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EVIDENCE FROM PRIVATE FIRMS

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ABSTRACT

We use variation in state corporate income tax rates to re-examine the relation between taxes and corporate leverage. Contrary to prior research, we find that corporate leverage rises after tax cuts for small private firms. An estimated dynamic equilibrium model shows that tax cuts make capital more productive and spur borrowing. Tax cuts also produce more distant default thresholds and lower credit spreads. These effects outweigh the lower interest tax deduction and lead to higher optimal leverage choices, especially for firms with flexible investment policies. The presence of the interest tax deduction raises consumer welfare in equilibrium.

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1. Introduction

Do taxes boost or depress corporate leverage? This old question has been at the center of corporate finance at least since Modigliani and Miller (1963), which emphasizes that the benefit from the interest tax deduction is valuable and likely larger than any countervailing costs of debt related to financial distress. This trade-off has received much attention in part because it informs corporate managers' decisions on financing capital and labor. More broadly, understanding the interaction between taxes and debt is of fundamental interest, as corporate debt can affect the pass-through of monetary policy to the real economy (Ottonello and Winberry 2020), stock market risk (Gomes and Schmid 2010), and macroeconomic stability (Brunnermeier and Sannikov 2014). As surveyed by (Fleckenstein et al. 2019), most prior empirical work finds a positive relation between debt and taxes, so the question appears to be settled and in line with the idea that firms value the interest tax deduction.

We tackle this old question in two new ways and find new answers. First, we use new data on private U.S. firms to study changes in state corporate income taxes since the 1990s. We often find a negative relation between taxes and leverage, especially for smaller private firms, despite the tax deductibility of interest during our sample period. We also find anticipation effects, as companies adjust capital structure after the enactment of tax laws, which are often effective one to two years later.

While at first surprising, these results are more than simply contradictory, given that most prior research uses samples of large public companies (Titman and Wessels 1988; Graham 1996; Heider and Ljungqvist 2015; Faccio and Xu 2015). Instead, we focus on private firms, which face different trade-offs than their public counterparts, as they are less well-capitalized and face more financial frictions (Brown et al. 2021). Moreover, examining private firms expands the scope of this earlier research in an interesting direction. For example, over half of new job creation in the United States occurs in privately held companies (Smith 2007), and most world economic activity occurs in smaller, bank-dependent, privately held companies.

Second, to rationalize our results, we estimate a dynamic model of firm leverage. The intuition from the model is based on the simple idea that the tax benefit of debt is offset by two tax-sensitive costs that are not necessarily small. First, tax cuts raise firm profits and thus lower the likelihood of default. Credit spreads fall, so firms have an incentive to raise leverage. Second, a tax cut boosts the marginal product of capital. Because external equity finance is either costly or unavailable, if a firm lowers leverage in response to the lower tax benefit, it incurs an opportunity cost in terms of the lost marginal product, so the firm optimally substitutes into capital by using more leverage. Interestingly, Hundsdoefer (2023) finds results consistent with these mechanisms. He studies a change in a VAT tax that leaves the interest tax deduction intact, finding that an increase in the VAT tax reduces leverage.

Turning to the details of our analysis, our primary data set comes from the Federal Reserve's Y-14 Collection, which is a supervisory data set that covers privately held, bank-dependent firms in the United States since 2011. The data set contains not only loan details but also firm-level accounting information.

Using the Y-14 data to conduct simple event studies, we first investigate how state corporate income tax cuts affect leverage. We focus on tax cuts because tax hikes seldom occur during our sample period. We use both the enactment and effective dates for the tax changes, as they are signed into law one to two years before becoming effective. As such, an ancillary contribution of our work is the collection of tax enactment dates.

We find that small firms' leverage first responds to the tax cuts in the enactment year with no significant reaction before enactment. The event study coefficients on or after the enactment dates are precisely estimated, ranging from approximately 4% in the enactment year to about 2% four years later. This result indicates the presence of anticipation effects, and the persistence of the results also points to a permanent increase in leverage in response to tax cuts. We also show that changes in long-term debt primarily drive this rise in leverage. Moreover, we find that bank assessments of loan default rates fall by 20-30 basis points after tax cuts. This last result is important because it supports the general notion from our model that default risk and the cost of debt are also tax-sensitive.

While small firms react to tax cuts, large firms do not, as we find no significant leverage changes for large firms after corporate income tax cuts. For our sample of public firms, we also find that leverage rises slightly in response to tax cuts, although effects do not persist.

We extensively examine the robustness of the event study results. We confront the biases that can arise in settings with heterogeneous treatment effects (de Chaisemartin and D'Haultfoeuille 2020). As in Romer and Romer (2010) or Giroud and Rauh (2019), we use narrative techniques to ensure tax cuts are not endogenously related to local economic activity. We also examine alternative thresholds to define firm size or corporate taxation status. In all cases, our results remain similar to those from our main specification. Our results are also similar when we exclude firms with access to private equity markets or financially distressed firms, when we expand or contract the list of control variables, or when we account for the timing gap between effective and enactment dates.

To provide economic intuition for these somewhat unusual results, we use our data to estimate a dynamic model of leverage and investment in the spirit of Hennessy and Whited (2005, 2007). In this class of infinite-horizon models, firms choose investment and debt to maximize the expected discounted value of distributions to shareholders. They also receive an exogeous interest tax deduction. In our model, firms face an exogenous stochastic tax rate, so they optimize their policies considering the possible future evolution of corporate taxes, as in Hennessy and Strebulaev (2020). This tax benefit is offset by two endogenous tax-sensitive costs. First, because debt need not be fully collateralized, firms can default, which occurs when they have insufficient internal resources to repay their debt. A higher tax rate exerts downward pressure on optimal debt because credit spreads rise, as both default thresholds and lender recovery fall. Second, firms optimally substitute between debt and capital as a means of transferring resources through time. Because debt can be thought of as a (negative) linear technology that transfers resources through time, when capital becomes more productive, firms want to use capital relative to this less productive technology, so capital rises and negative debt falls, that is, debt rises.

Ex ante, it is not clear which of these effects is quantitatively more important. To understand this issue, we estimate the model on our samples of large and small private firms, targeting, among other moments, the sensitivity of leverage to a dummy for a tax cut. Our intent in this sample split is roughly to separate firms that face more or fewer financial frictions (Hadlock and Pierce 2010). We find that we can match the positive sign in our sample of small firms but also the marginally negative sign in our sample of large firms. The main difference between these two estimations is the magnitude of the capital adjustment costs parameter, which is several times larger for the sample of large firms than the sample of small firms. With large adjustment costs, the substitution between debt and capital is weakened, and the effect of the tax benefit becomes relatively more important.

We use our model to quantify the welfare and misallocation effects of the tax benefit of debt. We compare two economies, both with corporate taxation, but one with an interest tax deduction and one without. We find a result with a second-best flavor: welfare is higher in the economy with a tax deduction. With profit taxation but without the deduction, firms find debt less attractive and hoard cash to offset the lack of external equity finance. This siphoning of resources into cash results in a lower capital stock, which, in equilibrium, results in a lower equilibrium real wage, more hours worked, and lower consumption and welfare. Misallocation also rises when we remove the tax benefit. Intuitively, the tax benefit boosts the use of debt to finance capital and offsets the adverse welfare effects of other frictions that shape corporate financing.

Finally, we examine alternative frictions that affect managers' leverage decisions. First, in models with collateral constraints, the cost of debt is lost financial flexibility, which is tax-sensitive. Second, in our baseline model, firms cannot issue external equity. We relax this assumption by allowing the existence of costly equity. Modest equity issuance costs equity issuance rare, so the tax-sensitive cost of debt continues to matter. Third, in our model, firms default when they have insufficient liquidity to repay debt. If we alter the model to allow firms to roll over debt unless they become insolvent, they are naturally less likely to default, so the tax-sensitive cost of debt is still present. Fourth, in our data, taxes often become effective after they are enacted. To embed this feature of tax legislation in our model, we add a news shock (Jaimovich and Rebelo 2009) to the model, finding a similar effect of tax news on leverage.

Our model builds upon a large literature in corporate finance that uses dynamic models of finance and investment to understand corporate leverage policy (e.g. Hennessy and Whited 2005; Wu 2018; Gao et al. 2021). Our model is intentionally standard, containing no new ingredients designed specifically to generate the seemingly counterintuitive result of a negative sensitivity of leverage to corporate taxes. In other words, this result was already hiding in models of this class. Interestingly, our result has a related precedent in Hines and Park (2019). In this model, distortionary subsidies that favor one type of capital over another reduce firm productivity. Lenders then raise borrowing costs in response, and this effect can lower overall investment, even though spending on tax-favored capital goods might rise.

We organize the paper as follows. Section 2 describes our data, Section 3 describes our empirical methodology, and Section 4 presents our results. In Section 5, we outline our model and present the estimation results and counterfactuals. Section 6 concludes.

2. Data

2.1 Data on Corporate Borrowing

Our firm-level income statement and balance sheet data come from Schedule H.1 of the Federal Reserve's Y-14Q data collection, which started in June 2012 to support the Federal Reserve's bank stress tests (Board of Governors of the Federal Reserve System 2022). These data are quarterly, and the sample period runs from the third quarter of 2011 to the present. The dataset contains information on the loan portfolios of the 33 largest banks in the United States, which originate approximately three-quarters of all U.S. commercial and industrial lending. The Y-14Q data contain borrower financial statements, which borrowers provide to satisfy loan collateral and covenant requirements. Banks provide loan-level data on all commercial and industrial loans in their portfolio whenever a loan exceeds \$1 million in the commitment amount, together with the most recent financial statement information of the associated borrower, if available. The smallest companies in the Y-14 collection do not have financial statement data, likely because it is too costly, so they substitute tax returns, which we do not observe. Given our focus on state corporate tax changes in the United States, we restrict the sample to all loans to U.S. addresses. While these data are quarterly, most firms only report financials annually, so we keep the financial statement information with a reporting date closest to the end of each calendar year, typically the information from the fourth quarter for the previous twelve months. Because some of the entities responsible for debt repayment can be subsidiaries, we merge the entities in the Y-14 Collection into parent firms using data from the National Establishment Time-Series (NETS) Database (Walls & Associates 2020) and the Business Entity and Company Family Tree of Standard & Poor's (S&P Global 2024). These two data sets span the period from 1990 to 2017.

We restrict the sample to domestic private borrowers, excluding foreign and U.S. state and local government entities, tribal governments, special purpose vehicles, individuals and private households, and other nonprofit organizations. We exclude public firms, as we study those separately using Compustat data (S&P Global 2020). We require each borrower to report a zip code, which we use to identify the borrower location, using the zip-county crosswalk distributed by the U.S. Department of Housing and Urban Development (United States Department of Housing and Urban Development 2021). We also exclude firms that have loans guaranteed by U.S. government agencies, that are in default (non-accrual status), or that borrow through establishments or subsidiaries located in multiple states in the same quarter. This last restriction mitigates the issue that firms are taxed based on their nexus in individual states, so they might not be particularly sensitive to a change in the tax rate in their home state if their revenues are dispersed across states. We also omit financial, regulated, and government entities, as outlined in Appendix A. See Brown et al. (2021) for a more detailed description of the data set.

Although we do not observe firm tax-filing status, the small size of the companies in the Y-14 data suggests that a significant portion of firms are subject to individual taxation of pass-through income. For example, half of the firm years with financial statements in our data have less than $19 million in total assets, and three-quarters have less than $68 million in total assets. Because we focus on corporate income taxes, we screen for pass-through entities such as sole proprietorships, partnerships, and S-corporations using the following restrictions. First, we exclude borrowing entities if the obligated entity or the guarantor of the debt is classified as "Individual" or "Sole Proprietor." Second, we exclude firm-year observations in which book assets fall below $100 million in the previous year. We impose this restriction because larger companies are much more likely to benefit from choosing corporate taxation and organizing as C-corporations. While pass-through entities have a lower tax burden relative to C-corporations, Smith et al. (2022) show that other aspects of organizing as a C-corporation are more important for large firms with more than $100 million in sales; for example, no restriction on the number of shareholders or types of ownership, a lower tax burden on retained earnings, and a wider range of deductions. Imposing this restriction is likely conservative, as the Joint Committee on Taxation reports that virtually all assets of C-corporations belong to those companies whose total assets exceed $100 million (Joint Committee on Taxation 2024).

For an observation to remain in the sample, we require the availability of total asset book value. We drop observations where total assets or net sales are equal to zero or negative, as well as observations with negative cash balances, current assets, current liabilities, depreciation, interest expense, tangible assets, or fixed assets. We also require the beginning-of-period book value of total assets to be available, as we use this variable to scale all financial variables. The resulting sample has 39,363 non-singleton firm-year observations from 2011 to 2017, where we truncate the end of the sample because we examine the dynamic effects of tax changes, as explained in Section 3, below. We winsorize all financial statement variables at the 1^{st} and 99^{th} percentiles to mitigate the effect of extreme observations.

While the Y-14 data provide some detail on firm-level accounting variables, its time period is limited. Therefore, we also use the Shared National Credit (SNC) data, which spans 1992 through the present (Board of Governors of the Federal Reserve System and Office of the Comptroller of the Currency and Federal Deposit Insurance Corporation 2020). While the SNC dataset has a longer time series, and while it has detailed data on loan commitment amounts, maturity, and credit utilization, it has the disadvantage that it does not contain firm-level balance sheet data.

The SNC program covers all syndicated deals that meet two criteria: they exceed $20 million ($100 million since 2018), and they are held by three or more unaffiliated institutions supervised by the Federal Reserve System, Federal Deposit Insurance Corporation, or the Office of the Comptroller of the Currency (OCC). Before 1999, this second inclusion criterion required deals to have at least two supervised unaffiliated lenders. The loan deals in this data collection comprise nearly the entire syndicated loan market in the United States in terms of loan amounts. We aggregate loan commitment amounts to the borrower level, and we include both the amounts of credit line sizes and term loan commitments. We restrict this sample to domestic firms and exclude government entities, utilities, financials, public administration entities, religious institutions (in terms of either 2-digit NAICS and the SNC industry definitions), and firms that have defaulted on their debt and firms that are either in non-accrual status or have "troubled-debt" restructurings. The resulting sample contains 50,203 non-singleton firm-year observations from 1993 to 2018 that have at least two consecutive years of data and over $100 million in loan commitments as of the prior year.

Our sample of public firms is from the CRSP-Compustat Fundamentals Annual database (Center for Research in Security Prices 2020). We use a sample period that starts in 1989 to conform to the sample in Heider and Ljungqvist (2015) and goes through 2017 to be comparable with the sample of private firms from the Y-14 data. Because a firm's headquarters location in Compustat is backfilled to its most recent headquarters location, we use a file provided by Standard & Poor's that contains the correct historical headquarters locations since 1994 ("CST_HIST", S&P Global (2020)). We exclude utilities, financials, and public administration entities (2-digit SIC code 49 and 1-digit SIC codes 6 and 9). We also exclude foreign firms (identified as having historical headquarters or incorporation locations outside of the United States), firms with missing headquarters locations, firms with negative or missing total assets, and firms with missing pre-tax earnings. The resulting sample has 91,314 non-singleton firm-year observations. The variable definitions for the CRSP-Compustat, SNC, and Y-14 samples are in Appendix A.

2.2 State Taxation and Economic Data

To identify the effective dates of state corporate taxes changes, we use the dataset on top statutory state corporate income tax rates since 1975 and other tax base characteristics assembled by Serrato and Zidar (2018a).[1] Although our data start in 1989, we use earlier tax changes to help calibrate the tax process for our model. For each tax change, we collect the corresponding enactment date, that is, the date the tax change became law. These two dates need not be the same. As an extreme example, Indiana approved a tax package in 2011 that lowered corporate income taxes from 8.5% to 6.25% between 2013 and 2017 (a 0.5% reduction in years 2013, 2014, 2015, and 2016 and a 0.25% reduction in 2017). In this example, the Indiana tax package enters our sample only once, with 2013 as the effective year and 2011 as the enactment year. Out of the 125 tax legislation packages since 1975, 79 become effective in the same year or retroactively, 33 become effective in the following year, and 13 become effective in two or more years. For several of the packages that are effective immediately, their rates gradually increase or decrease for up to five years.

We identify all corporate income tax changes since 2012 from The Tax Foundation (2020) and gather the corresponding enactment dates from the statutory compilation of all states (LexisNexis 2020). We collect data on all enactment dates prior to 2012 from amendments to the states' tax statutes. Specifically, we obtain electronic copies/scans of the tax statutes from each state's legislature/legal library. We then read through the statutes to identify the relevant corporate income tax rate changes and record the respective enactment dates, typically the date the state's governor signs the legislation.

Finally, we gather data on the effective dates of state sales tax rates and individual income tax rates between 2000 and 2020 from the Tax Policy Center (2023) and The Federation of

[1] Serrato and Zidar (2018b) collect these data from a variety of sources including Council of State Governments (2011), CCH Incorporated (2017), Bernthal et al. (2012), Chirinko and Wilson (2008), and Wilson (2009). For more details on the construction of this dataset, please refer to Serrato and Zidar (2018b).

Tax Administrators (2020), respectively, as well as information on Census Bureau Regions and Divisions from U.S. Census Bureau, Population Division (2017). We obtain annual data on state and county GDP as well as county population, employment, and per-capita income from the U.S. Bureau of Economic Analysis (United States Bureau of Economic Analysis 2020, 2019a,b). We also obtain monthly data on state unemployment rates from the U.S. Bureau of Labor Statistics (United States Bureau of Labor Statistics 2024), daily data on Aaa and Baa corporate bond yields from Moody's (Moody's 2024a,b).

2.3 Descriptive Statistics

We describe our sample of firms from the Y-14 data in Panel A of Table 1. The median company has $288 million in book assets, and the interquartile range is from $166 to $681 million. In addition, sample firms have total debt-to-assets of approximately 38% and long-term debt-to-assets of 31%. Both measures of leverage exhibit substantial variability. In comparison, as seen in Panel B, our sample of public firms from Compustat contains firms that are, on average, much larger, less leveraged, and less profitable. In addition, public firms have fewer fixed assets relative to their size, on average.

Appendix Figure B1 presents the distributions of size and leverage of the Y-14 and Compustat firms. While both samples contain a large fraction of firms with assets less than $500 million, there are many more very large firms in Compustat. Next, while both samples contain substantial fractions of firms with zero leverage, the Y-14 sample contains a much larger fraction with levels of elevated leverage.

Panel C of Table 1 describes the sample of firms from the SNC. Most of the firms in this sample are private and obtain most of their external financing through bank borrowing. We observe that these firms have a significant amount of slack in their credit lines. The median amount of loan commitments is $325 million dollars, but for the median firm-year, we see only about 51% of these commitments used.

Panel D summarizes the state corporate income tax hikes and tax cuts that become effective from 1987 to 2017. The 74 tax legislations constitute 24 tax hikes and 50 tax cuts. Tax hikes increase corporate income taxes by an average of 1.39%, while tax cuts reduce taxes by about 1.22%, where these figures include the cumulative effect of staggered tax legislation. Prior to the effective date of tax changes, state corporate income tax rates are higher in states with subsequent tax hikes than in states with tax cuts. For example, initial corporate income tax rates average 7.93% in tax hike states and 6.89% in tax cut states.

3. Empirical Approach

We estimate the effects of tax changes on firm leverage with difference-in-differences event study regressions of the form:

$$y_{it} = a_i + b_{mt} + \sum_{k=-2}^{k \geq +4} \bar{c}_k \mathbf{1}\{K_{it} = k\} + \boldsymbol{\gamma X} + \epsilon_{it}, \tag{1}$$

where i, m, t, and k denote firms, industries, years, and years relative to the event of interest, respectively. Dates preceding the event correspond to $k < 0$, and $k \geq 0$ corresponds to dates after the event, with $k \geq +4$ representing four or more years after the event. The dummy variables $\mathbf{1}\{K_{it} = k\}$ are leads and lags of the event indicator, so the corresponding dynamic treatment effects are given by $\bar{c}_k$. The outcome of interest is given by y_{it}, $\boldsymbol{X}$ is a vector of control variables described below, and $\bar{\alpha}_i$ and $\bar{\beta}_{mt}$ are firm and industry-year fixed effects. Given the inclusion of firm fixed effects, $\bar{\alpha}_i$, the event study estimates represent deviations from the average level of the outcome of interest for a given firm. As noted in Borusyak et al. (2024), this specification is valid under the assumption that the treatment effects are homogeneous across units and calendar time, depending only on k.

The model in equation (1) is identified up to a linear trend (Borusyak et al. 2024). One identifying assumption is the specification of a base year before which no pre-trends are present. We define this omitted category as years $k \leq -3$ relative to the event. We choose this lag length because tax legislation can be enacted one or two years before becoming effective. If firms react to enactments, then in our analysis of tax-effective dates as events, the lagged treatment effects might enter (1) with significant coefficients, so an omitted category greater than -3 would result in omitted variable bias.

Equation (1) is preferable to a standard difference-in-differences specification, such as:

$$y_{it} = \bar{\alpha}_i + \bar{\beta}_t + cD_{it} + \boldsymbol{\gamma X} + \epsilon_{it}, \tag{2}$$

where D_{it} is the interaction term dummy variable. Specifically, although the estimator in equation (1) is less efficient than that in (2) (Borusyak et al. 2024), equation 2 is only valid under the restrictive assumption that the $\bar{c}_k$'s in equation 1 are all equal for $k > 0$. This requirement means that the treatment leads to an immediate and permanent jump in the outcome variable and no further effects. If this assumption is violated, the estimate of c in equation (2) is biased and difficult to interpret because c is the weighted average of $\bar{c}_k$'s in equation 1, and not all the weights need to be positive (de Chaisemartin and D'Haultfoeuille 2020; Goodman-Bacon 2021; Borusyak et al. 2024). This work explains that bias in c stems from using post-treatment periods to provide counterfactuals for earlier periods.

The vector of controls, $\boldsymbol{X}$, differs for each of our three datasets because of data limitations.

For the regressions using the Y-14 data, we include firm internal credit rating fixed effects, which range from AAA to D, and the log of lagged firm sales. We include firm and 4-digit NAICS industry-by-year fixed effects. For our regressions using Compustat data, we include firm and 4-digit SIC industry-by-year fixed effects, as well as lagged levels of log total assets, the ratio of net income to assets, the ratio of property, plant, and equipment to total assets, and the market-to-book ratio. For the regressions using either Compustat or the SNC data, we include lagged levels and changes in the state unemployment rate and log GDP. For the Y-14 regressions, we also include county-level lagged levels and changes in log employment, per-capita income, log population, and log GDP.

Some of our controls are common across all three data sets. Following Serrato and Zidar (2018b), we control for the structure of the corporate tax base using the fourteen following measures: an indicator of having throwback rules, an indicator of having combined reporting rules, an indicator for whether the R&D tax credit applies to an incremental base that is a moving average of past expenditures, an indicator for whether the R&D tax credit applies to an incremental base that is fixed on a level of past expenditures, the number of years for loss carrybacks, number of years for loss carryforwards, an indicator for franchise taxes, an indicator for federal income tax deductibility, an indicator for the federal income tax base as the state tax base, an indicator for federal accelerated depreciation, an indicator for accelerated cost recovery system (MACRS) depreciation, an indicator for federal bonus depreciation, and sales and payroll apportionment weights. We extend these tax base measures through 2017 by collecting information from CCH Incorporated (2017) and LexisNexis (2020). Our final control is top statutory state individual income tax rates, which we obtain from The Federation of Tax Administrators (2020).

One potential concern with our tests is that contemporaneous changes in state individual income taxes might drive corporate leverage. For example, the documented leverage effects could be attributed spuriously to changes in consumer spending triggered by changes in personal taxation. Prior literature has demonstrated that reductions in state personal income taxes increase personal wages and employment, thereby leading to higher personal disposable income (Zidar 2019). Therefore, individual income tax cuts are likely to lead to higher consumer spending and, consequently, to higher firm investment opportunities and borrowing. Conversely, individual income tax hikes lead to reductions in firm investment opportunities and lower leverage ratios. To account for these possibilities, we also include event study indicators associated with state individual income tax cuts from $t = -2$ through $t \geq 4+$ for all of our specifications.

4. Results

4.1 Corporate Tax Cuts and Corporate Leverage

For our first set of tests, we use the regression specification in equation (1) to examine the relationship between corporate leverage and state corporate income tax cuts. We use the sample of private firms from the Y14 data spanning 2011–2017 and data on all state corporate income tax cuts since 2001. While this baseline specification does not include tax hikes, we control for the incidence of tax hikes throughout our specifications, and we examine tax hikes separately below.

For this analysis, we split the sample at the median value of lagged total assets ($288 million), with small firms defined as those with below-median total assets and large firms constituting the rest of the sample. This sample split intends to capture the different financial frictions that small firms face relative to large firms. While financial frictions are unobservable, Hadlock and Pierce (2010) shows firm size is one of the most informative predictors of external financial constraints. Anticipating, this sample split also provides an interesting contrast for our model to reconcile.

Figure 1 presents the effects of tax cuts on leverage outcomes in event time, with additional details in Appendix Table B1. We examine two main leverage measures: total debt and long-term debt, each of which is scaled by beginning-of-period total assets. The x-axis of each panel contains the time since the event of the tax cut enactment. The dots represent the coefficient point estimates, $\bar{c}_k$, in (1). The outer bars are the 90% confidence interval, and the inner bars are the 95% confidence interval.

Several results are of interest. First, as seen in Panel A, for our sample of small firms, we find significant positive responses of long-term leverage to tax cuts in event years zero through 4+. Small firms' long-term debt-to-assets ratios rise by almost 4% in the enactment year and remain elevated relative to pre-event levels four or more years later. Although our sample period is short, this evidence is consistent with the notion that leverage revisions are permanent. To understand the economic significance, we note that the average tax cut during this period was 1.2%. Given that private firms in the United States paid an average tax rate of 21% (Zwick et al. 2016), this change represents approximately a 6% reduction in the tax bill. The final coefficient of .021 in year four also implies a change in leverage of about 6%. This large response is likely a product of the low interest-rate environment during our sample period. A small change in the tax bill implies a large change in marginal q, and a large response of both investment and the debt needed to finance it.

Second, in Panel B, we find that when we measure leverage using total debt, we obtain a positive significant coefficient at the event date, but insignificant coefficients thereafter. This result makes sense because total debt contains short-term loans and revolving lines of credit, which are used to fund shorter-term expenses such as working capital. Third, in Panels C and D we present the equivalent results for our sample of large firms. In neither case do we find significant coefficients, and in neither case does this result stem from wider confidence intervals. Finally, we do not detect any significant pre-trends for either small or large firms.

The dynamics evident in our results challenge two important alternative interpretations. First, one could imagine that the effects upon enactment and prior to the effective dates of tax changes are simply the reactions of firms taking advantage of the interest deduction before the tax cut. However, this hypothesis is inconsistent with the persistence of the leverage responses. Second, it is possible that our results are a product of credit supply effects, with banks shifting lending to states in which their own revenues are taxed at a lower rate. However, this hypothesis is inconsistent with the effects we see before the effective dates, as banks would have no incentive to shift lending to states before the new tax code becomes relevant. In contrast, risky lending rates can immediately impound expectations of future firm default probabilities that are induced by tax cuts. These rate shifts then affect firm incentives to borrow. We provide more direct evidence of this hypothesis below.

In Figure 2, we repeat the analysis in Figure 1, but we use effective instead of enactment dates of legislation, as in Heider and Ljungqvist (2015). Again, details are in Appendix Table B2. The estimates in these specifications are noisy and often statistically insignificant. For example, for long-term debt, in Panel A, we find a significant positive effect in the subsample of small firms only at event date two. In contrast, we find no significant effects in Panel B for long-term debt or in Panels C and D, which correspond to the event studies for our subsample of large firms. We conclude that firms respond to enactments rather than the effective dates of tax cuts.

Next, we examine how tax cuts and hikes affect public firm leverage. For this analysis, we use data from Compustat for two subsamples: the same sample period that we use for the Y-14 data, and a sample period that extends from 1989 to 2011, as in Heider and Ljungqvist (2015). Figure 3 shows the evolution of the book values of long-term debt scaled by total assets. We find no significant treatment effects for either the large or small firms in the early sample. For the late sample, we find a significant positive response in year two for tax cuts and several significant positive responses for tax hikes. However, in this latter case, we also observe significant pre-trends, so the results are difficult to interpret. Overall, evidence of an effect of taxes on leverage for public firms is scant.

Our results starkly contrast with the empirical relations documented in the prior literature. For example, Titman and Wessels (1988), Heider and Ljungqvist (2015), and Faccio and

Xu (2015), all document that taxes are positively related to corporate leverage. A notable difference between these studies and ours is that we rely on samples of smaller privately held firms that face more financial frictions than public firms. Another difference between our work and the most closely related of these studies, Heider and Ljungqvist (2015), is our use of firm fixed effects versus their use of first differences to control for firm heterogeneity, which they rationalize as important in a staggered design, as this study precedes the literature on staggered difference-in-differences designs (Goodman-Bacon 2021).

In support of the notion that size and public status matter for the leverage-tax relation, our result of a negative relation is not without precedent, as Farre-Mensa and Ljungqvist (2015) also find a negative association between changes in book leverage and taxes in a sample of small private firms.

In sum, our results present two stylized facts to understand more deeply. First, the leverage of small private firms responds positively to enactments of tax cuts. Second, large private firms' leverage does not react to tax cuts.

4.2 Real Effects of Tax Cuts

Tax cuts are also likely to have real effects via a direct positive effect on the marginal product of capital and a fall in the cost of capital resulting from changes in default thresholds, as argued in Section 1. In Figure 4, we examine the response of capital expenditures to the tax enactment dates, with the details in Table B4. In Panels A and B, we consider investment scaled by tangible assets for small and large firms, and in Panels C and D, we similarly consider investment scaled by total assets. For both investment measures, for small firms, we find positive significant responses between 0.015 and 0.021 at the enactment date. In both cases, we also find slightly small, but still significant responses in event year three. In contrast, we find no reaction of investment to the tax enactment dates for the large firms. These results are likely weaker than our leverage results because of time-to-build lags and investment lumpiness (Doms and Dunne 1998), so changes materialize with some delay. Moreover, we cannot observe important components of investment, such as intangibles, in the Y-14 data, so we miss these potential effects. In sum, our results are consistent with the findings in Zwick and Mahon (2017) and Ohrn (2018) that the investment of small firms is more sensitive to tax incentives and that low-tax environments are associated with greater investment incentives.

This evidence of real effects is important. We argue below that models without endogenous investment, such as those based on the framework in Fischer et al. (1989), cannot generate a negative relation between taxes and leverage, so evidence that taxes affect real outcomes lends credence to our choice of a theoretical framework below in which factor demand is endogenous.

4.3 Alternative Specifications

Next, we examine the robustness of our results to several concerns, including heterogeneous treatment effects, equity issuance, the political economy of tax legislation, our thresholds for defining C-corporations or small firms, and several additional specification issues.

As states enact tax legislation at different points in time, and as the effects of this legislation differ across states, we investigate the robustness of our results to variation in treatment timing. Borusyak et al. (2024) show that in settings such as ours, treatment effect heterogeneity can bias event study estimates. They propose an estimator that imputes the fixed effects estimated from untreated observations onto treated observations. This estimator thus has an intuitive interpretation and deals with the problem of negative weights that arises in the presence of heterogeneous treatment effects. We use this technique to reestimate our main specification, which uses the sample of small private firms, enactment dates, and long-term debt. In Appendix Table B5, for the small firms, for event years 0–3, we find significant estimates that are similar to our baseline results in Figure 1. For the large firms, we again find small, insignificant coefficients.

We also consider an estimator from de Chaisemartin and D'Haultfoeuille (2020, 2021) that is not affected by treatment effect heterogeneity and is based on variation from firms that switch to treatment (tax enactment) for the first time. In Appendix Table B6, we find similar effects on leverage of .036 for small firms and no effect for large firms in the tax cut enactment year. We do not find any dynamic effects after the enactment year because the technique requires a complete panel for switchers in all three years following tax enactment, and we have a low number of remaining first-time switchers.

An important issue in our analysis is the endogeneity of the tax cuts, which can occur when politicians respond to changes in the economy with tax policy. While we do not find evidence of anticipation effects or pre-trends around tax enactments, these political economy concerns would nonetheless undermine the parallel trends assumption we make in our difference-in-differences analysis. Intuitively, if a tax cut happens during an economic boom when the government surplus is large, observed leverage changes might be due to the economic boom. To alleviate these concerns, we use the narrative approach from Romer and Romer (2010); Giroud and Rauh (2019), which argues that tax changes are less likely to be endogenously related to economic activity if the primary objective of the tax legislation is to achieve a long-run goal or to deal with an inherited budget deficit or a surplus. Using this classification, we find that many of the tax changes in our sample can be classified as exogenous, with the only exceptions being Connecticut in 2011 and 2015, New York in 2014, and Rhode Island in 2014. Appendix Table B7 shows that our leverage estimates are slightly larger when we exclude these states. We conclude that the leverage effects in Figure 1 are unlikely to be driven by political economy concerns.

Next, we examine more closely whether the leverage changes in Figure 1 result from equity adjustments instead of borrowing. This issue is important because our model is one of debt adjustments, so if our results are affected by equity adjustments, then a modification of our theoretical framework is in order. We consider this possibility unlikely for two reasons. First, since 2009, we observe firms raising finance through private equity markets in only 1,950 out of the 39,363 firm-year observations. Second, we redo the analysis for small private firms and long-term debt in Figure 1, except we exclude firm-year observations belonging to firms that ever have access to private placement markets. In Columns (1)–(4) of Appendix Table B9, we find similar event study estimates.

A further concern is that distressed companies might not have taxable profits, so our results might be an artifact of the tax advantage of debt not being relevant for a portion of our sample. We redo our analysis after excluding companies with bank internal ratings of "CCC" and lower. As shown in Columns (5)–(8) of Appendix Table B9, our results are once again similar when we impose this restriction.

One limitation of our analysis is our inability to identify the tax status of individual firms. We address this issue in two ways. First, we change the C-corporation thresholds for inclusion in the sample from \$100 million in prior-year assets to \$75 or \$50 million. Second, we use information from the Statistics of U.S. Businesses (SUSB) Annual Data Tables, which show the share of companies organized as C-corporations, S-corporations, sole proprietorships, and LLCs by NAICS industry and size bucket. We then exclude the five NAICS two-digit industries in which the SUSB data indicate that pass-throughs (S-corporations, LLCs, and partnerships) constitute significantly more than 50% of the firms with at least 500 employees: 61, 62, 71, 72, and 81. As we show in Appendix Table B9, lowering the cutoff for C-corporations results in tax coefficients of the same sign as those in Figure 1, which contains our baseline results. However, both the size and significance fall. This result makes sense, as lowering the size cutoff introduces more pass-through entities into the sample. Next, in columns (5)–(8), we show that removing the low-C-corporation industries makes our baseline coefficients in Figure 1 slightly larger.

Next, the results in Appendix Tables B10–B14, we consider several further specification choices. First, using the sample median to define small firms is arbitrary, so we use alternative size thresholds for small firms of \$150, \$200, \$250, or \$300 million in assets. Second, we include additional control variables typically used in leverage regressions, such as profitability, tangibility, and sales growth. Third, we consider the case of no controls. Fourth, we add dummy variables for corporate-tax and sales-tax effective dates to our specifications. Fifth, we consider two alternative measures of leverage: the log of debt and debt scaled by end-of-period assets, instead of current-period assets. Sixth, we include Census-region by year fixed effects to control for regional trends that might be correlated with tax cuts. In all but one case, we find similar results. The exception is the sample of small firms defined by the $150 million cutoff, for which a small sample size means we find insignificant results.

Next, we consider the issue that U.S. corporations pay state corporate taxes to all states with which they have "nexus," so the amount they owe to each state depends on apportionment formulas and the location of employment, property, and sales across states. Because we cannot merge the confidential Y-14 data with the administrative data necessary to construct nexus, we consider isolating those firms that operate in a single state. We take two approaches. First, if either the NETS or the S&P Cross Reference Database identifies a company as multi-state in any year, we exclude it from the sample. Second, we exclude firms that borrow through subsidiaries located in multiple states during the entire sample period, according to the Y-14 data. Appendix Table B15 shows that these sample restrictions produce results that are similar to our baseline findings in Figure 1, with positive responses of leverage for small firms and with no evidence of a response in large firms.

One issue with our event studies is the use of dummy variables for tax changes. This choice implies that small tax changes influence our results as much as large changes and possibly drive our results. To investigate this possibility, in Table B16, we present results when we limit the tax cuts that are at least 0.5% and at least 1%. Not surprisingly, when we consider these smaller sets of more significant events, we find slightly stronger effects with patterns of significance that are similar to those in our baseline results in Figure 1.

Next, we consider tax hikes. In Appendix Table B17, we repeat our baseline specification for the large and small Y-14 firms. In column (1), we find that hikes result in a fall in leverage for small firms in event year 4. In column (2), we also find falls in leverage in event years 1, 2, and 4 for large firms. We find similar results in columns (3) and (4) when we omit a tax hike used only to define a pre-trend and states with tax hikes not characterized as exogenous, according to the methodology in Giroud and Rauh (2019). While these results confirm our general intuition, they are less credible than our baseline results in Figure 1 because we do not have enough tax hike observations to identify pre-trends.

This result prompts the question of whether the effects of tax cuts and hikes are symmetric. Because the Y-14 sample period is short, we turn to the data on firms' syndicated bank borrowing from the Shared National Credit Program, which spans 1992 through 2018. Our dependent variable is the log of total syndicated commitments to a given firm in a given year. Because there are no balance-sheet variables in the SNC data, we cannot scale commitments by total assets, so we use the log. Table B18 indicates that total firm commitments increase around corporate tax cuts, as in our main specifications. However, these results are slightly smaller in magnitude, and with one exception, of marginal significance. In contrast, we find large and signification reductions in borrowing following tax hike enactments, with the average firm lowering commitments by 6–7%. Because the relative magnitudes of hikes and cuts over this period are roughly the same, we conclude that tax hikes have a larger effect. This result makes intuitive sense, as reducing debt incurs few, if any, costs, while increasing debt often entails costs. However, these results are also less robust to specification choices because we cannot observe firm size or firm-level controls.

5. Model

To understand the underlying economics behind the empirical patterns in our data, we turn to a dynamic model of an equilibrium economy. We first establish a baseline model, estimate its parameters, and analyze the optimal policies. We finish by exploring several different assumptions about the stringency and types of financial frictions in the model.

The economy consists of a representative consumer and a unit continuum of firms. The economy also contains a government and a financial intermediary, but these entities simply act as pass-through agents for the firms and the consumer. There are no aggregate shocks to consumers or firms in the economy.

Each of the infinitely lived firms uses capital and labor in a stochastic, decreasing returns-to-scale technology to generate output, y, according to

$$y = z^{\nu} \left(k^{\alpha} n^{1-\alpha}\right)^{\theta}, \tag{3}$$

where k is the stock of capital, n is labor, z is a productivity shock, α is capital's share, and θ governs the degree of returns to scale. We normalize the parameter ν to be $1-(1-\alpha)\theta$. In addition to this basic technology, we assume that the firm has a fixed component of operating costs, which we denote as f. The productivity shock, z, is lognormally distributed and follows a process given by:

$$\ln(z') = \rho \ln(z) + \sigma_z \varepsilon', \qquad \varepsilon' \sim \mathcal{N}(0,1), \tag{4}$$

where a prime indicates the subsequent period, and no prime indicates the current period.

Investment in capital, I, is defined by a standard capital stock accounting identity:

$$k' \equiv (1-\delta)k + I, \tag{5}$$

in which δ is the rate of capital depreciation. The price of the capital good has been normalized to one. Adjusting the capital stock incurs quadratic costs that take the form:

$$AC(k,k') = \frac{\psi(k' - (1-\delta)k)^2}{2k} \tag{6}$$

where ψ is a parameter that governs the magnitude of adjustment costs.

Taxation in our model is simple, as there is only corporate taxation at a stochastic rate τ, which follows an autoregressive process given by:

$$\tau' = \mu(1-\rho_\tau) + \rho_\tau \tau + \sigma_\tau u'. \tag{7}$$

where μ, ρ_τ, and σ_τ are the mean, serial correlation, and innovation standard deviation of the tax process. u' has a truncated normal distribution, which ensures the tax rate stays positive. This tax rate applies both to profits and financing activities, as described below.

The firm can finance its optimal investment program with external debt or internal equity in the form of retained earnings. We let p denote the stock of net debt, so $p > 0$ indicates that the firm has debt on the balance sheet, and $p < 0$ indicates that the firm has cash on the balance sheet. We assume that debt is raised through a competitive financial intermediary sector, which in turn raises the necessary funds from the representative consumer. Debt is a one-period discount bond on which the firm can default. Let the interest rate on debt be $\tilde{r}(k', p', z, \tau)$, so debt proceeds are $p'/(1+r(k', p', z, \tau)(1-\tau))$.[2] As outlined below, this interest rate is determined endogenously from the lender's zero-profit condition and is consequently a function of the model's state variables. If the firm opts to save instead, it earns the after-tax risk-free rate, r, with the interest taxed at a rate τ. Thus, the interest rate on debt can be expressed as:

$$r(k', p', z, \tau) = \begin{cases} \tilde{r}(k', p', z, \tau) & \text{if} \quad p > 0 \\ r & \text{if} \quad p \leq 0 \end{cases} \tag{8}$$

Cash flows to shareholders, $e(k, p, n, k', p', z, \tau)$, are then the firm's after-tax operating income plus net debt issuance, minus net expenditure on investment, and minus tax-deductible interest payments on debt, as follows:

$$\begin{aligned} e(k, p, n, k', p', z, \tau) &= (1-\tau)\left(z^\nu \left(k^\alpha n^{1-\alpha}\right)^\theta - wn - f\right) \\ &- (k' - (1-\delta)k) - AC(k,k') + \frac{p'}{1 + r(k', p', z, \tau)(1-\tau)} - p, \end{aligned} \tag{9}$$

where w is the wage rate, which is determined in equilibrium.

While a positive firm cash flow is distributed to the consumer, we assume that negative

[2] This formulation assumes that the firm takes the tax deduction when it issues the debt. While not in accord with real-world debt contracts in which the tax advantage accrues only later when interest is paid, this assumption reduces the state space and simplifies the default condition (Strebulaev and Whited 2012).

cash flows are not allowed, that is:

$$e(k, p, n, k', p', z, \tau) \geq 0. \tag{10}$$

This assumption eliminates external equity finance. Because our sample firms are private, they have no access to public equity markets and limited access to private equity markets, so this assumption reflects the tailoring of the model to our sample. Regardless, we relax the assumption below when we examine the robustness of the model.

The Bellman equation for the problem can then be expressed as:

$$\pi(k, p, z, \tau) \quad = \quad \max_{k', n, p'} \left\{ e(k, p, n, k', p', z, \tau) + \frac{1}{1+r} \mathbb{E}\pi\left(k', p', z', \tau'\right) \right\}, \tag{11}$$

subject to (5) and (10).

5.1 Loan contract

We assume that a perfectly competitive financial intermediary sector offers the firm a one-period loan contract that need not be fully collateralized, so the firm can default. We follow Gilchrist et al. (2013) and Michaels et al. (2019) by assuming that the firm's future market value is not collateralizable, so it defaults if it does not have enough resources on hand to repay its debt. While this assumption is particularly apt for our sample of smaller private firms, we examine below how the model behaves when we adopt a more lenient default condition in which lenders can extend credit as long as the firm has a positive present value (Hennessy and Whited 2007; Gao et al. 2021).

Default is triggered when debt repayment exceeds the firm's current after-tax profit plus the fraction, $1 - \xi$ of its capital that can be recovered in default:

$$(1-\tau)\left(z^{\nu}\left(k^{\alpha} n^{1-\alpha}\right)^{\theta} - wn - f\right) + (1-\xi)(1-\delta)k \equiv R(z, \tau, k, p) < p. \tag{12}$$

We subtract the wage bill from output in (12) because labor is paid in full, even if the firm defaults. Also, taxes get paid before the lender can recover any payments. Both of these assumptions follow absolute priority rules. Finally, because the tax deduction is taken when the firm issues debt, it is absent from this condition. For fixed levels of (k, n, p), (12) defines a region, denoted by Ω, over the joint domain of (z, τ) in which default occurs.

Given this default threshold, the contractual interest rate, $\tilde{r}(k', p', z, \tau)$, is determined by a zero-profit condition that must hold under free entry in the intermediation sector. The payoff to the lender outside of default is simply this contractual interest rate. Inside default, the lender recovers an amount equal to the left side of equation (12). Thus, under free entry and risk-neutrality, the face value of debt discounted at the risky rate, $\tilde{r}(k', p', z, \tau)$, must equal the expected payoff discounted at the risk-free rate. Therefore, $\tilde{r}(k', p', z, \tau)$ satisfies:

$$\frac{p'}{1+\tilde{r}(k', p', z, \tau)} = \\ \frac{1}{1+r}\left[\int_{\Omega}\Big(R(z', \tau', k', p')\Big)\, dG\left(z', \tau' \mid z, \tau\right) + \left(1 - G\left(\Omega \mid z, \tau\right)\right) p'\right]. \qquad (13)$$

For a given (p', k', z, τ), equations (12) and (13) pin down the loan contract.

5.2 Equilibrium

The economy also contains an infinitely-lived representative consumer who chooses consumption and labor each period to maximize the expected present value of her utility, with a discount factor of b. Her one-period utility function is given by $\ln(c) + \varphi(1 - n_s)$, in which c is consumption, n_s is the supply of labor, and φ is a parameter that governs the utility of leisure. She has three sources of income: labor, dividends, and the net tax revenue generated from the firms, T, which the government transfers to the consumer as a lump sum. Her one-period budget constraint is given by:

$$c + p'_d - p_d(1+r) = wn_s + \bar{e} + T, \qquad (14)$$

in which p_d is consumer wealth, and $\bar{e}$ is aggregate firm dividends.

Let ζ be the stationary distribution over the firm's states, (z, τ, k, p). We define equilibrium in this economy as follows.

Definition 1 *A competitive equilibrium consists of (i) optimal firm policies for capital, labor, and debt, $\{k', n, p'\}$, (ii) allocations to the consumer of consumption, c, and labor, n_s, and (iii) prices, (w, r), such that:*

1. *All firms solve the problem given by* (11)*.*
2. *The consumer maximizes her utility, subject to* (14)*.*
3. *The labor, bond, and output markets clear.*

$$n_s = \int n d\zeta \qquad (15)$$

$$p_d = \int p' d\zeta \qquad (16)$$

$$c = \int (y - I - AC) d\zeta. \qquad (17)$$

5.3 Solution

We solve the model using policy-function iteration and bisection, which yields an equilibrium wage and interest rates, a value function, $\pi(k, p, z, \tau)$, and policy functions for capital and debt, given by $k'(k, p, z, \tau)$ and $p'(k, p, z, \tau)$. In all model simulations, a defaulting firm is replaced with a viable random replacement.

5.4 Estimation

We estimate the model parameters using our sample of small firms from the Y14 data. We set a subset of our parameters outside the model to simplify computation. First, we set the consumer's discount factor equal to 0.96. Second, we set $\varphi = 2.5$, which implies that the consumer spends approximately one-third of her time working. Third, we set the average tax rate, μ in (7) equal to 0.2, following Nikolov and Whited (2014). This rate is lower than the statutory national rate because it accounts for personal taxes, as in Graham (1996). Finally, we set the capital share parameter equal to 0.3.

Next, we estimate the standard deviation and serial correlation of the tax process (7) using a low-dimensional simulated method of moments procedure. We simulate data from (7) and then match the following two targets in the simulated and real data: the average absolute value of the tax changes and the average length of the spell between tax changes. Because the estimation of this last quantity can be affected by truncation bias, we ensure that this source of bias is the same in the simulated and real data by simulating the tax process in equation (7) 10,000 times, with each simulation having the length of our sample period. We estimate ρ_τ to be 0.662 and σ_τ to be 0.022.

We estimate the remaining parameters (θ, σ, ρ, δ, ψ, ξ, and f) jointly by minimizing the distance between a list of targets constructed from model-simulated data and those computed with actual data. In this estimation procedure, we use the optimal weight matrix, as in Bazdresch et al. (2018), clustered by firm and year. Appendix B provides variable definitions for our actual data. In our simulated data, leverage, operating profits, and investment are given by p/k, $(z^\nu (k^\alpha n^{1-\alpha})^\theta - wn - f)/k$, and $(k' - (1-\delta)k)/k$.

We choose the following nine moments to match. The first six are the means and standard deviations of debt, investment, and operating income, all expressed as a ratio of assets. We also include the serial correlation of investment and operating income, which we calculate using the method in Han and Phillips (2010) to account for firm fixed effects. The last moment is an estimate of the coefficient in a two-way fixed effects regression of leverage on tax-change event dummies, with a cut coded as 1 and a hike coded as -1, to correspond to our empirical specifications, so a positive coefficient implies that a cut boosts leverage.

We discuss both global and local identification. For the former, we consider the sensitivity of moments to model parameters. As such, this exercise informs the behavior of the model but not the data. While all of the model parameters affect all of the moments, some of these moments are particularly useful for parameter identification. In Appendix Figure

B2, we plot the relations in the model between selected moments and parameters, with the baseline parameters from the small-firm estimates in Table 2. First, we see that the mean and standard deviation of investment help identify the capital depreciation rate, δ, and the adjustment cost parameter, ψ, respectively. In this class of models, steady-state investment rises with the depreciation rate, and the variance of investment naturally declines as quadratic adjustment costs induce more smoothing. Accordingly, the serial correlation of investment also helps identify ψ. The serial correlation, ρ, of the process for z is directly related to the estimated serial correlation of operating income, and all model variances increase with the standard deviation, σ, of the driving process, with the effect on the variance of operating income being the strongest. Mean operating income is mechanically decreasing in the profit function curvature, θ, and in the fixed cost of production, f. Nonetheless, we can separately identify these two parameters because the variance of operating profits is also mechanically decreasing in θ, while this variance is largely invariant to the fixed cost of production. Finally, leverage is useful for identifying the ξ, as higher values of ξ lead to lower recovery in default and thus lower leverage.

We also consider a local measure of identification from Andrews et al. (2017). This diagnostic reflects both the precision of the data moments and local versions of model-based moment-parameter sensitivities. However, it often does not reflect the underlying economics of the model because the diagnostic reflects nonlinear combinations of model sensitivities and data covariances. In Appendix Figure B3, we present a heat map representing this diagnostic. Each cell can be interpreted as the effect of a one standard deviation change in a moment on a parameter. The color coding ranges from dark purple (-0.02) to dark green (0.02). While all parameters are influenced by at least one moment, the production function curvature, θ, is influenced by nearly all of the moments, while the depreciation rate, δ is only influenced by the mean and serial correlation of investment.

Table 2, Panel A reports the model parameter estimates for our samples of small and large firms. All but one of the parameter estimates are significantly different from zero, with the exception being the adjustment cost parameter for the small firms. The estimates of the profit function curvature, depreciation rate, and the standard deviation and serial correlation of the driving process are within ranges typically reported for this class of models (Bazdresch et al. 2018). The fixed operating costs, f, for both estimations are around 0.15, amounting to just under half of steady-state operating profits. Overall, the parameter estimates for the large and small firms are quite similar, with two important exceptions. The estimates of the deadweight loss in default are 0.32 and 0.6 for the large and small firms, respectively, indicating elevated financial frictions for the small firms. Although seemingly high, these estimates are in line with the average recovery rate found in Kermani and Ma (2023). In addition, the estimate of the adjustment cost parameter for the large firms is nine times larger than the estimate for the small firms.

Table 2, panel B reports the model and data moments. With one exception, the simulated and data moments are insignificantly different from each other. This pattern is unsurprising, as the model is only overidentified by two degrees of freedom. In particular, we match the coefficient in a two-way-fixed-effects regression of leverage on a tax change dummy. For neither the large nor the small firms is the moment difference significantly different from zero. Moreover, the model matches both the sign and economic magnitude of these coefficients. This result is important because it implies that the model can generate either a positive or negative response of leverage to a tax cut, depending on the parameter values.

The model also roughly matches the coefficient from regressing debt issuance scaled by assets on leverage, which is an untargeted moment that characterizes leverage dynamics. For small firms, the coefficient in the model is -0.25, while in the data, it is -0.64. For large firms, the model coefficient is -0.13, while the data coefficient is -0.47. These coefficients approximate persistence, as they are roughly one minus a coefficient in an $AR(1)$ model. Thus, while in both cases, the model sensitivity is smaller, it captures the modest leverage persistence seen in the data.

5.5 Optimal Policies

Given these two parameterizations, we now explore the aspects of the model that produce different sensitivities of leverage to taxes. First, we explore the behavior of optimal investment, I/k, book leverage, p'/k', and market leverage, V'/k', which we depict in Figure 5, in which Panels A–C corresponds to the parameterization from the small private firms, and Panels B–F corresponds to the large private firms. Below we refer to these parameterizations using the terms "small firms" and "large firms." Each plot depicts one of these policies as a function of the productivity shock, z, and each line corresponds to a different tax rate (0.12, 0.20, 0.29). In all cases, the optimal policies are averaged over current leverage, p/k, and current capital, k, using the model steady-state distribution, ζ.

First, by comparing Panels A and D, we see that small-firm investment responds more to the z than large-firm investment. If the log shock moves from -0.25 to 0, investment rises by around .11 for the small firms at the median tax rate and .05 for the large firms. This response is a product of the much larger estimate of capital adjustment costs for large firms.

In Panels B and E, for both the large and small firms, optimal book leverage rises with the productivity shock, consistent with the evidence of procyclical debt issuance from Covas and Den Haan (2011). As capital productivity increases, firms find it optimal to transfer resources through time via capital rather than a storage technology, which in our model is $-p$. Equivalently, when productivity rises, optimal investment outlays outpace internal resources, and the firm opts for debt finance. While leverage policy rises with z for both model parameterizations, for the large firms, a higher tax rate produces slightly higher optimal debt (Panel E), while for the small firms, a higher tax rate optimally results in markedly lower leverage (Panel B). This result in the model is consistent with our own empirical evidence that the negative relation between debt and taxes is negative for small firms and slightly positive for large firms.

Next, comparing Panels B and C shows that the responses of market versus book leverage to the tax rate and the z shock are nearly identical for the small firms. For the large firms, the responses of market and book leverage to the tax rate are similar. However, for the large firms, market leverage falls with z. When z rises, capital and leverage respond weakly, but value rises more sharply, so the ratio of debt to the market value of the firm falls.

Part of the intuition behind the difference between the small and large firms in Figure 5 stems from the upward slope of the book-leverage policy functions. A fall in the τ shock makes capital more attractive relative to a storage technology ($-p$), just as a rise in the z shock does. For the model parameterization from the sample of small firms, this effect outweighs the increase in the tax benefit because these firms face small capital adjustment costs. For the model parameterization from the sample of large firms, this effect does not outweigh the tax advantage, as there is little substitution between capital and a storage technology in the face of high capital adjustment costs.

Further intuition behind the difference between the small and large firms in Figure 5 can be found in Figure 6. Panels A and C depict for the small and large firms, respectively, the endogenous after-tax risky interest rate on debt, $\tilde{r}(k', p', z, \tau)$, as a function of leverage for the median size firm, with each line corresponding to a different tax rate. The effect of leverage on the contract is intuitive, as higher inherited leverage in any period raises default risk and raises the risky rate.

In both cases, the effect of taxes depends on the level of leverage. When leverage lies below .47 for the small firms and .74 for the large firms, debt is safe, and the after-tax risky interest rate mechanically falls as the tax rate rises, reflecting the interest tax deduction. However, for higher levels of leverage, the relation between taxes and the after-tax risky interest rate reverses. When leverage is high, the after-tax interest rate rises monotonically in the tax rate, as a higher tax rate makes firms less profitable and more likely to default. This lower default threshold is, in turn, capitalized into the risky rate.

Panels B and D of Figure 6 show the relation for the median-leverage firm between the after-tax risky loan rate and the capital stock, for the small- and large-firm estimations, respectively. As seen in Panel B, for the small firms, the firm experiences higher equilibrium rates when it has low capital, but in Panel D, we find no such relation for the large firms. The high rates in Panel B occur because of the fixed cost of production. Low-capital firms are less likely to be able to cover the fixed cost and are more likely to default, so the risk premium is higher.

More generally, Figure 6 shows that for the model based on the large firm estimates, the firms in the model are unlikely ever to be in a range in which a higher tax rate results in a higher after-tax loan rate, while for the model parameterization base on the small firms, this event is a possibility and contributes to the negative effect of taxes on leverage.

In sum, two ingredients in our model offset the baked-in tax benefit and allow leverage to respond negatively to taxes. First, any tax-induced changes in default thresholds are capitalized into lending rates. Second, an increase in the corporate tax rate reduces the marginal product of capital, so firms find it optimal to lower both capital and the debt needed to finance its accumulation.

5.6 Comparative Statics and Counterfactuals

We examine how the sensitivity of leverage to a tax change responds when we change four parameters in the model: the volatility and serial correlation of the tax process in (7), the capital adjustment cost, and the deadweight cost of default. The results from this exercise are in Figure 7, where each panel is constructed by starting at the parameterization of the model from the sample of small firms. Then we let the parameter in question take on 10 values between the endpoints indicated on the x-axis of each panel, solve the model for each value, and plot the tax sensitivity from each solution. Recall that this sensitivity is computed so that a positive sign implies that a tax hike reduces leverage.

In Panel A, we find that the volatility of the tax process has a strong positive effect on the tax sensitivity. This effect is mechanical, as higher volatility implies that the tax changes are larger, so a regression on a dummy variable produces a larger coefficient. In Panel B, we find that this sensitivity rises sharply with the serial correlation of the tax process, especially for high levels of serial correlation. Intuitively, firms react more strongly to taxes when they expect the changes to persist. In Panel C, we see a sharp negative relation between the tax sensitivity of leverage and the parameter that represents the deadweight costs of default. When these costs are low, firms use much more debt and are likely to be close to the region in Figure 6 in which the risky after-tax interest rate responds positively to the tax rate. In Panel D, we see a negative relation between the tax sensitivity of debt and capital adjustment costs. For very high adjustment costs, this sensitivity flips sign so that a cut implies lower leverage, as a standard textbook model would imply. This result underlies the intuition behind the wrong-way sensitivity of leverage to taxes in Figure 5. When adjustment costs are low, firms easily substitute between capital and a storage technology ($-p$), so this effect outweighs the tax benefit. When adjustment costs are high, this substitution is dampened substantially, and the effect of the tax benefit is stronger.

Next, we consider the misallocation and welfare implications of removing the tax benefit of debt altogether while leaving in place two frictions that also shape corporate leverage policy: profit taxation, and no access to external equity finance.[3] The outcome of this experiment is not immediately clear because we are comparing two second-best outcomes, both of which contain distortionary frictions. For each economy, we compute consumer welfare, and the measure of capital-labor misallocation from Hsieh and Klenow (2009).

For the model parameterization based on the sample of small firms, when we remove the tax deduction, leverage falls from 0.36 to −0.26. This large fall is consistent with our model's overall negative effect of corporate taxation on leverage because we remove only the benefit of debt, leaving all costs intact. The misallocation measure worsens from 0.89 to 0.88, and the equilibrium effect on welfare is the same in percentage terms. Without the tax benefit of debt, firms hold net cash instead of net debt. This alternative use of internal resources shrinks the capital stock and, therefore, output. In equilibrium, hours worked rise, and both the real wage and consumption fall, causing a drop in consumer welfare of approximately 1.8%. This experiment emphasizes the importance of the effect of tax incentives on a firm's optimal substitution between debt and capital.

5.7 Model extensions and alternatives

In this section, we consider three different extensions of the baseline model in Section 5. The goal is to understand whether and how the results in Figures 5 and 6 depend upon the exact form of financial frictions in the model. We also consider a separate class of models.

First, we examine a model in which debt is constrained by collateral. Specifically, we replace equations (12) and (13) with a condition that prevents debt from surpassing a collateralizable fraction of the capital stock, as follows:

$$p \leq (1-\xi)(1-\delta)k \tag{18}$$

In (18), we have redefined the parameter ξ to be the fraction of the capital stock that cannot be used as collateral. In this case, the firm never defaults, and the interest rate on debt is the risk-free rate and thus invariant to firm size or leverage choices.

Second, we consider an alternative model in which we allow debt to be risky by reinstating equations (12) and (13), but we allow the firm to issue costly equity. Specifically, we replace

[3] See Glover et al. (2015) for a related experiment that focuses on firms' default outcomes. We do not consider the 2017 Tax Cuts and Jobs Act, which capped interest deductibility at 30% of income instead of eliminating it. Carrizosa et al. (2023) show that only 15% of U.S. public firms were affected by this provision.

the constraint in (10), with a condition that redefines negative cash flows to shareholders as $e(k, p, n, k', p', z, \tau)(1 + \lambda)$, in which λ is a linear cost of equity issuance. Because we have no data on private placement issuance costs for small firms, especially the implicit costs for those that never issue, we set $\lambda = 0.11$ (Hennessy and Whited 2007).

Third, we consider an extension where there is no external equity issuance, and default occurs only when firm value equals zero. Specifically, we replace equation (12) with:

$$\pi(k, p, z, \tau) < 0 \tag{19}$$

We estimate these three alternative models using our sample of small firms. The results are in Appendix Table B20. In all three cases, we find parameter estimates comparable to those from our baseline estimation. In Appendix Figure B4, we plot the policy functions from these three alternative models, finding in all cases a positive relation between the productivity shock and leverage and a negative relation between the tax shock and leverage, as in our baseline results in Figure 5. For the model with an equity issuance cost, a linear cost of 0.11 results in rare equity issuance, so it is not surprising that the behavior of a firm that rarely issues equity is similar to that of a firm that issues no equity. More generally, in all three alternative models, a fall in taxes makes capital more productive relative to a linear storage technology, just as in the baseline model. Moreover, in the two models with default, the effect of taxes on the default premium is similar. In the model with a collateral constraint, as explained in Rampini and Viswanathan (2013) and Li et al. (2016), the cost of debt is lost future financial flexibility, which manifests itself in the model as the expectation of the future Lagrange multiplier on the constraint in (10). Because this expectation depends on optimal policies chosen by the firm, it is naturally tax-sensitive, with higher taxes making the loss of financial flexibility more costly.

Next, we consider an extension of our model that addresses the differences in effective and enactment dates in our data. The baseline version of the model contains taxes that become effective immediately, yet in our data, 37% of the tax acts take effect at least one year after the legislation passes. To incorporate delayed taxation into our model, we replace (4) with a process that contains a news shock as in Jaimovich and Rebelo (2009):

$$\tau' = \rho_\tau \tau + \sigma_\tau u' + \sigma_v v, \qquad v, u' \sim \mathcal{N}(0, 1). \tag{20}$$

In this process, a realization of v today directly affects tomorrow's tax rate, τ'.

To understand how leverage responds to the news shock, v, we set the variance and serial correlation of the process for τ equal to their baseline values. Because we do not have a long enough time series on state corporate taxes to estimate a news shock variance, set the relative variances of u' and v so that conditional on $|u'| > |v|$, the mean of $\Delta\tau'$ is 1.15 percentage points larger than the mean of τ' conditional on $|u'| \leq |v|$, which is the difference in our data between the average anticipated tax change and the average unanticipated tax change. Specifically, we set the ratio $\sigma_v^2/(\sigma_u^2 + \sigma_v^2) = .579$. We then solve this augmented model and plot the policy functions for leverage in Panel D of Appendix Figure B4. We find that a higher news shock realization results in lower leverage, as in our baseline model estimated on the sample of small firms. The magnitude is smaller because the variance of the news shock is smaller than the variance of the tax change.

Next, we consider the response of leverage to taxes in a different class of dynamic models: those based on the frameworks in Fischer et al. (1989) and Leland (1994). In these contingent-claims models, the value of debt depends on the value of the default put option, and the optimal choice of leverage reflects default. Specifically, we consider the dynamic version of the model in Section III of Goldstein et al. (2001). We solve the model for several different underlying parameter values and plot the relation between optimal leverage and the tax rate in Appendix Figure B5. Five main parameters govern the optimal choice of leverage in this model: the risk-free interest rate, the debt issuance cost, the firm's default cost, and the drift and volatility of profit, which is characterized by a diffusion process. In each of Panels A–E in Appendix Figure B5, we plot the relation between optimal leverage and the tax rate for high and low values of each of these parameters.

In all cases, we find that a higher tax rate results in higher optimal leverage. Two main features of this model differ from ours and underly the result. First, in this class of models, shareholders can always cover negative cash flows. Thus, although taxes affect default thresholds, their effect on leverage is always positive because the assumption of deep-pocket shareholders implies that financial distress happens rarely, so the effect of taxes on the default put is small relative to the direct tax advantage of debt. Second, in this class of models, there is no capital, so there is no substitution between capital and a storage technology, that is, debt. The only important link remaining in the model between leverage and taxes is the interest tax deduction.

5.8 External Validity

One of the main implications of our model is testable: risky loan rates should rise with taxes because firms are more likely to default when taxes are higher. While the interest rate information in the Y-14 data is too sparse and noisy to test this hypothesis, the Y-14 collection contains information on banks' estimates of firms' default probabilities. Therefore, we can test the hypothesis that these default probabilities move with tax rates. We limit the sample to private firms and exclude observations corresponding to defaulted firms and to those with default probability estimates outside the $(0, 1)$ interval. We use a sample from the first quarter of 2012 through the fourth quarter of 2018. Requiring the sample to end in 2017, as for our firm balance sheet sample, would give us three years of data for most firms because the coverage of the risk metrics variables does not become complete until the end of 2014 (Beyhaghi et al. 2024). Unlike our firm balance sheet sample, the risk metrics panel is quarterly. Finally, we do not require firms to have available financial statement information, which results in a substantially larger sample of firm quarters.

We this firm-quarter data to estimate the evolution of default probabilities around corporate income tax cuts in an event study framework. We include the following controls: borrower and 4-digit NAICS by year-quarter fixed effects, state individual income tax cut and hike indicators, a set of tax base and tax credit controls, and lagged changes and levels of county GDP, employment, per-capita income, and population. We consider various bins of total commitment amount sizes: \$10–\$100, ≥\$10, \$10–\$50, \$20–\$100, \$50–\$100, ≥\$100, and ≥\$50 million. Ideally, we would like to compare small and large firms in the data, but we do not have balance sheet information for most observations in the risk metrics panel. However, Sufi (2009) and Brown et al. (2021) show that conditional on firms having credit line financing, credit line size is about 20-25% of firm assets, while Rauh and Sufi (2010) show that term loans and credit lines each constitute roughly half of firms' total bank debt. As such, firms in the \$10–\$100 or \$10-\$50 million buckets are likely to have between \$20–\$400 and \$20–\$200 million in assets, respectively, and be small firms.

Table 3 shows the evolution of default probabilities around state corporate income tax cuts. Columns (1)–(4) show that these probabilities decline significantly in the year of enactment or one year after enactment by 20-30 basis points. These magnitudes represent an approximate 10% fall from average default rates that average around 2%. (See Appendix Table B21.) This decline peaks two years after enactment and remains significant through three years after enactment. These results are driven by firms that have total commitments below \$50 or \$100 million. By contrast, we do not observe much change in default probabilities around corporate income tax cuts for the larger loan sizes.

Next, we examine a less direct measure of default probability from the SNC Program: one based on banks' internal credit risk assessment and another based on supervisory assessment. These variables are one when at least one of the firm's loan commitments is of non-pass credit quality and zero otherwise. A non-pass rating is associated with a significantly higher probability of banks setting aside loan loss reserves and charge-offs (Ivanov et al. 2023). In Appendix Table B19, we show the event study results for each measure of firm risk for both tax cuts and hikes. The first two columns show a 1.7— 2.2 percentage point fall in the probability of a non-pass rating accompanying tax cuts. These drops persist for roughly three years following enactment and are consistent across the two measures. In contrast, tax hikes are not associated with any change in firm risk. From the results in Table 3 and Appendix Table B19, we conclude that taxes affect firm default likelihood, so these results offer a useful check on the mechanism in the model whereby taxes affect borrowing terms.

6. Conclusion

Using comprehensive samples of both private and public companies and relying on simple event study techniques, we study the evolution of corporate borrowing around changes in state corporate income taxes since the 1990s. In contrast to much of the prior literature, we show that for small private firms, corporate taxes, on average, depress their leverage, that these effects last many years, and that companies adjust capital structure immediately upon the enactment of changes in state corporate tax policy. Interestingly, the leverage of large private firms does not change in response to tax legislation.

We rationalize these empirical results in a dynamic equilibrium model. The model is intentionally standard, with a tax benefit of debt baked in. Firms make optimal leverage, investment, and hiring decisions in an infinite horizon framework, while facing restricted external equity finance. We estimate the model parameters on different samples of firms and show that optimal leverage can either rise or fall with the tax rate. For the model estimated on our sample of small firms, the tax benefit is offset by a tax-sensitive cost that reflects the negative effect of taxes on firm health and, thus, a positive effect on equilibrium credit spreads. The taxation of profit also offsets the tax benefit, so a tax hike lowers optimal capital and, therefore, the debt needed to finance it. In contrast, for the model estimated on our sample of large firms, leverage exhibits a traditional positive relation to the corporate tax rate. Because this second parameterization of the model features sluggish investment, the investment response to a tax cut is muted, the accompanying need to borrow is also muted, and the fall in the tax benefit guides a fall in leverage.

We also use our model to examine the welfare effects of the tax benefit of debt. We find that welfare falls when the tax benefit is removed from the model. Without this extra incentive to finance investment, firms hold too much cash and too little capital. The equilibrium result is more equilibrium hours worked, lower consumption, and lower welfare. This result has a second-best flavor because the tax benefit incentivizes firms to get closer to a frictionless optimal capital stock when they face constraints on external equity finance.

Our main result of a negative sensitivity of leverage to taxes is for small private U.S. firms. While these firms do not constitute the majority of U.S. output, most firms outside of the United States are both small and private. Thus, our results have implications for large classes of firms in both developed and developing economies, and it is worth asking whether the same sets of frictions and trade-offs apply to firms in other parts of the world.

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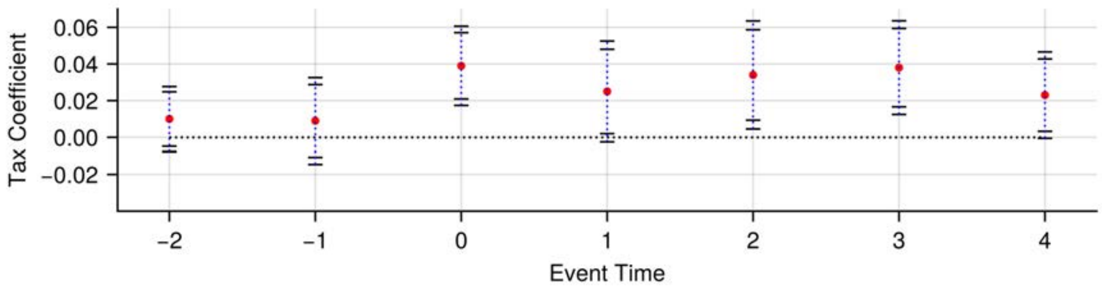
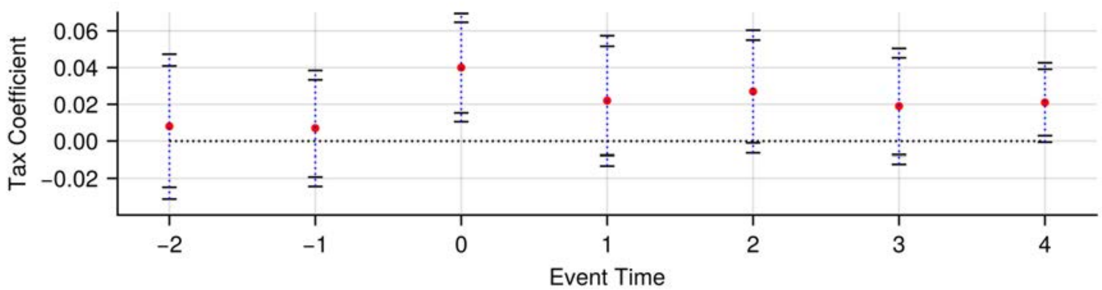
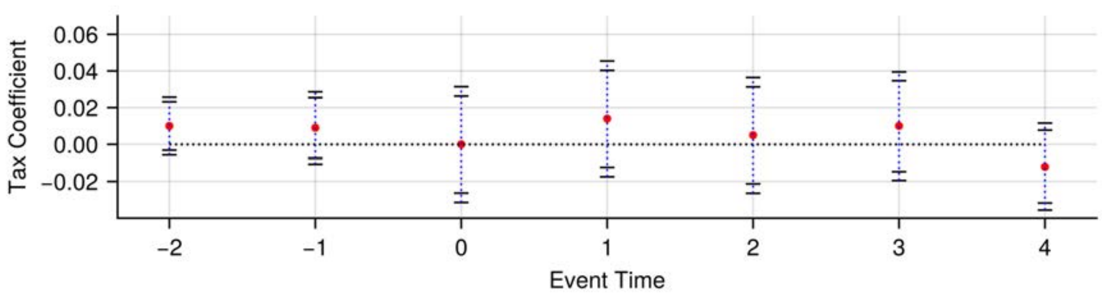
Panel A: Small Private Firms, Long-term Leverage



Panel B: Small Private Firms, All Leverage



Panel C: Large Private Firms, Long-term Leverage



Panel D: Large Private Firms, All Leverage

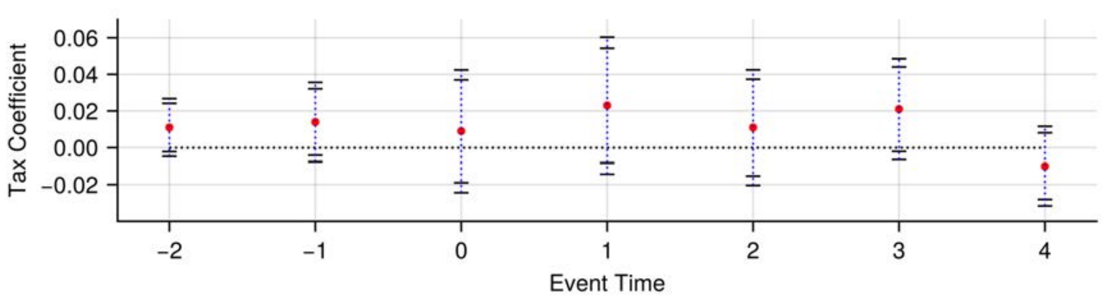
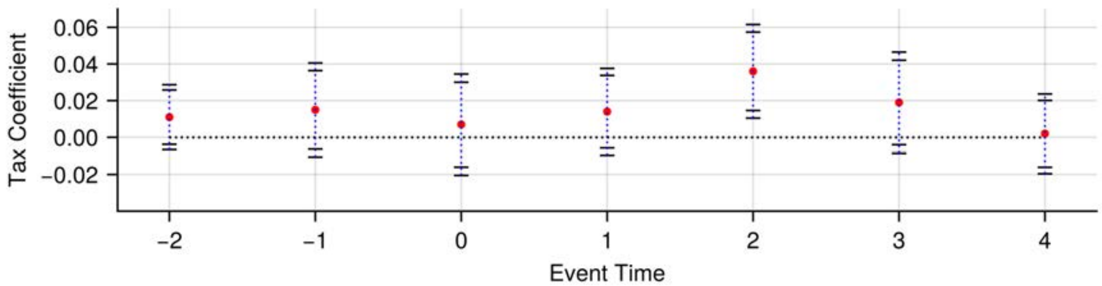
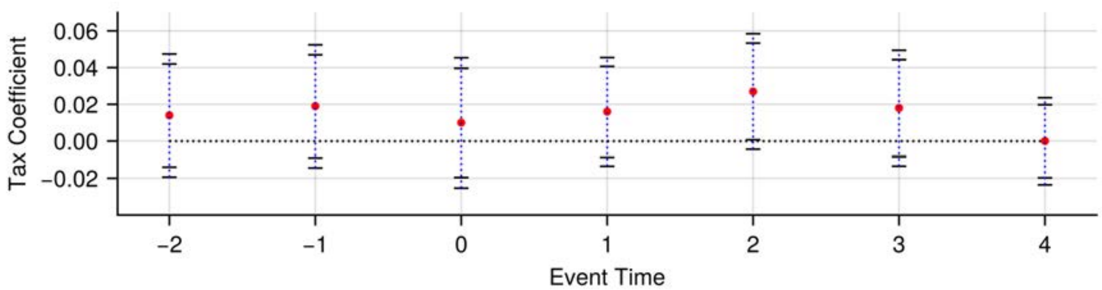


Figure 1: **Leverage event studies, enactment dates.** This figure plots the coefficients from Online Appendix Table B1, which contain estimates from firm-year panel regressions of the evolution of firm leverage around the enactment of state corporate income tax cuts. The data are our subsamples of small and large private firms from the Y-14 data. The dot represents the point estimate. The outer bars are the 90% confidence interval, and the inner bars are the 95% confidence interval.

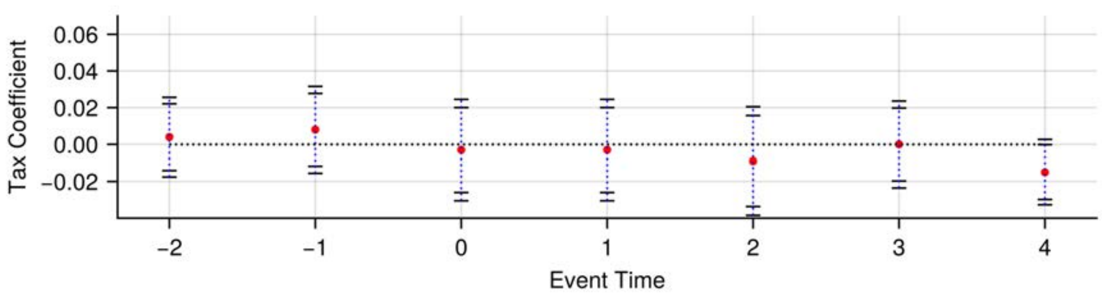
Panel A: Small Private Firms, Long-term Leverage



Panel B: Small Private Firms, All Leverage



Panel C: Large Private Firms, Long-term Leverage



Panel D: Large Private Firms, All Leverage

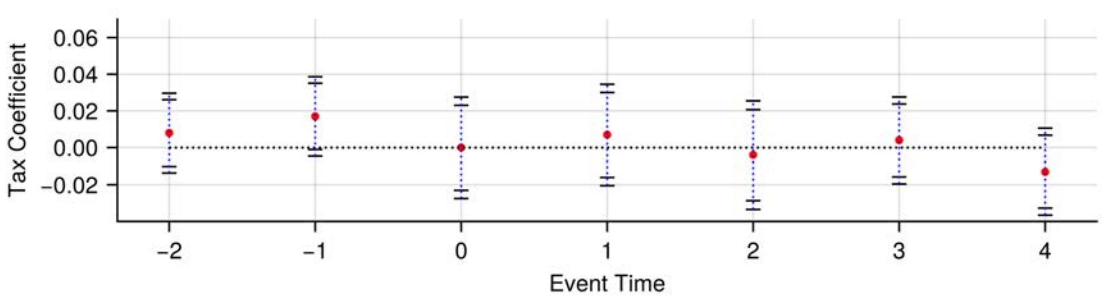
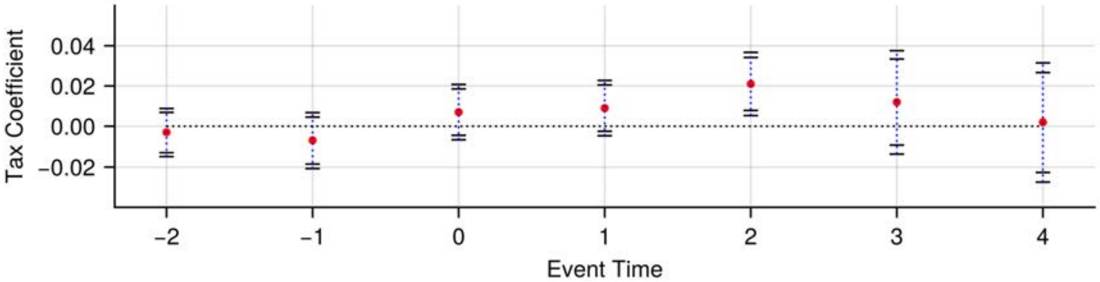
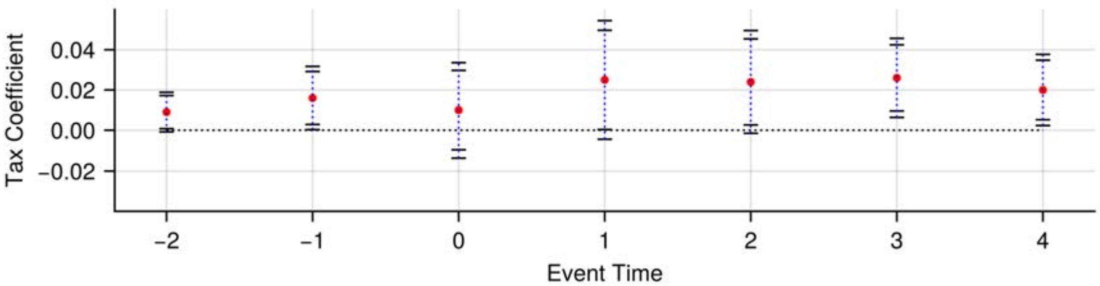


Figure 2: **Leverage event studies, effective dates.** This figure plots the coefficients from Online Appendix Table B2, which contain estimates from firm-year panel regressions of the evolution of firm leverage around the effective dates of state corporate income tax cuts. The data are our subsamples of small and large private firms from the Y-14 data. The dot represents the point estimate. The outer bars are the 90% confidence interval, and the inner bars are the 95% confidence interval.

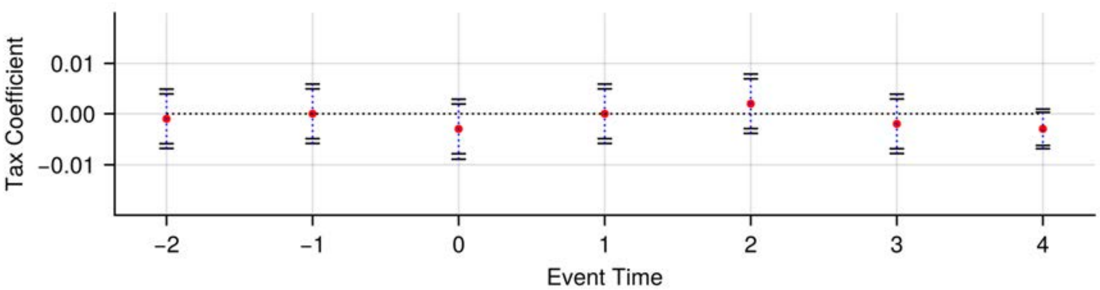
Panel A: Public Firms, Late Sample Period, Tax Cuts



Panel B: Public Firms, Late Sample Period, Tax Hikes



Panel C: Public Firms, Early Sample Period, Tax Cuts



Panel D: Public Firms, Early Sample Period, Tax Hikes

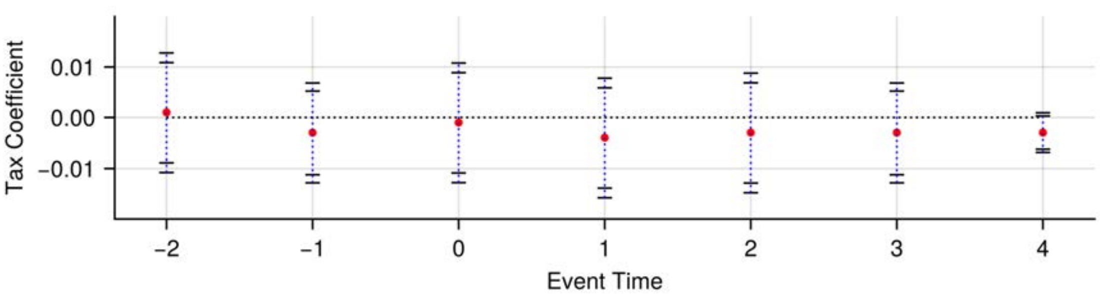
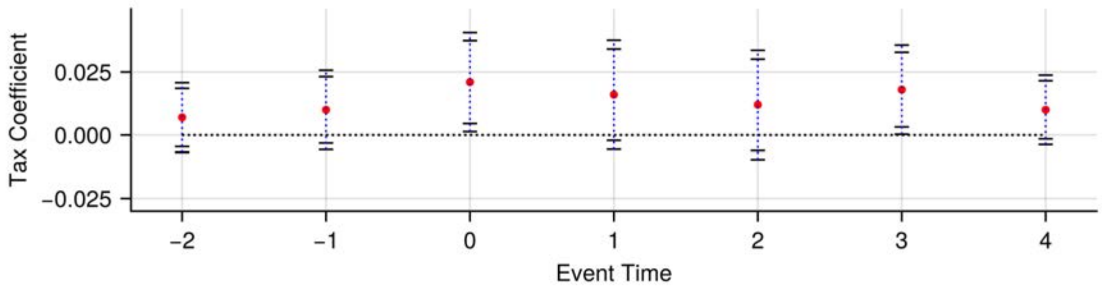
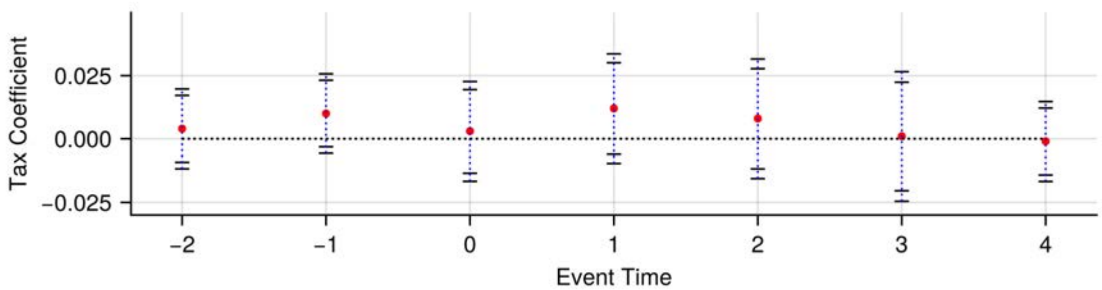


Figure 3: **Leverage event studies: public firms, enactment dates.** This figure plots the coefficients from Online Appendix Table B3, which contain estimates from firm-year panel regressions of the evolution of long-term firm leverage around the enactment dates of state corporate income tax changes. The sample runs from 1989 through 2017 and is restricted to public companies from the CRSP-Compustat database. Each regression contains both tax-hike and tax-cut dummies, and each pair of panels (A and B or C and D) corresponds to a single regression. Each dot represents a point estimate. The outer bars are the 90% confidence interval, and the inner bars are the 95% confidence interval.

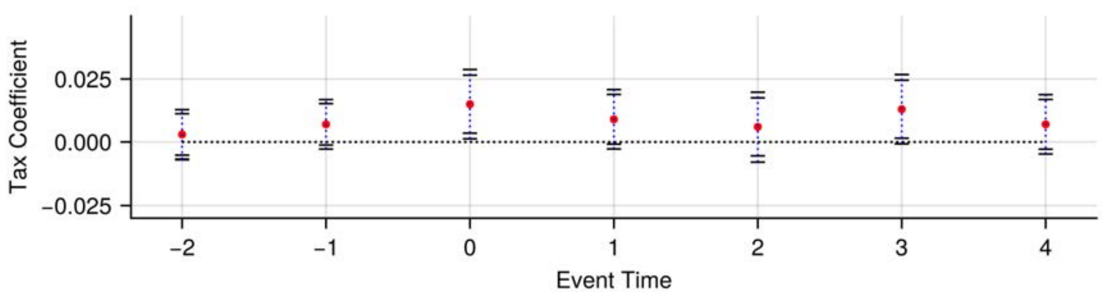
Panel A: Small Private Firms, Investment Scaled by Tangible Assets



Panel B: Large Private Firms, Investment Scaled by Tangible Assets



Panel C: Small Private Firms, Investment Scaled by Total Assets



Panel D: Large Private Firms, Investment Scaled by Total Assets

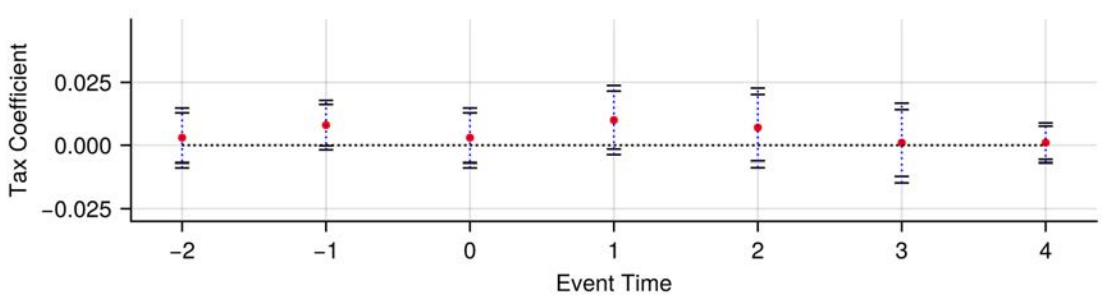


Figure 4: **Investment event studies, enactment dates.** This figure plots the coefficients from Table B4, which contain estimates from firm-year panel regressions of the evolution of firm investment around the enactment of state corporate income tax cuts. The data are our subsamples of small and large private firms from the Y-14 data. Each dot represents a point estimate. The outer bars are the 90% confidence interval, and the inner bars are the 95% confidence interval.

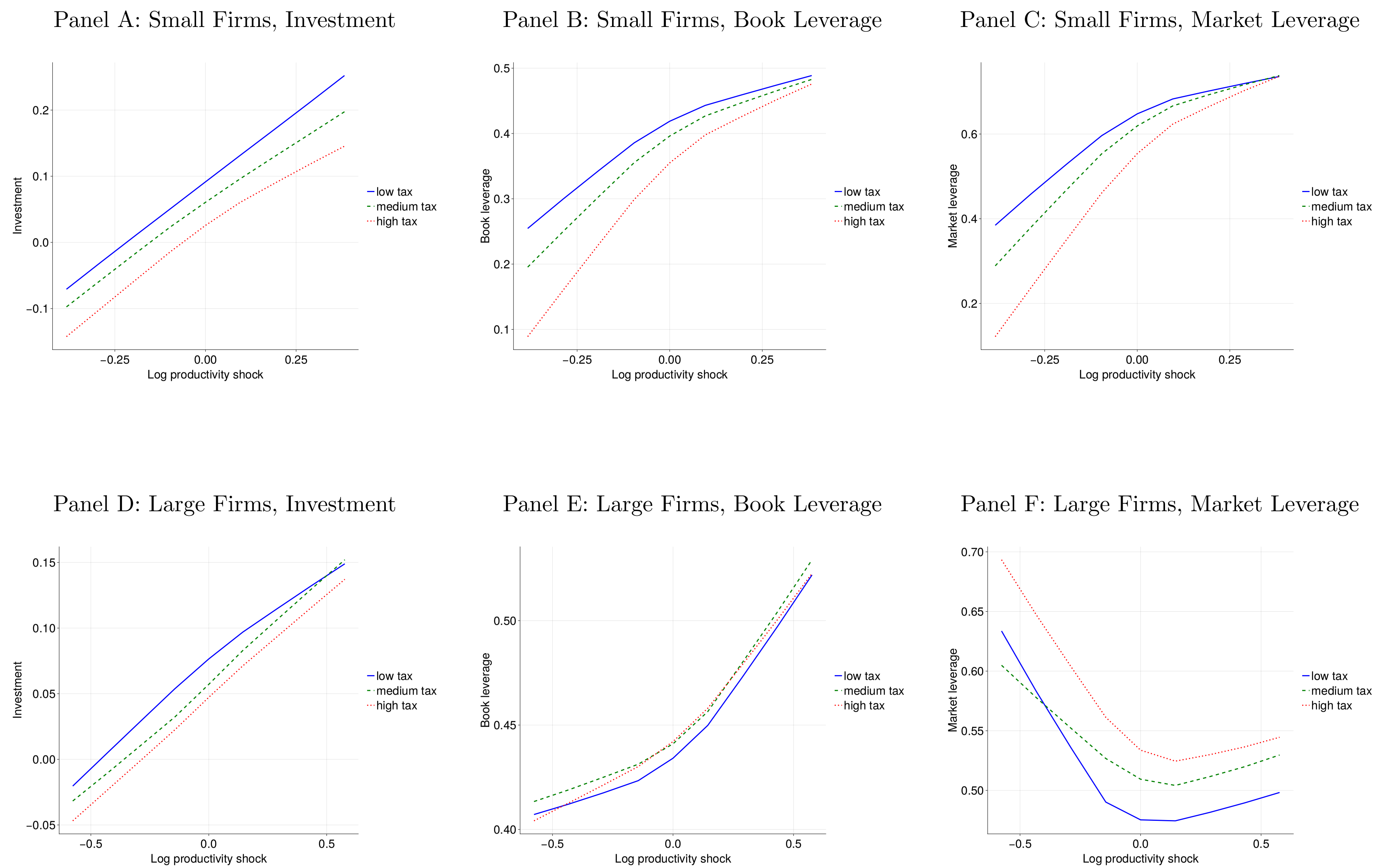


Figure 5: **Optimal Policies and Taxes.** This figure plots three policies as a function of the profitability shock: investment (I/k), the ratio of debt to capital (b/k or Book leverage), and the ratio of debt to the sum of debt and the market value of equity ($b/(v+b)$ or Market leverage). Each line represents a different level of the tax rate: low, medium, and high. The plotted policy functions are averages over the ergodic distribution of the model, conditioning upon the indicated values of the profitability shocks. Panel A depicts optimal policies for parameters estimated on our sample of small private firms, and Panel B depicts optimal policies for a different set of parameters estimated on our sample of large private firms.

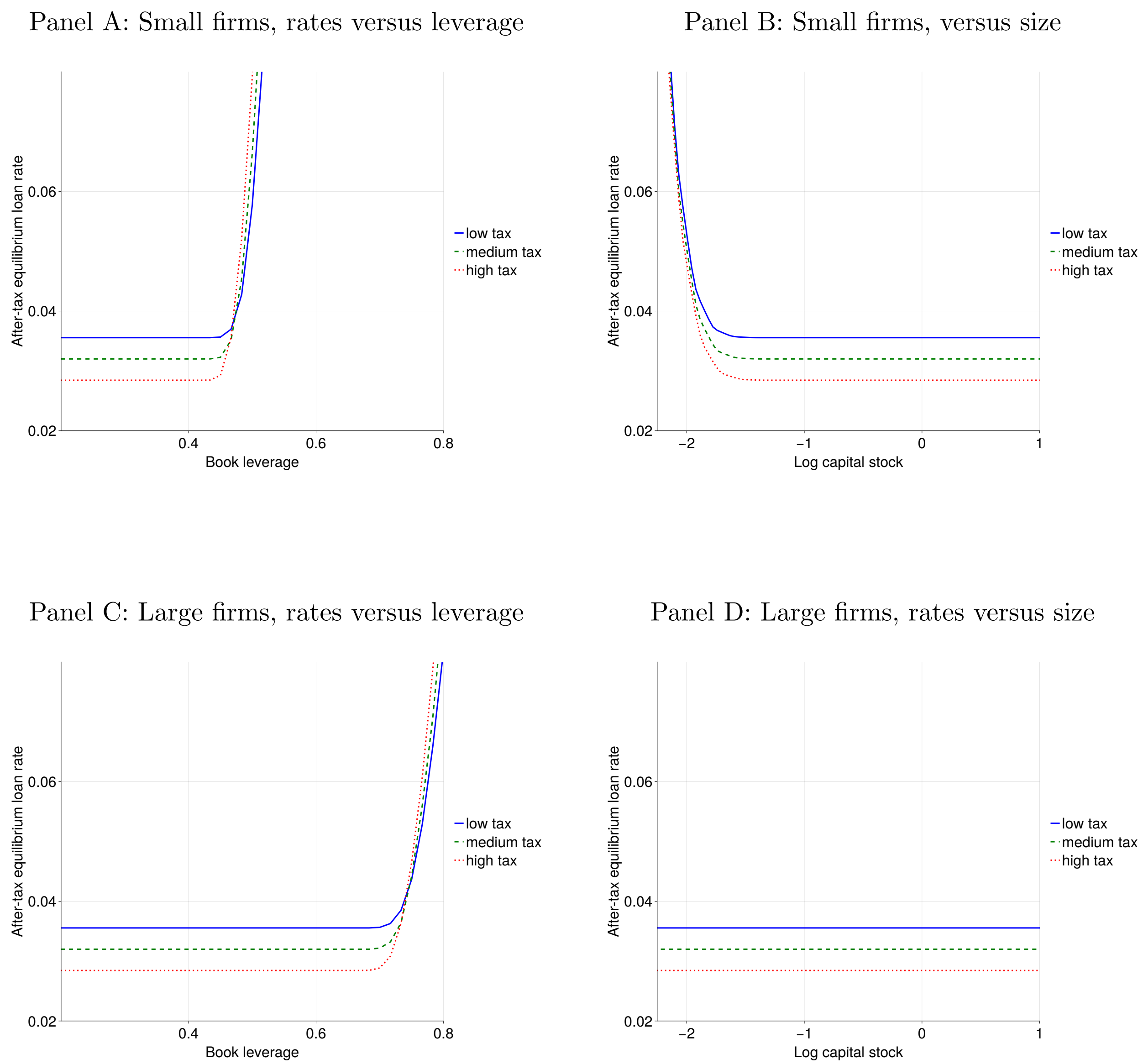


Figure 6: **After-tax equilibrium loan rates.** This figure plots after-tax, equilibrium risky loan rates as functions of firm size and firm leverage. Panel A depicts loan rates for parameters estimated on our sample of small private firms, and Panel B depicts loan rates for parameters estimated on our sample of large private firms.

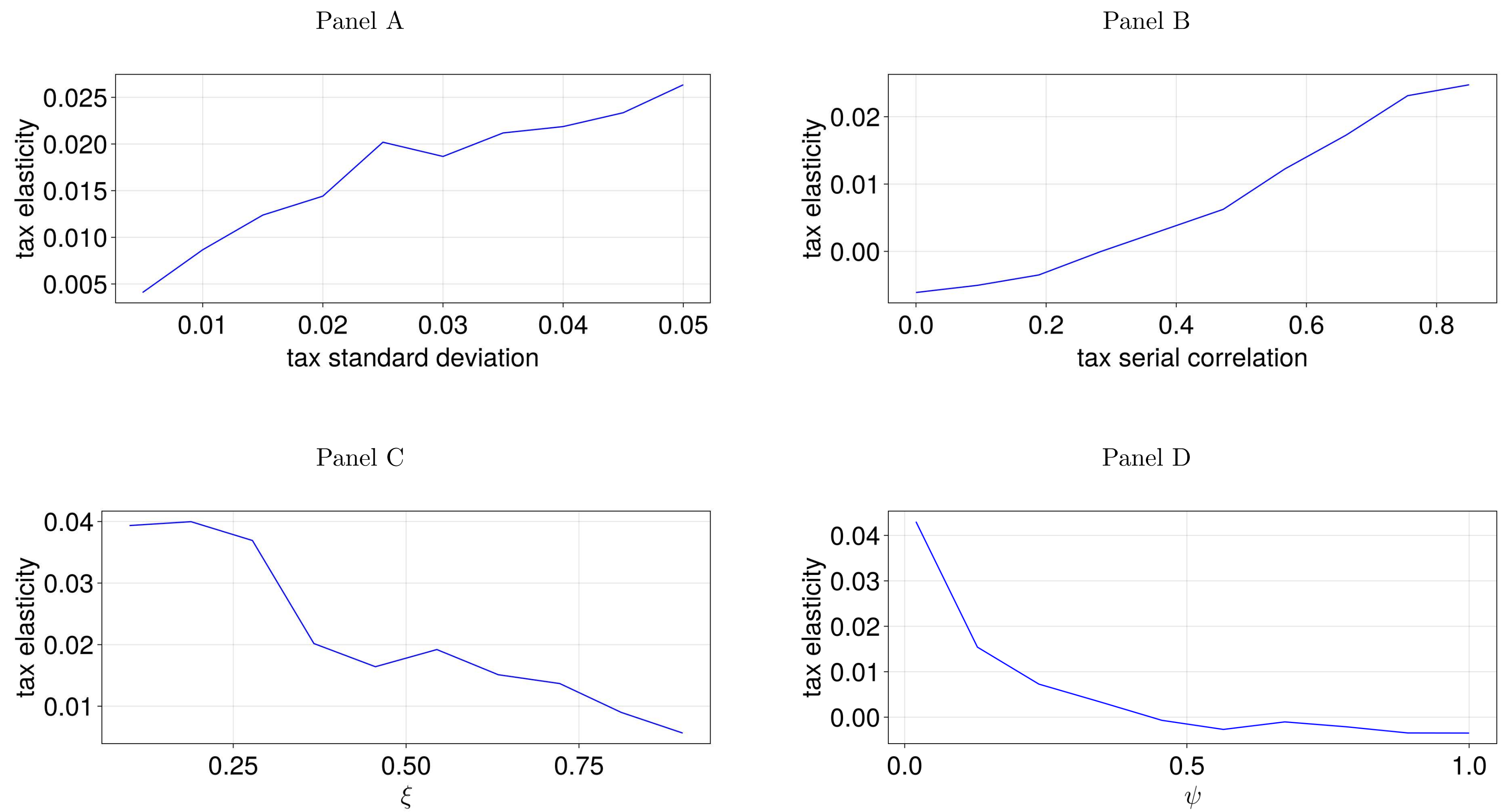


Figure 7: **Determinants of Debt–Tax Sensitivity.** All panels depict the effects of different model parameters on the sensitivity of leverage to a dummy representing a tax change. In Panel A, the parameter is the volatility of the tax process. In Panel B, the parameter is the serial correlation of the tax process. In Panel C, the parameter is the capital adjustment cost. In Panel D, the parameter is the deadweight cost of default.

Table 1: **Summary Statistics.** This table presents summary statistics for our sample of private firms from the Y-14 data (Panel A), our sample of public firms from Compustat (Panel B), firm-level loan characteristics from the SNC data (Panel C), and the tax changes used in our analyses (Panel D). The commitment amount refers to the sum of credit line and term loan commitments, while the utilized amount is the sum of term loans and the outstanding drawn amounts under credit lines. All firm financial variables in Panels A and B, with the exception of Book Assets, are scaled by lagged firm total assets.

	Mean	*St. Dev.*	*p10*	*p25*	*p50*	*p75*	*p90*
			Panel A: Y-14 Data				
Book Assets, $m	2,151	37,758	124	166	288	681	1,934
Net Income	0.07	0.13	-0.03	0.01	0.05	0.09	0.16
Net Sales	1.82	1.47	0.39	0.78	1.46	2.43	3.64
EBITDA	0.14	0.15	0.03	0.07	0.11	0.18	0.26
Long Term Debt	0.31	0.28	0.00	0.07	0.26	0.49	0.70
Total Debt	0.38	0.28	0.01	0.15	0.35	0.57	0.76
Fixed Assets	0.32	0.28	0.03	0.09	0.25	0.49	0.76
			Panel B: Compustat Data				
Book Assets, $m	3514	7722	35	135	633	2615	8991
Net Income	-0.06	0.30	-0.37	-0.06	0.03	0.07	0.12
Sales	1.01	0.77	0.21	0.48	0.83	1.36	1.99
EBITDA	0.03	0.27	-0.28	0.03	0.10	0.16	0.22
Long Term Debt	0.20	0.21	0.00	0.00	0.15	0.32	0.49
Total Debt	0.23	0.23	0.00	0.01	0.19	0.36	0.54
Fixed Assets	0.24	0.24	0.02	0.06	0.15	0.34	0.63
			Panel C: SNC Data				
Commitment Amount, $m	689	1,136	125	180	325	725	1516
Utilized Amount, $m	283	588	0	28	122	290	662
Utilization Ratio	0.48	0.35	0.00	0.09	0.51	0.80	0.95
Contract Maturity, months	68	26	37	56	61	79	97
			Panel D: Characteristics of Tax Changes				
Tax Cuts (N=50)							
Rate Change, %	-1.22	1.43	-2.25	-1.5	-0.72	-0.4	-0.22
Initial Rate, %	6.89	2.18	4.44	5.81	7.05	8.25	8.91
Resulting Rate, %	5.67	2.81	3.3	4.5	6.12	7.36	8.19
Tax Hikes (N=24)							
Rate Change, %	1.39	1.14	0.5	0.71	1.02	1.5	2.8
Initial Rate, %	7.93	1.85	6.0	6.5	7.95	9.0	9.78
Resulting Rate, %	9.31	2.41	7.0	7.99	8.7	10.2	12.61

Table 2: **Model Estimation.** This table reports the parameters and moments from the estimation of our model from Section 5. We use the subsamples of small and large firms from the Y-14 data. The sample period is 2011–2017. The parameter θ is the curvature of the production function; ρ is the serial correlation of the shock process; σ is the standard deviation of the shock process; δ is the capital depreciation rate; ψ governs quadratic capital adjustment costs; ξ is the deadweight cost of default; and f is the fixed operating cost. 'Debt-to-assets' is defined as long-term debt divided by lagged total assets; 'investment-to-assets' is defined as net capital expenditures divided by fixed assets; 'income-to-assets' is defined as EBITDA divided by lagged total assets; 'debt issuance' is defined as the change in debt scaled by lagged total assets. The t-statistics next to the simulated and actual moments are based on the null hypothesis of the equality of each of these moment pairs.

A. Parameter estimates

	θ	ρ	σ	δ	ψ	ξ	f
Small Firms	0.9268	0.6305	0.0992	0.0865	0.1106	0.6009	0.1641
	(0.0328)	(0.1953)	(0.0468)	(0.0021)	(0.1219)	(0.0402)	(0.0598)
Large Firms 0.9001	0.6215	0.1506	0.0858	0.9955	0.3218	0.1536	
(0.0173)	(0.0915)	(0.0271)	(0.0019)	(0.3412)	(0.1563)	(0.0318)	

B. Targeted moments

	Small firms			Large firms		
	Actual	Simulated	t-stat.	Actual	Simulated	t-stat.
Mean debt-to-assets	0.3677	0.3697	-0.2150	0.3944	0.3786	1.7983
Standard deviation debt-to-assets	0.1066	0.1137	-6.6722	0.1131	0.0738	0.9987
Mean investment-to-assets	0.1439	0.0917	0.4649	0.1131	0.0866	0.3901
Standard deviation investment-to-assets	0.0817	0.0984	-0.3893	0.0742	0.0424	0.2091
Serial correlation investment-to-assets	0.5925	0.2870	0.1320	0.5635	0.4288	0.0873
Mean income-to-assets	0.1547	0.1528	0.0399	0.1351	0.1731	-0.9351
Standard deviation income-to-assets	0.0593	0.0314	0.0690	0.0546	0.0531	0.0214
Serial correlation income-to-assets	0.8483	0.7190	0.1855	0.7908	0.6116	0.0438
Tax elasticity	0.0214	0.0173	0.0019	-0.0062	-0.0023	-0.0027

Table 3: **Probabilities of default and corporate tax cuts.** This table presents results from firm-year panel regressions of the evolution of firm probabilities of default around the enactment of state corporate income tax cuts. The sample runs from 2012 through 2019 and is restricted to private companies. The dependent variable in all specifications is the current-quarter probability of default estimate for each firm in the sample. Y-14 banks report the probability of default estimates as part of the Federal Reserve's Y-14 Collection. All specifications include both borrower and 4-digit NAICS by year-quarter fixed effects. The regressors are state corporate income tax hike indicators, state individual income tax cuts and hike indicators, a set of tax base and tax credit controls, as well as lagged changes and levels of county GDP, employment, per-capita income, and population. Columns (1) through (7) present results for borrowers with \$10–\$100, ≥\$10, \$10–\$50, \$20–\$100, \$50–\$100, ≥\$100, and ≥\$50 million in total commitments. The standard errors are double clustered at the state and 4-digit NAICS by year-quarter level.

	Probability of Default						
	10-100	10-	10-50	20-100	50-100	100-	50-
Event Year= -2	-0.0004	-0.0002	-0.0003	-0.0008	-0.0007	0.0008	0.0004
	(0.0007)	(0.0006)	(0.0008)	(0.0010)	(0.0020)	(0.0024)	(0.0016)
Event Year= -1	-0.0004	-0.0003	-0.0003	-0.0019	0.0006	0.0053	0.0018
	(0.0014)	(0.0014)	(0.0012)	(0.0018)	(0.0027)	(0.0033)	(0.0022)
Event Year= 0	-0.0024	-0.0023*	-0.0028**	-0.0034	0.0017	0.0012	0.0013
	(0.0015)	(0.0013)	(0.0011)	(0.0020)	(0.0048)	(0.0030)	(0.0023)
Event Year= $+1$	-0.0032**	-0.0029**	-0.0032**	-0.0031*	-0.0048	-0.0011	-0.0007
	(0.0014)	(0.0012)	(0.0014)	(0.0018)	(0.0047)	(0.0037)	(0.0032)
Event Year= $+2$	-0.0034***	-0.0033***	-0.0038***	-0.0034**	-0.0030	0.0003	0.0005
	(0.0011)	(0.0010)	(0.0009)	(0.0016)	(0.0048)	(0.0028)	(0.0027)
Event Year= $+3$	-0.0013*	-0.0015**	-0.0014*	-0.0008	-0.0006	-0.0003	-0.0012
	(0.0008)	(0.0007)	(0.0008)	(0.0009)	(0.0023)	(0.0020)	(0.0017)
Event Year=$\geq +4$	-0.0009	-0.0011*	-0.0009	-0.0000	0.0019	-0.0063*	-0.0005
	(0.0007)	(0.0006)	(0.0008)	(0.0009)	(0.0023)	(0.0032)	(0.0017)
Observations	172084	195589	145149	94336	22590	12865	39430
R^2	0.711	0.701	0.727	0.726	0.774	0.759	0.726
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
TB Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economy Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Appendix Appendix A: Variable Definitions

Y-14 Data Definitions

Below we present variable definitions for the financial statement data coming from the FR-Y-14Q Collection. The item numbers of data fields refer to Schedule H1 of the Y-14Q data.

Book Assets is defined as the book value of total assets of firm i as of the end of the previous year $t-1$, 'Total Assets Prior Year' (item #71).

Net Income is defined as the net income of firm i for the trailing twelve month period ending in year t, 'Net Income Current Year' (item #59) divided by total assets of firm i in year $t-1$, 'Total Assets Prior Year' (item #71).

Net Sales is defined as the net sales of firm i for the trailing twelve month period ending in year t, 'Net Sales Current Year' (item #54) divided by total assets of firm i in year $t-1$, 'Total Assets Prior Year' (item #71).

EBITDA – is defined as the Earnings Before Interest, Taxes, Depreciation & Amortization of firm i for the trailing twelve month period ending in year t, divided by total assets of firm i in year $t-1$, 'Total Assets Prior Year' (item #71).

Long-Term Leverage is defined as the book value of long-term debt of firm i as of the end of year t, 'Long-Term Debt' (item #78) + 'Current Maturities of Long Term Debt' (item #75), divided by total assets of firm i in year $t-1$, 'Total Assets Prior Year' (item #71).

Total Leverage is defined as the book value of total debt of firm i as of the end of year t, 'Short-Term Debt' (item #74) + 'Long-Term Debt' (item #78) + 'Current Maturities of Long Term Debt' (item #75), divided by total assets of firm i in year $t-1$, 'Total Assets Prior Year' (item #71).

Total Liabilities is defined as the book value of total liabilities of firm i as of the end of year t, 'Total Liabilities' (item #80), divided by total assets of firm i in year $t-1$, 'Total Assets Prior Year' (item #71).

Fixed Assets is defined as the book value of fixed assets of firm i as of the end of year t, 'Fixed Assets' (item #69), divided by total assets of firm i in year $t-1$, 'Total Assets Prior Year' (item #71).

Capital Expenditures is defined as the difference between capital expenditures and capital divestitures of firm i as of the end of year t, 'Capital Expenditures' (item #82). In the regression specifications, we scale this variable by either total assets of firm i in year $t-1$, 'Total Assets Prior Year' (item #71), or by current-year tangible assets. We define current-year tangible assets as the difference between the book value of total assets of firm i as of the end of year t, 'Total Assets Current Year' (item #70), and the sum of cash, depository accounts and marketable securities, 'Cash & Marketable

Securities' (item #61), inventory, 'Inventory Current Year' (item #64), and accounts receivable, 'Accounts Receivable (A/R) Current Year' (item #62), of firm i as of the end of year t.

Probability of default is defined as the most conservative (the maximum) probability of default estimate, 'Probability of Default' (item #88), assigned to firm i as of the end of quarter t across all the lenders firm i works with in quarter t. The Federal Reserve's Y-14 Collection requires banks to report their estimates of firms' probabilities of default and loss given default. Banks subject to "advanced approaches" for calculating regulatory capital are required to report the probability of default and loss given default estimates that these banks use to compute regulatory capital. The remaining banks in the data report the probability of default estimates that correspond to the firms internal risk rating assigned by these banks and loan-level loss given default estimates from banks' internal credit risk management systems.

Shared National Credit Data Definitions

Below we present definitions for the variables relying on loan data from the Shared National Credit Database:

Commitment Amount is defined as the total value of syndicated loan commitments of firm i as of the end of year t, where total commitments include both the size of credit line commitments and outstanding term loans.

Contract Maturity is defined as the average original contract maturity across all syndicated commitments of firm i in year t, weighted by the commitment amount of each contract relative to the firm's total syndicated commitments.

NonPass takes the value of one whenever at least one of firm i 's syndicated commitments are rated NonPass, or in one of the bottom four Supervisory Rating Scale credit risk categories, by lead banks or bank supervisors. The Supervisory Rating Scale is a five grade scale defined as follows from best to worst rating: 1) Pass—a loan facility defined to be in a good credit standing, 2) Special Mention—a loan facility with some credit weaknesses that could result in deterioration of loan repayment prospects, 3) Substandard—a loan facility with well-defined credit weaknesses that could result in some losses for the bank if these weaknesses are not corrected, 4) Doubtful—a loan facility with the problems described in the Substandard category with additional deficiencies that make successful collection highly unlikely, and 5) Loss—a loan facility that is considered non-collectable and should be charged-off. We have two versions of this measure—one in terms of lead banks' internal credit ratings and another one in terms of supervisory credit ratings. All information necessary to construct the credit risk measure comes from the Shared National Credit data.

Compustat Data Definitions

Below we present definitions for the variables relying on financial statement data from the Compustat database:

Long Term Leverage is defined as the value of long-term debt (item dltt) of firm i in year t, divided by the book value of total assets of firm i in year t (item at).

Market-to-book is defined as the product of the firm's equity prices (item prcc_f) and the number of shares outstanding (item cshpri) as of the end of the fiscal year plus preferred stock (item pstkl) plus long-term debt (the sum of items dltt and dlc), all divided by total assets. Market-to-book is in lags or in lag changes in all regression specifications.

Size is defined as the log of 1 plus the firm's total assets (item at) converted to 2012 dollars. Size is in lags or in lag changes in all regression specifications.

Tangibility is defined as the firm's property, plant, and equipment (item ppent) divided by the firm's total assets (item at). Tangibility is in lags or in lag changes in all regression specifications.

ROA is defined as the firm's income (item oibdp) divided by total assets (item at). ROA is in lags or in lag changes in all regression specifications.

Omitted Industries

We omit the firms in the following industries from our Y-14 sample: (three-digit NAICS code of "814" or four-digit SIC code of "8811"), utilities (two-digit NAICS code of "22" or two-digit SIC code of "49"), financials and real estate (two-digit NAICS code of "52" or one-digit SIC code of "6"), public administration entities (two-digit NAICS code of "92" or one-digit SIC code of "9"), educational institutions (three-digit NAICS code of "611" or four-digit SIC code of "8299"), hospitals (three-digit NAICS code of "622" or three-digit SIC code of "806"), religious institutions (three-digit NAICS code of "813" or three-digit SIC code of "866").

Appendix Appendix B: Additional Analyses

Tables B1–B4 present the results underlying Figures 1–4. Tables B5–B18 present various robustness checks for our empirical results in Section 4. Figure B20 presents the estimation results for the alternative models in Section 5.7. Figure B1 plots histograms of total firm assets and leverage for our samples of firms from the Y-14 data and from Compustat. Figure B2 plots comparative statics from our model in Section 5. Figure B3 presents the identification diagnostic from Andrews et al. (2017). Figure B5 presents comparative statics from the model in Goldstein et al. (2001).

The regression results using the Y-14 data presented in this Appendix share the following specifications. The sample for these tables runs from 2011 through 2017 and, unless specified otherwise, is restricted to private companies with at least $100 million in prior-year total assets. All specifications include both borrower and 4-digit NAICS-by-year fixed effects. Unless otherwise indicated, the control variables are the log of lagged firm sales, bank internal credit rating indicators for the firm in a 10-grade S&P scale, state corporate income tax hike indicators, state individual income tax cut and hike indicators, a set of tax base and tax credit controls, as well as lagged changes and levels of county GDP, employment, per-capita income, and population. The standard errors are double clustered at the state and 4-digit NAICS by year level.

Table B1: **Enactment Dates of Tax Cuts and Firm Leverage.** This table presents results from firm-year panel regressions of the evolution of firm leverage around the enactment of state corporate income tax cuts. Columns (1) and (3) present results for firms below the median in total assets, and columns (2) and (4) present results for firms above the median in total assets. The standard errors are double clustered at the state and 4-digit NAICS by year level.

Dependent variable:	*Long-Term Debt*		*Total Debt*	
Sample:	Small	Large	Small	Large
	(1)	(2)	(3)	(4)
Event Year= −2	0.010	0.010	0.008	0.011
	(0.009)	(0.008)	(0.020)	(0.008)
Event Year= −1	0.009	0.009	0.007	0.014
	(0.012)	(0.010)	(0.016)	(0.011)
Event Year= 0	0.039***	-0.000	0.040**	0.009
	(0.011)	(0.016)	(0.015)	(0.017)
Event Year= +1	0.025*	0.014	0.022	0.023
	(0.014)	(0.016)	(0.018)	(0.019)
Event Year= +2	0.034**	0.005	0.027	0.011
	(0.015)	(0.016)	(0.017)	(0.016)
Event Year= +3	0.038***	0.010	0.019	0.021
	(0.013)	(0.015)	(0.016)	(0.014)
Event Year=≥ +4	0.023**	-0.012	0.021*	-0.010
	(0.012)	(0.012)	(0.011)	(0.011)
R^2	0.879	0.857	0.874	0.862
N	18783	18787	18783	18787
Firm FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
Ratings FE	Yes	Yes	Yes	Yes
TB Controls	Yes	Yes	Yes	Yes
Economy Controls	Yes	Yes	Yes	Yes

Table B2: **Effective Dates of Tax Cuts and Firm Leverage.** This table presents results from firm-year panel regressions of the evolution of firm leverage around the effective dates of state corporate income tax cuts. Columns (1) and (3) present results for firms below the median in total assets, and columns (2) and (4) present results for firms above the median in total assets. The standard errors are double clustered at the state and 4-digit NAICS by year level.

Dependent variable:	*Long-Term Debt*		*Total Debt*	
Sample:	Small	Large	Small	Large
	(1)	(2)	(3)	(4)
Event Year= −2	0.011	0.004	0.014	0.008
	(0.009)	(0.011)	(0.017)	(0.011)
Event Year= −1	0.015	0.008	0.019	0.017
	(0.013)	(0.012)	(0.017)	(0.011)
Event Year= 0	0.007	-0.003	0.010	-0.000
	(0.014)	(0.014)	(0.018)	(0.014)
Event Year= +1	0.014	-0.003	0.016	0.007
	(0.012)	(0.014)	(0.015)	(0.014)
Event Year= +2	0.036**	-0.009	0.027	-0.004
	(0.013)	(0.015)	(0.016)	(0.015)
Event Year= +3	0.019	0.000	0.018	0.004
	(0.014)	(0.012)	(0.016)	(0.012)
Event Year=≥ +4	0.002	-0.015	-0.000	-0.013
	(0.011)	(0.009)	(0.012)	(0.012)
R^2	0.879	0.857	0.874	0.862
N	18783	18787	18783	18787
Firm FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
Ratings FE	Yes	Yes	Yes	Yes
TB Controls	Yes	Yes	Yes	Yes
Economy Controls	Yes	Yes	Yes	Yes

Table B3: **Tax Changes and Firm Leverage: Public Firms.** This table presents results from firm-year panel regressions of the evolution of long-term firm leverage around the enactment dates of state corporate income tax changes. The sample runs from 1989 through 2017 and is restricted to public companies from the CRSP-Compustat database. Each regression contains both tax-hike and tax-cut dummies, and each pair of columns corresponds to a single regression. Columns (1) and (2) correspond to the full sample. Columns (3) and (4) correspond to the early subsample. Columns (5) and (6) correspond to the late subsample. All specifications include borrower and 4-digit SIC by firm fiscal year fixed effects, and indicators for dates in which the tax changes became effective. The control variables are lagged levels of log firm assets, EBITDA, the market-to-book ratio, and asset tangibility, as well as lagged changes and levels of state GDP growth and unemployment rates. The standard errors are clustered at the state level.

	Full Sample		Early Sample		Late Sample	
	Cuts	Hikes	Cuts	Hikes	Cuts	Hikes
$t = -2$	-0.002	0.002	-0.001	0.001	-0.003	0.009*
	(0.002)	(0.005)	(0.003)	(0.006)	(0.006)	(0.005)
$t = -1$	-0.001	-0.001	-0.000	-0.003	-0.007	0.016**
	(0.002)	(0.005)	(0.003)	(0.005)	(0.007)	(0.008)
$t = 0$	-0.002	-0.002	-0.003	-0.001	0.007	0.010
	(0.003)	(0.006)	(0.003)	(0.006)	(0.007)	(0.012)
$t = +1$	0.000	-0.004	0.000	-0.004	0.009	0.025*
	(0.003)	(0.005)	(0.003)	(0.006)	(0.007)	(0.015)
$t = +2$	0.004	-0.004	0.002	-0.003	0.021***	0.024*
	(0.004)	(0.006)	(0.003)	(0.006)	(0.008)	(0.013)
$t = +3$	0.001	-0.005	-0.002	-0.003	0.012	0.026**
	(0.003)	(0.004)	(0.003)	(0.005)	(0.013)	(0.010)
$t \geq +4$	-0.001	-0.003	-0.003	-0.003	0.002	0.020**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.015)	(0.009)
R^2	.641	.641	.653	.653	.769	.769
N	91208	91208	77762	77762	15485	15485
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	1989-2017	1989-2017	1989-2011	1989-2011	2011-2017	2011-2017

Table B4: **Enactment Dates of Tax Cuts and Firm Investment.** This table presents results from firm-year panel regressions of the evolution of firm investment around the enactment of state corporate income tax cuts. Columns (1) and (3) present results for firms below the median in total assets, and columns (2) and (4) present results for firms above the median in total assets. The standard errors are double clustered at the state and 4-digit NAICS by year level.

Dependent variable:	*Capital Expenditures/Tangible Assets*		*Capital Expenditures/Total Assets*	
Sample:	Small	Large	Small	Large
	(1)	(2)	(3)	(4)
Event Year= −2	0.007	0.004	0.003	0.003
	(0.007)	(0.008)	(0.005)	(0.006)
Event Year= −1	0.010	0.010	0.007	0.008
	(0.008)	(0.008)	(0.005)	(0.005)
Event Year= 0	0.021**	0.003	0.015**	0.003
	(0.010)	(0.010)	(0.007)	(0.006)
Event Year= +1	0.016	0.012	0.009	0.010
	(0.011)	(0.011)	(0.006)	(0.007)
Event Year= +2	0.012	0.008	0.006	0.007
	(0.011)	(0.012)	(0.007)	(0.008)
Event Year= +3	0.018**	0.001	0.013*	0.001
	(0.009)	(0.013)	(0.007)	(0.008)
Event Year=$\geq$ +4	0.010	-0.001	0.007	0.001
	(0.007)	(0.008)	(0.006)	(0.004)
Observations	17732	17927	17837	18166
R^2	0.671	0.643	0.662	0.638
Firm FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
Ratings FE	Yes	Yes	Yes	Yes
TB Controls	Yes	Yes	Yes	Yes
Economy Controls	Yes	Yes	Yes	Yes

Table B5: **Tax Cuts and Firm Leverage: Borusyak et al. (2024) Estimator.** This table presents results from firm-year event study regressions in Borusyak et al. (2024). Columns (1) and (3) present results for small firms, while columns (2) and (4) present results for large firms. The sample runs from 2011 through 2017 and includes all firm years after which a private firm exceeds $100 million in total assets as of the previous year. Small firms are defined as those below $250 million in prior year total assets when they enter the sample. The standard errors are clustered at the state level.

Dependent variable:	*Long-Term Debt*			
Sample:	Small	Large	Small	Large
	(1)	(2)	(3)	(4)
Event Year= −2	0.006	-0.001	0.006	0.001
	(0.013)	(0.007)	(0.011)	(0.007)
Event Year= −1	0.013	0.004	0.010	0.009
	(0.012)	(0.008)	(0.011)	(0.008)
Event Year= 0	0.034**	-0.013	0.032**	-0.010
	(0.015)	(0.019)	(0.014)	(0.015)
Event Year= +1	0.028**	0.008	0.023**	0.009
	(0.013)	(0.009)	(0.011)	(0.009)
Event Year= +2	0.045**	0.017	0.038*	0.015
	(0.019)	(0.057)	(0.023)	(0.055)
Event Year= +3	0.019*	0.013	0.009	0.010
	(0.010)	(0.031)	(0.012)	(0.029)
Observations	20738	24683	20738	24683
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No
Industry × Year FE	No	No	Yes	Yes
Ratings FE	No	No	Yes	Yes

Table B6: **Tax Cuts and Firm Leverage: de Chaisemartin and D'Haultfoeuille (2020) Estimator.** This table presents difference-in-difference estimates of firm leverage responses to the enactment of state corporate income tax cuts following de Chaisemartin and D'Haultfoeuille (2020). All specifications include both borrower and 4-digit NAICS by year fixed effects, as well as bank internal credit rating indicators for the firm on a 10-grade S&P scale. The specifications labeled "No Controls" include only these variables, while the specifications labeled "Controls" include all of our standard controls from Table B1. Panel A presents results for small private firms, or those below the median in total assets, while Panel B presents results for firms above the median in total assets. The standard errors are computed using 1,000 bootstrap trials.

Dependent variable:	*Long-Term Leverage*					
	Panel A: Small Firms					
	No Controls			Controls		
	Estimate	Obs	Switchers	Estimate	Obs	Switchers
Event Year= 0	0.036**	10,586	161	0.036**	10,586	161
	(0.016)			(0.017)		
Event Year= +1	0.020	4,092	107	0.017	4,092	107
	(0.025)			(0.025)		
Event Year= +2	0.022	2,856	84	0.020	2,856	84
	(0.028)			(0.029)		
Event Year= +3	-0.025	1,387	57	-0.027	1,387	57
	(0.029)			(0.030)		

	Panel B: Large Firms					
	No Controls			Controls		
	Estimate	Obs	Switchers	Estimate	Obs	Switchers
Event Year= 0	-0.016	11,679	178	-0.032	11,679	178
	(0.019)			(0.022)		
Event Year= +1	-0.020	6,444	115	-0.032	6,444	115
	(0.022)			(0.024)		
Event Year= +2	-0.050*	3,649	87	-0.060**	3,649	87
	(0.026)			(0.028)		
Event Year= +3	-0.049	2,061	70	-0.052	2,061	70
	(0.038)			(0.039)		

Table B7: **Firm Leverage and Tax Enactments: Exogenous Tax Changes.** This table presents results from firm-year panel regressions of the evolution of firm leverage around the enactment of state corporate income tax cuts, excluding states with tax changes that are not classified as exogenous according to the algorithm in Giroud and Rauh (2019). Columns (1) and (3) present results for firms below the median in total assets, and columns (2) and (4) present results for firms above the median in total assets.

Dependent variable:	*Long-Term Debt*		*Total Debt*	
Sample:	Small	Large	Small	Large
	(1)	(2)	(3)	(4)
Event Year= −2	0.011	0.004	0.007	0.007
	(0.011)	(0.008)	(0.024)	(0.008)
Event Year= −1	0.018	0.003	0.009	0.010
	(0.017)	(0.012)	(0.021)	(0.010)
Event Year= 0	0.051***	0.004	0.040*	0.019
	(0.017)	(0.017)	(0.021)	(0.015)
Event Year= +1	0.023	0.012	0.009	0.030
	(0.017)	(0.021)	(0.022)	(0.023)
Event Year= +2	0.053**	0.003	0.021	0.022
	(0.020)	(0.022)	(0.024)	(0.019)
Event Year= +3	0.054***	-0.006	0.022	0.022
	(0.017)	(0.020)	(0.022)	(0.016)
Event Year=≥ +4	0.030**	-0.022	0.022*	-0.014
	(0.012)	(0.015)	(0.012)	(0.012)
Observations	17293	17106	17293	17106
R^2	0.882	0.863	0.878	0.866
Firm FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
Ratings FE	Yes	Yes	Yes	Yes
TB Controls	Yes	Yes	Yes	Yes
Economy Controls	Yes	Yes	Yes	Yes

Table B8: **Firm Leverage and Tax Enactments: Excluding Firms with Private Placements or rated CCC or worse.** This table presents results from firm-year panel regressions of the evolution of firm leverage around the enactment of state corporate income tax cuts. The sample in columns (1)–(4) is the same as that in Table B1 except that it excludes firms with private placements between 2009Q1 and 2021Q4. Similarly, columns (5)–(8) exclude firms rated "CCC" or worse by lenders relative to the specification in Table B1. Columns (1), (3), (5), and (7) present results for firms below the median in total assets, while columns (2), (4), (6), and (8) present results for firms above the median in total assets.

Exclude:	*Firms with Access to Private Placements*				*Firms rated CCC or worse*			
Dependent variable:	*Long-Term Leverage*		*Total Leverage*		*Long-Term Leverage*		*Total Leverage*	
Sample:	Small	Large	Small	Large	Small	Large	Small	Large
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Event Year= −2	0.010	0.003	0.008	0.005	0.009	0.007	0.007	0.009
	(0.008)	(0.007)	(0.018)	(0.007)	(0.008)	(0.009)	(0.019)	(0.010)
Event Year= −1	0.011	0.003	0.009	0.008	0.012	0.006	0.008	0.012
	(0.011)	(0.011)	(0.015)	(0.010)	(0.012)	(0.010)	(0.016)	(0.010)
Event Year= 0	0.035***	-0.006	0.036**	0.004	0.042***	0.002	0.041***	0.011
	(0.010)	(0.017)	(0.015)	(0.018)	(0.011)	(0.015)	(0.015)	(0.016)
Event Year= +1	0.025*	0.008	0.022	0.018	0.028*	0.015	0.022	0.025
	(0.014)	(0.016)	(0.018)	(0.019)	(0.015)	(0.014)	(0.018)	(0.015)
Event Year= +2	0.033**	0.004	0.026	0.009	0.034**	-0.001	0.024	0.004
	(0.015)	(0.017)	(0.018)	(0.017)	(0.015)	(0.016)	(0.018)	(0.015)
Event Year= +3	0.040***	0.001	0.021	0.011	0.034***	-0.004	0.010	0.005
	(0.012)	(0.015)	(0.016)	(0.013)	(0.012)	(0.015)	(0.016)	(0.014)
Event Year=≥ +4	0.027**	-0.012	0.024**	-0.009	0.024**	-0.017	0.019*	-0.012
	(0.010)	(0.012)	(0.010)	(0.011)	(0.010)	(0.012)	(0.010)	(0.010)
R^2	0.882	0.860	0.876	0.866	0.884	0.864	0.878	0.870
N	18145	17577	18145	17577	17332	17210	17332	17210

Table B9: **Firm Leverage and Tax Enactments: Alternative Sample Criteria for C-corps.** This table presents results from firm-year panel regressions of the evolution of firm leverage around the enactment of state corporate income tax cuts. The sample in columns (1)–(4) is restricted to private companies with at least \$75 or \$50 million in total assets as of the previous year and below \$265 million (the median value of total assets in our main estimation sample) between 2011 and 2017. The sample in columns (5)–(8) is the same as in Table B1, except that it excludes firms in two-digit NAICS industries 61, 62, 71, 72, and 81.

Sample:	*Above \$75 million*		*Above \$50 million*		*Above \$100 million, excluding passthrough industries*			
Dependent variable:	*LT Leverage*	*Tot Leverage*	*LT Leverage*	*Tot Leverage*	*LT Leverage*		*Tot Leverage*	
					Small	Large	Small	Large
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Event Year= −2	0.004	0.008	0.012	0.018*	0.008	0.011	0.007	0.012
	(0.007)	(0.011)	(0.008)	(0.010)	(0.009)	(0.008)	(0.020)	(0.008)
Event Year= −1	0.011	0.012	0.013*	0.017*	0.010	0.011	0.009	0.017
	(0.008)	(0.010)	(0.007)	(0.008)	(0.012)	(0.012)	(0.016)	(0.012)
Event Year= 0	0.018*	0.020**	0.016*	0.015*	0.044***	-0.001	0.046***	0.009
	(0.009)	(0.009)	(0.009)	(0.008)	(0.011)	(0.019)	(0.016)	(0.019)
Event Year= +1	0.008	0.011	0.005	0.005	0.027*	0.019	0.022	0.026
	(0.012)	(0.011)	(0.012)	(0.011)	(0.015)	(0.017)	(0.019)	(0.019)
Event Year= +2	0.023*	0.023**	0.013	0.015*	0.037**	0.002	0.028	0.010
	(0.012)	(0.009)	(0.011)	(0.008)	(0.015)	(0.017)	(0.018)	(0.016)
Event Year= +3	0.017	0.008	0.008	0.005	0.042***	0.012	0.019	0.021
	(0.016)	(0.011)	(0.011)	(0.009)	(0.015)	(0.016)	(0.017)	(0.015)
Event Year=≥ +4	0.015	0.017***	0.011	0.014*	0.024**	-0.009	0.019*	-0.008
	(0.009)	(0.006)	(0.008)	(0.007)	(0.011)	(0.013)	(0.011)	(0.012)
R^2	0.849	0.842	0.818	0.817	0.874	0.861	0.871	0.865
N	32451	32451	48902	48902	17311	17092	17311	17092

Table B10: **Firm Leverage and Tax Enactments: Alternative Size Thresholds.** This table presents results from firm-year panel regressions of the evolution of firm leverage around the enactment of state corporate income tax cuts. Columns (1), (4), and (7) present results for the full sample; columns (2), (5), and (8) present results for firms with prior-year total assets below $150, 200, 250, and 300 million; columns (3), (6), and (9) present results for firms above $150, 200, 250, and 300 million in prior-year total assets.

Cutoff:	*$150*		*$200*		*$250*		*$300*	
Dependent variable:	*Long-Term Debt*							
Sample:	Small	Large	Small	Large	Small	Large	Small	Large
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Event Year= -2	0.018	0.005	0.014	0.010	0.006	0.010	0.006	0.011
	(0.015)	(0.006)	(0.009)	(0.008)	(0.011)	(0.008)	(0.008)	(0.008)
Event Year= -1	-0.005	0.007	0.013	0.006	0.011	0.008	0.000	0.003
	(0.022)	(0.008)	(0.014)	(0.009)	(0.012)	(0.010)	(0.010)	(0.011)
Event Year= 0	-0.000	-0.001	0.028**	-0.001	0.036***	-0.000	0.033***	-0.010
	(0.026)	(0.012)	(0.014)	(0.012)	(0.012)	(0.016)	(0.009)	(0.015)
Event Year= +1	-0.000	0.008	0.029	0.009	0.022	0.013	0.016	0.014
	(0.026)	(0.010)	(0.018)	(0.011)	(0.016)	(0.016)	(0.014)	(0.016)
Event Year= +2	0.007	0.012	0.023	0.012	0.026*	0.003	0.030**	-0.002
	(0.023)	(0.013)	(0.017)	(0.011)	(0.015)	(0.016)	(0.015)	(0.016)
Event Year= +3	0.010	-0.003	0.027*	0.000	0.028**	0.007	0.030**	0.006
	(0.021)	(0.012)	(0.015)	(0.012)	(0.013)	(0.015)	(0.011)	(0.017)
Event Year=$\geq +4$	0.017	-0.013	0.026**	-0.014	0.025**	-0.018	0.023**	-0.013
	(0.015)	(0.013)	(0.012)	(0.010)	(0.011)	(0.012)	(0.011)	(0.011)
R^2	0.914	0.847	0.893	0.851	0.882	0.856	0.873	0.860
N	7903	29223	13757	23631	17768	19749	20870	16860

Table B11: **Firm Leverage and Tax Enactments: Controls from Canonical Leverage Regressions or No Controls.** This table presents results from firm-year panel regressions of the evolution of firm leverage around the enactment of state corporate income tax cuts. In columns (1)–(4) we include canonical leverage regressions controls: profitability (prior-year net income divided by prior-year sales), PPE (prior-year fixed assets divided by prior-year total assets), and sales growth (current year sales minus prior-year sales, divided by prior-year sales) in addition to the controls and fixed effects in Table B1. In Columns (5)–(8), we only include firm and industry-by-year fixed effects but no further controls. Columns (1), (3), (5), and (7) present results for firms below the median in total assets, while column (2), (4), (6), and (8) present results for firms above the median in total assets.

	Canonical Leverage Controls				*No Controls*			
Dependent variable:	*Long-Term Leverage*		*Total Leverage*		*Long-Term Leverage*		*Total Leverage*	
Sample:	Small	Large	Small	Large	Small	Large	Small	Large
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Event Year$= -2$	0.014	-0.004	0.014	-0.002	0.012	0.008	0.009	0.011*
	(0.011)	(0.011)	(0.023)	(0.010)	(0.009)	(0.006)	(0.018)	(0.006)
Event Year$= -1$	0.019	0.011	0.016	0.016	0.011	-0.000	0.007	0.003
	(0.015)	(0.014)	(0.019)	(0.011)	(0.009)	(0.009)	(0.013)	(0.007)
Event Year$= 0$	0.046***	0.001	0.051**	0.006	0.032***	-0.005	0.041***	0.004
	(0.015)	(0.015)	(0.021)	(0.014)	(0.010)	(0.017)	(0.013)	(0.016)
Event Year$= +1$	0.021	0.014	0.025	0.024	0.023**	0.008	0.024	0.021
	(0.018)	(0.019)	(0.024)	(0.020)	(0.011)	(0.014)	(0.016)	(0.015)
Event Year$= +2$	0.030*	-0.016	0.023	-0.012	0.035***	-0.000	0.036**	0.011
	(0.017)	(0.016)	(0.020)	(0.012)	(0.012)	(0.013)	(0.015)	(0.013)
Event Year$= +3$	0.033*	-0.001	0.031	0.009	0.035***	0.005	0.017	0.021*
	(0.017)	(0.020)	(0.020)	(0.015)	(0.010)	(0.011)	(0.015)	(0.011)
Event Year$=\geq +4$	0.004	-0.012	-0.005	-0.001	0.018**	-0.012	0.015	-0.006
	(0.012)	(0.012)	(0.010)	(0.014)	(0.009)	(0.010)	(0.009)	(0.010)
Log(Net Sales)	0.036***	0.017	0.049***	0.013				
	(0.010)	(0.014)	(0.010)	(0.013)				
Net Income	0.006	0.015	0.006	0.029*				
	(0.016)	(0.018)	(0.021)	(0.017)				
PPE	0.096***	0.055*	0.077**	0.063***				
	(0.025)	(0.028)	(0.029)	(0.024)				
Sales Growth	0.079***	0.078***	0.140***	0.088***				
	(0.014)	(0.016)	(0.019)	(0.018)				
R^2	0.899	0.890	0.896	0.899	0.878	0.856	0.872	0.860
N	11998	11694	11998	11694	18783	18787	18783	18787

Table B12: **Firm Leverage and Tax Enactments: Accounting for Effective Dates of Corporate and Sales Tax Changes.** This table presents results from firm-year panel regressions of the evolution of firm leverage around the enactment of state corporate income tax cuts. This specification starts with our baseline but also includes indicators for the effective dates of corporate tax cuts and hikes in an event study fashion. Similarly, columns (5)–(8) augment the specifications in Table B1 for adding indicators for the effective dates of state sales tax cuts and hikes in an event study fashion. Columns (1), (3), (5), and (7) present results for firms below the median in total assets, and column (2), (4), (6), and (8) present results for firms above the median in total assets.

Additional Controls:	*Indicators for Effective Dates of Tax Cuts and Hikes*				*Indicators for Sales Tax Cuts and Hikes*			
Dependent variable:	*Long-Term Leverage*		*Total Leverage*		*Long-Term Leverage*		*Total Leverage*	
Sample:	Small	Large	Small	Large	Small	Large	Small	Large
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Event Year= −2	0.003	0.009	0.003	0.009	0.007	0.004	0.007	0.007
	(0.012)	(0.008)	(0.021)	(0.009)	(0.010)	(0.008)	(0.021)	(0.008)
Event Year= −1	0.005	0.009	0.009	0.014	0.006	0.000	0.007	0.007
	(0.015)	(0.012)	(0.019)	(0.013)	(0.013)	(0.011)	(0.018)	(0.010)
Event Year= 0	0.041**	-0.013	0.053*	-0.004	0.036***	-0.007	0.035*	0.003
	(0.020)	(0.018)	(0.027)	(0.021)	(0.013)	(0.019)	(0.017)	(0.019)
Event Year= +1	0.039	0.014	0.039	0.023	0.020	0.014	0.019	0.026*
	(0.026)	(0.022)	(0.029)	(0.026)	(0.016)	(0.014)	(0.020)	(0.015)
Event Year= +2	0.062***	0.017	0.047*	0.023	0.035**	-0.002	0.032	0.006
	(0.020)	(0.022)	(0.027)	(0.024)	(0.017)	(0.013)	(0.019)	(0.014)
Event Year= +3	0.056***	0.034	0.025	0.041**	0.037**	0.002	0.021	0.014
	(0.019)	(0.020)	(0.024)	(0.020)	(0.016)	(0.012)	(0.017)	(0.013)
Event Year=≥ +4	0.032***	-0.004	0.030***	-0.005	0.021	-0.013	0.017	-0.005
	(0.010)	(0.019)	(0.010)	(0.015)	(0.014)	(0.010)	(0.015)	(0.012)
R^2	0.880	0.857	0.874	0.862	0.880	0.857	0.874	0.862
N	18783	18787	18783	18787	18783	18787	18783	18787

Table B13: **Enactment Dates of Tax Cuts and Firm Leverage: Alternative Measures of Leverage.** This table presents results from firm-year panel regressions of the evolution of firm leverage around the enactment of state corporate income tax cuts. In columns (1)–(4), we measure leverage using the natural log of long-term debt or total debt. In columns (5)–(8), we measure leverage using current-year long-term debt or total debt, both scaled by current-year total assets. Columns (1), (3), (5), and (7) present results for firms below the median in total assets, and columns (2), (4), (6), and (8) present results for firms above the median in total assets.

Dependent variable:	*Log(Long-Term Debt)*		*Log(Total Debt)*		*Long-Term Leverage*		*Total Leverage*	
Sample:	Small (1)	Large (2)	Small (3)	Large (4)	Small (5)	Large (6)	Small (7)	Large (8)
Event Year= -2	0.054	0.034	0.011	0.023	0.012*	0.004	0.012	0.005
	(0.051)	(0.042)	(0.077)	(0.032)	(0.006)	(0.006)	(0.014)	(0.006)
Event Year= -1	0.052	-0.029	0.023	-0.002	0.004	-0.003	0.002	0.001
	(0.072)	(0.066)	(0.073)	(0.042)	(0.009)	(0.007)	(0.011)	(0.007)
Event Year= 0	0.158**	-0.047	0.170**	-0.034	0.023***	-0.011	0.025**	-0.008
	(0.071)	(0.085)	(0.068)	(0.048)	(0.008)	(0.009)	(0.010)	(0.009)
Event Year= +1	0.131	-0.039	0.169**	-0.039	0.018	-0.002	0.020	0.000
	(0.087)	(0.088)	(0.076)	(0.056)	(0.011)	(0.010)	(0.013)	(0.012)
Event Year= +2	0.189*	-0.075	0.203**	-0.023	0.027**	-0.003	0.025*	-0.001
	(0.100)	(0.094)	(0.085)	(0.051)	(0.013)	(0.012)	(0.013)	(0.012)
Event Year= +3	0.215*	-0.027	0.152*	0.064	0.029***	0.008	0.014	0.017
	(0.108)	(0.136)	(0.088)	(0.065)	(0.010)	(0.013)	(0.014)	(0.013)
Event Year=$\geq +4$	0.174**	-0.039	0.141***	-0.038	0.021**	-0.003	0.019*	-0.002
	(0.076)	(0.119)	(0.050)	(0.046)	(0.010)	(0.009)	(0.010)	(0.010)
Observations	16110	17042	17174	17528	18783	18787	18783	18787
R^2	0.869	0.916	0.873	0.922	0.907	0.899	0.912	0.909

Table B14: **Enactment Dates of Tax Cuts and Firm Leverage: Accounting for Regional Trends.** This table presents results from firm-year panel regressions of the evolution of firm leverage around the enactment of state corporate income tax cuts. The specification includes Census Region-by-year fixed effects. Columns (1) and (3) present results for firms below the median in total assets, and columns (2) and (4) present results for firms above the median in total assets.

Dependent variable:	*Long-Term Leverage*		*Total Leverage*	
Sample:	Small	Large	Small	Large
	(1)	(2)	(3)	(4)
Event Year= -2	0.012	0.010	0.012	0.013*
	(0.010)	(0.008)	(0.018)	(0.007)
Event Year= -1	0.010	0.010	0.013	0.017*
	(0.013)	(0.009)	(0.015)	(0.009)
Event Year= 0	0.038***	-0.006	0.036**	0.009
	(0.012)	(0.017)	(0.015)	(0.017)
Event Year= +1	0.021	0.016	0.016	0.031*
	(0.015)	(0.016)	(0.019)	(0.018)
Event Year= +2	0.037**	-0.001	0.027	0.010
	(0.014)	(0.017)	(0.018)	(0.017)
Event Year= +3	0.037***	-0.003	0.005	0.014
	(0.013)	(0.014)	(0.019)	(0.013)
Event Year=$\geq$ +4	0.024**	-0.017	0.014	-0.011
	(0.012)	(0.012)	(0.011)	(0.011)
Observations	18783	18787	18783	18787
R^2	0.880	0.857	0.874	0.862

Table B15: **Enactment Dates of Tax Cuts and Firm Leverage: Single-state firms.** This table presents results from firm-year panel regressions of the evolution of firm leverage around the enactment of state corporate income tax cuts. The sample in columns (1)–(4) starts with the baseline from Table B1, but it also excludes firms that operate in multiple states according to the S&P or the NETS data. Specifically, if either the NETS or the S&P Cross Reference Database identifies a company as multi-state, we exclude it from the sample. In columns (5)–(8), starting with the same baseline sample, we exclude firms that borrow through subsidiaries located in multiple states during the sample period according to the Y-14 data. 897 firms corresponding to 3,109 firm-years borrow through subsidiaries in multiple states during the sample period. Columns (1), (3), (5), (7) present results for firms below the median value of total assets in the main sample, while columns (2), (4), (6), and (8) present results for firms above the median assets.

	Single state of operations				*Single state of borrowing*			
Dependent variable:	*Long-term Leverage*		*Total Leverage*		*Long-term Leverage*		*Total Leverage*	
Sample:	Small	Large	Small	Large	Small	Large	Small	Large
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Event Year= −2	0.002	0.006	0.004	0.007	0.011	0.007	0.012	0.010
	(0.010)	(0.012)	(0.025)	(0.012)	(0.009)	(0.008)	(0.019)	(0.008)
Event Year= −1	0.007	0.000	0.003	0.010	0.011	0.008	0.014	0.013*
	(0.015)	(0.020)	(0.022)	(0.021)	(0.011)	(0.008)	(0.015)	(0.007)
Event Year= 0	0.046**	-0.006	0.048**	0.004	0.048***	-0.001	0.050***	0.008
	(0.018)	(0.020)	(0.021)	(0.020)	(0.010)	(0.013)	(0.014)	(0.012)
Event Year= +1	0.032*	0.003	0.029	0.020	0.033***	0.018	0.035**	0.028*
	(0.017)	(0.024)	(0.023)	(0.024)	(0.011)	(0.014)	(0.016)	(0.015)
Event Year= +2	0.028*	0.007	0.029	0.016	0.043***	0.003	0.040**	0.014
	(0.017)	(0.024)	(0.025)	(0.026)	(0.012)	(0.015)	(0.015)	(0.013)
Event Year= +3	0.031	0.005	0.009	0.016	0.039***	0.009	0.022	0.023*
	(0.018)	(0.028)	(0.024)	(0.025)	(0.010)	(0.015)	(0.014)	(0.012)
Event Year=≥ +4	0.031**	-0.005	0.028*	-0.010	0.021**	-0.011	0.020**	-0.008
	(0.013)	(0.019)	(0.015)	(0.022)	(0.010)	(0.011)	(0.010)	(0.009)
Observations	10639	10186	10639	10186	17682	16916	17682	16916
R^2	0.901	0.872	0.882	0.876	0.882	0.866	0.877	0.870

Table B16: **Enactment Dates of Tax Cuts and Firm Leverage: Significant Tax Cuts.** This table presents results from firm-year panel regressions of the evolution of firm leverage around the enactment of state corporate income tax cuts, focusing on tax cut enactments that reduce corporate income tax rates by at least 0.5% (columns 1-3) or 1% (columns 4-6). Columns (1) and (3) present results for firms below the median in total assets, and columns (2) and (4) present results for firms above the median in total assets.

Dependent variable:	*Long-Term Leverage*			
	$\lvert\Delta\tau\rvert \geq 0.5\%$		$\lvert\Delta\tau\rvert \geq 1\%$	
Sample:	Small	Large	Small	Large
	(1)	(2)	(3)	(4)
Event Year= −2	0.017	0.010	0.009	0.007
	(0.011)	(0.008)	(0.012)	(0.010)
Event Year= −1	0.021	0.014	0.027*	0.012
	(0.013)	(0.011)	(0.015)	(0.013)
Event Year= 0	0.050***	0.002	0.045**	0.007
	(0.013)	(0.016)	(0.019)	(0.018)
Event Year= +1	0.034**	0.009	0.020	0.006
	(0.016)	(0.017)	(0.020)	(0.026)
Event Year= +2	0.043***	0.005	0.054**	-0.004
	(0.016)	(0.016)	(0.023)	(0.021)
Event Year= +3	0.047***	0.015	0.057***	0.003
	(0.014)	(0.014)	(0.017)	(0.016)
Event Year=≥ +4	0.027**	-0.015	0.031**	-0.022
	(0.012)	(0.013)	(0.013)	(0.015)
Observations	18783	18787	18783	18787
R^2	0.880	0.857	0.880	0.857

Table B17: **Leverage and Tax Hikes.** This table presents results from firm-year panel regressions of the evolution of firm leverage outcomes around the enactment of state corporate income tax hikes. Columns (1) and (3) present results for firms below the median in total assets, and columns (2) and (4) present results for firms above the median in total assets. Columns 1-2 exclude the state of New Jersey because that tax hike happens in 2018 and only identifies the pre-trend coefficients. Columns 3-4 further exclude the state of Connecticut because their tax hike is not classified as exogenous based on the Giroud and Rauh (2021) algorithm.

Dependent variable:	*Long-Term Leverage*			
Sample:	Small	Large	Small	Large
	(1)	(2)	(3)	(4)
Event Year= -2	-	-	-	-
	-	-	-	-
Event Year= -1	-	-	-	-
	-	-	-	-
Event Year= 0	-0.018	-0.033	-0.036	-0.055*
	(0.024)	(0.029)	(0.024)	(0.033)
Event Year= +1	-0.023	-0.037*	-0.034	-0.041
	(0.020)	(0.021)	(0.022)	(0.028)
Event Year= +2	-0.023	-0.042**	-0.037	-0.055**
	(0.025)	(0.021)	(0.027)	(0.024)
Event Year= +3	-0.032	-0.029	-0.039*	-0.052**
	(0.020)	(0.023)	(0.023)	(0.020)
Event Year=$\geq +4$	-0.034**	-0.038**	-0.039***	-0.041**
	(0.014)	(0.017)	(0.014)	(0.020)
Observations	18163	18220	17911	17932
R^2	0.879	0.858	0.879	0.858

Table B18: **Corporate Tax Enactments and Loan Commitments.** This table shows the evolution of firms' total loan commitments in response to corporate tax hike and cuts enactments in event time. We present event study estimates for the years $k = -2, \ldots, +5$ relative to the enactment year, where $k < 0$ corresponds to years preceding the enactment year, and $k \geq 0$ corresponds to dynamic effects relative to the enactment year. The sample runs from 1992 through 2018 and is restricted to private companies with at least $100 million in total commitments as of the previous year. All specifications include both borrower and 4-digit NAICS by year fixed effects, and some specifications include a set of tax base and tax credit controls, as well as lagged changes and levels of state GSP and unemployment rate. The standard errors are double clustered at the state and 4-digit NAICS by year level.Columns 1-4 present results for tax cuts and columns 5-8 for tax hikes. Columns 1-2 and 5-6 only include only 4-digit NAICS industry-by-year fixed effects, while columns 3-4 and 7-8 also include the tax base and economy controls from our baseline specifications. The odd columns present results for all firms with at least $100 million in commitment as of the previous year and the even columns further restrict the sample to firms with no loan loss reserves as of the previous year. Log(Commitments) is the natural log of the dollar value of firms' total syndicated commitments in a a given year.

Event Indicators:	*Tax Cuts*				*Tax Hikes*			
Dependent Variable:	*Log(Commitments)*							
Sample:	Full	Low-risk	Full	Low-risk	Full	Low-risk	Full	Low-risk
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Event Year= −2	0.016	0.020	0.003	0.010	0.007	0.014	0.007	0.016
	(0.016)	(0.015)	(0.016)	(0.016)	(0.029)	(0.029)	(0.026)	(0.026)
Event Year= −1	0.010	0.022	0.005	0.017	-0.057	-0.040	-0.058	-0.039
	(0.018)	(0.017)	(0.017)	(0.017)	(0.037)	(0.041)	(0.035)	(0.039)
Event Year= 0	0.017	0.029	0.010	0.022	-0.057	-0.061	-0.058	-0.059
	(0.018)	(0.020)	(0.017)	(0.018)	(0.045)	(0.046)	(0.044)	(0.046)
Event Year= +1	0.041**	0.049**	0.028	0.036*	-0.062**	-0.055**	-0.071**	-0.068**
	(0.019)	(0.019)	(0.018)	(0.019)	(0.027)	(0.024)	(0.030)	(0.028)
Event Year= +2	0.028*	0.035**	0.013	0.020	-0.064**	-0.060**	-0.060**	-0.059**
	(0.015)	(0.015)	(0.016)	(0.016)	(0.028)	(0.029)	(0.028)	(0.028)
Event Year= +3	0.021	0.025	-0.000	0.003	-0.075	-0.073	-0.071	-0.070
	(0.015)	(0.015)	(0.018)	(0.018)	(0.047)	(0.050)	(0.045)	(0.048)
Event Year= +4	0.005	0.006	-0.015	-0.015	-0.016	-0.023	-0.011	-0.018
	(0.021)	(0.021)	(0.025)	(0.026)	(0.041)	(0.042)	(0.041)	(0.041)
Event Year≥ +5	0.006	0.007	-0.006	-0.005	0.001	-0.010	0.004	-0.008
	(0.022)	(0.021)	(0.022)	(0.022)	(0.021)	(0.021)	(0.020)	(0.020)
R^2	.794	.798	.794	.798	.794	.798	.794	.798
N	50539	48397	50321	48202	50539	48397	50321	48202
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
TB Controls	No	No	Yes	Yes	No	No	Yes	Yes
Economy Controls	No	No	Yes	Yes	No	No	Yes	Yes

Table B19: **Corporate Tax Enactments and Bank Risk Metrics.** This table shows the evolution of firms' risk metrics around the enactment of state corporate income tax changes. The outcome variable, NonPass, in column 1 and 3 (column 2 and 4) takes the value of 1 whenever the lead banks (bank supervisors) rate all loans of firm i in year t as below the "pass" risk rating category in terms of the Supervisory Rating Scale. The supervisory ratings are available for the entire time series of the data, while lead bank ratings are only available starting in 1996.

Event Indicators:	*Tax Cuts*		*Tax Hikes*	
Dependent Variable:		*NonPass*		
Sample:	1996-	Full	1996-	Full
	(1)	(2)	(3)	(4)
Event Year= −2	-0.005	-0.003	0.005	0.007
	(0.010)	(0.007)	(0.015)	(0.016)
Event Year= −1	-0.007	0.000	0.001	0.006
	(0.008)	(0.005)	(0.020)	(0.024)
Event Year= 0	-0.014	-0.006	-0.021	-0.015
	(0.009)	(0.008)	(0.016)	(0.017)
Event Year= +1	-0.022**	-0.007	-0.008	-0.012
	(0.009)	(0.010)	(0.015)	(0.012)
Event Year= +2	-0.021**	-0.019***	-0.017	-0.012
	(0.009)	(0.007)	(0.021)	(0.015)
Event Year= +3	-0.017*	-0.010	-0.007	-0.002
	(0.009)	(0.010)	(0.013)	(0.012)
Event Year= +4	-0.012	-0.015*	-0.011	-0.001
	(0.008)	(0.008)	(0.014)	(0.010)
Event Year≥ +5	-0.008	-0.005	-0.004	-0.008
	(0.005)	(0.004)	(0.007)	(0.006)
R^2	0.401	0.429	0.401	0.429
N	47244	50203	47244	50203
Firm FE	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
TB Controls	No	No	No	No
Economy Controls	No	No	No	No

Table B20: **Alternative Model Estimations.** This table reports the parameters and moments from the estimation of variants of our model from Section 5. The first variant has a collateral constraint; the second has endogenous default; and the third has costly equity issuance. We use the subsample of small firms from the Y-14 data. The sample period is 2011–2017. The parameter θ is the curvature of the production function; ρ is the serial correlation of the shock process; σ is the standard deviation of the shock process; δ is the capital depreciation rate; ψ governs quadratic capital adjustment costs; ξ is the deadweight cost of default; and f is the fixed operating cost. 'Debt-to-assets' is defined as long-term debt divided by lagged total assets; 'investment-to-assets' is defined as net capital expenditures divided by fixed assets; 'income-to-assets' is defined as EBITDA divided by lagged total assets; 'debt issuance' is defined as the change in debt scaled by lagged total assets. The t-statistics next to the simulated and actual moments are based on the null hypothesis of the equality of each of these moment pairs.

A. Parameter estimates

	θ	ρ	σ	δ	ψ	ξ	f
Collateral	0.9339	0.6415	0.0773	0.0955	0.1376	0.5427	0.1616
	(0.0370)	(0.0169)	(0.0126)	(0.0024)	(0.1448)	(0.0534)	(0.0669)
Endogenous Default	0.9233	0.6421	0.0777	0.0888	0.1326	0.6602	0.1605
	(0.0472)	(0.0359)	(0.0098)	(0.0010)	(0.0564)	(2.1243)	(0.0742)
Equity Issuance	0.9484	0.6568	0.0900	0.1145	0.1580	0.6479	0.2043
	(0.0170)	(0.0248)	(0.0039)	(0.0021)	(0.0391)	(0.0114)	(0.0251)

B. Targeted moments

		Collateral		Endogenous default		Equity issuance	
	Actual	Simulated	t-stat.	Simulated	t-stat.	Simulated	t-stat.
Mean debt-to-assets	0.3677	0.3665	0.2624	0.3659	3.5807	0.3631	0.1102
Standard deviation debt-to-assets	0.1066	0.1185	-0.2963	0.1185	-0.7512	0.0791	0.1123
Mean investment-to-assets	0.1439	0.0987	0.3197	0.0918	0.2011	0.1180	0.3209
Standard deviation investment-to-assets	0.0817	0.0814	0.0064	0.0788	0.0370	0.0866	-0.0547
Serial correlation investment-to-assets	0.5925	0.3402	0.1179	0.3027	0.2188	0.2985	0.1547
Mean income-to-assets	0.1547	0.1572	-0.0337	0.1600	-0.0976	0.1592	-0.2690
Standard deviation income-to-assets	0.0593	0.0269	0.1117	0.0253	0.0766	0.0395	0.0694
Serial correlation income-to-assets	0.8483	0.6487	0.1279	0.6060	0.5852	0.7238	0.0584
Tax elasticity	0.0214	0.0122	0.0042	0.0154	0.0030	0.0209	0.0009

Table B21: Summary Statistics of Risk Metrics

Table B21 presents summary statistics for firm-quarters in the Federal Reserve's Y-14 Collection between 2012 and 2018. We present summary statistics for the probability of default. Commitment amount refers to the sum of credit line and term loan commitments, while utilized amount is the sum of term loans and the outstanding drawn amounts under credit lines.

variable	**mean**	**sd**	**p10**	**p25**	**p50**	**p75**	**p90**
	$10 < Total Commitments < $100 Million						
Probability of default	0.0214	0.0431	0.0016	0.0035	0.0076	0.0191	0.0500
	Total Commitments ≥ $10 Million						
Probability of default	0.0211	0.0426	0.0016	0.0033	0.0075	0.0191	0.0479
	$10 < Total Commitments < $50 Million						
Probability of default	0.0214	0.0432	0.0016	0.0035	0.0076	0.0191	0.0482
	$20 < Total Commitments < $100 Million						
Probability of default	0.0220	0.0428	0.0016	0.0037	0.0085	0.0200	0.0534
	$50 < Total Commitments < $100 Million						
Probability of default	0.0218	0.0419	0.0016	0.0034	0.0086	0.0200	0.0534
	Total Commitments ≥ $100 Million						
Probability of default	0.0185	0.0390	0.0015	0.0026	0.0066	0.0175	0.0401
	Total Commitments ≥ $50 Million						
Probability of default	0.0206	0.0414	0.0015	0.0032	0.0076	0.0191	0.0479

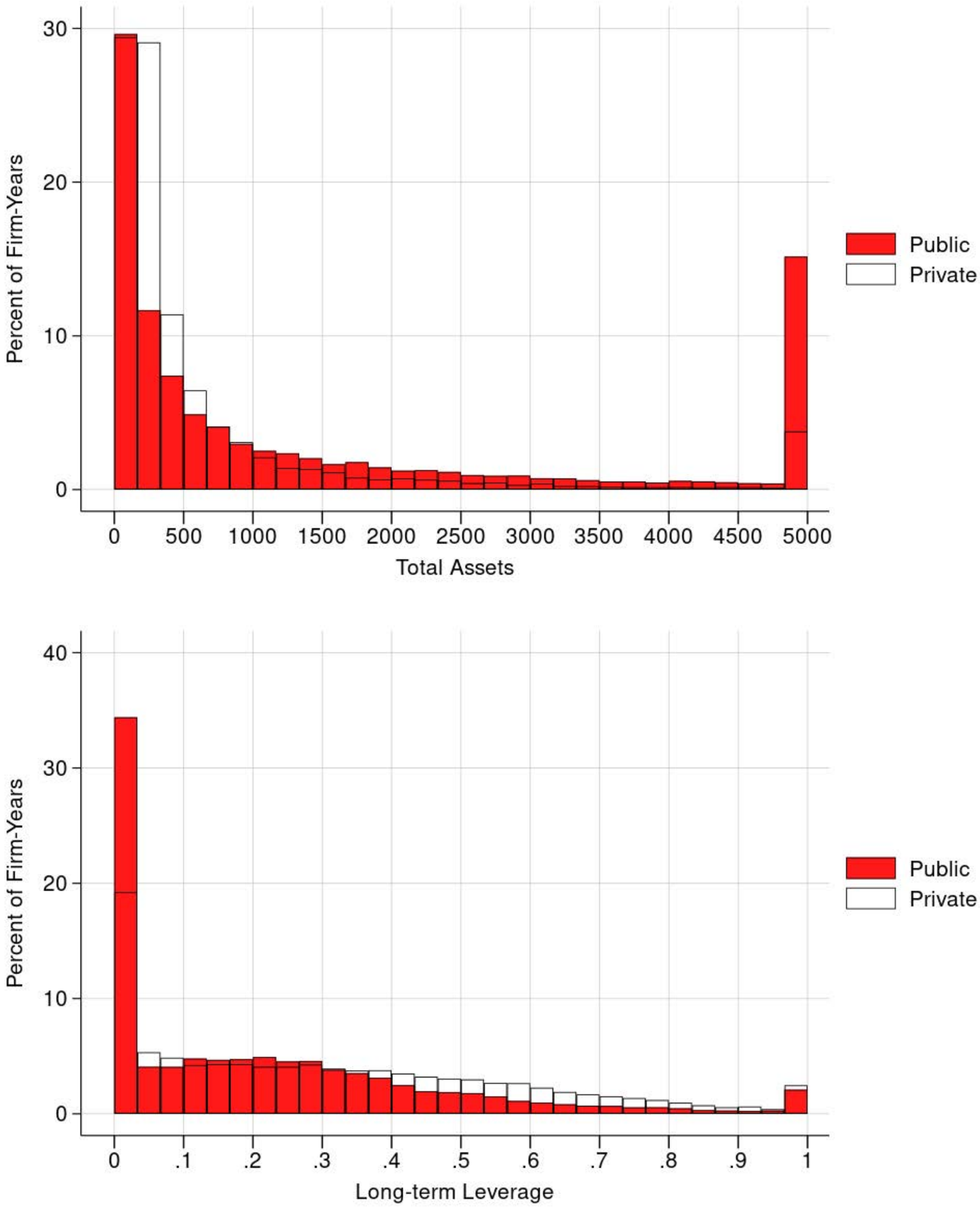


Figure B1: **Distribution of Leverage and Assets for Private and Public Firms.** This figure plots histograms of total firm assets and leverage for our samples of firms from the Y-14 data and from Compustat. White indicates the Y-14 sample, and red indicates the Compustat sample.

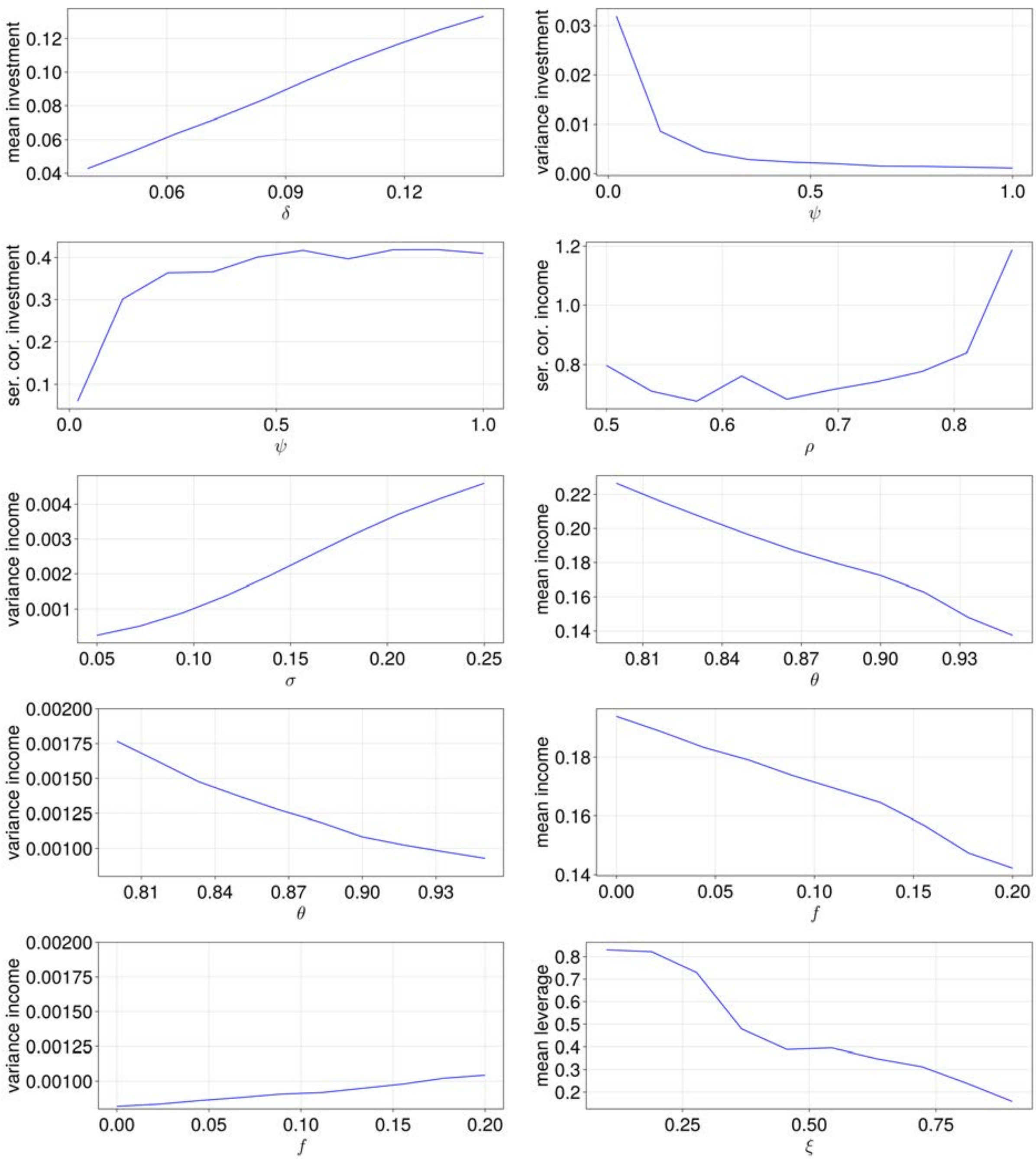


Figure B2: **Selected Model Comparative Statics.** Each panel in this figure plots a model-generated moment as a function of a model parameter. For each of these panels, the model is solved 10 times for different levels of the parameter on the x-axis, so the plot represents the observed relation between a moment and this parameter. The parameter θ is the curvature of the production function; ρ is the serial correlation of the shock process; σ is the standard deviation of the shock process; δ is the capital depreciation rate; ψ governs quadratic capital adjustment costs; ξ is the deadweight cost of default; and f is the fixed operating cost.

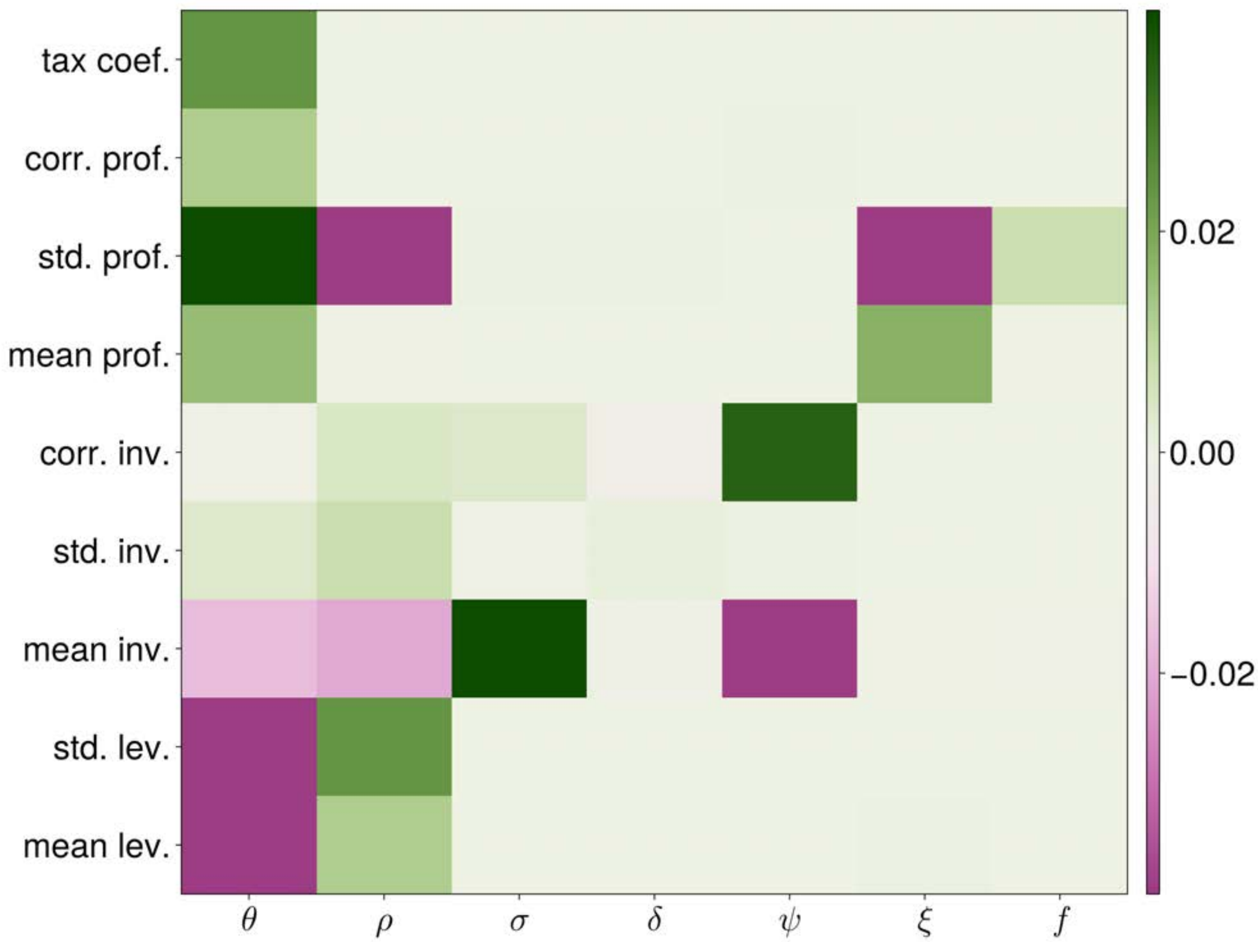


Figure B3: **Local Identification Diagnostic** This heat map depicts the identification diagnostic from Andrews et al. (2017). Each cell can be interpreted as the effect on a parameter of a one-standard-deviation change in a moment. The parameter θ is the curvature of the production function; ρ is the serial correlation of the shock process; σ is the standard deviation of the shock process; δ is the capital depreciation rate; ψ governs quadratic capital adjustment costs; ξ is the deadweight cost of default; and f is the fixed operating cost.

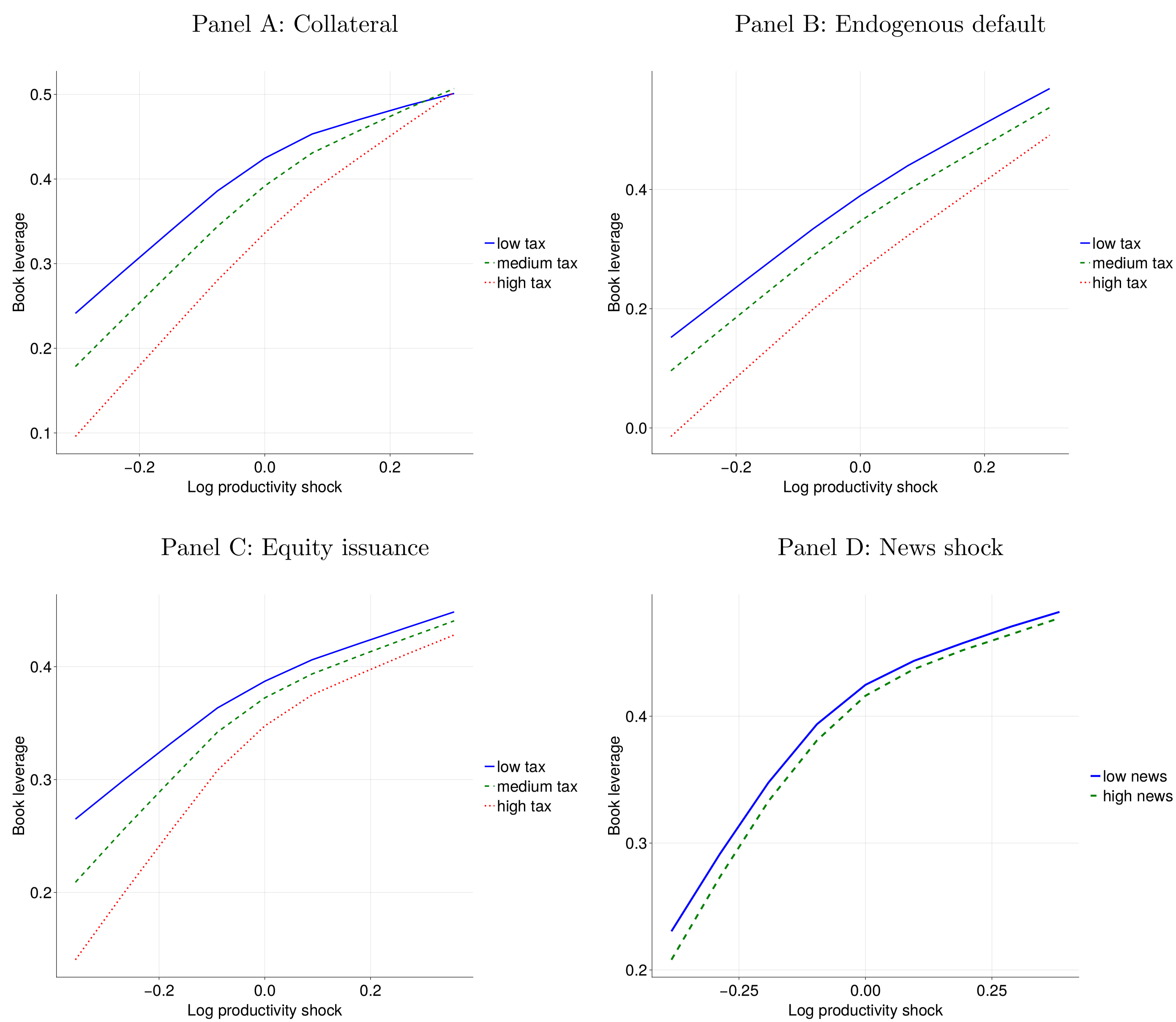


Figure B4: **Optimal Policies in Alternative Models.** This figure plots optimal leverage policy as a function of the profitability shock for three alternative models: a model with a collateral constraint and no equity issuance (Panel A), a model with endogenous default and no equity issuance (Panel B), and a model with a net-worth default condition and costly equity issuance (Panel C), and a model with a net-worth default condition, no equity issuance, and a news shock (Panel D)

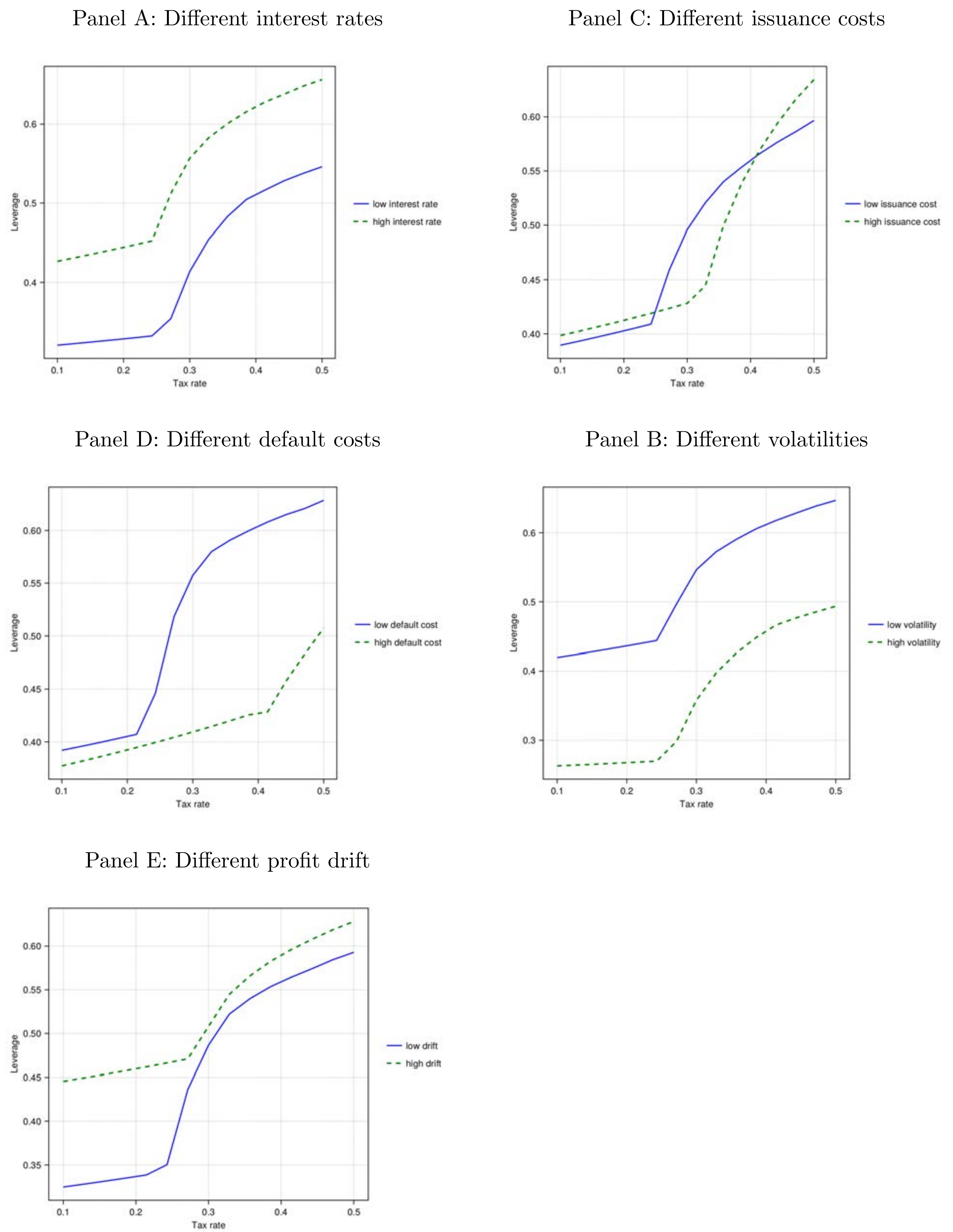


Figure B5: **Comparative statics from Goldstein et al. (2001)** This figure plots comparative statics from the dynamic model in Goldstein et al. (2001). Each panel contains plots of optimal leverage versus the tax rates for two different parameter values, which are indicated in the legend.