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

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This paper studies the impacts of limiting interest deductions on firms' investment and financing choices using U.S. tax data. The 2017 law known as the Tax Cuts and Jobs Act (TCJA) implemented an interest limitation for big, high-interest firms. Using an event study design comparing big and small high-interest firms, we rule out economically significant impacts of the interest limitation on investment and leverage, and find evidence that the interest limitation led firms to increase their equity issuance. A triple difference design that accommodates size-varying impacts of other TCJA policy changes yields similar results, as does a regression discontinuity design focusing on marginal firms that are just large enough to face the interest limitation. Our results indicate many firms do not use debt as their marginal source of financing and provide evidence consistent with capital structure models with fixed leverage adjustment costs. Furthermore, our results suggest limiting interest deductions is unlikely to have large impacts on investment or to address concerns about rising corporate debt levels.

*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 and Princeton for useful feedback. We also thank Eduard Boehm, Henrik Kleven, Ilyana Kuziemko, Richard Rogerson, Motohiro Yogo, and Owen Zidar for helpful comments and suggestions. 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@princeton.edu`

1 Introduction

Corporate tax codes around the world allow firms to deduct interest expense on their debt, but recent policy discussions have raised the possibility of limiting or eliminating interest deductions (U.S. House, 2016; Furman, 2020). Proponents of interest deductions have argued that they provide an incentive for investment and growth by lowering the cost of borrowing, while opponents suggest interest deductions narrow the tax base and encourage high levels of borrowing that increase macroeconomic risk.[1] Economic theory offers little guidance about the magnitude of these possible effects. Limiting interest deductions could lead to declines in investment if new investment is financed with debt, but firms can use debt, equity, or cash to finance new investment. Furthermore, reducing the tax benefit of debt may encourage firms to reduce leverage, but not if there are significant adjustment costs to doing so. Evaluating these arguments requires empirically measuring the impacts of limiting 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 face the interest limitation to small, high-interest firms that do

[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).

not. To classify firms as *ex ante* big and high interest, we average firms' interest relative to their limitation and receipts from 2015-2017. We label firms as big if their average receipts exceed $25 million, and as high interest if their 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, leverage or cash holdings. These results suggest the majority of new investment is not financed with debt and that firms may face significant leverage adjustment costs. 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 second research design: a triple difference. The triple difference design 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 but not the interest limitation. Therefore, the triple difference estimates are unlikely to be biased by other TCJA tax policy changes. Our triple difference estimates are strikingly similar to our event study estimates, corroborating our findings that the interest limitation has no statistically or economically significant impact on investment, leverage, or cash holdings while causing increases in equity issuance.

Both the event study and triple difference designs allow us to measure average investment and financing responses to the interest limitation. However, they rely on assumptions that firms of different sizes exhibit similar investment and financing behavior. To estimate the causal effect of the interest limitation while relying on a weaker set of assumptions, we implement a third research design: 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.[3] Our RD estimates, although less precise than the event study and triple difference estimates because they rely on a smaller number of firms close to the $25 million lagged receipts cutoffs, are consistent with our other results. We cannot

[3]This assumption is likely to hold because the tax reform including the specifics of the interest limitation was not passed by Congress until November 2017.

rule out zero impacts of the interest limitation on investment and financing choices, and the confidence intervals on our RD estimates include our event study and triple difference estimates.

Heterogeneity analysis using our more precise event study and triple difference designs suggests that firms' equity issuance behavior helps explain the lack of large investment responses to the interest limitation. Previous research suggests we should expect firms facing larger cost of capital changes and financially constrained firms to exhibit larger responses to the interest limitation (Zwick and Mahon, 2017; Liu and Mao, 2019; Saez, Schoefer and Seim, 2019). We find suggestive evidence that firms facing the largest predicted cost of capital changes from the interest limitation decrease their investment by more than other firms, but no evidence of differential investment declines among younger firms, lower profit firms, and firms not paying dividends, all common proxies for financial constraints. One key difference between these two groups is their equity issuance responses to the interest limitation. We find no evidence of equity issuance increases among higher cost of capital change firms, but strong evidence that equity issuance increases are concentrated among firms that appear financially constrained. In our setting, firms that appear financially constrained according to common proxies are more willing or able to substitute towards equity financing, mitigating potential investment impacts of the interest limitation and reinforcing that common proxies for financial constraints do not always identify firms that cannot access external financing (Farre-Mensa and Ljungqvist, 2016).

Our empirical results are consistent with leading theories of corporate capital structure including pecking order and dynamic trade-off theory. Pecking order theory suggests firms prefer to use internal cash financing before using external debt or equity financing (Frank and Goyal, 2008). The lack of investment declines that we estimate suggest firms use cash or equity to finance new investment, but equity issuance is infrequent. 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. Therefore, significant amounts of new investment must be financed with cash, consistent with pecking order theory, and both empirical and survey evidence that U.S. firms rely on cash financing for new investments (Yagan, 2015; Sharpe and Suarez, 2021).

Trade-off theories are built on the premise that firms choose leverage by weighing the tax benefits of debt against bankruptcy costs (Frank and Goyal, 2008; Ai, Frank and Sanati, 2021). Static trade-off theory suggests when the tax benefits of debt decline, leverage should decline. Our null leverage estimates contradict this simple prediction, but are consistent with more nuanced dynamic trade-off models incorporating costly leverage adjustment that can lead to inaction (Fischer, Heinkel and Zechner, 1989; Leary and Roberts, 2005; Danis, Rettl and Whited, 2014; Jeenas, 2019).

Our paper contributes to a broad literature that studies the impacts of corporate taxes on firm investment.[4] Existing work suggests that investment responds to changes in the user cost of capital, often estimating investment rate user cost 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 investment rate user cost elasticity of -0.03 with a 95% confidence interval spanning $[-0.82, 0.77]$. Our estimates are substantially smaller 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. If firms used debt as their marginal source of financing, we would expect to find estimates similar to those in previous work. The fact that our estimates are substantially smaller highlights that many firms are not using debt as their marginal financing source.

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 (MacKie-Mason, 1990; Rajan and Zingales, 1995; Graham, 1996; Booth, Aivazian, Demirguc-Kunt and Maksimovic, 2002; Heider and Ljungqvist, 2015; Faccio and Xu, 2015).[5]

[4]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 (2022); Chen, Jiang, Liu, Suarez-Serrato and Xu (2023); Duan and Moon (2023).

[5]A related literature studies the relationship between debt and taxes in multinational firms that may attempt to shift profits from high to low tax jurisdictions by having subsidiaries in high tax jurisdictions borrow from subsidiaries in low tax jurisdictions (Desai, Foley and Hines, 2004). Many countries have

One notable exception is Ivanov, Pettit and Whited (2022), who find a 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.

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 split sample heterogeneity analysis. Section 7 discusses the implications of our results for theories of investment and financing and reconciles our estimates with two existing papers that study the impact of the interest limitation using Compustat data (Carrizosa, Gaertner and Lynch, 2022; Sanati, 2023). 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

implemented regulations that attempt to limit multinationals' interest deductions stemming from this form of lending (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.

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 increases the marginal cost of borrowing by effectively raising firms' interest rates, and reduces the amount of cash firms have on hand by expanding the tax base and forcing firms to pay more in taxes. With interest rate r and tax rate τ, the marginal cost of borrowing with interest deductions is $1 + (1 - \tau)r$. Firms in Figure 1, quadrant B lose out on interest deductions, and therefore face a marginal cost of $1 + r$. While all firms in quadrant B face a higher marginal cost of borrowing, firms higher on the y-axis face larger cash shocks because they lose more interest deductions.

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.[6] We cannot reliably identify tax shelters in our data, but their presence leads to some small firms facing the interest limitation. In addition, businesses with agriculture and real estate components can opt out 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 3.5% of agriculture firms do. We show in section 4.5 that our results are robust to dropping real estate firms.

The interest limitation is generally applied at the entity level to C-corporations, S-

[6]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.

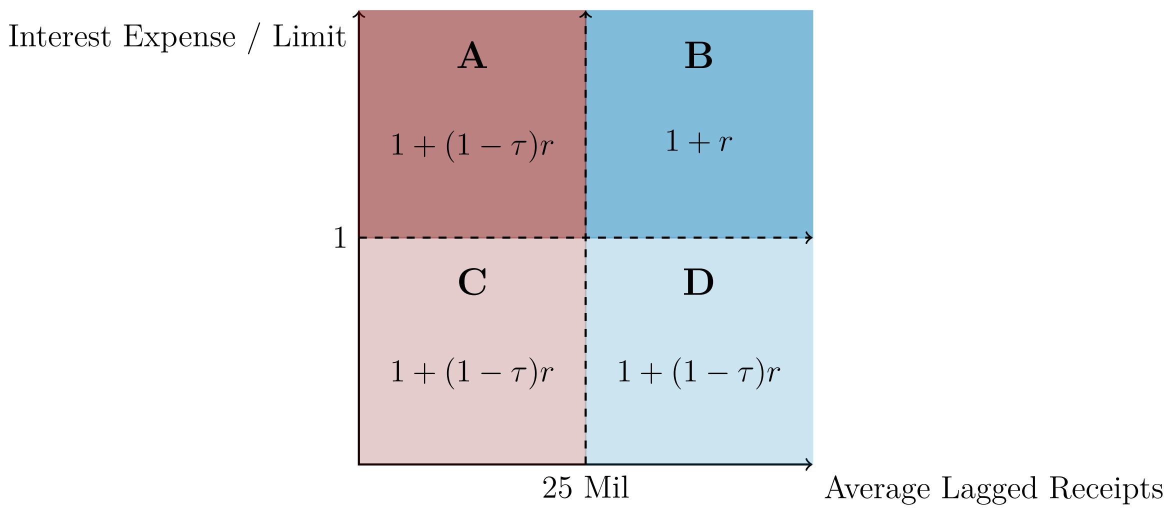


Figure 1: Marginal Cost of Borrowing

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 marginal borrowing.

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, 2022). 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 section 5.

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

The SOI samples provide substantial advantages over Compustat data that has been used to evaluate the U.S. interest limitation in other research (Carrizosa, Gaertner and Lynch, 2022; Sanati, 2023). Our data covers publicly- and privately-held firms across the size distribution, resulting in a larger and more representative sample. While Compustat data only includes publicly-held firms, privately-held firms do more than half of all investment

[7] These data are used in other academic papers such as Yagan (2015); Zwick and Mahon (2017); Kennedy, Dobridge, Landefeld and Mortenson (2022); Bellon, Dobridge, Gilje and Whitten (2023). C-corporations filing Form 1120 are stratified by total assets and net income. S-corporations filing Form 1120S are stratified by total assets and ordinary business income. Partnerships filing Form 1065 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.

in our data. Furthermore, the larger sample allows us to explore heterogeneous responses to the policy with more granularity and to pursue a regression discontinuity design with a substantial number of firms close to the $25 million lagged receipts cutoff.

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.[8] 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 405,013 firm-years, 69,035 unique firms, and 38,833 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, profits, debt, equity, and cash. We discuss the general definition of each variable in this section, and provide specific tax form line item numbers that contribute to each variable in Appendix A. *Assets* represent the book value of all firm assets. *Capital* is the book value of all tangible capital assets net of accumulated book depreciation. *Investment* equals the purchase price of all newly installed capital assets listed on Form 4562, a supplemental tax form filed to claim depreciation deductions. *Profits* are revenues less cost of goods sold and all components of total deductions except interest, depreciation, domestic production activities, and officer compensation deductions.

To measure firm financing responses to the interest limitation, we construct measures of their debt, equity issuance 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

[8] We add 2020 data to all of our analysis in Appendix C.

liabilities. *Trade credit* equals accounts payable. *Equity issuance* is non-negative annual changes in total paid-in capital, which equals the sum of common stock, preferred stock and additional paid in capital. *Cash* in the sum of cash and all other liquid securities.

The interest limitation only applies to firms with average lagged receipts over the previous three years exceeding $25 million, and the policy disallows interest deductions exceeding 30% of firm adjusted taxable income plus interest income and floor plan financing interest. *Receipts* are gross receipts plus dividends, interest, rents, royalties, capital gains and tax exempt interest. *Adjusted taxable income* is income minus deductions, adding back interest expense, depreciation, depletion and amortization, and subtracting interest income, or EBITDA.

One reason we use the corporate and partnership SOI samples as our primary data source is that 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 use firm level interest rates and debt financing fractions to measure firms' user cost of capital. While we do not directly observe the interest rates firms pay on specific debt securities, we calculate firms' *interest rate* as the ratio of total interest expense to total interest bearing liabilities.[9] In addition, we calculate firms' *debt financing fraction* as the ratio of all liabilities to assets.

The typical Hall and Jorgenson (1967) expression for the user cost of capital is

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

with financing costs ρ, depreciation rate δ, tax rate τ, and net present value of depreciation deductions z. We generalize the financing cost term to be a weighted average of debt and equity financing costs, so that $\rho = w_d(1 - \tau \mathbb{1}(Allow))r + w_e E)$ with fraction of financing from debt w_d, interest rate r, fraction of financing from equity $w_e = 1 - w_d$, equity flotation

[9] 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.

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 fraction w_d and interest rate r. 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.[10] We 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 user cost measure to estimate elasticities with respect to the user cost of capital. In Appendix B, we list the sources of each user cost parameter, and assess the sensitivity of our elasticities to different constructions of the user cost. We find that reasonable deviations in parameter values yield only small changes in our estimates.

Our implementation of the simple user cost expression above has two limitations. First, we measure firm financing breakdowns using all liabilities and assets on firm balance sheets because we cannot directly observe marginal financing sources. This imposes a strong assumption that the fraction of firms' marginal financing that comes from debt is the same as the fraction of all inframarginal financing that came from debt. Second, our generalization of the financing term does not allow for cash financing. As a result the interest limitation will have a larger impact on user costs for firms carrying significant amounts of inframarginal debt on their balance sheets, regardless of their marginal financing source.

3.3 Summary Statistics

We summarize important variables in Table 1 in 2017 U.S. dollars. Means exceed medians for most variables. The mean firm has $804.2 million in assets, $84.2 million in tangible capital, invests $26.9 million and has $206.7 million in debt. The median firm has $28.1 million in assets, $1.6 million in tangible capital, invests $195,000 and has $2.4 million in

[10] A more detailed user cost measure would use marginal tax rates for C-corporations, S-corporations and partnerships. Unfortunately, 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.

Table 1: Summary Statistics

	Mean	Std Dev	P10	P50	P90	Obs	Firms
Scaling							
Assets (Mil 2017 USD)	804.2	26,207.4	0.9	28.1	436.5	405,013	69,035
Financial Capital (Mil 2017 USD)	417.7	11,624.8	0.5	15.9	249.2	405,013	69,035
Tangible Capital (Mil 2017 USD)	84.2	1,167.0	0.0	1.6	57.7	405,013	69,035
Tax							
Interest Deductions / Lagged Assets	0.013	0.017	0.000	0.005	0.039	405,013	69,035
Interest Disallowed / Lagged Assets	0.003	0.013	0.000	0.000	0.000	128,161	69,035
Net Income / Lagged Assets	0.071	0.179	−0.076	0.020	0.294	405,013	69,035
Investment and Financing							
Investment / Lagged Capital	0.320	0.510	0.000	0.122	0.892	359,685	62,169
Debt / Lagged Assets	0.257	0.333	0.000	0.084	0.788	405,013	69,035
Debt / Lagged Financial Capital	0.340	0.427	0.000	0.144	0.958	404,876	69,026
Debt + SH Loans / Lagged Assets	0.303	0.367	0.000	0.138	0.877	405,013	69,035
Short Term Debt / Lagged Assets	0.074	0.169	0.000	0.000	0.272	405,013	69,035
Long Term Debt / Lagged Assets	0.179	0.291	0.000	0.011	0.644	405,013	69,035
Trade Credit / Lagged Assets	0.099	0.156	0.000	0.029	0.319	405,013	69,035
Equity Issuance / Lagged Assets	0.046	0.184	0.000	0.000	0.076	405,013	69,035
Cash / Lagged Assets	0.218	0.241	0.009	0.124	0.611	405,013	69,035
Additional Variables							
Payouts / Lagged Assets	0.045	0.156	0.000	0.000	0.094	405,013	69,035
Profits / Lagged Assets	0.125	0.239	−0.078	0.059	0.444	405,013	69,035
Payroll / Lagged Assets	0.333	0.479	0.000	0.153	0.962	405,013	69,035
Exec Comp / Lagged Assets	0.038	0.082	0.000	0.004	0.111	405,013	69,035
Interest Rate	0.060	0.076	0.004	0.040	0.109	405,013	69,035
Debt Financing Frac	0.556	0.338	0.042	0.596	1.000	405,013	69,035
User Cost of Capital	0.138	0.041	0.104	0.133	0.158	405,013	69,035

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.

debt.[11] 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. Following existing work, we scale investment by lagged capital (Desai and Goolsbee, 2004; Edgerton, 2010; Yagan, 2015; Ohrn, 2018), while our primary scaling variable for firm financing outcomes is lagged

[11] 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.

assets.[12]

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.

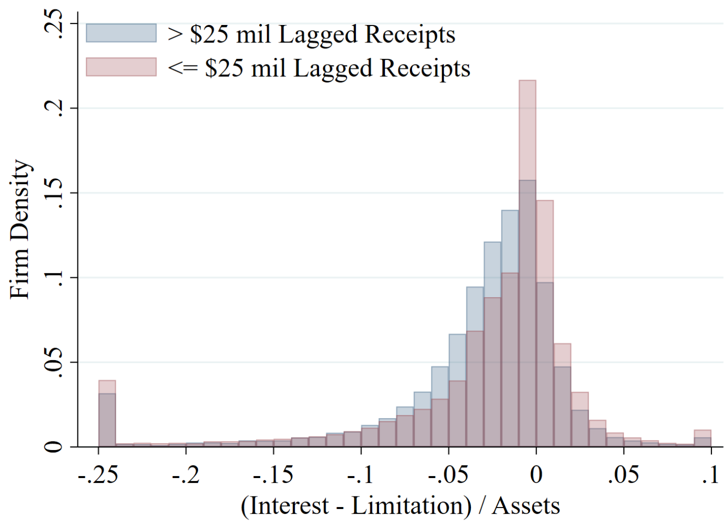


Figure 2: Distribution of Firm Interest Relative to Limitation

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.

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 $20 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%

[12]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 also present robustness tests where we scale debt measures by financial capital, defined as assets minus all non-debt liabilities.

of its payroll. Assuming a 21% tax rate, this interest disallowed implies a mean change in taxes of $4 million and a median change in taxes of $630,000.

Table 2: 2017 Means For Treatment and Control Groups

	Small		Big	
	Low Int	High Int	Low Int	High Int
Assets (Mil 2017 USD)	58.5	53.8	1,626.9	1,291.5
Capital (Mil 2017 USD)	4.5	13.4	162.5	173.1
Investment / Lagged Capital	0.35	0.22	0.33	0.28
Debt / Lagged Assets	0.17	0.47	0.22	0.47
Equity Issuance / Lagged Assets	0.05	0.11	0.03	0.06
Cash / Lagged Assets	0.27	0.17	0.21	0.12
Payouts / Lagged Assets	0.05	0.01	0.06	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
User Cost of Capital	0.14	0.14	0.13	0.15
Age	21.7	15.9	28.2	17.5
Obs	26,107	9,463	24,864	6,110

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 2 presents means of important variables for firms in the four quadrants of Figure 1 using a 2017 cross section of the data. We split firms into big and small and high- and low-interest groups based on their average receipts and interest relative to limitation over 2015-2017. High-interest firms are smaller, younger, have more debt, have less cash, fewer profits and payouts, and face higher interest rates. In 2017, the average big, high-interest firm has an interest rate of 8% and a debt financing fraction of 77%. With a marginal tax rate of 21% and a net present value of depreciation deductions of 0.94, the average firm's user cost would be 0.15, and the interest limitation would increase the user cost by 9%. With an investment rate user cost elasticity of negative two (Chen, Jiang, Liu, Suarez-Serrato and Xu, 2023), the interest limitation would cause an 18% decline in the average firm's investment rate. Appendix Table G.1 displays similar patterns for the group medians.[13]

[13] The median big, high-interest firm has an interest rate of 0.05 and a debt financing fraction of 0.84. If the

We present additional descriptive statistics exploring the distribution of debt, interest, and interest rates by industry in the post-reform period in the appendix. Appendix Figure F.1 shows the majority of debt is held by manufacturing, management and information firms. Firms in these three industries have the most interest deductions, and manufacturing and information firms have the most interest disallowed. Appendix Figure F.2 shows that manufacturing, management and information firms also face the highest interest rates (between 6% and 14%), while over 15% of firms have interest disallowed in mining, oil and gas, manufacturing and information. Firms in these industries also have the most interest disallowed scaling by lagged assets but have lower leverage on average than retail trade, agriculture and real estate firms. We intentionally exclude utility and finance firms, including private equity funds, from our sample to focus on firms that demand rather than supply external financing and that do not face rate of return regulations. We plan to study private equity funds in future work.

Using a 2017 cross section of our data, we show how assets, investment, debt, and other important variables are distributed among big and small, and high- and low-interest firms in Appendix Table G.2. Big, high-interest firms do 16% of 2017 investment in our data and hold 27% of 2017 debt, suggesting large changes in investment or leverage for these firms could have macroeconomic implications. Appendix Table G.2 also emphasizes the importance of including private firms not present in Compustat in the sample. 56% of 2017 investment in our data is done by private firms.

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

median big, high-interest firm faces a tax rate of 21% and has a net present value of depreciation deductions of 0.94, then the median firm's user cost would be 0.13, and the interest limitation would increase the median firm's user cost by 7%.

receipts are below the \$25 million threshold. Our event study specification is

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

where Y_{it} is an outcome for firm i in year t, Big_i is a dummy variable equal to 1 if firm i has average receipts from 2015-2017 above \$25 million, ξ_i is a firm fixed effect, and $\delta_{jp(i),t}$ is an industry by pre-period profitability quartile by year fixed effect.[14] 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 firms in the same industry and with similar levels of profitability that are more likely to face the same marginal tax rates, and have similar cost structures and investment durations.

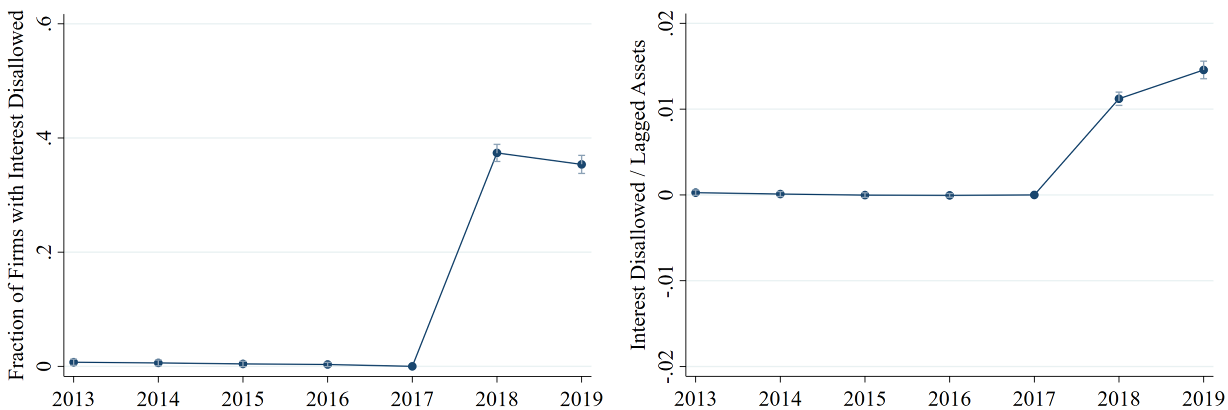
We restrict the estimation sample to only include firms with interest above their limitation averaging over 2015-2017 and denote 2017 as the omitted year. The coefficients of interest β_e capture the average relative difference in the outcome variable between big and small high-interest firms in the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

[14]We calculate profitability quartiles based on all firms in the estimation sample average profits scaled by lagged assets over 2015-2017.

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. Holding all else equal and applying a 21% corporate tax rate suggests 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. Panel (c) plots 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.

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 appear to have average lagged receipts above the size cutoff and interest above their limitation do not have interest deductions disallowed, while some firms that appear to have average lagged receipts below the size cutoff do have interest disallowed. We display the relative importance of each factor in Appendix Figure F.3.[15] While *ex ante* 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.[16]

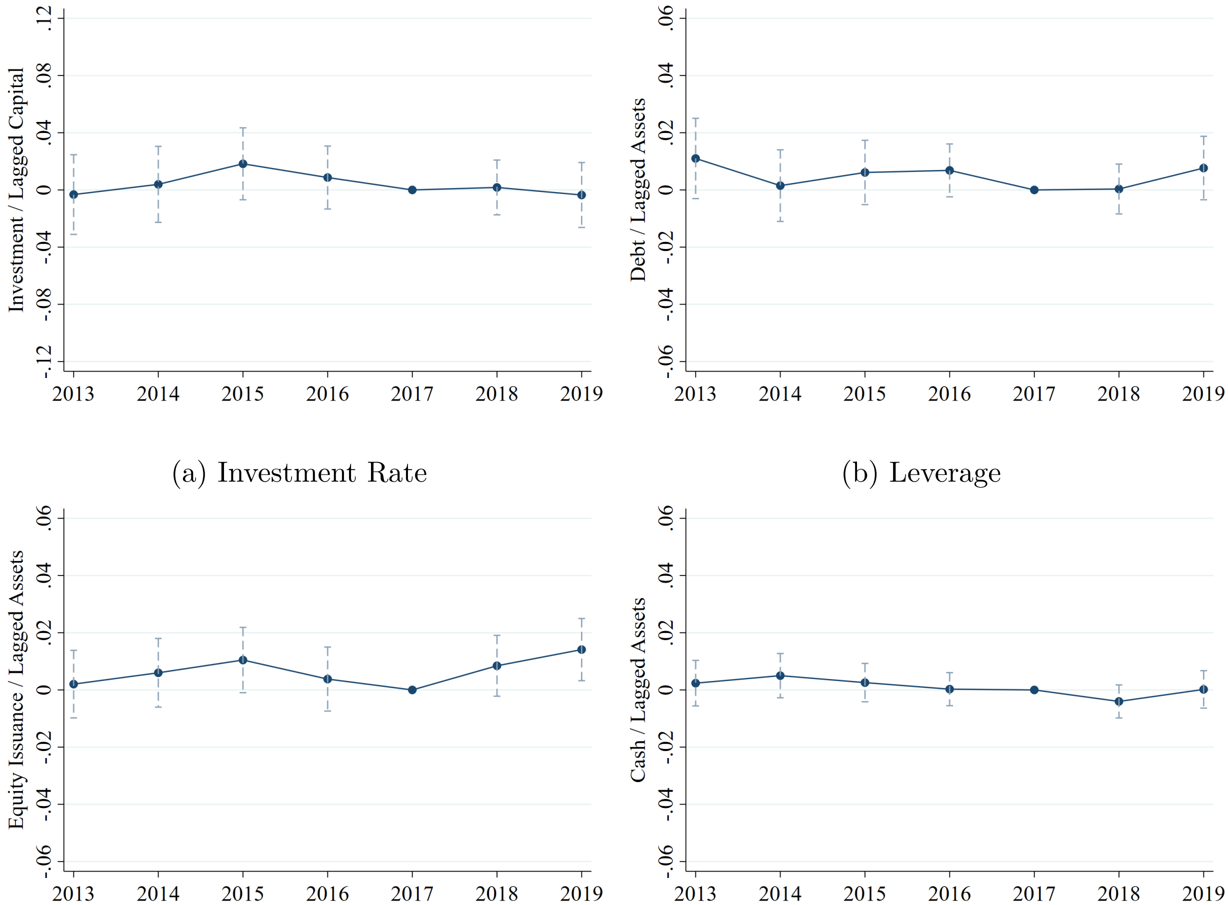
4.2 Event Study Investment and Financing Estimates

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, the leverage ratio, equity issuance and cash as outcome variables.

For all four outcomes, pre-reform coefficients cannot reject zero in any year, 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, leverage, and cash, we find no evidence

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

[16] We intentionally choose a broad sampling frame including firms that can elect 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 D.



(a) Investment Rate

(b) Leverage

(c) Equity Issuance

(d) Cash

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 scaled by lagged assets, equity issuance scaled by lagged assets, and cash scaled by lagged assets as outcome variables. 95% confidence intervals are constructed from standard errors clustered at the firm level.

of statistically significant responses to treatment in the post-reform period. In panel (c), we find insignificant increases in equity issuance in 2018, and statistically significant increases in equity issuance in 2019. We plot raw means of the treatment and control group by year for all four outcomes in Appendix Figure F.4. The raw means largely track each other in pre- and post-reform years, but the pre-trends are not as similar as those in Figure 4, suggesting the fixed effects in equation (2) improve the comparability of our treatment and control groups by focusing on within industry and profitability group variation.

To understand the magnitude of these firm responses, we reestimate equation (2) replacing the indicators for 2018 and 2019 with a single indicator for an observation being in 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 3. The estimates in column 1 suggest that the interest limitation has an insignificant and economically small effect on the investment rate. Our ITT estimates suggest investment declines by \$0.001 per dollar of lagged tangible capital assets with a standard error of \$0.009, relative to a pre-reform mean value of \$0.31 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 user cost elasticities in Table 3, the estimated percent change in the outcome variable for every 1% change in the user cost of capital. 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 user cost as the difference in the percent change in user cost for treatment and control firms. Specifically, we define

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

We calculate ITT and TOT user cost elasticities following equation (4) by using two different measures of the user cost of capital. To calculate an ITT elasticity, we use a measure of the user cost that mechanically assigns interest disallowed to every treatment firm, so that post-reform user cost 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 user cost that depends on whether firms have interest disallowed, so that the post-reform user cost financing term is $\rho = (w_d r(1 - \tau \mathbb{1}(Allow)) + w_e E)$ and only eliminates interest deductions from the user cost financing term for firms with interest disallowed. These user cost elasticity estimates are comparable to existing user cost elasticity estimates under the strong assumptions that firms use debt and equity for their marginal financing, and that the fraction of marginal financing from debt is the same as the fraction of all inframarginal financing from debt.

If the interest limitation mechanically applied to all treatment firms, it would have increased user costs by 13%. In practice the interest limitation does not apply to all treatment firms and only raises user costs by 7%. Scaling our ITT estimates by pre-reform outcome means and mechanical user cost changes yields an ITT investment rate user cost elasticity of -0.02 with a 95% confidence interval spanning $[-0.45, 0.42]$. Scaling by actual user cost changes yields a TOT investment rate user cost elasticity of -0.03 with a 95% confidence interval spanning $[-0.82, 0.77]$. We explore the sensitivity of our elasticity estimates to different constructions of the user cost of capital in Appendix B and find other reasonable parameter values yield only small changes in elasticity estimates.

Previous estimates of the investment rate user cost 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). These differences suggest that firms are not using debt as a source of marginal financing. We compare our investment elasticity estimates to prior work in more detail in section 7.

Column 2 of Table 3 suggests the interest limitation also has an insignificant and economically small impact on firm leverage ratios in 2018 and 2019. The point estimate in column 2 suggests debt increases by \$0.004 for each dollar of lagged assets, with a standard error of \$0.004. While our leverage point estimate is positive, the 95% confidence intervals

Table 3: Event Study Effect on Investment and Financing

Dependent Variable	(1) Investment Rate	(2) Leverage	(3) Equity Issuance	(4) Cash
β_{post}	-0.001	0.004	0.011	-0.002
	(0.009)	(0.004)	(0.005)	(0.003)
β_{post}^{TOT}	-0.002	0.010	0.030	-0.006
	(0.025)	(0.012)	(0.013)	(0.007)
Obs	$82,177$	$89,591$	$89,591$	$89,591$
Clusters	$14,838$	$16,107$	$16,107$	$16,107$
R^2	0.442	0.795	0.518	0.751
Pre-Reform Mean	0.315	0.470	0.064	0.136
ITT UCC Pct Change	0.13	0.13	0.13	0.13
ε^{ITT}	-0.02	0.06	1.36	-0.13
	(0.22)	(0.07)	(0.58)	(0.16)
TOT UCC Pct Change	0.07	0.07	0.07	0.07
ε^{TOT}	-0.03	0.12	2.58	-0.24
	(0.41)	(0.14)	(1.10)	(0.30)

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 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.

on our estimates rule out economically significant leverage changes and cannot reject zero. Our estimates are inconsistent with static trade-off theory, wihch implies firms should reduce leverage whenever the tax benefit of debt declines. However, the lack of leverage changes are consistent with dynamic trade-off theories featuring costly leverage adjustment (Fischer, Heinkel and Zechner, 1989; Leary and Roberts, 2005; Danis, Rettl and Whited, 2014), and empirical evidence that firms do not reduce their leverage in response to tax cuts (Heider and Ljungqvist, 2015).

Our leverage estimates imply an ITT leverage ratio user cost elasticity of 0.06 with a

95% confidence interval spanning [-0.08,0.21], and a TOT leverage ratio user cost elasticity of 0.12 with a 95% confidence interval spanning [-0.16,0.39]. Sanati (2023) evaluates the interest limitation using an RD design on publicly-held firms and Compustat data and reports a much larger leverage user cost elasticity of -15.59 with a 95% confidence interval spanning [-26.62, -4.56]. In section 7.3, we attempt to reconcile our estimates by implementing an RD based on only publicly-held firms in our data and continue to fail to reject zero leverage changes.

Column 3 of Table 3 suggests the interest limitation causes a statistically significant increase in equity issuance. The ITT point estimate in column 3 suggests equity issuance increases by $0.011 for each dollar of lagged assets, with a standard error of $0.005, implying an ITT equity issuance user cost elasticity of 1.36 with a 95% confidence interval spanning [0.22,2.5], and a TOT user cost elasticity of 2.58 with a 95% confidence interval spanning [0.42,4.74]. These estimates suggest that the interest limitation causes an increase in equity issuance as equity becomes more appealing relative to debt. To the best of our knowledge, this is the first estimate of an equity issuance user cost elasticity.

The ITT estimates in Column 4 of Table 3 suggest cash decreases by $0.002 per dollar of lagged assets with a standard error of $0.003 and do not reject zero. These estimates imply an ITT cash user cost elasticity of -0.13 with a 95% confidence interval spanning [-0.44,0.19], and a TOT cash user cost elasticity of -0.24 with a 95% confidence interval spanning [-0.84,0.36]. The interest limitation does not have an economically significant impact on firm cash holdings, consistent with firms using the same amounts of cash financing before and after the interest limitation. Once again, to the best of our knowledge, this is the first estimate of a cash user cost elasticity.

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, if the interest limitation causes a decline in investment, but larger firms disproportionately benefit from the tax rate changes included in the TCJA, this could cause an offsetting increase in investment for our treatment firms that explains our null results. However, we believe this is not a significant concern in our setting for three reasons.

First, the industry-profitability-year fixed effects in equation (2) ensure we compare treatment and control firms within the same industry and profitability groups that are more likely to face the same marginal tax rates, and have similar cost structures and investment durations.

Second, 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 reestimate 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 G.3 reports the average post-reform coefficients for the investment rate, leverage, equity issuance, and cash. All four point estimates are economically small and cannot reject zero.

Third, placebo event study regressions comparing big to small low-interest firms reveal no differential responses to other TCJA reforms by firm size. Appendix Table G.4 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 F.5 and ITT estimates of investment and financing outcomes in Appendix Figure F.6.

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, leverage and cash in response to the interest limitation, and find evidence that firms increase their equity issuance.

To implement the triple difference design, we estimate

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

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 $\delta_{jp(i),t}$ is an industry-profitability quartile-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 F.7. 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 F.8 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

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

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

To quantify the magnitude of our triple difference estimates of firm responses, we reestimate 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 4 replacing β_{post} with γ_{post} in equation (4).[17]

[17]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.

Post-reform coefficient estimates and elasticities from the triple difference design are strikingly similar to the event study results presented in Table 3 across all four outcomes. We estimate an ITT investment rate user cost elasticity of -0.01 [-0.52,0.51], and a TOT investment rate user cost elasticity of -0.01 [-1.28,1.25]. The corresponding ITT and TOT elasticities for leverage are -0.05 [-0.21,0.1] and -0.14 [-0.57,0.28], the ITT and TOT elasticities for equity issuance are 1.37 [0.15,2.59] and 3.66 [0.4,6.92], and the corresponding ITT and TOT elasticities for cash are 0.01 [-0.34,0.37] and 0.04 [-0.91,0.98].

Table 4: Triple Difference Effect on Investment and Financing

Dependent Variable	(1) Investment Rate	(2) Leverage	(3) Equity Issuance	(4) Cash
γ_{post}	-0.000	-0.003	0.010	0.000
	(0.009)	(0.004)	(0.004)	(0.003)
γ_{post}^{TOT}	0.002	-0.012	0.029	0.002
	(0.026)	(0.012)	(0.013)	(0.008)
Obs	$359,027$	$405,013$	$405,013$	$405,013$
Clusters	$61,511$	$69,035$	$69,035$	$69,035$
R^2	0.459	0.829	0.481	0.794
Pre-Reform Mean	0.315	0.470	0.064	0.136
ITT UCC Pct Change	0.11	0.11	0.11	0.11
ε^{ITT}	-0.01	-0.05	1.37	0.01
	(0.26)	(0.08)	(0.62)	(0.18)
TOT UCC Pct Change	0.05	0.04	0.04	0.04
ε^{TOT}	-0.01	-0.14	3.66	0.04
	(0.65)	(0.21)	(1.66)	(0.48)

Notes: This table reports event study 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 user cost.

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 D. 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 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 or leverage using alternative measures of liabilities, leverage ratio denominators, proxies for debt issuance using changes in debt, short-term debt, long-term debt, trade credit, or log debt. 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. However, we also rule out economically or statistically significant declines in shareholder payouts, payrolls, and executive compensation.

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

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 large enough to face the interest limitation.

5 Regression Discontinuity Design

The event study and triple difference estimates above both rule out economically significant investment, leverage and cash changes, 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 $25 million 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.[18]

5.1 Supplementing SOI Data with 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, so they may yield noisy RD estimates. In an effort to obtain more precise RD estimates, we also 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 user costs. Second, IRS staff do substantial manual editing of tax returns to improve

[18]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, 2022).

data quality in the SOI sample, while the E-filing records do not undergo similar processing.[19] 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.

Appendix Table G.5 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 and variance of the investment rate is higher in the E-filing data. Average leverage is lower in the E-filing data.

5.2 Regression Discontinuity Estimates

We construct two RD samples, one based on the SOI data and one based on the E-filing data. Using each data source, we restrict to years 2015-2019, and restrict 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.[20]

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.

[19] 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.

[20] In 2017, 49% of high-interest firms in our panel data are involved in an ownership link with a $>$ 50% stake.

Panels (a) and (b) plot first stage outcomes from Form 8990. Panel (a) shows there is a clear jump in the fraction of firms with interest disallowed around the $25 million cutoff, while panel (b) shows that the extensive margin jump corresponds to an increase in interest disallowed of roughly 1% of lagged assets.[21] We cannot reproduce panels (a) and (b) with the E-filing data because it does not include information on interest disallowed.

Figure 5, panels (c)-(f) display raw means of the investment rate, leverage, equity issuance and cash using the SOI data. 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.

Appendix Figure F.9 displays raw means of the investment rate, leverage, equity issuance and cash in evenly spaced $500,000 bins within a $5 million bandwidth using the E-filing data. We use a smaller bandwidth because the E-filing data contains significantly more observations. Once again, we observe no clear visual discontinuity for any of the four key outcome variables, strengthening the evidence that there is no large change in the investment rate or financing choices for marginal firms just above the lagged receipts cutoff.

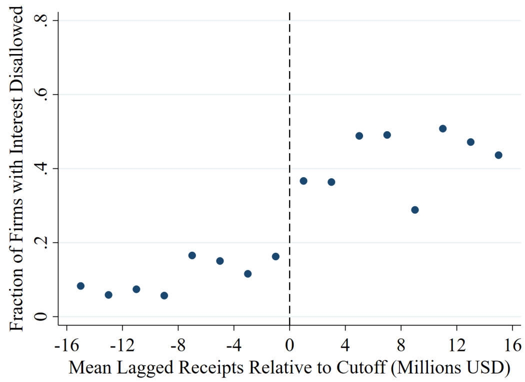
To complement our visual depictions of the RD design, we develop parametric estimates of the regression discontinuity by restricting to firms with interest above their limitation averaging over 2015 to 2017, dropping potential aggregators, and estimating

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

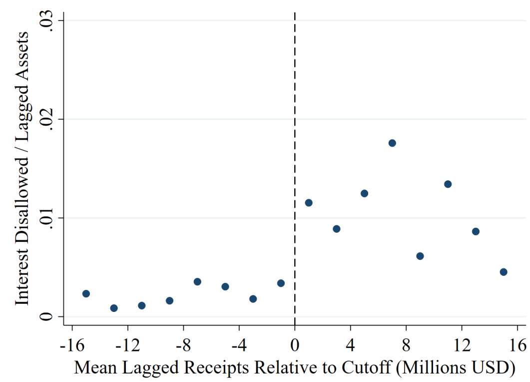
where outcome variable ΔY_i (investment rate, leverage, equity issuance and cash) is the average annual outcome over 2018 and 2019 minus the average annual outcome over 2015-2017, z_i is average lagged receipts over 2015-2017, $Big_i = 1$ if $z_i >$ \$25 million, and $f(z_i)$ are polynomials in the running variable separately on each side of the $25 million receipts threshold. Differencing the outcome increases precision and controls for the fact that firms on either side of the cutoff may have different outcome levels before the reform.

Our RD sample is constructed based on firm's interest relative to their limitation in a

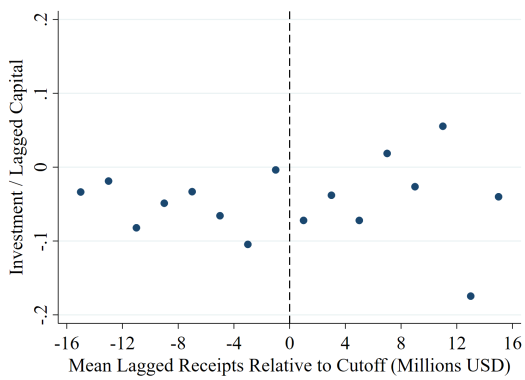
[21] Figure 5 also shows that some interest is disallowed from firms well below the $25 million receipts cutoff. This occurs for two reasons. First, some small business taxpayers face the interest limitation because they qualify as tax shelters, and second, our potential aggregators flag may not capture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



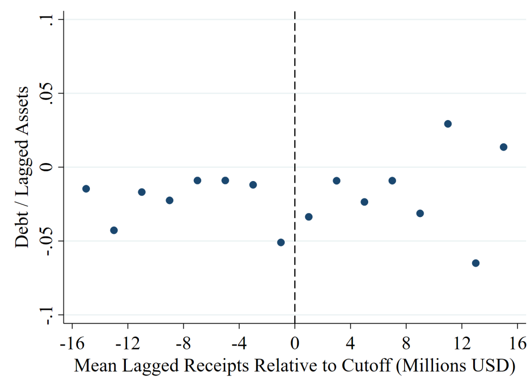
(a) Fraction with Interest Disallowed



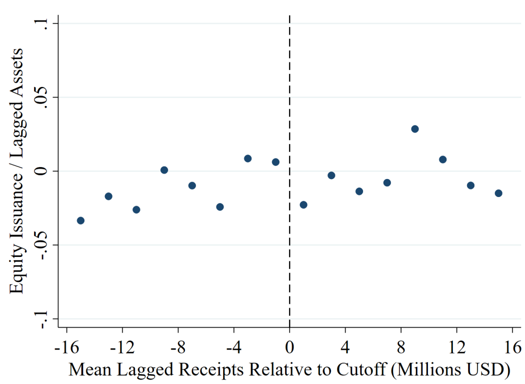
(b) Interest Disallowed



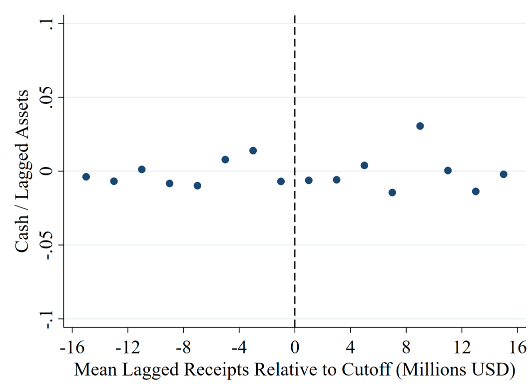
(c) Investment Rate



(d) Leverage



(e) Equity Issuance



(f) Cash

Figure 5: SOI 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 using the SOI data. 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 (c) displays average debt scaled by lagged assets, panel (d) displays average equity issuance scaled by lagged assets, and panel (e) displays average cash scaled by lagged assets.

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 Gelman and Imbens (2018) guidance to use first order polynomials when higher order coefficients are not statistically significant. We choose separate bandwidths in the SOI and E-filing data because of the drastically different sample sizes and differences in data quality. We use a \$16 million bandwidth in the SOI data and \$5 million bandwidth in the E-filing data, both based on optimal bandwidths for our outcome variables suggested by Calonico, Cattaneo and Titiunik (2014).

Table 5 displays reduced form and fuzzy RD estimates using the SOI and E-filing data that we also scale into user cost elasticities. We calculate elasticities following equation (4), but use reduced form RD estimates of mechanical and actual changes in user cost to calculate percent changes in user cost directly at the receipts cutoff. The E-filing data does not include information on interest disallowed or every variable required to construct our user cost of capital measure, so we cannot estimate the fuzzy RD specification in equation (8) or changes in user cost directly using the E-filing data. Therefore, to construct elasticities based on E-filing RD estimates we scale the reduced form RD coefficients by pre-reform means in the E-filing data and percent changes in the user cost estimated in the SOI data.

The RD results in Table 5 make three important points. First, our RD estimates are consistent with our event study and triple difference estimates. Confidence intervals on our

Table 5: Regression Discontinuity Effect on Investment and Financing

Dependent Variable	(1) Investment Rate	(2) Leverage	(3) Equity Issuance	(4) Cash
	Panel A: SOI Estimates			
β^{RF}	-0.019	0.003	-0.031	-0.008
	(0.047)	(0.026)	(0.025)	(0.017)
β^{IV}	-0.094	0.014	-0.158	-0.042
	(0.237)	(0.135)	(0.127)	(0.087)
Obs	1,607	1,676	1,676	1,676
Pre-Reform Mean	0.298	0.503	0.055	0.136
First Stage F-Stat	15.017	15.288	15.288	15.288
ITT UCC Pct Change	0.19	0.20	0.20	0.20
ε^{ITT}	-0.33	0.03	-2.84	-0.31
	(0.83)	(0.27)	(2.29)	(0.63)
TOT UCC Pct Change	0.08	0.09	0.09	0.09
ε^{TOT}	-0.82	0.06	-6.55	-0.71
	(2.07)	(0.61)	(5.28)	(1.46)
	Panel B: E-filing Estimates			
β^{RF}	0.053	-0.008	-0.016	-0.014
	(0.056)	(0.017)	(0.014)	(0.011)
Obs	3,968	4,006	4,006	4,006
Pre-Reform Mean	0.439	0.474	0.057	0.169
ε^{ITT}	0.64	-0.09	-1.44	-0.41
	(0.67)	(0.19)	(1.27)	(0.33)
ε^{TOT}	1.60	-0.20	-3.33	-0.94
	(1.69)	(0.43)	(2.94)	(0.76)

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 and E-filing RD samples. Robust standard errors are reported in parentheses. The SOI (E-filing) estimates use a bandwidth of $16 (5) million receipts. Pre-reform means are averages over 2015-2017 for firms above the receipts cutoff. ITT and TOT UCC Pct Change is the percent change in the user cost of capital, calculated as the RD estimate of β^{RF} using mechanical (ITT) or actual (TOT) user cost 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 user cost. We impute changes in user cost in the E-filing data using estimates from the SOI data.

RD estimates do not reject minimal investment, leverage and cash changes, nor can they reject increases in equity issuance. Second, our RD estimates are substantially less precise than our event study and triple difference estimates. Although the standard errors of the RD estimates are multiple times larger than the standard errors on event study or triple difference estimates, none of the RD estimates across outcomes or samples reject zero.[22] 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, and because they rule out existing RD estimates of the impacts of the interest limitation using Compustat data (Sanati, 2023). We attempt to reconcile differences between our and existing RD estimates in section 7.3.

5.3 Validating the RD Design

To validate the RD design, we first 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 in the SOI or E-filing data. Appendix Figure F.10 displays density plots of the distribution of firms around the $25 million lagged receipts cutoff in the SOI and E-filing data. McCrary tests suggest 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. Appendix Figure F.11 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,

[22] The standard errors on our investment rate estimates in column 1 of Table 5 are roughly five times the size of the event study and triple difference standard errors presented above. Despite using a bandwidth less than one-third the size of the SOI bandwidth, the E-filing estimates are based on more than twice as many observations as the SOI estimates. This leads to greater precision for the financing outcomes, but not investment because investment rates have a larger variance in the E-filing data. Even for leverage or equity issuance where the E-filing RD estimates are more precise than the SOI RD estimates, standard errors on the E-filing RD estimates are more than triple the size of our event study or triple difference standard errors.

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 G.6 displays estimates of β^{RF} from equation (7) using all low-interest firms in the SOI and E-filing data. We cannot reject zero impact of the opportunity to change from accrual to cash accounting on the investment rate, leverage, equity issuance or cash across either data set, suggesting the opportunity for firms below the receipts threshold to switch to cash accounting does not bias our RD estimates.

Finally, we show our RD results are not sensitive to the choice of bandwidth or polynomial degree. Appendix Figure F.12 presents reduced form RD estimates of equation (7) using the E-filing data varying the bandwidth and polynomial degree. Our main investment rate, leverage, equity issuance and cash estimates remain qualitatively similar regardless of the choice of bandwidth or polynomial degree. Given the stability of our RD estimates and their lack of precision, our additional heterogeneity and robustness analysis focuses on our event study and triple difference designs.

6 Heterogeneity

While the above results suggest the interest limitation has null impacts on investment, leverage and cash, and causes increases in equity issuance, these average estimates may mask heterogeneous responses among different types of firms. Previous research highlights that financially constrained firms are often more responsive to tax policy (Zwick and Mahon, 2017; Liu and Mao, 2019; Saez, Schoefer and Seim, 2019), while firms may have stronger responses to the interest limitation if they face larger cost of capital changes, higher interest rates, or larger tax changes.

To explore these possibilities, Figure 6 presents split sample ITT event study estimates of β_{post} from equation (2). We split firms into groups of above and below median predicted user cost changes, predicted interest disallowed, profitability, age, and interest rates, averaging splitting characteristics from 2015-2017. Our measure of predicted user cost changes is the difference between user costs if the interest limitation applied to all high-interest firms in the pre-reform period and firm's actual pre-reform user costs, and our measure of predicted interest disallowed is the amount of interest above firm's limitation in pre-reform years. We also split firms into groups that do and do not pay dividends from 2015-2017, into firms with positive and negative net incomes averaging from 2015-2017, and into C-corporations and pass-through entities.

The event study and triple difference estimates presented in previous sections measure average firm responses to the interest limitation exploiting variation within industry-profitability groups. However, firms with different levels of profitability may have different amounts of cash-on-hand, rely on external finance to different extents, and profits may generally be correlated with our other sample splits. Therefore, to explore heterogeneous responses to the interest limitation across firm characteristics our estimates of β_{post} in Figure 6 use only industry-year fixed effects, not industry-profitability-year fixed effects.[23]

If the user cost of capital is a sufficient statistic for investment, firms that experience larger user cost increases should decrease their investment by more. Consistent with this notion, we find above median predicted user cost change firms reduce investment by more than below median predicted user cost change firms. Our full sample results indicate that the interest limitation induces firms to increase equity issuance, but we see no evidence of increased equity issuance among high predicted user cost change firms. These firms have high predicted user cost changes in part because they have high debt financing fractions, and they appear unable or unwilling to substitute towards equity financing. These split sample results suggest that firms' willingness or ability to issue more equity plays an important role in firm responses to the interest limitation.

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).[24] However,

[23]We show our baseline results are similar with only industry-year fixed effects in Appendix Table D.8.

[24]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

our investment rate estimates in Figure 6, panel (a) suggest no clear differential declines in investment for firms that are younger, have lower profits, and do not pay dividends. The equity issuance estimates in Figure 6, panel (c) help rationalize the lack of investment declines among firms that appear constrained by our proxies. In our case, proxy measures of financial constraints identify sets of firms that respond to the interest limitation by using more equity financing, suggesting the younger, less profitable firms that do not pay dividends in our sample do not face steeply upward sloping capital supply curves or large wedges between internal and external capital costs (Farre-Mensa and Ljungqvist, 2016).[25]

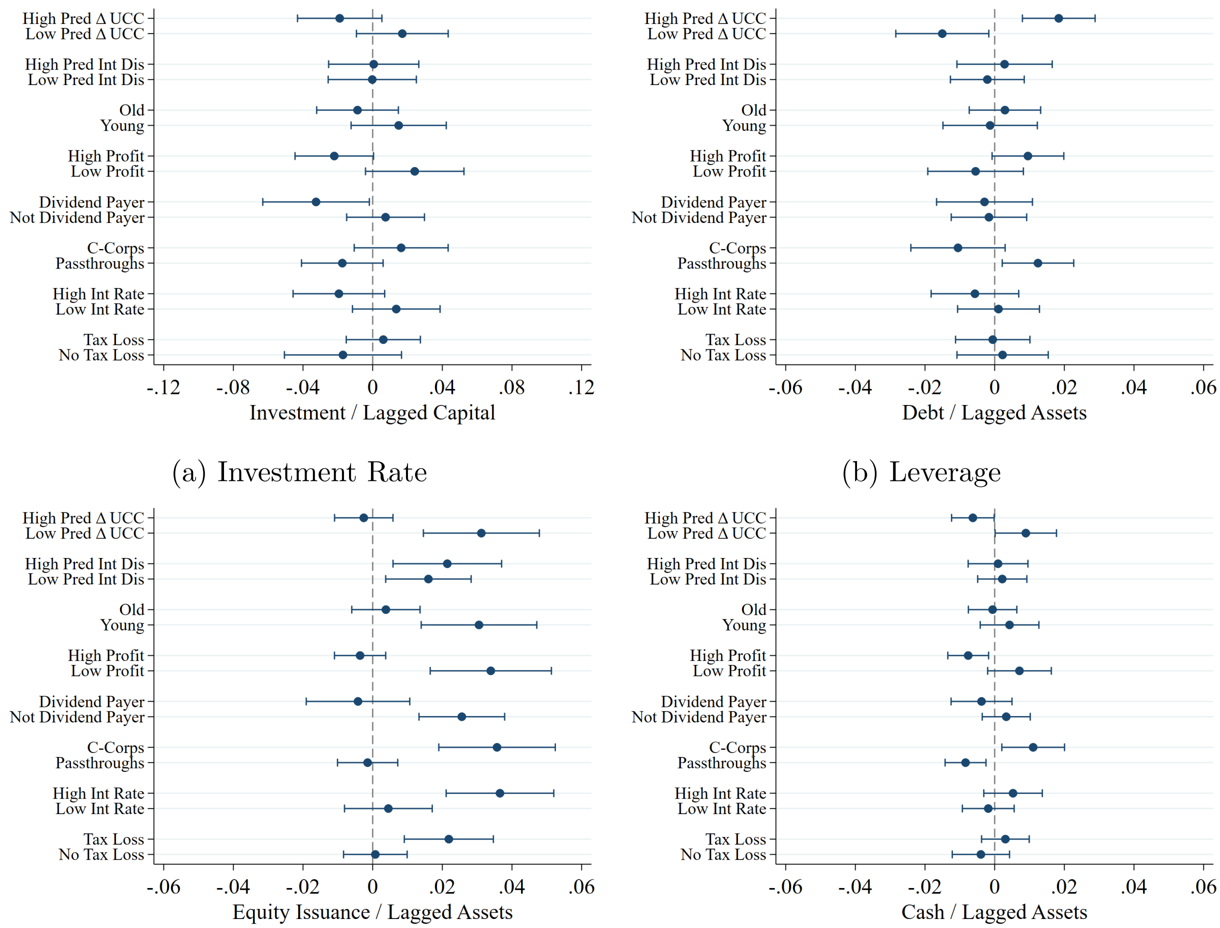
One possible reason we may rule out economically large investment responses is that the interest limitation was implemented during a time period with low interest rates, mitigating the value of interest deductions. While the federal funds rate was between 1.41 and 2.42 for all of 2018 and 2019, we show in Table 2 that big, high-interest firms in our data face an average interest rate of 8%. When we split firms into above and below median interest rate groups, the above median group averages a post-reform interest rate of 11% and the below median group averages a post-reform interest rate of 5%. Split sample estimates in Figure 6, panel (a) indicate above median interest rate firms experience relatively larger investment declines, but the declines are still small and cannot reject zero. Furthermore, above median interest rate firms issue more equity, reducing the likelihood they invest less because they continue to use more expensive debt financing.

Another potential explanation for ruling out economically large investment responses is that firms may not experience any immediate tax changes from the interest limitation if they have tax losses.[26] When we split firms into groups averaging positive and negative net

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

[25]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.

[26]This is less of a concern for pass-through businesses than C-corporations because pass-throughs use most of their losses contemporaneously (Goodman, Patel and Saunders-Scott, 2023).



(a) Investment Rate

(b) Leverage

(c) Equity Issuance

(d) Cash

Figure 6: 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, and using industry-by-year fixed effects. 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 scaled by lagged assets, equity issuance scaled by lagged assets and cash 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.

income from 2015-2017 in Figure 6, we find little relevant heterogeneity across firms that are more and less likely to face immediate tax changes from the interest limitation. Finally, we observe little relevant heterogeneity across any of the four outcomes in Figure 6 for firms with above versus below median predicted interest disallowed, suggesting the size of the cash flow shock firms face from increased taxes is not a key factor driving our results.

Appendix Figure F.13 present 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

How can economic theory explain our empirical results that suggest firm investment does not decline in response to the interest limitation? Neoclassical investment theory suggests that the user cost of capital is a sufficient statistic for investment (Hall and Jorgenson, 1967), implying that when the interest limitation raises firm's cost of capital, firms should invest less. To understand the magnitude of the expected effect, we calibrate a static, frictionless investment model as a benchmark (Moon, 2022). The investment user cost elasticity in the model is given by

$$\frac{dI/I}{d\Omega/\Omega} = \frac{1}{\delta}\frac{(\alpha_L+\alpha_K)\frac{1}{\epsilon}+\alpha_L-1}{1-(\alpha_L+\alpha_K)(\frac{1}{\epsilon}+1)}, \tag{9}$$

with capital and labor output elasticities α_K, α_L, elasticity of product demand ϵ, and depreciation δ. We derive the elasticity expression and describe the model in more detail in Appendix E. Assuming reasonable parameter values of $\alpha_L = 0.55, \alpha_K = 0.15, \epsilon = -5, \delta = 0.13$, the frictionless model implies an investment elasticity around -10, substantially larger than our estimated elasticities. Using our event study design, we estimate an ITT investment rate user cost elasticity of -0.02 $[-0.45, 0.42]$ and a TOT investment rate user cost elasticity of -0.03 $[-0.82, 0.77]$. Triple difference estimates of these elasticities have similar magnitudes and slightly larger confidence intervals.

One explanation for the gap between the benchmark elasticity and our estimated elasticities are investment frictions. Investment data is often lumpy, featuring periods of both inaction and large investment bursts (Doms and Dunne, 1998). More realistic models of firm behavior can reproduce lumpy investment patterns by incorporating convex and non-convex adjustment costs and partial irreversibility (Cooper and Haltiwanger, 2006; Winberry, 2021; Chen, Jiang, Liu, Suarez-Serrato and Xu, 2023). We also observe lumpy investment patterns in our data. Appendix Table G.9 shows that 24% of firm-years in our panel have zero investment while 34% of firm-years have large investment spikes exceeding 20% of lagged capital. Therefore, the firms we study are likely to face investment adjustment costs, and in an extreme case with very large adjustment costs, these frictions could explain the entire lack of investment responses to the interest limitation.

However, while investment frictions are likely to attenuate the benchmark frictionless elasticity, they are unlikely to be substantial enough to drive it all the way to zero. Existing estimates of investment rate user cost elasticities from samples of both publicly- and privately-held firms suggest parameter values around -2 with standard errors around 0.2 (Chen, Jiang, Liu, Suarez-Serrato and Xu, 2023). More broadly, a large empirical literature documents a significant relationship between costs of capital and investment, even in the presence of investment frictions (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).[27] Therefore, it seems unlikely that investment frictions alone can explain our zero elasticity estimates.[28]

A key difference between our paper and existing work is that the interest limitation changes our user cost of capital financing term ρ, while the majority of existing work studies changes in tax rates or bonus depreciation that change the user cost tax term $\frac{1-\tau z}{1-\tau}$. Changes in the tax term influence the after-tax price of all investment, while interest limitation induced changes in the financing term only impact firms if they rely on debt for their marginal financing. In other words, the cost of capital measure we use is a weighted average cost of

[27]To study how tax policy impacted investment, early research regressed the investment rate on the tax term of the user cost of capital, 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 user cost elasticities under the strong assumption that firm's average investment rate is the same as their average user cost. 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 user costs. 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.

[28]An alternative investment friction is that firms may incorrectly estimate their own costs of capital. However, Gormsen and Huber (2023) argue incorporating wedges between perceived and actual costs of capital into a benchmark investment model yields predictions in line with the empirical estimates in Zwick and Mahon (2017) that suggest a substantial relationship between the cost of capital and investment.

capital from different financing sources, and discrete investment projects may not be financed according to the weights we measure in the data based on firms inframarginal financing choices. If firms rely on debt for their marginal financing at the same rate they relied on debt for all inframarginal financing, we would expect to measure an investment rate user cost elasticity around -2. But if firms use less debt to finance new investment, we would expect to measure a smaller elasticity.

A simple example helps illustrate this point. Suppose a firm uses 60% debt and 40% equity, with an interest rate of 8%, a tax rate of 21%, equity costs of 7%, a 21% tax rate and net present value of depreciation deductions of 0.94. Eliminating interest deductions raises this firm's user cost of capital by 7%, from 0.148 to 0.158. Benchmark investment models assume this cost of capital change applies to every dollar of new investment, and assuming an elasticity of -2 should therefore lead to a 14% decline in investment. However, if a new investment is financed only with equity, the interest limitation will not change its cost at all. Taking our zero elasticity point estimates at face value suggests there is no change in the marginal cost of capital, implying that no new investment is financed with debt.[29]

If firms are not financing new investment with debt, there should be no detectable relationship between variation in the user cost financing term and firm's investment in our setting. We test this relationship directly by regressing investment on the user cost financing term using our event study sample. Specifically, we restrict to the high-interest firms in our baseline panel and estimate

$$Y_{it} = \beta(\rho_{it} + \delta) + \delta_{jp(i),t} + \xi_i + \varepsilon_{it}, \tag{10}$$

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 (10) from any correlation between firm investment and financial conditions, we instrument for $(\rho_{it} + \delta)$ with the interaction of Big_i and an

[29] To consider an intermediate case, suppose the investment rate post-reform event study estimate in table 3 was -0.02, a larger effect than the one we estimate that is still within our 95% confidence intervals. Then the investment rate user cost elasticity would be -0.89 (-0.02/.315/.07). For an effect of this size to result in an elasticity of -2, it would require the cost of capital change was .03, not .07, and therefore require that the firm in this example uses 13% debt and 87% equity to finance new investment.

indicator for the post-reform period. We display our coefficient estimates in Appendix Table G.8. The instrument has a strong positive relationship with our measures of the financing term. While OLS regressions suggest a negative correlation between the financing term and investment, 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 user cost financing term is positive. These regressions corroborate the lack of a clear negative relationship between the user cost financing term and investment in our setting, again suggesting firms rely on debt as a marginal source of financing substantially less than they have as an inframarginal source of financing.

7.2 Discussion of Borrowing Results

Economists typically use two broad classes of models to describe firm financing choices, pecking order and trade-off models. Pecking order models suggest firms prefer to use internal cash financing before using costly external debt or equity financing, while trade-off models suggest that firms choose leverage by weighing the tax benefits of debt against bankruptcy costs (Myers, 1984; Frank and Goyal, 2008; Frank, Goyal and Shen, 2020; Ai, Frank and Sanati, 2021).

The lack of investment responses to the interest limitation and the attenuation of our user cost elasticity estimates relative to both the frictionless investment model and existing empirical estimates suggests debt is not the marginal financing source for most firms. Furthermore, equity issuance is infrequent. 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. Taken together, these facts suggest firms primarily rely on cash financing for new investments, an observation that is consistent with the pecking order, and existing empirical and survey evidence (Yagan, 2015; Sharpe and Suarez, 2021).

Many of our results are consistent with dynamic investment models with costly external financing based on pecking order ideas. In this style of model, small firms receiving persistently high TFP shocks will borrow at a cost to scale up, but once firms achieve sufficient scale they can use existing revenues to finance most new investment. This provides an explanation for the lack of investment and borrowing responses to the limitation, because the limitation only applies to big firms that likely have sufficient scale to finance new investments with cash, but does not help explain the significant amounts of borrowing large firms continue to do in the data.

Our baseline borrowing results reject economically significant declines in leverage. These estimates reject static trade-off theory, which suggests firms should reduce leverage whenever the tax benefit of debt declines. However, our estimates are consistent with dynamic trade-off models that feature leverage adjustment costs that lead to inaction (Fischer, Heinkel and Zechner, 1989; Leary and Roberts, 2005; Danis, Rettl and Whited, 2014; Jeenas, 2019). In support of this interpretation, our data exhibit the large leverage changes that arise in models featuring fixed borrowing adjustment costs. Appendix Table G.9 shows that 32% of firm-years experience changes in debt scaled by lagged assets exceeding 5%.

Our empirical analysis suggests firms face fixed costs that are associated with the decision making process to alter their borrowing behavior, rather than costs associated with retiring long-term debt before its maturity. In Appendix Tables D.4 and D.5, we show that not only do we estimate no change in total debt in response to the interest limitation, we also estimate no change in short-term debt, which only includes mortgages, notes, and bonds due in less than one year. Big, high-interest firms have significant amounts of short-term debt on their balance sheet, 14% of assets in 2017. This implies substantial amounts of debt are coming off firm's balance sheets that face the interest limitation, and they are continuing to borrow at the same rates they were before the interest limitation was implemented.

One reasonable concern with the null leverage responses that we observe is that our post-reform analysis is focused on only 2018 and 2019. Without retiring long-term debt before its maturity, it may take firms substantial time to adjust leverage downwards, even when some debt is shorter-term. Therefore, our empirical estimates could be consistent with firms adjusting leverage downwards in response to the interest limitation, but slowly(Fama and French, 2002; Huang and Ritter, 2009). Additional quasi-experimental evidence suggests this is not the case. We extend all of our event study and triple difference results through 2020 in Appendix C. Event study estimates suggest a decline in both leverage and cash for big relative to small high-interest firms in 2020, but not 2018 or 2019. We attribute these declines to the COVID-19 pandemic differentially impacting big and small firms, not the interest limitations, for two reasons. First, the declines clearly begin in 2020 when COVID-19 started, rather than in 2018 when the interest limitation was implemented. Second the CARES act weakened the interest limitation in 2020. If there was a leverage or cash response to the interest limitation we would expect it to be smaller in 2020, not larger. Triple difference estimates confirm this interpretation. After controlling for the differential impacts of COVID-19 on big versus small firms, we find no leverage or cash changes in 2020. While we cannot rule out longer leverage adjustment periods over four or more years, the stability of our triple difference results does not support slow adjustment as an explanation for our results.[30]

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 face substantial fixed costs associated with changing their borrowing behavior.

7.3 Reconciling with Previous Estimates

Two existing papers study the U.S. interest limitation using Compustat data on publicly-held firms and find large investment and leverage responses to the policy. In this section, we attempt to reconcile our results with these estimates. First, Sanati (2023) studies the impact of the interest limitation using an RD design. 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 suggest null investment and financing responses to the interest limitation, but are imprecise. Simulations suggest RD estimates based on samples of this size from the same data generating process can lead to drastically diverging results, pointing to statistical power issues that limit our ability to learn about the effects of the interest limitation from such a small sample. Second, Carrizosa, Gaertner and Lynch (2022) use an event study design comparing big, high-interest firms to big, low-interest firms.

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

We also estimate large post-reform leverage declines in our data using this comparison, but we show these estimates are driven by mean reversion, not a response to the interest limitation.

7.3.1 RD Estimates for Public C-corporations

Appendix Table G.7 displays reduced form and fuzzy RD estimates of equations (7) and (8) using the 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, close to the optimal bandwidth across outcomes. Even with the significantly larger bandwidth, restricting to publicly-held firms leads to a very small sample with 142 total firms. Using this sample, our first stage F-statistics are much smaller. Despite the clear discontinuity at the \$25 million lagged receipts cutoff in our full data, restricting to so few firms leads to noisy measurement of variables on either side of the cutoff, resulting in a weak first stage. We cannot reject zero impact of the interest limitation on the investment rate, leverage, equity issuance or cash for publicly-held firms.

Our RD estimates for publicly-held firms diverge significantly from the RD estimates in Sanati (2023). Sanati (2023) estimates an investment rate user cost elasticity of -2.52 [-4.54,-0.50], and a leverage user cost elasticity of -15.59 [-26.62,-4.56]. These are ITT estimates because Compustat does not include information on which firms have interest disallowed. In contrast, we estimate an ITT investment rate user cost elasticity of -0.85 [-5.46,3.75] and an ITT leverage user cost elasticity of 1.89 [-3.35,7.14]. The leverage 95% confidence intervals do not overlap, and our investment rate confidence interval includes zero. One possible explanation for these differences is that RD estimates rely on large amounts of data close to the cutoff, making it fundamentally challenging to estimate an RD using only publicly-held firms.

To understand how noisy we might expect RD estimates to be with such a small sample, we take our SOI RD sample, restrict to observations within a \$16 million bandwidth, and construct 2,000 random samples of 71 observations on each side of the cutoff with replacement (to match the 142 publicly-held firms in our RD estimates). For each random sample, we reestimate our reduced form RD specification from equation (7) using leverage as an outcome. Appendix Figure F.14 plots the cumulative density function of these RD estimates across random samples. Close to 20% of the coefficient estimates have an absolute value > 0.1, suggesting there is a significant chance of obtaining a large coefficient estimate when the true coefficient is small simply by virtue of having a small sample. When we repeat the exercise randomly sampling 500 firms on either side of the cutoff, fewer than 1% of the coefficient estimates have an absolute value > 0.1.

7.3.2 Placebo-in-Time Estimates

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 leverage 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 firm leverage for big, high-interest relative to big, low-interest firms is likely to be driven by mean reversion rather than a response to the 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 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.[31]

To test for this dynamic, we construct four additional versions of our panel data set

[31]Similar concerns arise in the elasticity of taxable income literature (Saez, Slemrod and Giertz, 2012) and in evaluations of alternative minimum taxes on book income (Richmond, 2023).

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 reestimate equation (2). In Appendix Figure F.15, 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 leverage remains stable for big high-interest relative to low-interest firms in the years of the treatment definition, but leverage 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 leverage 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.

8 Conclusion

In this paper we use U.S. 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, leverage and cash holdings. Our event study and triple difference designs imply the interest limitation causes a modest increase in equity issuance. These findings suggest that big, high-interest firms are not using debt to finance new investment, and that firms face adjustment costs that can lead to inaction when the marginal cost of borrowing changes.

Extrapolating our results suggests eliminating interest deductions would raise substantial tax revenue without having a significant impact on either leverage or investment. However, our estimates are based on a set of high-interest firms that face the interest limitation in a low interest rate environment. While heterogeneity analysis does not suggest substantially larger responses to the interest limitation for firms with higher interest rates or that may face financial constraints, we should still use caution extrapolating our results to different macroeconomic environments or types of firms. Lower interest firms may face different incentives when making financing choices, while access to external capital may be more constrained in a higher interest rate environment. Either could lead firms to have different responses to the interest limitation. While our estimates provide the most granular estimates to date on the impacts of limiting interest deductions, any further policy experimentation should be carefully evaluated to bolster our understanding of the trade-offs between tax revenue, leverage and investment that arise from interest deductions.

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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	The 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	Max(0, first difference of (the sum of Schedule L, lines 22b and 23))	Max(0, first difference of (the sum of Schedule L, lines 22 and 23))	Max(0, first difference of Schedule L, line 21)
Cash	The sum of Schedule L, lines 1, 4, 5, and 6		
Assets	Schedule L, line 15		Schedule L, line 14
Capital	Schedule L, line 10b		Schedule L, line 9b
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	
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		
Gross 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.

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 User Cost of Capital Construction and Sensitivity

We take a data driven approach to measuring the user cost of capital. We generalize the typical Hall and Jorgenson (1967) expression for the user cost 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 user cost of capital expression.

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

We ultimately use our user cost of capital to construct elasticity estimates following equation (4). Restating that equation here,

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

the user cost enters the elasticity via the percent change in user cost term for treatment relative to control firms. To assess the sensitivity of our user cost elasticity estimates to different constructions of the user cost, we recalculate investment rate elasticities using different user cost 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 that are relevant for the interest limitation, and directly measures

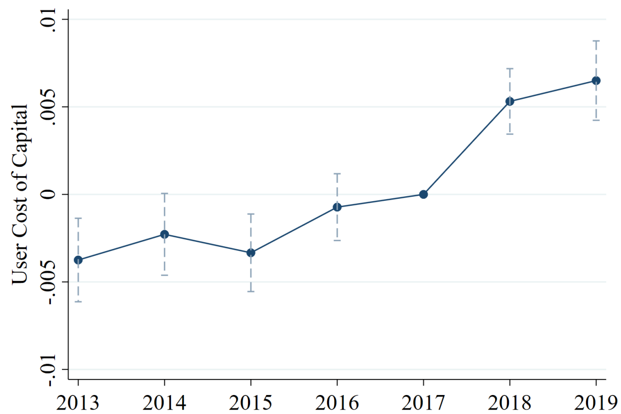


Figure B.1: User Cost of Capital Event Study Estimates

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

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 user cost 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 user cost elasticity estimates by less.

Table B.1: User 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 (2023)
τ	C-corps: marginal rate, S-corps: top individual rate	Our Data
z	Varies at 4-digit NAICS level	Zwick and Mahon (2017)

Notes: This table describes the parameters used to construct our user cost of capital measure and their sources.

Table B.2: Investment Rate Elasticity Sensitivity to User Cost 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	0.130	0.107	0.102	0.121	0.096
ε^{ITT}	-0.024	-0.030	-0.031	-0.026	-0.033
	(0.220)	(0.267)	(0.280)	(0.236)	(0.298)
TOT UCC Pct Change	0.072	0.061	0.057	0.066	0.053
ε^{TOT}	-0.044	-0.052	-0.056	-0.048	-0.060
	(0.397)	(0.468)	(0.501)	(0.433)	(0.539)
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

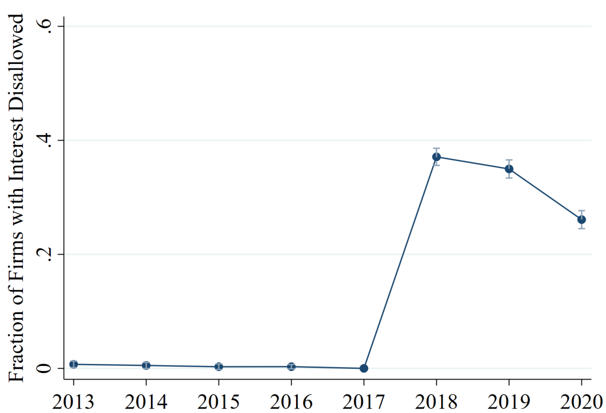
Notes: This table reports investment rate elasticity estimates while varying user cost parameters. The first row displays event study estimates of β_{post} from equation (2) using the investment rate as an outcome, replacing the indicators for 2018 and 2019 with a single indicator for an observation 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 ration 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 2020 Responses to the Interest Limitation

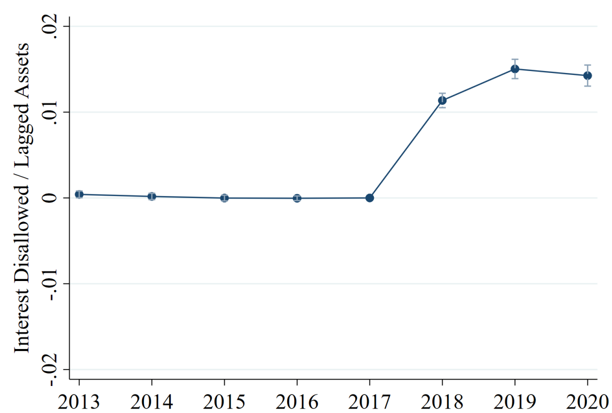
In this appendix, we extend our event study and triple difference estimates to include firm responses in 2020. To perform this analysis, we reconstruct our baseline panel data set, requiring that firms are present in one year between 2018-2020 instead of 2018-2019.

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 C.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.



(a) Indicator for Interest Disallowed



(b) Interest Disallowed

Figure C.1: First Stage Event Study Estimates

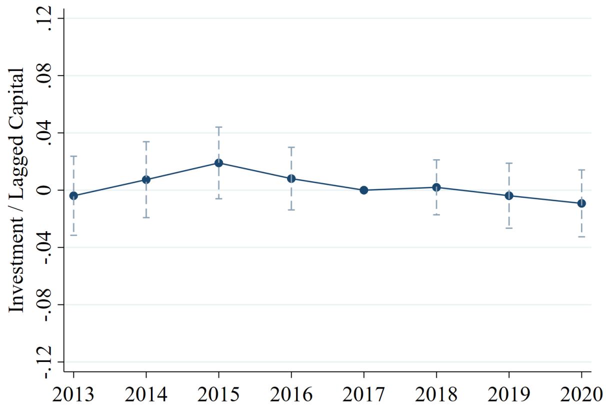
Notes: This figure plots event study estimates of β_e from equation (2) using the low-interest firms in panel data spanning 2013-2020. Panel (a) uses an indicator equal to 1 if interest is disallowed as the outcome variable, while panel (b) uses interest disallowed scaled by lagged assets as the outcome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

Figure C.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.

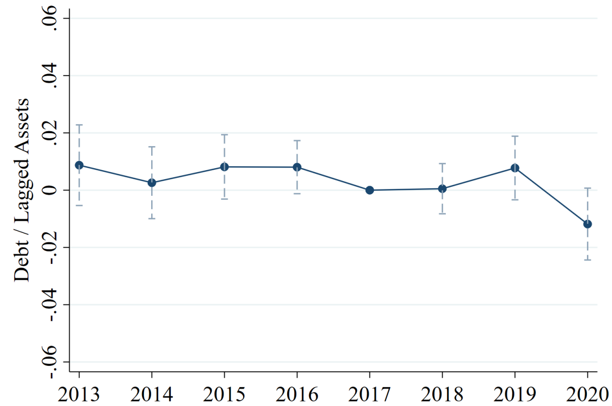
Panel (b) plots firm leverage responses. While the 2018 and 2019 coefficients are both close to 0 and reject declines in debt of more than 1% of lagged assets, the 2020 estimate shows a statistically significant decline in debt of roughly 2% of lagged assets. We attribute this decline to the COVID-19 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 leverage 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 C.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 C.2, panel (c) plots firm equity issuance responses. The 2020 coefficient shows little deviation from the 2018 or 2019 coefficient. Panel (d) plots firm cash responses. The 2020 estimate diverges from the 2018 and 2019 estimates and shows a statistically significant decline in cash 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 C.3.

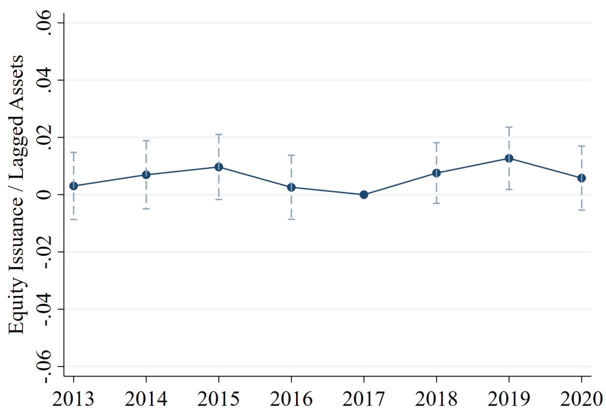
Figure C.3 plots estimates of β_e from equation (2) for our four main outcomes for low-interest firms that do not face the interest limitation. We observe sharp declines in β_e in 2020 for the investment rate, leverage and cash, and no change in equity issuance estimates. These declines suggest that 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.



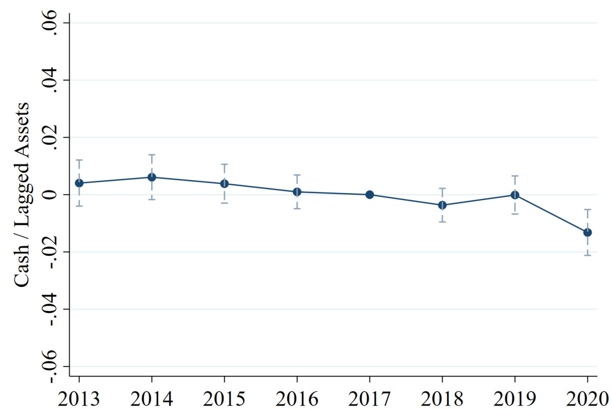
(a) Investment Rate



(b) Leverage



(c) Equity Issuance

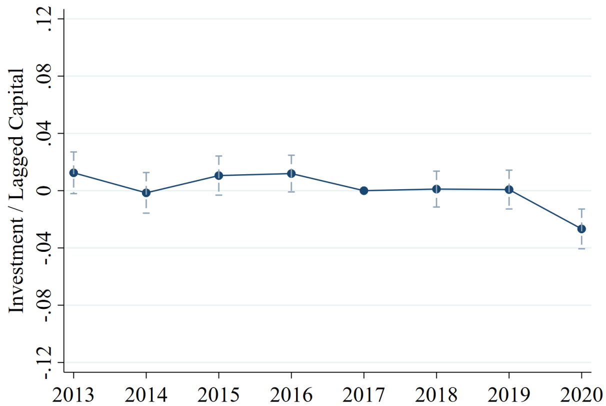


(d) Cash

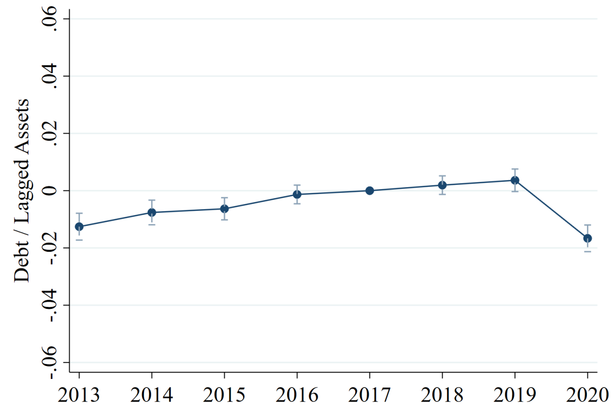
Figure C.2: Event Study Estimates

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 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 scaled by lagged assets as an outcome variable. 95% confidence intervals are constructed from standard errors clustered at the firm level.

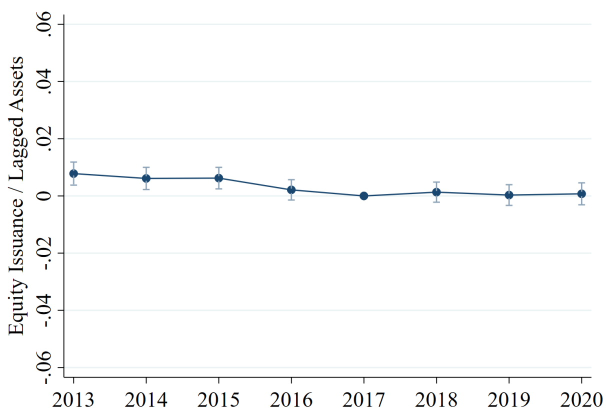
Figure C.4 plots estimates of γ_e from equation (5). Once we control our estimates for the differential impact of COVID-19 on big versus small firms, we cannot reject zero investment rate, leverage, or cash responses to the interest limitation, and estimate qualitatively similar equity issuance increases.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



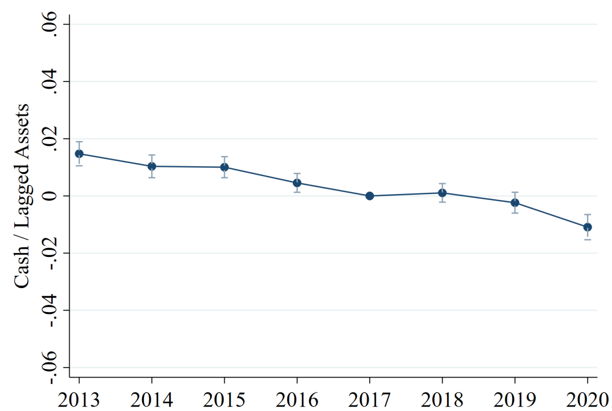
(a) Investment Rate



(b) Leverage



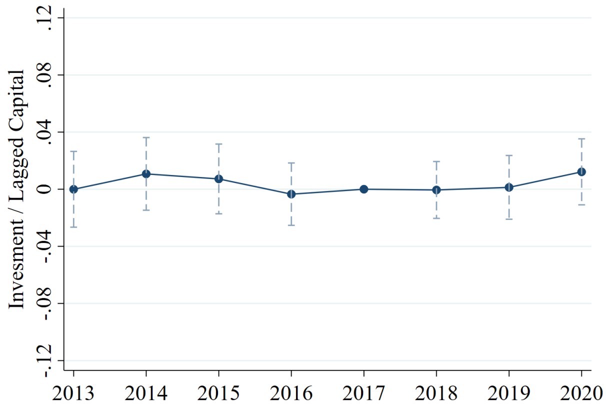
(c) Equity Issuance



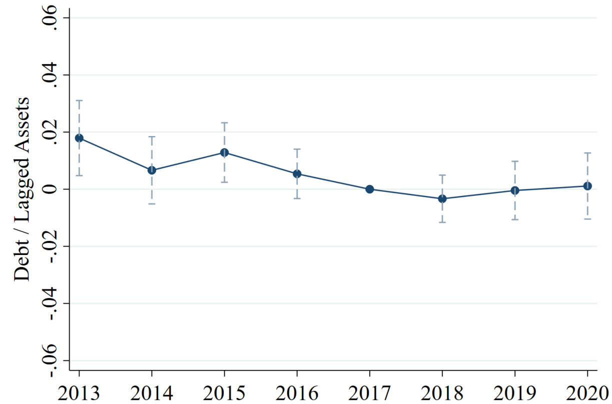
(d) Cash

Figure C.3: Placebo Event Study Estimates

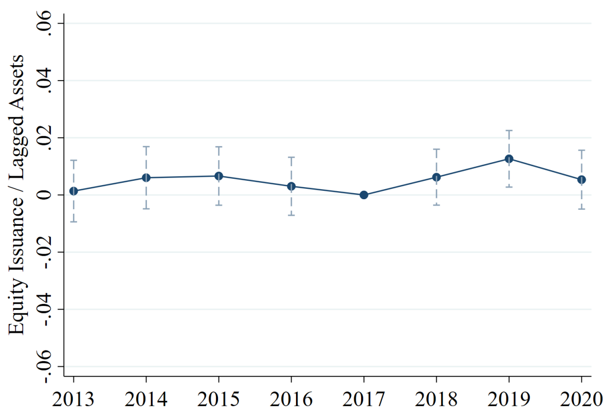
Notes: This figure plots event study estimates of β_e from equation (2) using the low-interest firms in panel data spanning 2013-2020. Panel (a) uses investment scaled by lagged capital as an outcome variable. Panel (b) uses debt scaled by lagged assets as an outcome variable. Panel (c) uses equity issuance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



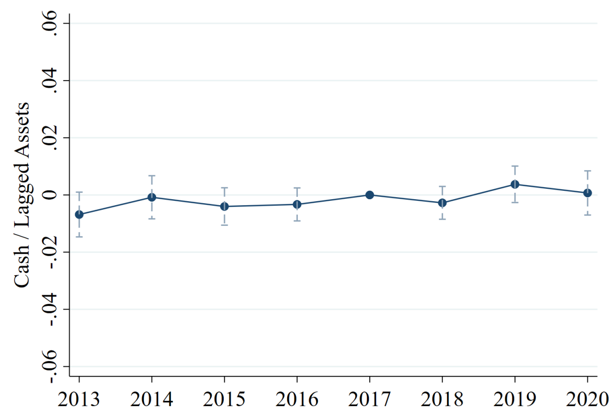
(a) Investment Rate



(b) Leverage



(c) Equity Issuance



(d) Cash

Figure C.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 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 scaled by lagged assets as an outcome variable. 95% confidence intervals are constructed from standard errors clustered at the firm level.

D Event Study and Triple Difference Robustness: Different Samples, Outcomes, and Specifications

We begin our additional event study and triple difference robustness checks by exploring the sensitivity of our results to treatment persistence. Appendix Figure F.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 D.1 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 D.2 and D.3 display event study estimates of β_{post} and triple difference estimates of γ_{post} alongside user cost 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 1.5% decline in the fraction of firms investing.

The null changes in leverage we observe in response to the interest limitation are also robust to a variety of alternative measures. Appendix Tables D.4 and D.5 display event study estimates of β_{post} and triple difference estimates of γ_{post} alongside user cost elasticity estimates using ten different measures of leverage or debt: debt scaled by financial capital, debt plus loans from stockholders scaled by assets and financial capital, changes in debt and debt plus loans from stockholders scaled by lagged assets to proxy for debt issuance, 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, 19 of the 20 estimates cannot reject zero.[32] 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, the lack of large leverage declines cannot be explained by our debt and leverage variables being stock rather than flow measures. 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 D.6 and D.7 present event study estimates of β_{post} and triple difference estimates of γ_{post} alongside user cost 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.

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 D.8, 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, leverage, equity issuance and cash.

[32] Our triple difference estimates suggest a statistically significant decline in log debt plus loans from stockholders.

The baseline estimates we display in Tables 3 and 4 use industry-profitability-year fixed effects and no control variables. The first row of each panel in Table D.8 displays event study or triple difference estimates using industry-year fixed effects instead of industry-profitability-year fixed effects, while the second row of each panel includes industry-profitability-year fixed effects and 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. In the event study design, this alternative scaling yields similar investment, equity and cash results but suggests there is a statistically significant increase in leverage. However, in Appendix Table D.4 we find no statistically significant increase in debt when using log(debt) and log(debt + loans from stockholders) as outcome variables, suggesting there is no clear increase in debt. Furthermore, triple difference estimates using the fixed pre-reform scaling variables yield similar investment leverage, equity and cash results as our baseline estimates.

The fourth row in each panel uses outcome variables winsorized at the 99th percentile rather than the 95th percentile. Our results for the event study and triple difference designs for the investment rate, equity issuance and cash remain similar. For leverage, the event study estimate does not reject zero, but the triple difference estimate suggests a decline in debt of \$0.013 per dollar of lagged assets that rejects zero. However, this result does not hold across leverage outcomes. For example, winsorizing at the 99th percentile, we cannot reject zero changes in debt scaled by lagged financial capital.

Primarily real estate and agriculture firms 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.[33] The fifth row of each panel drops real estate firms from our estimation sample and finds similar results.[34]

[33] 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 3.5% of observed firm-years, and all other firms opt out in 1.7% of observed firm-years.

[34] In Appendix Table D.1, 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 D.8 is an *ex ante* restriction eliminating many firms that could elect out, avoiding selection issues arising from only dropping firms that do elect out.

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.

Table D.1: 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	0.365	0.434	0.475	0.381	0.474	0.528
	(0.007)	(0.008)	(0.008)	(0.009)	(0.010)	(0.010)
Investment Rate	−0.001	0.002	0.004	−0.005	−0.002	0.000
	(0.009)	(0.010)	(0.011)	(0.011)	(0.013)	(0.014)
Leverage	0.004	0.003	0.002	0.004	0.003	0.003
	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)
Equity Issuance	0.011	0.013	0.012	0.017	0.021	0.020
	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)
Cash	−0.002	−0.002	−0.001	−0.000	0.000	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
			Panel B: ε^{TOT} Estimates			
Investment Rate	−0.03	0.09	0.14	−0.20	−0.08	0.00
	(0.41)	(0.42)	(0.40)	(0.49)	(0.51)	(0.47)
Leverage	0.12	0.09	0.05	0.12	0.09	0.06
	(0.14)	(0.15)	(0.14)	(0.15)	(0.17)	(0.15)
Equity Issuance	2.58	2.75	2.30	3.44	3.82	3.20
	(1.10)	(1.15)	(1.08)	(1.26)	(1.33)	(1.21)
Cash	−0.24	−0.22	−0.14	−0.02	0.01	0.13
	(0.30)	(0.32)	(0.30)	(0.37)	(0.39)	(0.36)
Obs	89,591	80,348	76,882	57,847	50,722	48,421
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 user cost for treatment relative to control firms.

Table D.2: Event Study Effect on Alternative Investment Measures

	(1)	(2)	(3)
Dependent Variable	log(Investment)	$\mathbb{1}$(Investment > 0)	$\mathbb{1}$(Investment $> 0.2 * $ Capital)
β_{post}	0.001	-0.004	-0.004
	(0.031)	(0.006)	(0.008)
β_{post}^{TOT}	0.002	-0.012	-0.010
	(0.085)	(0.017)	(0.023)
Obs	64,813	89,591	89,591
Clusters	13,034	16,107	16,107
ε^{ITT}	0.01	-0.04	-0.07
	(0.26)	(0.05)	(0.17)
ε^{TOT}	0.02	-0.07	-0.14
	(0.49)	(0.10)	(0.32)

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. We calculate $\varepsilon^{ITT,TOT}$ as the outcome variable coefficient estimate divided by the pre-reform mean of the outcome variable for treatment firms, divided by the mechanical (ITT) or actual (TOT) percent change in user cost for treatment relative to control firms. Coefficients for logged outcome variables are interpreted as percent changes in elasticity calculations.

Table D.3: Triple Difference Effect on Alternative Investment Measures

	(1)	(2)	(3)
Dependent Variable	log(Investment)	$\mathbb{1}$(Investment >0)	$\mathbb{1}$(Investment $>0.2*$ Capital)
γ_{post}	0.023	-0.015	0.002
	(0.031)	(0.006)	(0.009)
γ_{post}^{TOT}	0.062	-0.045	0.009
	(0.086)	(0.017)	(0.026)
Obs	$302,263$	$405,013$	$405,013$
Clusters	$56,380$	$69,035$	$69,035$
ε^{ITT}	0.21	-0.15	0.05
	(0.28)	(0.06)	(0.20)
ε^{TOT}	0.53	-0.40	0.14
	(0.71)	(0.16)	(0.53)

Notes: This table reports event study 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. We calculate $\varepsilon^{ITT,TOT}$ as the outcome variable coefficient estimate divided by the pre-reform mean of the outcome variable for big, high-interest firms, divided by the mechanical (ITT) or actual (TOT) percent change in user cost for big high-interest relative to small high-interest firms, net of the percent change for big low-interest relative relative to small low-interest firms. Coefficients for logged outcome variables are interpreted as percent changes in elasticity calculations.

Table D.4: Event Study Effect on Alternative Leverage Measures

	(1)	(2)	(3)	(4)	(5)
			Panel A		
Dependent Variable	$\frac{\text{Debt}}{\text{Fin Capital}}$	$\frac{\text{Debt+LSH}}{\text{Assets}}$	$\frac{\text{Debt+LSH}}{\text{Fin Capital}}$	$\frac{\Delta\text{Debt}}{\text{Assets}}$	$\frac{\Delta\text{Debt+LSH}}{\text{Assets}}$
β_{post}	-0.001	0.001	0.000	0.003	0.005
	(0.007)	(0.005)	(0.008)	(0.003)	(0.003)
β_{post}^{TOT}	-0.002	0.003	0.000	0.008	0.014
	(0.019)	(0.013)	(0.022)	(0.008)	(0.009)
Obs	89,584	89,591	89,584	89,591	89,591
Clusters	16,107	16,107	16,107	16,107	16,107
ε^{ITT}	-0.01	0.02	0.00	0.86	1.33
	(0.09)	(0.07)	(0.09)	(0.88)	(0.83)
ε^{TOT}	-0.02	0.04	0.00	1.64	2.53
	(0.17)	(0.14)	(0.17)	(1.67)	(1.57)
			Panel B		
Dependent Variable	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}	0.029	0.009	0.002	-0.000	-0.002
	(0.018)	(0.017)	(0.002)	(0.004)	(0.002)
β_{post}^{TOT}	0.074	0.023	0.005	-0.001	-0.006
	(0.045)	(0.042)	(0.006)	(0.011)	(0.005)
Obs	72,459	76,797	89,591	89,591	89,591
Clusters	13,793	14,464	16,107	16,107	16,107
ε^{ITT}	0.22	0.07	0.09	-0.01	-0.14
	(0.14)	(0.12)	(0.13)	(0.10)	(0.12)
ε^{TOT}	0.43	0.12	0.18	-0.02	-0.27
	(0.26)	(0.23)	(0.24)	(0.19)	(0.22)

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. We calculate $\varepsilon^{ITT,TOT}$ as the outcome variable coefficient estimate divided by the pre-reform mean of the outcome variable for treatment firms, divided by the mechanical (ITT) or actual (TOT) percent change in user cost for treatment relative to control firms. Coefficients for logged outcome variables are interpreted as percent changes in elasticity calculations.

Table D.5: Triple Difference Effect on Alternative Leverage Measures

	(1)	(2)	(3)	(4)	(5)
			Panel A		
Dependent Variable	$\frac{\text{Debt}}{\text{Fin Capital}}$	$\frac{\text{Debt+LSH}}{\text{Assets}}$	$\frac{\text{Debt+LSH}}{\text{Fin Capital}}$	$\frac{\Delta\text{Debt}}{\text{Assets}}$	$\frac{\Delta\text{Debt+LSH}}{\text{Assets}}$
γ_{post}	-0.004	-0.006	-0.005	0.000	0.001
	(0.006)	(0.004)	(0.007)	(0.003)	(0.003)
γ_{post}^{TOT}	-0.019	-0.022	-0.019	-0.002	0.001
	(0.019)	(0.013)	(0.021)	(0.008)	(0.009)
Obs	$404,869$	$405,013$	$404,869$	$405,013$	$405,013$
Clusters	$69,019$	$69,035$	$69,019$	$69,035$	$69,035$
ε^{ITT}	-0.06	-0.11	-0.06	0.01	0.39
	(0.09)	(0.08)	(0.10)	(0.98)	(0.92)
ε^{TOT}	-0.17	-0.29	-0.16	0.03	1.04
	(0.25)	(0.21)	(0.25)	(2.61)	(2.45)
			Panel B		
Dependent Variable	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}	-0.037	-0.047	0.001	-0.005	0.001
	(0.019)	(0.017)	(0.002)	(0.004)	(0.002)
γ_{post}^{TOT}	-0.113	-0.146	0.002	-0.020	0.003
	(0.054)	(0.050)	(0.006)	(0.011)	(0.005)
Obs	$266,244$	$287,773$	$405,013$	$405,013$	$405,013$
Clusters	$49,954$	$53,499$	$69,035$	$69,035$	$69,035$
ε^{ITT}	-0.32	-0.40	0.06	-0.16	0.08
	(0.17)	(0.15)	(0.14)	(0.11)	(0.13)
ε^{TOT}	-0.87	-1.09	0.17	-0.43	0.21
	(0.45)	(0.40)	(0.36)	(0.29)	(0.35)

Notes: This table reports event study 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. We calculate $\varepsilon^{ITT,TOT}$ as the outcome variable coefficient estimate divided by the pre-reform mean of the outcome variable for big, high-interest firms, divided by the mechanical (ITT) or actual (TOT) percent change in user cost for big high-interest relative to small high-interest firms, net of the percent change for big low-interest relative relative to small low-interest firms. Coefficients for logged outcome variables are interpreted as percent changes in elasticity calculations.

Table D.6: Event Study Effect on Equity, Payout and Labor Compensation

Dependent Variable	(1) log(Equity)	(2) $\mathbb{1}$(Equity > 0)	(3) Payouts	(4) Payroll	(5) Exec Comp
β_{post}	0.0218	0.0056	0.0020	0.0018	0.0001
	(0.0656)	(0.0085)	(0.0013)	(0.0032)	(0.0006)
β_{post}^{TOT}	0.0633	0.0156	0.0054	0.0050	0.0002
	(0.1896)	(0.0232)	(0.0037)	(0.0088)	(0.0015)
Obs	$22,626$	$89,591$	$89,591$	$89,591$	$89,591$
Clusters	$6,626$	$16,107$	$16,107$	$16,107$	$16,107$
ε^{ITT}	0.22	0.14	1.08	0.06	0.04
	(0.65)	(0.21)	(0.75)	(0.10)	(0.32)
ε^{TOT}	0.54	0.26	2.06	0.11	0.08
	(1.62)	(0.39)	(1.41)	(0.19)	(0.61)

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. We calculate $\varepsilon^{ITT,TOT}$ as the outcome variable coefficient estimate divided by the pre-reform mean of the outcome variable for treatment firms, divided by the mechanical (ITT) or actual (TOT) percent change in user cost for treatment relative to control firms. Coefficients for logged outcome variables are interpreted as percent changes in elasticity calculations.

Table D.7: Triple Difference Effect on Equity, Payout and Labor Compensation

Dependent Variable	(1) log(Equity)	(2) $\mathbb{1}$(Equity > 0)	(3) Payouts	(4) Payroll	(5) Exec Comp
γ_{post}	0.0172	0.0026	0.0042	0.0052	-0.0007
	(0.0647)	(0.0083)	(0.0015)	(0.0031)	(0.0006)
γ_{post}^{TOT}	0.0479	0.0098	0.0124	0.0142	-0.0021
	(0.1908)	(0.0248)	(0.0044)	(0.0091)	(0.0016)
Obs	87, 358	405, 013	405, 013	405, 013	405, 013
Clusters	24, 070	69, 035	69, 035	69, 035	69, 035
ε^{ITT}	0.21	0.07	2.72	0.19	-0.49
	(0.78)	(0.24)	(0.96)	(0.11)	(0.37)
ε^{TOT}	1.05	0.19	7.26	0.50	-1.31
	(3.96)	(0.63)	(2.55)	(0.30)	(0.99)

Notes: This table reports event study 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. We calculate $\varepsilon^{ITT,TOT}$ as the outcome variable coefficient estimate divided by the pre-reform mean of the outcome variable for big, high-interest firms, divided by the mechanical (ITT) or actual (TOT) percent change in user cost for big high-interest relative to small high-interest firms, net of the percent change for big low-interest relative relative to small low-interest firms. Coefficients for logged outcome variables are interpreted as percent changes in elasticity calculations.

Table D.8: Robustness Tests

Dependent Variable	(1) Int Disallow	(2) Investment Rate	(3) Leverage	(4) Equity Issuance	(5) Cash
	Panel A: Event Study Estimates				
Ind x Yr FE	0.013	0.001	0.000	0.018	0.002
	(0.000)	(0.009)	(0.004)	(0.005)	(0.003)
Controls	0.013	0.004	0.006	0.014	−0.001
	(0.000)	(0.009)	(0.004)	(0.005)	(0.003)
Fixed Pre-Reform Scale	0.017	0.008	0.013	0.010	0.000
	(0.001)	(0.007)	(0.004)	(0.004)	(0.002)
Winsorize at 99^{th} pctile	0.015	0.013	0.001	0.013	−0.004
	(0.001)	(0.027)	(0.006)	(0.008)	(0.004)
Drop Real Estate	0.015	0.001	0.004	0.013	−0.001
	(0.000)	(0.011)	(0.005)	(0.006)	(0.003)
Drop Aggregators	0.010	−0.014	0.011	0.010	−0.002
	(0.001)	(0.014)	(0.007)	(0.006)	(0.004)
Balanced Panel	0.012	−0.006	0.003	0.013	−0.004
	(0.000)	(0.012)	(0.006)	(0.005)	(0.004)
Drop Largest	0.013	−0.005	0.006	0.011	0.000
	(0.000)	(0.010)	(0.005)	(0.005)	(0.003)
	Panel B: Triple Difference Estimates				
Ind x Yr FE	0.012	0.001	−0.002	0.010	0.001
	(0.000)	(0.009)	(0.004)	(0.004)	(0.003)
Controls	0.012	−0.001	−0.003	0.009	−0.000
	(0.000)	(0.009)	(0.004)	(0.004)	(0.003)
Fixed Pre-Reform Scale	0.015	−0.013	−0.004	0.008	−0.005
	(0.001)	(0.007)	(0.004)	(0.003)	(0.002)
Winsorize at 99^{th} pctile	0.014	0.024	−0.013	0.021	0.004
	(0.001)	(0.027)	(0.006)	(0.008)	(0.004)
Drop Real Estate	0.013	−0.002	−0.001	0.015	0.000
	(0.000)	(0.011)	(0.005)	(0.005)	(0.003)
Drop Aggregators	0.008	−0.013	0.006	0.009	−0.001
	(0.001)	(0.015)	(0.006)	(0.005)	(0.004)
Balanced Panel	0.012	−0.005	−0.004	0.013	−0.001
	(0.000)	(0.011)	(0.005)	(0.005)	(0.003)
Drop Largest	0.012	−0.003	0.002	0.009	0.002
	(0.000)	(0.010)	(0.004)	(0.005)	(0.003)

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.

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 user cost 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

$$C\left(y; w, \Omega\right) = \left(\alpha_L + \alpha_K\right) \left[\frac{y}{A} \left(\frac{w}{\alpha_L}\right)^{\alpha_L} \left(\frac{\Omega}{\alpha_K}\right)^{\alpha_K}\right]^{\frac{1}{a_L + a_K}},$$

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

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

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

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

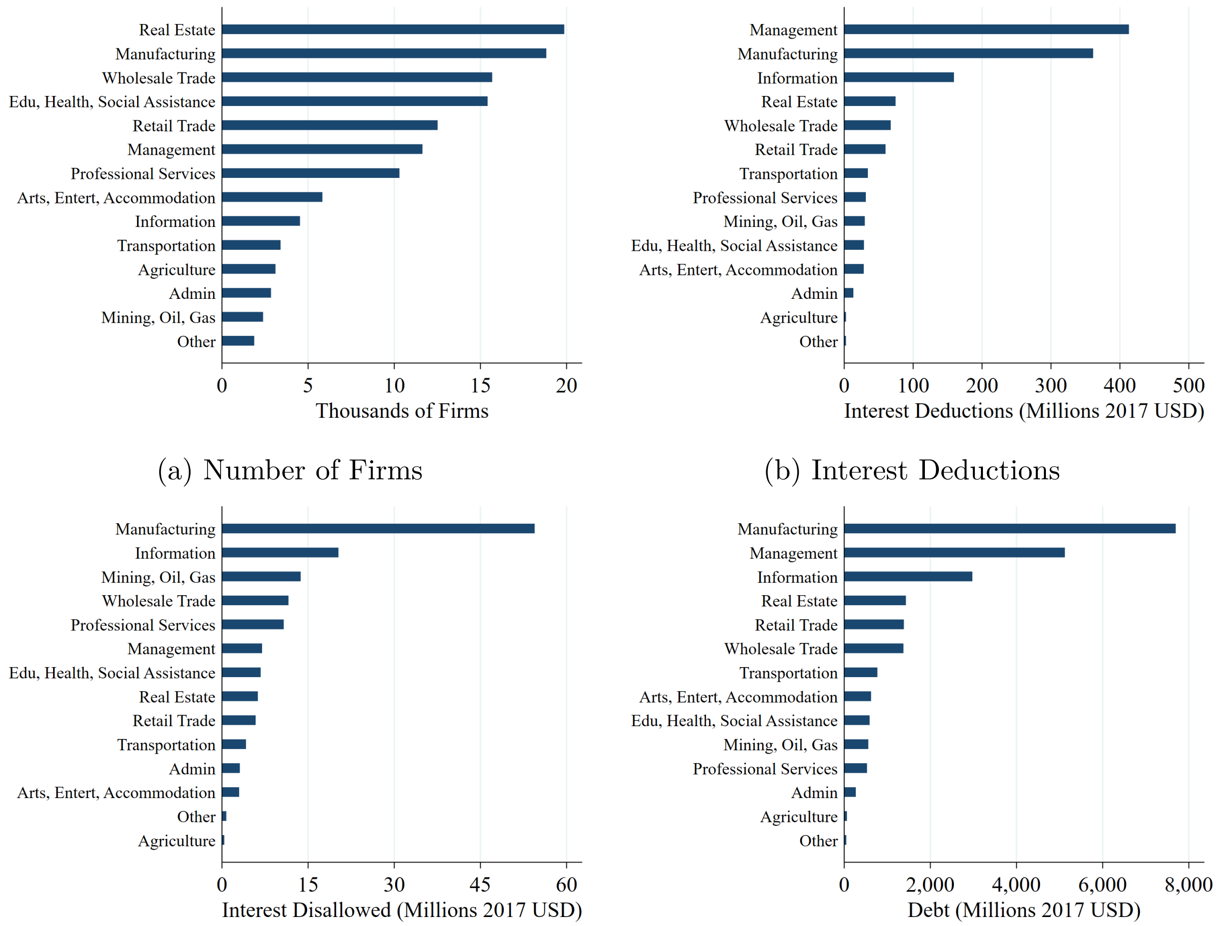
$$\frac{dK^*/K^*}{d\Omega/\Omega} = \frac{(\alpha_L + \alpha_K)\frac{1}{\epsilon} + \alpha_L - 1}{1 - (\alpha_L + \alpha_K)(\frac{1}{\epsilon} + 1)}.$$

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 user cost elasticity of -10.3.

F Appendix Figures



(a) Number of Firms

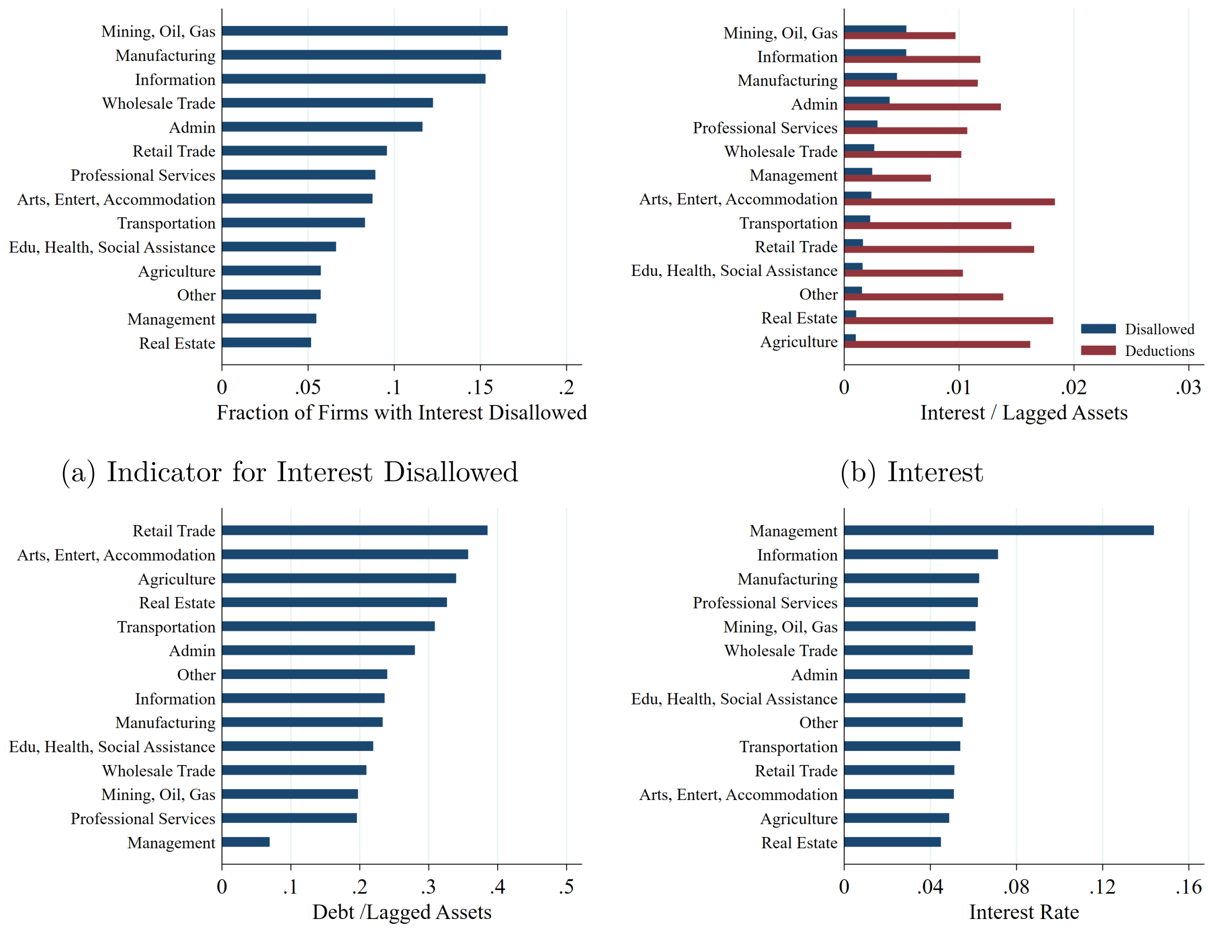
(b) Interest Deductions

(c) Interest Disallowed

(d) Debt

Figure F.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 F.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.

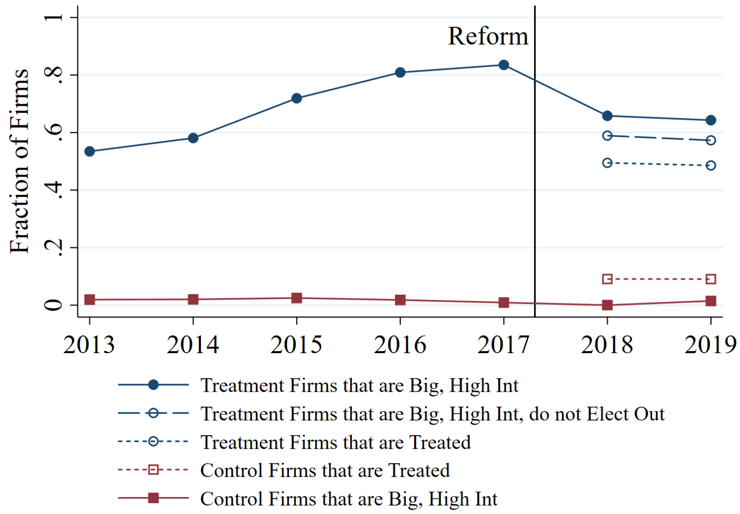
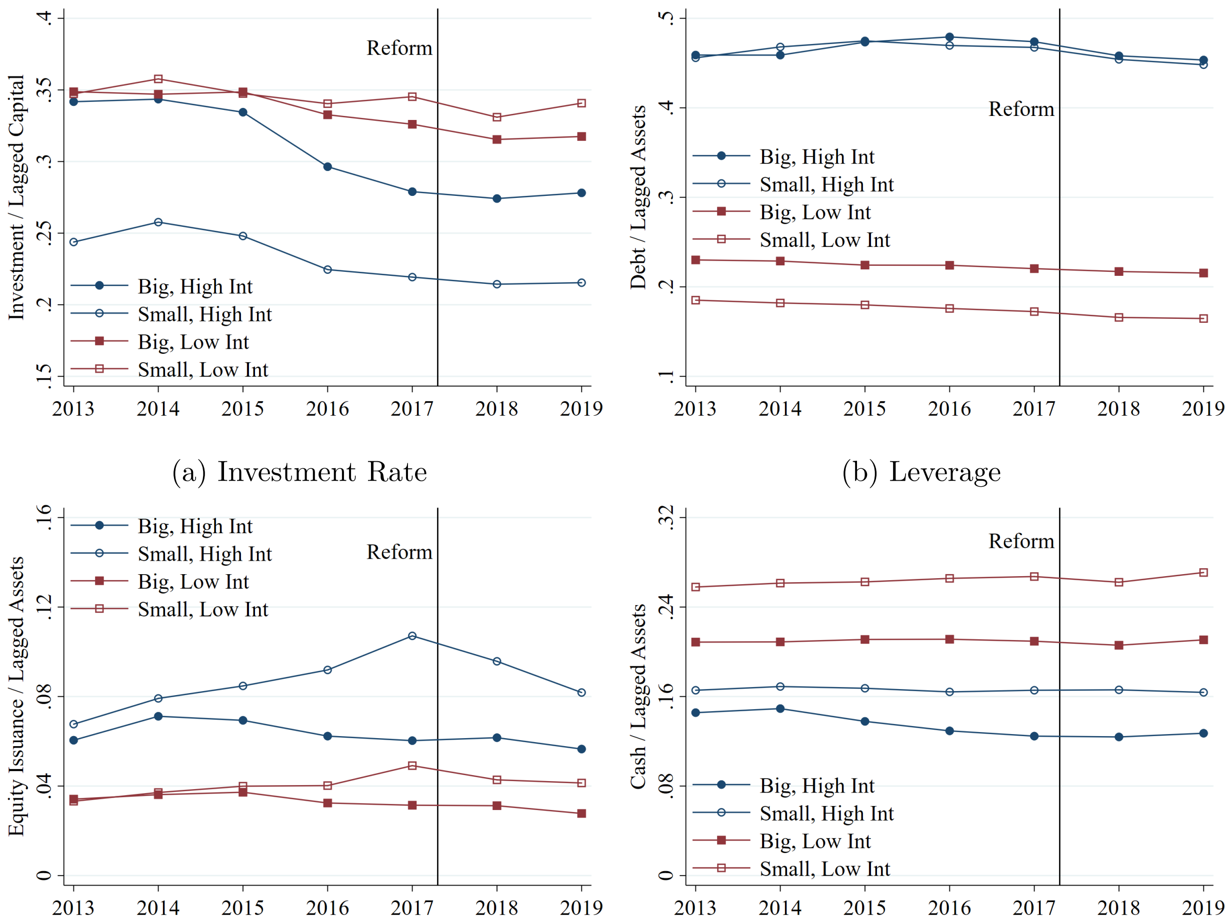


Figure F.3: Persistence of Treatment Status

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) Investment Rate

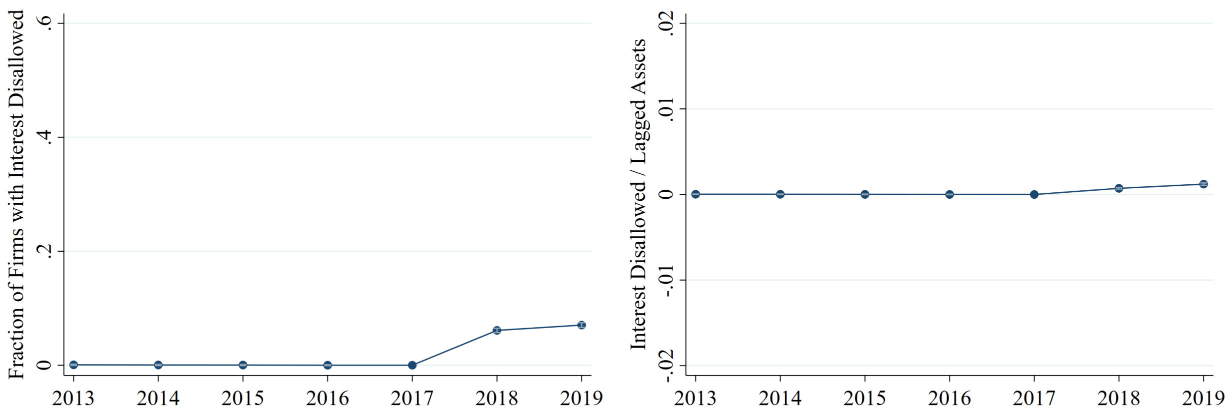
(b) Leverage

(c) Equity Issuance

(d) Cash

Figure F.4: Raw Means of Big and Small, High- and Low-Interest Firms

Notes: This figure plots raw means of investment and financing outcome variables for big high-interest firms, big low-interest firms, small high-interest firms, and small low-interest firms. Firms are defined as big if average receipts over 2015-2017 exceed $25 million and as high-interest if their interest expense exceeds their limitation averaging over 2015-2017. Panel (a) uses investment scaled by lagged capital as an outcome variable. Panel (b) uses debt scaled by lagged assets as an outcome variable. Panel (c) uses equity issuance scaled by lagged assets as an outcome variable, and panel (d) uses cash scaled by lagged assets as an outcome variable.

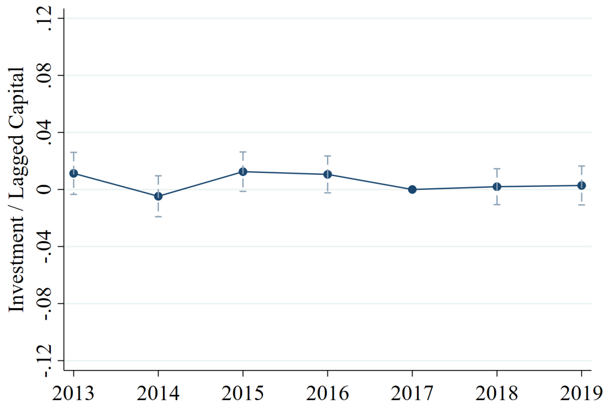


(a) Indicator for Interest Disallowed

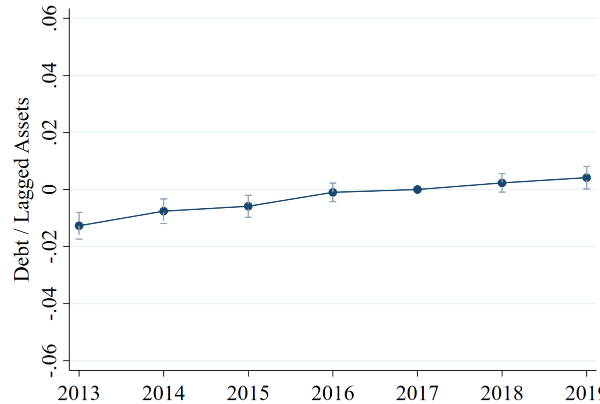
(b) Interest Disallowed

Figure F.5: First Stage Placebo Event Study Estimates

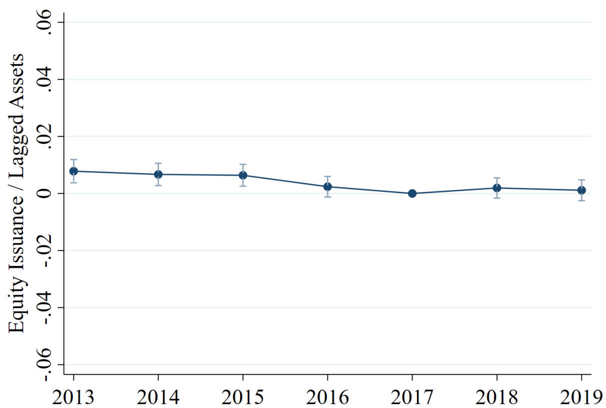
Notes: This figure plots event study estimates of β_τ from equation (2) using low-interest firms. Panel (a) uses an indicator equal to 1 if interest is disallowed as the outcome variable, while panel (b) uses interest disallowed scaled by lagged assets as the outcome variable. 95% confidence intervals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



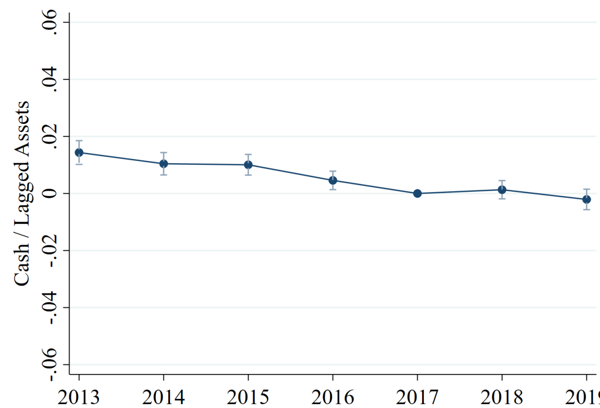
(a) Investment Rate



(b) Leverage



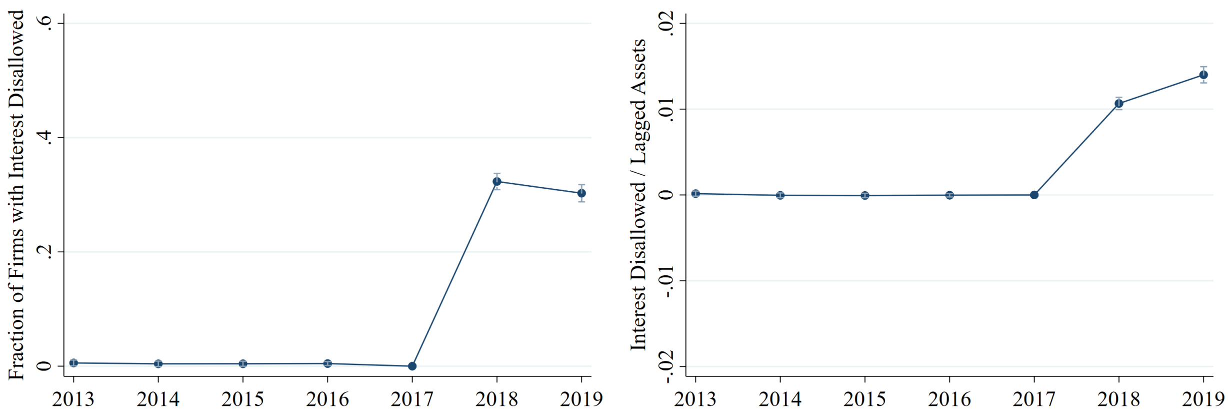
(c) Equity Issuance



(d) Cash

Figure F.6: 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 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 scaled by lagged assets as an outcome variable. 95% confidence intervals are constructed from standard errors clustered at the firm level.

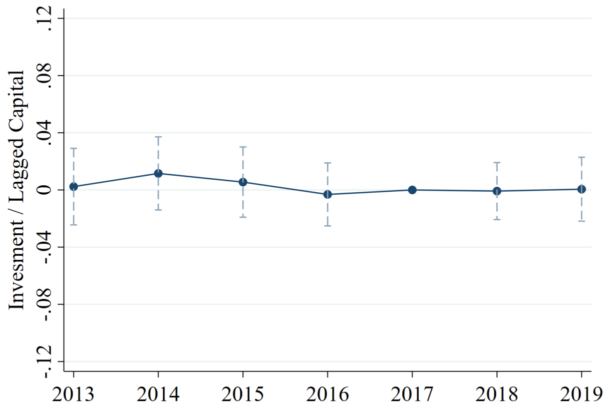


(a) Indicator for Interest Disallowed

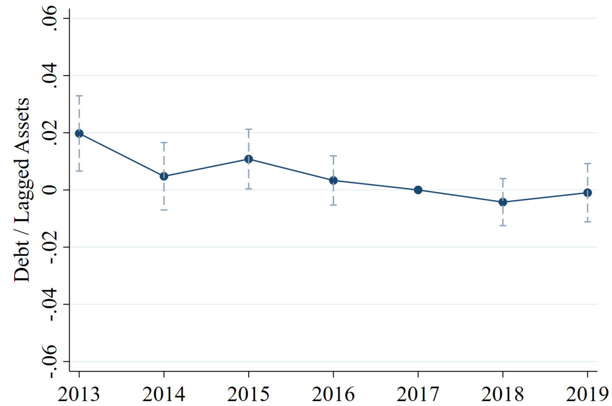
(b) Interest Disallowed

Figure F.7: Triple Difference First Stage Estimates

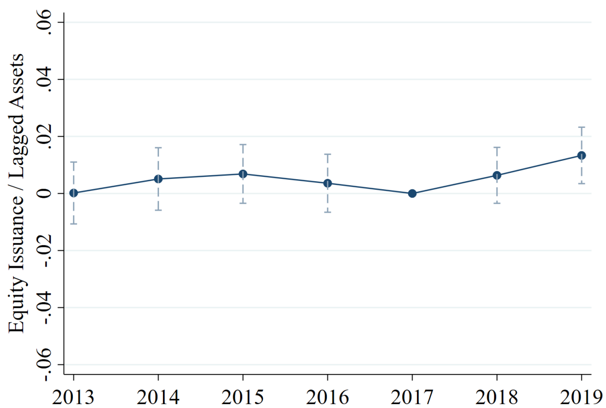
Notes: This figure plots triple difference estimates of γ_e from equation (5). Panel (a) uses an indicator equal to 1 if interest is disallowed as the outcome variable, while panel (b) uses interest disallowed scaled by lagged assets as the outcome variable. 95% confidence intervals are constructed from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



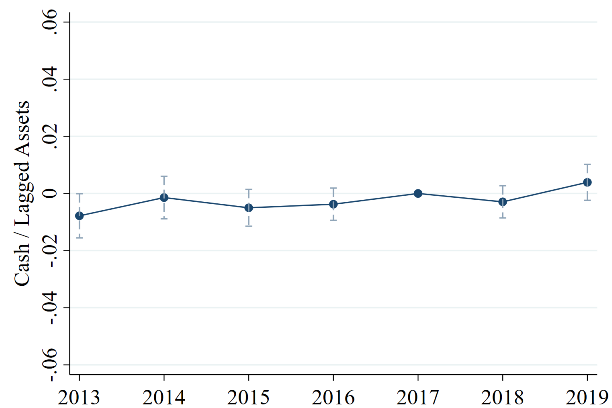
(a) Investment Rate



(b) Leverage



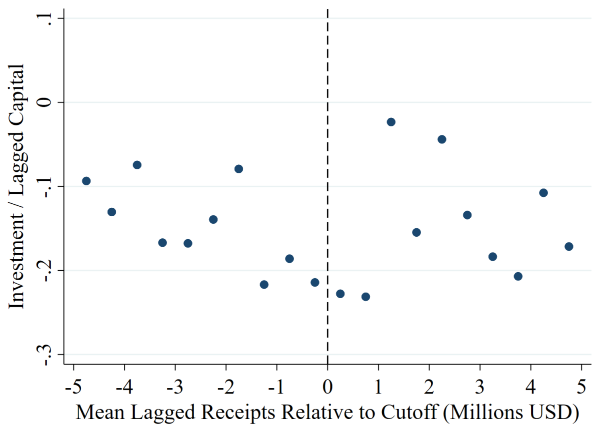
(c) Equity Issuance



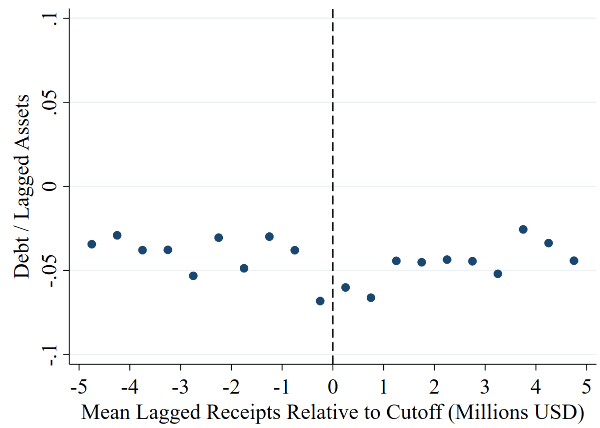
(d) Cash

Figure F.8: Triple Difference Investment and Financing Estimates

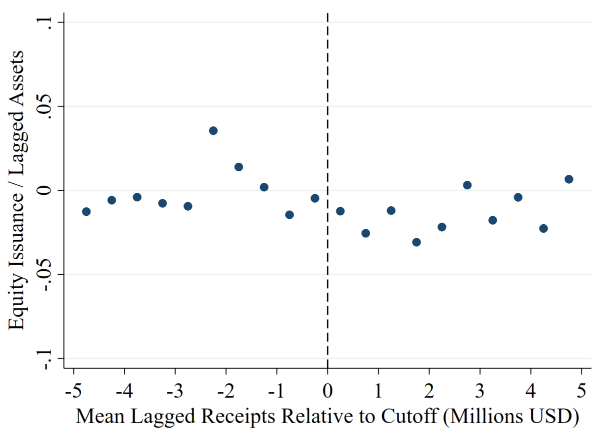
Notes: This figure plots triple difference estimates of γ_e from equation (5) using investment scaled by lagged capital, debt scaled by lagged assets, equity issuance scaled by lagged assets, and cash scaled by lagged assets as outcome variables. 95% confidence intervals are constructed from standard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



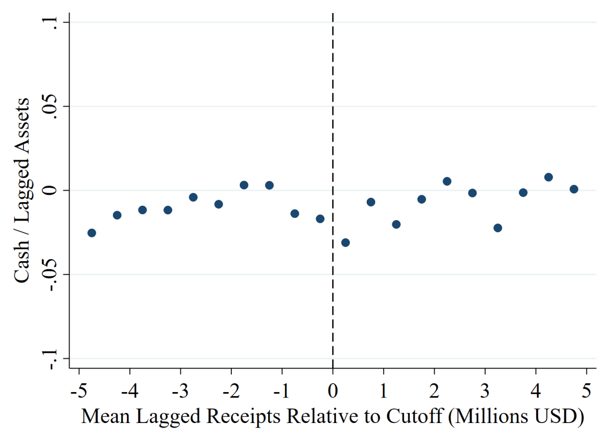
(a) Investment Rate



(b) Leverage



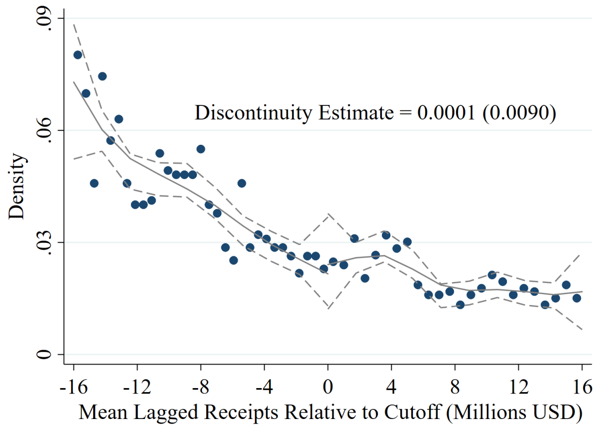
(c) Equity Issuance



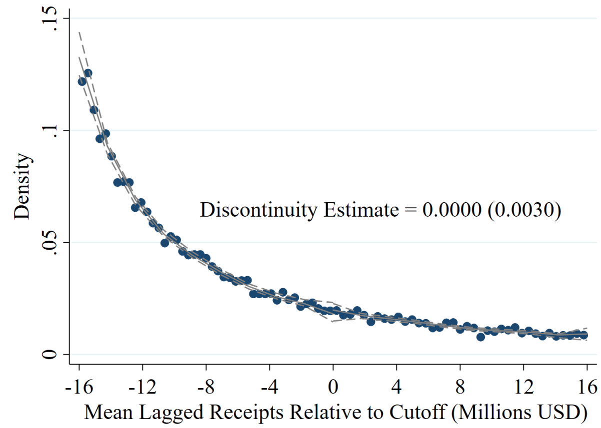
(d) Cash

Figure F.9: E-filing 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 using the E-filing data. Panel (a) displays averages for investment scaled by lagged capital, panel (b) displays average debt scaled by lagged assets, panel (c) displays average equity issuance scaled by lagged assets, and panel (d) displays average cash scaled by lagged assets.



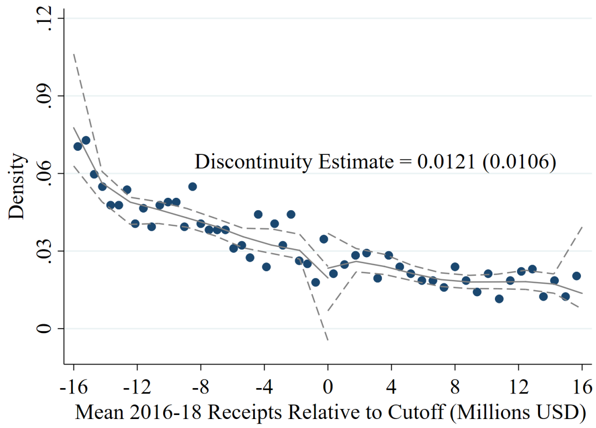
(a) SOI



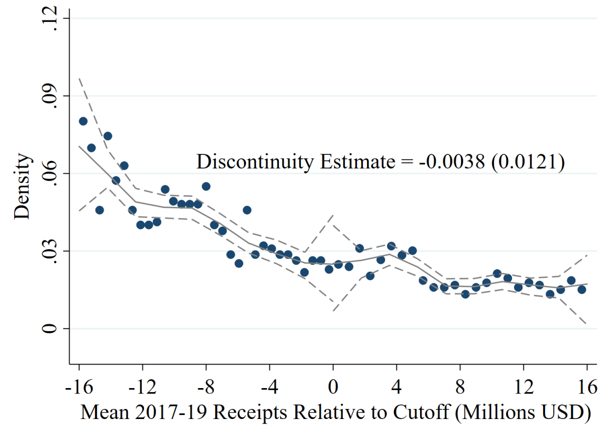
(b) E-filing

Figure F.10: Discontinuity Test

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.



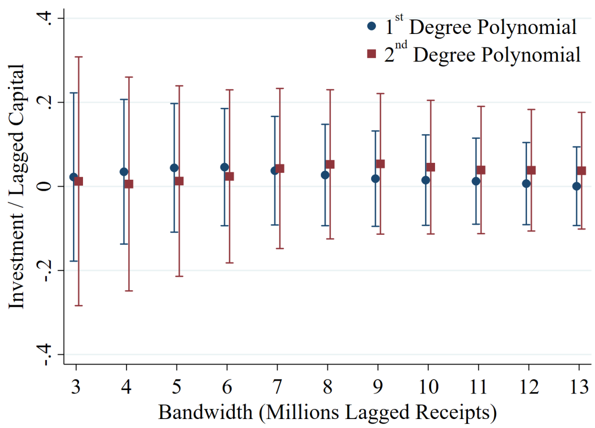
(a) 2016-18 SOI Receipts



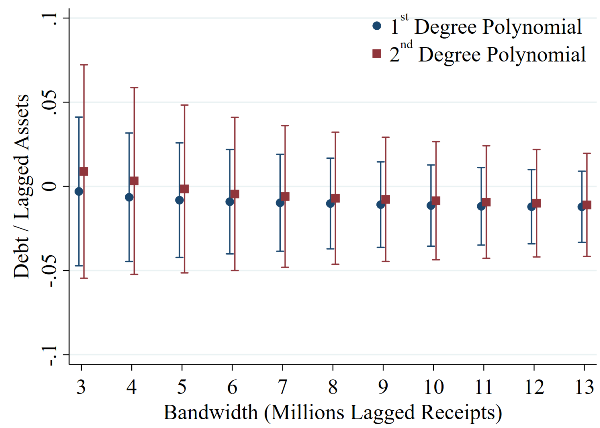
(b) 2017-19 SOI Receipts

Figure F.11: Discontinuity Test in Later Years

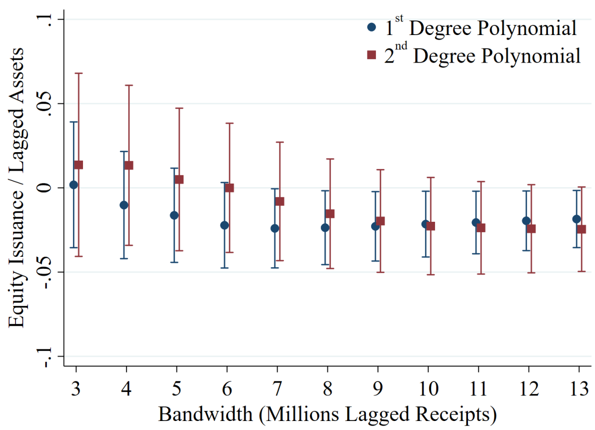
Notes: This figure reports the McCrary test for a discontinuity in the distribution density of average receipts in the SOI data over 2016-2018 in panel (a) and 2017-2019 in panel (b). Both figures use a \$26 million cutoff to adjust for inflation. Neither discontinuity estimate is statistically different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



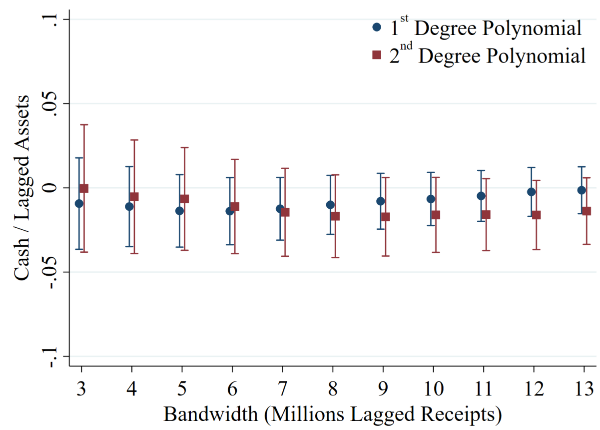
(a) Investment Rate



(b) Leverage



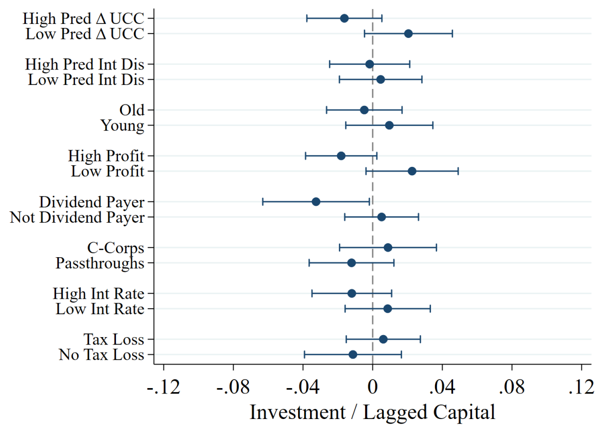
(c) Equity Issuance



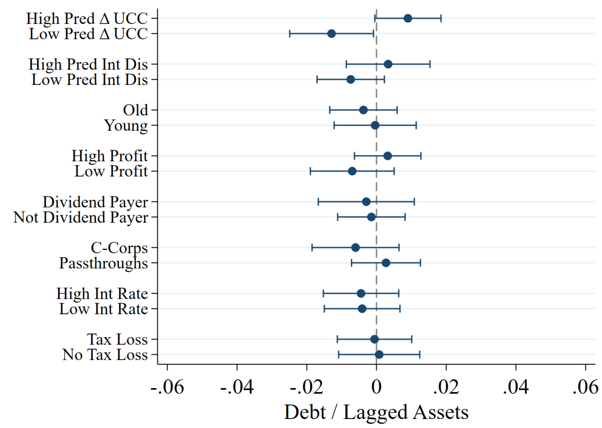
(d) Cash

Figure F.12: Regression Discontinuity Alternative Specifications

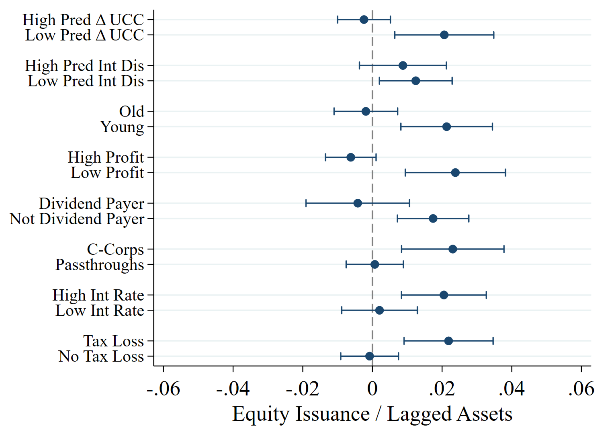
Notes: This figure plots regression discontinuity estimates of β^{RF} from equation (7) using the E-filing data and varying the bandwidth and polynomial order. Panel (a) uses investment scaled by lagged capital as an outcome variable, panel (b) uses debt scaled by lagged assets as an outcome variable,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



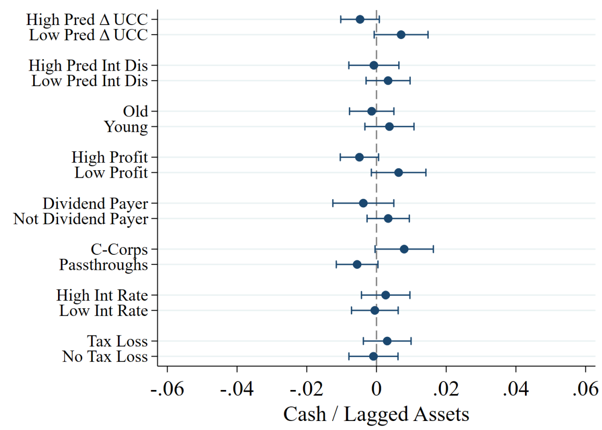
(a) Investment Rate



(b) Leverage



(c) Equity Issuance



(d) Cash

Figure F.13: Triple Difference Heterogeneity

Notes: This figure plots event study 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, and using industry-by-year fixed effects. 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 scaled by lagged assets, equity issuance scaled by lagged assets and cash 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.

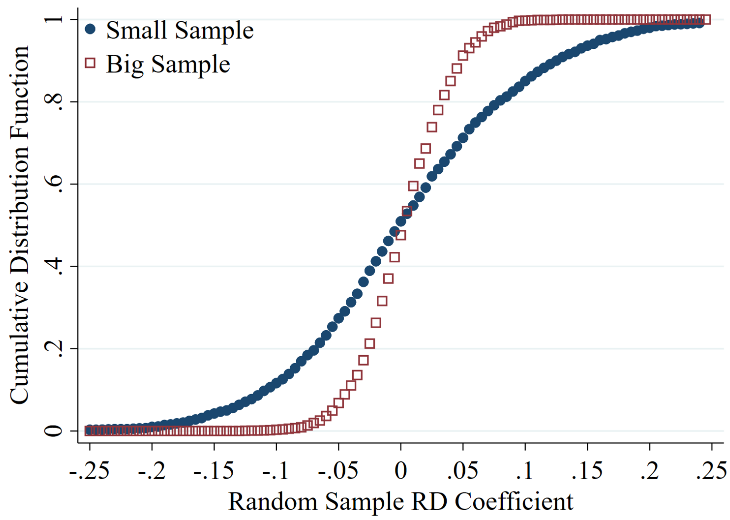


Figure F.14: Regression Discontinuity Estimates from Small Random Samples

Notes: This figure plots a CDF of 2,000 regression discontinuity estimates of β^{RF} from equation (7) using random samples of firms on each side of the $25 million cutoff from the SOI data. For each random sample we select firms with replacement within a $16 million bandwidth. The small sample series uses 71 firms on each side of the cutoff to match the 142 publicly-held firms in the RD estimation sample. The big sample series uses 500 firms on each side of the cutoff. The outcome variable for the regression discontinuity estimates is debt scaled by lagged assets.

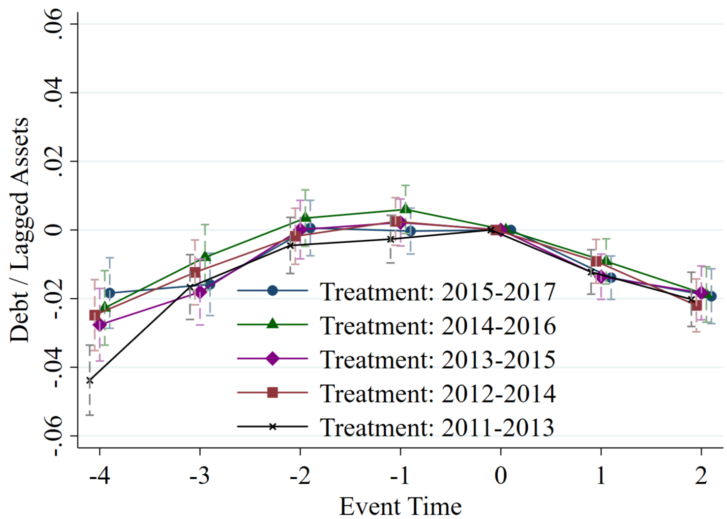


Figure F.15: Mean Reversion Around Treatment Definition Years

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.

G Appendix Tables

Table G.1: 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	117.2
Capital (Mil 2017 USD)	0.2	0.9	7.0	10.1
Investment / Lagged Capital	0.07	0.02	0.18	0.13
Debt / Lagged Assets	0.00	0.45	0.10	0.47
Equity Issuance / Lagged Assets	0.00	0.00	0.00	0.00
Cash / Lagged Assets	0.16	0.06	0.13	0.06
Payouts / Lagged Assets	0.00	0.00	0.00	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
User Cost of Capital	0.14	0.13	0.13	0.13
Age	17.5	10.5	22.5	11.0
Obs				

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 G.2: Fraction of Aggregates Across Groups

	(1) Total	(2)	(3)	(4) Fraction of Total	(5)	(6)	(7)
	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	11.81	0.01	0.02	0.70	0.27	0.63	0.37
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	66,544	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 G.3: Pass-through Responses to 2013 Individual Tax Rate Change

Dependent Variable	(1) Investment Rate	(2) Leverage	(3) Equity Issuance	(4) Cash
β_{post}	-0.013	-0.002	-0.003	-0.005
	(0.010)	(0.005)	(0.004)	(0.003)
Obs	52,004	54,756	54,756	54,756
Clusters	8,832	9,216	9,216	9,216
R^2	0.409	0.879	0.325	0.797

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 G.4: Placebo Event Study Effect on Investment and Financing

Dependent Variable	(1) Int Disallow	(2) Investment Rate	(3) Leverage	(4) Equity Issuance	(5) Cash
β_{post}	0.0009	0.002	0.003	0.002	-0.000
	(0.0001)	(0.006)	(0.002)	(0.002)	(0.002)
Obs	315,421	276,848	315,421	315,421	315,421
Clusters	52,928	46,673	52,928	52,928	52,928

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.

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

	Mean	Std Dev	P10	P50	P90	Obs
SOI Data						
Investment / Lagged Capital	0.253	0.401	0.000	0.086	0.729	1,632
Debt / Lagged Assets	0.442	0.371	0.000	0.411	0.996	1,691
Equity Issuance / Lagged Assets	0.063	0.215	0.000	0.000	0.111	1,691
Cash / Lagged Assets	0.170	0.199	0.015	0.095	0.461	1,691
E-filing Data						
Investment / Lagged Capital	0.349	0.742	0.000	0.094	0.838	20,667
Debt / Lagged Assets	0.424	0.390	0.000	0.374	0.928	20,747
Equity Issuance / Lagged Assets	0.053	0.185	0.000	0.000	0.108	20,747
Cash / Lagged Assets	0.174	0.222	0.010	0.089	0.480	20,747

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 G.6: Placebo Regression Discontinuity Effect on Investment and Financing

Dependent Variable	(1) Investment Rate	(2) Leverage	(3) Equity Issuance	(4) Cash
	Panel A: SOI Estimates			
β^{RF}	-0.031	0.021	-0.004	-0.003
	(0.045)	(0.013)	(0.010)	(0.012)
Obs	$6,798$	$7,394$	$7,394$	$7,394$
Pre-Reform Mean	0.415	0.202	0.028	0.262
	Panel B: E-filing Estimates			
β^{RF}	-0.019	-0.001	0.005	0.003
	(0.030)	(0.006)	(0.004)	(0.006)
Obs	$20,953$	$21,287$	$21,287$	$21,287$
Pre-Reform Mean	0.638	0.199	0.032	0.316

Notes: This table reports regression discontinuity estimates of β^{RF} from Equation (7) for all low-interest firms. Panel A reports estimates using the SOI data and Panel B reports estimates using the E-filing data. Robust standard errors are reported in parentheses. The SOI estimates use a bandwidth of $16 million receipts and the E-filing estimates use a bandwidth of $5 million receipts. 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 G.7: Regression Discontinuity Effect on Investment and Financing: Public Firms

Dependent Variable	(1) Investment Rate	(2) Leverage	(3) Equity Issuance	(4) Cash
β^{RF}	-0.047	0.076	-0.066	-0.037
	(0.129)	(0.107)	(0.143)	(0.102)
β^{IV}	-0.328	0.447	-0.391	-0.218
	(1.009)	(0.801)	(0.829)	(0.574)
Obs	137	142	142	142
Pre-Reform Mean	0.461	0.344	0.284	0.289
First Stage F-Stat	0.681	0.972	0.972	0.972
ITT UCC Pct Change	0.12	0.12	0.12	0.12
ε^{ITT}	-0.85	1.89	-2.00	-1.10
	(2.35)	(2.68)	(4.33)	(3.04)
TOT UCC Pct Change	0.09	0.08	0.08	0.08
ε^{TOT}	-1.18	2.63	-2.79	-1.53
	(3.26)	(3.73)	(6.03)	(4.23)

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 regression discontinuity sample. Robust standard errors are reported in parentheses. We use a bandwidth of $75 million receipts. Pre-reform means are averages over 2015-2017 for firms above the receipts cutoff. ITT and TOT UCC Pct Change is the percent change in the user cost of capital, calculated as the RD estimate of β^{RF} using mechanical (ITT) or actual (TOT) user cost 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 user cost.

Table G.8: User Cost Financing Term Regression Estimates

	(1)	(2)	(3)	(4)
Dependent Variable	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	-0.322	-0.083	-0.580	-0.147
	(0.043)	(0.008)	(0.138)	(0.027)
Obs	$82,177$	$82,177$	$64,813$	$64,813$
	Panel B: IV Estimates			
β	-0.807	-0.133	4.872	0.819
	(0.926)	(0.153)	(3.848)	(0.645)
First Stage Coefficient	0.008	0.047	0.008	0.047
	(0.001)	(0.003)	(0.001)	(0.003)
Obs	$82,177$	$82,177$	$64,813$	$64,813$

Notes: This table reports OLS and IV estimates of β from equation (10). 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 intereaction of a post reform indicator and Big_i as the independent variable.

Table G.9: Lumpy Investment and Financing Statistics

	All Firms	Small		Big	
		Low Int	High Int	Low Int	High Int
Investment					
Fraction Investing	0.76	0.62	0.62	0.91	0.91
Fraction Investment > 10% of Capital	0.48	0.36	0.28	0.65	0.58
Fraction Investment > 20% of Capital	0.34	0.28	0.21	0.45	0.39
Debt					
Fraction 0 Debt Change	0.29	0.43	0.19	0.24	0.10
Fraction Debt Change > 1% of Assets	0.24	0.17	0.26	0.28	0.38
Fraction Debt Change > 5% of Assets	0.16	0.11	0.19	0.18	0.27
Fraction Debt Change < -1% of Assets	0.31	0.27	0.43	0.30	0.38
Fraction Debt Change < -5% of Assets	0.16	0.14	0.20	0.16	0.21
Equity					
Fraction Equity Issuance > 1% of Assets	0.19	0.15	0.22	0.21	0.25
Fraction Equity Issuance > 5% of Assets	0.12	0.11	0.15	0.12	0.16
Fraction Buybacks > 1% of Assets	0.02	0.01	0.01	0.03	0.02
Fraction Buybacks > 5% of Assets	0.01	0.01	0.00	0.01	0.01
Obs	405,013	158,385	53,966	157,036	35,626

Notes: This table reports mean values for all firms and treatment and control groups from our panel data set describing the lumpiness of firm investment and financing. 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.