Journal of Accounting Research

DOI: 10.1111/1475-679X.12157
Journal of Accounting Research
Vol. 55 No. 3 June 2017
Printed in U.S.A.

CHICAGO BOOTH

Sharing Risk with the Government: How Taxes Affect Corporate Risk Taking

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Received 19 December 2015; accepted 18 October 2016

ABSTRACT

Using 113 staggered changes in corporate income tax rates across U.S. states, we provide evidence on how taxes affect corporate risk-taking decisions. Higher taxes reduce expected profits more for risky projects than for safe ones, as the government shares in a firm's upside but not in its downside. Consistent with this prediction, we find that risk taking is sensitive to taxes, albeit asymmetrically: the average firm reduces risk in response to a tax increase (primarily by changing its operating cycle and reducing R&D risk) but does not respond to a tax cut. We trace the asymmetry back to constraints on risk taking imposed by creditors. Finally, tax loss-offset rules moderate firms'

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Accepted by Christian Leuz. We gratefully acknowledge helpful comments from two anonymous reviewers; Morten Bennedsen, Nathan Goldman, Abhiroop Mukherjee, and Stefan Zeume (our discussants); Eli Bartov, Sanjeev Bhojraj, Robert Bloomfield, Agnes Cheng, Robert Engle, Michelle Hanlon, Shane Heitzman, Sudarshan Jayaraman, Andrew Karolyi, Anne Marie Knott, Clive Lennox, Ji-Chai Lin, Kenneth Merkley, Jeffrey Ng, Joseph Piotroski, Mark Soliman, K.R. Subramanyam, Ross Watts, John Wei, Haibin Wu, Eric Yeung, Paul Zarowin, and Zilong Zhang; as well as participants at various seminars and conferences. We thank Charles Choi and Chuchu Liang for competent research assistance. Zuo gratefully acknowledges generous financial support from the Institute for the Social Sciences at Cornell University. An online appendix to this paper can be downloaded at http://research.chicagobooth.edu/arc/journal-of-accounting-research/online-supplements.

sensitivity to taxes by allowing firms to partly share downside risk with the government.

JEL codes: G32; H32

Keywords: risk taking; corporate taxes

1. Introduction

Taxation is one of the most important tools governments use to influence the economy. Taxes affect many aspects of economic activity, from individuals' labor supply, consumption, and savings decisions to companies' hiring, location, and capital investment choices. In this paper, we ask how taxes on corporate income affect corporate risk taking. As Solow [1956] notes, risk taking is essential for both firms and economies to grow in the long run.

Income taxes affect corporate risk taking because they induce an asymmetry in a firm's payoffs. This basic insight can be traced back to early work on *individuals*' risk-taking choices in response to *personal* income taxes (Domar and Musgrave [1944], Feldstein [1969], Stiglitz [1969]) and to subsequent applications to the corporate setting (Green and Talmor [1985]). A simple numerical example serves to illustrate the intuition. Suppose there are two projects (A and B) and two equally likely outcomes ("good" and "bad"). Project A yields a profit of \$40 in both scenarios; project B yields a profit of \$100 in the good scenario but a loss of \$20 in the bad scenario. Project risk is idiosyncratic and hence diversifiable. Absent taxes, the expected profit of each project is \$40 and so a risk-neutral firm will be indifferent between the projects.

Now suppose the tax rate increases from zero to 30%. This reduces the expected after-tax profit of both projects, but risky project B is more affected than safe project A: B's expected profit falls to \$25 while A's falls to only \$28.[1] The greater reduction (of \$3) in project B's expected profit stems from the fact that the government shares in the profit but not in the loss. Given this asymmetry, a risk-neutral firm will now prefer the safe project to the risky project.[2] Generalizing from the example, we predict that firms should respond to a tax increase by choosing safer projects and thereby reducing the risks they take.

As Domar and Musgrave [1944] argue, introducing loss-offsets into the tax code can modify this prediction. Consider the extreme case in which losses can be completely written off against past or future profits. In this case, the pretax and posttax ordering of the two projects is identical because both the upside and the downside are reduced at the same tax rate.[3] In practice, the tax code permits at most a partial offset of losses, in which

[1] For project A, $\$40 \times (1 - 0.3) = \$28$; for project B, $0.5 \times [(1 - 0.3) \times \$100 - \$20] = \$25$.

[2] Firms are commonly modeled as being risk-neutral, but this is not crucial to the argument.

[3] In our numerical example, project B's expected profit with full loss offsets is \$28, the same as A's.

case the upside is reduced by more than the downside. A tax increase will then reduce the expected profit of the risky project by more than that of the safe project and firms should respond by reducing risk.

Absent other frictions, these arguments apply symmetrically to tax increases and tax cuts, so firms should respond to cuts by increasing risk. In practice, there is reason to expect asymmetry. As the literature on risk-shifting emphasizes, higher risk reduces the value of claims held by creditors. Whether a firm can respond to a tax cut by increasing risk then depends on the extent to which creditors constrain its behavior, for example, by means of debt covenants. In the presence of such constraints, the effect of a tax cut on risk taking is likely attenuated for many firms.

A key challenge when testing how taxes affect corporate policies is that a firm's tax status is often endogenous to its policies. For example, a firm's choice of investment projects will affect its future marginal tax rate. The literature confronts this identification challenge in various ways. One approach is to exploit changes in federal income tax rates. Unfortunately, federal tax changes suffer from two shortcomings: they are few and far between, and they affect virtually all firms in the economy at the same time, making it difficult to find control firms with which to establish a plausible counterfactual. A second approach is to exploit cross-country differences in tax policies. This typically results in a larger number of tax "shocks" than in studies using federal tax changes, but often requires implausible assumptions about treated firms and their controls being comparable despite operating in different countries.

We adopt a third approach, pioneered by Asker, Farre-Mensa, and Ljungqvist [2015], Heider and Ljungqvist [2015], and Farre-Mensa and Ljungqvist [2016]. The approach exploits the fact that U.S. companies pay not only federal income tax but also taxes in the various states in which they operate. As Heider and Ljungqvist note, state taxes are a meaningful part of U.S. firms' overall tax burden, accounting for about 21% of total income taxes paid in Compustat. Changes in state corporate income tax rates are numerous (we count 113 between 1990 and 2011) and, because they are staggered across states and time, lend themselves to a difference-in-differences research design. As long as the usual identifying assumptions plausibly hold, such a design can disentangle the effects of tax changes from other macroeconomic shocks that affect firms' risk taking by establishing a counterfactual—what level of risk would firms have chosen absent the tax change?—using as controls firms that experience similar economic conditions (in time, space, and industry) but are not themselves subject to a tax change.

By way of preview, we have four main results. First, we find that firms respond to tax increases by reducing their earnings volatility, as expected. To illustrate, a treated firm reduces its earnings volatility by 2.6% in response to the average tax increase of 136 basis points, relative to other firms in the same industry that are not subject to a tax change in their headquarter state that year.[4] This point estimate is likely conservative, given that firms are taxed not just in their home state but in every state in which they have operations (their so called "nexus" states). This means that a given state's tax change applies to less than a multistate firm's entire tax base, so tests that ignore the geographic distribution of a firm's tax base likely understate the sensitivity of risk taking to corporate income taxes. To address this issue, we construct a measure of state tax changes that takes into account each firm's tax exposure to each state. Doing so yields tax sensitivities that are between 25% and 60% larger than our baseline estimates.

Second, investigating possible channels, we find that the main ways in which firms achieve these risk reductions are efforts to shorten their operating cycles (which put less capital at risk, in particular in the form of inventories) and to find less risky ways to commercialize their R&D projects. We find no evidence that firms tinker with their operating leverage, nor that they change the level of their capital expenditures or R&D spending in response to state tax changes.

Third, we find evidence consistent with an asymmetric tax sensitivity: firms do not, on average, respond significantly to tax cuts, especially on a nexus-weighted basis. This finding is consistent with our argument that firms face constraints on their ability to increase risk, for example, in the form of covenants imposed by their creditors. If so, we expect firms with low financial leverage to face fewer constraints and so to be more responsive to tax cuts. Consistent with this conjecture, we find that low-leverage firms increase risk in response to tax cuts (by 6–7% for each percentage-point cut in taxes). High-leverage firms, on the other hand, do not.

Fourth, we find support for Domar and Musgrave's [1944] argument that the ability to offset losses against past or future profits should weaken the negative effect of income taxes on risk taking. To establish this result, we collect detailed information on how state tax loss carryback and carryforward rules have evolved over time. When we sort firms by their ability to offset tax losses, we find that the negative effect of tax increases on risk taking is largely driven by firms with a limited ability to offset losses. We also test how firms respond to the rule changes themselves using a difference-in-differences setup. Firms' responses to changes in offset rules broadly mirror their responses to changes in tax rates. In particular, they asymmetrically reduce risk when their ability to carry back losses is reduced.

A causal interpretation of our findings requires that the difference-in-differences identifying assumptions plausibly hold. Threats to identification in our setting principally come from three directions: a failure of the parallel-trends assumption, the possibility that local shocks trigger both states to change their tax rates and firms to adjust their risk taking, leading to a spurious correlation between tax and risk, and the potential for

[4] This effect is estimated over the three years following a tax increase; it becomes stronger when we give firms more time to adjust their risk profiles. Over the six years following a tax increase, for example, the average treated firm reduces risk by a cumulative 4.8%.

anticipation effects to bias the estimated tax sensitivities. We perform a battery of tests designed to investigate these identification challenges.

A standard dynamic test shows that treated and control firms exhibit similar trends in risk taking before the state tax rate changes, supporting the parallel-trends assumption. To remove potential confounding effects coming from unobserved changes in local economic conditions that diffuse across state borders, we restrict the sample of controls to firms in states neighboring treated firms' home states. This allows us to difference away unobserved confounding effects, assuming that economic conditions are similar in neighboring states while tax policies stop at the state's border. Our results are robust to this design. As for confounds whose effects coincide in time with the treatment and do not diffuse across state borders, we show that coincident policy changes (such as changes in state investment incentive programs) do not drive our results; moreover, an Altonji, Elder, and Taber [2005] type test suggests that selection on unobservables is not a major concern in our setting. Finally, we argue that changes in risk taking of the kind we identify in our setting are not reasonably subject to major adjustment costs, which lessens concerns that anticipation effects bias our estimates (Hennessy and Strebulaev [2015]). Empirically, we find no evidence that firms change risk in anticipation of future tax changes.

Our study makes contributions to several literatures. First, it contributes to the tax literature and to the literature on the effects of taxes on corporate policies by documenting that firms fine-tune their risk profiles when their tax rates change and that they do so in a way that is consistent with the theoretical insights of Domar and Musgrave [1944], Feldstein [1969], and Stiglitz [1969]. Prior work on the effects of taxes on corporate policies largely focuses on firms' capital structure (following Modigliani and Miller [1958]) and investment choices (following Hall and Jorgenson [1967]). The latter body of work is particularly relevant in our context. Its focus is on the effect of taxes on the *level* of investment. As Hines [1998] notes, this focus has met with little empirical success: "The apparent inability of tax incentives to stimulate aggregate investment spending is one of the major puzzles in the empirical investment literature" (p. 2). We extend this body of work by showing that corporate taxes affect firms' choice of risk taking.

Second, our study adds a new angle to the literature on corporate risk taking by identifying taxes as an important determinant. While prior work highlights many other determinants, tax has largely been ignored.[5] A notable exception is a recent working paper by Langenmayr and Lester

[5] Among these other determinants are managerial risk aversion and career concerns (May [1995], Gormley and Matsa [2016]), the sensitivity of CEO wealth to stock volatility (Coles, Daniel, and Naveen [2006]), option compensation (Rajgopal and Shevlin [2002], Gormley, Matsa, and Milbourn [2013]), inside debt (Cassell et al. [2012], Choy, Lin, and Officer [2014]), corporate governance (John, Litov, and Yeung 2008), Sarbanes–Oxley (Bargeron, Lehn, and Zutter [2010]), creditor rights (Acharya, Amihud, and Litov [2011]), and diversification (Faccio, Marchica, and Mura [2011]).

[2015], whose best identified evidence comes from a change in a rule governing tax loss-offsets affecting a limited sample of small Spanish firms.[6] Our study takes a broader perspective. Our main focus, motivated by the analysis of Domar and Musgrave [1944], is not on variation in tax loss-offset rules but on variation in tax rates. Specifically, we focus on the sensitivity of risk taking to tax rates, on the way tax loss-offset provisions moderate this sensitivity, and on asymmetry in this sensitivity in the presence of constraints imposed by creditors on a firm's ability to increase risk.

Compared to the extant empirical literature on corporate risk taking, our main advantage is identification: our difference-in-differences approach not only establishes a set of plausible counterfactuals taken from the same legal, regulatory, and business environment, but it also mitigates, when paired with a focus on adjacent states, omitted-variable biases resulting from the confounding influence of unobserved local shocks. These features of our research design go some way toward permitting a causal interpretation of our results, which is critical for academic research to be informative to policy makers (Leuz and Wysocki [2016]).

Finally, our results add nuance to the hedging literature. A parallel literature on risk management shows that firms hedge to reduce income volatility with a view to increasing debt capacity (Smith and Stulz [1985], Graham and Rogers [2002]). It is possible that increased hedging contributes to the tax-induced reduction in risk taking we observe. This would be interesting because the hedging literature has so far found little support for taxes being an important reason why firms engage in hedging.

2. *Research Design, Institutional Setting, and Sample and Data*

2.1 EMPIRICAL STRATEGY

We use a difference-in-differences framework to estimate the effect of changes in state corporate income tax rates on firms' risk-taking choices. Our baseline regression takes the following form:

$$\Delta Risk_{i,j,s,t} = \beta \Delta T^{+}_{s,t} + \gamma \Delta T^{-}_{s,t} + \theta \Delta Z_{s,t} + \delta \Delta X_{i,t-1} + \alpha_{j,t} + \varepsilon_{i,j,s,t}, \quad (1)$$

where:

i, j, s, and t = firms, industries, states, and years, respectively,

Δ = first-difference operator,

$Risk_{i,j,s,t}$ = measure of firm i's risk taking (defined in subsection 2.3),

$Z_{s,t}$ = state-level control variables measured as of year t,

[6] Using a sharp regression discontinuity design, Langenmayr and Lester find that Spanish firms with revenues just above EUR 20 million significantly reduced their earnings volatility when their ability to offset losses was limited in 2011, as compared to firms just below the revenue threshold. One of our empirical findings is similar, in that U.S. firms reduce risk when state-level offset rules become less generous, though given the differences in research design, we view Langenmayr and Lester's results as having more limited external validity.

$X_{i,t-1} =$ firm-level control variables measured as of $t-1$,[7]
$\alpha_{j,t} =$ SIC4 industry-by-year fixed effects,
$\varepsilon_{i,j,s,t} =$ error term.

Given the state-level nature of the tax variation we exploit, we cluster standard errors by state (Petersen [2009]).[8]

The variables of interest are $\Delta T^{+}_{s,t}$ and $\Delta T^{-}_{s,t}$, which measure the magnitude of a tax increase or a tax cut in either a firm's home state or across its nexus states in year t. Details of these tax changes and how they are computed are provided in subsection 2.2. Since each tax-change variable is measured in absolute terms, corporate risk taking is reduced in response to a tax increase if $\beta < 0$ and increased in response to a tax cut if $\gamma > 0$.

Estimating equation (1) in first-differenced form removes firm-specific fixed effects and potential confounding effects from time-invariant state-level conditions or policies (e.g., political parties or fiscal policies). An advantage of a first-differenced specification over a levels specification with firm fixed effects is that first-differencing can accommodate repeated treatments, treatment reversals, and asymmetry in firms' responses to tax changes. Including industry-by-year fixed effects removes unobserved time-varying industry shocks by comparing the behavior of treated and control firms in the same industry at the same point in time.

To illustrate our research design, consider Pennsylvania (PA). In 1991, PA raised its top corporate income tax rate from 8.5% to 12.25%. Following this tax increase, stock market-listed firms headquartered in PA reduced risk by about 10% on average. From the point of view of an individual firm in PA, this tax shock is plausibly exogenous: presumably, no firm would have lobbied for the tax increase. Exogeneity with respect to individual firms' characteristics is not, however, sufficient to establish causality: other coincident developments, such as changes in investment opportunities in PA, could be responsible for the reduction in corporate risk taking.

To control for such contemporaneous developments, our baseline model in equation (1) compares risk changes among PA firms to the contemporaneous risk changes among firms in the same industry that are located in other states without a tax change in 1991, say, in New York (NY). To the extent that PA firms and NY firms are faced with similar changes in their prospects, the contemporaneous change in risk among NY firms provides a counterfactual estimate of how PA firms' risk choices would have evolved absent the tax increase. The difference-in-differences, that is, the difference across firms in different states of the within-firm risk change around the tax increase, provides an estimate of the tax sensitivity of corporate risk taking.

[7] Consistent with prior research, we use beginning-of-year (i.e., year $t-1$) values for the firm-level controls as these variables are likely affected by a firm's concurrent risk-taking choices (Gow, Larcker, and Reiss [2016]).

[8] Our results are robust to alternative approaches (see online appendix).

Equation (1) generalizes this illustrative example in that it exploits variation in taxes across many states and years. For any change in corporate income taxes in state *s* and year *t*, the potential control states are all those states that did not change their corporate income taxes in that year. In addition, equation (1) also controls for time-varying firm and state factors, as well as unobserved time-invariant firm characteristics and time-varying industry shocks.

As discussed in section 1, a causal interpretation of the coefficients of interest in equation (1) requires that risk taking at treated and control firms follow parallel trends, that state tax changes do not coincide systematically with variation in local business cycles or other tax or nontax state policies that might independently affect firms' risk taking, and that changes in state tax policies be unanticipated. We present tests designed to address potential violations of these identifying assumptions in subection 3.2.

2.2 INSTITUTIONAL SETTING

2.2.1. Changes in State Corporate Income Tax Rates. Table A1 in the online appendix provides an overview of all changes in state corporate income tax rates over the period 1990–2011. We start in 1990 because one of our control variables requires two lags of cash flow statement data, and cash flow data are only available via Compustat since 1988. We end in 2011 to give firms time to adjust their risk profiles after taxes change. Panel A lists 40 tax increases in 24 states (including Washington, DC (DC)) affecting 1,152 sample firms in fiscal years 1990–2011, while panel B lists 73 tax cuts in 27 states (including DC) affecting 4,920 firms in fiscal years 1990–2011.[9] The average tax shock increases tax rates by 93 basis points and the average tax cut reduces tax rates by 55 basis points.

Our main variables of interest are the *magnitude of tax increase* and *magnitude of tax cut* in a firm's headquarter state in a given fiscal year, in each case measured as the absolute value of the difference between this year's and last year's tax rate. From time to time, firms move their headquarters from one state to another. Compustat provides information only on a firm's current headquarter state. To remedy this flaw, we use Heider and Ljungqvist's [2015] hand-collected data on firms' historical headquarter states. Based on these data, the average (median) treated firm experiences a tax increase of 136 (106) basis points and a tax cut of 53 (44) basis points.

[9] In coding which firms are affected by tax changes when, we are careful to capture whether a tax change affects firms with fiscal years *ending* or *beginning* on or after the effective date. This affects when it makes sense for a firm to react. We lose 8 of Heider and Ljungqvist's [2015] 121 tax changes, partly because our sample starts later, partly because two of their tax changes (in North Dakota in 2007 and 2009) affect none of the firms satisfying our sampling criteria, and partly because we lack a clear prediction for how changes from gross receipts taxes to income taxes (or vice versa) affect firm risk taking.

Firms are taxed in every state in which they have a physical presence (their so called "nexus" states).[10] To reduce the scope for profit-shifting and tax arbitrage, states do not attempt to measure actual profits earned in-state. Instead, under the 1957 Uniform Division of Income for Tax Purposes Act, a multistate firm's federal taxable income is apportioned to each nexus state using a formula based on an average of the fractions of the firm's total payroll, sales, and property located in that state. This has two consequences for our analysis. First, it is not necessary for us to map a firm's projects to a specific state (which data limitations prevent us from doing): a firm can respond to a tax change in state A by changing the risk profile of its projects in *any* state it operates in. Second, the extent to which a multistate firm is exposed to a given state income tax change depends on the extent of its nexus to that state.

To measure the magnitude of tax shocks experienced by multistate firms more accurately, we approximate the geographic distribution of their tax liabilities using location data for their subsidiaries, branches, and plants. Specifically, we match Compustat firms by name to the National Establishment Time Series (NETS) database, which contains a comprehensive record of all business establishments in the United States since 1989.[11] We then calculate the weighted change in state tax rates in a firm's nexus states in a fiscal year as follows:

$$\Delta tax\ rate_{i,t} = \sum_{s}\left(\frac{1}{2}\frac{employees_{i,s,t}}{employees_{i,total,t}} + \frac{1}{2}\frac{sales_{i,s,t}}{sales_{i,total,t}}\right)\Delta T_{s,t}, \qquad (2)$$

where:

$employees_{i,s,t}$ and $sales_{i,s,t}$ =	firm *i*'s number of employees and sales in state *s* in year *t*, respectively,
$employees_{i,total,t}$ and $sales_{i,total,t}$ =	the corresponding firm totals across all nexus states in year *t*, respectively,
$\Delta T_{s,t}$ =	the change in the corporate income tax rate in state *s* in year *t*.

Equation (2) approximates a firm's nexus with each state using a 50/50 average of the fractions of the firm's total employment and sales in that state. Based on the magnitude and sign of the weighted tax change in equation (2), we define two alternative variables of interest: *nexus-weighted tax increase* and *nexus-weighted tax cut*, in each case measured in absolute terms.

2.2.2. Tax Loss Carryback/Carryforward Rules. The effect of taxes on risk taking is moderated by tax loss-offset provisions (Domar and Musgrave

[10] As of 2011, three states (Nevada, South Dakota, and Wyoming) do not impose income taxes, and three states (Ohio, Texas, and Washington) impose gross receipts taxes rather than income taxes.

[11] Neumark, Zhang, and Wall [2007] assess NETS along various dimensions and conclude that it is generally reliable. The name match is borrowed from Farre-Mensa and Ljungqvist [2016].

[1944]). Most states have loss-offset rules. For example, in 2011, about one-third of states allowed firms to offset current losses against income earned in the past two or three years, and all states allow firms to carry current losses forward, for periods ranging from 5 to 20 years. To examine heterogeneous treatment effects, we collect data on state tax loss carryback/carryforward rules over our sample period. We also use changes in these rules as an alternative source of policy shocks to examine the effects of corporate taxation on firm risk taking.

Table A2 in the online appendix provides details of changes in state tax loss carryback/carryforward rules for our sample period. Panel A lists 15 increases in the loss carryback period in 11 states (including DC) affecting 430 sample firms in fiscal years 1990–2011, while panel B lists 36 reductions in the loss carryback period in 26 states (including DC) affecting 1,164 firms. At the state-level, the average increase is 2.13 years while the average reduction is 1.75 years. The average (median) treated firm experiences an increase of 2.04 (2) years and a reduction of 1.83 (1) years.

Panel C lists 47 increases in the loss carryforward period in 37 states (including DC) affecting 5,349 sample firms in fiscal years 1990–2011, while panel D lists 10 reductions in the loss carryforward period in eight states affecting 1,828 firms. The variation in carryforward periods is larger than for carryback periods. At the state level, increases average 6.43 years, while reductions average 8.2 years. The average (median) treated firm experiences an increase in the carryforward period of 6.65 (5) years and a reduction of 9.58 (10) years.

2.2.3. Other State-Level Tax Policy Changes. Appendix A shows that changes in state corporate income tax rates and loss-offset rules rarely coincide with each other or with changes in investment incentive programs (i.e., tax credits for investment, R&D, and job creation). It is thus unlikely that our results are confounded by coincident changes in these state policies. The only area of overlap we find is with bank taxes: 28 of the 40 corporate tax increases coincide with bank tax increases and 56 of the 73 corporate tax cuts coincide with bank tax cuts. Since bank tax changes could trigger changes in the supply of bank loans, we verify that our results are robust to controlling for changes in bank taxes.[12]

2.3 SAMPLE AND DATA

2.3.1. Sample. Our sample begins with all firm-year observations in the merged CRSP-Compustat database for fiscal years 1990–2011. We exclude financial firms (SIC = 6; 27,197 observations), utilities (SIC = 49; 7,174 observations), public-sector entities (SIC = 9; 2,187 observations), non-U.S.

[12] Banks have a unique status for state tax purposes (Koch [2005]). They are taxed on a different schedule from corporations and so are subject to their own tax changes. When a state increases its bank tax, it reduces the after-tax profit on every loan made to borrowers located in the state, regardless of the lender's own location. Variation in a state's bank taxes can thus induce variation in the supply of loans available to firms located in the state.

firms (17,289 observations), and firms headquartered outside the United States (954 observations). We delete firms without stock return data, firms not traded on a major U.S. stock exchange (NYSE, Amex, or NASDAQ), and firms with a CRSP share code >11 (47,666 observations). Observations with negative or missing total assets (30,281 observations) are also excluded. Requiring nonmissing data for our risk measures and control variables and their lagged values leaves us with a final panel of 64,447 firm-year observations for 8,046 firms.

2.3.2. Measures of Risk Taking. We view a firm as a portfolio of projects that can differ in their risks. At each instant, the firm can close down existing projects and add new ones. While we do not observe these project-level choices, we do observe the aggregate cash flows they generate. We thus measure corporate risk taking as the firm-level volatility of aggregate cash flows, defined in two different ways.

Our first measure of risk taking, *ROA volatility*, is the standard deviation of seasonally adjusted quarterly pretax returns on assets (*ROA*) over the three-year period from year t to $t+2$, where pretax *ROA* is operating income after depreciation (i.e., earnings before interest and taxes) divided by the book value of total assets and the seasonal adjustment for firm i in quarter q of year t is computed as $\Delta ROA_{i,t,q} = ROA_{i,t,q} - ROA_{i,t-1,q}$ (Correia, Kang, and Richardson [2015]). The first difference of *ROA volatility* for year t is the log-transformed standard deviation computed over years t to $t+2$ minus the log-transformed standard deviation computed over years $t-3$ to $t-1$.[13]

Our second measure of risk taking, *ROIC volatility*, is the standard deviation of seasonally adjusted quarterly pretax returns on invested capital (*ROIC*) over a three-year period from year t to $t+2$. Following Lundholm and Sloan [2012], we compute *ROIC* as operating income after depreciation divided by the sum of debt, minority interests, preferred stock, and common stock.[14]

Note that, because both variables are measured before interest and taxes, they capture business (or asset) risk rather than the effects of financing risk. This is important because it is well known that tax changes can prompt firms to change their financial leverage. Our measures are thus designed to isolate the effects of taxes on the real (rather than financial) risks firms take.

In a robustness test, we use two market-based measures as alternative measures of risk taking, namely, the standard deviation of stock returns

[13] To construct these measures, we use data from Compustat Quarterly for fiscal years 1987–2013.

[14] *ROIC* is also called return on net operating assets (*RNOA*). Some researchers view nonoperating cash as negative debt and subtract total cash from invested capital in computing *ROIC*. However, in the presence of financial frictions, nonoperating cash should not be viewed as negative debt (Acharya, Almeida, and Campello [2007]). Moreover, firms generally do not disclose how much cash they hold for nonoperating purposes (Lundholm and Sloan [2012]).

TABLE 1
Firm-Level Summary Statistics

			Percentile		
	Mean	SD	25th	50th	75th
Dependent variables					
ROA volatility (in %)	6.8	11.0	2.3	3.8	7.2
ROIC volatility (in %)	10.6	17.2	3.1	5.3	10.2
operating cycle	83.6	96.1	32.2	73.3	125.7
days inventory	75.7	76.8	12.9	59.7	108.7
operating leverage	2.5	3.6	0.9	1.8	3.4
capex (in %)	5.4	6.1	1.7	3.6	6.9
R&D (in %)	5.3	11.8	0.0	0.1	5.9
RQ	10.0	5.4	7.5	10.0	12.6
State characteristics					
GSP growth rate (in %)	2.7	2.6	1.1	2.7	4.2
state unemployment rate (in %)	5.9	1.9	4.6	5.5	6.9
Firm characteristics					
firm age	19.6	13.3	9.0	15.0	27.0
firm size (total assets, $m)	1,755.2	4,899.7	52.6	219.2	969.3
market/book	3.0	4.7	1.1	1.9	3.3
book leverage	0.162	0.179	0.002	0.111	0.267
cash surplus	0.035	0.199	–0.012	0.050	0.115
loss carryforward	0.363	0.481	0.000	0.000	1.000
sales growth	0.052	0.339	–0.057	0.050	0.166
stock return	0.166	0.744	–0.251	0.040	0.375

The sample consists of 64,447 firm-years for all nonfinancial and nonutility U.S. companies that are traded on the NYSE, Amex, or NASDAQ in fiscal years 1990 through 2011, as per the merged CRSP-Compustat Fundamentals Annual database. The table reports summary statistics for our dependent variables and the controls. For variable definitions and details of their construction, see appendix B. All variables are winsorized 1% in each tail.

and the de-leveraged standard deviation of stock returns. We prefer the earnings-based risk measures because they more likely reflect a firm's choice of risk (stock returns not being under the firm's control).

2.3.3. Control Variables and Descriptive Statistics. Following prior research (e.g., Coles, Daniel, and Naveen [2006]), our baseline specification in equation (1) controls for the following firm characteristics: age, size, market-to-book ratio, book leverage, cash surplus, loss carry-forward, sales growth, and annual stock return. (See appendix B for definitions.) Table 1 presents summary statistics. The average (median) *ROA volatility* is 6.8% (3.8%), and the average (median) *ROIC volatility* is 10.6% (5.3%). Given the skewed distribution of these two risk measures, we use their log-transformed values in our regression analysis. The average firm in our sample is 19.6 years old and has total assets of $1,755.2 million.

Our baseline specification also controls for two state-level variables intended to capture local variation in economic conditions: the real growth rate in gross state product (GSP) and the state unemployment rate. The mean home-state GSP growth rate is 2.7% and the mean unemployment rate is 5.9%. We consider further state-level controls when we address identification concerns.

3. *State Corporate Income Taxes*

3.1 BASELINE ESTIMATES OF THE EFFECT OF STATE CORPORATE INCOME TAX CHANGES ON RISK TAKING

Table 2 reports the results of estimating equation (1). Columns 1 and 2 model how firms respond to tax changes in their headquarter states. In the regression with *ROA volatility* as the dependent variable (column 1), the coefficient on *magnitude of tax increase* is –0.019 ($p = 0.007$), suggesting that firms reduce risk taking in response to a tax increase. The effect is both statistically and economically significant. The point estimate suggests that the average treated firm, whose home-state tax rate increases by 136 basis points, reduces its risk taking by 2.6% relative to other firms in the same industry that are not subject to tax changes in their own home state that year. In column 2, where we use *ROIC volatility* as the dependent variable, the coefficient on *magnitude of tax increase* is –0.020 ($p = 0.006$)—nearly identical to the point estimate in column 1.

The models shown in columns 1 and 2 relate the difference in volatility measured over fiscal years t to $t + 2$ and volatility measured over fiscal years $t - 3$ to $t - 1$ to tax changes occurring in fiscal year t. In columns 3 and 4, we lag the tax changes by one year to allow for delays in firms' responses to tax changes. This produces stronger results for *ROA volatility* and similar results for *ROIC volatility*: *ROA volatility* falls by 2.6 percentage points for every one-percentage-point increase in the tax rate ($p < 0.001$), while *ROIC volatility* falls by 1.9 percentage points ($p = 0.047$).

Columns 5 and 6 of table 2 model how firms respond to contemporaneous changes in their nexus-state weighted income tax rates. As discussed earlier, the weighted tax-change measures attempt to approximate the shock to a firm's actual state-tax burden. In column 5, where the dependent variable is *ROA volatility*, the coefficient on *nexus-weighted tax increase* is –0.024 ($p = 0.011$), suggesting that a one-percentage-point increase in a firm's nexus-weighted tax rate reduces its risk taking by 2.4% relative to control firms in the same industry and year. In column 6, the coefficient on *nexus-weighted tax increase* is –0.032 ($p = 0.005$) when we use *ROIC volatility* as the dependent variable. The effects estimated for nexus-weighted tax changes are thus larger than those for home-state tax changes, confirming our prediction that ignoring the geographic distribution of firms' tax bases understates the tax sensitivity of firms' risk taking. Results using home-state tax changes are hence conservative.[15]

[15] Among the control variables, we find that risk increases by less as the firm ages or grows in size. Firms with a higher market-to-book ratio change risk more, while firms with higher financial leverage, more cash surplus, and higher stock returns change risk less. Firms with

TABLE 2

Effect of Tax Changes on Firm Risk

	Change in Log . . .					
	ROA volatility (1)	*ROIC volatility* (2)	*ROA volatility* (3)	*ROIC volatility* (4)	*ROA volatility* (5)	*ROIC volatility* (6)
magnitude of tax increase	−0.019***	−0.020***				
	0.007	*0.007*				
magnitude of tax cut	0.016	0.018				
	0.016	*0.015*				
lagged tax increase			−0.026***	−0.019**		
			0.007	*0.009*		
lagged tax cut			0.000	0.000		
			0.015	*0.017*		
nexus-weighted tax increase					−0.024**	−0.032***
					0.009	*0.011*
nexus-weighted tax cut					0.014	0.017
					0.024	*0.028*
Change in . . .						
GSP growth rate	−0.003**	−0.003*	−0.003**	−0.003*	−0.003**	−0.003*
	0.001	*0.001*	*0.001*	*0.002*	*0.001*	*0.001*
state unemployment rate	0.009**	0.010*	0.009*	0.009*	0.009**	0.009*
	0.005	*0.006*	*0.005*	*0.005*	*0.005*	*0.006*
Lagged change in . . .						
log firm age	−0.526***	−0.556***	−0.526***	−0.553***	−0.527***	−0.556***
	0.040	*0.041*	*0.040*	*0.040*	*0.039*	*0.041*
log firm size	−0.242***	−0.325***	−0.242***	−0.325***	−0.242***	−0.325***
	0.018	*0.027*	*0.017*	*0.027*	*0.018*	*0.027*
log market/book	0.123***	0.164***	0.121***	0.163***	0.123***	0.164***
	0.007	*0.007*	*0.007*	*0.008*	*0.007*	*0.007*
book leverage	−0.300***	−0.389***	−0.301***	−0.392***	−0.300***	−0.389***
	0.032	*0.035*	*0.034*	*0.037*	*0.032*	*0.035*
cash surplus	−0.250***	−0.253***	−0.248***	−0.247***	−0.249***	−0.253***
	0.021	*0.021*	*0.020*	*0.020*	*0.021*	*0.021*
loss carryforward	0.018*	0.030***	0.017*	0.029***	0.018*	0.030***
	0.010	*0.010*	*0.010*	*0.010*	*0.010*	*0.010*
sales growth	0.042***	0.052***	0.042***	0.052***	0.042***	0.052***
	0.005	*0.005*	*0.005*	*0.005*	*0.005*	*0.005*
stock return	−0.047***	−0.055***	−0.046***	−0.054***	−0.047***	−0.055***
	0.002	*0.003*	*0.003*	*0.003*	*0.002*	*0.003*
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	21.3%	21.0%	21.4%	21.1%	21.3%	21.0%
Equal tax sensitivity? (F)	0.03	0.02	2.03*	0.74	0.17	0.25
No. of firms	8,046	7,999	8,041	7,994	8,046	7,999
No. of observations	64,447	64,221	64,435	64,200	64,447	64,221

We estimate OLS regressions to test whether, and by how much, firms change their risk profile in response to changes in state corporate income taxes. The dependent variable *change in log ROA* (or *ROIC*) *volatility* is defined as the difference between log *ROA* (or *ROIC*) *volatility* at t (i.e., computed over t to $t+2$) and log *ROA* (or *ROIC*) *volatility* at $t-3$ (i.e., computed over $t-3$ to $t-1$). For variable definitions and details of their construction, see appendix B. In columns 1 and 2, we use contemporaneous changes in the firm's home-state top marginal corporate income tax rate. In columns 3 and 4, we use lagged changes in the firm's home-state top marginal corporate income tax rate. Columns 5 and 6 use the contemporaneous nexus-weighted change in tax rates as defined in equation (2). The unit of analysis in each column is a firm-year. All specifications are estimated using OLS in first differences to remove firm fixed effects in the levels equations and include industry-by-year fixed effects to remove industry shocks. The fixed effects are not reported for brevity. Heteroskedasticity-consistent standard errors clustered at the state level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively. Reflecting the signed nature of the predictions, the test for equal tax sensitivity (tax increase = –tax cut) is one-sided.

The contemporaneous effect of tax cuts is to increase risk taking. The effect is large, but unlike the effect of tax increases, it is not statistically significant. For example, in column 1, the coefficient on *magnitude of tax cut* is 0.016 with a *p*-value of 0.322. The results for *ROIC volatility* or when using nexus-weighted tax changes show a similar pattern. When lagged, the effect of tax cuts on risk taking is close to zero.[16]

The finding that firms more strongly and more consistently respond to tax increases than to tax cuts could be a power problem,[17] or it could be due to external constraints on firms' ability to increase risk. A natural source of constraints on corporate risk taking is creditors, whose claims fall in value as risk increases. To investigate this possibility, we sort firms into those with low financial leverage (assumed to face fewer constraints) and those with high leverage (assumed to face more constraints), measured as of the end of the previous fiscal year. Table 3 reports the results. Consistent with our conjecture, firms with low leverage do, in fact, increase risk taking significantly in response to a tax cut, whereas firms with high leverage do not.[18]

A more direct way to test our conjecture would be to measure the tightness of contractual constraints imposed on borrowers in the form of covenants. While data on covenants are available only for a subset of our sample firms, and even then are patchy, we find quite similar results: firms with few or lax covenants increase risk more strongly in response to tax cuts than do firms with many or tight covenants; see table A5 in the online appendix.

3.2 IDENTIFICATION CHALLENGES

Threats to a causal interpretation of the findings reported in the previous section come from potential violations of our identifying assumptions. In this section, we report dynamic tests to shed light on the parallel-trends assumption, tests designed to deal with changes in local economic conditions that coincide with state tax changes, and tests that deal with potential anticipation effects. If anything, these refinements strengthen our conclusion insofar as they yield typically somewhat larger point estimates of the

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higher sales growth rates and loss carryforwards change risk more. The two state-level control variables are also marginally significant. Firms increase risk as the GSP growth rate falls and as the state unemployment rate increases.

[16] While the difference in the sensitivity to tax increases and to tax cuts is small and not statistically significant for contemporaneous tax changes, it is economically large and statistically significant for *ROA volatility* when we use lagged tax changes.

[17] This reduced sensitivity to tax cuts is not due to tax cuts being smaller, on average, than tax increases in our sample: as table A4 in the online appendix shows, similar results obtain when we focus on large tax cuts.

[18] Table 3 also shows that firms' sensitivity to tax *increases* does not vary with their leverage. This rules out an alternative interpretation of our baseline results, namely that firms reduce risk when taxes increase not because their tax function has become more convex but simply because doing so allows them to more easily increase their financial leverage.

TABLE 3

Effect of Tax Changes on Risk for Firms with Low or High Leverage

	Change in Log . . .			
	ROA volatility		*ROIC volatility*	
	Low Leverage (1)	High Leverage (2)	Low Leverage (3)	High Leverage (4)
magnitude of tax increase	−0.026***	−0.024***	−0.032**	−0.022**
	0.009	*0.008*	*0.012*	*0.009*
magnitude of tax cut	0.063***	−0.019	0.072***	−0.019
	0.018	*0.019*	*0.014*	*0.023*
Controls	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
R^2	25.6%	31.1%	25.3%	31.1%
Equal tax sensitivity? (F)				
Tax increases: (1) versus (2) or (3) versus (4)	0.03		0.41	
Tax cuts: (1) versus (2) or (3) versus (4)	15.44***		13.91***	
No. of firms	5,769	5,467	5,723	5,441
No. of observations	32,223	32,224	32,105	32,106

To test whether firms are constrained by their lenders from increasing risk in response to a tax cut, we partition the sample based on financial leverage (measured as of the end of the fiscal year before a tax change). Columns 1 and 3 focus on firms with book leverage below the sample median. Columns 2 and 4 focus on firms with book leverage above the sample median. For variable definitions and details of their construction, see appendix B. The unit of analysis is a firm-year. All specifications are estimated using OLS in first differences with industry-by-year fixed effects (not shown for brevity). The tests for equal tax sensitivity across columns 1 and 2 and across columns 3 and 4 are based on fully interacted models. Reflecting the signed nature of the predictions, the tests for equal tax sensitivity are one-sided. Heteroskedasticity-consistent standard errors clustered at the state level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

sensitivity of corporate risk taking to tax increases. We also continue to find evidence that firms respond to tax changes asymmetrically.

3.2.1. Parallel Trends. As in any difference-in-differences test, a causal interpretation of the effect of tax changes on risk taking requires that treated and control firms follow parallel trends absent the tax change. To test for parallel trends, table 4 includes lead terms of the tax change variables. These are measured as of year $t + 3$, given that we use 12 quarters of earnings data to construct our volatility measures. The point estimates for the lead terms are economically tiny and not statistically different from zero, suggesting that risk follows parallel trends at treated firms and controls before state income tax rate changes. One implication of these findings is that firms do not anticipate future changes in state income taxes (or if they do, that they wait to change risk until the tax changes affect shareholder wealth). This, in turn, lessens concerns regarding potential anticipation effects.

Table 4 also allows for potential delays in firms' responses to tax cuts and postshock reversals in the effect of tax increases by including three-year lags. The coefficient for lagged tax increases is negative, indicating that

TABLE 4

Testing for Pretrends, Delays, and Postevent Reversals

	Change in Log . . .	
	ROA volatility (1)	ROIC volatility (2)
magnitude of tax increase at t = +3	−0.001	−0.005
	0.025	*0.028*
magnitude of tax increase at t = 0	−0.019**	−0.017**
	0.009	*0.008*
magnitude of tax increase at t = –3	−0.016*	−0.014
	0.008	*0.008*
magnitude of tax cut at t = +3	−0.007	0.007
	0.011	*0.013*
magnitude of tax cut at t = 0	0.018	0.018
	0.014	*0.014*
magnitude of tax cut at t = –3	−0.001	−0.002
	0.014	*0.018*
Controls	Yes	Yes
Industry-year fixed effects	Yes	Yes
R^2	24.4%	23.6%
No. of firms	6,183	6,171
No. of observations	47,966	47,879

To investigate possible pretrends, delays, and reversals, we include lead and lag terms in the baseline regressions shown in table 2, columns 1 and 2. Recall that the change in *ROA volatility* or *ROIC volatility* compares earnings volatility in the period t to $t + 2$ to earnings volatility in the period $t - 3$ to $t - 1$. Accordingly, we use leads dated $t + 3$ and lags dated $t - 3$ to avoid inducing a mechanical correlation between the dependent variable and the lead or lag term. For variable definitions and details of their construction, see appendix B. The unit of analysis is a firm-year. All specifications are estimated using OLS in first differences to remove firm fixed effects in the levels equations and include industry-by-year fixed effects to remove industry shocks. The full set of controls (as in table 2) and fixed effects are included but not reported for brevity. Heteroskedasticity-consistent standard errors clustered at the state level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

firms do not subsequently reverse the reduction in risk following a tax increase. Given the relatively large point estimate, the effect of a tax increase appears not only persistent but also increasing over time. In column 1, the cumulative effect is –0.035 ($p < 0.001$), suggesting that a one-percentage-point increase in the state corporate income tax rate in year t reduces *ROA volatility* by 3.5% over the next six years (i.e., *ROA volatility* measured over years t to $t + 2$ and over years $t + 3$ to $t + 5$). Given an average tax increase of 136 basis points, the average treated firm thus reduces its risk by a cumulative 4.8%. For tax cuts, the coefficient on the lag term is economically small and statistically insignificant.

3.2.2. Local Business Cycle Effects and Other State-Level Confounds. What might be called "local shocks" are a standard challenge to research designs that exploit local policy changes. A priori, there is little reason to expect tax changes to be "exogenous" rather than to occur in response to local shocks. The identification concern then is that changes in local economic conditions coincide with, or even drive, state changes in tax rates, and that it is these changes in local economic conditions—rather than the tax changes—that cause firms to change their risk taking.

We next present tests designed to deal with local shocks of three different types: (1) observed or unobserved changes in local economic conditions that coincide with state tax changes and whose effects diffuse across state borders, and either (2) observed, or (3) unobserved changes in economic policies that coincide with state tax changes and whose effects stop at the state border.

To address local shocks of type (1), we perform a neighboring-state test,[19] by dropping far-away control states, restricting the set of control firms to those located in states bordering the treated state, and including neighboring-state-by-year fixed effects to difference away unobserved variation in local economic conditions.[20] The essence of a neighboring-state test is to exploit a policy discontinuity along a geographic boundary under the maintained assumption that there exists an unobserved time-varying confound that might bias the treatment effect of interest. The aim is to difference away the confound in order to obtain an unbiased estimate of the treatment effect. In our setting, the policy in question is a tax change ΔT_A in state A that applies only in state A but not in neighboring state B (this is the policy discontinuity). The outcome variable is the change in risk, ΔR. The potential time-varying confound (denoted by ΔY) could, for example, be business cycle variation.

The identifying assumption is that $\Delta Y_A \approx \Delta Y_B$ (both states are exposed to roughly the same business cycle variation). Under this identifying assumption, $(\Delta R_A|\Delta T_A, \Delta Y_A) - (\Delta R_B|\Delta Y_B)$ is a consistent estimate of the effect of taxes on risk taking, given that $\Delta Y_A - \Delta Y_B \approx 0$, and so the unobserved confound can be differenced away using a fixed effect common to states A and B in the tax-shock year. Economically speaking, cross-border neighbors establish the counterfactual risk-taking response to the local business cycle variation of firms not affected by a tax increase, and this counterfactual response is then subtracted from the treated firms' response to the treatment. In other words, comparing treated firms to their immediate neighbors helps ensure that trends are parallel after removing the effects (if any) of common variation in local conditions.[21]

Columns 1 and 2 of table 5 report the results. Compared to firms in neighboring states, treated firms reduce both *ROA volatility* and *ROIC volatility* by 2.5% ($p = 0.016$ in column 1 and $p = 0.037$ in column 2) in response to a one-percentage-point increase in the corporate tax rate. Thus,

[19] See Holmes [1998], Huang [2008], Dell [2010], and Dube, Lester, and Reich [2010].

[20] Note that, in so doing, we drop observations for states that are treated in another year but are not the neighbor of a treated state in the current year.

[21] The neighboring-state test does not assume that neighboring states have the same or similar policies. Assume there is variation in policies across neighboring states. If these policies do not themselves change at the time of the tax change, they are differenced away by our first-difference research design and so cannot confound the results.

TABLE 5

Potential Confounds: Local Business Cycle Effects, Other Tax Changes, and Determinants of Tax Rate Changes

	Change in Log …					
	ROA volatility (1)	*ROIC volatility* (2)	*ROA volatility* (3)	*ROIC volatility* (4)	*ROA volatility* (5)	*ROIC volatility* (6)
magnitude of tax increase	−0.025**	−0.025**	−0.032**	−0.034**	−0.036**	−0.041***
	0.010	*0.011*	*0.014*	*0.015*	*0.014*	*0.014*
magnitude of tax cut	0.000	−0.003	−0.013	−0.017	−0.006	−0.010
	0.014	*0.018*	*0.018*	*0.020*	*0.022*	*0.024*
Other coincident tax changes						
increase in state tax on banks			0.022	0.033	0.029	0.042*
			0.025	*0.027*	*0.023*	*0.025*
cut in state tax on banks			0.024	0.030	0.022	0.034
			0.029	*0.026*	*0.032*	*0.030*
increase in state investment tax credits			−0.004	−0.005	−0.005	−0.007
			0.006	*0.006*	*0.006*	*0.006*
cut in state investment tax credits			0.003	0.000	0.004	0.000
			0.005	*0.006*	*0.005*	*0.006*
increase in state R&D tax credits			0.004	0.003	0.005*	0.004*
			0.003	*0.003*	*0.003*	*0.002*
cut in state R&D tax credits			−0.004	−0.008***	−0.004	−0.008**
			0.006	*0.002*	*0.006*	*0.003*
increase in state job tax credits			0.025	0.027	0.020	0.023
			0.018	*0.019*	*0.018*	*0.019*
cut in state job tax credits			0.051	0.076	0.064	0.090
			0.063	*0.065*	*0.062*	*0.064*
Political conditions						
Lagged change in Democratic governor					0.046***	0.034**
					0.015	*0.015*
= 1 *if one year to next gubernatorial election*					−0.006	−0.014
					0.018	*0.020*
= 1 *if two years to next gubernatorial election*					0.004	0.008
					0.013	*0.014*
= 1 *if three years to next gubernatorial election*					−0.032	−0.034*
					0.020	*0.019*
Economic conditions						
Lagged change in state budget balance					0.120	0.168
					0.139	*0.145*
= 1 *if state bond rating downgraded in year* $t-1$					−0.017	−0.018
					0.021	*0.023*
Lagged change in GSP growth rate					0.002	0.004
					0.003	*0.003*
Lagged change in state unemployment rate					−0.016*	−0.014
					0.008	*0.011*
Lagged change in state union penetration					−0.005	−0.005
					0.006	*0.006*
Tax competition						
Lagged change in state's tax rate relative to highest tax rate among its neighboring states					0.000	0.007*
					0.004	*0.004*

(*Continued*)

TABLE 5—*Continued*

	Change in Log . . .					
	ROA volatility (1)	*ROIC volatility* (2)	*ROA volatility* (3)	*ROIC volatility* (4)	*ROA volatility* (5)	*ROIC volatility* (6)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Group-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	29.0%	28.8%	29.0%	28.8%	29.1%	28.8%
Equal tax sensitivity? (F)	1.84*	1.44	6.65***	4.35**	4.16**	3.51**
No. of firms	6,586	6,547	6,586	6,547	6,586	6,547
No. of observations	29,613	29,498	29,613	29,498	29,613	29,498

States may change corporate tax rates, and firms may change their risk profile, in response to unobserved changes in local business conditions. To examine this potential confound, we restrict the set of control firms to those located in a neighboring state, thus excluding far-away states (i.e., firms in states that neither experience a tax change nor border a state that does are excluded). This means that we drop observations for states that are treated in another year but are not the neighbor of a treated state in the current year. This reduces the sample compared to the baseline models shown in table 2. To address concerns stemming from the fact that corporate tax changes occasionally coincide with changes in state taxes on bank profits or in investment incentive programs (i.e., tax credits for investment, R&D, and job creation), columns 3 and 4 control explicitly for these concurrent changes. In columns 5 and 6, we further control for local political forces and economic conditions that may influence whether a state changes its corporate tax rate (namely, the governor's political affiliation, the election cycle, the state's budget balance, ratings downgrades, unemployment, growth, union penetration, and tax competition with neighboring states). All specifications are estimated using OLS in first differences with industry-by-year fixed effects and group-year fixed effects, where a treated state and its neighboring states are coded as a group. The full set of controls (as in table 2) and fixed effects are included but not reported for brevity. For variable definitions and details of their construction, see appendix B. Heteroskedasticity-consistent standard errors clustered at the state level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively. Reflecting the signed nature of the predictions, the test for equal tax sensitivity (tax increase = –tax cut) is one-sided.

controlling for unobserved local shocks increases the point estimates a little compared to the table 2 baseline. Tax cuts continue to have no effect on risk taking. Overall, these patterns confirm that our findings are not driven by unobserved variation that coincides with the tax changes but diffuses across state borders.

A related threat to identification is that there could be an interaction between time-varying local conditions and a state's time-invariant policies. Say states A and B are exposed to the same business cycle variation but state A is a business-friendly state while state B believes in heavy regulation. Then firms in state A will be more sensitive to the common business cycle variation than firms in state B. This, in turn, would violate a version of the identifying assumption, namely that, absent the tax change in state A, $\Delta R_A|\Delta Y_A \approx \Delta R_B|\Delta Y_B$. In this scenario, our test would wrongly attribute the difference in risk taking, $(\Delta R_A|\Delta T_A, \Delta Y_A) - (\Delta R_B|\Delta Y_B)$, to the tax change rather than to the moderating effect on ΔY_B of state B's heavy regulatory burden.

For such an interaction between time-varying local conditions and time-invariant policies to spuriously produce our results, it would have to be the case that firms' risk choices were systematically more sensitive to changes in economic conditions in tax-increasing states than in neighboring control states. We view this as unlikely for two reasons. First, while it is conceivable that a particular constellation of policy differences across neighboring states and local conditions could produce this identification challenge in some place at some point in our data, it is much less likely to systematically confound our results, given that we exploit not one but 113 tax changes that are neither clustered in time nor in space. Second, the large number of tax changes means that every state (bar Montana) that is treated at some time also acts as a control state at some other time in our panel. The effects of cross-state differences in the sensitivity of risk taking to local conditions thus cancel out.[22,23]

This leaves confounds whose variation coincides with the tax changes and whose influence stops at the state border. Some of these are observable, others unobservable. Prominent potential confounds of the observable variety include the state-level policy changes listed in appendix A. Columns 3 and 4 of table 5 control for changes in state taxes on bank profits and in investment incentive programs (i.e., tax credits for investment, R&D, and job creation), some of which coincide with corporate tax changes. Doing so increases the estimated effect of tax increases by around one-third (from –0.025 to around –0.033). The effect of tax cuts remains statistically insignificant, and the difference between the sensitivities to tax cuts and tax increases is economically large and statistically significant ($p = 0.007$ in column 3 and $p = 0.021$ in column 4).

Altonji, Elder, and Taber [2005] propose dealing with confounds of the unobservable variety by estimating the degree of selection on observables and using it as a guide to the degree of selection on unobservables. In our context, this entails gauging the potentially confounding role of state-level political and economic conditions that may affect state tax policy. As a first step, columns 5 and 6 of table 5 include the main determinants of state tax changes as identified by Heider and Ljungqvist [2015]. This increases the estimated sensitivity to tax increases further, to 3.6% ($p = 0.012$) for *ROA volatility* and 4.1% ($p = 0.006$) for *ROIC volatility*. The effect of tax cuts on risk taking continues to be economically small and statistically insignificant.

In the next step, we follow Christensen, Hail, and Leuz [2016], who propose a falsification test of the potential bias induced by selection on unobservables. The test regresses the part of the outcome variable that is related to observed determinants of the suspected confounds (here: the change

[22] If risk taking in business-friendly state A always responds to the business cycle while risk taking in state B does not, then the treatment effect is (A – B) when A is treated and (B – A) when B is treated, so that the overall treatment effect averages zero (i.e., (A – B) + (B – A) = 0). Our results reject this null hypothesis, at least for tax increases.

[23] A related concern is that tax changes in one state may trigger changes in the behavior of firms in a neighboring state. To investigate such cross-border spillovers, we conduct a test in the spirit of table 5 that restricts the set of control firms to those located in states *not neighboring* the treated state. As table A6 in the online appendix shows, our inferences are unchanged.

in risk taking predicted from our economic and political controls) on the treatment variable (here: the change in the tax rate). A small coefficient in the falsification test, compared to the estimated treatment effect in the baseline test, suggests that the baseline treatment effect is unlikely to reflect unobserved confounds. As table A7 in the online appendix shows, the point estimates in the falsification test are tiny and statistically insignificant. These results reinforce our conclusion that unobserved variation in local conditions is unlikely to be severe in our setting.

3.2.3. Anticipation Effects. If firms plan their current policies based on the tax rates they expect to face in the future, their observed responses to an actual tax change may not uncover the causal effect of taxes on their behavior. To see why, consider a tax increase that turns out smaller than expected. This may cause corporate policy to change in a way normally expected after a tax *cut* (since the tax rate increased by less than expected), which in turn would confound the interpretation of the observed treatment effect (as the econometrician does not observe the firm's expectations). Having said that, the absence of significant lead effects in table 4, discussed earlier, suggests that firms do not change risk in anticipation of future tax changes.

As Hennessy and Strebulaev [2015] note, anticipation effects only undermine identification if the corporate policy in question is subject to adjustment costs, so that the firm must plan ahead in order to reach its desired position over time given its expectations. The next subsection explores empirically various mechanisms by which firms may change risk taking in response to tax changes. Some of these are more plausibly subject to adjustment costs than others. The one that we find to be strongest in the data is a short-term mechanism with few obvious adjustment costs.

There is one (somewhat obvious) scenario, besides the absence of adjustment costs, for when anticipation effects do not pose an identification challenge: if policy changes are unanticipated. More formally, a necessary and sufficient condition for correct inference about causal effects is that the policy variable is a Martingale (Hennessy and Strebulaev [2015]), which in our context means that state tax rates follow a random walk. Using three unit root tests, Ljungqvist and Smolyansky [2016] largely fail to reject the null hypothesis of a random walk.[24]

Specifically, in separate augmented Dickey-Fuller tests on each state's time series of tax rates from 1969 to 2013, Ljungqvist and Smolyansky [2016] fail to reject the presence of a unit root in each state and DC, suggesting corporate tax rates follow a random walk in every state. Realizing that some states condition their tax policy on the tax policies of their neighbors (Heider and Ljungqvist [2015]), Ljungqvist and Smolyansky also test the null hypothesis that each state in a given regional "cluster" has a unit root while allowing for cross-sectional dependence in tax rates across

[24] These findings echo Barro [1990], who reports that federal taxes follow a random walk.

TABLE 6
Anticipation Effects

	Change in Log . . .					
	ROA volatility (1)	*ROIC volatility* (2)	*ROA volatility* (3)	*ROIC volatility* (4)	*ROA volatility* (5)	*ROIC volatility* (6)
magnitude of tax increase	−0.016**	−0.018**	−0.014**	−0.017**	−0.016**	−0.019**
	0.006	*0.008*	*0.006*	*0.007*	*0.007*	*0.008*
magnitude of tax cut	0.003	0.009	0.006	0.012	−0.012	−0.009
	0.021	*0.022*	*0.022*	*0.023*	*0.014*	*0.014*
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	22.1%	21.9%	22.2%	22.0%	23.3%	23.0%
No. of firms	7,433	7,391	7,363	7,321	6,685	6,652
No. of observations	59,130	58,915	58,476	58,263	52,699	52,511

If state tax rate changes are anticipated, measured treatment responses to realized tax rate changes may not capture causal effects (Hennessy and Strebulaev [2015]). To address this concern, we exclude firms headquartered in states whose tax rate changes are likely to be anticipated. Ljungqvist and Smolyansky [2016] test whether state tax rates follow a Martingale (which implies that changes in tax rates are unpredictable). Based on their findings, columns 1 and 2 exclude firms headquartered in Connecticut or Massachusetts while columns 3 and 4 exclude firms headquartered in New England (i.e., Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, or Vermont). Heider and Ljungqvist ([2015]) examine the political economy events surrounding all state tax changes affecting at least 100 firms, using a "narrative approach" to identify potentially anticipated tax changes. Based on their findings, columns 5 and 6 exclude firms headquartered in Colorado, Connecticut, Minnesota, or New York. For variable definitions and details of their construction, see appendix B. The unit of analysis in each column is a firm-year. All specifications are estimated using OLS in first differences to remove firm fixed effects in the levels equations and include industry-by-year fixed effects to remove industry shocks. The full set of controls (as in table 2) and fixed effects are included but not reported for brevity. Heteroskedasticity-consistent standard errors clustered at the state level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

states. Clusters are defined either as a state and its contiguous neighbors (giving 49 clusters, including DC's but excluding Alaska and Hawaii) or as states that are located in a given Census region. The null cannot be rejected at standard significance levels except in Connecticut and in Massachusetts and their respective contiguous neighbors. Within Census regions, the null is never rejected at the 5% level; it is rejected at the 10% level in New England.

To see whether anticipation effects in states whose tax rates do not follow a random walk may confound our results, table 6 excludes firms headquartered in Connecticut or Massachusetts (columns 1 and 2) or in New England as a whole (columns 3 and 4). Doing so marginally reduces the magnitude of the treatment effect of tax increases, to between –0.014 and –0.018 ($p < 0.05$). Tax cuts continue to have no significant effect on the average firm's choice of risk.

An alternative to this econometric way of classifying tax changes as potentially anticipated is the "narrative approach" of Romer and Romer [2010]. Heider and Ljungqvist [2015] examine the political economy events surrounding all state tax changes affecting at least 100 firms since 1989 to identify potentially anticipated tax changes. Based on their findings, columns

5 and 6 of table 6 exclude firms headquartered in Colorado, Connecticut, Minnesota, or New York (all of which experienced sequences of tax cuts). This yields sensitivities to tax increases of between –0.016 and –0.019 ($p < 0.05$), again marginally smaller than those reported in our baseline tests.

Collectively, the results in table 6 suggest that anticipation effects do not play a major role in contaminating our findings. This, in turn, increases our confidence in the external validity of our findings: to the extent that state tax rates truly follow a random walk, the patterns we document should apply more broadly than just in the setting and time period we study.

3.3 MECHANISMS

By what means do firms reduce risk in response to state corporate income tax increases? The reasonably fast reduction in earnings volatility (measured over the three-year period from t to $t + 2$) suggests that firms change the risk profile of their existing operations. One way to do so is to make changes to the operating cycle: the process by which cash is transformed into raw materials, work in progress, finished goods, accounts receivable, and eventually back into cash. Shortening the operating cycle (for example, by reducing the amount of cash tied up in inventory that could go unsold) puts less capital at risk and so reduces earnings volatility. Essentially, the firm can reduce its operating risk by reducing its investment in working capital, and it can do so relatively quickly and, potentially, without incurring substantial adjustments.

Panel A of table 7 provides evidence of such reductions in operating risk. In the year following a tax increase, we see firms reducing their operating cycles by an average of 1.7 days and 3.05 days for every one-percentage-point increase in their home-state or nexus-weighted tax rate, respectively. (Tax cuts have no effect on operating cycles.) Relative to the sample average operating cycle of 83.6 days, this implies a reduction of 2–3.6% for the average treated firm. About half of this reduction comes from a reduction in inventory holding periods, which fall by an average of 0.72–1.47 days in columns 2 and 5.[25]

Another way firms could reduce operating risk is by reducing operating leverage, that is, the sensitivity of profits to changes in output. In practice, reducing operating leverage requires turning fixed costs into variable costs. Whether the tax shocks are on average large enough to justify the expense involved in making costs more flexible is an open question. For example, making labor costs more flexible may involve protracted negotiations with unions and increase the risk of strikes (a form of adjustment

[25] Though not shown to conserve space, the two other components of the operating cycle, the average number of days to collect receivables and pay payables, do not change significantly.

TABLE 7

Effect of Tax Changes on Operational and Investment Choices

Panel A: Effect of tax changes on operational choices

	Change in ...					
	operating cycle (1)	*days inventory* (2)	*operating leverage* (3)	*operating cycle* (4)	*days inventory* (5)	*operating leverage* (6)
magnitude of tax increase	−1.702***	−0.724*	0.078			
	0.379	*0.375*	*0.099*			
magnitude of tax cut	−0.030	−0.246	0.173			
	0.956	*0.755*	*0.135*			
nexus-weighted tax increase				−3.052**	−1.473**	0.077
				1.163	*0.559*	*0.177*
nexus-weighted tax cut				−0.787	−0.134	0.158
				1.431	*1.014*	*0.193*
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	10.1%	11.5%	14.9%	10.1%	11.5%	14.9%
No. of firms	7,952	7,981	6,105	7,952	7,981	6,105
No. of observations	63,472	63,881	49,707	63,472	63,881	49,707

Panel B: Effect of tax changes on investment choices

	Change in ...					
	capex	*R&D*	*RQ*	*capex*	*R&D*	*RQ*
	Home-State Tax Changes			Nexus-Weighted Tax Changes		
	(1)	(2)	(3)	(4)	(5)	(6)
magnitude of tax increase at $t = +1$	0.000	0.000	−0.121	0.000	0.001	−0.114
	0.001	*0.001*	*0.114*	*0.001*	*0.001*	*0.110*
magnitude of tax increase at $t = 0$	0.000	−0.001	−0.049	−0.001	−0.001	−0.098
	0.001	*0.001*	*0.089*	*0.001*	*0.001*	*0.113*
magnitude of tax increase at $t = -1$	0.000	0.000	−0.167*	0.000	0.000	−0.231*
	0.001	*0.000*	*0.089*	*0.001*	*0.001*	*0.123*
magnitude of tax increase at $t = -2$	0.000	0.001	−0.085	0.001	0.001	−0.005
	0.000	*0.001*	*0.099*	*0.001*	*0.001*	*0.109*
magnitude of tax increase at $t = -3$	0.001	−0.001	−0.155*	0.002	−0.001	−0.231**
	0.001	*0.001*	*0.081*	*0.001*	*0.001*	*0.113*
magnitude of tax cut at $t = +1$	0.000	0.001	0.126	−0.001	0.002	0.227
	0.001	*0.002*	*0.130*	*0.001*	*0.002*	*0.169*
magnitude of tax cut at $t = 0$	−0.001	−0.003	−0.058	0.000	−0.004	0.002
	0.001	*0.002*	*0.122*	*0.002*	*0.003*	*0.128*
magnitude of tax cut at $t = -1$	0.000	0.002	−0.009	−0.001	0.003	−0.140
	0.001	*0.001*	*0.117*	*0.001*	*0.002*	*0.133*
magnitude of tax cut at $t = -2$	0.000	−0.001	−0.016	0.000	−0.002	−0.035
	0.001	*0.001*	*0.082*	*0.001*	*0.002*	*0.135*
magnitude of tax cut at $t = -3$	0.001	0.001	−0.146	0.002	0.001	−0.184
	0.001	*0.002*	*0.117*	*0.001*	*0.003*	*0.144*

(*Continued*)

TABLE 7—*Continued*

Panel B: Effect of tax changes on investment choices

	Change in ...					
	capex	*R&D*	*RQ*	*capex*	*R&D*	*RQ*
	Home-State Tax Changes			Nexus-Weighted Tax Changes		
	(1)	(2)	(3)	(4)	(5)	(6)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	16.7%	8.0%	29.5%	16.7%	8.0%	29.5%
No. of firms	7,323	7,379	3,771	7,323	7,379	3,771
No. of observations	57,747	58,498	28,833	57,747	58,498	28,833

We estimate OLS regressions to test whether, and by how much, firms change their operational and investment policies in response to changes in state corporate income taxes. Panel A focuses on a firm's operational choices. The dependent variable *change in operating cycle* (or *days inventory*) is defined as the difference between *operating cycle* (or *days inventory*) at time t and *operating cycle* (or *days inventory*) at $t-1$. The dependent variable *change in operating leverage* is defined as the difference between *operating leverage* at time t (i.e., computed over t to $t+2$) and *operating leverage* at $t-3$ (i.e., computed over $t-3$ to $t-1$). Panel B focuses on a firm's investment choices. To investigate possible pretrends, delays, and reversals, we include lead and lag terms in the regressions. The dependent variable *change in capex* (or *R&D, RQ*) is defined as the difference between *capex* (or *R&D, RQ*) at time t and *capex* (or *R&D, RQ*) at $t-1$. In both panels, columns 1–3 use changes in the firm's home-state corporate income tax rate while columns 4–6 use the nexus-weighted change in tax rates as defined in equation (2). For variable definitions and details of their construction, see appendix B. The unit of analysis in each column is a firm-year. All specifications are estimated using OLS in first differences to remove firm fixed effects in the levels equations and include industry-by-year fixed effects to remove industry shocks. We include the same controls and fixed effects as in table 2. These are not reported for brevity. Heteroskedasticity-consistent standard errors clustered at the state level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

cost). As columns 3 and 6 of panel A show, we fail to find evidence of firms changing their operating leverage in response to state tax changes.[26,27]

Given the further reduction in risk taking observed over the medium term (i.e., the three-year period $t+3$ to $t+5$ in table 4), firms may also change the risk profiles of their investment projects. Panel B of table 7 begins by showing that firms do not adjust the *level* of their capital expenditures or R&D spending in response to state tax changes: the tax sensitivity of either is both economically and statistically zero, consistent with Asker, Farre-Mensa, and Ljungqvist [2015].[28] This finding leaves open the possibility that firms instead respond to tax increases by changing project risk.

[26] Another way firms can fine-tune their risk profiles in response to state tax rate changes is hedging (Graham and Smith [1999], Graham and Rogers [2002]). Data on hedging activities are not systematically available.

[27] Table A8 in the online appendix reports two robustness tests. The first restricts the set of control firms to those located in neighboring states, following table 5. This reduces the effect of tax increases on changes in operating cycles somewhat, though the effect remains both economically and statistically significant. The second shows that firms do not change their operating risk in response to a tax cut, regardless of their financial leverage (coded as in table 3), possibly because doing so would reduce operational efficiency.

[28] As table A9 in the online appendix shows, we similarly find no evidence that firms change their M&A activities or reduce risk by engaging in diversifying acquisitions.

For example, firms may choose safer R&D projects (say, to enhance the quality or variety of their existing products) over riskier ones (say, to invent new products).

Project risk is not directly observable, but its effect on cash flows is potentially measurable. To see how, start from the observation that R&D has an asymmetric effect on sales: all else equal, successful R&D projects boost the firm's sales while failed R&D projects have no (immediate) effect. This insight gets around the problem that accounting data reveal only R&D inputs (i.e., spending), not R&D outputs (i.e., new products or processes that generate sales): an increase in the sensitivity of sales to R&D spend implies an increase in R&D outputs for a given amount of R&D spend. Next, consider a mean-preserving increase in R&D risk. This would increase the sensitivity of sales to R&D spend if the project succeeds and leave it unchanged if the project fails. On average, therefore, an increase in R&D risk results in an increase in the sensitivity of sales to R&D spend. The opposite holds for a reduction in R&D risk.

Using a measure called the Research Quotient™ (available on WRDS), panel B shows that the sensitivity of sales to R&D spend falls after a tax increase, consistent with firms reducing R&D risk. The effect is not immediate—it takes on average between one and three years for a tax increase to reduce the R&D sensitivity of sales—and not overly strong statistically.

A cautious interpretation of the findings in table 7 is that the most prominent mechanism by which firms reduce risk in response to state corporate income tax increases involves making changes to the operating cycle. Since such changes should be relatively easy to reverse, they should not involve substantial adjustment costs, reducing concerns about anticipation effects that are not already allayed by Ljungqvist and Smolyansky's [2016] finding that state tax rates mostly follow a random walk or by the auxiliary evidence reported in our previous subsection.

3.4 ROBUSTNESS TESTS

Before turning our attention to the moderating effect of tax loss-offset rules, we briefly consider two robustness tests.

Our baseline tests use *ROA volatility* and *ROIC volatility* to measure firm risk. Prior research on corporate risk taking often uses stock return volatility to measure a firm's choice of risk. Table A10 in the online appendix shows that our findings are robust to using equity volatility instead of earnings volatility: a firm's annual stock return volatility falls by around 2% following a tax increase and is invariant to tax cuts, and the difference between the two tax sensitivities is economically large and statistically significant.

Our results may be driven by tax-related changes in earnings management.[29] To investigate this concern, we test if a firm's performance-matched

[29] Scholes, Wilson, and Wolfson [1992] find that firms respond to anticipated reductions in federal tax rates by delaying recognizing income. Maydew [1997] provides evidence that firms shift income to benefit from loss offsets.

discretionary accruals (Kothari, Leone, and Wasley [2005]) vary with tax changes, but find no evidence that they do (see table A11 in the online appendix). This is consistent with Graham's [2006] observation that "tax incentives appear to be a second-order consideration, rather than a dominant influence on earnings management" (p. 663). In addition, equity-based measures of risk taking, such as those modeled in table A10 in the online appendix, are not affected by earnings management, further alleviating this concern.

4. *State Tax Loss-Offset Rules*

4.1 HETEROGENEOUS TREATMENT EFFECTS

According to Domar and Musgrave's [1944] theory, the effect of personal income taxes on individual risk taking is negative in the absence of loss offsets. The same is true in the corporate arena. However, if firms can offset losses against past or future profits, the effect of taxes on risk taking becomes more complex. On the one hand, income taxes discourage risk taking by reducing the per-unit benefit of risk taking. On the other hand, loss-offset rules essentially make the government shoulder part of the losses. Thus, both the benefit of risk taking and the level of after-tax cash flow risk are reduced. If complete offset of losses is possible, variation in tax rates may have no net effect on risk taking.

To test this prediction, we partition the sample based on the carryback and carryforward rules in effect in each firm's home state in a given fiscal year. Specifically, we code firms as having a low ability to offset losses when their home state allows no loss carrybacks and no more than 10 years of loss carryforwards. Otherwise, we code firms as having a high ability to offset losses.[30]

Table 8 presents the results of estimating equation (1) in the partitioned samples. For firms with a low loss-offset ability, the sensitivity of *ROA volatility* and *ROIC volatility* to a tax increase is –0.026 ($p = 0.010$ in column 1) and –0.033 ($p = 0.008$ in column 3), respectively. For firms with a high loss-offset ability, the sensitivity is –0.010 ($p = 0.391$ in column 2) and –0.004 ($p = 0.817$ in column 4), respectively.[31] These results suggest that the negative effect of tax increases on risk taking is largely driven by firms located in states with weak loss-offset provisions, consistent with the prediction that the effect of corporate income taxes on risk taking is attenuated by the ability to offset tax losses against past or future profits.

[30] These cutoffs are arbitrary but, as table A12 in the online appendix shows, not selective.

[31] The difference between the coefficients on *magnitude of tax increase* in the two subsamples, although economically large, is only statistically significant when we use *ROIC volatility* as the dependent variable.

TABLE 8
Heterogeneous Treatment Effects

	Change in Log . . .			
	ROA volatility		*ROIC volatility*	
	Low Loss Offset Ability (1)	High Loss Offset Ability (2)	Low Loss Offset Ability (3)	High Loss Offset Ability (4)
magnitude of tax increase	−0.026**	−0.010	−0.033***	−0.004
	0.009	*0.012*	*0.011*	*0.016*
magnitude of tax cut	0.009	0.012	0.017	0.014
	0.022	*0.017*	*0.019*	*0.019*
Controls	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
R^2	29.8%	26.9%	28.8%	26.8%
Equal tax sensitivity? (*F*)				
Tax increases: (1) versus (2) or (3) versus (4)	1.18		2.10*	
No. of firms	4,221	5,757	4,203	5,716
No. of observations	26,005	38,442	25,914	38,297

Tax loss carryback and carryforward rules dampen the impact of corporate income tax rate changes on firm risk. To test this, we partition sample firms based on the tax loss carryback and carryforward rules of their headquarter state. Columns 1 and 3 include firms headquartered in a state that (1) does not allow losses to be carried back and (2) does not permit losses to be carried forward for more than 10 years. Columns 2 and 4 include only the remaining sample firms. For variable definitions and details of their construction, see appendix B. The unit of analysis is a firm-year. All specifications are estimated using OLS in first differences with industry-by-year fixed effects (not shown for brevity). The tests for equal tax sensitivity across columns 1 and 2 and across columns 3 and 4 are based on fully interacted models. Reflecting the signed nature of the predictions, the tests for equal tax sensitivity are one-sided. Heteroskedasticity-consistent standard errors clustered at the state level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

4.2 STATE LOSS-OFFSET RULES AND RISK TAKING

Our baseline tests investigate firms' responses to tax rate changes while our tests of heterogeneous treatment effects examine if firms' responses to tax rate changes are moderated by tax loss-offset rules. We next test if changes in loss-offset rules affect risk taking independently. A reduction in the number of years that losses can be carried back or forward essentially reduces the extent to which the government shares in a firm's risks, analogous to a tax rate increase. Thus, we expect that reductions in the generosity of carryback or carryforward rules lead to lower risk taking. The opposite argument can be made for an increase in the length of carryback or carryforward periods (subject to creditors constraining firms' ability to increase risk).

Table 9 examines loss-offset rule changes both in a firm's home state (in panel A) and across its nexus states (in panel B).[32] We allow for

[32] For the latter, we estimate nexus-weighted changes in the length of tax loss carryback/carryforward periods using equation (2) (i.e., the formula used to estimate nexus-weighted

TABLE 9

Effect of Changes in Loss Carryback/Carryforward Rules on Firm Risk

Panel A: Home-state rule changes

	Change in Log ...			
	ROA volatility (1)	ROIC volatility (2)	ROA volatility (3)	ROIC volatility (4)
increase in length of carryback period	0.016	0.022	0.016	0.022
	0.014	*0.016*	*0.014*	*0.016*
reduction in length of carryback period	−0.023***	−0.019**	−0.023***	−0.019**
	0.007	*0.008*	*0.007*	*0.008*
increase in length of carryforward period	0.002**	0.003***	0.002**	0.003***
	0.001	*0.001*	*0.001*	*0.001*
reduction in length of carryforward period	0.001	0.001	0.001	0.001
	0.001	*0.001*	*0.001*	*0.001*
magnitude of tax increase			−0.019***	−0.020***
			0.007	*0.007*
magnitude of tax cut			0.014	0.015
			0.016	*0.016*
Controls	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
R^2	21.3%	21.0%	21.3%	21.0%
Equal tax sensitivity? (F)				
Increase in carryback = −reduction in carryback	0.24	0.03	0.23	0.04
Increase in carryforward = −reduction in carryforward	4.95**	5.49**	4.03**	4.66**
Reduction in carryback = −increase in carryforward	8.02***	3.89**	8.21***	3.93**
No. of firms	8,046	7,999	8,046	7,999
No. of observations	64,447	64,211	64,447	64,211

Panel B: Nexus-weighted rule changes

	Change in Log ...			
	ROA volatility (1)	ROIC volatility (2)	ROA volatility (3)	ROIC volatility (4)
nexus-weighted increase in carryback period	0.025	0.038	0.025	0.038
	0.024	*0.028*	*0.024*	*0.028*
nexus-weighted reduction in carryback period	−0.035***	−0.028***	−0.035***	−0.028***
	0.010	*0.010*	*0.010*	*0.010*
nexus-weighted increase in carryforward period	0.002	0.003**	0.002	0.003*
	0.002	*0.001*	*0.002*	*0.001*
nexus-weighted reduction in carryforward period	0.000	0.000	0.000	0.000
	0.002	*0.002*	*0.002*	*0.002*
nexus-weighted tax increase			−0.024**	−0.031***
			0.009	*0.011*

(*Continued*)

TABLE 9—*Continued*

Panel B: Nexus-weighted rule changes

	Change in Log . . .			
	ROA volatility (1)	*ROIC volatility* (2)	*ROA volatility* (3)	*ROIC volatility* (4)
nexus-weighted tax cut			0.012	0.015
			0.024	*0.028*
Controls	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
R^2	21.3%	21.0%	21.3%	21.0%
Equal tax sensitivity? (F)				
Increase in carryback = −reduction in carryback	0.22	0.13	0.21	0.15
Increase in carryforward = −reduction in carryforward	0.52	0.84	0.47	0.76
Reduction in carryback = −increase in carryforward	10.56***	5.70**	10.65***	5.70**
No. of firms	8,046	7,999	8,046	7,999
No. of observations	64,447	64,211	64,447	64,211

We estimate OLS regressions to test whether, and by how much, firms change their risk profile in response to changes in state tax loss carryback/carryforward rules. Panel A focuses on the change in the number of years a loss can be carried back or forward in a firm's headquarter state. Panel B focuses on the nexus-weighted change in the number of years a loss can be carried back or forward in the states a firm has nexus with. Both increases and reductions are measured in absolute terms. For variable definitions and details of their construction, see appendix B. The unit of analysis is a firm-year. All specifications are estimated using OLS in first differences to remove firm fixed effects in the levels equations and include industry-by-year fixed effects to remove industry shocks. The full set of controls (as in table 2) and fixed effects are not reported for brevity. Heteroskedasticity-consistent standard errors clustered at the state level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively. Reflecting the signed nature of the predictions, the test for equal tax sensitivity is one-sided.

asymmetric responses by separately including increases and reductions in the length of loss carryback or carryforward periods. For both home-state and nexus-weighted changes, and whether we model risk as *ROA volatility* or *ROIC volatility*, we find an asymmetric response to changes in carryback rules: firms reduce risk taking as carryback rules are made less generous but do not respond when the rules become more generous.[33] Since a shorter carryback period amounts to a tax increase, these patterns are consistent with our baseline finding that firms reduce risk taking in response to an increased tax burden but do not significantly increase risk taking in response to a reduced tax burden.

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changes in tax rates), after replacing tax rate changes with changes in the length of loss carryback/carryforward periods.

[33] The results are stronger for nexus-weighted changes than for home-state changes. They are robust to controlling for the tax rate changes from our baseline tests (see columns 3 and 4). This is not surprising: as appendix A shows, changes in state corporate income tax rates rarely coincide with changes in state tax loss-offset rules.

The response to changes in carryforward rules is different: firms respond to more generous carryforward rules by increasing risk (sometimes significantly so). However, these effects are economically small. Firms do not respond when carryforward rules become less generous.

The contrast between firms' risk-reducing response to less generous carryback rules and their indifference to less generous carryforward rules is consistent with claims in the literature that carrybacks allow firms to claim cash taxes back immediately when incurring losses whereas the benefit of carryforwards is more uncertain (Langenmayr and Lester [2015]).

Overall, using changes in state tax loss-offset rules yields results that reinforce our conclusion from using tax-rate changes that increasing a firm's tax burden reduces its willingness to take risk.

5. Conclusions

We ask whether and how corporate income taxes affect firms' risk taking. Based on theories of the effect of personal income taxes on individual risk taking, we predict a negative effect of corporate taxes on corporate risk taking. Using staggered changes in corporate tax rates across U.S. states, we provide evidence that firms reduce risk when tax rates increase, by shortening their operating cycles and by reducing the risks they take in their R&D projects. The effect of tax increases on risk taking is largely driven by firms located in states with few loss-offset opportunities, as theory would have predicted. Consistent with the interpretation that creditors use restrictive covenants to prevent firms from increasing risk ex post, we show that only firms with low financial leverage increase risk in response to tax cuts.

In addition to using a difference-in-differences regression with a comprehensive set of firm-level and state-level control variables, we employ a battery of refinements to establish causality: including industry-by-year fixed effects to control for time-varying industry shocks, adding lead terms to confirm parallel trends, using neighboring states to control for local shocks, and controlling for other coincident state-level policy changes. Of course, the extent to which our evidence can be viewed as causal will depend on the severity of any remaining identification concerns.

As in Heider and Ljungqvist [2015], an important caveat concerns the external validity of our findings. The state-level tax changes in our sample are generally small in magnitude, and it is possible that firms would respond differently if the tax shocks were larger. The relatively small magnitude of our tax changes may also be the reason why we do not see firms adjusting their long-term investment decisions in response to state tax changes.

We end with a brief discussion of potential policy implications. While raising taxes can increase the government's revenue, it may have the side effect of dulling risk-taking incentives in the corporate sector, which in turn may adversely affect innovation and economic growth. Moreover, if the government wishes to encourage risk taking, our findings suggest that merely reducing tax rates is unlikely to be effective without other policy changes.

APPENDIX A

Coincident State-Level Changes

Panel A: Coincident state-level changes for tax rate changes

		Tax Increases	Tax Cuts
Number of tax changes		40	73
... of which coincide with	Increase in length of state carryback periods	1	3
	Cut in length of state carryback periods	0	2
	Increase in length of state carryforward periods	0	4
	Cut in length of state carryforward periods	1	2
	Increase in state tax on banks	28	0
	Cut in state tax on banks	0	56
	Increase in state investment tax credit rate	1	6
	Cut in state investment tax credit rate	0	0
	Increase in state R&D credit rate	2	9
	Cut in state R&D credit rate	1	2
	Increase in state job creation credit	0	3
	Cut in state job creation credit	0	1
	Increase in state job creation grants	0	1
	Cut in state job creation grants	0	0

Panel B: Coincident state-level changes for tax loss carryback changes

		Increases in Carrybacks	Cuts in Carrybacks
Number of carryback changes		15	36
... of which coincide with	Increase in state tax on banks	1	0
	Cut in state tax on banks	0	0
	Increase in state investment tax credit rate	2	1
	Cut in state investment tax credit rate	0	0
	Increase in state R&D credit rate	0	2
	Cut in state R&D credit rate	0	0
	Increase in state job creation credit	2	0
	Cut in state job creation credit	0	0
	Increase in state job creation grants	0	0
	Cut in state job creation grants	0	0

Panel C: Coincident state-level changes for tax loss carryforward changes

		Increases in Carryfor-wards	Cuts in Carry-forwards
Number of carryforward changes		47	10
… of which coincide with	Increase in state tax on banks	1	0
	Cut in state tax on banks	3	1
	Increase in state investment tax credit rate	3	1
	Cut in state investment tax credit rate	0	0
	Increase in state R&D credit rate	2	2
	Cut in state R&D credit rate	0	0
	Increase in state job creation credit	2	0
	Cut in state job creation credit	1	0
	Increase in state job creation grants	0	0
	Cut in state job creation grants	0	0

Panel A reports state-level changes in economic quantities that coincide with either increases or cuts in state corporate income taxes and that have a plausible basis in theory to potentially affect corporate risk-taking decisions. We focus on changes in state taxes on banks and changes in state investment incentive programs (i.e., tax credits for investment, R&D, and job creation, as well as job creation grant programs). We also report state-level changes in the length of tax loss carryback/carryforward periods that coincide with state tax rate changes. For variable definitions and details of their construction, see appendix B.

Panel B reports state-level changes in economic quantities that coincide with either increases or cuts in the number of periods a state allows a company to carry back losses and that have a plausible basis in theory to potentially affect corporate risk-taking decisions. We focus on changes in state taxes on banks and changes in state investment incentive programs (i.e., tax credits for investment, R&D, and job creation, as well as job creation grant programs). For variable definitions and details of their construction, see appendix B.

Panel C reports state-level changes in economic quantities that coincide with either increases or cuts in the number of periods a state allows a company to carry forward losses and that have a plausible basis in theory to potentially affect corporate risk-taking decisions. We focus on changes in state taxes on banks and changes in state investment incentive programs (i.e., tax credits for investment, R&D, and job creation, as well as job creation grant programs). For variable definitions and details of their construction, see appendix B.

APPENDIX B
Variable Definitions

B.1 DEPENDENT VARIABLES

ROA volatility is defined as the standard deviation of the difference between quarterly *ROA* and *ROA* for the same quarter of the previous year, computed over a three-year period t to $t+2$ (requiring a minimum of four quarters of data). *ROA* (return on assets) is defined as operating income after depreciation (Compustat item *oiadpq*) over the book value of assets (Compustat item *atq*). We annualize *ROA volatility* by multiplying it by $\sqrt{4}$.

ROIC volatility is defined as the standard deviation of the difference between quarterly *ROIC* and *ROIC* for the same quarter of the previous year, computed over a three-year period t to $t+2$ (requiring a minimum of four quarters of data). *ROIC* (return on invested capital) is defined as operating income after depreciation (Compustat item *oiadpq*) over the sum of debt (Compustat items *dlttq* + *dlcq*), minority interests (Compustat item *mibtq*), preferred stock (*pstkq*), and common stock (*ceqq*). We annualize *ROIC volatility* by multiplying it by $\sqrt{4}$.

Operating cycle is defined as the sum of the average inventory holding period and the average number of days to collect receivables minus the average number of days to pay payables. The average inventory holding period is computed as average inventory (Compustat item *invt*) over cost of goods sold (Compustat item *cogs*), multiplied by 365. The average number of days to collect receivables is computed as average accounts receivable (Compustat item *rect*) over sales (Compustat item *sale*), multiplied by 365. The average number of days to pay payables is computed as average accounts payable (Compustat item *ap*) over purchases (cost of goods sold + ending inventory – beginning inventory), multiplied by 365.

Days inventory is defined as the average inventory holding period, computed as average inventory (Compustat item *invt*) over cost of goods sold (Compustat item *cogs*), multiplied by 365.

Operating leverage is measured as the sensitivity of EBIT to sales (Mandelker and Rhee [1984]). Specifically, it is estimated as the coefficient on the logarithm of quarterly sales in a firm-specific regression that regresses the logarithm of quarterly operating income after depreciation (Compustat item *oiadpq*) on the logarithm of quarterly sales (Compustat item *saleq*) over a three-year period from t to $t + 2$ (requiring a minimum of four quarters of data).

Capex is defined as net capital expenditure (Compustat item *capx – sppe*) over the book value of assets (Compustat item *at*).

R&D is defined as research and development expenditure (Compustat item *xrd*) over the book value of assets (Compustat item *at*). Following standard practice, we set *xrd* equal to zero when it is missing from Compustat.

RQ (short for research quotient) is a firm-year measure of the output elasticity of R&D (Knott [2008]), obtained from the WRDS RQ™ database. It represents the percentage increase in revenues (in year $t + 1$) resulting from a 1% increase in R&D (in year t), when other inputs and their elasticities are held constant.

B.2 INDEPENDENT VARIABLES: FIRM CHARACTERISTICS

Firm age is defined as the Compustat age.

Firm size is defined as the book value of total assets (Compustat item *at*) in year 2009 real dollars (deflated using the GDP deflator available at http://www.bea.gov/national/xls/gdplev.xls).

Market/book is defined as the ratio of the market value of equity (Compustat items *prcc_f* × *csho*) to the book value of equity (Compustat item *ceq*).

Book leverage is defined as long-term debt (Compustat item *dltt*) over the book value of assets (Compustat item *at*).

Cash surplus is defined as cash from assets-in-place (Compustat items *oancf* – *dpc* + *xrd*) over the book value of assets (Compustat item *at*).

Loss carryforward is an indicator set equal to one if the firm has positive net operating loss carryforward (Compustat item *tlcf*), and zero otherwise.

Sales growth is defined as the log of current year sales over last year sales (Compustat item *sale*).

Stock return is defined as cumulated monthly returns over the 12-month period ending at the fiscal year end (measured using data from CRSP).

B.3 INDEPENDENT VARIABLES: STATE-LEVEL CHARACTERISTICS

GSP growth rate is the real annual growth rate in gross state product (GSP) using data obtained from the U.S. Bureau of Economic Analysis.

State unemployment rate is the state unemployment rate, obtained from the U.S. Bureau of Labor Statistics.

State tax on banks captures changes in the rate at which a state taxes financial institutions with nexus to the state. (Both a physical presence in the state and out-of-state lending to borrowers located in the state constitute nexus.) The data come from the *Book of the States* and state codes accessed through Lexis-Nexis.

State investment tax credit rate is the rate at which a firm can deduct capital expenditures directly from its state corporate income tax liability (in addition to the usual depreciation deductions against taxable income). Data through 2006 come from Chirinko and Wilson [2008]. Data for subsequent years come from tax forms available on state Department of Revenue Web sites.

State R&D credit rate is the percentage of a firm's R&D expenditures that it can deduct directly from its state corporate income tax liability (in addition to the usual deduction against taxable income). Data through 2006 come from Wilson [2009]. Data for subsequent years come from tax forms available on state Department of Revenue Web sites.

State job creation credit is set equal to one if the state offers a tax credit in return for hiring new workers meeting certain requirements, and zero otherwise. The data come from appendix A1 in Neumark and Grijalva [2015].

State job creation grants is set equal to one if the state offers grant payments in return for hiring new workers meeting certain requirements, and zero otherwise. The data come from appendix A1 in Neumark and Grijalva [2015].

Democratic governor is an indicator set equal to one if the state is governed by a Democratic governor, and zero otherwise. Data come from the *Congressional Quarterly* (through 2008) and state election Web sites (after 2008).

State budget balance equals the difference between a state's *general revenues* and its *general expenditures* scaled by its *general expenditures*. The data come from the U.S. Census Bureau's State & Local Finances database, available at http://www.census.gov/govs/local.

State bond rating downgrade is an indicator set equal to one if the state's credit rating is downgraded by either Standard & Poor's or Moody's.

State union penetration is the fraction of private-sector employees in a state who belong to a labor union in year *t*. The data come from Hirsch and Macpherson [2003] as updated on their Web site, http://www.unionstats.com.

Tax competition is measured as the difference between a state's corporate income tax rate and the highest corporate income tax rate levied by any of the neighboring states.

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