Do Corporate Taxes Hinder Innovation?

Abhiroop Mukherjee, Manpreet Singh and Alminas Zaldokas*

April 28, 2014

Abstract

We exploit staggered changes in state-level corporate tax rates to show that an increase in taxes reduces future innovation. We address concerns regarding the endogeneity of tax changes by using instruments based on state politics. Our instruments exploit state-level variations in both the Republican/Democratic party legislative balance and the majority provision required to pass a tax increase. We also show that firms that are more exposed – those that have higher simulated tax rates, those located in states with laws that make shifting profits out of the state for tax reasons more difficult, and those which have more operations in the states that pass tax changes - are affected more, supporting our causal interpretation of findings.

JEL Classification: G30, G38, H25, O31

Keywords: Innovation, Corporate Taxes, Research and Development, Patents

*All at the Hong Kong University of Science and Technology. Abhiroop Mukherjee: amukherjee@ust.hk, Manpreet Singh: msingh@ust.hk and Alminas Zaldokas: alminas@ust.hk. We acknowledge helpful comments from Mario Daniele Amore, Lauren H. Cohen, Radha Gopalan, Denis Gromb, Daniel Ferreira, Claire Hong, Yifei Mao, and Xuan Tian, as well as the participants of HKUST Brown Bag seminar.

Introduction

Public debates on fiscal measures often involve arguments that corporate taxes discourage innovation as they reduce incentives to work hard and take risks. Such discussions have become particularly prominent after the financial crisis, when many governments have started facing a trade-off between austerity – which requires more attention to government balance sheets – and future growth. For instance, while introducing the administration's new framework for corporate taxation on February 22, 2012, President Barack Obama alluded to this issue by saying that:

"..my administration released a framework ... that lowers the corporate tax rate and broadens the tax base in order to increase competitiveness for companies across the nation. It cuts tax rates even further for manufacturers that are creating new products and manufacturing goods here in America."

Indeed, there may be several reasons why corporate taxes would matter for the future innovation. For instance, net cash flows from innovation projects could decline with higher taxes, and this might lead innovators to reduce effort and/or firms to abandon projects that become ex ante unattractive after the tax change. Innovation projects could be particularly sensitive to tax changes, given the inherent riskiness and high failure probabilities associated with them. Also, higher taxes may raise attractiveness of financing the firm using more debt, which in turn is not the favored form of financing for innovation. Finally, higher taxes might also lower internal cash flows that have been shown to be a major source of financing for innovation activities (Hall (1992); Himmelberg and Petersen (1994)). On the other hand, if R&D tax credits are in place, an increase in the corporate tax rate could make some firms switch from capital expenditures to R&D investment since the latter has a negative effective tax rate. Which of these effects dominate in the real world is, yet, an unanswered question.

In this paper we provide first empirical evidence on the consequences of corporate income tax changes for future innovative activities of affected firms. As the source of identification, we consider staggered corporate income tax changes at the US state level. Unlike federal tax changes, which occur infrequently and affect all firms simultaneously, states often change their corporate tax rates and they do so at different times, helping us isolate the effects of tax changes from other changes that might also affect firms' innovation policy. By focusing on patents as our measure of innovation in a difference-in-difference setting, we find that firms become less innovative following an increase in the rate at which their home state taxes corporate income. On average, a 1.5 percentage point increase in the state corporate income tax rate (a one standard deviation change) leads to a 3.3% decline in a number of patents granted to the state's firms in the two years following the tax change. When we consider increases and decreases separately, we find that most of this effect comes from increases. An average state corporate tax increase – which raises corporate taxes by around 1.1 percentage points – leads to a decline in patents in the next two years by 3.6%. We find very similar results when we use innovation quality measures, e.g. total patent citations or citations per patent, as well as innovation input measures, e.g. R&D investment, as our dependent variables.

Our causal interpretation of the difference-in-difference estimates requires that, conditional on the controls we use, the treated and non-treated firms are not systematically different. For example, states might change tax rates based on local economic conditions, that might also change a firm's incentive to innovate aside from tax change reasons. We address such concerns carefully. First, we show that states do not seem to change corporate taxes based on observable local economic conditions, such as growth or unemployment rates. Second, we look at other coincidental tax changes which take place in the states that changed taxes in our sample period, and show that accounting for other taxes does not alter our conclusions. Third, our first-difference results are robust to controlling for industry-year fixed effects, state fixed effects, region-year fixed effects, and a number of firm- and state-level controls.

Fourth, in a placebo test, we randomize the assignment of tax change years, keeping the distribution of the event years unchanged. For example, in 1997, there was a tax increase in

Vermont, and cuts in California, Connecticut and North Carolina. In our randomization, we pick four other states from the remaining 46 and assign one among them a tax increase and three among them a decrease in the same year. We do this for every year in which at least one state has changed taxes to get one placebo sample of state-year tax changes. Within this placebo sample, we estimate our baseline regression and store the coefficient. We repeat this procedure 5000 times, to get a distribution of placebo tax change regression coefficients, and compare the coefficient we obtain from the real data against this distribution. We find that the absolute value of the coefficient for tax increase in our baseline test is larger than the coefficients from 98.2% of the randomized placebo samples. This implies that unobserved or unmeasured time trends affecting aggregate innovation activities during our sample period are not producing the effects that we document.

Besides these tests, we also employ an instrumental variable approach to aid identification. Our approach relies on the political economy of changes in state corporate tax rates. Our main instrument for tax increases is a state-level dummy variable that takes a value of one when Democrats have enough legislators in both state chambers to pass a tax increase. We thus exploit both changes in the governing party within state, as well as state-level differences (and changes in them) in what constitutes a sufficient majority to pass a tax increase.[1] This instrument is likely to satisfy both types of necessary exclusion restrictions: a) whether or not a particular party has enough legislators to pass a tax increase is unlikely to affect a firm's future innovation; and b) since the majority requirement we look at pertains specifically to the numbers required to pass state tax increases, it is also unlikely that this particular measure of political balance affects innovation through anything other than taxes. We first show that this majority variable indeed has significant predictive power for tax increases. Then, we use predicted values of tax increases from the first stage regression and show that our instrumented tax increase leads to a decline in the number of patents.[2]

[1] Some US states require a simple majority to pass tax increases, while others require supermajority. These state-level requirements have also been changing in a staggered manner in our sample period, as we discuss below, benefitting identification.

[2] We carefully consider – and rule out – other possible interpretations of our instrument in Section 3.4.

In order to refine our results further, we use cross-sectional variation in terms of exposure to tax changes. First, we use simulated marginal tax rates (Blouin et al. (2010)), measured in the year of the tax change, and show that those firms that have higher marginal tax rates are the ones that file for a lower number of patents following tax increases. Additionally, we exploit firm-level variation in exposure to state tax changes based on the degree of operations the firm has in that state. Finally, we find that most of the effect comes from the firms that are more constrained in their ability to avoid taxes. In particular, we find that the effect is bigger among firms in states that have combined reporting laws, i.e. states 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.

Finally, we discuss some explanations for our results and in particular examine some potential reasons behind the asymmetric tax effect we document. As our preferred explanation, we suggest 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 to innovate. Some evidence in support of this prediction comes from our main regression results – while tax increases have an effect on patenting in the two years after the change, tax decreases increase innovation in the third year. To provide more evidence, we look at one very important input, particularly in the innovation context – the firm's innovative personnel – which the firm might find hard to adjust due to labor market and other frictions. We examine movements of innovators across firms and find that although tax decreases do not lead to new hires, a significant number of inventors part with their employers following a tax increase. We also discuss other explanations such as tax increases leading to higher leverage which discourages funding innovative projects.

Our results contribute to a few strands of the literature. First, previous literature has

Importantly, we address concerns regarding the exclusion restriction by showing that when we add a dummy for the majority requirement to pass the state budget in addition to the tax-majority dummy, the latter does not predict tax changes. Moreover, our instrument remains significant in the second stage even after controlling for the direct impact of general state partisan balance on innovation.

paid considerable attention to how R&D tax credits affect investment in innovation.[3] This literature has largely established that tax credits have a significant effect on R&D investment, in addition to spillover effects on innovative activity by other firms, and ultimately on long-run growth and welfare in general.[4] Surprisingly, however, the causal effect of general corporate tax policies on innovation – which have often been at the forefront of recent policy discourse, and, as we show here, an important determinant of innovation – have not drawn enough attention.

Moreover, this literature has focused on R&D spending, not on innovation outputs like patents or citations. While measuring innovation outputs properly is hard, when it comes to taxation, examining them is especially valuable for a number of reasons. First, what tax authorities treat as R&D often differs from what firms consider as productive innovation inputs. Many firms get involved in the legal investigations with Internal Revenue Service with what can be expensed as R&D for R&D tax credit purposes. The other side of the same coin is that firms are known to relabel other costs as R&D for the purposes of tax credits(Griffith (1996)). This makes it difficult to ascertain whether R&D spending response to tax changes, if any, are indeed changes in productive innovation inputs, or simply relabelling other expenditure for tax benefits. Finally, recent research establishes that firms differ widely in the productivity of their R&D investments (Hirshleifer et al. (2013)) and these differences in innovative efficiency translate into differences in future sales (Cohen et al. (2013)). Therefore, although we also provide evidence on the impact of corporate taxes on R&D, we primarily focus on the effects on patenting activity.

Second, we also contribute to the literature that discusses the determinants of innovation. For instance, Manso (2011) argues that managerial contracts that tolerate failure in the short run and reward success in the long run are best suited motivating innovation.

[3]Mansfield (1986), Bloom et al. (2002) , Wilson (2009), and Rao (2013). See Hall and Van Reenen (2000) for a comprehensive survey on this topic.

[4] See, for example, Katz (1986), Grossman and Helpman (1991), or Aghion and Howitt (1992) for theoretical studies; and Jaffe (1986), Jaffe et al. (1993), Branstetter and Sakakibara (2002) and Bloom et al. (2002) for empirical research.

Ferreira et al. (2012) show that private rather than public ownership spurs innovation. Empirical evidence shows that laws (Acharya and Subramanian (2009); Acharya et al. (2013); Acharya and Subramanian (2013)), stock liquidity (Fang et al. (2013)), investment cycles in financial markets (Nanda and Rhodes-Kropf (2012)), financial analysts (He and Tian (2013)), product market competition (Aghion et al. (2005)), investors' attitudes towards failure (Tian and Wang (2011)), financial development (Hsu et al. (2012)), hostile takeovers (Atanassov (2013); Sapra et al. (2013)), banking deregulation (Amore et al. (2013); Chava et al. (2013)), banking competition (Cornaggia et al. (2013)), and institutional ownership (Aghion et al. (2013)) all affect innovation. We contribute to this literature by showing that corporate taxes are also a first order determinant.

In addition, our paper also relates to the effects of corporate tax changes on corporate policies (Graham (2006); Blouin et al. (2010)). Recently, Asker et al. (2013) and Heider and Ljungqvist (2013) have shown that state corporate taxes changes affect firms' after-tax returns on investment and capital structure, and Doidge and Dyck (2014) find a positive effect on investment following a tax increase in Canada.

The rest of the paper proceeds as follows: We describe our data and provide summary statistics in Section 1, discuss our method of analysis in Section 2, describe the empirical results in Section 3, discuss some potential explanations for our results in Section 4, and conclude in Section 5.

1 Data and Summary Statistics

1.1 Data

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 et al. (2001). We focus the analysis on granted patents applied for in the period 1990 to 2006. We match the NBER patent data set with Compustat data following the procedures developed in Hall et al. (2001) and Bessen (2009).

We exclude firms in the financial sector (6000s SICs) and the public sector (9000s SICs). We also exclude observations if the firm's sales or assets are less than $1 million. Also, we exclude the firms with negative stock prices and firms with less than four observations to ensure that we correctly estimate the first difference regressions. We only look at the firms headquartered in the US. Availability of data on taxes restricts our analysis to 1990 onwards, and this reduces our sample size to 48,448 firm-year observations. All financial variables are initially deflated at 2000 price level using CPI data from Bureau of Labor Statistics and later winsorized at 1% on both tails of their distributions.

Our focus in this paper is on the outputs of the innovation process, which we measure using successful patent applications. This is a widely used approach for quantifying innovative performance (Griliches (1998)). We conduct our analysis at the firm, rather than at the state-level, since this allows us to control for unobserved time-invariant firm characteristics, and enables us to examine heterogeneity in the response to tax changes within a given state.

Since Compustat reports the address of a firm's current principal executive office, not its historic headquarter location, we obtain firm location information directly from the 10-K forms (we use the business address of the firm to identify the location of it's state).[5] We use simulated firm-level marginal tax rates obtained from Blouin et al. (2010). Finally, data on state R&D tax credits comes from Wilson (2009) and data on employment, sales, and assets at the parent-subsidiary level come from Lexis-Nexis' Corporate Affiliations database.

[5]We collect the information on firms' business address from Bill McDonald website: `http://www3.nd.edu/~mcdonald/10-K_Headers/10-K_Headers.html`. Hayong Yun and Bill McDonald have parsed all of the fields appearing in headers for 10-K forms available on the SEC's EDGAR website. The data includes all filings from 1994 to 2010. We backfill the data for years before 1994.

1.2 State Corporate Taxes

US states tax corporate profits (or, in the case of Ohio, Texas, and Washington, gross revenue) from corporate activities that take place within that state.[6] State taxes are a substantial part of U.S. firms' overall tax expenses. The average state tax rate in our sample is 6.8%, with a median of 7.3%. There is substantial variation in the top marginal tax rates in each state, from a low of 0.26% in Ohio in 2006 to a high of 12.3% in Iowa. During our sample period, there are a total of 32 instances of state tax increases, spread across 20 states, and 56 instances of tax cuts, spread across 24 states.[7] Figure 1 identifies these changes on the US map during our sample period. The average state corporate tax increase takes the tax rate from 6.67% to 7.76%, while the average corporate tax cut takes the tax rate from 6.8% to 6.1%. Our list of tax increases and decreases comes from Heider and Ljungqvist (2013), which contains more details on these changes and the structure of US state taxes.[8] Our specifications are also robust to using data on the actual tax rate in each state, obtained directly from the Tax Foundation's webpage (http://www.taxfoundation.org).

1.3 Summary Statistics

Table 1 reports summary statistics for the key variables we use in our analysis. As shown in the previous literature on the Compustat-NBER patent data set, patent statistics are skewed. In our sample, the average number of patents is approximately nine but the median is zero. Similar observations apply to patent citations – the average number of (adjusted) citations is 153, while the median is, again, zero. The detailed construction of all control variables is described in Appendix A.

[6]Except Nevada, South Dakota, and Wyoming, which constitute a small part of our sample. Dropping firms in these states from our sample does not change results.

[7]Out of 56 instances of tax cuts during our sample period, 5 instances include cuts in surcharges which can not be directly quantified.

[8]Heider and Ljungqvist (2013) mention that they use data obtained from the Tax Foundation, the Book of the States, a search of the "Current Corporate Income Tax Developments" feature published periodically in the Journal of State Taxation, and state codes accessed through Lexis-Nexis. We thank them for making this data publicly available.

2 Methodology

We use a difference-in-difference approach to examine the causal relationship between corporate taxes and future innovation. The main benefit of this approach is that it 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.

To illustrate, in 1991, Pennsylvania raised its top corporate income tax rate from 8.5% to 12.25%. We examine how the number of successful patent applications filed by firms headquartered in Pennsylvania changed in the three years following this increase. The tax rise can be treated as plausibly exogenous from the viewpoint of an individual firm in Pennsylvania, as long as that firm did not lobby for it (which is highly unlikely). However, it might be the case that innovation was declining throughout the nation at this time – so, if we found that patenting activity for Pennsylvania firms declined after 1991, it would be erroneous to ascribe it to the tax change. Hence, we compare the change in number of patents filed by the Pennsylvania firm to the change in the number of patents filed by a group of firms, otherwise identical, but headquartered in, say, New Jersey. We conduct our analysis using firm-level patent data and exploiting the information on the location of the firm's headquarters.

We look at the effect following three years after the tax change. We follow the literature and use $Ln(1 + \#Patents)$ as the measure of patent filings. Our baseline specification is then:

$$\Delta Ln(1 + \#Patents)_{i,s,t+k} = \beta_D \Delta T^-_{st} + \beta_I \Delta T^+_{st} + \delta \Delta X_{it} + \alpha_t + \gamma_i + \epsilon_{i,s,t+k}$$

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

We follow Heider and Ljungqvist (2013) in running our specification after taking first differences of all variables to control for firm-level unobserved heterogeneity.[9] Our main variables of interest are ΔT^+_{st} and ΔT^-_{st}, which are indicators equaling one if state s decreased

[9]Note that the main variable of interest is tax *changes*, so it is also natural to run the first difference specification, which regresses *change* in the dependent variable on *changes* in all independent variables.

or respectively increased its corporate tax rate in year t. Following Heider and Ljungqvist (2013), we lump all tax changes together by focusing on binary tax change indicators for two reasons. First, some tax increases (for example, California in 2002 and New Jersey in 2002) cannot be 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. We also provide specifications where we use the actual percentage point change in top marginal corporate income tax rate as a measure of ΔT.

We also include a set of firm level factors that affect innovation, ΔX_{it}. Specifically, we control for the change in logarithm of firm sales and capital-labor ratio, following the literature on the production function of patents (see, e.g., Galasso and Simcoe (2011), Aghion et al. (2013)). We include other lagged controls such as change in firm age, profitability, asset tangibility, cash holdings and the presence of a debt rating on the firm (to account for availability and ease of financing); R&D-to-sales ratio (to establish the effect on firms' innovative productivity); and the Herfindahl-Hirschman Index (HHI) based on the distribution of revenues of the firms in a particular three-digit SIC industry (as well as its square term, to account for non-linearities), to control for the impact of industry concentration on innovation. Since US patenting activity has increased substantially starting in the mid-1980s (see, e.g., Hall (2004)), we control for aggregate trends by including a full set of year fixed effects. Additionally, since our main variable of interest is the change in state taxes, we also incorporate state level economic indicators, namely, the change in the Gross State Domestic Product (GSP) in the previous year, the change in the growth rate of GSP, and the change in the state's unemployment rate. The details of the construction of control variables are contained in Appendix A. Also, in addition to accounting for unobserved firm-level heterogeneity in levels of innovation outputs by taking first differences of all variables, we account for unobserved firm-level heterogeneity in growth rates of innovation outputs by controlling for firm-fixed effects. Finally, since our tax treatment is defined at the state level, we cluster standard errors by state, following Bertrand et al. (2004).

3 Empirical Results

We begin our analysis by examining the impact of corporate income tax changes on future innovation by firms. We first look at the number of patents filed in the three years following a tax change. In addition, we do a series of robustness checks on our results. Among them, we highlight coincidental changes in other taxes. Next we report results of our placebo test. We further exploit instrumental variables estimation to provide an additional piece of causal evidence to our results. Then we look at firms' exposure to tax changes. Finally, we examine the importance of patents in terms of the number of future citations they receive.

3.1 Corporate Tax Changes

3.1.1 Main Results

In this section we provide our main baseline results where we study the change in the number of patents that a firm files (and are ultimately successful), following the change in tax rates. Results are presented in Table 2. In columns (1), (2) and (3), we use actual changes in taxes, while in columns (4), (5) and (6) we partition the changes in to positive and negative changes. In columns (7), (8) and (9) we replace the changes with binary indicators, which allows us to include tax changes that cannot be directly quantified, such as changes in tax surcharges.

Our results in Table 2 provide evidence that tax increases reduce future innovation. In the first three columns, we look at actual changes in tax rates as our main explanatory variable. Our evidence shows that innovation gets affected after two years following the tax change. Our results show that a 1.5 percentage point increase in the state corporate income tax rate (a one standard deviation change) leads to a 3.3% decline in a number of patents granted to the state's firms in the two years following the tax change. In columns (4)-(6), we split tax changes into increases and decreases, and find that most of our effect comes from increases, not tax decreases. In terms of economic magnitude, in the second year following a tax increase, our dependent variable ($Ln(1 + \#Patents)$) declines by 3.6%, while there is no significant effect after tax decreases.

Although instructive, looking at actual tax changes forces us to leave out tax changes that are not directly quantifiable. For example, some tax increases (for example, California in 2002 and New Jersey in 2002) cannot be 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, such as changes in tax surcharges. This could be problematic if the asymmetry between increases and decreases is driven by a power issue in our tests, arising from leaving out tax decreases that are not directly quantifiable, but potentially, ones that did affect innovation. In columns (7)-(9), we construct binary indicators for tax increases and decreases to correct for these issues. Our evidence shows that using these indicators for tax changes, future innovation falls by 0.051 two years following a tax decrease, which represents a 5.65% decline in number of patents from its mean level.[10]

Interestingly, our results also show that while tax cuts do not have any significant effect on future innovation in the first two years after the change, there is an increase in innovation in the third year. In terms of economic magnitude, there is a 2.11% increase in future patenting activity in the third year following a tax decrease. This is a smaller effect than what tax increases produce, even after taking into consideration that the average tax increase in our sample is larger than the average tax decrease (1.1% compared to 0.73%). However, the timing of the change is consistent with the explanation that when taxes increase, firms cut marginal innovation projects quickly. However, when tax cuts occur, firms take time to set up innovative capacity and hire new employees, so the fruits of any change in innovation policy might take longer to realize. We explore this explanation further, and present further evidence on the asymmetry of tax effects in the discussion section.

[10]0.051 is the average decline across the three specifications presented. Recall that this is the change in $\Delta Ln(1+\#Patents)$. Appendix B contains details on how we calculate percentage change in $\#Patents$ from this number.

3.1.2 Robustness

In this section we consider various refinements of, and potential concerns with, our analysis above, and present results in Table 3. We replicate our baseline specification, reported in Table 2, columns (7)-(9), where we exploit the indicator variables of tax changes that take care of all flavors of tax changes.

First, our original dummy variables do not distinguish between large and small tax changes.[11] In row (1), we separately consider large and small tax changes. We define large increases (or large decreases) to be those in which the tax rate changed by more than 1 percentage points.[12] In order to examine any potential differences in the effects of large vs. small changes, we re-estimate our baseline regression, replacing the tax increase and decrease dummies with four variables – large tax increases (average increase 2.95%), small tax increases (average increase 0.44%), large tax decreases (average decrease -2.11%), and small tax decreases (average decrease -0.42%). Our results show that future innovation is affected only by large tax increases while the other types of tax changes do not produce any significant effects. These tests also suggest that our asymmetry results are not driven by any difference in magnitude of tax increases vs. decreases.

Second, one might be concerned that one state has a disproportionately large number of firms in a certain industry that is more sensitive to tax changes than some other state. 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 headquartered in different states. Results remain virtually unchanged.

Also, it might be the case that there were broad region-level trends in innovation in the period we consider. In addition, some states are more innovative than the other ones, and

[11] We do not exclusively look at the largest tax changes, since such a sample might suffer from clear endogeneity problems. As we show in section 3.4, tax changes in general have little to do with the macroeconomic environment, and are more a function of the interaction between political balance and an institutional feature of the state. However, large tax changes are much more likely to be correlated with specific macroeconomic conditions at the state-level. This would severely limit an econometrician's ability to provide casual evidence, particularly because the exclusion restriction will now be much harder to argue for.

[12] Our results are invariant to the actual cut-off point.

also they differ in other innovation incentives that they provide to the firms. We account for these possibilities by incorporating state fixed effects to control for unobserved state-level heterogeneity as well as region-year trends in our specification, reported in row (3). This is in fact our most stringent specification that includes year, firm, state and region-year fixed effects. Our results are consistent.

Next, one might be concerned that patenting activity was in general increasing over time which might have had spurious correlation with tax changes becoming more frequent over time. We address this issue by interacting our tax dummies with the dummies indicating the sample period 1990-1999 and 2000-2006, respectively, and estimate the baseline regression. We obtain separate effect of tax changes in the 1990-1999 period from the 2000-2006 tax changes, and find similar results in both periods (row 4).

In row 5, we exclude firms that come from California and Massachusetts, and find that our result remains unaffected.

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 decreases were followed by some innovative firms being acquired and thus dropping out of our sample, with other firms maintaining their level of activity. To address this issue, we use a sample of firms that survive till the end of our sample. That is, we follow the same firms that existed in 1990 throughout our sample and new firms that enter into the sample after 1990 and do not drop out. Results presented in row 6 show the same pattern as our main results.

In row 7 we look at tax increases that were reversed later. The firm-years with tax changes that did not get reversed are dropped from these tests to ensure validity of our control sample. Surprisingly, even in this case, the treated firms' innovative activity does not seem to reverse. We observe a decline in the number of patents filed and the effect is persistent after two years of the initial increase in taxes.

In row 8 we look at the treatment effects for states with no reversals. The firm-years with reversals are dropped from the sample. We find a decline in the number of patents filed for states without reversal tax changes.

Next, we add further state-level macro controls in our base specification. Row 9 reports that our results are robust to the inclusion of additional state-level macro variables, namely, change in state budget surplus, debt outstanding and state tax revenue (as percentage of GSP).

In our base line specification we include firm fixed effects in our first-difference regression. One may be concerned about potential problems associated with over-differencing. In row 10 we report the specification without including firm fixed effects and find that our results are robust.

In the unreported results we also find that if we replace our measure $\Delta Ln(1{+}\#Patents)_{i,s,t+k}$ with $\Delta Ln(\#Patents)_{i,s,t+k}$ and limit the sample to the firms that file non-zero number of patents in both periods, and we find consistent results. Our results are also robust if we use Compustat headquarter information to identify firm's state, or we use the state where the highest proportion of firm's employees is located.[13]

3.2 Coincidental Changes in Other Taxes

Our argument that corporate income taxes have a causal effect on innovation activity requires us to rule out the possibility that some other omitted variable caused firm-level changes in innovation, and happened to coincide with our measure of tax changes. Because our regressions are robust to controlling industry-year effects, we can be confident that they are not driven by time-varying industry shocks. We also know that our results hold within-firm (since we ruled out firm-level unobserved heterogeneity in the level of innovation by first-differencing the data) and that they are robust to the inclusion of state fixed effects (rules out unobserved state-level heterogeneity), region-year fixed effects (rules out unobserved, time-varying regional economic conditions), as well as firm fixed effects applied to the first-differenced data (rules out firm-level unobserved heterogeneity in the growth rate

[13]Firms may strategically change their state of operation to avoid tax increase. Our results also hold for the firms that do not change their state during our sample period.

of innovation).

Further, recall that 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* across states and time. The most likely candidate for such an omitted variable that can come to mind is, therefore, coincident changes in other provisions in taxes.

Often state legislatures change more than one tax provision at the same time in the bills they pass. In this section, we show that the effect of corporate income taxes on innovation survives if we explicitly account for such coincidental tax changes.

First, Panel A of Table 4 shows that there is little tendency for states to change R&D tax credits, personal income tax rates, and capital gains tax rates at the same time as the corporate income tax rates. Consider the R&D tax credits. The table shows that only 8 out of our 56 instances of corporate income tax cuts coincided with increase in R&D tax credit.[14]

We explicitly account for such coincidental tax changes in Panel B of Table 4. In columns (1)-(3), we incorporate R&D tax credit changes in our basic specification. Our corporate tax change variable remains virtually unaltered when we include the R&D credit changes in our regressions, which shows that corporate taxes have a distinctly different effect on future innovation. The R&D credit changes which lie within our sample period also seem to have an asymmetric effect on future innovation, much like corporate taxes. A decrease in the R&D credit reduces future innovation, but an increase does not seem to have the same effect. In columns (4)-(6) and (7)-(9), we examine personal income tax and capital gains tax changes. In each of these specifications, accounting for coincidental changes in other taxes leaves our baseline results virtually unchanged.

[14]Heider and Ljungqvist (2013) show similar overlap numbers for their sample.

3.3 Placebo Test

In this section we address two important issues that can potentially bias inferences in innovation studies. These are the inability of the econometrician to rule out state-level trends in innovation activity in an accurate fashion, and the inability to account for the correct structure of the error covariance matrix from the regressions. Specifically, here we provide a test where we randomize the assignment of tax change years, keeping the distribution of the event years unchanged.

For example, in 1997, there was a tax increase in Vermont, and cuts in California, Connecticut and New Carolina. In our randomization, we pick four other states from the remaining 46 and assign one among them a tax increase and three a decrease in the same year (1997). We repeat this procedure for all tax-change years to get one pseudo tax change sample. We repeat this procedure 5000 times to get 5000 pseudo tax change samples. In each of these pseudo tax change samples, we run our baseline regression and save the relevant coefficients. Finally, we compare the coefficient from the actual tax change sample with those from these pseudo tax change samples.

We report the distribution of the coefficients in Figures 2A and 2B. Here the black line embedded in the graph represents the regression coefficient we observe in the actual tax changes sample (real data) in the second year after the tax change. Figure 2A shows that the coefficient for tax decreases in our placebo sample does not lie in the tails of the placebo distribution, while that for tax increases does – the tax increases coefficient in our baseline test in table 2, column (8) is less than the coefficients from the randomized placebo samples in 4910 out of 5000 cases.

This test shows that unobserved or unmeasured time trends affecting aggregate innovation activities during our sample period are not affecting our results; and the non-parametric bootstrap flavor of our analysis also gives us more assurance that incorrectly specifying the standard error matrix is not driving our results either.

3.4 Instrumental Variables Approach

To this point, our evidence shows that tax increases reduce future innovation by firms. However, concerns might remain regarding the exogeneity of tax changes which might be the product of some state-level macroeconomic factors that could also drive innovation directly. If this is the case, then in our analysis, we could be incorrectly assigning to tax changes the predictive power that actually comes from the omitted macro variable.

We examine such concerns in this section. First, our evidence in Table 5, shows that the observable state-level macro variables do not predict tax changes. Heider and Ljungqvist (2013) use a different list of macro variables and find the same result. Next, we also see (from Table 2) that these state-level macro variables do not systematically predict future innovation. However, any list of macro variables could remain potentially in-exhaustive.

Therefore, we use a two-stage least squares regression approach, where we look at whether or not either party has the number of legislators in the state congress and senate required to pass a tax increase. Specifically, we look at dummy variables that take a value of one when Democrats have enough legislators in both chambers to pass a tax increase (whether there is a supermajority requirement or not), and when no party has enough majority to pass the tax increases (Republicans having a sufficient majority being the omitted third case). We thus exploit both a) changes in the governing party within state, as well as b) state-level changes in what constitutes a sufficient majority to pass a tax increase.[15]

The data for this set of variables comes from Carl Klarner of the Indiana State University's Department of Political Science.[16] These variables are likely to satisfy both types of exclusion restrictions required for identification. First, it is unlikely that the exact number of legislators required by a party to pass a tax increase is something that a firm could have lobbied for. Second, since we are able to examine the majority requirement that specifically pertains to tax increases, it is likely that this variable affects innovation only through the tax channel.

[15]A point to note here is that our instruments apply only to tax increases, so in this section we do not compare increases vs. decreases.

[16]We thank Carl Kalrner for making his data publicly available.

The evidence from these regressions is presented in Table 6. Panel A presents the results from the first-stage regression, where we instrument tax increases with a dummy whether Democrats have enough legislators in both chambers to pass a tax increase.[17] We first show that the Democratic Party has the majority required for a tax increase in a state's legislatures, then tax increases are significantly more likely.

Table 6, Panel B presents results from the second stage, which is identical to the regressions in Table 2, except that the tax increase dummy is now replaced by its predicted value from the first-stage regressions reported in Panel A (the standard errors are corrected for this two-step estimation procedure, in addition to being clustered at the state level). In this table, to save space, we only present the coefficient on the (instrumented) tax increase variable, but the second stage regression also includes all first stage regressors and controls used in Table 2.

To make sure that our identification is indeed driven by the majority requirement with regards to the tax increases, in columns (4)-(6) we also control for Democrats having a simple majority (in both houses). This control takes care of a potential criticism that a strong one-party majority ensures a stable political environment that might, in itself, contribute to firm incentives to innovate. Recall that in IV regressions all first stage controls are also included in the second stage, so including the majority control variable in the second stage will directly soak up the effect a strong Democratic majority can have on future innovation. Inspite of this, the Democratic tax majority instrument retains its significance, providing evidence that it has incremental predictive power, and therefore, likely satisfies the second type of exculsion restriction mentioned above.

Perhaps even more pointedly, in columns (7)-(9), we employ a dummy for Democrats having sufficient majority in both houses to pass the budget – in addition to our usual instrument (the dummy for Democrats having sufficient majority in both houses to *pass a tax increase*). The budget majority dummy is insignificant, while the tax majority dummy con-

[17]We follow Angrist and Pischke (2009)'s advice to estimate OLS regressions, despite the fact that we have dummy variables as our instrument and the endogenous variable.

tinues to remain highly significant. This addresses a remaining concern regarding majority provisions themselves: While we control for Democratic majority in both houses in columns (4)–(6), one still might worry that it is more than a majority, maybe a supermajority in both houses that leads to a political environment sufficiently stable to directly affect innovation. If that were the case, the budget majority dummy should now pick up such effect. The fact that the tax majority dummy continues to be significant, in the presence of the budget majority dummy, shows that the difference between majority provisions required to pass a tax increase and that required to pass the budget matters. This is a very strong condition – one that is likely to hold only if the identification comes purely through the tax majority requirement channel. In addition, the budget majority dummy itself is not significant, which again is consistent with the view that it is not just any type of majority, but the precise majority requirement for passing tax increases, that matters for tax changes. Our second stage results are, assuringly, very similar to those presented earlier.

Finally, in columns (10)–(12) we used two instruments for tax increase – a dummy if Democrats have enough legislators in both chambers to pass a tax increase (whether there is a supermajority requirement or not), and a dummy when no party has enough majority to pass the tax increases. Our results are again consistent.

3.5 Exposure to Tax Changes

We now explore cross-sectional differences among firms that are subjected to our tax shocks. We measure exposure to tax changes in three ways. First, we distinguish firms by the marginal taxes that they face. Second, we distinguish firms according to their ability to use tax sheltering. Finally, we estimate exposure of firms to state level tax changes by using detailed proprietary data collected by Lexis-Nexis on the degree of operations parent firms and their subsidiaries have in each state.

3.5.1 Marginal Tax Rates

While the results we present above suggest that firms respond to tax increases by changing their innovation policy, our estimates are likely to be conservative, because firms differ in terms of their exposure to tax changes. For example, a firm that is unprofitable is exposed less to any changes in the state's top corporate income tax rate.[18] So, in this section we measure a firm's exposure to tax changes using the marginal tax rate (Blouin et al. (2010)), measured in the year of the tax change. If, indeed, the effect we show is through the corporate tax channel, then we should expect to see the strongest changes for firms that had the highest marginal rates before the tax change.

Table 7 presents these results. We use Blouin et al. (2010)'s simulated marginal tax rates (after interest expense) in period t to partition sample firms into those with marginal tax rates in the bottom 30 percentiles, middle 40 and top 30 percentiles, respectively. The firms in bottom 30 percentiles have an average marginal tax rate (MTR) of 7.5% and are least exposed to tax changes. In line with expectations, we do not see any change in their innovation outputs following the tax change. We find similar results for firms in the middle 40 percentile. All of our effect comes from firms in top 30 percentile (with an average MTR of 34.4%) – firms with higher marginal tax rates indeed file a lower number of patents in response to tax increases and higher number of patents in response to decreases. Overall, this test supports a causal interpretation of the tax sensitivity of innovation that we documented in previous sections.

[18]Of course, this does not imply that the top corporate income tax rate does not matter at all for a currently unprofitable firm – it can matter through the incentive channel. Consider a young, unprofitable firm with growth options, in the form of investment projects, that can lead to patentable innovations. The decision to undertake such an investment is clearly going to be a function of future increases in net income from the patentable innovation it can produce. This future net income can be impacted if the patent leads to a significant increase in revenues.

3.5.2 Tax Sheltering

Most large multi-state corporations are composed of a parent corporation and a number of subsidiaries. In certain instances, this provides such companies with the opportunity to use tax shelters of a type that is of particular concern to our analysis. Since our main variable of interest is innovation outputs as measured by patents, which is an intangible asset, companies can use a tax shelter that is frequently referred to in legal circles as a Delaware Trademark Holding Company or Passive Investment Company (PIC). Under this shelter, 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 have tax royalties, interest, or similar types of 'intangible income'. Profits of the company that would be taxable by the states in which it operates are shifted out of such states for tax accounting purposes, by having the tax-haven subsidiary charge a royalty to the rest of the business for the use of the trademark or patent. The strategy works since the royalty is tax-deductible for the parent as well as other subsidiaries, and hence, directly reduces the amount earmarked as profits in the states in which the company is taxable. So, in our case, this would reduce the responsiveness of a firm taking advantage of such opportunities to any tax changes. Then, the measures we provide above might contain an attenuation bias.[19]

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 be 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.

[19]Notice that this cannot be a reason that we find the asymmetry: if tax avoidance were in play, then we should have seen opposite effects – firms taking advantage of tax cuts by moving innovation activity into states that cut the tax, and firms shifting such activity out of tax-increasing states into tax havens. Such a tax strategy would have implied the opposite kind of asymmetry – firms responding to tax cuts but not to tax increases which they could potentially avoid.

In our sample period, sixteen US states had combined reporting requirements in place. For example, California had a combined reporting system but Massachusetts did not.[20]

We examine this hypothesis in Table 8. We interact our corporate tax change variables with dummy variables indicating whether the state in question had combined reporting rules in place. 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 (where they could not shift profits to a tax-haven subsidiary). The results form an interesting pattern. In the year immediately 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 (although the effect is slightly weaker in magnitude in states without combined reporting). However, in the next two years, the reduction continues to remain significant only in firms that are located in combined reporting states. This pattern is consistent with a hypothesis that firms in states that do not require combined reporting shift out innovation activity after experiencing a tax increase in their home state, but this shift takes time.

3.5.3 Location of Operations

In this section, we further measure the exposure to tax changes by looking at the distribution of firm's sales, employees, or assets across different states. 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

[20] Mazerov (2009) contains more details on combined reporting practices across US states. No state that did not already have combined reporting adopted it in our sample period. Of course the decision to locate and remain headquartered in particular states 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. First, studies on manufacturing firm location find little evidence that they move out of combined reporting states. Second, anecdotal evidence on the most innovative firms also seems to suggest that combined tax reporting requirements are not first order determinants of firm location choice: Despite its use of combined reporting, California is home to Silicon Valley. For instance, Intel Corporation has its headquarters in California while most of its chip plants are in Oregon, Arizona, and Colorado – all combined reporting states.

have been affected more than firm B. We test this prediction in Table 9.

In this table, 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. We use three different measures of firm activity from the Corporate Affiliations database – the number of employees, sales, assets.[21] In our previous example, firm A's exposure to the New Hampshire tax increase in 1999 is 0.75, while that of firm B is 0.25.[22] Panel A presents results from regression specifications which are identical to the regressions in Table 2, except that the tax increase dummy is now replaced by a firm's exposure to the increase. Since the exposure to the tax change variable is now measured at the firm level, we cluster standard errors by firm in these specifications. Columns (1)–(3) use the proportion of employees, columns (4)–(6) use proportion of total assets, while columns (7)–(9) use proportion of sales coming from each state. Overall, results presented in the table are consistent with our earlier evidence.

In Panel B, we look for evidence of sophisticated tax sheltering activity by examining differences between states with and without combined reporting rules in place, similar to Table 8. The main difference in this section is that we are able to incorporate information on firm activity in the states under scrutiny. The prediction here is that firms with higher exposure to tax increases in combined reporting states should see a greater decline in future innovation than firms with higher exposure to tax increases in states without combined reporting. The evidence is consistent with this hypothesis. In these tests, we separate out a firm's exposure to tax changes in states with combined reporting from its exposure to changes in states without combined reporting.[23] Our results show that higher exposure to

[21] An equally-weighted average of these three proportions gives similarly consistent results.

[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] If a firm experiences tax increases (decreases) in multiple states in the same year, then we use the following rule: Suppose the firm has 50% activity in state 1, which uses combined reporting, and 25% in state 2, which does not. Both states increase taxes in the same year. Then our measure of this firm's exposure to tax increase in a combined reporting state takes a value 0.5, while our measure of its exposure to tax increase in a state without combined reporting takes a value 0.25.

tax increases reduces future innovation in states with combined reporting but not in states without the rule in place.

3.6 Alternative Innovation Measures

So far our results indicate that firms respond to tax increases by filing a lower number of patents. In this section, we look at alternative measures of innovation: R&D investment as well as the quality of patents.

3.6.1 R&D

First, we 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 assets, and then we show results using $Ln(1 + R\&D)$ to ensure that changes in R&D (and not assets) are driving our results.

We report the results in Table 10. In columns (1) and (2), we report results for R&D to assets. Column (1) looks at a first difference specification with time fixed effects, while column (2) also includes firm fixed effects. We find that R&D to assets reacts to both tax increases and decreases. R&D investments decline following tax increases, and increase following tax cuts. In terms of economic magnitudes, R&D to assets declines by 4.5% of its sample mean after a tax increase and a tax cut (column (1)). This implies an elasticity of around 0.3 for tax increases, and 0.4 for tax cuts. Compared to the R&D tax credit literature, the elasticity of R&D to corporate taxes is much lower. These papers typically report an elasticity of R&D expenditure to R&D tax credit of around 1.[24]

However, when we look at $Ln(1 + R\&D)$ in columns (3) and (4), we find an asymmetric response pattern, with increases producing a greater response than decreases. Here, the elasticity of future R&D spending to tax increases is 0.1.

[24] Recall that the average tax increase (decrease) is 1.1 (0.73) percentage points. From a sample mean of 6.8%, this implies the average tax increase raises rates by 16.17%, and the average decrease lowers it by 10.74%.

Another feature of our R&D results worth pointing out is the timing of the R&D changes. All our results for R&D changes reported here occur in the year immediately after the tax increase, we find no significant effect in the following years. This is assuring, consistent with the view that as incentives to innovate change, firms change R&D expenditures quickly. However, since innovation output, which is a function of these expenditures (among other things) takes time to respond – we find an effect on innovation two to three years after the tax change.

Overall, in this section we find robust evidence that tax increases reduce future investment in R&D. Our results for the effect of tax decreases on future innovation are less robust – although we find some evidence that tax decreases increase investments in R&D.

3.6.2 Innovation Quality

The literature shows that patents differ greatly in terms of their relative importance. Therefore, simple patent counts (like the measure we use in the previous sections) do not necessarily capture the economic importance of the associated inventions (Harhoff et al. (1999); Hall et al. (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)). Forward citations reflect the technological importance of patents as perceived by the inventors themselves (Jaffe et al. (2000)) and better reflect their economic importance Hall et al. (2005). Moreover, perceptions of importance by experts in the relevant field are also captured well by citations (Albert et al. (1991)).

We use cite counts adjusted for truncation from the NBER dataset (see, e.g., Hall et al. (2001) and Hall et al. (2005)) to deal with the issue that citation data suffers from truncation problems. In Table 11, we run the same specification as in columns (7)-(9) of Table 2, but the dependent variables here measure the quality of innovation. In Panel A, we use $Ln(1 + \#(truncation - adjusted)citations)$. Since the total number of citations is correlated with the number of patents, in Panel B we look at an arguably stronger measure of innovation quality, namely, the number of citations per patent ($Ln(1 + \#(truncation - adjusted)citations/\#patents)$). In both of these panels, the first three columns present results for a first difference specification including time fixed effects, while the next three columns also includes firm fixed effects. 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, #(truncation-adjusted)citations/#patents, declines by 6.1% in the year following the tax change (calculated from the formula in Appendix B). Overall, these results show that the quality of innovation, measured by citations, declines following tax increases, but does not change significantly following tax declines. This mirrors our earlier evidence on the number of patents. Finally, all other results that we present throughout the paper for the number of patents (tables 3, 4, 6, 7, 8 and 9) also hold for these different measures of innovation quality, i.e. number of citations and number of citations/patent, and we present these in the internet appendix.[25]

4 Discussion on Asymmetry

In the previous sections, we presented evidence that the response of future innovation to taxes is asymmetric. Again, recall that this effect is not produced by any asymmetry between the number of tax increases and decreases we have in our sample – we actually have more instances of tax cuts than increases. Also, it is unlikely that this asymmetry is produced by the average tax decrease being smaller – first, the average tax cut in our sample is of an order of magnitude similar to that of the average tax increase; second, we find the same asymmetry when we explicitly account for the magnitude of the change in our regressions (table 2, columns (4)-(6)); and finally, the effect remains asymmetric when we consider only large tax changes, both increases and cuts (table 3, row 1). In this section we explore what contributes to this asymmetry.

[25] Available at: http://www.alminas.com/papers/Innotaxes_IA.pdf

4.1 Innovation Investment-Innovation Output Relationship

One of the reasons why innovation is sensitive to corporate taxation, as mentioned in the introduction, is that higher taxes reduce the potential rewards to innovation. This is likely to reduce the incentive to invest in innovation, and work hard to achieve it. Effort is notoriously hard to measure, but one place to search for evidence of the operation of this incentive channel is R&D. Consistent with this view, we do find evidence that firms reduce R&D expenditures following tax increases. Also, our R&D results presented above show that the R&D response to innovation is not as robustly asymmetric as our patent-based results are.

One simple explanation consistent with these empirical findings is that the relation between innovation inputs (R&D) and outputs (patent-based measures) is concave. This type of relationship is completely consistent with workhorse models of optimal R&D models, such as Grossman and Shapiro (1988), and is motivated in theory by the fact that increasing R&D expenditures without limit does not guarantee success in innovation. Given the concavity of the R&D to innovation output function, the reduction in R&D due to the tax increase is going to produce a bigger effect on patents than the corresponding increase in R&D due to the similarly sized tax cut.

4.2 The Leverage Tax Shield and Debtholder Preferences

Another potential explanation for response of firm innovation to tax changes operates through changes in firm financing structure to exploit debt shields against taxes. Here, we rely on Heider and Ljungqvist (2013), who 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 potentially do not like funding risky innovation projects. Note that this would require some amount of commitment from firm managers; once they raise debt and increase leverage, they would have to commit not to engage in risk shifting and pursue more risky – e.g., innovative projects.

In Panel A of Table 12 we divide firms based on their ability to change leverage. We measure firm's access to bond market with the existence of S&P credit rating in the year of tax change. We find that our effects are stronger among firms that have credit ratings, i.e. those that would, ex-ante, be in a better position to use the debt shield for taxes. In Panel B of Table 12 we divide firms based on whether they actually increased leverage following the tax change, and find that the drop in future patenting is present both among the firms that changed their leverage as well as those that did not. For firms that increase leverage following tax increases this evidence is consistent with the above hypothesis. However, the fact that even the firms that do not change leverage exhibit changes in future innovation indicate that changing leverage could not be the only reason behind changes in innovation.

4.3 Innovator Incentives

The asymmetry of our results can, perhaps, best be explained in terms of changed incentives for innovators. 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 to innovate.

Though the firm may not fire existing workers after the rise in taxes, they reduce R&D spending. Productive innovators might be affected by these cuts in R&D spending in terms of project funding, and they might realize that the prospect of increasing R&D and the potential upside in the remuneration for their future innovations is more limited now. The nature of the innovative projects might also change and the firm might be more willing to pursue less risky innovations. Observing this change in strategy, innovators might either have less incentives to innovate within the existing firm or even leave the firm and bring their new ideas to another firm.

On the other hand, although it is easy to cut back on effort, given the decreasing marginal productivity of their effort, innovators might find it hard to increase innovation if the firm experiences a similarly-sized tax decrease and wants to foster more innovation. New innovators might need to be hired and given labor market frictions this might take time, while current employees might need to acquire more skills or learn how to be more productive.

In other words, getting great new ideas might be exogenous to firm employees, but it is endogenous which firm will be able to use them.

Although we cannot observe such behavior directly, we explore this explanation by looking into whether the firms hire new innovators following the tax decreases, and whether their innovators leave following the tax rises. We tap into the individual inventor data from the Harvard Business School patent and innovator database[26] which holds data on both inventors (i.e., those individuals who produce the patent) and assignees (the entity that owns the patents, which could be a government, a firm, or an individual). We can thus track the mobility of inventors across different assignees.

We estimate 'New Hires' as the number of inventors who produce at least one patent at a new assignee (our sample firm) after producing a patent at another assignee in the sample within one year. Also, we estimate 'Leavers' to be the number of inventors who stop producing patents at the sample firm and produce at least one patent within a year at a new assignee firm.

We then link the number of new hires to the tax decreases and we link the number of leavers to the tax increases. In Table 13 we find that although tax decreases do not lead to new hires, in two years following the tax increase there is a significant number of inventors who leave the firm. These results are consistent with the explanation that firms are quick to lose their innovative inputs but may need longer time to build the knowledge, workforce and capacity to innovate.

5 Conclusion

Discussions on innovative competitiveness and corporate taxation have both, independently, emerged at the forefront of policy discourse. Some policy makers argue in favor of higher taxes on corporations to reduce inequality, while at the same time there is a strong demand for policies that make firms in their countries more innovative. Are these two objectives

[26] Available at http://dvn.iq.harvard.edu/dvn/dv/patent.

at loggerheads? Does changing corporate tax policy also affect future firm innovation? 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. Using difference-in-difference as well as instrumental variable estimation we find that the tax sensitivity of innovation is asymmetric: while firms respond to tax increases by filing a lower number of patents in the next three years, they do not react to tax cuts by increasing innovation.

References

Acharya, Viral, R. B. and K. V. Subramanian (2013). Labor laws and innovation. *Journal of Law and Economics, forthcoming.*

Acharya, V. V., R. P. Baghai, and K. V. Subramanian (2013). Wrongful discharge laws and innovation. *Review of Financial Studies, forthcoming.*

Acharya, V. V. and K. V. Subramanian (2009). Bankruptcy codes and innovation. *Review of Financial Studies 22*(12), 4949–4988.

Aghion, P., N. Bloom, R. Blundell, R. Griffith, and P. Howitt (2005). Competition and innovation: An inverted-U relationship. *Quarterly Journal of Economics 120*(2), 701–728.

Aghion, P. and P. Howitt (1992). A model of growth through creative destruction. *Econometrica 60*, 323–351.

Aghion, P., J. Van Reenen, and L. Zingales (2013). Innovation and institutional ownership. *American Economic Review 103*(1), 277–304.

Albert, M. B., D. Avery, F. Narin, and P. McAllister (1991). Direct validation of citation counts as indicators of industrially important patents. *Research Policy 20*(3), 251–259.

Amore, M. D., C. Schneider, and A. Žaldokas (2013). Credit supply and corporate innovation. *Journal of Financial Economics 109*(3), 835–855.

Angrist, J. D. and J. S. Pischke (2009). *Mostly Harmless Econometrics: An Empiricist's Companion.* Princeton University Press.

Asker, J., J. Farre-Mensa, and A. Ljungqvist (2013). Corporate investment and stock market listing: A puzzle? *Working Paper*.

Atanassov, J. (2013). Do hostile takeovers stifle innovation? Evidence from antitakeover legislation and corporate patenting. *Journal of Finance 68*(3), 1097–1131.

Bertrand, M., E. Duflo, and S. Mullainathan (2004). How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics 119*(1), 249–275.

Bessen, J. (2009). National Bureau of Economic Research PDP project user documentation. *Mimeo.*

Bloom, N., R. Griffith, and J. Van Reenen (2002). Do R&D tax credits work? Evidence from a panel of countries 1979–1997. *Journal of Public Economics 85*(1), 1–31.

Blouin, J., J. Core, and W. Guay (2010). Have the tax benefits of debt been overestimated ? *Journal of Financial Economics 98*(2), 195–213.

Branstetter, L. and S. Sakakibara (2002). When do research consortia work well and why? Evidence from Japanese panel data. *American Economic Review 92 (1)*, 143–159.

Chava, S., A. Oettl, A. Subramanian, and K. V. Subramanian (2013). Banking deregulation and innovation. *Journal of Financial Economics 109*(3), 759–774.

Cohen, L., K. Diether, and C. Malloy (2013). Misvaluing innovation. *Review of Financial Studies 26*(3), 635–666.

Cornaggia, J., Y. Mao, X. Tian, and B. Wolfe (2013). Does banking competition affect corporate innovation? *Journal of Financial Economics, forthcoming*.

Doidge, C. and A. Dyck (2014). Taxes and corporate policies: Evidence from a quasi natural experiment. *Journal of Finance, forthcoming*.

Fang, V., X. Tian, and S. Tice (2013). Does stock liquidity enhance or impede firm innovation? *Journal of Finance, forthcoming*.

Ferreira, D., G. Manso, and A. C. Silva (2012). Incentives to innovate and the decision to go public or private. *Review of Financial Studies, forthcoming*.

Galasso, A. and T. S. Simcoe (2011). CEO overconfidence and innovation. *Management Science 57*, 1469–1484.

Graham, J. (2006). A review of taxes and corporate finance. *Foundations and Trends in Finance 1*(7), 573–691.

Griffith, R. (1996). Tax competition: Is there any empirical evidence? *Insitute of Financial Studies, mimeo*.

Griliches, Z. (1998). Patent statistics as economic indicators: A survey. *R&D and productivity: The econometric evidence*, 287–343.

Grossman, G. and E. Helpman (1991). Innovation and growth in the global economy. *MIT Press*.

Grossman, G. M. and C. Shapiro (1988). Optimal dynamic R&D programs. Technical report, National Bureau of Economic Research Cambridge, Mass., USA.

Hall, B. and J. Van Reenen (2000). How effective are fiscal incentives for R&D? A review of the evidence. *Research Policy 29*(4), 449–469.

Hall, B. H. (1992). Investment and research and development at the firm level: Does the source of financing matter? Technical report, National Bureau of Economic Research.

Hall, B. H. (2004). Exploring the patent explosion. *Journal of Technology Transfer 30*(1-2), 35–48.

Hall, B. H., A. Jaffe, and M. Trajtenberg (2005). Market value and patent citations. *The RAND Journal of Economics 36*(1), 16–38.

Hall, B. H., A. B. Jaffe, and M. Trajtenberg (2001). The NBER patent citation data file: Lessons, insights and methodological tools. Technical report, National Bureau of Economic Research.

Harhoff, D., F. Narin, F. M. Scherer, and K. Vopel (1999). Citation frequency and the value of patented inventions. *Review of Economics and Statistics 81*(3), 511–515.

He, J. J. and X. Tian (2013). The dark side of analyst coverage: The case of innovation. *Journal of Financial Economics 109*(3), 856–878.

Heider, F. and A. Ljungqvist (2013). As certain as debt and taxes: Estimating the tax sensitivity of leverage from exogenous state tax changes. Technical report, Working Paper, NYU Stern School of Business.

Himmelberg, C. P. and B. C. Petersen (1994). R & D and internal finance: A panel study of small firms in high-tech industries. *Review of Economics and Statistics 76*(1), 38–51.

Hirshleifer, D., P.-H. Hsu, and D. Li (2013). Innovative efficiency and stock returns. *Journal of Financial Economics 107*(1), 632–654.

Hsu, P.-H., X. Tian, and Y. Xu (2012). Financial development and innovation: Cross-country evidence. *Journal of Financial Economics, forthcoming*.

Jaffe, A. B. (1986). Technological opportunity and spillovers of R&D: Evidence from firms' patents, profits and market value.

Jaffe, A. B., M. Trajtenberg, and M. S. Fogarty (2000). Knowledge spillovers and patent citations: Evidence from a survey of inventors. *The American Economic Review 90*(2), 215–218.

Jaffe, A. B., M. Trajtenberg, and R. Henderson (1993). Geographic localization of knowledge spillovers as evidenced by patent citations. *The Quarterly Journal of Economics 108*(3), 577–598.

Katz, M. L. (1986). An analysis of cooperative research and development. *The Rand Journal of Economics 7*(3), 527–543.

Mansfield, E. (1986). The R&D tax credit and other technology policy issues. *American Economic Review 76*(2), 190–194.

Manso, G. (2011). Motivating innovation. *Journal of Finance 66*(5), 1823–1860.

Mazerov, M. (2009). A majority of states have now adopted a key corporate tax reform - "combined reporting". *Center on Budget and Policy Priorities Discussion Paper*.

Nanda, R. and M. Rhodes-Kropf (2012). Investment cycles and startup innovation. *Journal of Financial Economics, forthcoming.*

Rao, N. (2013). Do tax credits stimulate R&D spending? The effect of the R&D tax credit in its first decade. *The Effect of the R&D Tax Credit in its First Decade (April 2013).*

Sapra, H., A. Subramanian, and K. Subramanian (2013). Corporate governance and innovation: Theory and evidence. *Journal of Financial and Quantitative Analysis, forthcoming.*

Tian, X. and T. Wang (2011). Tolerance for failure and corporate innovation. *Review of Financial Studies, forthcoming.*

Trajtenberg, M. (1990). A penny for your quotes: patent citations and the value of innovations. *The RAND Journal of Economics 21*(1), 172–187.

Wilson, D. J. (2009). Beggar thy neighbor? The in-state, out-of-state, and aggregate effects of R&D tax credits. *Review of Economics and Statistics 91*(2), 431–436.

Appendix A: Description of Variables

Variable Name	Description
No. of Patents	Total number of patents applied by firm i in financial year t
Adjusted Citations	Total citations received on patents applied adjusted for truncation (as described in Hall et al. (2001, 2005))
Generality Index	The NBER patent dataset consists of about 400 three-digit patent classes, according to USPTO definitions. The generality index is equal to 1-$\sum_{i=j}^{n_j} s_{ij}^2$, where s_{ij}^2 denotes the percentage of citations received by a patent i that belongs to the patent technology class j out of n_i patent classes.
Originality Index	The originality index is similar to generality index, except that its computation relies on the citations *made* rather than citations *received*: hence, it will take a high value if a patent cites other patents that belong to many different fields (high originality).
Tax Increase/Decrease	Dummy variable equal to 1 in the year of tax increase/deduction for the firms headquartered in state s, else zero
Ln(Sales)	Natural logarithm of total sales at 2000 dollars
Ln(1+age)	Natural logarithm of one plus age of the firm,where age is measured as number of years in Compustat
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.
Tangibility	Ratio of net plant, property and equipmen t(*ppent*) to book assets (*at*)
Profitability	Ratio of earnings before interest and taxes (*oibdp*) to turnover/sales (*sale*)
R&D/Sales	Ratio of expense on research and development (*xrd*) to turnover/sales (*sale*)

Appendix A: Description of Variables (Contd.)

Variable Name	Description
Cash Holdings	Cash and marketable securities to total assets
Rating	Dummy variable for firm-years rated by S&P
Log(Real GSP)	Natural logarithm of real gross state product
Real GSP Growth	Growth of gross state product
Budget Surplus (% of GSP)	Budget surplus as a percent of gross state product
Unemployment Rate	Unemployment rate in state as reported by Bureau of Labour Statistics
Democrats have sufficient majority for tax increase in both houses	Dummy variable equal to 1 if both chambers of the legislature have enough democrats to meet a super majority requirement for a tax increase (if there is such a requirement) or the democrats have control of the legislature when there is no such requirement, else zero
No party has sufficient majority for tax increase in both houses	Dummy variable equal to 1 if no party has enough seats and zero, if one party has enough seats in both chambers of the legislature to pass tax increases (whether there is a super majority requirement or not)
Democrats have simple majority in both houses	Dummy variable equal to 1 for democratic control of both chambers, else zero
No party has simple majority in both houses	Dummy variable equal to 1 if neither democrat nor republican have control of both chambers, else zero
Democrats have sufficient majority to pass budget in both houses	Dummy variable equal to 1 if both chambers of the legislature have enough democrats to meet a super majority requirement to pass budget (if there is such a requirement) or the democrats have control of the legislature when there is no such requirement, else zero
No party has sufficient majority to pass budget in both houses	Dummy variable equal to 1 if no party has enough seats and zero, if one party has enough seats in both chambers of the legislature to pass budget (whether there is a super majority requirement or not)
Super majority is not required for tax increase	Dummy variable equal to 1 for state-years in which legislative super majority is not required to increase taxes, else zero

Appendix B: Estimating Economic Magnitudes from Regression Coefficients

Our dependent variable in the baseline regressions in Table 2 is $\Delta Ln(1 + Patents)$. Thus, the regression coefficients on the tax change variables (for example, the coefficient β_I on the tax increase dummy ΔT^{+}_{st}) show how $Ln(1+Patents)$ evolves differently in firms affected by tax changes, compared to unaffected firms. The following section shows how one can back out the corresponding difference-in-difference coefficient on our actual variable of interest, the number of patents, from these observed difference-in-difference coefficients on $Ln(1 + Patents)$.

Take the case of β_I, which is a difference-in-difference coefficient, measuring how patenting activity changes at a firm affected by a tax increase as compared to an identical firm which is not affected:

$$\beta_I = E[\Delta Ln(1 + Patents)/\Delta T^{+}_{st} = 1] - E[\Delta Ln(1 + Patents)/\Delta T^{+}_{st} = 0]$$
$$= E[Ln(\tfrac{1+Patents_{t+1}}{1+Patents_t})/\Delta T^{+}_{st} = 1] - E[Ln(\tfrac{(1+Patents_{t+1})}{(1+Patents_t)})/\Delta T^{+}_{st} = 0]$$

Substituting Z_t=1+$Patents_t$:

$$\beta_I = E[Ln(\tfrac{Z_{t+1}}{Z_t})/\Delta T^{+}_{st} = 1] - E[Ln(\tfrac{Z_{t+1}}{Z_t})/\Delta T^{+}_{st} = 0]$$
$$= E[Ln(1 + \tfrac{\Delta Z_{t+1}}{Z_t})/\Delta T^{+}_{st} = 1] - E[Ln(1 + \tfrac{\Delta Z_{t+1}}{Z_t})/\Delta T^{+}_{st} = 0]$$

Now, consider the expansion of the logarithmic series in the expression $E[Ln(1 + \frac{\Delta Z_{t+1}}{Z_t})]$. This expansion works easily under the assumption $-1 < \frac{\Delta Z_{t+1}}{Z_t} <= 1$. For tax increases, we expect the support of ΔZ_{t+1} to be negative (firms reduce innovation following tax increases), and even if positive, we expect it to be reasonably bounded (even if some firms file for more patents following tax increases, filing does not increase very significantly) which will satisfy the above assumption.[27]

$$E[Ln(1 + \tfrac{\Delta Z_{t+1}}{Z_t})] = E[\tfrac{\Delta Z_{t+1}}{Z_t} - \tfrac{1}{2}(\tfrac{\Delta Z_{t+1}}{Z_t})^2 + \tfrac{1}{3}(\tfrac{\Delta Z_{t+1}}{Z_t})^3 - ..]$$

[27] To get a sense of 'reasonably bounded', even if the number of patents filed by a firm increases by 100%, this assumption will still be valid. For the average firm in our sample that successfully files for 8.44 patents a year, this assumption will be valid as long as it files for fewer than 17.89 patents in the following year.

We can approximate the above expression by ignoring the higher order terms:

$$E[Ln(1+\frac{\Delta Z_{t+1}}{Z_t})] \doteqdot E[\frac{\Delta Z_{t+1}}{Z_t}]$$

$$\text{So, } \beta_I \doteqdot E[\frac{\Delta Z_{t+1}}{Z_t}/\Delta T^+_{st}=1] \text{ - } E[\frac{\Delta Z_{t+1}}{Z_t}/\Delta T^+_{st}=0]$$

Following convention, let us consider the change in the number of patents starting from its sample mean, i.e., when $Z_t = 1+\mu_P$, where μ_P is the sample mean number of patents:

$$\beta_I = E[\frac{\Delta Z_{t+1}}{1+\mu_P}/\Delta T^+_{st}=1] - E[\frac{\Delta Z_{t+1}}{1+\mu_P}/\Delta T^+_{st}=0]$$

$$= \frac{E[\Delta Z_{t+1}/\Delta T^+_{st}=1] - E[\Delta Z_{t+1}/\Delta T^+_{st}=0]}{1+\mu_P}$$

$$= \frac{E[\Delta Patents_{t+1}/\Delta T^+_{st}=1] - E[\Delta Patents_{t+1}/\Delta T^+_{st}=0]}{1+\mu_P}$$

Then, the diff-in-diff coefficient on our variable of interest, the number of patents, is:

$$DD = E[\Delta Patents_{t+1}/\Delta T^+_{st}=1] - E[\Delta Patents_{t+1}/\Delta T^+_{st}=0] = \beta_I(1+\mu_P)$$

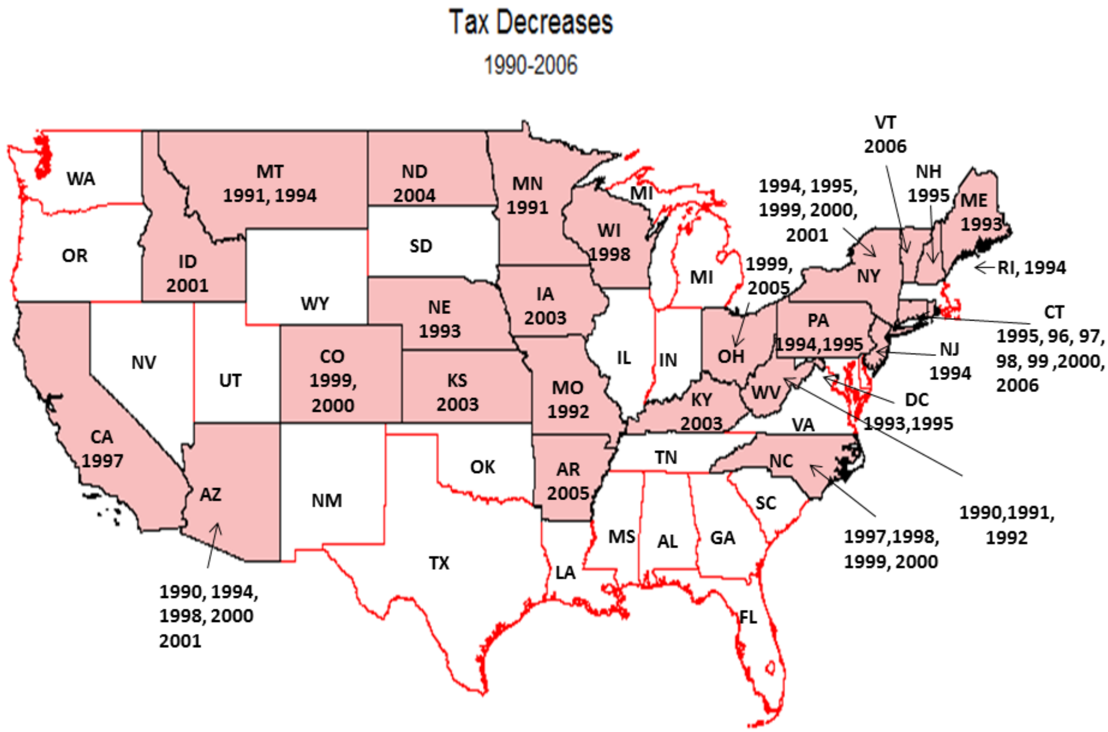
In terms of percentage changes, starting from the sample mean, the tax effect is therefore:

$$\frac{DD}{\mu_P} = \frac{\beta_I(1+\mu_P)}{\mu_P}$$

For example, if the coefficient β_I is -0.051, using the sample mean value of patents, $\mu_P = 9.26$:

$$Percentage\ change\ in\ Patents_{t+1} = \frac{(-0.051)(1+9.26)}{9.26} = -0.0565.$$

Figure 1: Geography of State Corporate Income Tax Changes, 1990-2006



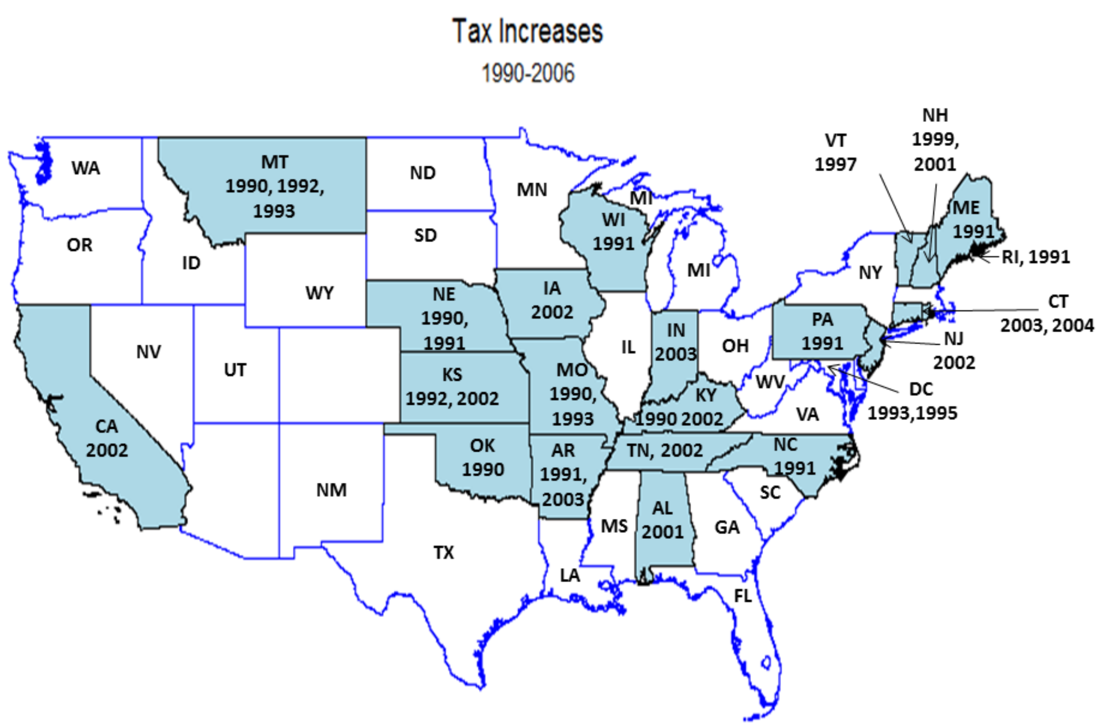
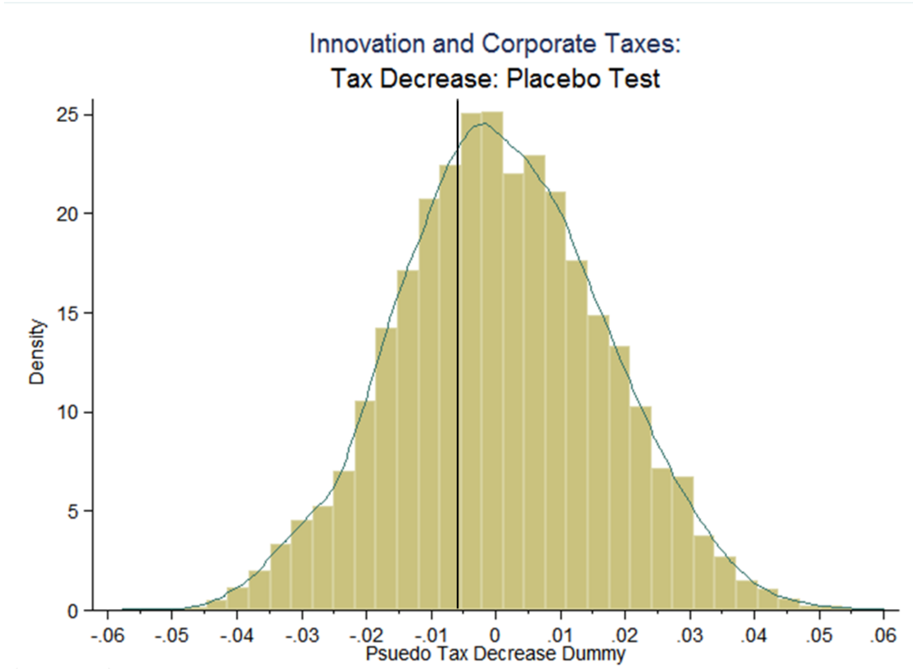
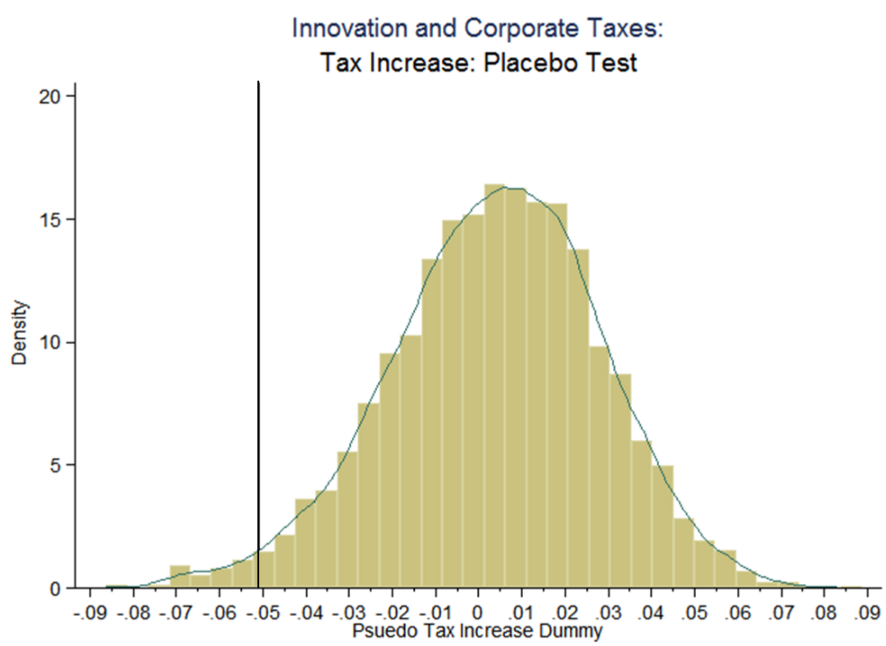


Figure 2: Innovation and Corporate Taxes: Non-parametric distribution of the coefficient on Tax Changes



2A: Tax Decreases



2B: Tax Increases

Table 1: Summary Statistics

This table reports descriptive statistics for our sample firms. All variables are defined in Appendix A. 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 et al., 2001). We focus our analysis on granted patents applied for in the period 1990 to 2006. We match the NBER patent data set with Compustat data following the procedures developed in Hall et al. (2001) and Bessen (2009). We exclude firms in the financial sector (6000s SICs) and the public sector (9000s SICs). We also exclude observations if the firm's sales or assets are less than $1 million, if the firm has a negative stock price, or if the firm has less than four observations (to ensure that we correctly estimate the first difference regression). We only look at the firms with headquarters in the USA. Data on state-level corporate taxes are available from 1990 onwards. This restricts our sample size to 48,448 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 to 2006.

Variables	Obs	Median	Mean	SD
Innovation Variables				
No. of Patents$_{i,t}$	48448	0.00	9.26	74.82
Ln(1+Patents)$_{i,t}$	48448	0.00	0.66	1.18
Adjusted Citations$_{i,t}$	48448	0.00	152.88	1509.50
Adjusted Ln(1+Cites)$_{i,t}$	48448	0.00	1.25	2.15
State Tax Variables				
Tax Rate$_{s,t}$(in %)	867	7.35	6.8	2.93
Tax Rate$_{s,t}$(in %, non-changes)	784	7	6.67	2.97
Δ Tax Rate$_{s,t}$ (in %, non-zero)	83	-0.22	-0.027	1.48
Δ^- Tax Rate$_{s,t}$(in %, non-zero)	51	-0.40	-0.73	1.22
Δ^+ Tax Rate$_{s,t}$(in %, non-zero)	32	.675	1.09	1.13
Firm Characteristics				
Ln(Sales)$_{i,t}$	47349	4.90	4.82	2.47
Ln(K/L)$_{i,t}$	46930	3.38	3.46	1.23
HHI$_{i,t}$	48448	0.13	0.19	0.16
Profitability$_{i,t}$	47259	0.09	-0.63	3.17
Tangibility$_{i,t}$	48411	0.19	0.25	0.21
Cash Holdings$_{i,t}$	48433	0.11	0.22	0.25
Debt Rating(1=yes)$_{i,t}$	48448	0.00	0.21	0.41
R&D/Assets$_{i,t}$	77959	0.00	0.06	0.13
Other State-level Variables				
Log(Real GSP)$_{s,t}$	850	11.9	11.78	1.04
Real GSP Growth$_{s,t}$	850	2.3	2.4	2.68
Unemployment Rate$_{s,t}$	867	5.17	5.1	1.4

Table 2: Patents

This table provides the regression results of the effect of corporate taxes on innovation. In columns (1), (2) and (3), we use actual changes in taxes, while in columns (4), (5) and (6) we partition the changes in to positive and negative changes. In columns (7), (8) and (9) we replace the changes with indicators which allows us to include tax changes that cannot be directly quantified (such as changes in tax surcharges), and estimate the following regression:

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

where i, s, t+k index firms, states, years with $k = 1$ to 3; Ln(1+#Patents)$_{i,s,t+k}$ measures innovation activity by firm i in state s in financial year t. ΔT^-_{st} and ΔT^+_{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. All regressions are with firm fixed effects and year fixed effects. Standard errors are clustered at state level and reported in parentheses. *,**, and *** indicate significance at 10%, 5% and 1% respectively.

	Δ Ln(1+Patents)$_{t+k}$								
	k=1	k=2	k=3	k=1	k=2	k=3	k=1	k=2	k=3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δ Tax Rate$_{s,t}$	-.010	-.022	-.004						
	(.012)	(.007)***	(.009)						
Δ^- Tax Rate$_{s,t}$				-.003	-.0008	-.007			
				(.015)	(.013)	(.022)			
Δ^+ Tax Rate$_{s,t}$				-.005	-.036	-.003			
				(.014)	(.005)***	(.006)			
Tax Decrease$_{s,t}$							.004	-.006	.019
							(.013)	(.010)	(.011)*
Tax Increase$_{s,t}$							-.026	-.051	-.066
							(.018)	(.015)***	(.036)*
Δ Ln(Sales)$_{i,t}$	.021	.005	-.006	.021	.005	-.006	.019	.004	-.004
	(.010)**	(.009)	(.011)	(.010)**	(.009)	(.011)	(.008)**	(.009)	(.010)
Δ Ln(K/L)$_{i,t}$	.005	-.004	-.002	.005	-.003	-.002	.001	-.004	-.006
	(.008)	(.009)	(.008)	(.008)	(.009)	(.008)	(.007)	(.008)	(.006)
Δ R&D/Sales$_{i,t}$	.002	.019	-.007	.002	.019	-.007	-.001	.021	-.006
	(.011)	(.010)*	(.010)	(.011)	(.010)*	(.010)	(.010)	(.009)**	(.009)
Δ HHI$_{i,t}$	-.026	-.166	-.082	-.027	-.167	-.081	-.029	-.208	-.218
	(.195)	(.201)	(.220)	(.195)	(.200)	(.220)	(.188)	(.198)	(.269)
Δ HHI Sq.$_{i,t}$	.087	.347	.095	.086	.347	.094	.116	.368	.259
	(.210)	(.238)	(.205)	(.210)	(.237)	(.205)	(.199)	(.226)	(.248)
Δ Profitability$_{i,t}$	-.002	.0001	.003	-.002	.0002	.003	-.002	.0008	.004
	(.003)	(.002)	(.003)	(.003)	(.002)	(.003)	(.003)	(.002)	(.003)
Δ Tangibility$_{i,t}$	-.048	-.020	.029	-.048	-.020	.029	-.044	-.023	.053
	(.058)	(.047)	(.082)	(.058)	(.047)	(.082)	(.054)	(.035)	(.062)
Δ Cash Holdings$_{i,t}$	.086	-.042	.087	.086	-.042	.087	.078	-.046	.090
	(.023)***	(.043)	(.045)*	(.023)***	(.043)	(.045)*	(.022)***	(.036)	(.040)**
Δ Rating$_{i,t}$	.037	.00003	-.009	.037	.0002	-.009	.032	-.004	-.008
	(.017)**	(.019)	(.021)	(.017)**	(.019)	(.021)	(.016)**	(.019)	(.021)
Δ Log(GSP)$_{s,t}$	.278	-.244	-.051	.270	-.230	-.048	.291	-.230	.006
	(.252)	(.325)	(.268)	(.255)	(.321)	(.265)	(.243)	(.288)	(.255)
Δ Real GSP	-.0002	.003	-.0008	-.0002	.003	-.0008	.0002	.003	-.001
Growth$_{s,t}$	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)
Δ Unemployment	.013	.013	.007	.013	.012	.008	.015	.013	.010
Rate$_{s,t}$	(.013)	(.014)	(.011)	(.013)	(.014)	(.011)	(.012)	(.015)	(.011)
No. of Firms	4323	4280	4064	4323	4280	4064	4331	4296	4087
Obs.	37062	33100	29098	37062	33100	29098	39003	34865	30741

Table 3: Robustness Checks

This table provides further robustness checks to the specification in columns (7)-(9) of Table 2. All regressions include firm level and state level controls, firm fixed effects and year fixed effects, not reported for brevity. Row (1) reports the results for a combined regression with large tax changes (i.e., greater than 1%) and small tax changes (i.e., less than -1%). Row (2) reports results with SIC4 industry-year fixed effects, instead of year fixed effects. Row (3) reports the results with state fixed effects and region-year trend. Row (4) report the results for a regression where we allow for our coefficients to vary by sample periods, 1990 to 1999 and 2000 to 2006. In row (5), we exclude firms located in California and Massachusetts from the sample. Row (6) reports the results for surviving firms from 1990 to 2006. Row (7) reports the treatment effects for firms located in states which suffer first 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 (8) reports the treatment effects for states with no reversals. The firm-years with reversals are dropped from the sample. In row (9) we include list of additional state-level macro variables, i.e. change in state budget surplus, debt outstanding and state tax revenue (as percentage of GSP). Row(10) reports the results when we do not include firm fixed effects. Standard errors are clustered at state level and reported in parentheses. *,**, and *** indicate significance at 10%, 5% and 1% respectively.

	Δ Ln(1+Patents)$_{t+1}$			Δ Ln(1+Patents)$_{t+2}$			Δ Ln(1+Patents)$_{t+3}$		
	Tax Decrease	Tax Increase	Obs.	Tax Decrease	Tax Increase	Obs.	Tax Decrease	Tax Increase	Obs.
(1) Large Tax Changes	.003	-.026	39003	.012	-.092	34865	.010	-.026	30741
	(.035)	(.030)		(.014)	(.022)***		(.029)	(.020)	
Small Tax Changes	.015	-.001		-.021	-.016		-.007	-.027	
	(.013)	(.027)		(.013)	(.026)		(.018)	(.048)	
(2) Industry-Year FEs	-.008	-.012	39003	-.002	-.030	34865	.006	-.054	30741
	(.014)	(.020)		(.014)	(.014)**		(.017)	(.034)	
(3) State FEs and Region Year trend	-.002	-.015	39003	-.009	-.037	34865	.013	-.046	30741
	(.011)	(.017)		(.011)	(.016)**		(.010)	(.021)**	
(4) Regime I: 1990 to 1999	.012	-.014	39003	-.011	-.071	34865	.012	-.025	30741
	(.014)	(.031)		(.012)	(.029)**		(.012)	(.037)	
Regime II: 2000 to 2006	-.019	-.031		.009	-.042		.052	-.091	
	(.015)	(.019)*		(.020)	(.019)**		(.025)**	(.040)**	
(5) Excluding CA and MA	-.007	-.020	28950	-.005	-.044	25994	.011	-.008	23031
	(.012)	(.022)		(.016)	(.022)***		(.012)	(.020)	
(6) Surviving Firms	.009	-.046	15291	-.012	-.088	14321	.043	-.079	13348
	(.019)	(.023)**		(.011)	(.021)***		(.014)***	(.059)	
(7) Reversals	-.0006	.004	35253	-.003	-.080	31427	-.028	-.005	27546
	(.020)	(.030)		(.031)	(.023)***		(.036)	(.045)	
(8) Non-Reversals	.002	-.037	37439	-.009	-.039	33399	.031	-.091	29381
	(.016)	(.017)**		(.010)	(.020)**		(.009)***	(.036)**	
(9) Additional Macro Controls	.003	-.024	39003	-.007	-.052	34865	.021	-.052	30741
	(.013)	(.019)		(.011)	(.016)***		(.011)*	(.029)*	
(10) Without Firm FEs	-.003	-.035	39003	-.008	-.058	34865	.009	-.068	30741
	(.011)	(.018)*		(.009)	(.018)***		(.013)	(.035)*	

Table 4: Coincidental Tax Changes

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. column (7), (8) and (9) in Table 2 with various coincidental tax changes. We control for decreases and increases in R&D tax credit (columns (1)-(3)), personal income tax rate (columns (4)-(6)) and capital gains rate (columns (7)-(9)) in state s at time t, respectively. All regressions include firm level and state level controls, firm fixed effects 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: Description of Coincidental Changes

		Tax Increases	Tax Decreases
Number of corporate tax changes		32	56
..which coincide with			
	increase in state R&D tax credit rate	1	8
	decrease in state R&D tax credit rate	0	1
	increase in state personal income tax rate	14	6
	decrease in state personal income tax rate	2	13
	increase in state capital gains tax rate	13	5
	decrease in state capital gains tax rate	2	14

Panel B: Results

	Δ Ln(1+Patents)$_{t+k}$								
	k=1	k=2	k=3	k=1	k=2	k=3	k=1	k=2	k=3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Tax Decrease$_{s,t}$	.004	-.006	.018	.004	-.006	.019	.004	-.006	.019
	(.012)	(.010)	(.011)*	(.013)	(.011)	(.010)*	(.013)	(.010)	(.011)*
Tax Increase$_{s,t}$	-.026	-.050	-.065	-.027	-.052	-.068	-.027	-.053	-.068
	(.017)	(.015)***	(.036)*	(.018)	(.015)***	(.036)*	(.018)	(.015)***	(.036)*
R& D Tax Credit Decrease$_{s,t}$	-.077	-.086	-.018						
	(.069)	(.029)***	(.038)						
R& D Tax Credit Increase$_{s,t}$	.001	.005	.006						
	(.018)	(.012)	(.012)						
Personal Income Tax Decrease$_{s,t}$				-.006	-.006	-.010			
				(.011)	(.012)	(.012)			
Personal Income Tax Increase$_{s,t}$				.013	.018	.019			
				(.011)	(.013)	(.012)			
Capital Gain Tax Decrease$_{s,t}$							.002	.014	-.008
							(.013)	(.010)	(.014)
Capital Gain Tax Increase$_{s,t}$							.009	.019	.013
							(.010)	(.012)	(.013)
Other Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
No. of Firms	4331	4296	4087	4331	4296	4087	4331	4296	4087
Obs.	39003	34865	30741	39003	34865	30741	39003	34865	30741

Table 5: Predictors of Tax Increase

This table reports the results for an OLS regression of Tax Increase dummy on state partisan balance data, state level economic controls and state and year fixed effects. We estimate the regressions in a system of panel data at state and year level. In column (1), we include macro-economic controls and state and year fixed effects. In column (2), we include state partisan balance variables for tax increase as additional predictors. In column (3) and column (4), we further include variables indicating overall state partisan balance and state partisan balance needed to pass the budget, respectively. Standard errors are clustered at state level reported in parentheses. *,**, and *** indicate significance at 10%, 5% and 1% respectively.

	Tax Increase Dummy			
	(1)	(2)	(3)	(4)
Democrats have sufficient majority for tax increase in both houses		.105 (.036)***	.096 (.043)**	.115 (.044)***
No party has sufficient majority for tax increase in both houses		.066 (.033)**	.044 (.038)	.041 (.040)
Democrats have simple majority in both houses			.009 (.037)	
No party has simple majority in both houses			.025 (.028)	
Democrats have sufficient majority to pass budget in both houses				-.013 (.039)
No party has sufficient majority to pass budget in both houses				.029 (.031)
Taxes as Percent of $GSP_{s,t-1}$	-.026 (.017)	-.022 (.017)	-.022 (.017)	-.023 (.017)
$Log(GSP)_{s,t-1}$	.099 (.103)	.101 (.100)	.100 (.100)	.092 (.099)
Real GSP $Growth_{s,t-1}$	-.001 (.003)	-.002 (.003)	-.002 (.003)	-.002 (.003)
Unemployment $Rate_{s,t-1}$	-.014 (.009)	-.010 (.009)	-.010 (.009)	-.010 (.009)
State FEs	YES	YES	YES	YES
Year FEs	YES	YES	YES	YES
Obs.	827	810	810	810
Adj. R^2	0.04	0.05	0.05	0.05

Table 6: Instrumental Variables Approach

This table reports the results from an instrumental variables (IV) regression. We instrument tax increases using state partisan balance data. For IV1-IV3, we use "Democrats have sufficient majority for tax increase in both houses" as our instrument for tax increases, with different state partisan balance variables and state level economic controls, tax revenue as percent of GSP, logarithm of real GSP, real GSP growth and unemployment rate, from column (2)-(4) of Table 5, respectively. In our IV4, we use "No party has sufficient majority for tax increase in both houses (NPTI)" as an additional instrument for IV3 specification. We estimate the IV regression in a system of panel data regression with firm fixed effects and year fixed effects. This table reports the estimated coefficient for instruments in stage I and the coefficient for the estimated value of tax increase in stage II regression for specification (7)-(9) of Table 2. The second stage regression uses all controls in Table 2, in addition to all regressors included in the first stage, but we only report the coefficients for the predicted tax increase dummy from stage I for brevity. Standard errors are clustered at state level and are adjusted to account for two-stage IV regression, reported in parentheses. *,**, and *** indicate significance at 10%, 5% and 1% respectively.

	Δ Ln(1+Patents)$_{t+k}$											
	IV1			IV2			IV3			IV4		
	(k=1)	(k=2)	(k=3)	(k=1)	(k=2)	(k=3)	(k=1)	(k=2)	(k=3)	(k=1)	(k=2)	(k=3)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Stage I												
Democrats have sufficient majority for tax increase in both houses	.060	.074	.091	.113	.134	.161	.155	.186	.22	.155	.186	.22
	(.026)***	(.028)**	(.031)***	(.048)**	(.049)***	(.053)***	(.05)***	(.057)***	(.062)***	(.053)***	(.057)***	(.062)***
No party has sufficient majority for tax increase in both houses (NPTI)										.025	.043	.066
										(.047)	(.049)	(.052)
Other Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Obs.	35318	31349	27511	35318	31349	27511	35318	31349	27511	35318	31349	27511
Adj. R^2	0.23	0.23	0.24	0.23	0.23	0.24	0.23	0.23	0.24	0.23	0.23	0.24
F-stat	30.66	29.1	39.5	25.3	23.4	32.0	38.0	28.62	39.5	38.0	28.6	39.5
Panel B: Stage II												
Est. Tax Increase$_{s,t}$	-.744	-.70	-.565	.265	.28	-.611	.071	.059	-.573	-.126	.014	-.55
	(.342)**	(.339)***	(.30)*	(.38)	(.27)	(.156)***	(.31)	(.31)	(.151)***	(.22)	(.27)	(.159)***
State partisan controls included		NPTI		NPTI & Overall state partisan balance			NPTI & State partisan balance needed to pass the budget			State partisan balance needed to pass the budget		
Macro controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm and state controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Obs.	35318	31349	27511	35318	31349	27511	35318	31349	27511	35318	31349	27511

Table 7: Marginal Tax Rate

This table provides the results from our baseline specification, i.e. column (7), (8) and (9) of Table 2. We use Blouin et al. (2010)'s simulated marginal tax rates (after interest expense) in period t to partition sample firms into those with marginal tax rates in the bottom 30 percentiles, middle 40 and top 30 percentiles, respectively. All regressions include firm level and state level controls, firm fixed effects and year fixed effects, not reported for brevity. We include $MTR_{i,t}$ as well to control for the level effect of marginal tax rate. Standard errors are clustered at state level and reported in parentheses. *,**, and *** indicate significance at 10%, 5% and 1% respectively.

	Δ Ln(1+Patents)$_{t+k}$		
	(k=1)	(k=2)	(k=3)
	(1)	(2)	(3)
Tax Decrease$_{s,t}$ × MTR$_{i,t}$ Bottom 30	-.002	-.020	-.020
	(.013)	(.018)	(.023)
Tax Decrease$_{s,t}$ × MTR$_{i,t}$ Middle	-.016	-.017	.013
	(.019)	(.019)	(.022)
Tax Decrease$_{s,t}$ × MTR$_{i,t}$ Top 30	.032	.026	.053
	(.015)**	(.016)*	(.019)***
Tax Increase$_{s,t}$ × MTR$_{i,t}$ Bottom 30	-.009	.018	.042
	(.028)	(.021)	(.025)*
Tax Increase$_{s,t}$ × MTR$_{i,t}$ Middle	.003	-.053	-.060
	(.025)	(.038)	(.039)
Tax Increase$_{s,t}$ × MTR$_{i,t}$ Top 30	-.075	-.093	-.161
	(.021)***	(.029)***	(.079)**
Controls	YES	YES	YES
No. of Firms	4320	4262	4064
Obs.	38752	34624	30531

Table 8: Tax Sheltering

This table provides cross-sectional results from our baseline specification in columns (7), (8) and (9) of Table 2. All regressions include firm-level and state-level controls, firm fixed effects and year fixed effects, not reported for brevity. Here, 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. $Combined_s$ is a dummy variable equal to 1 if the firm applying for the patent is located in a state with mandated combined reporting, else zero. Standard errors are clustered at state level and reported in parentheses. *,**, and *** indicate significance at 10%, 5% and 1% respectively.

	Δ Ln(1+Patents)$_{t+k}$		
	(k=1)	(k=2)	(k=3)
	(1)	(2)	(3)
Tax $Decrease_{s,t}$ × $Combined_s$	.016	-.004	.027
	(.032)	(.018)	(.012)**
Tax $Decrease_{s,t}$ × $Non\text{-}combined_s$	-.003	-.007	.017
	(.008)	(.012)	(.016)
Tax $Increase_{s,t}$ × $Combined_s$	-.031	-.059	-.127
	(.021)	(.016)***	(.033)***
Tax $Increase_{s,t}$ × $Non\text{-}combined_s$	-.023	-.044	-.015
	(.024)	(.024)*	(.021)
Controls	YES	YES	YES
No. of Firms	4331	4296	4087
Obs.	39003	34865	30741

Table 9: Location of Operations

This table provides evidence on firm's exposure to tax changes using specifications in columns (7), (8) and (9) in Table 2. Exposure to Tax Increase (or Decrease) is the proportion of 1) employees, 2) assets, or, 3) sales which comes from the state if the state's tax changes during that year and zero otherwise. All regressions include firm-level controls, firm fixed effects and year fixed effects, not reported for brevity. Panel B further partitions exposure to tax decreases and tax increases into those coming from states with and without combined reporting. Exposure to Tax Increase in Combined Reporting States is the proportion of activity coming from states that increased taxes and had combined reporting rules in place. Other exposure variables are analogously constructed. Data period: 1993 to 2005. Standard errors are clustered at firm level and reported in parentheses. *,**, and *** indicate significance at 10%, 5% and 1% respectively.

Panel A: Exposure to Tax Changes

	Δ Ln(1+Patents)$_{t+k}$								
	Prop. of Employees			Prop. of Total Assets			Prop. of Total Sales		
	(k=1)	(k=2)	(k=3)	(k=1)	(k=2)	(k=3)	(k=1)	(k=2)	(k=3)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Exposure to Tax Decrease$_{s,t}$	-.008	-.021	.005	-.005	-.023	.018	-.013	-.018	-.001
	(.019)	(.020)	(.022)	(.022)	(.022)	(.024)	(.018)	(.020)	(.021)
Exposure to Tax Increase$_{s,t}$	-.024	-.053	-.076	-.014	-.029	-.090	-.017	-.058	-.074
	(.034)	(.035)	(.037)**	(.033)	(.033)	(.037)**	(.033)	(.033)*	(.036)**
Firm Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
No. of Firms	2717	2574	2391	2717	2574	2391	2717	2574	2391
Obs.	17143	15035	13014	17143	15035	13014	17143	15035	13014

Panel B: Tax Sheltering and Exposure to Tax Changes

	Δ Ln(1+Patents)$_{t+k}$								
	Prop. of Employees			Prop. of Total Assets			Prop. of Total Sales		
	(k=1)	(k=2)	(k=3)	(k=1)	(k=2)	(k=3)	(k=1)	(k=2)	(k=3)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Exposure to Tax Decreases in Combined Reporting States	-.040	.026	.030	-.007	.047	.047	-.035	.024	.012
	(.038)	(.038)	(.042)	(.044)	(.043)	(.046)	(.037)	(.037)	(.041)
Exposure to Tax Decreases in States without Combined Reporting	.004	-.035	-.012	-.004	-.049	.011	-.007	-.030	-.012
	(.022)	(.025)	(.026)	(.025)	(.026)*	(.027)	(.022)	(.024)	(.025)
Exposure to Tax Increases in Combined Reporting States	-.045	-.083	-.128	-.021	-.047	-.164	-.032	-.085	-.136
	(.046)	(.047)*	(.053)**	(.046)	(.044)	(.053)***	(.045)	(.046)*	(.051)***
Exposure to Tax Increases in States without Combined Reporting	.010	.020	-.013	-.006	-.012	-.004	.011	-.008	-.012
	(.048)	(.047)	(.053)	(.045)	(.047)	(.047)	(.046)	(.045)	(.050)
Firm Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
No. of Firms	2717	2574	2391	2717	2574	2391	2717	2574	2391
Obs.	17143	15035	13014	17143	15035	13014	17143	15035	13014

Table 10: R&D Expenditures

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

$$\Delta \frac{\text{R\&D}}{\text{Assets}} \text{ or } \Delta \text{ Ln(1+R\&D)}_{i,s,t+1} = \beta_D \Delta T^-_{st} + \beta_I \Delta T^+_{st} + \delta \Delta X_{it} + \alpha_t + \gamma_i + \epsilon_{i,s,t+k}$$

where i, s, $t+1$ index firms, states and years. We measure R&D as (1) R&D expenditure (*xrd*) scaled by total assets (*at*) and (2) Ln(1+R&D(*xrd*)). ΔT^-_{st} and ΔT^+_{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. All regressions include firm-level and state-level controls (excluding ΔR&D/Sales$_{i,t}$) and year fixed effects, not reported for brevity. Some specifications also include firm fixed effects. Standard errors are clustered at state level and reported in parentheses. *,**, and *** indicate significance at 10%, 5% and 1% respectively.

	$\Delta \frac{\text{R\&D}}{\text{Assets}}{}_{t+1}$		Δ Ln(1+R&D)$_{t+1}$	
	(1)	(2)	(3)	(4)
Tax Decrease$_{s,t}$	.0027	.0036	-.002	-.001
	(.002)*	(.002)*	(.005)	(.006)
Tax Increase$_{s,t}$	-.0028	-.0028	-.018	-.016
	(.001)**	(.002)*	(.008)**	(.009)*
Other Controls	YES	YES	YES	YES
Firm FEs	NO	YES	NO	YES
No. of Firms	7256	7256	7256	7256
Obs.	64441	64441	64441	64441

Table 11: Innovation Quality

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

$$\Delta \text{ Ln}(1+\# \text{ Citations or } \tfrac{\text{Citations}}{\text{Patents}})_{i,s,t+k} = \beta_D \Delta T^-_{st} + \beta_I \Delta T^+_{st} + \delta \Delta X_{it} + \alpha_t + \gamma_i + \epsilon_{i,s,t+k}$$

where i, s, $t+k$ index firms, states, years with k = 1 to 3; Ln (1+#Citations or $\frac{\text{Citations}}{\text{Patents}})_{i,s,t+k}$ measures quality of innovation activity by firm i in state s in financial year t. ΔT^-_{st} and ΔT^+_{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. All regressions include firm-level and state-level controls, and year fixed effects, not reported for brevity. Col(4)-(6) include firm fixed effects as well, 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: Citations

	Δ Ln(1+Citations)$_{t+k}$					
	k=1	k=2	k=3	k=1	k=2	k=3
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Decrease$_{s,t}$	.003	.009	.014	.017	.018	.030
	(.029)	(.029)	(.038)	(.031)	(.037)	(.045)
Tax Increase$_{s,t}$	-.094	-.163	-.061	-.076	-.152	-.046
	(.056)*	(.061)***	(.056)	(.055)	(.056)***	(.057)
Other Controls	YES	YES	YES	YES	YES	YES
Firm FEs	NO	NO	NO	YES	YES	YES
No. of Firms	4331	4296	4087	4331	4296	4087
Obs.	39003	34865	30741	39003	34865	30741

Panel B: $\frac{\textbf{Citations}}{\textbf{Patents}}$

	Δ Ln(1+$\frac{\text{Citations}}{\text{Patents}}$)$_{t+k}$					
	k=1	k=2	k=3	k=1	k=2	k=3
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Decrease$_{s,t}$	.004	.015	-.003	.012	.021	-.002
	(.023)	(.024)	(.028)	(.026)	(.035)	(.035)
Tax Increase$_{s,t}$	-.044	-.097	-.022	-.030	-.090	-.011
	(.033)	(.041)**	(.028)	(.032)	(.040)**	(.025)
Other Controls	YES	YES	YES	YES	YES	YES
Firm FEs	NO	NO	NO	YES	YES	YES
No. of Firms	4331	4296	4087	4331	4296	4087
Obs.	39003	34865	30741	39003	34865	30741

Table 12: Firm Rating and Leverage

This table provides evidence on how firm's ability to change, and actual changes in leverage following tax changes impacts future innovation. Panel A of this table provides results of the effect of tax changes on innovation activity for the firms that have access to bond market i.e. have S&P bond rating, vs. those that do not have it. Rated is a dummy variable for firm-years rated by S&P. Panel B of 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. Δ $BL_{i,t+1}$ > (<) 0 is a dummy variable equal to 1 if firm increases (decreases) book leverage in period t+1, else zero. We include Δ $BL_{i,t+1}$ as well to control for the level effect of leverage. All regressions include controls, firm fixed effects 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: Firm Rating

	Δ Ln(1+Patents)$_{t+k}$		
	(k=2)	(k=3)	(k=4)
	(1)	(2)	(3)
Tax Decrease$_{s,t}$ × Rated$_{i,t}$	.042	-.010	.061
	(.039)	(.035)	(.032)*
Tax Decrease$_{s,t}$ × Non-Rated$_{i,t}$	-.009	-.005	.005
	(.021)	(.015)	(.017)
Tax Increase$_{s,t}$ × Rated$_{i,t}$	-.067	-.142	-.182
	(.030)**	(.051)***	(.092)**
Tax Increase$_{s,t}$ × Non-Rated$_{i,t}$	-.014	-.022	-.030
	(.023)	(.016)	(.034)
Other Controls	YES	YES	YES
No. of Firms	4129	3930	3666
Obs.	39003	34865	30741

Panel B: Leverage

	Δ Ln(1+Patents)$_{t+k}$		
	(k=2)	(k=3)	(k=4)
	(1)	(2)	(3)
Tax Decrease$_{s,t}$ × Δ $BL_{i,t+1}$ <0	.006	.009	.042
	(.017)	(.016)	(.014)***
Tax Decrease$_{s,t}$ × Δ $BL_{i,t+1}$≥0	-.013	.013	.006
	(.015)	(.011)	(.024)
Tax Increase$_{s,t}$ × Δ $BL_{i,t+1}$ >0	-.080	-.086	-.024
	(.031)**	(.035)**	(.054)
Tax Increase$_{s,t}$ × Δ $BL_{i,t+1}$≤0	-.036	-.079	-.043
	(.017)**	(.049)	(.039)
Other Controls	YES	YES	YES
No. of Firms	4129	3930	3666
Obs.	32086	28444	24916

Table 13: New Hires and Leavers

This table examines the effect of taxes on new inventor hires and leavers. New 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. Leavers refer to the number of the inventors who have produced a patent at the sample firm within one past year but produce at least one patent at a different firm. We use Ln(1+New Hires)$_{t+k}$ and Ln(1+Leavers)$_{t+k}$ as dependent variable for k = 1 to 5 and run the following regression.

$$\text{Ln(1+New Hires or Leavers)}_{i,s,t+k}=\beta_D \Delta T^{-}_{st} + \beta_I \Delta T^{+}_{st} + \delta \Delta X_{it} + +\alpha_i + \alpha_t + \epsilon_{i,s,t+k}$$

where i, s, t index firms, states, years and k = 1 to 5; ΔT^{-}_{st} and ΔT^{+}_{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 affects inventors. All regressions are with firm fixed effects and year fixed effects. Standard errors are clustered at state level and reported in parentheses. *,**, and *** indicate significance at 10%, 5% and 1% respectively.

	Ln(1+New Hires)$_{t+k}$					Ln(1+Leavers)$_{t+k}$				
	(k=1)	(k=2)	(k=3)	(k=4)	(k=5)	(k=1)	(k=2)	(k=3)	(k=4)	(k=5)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Tax Decrease$_{s,t}$	.024	.016	.046	-.035	-.009					
	(.028)	(.025)	(.033)	(.028)	(.034)					
Tax Increase$_{s,t}$						.062	.092	-.013	.014	.036
						(.041)	(.041)**	(.056)	(.063)	(.058)
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
No. of Firms	1310	1234	1141	1030	928	1299	1219	1122	1013	915
Obs.	7069	6332	5602	4903	4248	7062	6323	5589	4896	4234