Y_{it} = \sum_{e=2013, e \neq 2017}^{2023} \left[\gamma_e \mathbb{1}(t=e) \times Big_i \times HI_i + \phi_e \mathbb{1}(t=e) \times Big_i + \Omega_e \mathbb{1}(t=e) \times HI_i \right] + \delta_{jt} + \xi_i + \varepsilon_{it},$$

where Y_{it} is an outcome for firm i in year t , Big_i is a dummy variable equal to 1 if firm i has average receipts from 2015-2017 above \$25 million, HI_i is a dummy variable equal to 1 if firm i has interest expense above their limitation averaging over 2015-2017, ξ_i is a firm fixed effect, and δ_{jt} is a three digit NAICS industry-year fixed effect.

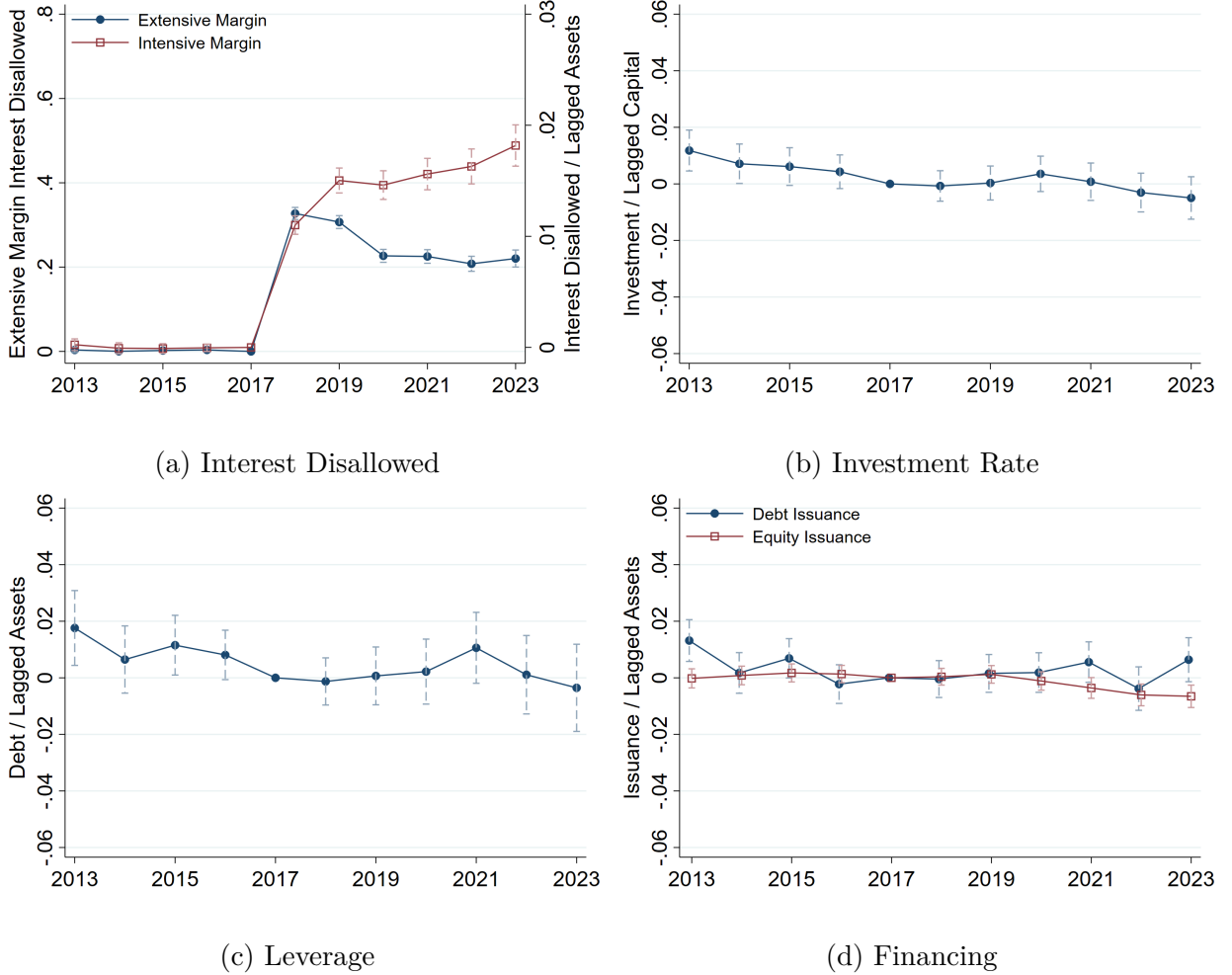


Figure 4: Triple Difference Estimates

Notes: This figure plots triple difference estimates of γ_e from equation (3) using (intensive and extensive margin) interest disallowed scaled by lagged assets, investment scaled by lagged capital, debt scaled by lagged assets, and debt and equity issuance scaled by lagged assets as outcome variables. 95% confidence intervals are constructed from standard errors clustered at the firm level.

We plot estimates of γ_e for our first stage outcomes in Figure 4, panel (a). The first stage of the triple difference design is similar to the first stage of the event study design because few low-interest firms have interest disallowed, regardless of size. Extensive margin interest disallowed attenuates somewhat over six post-reform years, while the quantity of interest

disallowed rises close to 2% of assets. Panels (b) - (d) of Figure 4 plot estimates of γ_e for our four key investment and financing outcomes. Despite actively controlling for potential size-varying impacts of other TCJA reforms by using a different counterfactual, our triple difference design yields nearly identical estimates to our event study design through 2019, and few deviations through 2023, with the exception of the triple difference suggesting an economically small decline in equity issuance in 2022 and 2023.

To quantify magnitudes we re-estimate equation (3), replacing 2018-2023 indicators with a single post-reform indicator. We display these γ_{post} estimates for investment and financing outcomes in Table 2, panel C. Post-reform coefficient estimates averaging over six years are similar to the average two-year event study results in panel B (and RD results in panel A). These estimates provide continued evidence that differential impacts of other TCJA policies across our treatment and control groups are unlikely to bias our event study results, while also suggesting that our null estimates are not driven by the short RD and event study post-reform analysis window. With an interest limitation in place from 2018-2021, and the 2022 tightening of the earnings component of the limitation to 30% of EBIT, we continue to find economically small and statistically insignificant responses.

Our triple difference design does come with some caveats. Readers may find the modified parallel trends assumption strong, since it requires stable trends over a longer window that includes the COVID-19 shock. Moreover, the CARES Act relaxed the limitation in 2019 (retroactively) and 2020, reducing the number of firms losing interest deductions and the quantity of interest deductions lost during the onset of the COVID-19 pandemic. Nonetheless, the limitation provides the best source of variation we currently have to analyze the investment and financing impacts of a substantial change in the tax advantage of debt absent significant simultaneous changes to costs. Overall, despite relying on different identifying assumptions, samples, and time horizons, our three research designs consistently show that the interest limitation had at most economically small and statistically insignificant effects on firms' investment and financing choices.

5 Robustness and Validation

Appendix C presents additional robustness checks validating all three quasi-experimental research designs. The estimates in Table 2 are intent-to-treat estimates that capture the effects of the limitation on firms *ex ante* likely to face the limitation. In Appendix C.2, we also estimate treatment-on-the-treated specifications for all three designs and continue to reject economically significant effects after scaling the reduced-form estimates to account for imperfect treatment persistence.

Focusing on our RD design in Appendix C.3, in addition to finding no evidence of manipulation of receipts to avoid the limitation, an RD among low-interest firms also yields null effects on investment and financing, indicating that the TCJA provision allowing small firms to switch from accrual to cash accounting does not bias our estimates. Furthermore, our results are robust to bandwidth and polynomial degree choice. Next, we attempt to improve our RD estimate precision by using the universe of electronically filed business tax returns, which contains ten times as many firms near the cutoff but is noisier because it lacks the manual editing that assures the high quality of the SOI data. RD estimates using E-filing data are similar but less precise than those from our other research designs, so we focus the remainder of the analysis on the more precise event study and triple difference designs.

Before turning to those designs, we develop RD estimates based only on the sample of public C-corporations in our tax data that would appear in Compustat to be comparable to Sanati and Beyhaghi (2025), which studies the impact of the interest limitation using an RD design and smaller samples from Compustat and Y-14Q data. We continue to be unable to reject null investment and financing responses to the limitation. One key difference between our analyses is that Sanati and Beyhaghi (2025) rely on sales in financial statement data as the running variable and to determine the receipts cutoff, which may be subject to meaningful measurement error relative to the receipts concept in the tax code.¹² This may

¹²Simulations suggest RD estimates based on Compustat or Y-14Q sample sizes and our data generating process can lead to diverging results, pointing to statistical power issues that limit our ability to learn about the effects of the interest limitation from smaller samples even in the absence of a misspecified discontinuity. Receipts are defined under Temp. Regs. Sec. 1.448-1T(f)(2)(iv) and include sales, interest, original issue discount, dividends, rents, royalties, and annuities. The regulation text can be viewed at <https://www.law.cornell.edu/cfr/text/26/1.448-1T>.

lead to a misspecified discontinuity, but is untestable in Compustat and Y-14Q data because they do not track whether firms have interest deductions disallowed.

Moving on to the event study and triple difference designs in Appendix C.4, our estimates remain similar when using samples with greater treatment persistence by excluding firms that elect out of the limitation, varying the number of pre-reform years (1–5) used to classify firms as high-interest, varying whether firms must have interest above their limitation on average or in every treatment definition year to enter the treatment group, and restricting to industries with higher distance to limit autocorrelation and lower interest volatility. We also continue to reject economically large changes in investment and financing when considering alternative outcome variables. For investment, we consider the net investment rate, scaling by net rather than gross capital, extensive-margin investment, and investment spikes exceeding 20% of lagged capital. For financing, we consider the ratio of debt to assets minus non-debt liabilities, the ratio of debt plus loans from shareholders to assets, debt plus loans from shareholders issuance, short- and long-term debt, the debt to equity ratio, and trade credit.

Our estimates are also robust to alternative fixed effects (year or industry-profitability-year), adding separate time trends for average age, revenue growth, sales and profits over 2015-2017, scaling by fixed pre-reform assets or capital, winsorizing at the 99th instead of 95th percentile, excluding real estate firms or auto dealers, excluding industries receiving the largest bonus depreciation benefits, restricting to the above median bonus exposure C-corporations facing larger TCJA rate cut and expensing benefits or the below median bonus exposure passthroughs facing smaller TCJA benefits, asset weighting, restricting to a balanced panel, excluding closely-related firms that may need to aggregate with other entities to determine relevant interest limitation cutoffs, and varying the minimum asset threshold for the analysis sample.

Given the lack of size balance between our treatment and control groups, we perform three additional checks to alleviate concerns that the inclusion of very large firms drives our results. We restrict our samples to only include firms in successively narrower ranges of the average 2015-2017 receipts distribution (between percentiles [1, 99], [5, 95], [10, 90], and [25, 75]). We drop all multinationals, and drop the top 25 percent (by 2015-2017 average receipts) of large firms. In each case, our results remain similar to our baseline estimates.

Finally, Carrizosa et al. (2022) estimate impacts of the interest limitation using Compustat data and an event study design comparing big, high-interest to big, low-interest firms. They find declines in debt of roughly 3% of lagged assets that reject zero, significantly larger than any of our estimates. In Appendix C.4, we show that their estimates appear to be driven by mean reversion, not a response to the limitation, justifying our decision to compare big and small high-interest firms.

6 Heterogeneity

Our average estimates may mask heterogeneous responses among different types of firms. For example, firms experiencing larger changes in their marginal tax benefit of debt or user cost of capital (UCC) as a result of the interest limitation may exhibit stronger responses. Alternatively, effects may be larger for firms facing greater interest-limitation induced liquidity shocks or for firms that are more financially constrained.

To explore these possibilities, Figure 5 presents split sample event study estimates of β_{post} from equation (2) and triple difference estimates of γ_{post} from equation (3). We examine several subgroups where reductions in the incentive to borrow or invest may be strongest. Namely, we split high-interest firms into groups of above and below median predicted UCC changes (when applying the limitation to pre-TCJA UCCs, as defined in section 7) and interest rates averaging over 2015-2017 characteristics. We also split firms into C-corporations and passthroughs and separate firms in loss status (defined as whether deductions exceeded income averaging over 2015-2017) from those that were not. Across our outcomes, we do not observe a clear pattern. While we suggestively see some larger investment declines among firms facing larger increases in marginal costs, evidence is more mixed for our debt outcomes, where we see mostly null or in some cases wrong-signed estimates. More importantly, the vast majority of the estimates still cannot reject zero.

Most of the cuts above capture some differences in limitation-induced effects on liquidity, but we additionally examine heterogeneity in responses across *ex ante* measures of financial constraints. Figure 5 shows little difference between firms with above versus below median 2015-2017 cash holdings. In Appendix Figure D.3 and Table C.14, we report more

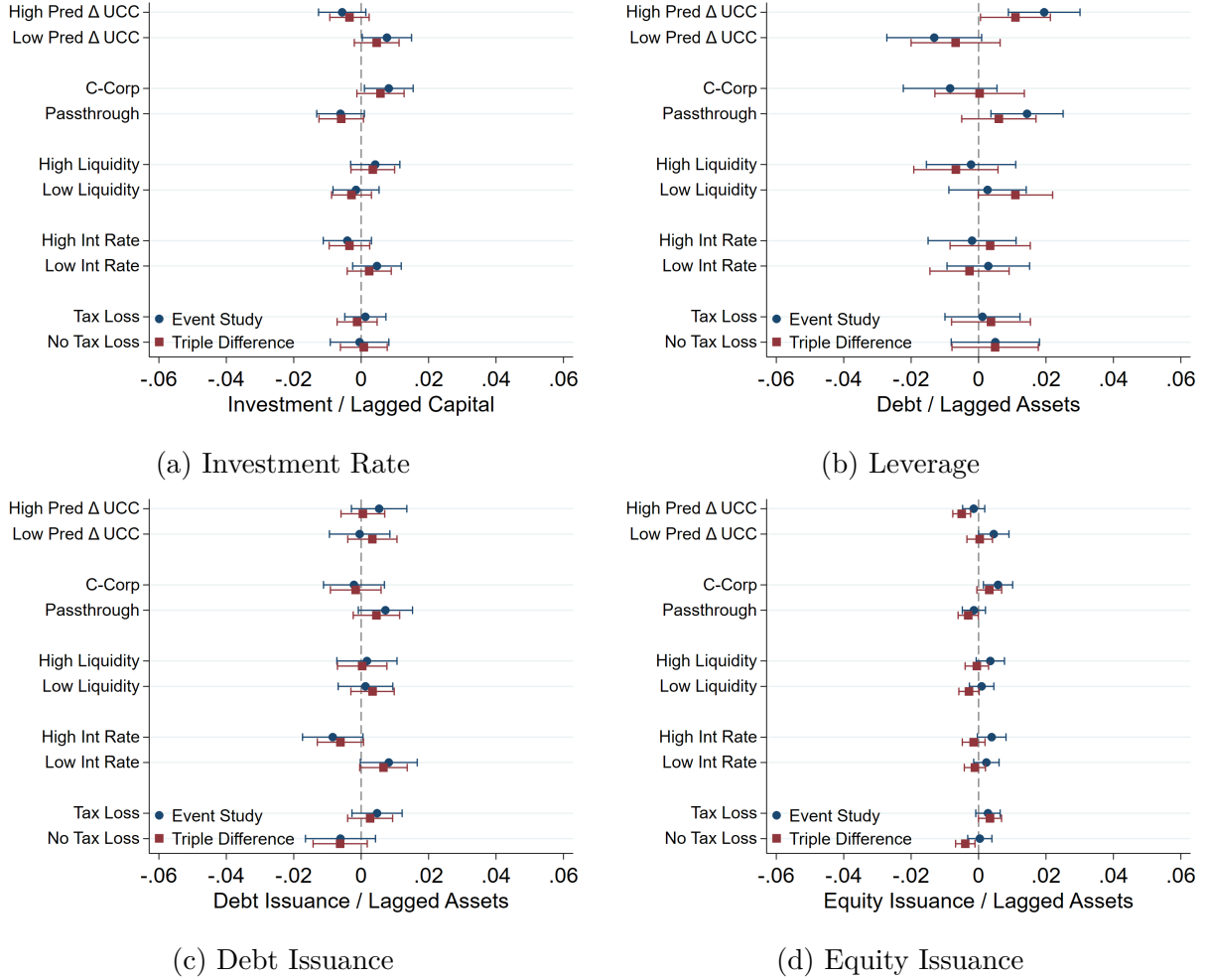


Figure 5: Heterogeneity

Notes: This figure plots event study estimates of β_{post} and triple difference estimates of γ_{post} from equations (2) and (3), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. We display these estimates for subgroups of our event study and triple difference samples to explore heterogeneous impacts of the interest limitation. The four panels in order use investment scaled by lagged capital, debt scaled by lagged assets, debt issuance scaled by lagged assets, and equity issuance scaled by lagged assets as outcome variables. 95% confidence intervals are constructed from standard errors clustered at the firm level. Each heterogeneity split divides firms into above and below median for the high and low groups, except where mentioned otherwise in the text.

heterogeneity results that show similarly null effects across additional proxies for financial constraints and alternative cuts that generate differential liquidity shocks.

Overall, we observe little evidence of meaningful responses to the limitation across subsamples. Part of the reason we do not find larger responses is because effects on liquidity

are modest (see Appendix D for details). The immediate effects of the limitation on cash flow are an order of magnitude smaller than those generated by bonus depreciation (Zwick and Mahon, 2017; Wroblewski, 2025), and we show that over the longer-run taxes paid rise by 0.2% of assets per year.¹³ Despite these small liquidity effects, the limitation has large impacts on the marginal tax benefit of debt and the marginal cost of debt-financed investment. In the next section, we measure these impacts on marginal benefits and costs, and propose mechanisms for why firms exhibit little response to these changes.

7 Economic Magnitudes and Mechanisms

7.1 Debt Estimates

Trade-off theory suggests that firms borrow until the marginal costs and benefits of debt are equal (Kraus and Litzenberger, 1973). Holding all else equal, a decline in the marginal tax benefit of debt should reduce borrowing. Thus, our null borrowing estimates suggest either that the limitation did not substantially reduce the marginal tax benefit of debt, or that borrowing is not particularly responsive to changes in this benefit.

To quantify how much the interest limitation changes the marginal tax benefit of debt, we build on Graham (1996, 2000) and use pre-TCJA income and deduction dynamics to simulate interest, income, and the limitation forward over time to calculate a marginal tax benefit of debt for each C- and S-corporation in our sample while accounting for net operating loss and interest limitation carryforwards.¹⁴ We describe our methodology in detail in Appendix E, which features two key innovations. First, we integrate an interest limitation with carryforwards into our simulations. Second, we apply our method to S-

¹³Despite the small per-firm liquidity impacts of the interest limitation, the policy increased aggregate tax liability of treated firms substantially. Applying our triple difference taxes paid estimate to partnerships, and scaling into aggregate dollar values, our estimates imply that the limitation increased affected firm's aggregate tax liability by \$92.8 billion from 2018 to 2023, in the same ballpark as the JCT revenue score of \$107.5 billion over fiscal years 2018-2023 after accounting for the CARES Act (Joint Committee on Taxation, 2017, 2020). Our taxes paid estimate is not directly comparable to the JCT revenue score, as the JCT score incorporates effects that our estimates do not capture, including effects on control firms and due to changes in opting out firms' depreciation schedules.

¹⁴We do not extend this calculation to partnerships because of the intractability of simulating income through tiered structures that obscure the ultimate ownership of income streams (Cooper et al., 2016), and the lack of empirical work documenting how and when partnerships use losses.

corporations by making data-driven assumptions about S-corporation loss use (Goodman et al., 2023). In short, we simulate interest, income, and the limitation forward for 20 years for each corporation in our sample and calculate the decline in the present value of taxes paid when interest deductions rise by 1% at various multiples of observed interest for each firm under post-TCJA tax policy, with and without the limitation. Multiplying average pre-TCJA interest to asset ratios for big, high-interest firms by the multiples of observed interest, Figure 6 plots the average marginal tax benefit of debt for all big, high-interest firms in our data against levels of interest, with and without the limitation, yielding two marginal tax benefit of debt curves.

At observed levels of interest for big, high-interest firms, the marginal tax benefit of debt for C-and S-corporations is 11.7%. With the interest limitation, the marginal tax benefit drops to 6.4%, a decline of 45%. Combining this change in the marginal effective tax rate with our event study or triple difference confidence intervals to obtain a leverage tax rate elasticity implies a range from $[-0.05, 0.03]$, rejecting the range of positive estimates from tax rate variation spanning $[0.18, 0.41]$ by at least a factor of three (Faccio and Xu, 2015; Heider and Ljungqvist, 2015; Fleckenstein et al., 2020).¹⁵

Despite our estimates of at most small declines in borrowing in response to the limitation, our results do not contradict traditional trade-off theory. Figure 6 also displays event study and triple difference estimates of interest declines for C- and S-corporations relative to the average pre-TCJA interest to asset ratio for big, high-interest firms.¹⁶ All that must be true to rationalize these results is that the marginal cost of debt curve for big, high-interest firms must slope steeply upwards around observed levels of interest, such that when the marginal tax benefit of debt declines because of the interest limitation, there is little change in observed interest. We display one such marginal cost curve in Figure 6, fitting an exponential

¹⁵Tax rate changes shift the marginal benefits and costs of debt simultaneously, while the interest limitation more cleanly isolates variation in the marginal tax benefit of debt. Therefore, leverage tax rate elasticities estimated from tax rate variation provide lower bounds on leverage responses to a *ceteris paribus* change in the marginal tax benefit of debt, or to an interest limitation, implying that there is an even larger gap between our estimated elasticity and predicted responses to the limitation based on existing positive elasticity estimates.

¹⁶We show our event study and triple difference results remain similar when dropping partnerships from the sample in Appendix Table E.1, suggesting restricting the marginal tax benefit of debt analysis to only C- and S-corporations does not lead to problematic selection.

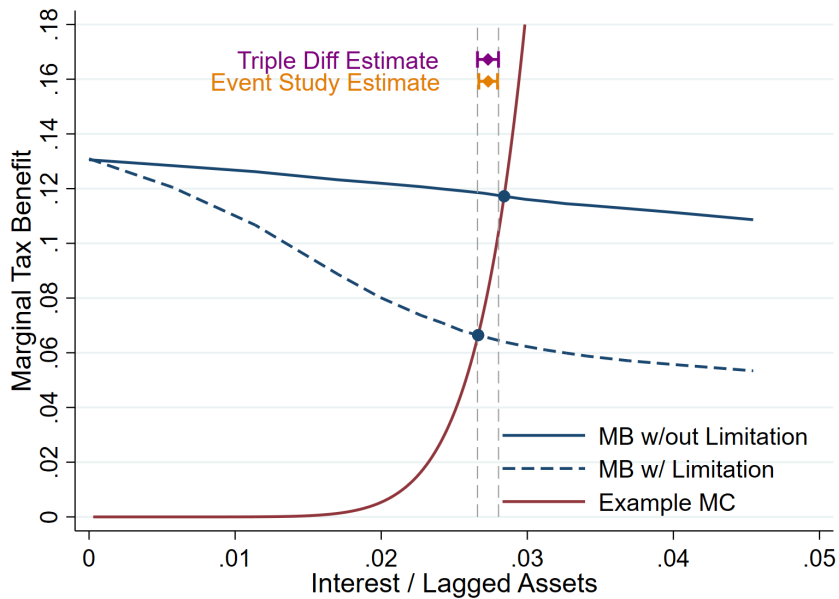


Figure 6: Marginal Benefits and Costs of Interest

Notes: This figure plots the marginal benefit (MB) of an additional dollar of interest for big, high-interest C-corporations and S-corporations. The solid blue line plots the MB of additional interest in the absence of the interest limitation, while the dashed line plots the MB of additional interest with a limitation, interpolating through the discrete points calculated at different interest multiples. The event study and triple difference estimates are average post-reform estimates of the decline in total interest for C- and S-corporations facing the limitation in Appendix Table E.1, plotted relative to the observed level of interest for big, high-interest firms in the absence of the limitation (0.028) on the x-axis. The example marginal cost (MC) curve in red is an exponential fit set to pass through the MB curve without the limitation at 100% of observed interest, (0, 0), and a level of interest within the range of the estimated declines in interest plotted in the figure.

through (0, 0), the observed pre-reform interest level and marginal benefit curve without the limitation, and a point implied by the estimated declines in interest.

The steep marginal cost curve region in Figure 6 is consistent with theory and the characteristics of the firms facing the limitation. As in Van Binsbergen et al. (2010), the marginal cost curve encompasses everything that is not a tax benefit of debt that firms incorporate into their capital structure choices. Big, high-interest firms facing the limitation have leverage ratios that are more than double those of low-interest firms. High leverage firms are likely to face higher bankruptcy risks (Scott, 1976), rely more on debt with strong creditor controls, and face tighter loan covenants that often bind at similar interest coverage ratio thresholds as the limitation, suggesting additional borrowing for these firms may be particularly costly

(Kermani and Ma, 2023; Hanlon and Heitzman, 2024). Using different tax policy variation, Van Binsbergen et al. (2010) find steeply upward sloping marginal cost of debt curves among publicly-held firms with lower credit ratings, who have similarly high debt levels.

Although we cannot rule out alternative mechanisms, they appear less consistent with our evidence. Longer-term triple difference estimates suggest the lack of borrowing declines does not reflect slow adjustment (Fama and French, 2002; Huang and Ritter, 2009).¹⁷ While issuance costs can lead to borrowing inaction (Fischer et al., 1989; Leary and Roberts, 2005; Danis et al., 2014; Jeenas, 2026), big, high-interest firms have short-term debt equal to 13% of assets in 2017, and Appendix Table C.13 shows no impact of the limitation on short-term, long-term, or total debt. Thus, substantial amounts of debt come due each year without any corresponding decline in borrowing, inconsistent with inaction. Both a leverage ratchet effect (Heider and Ljungqvist, 2015; Admati et al., 2018) and creditor evergreening (Fariae-Castro et al., 2024) predict a lack of borrowing declines among high-debt firms. However, Appendix Figure C.16 shows that we do observe debt declines among high-debt firms, just not differential declines for firms facing the limitation. Finally, future policy uncertainty or a lack of policy salience is unlikely to explain our results. As originally legislated, the 2022 tightening of the limitation to a 30% of EBIT limit went into effect, the policy clearly lowers the marginal tax benefit of debt, and we find evidence of modest interest bunching at the limit when it is feasible for firms to do so in Appendix Figure D.2.

7.2 Investment Estimates

The interest limitation lowers the marginal tax benefit of debt, and therefore raises the marginal cost of debt-financed investment. Our null investment estimates suggest that either the limitation did not have substantial impacts on the marginal cost of investment, or that investment is not as responsive to marginal cost changes as leading estimates predict.

Neoclassical investment theory frames the marginal cost of investment as the user cost of capital (UCC), arguing that when the UCC increases, investment should decline (Hall and Jorgenson, 1967). Inspired by this insight, a large body of work has estimated investment

¹⁷In Leary and Roberts (2005), firms adjust borrowing in each of the first four years after equity issuances and large valuation shocks, suggesting even slow adjustments should appear in the few years after a shock.

rate UCC elasticities, yielding an accepted range spanning $[-2, -8]$ (Chodorow-Reich, 2026).

To calculate a comparable investment rate UCC elasticity, we start with the typical Hall and Jorgenson (1967) expression for the UCC,

$$(4) \quad UCC = (\rho + \delta) \frac{1 - \tau z}{1 - \tau},$$

with financing cost ρ , depreciation rate δ , tax rate τ , and net present value of depreciation deductions z . While the original theory specifies that all financing comes from retained earnings, it has become common practice for government agencies and researchers to adapt ρ to account for both debt and equity financing (see Burnham and Carloni, 2022; Foertsch, 2022; Kennedy et al., 2026). Following this common practice, we expand $\rho = w^d(1 - \tau \mathbb{1}(Allow))r + w^e E$ into a weighted average of debt and equity financing costs with debt financing fraction w^d , interest rate r , equity financing fraction $w^e = 1 - w^d$, equity costs E , and $\mathbb{1}(Allow) = 1$ if a firm is allowed to deduct marginal interest. This presents a measurement challenge. The UCC is a marginal concept, but we do not observe the fraction of each dollar of investment that is financed with debt versus equity. In the absence of this information, the common approach is to assume that the fraction of marginal investment financed with debt is the same as the inframarginal fraction, using a firm's entire capital structure to impute marginal financing fractions.

We follow this approach to construct a measure of the UCC that is comparable with other studies, taking advantage of granular firm-by-year level data on interest rates, inframarginal debt financing fractions, and marginal tax rates (statutory tax rates for C-corporations, weighted averages of owner marginal tax rates for S-corporations, and top individual tax rates for partnerships). We complement this data with common calibrations of $\delta = 0.08$ and $E = 0.066$, while measuring z as the interaction of the current year bonus depreciation fraction with 4 digit industry average investment durations from Zwick and Mahon (2017).

To calculate investment rate UCC elasticities using our event study and triple difference estimates, we take the ratio of the percent change in investment rates (our estimates of β_{post} or γ_{post} divided by the average investment rate among treatment firms in the pre-reform period $\overline{I/K}_{pre}^T$) to the percent change in the UCC (the difference in the percent changes in

the UCC for treatment and control firms.) Specifically, we define

$$(5) \quad \varepsilon = \frac{\beta_{post} \text{ or } \gamma_{post}}{I/K_{pre}^T} \bigg/ \left(\frac{\Delta UCC^T}{UCC_{pre}^T} - \frac{\Delta UCC^C}{UCC_{pre}^C} \right).$$

To capture potential responses to the interest limitation that might bring firms below the limitation, and for consistency with other work, our primary UCC measure yields an intent-to-treat (ITT) elasticity by mechanically assigning $\mathbb{1}(Allow) = 0$ to every treatment firm after the limitation is in effect, so that post-reform UCC financing terms for treatment firms are $\rho = (w^d r + w^e E)$ and do not include interest deductions. Our measurement of the UCC takes into account other TCJA changes to tax rates and expensing, so that our elasticities can be interpreted as taking into account the broader set of TCJA tax reforms. When we use the average inframarginal financing fraction of firms in our data to calculate UCCs, our event study (triple difference) design yields an investment rate UCC elasticity of 0.15 (0.06) with a 95% confidence interval spanning $[-0.34, 0.64]$ ($[-0.51, 0.63]$), rejecting existing estimates in the literature by a factor of 6 (4).

Figure 7 shows that our investment rate UCC elasticity estimates reject others from the literature when the debt share of marginal financing is non-trivial. The right portion of the figure displays comparable elasticity estimates from the literature that allow output to vary. They are clustered in a range from $[-2, -8]$. These larger elasticity estimates arise from tax rate or depreciation variation in the tax term of the UCC $\frac{1-\tau z}{1-\tau}$ that changes the marginal cost of every dollar of investment. In the left portion of the figure, we scale our estimates of the impacts of the limitation, which only changes the marginal cost of debt-financed investment, using different UCC constructions by varying the assumed debt-financing fraction in increments of 0.02 in each direction (constraining fractions to $[0, 1]$). This yields a range of elasticity estimates and 95% confidence intervals depending on the assumed fraction of marginal financing coming from debt. Our event study (triple difference) estimates cannot reject an elasticity of -2 only when assumed debt-financing fractions fall below 13% (15%). Thus, our small elasticity estimates need not indicate that the limitation does not change the tax benefit of debt, or that businesses are unusually unresponsive to marginal cost of investment changes after TCJA. Indeed, we show above that the limitation meaningfully

changes the marginal tax benefit of debt, and TCJA reforms altering the marginal cost of every dollar of investment had significant impacts on investment (Chodorow-Reich et al., 2026; Kennedy et al., 2026). Rather, our elasticity estimates can be consistent with firms financing at most small shares of marginal investment with debt.

Our null borrowing estimates strongly support this explanation. Incremental borrowing appears prohibitively expensive for firms affected by the limitation, suggesting these firms are unlikely to finance marginal investment projects with debt, preferring cheaper alternatives. Moreover, empirical and survey evidence points to U.S. firms investing out of retained earnings (Yagan, 2015; Sharpe and Suarez, 2021). If firms finance at most small shares of marginal investment with debt, the limitation will not have a substantial impact on the true marginal cost of investment.

In Appendix F, we provide additional details regarding the construction of the UCC and marginal tax rates, explore the sensitivity of our elasticity estimates to different constructions of the UCC, show that accounting for interest limitation carryforwards leads to similar conclusions, and report treatment-on-the-treated (TOT) elasticities using a measure of the UCC that depends on whether firms have interest disallowed *ex post*.

Firms not using debt to finance marginal investment projects is not the only possible explanation for our small elasticity estimates, but other potential explanations appear less consistent with our data and setting. Our estimates over shorter and longer horizons suggest slow adjustment does not explain our results, whether it arises from frictions like convex or non-convex adjustment costs (Doms and Dunne, 1998; Cooper and Haltiwanger, 2006; Winberry, 2021; Chen et al., 2023), partial irreversibility (Abel and Eberly, 1994), or sticky discount rates (Gormsen and Huber, 2025).¹⁸ If the lack of investment responses were driven by high hurdle rates (Graham and Harvey, 2001), then other UCC changes would also not have significant impacts on investment, including TCJA reforms that altered the after-tax

¹⁸Time to build estimates typically range from 1-3 years (Mayer, 1960; Del Boca et al., 2010), much shorter than the six year triple difference time horizon. While we cannot explicitly rule out the possibility that interest limitation driven cost of capital changes do not transmit to discount rates, this idea conflicts with evidence in Gormsen and Huber (2025) showing that cost of capital changes transmit to discount rates at a rate of roughly 0.35 over a 5-6 year horizon, though with a confidence interval admitting rates below .2 and above .5. Even partial transmission of cost of capital shocks to discount rates is difficult to reconcile with our results. Our triple difference estimates only cannot reject an elasticity of -2 if we inflate them by a factor of roughly 4 to reflect 25% transmission over a six year horizon.

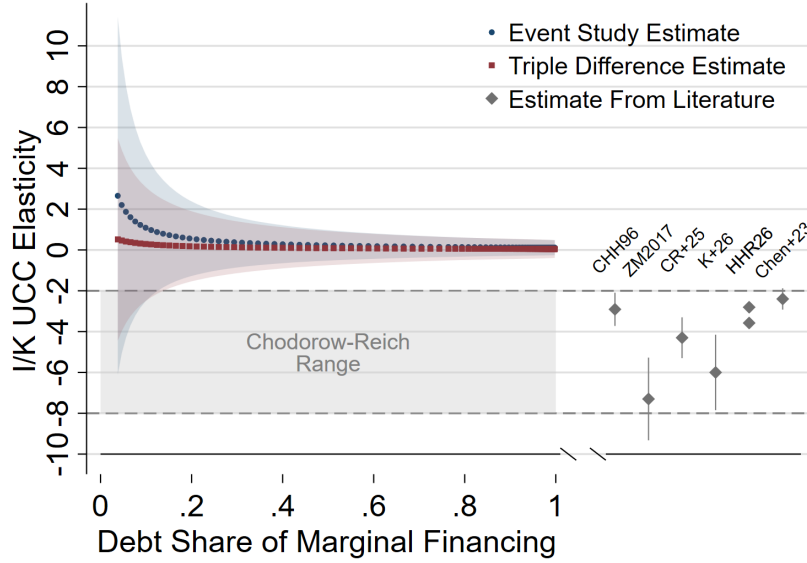


Figure 7: Investment Rate User Cost of Capital Elasticities

Notes: This figure compares investment rate user cost of capital elasticity estimates across settings. The left side of the figure plots investment rate UCC elasticities following equation (5), varying the assumed fraction of marginal financing that comes from debt in 0.02 increments up to 44 times in each direction (bounded by 0 and 1). We omit estimates when shares approach zero as confidence intervals widen exponentially and are undefined at exactly zero. 95% confidence intervals are calculated from standard errors clustered at the firm level, and represented by the shaded areas surrounding point estimates. The right side of the figure displays a range of investment rate UCC elasticity estimates from other papers in the literature (Cummins et al., 1996; Zwick and Mahon, 2017; Chen et al., 2023; Chodorow-Reich et al., 2026; Hartley et al., 2026; Kennedy et al., 2026). We obtain elasticities by dividing semi-elasticities reported in Chodorow-Reich (2026) Table 2 by average investment rates reported in each paper, with a few exceptions. Chen et al. (2023) report an elasticity directly. We divide the Chodorow-Reich et al. (2026) semi-elasticity estimate by the average investment rate of the big C-corporations in our data. Hartley et al. (2026) report an average estimate from an asset-level specification without a standard error. Rescaling their average estimate by a range of reasonable stable system eigenvalues as suggested in Chodorow-Reich (2026) yields the range of estimates between the two plotted points of -2.8 to -3.6.

value of every dollar of investment (Chodorow-Reich et al., 2026; Kennedy et al., 2026). Heterogeneity analysis in sections 6 and 8 shows null investment responses for higher liquidity firms, and for firms with leverage ratios from below 30% to above 60%, inconsistent with debt overhang (Myers, 1977) driven null responses. Further heterogeneity in Appendix D shows null investment impacts among firms losing high fractions of their interest deductions, inconsistent with null responses driven by the use of firm-wide rather than project specific

costs of capital (Bierman, 1993; Graham and Harvey, 2001; Kruger et al., 2015). Finally, we find no evidence that firms are financing non-investment expenditures with debt. Consistent with steeply upward sloping marginal costs that make debt-financing any marginal expenditures unattractive, we test for impacts of the limitation on shareholder payouts, payroll, and executive compensation in Appendix D, finding little evidence consistent with the limitation leading to economically significant declines.

Summary: Although we cannot completely rule out frictions or behavioral mechanisms, our evidence is more consistent with a simple explanation for our borrowing and investment results. The interest limitation leads to substantial declines in the marginal tax benefit of debt, but little if any decline in interest, leverage, or debt issuance. These facts are easily rationalized if big, high-interest firms facing the limitation are on steeply upward sloping regions of their marginal cost curves, consistent with their high debt levels, binding loan covenant thresholds, the possibility of creditor controls, and evidence from other highly indebted firms. This dynamic helps explain our investment results. Consistent with a pecking order (Myers and Majluf, 1984), when additional borrowing is particularly costly, affected firms will finance at most small fractions of new investment with debt, so a limitation will have little, if any, impact on the true marginal cost of investment.

8 External Validity

Our RD, event study, and triple difference designs suggest the interest limitation has economically small, statistically insignificant effects on investment, leverage, debt issuance, and equity issuance. Numerous robustness checks suggest these results possess a high degree of internal validity, and the consistency of estimates across designs indicates the responses do not vary much by firm size. Nevertheless, some questions of external validity remain. Namely, would we continue to observe little effect on investment and financing behavior if an interest limitation i) applied to lower interest firms, ii) disallowed a larger fraction of interest for high-interest firms, or iii) was implemented independently of the other simultaneous changes to business taxation made by the TCJA?

To develop evidence on these questions, inspired by Yagan (2015), we divide high-interest

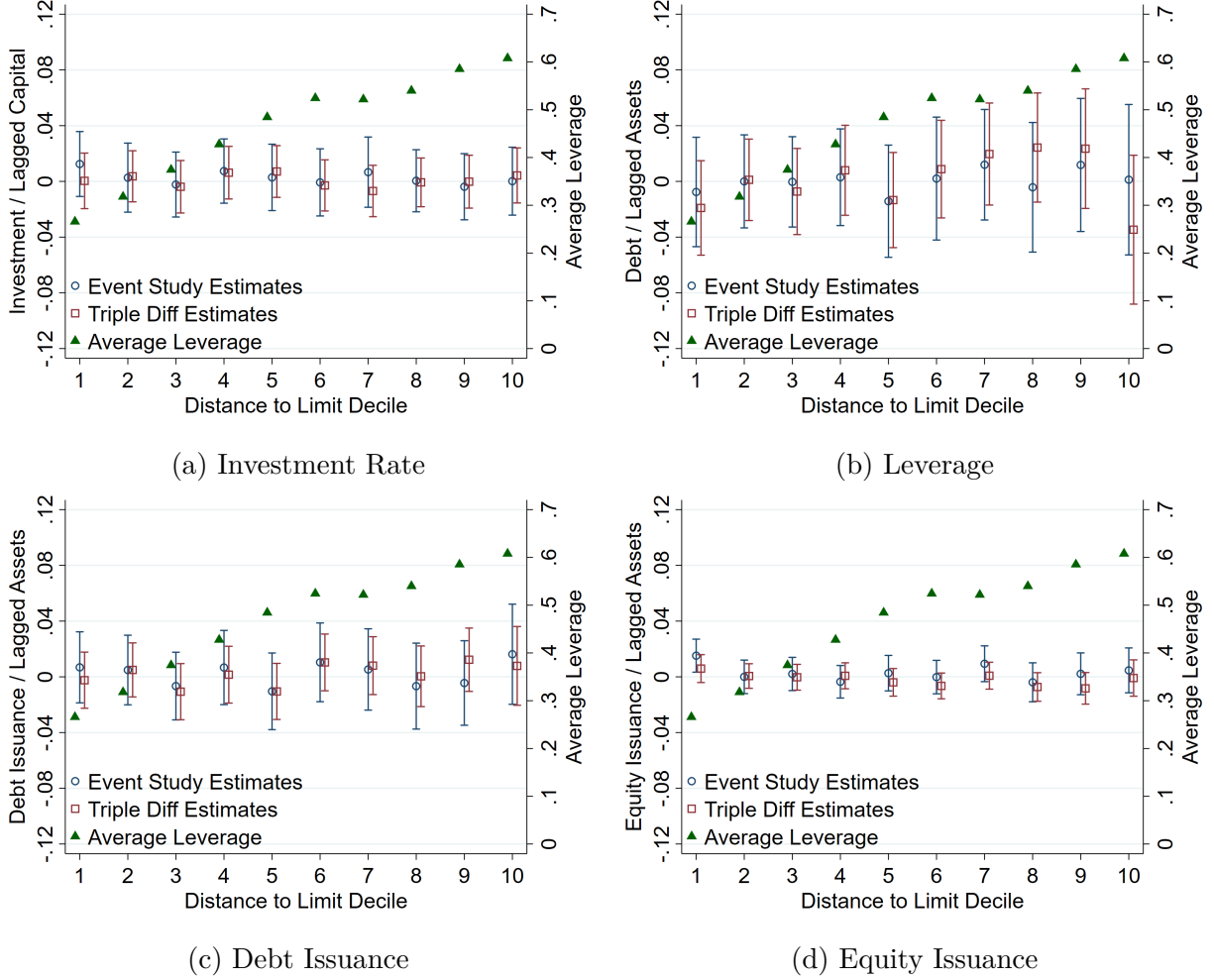


Figure 8: Event Study and Triple Difference External Validity

Notes: This figure plots event study estimates of β_{post} from equation (2), and triple difference estimates of γ_{post} from equation (3), replacing each individual post-reform year indicator and interaction with a single post-reform indicator and interaction. Each estimate comes from a separate regression including only high interest firms in the relevant decile of the degree to which firms interest exceeds their limitation averaging over 2015-2017. The left y-axes corresponds to coefficient estimates, while the right y-axes correspond to average leverage ratios in each decile across all sample years. Panel (a) uses investment scaled by lagged capital as an outcome, panel (b) uses debt scaled by lagged assets as an outcome, and panels (c) and (d) use debt and equity issuance scaled by lagged assets as outcome variables. 95% confidence intervals are constructed from standard errors clustered at the firm level, with Bonferroni corrections for multiple (ten) hypothesis testing.

firms into deciles by the degree to which their interest exceeds their limit averaging over 2015-2017, and re-estimate our event study and triple difference designs, running a separate regression while restricting to high-interest firms in each decile. Figure 8 plots these estimates, where the left y-axis corresponds to coefficient estimates and the right y-axis corresponds

to firms’ average leverage in each high-interest firm decile. Despite substantial variation in capital structures across deciles, essentially all of the coefficient estimates cannot reject zero and there is no trend in estimates across deciles. In sum, we find little evidence suggesting firms would decrease investment or borrowing by more if the limitation was tightened.¹⁹

To strengthen the evidence on the external validity of our estimates, we consider the 2022 tightening of the earnings component of the limitation to 30% of EBIT. Focusing on this later time period, Appendix Table C.9 displays investment and financing RD estimates. The tightened limit applies to more firms and disallows more interest deductions, but we continue to find no evidence of economically significant declines in investment or borrowing. Additionally, the sample period in our triple difference design includes two years after the tightening went into effect, yet we continue to estimate small null effects among originally treated firms losing a larger share of their interest deductions. We also implement our event study and triple difference design specifically around the 2022 tightening, focusing on the relatively lower interest firms newly subjected to a limitation and displaying estimates in Appendix Table C.16. With the caveat that these designs rely in part on the COVID era, we can again rule out all but small reductions in investment and borrowing.

Despite this evidence, extrapolating our results far from current limitation levels ultimately depends on lower-debt firms’ marginal cost of debt curves and their propensity to debt-finance marginal investment, parameters that remain uncertain. The limitation we study affects high-debt firms, whereas eliminating interest deductions would also reduce the marginal tax benefit of debt for lower-debt firms. These firms may occupy flatter regions of their marginal cost curves because firms’ first dollar of debt results in little bankruptcy risk and few covenant restrictions, making debt-financed investment more attractive.²⁰ On

¹⁹Firms with the greatest exposure to the limitation do not experience larger liquidity effects because they are also less likely to pay taxes. Appendix Figure D.1 shows that, although interest disallowed effects rise monotonically across deciles of *ex ante* interest in excess of the limitation, taxes paid effects do not.

²⁰Some structural models featuring endogenous investment and financing choices predict large leverage and investment declines in counterfactuals eliminating all interest deductions (Glover et al., 2015; Ivanov et al., 2025). Consistent with classes of dynamic models where small, low-debt firms borrow to scale up while larger firms obtain sufficient scale to finance investment using retained earnings (Cooley and Quadrini, 2001), one possible explanation for the divergence between these predictions and our results is that lower-debt firms exist on shallower sloping regions of their marginal cost of debt curves and are more likely to debt-finance investment. In this case, we would expect eliminating interest deductions to lead to larger borrowing and investment declines. However, alternative explanations for the divergence between model counterfactuals and our estimates are also possible. The interest limitation only disallows some interest deductions, while

the other hand, lower-debt firms may use little debt precisely because their borrowing costs are high, implying limited debt-financing of marginal investment. Despite this uncertainty, our empirical evidence strongly suggests that modest tightening of interest limitations from current levels will not substantially reduce investment or borrowing.

Finally, the TCJA policy environment raises a separate external validity concern. Although our research designs are intended to isolate the effect of the limitation from any differential impacts of simultaneous policy changes, we evaluate the limitation while full expensing and tax cuts are strengthening incentives and freeing cash for investment among both treatment and control firms. We therefore evaluate the limitation in an environment that may differ from future settings in which policymakers consider limiting interest deductions. However, this concern is mitigated for several reasons. First, full expensing is the policy-relevant setting for evaluating an interest limitation, as restrictions on interest deductions are often considered alongside full expensing to move business taxation toward a cash flow tax (Batchelder, 2026). Second, Appendix Table C.14 shows similar results when excluding industries receiving the largest full-expensing benefits or restricting the sample to firms receiving the largest or smallest TCJA benefits (C-corporations with above-median expensing exposure and passthroughs with below-median exposure). Third, as discussed earlier in this section, Appendix Tables C.9 and C.16 show that, despite no simultaneous tax cuts, losing interest deductions due to the 2022 tightening of the limitation did not significantly reduce investment or borrowing.²¹

9 Conclusion

In this paper we use tax data to estimate firm responses to the first broad interest limitation in modern U.S. history. Using regression discontinuity, event study, and triple difference

structural models do not incorporate a realistic tax loss system that mitigates the liquidity impacts of the interest limitation for high-debt firms (see Appendix D), and assume that interest deductions are taken when borrowing occurs rather than in future periods, overvaluing interest deductions by at least a discount factor.

²¹The few tax provisions that were changing during this period actually reduced, rather than increased, investment incentives and liquidity, which if anything, broadens the external validity of our findings. Specifically, mandatory amortization of Research and Experimental expenses began in 2022, increasing the cost of capital for R&D and raising tax burdens, while TCJA’s full expensing began phasing out after 2022, with bonus depreciation dropping from 100% in 2022 to 80% in 2023.

research designs, we find that the TCJA’s interest limitation has economically small and statistically insignificant impacts on firm investment and financing from 2018 through 2023. These results suggest that the TCJA limitation raised tax revenue without causing substantial investment or financing distortions, consistent with big, high-interest firms existing on steeply upward sloping regions of their marginal cost of debt curves, and not using debt to finance new investment. While external validity to much lower interest firms remains uncertain, heterogeneity analysis suggests some tightening of the interest limitation from current levels is unlikely to cause large declines in investment or borrowing.

Finally, this paper brings new data to old questions about firm capital structure choices. Debates over the conditions under which capital structure choices are irrelevant for firm value have spawned many theories of firm borrowing choices, often centered around a trade-off between tax benefits and costs of debt (Modigliani and Miller, 1958, 1963; Miller, 1977; DeAngelo and Masulis, 1980). The results in this paper do not imply that taxes are irrelevant for capital structure decisions. Instead, in a world with taxes and frictions, our results raise questions about the ways modern firms do use debt and about constraints on firms leverage capacity that are fruitful areas for further research.

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A Tax Return Line Item Variable Definitions

Table A.1: Key Variable Definitions in Terms of Tax Return Line Items

Variable	C corps	S corps	Partnerships
Investment	sum of Form 4562 lines 9, 14, 19 (columns a-i), 20 (columns a-d), and 25 (column h)		
Debt	Schedule L, lines 17 and 20		Schedule L, line 16 and 19b
Loans from shareholders	Schedule L, line 19		Schedule L, line 19a
Equity Issuance	Δ (sum of Schedule L, lines 22b and 23)	Δ (sum of Schedule L, lines 22 and 23)	Δ Schedule L, line 21
Cash	sum of Schedule L, lines 1, 4, 5, and 6		
Assets	Schedule L, line 15		Schedule L, line 14
Capital	Schedule L, line 10a		Schedule L, line 9a
Interest disallowed	Form 8990, line 31		
Interest deductions	Front page, line 18	Front page, line 13 plus Form 8825, line 9	Front page, line 15 plus Form 8825, line 9
Public flag	Schedule M-3, part I, line 3a	N/A	

Payroll	Front page, line 12 plus line 13 plus Form 1125-A line 3	Front page, line 7 plus line 8 plus Form 1125-A line 3	Front page, line 9 plus line 10 plus Form 1125-A line 3
Executive compensation	Front page, line 12	Front page, line 7	Front page, line 10
<i>Payouts</i>			
Buybacks	Max(0, Δ Schedule L line 27)	N/A	
plus Dividends,	Schedule M- 2, line 5a plus 5c	Max(0, Sched K line 16d) plus Max(0, Sched K line 17c)	Schedule M- 2, line 6a plus 6b
<i>Adjusted taxable income</i>			
Net income,	Front page, line 28	Schedule K, line 18	Analysis of Net Income (Loss), line 1
plus interest deductions,	Front page, line 18	Front page, line 13 plus Form 8825, line 9	Front page, line 15 plus Form 8825, line 9
minus interest income,	Front page, line 5	Schedule K, line 4	Schedule K, line 5

plus depreciation,	Front page, line 20	Front page, line 14, plus Schedule K, line 11, plus Form 8825, line 14	Front page, line 16c plus Schedule K, line 12, plus Form 8825, line 14
plus depletion,	Front page, line 21	Front page, line 15	Front page, line 17
plus amortization	Form 4562, line 44		
<i>Receipts</i>			
“Front page” gross receipts,	Front page, line 1c		
plus dividend income,	Front page, line 4	Schedule K, line 5a	Schedule K, line 6a
plus interest income,	Front page, line 5	Schedule K, line 4	Schedule K, line 5
plus gross rental income,	Front page, line 6	Form 8825, line 18a plus Schedule K, line 3a	
plus royalty income,	Front page, line 7	Schedule K, line 6	Schedule K, line 7
plus max(0, capital gains),	Front page, line 8	The sum of Schedule K, lines 7, 8a, and 9	The sum of Schedule K, lines 8, 9a, and 10
plus max(0, ordinary gains),	Front page, line 9	Front page, line 4	Front page, line 6
plus other income,	Front page, line 10	Front page, line 5 plus Schedule K, line 10	Front page, line 7 plus Schedule K, line 11

plus tax-exempt interest	Schedule K, line 9	Schedule K, line 16a	Schedule K, line 18a
<i>Profits</i>			
“Front page” gross receipts,	Front page, line 1c		
minus cost of goods sold	Front page, line 2		
minus total deductions	Front page, line 27	Front page, line 20	Front page, line 21
plus comp. to officers/partners	Front page, line 12	Front page, line 7	Front page, line 10
plus interest deductions	Front page, line 18	Front page, line 13 plus Form 8825, line 9	Front page, line 15 plus Form 8825, line 9
plus charitable contributions	Front page, line 19		
plus depreciation	Front page, line 20	Front page, line 14, plus Schedule K, line 11, plus Form 8825, line 14	Front page, line 16c plus Schedule K, line 12, plus Form 8825, line 14
plus net rental income	N/A	Form 8825, line 21	

Notes: Unless otherwise indicated, all C-corporation data comes from Form 1120, all S-corporation data comes from Form 1120S, and all partnership data comes from Form 1065. "Front page" refers to the first page of each of those forms. All Schedule L data comes from column (d) of the line indicated. All lines refer to the 2019 versions of the forms. In years prior to 2018, "profits" also adds back the domestic production activities deduction.

Taxes paid for C-corporations is line 31 on the front page of Form 1120. To measure taxes paid at the entity level for S-corporations, we calculate the tax paid by S-corporation

owners on the share of S-corporation income allocable to each owner and sum them up to the entity level (Goodman et al., 2024). We do not calculate taxes paid at the entity level for partnerships due to tiered structures that obscure the ultimate ownership of income streams (Cooper et al., 2016).

To identify members of an aggregated group, we assemble a set of parent-child links. Both the parents and children are considered aggregators. We include the following links for tax years 2017-2023:

1. Links between a parent C-corporation and its C-corporation subsidiaries reported on Form 851. In general, a subsidiary must be at least 80%-owned to be included on Form 851.
2. Links reported on Schedule K-1 of Form 1065 and Form 1120S, where the shareholder or partner is a firm (that is, an entity with an EIN) and the ownership share is at least 50%.
3. Links between a parent C-corporation and other C-corporations in which the parent has at least a 50% ownership share, as reported on Form 1120, Schedule K, line 5a.
4. Links between C-corporations and entities that have at least a 50% ownership share in that corporation, as reported on Form 1120, Schedule G, Part I.

B Additional Summary Statistics

Table B.1 provides summary statistics for additional outcome variables. Figure B.1 displays a histogram of firms' interest relative to their limitation scaled by assets averaging over 2015-2017, separately for firms with average receipts over the same time period larger and smaller than the \$25 million cutoff. While the interest limitation is high relative to average interest, there are still many big and small firms in our data with interest exceeding their limitations, providing a large sample for our event study design.

Appendix Figures B.2 and B.3 explore the distribution of debt, interest, and interest rates by industry in the post-reform period. The majority of debt is held by manufacturing, management and information firms, which also face the highest interest rates. Over 15% of firms have interest disallowed in mining, oil, and gas, manufacturing, and information.

Appendix Tables B.2 and B.3 show means and medians of the big and small and high- and low-interest firms that form the basis of our quasi-experimental design comparison groups in a 2017 cross section of the data. Appendix Table B.4 shows the distribution of assets, investment, and debt among big and small, and high- and low-interest firms. As reported in the text, big, high-interest firms and privately-held firms do substantial fractions of total investment and hold substantial fractions of all debt, emphasizing the importance of including privately-held firms in the analysis and showing that large changes in investment or borrowing for these firms could have macroeconomic implications.

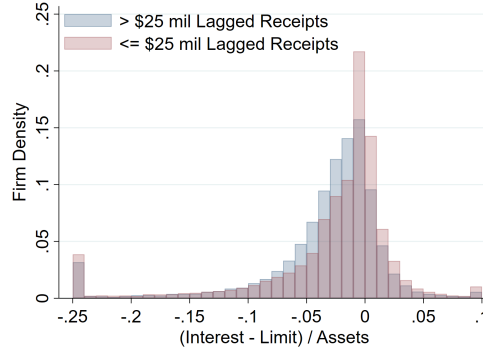


Figure B.1: Distribution of Firm Interest Relative to Limitation

Notes: This figure displays histograms of the average difference between firm's interest and their limitation, scaled by lagged assets, over 2015-2017. The blue bars correspond to large firms with average receipts over 2015-2017 exceeding \$25 million, and the red bars correspond to smaller firms with average receipts over 2015-2017 not exceeding \$25 million. Outlier observations from the tails of both distributions are stacked in the most negative and positive bars on the x-axis.

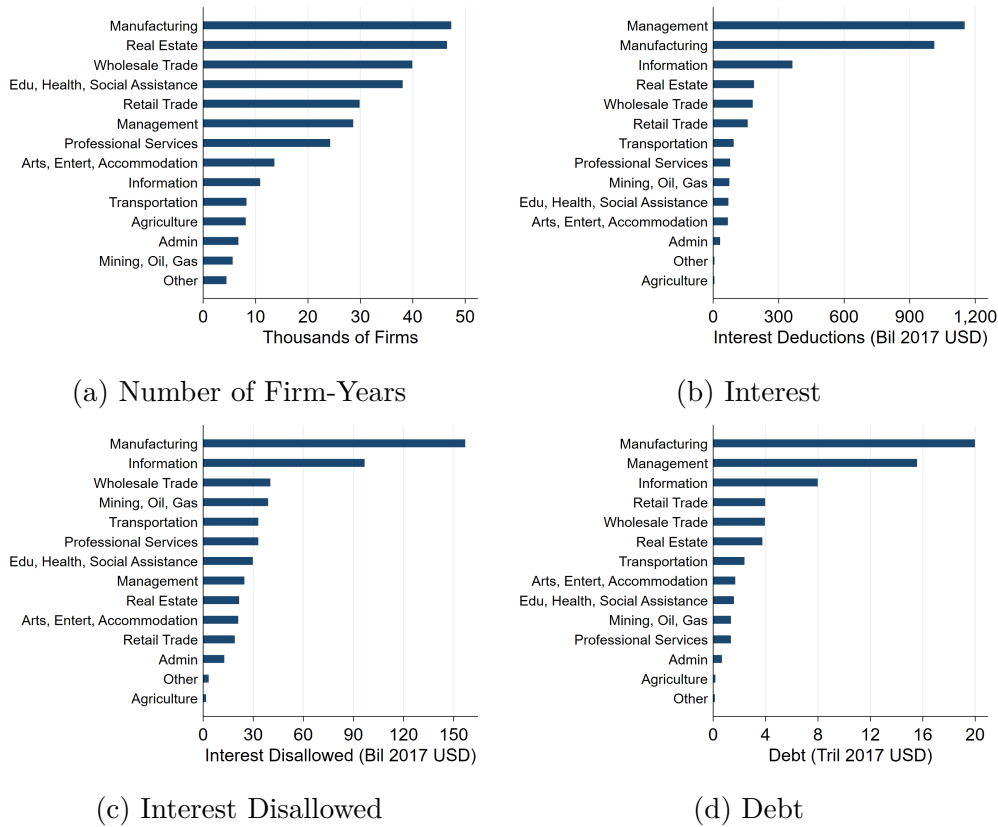


Figure B.2: Total Interest and Debt by Industry

Notes: This figure plots the total number of firm-year observations, and the total amount of interest deductions, interest, and debt in our panel data over years 2018 through 2023.

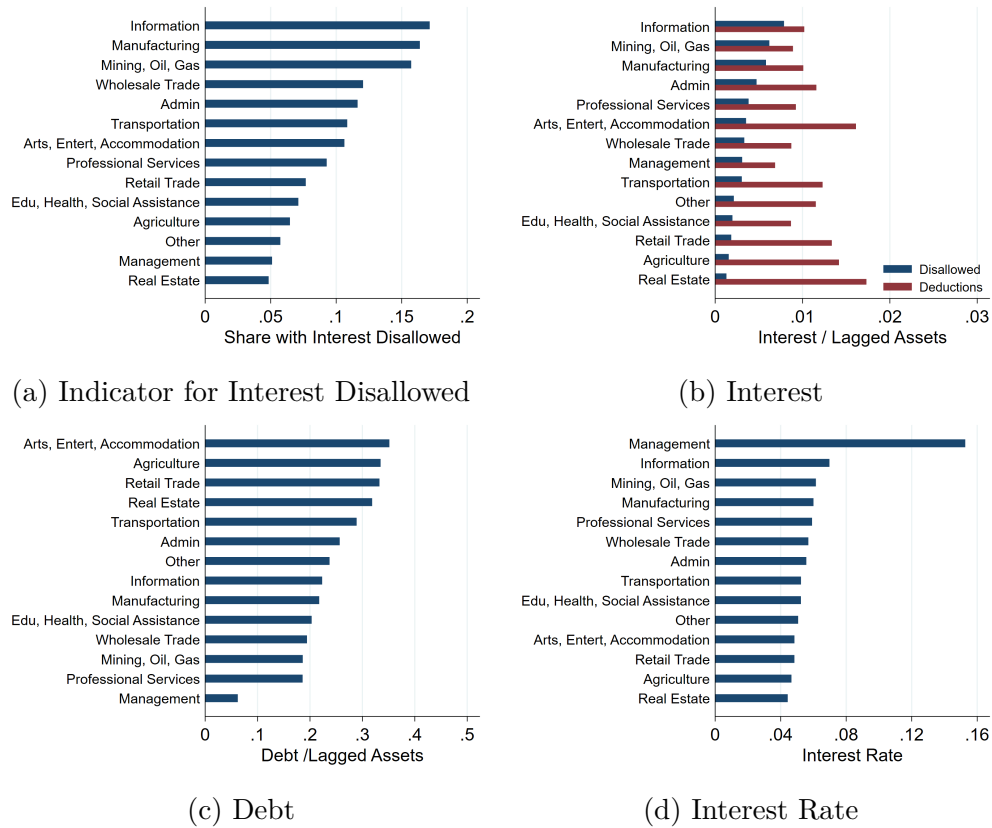


Figure B.3: Average Interest and Debt by Industry

Notes: This figure plots the fraction of firms with interest disallowed, the average amount of interest deductions, interest disallowed, and debt scaled by lagged assets, and interest rates by industry. Averages are for all firms in our panel data over years 2018 through 2023.

Table B.1: Additional Summary Statistics

	Mean	Std Dev	P10	P50	P90	Obs	Firms
Net Inv / Lagged Capital	0.043	0.141	−0.070	0.005	0.231	538,965	64,407
Inv / Lagged Net Capital	0.317	0.518	0.000	0.117	0.873	523,648	63,533
Extensive Margin Inv	0.754	0.431	0.000	1.000	1.000	588,324	69,751
Inv Spike	0.118	0.323	0.000	0.000	1.000	588,324	69,751
Debt / Lagged Financial Capital	0.328	0.421	0.000	0.131	0.940	588,163	69,746
Debt + LNS / Lagged Assets	0.289	0.358	0.000	0.124	0.849	588,324	69,751
Δ Debt + LNS / Lagged Assets	0.005	0.113	−0.108	0.000	0.130	588,324	69,751
Short Term Debt / Lagged Assets	0.068	0.157	0.000	0.000	0.241	588,324	69,751
Long Term Debt / Lagged Assets	0.174	0.285	0.000	0.012	0.622	588,324	69,751
Debt / Equity	0.800	1.630	0.000	0.104	2.354	533,922	67,168
Trade Credit / Lagged Assets	0.098	0.153	0.000	0.029	0.314	588,324	69,751
Cash / Lagged Assets	0.231	0.246	0.011	0.138	0.632	588,324	69,751
Taxes / Lagged Assets	0.021	0.039	0.000	0.003	0.065	450,389	53,267
Payroll / Lagged Assets	0.317	0.448	0.000	0.150	0.904	588,324	69,751
R&D / Lagged Assets	0.010	0.044	0.000	0.000	0.008	588,324	69,751
Advertising / Lagged Assets	0.010	0.021	0.000	0.001	0.035	460,528	53,557
Executive Compensation / Lagged Assets	0.035	0.075	0.000	0.004	0.103	588,324	69,751
Payouts / Lagged Assets	0.086	0.203	0.000	0.001	0.246	588,324	69,751
Profits / Lagged Assets	0.123	0.230	−0.075	0.060	0.433	588,324	69,751

Notes: This table reports summary statistics for our entire unbalanced panel data set spanning 2013-2023. To preserve taxpayer anonymity, percentile statistics are reported as the means of the 11 observations nearest the true percentile.

Table B.2: 2017 Means For Treatment and Control Groups

	Small		Big	
	Low Int	High Int	Low Int	High Int
Assets (Mil 2017 USD)	56.0	53.8	1,640.9	1,302.2
Capital (Mil 2017 USD)	7.3	17.1	311.2	313.3
Investment / Lagged Capital	0.08	0.07	0.11	0.11
Debt / Lagged Assets	0.17	0.47	0.22	0.47
Debt Issuance / Lagged Assets	0.00	0.01	0.01	0.01
Equity Issuance / Lagged Assets	0.01	0.02	0.01	0.01
Cash / Lagged Assets	0.27	0.17	0.21	0.13
Payouts / Lagged Assets	0.10	0.02	0.10	0.01
Age	22.0	16.2	28.3	17.7
Obs	24,513	8,822	24,523	5,911

Notes: This table reports mean values for big and small, high- and low-interest firms from the 2017 cross section of our panel data. Firms are classified as small if their average receipts over 2015-2017 do not exceed \$25 million and firms are classified as low interest if their interest does not exceed their limitation averaging over 2015-2017.

Table B.3: 2017 Medians For Treatment and Control Groups

	Small		Big	
	Low Int	High Int	Low Int	High Int
Assets (Mil 2017 USD)	5.9	11.4	70.3	116.6
Capital (Mil 2017 USD)	0.9	2.3	19.2	24.7
Investment / Lagged Capital	0.02	0.01	0.06	0.06
Debt / Lagged Assets	0.00	0.44	0.10	0.47
Debt Issuance / Lagged Assets	0.00	−0.00	0.00	0.00
Equity Issuance / Lagged Assets	−0.00	−0.00	−0.00	−0.00
Cash / Lagged Assets	0.17	0.06	0.13	0.06
Payouts / Lagged Assets	0.00	0.00	0.02	0.00
Age	18.5	11.0	22.5	11.5
Obs	24,513	8,822	24,523	5,911

Notes: This table reports median values for big and small, high- and low-interest firms from the 2017 cross section of our panel data. Firms are classified as small if their average receipts over 2015-2017 do not exceed \$25 million and firms are classified as low interest if their interest does not exceed their limitation averaging over 2015-2017. To preserve taxpayer anonymity, medians are reported as the means of the 11 observations nearest the true median.

Table B.4: Fraction of Aggregates Across Groups

	(1) Total	(2)	(3)	(4)	(5)	(6)	(7)
	Tril 2017 USD	Fraction of Total					
		Treatment and Control Groups					
		Small		Big			
		Low Int	High Int	Low Int	High Int	Public	Private
Assets	49.79	0.03	0.01	0.81	0.15	0.66	0.34
Int Deductions	0.59	0.01	0.02	0.63	0.34	0.62	0.38
Investment	0.82	0.02	0.02	0.81	0.16	0.45	0.55
Debt	11.34	0.01	0.01	0.71	0.26	0.66	0.34
Equity	13.13	0.07	0.03	0.59	0.30	0.53	0.47
Cash	7.81	0.03	0.01	0.90	0.06	0.75	0.25
Obs	63,769	0.38	0.14	0.38	0.09	0.04	0.96

Notes: This table reports aggregate statistics for treatment and control groups from the 2017 cross section of our unbalanced panel data in column 1. Columns 2-5 show the fraction of the total in each of the big and small and low- and high-interest firm groups. Columns 6 and 7 show the fraction of the total in publicly- and privately-held firms.

C Robustness and Validity of Research Designs

C.1 Evaluating the Event Study Design Through 2020

In this section, we extend our event study estimates through 2020 to explore whether COVID-19 had differential impacts on big and small firms. To perform this analysis, we reconstruct our event study sample, but add an additional post-reform year. Event study estimates of responses to the limitation in 2020 may be confounded by two factors. First, the earnings component of the limitation was loosened by the CARES Act to cap interest deductions at 50% rather than 30% of EBITDA in 2020. Second, COVID may have differentially impacted larger treatment and smaller control firms.

Figure C.1 plots estimates of β_e from equation (2) using our extended event study panel including 2020, comparing big and small high-interest firms in the high-interest series and big and small low-interest firms in the low-interest series. Panels (a) and (b) show that among high-interest firms, our event study estimates of extensive and intensive margin interest disallowed both decline in 2020 relative to 2019, consistent with the loosening of the interest limitation from 30% to 50% of EBITDA. As expected, there is very little extensive or intensive margin interest disallowed among low-interest firms.

Figure C.1 panels (c) - (f) display estimates of β_e from equation (2) for our four main outcomes: the investment rate, leverage, debt issuance, and equity issuance. Panel (c) and panel (f) show that investment rates and equity issuance trend similarly for big relative to small high-interest firms and big relative to small low-interest firms through 2020, although there is a small decline in investment rates for all big relative to all small firms in 2020. These patterns are more extreme for the leverage and debt issuance estimates in panels (d) and (e). Big, high-interest firms exhibit a clear decline in leverage relative to smaller high-interest firms in 2020. These 2020 drops are unlikely to represent a response to the limitation, as the policy was implemented two years prior and was weakened in 2020 by the CARES Act. Instead, it is plausible that COVID differentially impacted larger and smaller firms, violating the parallel trends assumption underlying the event study design.

The estimates of β_e for low-interest firms in Figure C.1 suggest that COVID differentially

impacted all large relative to small firms, not just large, high-interest firms. The leverage and debt issuance of big relative to small low-interest firms declines in 2020 by an almost identical amount when compared to big and small high-interest firms. Therefore, triple difference estimates that control for the differential impacts of COVID on big versus small firms appear more appropriate to evaluate firm responses to the interest limitation over the longer-run.

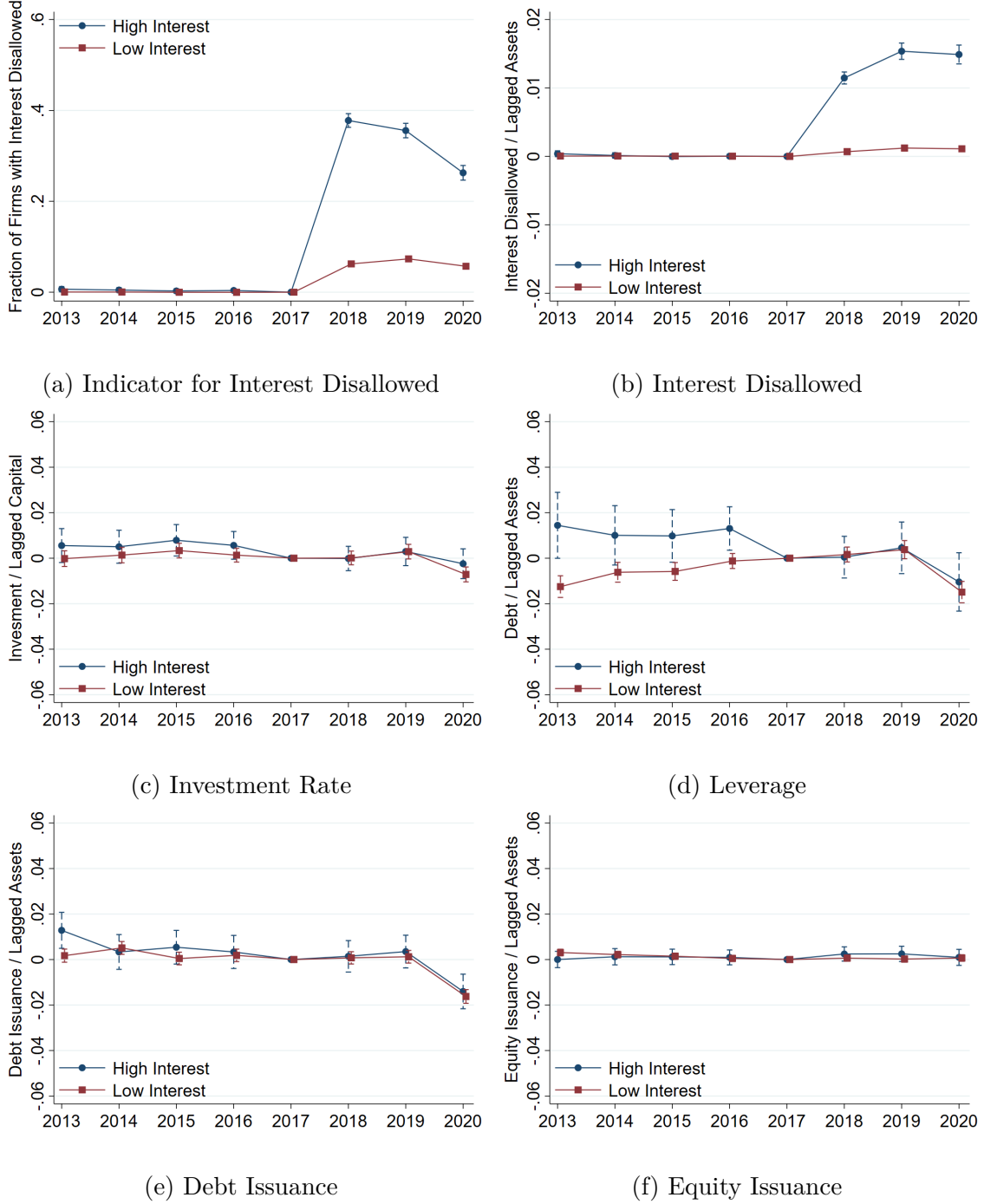


Figure C.1: Event Study Estimates Through 2020

Notes: This figure plots event study estimates of β_e from equation (2) separately for the high-interest and low-interest firms in our modified event study sample that spans 2013-2020. Panel (a) uses an indicator equal to 1 if interest is disallowed as the outcome variable, while panel (b) uses interest disallowed scaled by lagged assets as the outcome variable. Panel (c) uses investment scaled by lagged capital as an outcome variable. Panel (d) uses debt scaled by lagged assets as an outcome variable. Panel (e) uses debt issuance scaled by lagged assets as an outcome variable, and panel (f) uses equity issuance scaled by lagged assets as an outcome variable. 95% confidence intervals are constructed from standard errors clustered at the firm level.

C.2 Persistence and Treatment-on-the-Treated Estimates

Across all three research designs, we define treatment and control groups based on *ex ante* measures of which firms are big and high-interest, and then implement intent-to-treat (ITT) specifications that yield estimates of the difference in outcome trajectories of treatment and control group firms. This approach captures a broad set of potential responses among all firms that might face the limitation, including responses of firms that might reduce borrowing and investment to avoid losing interest deductions.

Our estimates of the impacts of the limitation on interest disallowed show that firms in our treatment groups disproportionately lose interest deductions, but also show our treatment definitions do not perfectly identify every firm losing interest deductions. Small firms can face the limitation if they are deemed to be a tax shelter by the IRS, or if they are forced to aggregate receipts with larger firms for the purposes of the limitation. Big, high-interest firms may also not lose interest deductions for three reasons. First, some firms defined as big or high interest from 2015-2017 do not continue to have receipts above the threshold or interest that exceeds their limitation in the post-reform period. Second, some firms with agriculture and real estate components elect out of the limitation in exchange for a less generous depreciation method. Third, some firms that have receipts above the threshold and interest above their limitation do not appear to have interest deductions disallowed.

To measure the impacts of *ex post* losing interest deductions, we implement treatment-on-the-treated versions of each quasi-experimental research design. Starting with our RD, equation (1) is the reduced form of a fuzzy RD design where Big_i is the instrument for $Disallow_i$, defined as a dummy variable for firm i having interest deductions disallowed in the post-reform period. We estimate this fuzzy RD with the following equation

$$(C.1) \quad \Delta Y_i = \psi^{IV} Disallow_i + f(z_i) + \varepsilon_i.$$

Estimates of ψ^{IV} in (C.1) are TOT estimates of the local average treatment effect of losing interest deductions for firms at the receipts cutoff.

To implement a TOT version of our event study design, we interact our time-invariant treatment definition Big_i with year dummies and use these variables as instruments for

interactions of a time-varying indicator for losing interest deductions with year dummies. Specifically, we define $Disallow_{it}$ as a time-varying indicator for having interest disallowed in years after 2017 (and being a big, high-interest firm before 2018 that would have been subject to the limitation), and estimate

$$(C.2) \quad Y_{it} = \sum_{e=2013, e \neq 2017}^{2019} \beta_e^{TOT} \mathbb{1}(t = e) \times Disallow_{it} + \delta_{jt} + \xi_i + \varepsilon_{it},$$

instrumenting for $\mathbb{1}(t = e)Disallow_{it}$ with $\mathbb{1}(t = e)Big_i$ and replacing post-reform yearly indicators with a single post-reform indicator to obtain β_{post}^{TOT} coefficients.

To implement a TOT version of our triple difference design, we define Big_{it} as a time-varying indicator for having average lagged receipts above the size cutoff in each year, HI_{it} as a time-varying indicator for firms having interest above their limitation in each year, and estimate

$$(C.3) \quad Y_{it} = \sum_{e=2013, e \neq 2017}^{2023} \left[\gamma_e^{TOT} \mathbb{1}(t = e) \times Disallow_{it} + \phi_e \mathbb{1}(t = e) \times Big_{it} + \Omega_e \mathbb{1}(t = e) \times HI_{it} \right] + \delta_{jt} + \xi_i + \varepsilon_{it},$$

instrumenting for $\mathbb{1}(t = e)Disallow_{it}$ with $\mathbb{1}(t = e) \times Big_i \times HI_i$, for $\mathbb{1}(t = e)Big_{it}$ with $\mathbb{1}(t = e)Big_i$, and for $\mathbb{1}(t = e)HI_{it}$ with $\mathbb{1}(t = e)HI_i$.

We display the estimates from the TOT implementations of each research design in Table C.1. Inflating our estimates to account for imperfect treatment persistence yields less precision, but when we scale our estimates into investment rate user cost of capital elasticities in Appendix F, Figure F.2, we continue to reject conventional elasticity estimate ranges.

We display the relative importance of the key factors leading to a lack of treatment persistence in the context of our event study in Figure C.2. Just over 70% of treatment firms are big and high interest each year from 2015-2017. In 2018, 65.6% of treatment firms are still big and high interest, 6.8% of treatment firms elect out, and 9.6% of treatment firms continue to be big and high interest, do not elect out, but still do not have any interest disallowed.²² Furthermore, 9.1% of control firms have interest disallowed. To explore whether

²²Part of the latter group can be explained by firms that should file Form 8990 not filing the form. For

alternative treatment definitions or sample restrictions aimed at increasing persistence lead to difference results, Table C.2 displays average-post-reform event study estimates while defining firms as high interest if their interest is higher than their limitation averaging, or in every year, over pre-reform periods of 1-5 years, and while excluding firms that elect out of the limitation. First stage and investment and financing estimates are stable across the vast majority of different treatment definitions on the full sample, and the first stage strengthens but impacts on investment and financing remain similar after excluding firms that elect out.

Expanding our exploration of alternative sample restrictions aimed at increasing treatment persistence, Figures C.3 and C.4 display estimates of β_e and γ_e while restricting to firms in 3-digit industries with above 25th and 50th percentile distance to limitation $(\text{Interest}_t - \text{Limitation}_t)/\text{Assets}_t$ autocorrelations, and with below 75th and 50th percentile pre-reform total interest variance. We observe very little variation in estimates of the first stage or impacts on investment and financing across any restricted sample.

In summary, while the *ex ante* treatment definitions in our research designs do not perfectly predict interest disallowed in the post-reform period, there is a substantial increase in the number of firms with interest disallowed and the amount of interest disallowed for our treatment relative to control groups, and modifying treatment definitions or samples to increase persistence yields similar results that continue to rule out economically large responses to the limitation.

example, only 94% of C- and S-corporations with an interest carryforward in 2019 that exist in the data in 2018 also file an 8990 in 2018. Conversations with IRS professionals involved in the construction of the SOI samples suggest that the missing data stems from taxpayer confusion over Form 8990 only being filed as a pdf attachment, especially in 2018, the first year firms were required to file the form.

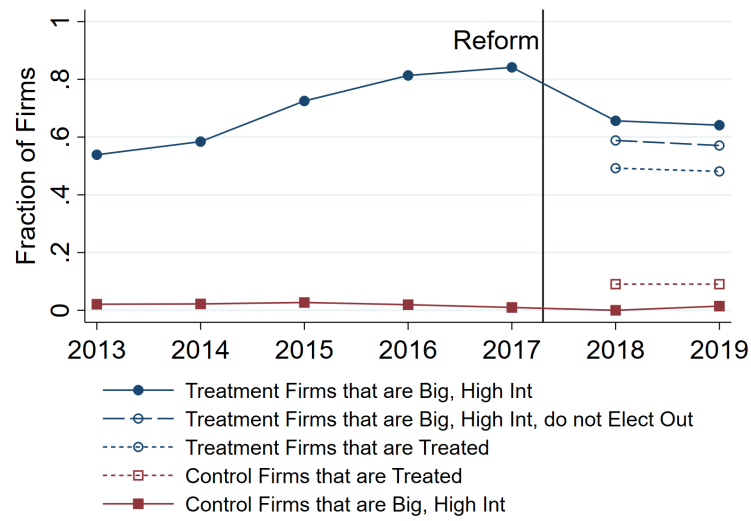
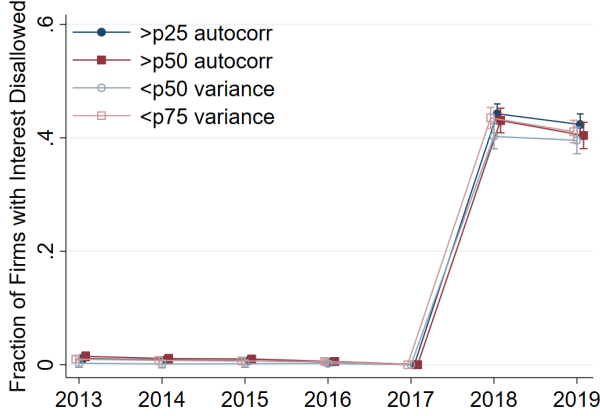
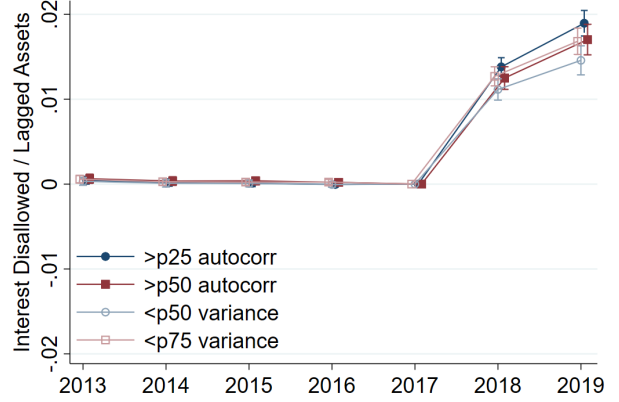


Figure C.2: Persistence of Treatment Status

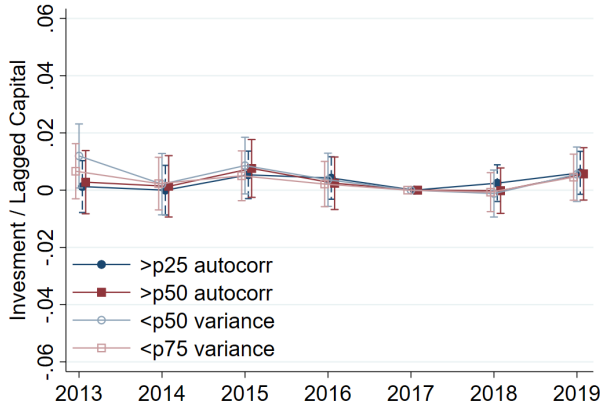
Notes: This figure plots the fraction of firms classified as big and high-interest based on 2015-2017 that have interest above their limitation, receipts above the relevant size cutoff, and interest disallowed in each year.



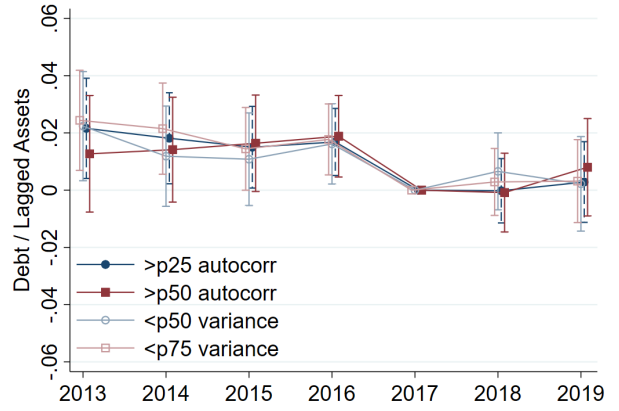
(a) Any Interest Disallowed



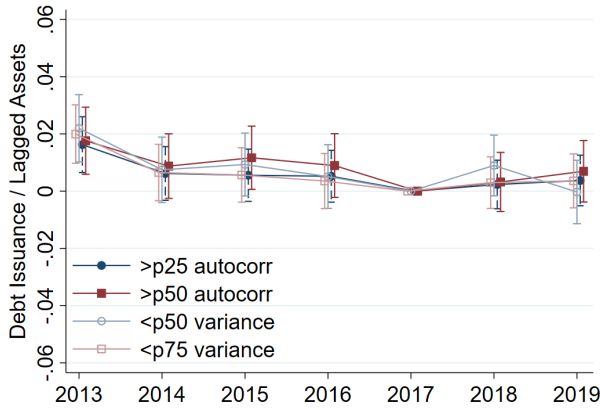
(b) Interest Disallowed



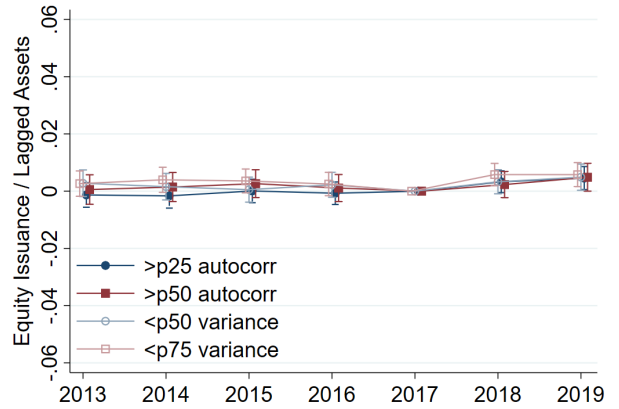
(c) Investment Rate



(d) Leverage



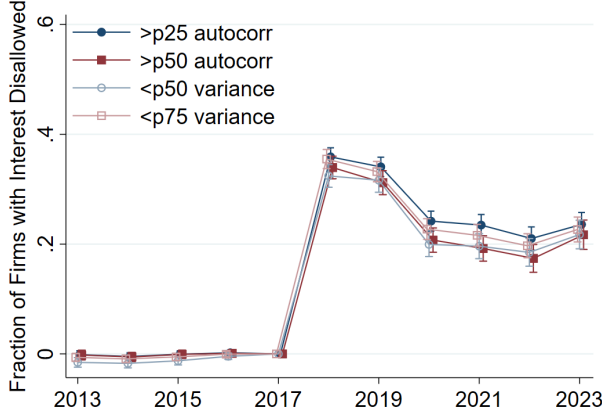
(e) Debt Issuance



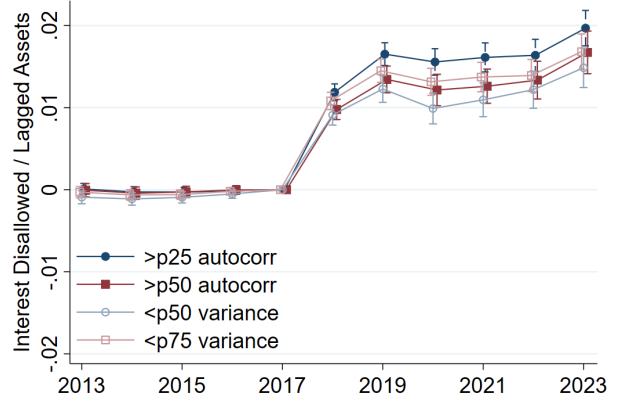
(f) Equity Issuance

Figure C.3: Event Study Persistence Subsamples

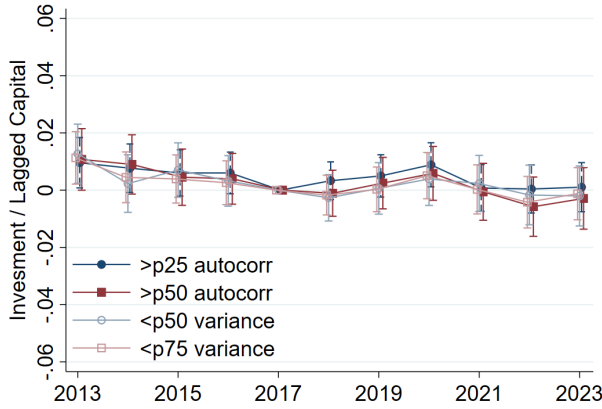
Notes: This figure plots event estimates of β_e from equation (2) while restricting the sample to firms with above 25th and 50th percentile pre-reform distance to interest limitation autocorrelation, and firms with below 75th and 50th percentile pre-reform total interest variance. 95% confidence intervals are constructed from standard errors clustered at the firm level.



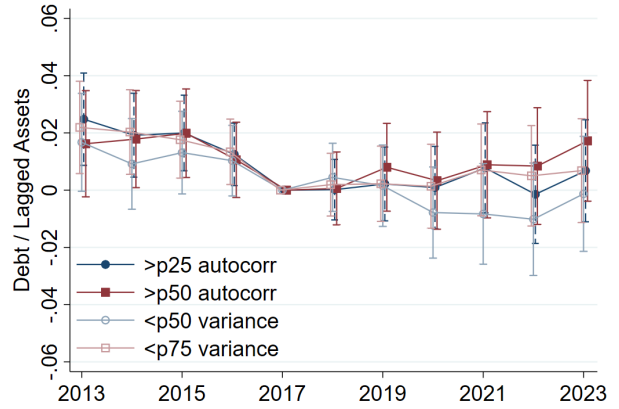
(a) Any Interest Disallowed



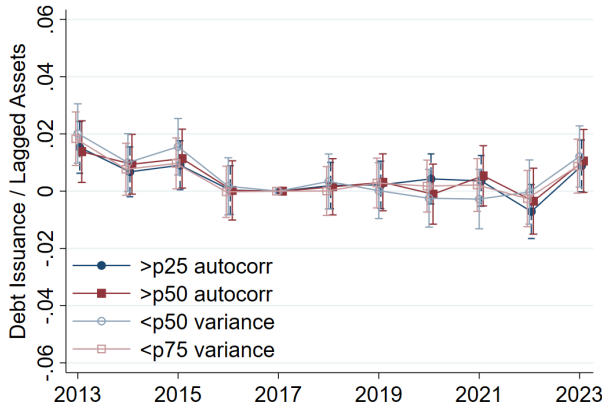
(b) Interest Disallowed



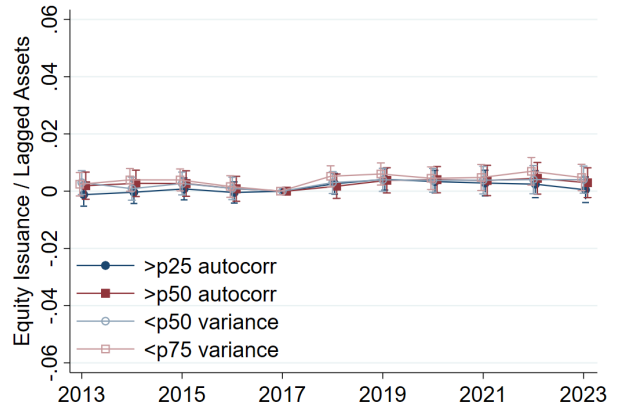
(c) Investment Rate



(d) Leverage



(e) Debt Issuance



(f) Equity Issuance

Figure C.4: Triple Difference Persistence Subsamples

Notes: This figure plots event estimates of γ_e from equation (3) while restricting the sample to firms with above 25th and 50th percentile pre-reform distance to interest limitation autocorrelation, and firms with below 75th and 50th percentile pre-reform total interest variance. 95% confidence intervals are constructed from standard errors clustered at the firm level.

Table C.1: TOT Effects of Interest Limitation on Investment and Financing

Outcome	(1) Investment Rate	(2) Leverage	(3) Debt Issuance	(4) Equity Issuance
Panel A: Regression Discontinuity Estimates				
ψ^{IV}	-0.0455 (0.0938)	0.0377 (0.1766)	-0.0466 (0.1119)	-0.0258 (0.0416)
Obs	715	734	734	734
First Stage F-Stat	6.6557	7.0773	7.0773	7.0773
Pre-Reform Mean	0.0930	0.5227	0.0179	0.0163
Panel B: Event Study Estimates				
β_{post}^{TOT}	0.0041 (0.0068)	0.0054 (0.0123)	0.0057 (0.0084)	0.0071 (0.0039)
Obs	79,150	84,737	84,737	84,737
Clusters	14,243	15,274	15,274	15,274
Pre-Reform Mean	0.1178	0.4704	0.0256	0.0169
Panel C: Triple Difference Estimates				
γ_{post}^{TOT}	0.0012 (0.0078)	-0.0010 (0.0154)	0.0018 (0.0089)	-0.0054 (0.0042)
Obs	538,576	588,318	588,318	588,318
Clusters	64,024	69,751	69,751	69,751
Pre-Reform Mean	0.1177	0.4694	0.0255	0.0170

Notes: This table reports estimates from our regression discontinuity, event study, and triple difference research designs. In Panel A, we display estimates of ψ^{IV} from equation (C.1). In Panel B, we display estimates of β_{post}^{TOT} from equation (C.2), replacing post-reform indicators and interactions with a single post-reform indicator and interaction. In Panel C, we display estimates of γ_{post}^{TOT} from equation (C.3), replacing post-reform indicators and interactions with a single post-reform indicator and interaction. The estimation samples vary across designs as described in the text. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for treatment firms in the sample for each design before 2018.

Table C.2: Event Study Robustness Varying Treatment Definitions

	Int > Limit Avging Over Years					Int > Limit Every Year				
	(1) Has Int Dis.	(2) Inv. Rate	(3) Leverage	(4) Debt Iss.	(5) Eq. Iss.	(6) Has Int Dis.	(7) Inv. Rate	(8) Leverage	(9) Debt Iss.	(10) Eq. Iss.
Panel A: β_{post} Estimates, Baseline Sample										
1 Year	0.358 (0.007)	-0.002 (0.002)	-0.001 (0.004)	0.000 (0.003)	0.002 (0.001)	0.358 (0.007)	-0.002 (0.002)	-0.001 (0.004)	0.000 (0.003)	0.002 (0.001)
2 Years	0.367 (0.007)	-0.002 (0.003)	-0.000 (0.004)	0.001 (0.003)	0.002 (0.001)	0.384 (0.008)	-0.003 (0.003)	0.001 (0.005)	-0.001 (0.003)	0.003 (0.002)
3 Years	0.365 (0.007)	-0.001 (0.003)	0.000 (0.005)	0.001 (0.003)	0.002 (0.001)	0.383 (0.008)	0.000 (0.003)	0.002 (0.006)	-0.001 (0.004)	0.003 (0.002)
4 Years	0.362 (0.007)	-0.001 (0.003)	0.001 (0.005)	0.001 (0.003)	0.002 (0.001)	0.384 (0.009)	0.000 (0.003)	0.003 (0.006)	-0.002 (0.004)	0.004 (0.002)
5 Years	0.357 (0.007)	-0.000 (0.003)	0.003 (0.005)	0.003 (0.003)	0.002 (0.001)	0.380 (0.009)	-0.001 (0.004)	0.001 (0.006)	-0.002 (0.004)	0.003 (0.002)
Panel B: β_{post} Estimates, Drop Electing Out Firms										
1 Year	0.420 (0.007)	-0.001 (0.003)	-0.003 (0.005)	0.000 (0.003)	0.002 (0.001)	0.420 (0.007)	-0.001 (0.003)	-0.003 (0.005)	0.000 (0.003)	0.002 (0.001)
2 Years	0.432 (0.007)	-0.000 (0.003)	-0.001 (0.005)	0.002 (0.003)	0.003 (0.002)	0.465 (0.008)	-0.001 (0.003)	0.000 (0.006)	0.000 (0.004)	0.004 (0.002)
3 Years	0.433 (0.007)	0.001 (0.003)	-0.001 (0.005)	0.001 (0.003)	0.003 (0.002)	0.472 (0.009)	0.002 (0.004)	0.002 (0.006)	-0.000 (0.004)	0.004 (0.002)
4 Years	0.432 (0.008)	0.000 (0.003)	0.000 (0.005)	0.002 (0.004)	0.003 (0.002)	0.484 (0.010)	0.002 (0.004)	0.002 (0.007)	-0.002 (0.005)	0.006 (0.002)
5 Years	0.428 (0.008)	0.002 (0.003)	0.001 (0.005)	0.004 (0.004)	0.003 (0.002)	0.485 (0.010)	0.001 (0.004)	0.000 (0.007)	-0.001 (0.005)	0.005 (0.002)

Notes: This table reports event study estimates of β_{post} from equation (2), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. Each row corresponds to a different treatment definition requiring interest to exceed the firm's limitation on average (in columns 1–5) or in every year (in columns 6–10) over the N years prior to the reform, with N varying by row. Panel A uses all high-interest firms from our panel data based on the relevant treatment definition. Panel B drops firms that elect out of the interest limitation. Standard errors are clustered at the firm level and reported in parentheses.

C.3 Regression Discontinuity Robustness

In this appendix we perform numerous robustness checks for our RD design. We begin with robustness checks using the SOI data that we use across all three quasi-experimental designs. To validate the baseline RD, we evaluate the assumption that firms did not exactly manipulate their past receipts around the \$25 million cutoff. If firms were able to manipulate their receipts, we would observe bunching at the \$25 million cutoff, but we see none. Figure C.5 displays a density plot of firms by their receipts. A McCrary test suggests there is no discontinuity in the distribution of lagged receipts around the cutoff.

Next, we address potential bias in our RD estimates from a simultaneous policy change that corresponds specifically to the \$25 million lagged receipts cutoff. Before the TCJA, most businesses were required to use accrual rather than cash accounting for tax purposes, recording expenses when commitments were made for the exchange of goods and services. After TCJA, firms below the receipts cutoff were allowed to switch to cash accounting, which may provide short-term tax savings if small firms defer taxable income to when cash commitments for goods and services are actually exchanged. To check whether the change to cash accounting biases our RD estimates, we perform placebo RD estimates for low-interest firms. Appendix Table C.3 displays estimates of ψ from equation (1) using all low-interest firms. We cannot reject zero impact of the opportunity to change from accrual to cash accounting on the investment rate, leverage, debt issuance, or equity issuance, suggesting the option to switch to cash accounting does not bias our RD estimates.

In addition, Figure C.6 presents estimates of ψ from equation (1) varying the bandwidth and polynomial degree, while Table C.4 present estimates of equation (1) including potential aggregators in the sample. Our RD results are not sensitive to these choices.

C.3.1 RD ESTIMATES USING E-FILING DATA

Regression discontinuity designs require many observations close to the relevant policy cutoff. The corporate and partnership SOI data sets that we use throughout this paper are stratified random samples that do not include all firms close to the \$25 million receipts cutoff. In an effort to obtain more precise RD estimates, we develop additional data utilizing electronically

filed business tax returns covering the universe of electronic filers rather than a sample.

Using E-filing records comes with substantial trade-offs. First, the E-filing records do not include many data fields present in the SOI samples, including 2018 Form 8990 information on interest disallowed. Second, IRS staff do substantial manual editing of tax returns to improve data quality in the SOI sample, while the E-filing records do not undergo similar processing.²³ Corrections in the SOI data lead to differences in some of our outcome variables between the SOI and E-filing data. Given these issues, we view RD estimates using E-filing data as a useful complement to estimates based on the SOI data, not a replacement.

Table C.5 displays summary statistics for our four key outcome variables for all firms within \$12 of the \$25 million gross receipts cutoff in both data sets. The E-filing data contains ten times as many firms in this range as the SOI data, but the variance of all four outcome variables is higher in the E-filing data.

Our approach to the RD in the E-filing data is identical to our approach in the SOI data, except that we do not estimate a first stage because of missing 8990 data. We continue to use a triangular kernel, a first degree polynomial, and a \$12 million bandwidth.

Figure C.7 displays raw means of the investment rate, leverage, debt issuance, and equity issuance in evenly spaced \$2 million bins within a \$12 million bandwidth using the E-filing data. Once again, we observe no clear visual discontinuity for any of the four key outcome variables, strengthening the evidence that there is no large change in the investment rate or financing choices for firms just above the receipts cutoff. Figure C.5 also shows no bunching around the receipts cutoff in the E-filing data.

Table C.6 displays reduced form estimates of ψ from equation (1) using the SOI and E-filing data. Our E-filing estimates cannot reject zero for the investment rate, leverage, and debt issuance, and show a marginally significant decline in equity issuance. Continuing to use the E-filing data, Table C.7 shows null results for a placebo RD on low-interest firms, while Figure C.8 shows our estimates remain qualitatively similar regardless of the choice of bandwidth or polynomial degree.

²³For example, firms often mislabel mortgages, notes and bonds as other liabilities on their tax returns, and IRS staff manually correct these mistakes in the SOI data.

C.3.2 RECONCILING RD ESTIMATES WITH OTHER RESEARCH

In a paper written at the same time as ours, Sanati (2022) estimates a similar RD using Compustat data on publicly held firms ($N=194$ for their debt issuance outcome). In an updated version (Sanati and Beyhaghi, 2025), the RD is estimated using Y-14Q data on a small sample of publicly and privately held firms ($N=595$ for their debt issuance outcome). Both versions of their paper find significant investment rate declines and very large debt issuance declines in response to the interest limitation. The most recent version finds a debt issuance decline equal to 9.6% (with standard error of 2.1%) of assets less cash and marketable securities²⁴, while their work using only publicly held firms estimates a debt issuance decline of 42.9% (with standard error of 16.5%) of assets less cash and marketable securities. Converting to debt issuance scaled by assets would yield estimated declines of roughly 8.54% and 41.74%.

While we cannot subset our data to exactly match the Y-14Q data, we can subset our SOI data to publicly-held firms in an attempt to align with their estimates. Table C.8 displays reduced form and fuzzy RD estimates of equations (1) and (C.1) using SOI data for our four main outcomes, restricting to only publicly-held corporations. Due to the smaller sample we use a bandwidth of \$75 million lagged receipts. Even with the significantly larger bandwidth, restricting to publicly-held firms leads to a small sample with 418 total firms. We cannot reject zero impact of the interest limitation on the investment rate, leverage, debt issuance, or equity issuance. However, the confidence intervals are wider than our event study, triple difference, or baseline RD estimates and admit substantial declines or increases in borrowing.

To understand how noisy we might expect RD estimates to be in smaller samples, we take our SOI RD sample, restrict to observations within a \$12 million bandwidth, and construct 2,000 random samples of 298 observations on each side of the cutoff with replacement (matching the sample size in Sanati and Beyhaghi (2025)). For each random sample, we re-estimate our RD specification from equation (1) using debt issuance as an outcome. Figure C.9 plots the cumulative density function of these RD estimates across random samples. The 95% confidence interval on the full sample RD estimate spans $[-0.053, 0.034]$. More

²⁴Big, high-interest firms in our data have cash equaling 12% of assets, so to align with our debt:assets ratio it would be reasonable to deflate their estimates by a factor of 0.89 ($1/1.12$).

than 7% of the coefficient estimates from the simulated samples fall outside this confidence interval, differing from the baseline point estimate by more than its absolute value. Our larger sample yields imprecise estimates, and even small declines in sample size yield noisier estimates, pointing to statistical power issues that limit our ability to learn about the effects of the interest limitation using an RD with such a small sample.

One important additional note is that our TOT estimates are substantially larger than our ITT estimates. All the estimates in Sanati (2022) and Sanati and Beyhaghi (2025) are ITT estimates because they lack tax data necessary to observe which firms actually lose interest deductions. Guided by our results for publicly-held firms in Table C.8, we inflate their ITT estimates by a factor of 3.5. The magnitude of the resulting TOT estimates is a debt issuance decline exceeding 146% of assets using the Compustat estimates, or exceeding 30% of assets using the Y-14Q data. The size of these estimated responses far exceeds responses of debt to other tax changes measured in the literature. Hanlon and Heitzman (2022) refer to the magnitude of the ITT Compustat estimates as “implausibly large”, while a TOT inflation of the Y-14Q estimates yields a similar magnitude. Moreover, given that the \$25 million lagged receipts cutoff is not based on sales reported in the financial statement data that they must rely on, it is likely they are measuring treatment status with substantial error, particularly close to the cutoff.²⁵ This could attenuate the first stage further, generating even larger TOT estimates, or lead to a fully misspecified discontinuity without any first stage.

C.3.3 30% OF EBIT TIGHTENING RD ESTIMATES

As originally legislated by the TCJA, the earnings component of the limitation was tightened from 30% of EBITDA to 30% of EBIT beginning in 2022. This tightening disallows more interest deductions from firms already losing deductions, newly disallows deductions from firms with interest above the 30% of EBIT but not the 30% of EBITDA limit, and reduces investment incentives by excluding depreciation from the earnings component of the limitation, because investment generates depreciation that lowers EBIT but not EBITDA. This variation provides an opportunity to test whether tighter interest limitations would

²⁵Receipts are defined under Temp. Regs. Sec. 1.448-1T(f)(2)(iv) and include sales, interest, original issue discount, dividends, rents, royalties, and annuities. The regulation text can be viewed at <https://www.law.cornell.edu/cfr/text/26/1.448-1T>.

lead to larger declines in investment or borrowing, in the absence of the many simultaneous reforms implemented as part of the TCJA.

To evaluate the 2022 tightening with an RD, we build analysis samples from the SOI and E-filing data following the same procedure as the one we use to build our primary RD analysis sample, but using 2019-2021 as the pre-reform years in which we define whether firms are big and high-interest, 2022-2023 as the post-reform years, and a high-interest definition based on the new limitation. Table C.9 displays RD estimates of ψ from equation (1) using these alternative samples. Across all four primary outcomes, we continue to not find evidence consistent with the interest limitation leading to statistically or economically significant declines in investment or borrowing.

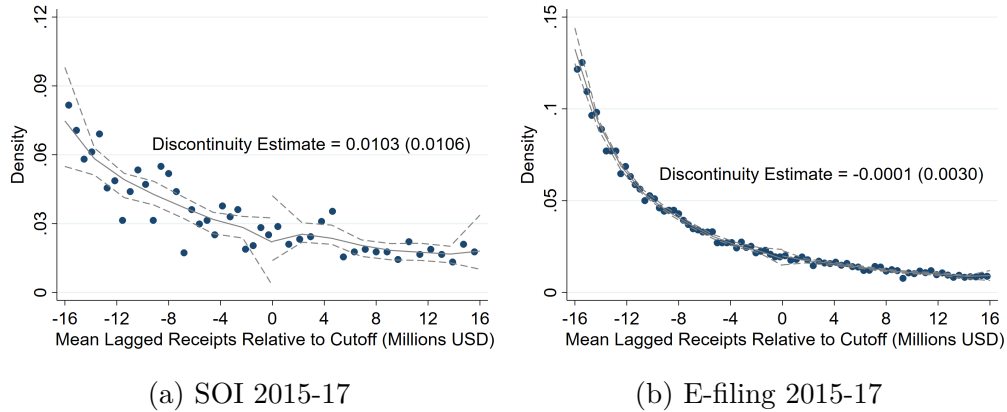


Figure C.5: Discontinuity Tests

Notes: This figure reports the McCrary test for a discontinuity in the distribution density of average receipts at the \$25 million cutoff. Panel (a) performs the test on the SOI data using 2015-2017 receipts. Panel (b) performs the test on the E-filing data using 2015-2017 receipts. Neither of the discontinuity estimates are statistically different from zero.

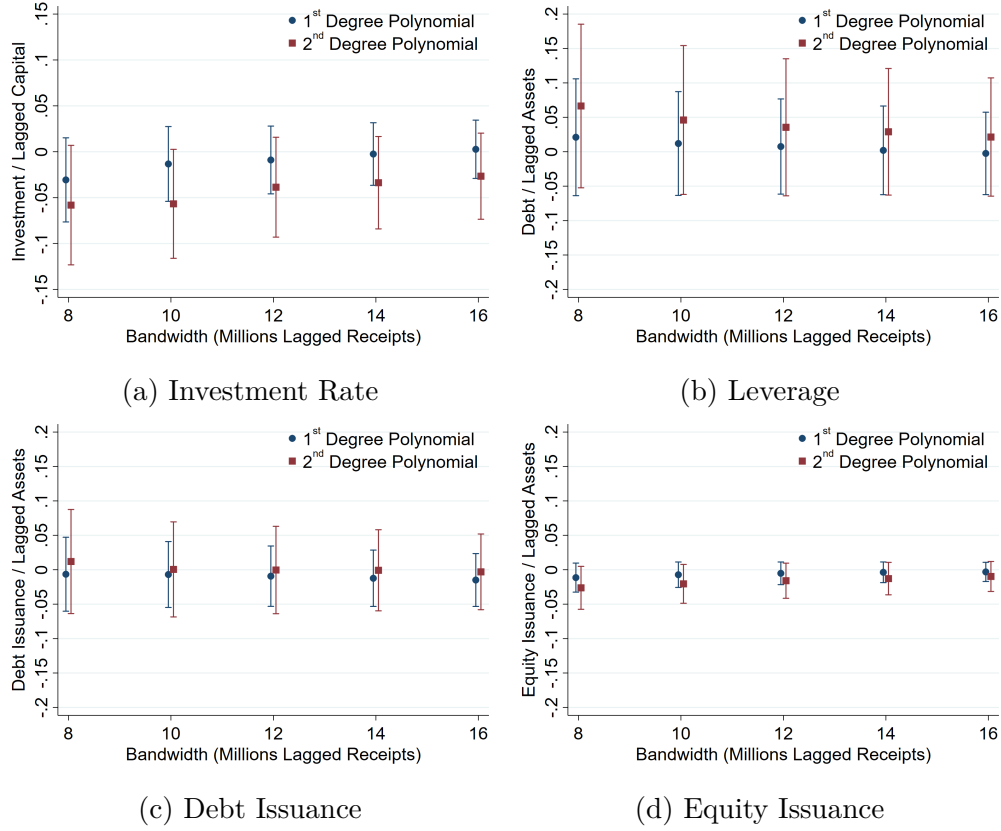


Figure C.6: Regression Discontinuity Alternative Specifications

Notes: This figure plots regression discontinuity estimates of ψ from equation (1) using the SOI data and varying the bandwidth and polynomial order. Panel (a) uses investment scaled by lagged capital as an outcome variable, panel (b) uses debt scaled by lagged assets as an outcome variable, panel (c) uses debt issuance scaled by lagged assets as an outcome variable, and panel (d) uses equity issuance scaled by lagged assets as an outcome variable. 95% confidence intervals are constructed from robust standard errors.

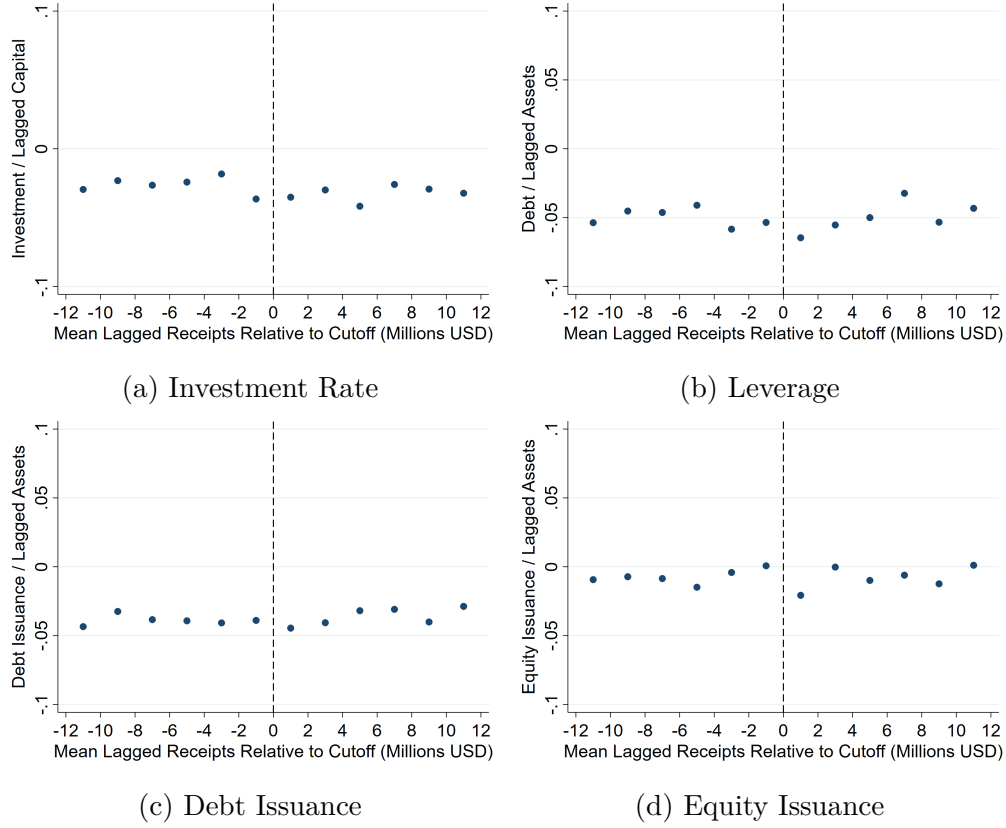


Figure C.7: E-filing Regression Discontinuity Binned Scatter Plots

Notes: This figure plots pre-post mean differences in evenly spaced \$2 million receipts bins around the \$25 million cutoff using the E-filing data and different variables in each panel. Panel (a) uses investment scaled by lagged capital, panel (b) uses debt scaled by lagged assets, panel (c) uses debt issuance scaled by lagged assets, and panel (d) uses equity issuance scaled by lagged assets.

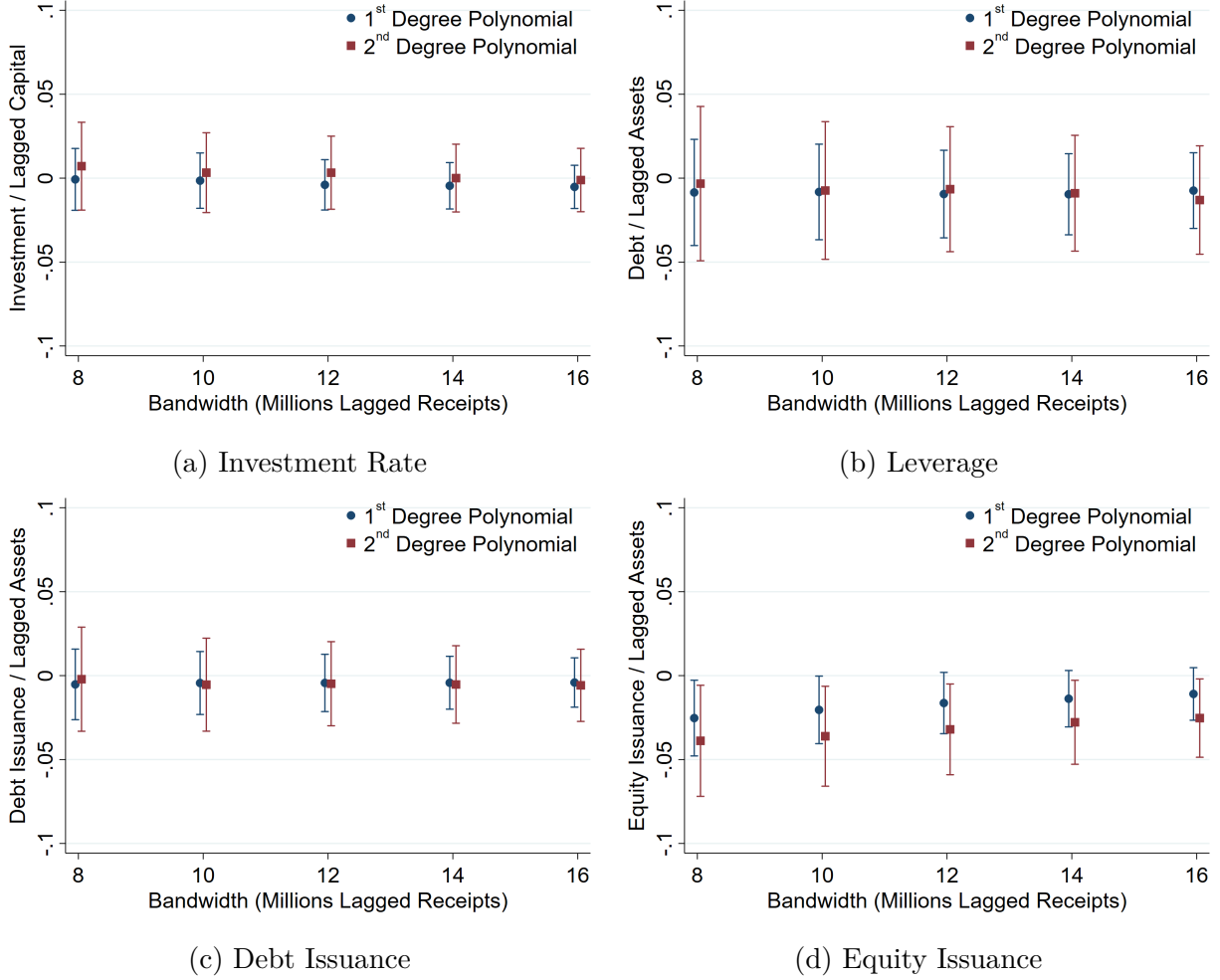


Figure C.8: Regression Discontinuity Alternative Specifications Using E-filing Data

Notes: This figure plots regression discontinuity estimates of ψ from equation (1) using the E-filing data and varying the bandwidth and polynomial order. Panel (a) uses investment scaled by lagged capital as an outcome variable, panel (b) uses debt scaled by lagged assets as an outcome variable, panel (c) uses debt issuance scaled by lagged assets as an outcome variable, and panel (d) uses equity issuance scaled by lagged assets as an outcome variable. 95% confidence intervals are constructed from robust standard errors.

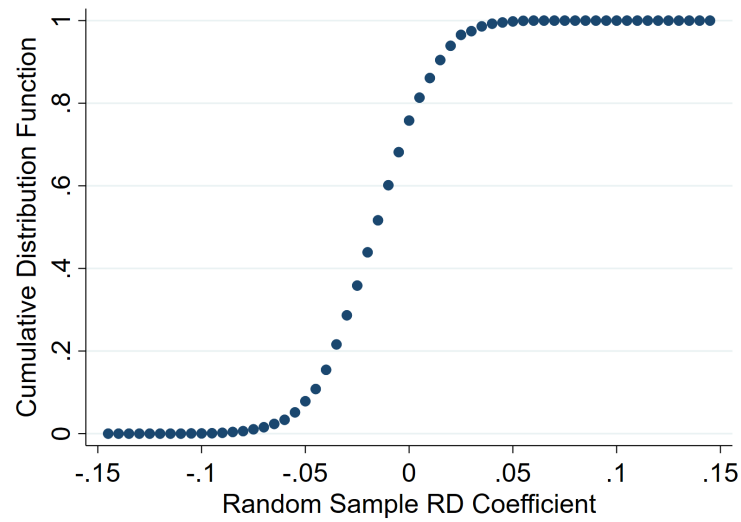


Figure C.9: Regression Discontinuity Estimates from Small Random Samples

Notes: This figure plots a CDF of 2,000 regression discontinuity estimates of ψ from equation (1) using random samples of firms from the SOI data. For each random sample we select 298 firms on each side of the cutoff with replacement within a \$12 million bandwidth of the \$25 million receipts cutoff. The outcome variable for the regression discontinuity estimates is debt issuance scaled by lagged assets.

Table C.3: Placebo Regression Discontinuity Effect on Investment and Financing

Outcome	(1) Investment Rate	(2) Leverage	(3) Debt Issuance	(4) Equity Issuance
ψ	0.003 (0.008)	0.012 (0.011)	0.004 (0.007)	-0.004 (0.003)
Obs	3,693	3,818	3,818	3,818
Pre-Reform Mean	0.110	0.196	0.007	0.007

Notes: This table reports regression discontinuity estimates of ψ from Equation (1) for all low-interest firms using a bandwidth of \$12 million receipts in the SOI data. Robust standard errors are reported in parentheses. Pre-reform means are averages over 2015-2017 for firms above the receipts cutoff.

Table C.4: Regression Discontinuity Effects With Potential Aggregators

Outcome	(1) Investment Rate	(2) Leverage	(3) Debt Issuance	(4) Equity Issuance
ψ	0.012 (0.011)	-0.004 (0.022)	-0.015 (0.012)	-0.005 (0.006)
Obs	2,186	2,297	2,297	2,297
Pre-Reform Mean	0.107	0.468	0.023	0.021

Notes: This table reports regression discontinuity estimates of ψ from Equation (1) for all high-interest firms, including potential aggregators, using a bandwidth of \$12 million receipts in our SOI data. Robust standard errors are reported in parentheses. Pre-reform means are averages over 2015-2027 for firms above the receipts cutoff.

Table C.5: Summary Statistics in SOI and E-filing Data

	Mean	Std Dev	P10	P50	P90	Obs
<i>SOI Data</i>						
Investment / Lagged Capital	0.071	0.103	0.000	0.031	0.219	717
Debt / Lagged Assets	0.471	0.351	0.003	0.453	0.974	734
Debt Issuance/ Lagged Assets	0.001	0.103	-0.120	-0.001	0.127	734
Equity Issuance / Lagged Assets	0.011	0.050	-0.018	-0.000	0.062	734
<i>E-filing Data</i>						
Investment / Lagged Capital	0.069	0.112	0.000	0.026	0.198	7,990
Debt / Lagged Assets	0.452	0.391	0.000	0.418	0.964	8,012
Debt Issuance / Lagged Assets	-0.050	0.255	-0.326	-0.006	0.172	8,012
Equity Issuance / Lagged Assets	0.004	0.140	-0.029	0.000	0.045	8,012

Notes: This table reports summary statistics on average 2015-2017 outcomes for firms within \$12 million of the \$25 million lagged receipts cutoff in the SOI and E-filing data. To preserve taxpayer anonymity, percentile statistics are reported as the means of the 11 observations nearest the true percentile.

Table C.6: RD Effect on Investment and Financing Using SOI and E-filing Data

Outcome	(1) Investment Rate	(2) Leverage	(3) Debt Issuance	(4) Equity Issuance
Panel A: SOI Estimates				
ψ	-0.009 (0.019)	0.008 (0.035)	-0.009 (0.022)	-0.005 (0.008)
Obs	715	734	734	734
Pre-Reform Mean	0.093	0.523	0.018	0.016
Panel B: E-filing Estimates				
ψ	-0.004 (0.008)	-0.009 (0.013)	-0.004 (0.009)	-0.016 (0.009)
Obs	7,990	8,012	8,012	8,012
Pre-Reform Mean	0.102	0.528	0.031	0.014

Notes: This table reports regression discontinuity estimates of ψ from Equation (1) for all high-interest firms in both SOI and E-filing data using a bandwidth of \$12 million receipts. Robust standard errors are reported in parentheses. Pre-reform means are averages over 2015-2017 for firms above the receipts cutoff.

Table C.7: Placebo Regression Discontinuity Effect Using E-filing Data

Outcome	(1) Investment Rate	(2) Leverage	(3) Debt Issuance	(4) Equity Issuance
ψ	−0.005 (0.003)	−0.003 (0.004)	−0.000 (0.003)	−0.001 (0.003)
Obs	48,923	49,022	49,022	49,022
Pre-Reform Mean	0.116	0.216	0.012	0.010

Notes: This table reports regression discontinuity estimates of ψ from Equation (1) for all low-interest firms using a bandwidth of \$12 million receipts in the E-filing data. Robust standard errors are reported in parentheses. Pre-reform means are averages over 2015-2017 for firms above the receipts cutoff.

Table C.8: Regression Discontinuity Effect on Investment and Financing: Public Firms

Outcome	(1) Investment Rate	(2) Leverage	(3) Debt Issuance	(4) Equity Issuance
ψ	0.021 (0.111)	−0.048 (0.058)	−0.043 (0.029)	0.008 (0.020)
ψ^{IV}	0.069 (0.365)	−0.154 (0.195)	−0.139 (0.103)	0.027 (0.063)
Obs	390	418	418	418
Pre-Reform Mean	0.476	0.337	0.033	0.084
First Stage F-Stat	7.587	8.715	8.715	8.715

Notes: This table reports regression discontinuity estimates of ψ from Equation (1) and ψ^{IV} from Equation (C.1) for all public, high-interest firms in our SOI RD sample using a bandwidth of \$75 million receipts. Robust standard errors are reported in parentheses. Pre-reform means are averages over 2015-2017 for firms above the receipts cutoff.

Table C.9: 2022 Regression Discontinuity Effect on Investment and Financing

Outcome	(1) Investment Rate	(2) Leverage	(3) Debt Issuance	(4) Equity Issuance
Panel A: SOI Estimates				
ψ	0.013 (0.026)	0.090 (0.045)	0.043 (0.026)	0.006 (0.014)
Obs	855	878	878	878
Pre-Reform Mean	0.128	0.450	0.020	0.024
Panel B: E-filing Estimates				
ψ	0.014 (0.009)	-0.018 (0.016)	-0.015 (0.012)	0.001 (0.009)
Obs	9,963	9,982	9,982	9,982
Pre-Reform Mean	0.131	0.469	0.008	0.018

Notes: This table reports regression discontinuity estimates of ψ from Equation (1) for all high-interest firms in both SOI and E-filing data. Using both data sources, we define high-interest based on the 30% of EBIT cutoff that goes into effect in 2022, and construct alternative RD samples of high-interest firms using 2022-2023 as the post-reform period and 2019-2021 as the pre-reform period. Both sets of estimates use a bandwidth of \$12 million. Robust standard errors are reported in parentheses.

C.4 Event Study and Triple Difference Robustness

In this appendix, we discuss evidence supporting the parallel trends assumption in our event study design, present robustness checks of the event study and triple difference designs, compare big, high- and low-interest firms, and implement event study and triple difference designs focused on the 2022 tightening of the limitation.

C.4.1 VALIDATING PARALLEL TRENDS

Our event study design relies on a parallel trends assumption that the outcomes of the larger treatment and smaller control firms would have evolved similarly in the absence of the interest limitation. First, in Figures C.10, C.11, and C.12, we plot the raw means of our core investment and financing variables, plus many other constructions of investment and borrowing outcomes, for big and small, high- and low-interest firms.

Moving beyond visual inspection of parallel trends we present three additional pieces of analysis that provide evidence supporting this assumption. First, we cannot reject zero differential responses between big and small high-interest firms to a previous change in the tax rate, supporting the basic assumption underlying our event study design that the outcomes of big and small high-interest firms would have evolved similarly in the absence of the interest limitation, even in the presence of a simultaneous tax rate change. In 2013, the top individual tax rate increased from 35% to 39.6%, raising the tax rate on pass-through businesses. To explore the impacts of this tax rate change, we construct a panel data set mimicking our baseline data construction, but spanning 2008-2014, and defining high-interest and size status in 2010-2012. Using this sample, we re-estimate equation (2) using only pass-through businesses, and using a single post-reform indicator for years 2013-2014. Table C.10 reports the average post-reform coefficients for the investment rate, leverage, debt issuance, and equity issuance. All four estimates are economically small and cannot reject zero.

Second, placebo event study regressions comparing big to small low-interest firms reveal no differential responses to other TCJA reforms by firm size. Table C.11 displays estimates of β_{post} from equation (2) estimated on the low-interest firms in our data, while Figure C.13 displays year-by-year estimates. Column 1 of Table C.11 uses interest disallowed as an

outcome variable. We find statistically but not economically significant increases in interest disallowed of less than 0.1% of assets, suggesting the placebo event study measures size-varying impacts of other TCJA reforms, not the limitation. The four remaining columns show that we cannot reject zero differential investment or financing response between big and small low-interest firms to all of the simultaneous TCJA reforms. These estimates support the assumption that the outcomes of big and small high-interest firms would have evolved similarly in the absence of the limitation, even in the presence of other TCJA reforms.

Third, to address the lack of common support across our bigger and smaller firms, we re-estimate our event study and triple difference designs, restricting the sample to only include firms in successively narrower ranges of the average 2015-2017 receipts distribution (between percentiles [1, 99], [5, 95], [10, 90], and [25, 75]). We plot coefficient estimates using these restrictions in Figures C.14 and C.15, continuing to find similar pre-trends and results.

C.4.2 ROBUSTNESS CHECKS

We begin our event study and triple difference robustness checks by showing our estimates are robust to a variety of different outcome constructions for investment and financing variables. Table C.12 displays event study estimates of β_{post} and triple difference estimates of γ_{post} using net investment scaled by capital, investment scaled by capital netting out accumulated depreciation, extensive margin investment and an indicator for investment bursts exceeding 20% of lagged capital as outcome variables. Across our four additional investment measures and both research designs, seven of the eight estimates cannot reject zero, while our triple difference estimates suggest an economically small but statistically significant 1.9% decline in the fraction of firms investing.

The null changes in leverage and debt issuance that we observe in response to the interest limitation are also robust to a variety of alternative measures. Table C.13 displays event study estimates of β_{post} and triple difference estimates of γ_{post} using seven different measures of leverage or debt. First, scaling debt by assets can provide a flawed measure of leverage because assets must equal liabilities plus shareholder equity, so an increase in non-debt liabilities could decrease the leverage ratio (Welch, 2011). Therefore, we scale by financial capital, differencing non-debt liabilities out of assets so that liability changes do not influence

leverage. In addition, we use other debt measures scaled by assets: debt plus loans from shareholders, net issuance of debt plus loans from shareholders, short-term debt, long-term debt, and trade credit. We also use the debt to equity ratio ($\text{debt} / (\text{assets} - \text{liabilities})$) as an outcome variable. Across our eight additional measures and both research designs, 12 of the 14 estimates cannot reject zero. The event study design suggests the debt to equity ratio increases by 0.11, inconsistent with substitution from debt towards equity, and the triple difference design suggests trade credit increases by 0.4% of assets, consistent with small substitution away from traditional debt towards trade credit.

Our event study and triple difference results are also robust to a number of different specifications, sample restrictions, and data processing choices. We present the majority of these tests in Table C.14, which displays event study estimates of β_{post} in columns 1-4 and triple difference estimates of γ_{post} in columns 5-8 for the investment rate, leverage, debt issuance, and equity issuance. Across our core investment and financing outcome variables, the vast majority of tests appear very similar to our baseline estimates, while some indicate economically small increases in equity issuance.

The baseline estimates we display in Tables 2 use three digit NAICS industry-year fixed effects and no control variables. The first two rows of Table C.14 display estimates using year and industry-profitability-year fixed effects instead of industry-year fixed effects, binning firms into profitability quartiles based on their average profits scaled by lagged assets from 2015-2017. The third row of each panel adds interactions between year fixed effects and average age, revenue growth, sales, and profits over 2015-2017. Neither specification modification substantially alters the results.

The fourth row of each panel scales outcome variables by average pre-reform assets or capital over 2015-2017 rather than using lagged assets or capital as the denominator for the outcome variable. The fifth row in each panel uses outcome variables winsorized at the 99th percentile rather than the 95th percentile. For both, we continue to find similar results.

Businesses with significant real estate and agriculture components are allowed to opt out of the limitation in exchange for using a slower depreciation system, while auto dealers are allowed to add floor plan financing to their limitation. In practice, many real estate firms opt out of the limitation while few other firms do. The sixth and seventh rows of each panel

drop real estate firms and auto dealers from our estimation sample and find similar results.²⁶

Some firms that appear small in our data may face the limitation because of aggregation rules. The eighth row of each panel drops all firms we flag as potential aggregators and finds similar results. The ninth row restricts to a balanced panel of firms that appear in every year of our data to address concerns about sample attrition. Results remain unchanged.

Very large firms may have different investment opportunities and access to capital markets than smaller firms. Furthermore, multinational firms may have benefited from TCJA international tax changes. To alleviate concerns that the presence of either set of firms in our treatment group drive our results, the tenth row of each panel drops the largest quarter of treatment firms from the estimation sample, and the eleventh row drops all multinational firms (defined as firms with a ratio of the sum of all gross receipts from CFCs on Form 5471 Schedule C line 1c to total gross receipts that exceeds 5%). Dropping the very largest or multinational firms from our sample does not substantially change our results.

The TCJA also implemented 100% bonus depreciation, which could disproportionately benefit larger treatment versus smaller control firms. The twelfth row drops firms in four digit industries in the top 10% of bonus exposure with the lowest net present value of depreciation deductions, as measured by Zwick and Mahon (2017). The thirteenth and fourteenth rows isolate firms that are likely to face the largest and smallest benefits from non-limitation domestic TCJA provisions, restricting to above median bonus exposure C-corporations and below median bonus exposure passthroughs while excluding multinationals. The fifteenth row weights our baseline regressions by average assets from 2015-2017 instead of using equal weights for each firm. In each case our results remain similar.

Finally, in Table C.15, we vary the \$100,000 asset cutoff we use to construct our analysis sample, and show both our event study and triple difference estimates remain similar when varying the cutoff to \$50,000, \$200,000, or \$1,000,000.

²⁶In our panel data set from 2018-2019, real estate firms opt out of the limitation in 16.5% of observed firm-years, agriculture firms opt out in 3.4% of observed firm-years, and all other firms opt out in 1.6% of observed firm-years. In Table C.2, we show our results are stable when dropping any firm that ever elects out of the interest limitation. Dropping real estate firms in Table C.14 is an *ex ante* restriction eliminating many firms that could elect out, avoiding selection issues arising from only dropping firms that do elect out.

C.4.3 COMPARING BIG, HIGH- AND LOW-INTEREST FIRMS

Carrizosa et al. (2022) study firm responses to the interest limitation using Compustat data and an event study design that compares big, high-interest firms to big, low-interest firms. They focus on firm leverage as an outcome and find declines in debt for treatment relative to control firms of roughly 3% of lagged assets that reject zero, significantly larger than our event study or triple difference estimates.

We implement a similar research design on all the big firms in our data, keeping firms with average receipts above \$25 million over 2015-2017 and assigning firms to the treatment group if their interest exceeds their limitation averaging over 2015-2017. Using this design, we argue that a post-reform decline in leverage for big, high-interest relative to big, low-interest firms is likely to be driven by mean reversion rather than a response to the limitation.

Debt and interest are highly correlated. Figure B.1 shows that high-interest firms facing the limitation are in the right tail of the interest distribution in the years that are used to determine their treatment status, suggesting we should expect some reversion to lower levels of interest and debt. If treatment firms are selected to have especially high interest and debt from 2015-2017, we should expect their leverage to be lower in years before the treatment definition, stable during the years of the treatment definition, and decline again after the treatment definition relative to low-interest firms. If these dynamics are not driven by the limitation, we should also observe the same pattern using an identical treatment definition in different years (Richmond, 2024).

To test for this dynamic, we construct four additional versions of our event study analysis data covering the same number of years, but starting in earlier years and using three year treatment definitions Big_{iF} with final year F based on earlier sets of years $\{2014-2016, 2013-2015, 2012-2014 \text{ and } 2011-2013\}$. In each panel data set with a treatment definition ending in year F , we estimate

$$(C.4) \quad Y_{it} = \sum_{e=F-4, e \neq F}^{F+2} \beta_e \mathbb{1}(t = e) \times Big_{iF} + \delta_{jt} + \xi_i + \varepsilon_{it}.$$

In Figure C.16, we plot each of these placebo-in-time estimates setting event time equal to zero in the last year of the treatment definition F for each data set. Each series in the

figure shows that leverage remains stable for big, high- relative to low-interest firms in the years of the treatment definition, but leverage is lower for big, high- relative to low-interest firms in the years before and after the treatment definition, regardless of the treatment definition years. This strongly suggests that post-reform declines in leverage are driven by mean reversion for higher interest relative to lower interest firms, not a response to the limitation, and that comparing the outcomes of higher and lower interest firms in an event study design will not yield unbiased estimates of firm responses to the limitation.

Carrizosa et al. (2022) also implement a triple difference using Canadian Compustat firms and find marginally significant (at the 10% level) declines in leverage. However, for the comparison between big, high- and low-interest Canadian and U.S. firms to be valid, the time series process of leverage for Canadian and American Compustat firms must be the same, so that there are similar amounts of mean reversion across groups. The authors do not test this assumption, and also suggest the comparison may have issues because there are very few Canadian firms, Canadian firms likely have exposure to U.S. markets, and the Canadian firms differ from U.S. firms on many margins.

Finally, in a working paper released after earlier drafts of this manuscript, Hanlon and Heitzman (2024) use Compustat and implement an empirical approach that is similar to the placebo-in-time approach of Figure C.16 to study debt issuance responses to the limitation, relying on past comparisons between big, high- and low-interest firms to identify mean reversion. Differencing out average responses to placebo treatment definitions starting in 1990 from average responses to the limitation, they find marginally significant declines in debt issuance of less than 1% of assets that cannot reject zero response at the 5% level, with a 95% confidence interval spanning $[-0.019, 0.001]$. Our null event study debt issuance confidence interval spans $[-0.004, 0.008]$, overlapping with their estimates. Assuming independence, we cannot reject that the two estimates are the same at the 5% level. In our data, differencing out the earlier series in Figure C.16 would yield a similar null estimate to our other designs, but such an approach to estimating the impacts of the limitation relies on an additional assumption that the time series process governing the evolution of debt for high-interest firms is stable over time. Rather than rely on this additional assumption, we prefer to compare big and small high-interest firms.

C.4.4 30% OF EBIT TIGHTENING EVENT STUDY AND TRIPLE DIFFERENCE ESTIMATES

To evaluate the 2022 tightening with our event study and triple difference designs, we build analysis samples from the SOI data following the same procedure as the one we use to build our primary event study and triple difference analysis samples, but using 2019-2021 as the pre-reform years in which we define whether firms are big and high-interest, 2022-2023 as the post-reform years, and a high-interest definition based on the new limitation: firms whose pre-reform interest exceeds the 30% of EBIT but not 30% of EBITDA threshold. Table C.16 displays event study and triple difference estimates of β_{post} from equation (2) and γ_{post} from equation (3) using these alternative samples. Across all four primary outcomes, we continue to not find evidence consistent with the interest limitation leading to statistically or economically significant declines in investment or borrowing.

That said, because of the timing of the 2022 tightening, treatment and control groups are formed during COVID to derive *ex ante* classifications. This creates a particular challenge for the event study design given that the placebo for our main analysis failed in 2020 because big and small firms experienced differential shocks during the pandemic. As a result, the event study estimates of the 2022 tightening exhibit pretrends, which warrants caution in drawing strong conclusions from these estimates alone.²⁷ Nonetheless, for completeness we include the results from all of our research designs.

²⁷The triple differences design is more robust to this issue, though there are negative preperiod estimates for the debt issuance measure.

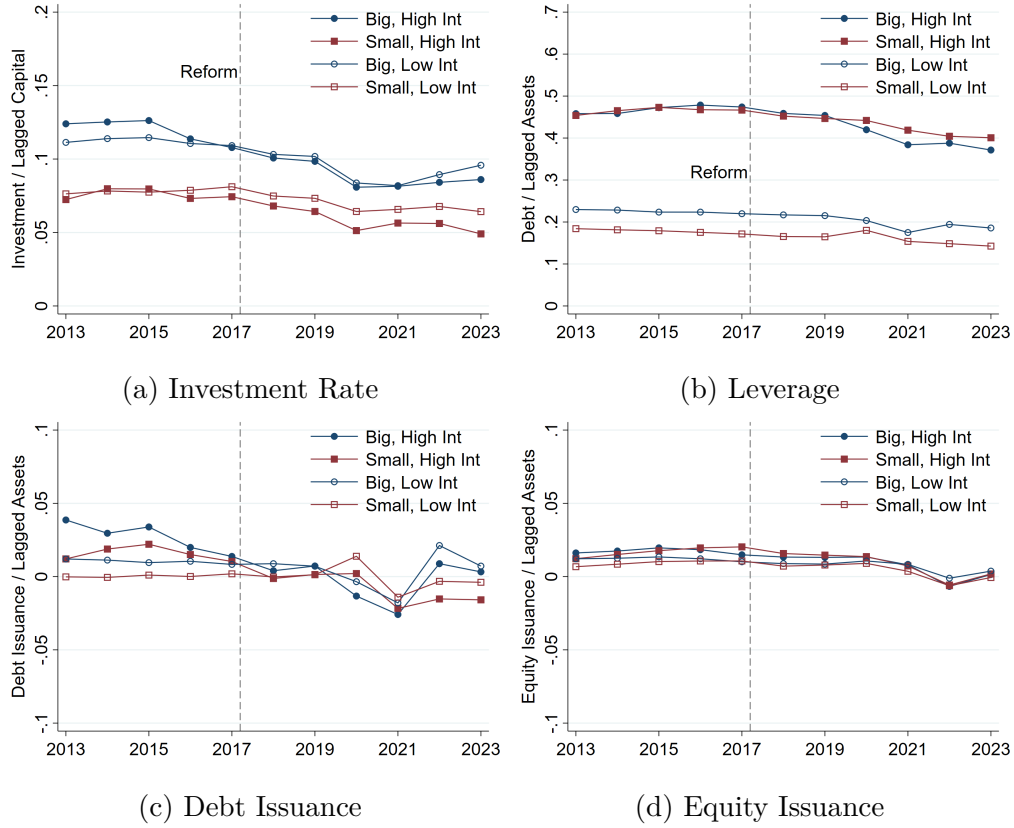


Figure C.10: Raw Means of Core Investment and Financing Variables

Notes: This figure plots raw means of the investment rate, leverage, and debt and equity issuance for big high-interest, big low-interest, small high-interest, and small low-interest firms, all defined based on average interest relative to limitation and receipts over 2015-2017.

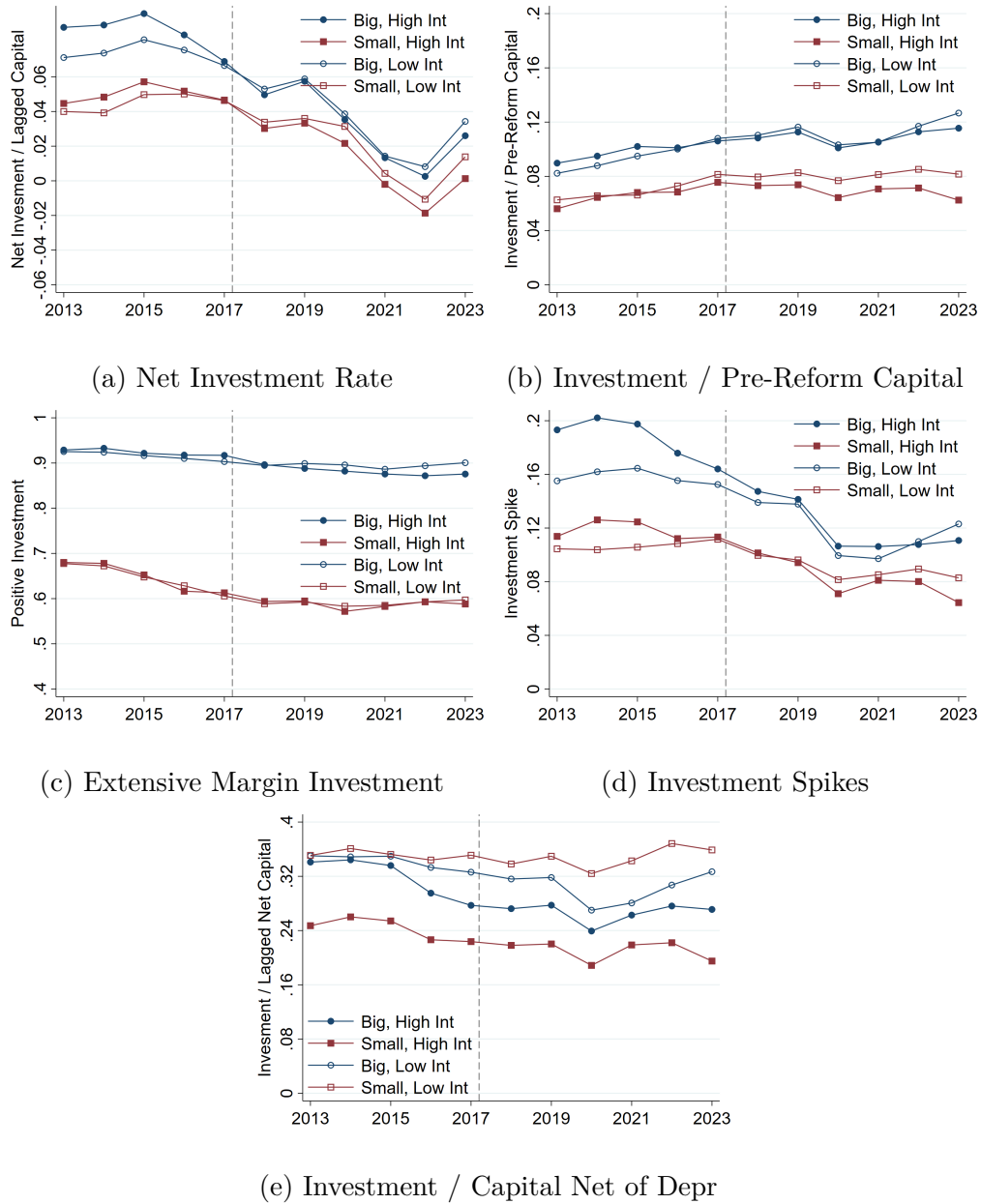


Figure C.11: Investment Raw Means of Treatment and Control Groups

Notes: This figure plots raw means of auxiliary investment measures for big high-interest, big low-interest, small high-interest, and small low-interest firms, all defined based on average interest relative to limitation and receipts over 2015-2017.

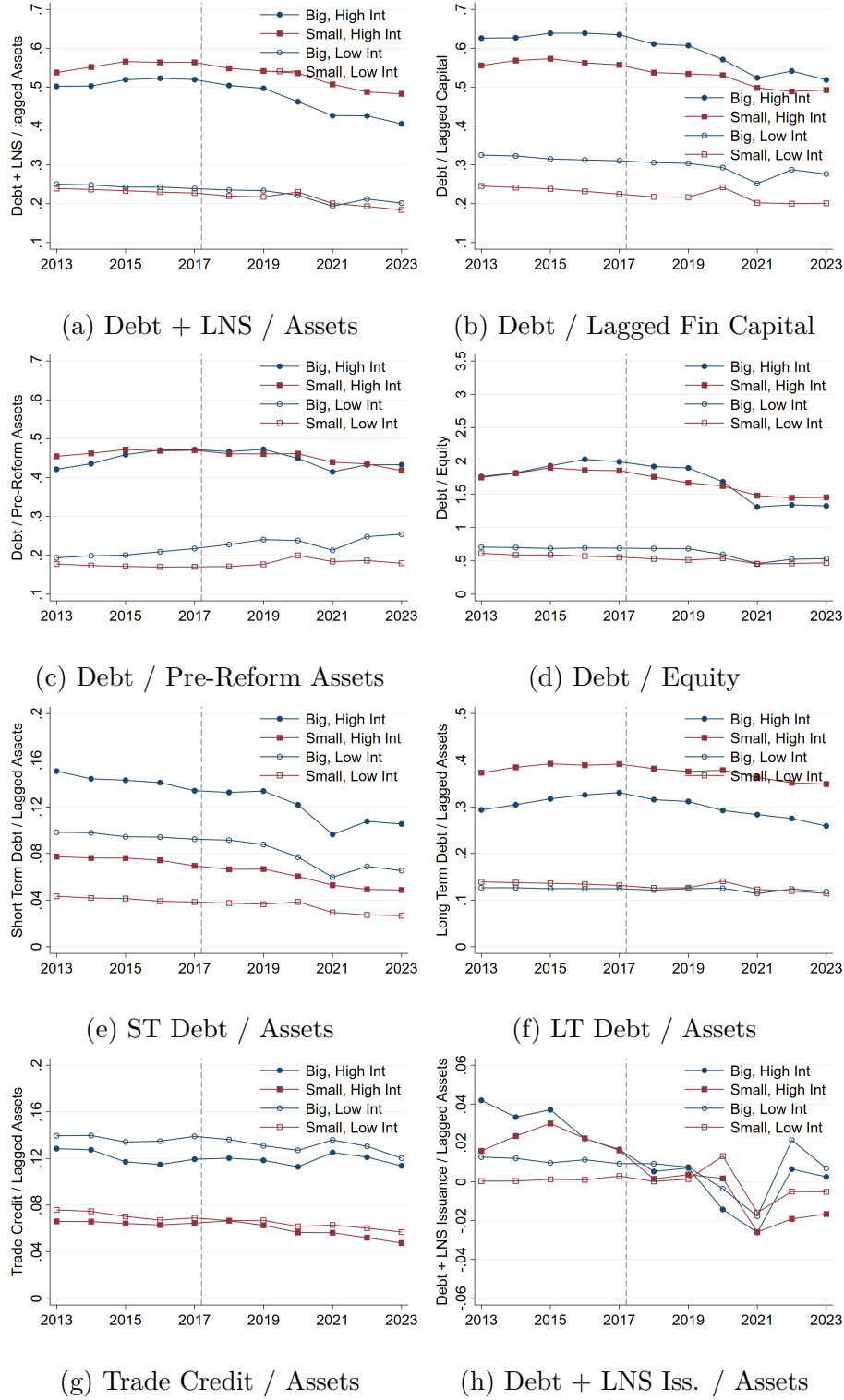
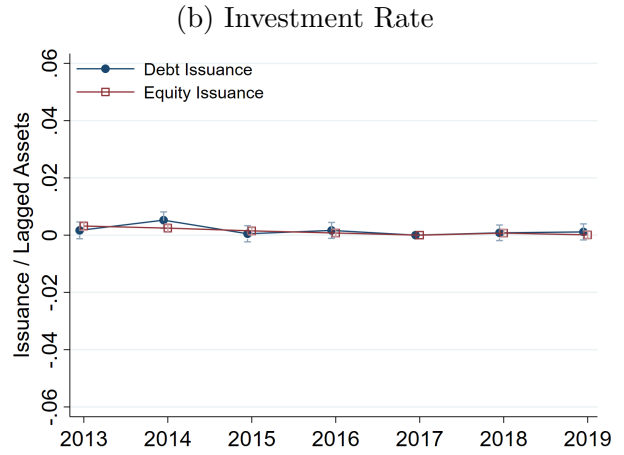
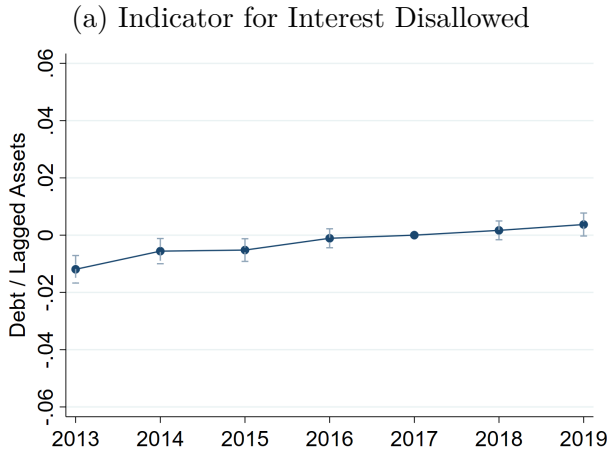
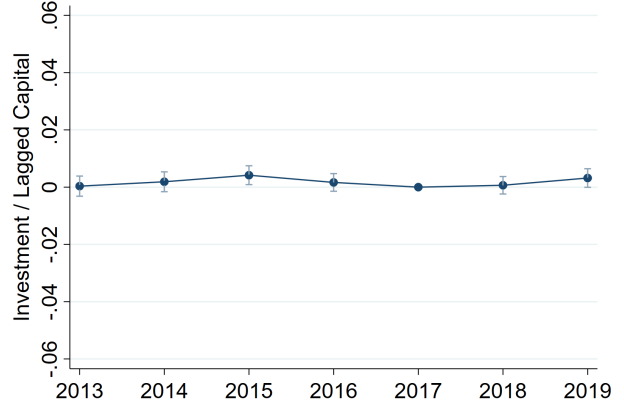
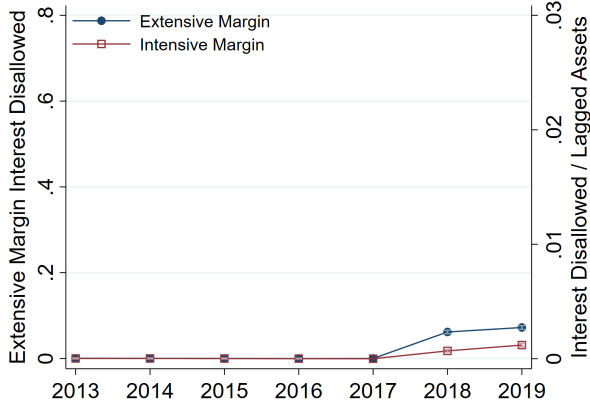


Figure C.12: Debt Raw Means of Treatment and Control Groups

Notes: This figure plots raw means of auxiliary debt measures for big high-interest, big low-interest, small high-interest, and small low-interest firms, all defined based on average interest relative to limitation and receipts over 2015-2017.



(a) Indicator for Interest Disallowed

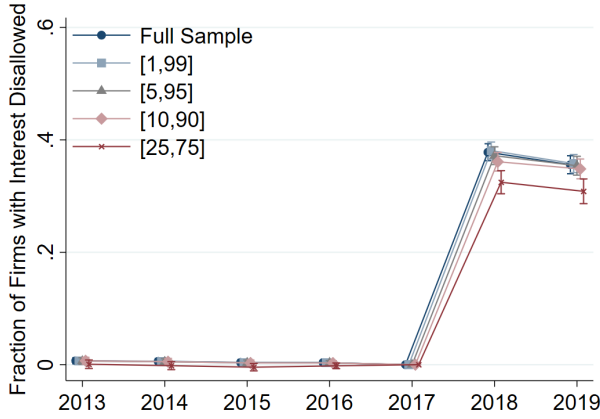
(b) Investment Rate

(c) Leverage

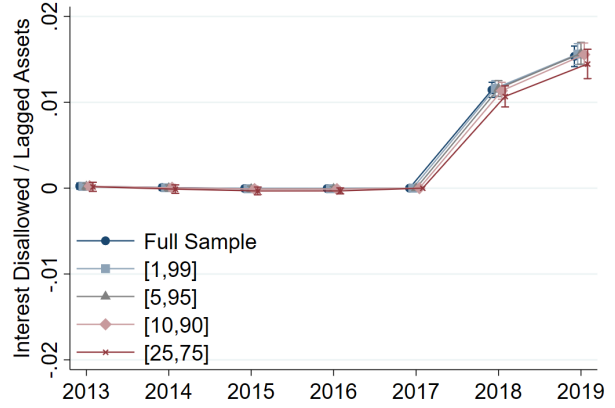
(d) Issuance

Figure C.13: Placebo Event Study Estimates

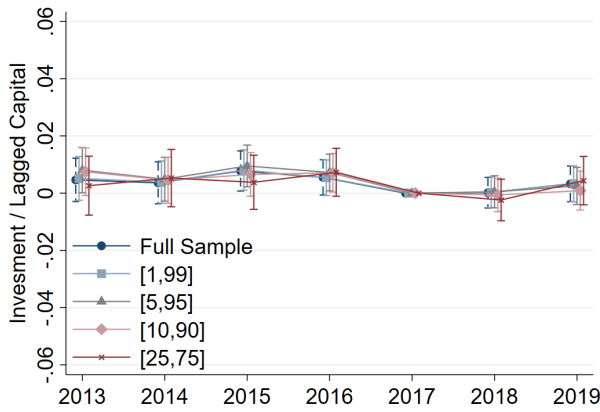
Notes: This figure plots event study estimates of β_e from equation (2) using low-interest firms. Outcome variables include (intensive and extensive margin) interest disallowed scaled by lagged assets, investment scaled by lagged capital, debt scaled by lagged assets, and debt and equity issuance scaled by lagged assets. 95% confidence intervals are constructed from standard errors clustered at the firm level.



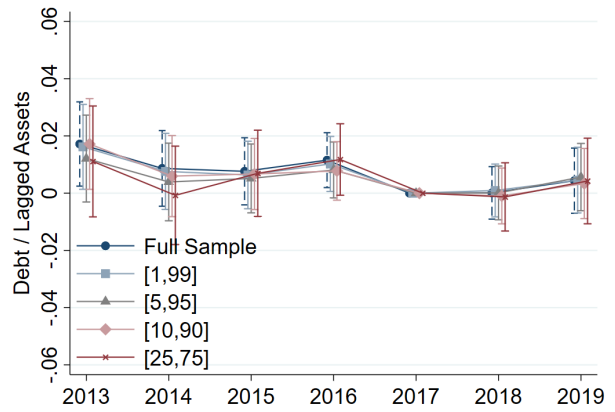
(a) Any Interest Disallowed



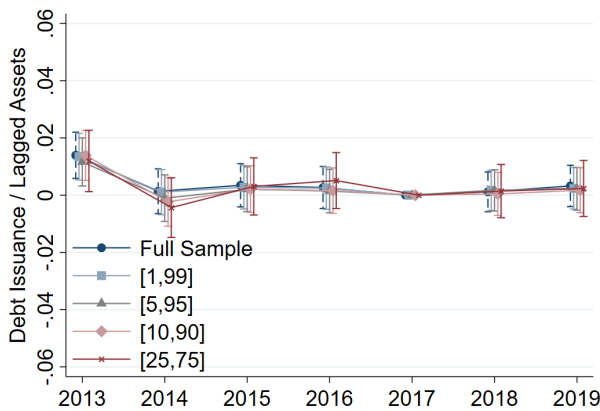
(b) Interest Disallowed



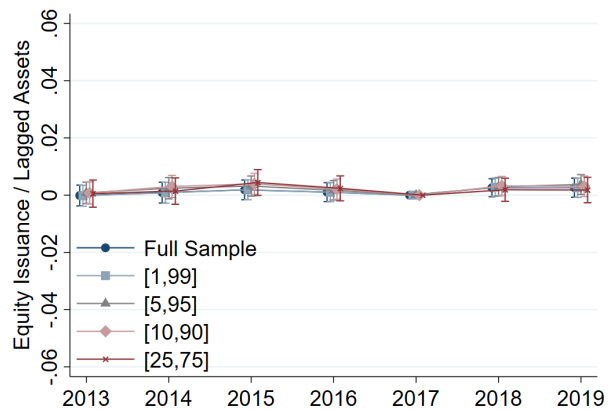
(c) Investment Rate



(d) Leverage



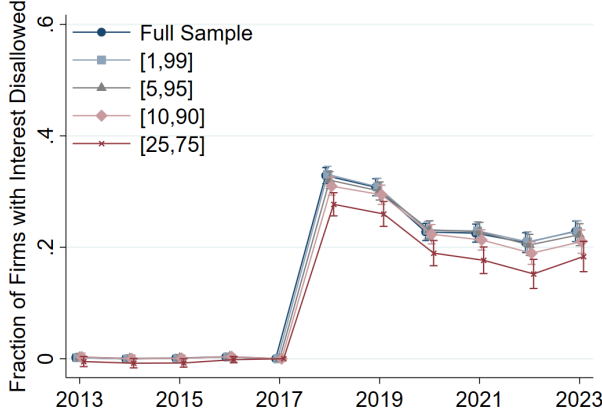
(e) Debt Issuance



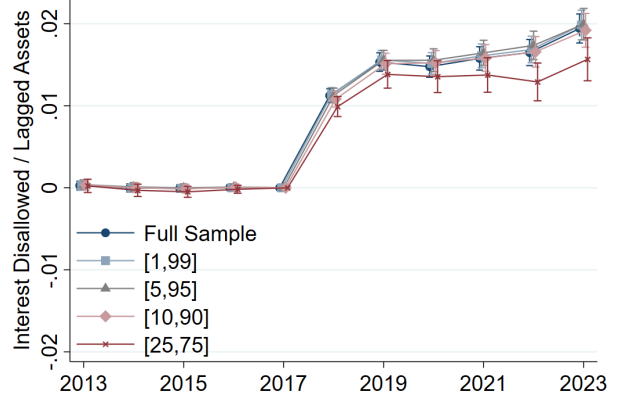
(f) Equity Issuance

Figure C.14: Event Study Bandwidth Restrictions

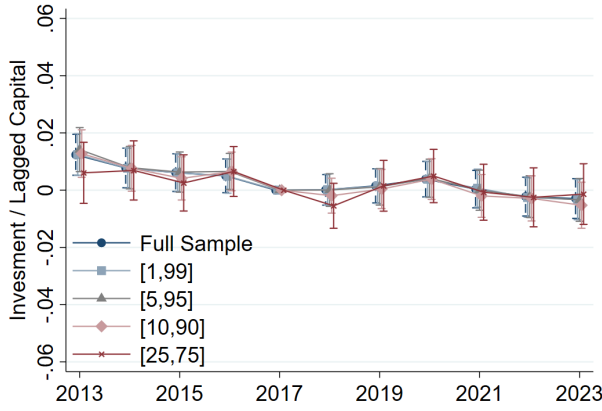
Notes: This figure plots event estimates of β_e from equation (2) while restricting the sample to firms with average receipts over 2015-2017 within the interval between the Xth and Yth percentile of the receipts distribution, as specified in the legend. 95% confidence intervals are constructed from standard errors clustered at the firm level.



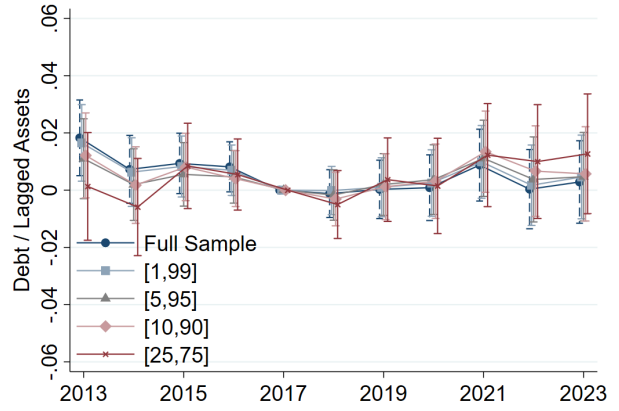
(a) Any Interest Disallowed



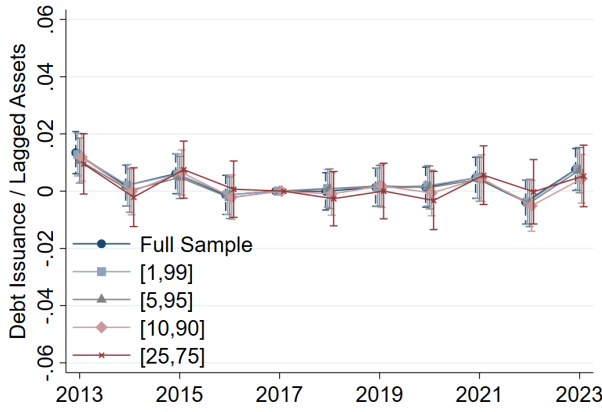
(b) Interest Disallowed



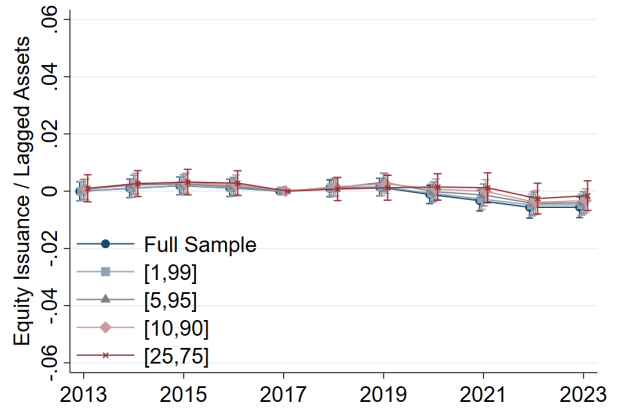
(c) Investment Rate



(d) Leverage



(e) Debt Issuance



(f) Equity Issuance

Figure C.15: Triple Difference Bandwidth Restrictions

Notes: This figure plots triple difference of γ_e from equation (3) while restricting the sample to firms with average receipts over 2015-2017 within the interval between the Xth and Yth percentile of the receipts distribution, as specified in the legend. 95% confidence intervals are constructed from standard errors clustered at the firm level.

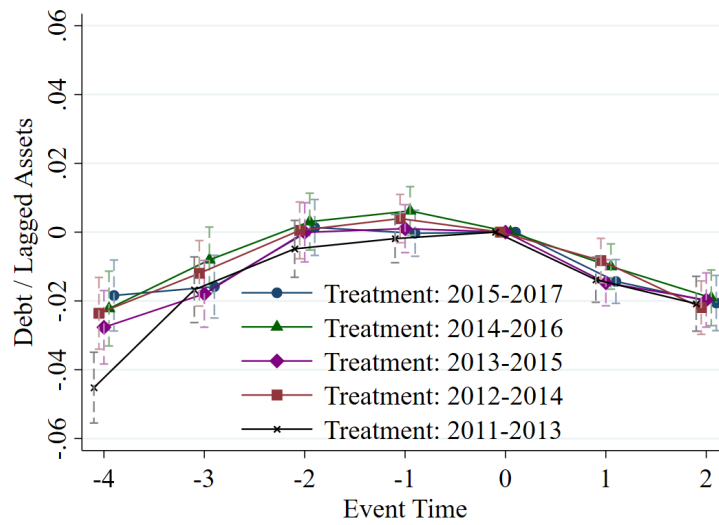


Figure C.16: Mean Reversion Around Treatment Definition Years

Notes: This figure plots event study estimates of β_e following equation (C.4), but the estimates come from seven year panel data sets spanning different years and using different sets of years to define which firms are big and high interest. The omitted year from each event study series is the last year of the treatment definition. 95% confidence intervals are constructed from standard errors clustered at the firm level.

Table C.10: Pass-through Responses to 2013 Individual Tax Rate Change

Outcome	(1) Investment Rate	(2) Leverage	(3) Debt Issuance	(4) Equity Issuance
β_{post}	-0.0050 (0.0028)	-0.0054 (0.0049)	-0.0060 (0.0035)	-0.0016 (0.0014)
Obs	50,986	53,061	53,061	53,061
Clusters	8,795	9,110	9,110	9,110

Notes: This table reports event study estimates of passthrough firm responses to the 2013 individual top tax rate change. The estimation sample is the high interest firms in a panel data set with the same restrictions as our baseline panel data set, but spanning years 2008-2014, and dropping all C-corporations. Firms are classified as high interest based on their average interest relative to their limitation over 2010-2012. The regression specification follows equation (2), but the omitted year is 2012, and β_{post} represents a two year post-reform average coefficient over 2013-2014. Standard errors are clustered at the firm level and reported in parentheses.

Table C.11: Placebo Event Study Effect on Investment and Financing

Outcome	(1) Int Disallowed	(2) Investment Rate	(3) Leverage	(4) Debt Issuance	(5) Equity Issuance
β_{post}	0.0009 (0.0001)	0.0017 (0.0014)	0.0028 (0.0016)	0.0010 (0.0012)	0.0006 (0.0006)
Obs	303,475	276,093	303,475	303,475	303,475
Clusters	51,014	46,236	51,014	51,014	51,014
Pre-Reform Mean	0.000	0.111	0.224	0.010	0.012

Notes: This table reports event study estimates of β_{post} from equation (2), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. The estimation sample includes all firms in our panel data with interest below their limitation averaging over 2015-2017. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for treatment firms in all years before 2018.

Table C.12: Effects of Interest Limitation on Alternative Investment Measures

Outcome	(1) Net Inv / Cap	(2) Inv/Net Cap	(3) $\mathbb{1}(\text{Inv} > 0)$	(4) Inv Spike
Panel A: Event Study Estimates				
β_{post}	-0.0006 (0.0033)	0.0066 (0.0098)	-0.0030 (0.0063)	-0.0022 (0.0069)
Obs	79,150	78,106	84,737	84,737
Clusters	14,243	14,122	15,274	15,274
Pre-Reform Mean	0.0840	0.3141	0.9233	0.1835
Panel B: Triple Difference Estimates				
γ_{post}	0.0007 (0.0030)	0.0075 (0.0090)	-0.0193 (0.0058)	0.0038 (0.0063)
Obs	538,576	523,043	588,318	588,318
Clusters	64,024	62,934	69,751	69,751
Pre-Reform Mean	0.0841	0.3142	0.9229	0.1826

Notes: This table reports estimates from our event study and triple difference research designs. In Panel A, we estimate β_{post} from equation (2), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. In Panel B, we estimate γ_{post} from equation (3), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. The estimation samples vary across designs as described in the text. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for treatment firms in all years before 2018. The net investment outcome variable in column 1 is the net change in the capital stock scaled by the lagged capital stock. The investment scaled by net capital outcome variable in column 2 is investment scaled by capital minus accumulated depreciation. The outcome variable in column 4 is an indicator for investment spikes, which is equal to 1 whenever capital expenditures exceed 20% of the previous period capital stock.

Table C.13: Effects of Interest Limitation on Alternative Borrowing Measures

Outcome	(1) <u>Debt</u> <u>Fin Capital</u>	(2) <u>Debt+LSH</u> <u>Assets</u>	(3) <u>ΔDebt+LSH</u> <u>Assets</u>	(4) <u>Short-Term Debt</u> <u>Assets</u>	(5) <u>Long-Term Debt</u> <u>Assets</u>	(6) <u>Debt</u> <u>Equity</u>	(7) <u>Trade Credit</u> <u>Assets</u>
Panel A: Event Study Estimates							
β_{post}	−0.0011 (0.0073)	0.0005 (0.0049)	0.0037 (0.0033)	0.0014 (0.0023)	−0.0013 (0.0042)	0.1142 (0.0310)	−0.0018 (0.0018)
Obs	84,730	84,737	84,737	84,737	84,737	64,658	84,737
Clusters	15,274	15,274	15,274	15,274	15,274	12,667	15,274
Pre-Reform Mean	0.6357	0.5151	0.0285	0.1423	0.3165	1.9250	0.1212
Panel B: Triple Difference Estimates							
γ_{post}	0.0020 (0.0067)	−0.0021 (0.0048)	0.0041 (0.0027)	0.0008 (0.0022)	0.0004 (0.0041)	0.0542 (0.0309)	0.0043 (0.0017)
Obs	588,153	588,318	588,318	588,318	588,318	532,717	588,318
Clusters	69,742	69,751	69,751	69,751	69,751	65,971	69,751
Pre-Reform Mean	0.6343	0.5144	0.0286	0.1413	0.3167	1.9186	0.1215

Notes: This table reports estimates from our event study and triple difference research designs. In Panel A, we estimate β_{post} from equation (2), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. In Panel B, we estimate γ_{post} from equation (3), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. The estimation samples vary across designs as described in the text. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for treatment firms in all years before 2018.

Table C.14: Robustness Tests

Outcome	Event Study Estimates				Triple Difference Estimates			
	(1) Investment Rate	(2) Leverage	(3) Debt Issuance	(4) Equity Issuance	(5) Investment Rate	(6) Leverage	(7) Debt Issuance	(8) Equity Issuance
Yr FE	0.0014 (0.0022)	0.0018 (0.0038)	0.0019 (0.0027)	0.0034 (0.0012)	-0.0002 (0.0024)	0.0012 (0.0044)	0.0022 (0.0026)	-0.0009 (0.0012)
Ind x Prof x Yr FE	0.0002 (0.0026)	0.0051 (0.0046)	0.0026 (0.0032)	0.0015 (0.0014)	0.0018 (0.0024)	0.0021 (0.0044)	0.0016 (0.0026)	-0.0010 (0.0012)
Controls	0.0009 (0.0026)	0.0040 (0.0045)	0.0027 (0.0031)	0.0021 (0.0014)	0.0018 (0.0024)	0.0021 (0.0044)	0.0016 (0.0026)	-0.0010 (0.0012)
Fixed Pre-Reform Scale	0.0044 (0.0026)	0.0051 (0.0045)	0.0030 (0.0031)	0.0034 (0.0014)	0.0004 (0.0025)	-0.0006 (0.0049)	0.0013 (0.0026)	-0.0011 (0.0012)
Winsorize at 99 th pctl	0.0044 (0.0056)	-0.0024 (0.0064)	-0.0001 (0.0049)	0.0100 (0.0050)	-0.0030 (0.0055)	-0.0010 (0.0070)	-0.0017 (0.0045)	-0.0041 (0.0018)
Drop Real Estate	0.0040 (0.0029)	0.0022 (0.0052)	0.0029 (0.0035)	0.0044 (0.0016)	0.0032 (0.0028)	0.0026 (0.0052)	0.0018 (0.0030)	0.0036 (0.0014)
Drop Auto Dealers	0.0021 (0.0026)	0.0017 (0.0047)	0.0015 (0.0031)	0.0027 (0.0015)	0.0005 (0.0025)	0.0000 (0.0046)	0.0008 (0.0026)	-0.0020 (0.0013)
Drop Aggregators	-0.0071 (0.0047)	0.0022 (0.0086)	0.0057 (0.0064)	0.0015 (0.0021)	-0.0019 (0.0043)	0.0038 (0.0079)	0.0044 (0.0052)	0.0006 (0.0017)
Balanced Panel	-0.0047 (0.0040)	-0.0007 (0.0069)	-0.0011 (0.0054)	0.0025 (0.0021)	-0.0037 (0.0035)	-0.0010 (0.0070)	-0.0012 (0.0042)	-0.0041 (0.0018)
Drop Largest	-0.0011 (0.0028)	0.0044 (0.0048)	0.0023 (0.0033)	0.0031 (0.0015)	-0.0016 (0.0027)	0.0096 (0.0048)	0.0020 (0.0028)	-0.0006 (0.0013)
Drop Multinationals	-0.0005 (0.0027)	0.0012 (0.0047)	0.0019 (0.0032)	0.0022 (0.0014)	-0.0015 (0.0026)	0.0004 (0.0047)	0.0005 (0.0028)	-0.0010 (0.0013)
Drop Bonus Ind	0.0040 (0.0029)	0.0002 (0.0052)	0.0020 (0.0035)	0.0044 (0.0016)	0.0020 (0.0028)	0.0005 (0.0051)	0.0016 (0.0030)	0.0030 (0.0014)
Most TCJA Exposed	0.0077 (0.0069)	-0.0129 (0.0149)	-0.0054 (0.0095)	0.0029 (0.0039)	0.0055 (0.0054)	-0.0068 (0.0104)	-0.0063 (0.0062)	0.0003 (0.0028)
Least TCJA Exposed	-0.0037 (0.0063)	0.0100 (0.0095)	0.0081 (0.0069)	0.0028 (0.0028)	-0.0051 (0.0071)	0.0111 (0.0132)	0.0039 (0.0078)	0.0042 (0.0031)
Asset Weighted	0.0006 (0.0056)	-0.0070 (0.0096)	-0.0116 (0.0074)	0.0007 (0.0058)	0.0117 (0.0092)	-0.0127 (0.0248)	-0.0083 (0.0090)	-0.0151 (0.0078)

Notes: This table reports robustness tests of event study estimates of β_{post} from equation (2) in columns (1)–(4) and triple difference estimates of γ_{post} from equation (3) in columns (5)–(8), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. Standard errors are clustered at the firm level and reported in parentheses.

Table C.15: Event Study and Triple Difference Effects Varying Asset Cutoff

Asset Cutoff	Event Study Estimates				Triple Difference Estimates			
	(1) Investment Rate	(2) Leverage	(3) Debt Issuance	(4) Equity Issuance	(5) Investment Rate	(6) Leverage	(7) Debt Issuance	(8) Equity Issuance
\$50,000	0.0016 (0.0026) 80,101	0.0020 (0.0046) 85,802	0.0023 (0.0031) 85,802	0.0029 (0.0014) 85,802	0.0007 (0.0024) 550,505	0.0024 (0.0044) 602,079	0.0021 (0.0026) 602,079	−0.0012 (0.0012) 602,079
\$100,000	0.0015 (0.0026) 79,150	0.0020 (0.0045) 84,737	0.0021 (0.0031) 84,737	0.0026 (0.0014) 84,737	0.0005 (0.0024) 538,576	0.0021 (0.0044) 588,318	0.0017 (0.0026) 588,318	−0.0014 (0.0012) 588,318
\$200,000	0.0023 (0.0026) 77,537	0.0009 (0.0045) 83,012	0.0014 (0.0031) 83,012	0.0026 (0.0014) 83,012	0.0009 (0.0024) 522,588	0.0014 (0.0044) 570,449	0.0013 (0.0026) 570,449	−0.0015 (0.0012) 570,449
\$1,000,000	0.0024 (0.0027) 68,645	−0.0035 (0.0047) 73,479	−0.0004 (0.0031) 73,479	0.0029 (0.0015) 73,479	0.0008 (0.0025) 455,511	−0.0042 (0.0044) 496,021	0.0007 (0.0025) 496,021	−0.0022 (0.0013) 496,021

Notes: This table reports event study estimates of β_{post} from equation (2) in columns (1)–(4) and triple difference estimates of γ_{post} from equation (3) in columns (5)–(8), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. Each set of rows reports coefficients, standard errors, and sample sizes from regressions run on a sample constructed with a different minimum asset cutoff, where \$100,000 is the cutoff in our baseline sample. Standard errors are clustered at the firm level and reported in parentheses.

Table C.16: Effects of 2022 Tightening on Investment and Financing

	(1)	(2)	(3)	(4)
Outcome	Investment Rate	Leverage	Debt Issuance	Equity Issuance
Panel A: Event Study Estimates				
β_{post}	0.0000 (0.0049)	0.0212 (0.0071)	0.0022 (0.0061)	-0.0052 (0.0027)
Obs	28,038	28,370	28,370	28,370
Clusters	4,847	4,890	4,890	4,890
Pre-Reform Mean	0.1211	0.4291	0.0132	0.0090
Panel B: Triple Difference Estimates				
β_{post}	-0.0045 (0.0046)	0.0018 (0.0064)	-0.0061 (0.0054)	-0.0036 (0.0025)
Obs	273,289	307,191	307,191	307,191
Clusters	46,867	52,839	52,839	52,839
Pre-Reform Mean	0.1211	0.4291	0.0132	0.0090

Notes: This table reports estimates from our event study and triple difference research designs, using a panel constructed to have a post-reform period of 2022-2023, and a pre-reform period before 2022. In the panel we define firms as big and high interest using 2019-2021 information, and using an EBIT definition of adjusted taxable income. In Panel A, we estimate β_{post} from equation (2), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. In Panel B, we estimate γ_{post} from equation (3), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. The estimation samples vary across designs as described in the text. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for treatment firms in all years before 2022.

D Additional Outcomes and Heterogeneity

In this appendix, we compare the cash flow impacts of an interest limitation with bonus depreciation, estimate the impacts of the limitation on taxes paid, confirm the salience of the policy by showing evidence of interest bunching at the limitation, and explore both additional heterogeneity cuts using financial constraint proxies and the impacts of the limitation on an additional set of expenditures that could be financed with debt.

First, we use a numerical example to demonstrate that the interest limitation should have much smaller immediate cash flow impacts than bonus depreciation. Consider a firm intending to purchase a \$1,000 piece of industrial machinery depreciated according to the Modified Accelerated Cost Recovery System (MACRS) on a seven year time line. The first two sets of rows in Table D.1 show the yearly depreciation deductions and tax benefits stemming from the capital expenditure under MACRS, and under 100% bonus depreciation, where the firm can immediately deduct the full cost of the capital expenditure. The firm receives an additional \$180 of immediate tax savings. If the expenditure was financed with a five year loan with an 8% interest rate, losing the interest deductions would only increase taxes by \$17 in the first year.

Table D.2 displays event study and triple difference estimates of the impacts of the interest limitation on interest deductions, net income, total interest, taxes paid, and changes in cash. Interest deductions decline by statistically significant amounts, and over the long-run, net incomes rise. However, this leads to only modest declines in the amount of total interest or increases in the amount of taxes paid. Our triple difference estimates suggest taxes paid increase by 0.2% of assets on average over the six years after the reform. This does not present a liquidity puzzle. We cannot reject declines in net cash that exceed 0.2% of assets over the same time horizon. Average pre-reform mean cash balances exceed 13% of assets, so the 0.2% decline only represents a 1.5% decline in cash. This evidence suggests the interest limitation causes small increases in taxes paid over the longer-run that are fully absorbed by firm's cash holdings.

While these effects represent small relative changes in cash, they represent large relative change in taxes paid because big, high interest firms facing the limitation pay relatively

few taxes to begin with, underscoring the importance of estimating taxes paid impacts over a longer-run horizon. Average taxes paid for big, high-interest firms is only 0.4% of assets before the limitation comes into effect, an order of magnitude smaller than the 2.7% of assets that lower-interest firms average. This indicates that there may not be large liquidity effects of the limitation for any subgroup of firms, because firms losing more interest deductions are also less likely to pay taxes. Figure D.1 repeats the exercise in Figure 8, showing average post-reform event study and triple difference estimates from separate regressions restricting to high-interest firms in each specific decile of *ex ante* interest in excess of limitation. As expected, the frequency and size of interest deductions disallowed increases roughly monotonically across deciles. Firms in the top distance to limit decile lose interest deductions equivalent to 4-5% of assets. However, there is no monotonic increase in taxes paid across deciles. Firms with higher levels of interest disallowed are much less likely to be paying taxes. Deductions exceed income slightly less than 50% of the time in the first two deciles, around 62% of the time for firms in the fifth and sixth deciles, and over 75% of the time in the top two deciles. Marginal tax rates for C- and S-corporations are zero 45% of the time in the bottom two deciles, 55% of the time in the fifth and six deciles, and over 75% of the time in the top two deciles. Overall, the correlation between high interest deductions and high total deductions means that firms losing the most interest deductions are also less likely to be paying taxes, muting the liquidity impacts of the interest limitation.

Despite the small average liquidity effects of the limitation, the policy is still salient. Figure D.2 plots the fraction of firms in our data by level of interest as a percent of the limitation bins, pooling all firms, and splitting them into adjusted taxable income (ATI) > 0 and ≤ 0 groups. The limitation is set at 30% of ATI plus interest income, so earnings factor into the limitation for positive ATI firms, while non-positive ATI firms face a limitation equal to their interest income. We find modest evidence of bunching in the interest relative to limitation distribution, and bunching is clearly concentrated among non-positive ATI firms, who do not need to account for transitory components of income that may be difficult to match interest expense to. This evidence suggests the policy is salient to firms, but is also consistent with the small total interest responses we display in Figure 6.

To explore whether typical proxies for financial constraints drive some impacts of the

interest limitation, we perform additional split sample heterogeneity analysis in Figure D.3, splitting high-interest firms into above and below median age, profitability, ratio of short-term to long-term debt, tangibility, and into groups of firms that do and do not pay dividends over 2015-2017. Youth, low profitability, low tangibility, and not paying dividends are all used as common proxies for financial constraints because younger firms often lack stable cash flows and credit histories, lower profitability firms have fewer incoming revenues to cover operating expenditures, firms not paying dividends cannot shrink their existing dividends to cover cash needs, and firms with low tangibility have less capital to pledge as collateral (Kaplan and Zingales, 1997; Lamont et al., 2001; Whited and Wu, 2006; Hadlock and Pierce, 2010; Zwick and Mahon, 2017; Liu and Mao, 2019; Saez et al., 2019; Cloyne et al., 2023). Across these splits of the data, we find no evidence of larger investment declines among firms not paying dividends, firms with lower profits, younger firms, or lower capital intensity firms. Turning to our financing outcomes, we find little evidence of economically significant leverage or debt issuance declines across these cuts of the data. While this could be explained by firms placing low value on future interest deductions (Edgerton, 2010; Zwick and Mahon, 2017), we also do not find clear borrowing declines among firms with a high ratio of short- to long-term debt, or firms without tax losses, despite the relative immediacy of their interest deductions.²⁸ As a last check to explore whether firms respond to changes in average rather than marginal costs, we split the sample by an *ex ante* measure of the fraction of total interest that each firm has disallowed, based on 2015-2017 characteristics. We find no evidence of larger average cost changes leading to larger investment or borrowing declines.

Finally, we explore whether the interest limitation leads to declines in other behavior that may be financed with debt: payouts to shareholders, payroll, and executive compensation. We display event study and triple difference estimates using these outcome variables in Table D.3. Consistent with the lack of leverage and debt issuance declines that we estimate, we find no evidence of statistically significant declines in any outcome using the event study design, and only find small but statistically significant declines in executive compensation using the triple difference design. Some estimates are actually positive, though the existence

²⁸An upwardly sloping yield curve implies the value of interest deductions may be smaller for short-term debt when the deductions are more immediate.

of pretrends suggests these effects are overstated.

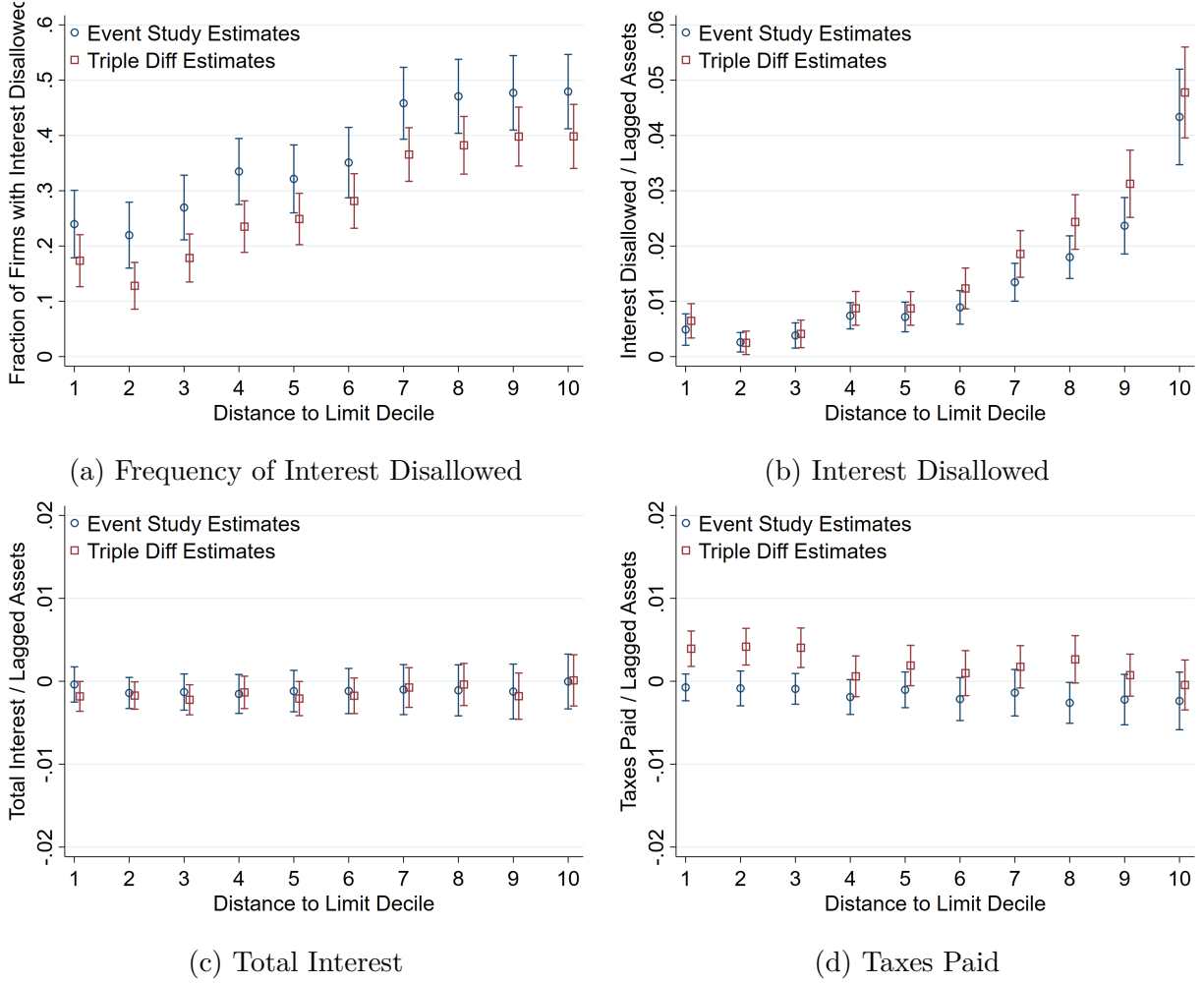


Figure D.1: Event Study and Triple Difference First Stage by Decile

Notes: This figure plots event study estimates of β_{post} from equation (2), and triple difference estimates of γ_{post} from equation (3), replacing the post-reform year indicators and interactions with a single post-reform indicator and interaction. Each estimate comes from a separate regression including only high interest firms in the relevant decile of the degree to which firms interest exceeds their limitation averaging over 2015-2017. The outcome variables across panels are an indicator for any interest disallowed, interest disallowed scaled by lagged assets, total interest scaled by lagged assets, and taxes paid scaled by lagged assets. 95% confidence intervals are constructed from standard errors clustered at the firm level, with Bonferroni corrections for multiple (ten) hypothesis testing.

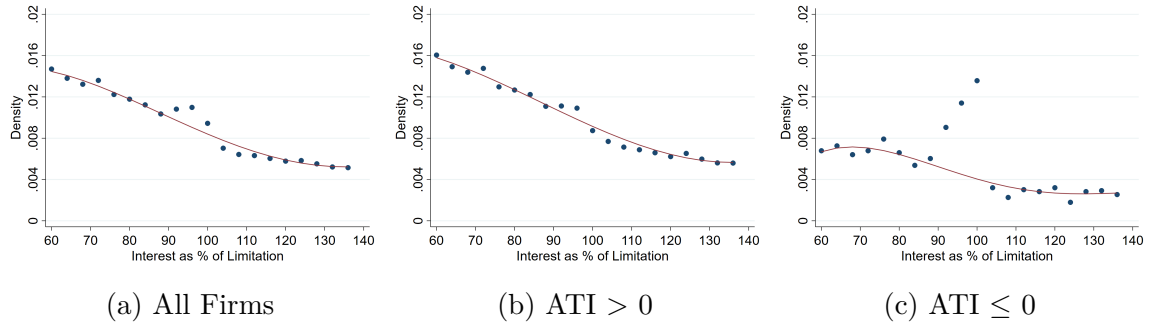


Figure D.2: Interest Relative to Limitation Bunching

Notes: This figure reports densities of total interest as a percentage of the limitation in 2018 and 2019 Form 990 data. Panel (a) plots the density for all firms in our data. Panel (b) plots the density for all firms with positive adjusted taxable income (ATI). Panel (c) plots the density for all firms with non-positive ATI. The plots include fourth degree polynomials fit through all points with interest below 88% and above 112% of the limitation.

Table D.1: Tax Savings on a \$1,000 Capital Expenditure

	Year							
	1	2	3	4	5	6	7	8
<i>Normal Depreciation</i>								
Depreciation deduction	\$143	\$245	\$175	\$125	\$89	\$89	\$89	\$45
Tax savings	\$30	\$51	\$37	\$26	\$19	\$19	\$19	\$9
<i>100% bonus depreciation</i>								
Depreciation deduction	\$1,000	\$0	\$0	\$0	\$0	\$0	\$0	\$0
Tax savings	\$210	\$0	\$0	\$0	\$0	\$0	\$0	\$0
<i>Interest deductions (5-year, 8% rate)</i>								
Interest deduction	\$80	\$80	\$80	\$80	\$80	\$0	\$0	\$0
Tax savings	\$17	\$17	\$17	\$17	\$17	\$0	\$0	\$0

Notes: This table illustrates tax savings on a \$1,000 capital expenditure depreciable according to the Modified Accelerated Cost Recovery System (MACRS) that has a seven year life, such as industrial machinery. MACRS uses a half-year convention for the purchase year and a 200 percent declining balance method (2× straight line until straight line is greater). We assume a 21% corporate tax rate. The first two sets of rows show tax savings under normal MACRS depreciation rules and 100% bonus depreciation. The last set of rows shows tax savings from interest deductions if the project was financed with a 5 year loan with an 8% interest rate. All dollar values are rounded to the nearest dollar.

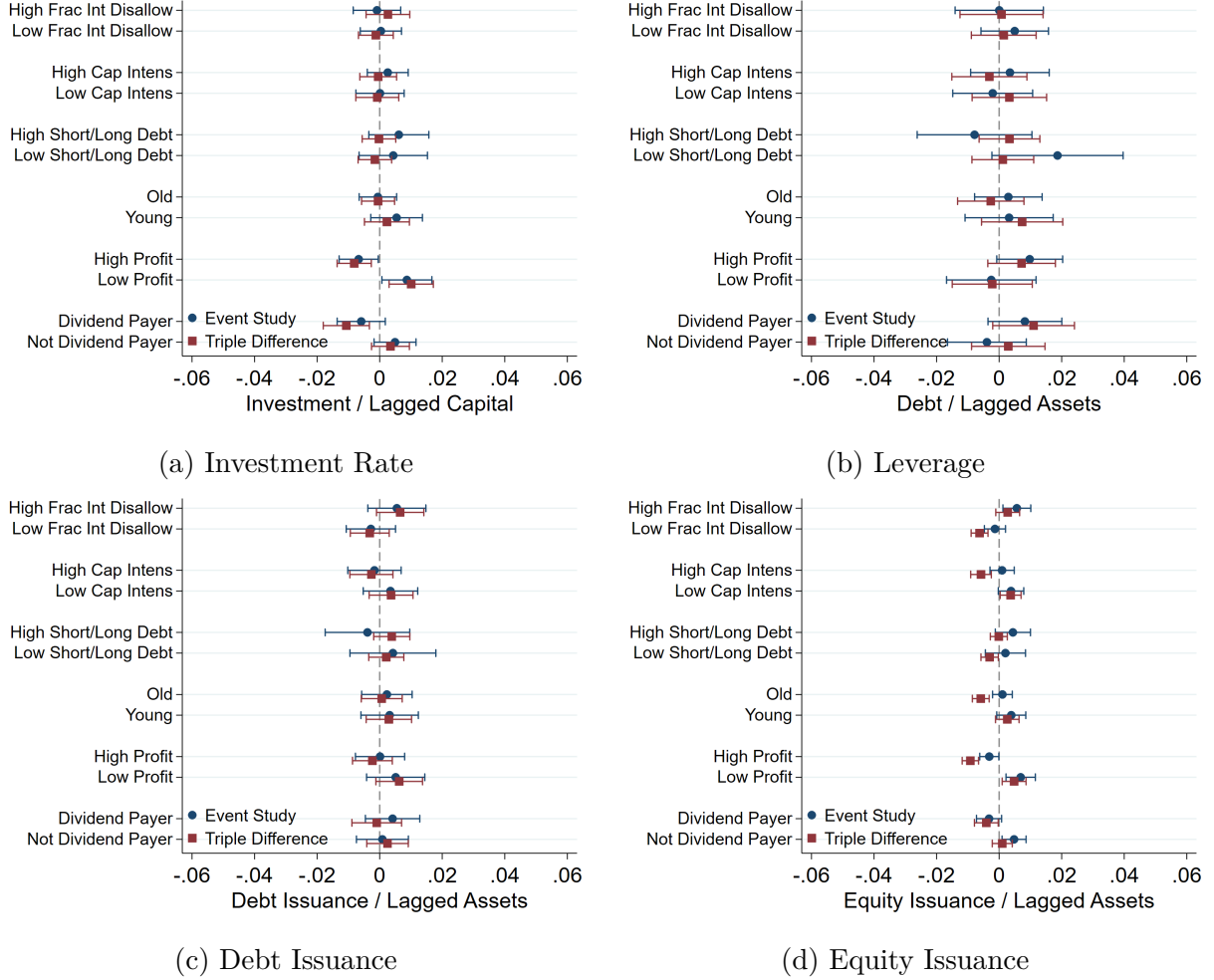


Figure D.3: Additional Heterogeneity

Notes: This figure plots event study estimates of β_{post} from equation (2), and triple difference estimates of γ_{post} from equation (3), replacing the post-reform year indicators and interactions with a single post-reform indicator and interaction. We display these estimates for subgroups of our event study and triple difference samples to explore heterogeneous impacts of the interest limitation. The four panels in order use investment scaled by lagged capital, debt scaled by lagged assets, debt issuance scaled by lagged assets, and equity issuance scaled by lagged assets as outcome variables. 95% confidence intervals are constructed from standard errors clustered at the firm level. Each heterogeneity split divides firms into above and below median for the high and low groups, except where mentioned otherwise in the text.

Table D.2: Effects of Interest Limitation on Interest Deductions, Taxes, and Cash

Outcome	(1) Int Deductions	(2) Net Income	(3) Total Int	(4) Taxes	(5) Δ Cash
Panel A: Event Study Estimates					
β_{post}	−0.0066 (0.0003)	−0.0007 (0.0018)	−0.0007 (0.0003)	−0.0012 (0.0002)	−0.0002 (0.0024)
Obs	84,737	84,737	84,737	43,146	84,737
Clusters	15,274	15,274	15,274	7,887	15,274
Pre-Reform Mean	0.0288	−0.0218	0.0292	0.0035	0.0067
Panel B: Triple Difference Estimates					
γ_{post}	−0.0055 (0.0003)	0.0035 (0.0019)	−0.0010 (0.0003)	0.0021 (0.0004)	−0.0033 (0.0020)
Obs	588,318	588,318	588,318	450,161	588,318
Clusters	69,751	69,751	69,751	53,045	69,751
Pre-Reform Mean	0.0287	−0.0222	0.0292	0.0039	0.0067

Notes: This table reports estimates from our event study and triple difference research designs. In Panel A, we estimate β_{post} from equation (2), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. In Panel B, we estimate γ_{post} from equation (3), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. The estimation samples vary across designs as described in the text. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for treatment firms in all years before 2018. All outcome variables are scaled by lagged total assets. The taxes outcome variable is only available for C- and S-corporations.

Table D.3: Effects of Interest Limitation on Payouts and Labor Compensation

Outcome	(1) Payouts	(2) Payroll	(3) Exec Comp
Panel A: Event Study Estimates			
β_{post}	0.0012 (0.0014)	0.0063 (0.0035)	0.0007 (0.0006)
Obs	84,737	84,737	84,737
Clusters	15,274	15,274	15,274
Pre-Reform Mean	0.0171	0.2551	0.0137
Panel B: Triple Difference Estimates			
γ_{post}	0.0053 (0.0017)	0.0082 (0.0033)	-0.0013 (0.0006)
Obs	588,318	588,318	588,318
Clusters	69,751	69,751	69,751
Pre-Reform Mean	0.0169	0.2565	0.0138

Notes: This table reports estimates from our event study and triple difference research designs. In Panel A, we estimate β_{post} from equation (2), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. In Panel B, we estimate γ_{post} from equation (3), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. The estimation samples vary across designs as described in the text. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for treatment firms in all years before 2018. Payouts, payroll, and executive compensation are scaled by lagged assets.

E Marginal Tax Benefits of Debt

Our marginal tax benefit of debt (MTBD) calculation builds directly on existing marginal effective tax rate (METR) calculations (Auerbach, 1983; Shevlin, 1990; Graham, 1996, 2000; Chodorow-Reich et al., 2026). For all of our MTBD calculations, we use a panel of SOI tax return data spanning 2005 to 2017 for firms that appear in our analysis data.

We begin by describing our implementation of the MTBD calculation for C-corporations. For each firm, we calculate the standard deviation of year-to-year changes in taxable income plus interest deductions (total income minus total deductions minus special deductions plus interest deductions), adjusted taxable income, and interest income, and simulate trajectories of each variable 20 years into the future. In the simulation, we start each firm at their observed level in 2017, and determine changes by drawing from a normal distribution with mean zero and each firm’s historical standard deviation for each variable.

We also simulate forward interest deductions. In an effort to obtain a more realistic interest deduction process, we use an AR(1) to simulate the ratio of interest expense to assets. Call that ratio x , then $x_t = \rho * x_{t-1} + (1 - \rho)\bar{x} + \varepsilon$, where ρ is a firm-specific autocorrelation of the ratio, $\bar{x}$ is the average of the ratio, and the innovation ε is a draw from a normal distribution with standard deviation equal to the standard deviation of the expense ratio times $\sqrt{1 - \rho^2}$.²⁹

Each firm begins the simulation with its current stock of net operating loss (NOL) carryforwards and can deduct them against income in future years. We abstract from NOL carrybacks because they are rare in the data and add substantial complexity (Zwick, 2021). The interest limitation also features its own carryforward system, independent of NOL carryforwards. In each year, we calculate each firms’ interest limitation as $0.3 * \text{ATI} + \text{Interest Income}$. Each firm begins the simulation with zero interest limitation carryforwards, but if interest deductions exceed the limitation, they are added to an interest limitation carryforward stock that can be deducted in future years when firms’ interest falls below their limitation. When carried forward interest deductions are used they reduce taxable income, either reducing

²⁹This ensures a reasonable unconditional variance from the simulated process. If $x_t = \rho x_{t-1} + \varepsilon_t$ follows a stationary AR(1) with $\varepsilon \sim \mathcal{N}(0, \sigma_\varepsilon^2)$, then the variance of the AR(1) innovation is $\sigma_\varepsilon^2 = \sigma_x^2(1 - \rho^2)$.

taxes paid or increasing firm stocks of NOL carryforwards.

We make two adjustments to simulated changes in taxable income plus interest deductions to account for the fact that losses are less persistent than gains (Chodorow-Reich et al., 2026). First, the standard deviation used to simulate changes in income is restricted to a minimum of eighty percent of the absolute value of base year income, ensuring a non-trivial chance of switching between positive and negative values. Second, in years directly after taxable income plus interest deductions is negative, we draw changes from a distribution where the standard deviation is doubled to better match asymmetric volatility following gains and losses. We also account for general business tax credits. To assign credit take up and amounts in the simulation, we calculate the share of years in which each firm claimed credits, then assign the log mean of credits available, where each simulated firm-year is assigned an indicator for claiming credits set to 1 with probability equal to the firm-specific historical take up rate.

Our tax policy environment in the simulations is focused on replicating post-TCJA policy with and without the interest limitation. Therefore, statutory corporate tax rates are set at 21% and post-TCJA NOL carryforwards can only reduce taxable income by 80%. Given our focus on domestic operations in the post-TCJA period, we do not consider the domestic production activities deduction or foreign tax credits. We focus on the 30% of EBITDA limit initially implemented by the TCJA and do not update the limit to 30% of EBIT in later simulation years. Therefore, our simulation exercise is likely to understate the impacts of the limitation on the marginal tax benefit of debt.

Following the process described above, we simulate interest, income, and the limitation forward 50 times over a 20 year period for each C-corporation in our sample and calculate the decline in the present value of taxes paid when interest deductions rise by 1% in the base year at various multiples of observed interest $\{0, 0.2, 0.4, 0.6, 0.8, 0.85, 0.90, 0.95, 1.0, 1.05, 1.10, 1.15, 1.2, 1.3, 1.4, 1.6, 1.8, 2.0\}$ for each firm under post-TCJA tax policy, with and without the limitation. We average over the 50 forward simulations for each firm to obtain a firm-specific marginal tax benefit of debt curve, which describes the marginal tax benefit of an additional dollar of interest at each multiple of observed interest. Figure 6 plots the average marginal tax benefit of debt curve for all big, high-interest firms in our data with

and without the limitation, interpolating through the calculated points at each multiple of interest.

We repeat this entire process for S-corporations with a few key modifications. S-corporation net operating losses are used by owners at the individual level, and Goodman et al. (2023) show the majority of S-corporation losses are used immediately. 68% are used in the year of the loss, and 87% are used within five years of the loss. Given these statistics, and the infeasibility of simulating each S-corporation owner income process, when S-corporations have an NOL, we assume they use 68% of the loss immediately, and 3.8% of the loss in each subsequent year until exhaustion. We assume S-corporations face a tax rate equal to the average entity-level tax rate across all big, high-interest S-corporations after TCJA in our data, a rate of 19.1%, constructed by averaging over owner-specific marginal tax rates for each entity. We describe the construction of S-corporation tax rates in more detail in Appendix F. We also abstract from general business credits for S-corporations because they are applied at the owner level.

We do not extend this procedure to partnerships because of the intractability of simulating income through tiered structures that obscure the ultimate ownership of income streams (Cooper et al., 2016), and the lack of empirical work documenting how and when partnerships use losses.

To verify the accuracy of our simulations, we use all years before 2016 as a pre-period, simulate forward from 2015 to 2016, and compare simulated 2016 values to realized 2016 values in the data. Figure E.1 and E.2 compare simulated and actual 2016 values of the log of taxable income before interest, adjusted taxable income, interest deductions, and interest income, for C- and S-corporations respectively. Each figure is a binned scatter plot of simulated versus actual values, with a 45 degree line overlaid as a reference representing perfectly accurate prediction. Our forward simulation fits realized data values well, though we do slightly overshoot the amount of interest income for both C- and S-corporations.

E.1 C- and S-Corporation Results

Given the computational challenges associated with extending our marginal tax benefit of debt calculation to partnerships, we instead calculate marginal tax benefits for C- and S-

corporations and show that our empirical results look very similar when restricting to C- and S-corporations. Table E.1 displays event study and triple difference estimates of the impacts of the interest limitation on the extensive and intensive margin of interest disallowed, total interest, the investment rate, leverage, debt and equity issuance, and cash changes. The estimates look similar to our estimates that include partnerships, suggesting restricting the marginal tax benefit of debt calculation to only C-corporations and S-corporations is unlikely to effect our results.

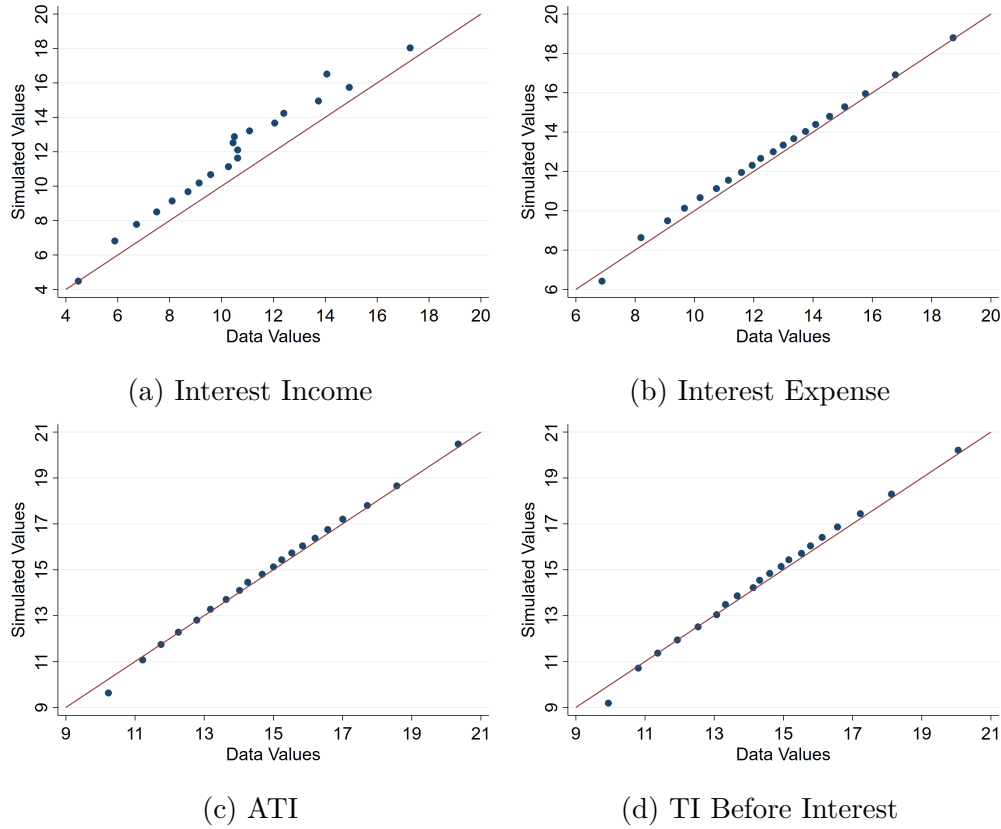


Figure E.1: C-Corporation Forward Simulation Validation

Notes: This figure plots simulated and actual 2016 values of interest income, interest expense, adjusted taxable income, and taxable income before interest from our marginal effective tax rate simulation for C-corporations. Each panel includes a 45 degree line.

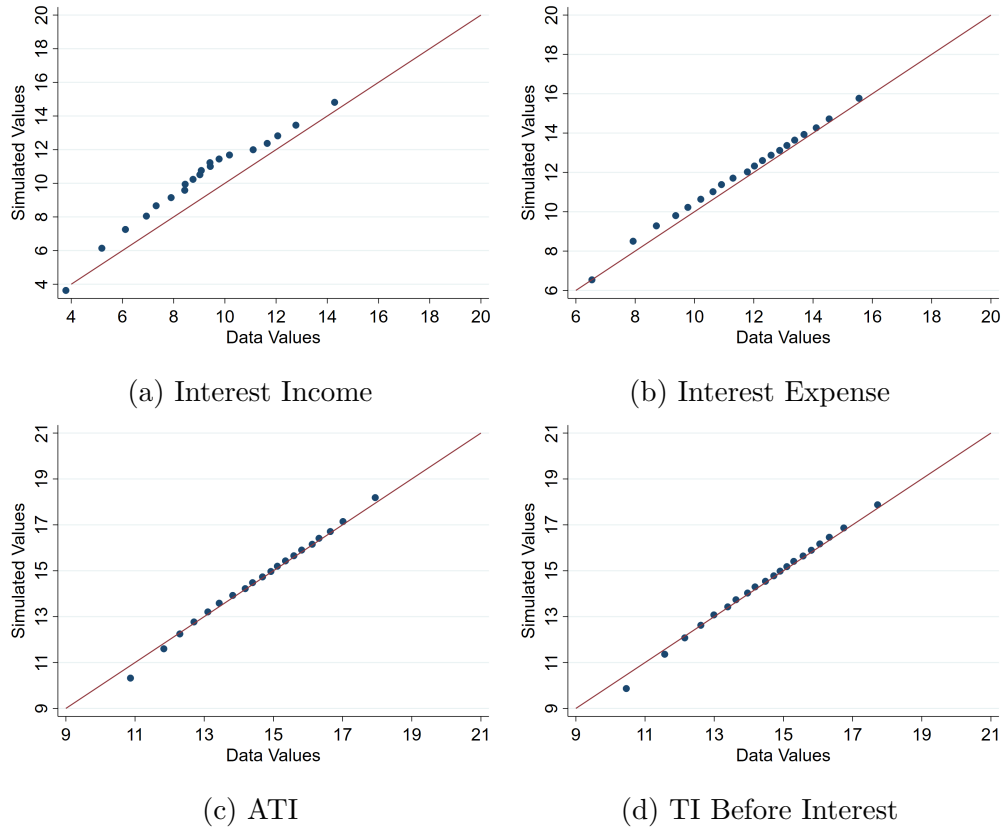


Figure E.2: S-Corporation Forward Simulation Validation

Notes: This figure plots simulated and actual 2016 values of interest income, interest expense, adjusted taxable income, and taxable income before interest from our marginal effective tax rate simulation for S-corporations. Each panel includes a 45 degree line.

Table E.1: Effects of Interest Limitation Excluding Partnerships

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	1 (Int Disallow)	Int Disallow	Total Int	Investment Rate	Leverage	Debt Issuance	Equity Issuance	Δ Cash
Panel A: Event Study Estimates								
β_{post}	0.4347 (0.0081)	0.0165 (0.0006)	-0.0011 (0.0004)	0.0061 (0.0030)	-0.0016 (0.0057)	-0.0014 (0.0038)	0.0043 (0.0017)	0.0005 (0.0030)
Obs	62,329	62,329	62,329	57,807	62,329	62,329	62,329	62,329
Clusters	11,010	11,010	11,010	10,166	11,010	11,010	11,010	11,010
Pre-Reform Mean	0.0000	0.0000	0.0284	0.1160	0.4737	0.0260	0.0213	0.0070
Panel B: Triple Difference Estimates								
β_{post}	0.3013 (0.0068)	0.0177 (0.0007)	-0.0011 (0.0003)	0.0035 (0.0028)	0.0036 (0.0053)	-0.0014 (0.0031)	0.0013 (0.0014)	-0.0026 (0.0025)
Obs	460,437	460,437	460,437	425,891	460,437	460,437	460,437	460,437
Clusters	53,472	53,472	53,472	49,686	53,472	53,472	53,472	53,472
Pre-Reform Mean	0.0000	0.0000	0.0284	0.1161	0.4717	0.0259	0.0214	0.0069

Notes: This table reports estimates from our event study and triple difference research designs, excluding all partnerships from the estimation samples. In Panel A, we estimate β_{post} from equation (2), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. In Panel B, we estimate γ_{post} from equation (3), replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. The estimation samples vary across designs as described in the text. Standard errors are clustered at the firm level and reported in parentheses. The pre-reform mean is the average value of the outcome variable in each column for treatment firms in all years before 2018.

F The User Cost of Capital and Elasticities

We take a data driven approach to measuring the user cost of capital (UCC), generalizing the typical Hall and Jorgenson (1967) expression for the cost of capital to account for debt and equity financing

$$(F.1) \quad UCC_{ijt} = (\rho_{it} + \delta) \frac{1 - \tau_{it} z_{jt}}{1 - \tau_{it}},$$

$$(F.2) \quad \rho_{ijt} = w_{it}^d (1 - \tau_{it} \mathbb{1}(Allow)_{it}) r_{it} + w_{it}^e E.$$

Using our data to measure the UCC allows us to construct a measure that varies at the firm, industry, and year level. For portions of the UCC expression that are not critical for our variation (depreciation rate δ and equity costs E), we use common static calibrations. We measure z as the interaction of the current year bonus depreciation fraction with average investment duration at the 4 digit industry-by-year level from Zwick and Mahon (2017).

We measure marginal tax rates on a firm-year level, using statutory tax rates based on taxable income for C-corporations, and entity level weighted averages of S-corporation owner marginal tax rates for S-corporations. For partnerships, we use top individual statutory tax rates in each year. Breaking down financing costs ρ_{it} , we measure each firms' debt financing fraction in each year w_{it}^d as the ratio of all liabilities to assets, the equity financing fraction $w_{it}^e = 1 - w_{it}^d$, and the interest rate r_{it} as the ratio of total interest to total interest bearing liabilities.³⁰ $\mathbb{1}(Allow)_{it} = 1$ if a firm does not have interest disallowed, and 0 otherwise. We impose a value of 0 for all big, high-interest treatment firms to obtain ITT elasticities, and use a firm-by-year level measure of $\mathbb{1}(Allow)_{it}$ to obtain TOT elasticities. Table F.1 summarizes our measurement of each UCC parameter.

We use our UCC measure to construct elasticity estimates following equation (5). the UCC enters the elasticity via the percent change in UCC term for treatment relative to control firms. To assess the sensitivity of our UCC elasticity estimates to different constructions, we recalculate investment rate UCC elasticities based on our event study estimates but

³⁰For firms with no interest bearing liabilities we impute interest rates as the median within the 4-digit industry.

using different UCC constructions in Table F.2. Column 1 displays our baseline estimates. Column 2 uses the ratio of all interest bearing liabilities to the sum of all interest bearing liabilities and all paid in capital as the debt financing fraction instead of the ratio of all liabilities to assets. This alternative debt financing fraction measure focuses specifically on interest bearing liabilities that are relevant for the interest limitation, and directly measures total equity financing within the firm. In column 3, we assume a higher depreciation rate of 0.12. In column 4, we assume higher equity flotation costs of 0.107 (estimated flotation costs for small firms in Hennessy and Whited (2007)). In column 5 we assume both higher depreciation rates and higher equity flotation costs. In column 6, we use the top individual statutory tax rate as S-corporation’s marginal tax rate rather than weighted averages of owner marginal tax rates. The alternative construction in column 5 yields the largest changes in UCC elasticities, inflating our estimates by roughly 35%, but the high equity flotation cost is unlikely to be realistic for our large treatment firms. Other alternative assumptions inflate UCC elasticity estimates by less, while using top individual marginal tax rates for S-corporations shrinks our elasticity estimates by roughly 12%.

We also show year by year event study and triple difference coefficient estimates using the user cost of capital as an outcome in Figure F.1. The user cost of capital exhibits a clear increase for treatment relative to control firms when the interest limitation goes into effect.

Figure F.2 plots treatment-on-the-treated investment rate UCC elasticities while varying assumed debt-financing fractions as in Figure 7. Even when we inflate our estimates to account for imperfect persistence, using our event study (triple difference) estimates we only fail to reject elasticities of -2 or below when debt financing fractions drop below 22% (42%).

F.1 Accounting for Interest Limitation Carryforwards

Disallowed interest deductions can be carried forward and used in future years when firms’ interest falls below their limitation. Using a first-in, first-out (FIFO) assumption, we calculate the use rates of carried forward interest deductions using information from Form 8990. Conceptually, suppose we observe \$1,000 in interest disallowed in year t . We calculate how much of that \$1,000 is used as an interest deduction in year $t + 1, t + 2, \dots, t + 5$, while assuming that all interest disallowed in year t is used before any interest that may be disallowed in

future years. Interest is disallowed for the first time in 2018, and our data extends through 2023, so we can observe use rates through five future years for 2018 interest, four future years for 2019 interest, etc.

Table F.3 displays the average share of interest disallowed that is used one through five years after the interest is disallowed for C-corporations, S-corporations, and partnerships in the first 5 columns. In the sixth column, the table shows the net present value of each dollar of interest deduction disallowed using a 6% discount rate, accounting for originally disallowed interest used over the next five years.³¹ On an NPV basis, each dollar of C-corporation interest disallowed only results in a loss of \$0.66 worth of interest deductions, while the same number is \$0.55 for S-corporations and \$0.76 for partnerships. Even applying the conservative S-corporation number to inflate our elasticity estimates by $(1/0.55)$, our event study (triple difference) estimates still reject elasticities below -0.62 (-0.93), well outside the accepted range in the literature of $[-2,-8]$ (Chodorow-Reich, 2026). Therefore, an overstatement of UCC changes due to interest limitation carryforwards does not appear to explain differences between our elasticities and estimates from different settings using tax term variation in the UCC. We also account for carryforwards in our calculation of the marginal tax benefit of debt in Appendix E.

Table F.1: User Cost of Capital Parameters

Parameter	Value	Source
$\mathbb{1}(Allow)$	$\{0,1\}$	Our Data
r	Interest Rate	Our Data
w_d	Liabilities / Assets	Our Data
E	0.066	OTA (2014)
δ	0.08	Sanati (2022)
τ	C-, S-corps: marginal rate, Pships: top individual rate	Our Data
z	Varies at 4-digit NAICS level	Zwick and Mahon (2017)

Notes: This table describes the parameters used to construct our user cost of capital measure and their sources. OTA (2014) refers to Office of Tax Analysis, US Department of the Treasury (2014).

³¹Given declining use rates over time, we assume no carryforward use after the fifth year.

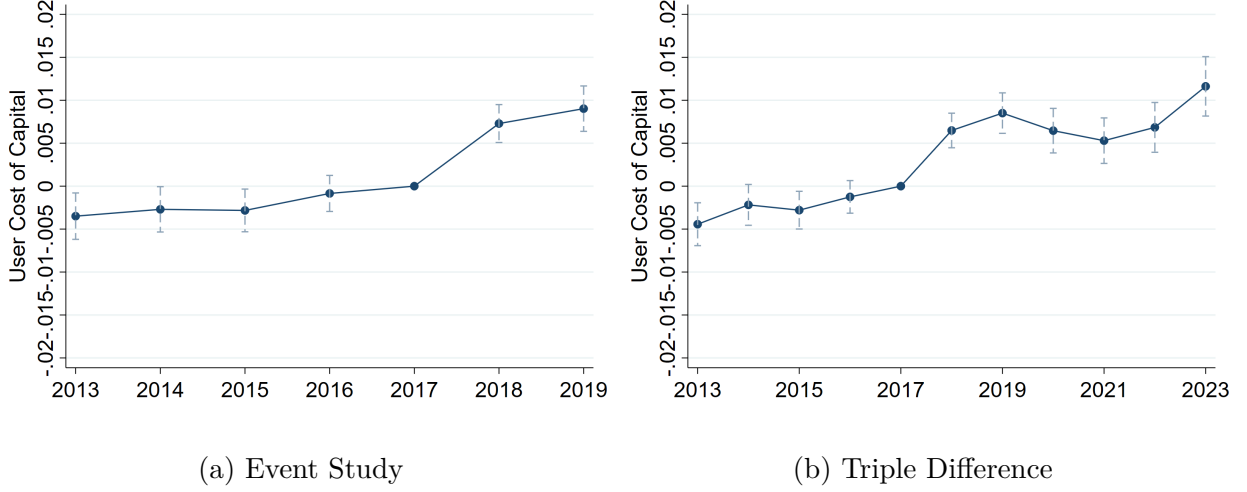


Figure F.1: User Cost of Capital Event Study and Triple Difference Estimates

Notes: This figure plots event study estimates of β_e from equation (2) and triple difference estimates of γ_e from equation (3) using the user cost of capital as an outcome variable. 95% confidence intervals are constructed from standard errors clustered at the firm level.

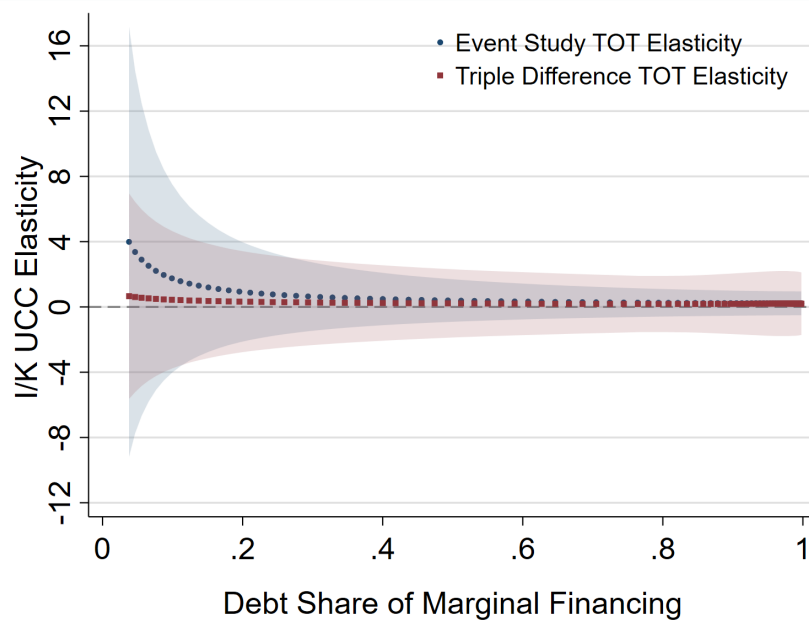


Figure F.2: TOT Investment Rate User Cost of Capital Elasticities

Notes: This figure plots treatment-on-the-treated (TOT) investment rate user cost of capital elasticities implied by our average, post-reform event study and triple difference estimates, varying the assumed fraction of marginal financing that comes from debt, as in Figure 7. 95% confidence intervals are calculated from standard errors clustered at the firm level, and represented by the shaded areas.

Table F.2: Investment Rate UCC Elasticity Sensitivity to UCC Parameters

	(1)	(2)	(3)	(4)	(5)	(6)
β_{post}	0.0015 (0.0026)	0.0015 (0.0026)	0.0015 (0.0026)	0.0015 (0.0026)	0.0015 (0.0026)	0.0015 (0.0026)
UCC Pct Change	0.0877	0.0792	0.0685	0.0818	0.0648	0.0996
ε	0.1477 (0.2487)	0.1634 (0.2751)	0.1890 (0.3182)	0.1582 (0.2664)	0.1997 (0.3363)	0.1299 (0.2188)
S-corporation τ	Firm-Level	Firm-Level	Firm-Level	Firm-Level	Firm-Level	Top Indiv
Debt Fraction	$\frac{\text{Liabilities}}{\text{Assets}}$	$\frac{\text{Int Bearing Liab}}{\text{Int Bearing Liab} + \text{Equity}}$	$\frac{\text{Liabilities}}{\text{Assets}}$	$\frac{\text{Liabilities}}{\text{Assets}}$	$\frac{\text{Liabilities}}{\text{Assets}}$	$\frac{\text{Liabilities}}{\text{Assets}}$
E	0.066	0.066	0.066	0.107	0.107	0.066
δ	0.080	0.080	0.120	0.080	0.120	0.080

Notes: This table reports investment rate UCC elasticity estimates while varying user cost of capital parameters. The first row displays event study estimates of β_{post} from equation (2) using the investment rate as an outcome, replacing post-reform year indicators and interactions with a single post-reform indicator and interaction. The estimation sample includes all high-interest firms in our panel data. Standard errors are clustered at the firm level and reported in parentheses. UCC Pct Change is the percent change in the user cost of capital, calculated as the percent change in the user cost of capital for treatment relative to control firms. We calculate ε as the investment rate coefficient estimate divided by the pre-reform investment rate mean, divided by the percent change in the UCC. The first column uses our baseline user cost construction. The second column uses an alternative measure of the debt financing fraction, the ratio of debt plus loans from stockholders to debt plus loans from stockholders plus total paid in capital. The third column assumes a higher depreciation rate of 0.12. The fourth column assumes higher equity flotation costs of 0.107. The fifth column assumes a higher depreciation rate and flotation cost. The sixth column use the top individual tax rate for S-corporations instead of firm-level weighted averages of owner marginal tax rates.

Table F.3: Share of Disallowed Interest Deductions Used

	(1)	(2)	(3)	(4)	(5)	(6)
	Relative Year Use Rate					
Entity Type	$t + 1$	$t + 2$	$t + 3$	$t + 4$	$t + 5$	NPV of Disallowed Interest
C-Corporations	0.12	0.13	0.06	0.04	0.03	0.66
S-Corporations	0.24	0.13	0.05	0.04	0.02	0.55
Partnerships	0.09	0.07	0.04	0.05	0.01	0.76

Notes: This table reports the rate at which disallowed interest deductions are used via the interest limitation carryforward system in each year relative to the year interest is first disallowed. Columns 1-5 report the share of interest disallowed that is carried forward and used in relative year $t + k$ by each respective entity type. The last year of our data is 2023, so we examine average use rates in $t + 1$ for interest disallowed in 2018-2022, average use rates in $t + 2$ for interest disallowed in 2018-2021, average use rates in $t + 3$ for interest disallowed in 2018-2020, average use rates in $t + 4$ for interest disallowed in 2018-2019, and average use rates in $t + 5$ for interest disallowed in 2018. We calculate use rates utilizing a first-in, first-out accounting method. Column 6 displays how much interest disallowed is actually lost on a net present value basis using a 6% discount rate, assuming no interest is used via carryforwards after 5 years.