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

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Abstract

This paper studies the impacts of limiting interest deductions on firms' investment and financing choices using U.S. tax data. The 2017 Tax Cuts and Jobs Act capped interest deductions for big, high-interest firms. Using three research designs exploiting this variation, we find evidence that the interest limitation had no impact on investment or debt issuance and caused some increase in equity issuance. Our results suggest that firms primarily use cash to finance new investment and place low value on future interest deductions, such that limiting interest deductions is unlikely to have large impacts on investment or substantially reduce corporate borrowing.

*This work expresses the views of the authors themselves and does not necessarily reflect the views of the Internal Revenue Service (IRS) or U.S. Treasury Department. All data analysis for this project involving confidential taxpayer information was done by IRS or Treasury employees, on IRS computers, and at no time was confidential taxpayer information outside of the IRS computing environment. All results have been reviewed to ensure that no confidential information is disclosed. We thank seminar participants at the Office of Tax Analysis, Princeton, Maryland, MIT, and Hoover for useful feedback. We also thank Eduard Boehm, Mara Faccio, Michael Faulkender, Kilian Huber, Henrik Kleven, 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

Since Modigliani and Miller (1958, 1963), economists have studied how firms make investment and financing choices, and in particular, how taxes influence these choices. A key channel through which taxes may impact firm decision-making is the asymmetric tax treatment of debt and equity, as most corporate tax codes around the world allow firms to deduct interest expense on their debt. Both the theoretical and empirical literatures have argued that this tax advantage is a first-order reason for firms to borrow, lowering costs of capital for firms using debt financing. As a result, proponents of interest deductions suggest they provide an incentive for investment and growth, while opponents argue interest deductions narrow the tax base while encouraging high levels of borrowing that increase macroeconomic risk.[1] Assessing these arguments requires empirically measuring the impacts of interest deductions.

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 interest limitation only applies to firms with more than $25 million in average receipts over the three previous years. Exploiting the fact that the interest limitation applies to big but not small high-interest firms, we use complementary event study, triple difference, and regression discontinuity designs to evaluate the impacts of the interest limitation focusing on the first two years after implementation.

Our first research design is an event study approach that compares outcome trends for big, high-interest firms that *ex ante* face the interest limitation to small, high-interest firms that do not. We classify firms as big and high interest if from 2015-2017 their average receipts

[1]Concerns 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, Laeven and Moreno, 2022).

[2]Prior 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).

exceed $25 million and their average interest exceeds their limitation. Using our event study design, we estimate that the interest limitation does not have a statistically or economically significant impact on investment, debt issuance, or cash changes. We also find that firms respond to the interest limitation by increasing their equity issuance.

One concern with our event study design is that other tax changes included in the TCJA could differentially impact big and small firms, biasing our event study estimates. We address this concern with our triple difference design. The triple difference compares big and small high-interest firms, but nets out any differential outcome trends between big and small low-interest firms that face other TCJA changes, and possible contemporaneous size-varying shocks, but not the interest limitation. Our triple difference estimates are strikingly similar to our event study estimates, corroborating our findings.

Both the event study and triple difference designs allow us to measure average investment and financing responses to the interest limitation. While we find no evidence of event study pretrends and the placebo test implemented by the triple difference indicates no differential changes in firm outcomes by size, both rely on ultimately untestable assumptions of parallel trends in investment and financing behavior for firms of different sizes. To estimate the causal effect of the interest limitation while relying on a weaker set of assumptions, we also implement a regression discontinuity (RD). The RD design measures the impact of the interest limitation on the marginal firm that is just large enough to face the limitation, and only requires that firms do not manipulate their pre-reform receipts to end up below the $25 million cutoff. Our RD estimates, although less precise than the event study and triple difference estimates because identification is driven by the smaller number of firms close to the $25 million lagged receipts cutoff, are consistent with our other results. We cannot rule out zero impacts of the interest limitation on investment and financing choices, nor can we reject tests of equality between our RD results and results from the other two designs.

Our results 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 fixed effects, polynomials, and bandwidths), samples (unbalanced and balanced), and outcome variables (stocks and flows, alternative scalings). We also find null effects on other potential margins of response including shareholder payouts, payrolls, and executive compensation.

While internally valid, our core results do not necessarily apply to smaller firms or firms with lower interest that could face an expanded limitation. However, the similarity of our estimates across designs, and in particular the RD comparing firms narrowly on either side of the receipts threshold, suggests responses for smaller and larger firms are unlikely to differ. To address potential responses of lower interest firms, we re-estimate our main event study design while dividing firms into deciles based on the degree to which their interest exceeds the 30% of earnings limit. We find null investment and debt issuance responses in every distance-to-limit decile and no trend in estimates across deciles despite significant variation in capital structure across deciles. The lack of variation in responses across deciles suggests responses for lower interest firms are also unlikely to differ from our core estimates.

The quasi-experimental results in this paper present a puzzle for standard theories of firm investment and borrowing choices. Neoclassical investment theory suggests that an increase in firm's weighted average cost of capital (WACC) should lead to a decline in investment (Hall and Jorgenson, 1967), while tradeoff theory suggests limiting the tax benefits of debt should lead to a decline in borrowing (Frank and Goyal, 2008). Our evidence contradicts both predictions, and is not consistent with alternative mechanisms including high hurdle rates, fixed adjustment costs for investment or borrowing, a leverage ratchet effect, or creditor evergreening.[3] The shortcomings of these potential explanations raise questions about how firms are financing investment projects and how they are making capital structure choices.

We argue the lack of investment declines we estimate in response to the interest limitation suggest that firms are primarily using cash to finance new investment. Firms make investment choices on the project level, and financing for each individual project could come from debt, equity, or cash. The interest limitation raises WACCs by increasing the cost of debt, but will not increase the cost of a new investment project if the project is not financed with debt. The null investment responses we estimate to the interest limitation suggest the cost of new investment projects does not change, and therefore that new investment projects are not financed with debt. In addition, equity issuance is too infrequent to generally provide

[3]For explanations of these various mechanisms, see Graham and Harvey (2001); Gormsen and Huber (2024); Cooper and Haltiwanger (2006); Chen, Jiang, Liu, Suarez-Serrato and Xu (2023); Leary and Roberts (2005); Admati, DeMarzo, Hellwig and Pfleiderer (2018); Faria-e-Castro, Paul and Sánchez (2024).

year-to-year financing of new projects. Big, high-interest firms only issue equity in 33% of all firm-years before the reform, but make some positive investment in 92% of firm-years. The remaining option is that firms use cash to finance new investment projects, a conclusion consistent with a pecking order, and existing empirical and survey evidence for U.S. firms (Yagan, 2015; Sharpe and Suarez, 2021).

If firms primarily finance investment projects with cash, what are the impacts of the interest limitation on firms with less cash flexibility? Additional heterogeneity analysis suggests that any equity issuance response we do observe to the limitation is concentrated in firms with less cash flexibility that need to turn to other forms of financing to continue investing. Using common proxies for financial constraints to identify firms that are likely to have less cash flexibility, we find younger firms, lower profit firms, and firms not paying dividends do not decrease investment in response to the interest limitation, but do issue more equity. In our setting, firms that appear financially constrained mitigate potential investment impacts of the limitation by issuing equity, reinforcing that common proxies for financial constraints do not always identify firms that cannot access external financing (Farre-Mensa and Ljungqvist, 2016).

To explain the lack of borrowing declines in response to the interest limitation, we argue that firms place low value on future interest tax shields. Businesses are only sophisticated to a degree when making decisions.[4] Existing research shows that heuristics and operational constraints play significant roles in firm decision-making (Jagannathan, Matsa, Meier and Tarhan, 2016; Gormsen and Huber, 2024), while firms sharply discount, or even ignore, future tax benefits (Edgerton, 2010; Zwick and Mahon, 2017). However, leading capital structure theories leave no room for low valuations of future interest tax shields. Static tradeoff theory does not account for the fact that interest deductions arrive in the future, while modern dynamic models with endogenous investment and financing choices often assume interest is deducted when borrowing occurs for tractability (Glover, Gomes and Yaron, 2015; Ivanov, Pettit and Whited, 2024). Both theoretical frameworks suggest borrowing should decline when firms lose interest deductions, but do not account for the fact that firms may not value future interest tax shields.

[4]This mechanism can also help explain the muted investment response.

Additional evidence supports our interpretation that firms place low value on future tax shields. First, we find suggestive evidence of larger debt issuance declines among firms without tax losses that pay taxes today. Second, splitting our event study sample into above and below median interest rate firms, we find evidence of small but statistically significant borrowing declines among the highest interest rate firms, suggesting interest deductions are more salient for firms paying the highest interest rates, or that discounting drives valuations of future interest deductions down, but not to zero when they hold substantial value.

Our paper contributes to a broad literature that studies the impacts of corporate taxes on firm investment.[5] Existing work suggests that investment responds to changes in the cost of capital, often estimating investment rate cost of capital elasticities around negative two when using samples including publicly- and privately-held firms (Chen, Jiang, Liu, Suarez-Serrato and Xu, 2023). In contrast, using our event study design, we estimate an elasticity of 0.00 with a 95% confidence interval spanning [−0.72, 0.73]. Furthermore, instrumental variable regressions suggest there is no direct relationship between interest limitation induced variation in financing costs and investment. Our estimates are substantially smaller than others in the literature because previous research focuses on changes in the tax rate and investment incentives that modify the after-tax price of all investment, while the interest limitation only changes the after-tax price of debt-financed investment.

This research also contributes to the empirical literature that tests 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.[6] One notable exception is Ivanov, Pettit and Whited (2024), who find a

[5] Contributions include Hall and Jorgenson (1967); Hassett and Hubbard (2002); Desai and Goolsbee (2004); House and Shapiro (2008); Edgerton (2010); Zwick and Mahon (2017); Ohrn (2018); Liu and Mao (2019); Giroud and Rauh (2019); Maffini, Xing and Devereux (2019); Dobridge, Landefeld and Mortenson (2021); Kennedy, Dobridge, Landefeld and Mortenson (2024); Chen, Jiang, Liu, Suarez-Serrato and Xu (2023); Curtis, Garrett, Ohrn, Roberts and Suarez-Serrato (2023); Duan and Moon (2023).

[6] See MacKie-Mason (1990); Rajan and Zingales (1995); Graham (1996); Booth, Aivazian, Demirguc-Kunt and Maksimovic (2002); Heider and Ljungqvist (2015); Faccio and Xu (2015). A related literature studies the relationship between debt and taxes in multinational firms. Many countries have implemented regulations that attempt to limit multinationals' interest deductions stemming from inter-subsidiary lending that facilitates profit shifting (Desai, Foley and Hines, 2004; Blouin, Huizinga, Laeven and Nicodeme, 2014; Alberternst and Sureth-Sloane, 2016; Bilicka, Qi and Xing, 2022). In contrast to these narrowly tailored limitations, the interest limitation we study applies more broadly to domestic and international firms.

negative relationship between debt and U.S. state tax rate changes for smaller private firms, highlighting that changes in the tax rate change the marginal benefits and costs of debt by altering the value of interest deductions, the after-tax value of all profits, 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.

Two other papers study the U.S. interest limitation and find large investment and borrowing responses to the policy. First, Carrizosa, Gaertner and Lynch (2022) use an event study design comparing big, high-interest to big, low-interest firms in Compustat. We also estimate large post-reform borrowing declines in our data using this comparison, but show these estimates appear to be driven by mean reversion, not a response to the limitation. Second, Sanati and Beyhaghi (2024) use an RD design in Compustat and Y-14Q data. Relative to these other data sources we possess a much 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 interest limitation, each critical for relying on variation around a discontinuity.

The rest of the paper proceeds as follows. In section 2, we describe the interest limitation and other relevant tax variation. In section 3, we describe the tax data and show summary statistics. Section 4 presents event study and triple difference estimates of the impact of the interest limitation. Section 5 presents our RD estimates, and section 6 presents subsample analysis. Section 7 discusses the implications of our results for theories of investment and financing, and section 8 concludes.

2 Tax Policy Background

2.1 The Interest Limitation

In December 2017, the United States passed a major tax reform commonly referred to as the Tax Cuts and Jobs Act (TCJA) that introduced a limitation on interest deductions. The limitation stipulates that interest deductions in a given year cannot exceed 30% of a firm's adjusted taxable income plus interest income. When the law was written, adjusted taxable income was defined as net income before interest expense and interest income, depreciation, depletion and amortization, roughly equivalent to the accounting concept of earnings before interest, taxes, depreciation, and amortization (EBITDA). TCJA also provided that, beginning in 2023, the definition of adjusted taxable income would no longer add back depreciation, depletion, and amortization, bringing adjusted taxable 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. Therefore, among firms with interest expense greater than 30% of adjusted taxable income, 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 directly raises the cost of debt financing, increasing firm's weighted average cost of capital, by disallowing interest deductions. With interest rate r and tax rate τ, the interest limitation increases the marginal cost of borrowing from $1 + (1 - \tau)r$ (in quadrants A, C, and D in Figure 1) to $1 + r$ (in quadrant B). Going forward, the big, high-interest firms in quadrant B will not be allowed to deduct interest payments associated with new investment projects. The limitation also expands the tax base, resulting in more taxes paid today.

There are some exceptions to the general interest limitation rules. Small businesses that are deemed to be tax shelters still face the interest limitation even if their lagged receipts are below the \$25 million threshold, meaning that some small firms face the interest limitation.[7] In addition, businesses with agriculture and real estate components can opt out

[7] The 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.

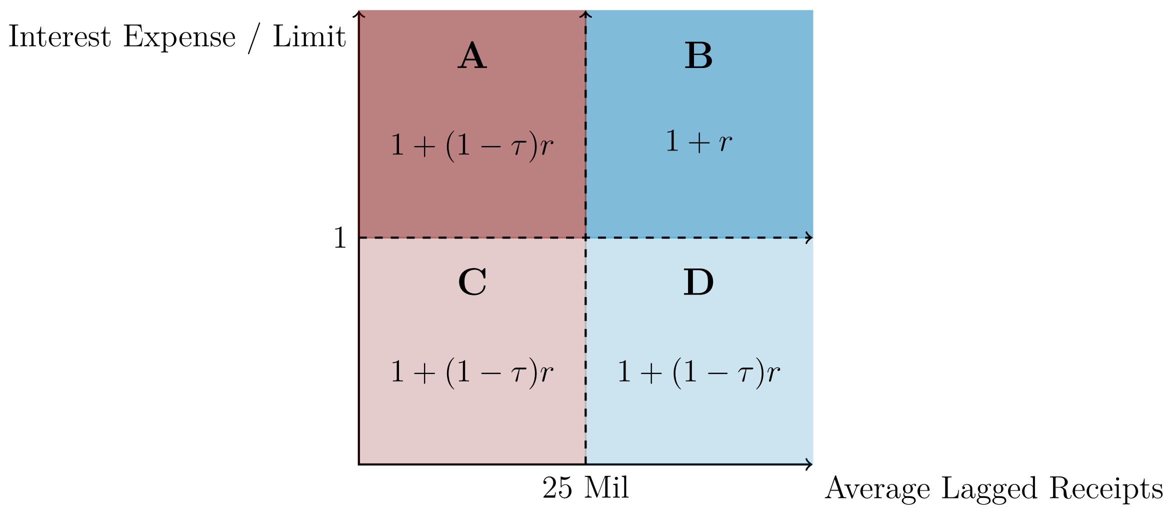


Figure 1: Marginal Cost of Borrowing

Notes: This figure displays the marginal cost of debt for firms with interest rate r and tax rate τ. The four quadrants represent larger and smaller and higher and lower interest firms. Only the firms in quadrant B with average lagged receipts exceeding \$25 million and with interest expense above their limit face the interest limitation and cannot take interest deductions on new borrowing.

of the interest limitation, but in exchange are required to use a less generous depreciation method. In practice, 16.5% of all real estate firms elect out of the interest limitation, and only 3.4% of agriculture firms do.

The interest limitation is generally applied at the entity level to C-corporations, S-corporations and partnerships. If any of these entities have interest exceeding their limitation, the excess business interest expense is disallowed, lowering the amount of interest deductions the firm can take that year. Excess business interest expense can be carried forward to future years. To head off tax avoidance strategies involving firms dividing into multiple related entities that individually qualify as small business taxpayers to avoid the interest limitation, the relevant lagged receipts number to determine whether a firm is a small business taxpayer may aggregate the receipts of multiple taxpayers if one corporation owns more than 50% of another.

In March 2020, the United States passed the Coronavirus Aid, Relief, and Economic Security (CARES) Act in an attempt to provide relief to the economy during the economic downturn brought about by COVID-19. One provision of the CARES act modified the interest limitation, raising the share of EBITDA used to calculate the interest limitation from 30% to 50% in 2020. In addition, the CARES Act applied this increase in limit retroactively to C-corps and S-corps in 2019, and granted relief to partnerships in 2019 under more complicated rules. These changes applied to 2019 but were not passed into law until March 2020. Therefore, while CARES changes may have impacted firms' economic decisions in 2020, they should not have impacted firm decisions in 2019.

2.2 Additional TCJA Policy Changes

The TCJA made many other changes to the individual and corporate tax codes. Auerbach (2018), Joint Committee on Taxation (2018) and Barro and Furman (2018) provide detailed discussions of the legislative changes. 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. In this section, rather than cover each change exhaustively, we briefly discuss the changes in TCJA that are relevant for evaluating the validity of our empirical strategy, particularly the difference in tax incentives faced by big and small firms before and after the reform.

The reform modified both corporate and individual tax rates. 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% (Goodman, Lim, Sacerdote and Whitten, 2022; Kennedy, Dobridge, Landefeld and Mortenson, 2024). Therefore, C-corporations, which tend to be larger, and pass-through businesses, which tend to be smaller, faced different tax rates before the reform, and their tax rates changed by different amounts as a result of the reform.

The TCJA also changed two tax incentives for investment for large and small firms. Section 179 expensing allows businesses to immediately deduct a limited amount of investment expenses, while bonus depreciation allows firms to accelerate the timing of depreciation deductions on all qualifying investment, moving depreciation deductions from the future to the present. The TCJA increased the amount businesses could expense with section 179 from $500,000 to $1 million. In addition, bonus depreciation was set at 50% before TCJA, meaning firms could deduct 50% of eligible investment costs immediately, and was increased to 100% after TCJA. These changes are likely to increase investment incentives for large firms more than small firms because section 179 only applies to the first $1 million of investment, while bonus depreciation is not capped.

Finally, one TCJA change also coincides with the $25 million lagged receipts cutoff for small business taxpayers. After TCJA, small businesses below the size threshold were allowed to switch from accrual to cash accounting, allowing firms to only record 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 allowing small firms to defer taxable income to when cash is actually exchanged. We find this change has no impact on firm investment or financing choices among firms close to the $25 million cutoff in our RD analysis below.

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 produced by SOI and 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.[8] Once the sample is selected in each year, SOI manually edits many variables for accuracy and consistency.

[8] C-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. 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 1120S filers with at least $50 million of assets or $10 million of ordinary business income.

3.2 Analysis Sample and Variable Definitions

Our main analysis sample is an unbalanced panel of C-corporations, S-corporations and partnerships. We construct the sample by appending yearly SOI corporate and partnership files from 2013-2019.[9] We restrict the panel to only include firms i) with positive income or deductions, ii) with assets and lagged assets never below $100,000, and iii) present in at least one year between 2015-2017 and one year after 2017. We also drop financial and utility firms. This unbalanced panel has 404,774 firm-years, 68,997 unique firms, and 38,807 firms that appear in every year of the sample.

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, equity, and cash. 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 deductions.

To track firm financing responses to the interest limitation we measure debt, equity, and cash. 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 interest limitation. *Debt* is the sum of mortgages, notes and bonds due in less than and greater than one year, which we also split into short- and long-term debt. We also track loans from stockholders, which are required by law to bear interest, and use the sum of short-term debt, long-term debt and loans from stockholders 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. *Cash* is the sum of cash and all other liquid securities. Our primary outcome measures are flows rather than stocks to focus on firm policies. Debt issuance is $\text{Debt}_t - \text{Debt}_{t-1}$, equity issuance is non-negative changes in total paid in capital (Yagan, 2015), and cash changes are $\text{Cash}_t - \text{Cash}_{t-1}$. Our debt issuance and cash change measures are net concepts because we do not observe individual transactions and both frequently come off firm balance sheets.

[9] We add 2020 data to all of our analysis in Appendix F.

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 and floor plan financing interest. *Receipts* are gross receipts plus dividends, interest, rents, royalties, capital gains and tax exempt interest. *Adjusted taxable income* is EBITDA, or income net of interest, minus deductions net of interest, depreciation, depletion and amortization.

We use the corporate and partnership SOI samples as our primary data source in part because they include information from Form 8990 that allows us to directly observe which firms lose interest deductions. This information is not available outside of the SOI samples. 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 current year interest on Form 8990 for firms that file an 8990.

We also measure firms' tax-adjusted weighted average cost of capital (WACC) using firm level interest rates and debt financing fractions.

$$MPK = \Omega = (\rho + \delta)\frac{1 - \tau z}{1 - \tau}, \tag{1}$$

with financing costs ρ, depreciation rate δ, tax rate τ, and net present value of depreciation deductions z. $\rho = w_d(1 - \tau \mathbb{1}(Allow))r + w_e E$, 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 flotation costs E, and $\mathbb{1}(Allow) = 1$ if a firm does not have interest disallowed.

For each firm-year in our data we use each firm's *debt financing fraction* w_d, the ratio of all liabilities to assets, and *interest rate* r, the ratio of total interest expense to total interest bearing liabilities.[10] For C-corporations, we use the statutory corporate tax rate in each year, and for pass-throughs we use the top individual statutory tax rate in each year.[11] We

[10]For firms with no interest bearing liabilities on their balance sheet, we impute interest rates as the median interest rate within their 4 digit NAICS industry.

[11]A more detailed WACC measure would use marginal tax rates for C-corporations, S-corporations and partnerships. However, tiered partnership structures make it difficult to track the ultimate recipient of significant amounts of partnership income (Cooper, McClelland, Pearce, Prisinzano, Sullivan, Yagan, Zidar and Zwick, 2016). Using the top individual tax rate allows us to use a consistent marginal tax rate measure for all pass-through businesses, and incorporating entity level marginal tax rates for S-corporations leads to similar results.

use common calibrations of $\delta = 0.08$ and $E = 0.066$, and measure z as the interaction of the current year bonus depreciation fraction with average investment duration made publicly available at the 4 digit NAICS code level by Zwick and Mahon (2017).

We use our WACC measure to estimate elasticities with respect to the WACC. In Appendix B, we list the sources of each cost of capital parameter, and assess the sensitivity of our elasticities to different constructions of the WACC. We find that reasonable deviations in parameter values yield only small changes in our estimates.

3.3 Summary Statistics

We summarize important variables in Table 1 in 2017 U.S. dollars. Means exceed medians for most variables.[12] To account for this 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, debt issuance in year t is $(\text{Debt}_t - \text{Debt}_{t-1})/\text{Assets}_{t-1}$.

Figure 2 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 where firms facing the interest limitation have a significant amount of interest disallowed. The average big, high-interest firm that faces the interest limitation in 2018 and 2019 has \$21 million in interest disallowed, roughly 10% of its total income and 25% of its payroll. The median big, high-interest firm that faces the interest limitation in 2018 and 2019 has \$3 million in interest disallowed, roughly 8% of its total income and 15% of its payroll. Assuming a 21% tax rate, this implies firms losing interest deductions owed a mean increase in taxes of roughly \$4 million and a median increase in taxes of \$637,000.

We present additional descriptive statistics in the appendix. Appendix Tables H.1 and H.2 display means and medians of important variables separately for firms in the four quad-

[12] To preserve taxpayer privacy all percentile P cutoffs reported in this paper are averages across all values in the $(P-1, P+1)$th percentiles of the relevant variable distribution.

Table 1: Summary Statistics

	Mean	Std Dev	P10	P50	P90	Obs	Firms
Scaling							
Assets (Mil 2017 USD)	804.6	26,215.2	0.9	28.1	436.7	404,774	68,997
Financial Capital (Mil 2017 USD)	417.9	11,628.2	0.5	15.9	249.3	404,774	68,997
Tangible Capital (Mil 2017 USD)	157.9	2,171.9	0.0	4.6	123.1	404,774	68,997
Tax							
Interest Deductions / Lagged Assets	0.013	0.017	0.000	0.005	0.039	404,774	68,997
Interest Disallowed / Lagged Assets	0.003	0.013	0.000	0.000	0.998	128,080	68,997
Net Income / Lagged Assets	0.071	0.179	−0.076	0.020	0.294	404,774	68,997
Investment and Financing							
Investment / Lagged Capital	0.095	0.139	0.000	0.040	0.270	369,055	63,122
Debt / Lagged Assets	0.257	0.333	0.000	0.084	0.788	404,774	68,997
Debt / Lagged Financial Capital	0.340	0.427	0.000	0.145	0.958	404,651	68,989
Debt + SH Loans / Lagged Assets	0.303	0.366	0.000	0.138	0.877	404,774	68,997
Short Term Debt / Lagged Assets	0.074	0.169	0.000	0.000	0.272	404,774	68,997
Long Term Debt / Lagged Assets	0.179	0.291	0.000	0.011	0.643	404,774	68,997
Trade Credit / Lagged Assets	0.099	0.156	0.000	0.029	0.319	404,774	68,997
Equity Issuance / Lagged Assets	0.046	0.185	0.000	0.000	0.076	404,774	68,997
Cash / Lagged Assets	0.218	0.241	0.009	0.124	0.611	404,774	68,997
Additional Variables							
Payouts / Lagged Assets	0.084	0.203	0.000	0.004	0.236	404,774	68,997
Profits / Lagged Assets	0.125	0.239	−0.078	0.059	0.444	404,774	68,997
Payroll / Lagged Assets	0.333	0.479	0.000	0.153	0.962	404,774	68,997
Exec Comp / Lagged Assets	0.038	0.082	0.000	0.004	0.111	404,774	68,997
Interest Rate	0.060	0.076	0.004	0.040	0.109	404,774	68,997
Debt Financing Frac	0.556	0.338	0.042	0.596	1.000	404,774	68,997
User Cost of Capital	0.138	0.041	0.104	0.134	0.159	404,774	68,997

Notes: This table reports summary statistics for our entire unbalanced panel data set spanning 2013-2019. To preserve taxpayer anonymity, percentile statistics are reported as the means of all observations in the (P-1,P+1)th percentiles. Interest disallowed statistics are reported only for the post-reform period when the variable is non-zero.

rants of Figure 1 using a 2017 cross section of the data. High-interest firms are smaller, younger, have more debt, have less cash, fewer profits and payouts, and face higher interest rates. Appendix Figures G.1 and G.2 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.

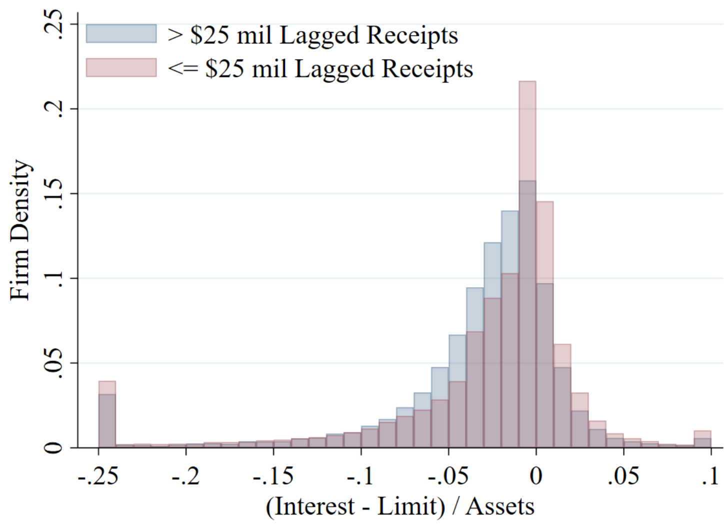


Figure 2: Distribution of Firm Interest Relative to Limitation

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. We stack observations from the tails of both distributions in the most negative and positive bars on the x-axis to focus attention on the center of the distribution.

Appendix Table H.3 shows the distribution of assets, investment, and debt among big and small, and high- and low-interest firms. In 2017, big, high-interest firms do 16% of investment and hold 27% of debt, suggesting large changes in investment or borrowing for these firms could have macroeconomic implications, while 56% of investment is done by private firms, emphasizing the importance of including private firms in the analysis.

4 Event Study and Triple Difference Designs

First, we analyze the effect of the interest limitation on firm investment and financing choices using an event study research design. This 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 face the interest limitation because their average lagged receipts are below the \$25 million threshold. Our event study specification is

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

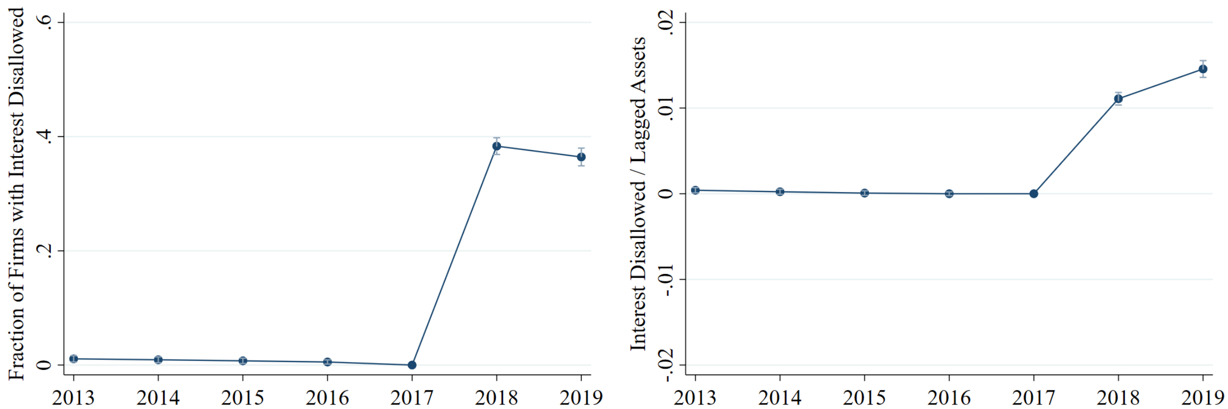
where Y_{it} is an outcome for firm i in year t, Big_i is a dummy variable equal to 1 if firm i has average receipts from 2015-2017 above \$25 million, ξ_i is a firm fixed effect, and δ_{jt} is a three digit NAICS industry j by year fixed effect. We include the latter group of fixed effects because while 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 narrowly defined industry that are more likely to face the same marginal tax rates, and have similar cost structures and investment durations.

We restrict the estimation sample to only include firms with interest above their limitation averaging over 2015-2017 and denote 2017 as the omitted year. The coefficients of interest β_e capture the average relative difference in the outcome variable between big and small high-interest firms in the same industry in year e. The firm fixed effects control for any time-invariant heterogeneity across firms, while the industry-year fixed effects control for time-varying heterogeneity across industry groups.

4.1 Event Study First Stage

We intentionally use a pre-reform measure of treatment status Big_i in equation (2) to avoid endogeneity between the treatment definition and the investment and financing outcome variables of interest in the post-reform period. Therefore, this specification yields intent-to-treat (ITT) estimates that measure the impact of the treatment definition on firm outcomes. For estimates of Equation (2) to capture firm responses to the interest limitation, not just firm responses to the treatment definition, firms with receipts above \$25 million and interest above their limitation on average over 2015-2017 must have interest disallowed in the post-reform period.

Figure 3, panels (a) and (b) display estimates of the β_e coefficients from equation (2) using an indicator for firms having any interest disallowed and interest disallowed scaled by lagged assets as outcome variables. Panel (a) shows that the treatment definition identifies firms facing the interest limitation that lose interest deductions. In the pre-reform period, by definition, zero treatment and control firms have any interest disallowed. In 2018, the fraction of treatment relative to control firms with interest disallowed jumps to almost 40%. Panel (b) shows that on the intensive margin, interest disallowed increases by 1.1% of lagged assets in 2018 and 1.5% of lagged assets in 2019. Applying a 21% corporate tax rate implies the interest limitation increases taxes by 0.3% of assets over 2018-2019.



(a) Indicator for Interest Disallowed

(b) Interest Disallowed

Figure 3: First Stage Event Study Estimates

Notes: This figure describes the first stage of our event study design. Panels (a) and (b) plot event study estimates of β_e from equation (2). 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. 95% confidence intervals are constructed from standard errors clustered at the firm level.

There are three reasons treatment status does not perfectly identify firms with interest disallowed. First, some firms defined as big or high interest from 2015-2017 do not continue to have average lagged receipts above $25 million or interest that exceeds their limitation in the post-reform period. Second, some firms with agriculture and real estate components are able to elect out of the interest limitation in exchange for a less generous depreciation method. Third, some firms that have average lagged receipts above the size cutoff and interest above their limitation do not have interest deductions disallowed, while some firms that have average lagged receipts below the size cutoff do have interest disallowed. We display the relative importance of each factor in Appendix Figure G.3.[13] While *ex ante*

[13]Over 70% of treatment firms are big and high interest each year from 2015-2017. In 2018, 65.8% of

big and 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.[14]

4.2 Event Study Investment and Financing Estimates

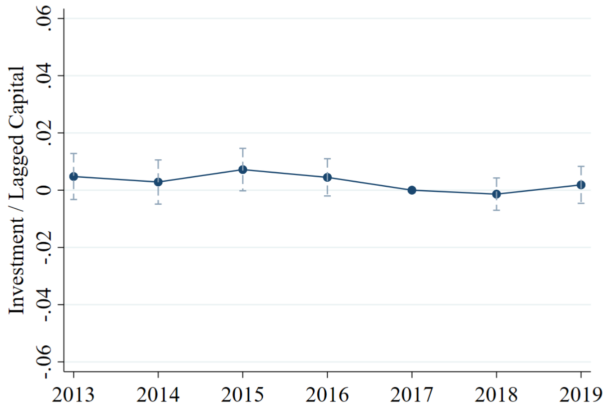
Having established that our treatment definition identifies firms facing the interest limitation, and quantified the size of the shock, we turn to the central question of this paper: does the interest limitation impact firm investment and financing choices? To answer this question graphically using our event study design, we display ITT event study estimates of β_e from equation (2) in Figure 4 using the investment rate, debt issuance, equity issuance, and cash changes as outcome variables.

For all four outcomes, pre-reform coefficients do not reject zero (with the exception of debt issuance in 2013), suggesting the outcomes of the treatment and control groups are likely to continue to evolve similarly in the absence of the policy. For the investment rate, debt issuance, and cash changes, we find no evidence of statistically significant responses to treatment in the post-reform period. In panel (c), we find statistically significant increases in equity issuance in 2018 and 2019.

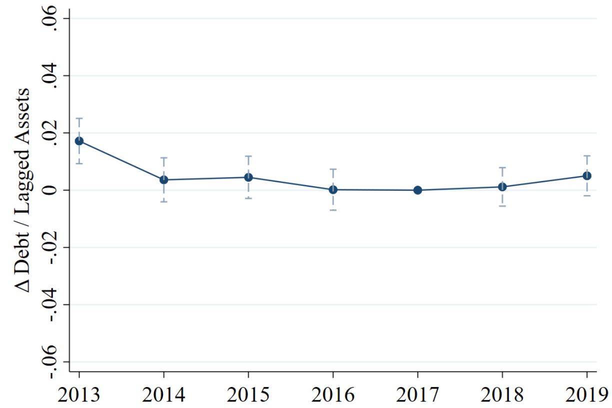
To understand the magnitude of these firm responses, we re-estimate equation (2) replacing the indicators for 2018 and 2019 with a single indicator for an observation being in

treatment firms are still big and high interest, 6.9% of treatment firms elect out, and 9.5% of treatment firms continue to be big and high interest, do not elect out, but still do not have any interest disallowed. Part of the latter group can be explained by firms that should file Form 8990 not filing the form. For 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. Furthermore, 9.1% of control firms have interest disallowed.

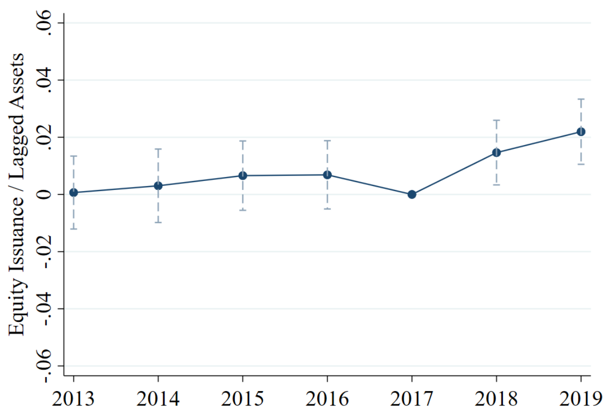
[14]We intentionally choose a broad sampling frame including firms that can elect out of the interest limitation to maximize our sample size and capture precise ITT estimates that stand alone as interesting policy-relevant parameters. The interest limitation was written to not apply to two high leverage industries, agriculture and real estate, but firms in these industries that elect out of the interest limitation may reduce investment due to the less generous depreciation rules they are required to follow in exchange. Our ITT estimates capture these potential effects. Our results remain similar when we make different sampling choices that increase treatment persistence like excluding firms that elect out or defining firms as high interest only if they have interest above their limitation in all three years from 2015-2017. Increased treatment persistence is offset by less precise ITT estimates, resulting in similar treatment-on-the-treated confidence intervals. We discuss these robustness checks in section 4.5 and Appendix C.



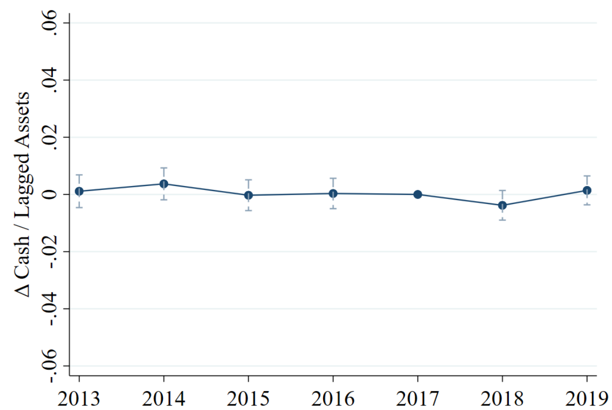
(a) Investment Rate



(b) Debt Issuance



(c) Equity Issuance



(d) Cash Changes

Figure 4: Event Study Estimates

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

year 2018 or 2019. The resulting β_{post} coefficients represent ITT estimates of the average post-reform response for treatment relative to control firms. To account for the imperfect persistence of treatment status over time, we also pursue an instrumental variables approach to obtain treatment-on-the-treated (TOT) estimates of the impact of the interest limitation. We define $Disallow_{it}$ as an indicator for having interest disallowed in years after 2017, and for firms having interest above their limitation and average lagged receipts above \$25 million in years before 2018, and estimate

$$Y_{it} = \sum_{e=2013, e\neq 2017}^{2019} \beta_e^{TOT} \mathbb{1}(t=e) \times Disallow_{it} + \delta_{jp(i),t} + \xi_i + \varepsilon_{it}, \tag{3}$$

instrumenting for $\mathbb{1}(t=e)Disallow_{it}$ with $\mathbb{1}(t=e)Big_i$ and replacing the indicators for 2018 and 2019 with a single indicator for an observation being in year 2018 or 2019 to obtain β_{post}^{TOT} coefficients.

We display our estimates of β_{post} from equation (2) and β_{post}^{TOT} from equation (3) in Table 2. The estimates in column 1 suggest that the interest limitation has a precise null effect on the investment rate. Our ITT estimates suggest investment declines by \$0.000 per dollar of lagged tangible capital assets with a standard error of \$0.003, relative to a pre-reform mean value of \$0.12 per dollar of lagged capital. The TOT estimates are between two and three times larger than the ITT estimates, reflecting how treatment firms do not always have interest disallowed in the post-reform period.

We also scale our estimates into weighted average cost of capital (WACC) elasticities in Table 2, the estimated percent change in the outcome variable for every 1% change in the WACC. To calculate the percent change in each outcome, we divide our estimates of β_{post} by the average value of the outcome among treatment firms in the pre-reform period $\overline{Y}_{pre}^{T}$. We calculate the percent change in the WACC as the difference in the percent change in WACC for treatment and control firms. Specifically, we define

$$\varepsilon = \frac{\beta_{post}}{\overline{Y}_{pre}^{T}} \Big/ \Big(\frac{\Delta WACC^T}{\overline{WACC}_{pre}^{T}} - \frac{\Delta WACC^C}{\overline{WACC}_{pre}^{C}} \Big). \tag{4}$$

We calculate ITT and TOT elasticities following equation (4) by using two different measures of the WACC. To calculate an ITT elasticity, we use a measure of the WACC that mechanically assigns interest disallowed to every treatment firm, so that post-reform WACC financing terms for treatment firms are $\rho = (w_d r + w_e E)$ and do not include interest deductions. To calculate a TOT elasticity, we use a measure of the WACC that depends on whether firms have interest disallowed, so that the post-reform financing term is $\rho = (w_d r(1 - \tau \mathbb{1}(Allow)) + w_e E)$ and only eliminates interest deductions from the WACC financing term for firms with interest disallowed.

If the interest limitation mechanically applied to all treatment firms, it would have increased WACCs by 10%. In practice the interest limitation does not apply to all treatment firms and only raises WACCs by 6%. Scaling our ITT estimates by pre-reform outcome means and mechanical WACC changes yields an ITT investment rate WACC elasticity of 0.00 with a 95% confidence interval spanning [-0.41, 0.42]. Scaling by actual WACC changes yields a wider TOT investment rate WACC elasticity confidence interval spanning [-0.72, 0.73]. We explore the sensitivity of our elasticity estimates to different constructions of the WACC in Appendix B and find other reasonable parameter values yield only small changes in elasticity estimates.

Previous estimates of the investment rate cost of capital elasticity using publicly- and privately-held firms are around negative two and reject the lower bounds of our confidence intervals (Chen, Jiang, Liu, Suarez-Serrato and Xu, 2023). The differences between our and previous elasticity estimates suggest that firms are not using debt to finance new investment projects, as other estimates have typically been identified using variation in the after tax return of every dollar of investment, not only debt financed investment. We compare our investment elasticity estimates to prior work in more detail in section 7.

Column 2 of Table 2 suggests the interest limitation also has an insignificant and economically small impact on debt issuance in 2018 and 2019. The ITT point estimate in column 2 suggests debt issuance increases by \$0.002 for each dollar of lagged assets, with a standard error of \$0.003. The TOT confidence interval spans from [-0.011, 0.021].

Column 3 of Table 2 suggests the interest limitation causes a statistically significant increase in equity issuance. The ITT point estimate in column 3 suggests equity issuance increases by \$0.019 for each dollar of lagged assets, with a standard error of \$0.005, while the TOT confidence interval spans from [0.023, 0.078]. These estimates cannot completely rule out some substitution away from debt towards equity in response to the interest limitation, but do suggest the magnitude of any substitution is small. For example, we cannot rule out debt issuance declining by 1% of assets and equity issuance increasing by 2% of assets.

The ITT estimate in Column 4 of Table 2 suggests cash changes decrease by \$0.002 per dollar of lagged assets with a standard error of \$0.002. The TOT confidence interval spans

Table 2: Event Study Effect on Investment and Financing

Outcome	(1) Investment Rate	(2) Debt Issuance	(3) Equity Issuance	(4) Cash Changes
β_{post}	0.000	0.002	0.019	-0.002
	(0.003)	(0.003)	(0.005)	(0.002)
β_{post}^{TOT}	0.000	0.005	0.051	-0.005
	(0.007)	(0.008)	(0.014)	(0.006)
Obs	83,249	89,523	89,523	89,523
Clusters	14,960	16,098	16,098	16,098
R^2	0.504	0.226	0.516	0.140
Pre-Reform Mean	0.123	0.027	0.064	0.006
ITT WACC % Δ	0.104	0.096	0.096	0.096
ε^{ITT}	0.002	0.725	3.028	-3.177
	(0.213)	(1.156)	(0.827)	(3.678)
TOT WACC % Δ	0.059	0.052	0.052	0.052
ε^{TOT}	0.003	1.338	5.589	-5.864
	(0.372)	(2.133)	(1.526)	(6.789)

Notes: This table reports event study estimates of β_{post} from equation (2) and β_{post}^{TOT} from equation (3), replacing the indicators for 2018 and 2019 with a single indicator for an observation being in year 2018 or 2019. The estimation sample includes all firms in our panel data with interest exceeding 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. ITT and TOT WACC percent changes are the percent change in the weighted average cost of capital, calculated as the mechanical (ITT) or actual (TOT) percent change in the weighted average cost of capital for treatment relative to control firms. We calculate ε as the outcome variable coefficient estimate divided by the pre-reform mean of the outcome variable, divided by the percent change in the weighted average cost of capital.

[-0.017, 0.007], suggesting the interest limitation does not have an economically significant impact on cash changes, and that firms do not rely more on liquidity to finance investment after the interest limitation.

4.3 Validating the Event Study Design

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. Visual inspection of parallel trends in Figure 4 shows that outcomes for treatment and control firms trended similarly for five years before the reform, suggesting they would likely continue to do so in the absence of the reform.

A key threat to our event study design is that time-varying shocks may coincide with the implementation of the interest limitation. In particular, other TCJA tax policy changes implemented in 2018 could differentially impact the larger treatment and smaller control firms, biasing our event study estimates. For example, our null results could be explained by the interest limitation causing a decline in investment, but being offset by larger firms disproportionately benefiting from the tax rate changes included in the TCJA.

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 discuss the latter two checks in Appendix C.

4.4 Triple Difference Design

Building on our placebo event study estimates using low-interest firms, we also implement a triple difference design that compares big and small high-interest firms, netting out the difference in outcome 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 interest limitation. High- and low-interest firms both face other TCJA reforms, so other policy changes are unlikely to bias the triple difference estimates.

Despite actively controlling for potential size-varying impacts of other TCJA reforms by using a different counterfactual, we continue to find similar results. We rule out economically significant changes in investment, debt issuance and cash changes in response to the interest limitation, and find evidence that firms increase their equity issuance.

To implement the triple difference design, we estimate

$$
\begin{aligned}
Y_{it} = \sum_{e=2013, e\neq 2017}^{2019} & \gamma_e \mathbb{1}(t=e) \times Big_i \times HI_i + \phi_e \mathbb{1}(t=e) \times Large_i \\
& + \psi_e \mathbb{1}(t=e) \times HI_i + \delta_{jt} + \xi_i + \varepsilon_{it} \qquad (5)
\end{aligned}
$$

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. We estimate equation (5) on all high- and low-interest firms in our data, and estimates of γ_e from equation (5) represent the difference between our event study estimates for high-interest firms and our placebo event study estimates for low-interest firms. The γ_e estimates once again are ITT estimates because not all big, high-interest firms face the limitation.

We plot estimates of γ_e for our first stage outcomes in Appendix Figure G.4. The first stage of the triple difference design is similar to the first stage of the event study design because few low-interest firms, regardless of size, have interest disallowed. Appendix Figure G.5 plots ITT estimates of γ_e for our four key investment and financing outcomes. The results are similar to the event study results presented in Figure 4.

To account for the imperfect persistence of treatment status over time, we again pursue an instrumental variables approach to obtain TOT estimates. We define Big_{it} as an indicator for a firm with average lagged receipts above the size cutoff in each year, HI_{it} as an indicator for interest above firm's limitation in each year, and estimate

$$
\begin{aligned}
Y_{it} = \sum_{e=2013, e\neq 2017}^{2019} & \gamma_e^{TOT} \mathbb{1}(t=e) \times Disallow_{it} + \phi_e \mathbb{1}(t=e) \times Big_{it} \\
& + \psi_e \mathbb{1}(t=e) \times HI_{it} + \delta_{jt} + \xi_i + \varepsilon_{it}, \qquad (6)
\end{aligned}
$$

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 $\mathbb{1}(t=e)HI_i$.

Table 3: Triple Difference Effect on Investment and Financing

Outcome	(1) Investment Rate	(2) Debt Issuance	(3) Equity Issuance	(4) Cash Changes
γ_{post}	-0.001	0.000	0.011	-0.004
	(0.003)	(0.003)	(0.004)	(0.002)
γ_{post}^{TOT}	-0.002	-0.001	0.030	-0.011
	(0.007)	(0.008)	(0.012)	(0.006)
Obs	368,620	404,762	404,762	404,762
Clusters	62,693	68,995	68,995	68,995
R^2	0.469	0.192	0.479	0.143
Pre-Reform Mean	0.123	0.027	0.064	0.006
ITT WACC % Δ	0.09	0.08	0.08	0.08
ε^{ITT}	-0.07	0.17	2.06	-8.04
	(0.25)	(1.32)	(0.84)	(4.26)
TOT WACC % Δ	0.04	0.03	0.03	0.03
ε^{TOT}	-0.16	0.47	5.57	-21.75
	(0.59)	(3.56)	(2.28)	(11.52)

Notes: This table reports triple difference estimates of γ_{post} from equation (5) and γ_{post}^{TOT} from equation (6), replacing the indicators for 2018 and 2019 with a single indicator for an observation being in year 2018 or 2019. The estimation sample includes all firms in our panel data. 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 big, high-interest firms in all years before 2018. ITT and TOT UCC Pct Change is the percent change in the user cost of capital, calculated as the mechanical (ITT) or actual (TOT) percent change in the user cost of capital for treatment relative to control firms. We calculate ε as the outcome variable coefficient estimate divided by the pre-reform mean of the outcome variable, divided by the percent change in the weighted average cost of capital.

To quantify the magnitude of our triple difference estimates of firm responses, we re-estimate equations (5) and (6), replacing the indicators for 2018 and 2019 with a single indicator for an observation being in either year. The resulting γ_{post} coefficients represent ITT and TOT estimates of the average post-reform response for treatment relative to control firms. We display these estimates for our four central outcomes and scale these estimates into ITT and TOT user cost elasticities in Table 3 replacing β_{post} with γ_{post} in equation (4).[15] Post-reform coefficient estimates and elasticities from the triple difference design are strikingly similar to the event study results presented in Table 2 across all four outcomes. Therefore, differential impacts of other TCJA policies across our treatment and control groups are unlikely to be biasing our results.

4.5 Robustness: Different Samples, Outcomes, and Specifications

We validate our event study and triple difference designs with a number of additional robustness checks that we discuss in more detail in Appendix C. Our results remain similar when we use different samples constructed to have higher treatment persistence by eliminating firms that elect out of the interest limitation or using a treatment definition requiring firms have interest above their limitation in every year from 2015-2017.

Our investment and financing results remain similar when considering alternative outcome variables. We continue to reject economically large changes in investment when using alternative outcomes include scaling investment by net rather than gross capital, log investment, extensive margin investment, or an indicator for investment spikes larger than 20% of lagged capital. We also continue to reject economically large changes in debt issuance or leverage ratios using alternative measures of the stock or flow of debt, different scaling denominators, short-term debt, long-term debt, trade credit, or log debt.

Given the lack of substantial borrowing declines, it seems unlikely the interest limitation leads to declines in other real outcomes, and we also rule out economically or statistically significant declines in shareholder payouts, payrolls, and executive compensation. To help explain what firms use additional equity issuance for, we also directly measure taxes paid and find an increase in taxes corresponding to approximately 20% of the interest deductions disallowed after controlling for size-varying impacts of other reforms.

Our results remain similar when using industry-profitability-year fixed effects instead of industry-year fixed effects, and when adding separate time trends for average age, revenue

[15] The elasticity scaling for triple difference estimates follows equation (4) with one modification. Instead of using the difference in the percent change in user cost between big and small high-interest firms, the triple difference scaling uses the difference in the percent change in user cost between big and small high-interest firms, net of the difference in the percent change in user cost between big and small low-interest firms.

growth, sales and profits over 2015-2017, when scaling by fixed, pre-reform assets and capital instead of lagged measures, when winsorizing at the 99th instead of 95th percentile, when dropping real estate firms that may elect out of the interest limitation, when restricting to a balanced panel, and when dropping firms that may need to aggregate with other entities to determine relevant interest limitation cutoffs.

Carrizosa, Gaertner and Lynch (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 the debt to assets ratio as an outcome and find declines in this ratio for treatment relative to control firms of roughly 3% of lagged assets that reject zero, significantly larger than our ITT event study or triple difference estimates. We detail in Appendix C that their estimates appear to be driven by mean reversion, not a response to the interest limitation, justifying our choice to compare big and small high-interest firms.

Finally, very large firms may have substantially different investment opportunities and access to capital markets than smaller firms. To alleviate concerns that the very largest firms in our treatment group drive our results, we drop the largest quarter of the large firms from each of our estimation samples and continue to find similar results. We discuss our regression discontinuity design in the next section, which more stringently addresses this concern by focusing only on the firms just above and below the receipts cutoff for the interest limitation.

5 Regression Discontinuity Design

The event study and triple difference estimates above both rule out economically significant investment, debt issuance and cash change responses, and suggest firms increase equity issuance in response to the interest limitation. To increase confidence in these results, we also use a regression discontinuity (RD) design to estimate the causal effect of the interest limitation on the marginal firm that is just large enough to face the policy. This design compares high-interest firms that face the interest limitation because their average lagged receipts are just above the $25 million threshold to high-interest firms that do not face the interest limitation because their average lagged receipts are just below the threshold.

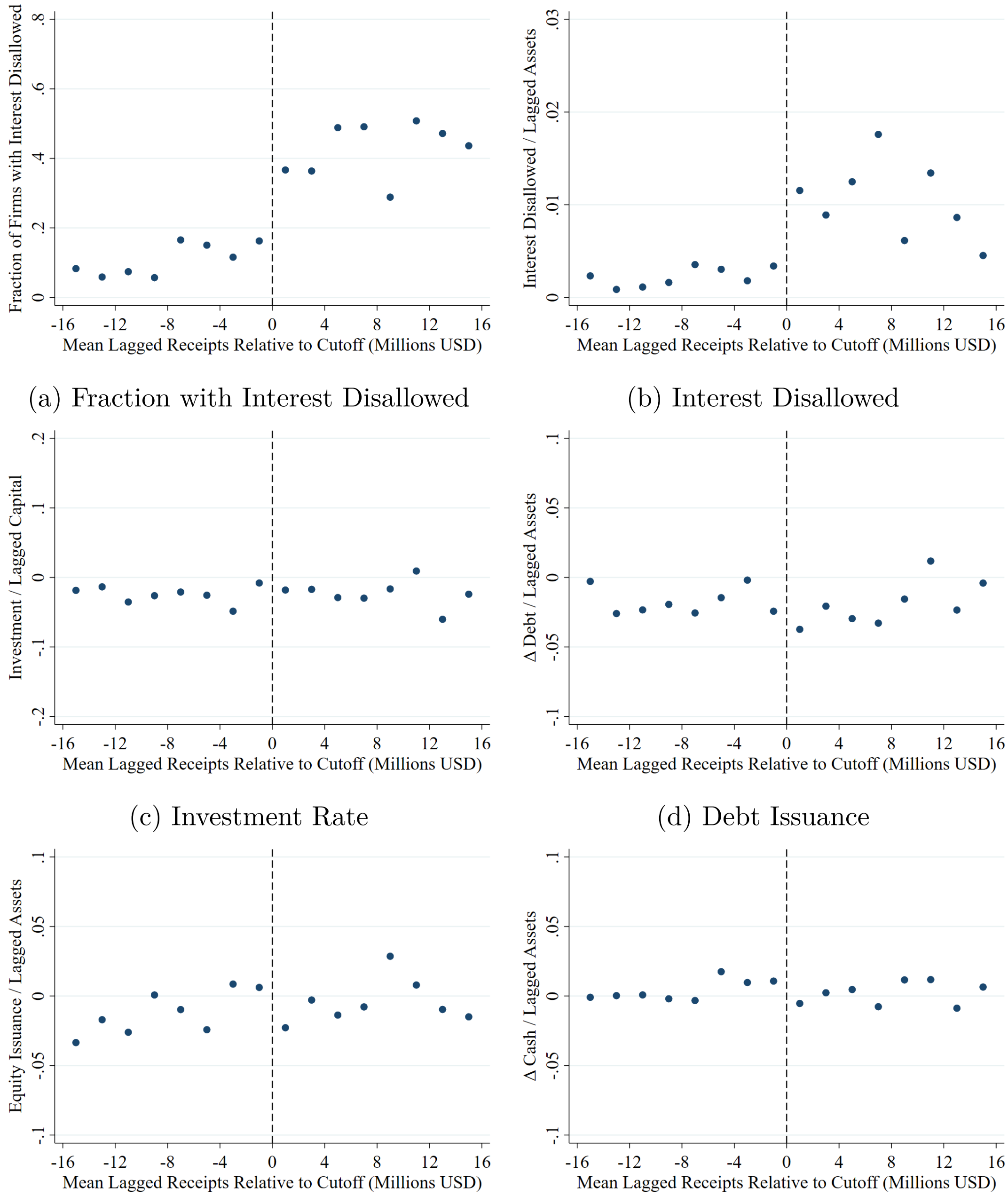
Our RD design delivers estimates of the local average treatment effect of the interest limitation for firms close to the $25 million threshold and relies on a less stringent identification assumption that firms cannot precisely manipulate their past receipts (Lee, 2008; Lee and Lemieux, 2010). This assumption is unlikely to be violated because the TCJA was not passed until December 2017 and was not widely anticipated.[16]

To implement our RD, we restrict our SOI panel to years 2015-2019, and to firms with interest above their limitation averaging over 2015 to 2017. While the interest limitation is written to apply to high-interest firms with average lagged receipts above but not below $25 million, this is not always true in our data. To head off avoidance strategies involving firms dividing into related entities that individually are small enough to avoid the interest limitation, the relevant lagged receipts number for a given firm may aggregate the receipts of multiple taxpayers if one corporation owns more than 50% of another. To avoid including firms in our RD samples that appear to be below the $25 million receipts cutoff, but that are actually large enough to face the interest limitation due to aggregation rules, we exclude all potential aggregators that are parents or children in ownership links with $> 50\%$ stakes.

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. Figure 5 plots outcomes in evenly spaced $2 million receipts bins within a $16 million bandwidth around the cutoff using the SOI data. Panels (a) and (b) plot first stage outcomes from Form 8990. Panels (a) and (b) shows there is a clear extensive and intensive margin jump in interest disallowed around the $25 million cutoff.[17] Panels (c)-(f) display raw means of the investment rate, debt issuance, equity issuance and cash changes. There is no clear visual discontinuity for any of the four key outcome variables, suggesting no obvious changes in the investment rate or financing choices for marginal firms just above the lagged receipts cutoff.

[16] The actual legislation was not introduced in the Senate until November 2017. Multi-year anticipation of the legislation is also unlikely because business tax policy is highly partisan in the U.S. and the results of the November 2016 presidential election were difficult to predict (Kennedy, Dobridge, Landefeld and Mortenson, 2024).

[17] Figure 5 also shows that some interest is disallowed from firms well below the $25 million receipts cutoff. This occurs for two reasons. First, some small business taxpayers face the interest limitation because they qualify as tax shelters, and second, our potential aggregators flag may not capture all brother-sister and combined corporate groups that are required to aggregate receipts across multiple taxpayers.



(a) Fraction with Interest Disallowed

(b) Interest Disallowed

(c) Investment Rate

(d) Debt Issuance

(e) Equity Issuance

(f) Cash Changes

Figure 5: Regression Discontinuity Binned Scatter Plots

Notes: This figure plots average values of outcome variables in evenly spaced \$2 million receipts bins around the \$25 million cutoff. Panel (a) displays averages for having interest disallowed, while panel (b) displays average interest disallowed scaled by lagged assets. Panel (c) displays averages for investment scaled by lagged capital, panel (d) displays average debt issuance scaled by lagged assets, panel (e) displays average equity issuance scaled by lagged assets, and panel (f) displays average cash changes scaled by lagged assets.

To complement our visual depictions of the RD design, we develop parametric estimates of the regression discontinuity by restricting to firms with interest above their limitation averaging over 2015 to 2017, dropping potential aggregators, and estimating

$$\Delta Y_i = \alpha + \beta^{RF} Big_i + f(z_i) + \varepsilon_i, \tag{7}$$

where outcome variable ΔY_i (investment rate, debt issuance, equity issuance and cash changes) is the average annual outcome over 2018 and 2019 minus the average annual outcome over 2015-2017, 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 \$25 million receipts threshold. Differencing the outcome increases precision and controls for different pre-reform outcome levels.

Our RD sample is constructed based on firm's interest relative to their limitation in a pre-reform period, and we use 2015-2017 average receipts as the running variable. Therefore, not every firm above the size threshold faces the interest limitation, and equation (7) 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

$$\Delta Y_i = \alpha + \beta^{IV} Disallow_i + f(z_i) + \varepsilon_i. \tag{8}$$

Estimates of β^{IV} from equation (8) represent TOT estimates of the local average treatment effect of the interest limitation on firms that actually have interest deductions disallowed at the cutoff, while estimates of β^{RF} represent ITT estimates of the local average impact of the interest limitation.

To implement the RD design, we choose a triangular kernel and first degree polynomial, following guidance from Gelman and Imbens (2018) to use first order polynomials when higher order coefficients are not statistically significant. We use a \$16 million bandwidth based on optimal bandwidths for our outcome variables suggested by Calonico, Cattaneo and Titiunik (2014).

Table 4 displays reduced form and fuzzy RD estimates that we also scale into WACC elasticities. We calculate elasticities following equation (4), but use reduced form RD estimates of mechanical and actual changes in WACCs to calculate percent changes in WACCs directly at the receipts cutoff.

Table 4: Regression Discontinuity Effect on Investment and Financing

Outcome	(1) Investment Rate	(2) Debt Issuance	(3) Equity Issuance	(4) Cash Changes
β^{RF}	0.004	−0.024	−0.031	−0.015
	(0.014)	(0.015)	(0.025)	(0.011)
β^{IV}	0.020	−0.123	−0.158	−0.077
	(0.072)	(0.084)	(0.128)	(0.057)
Obs	1,625	1,676	1,676	1,676
Pre-Reform Mean	0.101	0.023	0.055	0.004
First Stage F-Stat	15.220	15.288	15.288	15.288
ITT WACC % Δ	0.166	0.173	0.173	0.173
ε^{ITT}	0.234	−5.941	−3.247	−19.737
	(0.843)	(3.731)	(2.618)	(14.052)
TOT WACC % Δ	0.070	0.079	0.079	0.079
ε^{TOT}	0.552	−12.982	−7.095	−43.128
	(1.993)	(8.153)	(5.721)	(30.707)

Notes: This table reports regression discontinuity estimates of β^{RF} from Equation (7) and β^{IV} from Equation (8) for all high-interest firms in the SOI RD sample using a bandwidth of \$16 million receipts. Robust standard errors are reported in parentheses. Pre-reform means are averages over 2015-2017 for firms above the receipts cutoff. ITT and TOT WACC percent changes are the percent change in the weighted average cost of capital, calculated as the RD estimate of β^{RF} using mechanical (ITT) or actual (TOT) weighted average cost of capital as the outcome variable, divided by the pre-reform mean of the relevant cost of capital measure. We calculate ε as the ITT coefficient divided by the pre-reform mean of the outcome variable, divided by the relevant percent change in the weighted average cost of capital.

The RD results in Table 4 make three important points. First, our RD estimates are consistent with our event study and triple difference estimates. Confidence intervals on our RD estimates do not reject minimal investment, debt issuance and cash changes, nor can they reject increases in equity issuance. We can never reject the null hypothesis of equality between our RD and event study or triple difference TOT estimates for any of our four investment and financing outcomes, with p-values always exceeding 0.1. Second, our RD estimates are substantially less precise than our event study and triple difference estimates. The standard errors of the RD estimates are multiple times larger than the standard errors on event study or triple difference estimates, and none of our estimates across outcomes reject zero. Third, despite their lack of precision, our RD estimates still provide useful information by delivering comprehensive estimates of the local average treatment effect of the interest limitation for firms just large enough to face the policy.

5.1 Robustness and Validation of the RD Design

We present additional estimates in Appendix D to validate our RD design and assess its robustness. First, we find no evidence of bunching at the $25 million lagged receipts cutoff either before or after the policy was implemented, suggesting no manipulation around the cutoff, and a general lack of responses to the limitation. Second, when we estimate the same RD on low-interest firms, we find no effect on key investment and financing variables, suggesting the option TCJA provided to allow small firms below the $25 million receipts cutoff to switch from accrual to cash accounting does not bias our RD estimates. Third, we find our RD estimates are robust to choice of bandwidth and polynomial degree. Fourth, in an effort to increase precision of our RD estimates, we develop new data using the universe of electronically filed business tax returns. This data is missing first stage information from Form 8990 but contains more than ten times the number of firms within $16 million of the $25 million receipts cutoff. Our RD estimates using E-filing data remain similar, but are still less precise than our event study and triple difference designs.[18]

Fifth, Sanati and Beyhaghi (2024) study the impact of the interest limitation using an RD design and Compustat and Y-14Q data, resulting in much smaller samples. Using the tax data, we also develop RD estimates based only on the small sample of public C-corporations that would appear in Compustat. Our RD estimates continue to be unable to reject null investment and financing responses to the interest limitation.[19] One key difference

[18] The negative but insignificant coefficient on debt issuance from our main RD analysis using the SOI data hovers around zero in this larger sample.

[19] Simulations suggest RD estimates based on Compustat or Y-14Q sample sizes and our data generating process can lead to drastically diverging results, pointing to statistical power issues that limit our ability to

between our analyses is that Sanati and Beyhaghi (2024) 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 used for the interest limitation.[20] This may 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. Setting aside any potential misspecification or attenuation, inflating their ITT estimates to TOT estimates using our measure of the first stage yields improbably large estimated responses. See Appendix D for details.

Given the stability of our RD estimates across specifications and data sets, and their lack of precision, our additional heterogeneity and robustness analysis focuses on our event study and triple difference designs.

6 Subsample Analysis

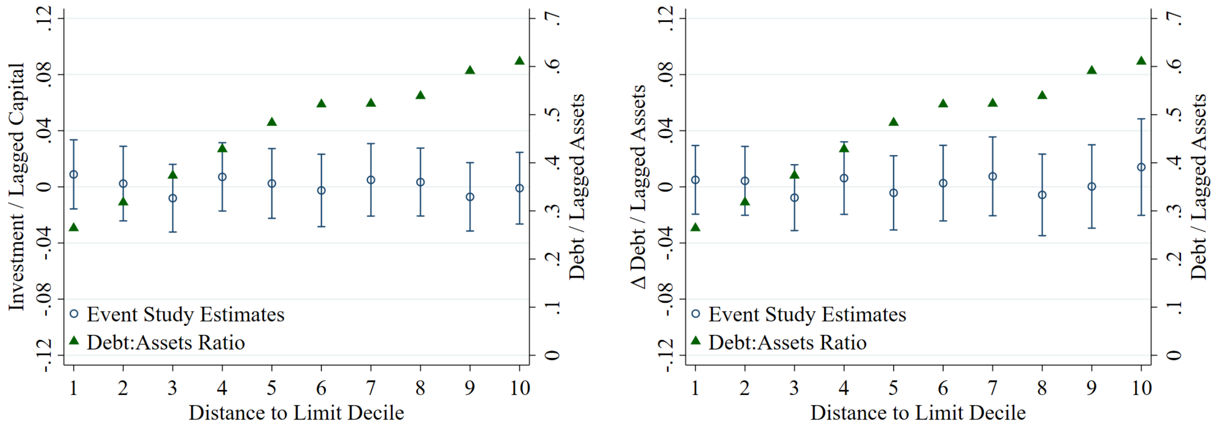
6.1 External Validity

Our event study design suggests the interest limitation has null impacts on investment and debt issuance. Numerous robustness checks suggest these results have a high degree of internal validity, but do little to help us understand the external validity of our results. If the interest limitation was tightened to apply to lower interest firms, would we continue to see similar null effects on investment and borrowing?

To develop evidence on this question, we re-estimate our event study design separately by decile of the degree to which firm's interest exceeds their limitation averaging over 2015-2017. Figure 6 plots these estimates using the investment rate and debt issuance as outcomes in panels (a) and (b) respectively, where the left y-axis corresponds to coefficient estimates and the right y-axis corresponds to firms average debt to assets ratio. Despite substantial variation in capital structures across deciles, all of the coefficient estimates cannot reject zero and there is no trend in estimates across deciles. Appendix Figure G.6 shows a similar

learn about the effects of the interest limitation from smaller samples.

[20]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.



(a) Investment Rate

(b) Debt Issuance

Figure 6: Investment and Debt Issuance External Validity

Notes: This figure plots event study estimates of β_{post} from equation (2), replacing the 2018 and 2019 indicators and interactions with a single post-reform dummy and interaction in each equation. We display these estimates by 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 debt to assets ratios in each decile across all sample years. Panel (a) uses investment scaled by lagged capital as an outcome, and panel (b) uses debt issuance scaled by lagged assets as an outcome. 95% confidence intervals are constructed from standard errors clustered at the firm level.

lack of trend across deciles for the investment rate, debt issuance, equity issuance, and cash changes, for our more precise event study and triple difference designs. In sum, we find little evidence suggesting firms would decrease investment or borrowing by more if the interest limitation was tightened to apply to lower interest firms.

6.2 Heterogeneity

Our average estimates may mask heterogeneous responses among different types of firms. Firms facing larger WACC changes, or facing financial constraints, may have stronger responses to the interest limitation. Similarly, firms facing fewer immediate tax implications of the interest limitation may have more muted responses.

To explore these possibilities, Figure 7 presents split sample ITT event study estimates of β_{post} from equation (2). We split firms into groups of above and below median predicted WACC changes, profitability, age, interest rates, and ratios of short to long term debt[21],

[21] For this split, we restrict the sample to firms that use both short and long-term debt to avoid edge cases

averaging splitting characteristics from 2015-2017. Our measure of predicted WACC changes is the difference between WACCs if the interest limitation applied to all high-interest firms in the pre-reform period and firm's actual pre-reform WACC. We also split firms into groups that do and do not pay dividends from 2015-2017, and into firms with positive and negative net incomes averaging from 2015-2017.

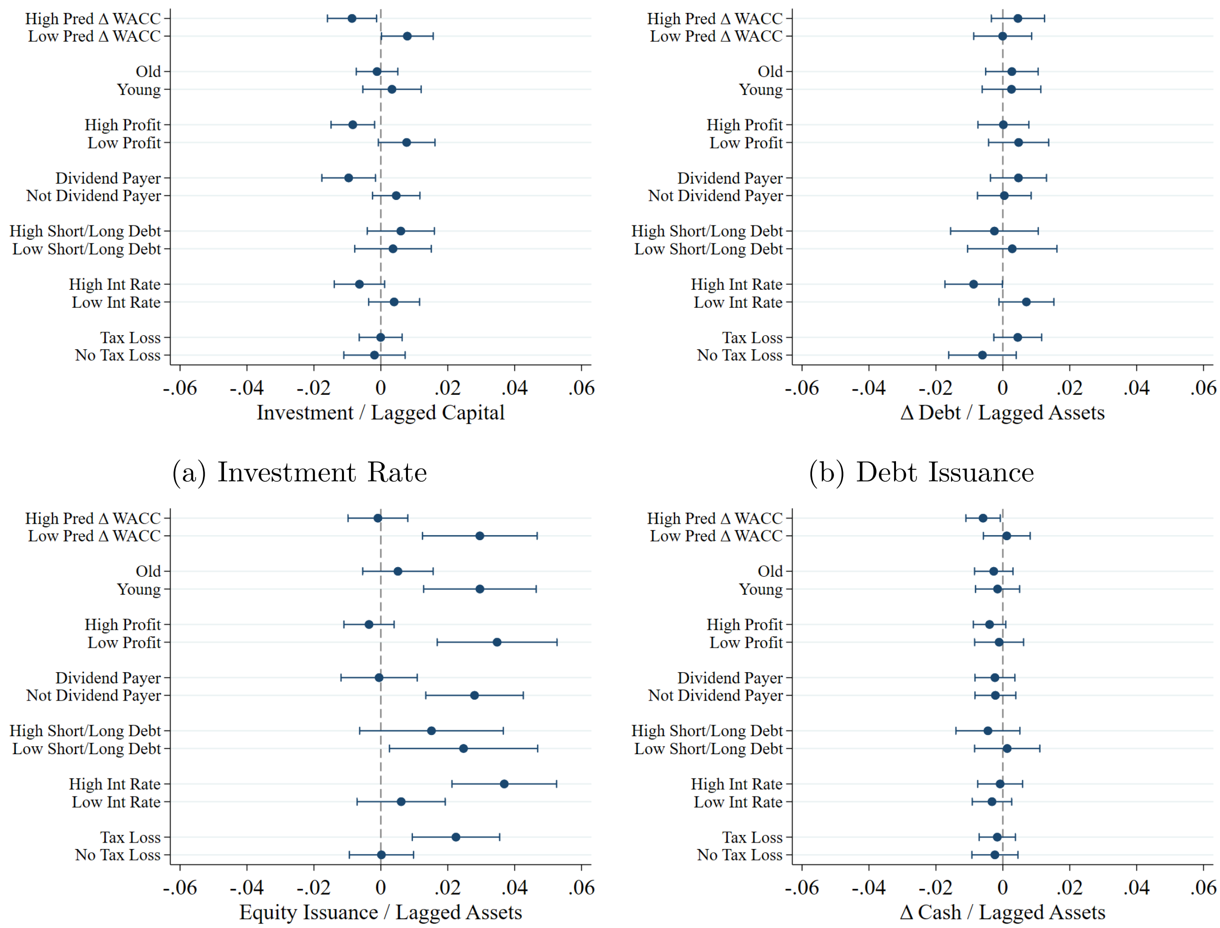
In line with basic predictions, when we split our sample by predicted WACC changes, the firms with larger predicted WACC changes do exhibit larger investment declines, though the magnitude of the decline is small. One reason our average investment estimates may be null, and the magnitude of estimate for firms facing the largest WACC remains small, is that firms may use cash to finance new investment projects. If new investment is financed with cash, while the interest limitation will change a firm's WACC, it will not change the marginal cost of a new investment project.

A natural next question is whether firms with less cash flexibility exhibit larger investment declines. Our estimates focusing on younger firms, smaller firms, and firms not paying dividends, all common proxies for financial constraints, suggest this is not the case. Instead, estimates in panel (c) show that equity issuance increases are concentrated among these groups of firms. There are two key implications of this result. First, cash constrained firms turn to equity financing to mitigate potential investment impacts of the interest limitation. Second, dividend payment, profitability and age are all often used as proxy measures of financial constraints because dividend payers can always reduce payouts while non-dividend payers cannot, firms with lower profits have less cash-on-hand, and younger firms often lack stable cash flows and credit histories (Cloyne, Ferreira, Froemel and Surico, 2023).[22] However, in our setting, proxy constrained firms issue more equity, suggesting they do not face steeply upward sloping capital supply curves or large wedges between internal and external capital costs (Farre-Mensa and Ljungqvist, 2016).[23]

Figure 7, panels (b) and (d) show few heterogeneous cash changes or debt issuance

where firms are restricted to only one type of borrowing.

[22] Research in public finance often attempts to identify financially constrained firms by *ex ante* measures of size, dividend payment, or cash flow (Zwick and Mahon, 2017; Liu and Mao, 2019; Saez, Schoefer and Seim, 2019). In work on monetary policy, researchers have used similar tags, in addition to measures of distance to default (Ottonello and Winberry, 2020). In finance, researchers have built indices that attempt to measure the degree of financing constraints taking similar variables as inputs such as cash flow, leverage, dividends, cash holdings, sales and sales growth (Kaplan and Zingales, 1997; Lamont, Polk and Saá-Requejo,



(a) Investment Rate

(b) Debt Issuance

(c) Equity Issuance

(d) Cash Changes

Figure 7: Event Study Heterogeneity

Notes: This figure plots event study estimates of β_{post} from equation (2), replacing the 2018 and 2019 indicators and interactions with a single post-reform dummy and interaction in each equation. We display these estimates for subsamples of our estimation sample to explore heterogeneous impacts of the interest limitation. The four panels in order use investment scaled by lagged capital, debt issuance scaled by lagged assets, equity issuance scaled by lagged assets and cash changes 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.

responses to the interest limitation among subgroups. One explanation for the lack of debt

2001; Whited and Wu, 2006; Hadlock and Pierce, 2010).

[23]Dávila and Hébert (2023) suggest optimal corporate taxes should allow deductions for interest expense and retained earnings to use payouts as a tax base rather than profits. In their model, a payout tax is optimal because it only impacts firms paying dividends, while firms not paying dividends endogenously have better investment opportunities that they are unable to pursue because of financial constraints. In our case, firms not paying dividends that face the interest limitation raise more equity financing, suggesting a lack of access to external financing is unlikely to be restricting their investment.

issuance declines is that firms may place low value on interest deductions because they can only be used in the future (Edgerton, 2010; Zwick and Mahon, 2017). Firms without tax losses that get immediate benefits from interest deductions show suggestive evidence of larger debt issuance declines. Firms with a high ratio of short- to long-term debt have more immediate interest deductions, but still do not exhibit significant debt issuance declines. However, this is not necessarily surprising. An upwardly sloping yield curve implies the value of interest deductions is smaller for short-term debt when the deductions are more immediate, and when we focus on the above median interest rate firms in our sample, we do measure significant debt issuance declines. Overall, we see some evidence of debt issuance declines among firms with the most exposure to immediate tax benefits and facing very high interest rates, but the magnitudes of these declines are small.

Appendix Figure G.7 presents all of the same split sample estimates for our triple difference design. Results are similar, corroborating the heterogeneity patterns discussed above.

7 Discussion

7.1 Discussion of Investment Results

Neoclassical investment theory suggests that the WACC is a sufficient statistic for investment (Hall and Jorgenson, 1967), implying that when the interest limitation raises firm's cost of capital, they should invest less. In contrast, we find null investment responses to the interest limitation. In this section, we argue that we do not observe investment declines in response to the interest limitation because firms finance new investment projects with cash.

Firms make investment choices on a project level, and financing for each individual project could come from debt, equity, or cash. The interest limitation raises WACCs by increasing the cost of debt, but will not increase the cost of a marginal investment project if the project is not financed with debt. The null investment responses we estimate to the interest limitation suggest the cost of new investment projects does not change, and therefore that new investment projects are not financed with debt. In addition, equity issuance is too infrequent to provide year-to-year financing of new projects. Big, high-interest firms only issue equity in 33% of all firm-years before the reform, but make some positive investment in 92% of firm-years. The remaining option is that firms use cash to finance new investment projects. As shown in Figure 7, when firms have limited cash flexibility, they issue more equity and continue to invest at similar rates.

Our results seem to contradict a large literature in public finance that documents strong relationships between the cost of capital and investment (Cummins, Hassett and Hubbard, 1994, 1995, 1996; Hassett and Hubbard, 2002; Desai and Goolsbee, 2004; Edgerton, 2010; Zwick and Mahon, 2017; Chen, Jiang, Liu, Suarez-Serrato and Xu, 2023). However, a key difference between our work and this literature is that other work has focused on tax rate or depreciation variation in the tax term of the WACC, $\frac{1-\tau z}{1-\tau}$, which changes the after-tax value of every dollar of investment. In contrast, the interest limitation only changes the after-tax value of debt-financed investment.

If firms are not financing new investment with debt, there should be no detectable relationship between variation in the WACC financing term and investment in our setting. We test this relationship directly by regressing investment on the WACC financing term using our event study sample. We estimate

$$Y_{it} = \beta(\rho_{it} + \delta) + \delta_{jt} + \xi_i + \varepsilon_{it}, \tag{9}$$

with investment outcomes Y_{it} and financing term $(\rho_{it} + \delta)$. We use both the investment rate and log investment as outcomes and use both the raw and logged financing term for $(\rho_{it} + \delta)$. To address potential endogeneity in equation (9) from any correlation between firm investment and financial conditions, we instrument for $(\rho_{it} + \delta)$ with the interaction of Big_i and an indicator for the post-reform period. We display our coefficient estimates in Appendix Table H.4. The instrument has a strong positive relationship with our measures of the financing term. None of our IV estimates can reject zero, and the log-log regression coefficient point estimate of the investment rate elasticity with respect to the WACC financing term is positive. These regressions corroborate the lack of a clear negative relationship between the WACC financing term and investment in our setting, again suggesting firms are not relying on debt to finance investment.

The magnitude of our investment cost of capital elasticity estimates rule out other potential explanations for the lack of investment declines in response to the interest limitation. To understand the magnitude of possible responses, we first calibrate a static, frictionless investment model as a benchmark in Appendix E (Moon, 2022). The model yields an investment cost of capital elasticity around -10 under reasonable parameter assumptions. Using our event study design, we estimate an ITT investment rate cost of capital elasticity of 0.00 $[-0.41, 0.42]$ and a TOT investment rate cost of capital elasticity of 0.00 $[-0.72, 0.73]$. Triple difference estimates of these elasticities have similar magnitudes and slightly larger confidence intervals.

Many potential investment frictions, including adjustment costs, partial irreversibility, high hurdle rates, and incorrect estimates of WACCs, should reduce the frictionless benchmark elasticity (Doms and Dunne, 1998; Graham and Harvey, 2001; Cooper and Haltiwanger, 2006; Winberry, 2021; Chen, Jiang, Liu, Suarez-Serrato and Xu, 2023; Gormsen and Huber, 2024). However, while investment frictions are likely to attenuate the benchmark frictionless elasticity, they are unlikely to drive it all the way to zero. Existing estimates of investment rate cost of capital elasticities from samples of both publicly- and privately-held firms in settings with clear frictions suggest elasticity values around -2 (with standard error of 0.2) (Chen, Jiang, Liu, Suarez-Serrato and Xu, 2023).[24] Gormsen and Huber (2024) argue incorporating wedges between perceived and actual costs of capital into a benchmark investment model yields a substantial relationship between the cost of capital and investment. Clear investment responses to other tax policy changes (Kennedy, Dobridge, Landefeld and Mortenson, 2024) indicate that changes to the WACC impact investment in the presence of

[24]To study how tax policy impacted investment, early research regressed the investment rate on the tax term of the WACC in this paper, relying on variation from tax reforms at the industry level (Cummins, Hassett and Hubbard, 1994, 1995, 1996; Hassett and Hubbard, 2002; Desai and Goolsbee, 2004; Edgerton, 2010). This work reached a consensus that point estimates from these regressions for large, publicly-held firms were in the range from [-1,-0.5], often interpreting these point estimates as investment rate cost of capital elasticities under the strong assumption that firm's average investment rate is the same as their average cost of capital. Using similar variation and tax data on publicly- and privately-held firms to study bonus depreciation, Zwick and Mahon (2017) estimate a coefficient of -1.6 (s.e. 0.096). Estimates from these studies can be interpreted as ITT estimates because industry level variation in tax rates and investment incentives does not identify the specific firms facing changes in WACCs. Our ITT estimates reject the consensus range of estimates for large, publicly-held firms, and both our ITT and TOT estimates reject more recent estimates on publicly- and privately-held firms, even before accounting for inflation of previous estimates if WACCs exceed investment rates.

elevated hurdle rates (Graham and Harvey, 2001), but they do not in our setting.

Therefore, it seems unlikely that investment frictions alone can explain our zero elasticity estimates, and more likely that firms use cash to finance investment projects. This conclusion is not without precedent. Theories abound that firms have a pecking order (Myers, 1984), while existing empirical and survey evidence for U.S. firms suggests cash is a key source of financing (Yagan, 2015; Sharpe and Suarez, 2021).

7.2 Discussion of Borrowing Results

Static tradeoff theory yields clear predictions that when the tax benefit of debt declines, firms should borrow less, while modern dynamic models with endogenous investment and financing choices yield large borrowing declines in counterfactuals eliminating interest deductions (Glover, Gomes and Yaron, 2015; Ivanov, Pettit and Whited, 2024). In contrast, we find null debt issuance responses to the interest limitation. In this section, we argue that firms place low value on future interest deductions, a possibility often overlooked in the capital structure literature (Hanlon and Heitzman, 2010). In static tradeoff theory, there is no framework for discounting future interest deductions, while in dynamic models interest deductions are usually assumed to be taken when the borrowing occurs for tractability.[25]

Businesses are only sophisticated to a degree when making decisions. Existing research shows that heuristics and operational constraints play significant roles in firm decision-making (Jagannathan, Matsa, Meier and Tarhan, 2016; Gormsen and Huber, 2024), while firms sharply discount, or even ignore, future tax benefits (Edgerton, 2010; Zwick and Mahon, 2017). When a firm considers a new debt issuance, the interest deductions it receives will be spread over many years, and 71% of debt held by big, high-interest firms is due in more than one year in 2017. In our existing dynamic models firms would benefit from interest deductions immediately, but in practice firms may have to wait years. If firms discount these future interest deductions, there would be no reason for borrowing to decline in response to

[25]Dynamic models typically also assume for tractability that firms can borrow, or save, but cannot do both. This means the only savings technology available to a firm that is borrowing is capital investment. In contrast, in the data, the high-interest firms that face the limitation have substantial debt and liquidity on their balance sheets. In the model setting, the only way for a firm to obtain cash is to borrow (and therefore invest) less, a tradeoff that firms do not face in the data.

the interest limitation.

While placing low value on future interest deductions helps explain our results on average, some firms face differential immediacy of interest deductions. First, firms with tax losses get no benefit from immediate deductions, instead carrying them forward to future years. In Figure 7, we show suggestive evidence of larger debt issuance declines among firms without tax losses that benefit from immediate interest deductions.

Second, firms with more short-term debt have more imminent interest deductions, suggesting they should reduce borrowing by more in response to the interest limitation. While we do not see evidence of this in Figure 7, the interaction between placing low value on future deductions and an upwardly sloping yield curve makes the interest limitation well targeted to not induce large borrowing declines. Firms with short-term debt and interest deductions in the near future face lower interest rates and have less valuable interest deductions, while firms with long-term debt and interest deductions further in the future have more valuable interest deductions that are discounted. When we focus on the highest interest rate firms in Figure 7, we find modest debt issuance declines.

Alternative explanations for the lack of borrowing declines appear inconsistent with our evidence. Borrowing inaction can be caused by fixed costs, but these costs are typically modeled as debt issuance costs (Fischer, Heinkel and Zechner, 1989; Leary and Roberts, 2005; Danis, Rettl and Whited, 2014; Jeenas, 2024). In Appendix Tables C.6 and C.7, we estimate no impact of the interest limitation on short-term debt, long-term debt, or total debt. Big, high-interest firms have significant amounts of short-term debt on their balance sheet, 14% of assets in 2017, implying substantial amounts of debt are coming off firm's balance sheets that face the interest limitation, and they are actively choosing to continue borrowing at the same rates they were before the interest limitation was implemented. While this behavior could be explained by a fixed cost associated with the firm decision to change borrowing policy, it is inconsistent with an issuance fixed cost.

Alternative stories like a leverage ratchet effect (Admati, DeMarzo, Hellwig and Pfleiderer, 2018) or creditor evergreening (Faria-e-Castro, Paul and Sánchez, 2024) also predict a lack of borrowing declines among high-debt firms. However, while exploring responses of big, high-interest and big, low-interest firms in Appendix Figure C.4, we observe declines in debt for high-debt firms, we just do not observe differential declines in debt for firms facing the limitation. Our setting also suggests the lack of borrowing declines cannot be explained by future policy uncertainty. The interest limitation was a pay-for, not an expenditure, so rather than being set to expire in 2022, it was originally written into law to tighten in 2022 to a 30% of EBIT limit.

Finally, our estimates suggest the lack of borrowing declines cannot be explained by a slow response to the interest limitation (Fama and French, 2002; Huang and Ritter, 2009). We extend all of our event study and triple difference results through 2020 in Appendix F and do not find clear responses to the limitation in any of the three years following implementation. While we cannot rule out longer borrowing adjustment periods, the stability of our results does not support slow adjustment as an explanation.[26]

In summary, there is not a single theory of investment and financing that can account for the empirical moments we estimate in this paper. Nonetheless, our results suggest that big, high-interest firms facing the interest limitation primarily use cash to finance new investment, and that these firms do not appear to value future interest deductions when making borrowing choices.

8 Conclusion

In this paper we use tax data to estimate firm responses to the first broad interest limitation in modern U.S. history. Using event study, triple difference, and regression discontinuity research designs, we find that the interest limitation has economically small and statistically insignificant impacts on firm investment, debt issuance, and cash changes. Our event study and triple difference designs imply the interest limitation causes a modest increase in equity issuance. These findings suggest that limiting interest deductions raises tax revenue without having a significant impact on either borrowing or investment, that big, high-interest firms are not using debt to finance new investment, and that firms place low value on future interest deductions. While external validity to lower interest firms remains uncertain, heterogeneity

[26] Leary and Roberts (2005) show firms adjust borrowing in all of the first four years after equity issuances and large valuation shocks, suggesting slow adjustments should appear in the first few years after a shock.

analysis suggests extrapolation is reasonable, and future work on the 2022 tightening of the interest limitation to 30% of EBIT rather than EBITDA should provide additional useful evidence.

Finally, this paper brings new data to old debates about firm capital structure choices. The conditions under which the irrelevance of capital structure choices for firm value do or do not break down have spawned many theories of firm borrowing choices, often centered around a tradeoff between the tax benefits of debt and some cost (Modigliani and Miller, 1958, 1963; Miller, 1977; DeAngelo and Masulis, 1980). The results in this paper imply firms may not value the tax benefits of debt in the way researchers have thought, suggesting further questions for future work. If firms do not value interest deductions, why is borrowing so prevalent? If policymakers are concerned about rising corporate debt levels, will other policy changes impacting borrowing costs reduce aggregate borrowing?

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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	Max(0, Δ (sum of Schedule L, lines 22b and 23))	Max(0, Δ (sum of Schedule L, lines 22 and 23))	Max(0, 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 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
Payouts			
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
Adjusted taxable income			
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		
Receipts			
"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
Profits			
"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, White and Whitten, 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, McClelland, Pearce, Prisinzano, Sullivan, Yagan, Zidar and Zwick, 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 the union of 2018 and 2019 tax years:

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 Weighted Average Cost of Capital Construction and Sensitivity

We take a data driven approach to measuring the weighted average cost of capital (WACC). We generalize the typical Hall and Jorgenson (1967) expression for the cost of capital to account for debt and equity financing

$$MPK = (\rho + \delta)\frac{1 - \tau z}{1 - \tau}, \tag{B.1}$$

$$\rho = w_d(1 - \tau \mathbb{1}(Allow))r + w_e E) \tag{B.2}$$

with depreciation rate δ, tax rate τ, and net present value of depreciation deductions z, fraction of financing from debt w_d, fraction of financing from equity $w_e = 1 - w_d$, interest rate r, equity flotation cost E and $\mathbb{1}(Allow) = 1$ if a firm does not have interest disallowed. Table B.1 lists how we measure each parameter in our WACC expression.

Using our data to measure the cost of capital allows us to construct a cost measure that varies at the firm-year level. Figure B.1 plots event study coefficients of equation (2) using our WACC measure as an outcome variable. The WACC increases sharply in 2018 for treatment relative to control firms.

We ultimately use our WACC to construct elasticity estimates following equation (4). Restating that equation here,

$$\varepsilon = \frac{\beta_{post}}{\overline{Y}^T_{pre}} \Big/ \Big(\frac{\Delta UCC^T}{\overline{WACC}^T_{pre}} - \frac{\Delta WACC^C}{\overline{WACC}^C_{pre}}\Big),$$

the WACC enters the elasticity via the percent change in WACC term for treatment relative to control firms. To assess the sensitivity of our WACC elasticity estimates to different constructions of the WACC, we recalculate investment rate elasticities using different WACC constructions in Table B.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

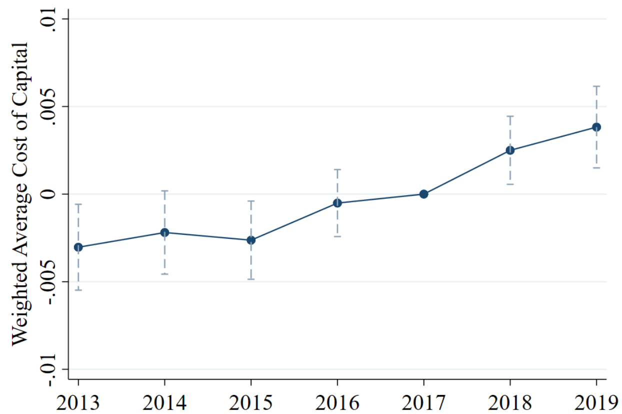


Figure B.1: Weighted Average Cost of Capital Event Study Estimates

Notes: This figure plots event study estimates of β_e from equation (2) using high-interest firms and the weighted average cost of capital as an outcome variable. 95% confidence intervals are constructed from standard errors clustered at the firm level.

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)), and in column 5 we assume both higher depreciation rates and higher equity flotation costs. The alternative construction in column 5 yields the largest changes in WACC 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 WACC elasticity estimates by less.

Table B.1: Weighted Average Cost of Capital Parameters

Parameter	Value	Source
$\mathbb{1}(Allow)$	{0,1}	Our Data
r	Interest Expense / Interest Bearing Liabilities	Our Data
w_d	Liabilities / Assets	Our Data
E	0.066	OTA (2014)
δ	0.08	Sanati (2022)
τ	C-corps: marginal rate, P-throughs: 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 weighted average cost of capital measure and their sources.

Table B.2: Investment Rate Elasticity Sensitivity to WACC Parameters

	(1)	(2)	(3)	(4)	(5)
β_{post}	0.001	0.001	0.001	0.001	0.001
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
ITT UCC Pct Change	0.104	0.086	0.082	0.097	0.077
ε^{ITT}	0.000	0.000	0.000	0.000	0.000
	(0.235)	(0.284)	(0.297)	(0.251)	(0.317)
TOT UCC Pct Change	0.059	0.051	0.047	0.055	0.045
ε^{TOT}	0.000	0.000	0.000	0.000	0.000
	(0.413)	(0.478)	(0.519)	(0.443)	(0.542)
Debt Fraction	$\frac{\text{Liabilities}}{\text{Assets}}$	$\frac{\text{Int Bearing Liab}}{\text{Int Bearing Liab + Equity}}$	$\frac{\text{Liabilities}}{\text{Assets}}$	$\frac{\text{Liabilities}}{\text{Assets}}$	$\frac{\text{Liabilities}}{\text{Assets}}$
E	0.066	0.066	0.066	0.107	0.107
δ	0.080	0.080	0.120	0.080	0.120

Notes: This table reports investment rate elasticity estimates while varying user cost parameters. The first row displays event study estimates of β_{post} from equation (2) using the investment rate as an outcome, replacing the indicators for 2018 and 2019 with a single indicator for an observation being in year 2018 or 2019. The estimation sample includes all high-interest firms in our panel data. Standard errors are clustered at the firm level and reported in parentheses. ITT and TOT UCC Pct Change is the percent change in the user cost of capital, calculated as the mechanical (ITT) or actual (TOT) percent change in the user cost of capital for treatment relative to control firms. We calculate ε as the outcome variable coefficient estimate divided by the pre-reform mean of the outcome variable, divided by the percent change in user cost. 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.

C Event Study and Triple Difference Robustness

In this section, we discuss evidence supporting the parallel trends assumption in our event study design, and present robustness checks of both the event study and triple difference designs.

C.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. Moving beyond visual inspection of parallel trends we present two additional pieces of analysis that provide evidence support this assumption

First, we find no 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. We define firms as high-interest if their interest is on average above their limitation from 2010-2012, and define firms as big if their average receipts from 2010-2012 exceed $25 million. Using this sample, we re-estimate equation (2) using only pass-through businesses, omitting 2012 as a base year and using a single post-reform indicator for years 2013-2014. Appendix Table C.1 reports the average post-reform coefficients for the investment rate, debt issuance, equity issuance, and cash changes. All four point 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. Appendix Table C.2 displays estimates of β_{post} from equation (2) estimated on the low-interest firms in our data. Column 1 uses interest disallowed as an outcome variable. We find statistically but not economically significant increases in interest disallowed, suggesting the placebo event study measures size-varying impacts of other TCJA reforms, not the interest limitation. The four remaining columns show that we cannot detect any 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 interest limitation, even in the presence of the other TCJA reforms. We plot the low-interest firm year-by-year event study estimates of first stage outcomes in Appendix Figure C.1 and ITT estimates of investment and financing outcomes in Appendix Figure C.2.

C.2 Robustness Checks

We begin our additional event study and triple difference robustness checks by exploring the sensitivity of our results to treatment persistence. Appendix Figure G.3 shows the lack of treatment persistence that attenuates our event study and triple difference ITT relative to TOT estimates comes from high-interest firms not continuing to have interest above their limitation, from firms electing out of the interest limitation, and from high-interest firms not having interest disallowed. Our results look similar when we use different samples constructed to have higher treatment persistence. Appendix Table C.3 displays event study estimates of β_{post} from equation (2) using an indicator for interest disallowed and our four main investment and financing outcome variables. Results remain similar across samples dropping firms that ever elect out of the interest limitation and high-interest firms that do not have interest disallowed, and using a high-interest definition requiring a firm has interest above their limitation in each year 2015-2017 instead of averaging over three years. The sample restrictions and alternative high-interest definition increase persistence so larger fractions of treatment firms have interest disallowed in the post-reform period, but also restrict the size of the sample, inflating standard errors. These offsetting effects lead to similar TOT elasticity confidence intervals.

Our event study and triple difference estimates are also robust to a variety of different outcome constructions for investment and financing variables. Appendix Tables C.4 and C.5 display event study estimates of β_{post} and triple difference estimates of γ_{post} alongside cost of capital elasticity estimates using log investment, extensive margin investment and an indicator for investment bursts exceeding 20% of lagged capital as outcome variables. Across our three additional investment measures and both research designs, five of the six estimates cannot reject zero, while our triple difference estimates suggest an economically small but statistically significant 1.4% decline in the fraction of firms investing.

The null changes in borrowing we observe in response to the interest limitation are also robust to a variety of alternative measures. Appendix Tables C.6 and C.7 display event study estimates of β_{post} and triple difference estimates of γ_{post} alongside cost of capital elasticity estimates using ten 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 liability out of assets so that liability changes do not influence leverage. In addition, we use other debt measures: debt plus loans from stockholders scaled by assets and financial capital, debt scaled by lagged assets, changes in debt plus loans from stockholders scaled by lagged assets, log debt and log debt plus loans from stockholders, short-term debt, long-term debt, and trade credit all scaled by lagged assets. Across our ten additional measures and both research designs, 18 of the 20 estimates cannot reject zero. This evidence makes three points. First, the interest limitation does not lead to an economically significant reduction in debt regardless of the specific leverage ratio or debt measure. Second, we do not find evidence of declines in stock or flow measures of debt in response to the interest limitation. Third, the interest limitation does not lead to significant substitutions between short- and long-term debt or trade credit.

One alternative hypothesis given the lack of significant investment declines in response to the interest limitation is that firms use debt to support payouts to shareholders or labor compensation, not investment. If this were the case, we would expect the interest limitation to lead to fewer payouts or less labor compensation. Appendix Tables C.8 and C.9 present event study estimates of β_{post} and triple difference estimates of γ_{post} alongside cost of capital elasticity estimates for additional payout and payroll outcomes. Our event study estimates suggest no significant payout (dividends plus share buybacks), payroll or executive compensation response to the interest limitation, while our triple difference estimates suggest an economically small but statistically significant increase in payouts, and cannot reject zero payroll or executive compensation response. These results suggest the increase in borrowing costs from the interest limitation does not lead to a decrease in payouts or payrolls.

To verify the salience of the shock, we directly measure taxes paid for C- and S-corporations, excluding partnerships because of tiered structures that obscure the ultimate ownership of income streams (Cooper, McClelland, Pearce, Prisinzano, Sullivan, Yagan, Zidar and Zwick, 2016). Figure C.3 plots event study estimates of β_e in Panel A and triple difference estimates of γ_e in Panel B using taxes paid scaled by lagged assets as an outcome variable. Our triple difference design shows that after controlling for other TCJA reforms, our treatment definition identifies a set of firms facing a tax increase equal to roughly 20% of the interest deductions disallowed.

Our event study and triple difference results are also robust to a number of different specifications, sample restrictions, and data processing choices. We present these tests in Appendix Table C.10, which displays event study estimates of β_{post} in Panel A and triple difference estimates of γ_{post} in Panel B for interest disallowed, as well as the investment rate, debt issuance, equity issuance and cash changes.

The baseline estimates we display in Tables 2 and 3 use three digit NAICS industry-year fixed effects and no control variables. The first row of each panel in Table C.10 displays event study or triple difference estimates using 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 second 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 third 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 fourth 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.

Business with significant real estate and agriculture components are allowed to opt out of the interest limitation in exchange for using a slower depreciation system. In practice, many real estate firms opt out of the interest limitation while few other firms do.[27] The fifth row of each panel drops real estate firms from our estimation sample and finds similar results.[28]

Some firms that appear small in our data may face the interest limitation because of aggregation rules. The sixth row of each panel drops all firms we flag as potential aggregators and finds similar results. The seventh row of each panel restricts to a balanced panel of firms that appear in ever year of our data to address concerns about sample attrition. Results remain unchanged. Finally, very large firms may have substantially different investment opportunities and access to capital markets than smaller firms. To alleviate concerns that the very largest firms in our treatment group drive our results, the seventh row of each panel drops the largest quarter of treatment firms from the estimation sample. Dropping the very largest firms from our sample does not substantially change our results.

C.3 Comparing Big High- and Low-Interest Firms

Carrizosa, Gaertner and Lynch (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 the debt to assets ratio for treatment relative to control firms of roughly 3% of lagged assets that reject zero, significantly larger than our ITT 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 the debt to assets ratio for big, high-interest relative to big, low-interest firms is likely to be driven by mean reversion rather than a response to the interest limitation.

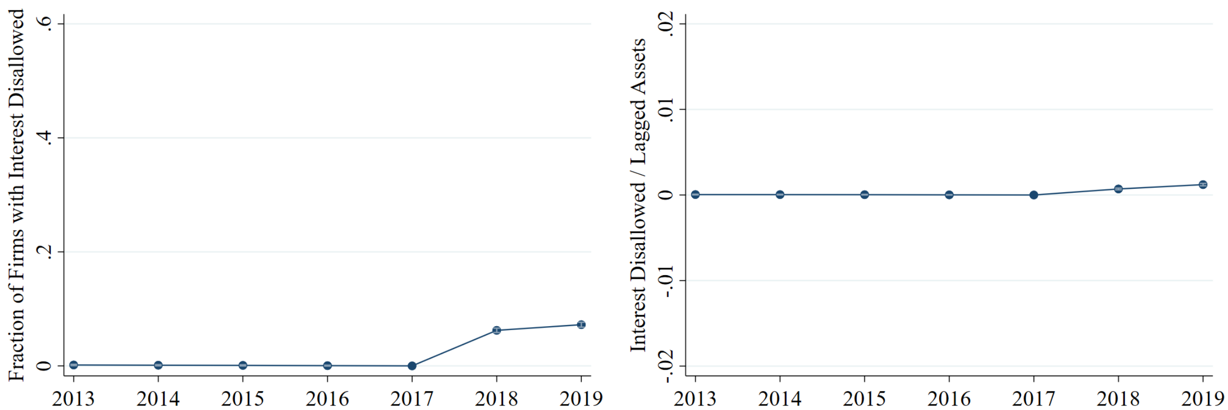
Debt and interest are highly correlated. Figure 2 shows that high interest firms facing the interest limitation are in the right tail of the interest distribution in the years that are used to

[27] In our panel data set from 2018-2019, real estate firms opt out of the interest 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.

[28] In Appendix Table C.3, we show our results are stable when dropping any firm that ever elects out of the interest limitation. Dropping real estate firms in Appendix Table C.10 is an *ex ante* restriction eliminating many firms that could elect out, avoiding selection issues arising from only dropping firms that do elect out.

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 treatment relative to control firm 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. If these dynamics are not driven by the interest 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 panel data set covering the same number of years, but starting in earlier years and using treatment definitions based on earlier years (2014-2016, 2013-2015, 2012-2014 and 2011-2013). In each panel data set with a treatment definition based on earlier years, we re-estimate equation (2). In figure C.4, we plot each of these placebo-in-time estimates setting event time equal to zero in the last year of the treatment definition for each panel. Each series in the figure shows that debt to assets remains stable for big high-interest relative to low-interest firms in the years of the treatment definition, but debt to assets is lower for big high-interest 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 debt to assets are driven by mean reversion for higher interest relative to lower interest firms, not a response to the interest 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 interest limitation.

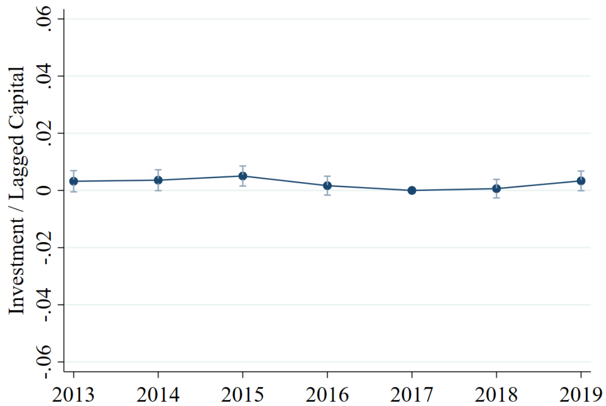


(a) Indicator for Interest Disallowed

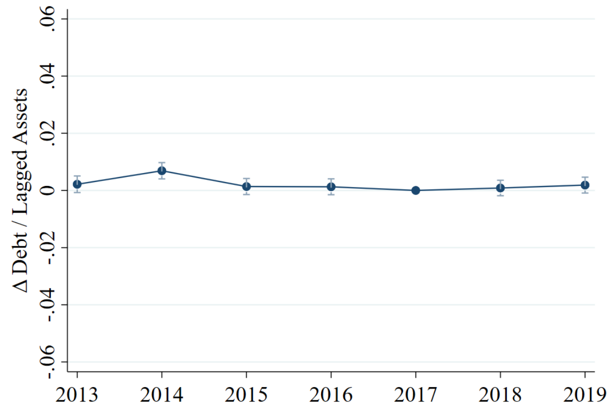
(b) Interest Disallowed

Figure C.1: First Stage Placebo Event Study Estimates

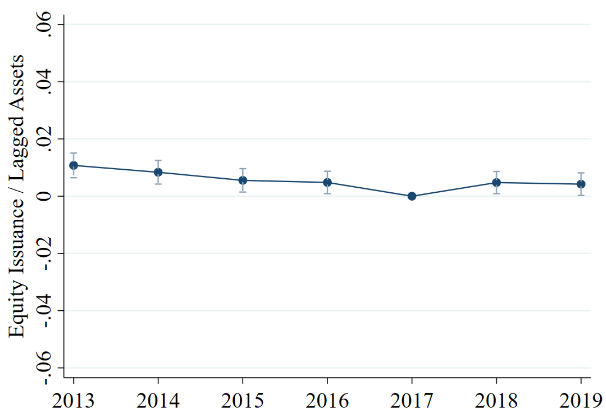
Notes: This figure plots event study estimates of β_τ from equation (2) using low-interest firms. Panel (a) uses an indicator equal to 1 if interest is disallowed as the outcome variable, while panel (b) uses interest disallowed scaled by lagged assets as the outcome variable. 95% confidence intervals are constructed from standard errors clustered at the firm level.



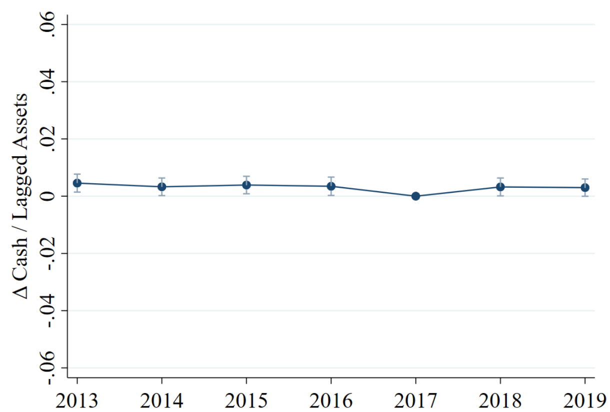
(a) Investment Rate



(b) Debt Issuance



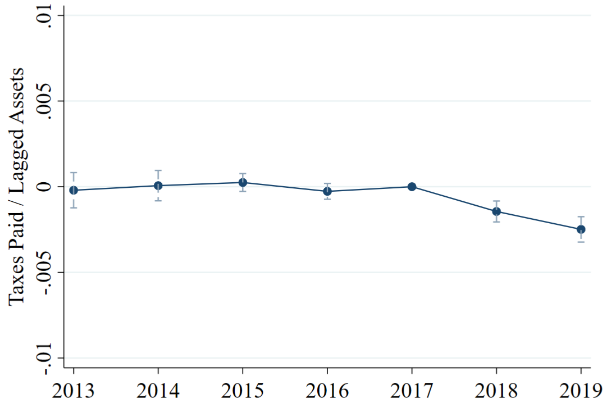
(c) Equity Issuance



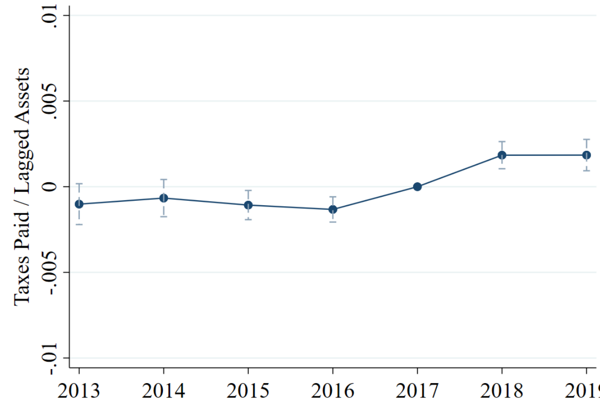
(d) Cash Changes

Figure C.2: Placebo Event Study Estimates

Notes: This figure plots event study estimates of β_τ from equation (2) using low-interest firms. Panel (a) uses investment scaled by lagged capital as an outcome variable. Panel (b) uses debt issuance scaled by lagged assets as an outcome variable. Panel (c) uses equity issuance scaled by lagged assets as an outcome variable, and panel (d) uses cash changes scaled by lagged assets as an outcome variable. 95% confidence intervals are constructed from standard errors clustered at the firm level.



(a) Event Study



(b) Triple Difference

Figure C.3: Taxes Paid Event Study and Triple Difference Estimates

Notes: This figure plots event study estimates of β_e following equation (2) in panel (a) and triple difference estimates of γ_e following equation (5) in panel (b), using taxes paid scaled by lagged assets as an outcome. The sample includes all C- and S-corporations in our core samples, excluding partnerships because of the infeasibility of tracing income streams through complicated ownership networks to measure taxes paid. 95% confidence intervals are constructed from standard errors clustered at the firm level.

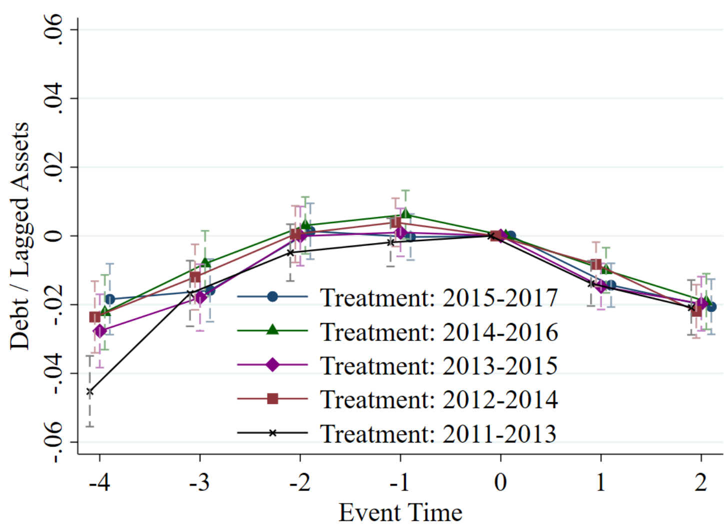


Figure C.4: Mean Reversion Around Treatment Definition Years

Notes: This figure plots event study estimates of β_e following equation (2), 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.1: Pass-through Responses to 2013 Individual Tax Rate Change

Outcome	(1) Investment Rate	(2) Debt Issuance	(3) Equity Issuance	(4) Cash Changes
β_{post}	-0.005	-0.005	-0.005	0.002
	(0.003)	(0.004)	(0.004)	(0.002)
Obs	52,494	54,737	54,737	54,737
Clusters	8,889	9,216	9,216	9,216
R^2	0.426	0.238	0.325	0.148

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 and firms that switches entity type. 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.2: Placebo Event Study Effect on Investment and Financing

Outcome	(1) Int Disallow	(2) Investment Rate	(3) Debt Issuance	(4) Equity Issuance	(5) Cash Changes
β_{post}	0.0009	0.0018	0.0009	0.0034	0.0026
	(0.0001)	(0.0015)	(0.0012)	(0.0017)	(0.0013)
Obs	315,229	285,361	315,229	315,229	315,229
Clusters	52,896	47,732	52,896	52,896	52,896
R^2	0.305	0.462	0.175	0.445	0.143
Pre-Reform Mean	0.000	0.340	0.011	0.034	0.015

Notes: This table reports event study estimates of β_{post} from equation (2), replacing the indicators for 2018 and 2019 with a single indicator for an observation being in year 2018 or 2019. 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.3: Event Study Robustness Varying Samples To Stengthen First Stage

	Int > Limit Avging 2015-17			Int > Limit 2015, 16 and 17		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: β_{post} Estimates					
Has Int Disallow	0.372	0.442	0.483	0.390	0.485	0.539
	(0.007)	(0.007)	(0.008)	(0.009)	(0.009)	(0.010)
Investment Rate	0.000	0.002	0.003	−0.002	0.000	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Debt Issuance	0.002	0.003	0.003	−0.003	−0.003	−0.003
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Equity Issuance	0.019	0.022	0.022	0.023	0.029	0.029
	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)	(0.008)
Cash Changes	−0.002	−0.002	−0.002	−0.001	−0.000	−0.000
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
	Panel B: ε^{TOT} Estimates					
Investment Rate	0.00	0.25	0.34	−0.28	0.04	0.15
	(0.37)	(0.39)	(0.38)	(0.45)	(0.47)	(0.44)
Debt Issuance	1.34	1.96	1.68	−2.10	−1.61	−1.70
	(2.13)	(2.24)	(2.19)	(2.35)	(2.48)	(2.30)
Equity Issuance	5.59	5.97	5.41	5.95	6.71	5.89
	(1.53)	(1.62)	(1.54)	(1.74)	(1.87)	(1.71)
Cash Changes	−0.27	−0.26	−0.29	−0.10	−0.03	−0.05
	(0.32)	(0.33)	(0.32)	(0.38)	(0.41)	(0.38)
Obs	89,523	80,283	76,820	57,807	50,682	48,386
Drop Electing Out Firms		✓	✓		✓	✓
Drop Noncompliant Firms			✓			✓

Notes: This table reports event study estimates of β_{post} from equation (2), replacing the indicators for 2018 and 2019 with a single indicator for an observation being in year 2018 or 2019. The baseline estimation sample in columns 1-3 includes all firms in our panel data with interest exceeding their limitation averaging over 2015-2017, while the baseline estimation sample in columns 4-6 includes all firms in our panel data with interest exceeding their limitation every year from 2015-2017. Standard errors are clustered at the firm level and reported in parentheses. ε^{TOT} is calculated as the coefficient estimate divided by the pre-reform mean of the outcome variables divided by the percent change in the actual weighted average cost of capital for treatment relative to control firms.

Table C.4: Event Study Effect on Alternative Investment Measures

Outcome	(1) Inv/Net Cap	(2) log(Inv)	(3) $\mathbb{1}(\text{Inv} > 0)$	(4) $\mathbb{1}(\text{Inv} > 0.2 * \text{Cap})$
β_{post}	0.001	-0.022	-0.003	-0.008
	(0.009)	(0.030)	(0.006)	(0.007)
β_{post}^{TOT}	0.002	-0.060	-0.007	-0.021
	(0.025)	(0.080)	(0.017)	(0.018)
Obs	$82,118$	$64,772$	$89,523$	$89,523$
Clusters	$14,831$	$13,026$	$16,098$	$16,098$
R^2	0.441	0.821	0.673	0.436
Pre-Reform Mean	0.315	14.245	0.923	0.187

Notes: This table reports event study estimates of β_{post} from equation (2) and β_{post}^{TOT} from equation (3), replacing the indicators for 2018 and 2019 with a single indicator for an observation being in year 2018 or 2019. The estimation sample includes all firms in our panel data with interest exceeding 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.5: Triple Difference Effect on Alternative Investment Measures

Outcome	(1) Inv/Net Cap	(2) log(Inv)	(3) $\mathbb{1}(\text{Inv} > 0)$	(4) $\mathbb{1}(\text{Inv} > 0.2 * \text{Cap})$
γ_{post}	0.001	0.028	-0.014	-0.004
	(0.009)	(0.031)	(0.006)	(0.007)
γ_{post}^{TOT}	0.006	0.071	-0.042	-0.011
	(0.025)	(0.084)	(0.017)	(0.019)
Obs	$358,904$	$302,220$	$404,762$	$404,762$
Clusters	$61,493$	$56,369$	$68,995$	$68,995$
R^2	0.459	0.847	0.703	0.399
Pre-Reform Mean	0.315	14.245	0.923	0.187

Notes: This table reports triple difference estimates of γ_{post} from equation (5) and γ_{post}^{TOT} from equation (6), replacing the indicators for 2018 and 2019 with a single indicator for an observation being in year 2018 or 2019. The estimation sample includes all firms in our panel data. Standard errors are clustered at the firm level and reported in parentheses.

Table C.6: Event Study Effect on Alternative Borrowing Measures

	(1)	(2)	(3)	(4)	(5)
			Panel A		
Outcome	$\frac{\text{Debt}}{\text{Fin Capital}}$	$\frac{\text{Debt+LSH}}{\text{Assets}}$	$\frac{\text{Debt+LSH}}{\text{Fin Capital}}$	$\frac{\text{Debt}}{\text{Assets}}$	$\frac{\Delta\text{Debt+LSH}}{\text{Assets}}$
β_{post}	-0.001	-0.002	-0.001	0.001	0.004
	(0.007)	(0.005)	(0.008)	(0.004)	(0.003)
β_{post}^{TOT}	-0.004	-0.004	-0.002	0.002	0.010
	(0.019)	(0.013)	(0.021)	(0.012)	(0.009)
Obs	89,516	89,523	89,516	89,523	89,523
Clusters	16,098	16,098	16,098	16,098	16,098
R^2	0.705	0.784	0.683	0.794	0.241
Pre-Reform Mean	0.634	0.515	0.699	0.470	0.030
			Panel B		
Outcome	log(Debt)	log(Debt + LSH)	$\frac{\text{Short Term Debt}}{\text{Assets}}$	$\frac{\text{Long Term Debt}}{\text{Assets}}$	$\frac{\text{Trade Credit}}{\text{Assets}}$
β_{post}	0.008	-0.013	0.002	-0.003	-0.002
	(0.018)	(0.017)	(0.002)	(0.004)	(0.002)
β_{post}^{TOT}	0.019	-0.033	0.005	-0.009	-0.005
	(0.046)	(0.042)	(0.006)	(0.011)	(0.005)
Obs	72,404	76,742	89,523	89,523	89,523
Clusters	13,787	14,458	16,098	16,098	16,098
R^2	0.923	0.923	0.806	0.808	0.806
Pre-Reform Mean	17.532	17.622	0.144	0.317	0.121

Notes: This table reports event study estimates of β_{post} from equation (2) and β_{post}^{TOT} from equation (3), replacing the indicators for 2018 and 2019 with a single indicator for an observation being in year 2018 or 2019. The estimation sample includes all firms in our panel data with interest exceeding 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.7: Triple Difference Effect on Alternative Borrowing Measures

	(1)	(2)	(3)	(4)	(5)
			Panel A		
Outcome	$\frac{\text{Debt}}{\text{Fin Capital}}$	$\frac{\text{Debt+LSH}}{\text{Assets}}$	$\frac{\text{Debt+LSH}}{\text{Fin Capital}}$	$\frac{\text{Debt}}{\text{Assets}}$	$\frac{\Delta\text{Debt+LSH}}{\text{Assets}}$
γ_{post}	-0.004	-0.005	-0.004	-0.002	0.002
	(0.006)	(0.004)	(0.007)	(0.004)	(0.003)
γ_{post}^{TOT}	-0.015	-0.017	-0.015	-0.008	0.002
	(0.019)	(0.013)	(0.021)	(0.012)	(0.009)
Obs	404,633	404,762	404,633	404,762	404,762
Clusters	68,981	68,995	68,981	68,995	68,995
R^2	0.756	0.830	0.750	0.828	0.203
Pre-Reform Mean	0.634	0.515	0.699	0.470	0.030
			Panel B		
Outcome	log(Debt)	log(Debt + LSH)	$\frac{\text{Short Term Debt}}{\text{Assets}}$	$\frac{\text{Long Term Debt}}{\text{Assets}}$	$\frac{\text{Trade Credit}}{\text{Assets}}$
γ_{post}	-0.044	-0.057	0.001	-0.005	0.002
	(0.019)	(0.017)	(0.002)	(0.004)	(0.002)
γ_{post}^{TOT}	-0.130	-0.169	0.002	-0.016	0.006
	(0.052)	(0.049)	(0.006)	(0.011)	(0.005)
Obs	266,146	287,662	404,762	404,762	404,762
Clusters	49,937	53,481	68,995	68,995	68,995
R^2	0.918	0.921	0.823	0.824	0.833
Pre-Reform Mean	17.532	17.622	0.144	0.317	0.121

Notes: This table reports triple difference estimates of γ_{post} from equation (5) and γ_{post}^{TOT} from equation (6), replacing the indicators for 2018 and 2019 with a single indicator for an observation being in year 2018 or 2019. The estimation sample includes all firms in our panel data. Standard errors are clustered at the firm level and reported in parentheses.

Table C.8: Event Study Effect on Equity, Payouts, and Labor Compensation

Outcome	(1) log(Equity)	(2) $\mathbb{1}$(Equity > 0)	(3) Payouts	(4) Payroll	(5) Exec Comp
β_{post}	0.0497	0.0045	0.0012	0.0051	0.0004
	(0.0626)	(0.0084)	(0.0014)	(0.0036)	(0.0006)
β_{post}^{TOT}	0.1394	0.0122	0.0034	0.0137	0.0011
	(0.1779)	(0.0227)	(0.0037)	(0.0097)	(0.0016)
Obs	$22,603$	$89,523$	$89,523$	$89,523$	$89,523$
Clusters	$6,625$	$16,098$	$16,098$	$16,098$	$16,098$
R^2	0.826	0.494	0.393	0.883	0.822
Pre-Reform Mean	15.836	0.326	0.017	0.256	0.014

Notes: This table reports event study estimates of β_{post} from equation (2) and β_{post}^{TOT} from equation (3), replacing the indicators for 2018 and 2019 with a single indicator for an observation being in year 2018 or 2019. The estimation sample includes all firms in our panel data with interest exceeding 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.9: Triple Difference Effect on Equity, Payouts, and Labor Compensation

Dependent Variable	(1) log(Equity)	(2) $\mathbb{1}$(Equity > 0)	(3) Payouts	(4) Payroll	(5) Exec Comp
γ_{post}	0.0280	0.0018	0.0068	0.0063	-0.0004
	(0.0637)	(0.0082)	(0.0017)	(0.0031)	(0.0005)
γ_{post}^{TOT}	0.0771	0.0061	0.0196	0.0183	-0.0007
	(0.1854)	(0.0241)	(0.0048)	(0.0088)	(0.0015)
Obs	$87,303$	$404,762$	$404,762$	$404,762$	$404,762$
Clusters	$24,057$	$68,995$	$68,995$	$68,995$	$68,995$
R^2	0.840	0.540	0.815	0.921	0.884
Pre-Reform Mean	15.836	0.326	0.017	0.256	0.014

Notes: This table reports triple difference estimates of γ_{post} from equation (5) and γ_{post}^{TOT} from equation (6), replacing the indicators for 2018 and 2019 with a single indicator for an observation being in year 2018 or 2019. The estimation sample includes all firms in our panel data. Standard errors are clustered at the firm level and reported in parentheses.

Table C.10: Robustness Tests

Outcome	(1) Int Disallowed	(2) Investment Rate	(3) Debt Issuance	(4) Equity Issuance	(5) Cash Changes
	Panel A: Event Study Estimates				
Ind x Prof x Yr FE	0.013	−0.001	0.003	0.011	−0.003
	(0.000)	(0.003)	(0.003)	(0.005)	(0.002)
Controls	0.013	0.000	0.003	0.018	−0.001
	(0.000)	(0.003)	(0.003)	(0.005)	(0.002)
Fixed Pre-Reform Scale	0.013	−0.001	0.003	0.011	−0.003
	(0.000)	(0.003)	(0.003)	(0.005)	(0.002)
Winsorize at 99^{th} pctile	0.015	0.004	−0.000	0.020	−0.004
	(0.001)	(0.006)	(0.005)	(0.008)	(0.004)
Drop Real Estate	0.015	0.003	0.003	0.023	−0.001
	(0.000)	(0.003)	(0.003)	(0.006)	(0.003)
Drop Aggregators	0.010	−0.007	0.003	0.014	−0.002
	(0.001)	(0.004)	(0.005)	(0.006)	(0.003)
Balanced Panel	0.012	−0.003	−0.002	0.014	−0.003
	(0.000)	(0.003)	(0.004)	(0.006)	(0.003)
Drop Largest	0.013	−0.003	0.002	0.018	−0.000
	(0.000)	(0.003)	(0.003)	(0.005)	(0.002)
	Panel B: Triple Difference Estimates				
Ind x Prof x Yr FE	0.012	−0.001	0.000	0.011	−0.004
	(0.000)	(0.003)	(0.003)	(0.004)	(0.002)
Controls	0.012	0.000	0.000	0.011	−0.004
	(0.000)	(0.003)	(0.003)	(0.004)	(0.002)
Fixed Pre-Reform Scale	0.015	−0.001	0.000	0.009	−0.004
	(0.001)	(0.002)	(0.003)	(0.003)	(0.002)
Winsorize at 99^{th} pctile	0.014	−0.003	−0.002	0.021	−0.005
	(0.001)	(0.006)	(0.004)	(0.008)	(0.003)
Drop Real Estate	0.013	0.002	0.001	0.018	−0.004
	(0.000)	(0.003)	(0.003)	(0.005)	(0.003)
Drop Aggregators	0.008	−0.006	0.007	0.012	−0.004
	(0.001)	(0.004)	(0.005)	(0.005)	(0.003)
Balanced Panel	0.012	−0.001	−0.003	0.013	−0.003
	(0.000)	(0.003)	(0.004)	(0.005)	(0.003)
Drop Largest	0.012	−0.003	0.002	0.010	−0.002
	(0.000)	(0.003)	(0.003)	(0.005)	(0.002)

Notes: This table reports robustness tests of event study estimates of β_{post} from equation (2) in Panel A and triple difference estimates of γ_{post} from equation (5) in Panel B, replacing indicators for 2018 and 2019 with a single indicator for years 2018 or 2019. Standard errors are clustered at the firm level and reported in parentheses.

D 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 design, 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 past receipts, we would observe bunching at the $25 million threshold averaging receipts over 2015-2017. However, we see no bunching at this threshold. Figure D.1 displays a density plot of the distribution of firms around the $25 million lagged receipts cutoff. A McCrary test suggests there is no discontinuity in the distribution of lagged receipts around the cutoff. We also do not find evidence of bunching using 2016-2018 receipts or 2017-2019 receipts, suggesting endogenous receipts responses in later years are not a concern. Figure D.2 plots discontinuity tests using the two later sets of years and finds no evidence of discontinuities at 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 $25 million lagged receipts threshold 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 D.1 displays estimates of β^{RF} from equation (7) using all low-interest firms. We cannot reject zero impact of the opportunity to change from accrual to cash accounting on the investment rate, debt issuance, equity issuance or cash changes, suggesting the opportunity for firms below the receipts threshold to switch to cash accounting does not bias our RD estimates.

In addition, we show our RD results are not sensitive to the choice of bandwidth or polynomial degree. Figure D.3 presents reduced form RD estimates of equation (7) varying the bandwidth and polynomial degree. Our main estimates remain qualitatively similar regardless of the choice of bandwidth or polynomial degree.

D.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 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 information from Form 8990 that tracks interest disallowed and allows us to measure total interest, firm-level interest rates and WACCs. 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.[29] Corrections in the SOI data lead to substantial 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 D.2 displays summary statistics for our four key outcome variables for all firms within $16 million of the $25 million lagged receipts cutoff in the SOI and E-filing data. There are more than ten times as many firms in the E-filing data as in the SOI data. The average of the investment rate is higher in the E-filing data. The variance of all four core outcomes 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 cannot estimate a first stage or elasticities. We continue to use a triangular kernel and a first degree polynomial. We use a $5 million bandwidth, the optimal bandwidth suggested by Calonico, Cattaneo and Titiunik (2014).

Figure D.4 displays raw means of the investment rate, debt issuance, equity issuance and cash changes in evenly spaced $500,000 bins within a $5 million bandwidth using the E-filing data. Once again, we observe no clear visual discontinuity for any of the four key outcome

[29] For 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.

variables, strengthening the evidence that there is no large change in the investment rate or financing choices for marginal firms just above the lagged receipts cutoff. Figure D.1 also shows no bunching around the $25 million lagged receipts cutoff in the E-filing data.

Table D.3 displays reduced form estimates of β^{RF} from equation (7) using the E-filing data. Our estimates cannot reject 0 across any of the four key investment and financing variables. Continuing to use the E-filing data, Table D.4 shows null results for a placebo RD on low-interest firms, while Figure D.5 shows our estimates remain qualitatively similar regardless of the choice of bandwidth or polynomial degree.

D.2 Reconciling 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, 2024), 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 securities[30], 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 the sample of publicly held firms in an attempt to align with their estimates. Table D.5 displays reduced form and fuzzy RD estimates of equations (7) and (8) 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 382 total firms. Considering our ITT estimates, we cannot reject zero impact of the interest

[30] Big, high-interest firms in our data have cash equalling 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).

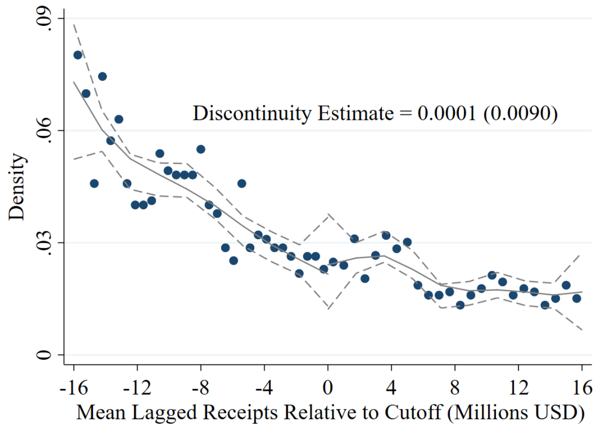
limitation on the investment rate, debt issuance, equity issuance or cash changes. However, the confidence intervals are wider than our event study, triple difference, or baseline RD estimates and admit substantial declines or increases in debt issuance.

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 $16 million bandwidth, and construct 2,000 random samples of 360 observations on each side of the cutoff with replacement (exceeding the sample size in Sanati and Beyhaghi (2024)). For each random sample, we re-estimate our reduced form RD specification from equation (7) using debt issuance as an outcome. Figure D.6 plots the cumulative density function of these RD estimates across random samples. The 95% confidence interval on the full sample RD estimate spans [-0.053,0.005]. More than 28% of the coefficient estimates from the simulated samples fall outside this confidence interval, differing from the baseline point estimate by more than the absolute value of the point estimate, 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.

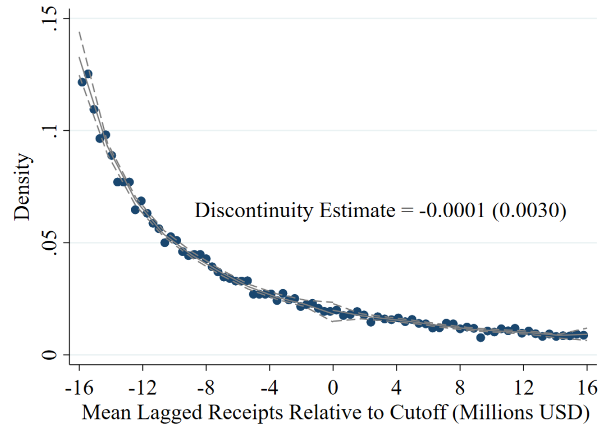
One important additional note is that our TOT estimates are substantially larger than our ITT estimates. All the estimates in Sanati (2022) and Sanati and Beyhaghi (2024) are ITT estimates because they lack tax data necessary to estimate which firms actually have interest disallowed in the post-reform period. Guided by our results for public firms in Table D.5, 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.[31] This could attenuate the first stage further, generating even larger TOT estimates, or lead to a fully misspecified discontinuity

[31] 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.

without any first stage.



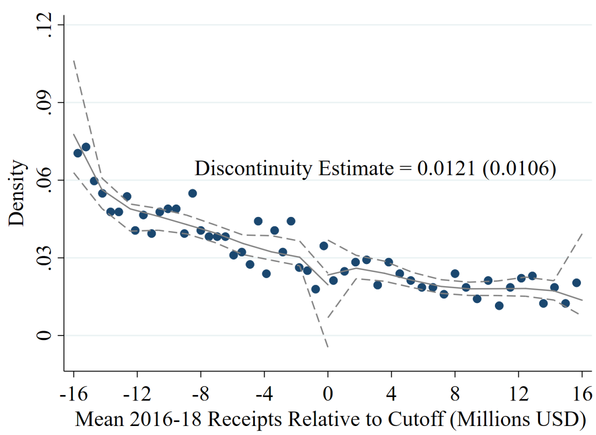
(a) SOI



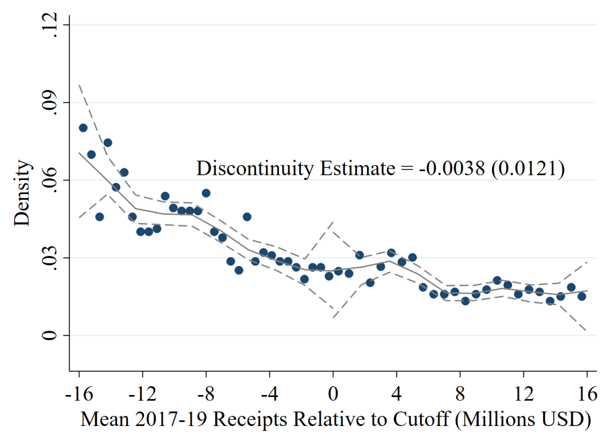
(b) E-filing

Figure D.1: Discontinuity Tests

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



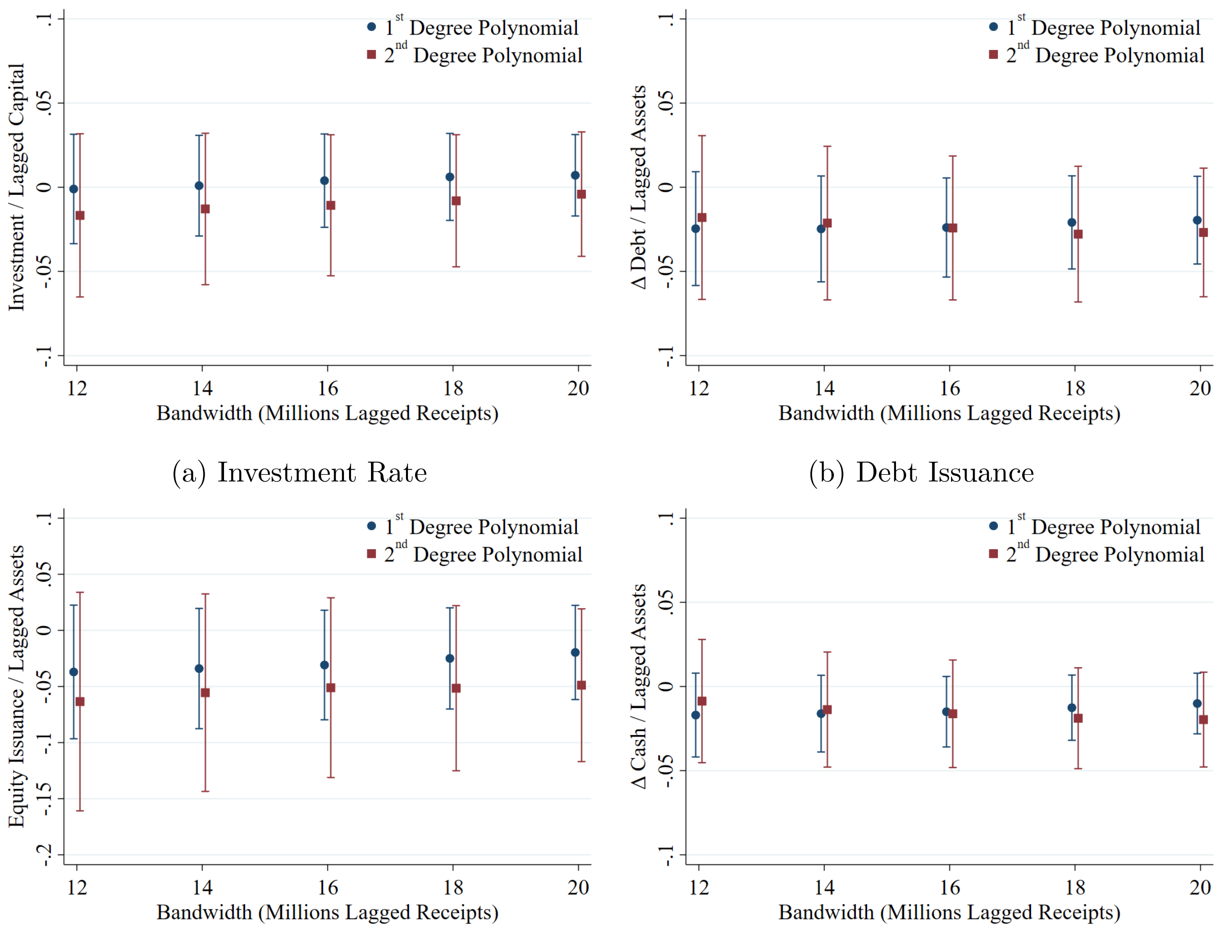
(a) 2016-18 SOI Receipts



(b) 2017-19 SOI Receipts

Figure D.2: Discontinuity Tests in Later Years

Notes: This figure reports the McCrary test for a discontinuity in the distribution density of average receipts in the SOI data over 2016-2018 in panel (a) and 2017-2019 in panel (b). Both figures use a $26 million cutoff to adjust for inflation. Neither discontinuity estimate is statistically different from zero.



(a) Investment Rate

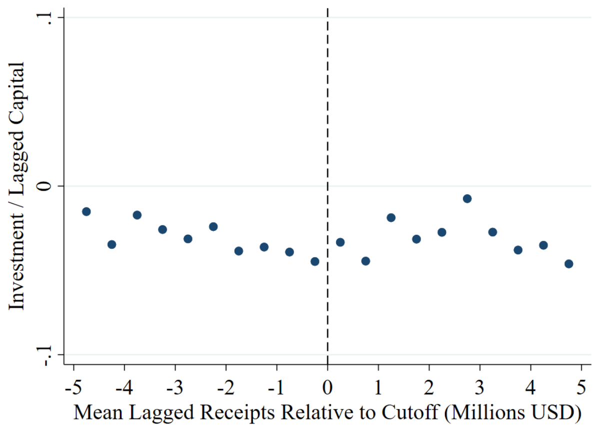
(b) Debt Issuance

(c) Equity Issuance

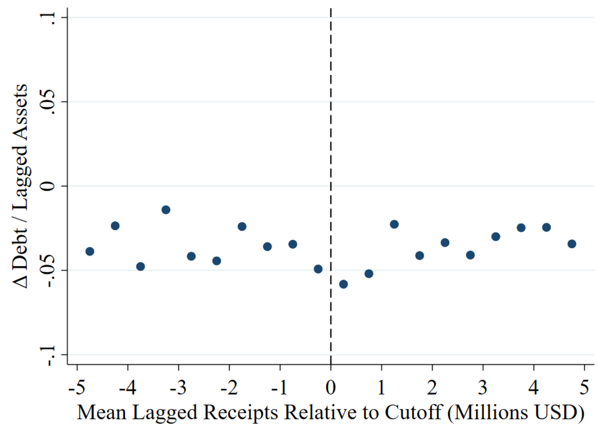
(d) Cash Changes

Figure D.3: Regression Discontinuity Alternative Specifications

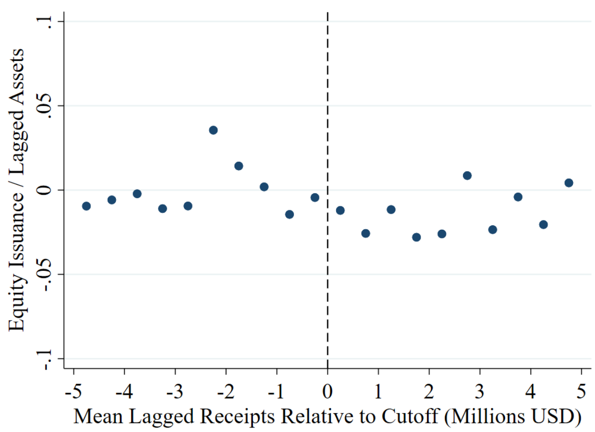
Notes: This figure plots regression discontinuity estimates of β^{RF} from equation (7) using the SOI data and varying the bandwidth and polynomial order. Panel (a) uses investment scaled by lagged capital as an outcome variable, panel (b) uses debt issuance scaled by lagged assets as an outcome variable, panel (c) uses equity issuance scaled by lagged assets as an outcome variable, and panel (d) uses cash changes scaled by lagged assets as an outcome variable. 95% confidence intervals are constructed from robust standard errors.



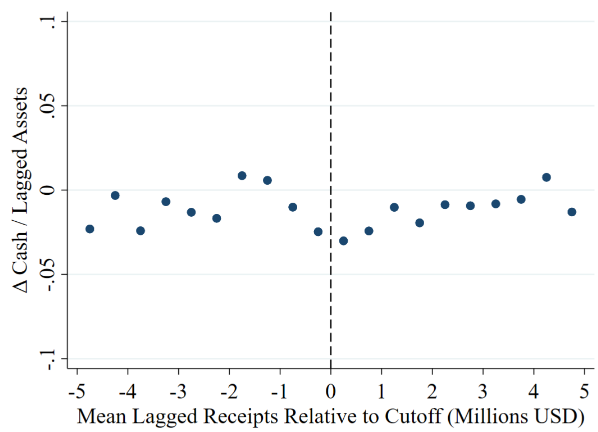
(a) Investment Rate



(b) Debt Issuance



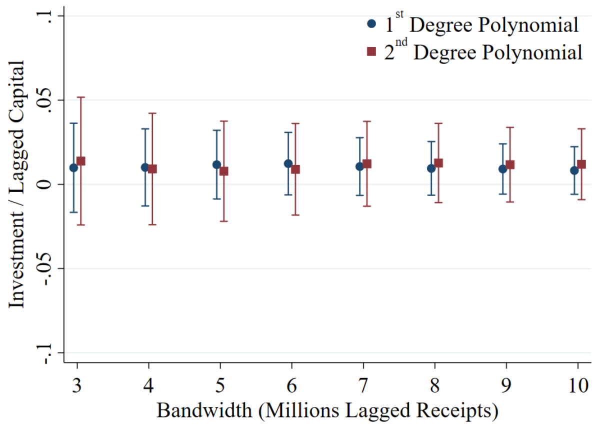
(c) Equity Issuance



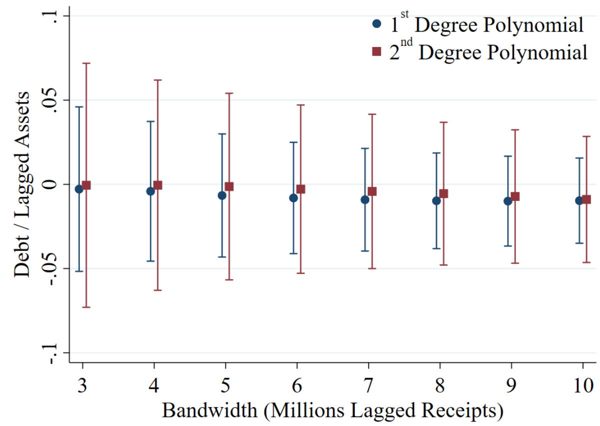
(d) Cash Changes

Figure D.4: E-filing Regression Discontinuity Binned Scatter Plots

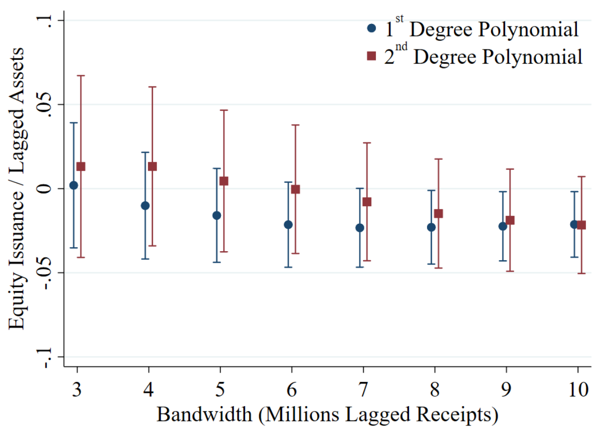
Notes: This figure plots average values of outcome variables in evenly spaced $500,000 receipts bins around the $25 million cutoff using the E-filing data. Panel (a) displays averages for investment scaled by lagged capital, panel (b) displays average debt issuance scaled by lagged assets, panel (c) displays average equity issuance scaled by lagged assets, and panel (d) displays average cash changes scaled by lagged assets.



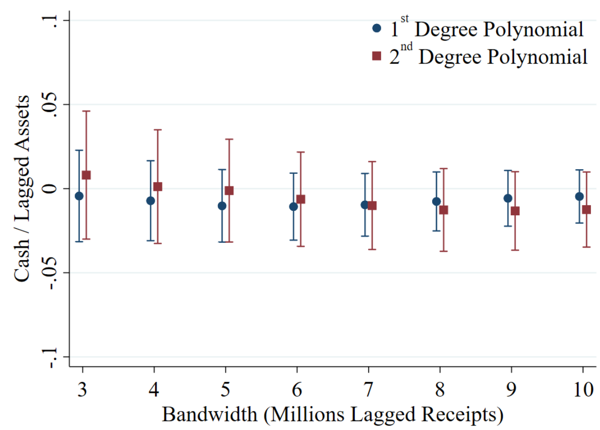
(a) Investment Rate



(b) Debt Issuance



(c) Equity Issuance



(d) Cash Changes

Figure D.5: Regression Discontinuity Alternative Specifications Using E-filing Data

Notes: This figure plots regression discontinuity estimates of β^{RF} from equation (7) 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 issuance scaled by lagged assets as an outcome variable, panel (c) uses equity issuance scaled by lagged assets as an outcome variable, and panel (d) uses cash changes scaled by lagged assets as an outcome variable. 95% confidence intervals are constructed from robust standard errors.

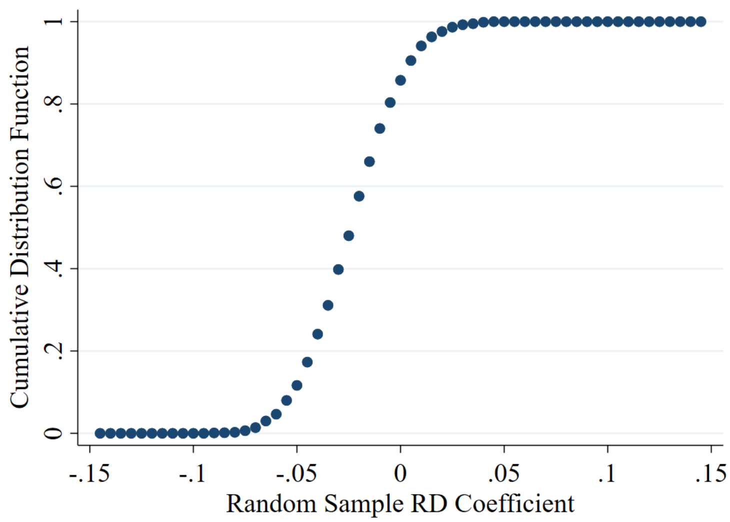


Figure D.6: Regression Discontinuity Estimates from Small Random Samples

Notes: This figure plots a CDF of 2,000 regression discontinuity estimates of β^{RF} from equation (7) using random samples of firms from the SOI data. For each random sample we select 360 firms on each side of the cutoff with replacement within a \$16 million bandwidth of the \$25 million receipts cutoff. The outcome variable for the regression discontinuity estimates is debt issuance scaled by lagged assets.

Table D.1: Placebo Regression Discontinuity Effect on Investment and Financing

Outcome	(1) Investment Rate	(2) Debt Issuance	(3) Equity Issuance	(4) Cash Changes
β^{RF}	-0.004	0.009	-0.004	-0.011
	(0.011)	(0.009)	(0.010)	(0.010)
Obs	7,031	7,393	7,393	7,393
Pre-Reform Mean	0.113	0.008	0.028	0.018

Notes: This table reports regression discontinuity estimates of β^{RF} from Equation (7) for all low-interest firms using a bandwidth of \$16 million receipts in the SOI data. Robust standard errors are reported in parentheses. Pre-reform means are the average level value of the outcome variable over 2015-2017 for firms with average receipts above the \$25 million cutoff.

Table D.2: Summary Statistics in SOI and E-filing Data

	Mean	Std Dev	P10	P50	P90	Obs
SOI Data						
Investment / Lagged Capital	0.076	0.113	0.000	0.031	0.228	1,630
Δ Debt / Lagged Assets	0.003	0.101	-0.114	-0.000	0.125	1,676
Equity Issuance / Lagged Assets	0.063	0.215	0.000	0.000	0.110	1,676
Δ Cash / Lagged Assets	0.008	0.071	-0.054	0.001	0.079	1,676
E-filing Data						
Investment / Lagged Capital	0.076	0.122	0.000	0.026	0.231	20,722
Δ Debt / Lagged Assets	-0.004	0.117	-0.154	0.000	0.129	20,738
Equity Issuance / Lagged Assets	0.054	0.185	0.000	0.000	0.109	20,738
Δ Cash / Lagged Assets	0.002	0.104	-0.091	0.089	0.093	20,738

Notes: This table reports summary statistics on average 2018-2019 outcomes for firms within \$16 million of the \$25 million lagged receipts cutoff from the SOI and E-filing data. To preserve taxpayer anonymity, percentile statistics are reported as the means of all observations in the (P-1,P+1)th percentiles.

Table D.3: Regression Discontinuity Effect on Investment and Financing Using E-filing Data

Outcome	(1) Investment Rate	(2) Debt Issuance	(3) Equity Issuance	(4) Cash Changes
β^{RF}	0.012	-0.009	-0.016	-0.017
	(0.010)	(0.011)	(0.014)	(0.010)
Obs	$3,990$	$4,009$	$4,009$	$4,009$
Pre-Reform Mean	0.107	0.032	0.057	0.012

Notes: This table reports regression discontinuity estimates of β^{RF} from Equation (7) for all high-interest firms in the E-filing RD sample using a bandwidth of $5 million receipts. Robust standard errors are reported in parentheses. Pre-reform means are averages over 2015-2017 for firms above the receipts cutoff.

Table D.4: Placebo Regression Discontinuity Effect on Investment and Financing Using E-filing Data

Outcome	(1) Investment Rate	(2) Debt Issuance	(3) Equity Issuance	(4) Cash Changes
β^{RF}	-0.000	0.000	0.005	0.003
	(0.004)	(0.003)	(0.004)	(0.005)
Obs	$21,285$	$21,327$	$21,327$	$21,327$
Pre-Reform Mean	0.116	0.010	0.032	0.035

Notes: This table reports regression discontinuity estimates of β^{RF} from Equation (7) for all low-interest firms using a bandwidth of $5 million receipts in the E-filing data. Robust standard errors are reported in parentheses. Pre-reform means are the average level value of the outcome variable over 2015-2017 for firms with average receipts above the $25 million cutoff.

Table D.5: Regression Discontinuity Effect on Investment and Financing: Public Firms

Outcome	(1) Investment Rate	(2) Debt Issuance	(3) Equity Issuance	(4) Cash Changes
β^{RF}	0.019	-0.058	0.007	0.044
	(0.109)	(0.060)	(0.081)	(0.052)
β^{IV}	0.068	-0.202	0.023	0.152
	(0.386)	(0.220)	(0.281)	(0.191)
Obs	355	382	382	382
Pre-Reform Mean	0.436	0.342	0.288	0.310
First Stage F-Stat	6.131	6.986	6.986	6.986
ITT WACC % Δ	0.20	0.24	0.24	0.24
ε^{ITT}	0.21	-0.72	0.10	0.59
	(1.22)	(0.74)	(1.19)	(0.71)
TOT WACC % Δ	0.21	0.22	0.22	0.22
ε^{TOT}	0.21	-0.77	0.10	0.64
	(1.21)	(0.80)	(1.28)	(0.77)

Notes: This table reports regression discontinuity estimates of β^{RF} from Equation (7) and β^{IV} from Equation (8) 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. ITT and TOT WACC Pct Change is the percent change in the weighted average cost of capital, calculated as the RD estimate of β^{RF} using mechanical (ITT) or actual (TOT) cost of capital as the outcome variable, divided by the pre-reform mean of the relevant user cost measure. We calculate ε as the ITT coefficient divided by the pre-reform mean of the outcome variable, divided by the relevant percent change in cost of capital.

E Frictionless Investment Model

In this appendix, we describe a frictionless investment model following the construction in Moon (2022) and calibrate the model to derive a prediction for the investment cost of capital elasticity. The setup is intentionally standard.

1. Output is $y = AL^{\alpha_L}K^{\alpha_K}$, with $0 < \alpha_L + \alpha_K < 1$.
2. Investment is $I_t = K_t - (1-\delta)K_{t-1}$ with depreciation rate δ. This implies that at steady state, $I = \delta K$.
3. The exogenous cost of labor is w.
4. The cost of capital is Ω.

The firm problem can be written as

$$\min_{L,K} wL + \Omega K \text{ s.t. } y = AL^{a_L}K^{a_K}.$$

This formulation implies a cost function and marginal cost function

$$\begin{aligned} C\left(y; w, \Omega\right) &= \left(\alpha_L + \alpha_K\right)\left[\frac{y}{A}\left(\frac{w}{\alpha_L}\right)^{\alpha_L}\left(\frac{\Omega}{\alpha_K}\right)^{\alpha_K}\right]^{\frac{1}{a_L+a_K}}, \\ MC\left(y; w, \Omega\right) &= \left[\frac{y^{1-\alpha_L-\alpha_K}}{A}\left(\frac{w}{\alpha_L}\right)^{a_L}\left(\frac{\Omega}{\alpha_K}\right)^{a_K}\right]^{\frac{1}{a_L+a_K}}. \end{aligned}$$

We assume a downward sloping inverse product demand curve given by $p = Dy^{1/\epsilon}$ with product demand elasticity ϵ. This implies total revenue is $TR(y;\epsilon) = Dy^{1/\epsilon+1}$ and marginal revenue is $MR(y;\epsilon) = (1/\epsilon+1)Dy^{1/\epsilon}$. Firms maximize profits by setting marginal revenue equal to marginal cost, which yields an expression for capital K

$$K = \left[\left(\frac{1}{\epsilon}+1\right)^{a_L+\alpha_K} AD^{\alpha_L+\alpha_K}\left(\frac{\alpha_L}{w}\right)^{\alpha_L}\left(\frac{\alpha_K}{\Omega}\right)^{\left(1-(\alpha_L+\alpha_K)\left(\frac{1}{\epsilon}\right)-\alpha_L\right)\right)}\right]^{\frac{1}{1-(\alpha_L+\alpha_K)\left(\frac{1}{\epsilon}+1\right)}}.$$

The interest limitation changes the expected rate of return. The implied change in the capital stock for a small change in the cost of capital Ω is given by

$$\frac{dK^*}{d\Omega} = \left(\frac{(\alpha_L + \alpha_K)\frac{1}{\epsilon} + \alpha_L - 1}{1 - (\alpha_L + \alpha_K)(\frac{1}{\epsilon} + 1)} \right) \left(\frac{K^*}{\Omega} \right),$$

implying we can write the capital stock elasticity as

$$\frac{dK^*/K^*}{d\Omega/\Omega} = \frac{(\alpha_L + \alpha_K)\frac{1}{\epsilon} + \alpha_L - 1}{1 - (\alpha_L + \alpha_K)(\frac{1}{\epsilon} + 1)}.$$

In steady state, $I = \delta K$, so the investment elasticity is given by

$$\frac{dI/I}{d\Omega/\Omega} = \frac{1}{\delta} \frac{(\alpha_L + \alpha_K)\frac{1}{\epsilon} + \alpha_L - 1}{1 - (\alpha_L + \alpha_K)(\frac{1}{\epsilon} + 1)}.$$

Plugging reasonable parameter values into this expression such as $\alpha_L = 0.55, \alpha_K = 0.15, \epsilon = -5, \delta = 0.13$ yields large elasticity estimates. This particular parameterization yields an investment cost of capital elasticity of -10.3.

F 2020 Responses to the Interest Limitation

In this appendix, we extend our event study and triple difference estimates to include firm responses in 2020. To perform this analysis, we reconstruct our baseline panel data set, requiring that firms are present in one year between 2018-2020 instead of 2018-2019.

Estimates of firm responses to the interest limitation in 2020 may be confounded by two factors. First, the interest limitation was loosened by the CARES act to cap interest deductions at 50% of EBITDA rather than 30% of EBITDA in 2020. Second, COVID created a large economic shock in 2020 which may have differentially impacted treatment and control firms. Nevertheless, these estimates could provide useful information about the medium-run responses of firms to the interest limitation.

Figure F.1 plots estimates of β_e from equation (2) using high-interest firms from our extended panel including 2020. The fraction of firms with interest disallowed and the amount of interest disallowed both decline in 2020 relative to 2019, consistent with the loosening of the interest limitation from 30% of EBITA to 50% of EBITDA.

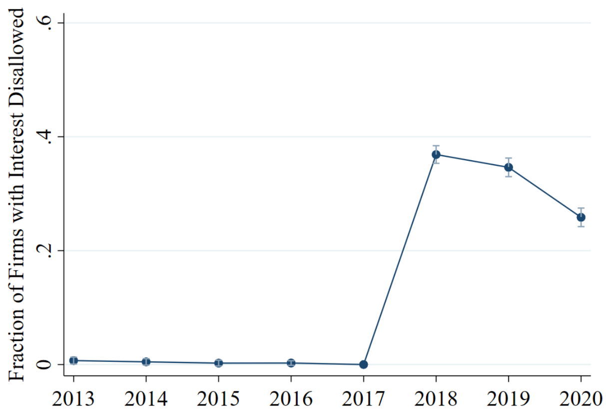
Figure F.2 plots estimates of β_e from equation (2) for our four main outcomes using high-interest firms from our extended panel. Panel (a) plots firm investment rate responses. The investment rate point estimate drops below zero in 2020, but this decline could be due to COVID-19 differentially impacting larger treatment firms rather than the interest limitation and still does not reject zero.

Panel (b) plots firm debt issuance responses. While the 2018 and 2019 coefficients are both close to 0 and reject declines in debt issuance of more than 1% of lagged assets, the 2020 estimate shows a statistically significant decline in debt issuance of roughly 2% of lagged assets. We attribute this decline to the COVID-19 shock differentially impacting large relative to small high interest firms, and not to the interest limitation, for three reasons. First, we would expect any response to the interest limitation to begin in 2018, not 2020. Instead, we estimate zero detb issuance responses in 2018 or 2019. Second, the interest limitation was loosened from 30% of EBITDA plus interest income to 50% of EBITDA plus interest income in 2020. If the interest limitation were to have an effect, we would expect that effect to be smaller, not larger, in 2020, when the limitation applies to fewer firms and disallows fewer interest deductions. Third, placebo event study estimates in Figure F.3 comparing larger and smaller low interest firms that do not face the interest limitation show a nearly identical pattern with zero estimates in 2018 and 2019 and a similarly sized decline in 2020.

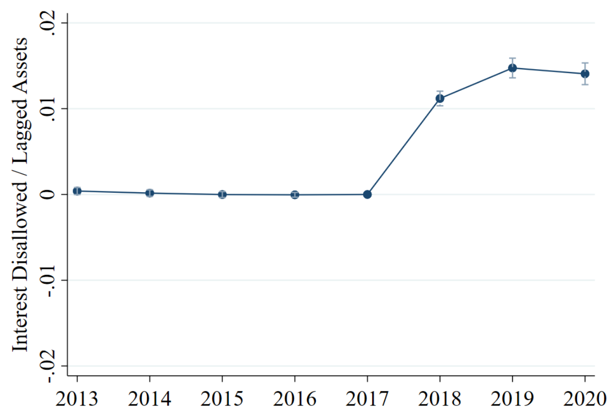
Figure F.2, panel (c) plots firm equity issuance responses. The 2020 coefficient shows little deviation from the 2018 or 2019 coefficient. Panel (d) plots cash change responses. The 2020 estimate diverges from the 2018 and 2019 estimates and shows a statistically significant decline in cash changes of roughly 1% of lagged assets, but we again attribute this decline to the COVID-19 shock, not the interest limitation. The decline in cash does not correspond with the implementation of the policy, and we see an identical decline in cash for larger versus smaller low interest firms in 2020 in Figure F.3.

Figure F.3 plots estimates of β_e from equation (2) for our four main outcomes for low-interest firms that do not face the interest limitation. We observe sharp declines in β_e in 2020 for the investment rate, debt issuance and cash changes, and no change in equity issuance estimates. These declines suggest that COVID-19 differentially impacted large versus smaller low-interest firms and triple difference estimates that control for those impacts may be more appropriate to evaluate firm responses to the interest limitation in 2020.

Figure F.4 plots estimates of γ_e from equation (5). Once we control our estimates for the differential impact of COVID-19 on big versus small firms, we cannot reject zero investment rate, debt issuance, or cash change responses to the interest limitation, and estimate qualitatively similar equity issuance increases. These results suggest the interest limitation had similar impacts on firms investment and financing choices in 2020.



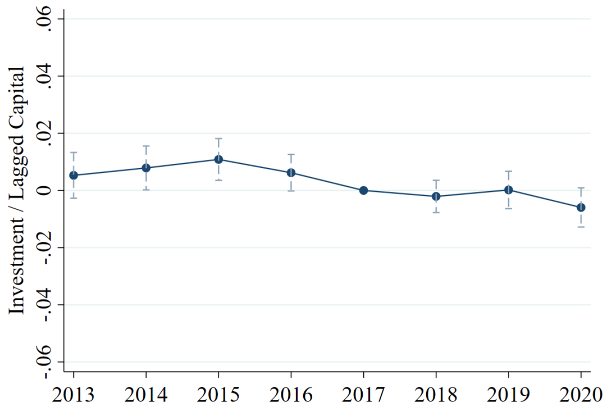
(a) Indicator for Interest Disallowed



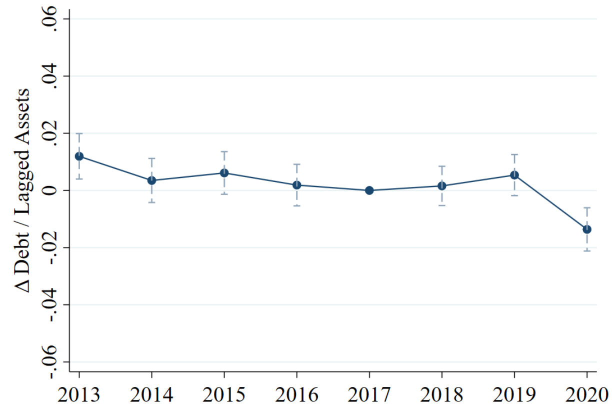
(b) Interest Disallowed

Figure F.1: First Stage Event Study Estimates

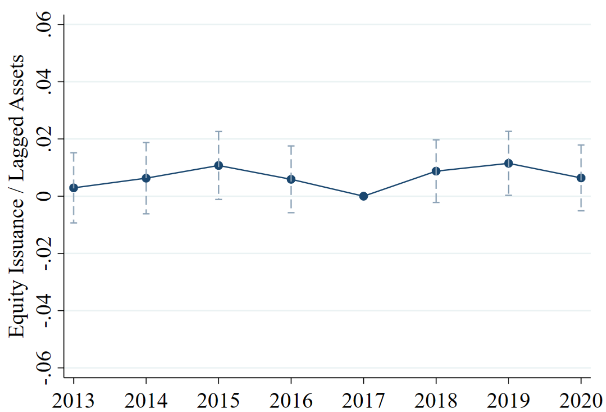
Notes: This figure plots event study estimates of β_e from equation (2) using the high-interest firms in panel data spanning 2013-2020. Panel (a) uses an indicator equal to 1 if interest is disallowed as the outcome variable, while panel (b) uses interest disallowed scaled by lagged assets as the outcome variable. 95% confidence intervals are constructed from standard errors clustered at the firm level.



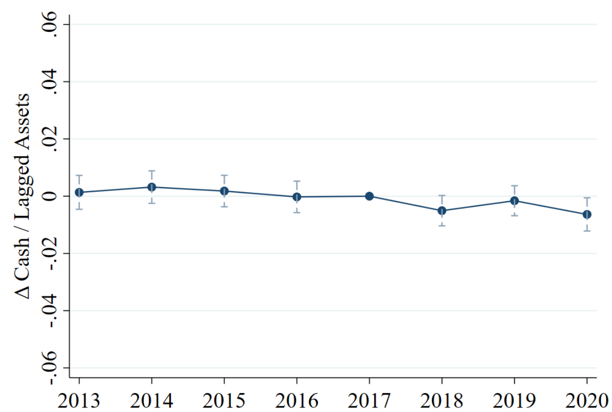
(a) Investment Rate



(b) Debt Issuance



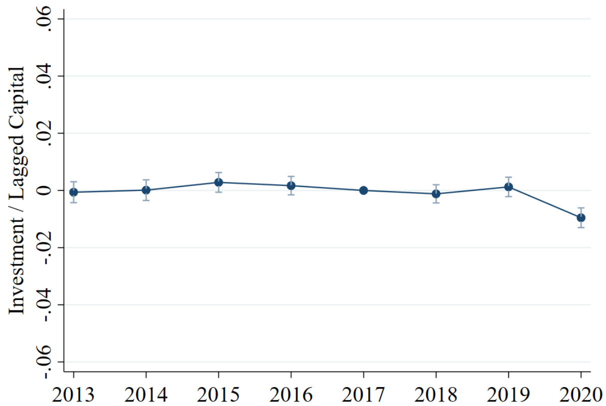
(c) Equity Issuance



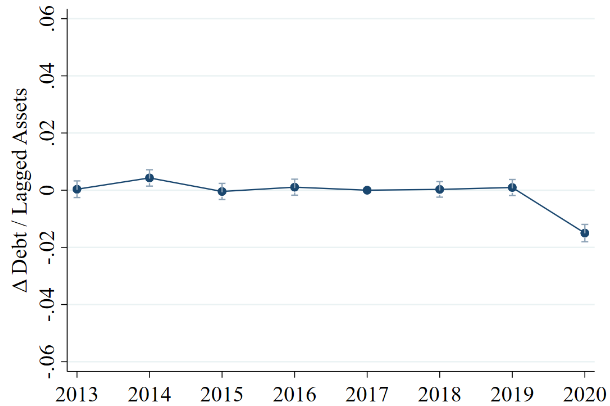
(d) Cash Changes

Figure F.2: Event Study Estimates

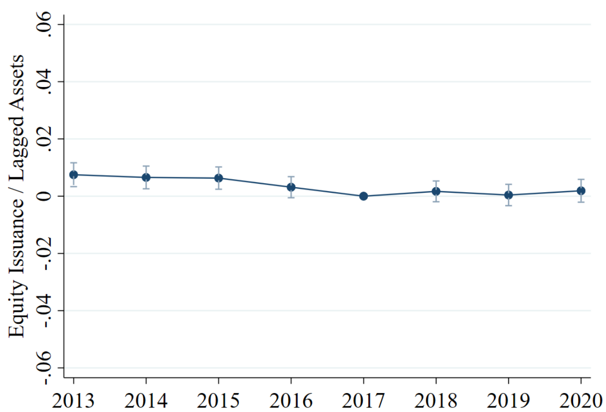
Notes: This figure plots event study estimates of β_e from equation (2) using the high-interest firms in panel data spanning 2013-2020. Panel (a) uses investment scaled by lagged capital as an outcome variable. Panel (b) uses debt issuance scaled by lagged assets as an outcome variable. Panel (c) uses equity issuance scaled by lagged assets as an outcome variable, and panel (d) uses cash changes scaled by lagged assets as an outcome variable. 95% confidence intervals are constructed from standard errors clustered at the firm level.



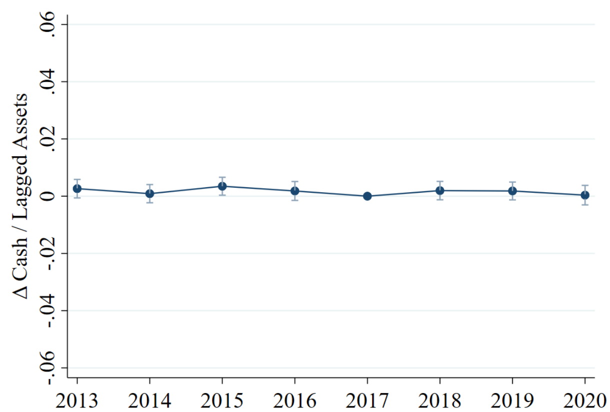
(a) Investment Rate



(b) Debt Issuance



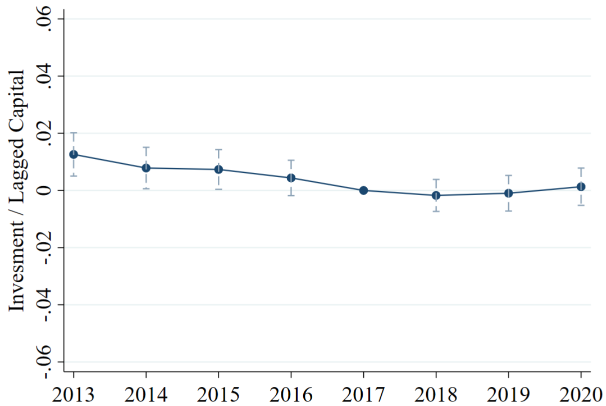
(c) Equity Issuance



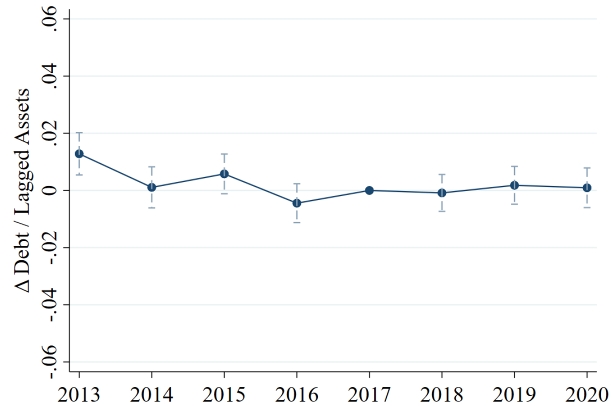
(d) Cash Changes

Figure F.3: Placebo Event Study Estimates

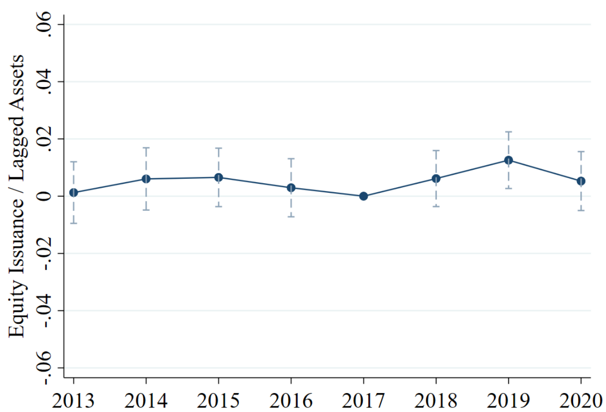
Notes: This figure plots event study estimates of β_e from equation (2) using the low-interest firms in panel data spanning 2013-2020. Panel (a) uses investment scaled by lagged capital as an outcome variable. Panel (b) uses debt issuance scaled by lagged assets as an outcome variable. Panel (c) uses equity issuance scaled by lagged assets as an outcome variable, and panel (d) uses cash changes scaled by lagged assets as an outcome variable. 95% confidence intervals are constructed from standard errors clustered at the firm level.



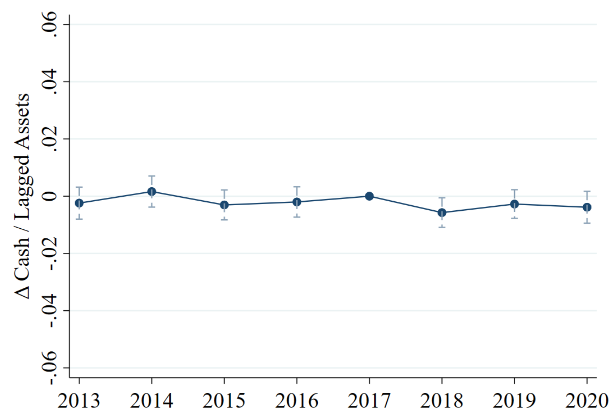
(a) Investment Rate



(b) Debt Issuance



(c) Equity Issuance

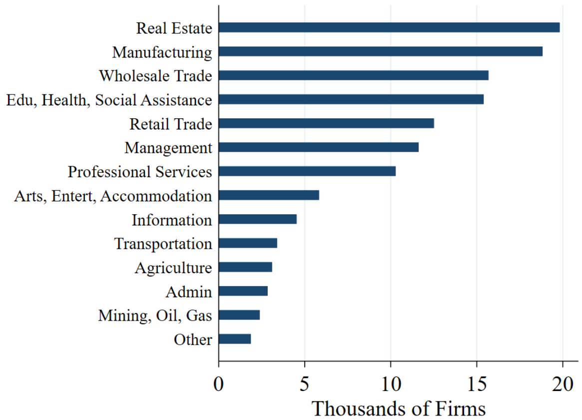


(d) Cash Changes

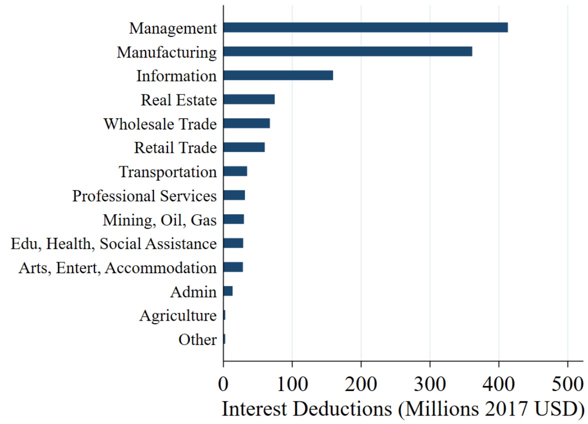
Figure F.4: Triple Difference Investment and Financing Estimates

Notes: This figure plots triple difference estimates of γ_e from equation (5) using panel data spanning 2013-2020. Panel (a) uses investment scaled by lagged capital as an outcome variable. Panel (b) uses debt issuance scaled by lagged assets as an outcome variable. Panel (c) uses equity issuance scaled by lagged assets as an outcome variable, and panel (d) uses cash changes scaled by lagged assets as an outcome variable. 95% confidence intervals are constructed from standard errors clustered at the firm level.

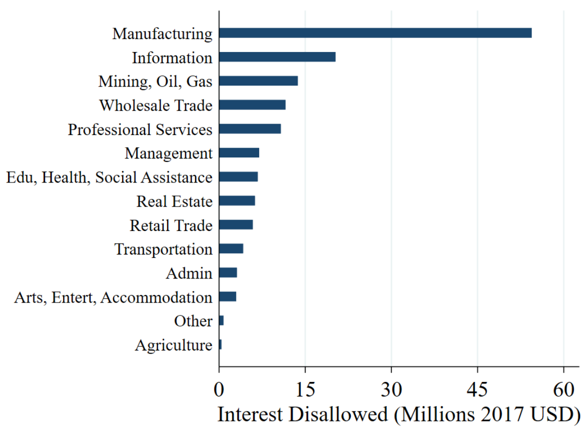
G Additional Appendix Figures



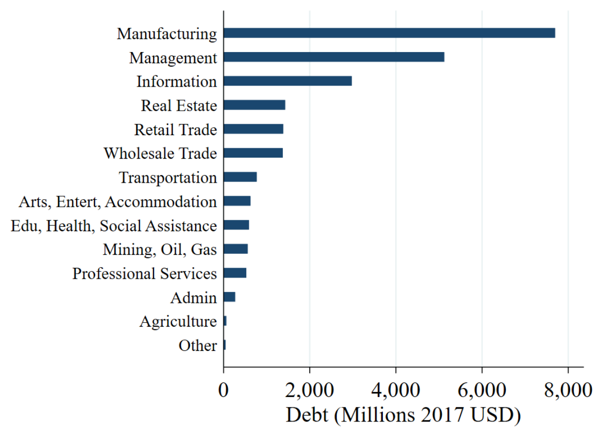
(a) Number of Firms



(b) Interest Deductions



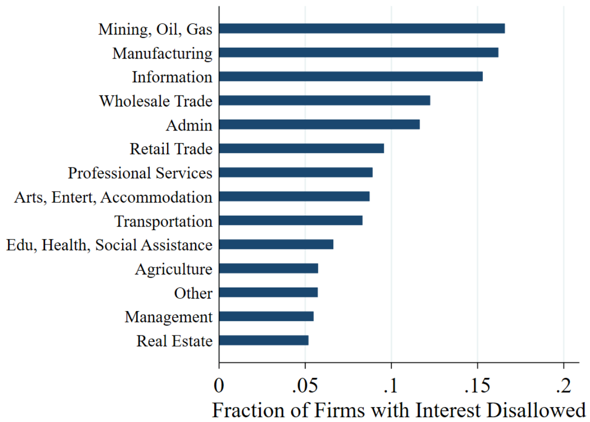
(c) Interest Disallowed



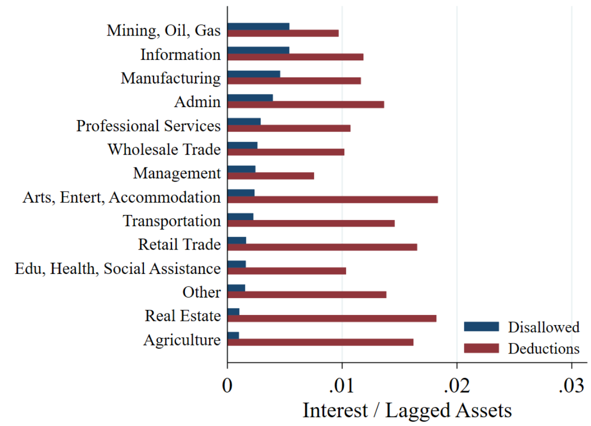
(d) Debt

Figure G.1: Total Interest and Debt by Industry

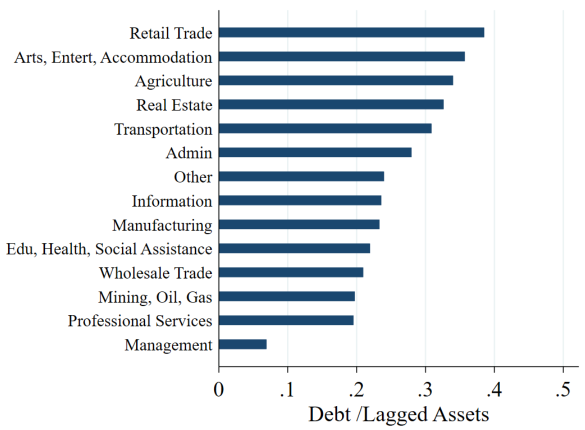
Notes: This figure plots the total number of firms, and the total amount of interest deductions, interest disallowed and debt in our panel data over years 2018 and 2019.



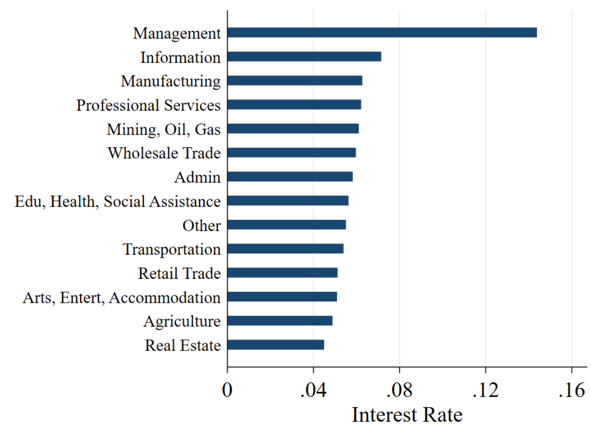
(a) Indicator for Interest Disallowed



(b) Interest



(c) Debt



(d) Interest Rate

Figure G.2: Average Interest and Debt by Industry

Notes: This figure plots the fraction of firms with interest denied, 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 and 2019.

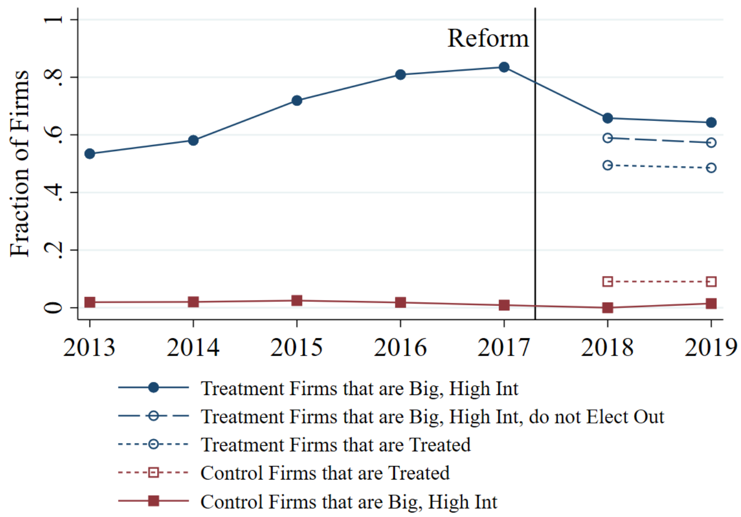
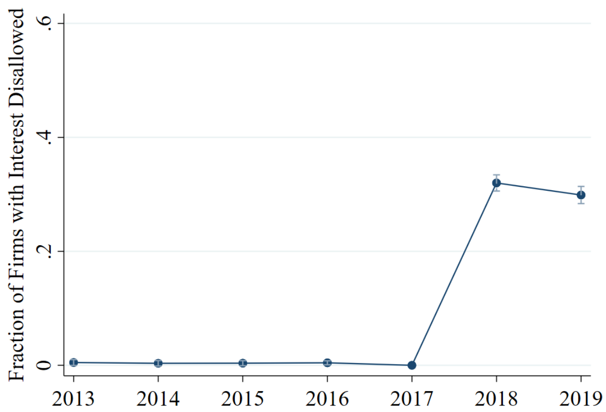
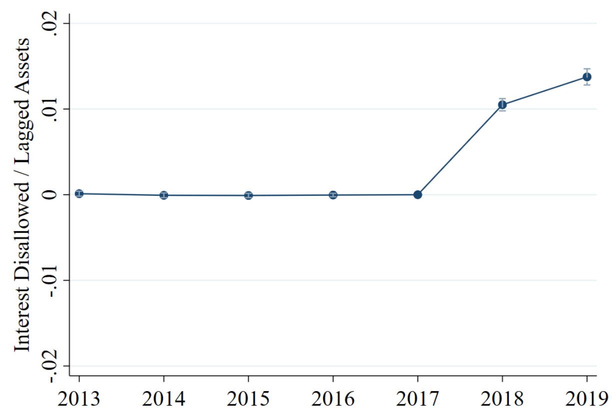


Figure G.3: Persistence of Treatment Status

Notes: This figure describes the persistence of treatment status in our event study design. We plot by plotting the fraction of firms classified as high interest and big based on 2015-2017 that have interest above their limitation, receipts above the relevant size cutoff, and interest disallowed in the post-reform period.



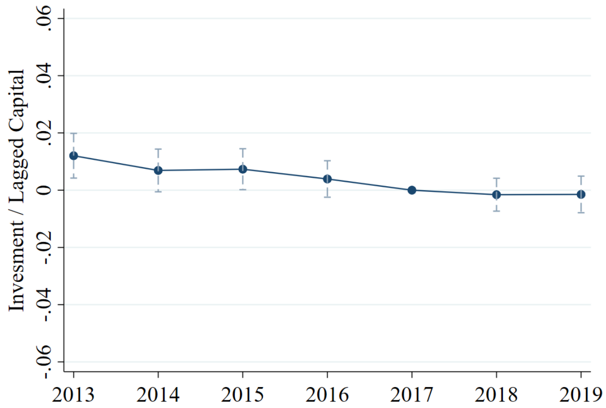
(a) Indicator for Interest Disallowed



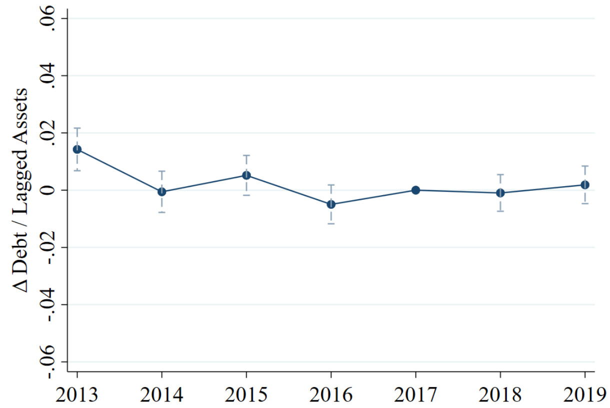
(b) Interest Disallowed

Figure G.4: Triple Difference First Stage Estimates

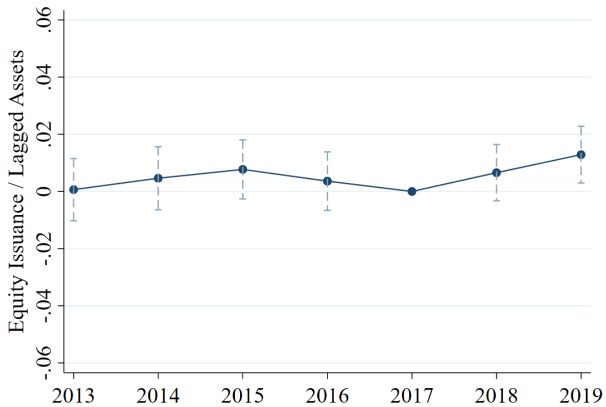
Notes: This figure plots triple difference estimates of γ_e from equation (5). Panel (a) uses an indicator equal to 1 if interest is disallowed as the outcome variable, while panel (b) uses interest disallowed scaled by lagged assets as the outcome variable. 95% confidence intervals are constructed from standard errors clustered at the firm level.



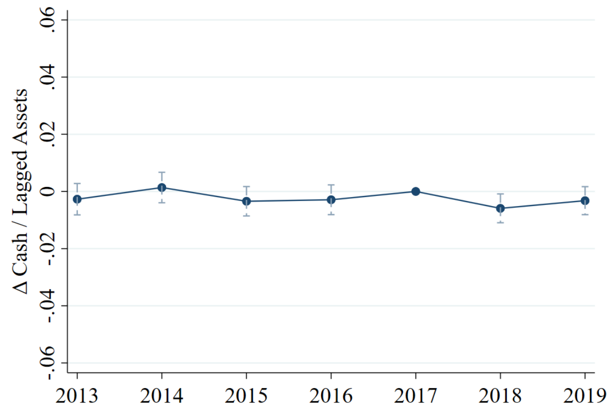
(a) Investment Rate



(b) Debt Issuance



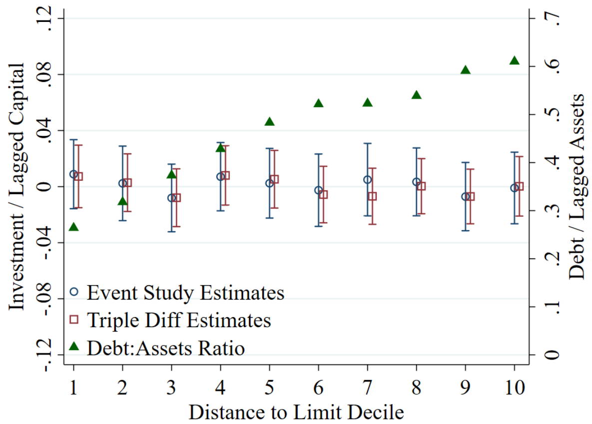
(c) Equity Issuance



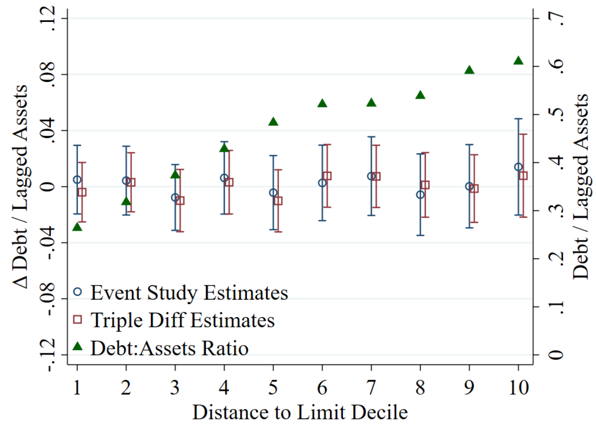
(d) Cash Changes

Figure G.5: Triple Difference Investment and Financing Estimates

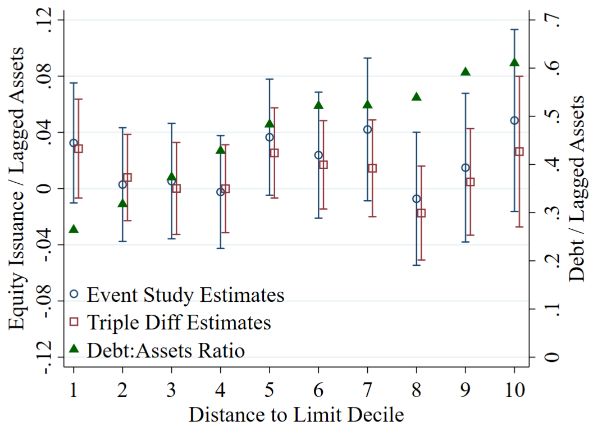
Notes: This figure plots triple difference estimates of γ_e from equation (5) using investment scaled by lagged capital, debt issuance scaled by lagged assets, equity issuance scaled by lagged assets, and cash changes scaled by lagged assets as outcome variables. 95% confidence intervals are constructed from standard errors clustered at the firm level.



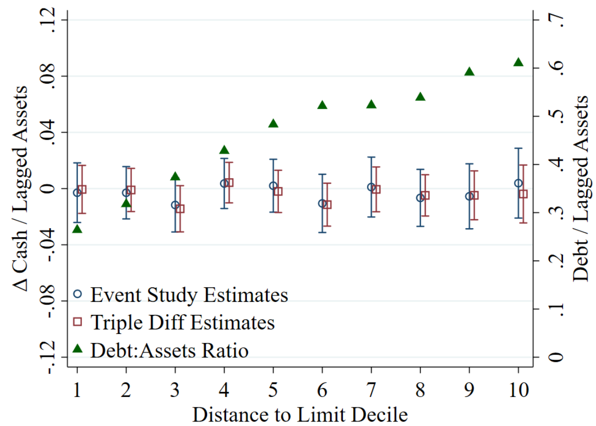
(a) Investment Rate



(b) Debt Issuance



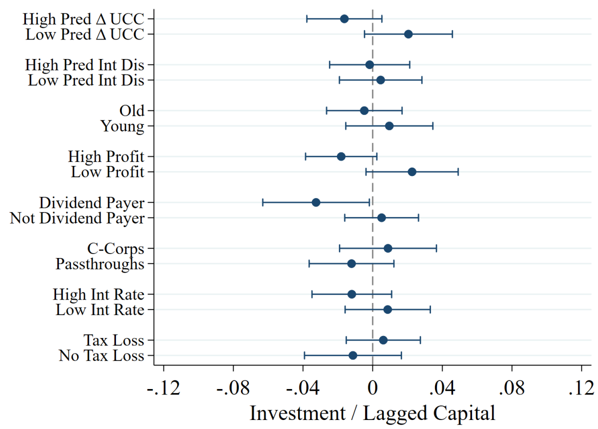
(c) Equity Issuance



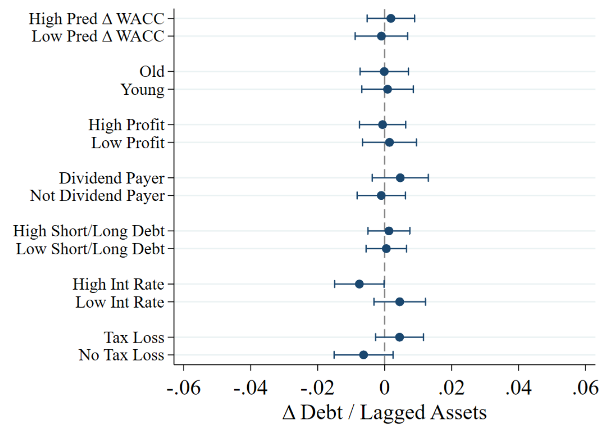
(d) Cash Changes

Figure G.6: Equity and Cash External Validity

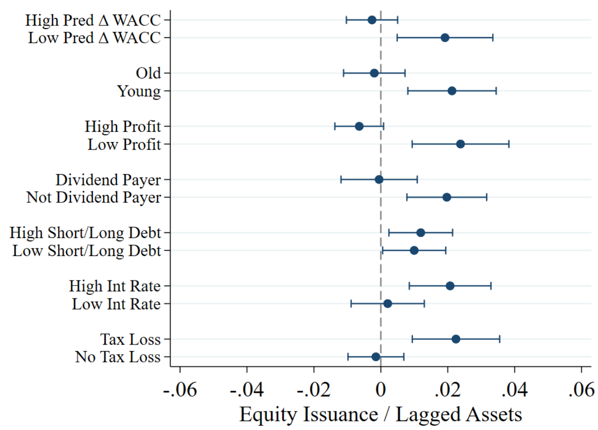
Notes: This figure plots event study estimates of β_{post} from equation (2) and triple difference estimates of γ_{post} from equation (5), replacing the 2018 and 2019 indicators and interactions with a single post-reform dummy and interaction in each equation. We display these estimates by decile of 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 debt to assets ratios in each decile.The panels respectively use investment scaled by lagged capital, debt issuance scaled by lagged assets, equity issuance scaled by lagged assets, and cash changes scaled by lagged assets as outcomes 95% confidence intervals are constructed from standard errors clustered at the firm level.



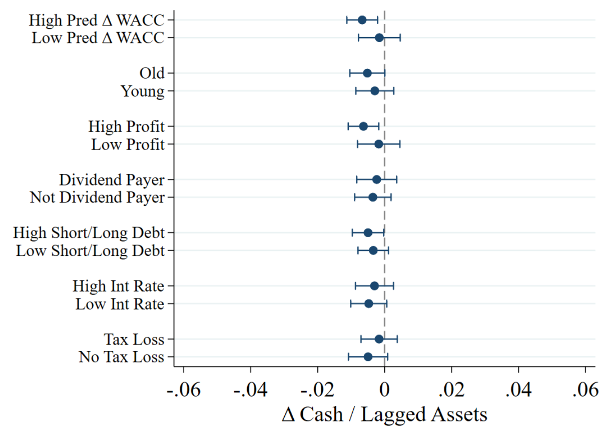
(a) Investment Rate



(b) Debt Issuance



(c) Equity Issuance



(d) Cash Changes

Figure G.7: Triple Difference Heterogeneity

Notes: This figure plots triple difference estimates of γ_{post} from equation (5), replacing the 2018 and 2019 indicators and interactions with a single post-reform dummy and interaction. We display these estimates for subsamples of our estimation sample to explore heterogeneous impacts of the interest limitation. The four panels in order use investment scaled by lagged capital, debt issuance scaled by lagged assets, equity issuance scaled by lagged assets and cash changes 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. Sample splits only subset the high-interest firms and use the entire set of low-interest firms as controls.

H Additional Appendix Tables

Table H.1: 2017 Means For Treatment and Control Groups

	Small		Big	
	Low Int	High Int	Low Int	High Int
Assets (Mil 2017 USD)	58.4	53.8	1,627.3	1,291.7
Capital (Mil 2017 USD)	7.5	17.4	307.0	313.6
Debt / Lagged Assets	0.17	0.47	0.22	0.47
Cash / Lagged Assets	0.27	0.17	0.21	0.12
Investment / Lagged Capital	0.08	0.08	0.11	0.11
Δ Debt / Lagged Assets	0.00	0.01	0.01	0.01
Equity Issuance / Lagged Assets	0.05	0.11	0.03	0.06
Δ Cash / Lagged Assets	0.01	0.00	0.01	0.00
Payouts / Lagged Assets	0.10	0.02	0.10	0.01
Profits / Lagged Assets	0.16	−0.00	0.15	0.01
Interest Rate	0.05	0.07	0.06	0.08
Debt Financing Fraction	0.45	0.72	0.55	0.77
Weighted Average Cost of Capital	0.14	0.14	0.13	0.15
Age	21.7	15.9	28.2	17.5
Obs	26,083	9,458	24,859	6,109

Notes: This table reports mean values for treatment and control groups 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 H.2: 2017 Medians For Treatment and Control Groups

	Small		Big	
	Low Int	High Int	Low Int	High Int
Assets (Mil 2017 USD)	5.6	11.2	70.1	117.0
Capital (Mil 2017 USD)	7.5	17.4	307.0	313.6
Debt / Lagged Assets	0.00	0.45	0.10	0.47
Cash / Lagged Assets	0.16	0.06	0.13	0.06
Investment / Lagged Capital	0.02	0.01	0.06	0.06
Δ Debt / Lagged Assets	0.00	-0.00	0.00	-0.00
Equity Issuance / Lagged Assets	0.00	0.00	0.00	0.00
Δ Cash / Lagged Assets	0.16	-0.00	0.00	-0.00
Payouts / Lagged Assets	0.00	0.00	0.02	0.00
Profits / Lagged Assets	0.06	0.00	0.10	0.02
Interest Rate	0.04	0.04	0.04	0.05
Debt Financing Fraction	0.41	0.86	0.57	0.84
Weighted Average Cost of Capital	0.14	0.13	0.13	0.13
Age	17.5	10.5	22.5	11.0
Obs	26,083	9,458	24,859	6,109

Notes: This table reports median values for treatment and control groups 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 preseve taxpayer anonymity, medians are reported as the means of all observations in the 49th-51st percentiles.

Table H.3: Fraction of Aggregates Across Groups

	(1) Total	(2)	(3)	(4)	(5)	(6)	(7)
		Fraction of Total					
	Tril 2017 USD	Treatment and Control Groups					
		Small		Big			
		Low Int	High Int	Low Int	High Int	Public	Private
Assets	50.38	0.03	0.01	0.80	0.16	0.65	0.35
Int Deductions	0.60	0.01	0.02	0.63	0.35	0.61	0.39
Investment	0.83	0.02	0.02	0.80	0.16	0.44	0.56
Debt	11.47	0.01	0.01	0.71	0.27	0.65	0.35
Equity Issuance	1.22	0.07	0.03	0.63	0.27	0.35	0.65
Cash	7.85	0.03	0.01	0.90	0.06	0.74	0.26
Profits	0.61	-0.02	-0.03	1.01	0.05	0.05	0.95
Obs	66,509	0.39	0.14	0.37	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 shows the fraction of the total in each of the big and small and low- and high-interest firms. Columns 6 and 7 show the fraction of the total in public and private firms.

Table H.4: WACC Financing Term Regression Estimates

	(1)	(2)	(3)	(4)
Outcome	Investment Rate	Investment Rate	log(Investment)	log(Investment)
Independent Variable	$\rho + \delta$	$\log(\rho + \delta)$	$\rho + \delta$	$\log(\rho + \delta)$
	Panel A: OLS Estimates			
β	-0.111	-0.028	-0.594	-0.149
	(0.014)	(0.003)	(0.138)	(0.028)
Obs	83,249	83,249	64,772	64,772
	Panel B: IV Estimates			
β	-0.711	-0.112	4.606	0.706
	(0.470)	(0.073)	(8.014)	(1.222)
First Stage Coefficient	0.005	0.032	0.005	0.032
	(0.001)	(0.004)	(0.001)	(0.004)
Obs	83,249	83,249	64,772	64,772

Notes: This table reports OLS and IV estimates of β from equation (9). The estimation sample includes all high-interest firms in our panel data. Standard errors are clustered at the firm level and reported in parentheses. First stage estimates are from regressions using the independent variable reported in the table as the dependent variable, and the interaction of a post reform indicator and Big_i as the independent variable.