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ABSTRACT

We exploit staggered changes in state-level corporate tax rates to show that an increase in taxes reduces future innovation. A variety of tests, including those based on policy discontinuity at contiguous counties straddling borders of politically similar states, show that local economic conditions do not drive our results. The effect we document is consistent across the innovation spectrum: taxes affect not only patenting and R&D investment but also new product introductions, which we measure using textual analysis. Our empirical results are consistent with models that highlight the role of higher corporate taxes in reducing innovator incentives and discouraging risk-taking.

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1. Introduction

Corporate tax systems are prominent in much of the political and economic discourse around the world today. In many countries, including the US, corporate tax rates are high and the tax code is complex, resulting in a heated debate on how to rationalize such codes. Some policy makers in these countries argue in favor of lower taxes to make firms more innovative and competitive; at the same time others suggest that lowering corporate taxes would distort the balance of government budgets and increase inequality. A key source of disagreement in this debate is whether or not corporate taxes have any real effect on the innovative competitiveness of economies.

In this paper we examine staggered corporate income tax changes at the US state level over 1990–2006, and find that higher corporate taxes indeed reduce future innovation by affected firms. In particular, our conservative estimates imply that about 67% of treated firms affected by a tax increase file for approximately one fewer

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patent following the increase, as compared to a neighboring firm exposed to similar economic conditions but not affected by any tax change. This is approximately a 5% change in patenting activity. To allow for asymmetric effects, we consider tax increases and decreases separately, and find that most of this effect comes from the increases.

Importantly, we find that the decline in innovation is not limited to patenting activity—the drop in patenting is accompanied by a 4.3% decline in research and development (R&D) expenditures as well as a 5.1% decline in final innovation output—new product introductions—which we measure using a novel textual analysis-based metric. These findings, taken together, imply that the effect of corporate taxes pervades all stages of innovation. To the extent that innovation is regarded as a key determinant of economic growth, we believe that these results are not only academically interesting, but also timely and policy-relevant.

In our study, we are mindful that states might change tax rates based on local economic conditions. The latter might directly affect a firm's incentives to innovate, aside from the effect of taxes. We perform a variety of tests to rule out this possibility. For example, in one test we confine our analysis to firms located in contiguous counties on two sides of a state border. We directly net out the effects of local economic conditions non-parametrically by controlling for county-pair-year fixed effects. To ensure that the selection of firms into politically different states is not driving our results, we modify existing tests in the literature, and restrict our analysis to counties in adjacent states that are genuinely similar in terms of policies and political balance. Further, in order to address the possibility that predictability of tax changes might bias estimates (Hennessy and Strebulaev, 2015), we confine our analysis to unpredictable tax changes—changes which were difficult to predict the year before using media reports and macroeconomic data—and continue to find similar effects.

Next, to further validate our findings, we explore cross-sectional variation among firms in terms of their exposure to tax changes. Consistent with our hypotheses, we find that those firms that have higher marginal tax rates and those that are more constrained in their ability to avoid taxes are affected more. We establish the latter result by exploiting differences among states in terms of combined reporting laws, i.e., laws that restrict firms' ability to shift profits to a tax-haven subsidiary, and then have this subsidiary charge a royalty to the rest of the business for the use of the trademark or patent.

The debate we focus on in this paper has become particularly prominent today, when many governments around the world face a sharp trade-off between austerity and future growth. For example, an important sticking point surrounding the latest Greek austerity deal was the International Monetary Fund's (IMF) view that growth will be hurt by the proposed corporate tax increase, which was a crucial part of the Greek plan to raise revenues. Supporters of this view argue that the worldwide decrease in corporate taxes in the last three decades has indeed coincided with significant technological progress and growth

(e.g., Brill and Hassett, 2007).¹ Indeed, there are many possible reasons why corporate taxes may matter for future growth. For instance, the decline in after-tax profits from innovation projects following tax increases can lead innovators to reduce or redirect effort, affecting aggregate innovation activity. Alternatively, any increase in taxes which accentuates the progressivity of the tax schedule might discourage more risky innovative projects. In addition, a tax increase may raise the attractiveness of debt to firms (Heider and Ljungqvist, 2015), which in turn is not the favored form of financing for innovation. Moreover, taxes might also lower internal cash flows, that have been shown to be a major source of financing for innovation activities (e.g., Himmelberg and Petersen, 1994).

On the other hand, other authors remind that supply-side policies implemented in the 1980s resulted mainly in excess consumption, rather than an expansion in productive capacity (Tassey, 1997, p. 13). They point out that Scandinavian countries like Sweden have both high taxes and innovation (Krugman, 2016). This view is sometimes supported by media reports claiming that—even in within-country settings like in the US—there seem to be no significant associations between business tax burdens and state economic performance, including innovativeness (Florida, 2011). Proponents of this view express serious doubts on whether raising the corporate tax rate actually hinders innovation activity in the real world, given the tax deductibility of R&D expenditures, the existence of R&D tax credits, as well as a plethora of sophisticated tax avoidance strategies that many firms are known to adopt.

The evidence we uncover supports the first view. In the final section of the paper, we investigate potential channels that are likely to be behind our result. We start by developing empirical predictions relevant for our analysis from an extension of Romer's (1990) endogenous growth model with taxes and occupational choice, following the structure of Jaimovich and Rebelo (2015). In this setting, corporate tax changes induce less innovation in general equilibrium due to innovators choosing to shift to less innovative activities. We look for direct empirical support for this channel by examining movements of innovators across firms using a database of patent assignees, and find that tax increases indeed lead to a significant number of inventors parting with their employers.²

Second, we examine the conjecture that an increase in tax progressivity (which is how most of the tax changes are implemented) reduces incentives for firms to undertake innovation projects, particularly if the innovation projects are risky [similar to Gentry and Hubbard (2000)]. Consistent with this view, we find a systematic decline in the riskiness of innovation projects undertaken by firms

¹ We report similar associations between international corporate taxes and country-level patenting activity in the Internet Appendix, Table A.1.

² Such movements of innovative personnel can also explain the asymmetry in our findings: we find that tax increases lead to an increase in inventor turnover in the 2 years following the change, while the effect takes much longer to show up following tax cuts. This is consistent with the view that while firms may be quick to lose their innovative inputs, they may need a longer period of time to build the knowledge, workforce, and capacity required to innovate.

following tax increases. Finally, we also consider other explanations for the response of firm innovation to tax changes such as changes in firm financing structure, and find partial support in the data for some of them.

Our results contribute to a few strands of literature. First, we relate to the literature on the effects of corporate taxes on investment, productivity, and economic growth (Jorgenson, 1963; Hall and Jorgenson, 1967; Levine, 1991; Auerbach and Hassett, 1992; Cummins, Hassett, and Hubbard, 1996; Cullen and Gordon, 2007; Djankov, Ganser, McLiesh, Ramalho, and Shleifer, 2010; Romer and Romer, 2010; Mertens and Ravn, 2012). None of these papers have examined investments in or output of the innovation process like we do in this paper. This is an important difference for at least two reasons. First, it is crucial to understand which economic policies drive innovation in particular, given the centrality of innovation in driving economic growth. Second, innovation involves higher risks of failure, little chance of redeployability of assets invested in it if projects fail, and is much more human-capital intensive than physical capital investment. So, mechanisms underlying the innovation process can be quite different from those underlying physical capital expenditures. For instance, the predictions that we derive and test from the theory models (Section 5) particularly rely on assumptions that are more likely to hold for innovative investments, as opposed to capital expenditures.

Second, a branch of literature has looked at R&D tax credits and has largely established that such credits have a significant influence on R&D investment (Mansfield, 1986; Jaffe, 1986; Katz, 1986; Grossman and Helpman, 1991; Aghion and Howitt, 1992; Jaffe, Trajtenberg, and Henderson, 1993; Bloom, Griffith, and Van Reenen, 2002; Wilson, 2009; Branstetter and Sakakibara, 2002; Rao, 2016). Surprisingly, however, the causal effect of general corporate tax policies on innovation—which has often been at the forefront of recent policy discourse—has not drawn enough academic attention. Moreover, this literature has focused on R&D spending, and has not examined innovation outputs such as patents or new products. Examining innovation outputs is particularly valuable in the context of taxation for two reasons. First, what tax authorities treat as R&D often differs from what firms consider productive innovation inputs. On the other hand, firms are also known to sometimes relabel other costs as R&D for the purposes of tax credits (Griffith, 1996). This makes it difficult to ascertain whether the R&D spending response to tax changes indeed reflects changes in productive innovation inputs, or is simply a relabeling of other expenditures for tax purposes. Second, firms differ widely in the productivity of their R&D investments (Hirshleifer, Hsu, and Li, 2013; Cohen, Diether, and Malloy, 2013). Therefore, although we also provide evidence on the impact of corporate taxes on R&D, we believe that our examination of patenting activity and new product introductions adds significant new insights on the impact of fiscal instruments on innovation.

To the best of our knowledge, the only other paper that examines the effect of fiscal policy on patenting is a contemporaneous paper by Atanassov and Liu (2014). Their main finding that corporate taxes hurt innovative

activities is consistent with ours. However, our analysis shows a stronger effect coming from tax increases, while Atanassov and Liu (2014) find that tax cuts matter more. This contrast in findings is caused by the different estimation methodologies we employ, as well as our list of tax changes being more comprehensive. For example, we identify tax effects based on tax policy discontinuity across politically similar contiguous counties and consider unpredictable changes in taxes. We discuss our differences in more detail in Section 6.

Moreover, we also contribute to the literature on factors that affect innovation by showing that corporate taxes are a first-order determinant.³ Finally, our paper also relates to literature on the effects of corporate tax changes on corporate policies (Graham, 2006; Blouin, Core, and Guay, 2010; Asker, Farre-Mensa, and Ljungqvist, 2015; Heider and Ljungqvist, 2015; Faulkender and Smith, 2016). Our main difference lies in our focus on innovation—which is an important ingredient in models of economic growth—and also in our improvements in some of the existing methodology to aid identification based on tax policy changes.

Overall, our results provide first evidence that tax increases lead to a decrease in innovation across every step of innovation process—R&D, patents, and our newly developed measure of new products—and that the tax effect is neither driven by local economic changes nor by biases related to the predictability of tax changes. Our examination of specific predictions from recent theoretical models on how taxes might affect innovation is also new to the literature, as is our analysis of tax sheltering provisions such as combined reporting laws.

The rest of the paper proceeds as follows. We describe our data and provide summary statistics in Section 2, discuss our method of analysis and empirical results in Section 3, examine heterogeneity of the effect in Section 4, discuss specific tests of relevant theory models in Section 5, present robustness and further analysis in Section 6, and conclude in Section 7.

2. Data

The patent data set used in our analysis is assembled by the National Bureau of Economic Research (NBER), which contains information on all patents awarded by the US Patent and Trademark Office (USPTO) as well as citations made to these patents (Hall, Jaffe, and Trajtenberg, 2001). We match the NBER patent data set with Compustat following the procedures developed in Hall, Jaffe, and Trajtenberg (2001) and Bessen (2009). Historical analysis of state tax changes requires the correct identification of the state that taxed a firm's profits in each year. We follow the literature and primarily rely on a firm's headquarter

³ Among many others, Aghion, Bloom, Blundell, Griffith, and Howitt (2005); Aghion, Van Reenen, and Zingales (2013); He and Tian (2013); Tian and Wang (2014); Hsu, Tian, and Xu (2014); Acharya and Subramanian (2009); Acharya, Baghai, and Subramanian (2013), 2014; Manso (2011); Fang, Tian, and Tice (2014); Nanda and Rhodes-Kropf (2013); Amore, Schneider, and Zaldokas (2013); Chava, Oettl, Subramanian, and Subramanian (2013); Cornaggia, Mao, Tian, and Wolfe (2015), and Ferreira, Manso, and Silva (2014). Chemmanur and Fulghieri (2014) contain a recent summary of this literature.

state. Since Compustat reports the address of a firm's current principal executive office, not its historic head-quarter location, we obtain (time-varying) firm location information from 10-K filings. In particular, we use the business address of the firm to identify the location of its headquarters.⁴ Due to constraints on the joint availability of tax, business address location, and patent data, we focus our analysis on granted patents applied for in the period 1990–2006.

We exclude firms in the financial sector (6000s Standard Industrial Classification (SIC) codes) and the public sector (9000s SICs) in constructing the final dataset, as patents might not be good measures of the output of innovative activities in these sectors. We only look at firms headquartered in the US. All financial variables are initially deflated at 2000 price level using Consumer Price Index (CPI) data from the Bureau of Labor Statistics (BLS), and later winsorized at 1% on both tails of their distributions. Our final sample, for which we have non-missing values for all our control variables, consists of 47,632 firm-year observations.

We rely on the BLS for the state-level macroeconomic variables. Moreover, to capture innovator employment effects we use patent assignee data from Harvard Business School Patent Network Dataverse. We also use simulated firm-level marginal tax rates from [Blouin, Core, and Guay \(2010\)](#) and data on state R&D tax credits from [Wilson \(2009\)](#). In addition, in order to gauge a firm's operations across different states, we exploit rich data on employment, sales, and assets at the parent-subsidiary level from LexisNexis' Corporate Affiliations database. This database contains the list of subsidiaries for all major publicly traded companies with US headquarters since 1994, and currently provides data on more than 6,881 US public parents and 154,247 subsidiaries.

Further, we hand-construct a database of major new product announcements by a textual search of the LexisNexis News database for company press releases, followed by an analysis of equity returns (from the Center for Research in Security Prices (CRSP)) around the announcement day. The detailed construction of all variables is described in the Appendix, while summary statistics for our key variables are reported in [Table 1](#).

[Table 1](#) shows that corporate profit taxes constitute a substantial expense for our firms, with the average (median) top marginal state tax rate in our sample being 6.8% (7.35%). During our sample period there were 32 instances of state tax increases, spread across 20 states, and 51 instances of tax cuts, spread across 24 states. [Fig. 1](#) depicts these changes on the US map. The average state corporate tax increase in our sample is 1.09%, while the average corporate tax cut is 0.73%. While the original list of tax changes that we use in the baseline specifications comes from [Heider and Ljungqvist \(2015\)](#), we confirm these tax changes, and conduct our study on tax predictability and

Table 1

Summary statistics.

This table reports descriptive statistics for our sample firms. All variables are defined in the Appendix. We start with the patent data set assembled by the National Bureau of Economic Research (NBER) which contains information on all the patents awarded by the US Patent and Trademark Office (USPTO) as well as the citations made to these patents ([Hall, Jaffe, and Trajtenberg, 2001](#)). We focus our analysis on granted patents applied for in the period of 1990–2006. We match the NBER patent data set with Compustat data following the procedures developed in [Hall, Jaffe, and Trajtenberg \(2001\)](#) and [Bessen \(2009\)](#). We exclude firms in the financial sector (6,000s SICs) and the public sector (9,000s SICs). We also exclude observations if the firm's sales or assets are less than \$1 million, if the firm's reported stock price is negative, or if the firm has fewer than four observations (to ensure that we correctly estimate the first difference regression). We only look at the firms with headquarters in the US. Data on state-level corporate taxes are available from 1990 onward. We drop observations for which data are not available for all the control variables. This restricts our baseline sample (before first-differencing but that for which all control variables are available) size to 47,632 firm-year observations. All the financial variables are initially deflated at 2000 price level using CPI data from BLS and later winsorized at 1% on both sides of the distribution. Data period: 1990–2006.

Variables	Obs	Median	Mean	SD
Firm-level variables				
No. of patents _{<i>i, t</i>}	47,632	0.00	9.11	75.10
Adjusted citations _{<i>i, t</i>}	47,632	0.00	149.59	1511.62
Ln(Sales) _{<i>i, t</i>}	47,632	4.86	4.79	2.44
Ln(K/L) _{<i>i, t</i>}	47,632	3.37	3.45	1.24
HHI _{<i>i, t</i>}	47,632	0.13	0.19	0.16
Profitability _{<i>i, t</i>}	47,632	0.09	−0.52	2.86
Tangibility _{<i>i, t</i>}	47,632	0.20	0.25	0.21
Debt rating(1=yes) _{<i>i, t</i>}	47,632	0.00	0.21	0.41
R&D/Sales _{<i>i, t</i>}	47,632	0.02	0.28	0.93
State-level variables				
Tax rate _{<i>s, t</i>} (in %)	867	7.35	6.8	2.93
Δ Tax rate _{<i>s, t</i>} (in %, nonzero)	83	−0.22	−0.027	1.48
Δ [−] Tax rate _{<i>s, t</i>} (in %, nonzero)	51	−0.40	−0.73	1.22
Δ ⁺ Tax rate _{<i>s, t</i>} (in %, nonzero)	32	0.675	1.09	1.13
Log(Real GSP) _{<i>s, t</i>}	824	11.95	11.83	1.03
Taxes as % of GSP _{<i>s, t</i>}	841	5.52	5.57	1.05
Log(Population) _{<i>s, t</i>}	841	15.17	15.05	1.01
Unemployment rate _{<i>s, t</i>}	841	5.10	5.18	1.40

exogeneity by using information from the Fiscal Surveys of States, Book of States, Factiva News database, Tax Foundation Special Reports, regional Federal Reserve Reports, along with other books and online news articles.

3. Empirical findings

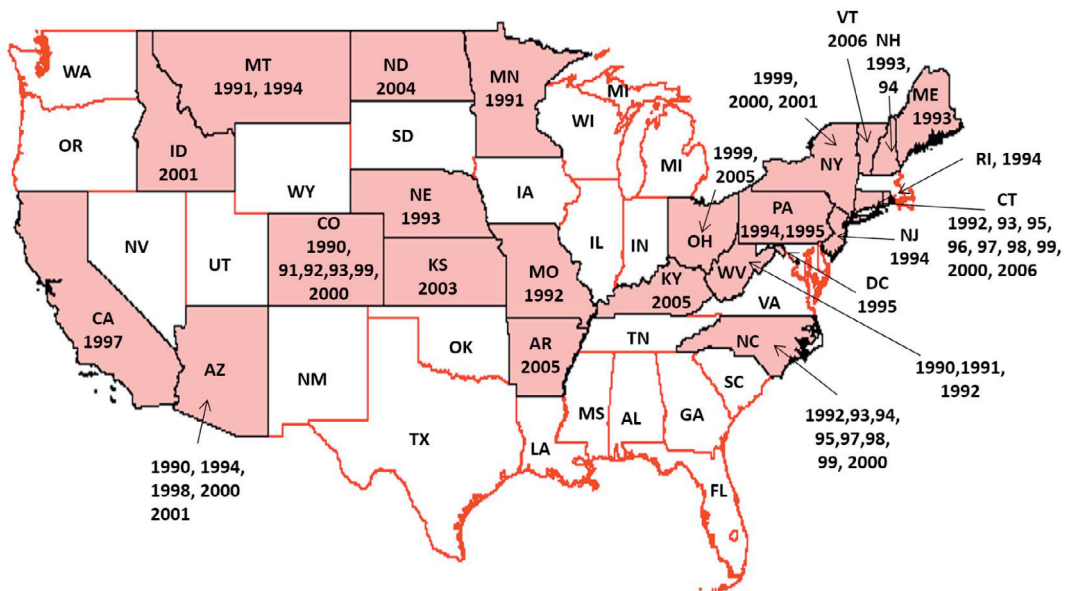
We use a difference-in-differences approach, which allows us to control for time-invariant, firm-specific omitted variables as well as time-varying industry trends and nationwide shocks, following the methodology of [Heider and Ljungqvist \(2015\)](#).

We first present evidence on all US publicly listed firms over 1990–2006 and all tax changes. However, we recognize two potential concerns with such analysis.

First, if tax changes were predictable, then the coefficient estimate we present might be biased ([Hennessy and Strebulaev, 2015](#)). To address this concern, we present results focusing on unpredictable tax changes, following a model-based approach where we predict tax changes based on state macroeconomic conditions and media discussions. We also perform tests addressing the predictability issue in a different way, by following a

⁴ We thank Alexander Ljungqvist for these data. For a detailed description of the data, please see [Heider and Ljungqvist \(2015\)](#). Our results are not altered if we consider alternative state locations, e.g., based on state name counts in 10-K forms as in [Garcia and Norli \(2012\)](#) (Table A.9 of the Internet Appendix).

1A: Tax decreases



1B: Tax increases

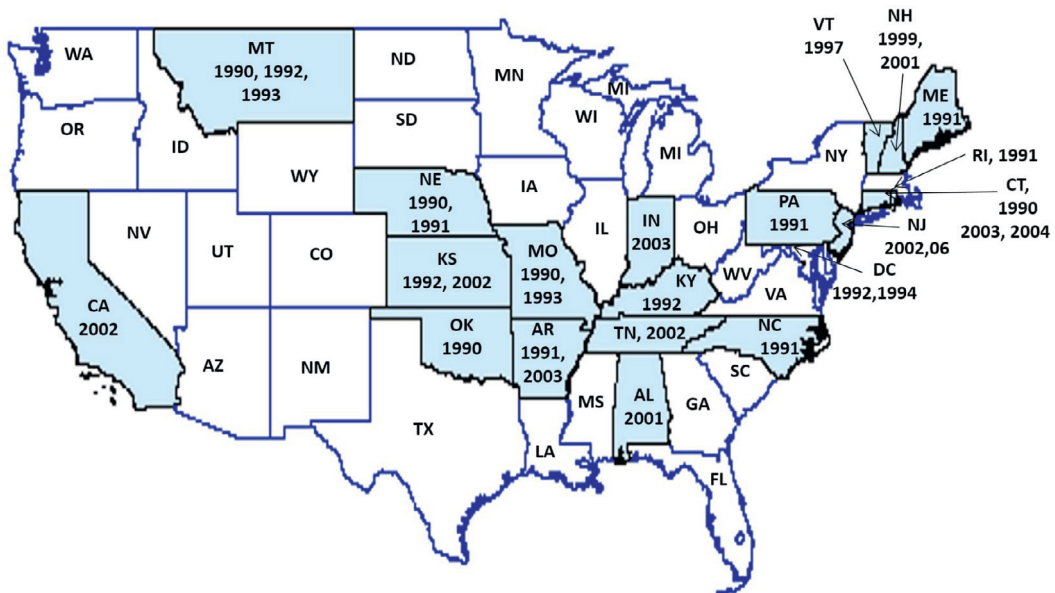


Fig. 1. Geography of state corporate income tax changes, 1990–2006. The figure below provides detailed geography of state corporate income tax changes during 1990–2006. The shaded areas provide the location of tax changes.

'narrative' approach, where we read through archival records on the political and economic environment around the tax changes and identify those changes that were less likely to be anticipated.

The second concern is that both tax changes and firm innovation could be driven by underlying local economic conditions, leading to incorrect estimation of the tax effect. If this is the case, then our large sample tests by themselves may not be sufficient to draw conclusions on causal effects. We address this issue by concentrating on firms that are located in contiguous counties on either side of a state border. These firms are exposed to similar economic conditions, but only one of the states changes taxes, affecting firms in that state but not those across the border. In addition, in further tests, we restrict our analysis to bordering counties in adjacent states that are genuinely similar in terms of political balance (either the governor belonged to the same party in both states, or, if the state houses both belonged to one party, they belonged to the same party in both states) or pre-existing fiscal policies. This alleviates concerns that firms on either side of the border chose to locate on their particular side because of very different state policy environments, and hence they are fundamentally different.

We first present results on patenting activity, and then also examine alternative innovation measures, namely, R&D investment, new product announcements, and patent citations.

3.1. Patenting activity

We start our analysis by plotting a simple univariate chart which shows the effect of tax changes on future patenting activities. In Fig. 2, we depict the change in the number of (eventually successful) patent applications that a firm files (measured in the log scale), following a change in tax rates. The top panel of the figure presents event-time averages of the dependent variable, plotted separately for the treatment and the control groups around tax decreases (and, respectively, tax increases). The bottom panel shows the difference in innovation between the treatment and the control groups averaged in event time, and the 5% confidence interval around this difference.

The patterns in the figure are striking. First, the bottom panel shows that there are no discernible pre-trends in our data—the difference between the treatment and control groups is statistically insignificant in the 3 years prior to the tax change. Second, while tax cuts have a positive but economically small effect on future patenting activity, which is statistically significant in the 2 years after the change, tax increases produce an effect that is more than twice the magnitude of the effect following tax cuts.

3.1.1. Large sample evidence

Next, we focus on a multivariate regression setting, based on a difference-in-differences approach, which allows us to control for time-invariant, firm-specific omitted variables, as well as time-varying industry trends and nationwide shocks to the variables of interest.

We estimate our difference-in-differences model after taking first differences of all variables to control for firm-

level unobserved heterogeneity. As our main variables of interest are tax changes, it is natural to run the first difference specification.

We start by examining percentage increases and cuts in tax rates as our main explanatory variables. Our specification is:

$$\Delta \ln(1 + \# \text{Patents})_{i,s,t+k} = \beta_D \text{Tax decrease}_{st} + \beta_I \text{Tax increase}_{st} + \delta \Delta X_{it} + \alpha_t + \epsilon_{i,s,t+k},$$

where i, s, t index firms, states, and years; $k = 1-3$ indices years following a tax change (if any); while Δ is the first difference operator.

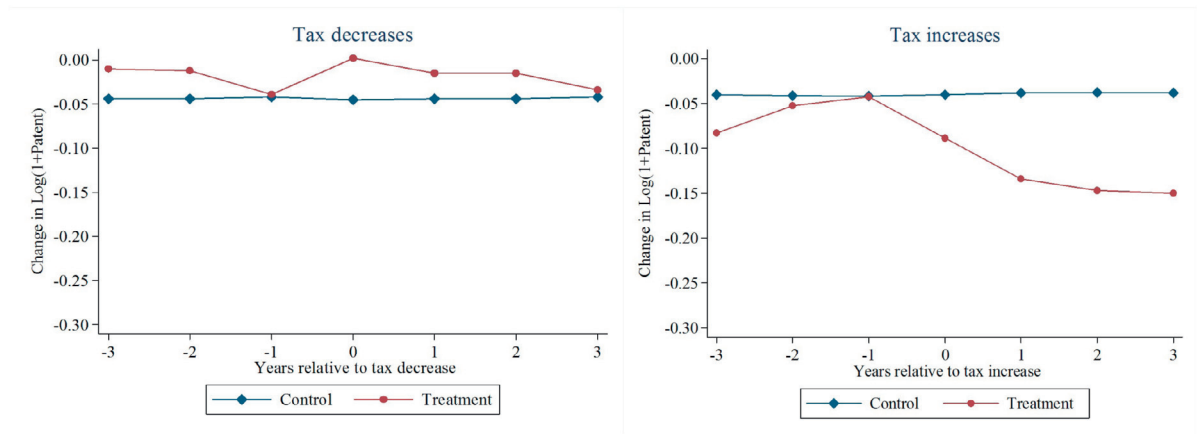
Our main variables of interest are Tax decrease_{st} and Tax increase_{st}. We control for a set of firm-level factors that affect innovation, ΔX_{it} . These include the logarithm of firm sales and capital-labor ratio (Galasso and Simcoe, 2011; Aghion, Van Reenen, and Zingales, 2013), change in profitability, asset tangibility, presence of a debt rating on the firm (to account for availability and ease of financing), R&D-to-sales ratio, and the Herfindahl–Hirschman Index (HHI) based on the distribution of revenues of firms in a particular three-digit SIC industry (as well as its square term, to account for nonlinearities). Since US patenting activity has increased substantially starting in the mid-1980s (Hall, 2004), we control for aggregate trends by including year fixed effects. Additionally, since our main variable of interest is the change in state taxes, we also incorporate state-level economic indicators, namely, change in Gross State Product (GSP), change in total tax revenues of the state as a proportion of GSP, change in the state's population (in logs), and change in the state's unemployment rate. Since our tax treatment is defined at the state level, we cluster standard errors by state, following Bertrand and Mullainathan (2003) and Bertrand, Duflo, and Mullainathan (2004). As we show in Table A.3 in the Internet Appendix, clustering standard errors by firm, year, industry, or firm and year, or industry and year, or state and year do not affect our conclusions.

We report the results in columns 1–3 of Table 2, Panel A, where we use annual percentage tax changes as our key independent variables. We find that most of our effect comes from tax increases rather than tax cuts. In terms of economic magnitude, in the second year following a tax increase—which raises corporate taxes by around 1.1 percentage points—approximately 37% of treated firms patent one fewer innovation project, while there is no significant effect after tax decreases.⁵

We next move to our main specification where instead of using percentage tax changes, we use indicator variables, equaling one if state s increased (respectively, decreased) its corporate tax rate in year t . Following Heider and Ljungqvist (2015), we lump all tax changes together in most of our tests by focusing on binary tax change indicators for two reasons. First, some tax increases (e.g., suspension of net operating loss deductions by California in

⁵ A sample average tax hike of 1.1 percentage points leads to a 3.7% (coefficient in Table 2, column 2, 0.034, times 1.1) decline in a number of patents. Relative to an average of 9.11 patents per firm-year, this is a reduction of 0.37 (–0.037 times 9.11) patents.

2A: Treatment and control



2B: Difference-in-differences

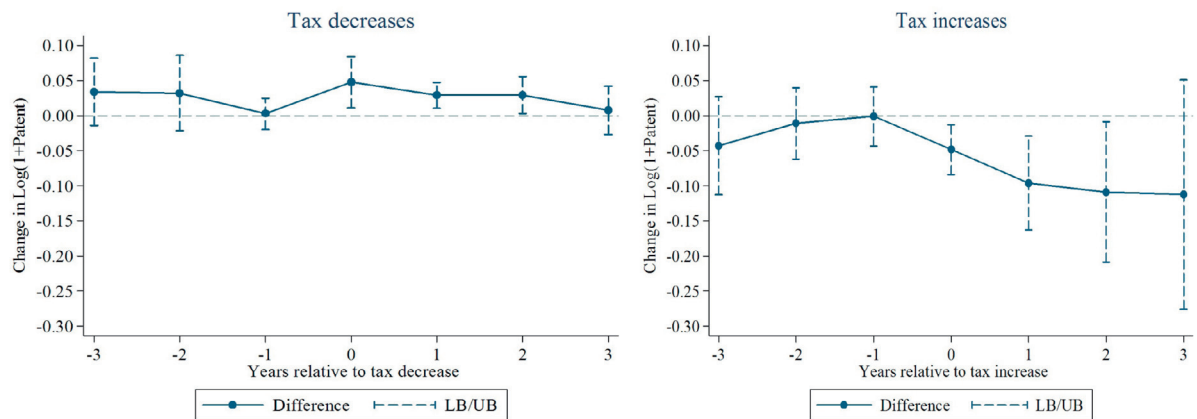


Fig. 2. Innovation and corporate taxes: pre-trends and post-trends. The figure below plots the change in the number of patents that a firm files measured in the log scale, following the change in tax rates. The top panel of the figure presents event-time averages of the $\Delta \ln(1+\text{Patents})_{i,s,t+k}$, where $i, s, t+k$ index firms, states, years with $k = -3$ to 3; plotted separately for the treatment and the control groups following, respectively, tax decreases and tax increases. The bottom panel shows the difference in future innovation between the treatment and the control groups averaged in event time, and 5% confidence intervals (Lower Bound, LB/Upper Bound, UB) around this difference. Please refer to Section 2 and Table 1 for data selection.

2002) cannot be easily quantified in terms of changes in marginal tax rates, though their directional effects are unambiguous. Second, many of the tax changes apply to different provisions of the tax code (e.g., reduction in federal income tax deductibility from 100% to 50% by Missouri in 1993). In this setting, the advantage of the first difference method over specifying the regression equation in levels is that when the tax dummy variable is measured as a

change, we do not have to (1) reset the variable every time a state reverses a tax change, even if the reversal is partial, and (2) leave out consecutive tax increases or decreases in the same state.⁶

⁶ For instance, Atanassov and Liu (2014) follow the alternative approach. When a state changes tax rates, they code tax change dummy as

Table 2

Patents.

This table provides results for our tests on effect of state-level corporate taxes on future patenting activity. In columns 1–3 of Panel A, we examine increases and cuts in tax rates, measured in terms of the percentage point change where possible. In columns 4–6, we replace the changes with the indicators which allows us to include tax changes that cannot be directly quantified, and estimate the following regression:

$$\Delta \text{Ln}(1 + \# \text{Patents})_{i,s,t+k} = \beta_D \text{Tax decrease}_{st} + \beta_I \text{Tax increase}_{st} + \delta \Delta X_{it} + \alpha_t + \epsilon_{i,s,t+k},$$

where $i, s, t+k$ index firms, states, years with $k = 1$ to 3; $\text{Ln}(1 + \# \text{Patents})_{i,s,t+k}$ measures innovation activity by firm i in state s in financial year t . Tax decrease_{st} and Tax increase_{st} are indicators equaling one if state s decreased or increased its corporate tax rate in year t ; X_{it} are firm-level factors that can affect innovation. The results are based on a large sample, i.e., we consider all US publicly listed firms at the intersection of Compustat and the NBER patents database. Refer Table 1 for data selection and Appendix for variable definitions. In Panel B, columns 1–3, we take the position of an agent trying to predict tax changes out-of-sample using the macro information and available news on tax changes and only consider unpredictable tax changes based on the prediction model. Panel B, columns 4–6, provides results for our tests motivated by the ‘narrative’ studies of Romer and Romer (2010) and Mertens and Ravn (2012). We read through the records on our tax changes in the news media, the Internet, Book of States, Fiscal Surveys of States, Federal Reserve policy documents, and online state legislative archives to understand the precise background and timing of the tax changes. Then we exclusively examine those changes that were both unanticipated [i.e., could not have been predicted more than a year ahead, as in Mertens and Ravn (2012)], as well as unrelated to local economic conditions. Panel C reports the results based on one-to-one matching between firms located in contiguous bordering counties, where for each firm that experienced a tax change we assign a firm headquartered in the county across the border that is closest in asset size to the treated firm. In Panel D, we restrict our sample to firms in contiguous counties straddling a state border, and control for county-pair-year fixed effects. Our specification is then:

$$\Delta \text{Ln}(1 + \# \text{Patents})_{i,s,t+k} = \beta_D \text{Tax decrease}_{st} + \beta_I \text{Tax increase}_{st} + \delta \Delta X_{it} + \alpha_t + \gamma_{ct} + \epsilon_{i,s,t+k}$$

where c, i, s, t index county-pairs, firms, states, and years; $k = 1-3$ indices years following a tax change (if any); while Δ is the first difference operator. Columns 1–3 report results with county-pair-year fixed effects and county-pair-year as well as industry-year fixed effects in columns 4–6. In Panel E, columns 1–3, we further restrict our analysis to counties in adjacent states that are similar in terms of political balance (either the governor belonged to the same party in both states, or, if the state houses both belonged to one party, they belonged to the same party in both states). In Panel E, columns 4–6, we focus on firms in counties in adjacent states that have similar pre-existing fiscal policies, for example, if we restrict all state taxes (corporate taxes, wage taxes, and capital gains taxes) as well as the budget surplus (as a percentage of GSP) across the bordering states to be within one standard deviation of each other. In Panel E, columns 7–9, we only consider unpredictable tax changes for firms located in contiguous bordering counties with similar pre-existing fiscal policies. All regressions include firm-level and state-level controls, not reported for brevity. These controls include the logarithm of firm sales and capital-labor ratio, change in profitability, asset tangibility, presence of a debt rating on the firm, R&D-to-sales ratio, HHI and its square term, change in state's GSP, change in total tax revenues of the state as a proportion of GSP, change in the state's population (in logs), and change in the state's unemployment rate. Standard errors are clustered at state-level and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Panel A: Large sample

	$\Delta \text{Ln}(1 + \# \text{Patents})_{t+k}$					
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)	$k = 1$ (4)	$k = 2$ (5)	$k = 3$ (6)
$\Delta^- \text{Taxrate}_{s,t}$	0.007 (0.016)	−0.020 (0.014)	0.007 (0.019)			
$\Delta^+ \text{Taxrate}_{s,t}$	−0.019 (0.012)	−0.034 (0.005)***	−0.001 (0.007)			
Tax decrease _{s,t}				−0.004 (0.013)	−0.001 (0.009)	0.004 (0.010)
Tax increase _{s,t}				−0.055 (0.014)***	−0.053 (0.020)***	−0.060 (0.037)
Controls	YES	YES	YES	YES	YES	YES
Year FEs	YES	YES	YES	YES	YES	YES
Obs.	40,092	35,433	30,812	42,192	37,317	32,557

Panel B: Unpredictable tax changes and the narrative approach

	Unpredictable tax changes			Narrative approach		
	$\Delta \text{Ln}(1+\#\text{Patents})_{t+k}$					
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)	$k = 1$ (4)	$k = 2$ (5)	$k = 3$ (6)
Tax decrease $_{s, t}$	0.022 (0.014)	−0.005 (0.014)	0.012 (0.010)	−0.001 (0.017)	−0.008 (0.012)	−0.007 (0.014)
Tax increase $_{s, t}$	−0.055 (0.014)***	−0.053 (0.020)***	−0.060 (0.037)	−0.052 (0.019)***	0.002 (0.033)	0.004 (0.028)
Controls	YES	YES	YES	YES	YES	YES
Year FEs	YES	YES	YES	YES	YES	YES
Obs.	42,192	37,317	32,557	42,192	37,317	32,557

continued on next page.

Table 2
(continued)

Panel C: Matching between firms located in contiguous bordering counties									
	$\Delta \text{Ln}(1+\#\text{Patents})_{t+k}$								
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)						
Tax decrease _{s, t}	−0.044 (0.028)	−0.043 (0.030)	−0.001 (0.031)						
No. of pairs	579	517	442						
Tax increase _{s, t}	−0.046 (0.041)	−0.128 (0.049)***	−0.049 (0.056)						
No. of pairs	257	230	173						
Panel D: Contiguous bordering counties									
	$\Delta \text{Ln}(1+\#\text{Patents})_{t+k}$								
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)	$k = 1$ (4)	$k = 2$ (5)	$k = 3$ (6)			
Tax decrease _{s, t}	−0.019 (0.015)	−0.018 (0.018)	0.008 (0.032)	−0.027 (0.021)	−0.025 (0.024)	−0.008 (0.019)			
Tax increase _{s, t}	−0.062 (0.031)**	−0.081 (0.037)**	−0.021 (0.060)	−0.050 (0.026)**	−0.042 (0.040)	−0.034 (0.054)			
Controls	YES	YES	YES	YES	YES	YES			
County-pair-year FEs	YES	YES	YES	YES	YES	YES			
Industry-year FEs	NO	NO	NO	YES	YES	YES			
Obs.	6,204	5,337	4,508	6,204	5,337	4,508			
Panel E: Bordering counties with similar economic policies and unpredictable tax changes									
	Politically similar			Similar economic policies			Similar economic policies with unpredictable tax changes		
				$\Delta \text{Ln}(1+\#\text{Patents})_{t+k}$					
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)	$k = 1$ (4)	$k = 2$ (5)	$k = 3$ (6)	$k = 1$ (7)	$k = 2$ (8)	$k = 3$ (9)
Tax decrease _{s, t}	−0.036 (0.028)	−0.028 (0.024)	0.005 (0.023)	−0.025 (0.026)	−0.010 (0.035)	−0.015 (0.035)	0.012 (0.033)	−0.034 (0.037)	0.014 (0.042)
Tax increase _{s, t}	−0.071 (0.031)**	−0.066 (0.062)	−0.098 (0.072)	−0.119 (0.040)***	0.009 (0.037)	−0.064 (0.058)	−0.122 (0.041)***	0.010 (0.037)	−0.068 (0.058)
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
County-pair-year FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry-year FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES
Obs.	4,132	3,630	3,061	2,960	2,519	2,169	2,960	2,519	2,169

We report the results in columns 4–6 of Table 2, Panel A. We find that the average treated firm files one fewer patent following a tax increase, as compared to an average control firm.⁷ Again, we do not find any systematic effect following tax cuts. We discuss some possible reasons on the asymmetry of our results in Section 6. One plausible explanation is that firms may be able to cut innovation projects more quickly following tax increases, but they might take longer to set up innovative capacity and hire new employees after a tax cut. Such a longer response time

of tax decreases might make it harder to tease out clean causal effects. By the time higher effort or investment following these tax cuts starts bearing fruit, other potentially confounding effects that interfere with innovation might also kick in. Such noise could lead to a reduction in the magnitude and significance of the tax decrease effect that we identify.

Also, since our dependent variable is measured as change in patents, and we find no reversals later, our results imply that patenting declines in the first few years within the tax change, and stays at the new lower level thereafter.⁸ It may seem counterintuitive to some that the effect shows up so soon on patenting (or, in later results, on new product introductions), which many believe is a time-consuming process. However, we believe that this is expected in our setting. Here we study changes in taxes,

one; if it reverses the change partly later, they code it back to zero. This may be an issue if the tax effect is asymmetric in the data (like we find), since it imposes symmetry on how the original rise and the subsequent cut affects innovation. Instead, we impose no symmetry in our analysis. Moreover, if the same state changes taxes in the same direction again in a few years, in the alternative approach one has to ignore the change, since the dummy is already set to one. We do not have to do this.

⁷ Tax increases are followed in the next 2 years by a decline of (0.055+0.053) times 9.11 = 0.98 patents per treated firm.

⁸ We report effects only in the 3 years following the tax changes, since we do not find any statistically robust results in later years.

which affect the net profit (in particular, they affect the revenue side of taxable profits and not just costs) from innovation projects. When tax increases hit a firm, some of its projects in more advanced stages—even those ready to be patented—may no longer remain profitable, leading the firm not to file patents for these projects. So, the effect in the first couple of years could simply reflect that firms reduce patenting activity over the stock of ready projects, rather than new innovation. But—crucially—patenting activity *does not recover* later, so the decline in later years has to come from a decline in the flow of new innovation projects.⁹

In the following sections, we address the two main concerns with any analysis that tries to identify the effect of legal changes such as changes in taxes: (1) some of the tax changes might have been predicted and so firms could have adjusted in advance, in which case our estimates could be biased; (2) spurious correlation as tax changes could be driven by economic conditions that at the same time affect firm innovation directly.

3.1.2. Unpredictable tax changes

We first address the predictability of tax changes by adopting a model-based approach. In particular, we use an econometric model to predict tax changes out-of-sample using information on state-level economic conditions and news coverage, and report the results in columns 1–3 of Panel B, Table 2.

To start with, we perform a textual search analysis of major newspapers in the United States in our sample period. We search for keywords indicative of corporate tax changes (we use the full list of newspapers covered by Factiva News database; Internet Appendix A.2 lists our exact keywords). Then we count the number of articles in the year before the tax cuts (increases) as our proxy for a “predictive discussions in the media regarding tax cuts (increases).”¹⁰

We augment this sample with state-level macro variables, and take the position of an agent trying to predict tax changes out-of-sample using macro information and available news on the topic. We use past information on tax changes in a regression framework, with an updated sample each year. That is, in our estimations that predict tax changes in 1990, we take the position of a firm manager who could use all information (news and state-level macro data) from 1978 to 1989 to form her impression about the relationship between state economic conditions and tax changes. We perform such exercises separately for increases and decreases for each year in the sample. For instance, in order to predict tax changes in 1991, we keep the

same methodology but we instead use a regression sample from 1978 to 1990.

Next, we take the predicted value for each state and year, and classify tax changes as ‘unpredictable’ if the predicted probability of a change in that year is less than an optimally determined cutoff.¹¹

Our results remain similar even when we use unpredictable tax changes defined in this way.

3.1.3. Narrative approach

Our next set of tests address the concern that while the tax changes we examined in the previous section might have been unpredictable, they could have been driven by the local economic conditions that also simultaneously affect innovation. To address this issue, we take a different approach, motivated by the so-called ‘narrative’ approach of [Romer and Romer \(2010\)](#) and [Mertens and Ravn \(2012\)](#) [see also [Mertens \(2015\)](#), and [Giroud and Rauh \(2015\)](#) on corporate taxes]. Here, we carefully check background information on the passage of our tax changes. In particular, we read through archival records on our tax changes in the news media, the Internet, Book of States, Fiscal Surveys of States, Federal Reserve policy documents, and online state legislative archives to understand the precise background and timing of the tax changes. Then we exclusively examine those changes that were both unanticipated [could not have been predicted more than a year ahead, as in [Mertens and Ravn \(2012\)](#)] as well as unrelated to local economic conditions.

While state tax changes are often driven by economic conditions and are planned, this is not always the case. For example, in some instances, corporate taxes were made a campaign issue in closely fought elections. An example of such a change is Colorado’s cut in corporate taxes in 1999, which followed a close gubernatorial election victory for Republican Bill Owens by a margin of 49.1–48.4%. Bill Owens campaigned on a pledge to reduce taxes, a policy that had been opposed by the incumbent governor who contested Bill Owens. In other instances, states had to raise corporate taxes due to Court rulings on the way their school education systems were funded by local taxpayers. An example of such a change is Vermont’s corporate tax increase from 8.25% to 9.75% in 1997 to fund unexpected expenditure arising out of a need to restructure the state’s school funding system, following the Supreme Court’s ruling in *Brigham et al.v. State of Vermont*.¹²

⁹ In fact, in this setting, if patenting does not change in the first few years, that would indicate that the firm continues to patent the stock of ready projects as before although the net after-tax profit flows from these projects have changed, which is counterintuitive.

¹⁰ Note that one might be concerned that some of these articles might not have been referring to the precise tax cut that eventually took place. However, in our context, we think such discussions are still relevant: firms might not have been readied for the *precise* cut that eventuated, but as long as they were expecting *some sort of* cut, they might have made adjustments beforehand.

¹¹ We use a linear probability model to predict tax changes. The cutoff is chosen by minimizing the Euclidian distance between the ideal in-sample fit and the observed fit given the cutoff, i.e., minimizing $d = [Pr(\hat{Y} = 0|Y = 0) - 1]^2 + [Pr(\hat{Y} = 1|Y = 1) - 1]^2$. This way of choosing the cutoff balances type-I and type-II errors in classification. As it turns out, predicting tax changes—even using a long list of variables—is difficult: the *F*-statistics for the joint significance of even our most significant variables is less than 5 [*R*-squares have a similar low magnitude of 10–12% both in our tests and in similar tests in [Heider and Ljungqvist \(2015\)](#)]. As shown in Internet Appendix Table A.4, these results are robust to using fixed predictability cutoffs at 10%, 20%, using 10-year rolling windows for estimation, or on estimating our model based only on state-level macroeconomic data (without news coverage data).

¹² We do not claim that tax-change-funded education expenditures might not directly affect innovation. However, an increase in education expenditures would probably lead to more innovation, which biases us

Note that it is challenging to find background coverage of all state-level corporate tax changes. Large media outlets (such as the *Wall Street Journal* or the *New York Times*) often provide no news on tax changes, especially in small states. Even if we are able to uncover information from the relevant legislatures, sometimes it is unclear how far in advance the change could have been predicted. Overall, out of the 87 tax changes we examine, we are able to uncover enough background information to categorize 55 of them with confidence.¹³ Out of these 55, we restrict our analysis here to the 29 changes (11 increases and 18 cuts) that were clearly unrelated to local economic conditions and unanticipated.

It turns out that tax cuts were more predictable than tax increases. Tax cuts are more frequently financed by better economic conditions, and they are often staggered, hence predictable. But even when we explicitly account for this and examine solely those tax cuts that suffer from neither issue, we still fail to uncover any significant effects from these cuts. On the other hand, tax increases continue to remain significant in the data. We present these results in columns 4–6 of Panel B of Table 2. Overall, when we restrict our analysis to plausibly unanticipated tax changes unrelated to state-level economic conditions, we continue to find consistent results.

3.1.4. Contiguous counties straddling state borders

While the narrative approach addresses the issues described above, some skeptics might express concern at the element of arbitrariness involved in classifying tax changes in this fashion. For example, for certain changes, there might be conflicting views in the media regarding underlying cause. So we take a complementary and arguably less arbitrary approach here, based on policy discontinuity at state borders.

First, we concentrate on firms that are located in contiguous counties on either side of a state border. Such focus on a narrower geography allows us to control for potentially unobserved time-varying economic heterogeneity across treated and control firms more accurately. These firms should be subject to similar economic conditions due to their close geographic proximity, but they are subject to different tax changes: one firm of the pair is not exposed to changing taxes in that year, while the other one is.

Panel C of Table 2 reports simple difference-in-differences estimates based on one-to-one matching between firms located in contiguous bordering counties, where for each firm that experienced a tax change we find

against finding a negative effect from the resultant tax increase like we find. Moreover, most of these education-related changes concerned how neighborhood primary and high schools were funded, i.e., either through local or state-level taxes. In that case, even if better school education systems might affect innovation, this would presumably take years to show up in the data, whereas the effect from the tax increase needed to finance the change is likely to show up much sooner. Finally, we perform an additional robustness test where we leave out three (out of 29) changes that were related to university, college, or vocational education budgets. Our results remain very similar.

¹³ These numbers are similar to Giroud and Rauh (2015), who can identify background information on 107 out of a total of 161 tax changes they examine.

a (“matching”) firm headquartered in the county across the border that is closest in asset size to the treated firm.

In Panel D of Table 2, we perform a regression analysis where we augment our baseline specification above by including county-pair-year fixed effects to account for time-varying unobserved heterogeneity, caused by changing local economic conditions. In particular, we assume that even if changes in economic conditions vary across states, in our narrowly defined geographic region (a pair of neighboring counties) they change similarly on either side of the state border. Under this assumption, our county-pair-year fixed effects absorb any local economic shocks, allowing us to identify the tax effect over and above any such variations. Our specification is then:

$$\Delta \ln(1 + \#Patents)_{i,s,t+k} = \beta_D Tax\ decrease_{st} + \beta_I Tax\ increase_{st} + \delta \Delta X_{it} + \alpha_t + \gamma_{ct} + \epsilon_{i,s,t+k},$$

where c, i, s, t index county-pairs, firms, states, and years; $k = 1-3$ indices years following a tax change (if any); while Δ is the first difference operator.

Columns 1–3 of Panel D present these results. In columns 4–6, we also control for time-varying, industry-level unobserved heterogeneity by adding industry-year fixed effects, in addition to county-pair-year fixed effects.

Both our matching and regression-based results show that tax increases do indeed have additional explanatory power for future innovation, over and above that of local economic conditions. In terms of economic magnitudes, our most conservative estimates from these tests imply that about 67% of treated firms affected by a tax increase obtain approximately one fewer patent following a tax increase (compared to a mean of 13.3 patents per firm-year obtained by firms in our contiguous counties sample).¹⁴ We do not observe any discernible change in patenting following tax cuts.

Note again that the underlying identifying assumption in columns 4–6 is that a pair of firms in neighboring counties on either side of a state border, and in the same industry, are exposed to roughly the same local economic variations and that both firms respond to such changes similarly. In particular, since we estimate difference-in-differences specifications, our methodology removes all persistent cross-state differences. Moreover, since different states change taxes at different times, many firms operating in a given state end up in the treatment group in 1 year, and in the control group in some other years. This makes it unlikely that our results are driven by our treatment firms' propensity to respond differently to local economic changes, as compared to our control firms.

3.1.5. Contiguous counties, refinements: politically similar states and unpredictable tax changes

We perform two further tests based on contiguous county-firm analysis. First, such discontinuity tests might not be valid if, for example, firms on either side of the border have chosen to be located on that particular side

¹⁴ The coefficient in column 4 of Panel D, -0.05 , times 13.3 , the mean value of patents, yields -0.67 .

because of very different state policies. This is likely to be a concern when policies in the two states differ drastically, for instance, because they lie on the opposite sides of the political spectrum. In order to avoid this problem, we further restrict our analysis to counties in adjacent states that are genuinely similar in terms of political balance (either the governor belonged to the same party in both states, or, if the state houses both belonged to one party, they belonged to the same party in both states). We find that our results are robust (Panel E, columns 1–3).¹⁵ Similar results obtain if we only compare those states that have similar pre-existing fiscal policies, for example, if we restrict all state taxes (corporate taxes, wage taxes, and capital gains taxes) as well as the budget surplus (as a percentage of GSP) across the bordering states to be within one standard deviation of each other (Panel E, columns 4–6). These results limit concerns about self-selection into state political regimes driving our results.

Finally, in columns 7–9 of the same panel we address concerns regarding local economic effects and the predictability of tax changes simultaneously. Even when we restrict our analysis to unpredictable tax changes and contiguous-county-level neighboring firms straddling policy-wise similar state borders, we find similar results as before.

Note that while the tax increase effect we document always shows up within the first 2 years, in some specifications the effect shows up significantly in year 1, in others also in year 2. We would like to point out that several of the specifications that we use here are not merely slight modifications of methodology but involve drastically different samples, econometric specifications (different high-dimensional fixed effects, for example), and different definitions of what is considered the treatment group (firms affected by unpredictable tax changes versus all changes, for example). This is especially true in the case of our bordering county-level analysis. Focusing on the narrow geography of bordering counties reduces our sample size to less than 10% of the full sample in some of these tests, so the fact that we can still uncover significant effects even within this sample—albeit not always in the same year—is reassuring.

Further, we also find it comforting that the tax increase effect we document always shows up within the first 2 years, across these very different methods of analysis. Also, even in the cases where the coefficients on either of the first 2 years are not statistically significant at conventional levels, they almost always have the expected sign. This makes us think that one possible reason behind this empirical pattern is limited statistical power.

3.1.6. Other tests on local economic effects

We provide three additional sets of tests that address concerns regarding local economic conditions from different alternative angles. As these additional approaches rely on methodologies and sample periods different from our specifications above, we describe the methods used below

in more detail in the Internet Appendix and only report a brief description of results in this section.

In our first additional test, we rely only on firms that differ in their headquarter and R&D states for our identification. If the firm's headquarter state is the most relevant one for tax purposes, while economic conditions surrounding innovation matter most at the level of the R&D state, then we can identify the tax effect on innovation by exploiting the difference in economic conditions between the two states. Here we control for the local economic shocks that change innovative conditions at the firm R&D state level by using R&D state times year fixed effects. In other words, by controlling for R&D state times year fixed effects we are able to see how innovation changed through the channel of headquarter instructing its subsidiaries at R&D state to change innovation policies rather than due to local economic conditions. We define a firm's R&D state as one in which most of the innovators that file a patent for the firm are located, and in Table A.5 of the Internet Appendix show that even after controlling for these R&D state times year fixed effects, tax increases lead to a lower number of patents.

Second, in Table A.6 in the Internet Appendix, we report results where we collect data on tax changes in the 1980s, and look at state corporate tax policy changes which occurred only as a response to negative shocks to the state fiscal position caused by federal legislation. The Economic Recovery Tax Act of 1981 (ERTA81) implemented accelerated depreciation schedules (through its implementation of the accelerated cost recovery system (ACRS)), thereby reducing current tax revenues for states that followed federal rules. To offset this reduction, four states (Indiana, Iowa, Nebraska, and Wisconsin) increased the corporate income tax rate (Aronson and Hilley, 1986; Giroud and Rauh, 2015). Even in this test, we find that innovation activity declined 2 years after the tax change in affected states, with no significant effect in other years. Our magnitudes from this test are also similar to our overall sample results.

Third, we adopt an instrumental variables approach. In particular, we rely on state-level differences (and changes therein) in super-majority requirements to pass a tax increase, as well as its interaction with state political balance, as our instruments. Our results, reported in Tables A.7 and A.8 of the Internet Appendix, are consistent.

3.2. Alternative innovation measures

Patents can be thought of as an intermediate product in the innovation process, R&D investment being the input and new products being one of the outputs (the other type of output might be process innovations, which is difficult to capture in the data).

3.2.1. R&D

We first consider the response of R&D spending to tax changes. We run regressions similar to those presented in Table 2 with two different measures of R&D. First, we examine the ratio of R&D investments to sales, and then we show results using $\ln(1 + \text{R\&D})$ to ensure that changes in R&D (and not sales) are driving our results.

¹⁵ Our results are very similar if we restrict our test to ensuring that the governors in both states are from the same party.

Table 3

R&D expenditures.

This table studies the effect of tax changes on R&D expenditure and provides results for the following regression:

$$\Delta \frac{R\&D}{Sales} \text{ or } \Delta \ln(1 + R\&D)_{i,s,t+k} = \beta_0 \text{Tax decrease}_{st} + \beta_1 \text{Tax increase}_{st} + \delta \Delta X_{it} + \alpha_t + \epsilon_{i,s,t+k},$$

where $i, s, t+k$ index firms, states, and years, with $k = 1$ to 3. We measure R&D as (1) R&D expenditure (xrd) scaled by lagged total sales($sale$) and (2) $\ln(1+R\&D(xrd))$. Tax decrease $_{st}$ and Tax increase $_{st}$ are indicators equaling one if state s decreased or increased its corporate tax rate in year t ; X_{it} are firm-level factors that can affect research expenditure. Refer to Table 1 for data selection and Appendix for variable definitions. Columns 1–3 present results for R&D investments to sales; columns 4–6 show results using $\ln(1 + R\&D)$. Panel A reports results based on large sample, like Panel A of Table 2 while Panel B reports results, like columns 7–9 in Panel E of Table 2, where we only consider unpredictable tax changes for firms located in contiguous bordering counties with similar pre-existing fiscal policies. All regressions include firm-level and state-level controls (excluding $\Delta R\&D/Sales_{i,t}$) and year fixed effects, not reported for brevity. Small sample regressions include county-pair-year as well as industry-year fixed effects. Standard errors are clustered at state-level and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Panel A: Large sample						
	$\Delta \frac{R\&D}{Sales}_{t+k}$			$\Delta \ln(1+R\&D)_{t+k}$		
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)	$k = 1$ (4)	$k = 2$ (5)	$k = 3$ (6)
Tax decrease $_{s,t}$	0.005 (0.004)	0.010 (0.007)	0.010 (0.005)**	−0.005 (0.005)	−0.015 (0.004)***	0.001 (0.011)
Tax increase $_{s,t}$	−0.012 (0.006)*	0.004 (0.011)	0.005 (0.007)	−0.023 (0.012)**	−0.019 (0.010)**	−0.019 (0.008)**
Controls	YES	YES	YES	YES	YES	YES
Obs.	42,184	37,160	32,282	42,192	37,317	32,557
Panel B: Bordering counties with similar economic policies and unpredictable tax changes						
	$\Delta \frac{R\&D}{Sales}_{t+k}$			$\Delta \ln(1+R\&D)_{t+k}$		
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)	$k = 1$ (4)	$k = 2$ (5)	$k = 3$ (6)
Tax decrease $_{s,t}$	0.002 (0.024)	−0.003 (0.043)	−0.033 (0.030)	−0.010 (0.022)	0.013 (0.021)	−0.034 (0.016)**
Tax increase $_{s,t}$	−0.008 (0.032)	−0.024 (0.035)	0.018 (0.083)	0.007 (0.017)	−0.032 (0.016)**	−0.045 (0.039)
Controls	YES	YES	YES	YES	YES	YES
County-pair-year FEs	YES	YES	YES	YES	YES	YES
Industry-year FEs	YES	YES	YES	YES	YES	YES
Obs.	2,960	2,510	2,157	2,960	2,519	2,169

We report the results in Table 3. In Panel A, columns 1–3 present results for R&D investments to sales, while columns 4–6 show results using $\ln(1 + R\&D)$. In terms of economic magnitudes, our most conservative measure (column 1) shows that R&D to sales declines by 4.3% of its sample mean after a tax increase.¹⁶

In Panel B of Table 3, we repeat our tests but again focus on unpredictable tax changes and contiguous-county-level neighboring firms straddling policy-wise similar state borders, as in columns 7–9 of Panel E in Table 2, and show that the tax increase effect documented earlier remains when we use the unscaled R&D measure, although the effect loses statistical significance when we scale by sales, probably due to the lack of statistical power in the small sample. There is some evidence that R&D also responds to tax cuts, but this evidence is not robust. For example, tax declines are followed by an R&D increase in some specifications (Panel A, column 3) and declines in others (Panel A, column 5, Panel B, column 6).

Overall, we find that R&D also declines following tax increases, and local economic effects or predictability-driven biases are unlikely to be driving these results.

3.2.2. New product announcements

We now consider whether major new product launches are affected by tax changes. In constructing measures of new product announcements we combine textual analysis with event studies conducted on stock market returns. We first search LexisNexis News database for company press releases that are tagged under the subject “New Products” and where their headlines include keywords (with the roots of words) such as “Launch,” “Product,” “Introduce,” “Begin,” “Unveil.” We download all such press releases and parse out the firm ticker and the date of the announcement from the text. To be consistent with our baseline sample, we only consider firms listed on NYSE, Nasdaq or Amex. Using this criterion, we obtain 98,221 unique firm press releases.

We then identify material information about new products among these press releases. Here we rely on the intuition that if the press release containing our new product keywords indeed refers to a major innovation, the equity market should respond to the news. Similar to Kogan, Panikolaou, Seru, and Stoffman (2015), who estimate the

¹⁶ The change in R&D to sales, given by coefficient in Table 3, column 1, is −0.012, so the percentage change is −0.012/0.28, where 0.28 is the sample mean of R&D to sales.

Table 4

New product announcements.

This table studies the effect of tax changes on the product announcement. We use different measures of product announcements and estimate the following equation:

$$\Delta \text{Sum of all positive CARs or } \Delta \text{Ln}(1 + \# \text{Major new products})_{i,s,t+k} = \beta_D \text{Tax decrease}_{st} + \beta_I \text{Tax increase}_{st} + \delta \Delta X_{it} + \alpha_t + \epsilon_{i,s,t+k}$$

where $i, s, t+k$ index firms, states, and years, with $k = 1$ to 3. Tax decrease_{st} and Tax increase_{st} are indicators equaling one if state s decreased or increased its corporate tax rate in year t ; X_{it} are firm-level factors that can affect new product announcements. We implement event-study methodology by fitting a market model over $(-246, -30)$ period to get the expected returns on the firm's stock, and then estimating cumulative abnormal returns (CARs) over 3 $(-1, 1)$ day period around the announcement. Refer to Table 1 for data selection and Appendix for variable definitions. Columns 1–3 report the results with sum of all positive cumulative abnormal product announcement returns over the year as the dependent variable. Here we include firm size and book-to-market ratio as additional controls. In columns 4–6, the dependent variable is the number of product announcements with 3-day event CARs above the 75th percentile, year by year after adjusting for firm size and book-to-market ratio. Panel A reports results based on a large sample, like Panel A of Table 2 while Panel B reports results, like columns 7–9 in Panel E of Table 2, where we only consider unpredictable tax changes for firms located in contiguous bordering counties with similar pre-existing fiscal policies. All regressions include firm-level and state-level controls and year fixed effects, not reported for brevity. Small sample regressions include county-pair-year as well as industry-year fixed effects. Standard errors are clustered at state-level and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Panel A: Large sample

	$\Delta(\text{Sum of all positive CARs})_{t+k}$			$\Delta \text{Ln}(1 + \# \text{Major new products})_{t+k}$		
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)	$k = 1$ (4)	$k = 2$ (5)	$k = 3$ (6)
Tax decrease _{s, t}	0.0009 (0.002)	0.001 (0.002)	−0.0002 (0.001)	0.007 (0.008)	−0.002 (0.007)	0.008 (0.006)
Tax increase _{s, t}	−0.004 (0.002)*	0.001 (0.003)	−0.003 (0.002)	−0.048 (0.019)**	0.014 (0.012)	−0.018 (0.017)
Controls	YES	YES	YES	YES	YES	YES
Obs.	42,057	37,188	32,434	42,057	37,188	32,434

Panel B: Bordering counties with similar economic policies and unpredictable tax changes

	$\Delta(\text{Sum of all positive CARs})_{t+k}$			$\Delta \text{Ln}(1 + \# \text{Major new products})_{t+k}$		
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)	$k = 1$ (4)	$k = 2$ (5)	$k = 3$ (6)
Tax decrease _{s, t}	0.005 (0.005)	−0.002 (0.006)	−0.005 (0.007)	0.051 (0.047)	0.024 (0.037)	0.036 (0.020)
Tax increase _{s, t}	−0.001 (0.007)	0.006 (0.004)	−0.004 (0.003)*	−0.089 (0.043)**	0.014 (0.025)	−0.122 (0.054)**
Controls	YES	YES	YES	YES	YES	YES
County-pair-year FEs	YES	YES	YES	YES	YES	YES
Industry-year FEs	YES	YES	YES	YES	YES	YES
Obs.	2,940	2,501	2,152	2,940	2,501	2,152

value of the patent by relying on the stock price reaction to its grant, we look at a firm's stock price reaction to measure the expected value of the product announcement.

We implement an event-study methodology by fitting a market model over $(-246, -30)$ period to get the expected returns on the firm's stock, and then estimate cumulative abnormal returns over a 3-day period $(-1, 1)$ around the announcement. After doing so, we are left with 56,797 announcements. To estimate the value/total number of material firm announcements over the year, we either (a) sum all positive¹⁷ cumulative abnormal returns around product announcements made by firms over the year, or (b) count the number of announcements with cumulative abnormal returns above the 75th percentile in the respective calendar year. The first method is designed to capture the total incremental value of all new product introductions by a firm during the year. The second method instead uses a

count measure to control for any outlier abnormal returns, and is meant to distill major new innovations introduced by the firm. We estimate specifications similar to our baseline regressions, but instead of patent count we use our measures of new products here.

We report the results in Table 4, Panel A. In columns 1–3 we rely on the sum of all positive cumulative abnormal announcement returns over the year. We find that shareholder value added by new product announcements drops by 50 basis points in the first year after a tax increase. In columns 4–6 we rely on the number of announcements above the 75th percentile, and find similar results. In terms of economic magnitudes, the number of major new product introductions by firms drops by 5.1% in the first year after a tax rise.

In Panel B of Table 4, we repeat our tests but again focus on unpredictable tax changes and contiguous-county-level neighboring firms straddling policy-wise similar state borders, as in Table 2, Panel E. We observe similar results as in the previous panel.

Of course, we recognize that one shortcoming of this measure is that if the stock market anticipates new

¹⁷ We only consider positive abnormal returns to remove any confounding product announcements that were not associated with new product introductions. For instance, these could be “delays in new product introductions” or “new product recalls.”

Table 5

Innovation quality.

This table studies the effect of tax changes on the quality of innovation and provides results for the following regression:

$$\Delta \ln \left(1 + \# \text{Citations or } \frac{\text{Citations}}{\text{Patents}} \right)_{i,s,t+k} = \beta_D \text{Tax decrease}_{st} + \beta_I \text{Tax increase}_{st} + \delta \Delta X_{it} + \alpha_t + \epsilon_{i,s,t+k},$$

where $i, s, t+k$ index firms, states, years with $k = 1$ to 3; $\ln(1 + \# \text{Citations or } \frac{\text{Citations}}{\text{Patents}})_{i,s,t+k}$ measures quality of innovation activity by firm i in states in financial year t . Tax decrease_{st} and Tax increase_{st} are indicators equaling one if state s decreased or increased its corporate tax rate in year t ; X_{it} are firm-level factors that can affect innovation. Citations are adjusted for truncation bias. Panel A reports results based on a large sample, like Panel A of Table 2, while Panel B reports results, like columns 7–9 in Panel E of Table 2, where we only consider unpredictable tax changes for firms located in contiguous bordering counties with similar pre-existing fiscal policies. All regressions include firm-level and state-level controls, and year fixed effects, not reported for brevity. Small sample regressions include county-pair-year as well as industry-year fixed effects. Standard errors are clustered at state-level and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Panel A: Large sample

	$\Delta \ln(1 + \# \text{Citations})_{t+k}$			$\Delta \ln(1 + \frac{\text{Citations}}{\text{Patents}})_{t+k}$		
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)	$k = 1$ (4)	$k = 2$ (5)	$k = 3$ (6)
Tax decrease _{s, t}	0.0008 (0.032)	0.025 (0.026)	0.003 (0.032)	0.002 (0.023)	0.029 (0.021)	−0.010 (0.023)
Tax increase _{s, t}	−0.124 (0.039)***	−0.144 (0.063)**	−0.046 (0.068)	−0.058 (0.025)**	−0.084 (0.043)*	−0.011 (0.035)
Controls	YES	YES	YES	YES	YES	YES
Obs.	42,192	37,317	32,557	42,192	37,317	32,557

Panel B: Bordering counties with similar economic policies and unpredictable tax changes

	$\Delta \ln(1 + \# \text{Citations})_{t+k}$			$\Delta \ln(1 + \frac{\text{Citations}}{\text{Patents}})_{t+k}$		
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)	$k = 1$ (4)	$k = 2$ (5)	$k = 3$ (6)
Tax decrease _{s, t}	−0.053 (0.107)	0.105 (0.135)	−0.030 (0.159)	−0.077 (0.093)	0.111 (0.115)	−0.038 (0.101)
Tax increase _{s, t}	−0.167 (0.070)**	−0.035 (0.087)	−0.064 (0.079)	−0.140 (0.048)***	0.023 (0.065)	−0.041 (0.059)
Controls	YES	YES	YES	YES	YES	YES
County-pair-year FEs	YES	YES	YES	YES	YES	YES
Industry-year FEs	YES	YES	YES	YES	YES	YES
Obs.	2,960	2,519	2,169	2,960	2,519	2,169

products even before the company first discusses them in a newsworthy fashion, then the market-return screen employed here will be noisy.

3.2.3. Innovation quality

The literature shows that patents differ greatly in terms of their relative importance. Therefore, simple patent counts do not necessarily capture the economic importance of the associated inventions (Harhoff, Narin, Scherer, and Vopel, 1999; Hall, Jaffe, and Trajtenberg, 2005). In this section, we follow the literature in measuring innovation quality by weighting each patent using the number of future citations that it received from subsequent patents (Trajtenberg, 1990). In addition to capturing economic value (Hall, Jaffe, and Trajtenberg, 2005), forward citations also reflect the technological importance of patents as perceived by the inventors themselves (Jaffe, Trajtenberg, and Fogarty, 2000) and experts (Albert, Avery, Narin, and McAllister, 1991).

We use cite counts adjusted for truncation from the NBER data set (Hall, Jaffe, and Trajtenberg, 2001, 2005) to deal with the issue of citation data suffering from truncation problems. In Table 5, we report the same specification as in Panel A of Table 2, but the dependent variables here measure the quality of innovation. In the

first three columns, we use $\ln(1 + (\text{truncation adjusted citations}))$ as our measure of innovation quality. Since the total number of citations is correlated with the number of patents, in the last three columns we look at an arguably stronger measure of innovation quality, namely, the number of citations per patent, $(\ln(1 + (\text{truncation adjusted citations})/\#\text{patents}))$. This measure reflects the quality of the average patent that the firm files following the tax changes. In terms of economic magnitude, our second measure, the number of truncation adjusted citations per patent, declines by 14.2% in the 2 years following a tax increase. Overall, these results show that the quality of innovation also declines following tax increases. This mirrors our earlier evidence on the number of patents.

In Panel B of Table 5, we repeat our tests but again focus on unpredictable tax changes and contiguous-county-level neighboring firms straddling policy-wise similar state borders, and show that our results remain similar.

Note that the effect on innovation quality most likely comes from the altered incentives for firms to take more risk, which we discuss in Section 5. Alternatively, the effect on citations might not be related to changed innovation quality but might result from geographic spillovers of innovation activity. For instance, similar firms might cluster in the same state and could be more likely to cite each

other's patents. If the innovative activity of the potential citing firms is also affected, we might see a reduction in the citations. Of course, such an explanation does not contradict our findings on the effect of state-level corporate taxes on innovation activity but rather suggests wider economic implications of corporate taxes.

In the rest of the analysis, we focus on the number of patents as our measure of innovation output. We do so because of a few reasons. First, we believe that in our context, patents might be a better way to gauge the impact on innovation since they are less susceptible to potential reclassifications between R&D and other investment for tax purposes because of R&D tax incentives. Second, we are interested in observing how the output of innovation is affected rather than just the investment into it. Finally, although we provide evidence on our newly constructed measure of new product announcements, we prefer to show our baseline results on a more widely accepted measure of innovation output.

Also, we focus on our large sample results in the following tests. While our analysis above on the effect of tax changes on innovation activity carefully accounts for local economic effects and the issue of predictability, such tightness of identification makes us sacrifice sample size. When we move to our cross-sectional tests below, we often do not have a sufficient number of patenting firms within the various sub-groups if we focus solely on firms in bordering counties across state borders.

4. Heterogeneity of the effect

4.1. Marginal tax rates

Naturally, firms differ in terms of their exposure to tax changes depending on their (past and current) earnings. For example, a firm that is unprofitable is exposed little, if at all, to any changes in the state's top corporate income tax rate.¹⁸ In this section, we measure a firm's exposure to tax changes using the marginal tax rate (MTR) (Blouin, Core, and Guay, 2010), measured in the year of the tax change. Since most of the state tax changes are implemented through changes in top tax rates and other measures that disproportionately affect top bracket tax payers, we should expect to see the strongest effects for firms that had high marginal rates in the tax change year.

Panel A of Table 6 presents these results. We use simulated MTRs from Blouin, Core, and Guay (2010) to partition sample firms into those with MTRs in the bottom 33 and top 67 percentiles, respectively, and present a specification where we look at the effect of tax changes separately for these two groups by estimating interactions between our tax changes and these MTR group dummies. The firms in the bottom percentiles have an average MTR of 7.7% and

are least exposed to tax changes. As expected, we do not see any change in their innovation outputs in the predicted direction following the tax change. All of our effect comes from firms in the top MTR percentiles (with an average MTR of 29.6%)—firms with high marginal tax rates indeed file a lower number of patents in response to tax increases. Interestingly, we also see evidence that when a state increases taxes, firms that are not exposed to such changes (in low MTR percentiles) can gain at the expense of high MTR firms, indicating that tax changes might have some within-state distributional effects in terms of patenting activity.

4.2. Tax sheltering

Firms also differ in their ability to shelter taxes. One way of sheltering taxes is to exploit the multi-state nature of firm operations and use differences across states in terms of tax shelters.¹⁹ For instance, companies can use a tax shelter that is frequently referred to in legal circles as a Delaware Trademark Holding Company, or a Passive Investment Company. Under this scheme, a corporation transfers ownership of its trademarks and patents to a subsidiary corporation located in a state such as Delaware or Nevada that does not tax royalties or other types of intangible income. Profits that would be taxable by the states in which a firm operates can be shifted out of such states for tax accounting purposes by the tax-haven subsidiary charging a royalty to the rest of the business for the use of the patent. The strategy works since the royalty is tax-deductible for the parent as well as other subsidiaries, and hence, directly reduces the amount of taxable profits in these states.²⁰

In order to test for the presence of such sophisticated tax strategies and their effects, we exploit a corporate tax provision, called “combined reporting,” that is designed to address a variety of such corporate income tax avoidance strategies. Combined reporting requires that the parent and its subsidiaries are treated as one corporation for state income tax purposes. Their nationwide profits are added together (“combined”). Each state then taxes a share of the combined income, where the share is calculated by a formula that takes into account the corporate group's level of activity in the state as compared to its activity in other states. In our sample period, 16 US states had combined reporting requirements in place. For example, California had a combined reporting system but Massachusetts did not.²¹

¹⁹ Another way of sheltering taxes is to exploit cross-country differences in tax rules. We find that our results remain similar even if we focus on purely domestic firms (Section 6, Table 10, row 10).

²⁰ Note that this cannot be a reason behind the asymmetry that we find. In fact, tax avoidance should lead to firms taking advantage of tax cuts by moving innovation activity into states that cut the tax, and to firms shifting such activity out of tax-increasing states into tax havens.

²¹ Mazerov (2009) contains more details on combined reporting practices across US states. No state adopted combined reporting in our sample period. Of course, the decision to locate and remain headquartered in a particular state is a firm's choice. But, according to Mazerov (2009), there is little evidence that companies move locations based on states having combined reporting requirements.

¹⁸ There indeed might be some effect even for currently unprofitable firms. Consider a young, unprofitable firm with growth options, in the form of investment projects, which can lead to patentable innovations. The decision to undertake such an investment is clearly going to be a function of future flows of net income from the patentable innovation it can produce. Even if the firm is currently unprofitable, the innovation might make it profitable enough in the future to care about tax rates.

Table 6

Heterogeneity of the effect.

This table provides the results from our baseline specification, i.e., Panel A of Table 2. In Panel A, we use simulated marginal tax rates (after interest expense) from Blouin, Core, and Guay (2010) in period t to partition sample firms into those with marginal tax rates in the bottom 33 percentile and top 67 percentile, respectively. We include $MTR_{i,t}$ as well to control for the level effect of marginal tax rate. In Panel B, we partition firms based on whether their state requires combined tax reporting. States that require combined reporting in our sample period are OR, MT, ID, CA, AZ, UT, CO, NE, KS, ND, MN, IL, NH, ME, AK, and HI. Combined _{i} is a dummy variable equal to one if the firm applying for the patent is located in a state with mandated combined reporting, else zero. Panel C reports the results where we estimate the exposure of firms to state-level tax changes by using detailed proprietary data collected by LexisNexis on the degree of operations parent firms and their subsidiaries have in each state. We use apportionment rules in the state to measure exposure to tax changes. All regressions include firm-level and state-level controls, and year fixed effects, not reported for brevity. Standard errors are clustered at state-level and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Panel A: Marginal tax rate			
	$\Delta \ln(1+Patents)_{t+k}$		
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)
Tax decrease _{s,t} \times MTR _{i,t} Bottom 33	0.004 (0.013)	0.012 (0.011)	−0.0003 (0.017)
Tax decrease _{s,t} \times MTR _{i,t} Top 67	−0.008 (0.014)	−0.008 (0.009)	0.005 (0.012)
Tax increase _{s,t} \times MTR _{i,t} Bottom 33	−0.013 (0.021)	0.013 (0.018)	0.064 (0.026)**
Tax increase _{s,t} \times MTR _{i,t} Top 67	−0.073 (0.016)***	−0.077 (0.029)***	−0.107 (0.057)*
Controls	YES	YES	YES
Obs.	41,859	36,997	32,286
Panel B: Tax sheltering			
	$\Delta \ln(1+Patents)_{t+k}$		
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)
Tax decrease _{s,t} \times Combined _{s}	−0.002 (0.023)	−0.002 (0.016)	0.004 (0.010)
Tax decrease _{s,t} \times Non-combined _{s}	−0.006 (0.014)	−0.0003 (0.011)	0.005 (0.013)
Tax increase _{s,t} \times Combined _{s}	−0.054 (0.025)**	−0.065 (0.022)***	−0.132 (0.042)***
Tax increase _{s,t} \times Non-combined _{s}	−0.056 (0.015)***	−0.044 (0.029)	−0.006 (0.018)
Controls	YES	YES	YES
Obs.	42,192	37,317	32,557
Panel C: Location of operations			
	$\Delta \ln(1+Patents)_{t+k}$		
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)
Exposure to tax decrease	0.0004 (0.022)	0.029 (0.023)	−0.007 (0.024)
Exposure to tax increase	−0.054 (0.041)	−0.110 (0.049)**	−0.063 (0.053)
Controls	YES	YES	YES
Obs.	13,455	11,435	9,549

We report the results in Table 6, Panel B. In particular, as with our MTR analysis, we interact our corporate tax change variables with two dummy variables indicating whether the state in question had combined reporting rules in place, or not. If tax avoidance is important in the data, then we should see our corporate tax changes having the most effect on firms located in states that had combined reporting rules in place.

Our results reveal an interesting pattern. In the first year after the tax increase, firms located in states with a

combined reporting requirement, as well as firms located in other states, experience a decline in patents. However, in the next 2 years, the reduction is significant only in firms that are located in combined reporting states. This pattern is consistent with a view that firms in states that do not require combined reporting shift out patenting activity after experiencing a tax increase in their home state, but this shift takes time.

4.3. Location of operations and apportionment rules

Finally, we look at the geographic distribution of firm operations across different states in terms of employees, sales, and assets, as recorded in LexisNexis' Corporate Affiliations database. In particular, we construct a measure of exposure to tax changes by looking at the proportion of firm activity that takes place within the borders of the state that experiences the tax change. To illustrate, consider two firms, A and B, both headquartered in New Hampshire. Firm A has 75% of its operations in New Hampshire, and 25% in Texas; firm B has 25% of its operations in New Hampshire and 75% in Texas. So, when New Hampshire increased its corporate tax rate in 1999, firm A should have been affected more than firm B. In defining how much activity the firm generated in a particular state, the states use a weighted average of sales, property, and payroll activity. The weights—called apportionment weights—used in these formulae differ across states and have also changed over time. While traditionally a lot of states apportioned firms' profits based on the equally weighted average across three activity groups, recently states have been increasing the weight on sales, which are argued to be less dependent on the firm's decision on where to locate its production and employees (Merriman, 2015).

We gather the apportionment formulae for each state from Merriman (2015) and apply them to our firm operations data, where we proxy firm's payroll expenditure across states by the distribution of its employees across different subsidiaries, and proxy property weights by the distribution of its assets across different subsidiaries.²²

We report results using this approach in Table 6, Panel C. Even when we take into account state laws regarding different apportionment formulae across states and firm-level subsidiary structure, we find consistent results. In terms of economic magnitudes, our evidence shows that a treated firm, with 100% in-state exposure, files for one fewer patent (compared to its sample mean of 9.11) in the 2 years following an increase in taxes in its state. Again, we do not find any significant effect following tax cuts.

5. Discussion on theory and related tests

5.1. Innovator incentives

In the previous sections, we presented empirical evidence that tax increases lead to lower innovation activity. In this section we return to the reasons behind the tax effect we document. In particular, we discuss and test specific predictions from existing theory models.

5.1.1. Taxes in a general equilibrium model of innovator incentives

We first present a key prediction from a version of Romer's (1990) endogenous growth model with taxes and

occupational choice, applied specifically to innovation following the structure in Jaimovich and Rebelo (2015). In Jaimovich and Rebelo (2015), innovation occurs in the sector producing intermediate inputs for the final consumption good. Innovations expand the set of available intermediate inputs and successful innovators are assumed to have perpetual patents on the newly generated intermediate goods from their innovation. We summarize relevant sections of their model structure in Internet Appendix A.3.1. For our purpose, the main prediction (Proposition 1 in Internet Appendix A.3.1) from this theory is that taxes affect agents' decision regarding whether to work as innovators or regular workers—the threshold ability level above which an agent chooses to innovate increases with the corporate tax rate—so, corporate taxes affect innovation adversely.²³

The main testable prediction in this setting is that a rise in corporate taxes will reduce future innovation, like we find in the data. For the specific channel proposed here, the relevant testable implication from the theory is that employees in innovative firms leave after taxes are raised on the firm's profits (extensive margin), or they switch to less innovative activities within the same firm (intensive margin).

Note that employee departures from the firm are not necessarily caused by the firm firing existing workers after the rise in taxes, but can result from voluntary departures of employees who realize that their best ideas might receive better nurture and yield higher post-tax compensation elsewhere. For instance, firms could (and, as we show, do) reduce R&D spending following tax increases. Productive innovators might be affected by these cuts in R&D spending in terms of project funding, or they might realize that the prospect of increasing R&D and the potential upside in remuneration for their future innovations is more limited now. Realizing this, innovators might either have fewer incentives to innovate within the existing firm, or they might even leave the firm and take their new ideas to another employer who is more likely to encourage innovation.

5.1.2. A test of the innovator incentives hypothesis

In order to test the prediction above on the extensive margin, i.e., whether innovators currently employed by a firm leave following tax rises affecting it, we exploit individual inventor data from the Harvard Business School

²² If a firm has operations in several states, all of which experience a tax increase in the same year, then we calculate the total exposure of the firm to tax increases in that year by adding the proportion of activities the firm had in all affected states.

²³ In particular, this model demonstrates a key source of confusion that can affect studies like ours. In partial equilibrium set-ups, the price or quantity of the innovation good produced may not be directly affected by corporate taxes. That is, while tax costs increase on the revenue, offsetting tax benefits are obtained on the cost of the project, because of tax deductibility of investments in innovation. However, as we demonstrate in a general equilibrium setting, this does not imply that taxes do not matter for innovation. Even if price and quantity do not change in partial equilibrium, the size of after-tax profit declines, which reduces the pie available to innovators and thus reduces the incentive to innovate overall. Such an effect elicits a general equilibrium response in terms of occupational choice and makes some innovators switch to less innovative tasks (become regular workers), affecting aggregate innovation in response to tax changes.

Table 7

Innovator turnover.

This table examines the effect of taxes on inventor turnover. Net leavers is measured as a difference between Leavers and Hires. Hires refers to the number of inventors who produce at least one patent at the sample firm after producing a patent at a different firm within 1 year and a year after, including inventors who file their first patent with the sample firm. Leavers refer to the number of inventors who have produced a patent at the sample firm within one past year or a year before but produce at least one patent at a different firm, including inventors who produce their last patent in the sample firm. We use $\text{Ln}(1 + \text{Net leavers})_{i,t+k}$ as dependent variable for $k = 1-5$ and run the following regression.

$$\text{Ln}(1 + \text{Net leavers})_{i,s,t+k} = \beta_D \text{Tax decrease}_{st} + \beta_I \text{Tax increase}_{st} + \delta \Delta X_{it} + \alpha_t + \epsilon_{i,s,t+k},$$

where i, s, t index firms, states, years and $k = 1$ to 5; Tax decrease_{st} and Tax increase_{st} are indicators equaling one if state s decreased and increased its corporate tax rate in year t , respectively; X_{it} are firm-level factors that affect inventors. All regressions are with year fixed effects. Standard errors are clustered at state-level and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)	$k = 4$ (4)	$k = 5$ (5)
Tax decrease _{s, t}	−0.013 (0.011)	0.0009 (0.012)	−0.008 (0.013)	−0.018 (0.013)	−0.021 (0.013)*
Tax increase _{s, t}	0.048 (0.023)**	0.061 (0.034)*	0.035 (0.037)	0.014 (0.039)	−0.037 (0.026)
Controls	YES	YES	YES	YES	YES
Obs.	41,557	36,740	32,040	27,695	23,721

Patent Dataverse which contains information on both inventors (i.e., those individuals who produce the patent) and assignees (i.e., entities such as firms, individuals, or even governments that own the patents). We can thus track the mobility of inventors across different assignees.

We estimate “Hires” as the number of inventors who produce at least one patent at a new assignee firm in our sample within 1 year after producing a patent at a different assignee. Simultaneously, we estimate “Leavers” as the number of inventors who stop producing patents at a sample firm where they have previously produced a patent. Next, we calculate Net leavers as the difference between Leavers and Hires.

We then examine how many innovator departures follow tax increases. In Table 7, we show that in 2 years following the tax increase, a significant number of inventors leave their firms and are not replaced in their organizations, as evidenced by our positive sign on the Net leavers variable. On the other hand, tax decreases do not lead to net new hires immediately—on average it takes about 5 years for the effect of the tax cut to show up in our data.²⁴

Next, we examine whether employees or innovators who remain in their existing firms also suffer from a decline in innovation productivity following tax increases. To test for such effects, we first repeat our baseline specification using a measure of innovative productivity of employees. We follow Acharya, Baghai, and Subramanian (2014) and estimate the log of the number of patents per 1,000 firm employees, as reported in Compustat. In addition,

we address concerns that all employees at a firm may not be involved in the innovation process, which is typically carried out by the R&D department, and scale the number of patents by the number of innovators who applied for a patent at the firm in the current year, and have not yet filed any patent for a different firm (the complement of our Leavers measure, calculated based on the existing firm employees at the start of the year). Both dependent variables provide a measure of employee innovative effort inside the firm.

Our results in Table 8 reveal a pattern similar to those in previous tables—tax increases are followed by a decline in average innovative productivity of firm employees, while we are unable to uncover any increases in productivity following tax cuts. In terms of economic magnitudes, the number of patents filed per innovator declines by about 2.5 percentage points following the average tax increase in our sample.

This evidence is broadly consistent with model predictions on innovator incentives.

5.2. Uncertain nature of innovation investments

Innovation investments are highly uncertain and often irreversible, making returns to innovation risky. This uncertain nature of innovation can make it particularly susceptible to changes in taxes that disproportionately penalize high project payoffs (successful innovations), i.e., taxes that accentuate the progressivity of the tax code. We present a stylized model in Internet Appendix A.3.2 to illustrate this possibility. Here we summarize a test of a prediction arising from this theoretical framework.

5.2.1. Convex tax schedules

If the rewards to innovation are more variable than the rewards to safe investments, an increase in the convexity of the tax schedule can discourage innovative activity by raising the average tax burden on risky innovation (Gentry and Hubbard, 2000).²⁵ This happens because increasing the top tax rate reduces the rewards from extremely successful outcomes, while failures are untaxed; so investment in projects with particularly uncertain payoffs should decline if the top tax rate is changed by more than the rates in the other brackets (Proposition 2 in the theory model outlined in Internet Appendix A.3.2).

5.2.2. A test of the innovation riskiness hypothesis

In the absence of any direct measure of ex ante innovation risk, we test the above hypothesis by examining the ex post riskiness of projects. If a firm chooses to forgo the more risky innovation projects, then, on average, it would end up with fewer projects that are highly valuable and also fewer projects that are not valuable at all. We use two tests, both based on this idea.

²⁵ The tax schedule can become more convex, for example, with a top bracket tax change under a progressive rate or an increase in surcharges which affect the tax bill of high tax firms disproportionately more. These two types of changes are the most common ways through which US states alter corporate taxes.

²⁴ Net hires = − Net leavers, in the way we define our variable.

Table 8

Innovator productivity.

This table studies the effect of tax changes on the productivity of employees/inventors and provides results for the following regression:

$$\Delta \ln \left(1 + \frac{\# \text{Patents}}{\# \text{Employees}} \text{ or } \frac{\# \text{Patents}}{\# \text{Inventors}} \right)_{i,s,t+k} = \beta_D \text{Tax decrease}_{st} + \beta_I \text{Tax increase}_{st} + \delta \Delta X_{it} + \alpha_t + \epsilon_{i,s,t+k},$$

where $i, s, t+k$ index firms, states, years with $k = 1$ to 3; $\ln(1 + \frac{\# \text{Patents}}{\# \text{Employees}} \text{ or } \frac{\# \text{Patents}}{\# \text{Inventors}})_{i,s,t+k}$ measures productivity of employees or inventors of firm i in states in financial year t . Tax decrease_{st} and Tax increase_{st} are indicators equaling one if state s decreased or increased its corporate tax rate in year t ; X_{it} are firm-level factors that can affect innovation. Number of inventors excludes innovators who have produced a patent at the sample firm within one past year but produce at least one patent at a different firm. All regressions include firm-level and state-level controls and year fixed effects, not reported for brevity. Standard errors are clustered at state-level and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	$\Delta \ln(1 + \frac{\# \text{Patents}}{\# \text{Employees}})_{t+k}$			$\Delta \ln(1 + \frac{\# \text{Patents}}{\# \text{Inventors}})_{t+k}$		
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)	$k = 1$ (4)	$k = 2$ (5)	$k = 3$ (6)
Tax decrease _{s, t}	0.002 (0.018)	0.031 (0.020)	−0.014 (0.013)	0.004 (0.008)	−0.007 (0.005)	0.0003 (0.004)
Tax increase _{s, t}	−0.037 (0.027)	−0.059 (0.031)*	−0.108 (0.052)**	−0.006 (0.009)	−0.020 (0.010)**	−0.024 (0.009)***
Controls	YES	YES	YES	YES	YES	YES
Obs.	42,184	37,301	32,535	42,146	37,274	32,518

That is, we examine patenting risk by testing how taxes affect the volatility of patent citations. Using citations to proxy for the value of an innovation will lead to the prediction that the cross-section of projects patented by a firm after the tax increase will be less dispersed in terms of future citations that they receive following patenting. To this end, following Amore, Schneider, and Žaldokas (2013), we analyze the distribution of citations to patents granted to a firm before and after the tax change in Table 9, Panel A. We find that, consistent with the theoretical prediction, standard deviation of citations decreases by approximately 13.6% in the 3 years after a tax increase. However, we do not find a similar increase in citation standard deviation following tax cuts.

In Panel B, we examine another way of measuring whether an affected firm ends up with fewer projects that are highly valuable as well as fewer projects that are not valuable at all. Here, we use “Highly cited patents” (top 10% most cited patents among all patents filed by all firms in a year) as our measure of valuable innovations, and patents with zero citations in next 5 years (“Zero-cite patents”) as our measure of innovations that turn out to be less valuable, ex post. Again, as predicted, we see a simultaneous decline in both types of patents after tax increases.

Also, of particular note is that our results are less asymmetric here: the numbers of both the least and most valuable patents respond also to tax cuts—in the direction predicted by theory—although only the coefficient on Highly cited patents is statistically significant.

Overall, we also find support for a prediction of the innovation risk theory in the data.

6. Robustness and further analysis

6.1. Robustness of main results

In this section we consider various refinements of, and address potential concerns with, our baseline large-sample evidence. We report the results in Table 10.

First, our specification does not distinguish between large changes affecting the effective top marginal tax rates, and other changes that affect the tax bill, for example, reduction in federal income tax deductibility. In row 1 of Table 10, we separately consider large tax changes (more than 1% increases and decreases) and the other tax changes. Here we re-estimate our baseline regression, replacing the tax increase and decrease dummies with four variables—large tax increases (average increase 2.95%), other tax increases (covers all other changes to the tax bill, including those hard to quantify directly), large tax decreases (average decrease −2.11%), and other tax decreases. Our results show that future innovation is affected mostly by large tax increases.

Second, one might be concerned that some affected states have a disproportionately large number of firms in a certain industry that is more sensitive to tax changes than other states. We mitigate this concern by incorporating industry-year fixed effects in a robustness test, reported in row 2, so that in these specifications we are essentially comparing firms within the same industry but located in different states. Results remain virtually unchanged. In row 3, we exclude firms that come from California and Massachusetts, home to many innovative industries, and find that our result remains significant. That is, our results are not exclusively driven by tax changes in these two states.

Next, one might be concerned that the effect of taxes on innovation might have been specific to a certain period. We obtain separate estimates of tax change effects in the 1990–1999 and the 2000–2006 periods, and find the presence of tax effects on innovation in both periods (row 4). Interestingly, here we also see a statistically significant increase in patenting following tax cuts in the more recent subperiod.

The next concern we address is whether our results are affected by a lot of innovative firms getting acquired. For instance, it could be the case that tax increases were followed by some innovative firms being acquired because, say, they were so far unprofitable, and thus could reduce

Table 9

Innovation risk.

This table studies the effect of tax changes on the riskiness of innovation projects. We estimate our baseline specification for different measures of riskiness of innovation projects and run the following regression:

$$\Delta \ln(1 + \sigma(\text{Citations})) \text{ or } \Delta \ln(\# \text{Zero} - \text{cite or Highly cited patents})_{i,s,t+k} = \beta_D \text{Tax decrease}_{st} + \beta_I \text{Tax increase}_{st} + \delta \Delta X_{it} + \alpha_t + \epsilon_{i,s,t+k}.$$

Panel A reports the results with $\ln(1 + \sigma(\text{Citations}))$ as a measure of riskiness of innovation projects. We only keep tax changes in top brackets and surcharges, which are progressive in nature and do not consider non-progressive tax changes like NOL suspensions. In Panel B we repeat our baseline with $\Delta \ln(1 + \# \text{Zero-cite patents})$ and $\Delta \ln(1 + \# \text{Highly cited patents})$ as dependent variables. All regressions include firm-level and state-level controls and year fixed effects, not reported for brevity. Standard errors are clustered at state-level and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Panel A: Standard deviation of citations			
	$\Delta \ln(1 + \sigma(\text{Citations}))_{t+k}$		
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)
Top tax decrease _{s, t}	0.025 (0.023)	0.010 (0.022)	0.006 (0.038)
Top tax increase _{s, t}	−0.045 (0.064)	−0.136 (0.054)**	0.069 (0.076)
Controls	YES	YES	YES
Obs.	8,252	7,362	6,501

Panel B: Zero-cite patents and Highly cited patents						
	$\Delta \ln(1 + \# \text{Zero-cite Patents}))_{t+k}$			$\Delta \ln(1 + \# \text{Highly Cited Patents}))_{t+k}$		
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)	$k = 1$ (4)	$k = 2$ (5)	$k = 3$ (6)
Top tax decrease _{s, t}	−0.022 (0.022)	−0.018 (0.016)	0.028 (0.025)	−0.012 (0.012)	0.012 (0.013)	0.041 (0.017)**
Top tax increase _{s, t}	−0.072 (0.030)**	0.023 (0.018)	0.002 (0.038)	−0.045 (0.026)*	−0.001 (0.040)	0.005 (0.036)
Controls	YES	YES	YES	YES	YES	YES
Obs.	11,643	10,323	9,067	11,643	10,323	9,067

the tax bill of the acquirer. To address this issue, we use a sample of firms that survive until the end of our sample. Results presented in row 5 show the same pattern as our main results.

In row 6, we look at tax increases that were reversed later. Firm-years with tax changes that did not get reversed are dropped from these tests to ensure the validity of our control sample. We still find that innovation declines following a tax increase. In row 7, we look at treatment effects for states with no reversals. In this test, firm-years with reversals are dropped from the sample. Again, we find a decline in the number of patents following tax increases, but now we also find an increase in the number of patents following tax cuts. This time the effect following tax cuts is statistically significant and economically non-negligible, although, again, much smaller than the effect following tax rises—28% of firms facing such a tax cut file for one more patent successfully in the third year after the cut.²⁶ Note that such analyses (rows 6 and 7), while interesting, might have a look-ahead bias. In most cases, firms would not have known with certainty at the time of the tax change whether it would be reversed later or not. Ex ante, it is difficult to predict whether a tax change will last—even changes declared to be temporary can keep get-

ting renewed and eventually last extended periods of time, e.g., Connecticut has had a “temporary” tax surcharge for over a decade.

Row 8 reports that our results are also robust to the inclusion of additional state-level macro variables such as the change in state budget surplus and debt outstanding. In row 9, we add firm fixed effects to our specification, and find very similar results. Note that this particular result does not have the same interpretation as the others: since our dependent variable is measured as a change, adding fixed effects shows the effect of taxes on the *growth rate* of innovation rather than its level.

In the next row 10, we examine purely domestic firms. Focusing on them alleviates concerns regarding sophisticated tax avoidance strategies adopted by firms exploiting various international treaties and tax-haven countries (Graham and Tucker, 2006), as well as concerns regarding effective tax rates facing firms being different depending on the foreign country rates where it operates (Faulkender and Smith, 2016). We define domestic firms as those that do not have any foreign income (using those that do not pay foreign taxes produces similar results). Our effects are marginally stronger in this sample.

In row 11 we drop all controls, to allay concerns that some of our control variables are choice variables for individual firms, and again find a similar pattern.

²⁶ Note, 0.031, the coefficient in Table 10, row 7, column 7, times the sample average number of patents, 9.11 = 0.28.

Table 10

Robustness checks.

This table provides further robustness checks to the specification in Panel A of Table 2. All regressions include firm-level and state-level controls and year fixed effects, not reported for brevity. Row 1 reports the results for a regression where we allow for our coefficients to vary by tax changes greater than 1% and other tax changes. Row 2 reports results with SIC4 industry-year fixed effects, instead of year fixed effects. In row 3, we exclude firms located in California and Massachusetts from the sample. Row 4 reports the results for a regression where we allow for our coefficients to vary by sample periods, 1990 to 1999 and 2000 to 2006. Row 5 reports the results for firms that survive to 2006. Row 6 reports the treatment effects for firms located in states which first experience a tax increase and then a tax cut (reversals). The firm-years with tax changes that did not get reversed are dropped from the sample. Row 7 reports the treatment effects for states with no reversals. The firm-years with reversals are dropped from the sample. In row 8, we include a list of additional state-level macro variables, i.e., the change in state budget surplus and debt outstanding. Row 9 reports the results when we include firm fixed effects. In row 10, we report results for domestic firms (i.e., Pretax Income-Foreign(*ptis_f*)=0). Row 11 reports the results without any controls. Row 12 reports the results for tests where we separate the effective date of change and the announcement year of the expected change. For the cases when states announce tax changes long before the effective date of the change, we treat the announcement year as the year of (expected) change, and use it instead of the effective year in the analysis. Standard errors are clustered at state-level and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	$\Delta \text{Ln}(1+\#\text{Patents})_{t+1}$			$\Delta \text{Ln}(1+\#\text{Patents})_{t+2}$			$\Delta \text{Ln}(1+\#\text{Patents})_{t+3}$		
	Tax decrease	Tax increase	Obs.	Tax decrease	Tax increase	Obs.	Tax decrease	Tax increase	Obs.
(1) $ \Delta \text{Tax} \geq 1\%$	−0.017 (0.028)	−0.036 (0.018)***	42,192	−0.005 (0.031)	−0.077 (0.024)***	37,317	−0.007 (0.023)	−0.010 (0.019)	32,557
Other tax changes	0.007 (0.011)	−0.018 (0.027)		−0.011 (0.019)	0.0003 (0.037)		−0.018 (0.022)	−0.007 (0.037)	
(2) Industry-year FEs	−0.008 (0.012)	−0.032 (0.012)***	42,192	0.004 (0.008)	−0.030 (0.016)*	37,317	−0.0007 (0.009)	−0.042 (0.025)*	32,557
(3) Excluding CA and MA	−0.012 (0.011)	−0.050 (0.017)***	31,492	−0.0003 (0.008)	−0.038 (0.029)	27,971	−0.006 (0.009)	0.005 (0.018)	24,525
(4) Regime I: 1990 to 1999	−0.006 (0.012)	−0.049 (0.016)***	42,192	−0.011 (0.008)	−0.048 (0.030)	37,317	−0.005 (0.008)	0.003 (0.029)	32,557
Regime II: 2000 to 2006	0.002 (0.018)	−0.058 (0.018)***		0.034 (0.024)	−0.056 (0.024)**		0.042 (0.024)*	−0.099 (0.042)**	
(5) Surviving firms	−0.001 (0.018)	−0.068 (0.025)***	15,457	−0.013 (0.014)	−0.089 (0.035)**	14,478	0.011 (0.013)	−0.070 (0.050)	13,498
(6) Reversals	−0.027 (0.018)	−0.043 (0.023)*	38,147	−0.014 (0.024)	−0.084 (0.024)***	33,647	−0.011 (0.017)	0.015 (0.035)	29,174
(7) Non-reversals	−0.0003 (0.016)	−0.058 (0.017)**	40,447	−0.009 (0.010)	−0.039 (0.020)**	35,711	0.031 (0.009)***	−0.091 (0.036)**	31,089
(8) Additional macro controls	0.003 (0.014)	−0.024 (0.016)***	42,192	0.002 (0.008)	−0.035 (0.023)	37,317	0.009 (0.010)	−0.092 (0.039)*	32,557
(9) With firm FEs	−0.001 (0.014)	−0.048 (0.012)***	42,192	0.0004 (0.009)	−0.043 (0.018)***	37,317	0.011 (0.010)	−0.058 (0.035)*	32,557
(10) Domestic firms	−0.002 (0.013)	−0.064 (0.017)***	32,668	−0.009 (0.009)	−0.054 (0.019)***	29,406	−0.0005 (0.011)	−0.065 (0.040)	26,139
(11) Without controls	−0.004 (0.010)	−0.059 (0.012)***	48,057	−0.002 (0.008)	−0.050 (0.019)***	42,764	0.0005 (0.018)	−0.064 (0.038)*	37,572
(12) Announcements of tax changes	−0.003 (0.016)	−0.053 (0.016)***	41,375	0.004 (0.009)	−0.049 (0.021)**	36,560	0.002 (0.012)	−0.060 (0.037)	31,924

Finally, in row 12, we separate the effective date of change and the announcement year of the expected change. In particular, for each year in our sample period, we read through the Fiscal Survey of States as well as the Book of States (sub-chapter on Trends in State Taxation of the chapter on State Finances). We find that there are instances in which states announce tax changes sufficiently before the effective date of the change. For these cases, we treat the announcement year as the year of (expected) change, and drop the effective year from the analysis. This change affects one out of 31 tax increases, and 11 out of 56 tax cuts in our sample. However, it turns out that our results are consistent, in particular, we still do not find any significant effects of tax decreases on future innovation.

In additional results (reported in Table A.9 of the Internet Appendix), we find that our results are also robust if we use Compustat headquarter information to identify a firm's state, if we use the state name counts in 10-K forms (Garcia and Norli, 2012), or if we use the state where the highest proportion of firm's employees are located. Finally, while firms may strategically change their state of

operation to avoid tax increases, our results also hold for the firms that do not change their states during our entire sample period.

Overall, we find that our basic result on tax increases is robust. In some specifications tax cuts also have an effect, but this effect is smaller when present, and not robust.

6.2. Accounting for coincidental changes in other state taxes

Our identification relies on *staggered* changes in corporate taxes. So, incorrect interpretation of causality would require that changes in some omitted variables were coincident in a similar staggered fashion. In particular, state legislatures can change multiple laws at the same time. The most likely candidate for such an omitted variable is some coincident change in a different fiscal policy instrument, which might also affect innovation, e.g., R&D tax credit, tax on wages, or capital gains taxes.²⁷

²⁷ For instance, Galasso, Schankerman, and Serrano (2013) show that capital gains taxes affect trading in patents.

Table 11

Potential confounding factors.

Panel A provides changes in other tax rates that coincide with the corporate tax changes. Panel B provides the results from our baseline specification, i.e., Panel A of Table 2, after controlling for potential confounding factors. We control for region-year fixed effects, state fixed effects, and various coincidental tax changes, such as decreases and increases in R&D tax credit and personal income tax rate (either tax rate on wages, or tax rate on long capital gains) in state s at time t , respectively. All regressions include firm-level and state-level controls and year fixed effects, not reported for brevity. Standard errors are clustered at state-level and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Panel A: Coincidence of tax changes			
		Tax increases	Tax decreases
Number of corporate tax changes ..which coincide with		32	51
	increase in state R&D tax credit rate	1	8
	decrease in state R&D tax credit rate	0	1
	increase in state wage tax rate	14	6
	decrease in state wage tax rate	2	13
	increase in state capital gains tax rate	13	5
	decrease in state capital gains tax rate	2	14

Panel B: Controlling for coincident tax changes and regional trends			
	$\Delta \ln(1+\#Patents)_{t+k}$		
	$k = 1$ (1)	$k = 2$ (2)	$k = 3$ (3)
Tax decrease _{s, t}	−0.006 (0.012)	0.005 (0.008)	0.011 (0.009)
Tax increase _{s, t}	−0.047 (0.014)***	−0.035 (0.021)*	−0.030 (0.023)
R&D tax credit decrease _{s, t}	−0.070 (0.039)*	−0.039 (0.051)	0.009 (0.037)
R&D tax credit increase _{s, t}	−0.004 (0.019)	0.016 (0.012)	−0.006 (0.013)
Personal income tax increase _{s, t}	0.008 (0.010)	0.019 (0.013)	−0.001 (0.012)
Personal income tax decrease _{s, t}	0.0009 (0.009)	−0.0009 (0.009)	−0.019 (0.011)
Controls	YES	YES	YES
Obs.	42,192	37,317	32,557

Our analysis in Panel A of Table 11 shows that there is little tendency for states to change R&D tax credits, top tax rates on wage income, and capital gains tax rates on long gains at the same time as the corporate income tax rates. For example, only eight out of our 51 instances of state corporate income tax cuts coincided with an increase in the R&D tax credit.

We explicitly account for potential confounding tax changes in Panel B of Table 11. To account for unobserved, time-invariant, state-level heterogeneity, we incorporate state fixed effects. We also control for the possibility of differential regional trends in innovation by incorporating region-year fixed effects in our specification. Finally, we explicitly account for changes in the R&D tax credit, and changes in personal income taxes (measured by a change in either tax on wages or capital gains taxes) in the firm's state by using increase and decrease dummies similar to our corporate tax change variables. The evidence presented in the table shows that the significance of corporate tax change variables remains virtually unaltered when we account for these potential confounds.

6.3. Other mechanisms and more on asymmetry

In Section 5 we discussed two hypotheses consistent with our overall empirical results, and presented tests specific to their predictions. A different potential explanation for the response of firm innovation to tax changes operates through changes in firm financing structure, arising from firms' keenness to exploit debt shields against taxes. For instance, Heider and Ljungqvist (2015) show that firms respond to tax increases by increasing leverage. Higher leverage allows them to reap greater tax benefits but might come at the price of lower future innovation, since debt-holders might not like funding risky innovation projects (in which they do not share the upside if the project is successful, but bear the costs of failure). Moreover, as Heider and Ljungqvist (2015) show, firms' debt response to tax changes is also asymmetric—firms respond significantly to tax increases but not to tax cuts. So, the debt channel can also be an explanation for the asymmetry we document.

In Table 12, we divide firms based on whether they actually changed their leverage following the tax change. We find that while the drop in future patenting is present both among the firms that increased their leverage following a tax increase, as well as those that did not, our results are

Table 12

Firm leverage.

This table provides evidence on how a firm's actual changes in leverage following tax changes impact future innovation. This table provides the results of the effect of tax changes on innovation activity for the firms that changed leverage, and did not change leverage following the tax change. $\Delta BL_{i,t+1} > (<) 0$ is a dummy variable equal to one if a firm increases (decreases) book leverage (BL) in period $t+1$, else zero. We include $\Delta BL_{i,t+1}$ as well to control for the level effect of leverage. All regressions include controls and year fixed effects, not reported for brevity. Standard errors are clustered at state-level and reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	$\Delta \ln(1+\#Patents)_{t+k}$		
	$k = 2$ (1)	$k = 3$ (2)	$k = 4$ (3)
Tax decrease _{s,t} $\times \Delta BL_{i,t+1} < 0$	0.006 (0.011)	0.004 (0.013)	0.007 (0.017)
Tax decrease _{s,t} $\times \Delta BL_{i,t+1} \geq 0$	−0.007 (0.016)	0.0003 (0.010)	0.003 (0.021)
Tax increase _{s,t} $\times \Delta BL_{i,t+1} > 0$	−0.076 (0.031)**	−0.074 (0.038)*	−0.048 (0.062)
Tax increase _{s,t} $\times \Delta BL_{i,t+1} \leq 0$	−0.047 (0.025)*	−0.072 (0.046)	−0.041 (0.045)
Controls	YES	YES	YES
Obs.	34,039	29,905	26,003

indeed stronger for firms that altered leverage in the predicted direction following the tax increase. This result indicates that changing leverage may also be responsible for our documented effects. However, they cannot be the only reason behind changes in innovation, since firms that do not alter leverage in the tax-motivated direction also show an effect on patenting.

One other plausible channel through which taxes could affect innovation is through their impact on internal cash flows. Since internal cash flows have been shown to be a major source of financing for innovation (Himmelberg and Petersen, 1994), a tax-induced reduction in such financing might reduce innovation, particularly for those firms that are constrained in their ability to obtain outside financing. We explore this possibility using a variety of measures of financial constraints but do not find any supporting evidence.

There are two reasons why this may not be surprising. First, tax changes affect large, profitable firms disproportionately more. This is because these are the firms that face high effective tax rates, and most of the tax changes in our sample affect high tax-paying firms disproportionately more. However, at the same time these firms are less likely to be financially constrained—large profitable firms are typically *not* constrained in their ability to find outside financing. Second, it could also be that empirical proxies of financial constraints are noisy, especially in the context of our state tax changes. Specific to our experiment, Farre-Mensa and Ljungqvist (2016) show that firms classified as constrained according to popular financial constraint measures are in fact able to obtain financing following tax increases. Such noise might be reducing the power of our tests.

We conclude with a few more thoughts on the asymmetry of our results.

First, while the general equilibrium model in Section 5 does not specifically account for labor mar-

ket frictions, the latter can lead to asymmetric adjustment costs and can thus possibly explain the asymmetry in our results. As our results in Table 7 show, firms are quick to lose their innovative personnel following tax increases, but they need more time to build the knowledge, workforce, and capacity to innovate following tax cuts. New innovators might need to be hired, and, given labor market frictions, this might take time. Current employees might need to acquire more skills or learn how to be more innovative, and this might also be time-consuming. Moreover, it might be easier for innovators to cut back on effort if they are discouraged by a change in firm policy or compensation opportunities following a tax increase, than it is to find great new ideas to increase innovation if the firm experiences a similarly sized tax cut. In other words, getting great new ideas might be exogenous to firm employees, but labor market mobility makes it an endogenous outcome as to which firm will be able to make use of them.

Second, an alternative explanation could be that the relation between innovation inputs (R&D) and outputs (patent-based measures) is concave. If so, any reduction in R&D due to a tax increase is likely to produce a larger effect on patents than a similarly sized increase in R&D (due to a tax cut). However, our R&D results presented in Table 3 show that the R&D response to innovation is itself asymmetric, so this is unlikely to be the driving force.

One might also hypothesize that tax decreases are different in nature from tax increases. For instance, the average tax increase is larger than the average tax cut. However, even when we condition on large changes in Table 10, row 1, we still cannot identify significant effects following tax cuts, making this an unlikely explanation. Second, from an ex ante standpoint, firms might perceive tax decreases as more temporary, since states might need to reverse them in the future in order to balance their budgets. In addition, firms may in fact even be prone to lobbying for tax decreases, thus, diminishing the exogeneity of these types of changes. Unfortunately, we do not have direct evidence to understand whether this is affecting our results.²⁸

Finally, across our many different specifications and tests, we consistently find evidence that tax increases reduce future innovation, while decreases do not have any robust effect. On the other hand, as mentioned in the introduction, Atanassov and Liu (2014) find that tax cuts affect innovation more than tax increases. We find that our different findings are caused by two methodological differences: First, we specify our regression model in first differences, following the previous literature (Heider and Ljungqvist, 2015), while Atanassov and Liu (2014) estimate their model in levels. As explained in Section 3, the advantage of the first difference setting is that since the tax dummy variable is measured as a change, we do

²⁸ Also, in the set-up we examine, the variation in tax rates is bounded within a relatively narrow range (up or down by a few percentage points at most). While this set-up buys us the opportunity to cleanly identify our effects, one has to exercise caution in extending our findings to other countries or contexts. Dramatic tax cuts, for example, might indeed have an effect on future innovation: we cannot necessarily predict from our analysis how firms will behave when faced with such changes.

not have to (1) reset the variable every time a state reverses a tax change, even if the reversal is partial, and (2) leave out consecutive tax increases or decreases in the same state. Second, the list of tax changes we consider is more comprehensive: [Atanassov and Liu \(2014\)](#) do not consider many of the changes in tax codes—even large changes—that we (as well as [Heider and Ljungqvist, 2015](#)) do. In terms of changes in top tax rates, [Atanassov and Liu \(2014\)](#) leave out Missouri's increase in top corporate tax rate from 5% to 6.25% in 1993, Connecticut's cut from 8.5% to 7.5% in 2000, and Arizona's cut by 1% in 2001. Moreover, they do not consider large changes which have a more than 1% impact on effective top marginal tax rates but are implemented through changes in surcharges (e.g., Connecticut's imposition of a 20% surcharge on the tax bill in 2003, or its repeal of a 10% tax surcharge in 1992). When we focus on larger tax changes using our list, we still un-

cover an empirical pattern similar to the rest of our tests (see, for example, [Table 10](#), row 1).

7. Conclusion

Public debates on fiscal policy often involve arguments that corporate taxes discourage innovation, as they reduce incentives to put in effort and take risks. In this paper, we use staggered changes in state corporate tax rates in the US to examine the importance of tax policy on future innovation by firms. We find evidence that firms respond to tax increases by filing a lower number of patents, investing less in R&D, and bringing fewer new products into the market, which, taken together, suggests that higher corporate taxes indeed reduce innovator incentives and discourage risk-taking. We find weaker results on increasing innovation in response to tax cuts.

Appendix. Description of variables

Variable name	Description
#Patents	Total number of patents applied for by firm <i>i</i> in financial year <i>t</i>
#Citations	Total citations received on patents applied for adjusted for truncation [as described in Hall, Jaffe, and Trajtenberg (2001, 2005)]
Ln(Sales)	Natural logarithm of total sales at 2000 dollars
Ln(K/L)	Natural logarithm of capital-to-labor ratio, where capital is represented by net property, plants, and equipment (PPE), and labor is the number of employees
HHI	Herfindahl–Hirschman Index computed as the sum of squared market shares of all firms based on sales in a given three-digit SIC industry in each year
Profitability	Ratio of earnings before interest and taxes (<i>oibdp</i>) to sales (<i>sale</i>)
Tangibility	Ratio of net plant, property, and equipment (<i>ppent</i>) to book assets (<i>at</i>)
Book leverage	Debt over debt plus equity
Debt rating	Dummy variable for firm-years rated by Standard & Poor's (S&P)
R&D/Sales	Ratio of expense on research and development (<i>xrd</i>) to sales (<i>sale</i>)
Tax increase/decrease	Dummy variable equal to one in the year of corporate tax increase/deduction for the firms headquartered in state <i>s</i> , else zero
Log(Real GSP)	Natural logarithm of real Gross State Product
Taxes (% of GSP)	Tax revenue as a percent of Gross State Product
Log(Population)	Natural logarithm of state population
Unemployment rate	Unemployment rate in a state
Budget surplus	Budget surplus as a percent of Gross State Product
Debt outstanding	Debt outstanding as a percent of Gross State Product
R&D tax credit increase/decrease	Dummy variable equal to one in the year of state R&D tax credit increase/decrease for the firms headquartered in state <i>s</i> , else zero. 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)
Marginal tax rate	Simulated marginal tax rates (after interest expense) from Blouin, Core, and Guay (2010)
Personal income tax increase/decrease	Dummy variable equal to one in the year of state personal income tax (either tax rate on wages, or tax rate on long-term capital gains) increase/decrease for the firms headquartered in state <i>s</i> , else zero. State taxes on wages is the maximum state tax rate on wage income, estimated for an additional \$1,000 of income on an initial \$1,500,000 of wage income (split evenly between husband and wife). The taxpayer is assumed to be married and filing jointly. State capital gains tax rate is the maximum state tax rate on long-term capital gains
Exposure to tax	Proportion of firm activity that takes place within the borders of the state that experiences a tax change. Exposure of firms to state-level tax changes is estimated from the degree of operations that parent firms and their subsidiaries have in each state. We use apportionment rules in the state to measure the exposure to tax changes for combined reporting states and assume that the exposure for non-combined reporting states is their aggregated fraction of sales
Combined	Dummy variable equal to one if the firm applying for the patent is located in a state with mandated combined reporting, else zero. States that require combined reporting in our sample period are OR, MT, ID, CA, AZ, UT, CO, NE, KS, ND, MN, IL, NH, ME, AK, and HI
New product announcement	We implement event-study methodology by fitting a market model over (−246,−30) period to get the expected returns on the firm's stock, and then estimating cumulative abnormal returns over 3-day (−1,1) period around the announcement. To estimate the total number of material firm's announcements over the year, we either (a) sum all positive cumulative abnormal returns over the year ("Sum of all positive CARs"), or (b) count the number of announcements with the cumulative abnormal returns above the 75 percentile year by year after adjusting for firm size and book-to-market ratio ("#Major new products"). Please see Section 3.2.2 for a detailed description

(continued on next page)

(continued)

Variable name	Description
Net leavers	The difference between Leavers and Hires. Leavers refer to the number of inventors who have produced a patent at the sample firm within one past year or a year before but produce at least one patent at a different firm, including inventors who produce their last patent in the sample firm. Hires refers to the number of inventors who produce at least one patent at the sample firm after producing a patent at a different firm within one year and a year after, including inventors who file their first patent with the sample firm
Top tax increase/decrease	Dummy variable equal to one in the year of corporate tax increase/deduction in top brackets and surcharges for the firms headquartered in state s , else zero. We only keep tax changes in top brackets and surcharges, which are progressive in nature and do not consider non-progressive tax changes like Net Operating Loss (NOL) suspensions
σ (Citations)	Standard deviation of citations in the next five years of patents applied for by firm i in financial year t
Zero-cite patents	Total number of patents applied for by firm i in financial year t with zero citations in the following five years
Highly cited patents	Total number of top 10% most cited patents in a year applied for by firm i in financial year t

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