Do Corporate Taxes Hinder Innovation?

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Abstract

We exploit staggered changes in state-level corporate tax rates to show that an increase in taxes reduces future innovation. Our evidence, which also exploits policy discontinuity at contiguous counties straddling state borders, shows that local economic conditions do not drive our results. The effect we document is consistent across the innovation spectrum: taxes affect not only patenting and R&D investment but also new product introductions, which we measure using textual analysis. In the cross-section, we document that the tax effect is stronger among firms that face higher marginal tax rates, those that have a higher proportion of operations in states that change taxes, and those that are located in states which make shifting profits out of the state for tax reasons more difficult. Finally, we examine potential channels, and find that our empirical results are particularly consistent with models that highlight the role of higher corporate taxes in reducing innovator incentives and discouraging risk-taking.

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Introduction

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 at loggerheads? Does changing corporate tax policy also affect future firm innovation? Such debates have become particularly prominent today when many governments face 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...*"

Indeed, there are many possible reasons why corporate taxes may matter for future innovation. For one, macroeconomic models of growth through endogenous technological progress suggest that corporate taxes affect growth through their effect on innovation activity. For instance, Jaimovich and Rebelo (2012) show that the decline in after-tax profits from innovation projects following tax hikes can lead innovators to reduce or redirect effort, affecting aggregate innovation activity. Alternatively, any increase in taxes which accentuates the progressivity of the tax schedule might discourage taking innovative projects, which are typically more risky (Gentry and Hubbard (2000)). In addition, a tax hike may raise the attractiveness of debt to firms (Heider and Ljungqvist (2014)), which in turn is not the favored form of financing for innovation. Moreover, taxes might also lower internal cash flows that have been shown to be a major source of financing for innovation activities (e.g. Himmelberg and Petersen (1994)).

However, given the tax deductibility of R&D expenditures and the existence of R&D tax credits, as well as the plethora of sophisticated tax avoidance strategies that some firms adopt, many policy makers doubt whether raising the corporate tax rate actually hinders innovation activity in the real world.

In this paper we provide empirical evidence on the consequences of corporate income tax changes for future innovative activities of affected firms. As our source of identification, we consider staggered corporate income tax changes at the US state level. Unlike federal tax changes that 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 firm innovation. 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. In terms of economic magnitude, a 1.5 percentage point increase in the state corporate income tax rate from a mean value of around 7% (a one standard deviation change) leads to approximately 37% of affected firms to file one fewer patent within the next two years, compared to a mean of about 9.1 patents per firm-year. When we consider tax increases and decreases separately, we find that most of this effect comes from increases.

Importantly, we also find that the decline in innovation is not limited to patenting activity – the drop in patenting is accompanied by a decline in R&D investment as well as a decline in final innovation output, new product introductions, which we measure using a novel textual analysis-based metric. These findings, taken together, imply that the effect of corporate taxes seems to pervade all the stages of innovation.

Our causal interpretation of the difference-in-difference estimates requires that, conditional on the controls we use, our treated and non-treated firms are not systematically different. For example, one issue for our analysis might be that states change tax rates based on local economic conditions, which might directly affect a firm's incentives to innovate, aside from its indirect effect through taxes. We perform a series of tests to address such concerns carefully. First, our results are robust to many perturbations, including the addition of region-year fixed effects, and importantly, other coincidental tax changes. Second, we create pseudo corporate tax change samples, where for each state experiencing a tax change we randomly select a state that borders it, and assign the change to the bordering state. We repeat this exercise 5000 times for both tax increases and decreases, and compare the observed coefficients for tax changes in our baseline test with those from these randomized placebo samples. Again, we find evidence consistent with corporate tax changes having an effect not explained by local economic conditions.

Finally, we perform a stringent test to rule out local economic condition effects contaminating our evidence, based on policy discontinuity at state borders. In particular, we confine our analysis to firms located in contiguous counties on two sides of a state border. In this test, we directly net out the effects of local economic conditions non-parametrically by controlling for county-pair-year fixed effects. Our results show that the average firm that faces a tax rise files approximately one fewer patent two years following the increase, as compared to its neighbor on the other side of the state border that is not affected by any tax change.

In order to refine our analysis further, we use cross-sectional variation in terms of exposure to tax changes. First, we use simulated marginal tax rates (Blouin, Core, and Guay (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 smaller number of patents following tax increases. Next, we exploit firm-level variation in exposure to state tax changes, based on the degree of operations that the firm has in that state and the state's formula for apportioning corporate income for taxes. We document that the tax effect is indeed stronger among firms that have a higher exposure in states that change taxes. Finally, we find that most of the effect comes from firms that are more constrained in their ability to avoid taxes. In particular, our evidence shows that the effect is stronger 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.

In the final section of the paper we return to the theoretical reasons, described above, behind the tax effect we document. First, we present empirical predictions relevant for our analysis from a general equilibrium model of endogenous growth with taxes. Specifically, we use an extension of Romer (1990)'s endogenous growth model with taxes and occupational choice, following the structure of Jaimovich and Rebelo (2012). In the model, corporate tax changes induce less innovation due to potential innovators choosing to shift to less innovative activities. Intuitively, such an effect arises because innovator compensation (as opposed to worker pay) is more sensitive to corporate taxes in equilibrium. To provide evidence on this channel, we look at the firm's innovative personnel. We examine movements of innovators across firms by using a rich database of patent assignee identities from the Harvard Business School Patent Network Dataverse, and find that tax increases lead to a significant number of inventors parting with their employers. Given labor market frictions, the movements of innovative personnel can also explain the asymmetry in our findings: indeed, we find that tax increases lead to an increase in inventor turnover in two years following the change, while the effect takes much longer to show up following tax cuts. This is consistent with the view that while firms may be quick to lose their innovative inputs, they may need a longer period of time to build the knowledge, workforce and capacity required to innovate.

Second, we examine the conjecture that an increase in tax progressivity (which is how most of the tax changes are implemented) reduces incentives for firms to undertake innovation projects, particularly if the innovation projects are risky. We present a stylized model to clarify this intuition, in the setting of Gentry and Hubbard (2000). Next, we show that – consistent with the model's prediction – there is a systematic decline in risky innovation projects undertaken by firms following tax increases.

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

Our results contribute to a few strands of the literature. First, we relate to the literature on corporate tax effects on investment, productivity and economic growth that started with Jorgenson (1963) and Hall and Jorgenson (1967). These theoretical models largely argued that if investment is tax deductible (through R&D expenses or future depreciation), an increase in the corporate tax rate should not have a first-order effect on firm's investment decisions. On the other hand, some theory models (e.g., Levine (1991)) argue that corporate taxes can affect investment and growth. Many empirical studies have also found a negative effect of corporate taxes on investment (e.g. Auerbach and Hassett (1992), Cummins, Hassett, and Hubbard (1996), Cullen and Gordon (2007) and Djankov, Ganser, McLiesh, Ramalho, and Shleifer (2010)). We show how one particular type of investment that is critical for firm growth – investment in innovative activities – gets affected by changes in corporate taxes.

A separate branch of this literature has looked at R&D tax credits and has largely established that such tax credits have a significant influence on R&D investment (e.g. Mansfield (1986), Jaffe (1986), Katz (1986), Grossman and Helpman (1991), Aghion and Howitt (1992), Jaffe, Trajtenberg, and Henderson (1993), Bloom, Griffith, and Van Reenen (2002), Wilson (2009), Branstetter and Sakakibara (2002) and Rao (2013)). Surprisingly, however, the causal effect of general corporate tax policies on innovation – which has often been at the forefront of recent policy discourse – has not drawn enough attention.

Moreover, this literature has focused on R&D spending and has not examined innovation outputs such as patents or new products. Examining innovation outputs is particularly valuable in the context of taxation for a few reasons. First, what tax authorities treat as R&D often differs from what firms consider as productive innovation inputs. Many firms get involved in legal investigations with Internal Revenue Service as they disagree on what can be expensed as R&D for R&D tax credit purposes. On the other hand, firms are also known to sometimes re-label other costs as R&D for the purposes of tax credits (Griffith (1996)). This makes it difficult to ascertain whether the R&D spending response to tax changes indeed reflects changes in productive innovation inputs or is simply a re-labeling of other expenditure for tax purposes. Finally, firms differ widely in the productivity of their R&D investments (Hirshleifer, Hsu, and Li (2013)) and these differences in innovative efficiency translate into differences in future sales (Cohen, Diether, and Malloy (2013)). Therefore, although we also provide evidence on the impact of corporate taxes on R&D, we believe that our examination of patenting activity and new product introductions add significant new insights on the impact of fiscal instruments on innovation.

To the best of our knowledge, the only other paper that examines fiscal policy effects on patenting is a contemporaneous working paper by Atanassov and Liu (2014). Their main finding that corporate taxes hurt innovative activities is consistent with ours. However, our analysis shows a stronger effect coming from tax increases, while Atanassov and Liu (2014) find that tax cuts matter more. This contrast in findings is caused by the different estimation methodologies we employ (we specify our regression model in first differences while Atanassov and Liu (2014) estimate their model in levels) as well as our list of tax changes being more comprehensive (for instance, Atanassov and Liu (2014) do not consider many changes in taxes – even large changes with more than 1% impact on firm tax bills – that we do).

Moreover, we also contribute to the literature that discusses the determinants of innovation. Empirical evidence shows that product market competition (Aghion, Bloom, Blundell, Griffith, and Howitt (2005)), institutional ownership (Aghion, Van Reenen, and Zingales (2013)), presence of financial analysts (He and Tian (2013)), investors' attitudes towards failure (Tian and Wang (2014)), financial development (Hsu, Tian, and Xu (2014)), labor laws (Acharya and Subramanian (2009); Acharya, Baghai, and Subramanian (2014); Acharya, Baghai, and Subramanian (2013)), managerial contracts (Manso (2011)), stock liquidity (Fang, Tian, and Tice (2014)), investment cycles in financial markets (Nanda and Rhodes-Kropf (2012)), banking deregulation (Amore, Schneider, and Žaldokas (2013); Chava, Oettl, Subramanian, and Subramanian (2013); Cornaggia, Mao, Tian, and Wolfe (2015)), private rather than public ownership

(Ferreira, Manso, and Silva (2014)) all affect innovation.[1] We contribute to this literature by showing that corporate taxes are a first order determinant.

In addition, our paper also relates to the effects of corporate tax changes on corporate policies (Graham (2006); Blouin, Core, and Guay (2010)). Recently, Asker, Farre-Mensa, and Ljungqvist (2014) and Heider and Ljungqvist (2014) have shown that state corporate tax changes affect firms' after-tax returns on investment and capital structure, Doidge and Dyck (2015) have shown that firms adjust leverage, payout, cash holdings, and investment in response to changing tax incentives in Canada, while Faulkender and Smith (2014) have provided evidence that multinational firms have higher leverage ratios when they operate in countries with higher tax rates.

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

Our focus in this paper is to examine the effect of corporate tax changes on the outputs of the innovation process, which we primarily measure using successful patent applications (Griliches (1998)). The patent data set used in our analysis is assembled by the National Bureau of Economic Research (NBER), which contains information on all patents awarded by the US Patent and Trademark Office (USPTO) as well as citations made to these patents (Hall, Jaffe, and Trajtenberg (2001)).

We match the NBER patent data set with Compustat data following the procedures developed in Hall, Jaffe, and Trajtenberg (2001) and Bessen (2009). Historical analysis of state tax changes requires the correct identification of the state that taxed a firm's profits in each year.

[1] Chemmanur and Fulghieri (2014) contains a recent summary of this literature.

We primarily rely on a firm's headquarter state. Since Compustat reports the address of a firm's current principal executive office, not its historic headquarter location, we obtain (time-varying) firm location information from 10-K filings. In particular, we use the business address of the firm to identify the location of its state.[2] Due to constraints on the joint availability of tax, business address location, and patent data, we focus our analysis on granted patents applied for in the period 1990 to 2006.

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

We rely on the Bureau of Labor Statistics for the state-level macroeconomic controls. Moreover, to capture innovator employment effects we use patent assignee data from Harvard Business School Patent Network Dataverse. We also use simulated firm-level marginal tax rates from Blouin, Core, and Guay (2010) and data on state R&D tax credits from Wilson (2009).

In addition, in order to gauge firm's operations across different states, we also exploit rich data on employment, sales, and assets at the parent-subsidiary level from Lexis-Nexis' Corporate Affiliations database. This database contains the list of subsidiaries for all major publicly traded companies with US headquarters since 1994, and currently provides data on more than 6,881 US public parents and 154,247 subsidiaries.

Finally, we hand-construct a database of major new product announcements by a textual search of the LexisNexis News database for company press releases, followed by an analysis of

[2]We thank Alexander Ljungqvist for this data. For a detailed description of this data, please see Heider and Ljungqvist (2014). Our results are not altered if we consider state locations based on the state name counts in 10-K forms as in Garcia and Norli (2012).

equity returns (from CRSP) around the announcement day.

The detailed construction of all variables is described in Appendix A.1, while summary statistics for our key variables are reported in Table 1.

Table 1 shows that corporate profit taxes constitute a substantial expense for our firms, with the average (median) top marginal state tax rate in our sample being 6.8% (7.35%). During our sample period there were 32 instances of state tax increases, spread across 20 states, and 51 instances of tax cuts, spread across 24 states. Figure 1 identifies these changes on the US map. The average state corporate tax increase in our sample is 1.09%, while the average corporate tax cut is 0.73%. The list of tax changes that we use in the baseline specifications comes from Heider and Ljungqvist (2014). As the authors mention, the data in Heider and Ljungqvist (2014) is originally obtained from the Tax Foundation, the Book of the States, the Current Corporate Income Tax Developments feature in the Journal of State Taxation, and state codes accessed through Lexis-Nexis.[3] We reconfirm these tax changes by reading through the historical archives of Fiscal Surveys of States, published semi-annually by National Governors Association and National Association of State Budget Officers.[4]

2 Methodology

We use a difference-in-differences 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. For instance, when Pennsylvania raised its top corporate income tax rate in 1991 from 8.5% to 12.25%, we intend to examine how the number of successful patent applications filed by firms headquartered in Pennsylvania changed in the years following this increase, as compared to a group of firms – otherwise identical –

[3]Heider and Ljungqvist (2014) contains more details on these changes and the structure of US state taxes.

[4]Historical archives are available at http://www.nasbo.org/publications-data/fiscal-survey-of-the-states/historical-archives.

but headquartered in, say, New Jersey, where the tax rate was unchanged in that year. 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.

In our approach, we follow Heider and Ljungqvist (2014) and estimate our specification after taking first differences of all variables to control for firm-level unobserved heterogeneity. As our main variables of interest are tax *changes*, so it is natural to run the first difference specification, which regresses the *change* in the dependent variable on *changes* in all independent variables. Also, this has the considerable advantage that we do not have to reset the tax change dummy variables to zero every time a state reverses a change. The latter is problematic since it implicitly imposes symmetry on the effects of a tax increase and its subsequent reversal. We use $Ln(1 + \#Patents)$ as the primary measure of a firm's innovative activities. Our baseline specification is then:

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

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

Our main variables of interest are ΔT^+_{st} and ΔT^-_{st}. These are indicators equaling one if state s increased (respectively, decreased) its corporate tax rate in year t. Following Heider and Ljungqvist (2014), we lump all tax changes together by focusing on binary tax change indicators in our baseline specification for two reasons. First, some tax increases (e.g., suspension of net operating loss deductions by California in 2002) cannot be easily quantified in terms of changes in marginal tax rates, though their directional effects are unambiguous. Second, many of the tax changes apply to different provisions of the tax code (e.g., reduction in federal income tax deductibility from 100% to 50% by Missouri in 1993). We also provide specifications where we use the actual percentage point change in top marginal corporate income tax rate as a measure of ΔT.

As controls, we 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, Van Reenen, and Zingales (2013)). We include other lagged controls such as change in profitability, asset tangibility, 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 to control for the impact of industry concentration on innovation (as well as its square term to account for non-linearities). Since US patenting activity has increased substantially starting in the mid-1980s (e.g., Hall (2004)), we control for aggregate trends by including year fixed effects.

Additionally, since our main variable of interest is the change in state taxes, we also incorporate state level economic indicators, namely, the change in the Gross State Product (GSP), the change in the tax revenue of state as proportion of GSP, the change in the state's population (in logs) and the change in the state's unemployment rate. Finally, since our tax treatment is defined at the state level, we cluster standard errors by state, following Bertrand, Duflo, and Mullainathan (2004).

3 Empirical Results

3.1 Main Results

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

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

Next, we focus on a multivariate regression setting, which allows us to control for differences among firms in innovative behavior unrelated to state tax changes. Our results in Table 2 provide evidence that tax increases reduce future innovation. In the first three columns, we look at the actual changes in tax rates as our main explanatory variables. Our evidence shows that the number of patents gets affected in the two years following the tax change. In particular, a 1.5 percentage point increase in the state corporate income tax rate (a one standard deviation change) leads to a 4.1% (coefficient in Table 2, column (2), 0.027, times 1.5) decline in a number of patents granted to the state's firms in the two years following the tax change. In terms of economic magnitude, this means that our average firm obtains 0.37 fewer patents (9.11 times 0.041) by the second year following a tax change. Since the number of patent grants has to be an integer, it is, perhaps, more reasonable to discuss economic magnitudes in terms of the fraction of firms that changed patenting activity. Going by this metric, the economic magnitude means that a 1.5 percentage point tax increase causes approximately 37% of affected firms to patent one fewer innovation project, compared to a mean of about 9.11 patents per firm-year. Our estimated effect implies an elasticity of patents to corporate taxes of 0.6.[5]

In columns (4)-(6), we split tax changes into increases and decreases and find that most of our effect comes from tax increases rather than tax cuts. In terms of economic magnitude, in the second year following a tax increase – which raises corporate taxes by around 1.1 percentage

[5]From a sample mean effective total corporate tax rate of about 22.44% (federal plus state), a tax increase of 1 percentage point raises rates by 4.5%, relative to the base rate. So, the elasticity is 0.027/0.045=0.6.

points – approximately 37% of treated firms patent one fewer innovation project, while there is no significant effect after tax decreases.[6] In columns (7)-(9), we construct binary indicators for tax increases and decreases, as described in Section 2, and show that the average treated firm files one fewer patent following a tax increase, as compared to an average control firm (tax increases are followed in the next two years by a decline of (0.055+0.053) times 9.11 = 0.98 patents per treated firm). Again, we do not find any systematic effect following tax cuts. Finally, we report effects only in the three years following the tax changes, since we do not find any significant results in later years.[7]

There could be two potential statistical issues behind our tax decrease effects being insignificant in most of our specifications. First, the average tax increase is about 50% larger than the average tax cut. However, even when we condition on large changes in the next section, we still cannot identify significant effects following tax cuts, making this an unlikely explanation. Second, firms may be able to cut innovation projects more quickly following tax increases, but they might take longer to set up innovative capacity and hire new employees after a tax cut. We show some evidence consistent with this view using other measures of innovation, such as new product introductions, reported in later sections. Such a longer response time of tax decreases might make it harder to tease out clean causal effects. By the time higher effort or investment following these cuts starts bearing fruit, other potentially confounding effects that interfere with innovation might also kick in. Such noise could lead to a reduction in the magnitude and significance of the tax decrease effect we identify.

Finally, in the set-up we examine, the variation in tax rates is bounded within a relatively narrow range (up or down by a few percentage points at most). While this set-up buys us the opportunity to cleanly identify our effects, one has to exercise caution in extending our findings

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

[7] Since our dependent variable is measured as a change, this does not, however, mean that innovation activity recovers after three years.

to other countries or contexts. Dramatic tax cuts, for example, might indeed have an effect on future innovation: we cannot necessarily predict from our analysis how firms will behave when faced with such changes.

3.2 Robustness

In this section we consider various refinements of, and address potential concerns with, our baseline specification (Table 2, columns (7)-(9)). We report the results in Table 3.

First, our baseline specification does not distinguish between large changes affecting the effective top marginal tax rates, and other changes that affect the tax bill, for example, reduction in federal income tax deductibility. In row (1) of Table 3, we separately consider large tax changes (more than 1% increases and decreases) and the other tax changes.[8] Here we re-estimate our baseline regression, replacing the tax increase and decrease dummies with four variables – large tax increases (average increase 2.95%), other tax increases (covers all other changes to the tax bill), large tax decreases (average decrease -2.11%), and other tax decreases. Our results show that future innovation is affected mostly by large tax increases.

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

Next, one might be concerned that the effect of taxes on innovation might have been specific to a certain period. We obtain separate estimates of tax change effects in the 1990-1999 period from the tax changes in the 2000-2006 period, and find the presence of tax effects on

[8] Our results are not sensitive to the actual cut-off point.

innovation in both periods (row (4)). Interestingly, here we also see a statistically significant – but economically small – increase in patenting following tax cuts in the more recent sub-period.

The next concern we address is whether our results are affected by a lot of innovative firms getting acquired. For instance, it could be the case that tax increases were followed by some innovative firms being acquired because, say, they were so far unprofitable, and thus could reduce the tax bill of the acquirer. To address this issue, we use a sample of firms that survive till the end of our sample. Results presented in row (5) show the same pattern as our main results.

In row (6) we look at tax increases that were reversed later. The firm-years with tax changes that did not get reversed are dropped from these tests to ensure the validity of our control sample. We still find that innovation declines following a tax increase. In row (7) we look at the treatment effects for states with no reversals. The firm-years with reversals are dropped from the sample. Again, we find a decline in the number of patents following tax increases, but now we also find an increase in the number of patents following tax cuts. This time the effect following tax cuts is statistically significant and economically non-negligible, although again smaller than the effect following tax hikes – 28% of firms facing such a tax cut file for one more patent successfully in the third year after the cut (0.031, the coefficient in Table 3, row (7), column (7), times the sample average number of patents, 9.11 = 0.28). Note that such analyses (rows (6) and (7)), while interesting, might have a look-ahead bias. In most cases, firms would not have known with certainty at the time of the tax change whether it would be reversed later or not. Ex ante, it is difficult to predict whether a tax change will last – even changes declared to be temporary can keep getting renewed and eventually last extended periods of time, e.g., Connecticut has had a "temporary" tax surcharge for over a decade.

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

In the next row (10), we examine firms that are purely domestic. This alleviates concerns regarding sophisticated tax avoidance strategies adopted by firms exploiting various international treaties and tax haven countries (Graham and Tucker (2006)), as well as concerns regarding the effective tax rates facing firms being different depending on the foreign country rates where it operates (Faulkender and Smith (2014)). We define domestic firms as those that do not have any foreign income (using those that do not pay foreign taxes produces similar results). Our effects are marginally stronger in this sample. Finally, in the row (11), we drop all controls, to allay concerns that some of our control variables are choice variables for individual firms, and again find a similar pattern.

In unreported results (available on request), 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 firms that file non-zero number of patents in both periods, we find consistent results. Our results are also robust if we use Compustat headquarter information to identify firm's state, if we use the state name counts in 10-K forms (Garcia and Norli (2012)), or we use the state where the highest proportion of firm's employees is located. Finally, while firms may strategically change their state of operation to avoid tax increases, our results also hold for the firms that do not change their state during our entire sample period.

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

3.3 Potential Confounding Factors

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. First, some states could be more innovative than others, and they might also differ in the innovation incentives that they provide to firms, making it important to account for potential state-specific unobserved heterogeneity. Second, there could broad regional trends in innovation activity.[9] Finally, state legislatures can change multiple laws at the same time. The most likely candidate for such an omitted variable is some coincident change in a different fiscal policy instrument, which might also affect innovation, e.g. R&D tax credit, tax on wages, and capital gains taxes.[10] In this section, we show that the effect of corporate income taxes on innovation survives if we explicitly account for all of these candidate confounding factors.

Our analysis in Panel A of Table 4 shows that there is little tendency for states to change R&D tax credits, top tax rates on wage income, and capital gains tax rates on long gains at the same time as the corporate income tax rates. Consider the R&D tax credit: the table shows that only 8 out of our 51 instances of state corporate income tax cuts coincided with an increase in the R&D tax credit.

We explicitly account for potential confounding factors in Panel B of Table 4. To account for unobserved, time-invariant, state-level heterogeneity, we incorporate state fixed effects. Moreover, although we cannot control for state-year trends (since our independent variable of interest is a state-year level variable), we control for the possibility of differential regional trends in innovation by incorporating region-year fixed effects in our specification. Finally, we explicitly account for changes in the R&D tax credit,and changes in personal income taxes (measured by a change in either tax on wages, or change in capital gains taxes) in the firm's state by using increase and decrease dummies similar to our corporate tax change variables ΔT^{+}_{st} and ΔT^{-}_{st}.[11]

The evidence presented in the table shows that the significance of corporate tax change variables remains virtually unaltered when we account for these potential confounds, which

[9]One reason for this might be that states in the same region follow similar economic policies or are exposed to similar economic conditions.

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

[11]We do not consider these components of personal income tax separately as there is a very high overlap between these tax changes. Our results on corporate taxes remain virtually unchanged even if we do.

shows that corporate taxes have a distinctly different effect on future innovation. The R&D tax credit changes 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 any significant effect. Personal income tax changes are not significantly related to future innovation.

Besides these tests, we also employ an instrumental variable approach to address concerns regarding potentially unobservable local economic conditions that might be correlated with the tax changes. We discuss our methodology and present our results in Appendix A.2. In particular, we rely on state-level differences (and changes in them) in super-majority requirements to pass a tax increase, as well as its interaction with state political balance, as our instruments. As it turns out, predicting tax changes even using a long list of variables is difficult, as the F-statistics for the joint significance of even our most significant variables is less than 5 (R-squares have a similar low magnitude of 10-12% both in our tests and in similar tests in Heider and Ljungqvist (2014)). Still, even after accounting for this, our results in the appendix are consistent with the basic conclusions from our baseline specifications.

3.4 Local Economic Effects

One potential concern with our analysis is that changes in local economic conditions might independently affect firm innovation, and might be correlated with tax changes. This might result in the treatment effect we identify incorrectly reflecting the response to underlying economic changes that was coincident with – or might even have caused – the tax change, rather than the tax change itself. In this section, we design tests to precisely disentangle local economic effects from effects that come directly and purely from tax changes.

3.4.1 Contiguous Counties Straddling State Borders

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

Panel A of Table 5 reports simple difference-in-differences estimates based on one-to-one matching between firms located in contiguous bordering counties, where for each firm that experienced a tax change we find a ("matching") firm headquartered in the county across the border that is closest inasset size to the treated firm.

In Panel B of Table 5 we perform a regression analysis where we again only focus on the firms that are located in neighboring counties straddling state borders. In this analysis, we account for time-varying unobserved heterogeneity, caused by changing local economic conditions, by including county pair-year fixed effects. Here we assume that even if changes in economic conditions vary across states, in our narrowly defined geographic region (a pair of neighboring counties) they change similarly on either side of the state border. Under this assumption, the county pair-year fixed effects absorb the local economic shocks, allowing us to identify the tax effect over and above any such variations. Columns (1)-(3) of Panel B presents these results. In columns (4)-(6), we also control for time-varying, industry level unobserved heterogeneity by adding industry-year fixed effects, in addition to county-pair year fixed effects.

Both our matching and regression-based results show that tax increases do indeed have additional explanatory power for future innovation, over and above that of local economic conditions. In terms of the economic magnitudes, our most conservative estimates from these tests imply that about 67% of treated firms affected by a tax hike obtain approximately one fewer patent following a tax increase (compared to a mean of 13.3 patents per firm-year obtained by firms in our contiguous counties sample).[12] We do not observe any discernible change in patenting following tax cuts.

[12]The coefficient in column (4) of Panel B, -0.05, times 13.3, the mean value of patents, yields -0.67.

Note again that the underlying identifying assumption in columns (4)-(6) is that a pair of firms in neighboring counties on either side of a state border, and in the same industry, are exposed to roughly the same local economic cycle variations, and that both firms respond to such changes similarly. In particular, our approach does not require that neighboring states are homogeneous in rules or laws affecting business: since we estimate difference-in-differences specifications, our methodology removes all persistent cross-state differences in such rules or laws. Moreover, since different states change taxes at different times, many firms operating in a given state end up in the treatment group in one year, and in the control group in some other year.[13] This makes it highly unlikely that our results are driven by our treatment firms' propensity to respond differently to local economic changes, as compared to our control firms.

Overall, in this section, we exploit policy discontinuity at contiguous counties straddling state borders to disentangle the effect of corporate tax changes from that of local economic conditions, and our evidence shows that tax changes affect innovation significantly, *over and above* changes in economic conditions that might have led states to alter taxes.

3.4.2 Placebo Tax Changes

Moreover, if our results are driven by regional economic conditions, then we would expect our results to remain even if we coded our tax change dummy as one for firms in states bordering the state that changed the tax. Since these states were likely subject to similar economic conditions (but did not change any taxes), if our results are still similar to what we document earlier, then the effect we observe could not have been produced by tax changes.

In this section we provide results from a randomized assignment of tax change years to bordering states, keeping the distribution of the event years unchanged. For each tax change state in the sample, we create a pseudo tax change state. In particular, instead of the state that actually experiences the tax change we assign the change to one randomly selected state

[13] For instance, when Pennsylvania increased taxes in 1991, its firms were in the treatment group and firms in bordering New Jersey counties were in the control group; when New Jersey increased taxes in 2002, the treatment and control groups were exactly reversed.

that borders it. In each of the pseudo tax change samples we run our baseline regression as in Table 2, column (7) and save the relevant coefficients. We repeat this procedure 5000 times to obtain a (non-parametric) distribution of coefficients obtained from our placebo tax change samples.

In Figure 3, we plot these distributions of the coefficients. The black line embedded in the graph represents the regression coefficient obtained using the actual data in the first year after the tax change. Figure 3A shows that the coefficient for tax decreases in our placebo sample does not lie in the tails of the placebo distribution, while Figure 3B shows that the tax increase coefficient does (p-value$<$0.001). Thus, unobserved local economic conditions affecting innovation activities at the bordering state level are not driving our results.

3.5 Exposure to Tax Changes

We now explore cross-sectional differences among firms that are subjected to the tax changes.

3.5.1 Marginal Tax Rates

First, firms differ in terms of their exposure to tax changes depending on their (past and current) earnings. For example, a firm that is unprofitable is exposed little, if at all, to any changes in the state's top corporate income tax rate.[14] In this section, we measure a firm's exposure to tax changes using the marginal tax rate (Blouin, Core, and Guay (2010)), measured in the year of the tax change. Since most of the state tax changes are implemented through changes in top tax rates and other measures that disproportionately affect top bracket tax payers, if the effect we show is indeed through the corporate tax channel, then we should expect to see the strongest effects for firms that had high marginal rates in the tax change year.

Panel A of Table 6 presents these results. We use Blouin, Core, and Guay (2010)'s simulated

[14]Of course, we do not imply that the top corporate income tax rate does not matter at all for a currently unprofitable firm. Consider a young, unprofitable firm with growth options, in the form of investment projects, which can lead to patentable innovations. The decision to undertake such an investment is clearly going to be a function of future increases in net income from the patentable innovation it can produce. Even if the firm is currently unprofitable, the innovation might make it profitable enough in the future to care about tax rates.

marginal tax rates to partition sample firms into those with marginal tax rates in the bottom 33 percentiles and top 67 percentiles, respectively. The firms in bottom 33 percentiles have an average marginal tax rate (MTR) of 7.7% and are least exposed to tax changes. As expected, we do not see any change in their innovation outputs in the predicted direction following the tax change. All of our effect comes from firms in the top 67 percentiles (with an average MTR of 29.6%) – firms with high marginal tax rates indeed file a lower number of patents in response to tax increases. Interestingly, we also see evidence that when a state increases taxes, firms that are not exposed to such changes (in low MTR percentiles) can gain at the expense of the high MTR firms, indicating that tax changes might have some within-state distributional effects in terms of patenting activity.

3.5.2 Location of Operations and Apportionment Rules

Second, we measure a firm's exposure to tax changes by looking at the distribution of firm's operations across different states in terms of its employees, sales, and assets, as recorded in Lexis-Nexis' Corporate Affiliations database. In particular, we construct a measure of exposure to tax changes by looking at the proportion of firm activity that takes place within the borders of the state that experiences the tax change. To illustrate, consider two firms, A and B, both headquartered in New Hampshire. Firm A has 75% of its operations in New Hampshire, and 25% in Texas; firm B has 25% of its operations in New Hampshire and 75% in Texas. So, when New Hampshire increased its corporate tax rate in 1999, firm A should have been affected more than firm B. In defining how much activity the firm generated in a particular state, the states use a weighted average of sales, property, and payroll activity. The weights – called apportionment weights – used in these formulae differ across states and have also changed over time. While traditionally a lot of states apportioned firms' profits based on the equally weighted average across three activity groups, recently states have been increasing the weight on sales, which are argued to be less dependent on the firm's decision on where to locate its production and employees (Merriman (2014)).

We gather the apportionment formulae from Merriman (2014), and apply them to our firm operations data, where we proxy firm's payroll expenditure across states by the distribution of its employees across different subsidiaries, and the property weights by the distribution of its assets across different subsidiaries.[15]

We report the results using this approach in Table 6, Panel B. Even when we take into account state laws regarding different apportionment formulae across states and firm-level subsidiary structure, we find consistent results. In terms of economic magnitudes, our evidence shows that a treated firm, with 100% in-state exposure, files for one fewer patent (compared to its sample mean of 9.11) in two years following a 1% increase in taxes in its state. Again, we cannot identify any significant effect following tax cuts.

3.5.3 Tax Sheltering

Finally, firms differ in their ability to shelter taxes. In Table 3, row (10), we showed that our results remain significant if we focus on purely domestic firms. However, even within such firms, in certain instances, the multi-state nature of their operations provides them with the opportunity to use state tax shelters. For instance, companies can use a tax shelter that is frequently referred to in legal circles as a Delaware Trademark Holding Company, or a Passive Investment Company (PIC). Under this scheme, a corporation transfers ownership of its trademarks and patents to a subsidiary corporation located in a state – such as Delaware or Nevada – that does not tax royalties or other types of intangible income. Profits that would be taxable by the states in which a firm operates can be shifted out of such states for tax accounting purposes by the tax haven subsidiary charging a royalty to the rest of the business for the use of the patent. The strategy works since the royalty is tax-deductible for the parent as well as other subsidiaries, and hence, directly reduces the amount earmarked as profits in the states in which

[15] 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.

the company is taxable.[16]

In order to test for the presence of such sophisticated tax strategies and their effects, we exploit a corporate tax provision, called "Combined Reporting", that is designed to address a variety of such corporate income tax avoidance strategies. Combined reporting requires that the parent and its subsidiaries are treated as one corporation for state income tax purposes. Their nationwide profits are added together ("combined"). Each state then taxes a share of the combined income, where the share is calculated by a formula that takes into account the corporate group's level of activity in the state as compared to its activity in other states. In our sample period, sixteen US states had combined reporting requirements in place. For example, California had a combined reporting system but Massachusetts did not.[17]

We report the results in Table 6, Panel C. In particular, 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. The results reveal an interesting pattern. In the first year after the tax increase, firms located in states with a combined reporting requirement, as well as firms located in other states, experience a decline in patents. However, in the next 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 patenting activity after experiencing a tax increase in their home state, but this shift takes time.

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

[17]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 a particular state is a firm's choice. But, according to Mazerov (2009), there is little evidence that companies move locations based on states having combined reporting requirements.

3.6 Alternative Innovation Measures

Patents can be thought of as an intermediate product in the innovation process, R&D investment being the input and a new product being one of the outputs (the other type of output might be process innovations, which are difficult to capture in the data). Here we examine the effects of taxes on this entire span of the innovation process. Finally, we examine whether the quality of innovation is affected by taxes.

3.6.1 New Product Announcements

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

We next identify material information about new products among these press releases. The idea here is that if the press release containing our new product keywords indeed refers to a major innovation, the equity market should respond to the news. Similar to Kogan, Papanikolaou, Seru, and Stoffman (2012), who estimate the value of the patent by relying on the stock price reaction to its grant, we look at firm's stock price reaction to measure the expected value of the product announcement.

We implement an event study methodology by fitting a market model over (-246,-30) period to get the expected returns on the firm's stock, and then estimating cumulative abnormal returns over three (-1,1) day period around the announcement. After estimating abnormal returns, we are left with 56,797 announcements. To estimate the value/total number of material firm announcements over the year, we either (a) sum all positive cumulative abnormal returns around product announcements made by firms over the year, or (b) count the number of announcements with cumulative abnormal returns above the 75th percentile in the sample (i.e. 2.61%). The first method is designed to capture the total incremental value of all new product introductions by a firm during the year. We only consider positive abnormal returns to remove any confounding product announcements that were not associated with new product introductions.[18] The second method instead uses a count measure to control for any outlier abnormal returns, and is meant to distil major new innovations introduced by the firm. We estimate specifications similar to our baseline regressions but instead of patent count we use our measures of new products here.

We report the results in Table 7. In Panel A we rely on the sum of all positive cumulative abnormal announcement returns over the year. We find that shareholder value added by new product announcements drops by 50 basis points in the first year after a tax increase. Curiously, with this metric, tax decreases do lead to an increase in shareholder value added by new product announcements by 20 basis points, but the change comes later, in the fourth year following the cut. Again, this is consistent with the view that fostering innovation is more time consuming than cutting back on new product launches. In Panel B we rely on the number of announcements above 75th percentile, and find similar results. In terms of economic magnitudes, the number of major new product introductions by firms drop by 5.1% in the first year after a tax hike, and by a further 6.5% in the fourth year, while the number of major new products introduced rises by 3.4% in the fourth year following a tax cut.

3.6.2 R&D

We next 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

[18]For instance, these could be "delays in new product introductions" or "new product recalls".

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

Consistent with our previous findings, we find that R&D also declines following tax increases. We report the results in Table 8. In terms of economic magnitudes, column (1) shows that R&D to sales declines by 4.3% of its sample mean after a tax increase (the change in R&D to sales, given by coefficient in Table 8, column (1), is -0.012, so the percentage change is -0.012/0.28, where 0.28 is the sample mean of R&D to sales), while column (2) shows that R&D declines by about 2.3% (column (2)). Recall that the average tax increase is about 1.1 percentage points. From a sample mean effective tax rate of about 22.44% (federal plus state), this implies the average tax increase raises rates by 4.9%, relative to the base rate. This implies an elasticity of R&D expenditure to corporate tax increases of around 0.47 (0.023/0.049=0.47). Compared to the R&D tax credit literature that reports an elasticity of R&D expenditure to R&D tax credit of around 1, the elasticity of R&D to corporate taxes is lower.

3.6.3 Innovation Quality

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

We use cite counts adjusted for truncation from the NBER dataset (e.g., Hall, Jaffe, and Trajtenberg (2001) and Hall, Jaffe, and Trajtenberg (2005)) to deal with the issue that citation data suffers from truncation problems. In Table 9, we report the same specification as in columns (7)-(9) of Table 2, but the dependent variables here measure the quality of innovation. In the first three columns, we use $Ln(1 + \#(truncation\ adjusted\ citations))$ as our measure of innovation quality. Since the total number of citations is correlated with the number of patents, in the last three columns we look at an arguably stronger measure of innovation quality, namely, the number of citations per patent ($Ln(1+\#(truncation\ adjusted\ citations)/\#patents)$). This measure reflects the quality of the average patent that the firm files following the tax changes. In terms of economic magnitude, our second measure, the number of truncation adjusted citations per patent, declines by 14.2% in the two years following the tax change. Overall, these results show that the quality of innovation also declines following tax increases. This mirrors our earlier evidence on the number of patents.

Note that the effect on innovation quality most likely comes from the altered incentives for firms to take more risk, which we discuss in the following section. Alternatively, the effect on citations might not be related to the changed innovation quality but might result from the geographic spillovers of innovation activity. For instance, similar firms might cluster in the same state and could be more likely to cite each other's patents. If the innovative activity of the potential citing firms is also affected, we might see a reduction in the citations. Of course, such an explanation does not contradict our findings on the effect of state level corporate taxes on innovation activity, but rather suggests wider economic implications that corporate taxes might have on innovation activity.

4 Discussion

In the previous sections, we presented evidence on the response of innovation to tax changes. In this section, we explore what contributes to this effect by presenting relevant parts of existing theory models particularly suited to our question and deriving and testing specific predictions from them.

4.1 Innovator Incentives

4.1.1 Taxes in a General Equilibrium Model of Innovator Incentives

We first present a version of Romer (1990)'s endogenous growth model with taxes and occupational choice, following the structure in Jaimovich and Rebelo (2012), and applied specifically to our context of innovation outputs. [19] In Jaimovich and Rebelo (2012), innovation occurs in the sector producing intermediate inputs for the final consumption good. Innovations expand the set of available intermediate inputs and successful innovators are assumed to have perpetual patents on the newly-generated intermediate goods from their innovation. We summarize their model in Appendix A.3 and show that corporate tax changes affect agents' decision regarding whether to work as innovators or regular workers, and this decision has further implications for the number of patents in the economy. The model delivers the following Proposition:

Proposition 1: Threshold ability level above which the agent chooses to innovate increases with the corporate tax rate. Hence, corporate taxes affect innovation adversely through their effect on the incentives to innovate.

In particular, this model demonstrates a key source of confusion that can affect studies like ours. In partial equilibrium set-ups, the price or quantity of the innovation good produced is not directly affected by corporate taxes. This is because while tax costs increase on the revenue, offsetting tax benefits are obtained on the cost of the project, reflecting tax deductibility of investments in innovation.

However, as we demonstrate in Appendix A.3, in a general equilibrium setting, this does not imply that taxes do not matter for innovation. Even if price and quantity do not change in partial equilibrium, the size of after tax profit declines, which reduces the pie available to innovators and thus reduces incentive to innovate overall. Such an effect elicits a general equilibrium response in terms of occupational choice. This makes some innovators switch to

[19] Note that this is not the only model that relates the returns to innovation to taxes (see, for example, King and Levine (1993)).

less innovative tasks (become regular workers in the model), affecting aggregate innovation in response to tax changes.

Testing *Proposition 1* in the data requires interpreting a few assumptions to make the prediction amenable to empirical analysis. First, in the model, innovation only happens in entrepreneurial firms and the outside option for innovators is shutting the entrepreneurial firm and entering the labor force as a "worker". However, in a more general sense, one can consider all innovative firms (or research divisions within a firm) as the "entrepreneurial firm" from the model, and employees at less innovative firms (or in non-research divisions within the firm) as the "workers" from the model.[20] The outside option to researchers then naturally becomes employment as a "worker", as defined above – either inside or outside their existing firms.

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

[20] In the model, post-tax profits in innovative firms accrue to innovators. One might argue that firm profits do not accrue to research personnel working therein. However, innovative firms do motivate their key innovative employees with compensation contracts linked to sales contributions of new products, or value of patents produced by the innovator's research (stock options are popular in many innovative firms, year-end bonuses are also linked to divisional performance). This is sufficient to generate the response central in the model.

4.1.2 A Test of Proposition 1

In order to test *Proposition 1*, we first examine the extensive margin, i.e., whether innovators currently employed by a firm leave following tax rises affecting it. We tap into the individual inventor data from the Harvard Business School Patent Dataverse which holds data on both inventors (i.e., those individuals who produce the patent) and assignees (i.e., entities such as firms, individuals, or even governments that own the patents). We can thus track the mobility of inventors across different assignees.

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

We then examine how many innovator departures follow tax increases. In Table 10, we find that in two years following the tax increase there is a significant number of inventors who leave the firm and are not replaced in their organizations, as evidenced by our positive sign on the Net Leavers variable. On the other hand, tax decreases do not lead to net new hires immediately – on average it takes about five years for the effect of the tax cut to show up in our data.[21]

This evidence is broadly consistent with our model. Also, while the model does not specifically account for labor market frictions, the latter can lead to asymmetric adjustments costs and thus can possibly explain the asymmetry in our baseline results. As our results show, firms are quick to lose their innovative personnel following tax hikes, but they need more time to build the knowledge, workforce and capacity to innovate following tax cuts. New innovators might need to be hired, and, given labor market frictions, this might take time. Current employees might need to acquire more skills or learn how to be more innovative, and this might

[21] Net Hires = - Net Leavers, in the way we define our variable.

also be time-consuming. Moreover, it might be easier for innovators to cut back on effort if they are discouraged by a change in firm policy or compensation opportunities than it is to find great new ideas to increase innovation if the firm experiences a similarly-sized tax decrease and wants to foster more innovation. In other words, getting great new ideas might be exogenous to firm employees, but labor market mobility makes it an endogenous outcome which firm will be able to make use of them. Finally, note also that such an explanation is consistent with all our earlier results in which tax decreases – if ever significant – have an effect much later than tax increases do (Table 3, rows (4) and (7); Table 7).

Next, we examine whether employees or innovators who remain in their existing firms also suffer from a decline in productivity following tax increases. Recall that in our broader interpretation of the model structure, job changes can happen even within the firm – employees move from more innovative to less innovative jobs.

To test for such an intensive margin, we first repeat our tests in Table 2, using measures of innovative productivity of employees. Our measure is $Ln(Patents/Employee)$, which is the log of the number of patents per 1,000 firm employees (from Compustat), following Acharya, Baghai, and Subramanian (2014). Next, we address concerns that all employees at a firm may not be involved in the innovation process, which is typically carried out by the R&D department, and scale the number of patents by the number of innovators who applied for a patent at the firm in the current year, and have not yet filed any patent for a different firm (the complement of our Leavers measure, calculated among existing firm employees at the start of the year). Both dependent variables provide a measure of employee innovative effort inside the firm.

Our results in Table 11 reveal a pattern similar to those in previous tables – tax increases are followed by a decline in average innovative productivity of firm employees, while we are unable to uncover any increases in productivity following tax cuts. In terms of economic magnitudes, the number of patents filed per innovator declines by about 2.5 percentage points following the average tax increase in our sample.

Results in Table 10 and Table 11 are consistent with the view that innovation requires creative, new ideas, which are easy to shoot down but difficult to come by. Discouraging innovation can lead to quick abandonment of projects, while encouraging employees to innovate is more difficult.

4.2 Uncertain Nature of Innovation Investments

Innovation investments are highly uncertain and therefore returns to innovation are risky. This uncertain nature of innovation can make it particularly susceptible to changes in taxes that particularly penalize high project payoffs (successful innovations) such as those that accentuate the progressive nature of the tax code. Since most of our tax changes are changes to the top marginal tax rate, they are more likely to penalize projects with a high variance (with potentially very high payoffs), as compared to similar projects with a lower variance. We present a stylized model in this subsection to illustrate this possibility and an empirical test of a prediction arising from this model.

4.2.1 Convex Tax Schedules

Following Gentry and Hubbard (2000), we show that if the rewards to innovation are more variable than the rewards to safe investments, an increase in the convexity of the tax schedule can discourage innovative activity by raising the average tax burden on risky innovation.[22]

Assume that the firm faces two projects, each of which requires an investment I. A safe project earns S for sure. An innovative project faces uncertain income, and earns H with a probability of p, and L otherwise.

The firm is subject to a piecewise-linear income tax system with three brackets and increasing marginal tax rates across the brackets. The first bracket has a marginal tax rate of T_1 and covers the first E_1 dollars of income. The second bracket has a marginal tax rate of T_2 and

[22]The tax schedule can become more convex, for example, with a top bracket tax change under a progressive rate or an increase in surcharges which affect the tax bill of high tax firms disproportionately more. These two tax changes are quite common in US state corporate tax systems.

covers income between E_1 and E_2 dollars. In the third bracket, a marginal tax rate of T_3 applies to income above E_2 dollars. All investment is tax deductible. Consider the case where:

$$L - I < E_1 < S - I < E_2 < H - I.$$

The firm makes a decision whether to invest in the safe project or in an innovative project based on its expected after-tax income. In particular, the firm will choose innovative project if:

$$(1 - p)(1\text{-}T_1)(L - I) + p[(1\text{-}T_3)(H - I) + (T_2\text{-}T_1)E_1 + (T_3\text{-}T_2)E_2]$$
$$>(1\text{-}T_2)(S - I)+(T_2\text{-}T_1)E_1$$

We can now examine the comparative statics of this expression. For expositional simplicity, we model an increase in the convexity of the schedule as an increase in the top corporate tax rate T_3. The derivative of firm's decision expression with respect to the highest marginal tax rate T_3 is:

$$p(E_2\text{-}H + I)$$

This expression is negative, given the assumption that the successfully innovating firm ends up in the highest marginal tax bracket. Thus, since the top tax rate reduces the rewards from extremely successful outcomes, the investment in projects with particularly uncertain payoffs should decline if the top tax rate is changed by more than the rates in the other brackets.

In the data, most of the tax changes we examine were implemented by changing the top tax rate in the state, so that the set-up above becomes relevant. In terms of the two projects in the model, success in innovation projects is much more likely to be risky with outcomes being highly uncertain. This then would predict that payoffs from innovation activity are riskier, and hence innovation will suffer particularly when corporate taxes are raised, as we have shown in the data.

To explore further testable implications from the model, we derive the following proposition:

Proposition 2: An increase in top bracket taxes will reduce incentives for firms to undertake innovation projects, particularly if the innovation projects are more risky.

4.2.2 A Test of Proposition 2

Following *Proposition 2*, if a firm chooses to forego the more risky innovation projects, then, on average, it would end up with fewer projects that are highly valuable and also fewer projects that are not valuable at all. We use two tests, both based on this idea.

In the absence of any direct measure of ex-ante innovation risk, we test *Proposition 2* by looking at the riskiness of the projects ex-post. That is, we examine patenting risk by testing how taxes affected the volatility of patent citations. Using citations to proxy for the value of an innovation will lead to the prediction that the cross-section of projects patented by a firm after the tax increase will be less dispersed in terms of future citations that they receive in the first few years following patent filing. To this end, following Amore, Schneider, and Žaldokas (2013), we analyze the distribution of citations to patents granted to a firm before and after the tax change in Table 12, Panel A. We find that, consistent with model predictions, standard deviation of citations goes down by approximately 13.6% in the three years after a tax increase. However, we do not find any similar increase in citation standard deviation following tax cuts.

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

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

Overall, we find support for *Proposition 2* in the data.

4.3 Leverage Tax Shield and Debtholder Preferences

Another potential explanation for the response of firm innovation to tax changes operates through changes in firm financing structure to exploit debt shields against taxes. For instance, Heider and Ljungqvist (2014) show that firms respond to tax increases by increasing leverage. Higher leverage allows them to reap greater tax benefits but might come at the price of lower future innovation, since debt-holders might not like funding risky innovation projects (in which they do not share the upside if the project is successful but bear the costs of failure).[23] Moreover, as Heider and Ljungqvist (2014) show, firms' debt response to tax changes is also asymmetric – firms respond significantly to tax hikes but not to tax cuts. So, the debt channel can also be an explanation for the asymmetry we document.

In Table 13 we divide firms based on whether they actually changed their leverage following the tax change. We find that while the drop in future patenting is present both among the firms that increased their leverage following a tax hike, as well as those that did not, in the first year our results are indeed stronger for firms that altered leverage in the predicted direction following the tax increase. This result indicates that changing leverage may also be responsible for our documented effects. However, they cannot be the only reason behind changes in innovation, since firms that do not alter leverage in the tax-motivated direction also show an effect on patenting.

4.4 Other Explanations and More on Asymmetry

One other plausible channel through which taxes could affect innovation is through their impact on internal cash flows. Since internal cash flows have been shown to be a major source of financing for firm innovation (e.g. Himmelberg and Petersen (1994)), a tax-induced reduction in such financing might reduce innovation, particularly for those firms that are constrained in

[23] Note that this would require some amount of commitment from firm managers: once they raise debt and increase leverage, they have to credibly commit not to engage in risk shifting and pursue more risky, e.g. innovative, projects.

their ability to obtain outside financing. We explored this possibility using a variety of measures of financial constraints, but did not find any supporting evidence.

There are two reasons why this may not be surprising. First, tax changes affect large, profitable firms disproportionately more. This is because these are the firms that face high effective tax rates, and most of the changes in our sample affect high tax paying-firms disproportionately more. However, at the same time these firms are less likely to be financially constrained – large profitable firms are typically *not* constrained in their ability to find outside financing. Second, it could also be that empirical proxies of financial constraints typically used are noisy, especially in the context of our state tax changes. Specific to our experiment, Farre-Mensa and Ljungqvist (2013) show that firms classified as constrained according to popular financial constraint measures are in fact able to obtain financing following tax increases, casting doubt on the proxies used to classify them. Such noise might be reducing the power of our tests.

Regarding the asymmetry in the patenting response to tax changes, one alternative explanation could be that the relation between innovation inputs (R&D) and outputs (patent-based measures) is concave.[24] If so, any reduction in R&D due to a tax increase is likely to produce a larger effect on patents than a similarly-sized increase in R&D (due to a tax cut). However, our R&D results presented in Table 8 show that the R&D response to innovation is itself asymmetric, so this is unlikely to be important.

Finally, one might also argue that tax decreases are different in nature from tax increases. From an ex-ante standpoint, firms might perceive tax decreases as more temporary, since states might need to reverse them in the future in order to balance their budgets. In addition, firms may in fact be even prone to lobbying for tax decreases, thus, diminishing the exogeneity of these types of changes. Unfortunately, we do not have evidence to understand whether this is affecting our results.

[24]This type of relationship can be motivated in theory by the fact that increasing R&D expenditures without limit does not guarantee success in innovation.

5 Conclusion

Public debates on fiscal policy often involve arguments that corporate taxes discourage innovation as they reduce incentives to put in effort and take risks. In this paper, we use staggered changes in state corporate tax rates in the US to examine the importance of tax policy on future innovation by firms. We find evidence that firms respond to tax increases by filing a lower number of patents, investing less in R&D, and bringing fewer new products in the market, suggesting that higher corporate taxes reduce innovator incentives and discourage risk taking. We find weaker results on increasing innovation in response to tax cuts. Such an asymmetry is consistent with the view that encouraging innovation is more difficult and time-consuming than discouraging it.

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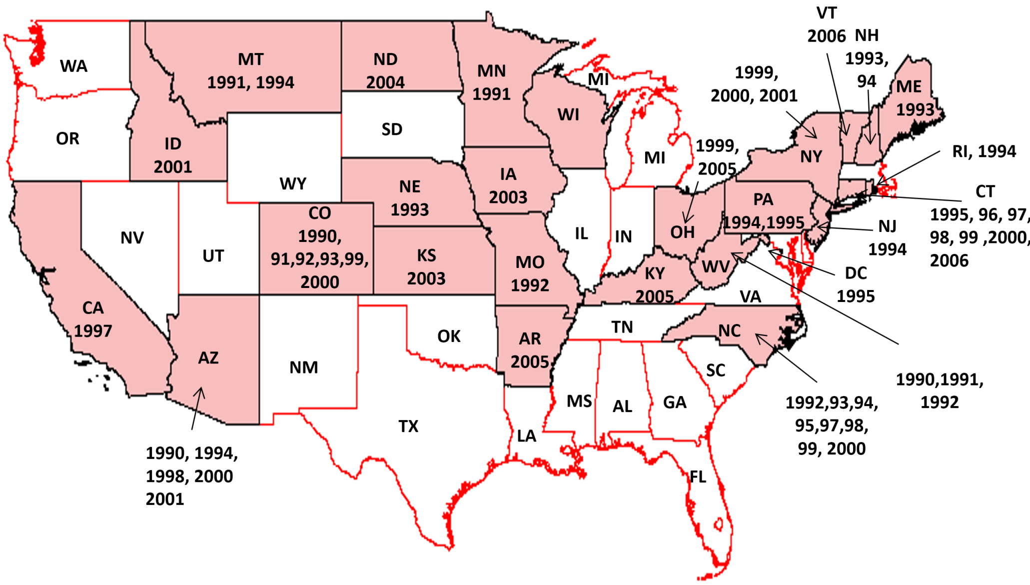
Figure 1: Geography of State Corporate Income Tax Changes, 1990-2006

The figure below provides detailed geography of state corporate income tax changes during 1990-2006. The colored areas provide the location of tax change.

1A: Tax Decreases



1B: Tax Increases

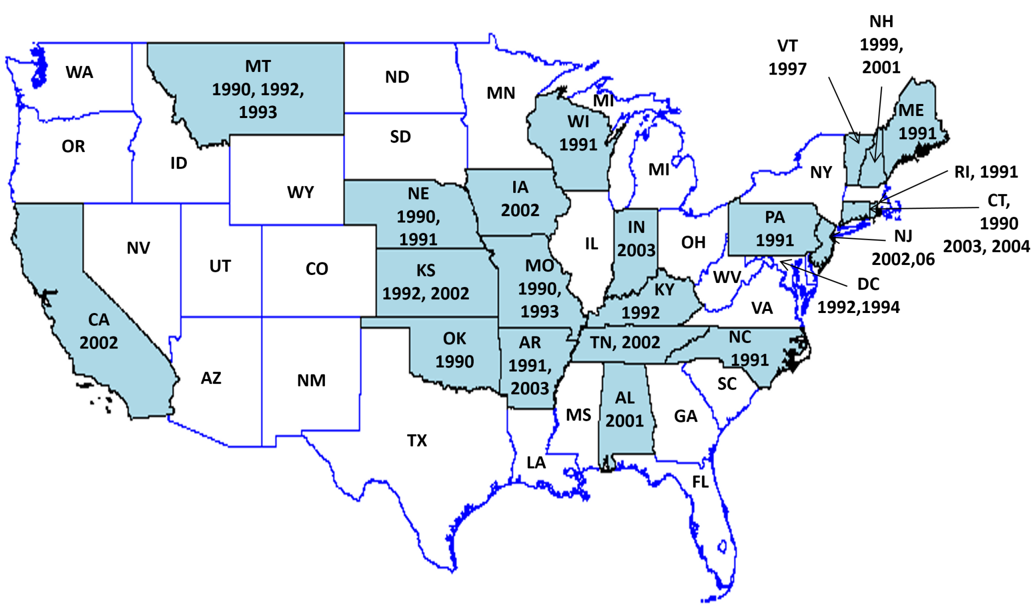
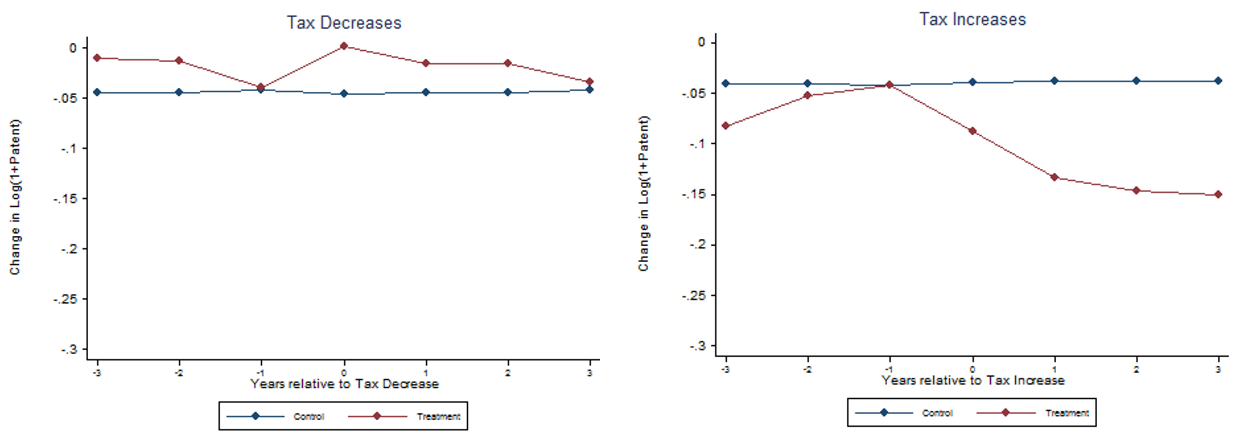


Figure 2: Innovation and Corporate Taxes: Pre-Trends and Post-Trends

The figure below plots the change in the number of patents that a firm files measured in the log scale, following the change in tax rates. The top panel of the figure presents event-time averages of the Δ Ln(1+#Patents)$_{i,s,t+k}$ where i, s, t+k index firms, states, years with k = -3 to 3; plotted separately for the treatment and the control groups following respectively tax decreases and tax increases. The bottom panel shows the difference in future innovation between the treatment and the control groups averaged in event time, and 5% confidence intervals around this difference.

2A: Treatment and Control



2B: Difference-in-Differences

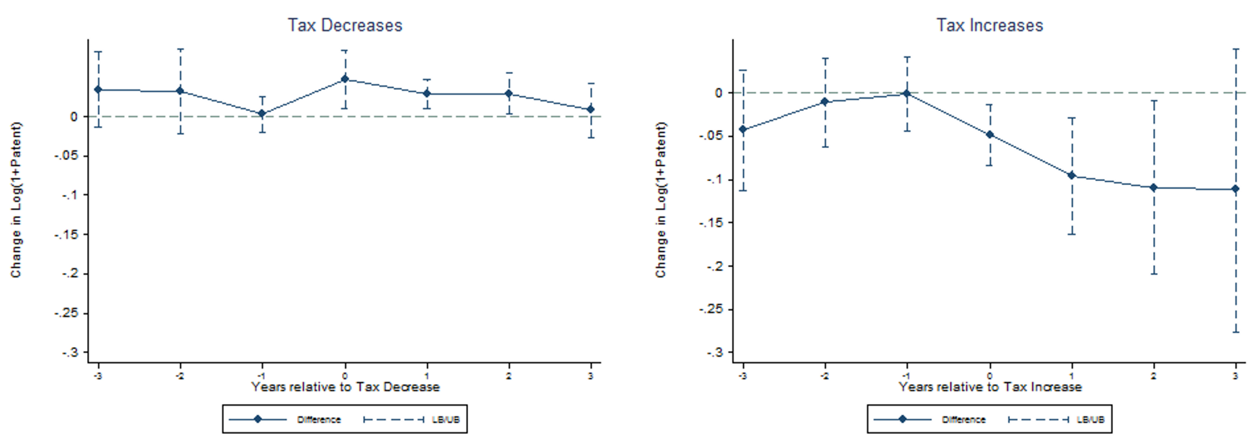
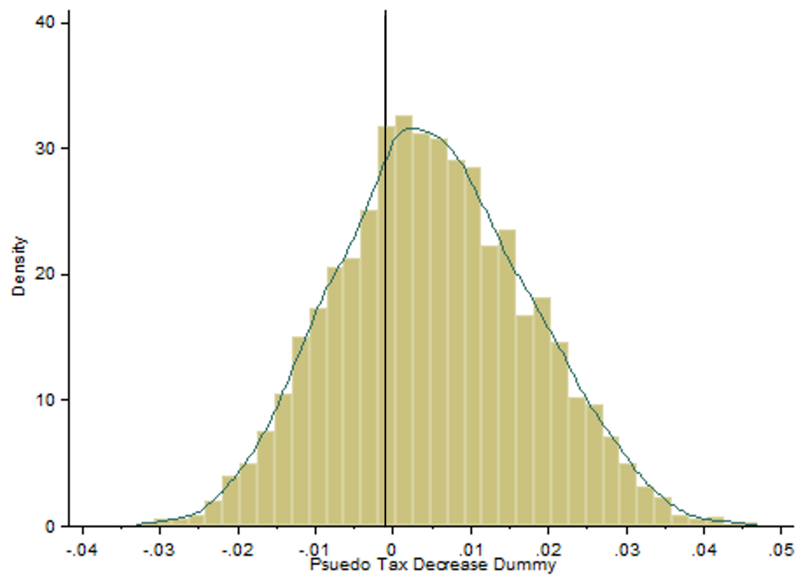


Figure 3: Non-Parametric Distribution of the Coefficient on Placebo Tax Changes

The figure below provides the result of a randomized assignment of tax change years to bordering states, keeping the distribution of the event years unchanged. For each tax change in the sample, we create a pseudo tax change sample, where instead of the state that actually experiences the tax change we assign it to one randomly selected state that borders it. In each of the pseudo tax change samples we run our baseline regression as in Table 2, column (7) and save the relevant coefficients. Here, we plot the distribution of the coefficients. The black line embedded in the graph represents the regression coefficient obtained using the actual tax changes in the first year after the tax change.

3A: Tax Decreases



3B: Tax Increases

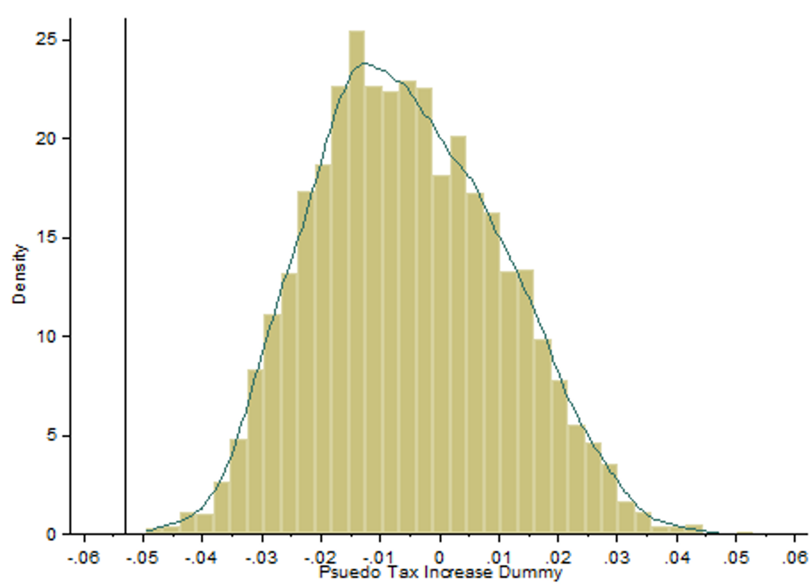


Table 1: Summary Statistics

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

Variables	Obs	Median	Mean	SD
Firm Level Variables				
No. of Patents$_{i,t}$	47632	0.00	9.11	75.10
Adjusted Citations$_{i,t}$	47632	0.00	149.59	1511.62
Ln(Sales)$_{i,t}$	47632	4.86	4.79	2.44
Ln(K/L)$_{i,t}$	47632	3.37	3.45	1.24
HHI$_{i,t}$	47632	0.13	0.19	0.16
Profitability$_{i,t}$	47632	0.09	-0.52	2.86
Tangibility$_{i,t}$	47632	0.20	0.25	0.21
Debt Rating(1=yes)$_{i,t}$	47632	0.00	0.21	0.41
R&D/Sales$_{i,t}$	47632	0.02	0.28	0.93
State Level Variables				
Tax Rate$_{s,t}$(in %)	867	7.35	6.8	2.93
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
Log(Real GSP)$_{s,t}$	824	11.95	11.83	1.03
Taxes as % of GSP$_{s,t}$	841	5.52	5.57	1.05
Log(Population)$_{s,t}$	841	15.17	15.05	1.01
Unemployment Rate$_{s,t}$	841	5.10	5.18	1.40

Table 2: Baseline Results

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 into positive and negative changes. In columns (7), (8) and (9) we replace the changes with the indicators which allows us to include tax changes that cannot be directly quantified, and estimate the following regression:

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

where i, s, $t+k$ index firms, states, years with k = 1 to 3; $\text{Ln(1+\#Patents)}_{i,s,t+k}$ 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 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$_{s,t}$	-.016	-.027	.003						
	(.011)	(.005)***	(.008)						
Δ^- Taxrate$_{s,t}$				.007	-.020	.007			
				(.016)	(.014)	(.019)			
Δ^+ Taxrate$_{s,t}$				-.019	-.034	-.001			
				(.012)	(.005)***	(.007)			
Tax Decrease$_{s,t}$							-.004	-.001	.004
							(.013)	(.009)	(.010)
Tax Increase$_{s,t}$							-.055	-.053	-.060
							(.014)***	(.020)***	(.037)
Δ Ln(Sales)$_{i,t}$	.023	.009	-.004	.023	.009	-.004	.022	.007	-.002
	(.007)***	(.005)*	(.006)	(.007)***	(.005)*	(.006)	(.006)***	(.005)	(.006)
Δ Ln(K/L)$_{i,t}$	-.0004	-.010	-.017	-.0003	-.010	-.017	-.003	-.011	-.017
	(.006)	(.007)	(.007)**	(.006)	(.007)	(.007)**	(.006)	(.007)	(.006)***
Δ HHI$_{i,t}$	-.115	-.204	-.184	-.115	-.205	-.184	-.113	-.249	-.309
	(.153)	(.176)	(.166)	(.153)	(.176)	(.166)	(.148)	(.181)	(.215)
Δ HHI Sq.$_{i,t}$	.144	.332	.229	.143	.331	.228	.178	.363	.377
	(.169)	(.198)*	(.162)	(.169)	(.197)*	(.162)	(.159)	(.195)*	(.211)*
Δ R& D/Sales$_{i,t}$	.007	.017	-.001	.007	.017	-.001	.004	.020	-.002
	(.009)	(.006)***	(.008)	(.009)	(.006)***	(.008)	(.008)	(.005)***	(.007)
Δ Profitability$_{i,t}$	-.003	.0005	.002	-.003	.0005	.002	-.003	.001	.002
	(.003)	(.002)	(.002)	(.003)	(.002)	(.002)	(.003)	(.002)	(.002)
Δ Tangibility$_{i,t}$	-.057	.039	.043	-.057	.039	.043	-.056	.036	.046
	(.035)	(.036)	(.046)	(.035)	(.036)	(.046)	(.032)*	(.037)	(.036)
Δ Rating$_{i,t}$	.035	.00006	-.019	.035	.0002	-.019	.029	-.003	-.016
	(.015)**	(.018)	(.017)	(.015)**	(.018)	(.017)	(.014)**	(.018)	(.018)
Δ Log(GSP)$_{s,t}$	.236	.155	-.123	.252	.173	-.116	.319	.121	.050
	(.212)	(.226)	(.191)	(.210)	(.232)	(.193)	(.203)	(.228)	(.185)
Δ Taxes as % of GSP$_{s,t}$	-.0001	.008	-.005	.0002	.009	-.005	.008	.008	.026
	(.022)	(.013)	(.017)	(.022)	(.013)	(.017)	(.018)	(.014)	(.010)**
Δ Log(Population)$_{s,t}$	.960	.770	1.261	.888	.724	1.239	.753	.740	1.024
	(.425)**	(.356)**	(.358)***	(.423)**	(.349)**	(.352)***	(.417)*	(.384)*	(.336)***
Δ Unemployment Rate$_{s,t}$	.017	.014	-.002	.017	.014	-.002	.018	.012	.002
	(.011)	(.014)	(.012)	(.012)	(.015)	(.012)	(.011)	(.015)	(.012)
Obs.	40092	35433	30812	40092	35433	30812	42192	37317	32557

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 and year fixed effects, not reported for brevity. Row (1) reports the results for a regression where we allow for our coefficients to vary by tax changes greater than 1% and other tax changes. Row (2) reports results with SIC4 industry-year fixed effects, instead of year fixed effects. In row (3), we exclude firms located in California and Massachusetts from the sample. Row (4) reports the results for a regression where we allow for our coefficients to vary by sample periods, 1990 to 1999 and 2000 to 2006. Row (5) reports the results for firms that survive to 2006. Row (6) reports the treatment effects for firms located in states which first experience a tax increase and then a tax cut (reversals). The firm-years with tax changes that did not get reversed are dropped from the sample. Row (7) reports the treatment effects for states with no reversals. The firm-years with reversals are dropped from the sample. In row (8) we include a list of additional state-level macro variables, i.e. the change in state budget surplus and debt outstanding. Row (9) reports the results when we include firm fixed effects. In row (10) we report results for domestic firms (i.e., Pretax Income-Foreign($ptis\_f$)=0). Row (11) reports the results without any controls. 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) |ΔTax|≥1%	-.017	-.036	42192	-.005	-.077	37317	-.007	-.010	32557
	(.028)	(.018)***		(.031)	(.024)***		(.023)	(.019)	
Other Tax Changes	.007	-.018		-.011	.0003		-.018	-.007	
	(.011)	(.027)		(.019)	(.037)		(.022)	(.037)	
(2) Industry-Year FEs	-.008	-.032	42192	.004	-.030	37317	-.0007	-.042	32557
	(.012)	(.012)***		(.008)	(.016)*		(.009)	(.025)*	
(3) Excluding CA and MA	-.012	-.050	31492	-.0003	-.038	27971	-.006	.005	24525
	(.011)	(.017)***		(.008)	(.029)		(.009)	(.018)	
(4) Regime I: 1990 to 1999	-.006	-.049	42192	-.011	-.048	37317	-.005	.003	32557
	(.012)	(.016)***		(.008)	(.030)		(.008)	(.029)	
Regime II: 2000 to 2006	.002	-.058		.034	-.056		.003	-.099	
	(.018)	(.018)***		(.024)	(.024)**		(.024)*	(.042)**	
(5) Surviving Firms	-.001	-.068	15457	-.013	-.089	14478	.011	-.070	13498
	(.018)	(.025)***		(.014)	(.035)**		(.013)	(.050)	
(6) Reversals	-.027	-.043	38147	-.014	-.084	33647	-.011	.015	29174
	(.018)	(.023)*		(.024)	(.024)***		(.017)	(.035)	
(7) Non-Reversals	-.0003	-.058	40447	-.009	-.039	35711	.031	-.091	31089
	(.016)	(.017)**		(.010)	(.020)**		(.009)***	(.036)**	
(8) Additional Macro Controls	.003	-.024	42192	.002	-.035	37317	.009	-.092	32557
	(.014)	(.016)***		(.008)	(.023)		(.010)	(.039)*	
(9) With Firm FEs	-.001	-.048	42192	.0004	-.043	37317	.011	-.058	32557
	(.014)	(.012)***		(.009)	(.018)***		(.010)	(.035)*	
(10) Domestic Firms	-.002	-.064	32668	-.009	-.054	29406	-.0005	-.065	26139
	(.013)	(.017)***		(.009)	(.019)***		(.011)	(.040)	
(11) Without Controls	-.004	-.059	48057	-.002	-.050	42764	.0005	-.064	37572
	(.010)	(.012)***		(.008)	(.019)***		(.018)	(.038)*	

Table 4: Potential Confounding Factors

Panel A provides changes in other tax rates that coincide with the corporate tax changes. Panel B provides the results from our baseline specification, i.e. column (7), (8) and (9) in Table 2 after controlling for potential confounding factors. We control for region-year fixed effects, state fixed effects and various coincidental tax changes–decreases and increases in R&D tax credit and personal income tax rate (either tax rate on wages, or tax rate on long capital gains) in state s at time t, respectively. All regressions include firm level and state-level controls and year fixed effects, not reported for brevity. Standard errors are clustered at state-level and reported in parentheses. *,**, and *** indicate significance at 10%, 5% and 1% respectively.

Panel A: Description of Coincidental Changes

		Tax Increases	Tax Decreases
Number of corporate tax changes		32	51
..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 wage tax rate	14	6
	decrease in state wage tax rate	2	13
	increase in state capital gains tax rate	13	5
	decrease in state capital gains tax rate	2	14

Panel B: Results

	Δ Ln(1+Patents)$_{t+k}$		
	k=1	k=2	k=3
	(1)	(2)	(3)
Tax Decrease$_{s,t}$	-.006	.005	.011
	(.012)	(.008)	(.009)
Tax Increase$_{s,t}$	-.047	-.035	-.030
	(.014)***	(.021)*	(.023)
R& D Tax Credit Decrease$_{s,t}$	-.070	-.039	.009
	(.039)*	(.051)	(.037)
R& D Tax Credit Increase$_{s,t}$	-.004	.016	-.006
	(.019)	(.012)	(.013)
Personal Income Tax Increase$_{s,t}$	.008	.019	-.001
	(.010)	(.013)	(.012)
Personal Income Tax Decrease$_{s,t}$	.0009	-.0009	-.019
	(.009)	(.009)	(.011)
Controls	YES	YES	YES
Obs.	42192	37317	32557

Table 5: Local Economic Conditions

This table provides results for our tests on how local economic conditions affect our estimates. We limit our sample to firms located in contiguous bordering counties. Panel A reports the results based on one-to-one matching between firms located in contiguous bordering counties, where for each firm that experienced a tax change we assign a firm headquartered in the county across the border that is closest in asset size to the treated firm. Panel B reports the regression based on our baseline specification (column (7)-(9) in Table 2) where we restrict our sample to firms in contiguous counties straddling a state border, and control for county-pair-year fixed effects (column (1)-column(3)) and county-pair-year as well as industry-year fixed effects (column (4)-column(6)). All regressions include firm level and state level controls, 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: Matching between Firms Located in Contiguous Bordering Counties

	Δ Ln(1+Patents)$_{t+k}$		
	k=1	k=2	k=3
	(1)	(2)	(3)
Tax Decrease$_{s,t}$	-.044	-.043	-.001
	(.028)	(.030)	(.031)
No. of Pairs	579	517	442
Tax Increase$_{s,t}$	-.046	-.128	-.049
	(.041)	(.049)***	(.056)
No. of Pairs	257	230	173

Panel B: Contiguous Bordering Counties

	Δ Ln(1+Patents)$_{t+k}$					
	k=1	k=2	k=3	k=1	k=2	k=3
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Decrease$_{s,t}$	-.019	-.018	.008	-.027	-.025	-.008
	(.015)	(.018)	(.032)	(.021)	(.024)	(.019)
Tax Increase$_{s,t}$	-.062	-.081	-.021	-.050	-.042	-.034
	(.031)**	(.037)**	(.060)	(.026)**	(.040)	(.054)
Controls	YES	YES	YES	YES	YES	YES
County-Pair Year FEs	YES	YES	YES	YES	YES	YES
Industry-Year FEs	NO	NO	NO	YES	YES	YES
Obs.	6204	5337	4508	6204	5337	4508

Table 6: Exposure to Tax Changes

This table provides the results from our baseline specification, i.e. column (7), (8) and (9) of Table 2. In Panel A, we use Blouin, Core, and Guay (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 33 percentiles and top 67 percentiles, respectively. We include $\text{MTR}_{i,t}$ as well to control for the level effect of marginal tax rate. Panel B reports the results where 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. We use apportionment rules in the state to measure exposure to tax changes. In Panel C, 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. All regressions include firm-level and state-level controls (except Panel B), 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: Marginal Tax Rate

	Δ Ln(1+Patents)$_{t+k}$		
	k=1	k=2	k=3
	(1)	(2)	(3)
Tax Decrease$_{s,t}$ × MTR$_{i,t}$ Bottom 33	.004	.012	-.0003
	(.013)	(.011)	(.017)
Tax Decrease$_{s,t}$ × MTR$_{i,t}$ Top 67	-.008	-.008	.005
	(.014)	(.009)	(.012)
Tax Increase$_{s,t}$ × MTR$_{i,t}$ Bottom 33	-.013	.013	.064
	(.021)	(.018)	(.026)**
Tax Increase$_{s,t}$ × MTR$_{i,t}$ Top 67	-.073	-.077	-.107
	(.016)***	(.029)***	(.057)*
Controls	YES	YES	YES
Obs.	41859	36997	32286

Panel B: Location of Operations

	Δ Ln(1+Patents)$_{t+k}$		
	k=1	k=2	k=3
	(1)	(2)	(3)
Exposure to Tax Decrease	.0004	.029	-.007
	(.022)	(.023)	(.024)
Exposure to Tax Increase	-.054	-.110	-.063
	(.041)	(.049)**	(.053)
Controls	YES	YES	YES
Obs.	13455	11435	9549

Panel C: Tax Sheltering

	Δ Ln(1+Patents)$_{t+k}$		
	k=1	k=2	k=3
	(1)	(2)	(3)
Tax Decrease$_{s,t}$ × Combined$_s$	-.002	-.002	.004
	(.023)	(.016)	(.010)
Tax Decrease$_{s,t}$ × Non-combined$_s$	-.006	-.0003	.005
	(.014)	(.011)	(.013)
Tax Increase$_{s,t}$ × Combined$_s$	-.054	-.065	-.132
	(.025)**	(.022)***	(.042)***
Tax Increase$_{s,t}$ × Non-combined$_s$	-.056	-.044	-.006
	(.015)***	(.029)	(.018)
Controls	YES	YES	YES
Obs.	42192	37317	32557

Table 7: New Product Announcements

This table studies the effect of tax changes on the product announcement. We estimate our baseline specification, i.e. column (7), (8) and (9) of Table 2, for different measures of product announcements. We implement event study methodology by fitting a market model over (-246,-30) period to get the expected returns on the firm's stock, and then estimating cumulative abnormal returns over three (-1,1) day period around the announcement. Panel A reports the results with sum of all positive cumulative abnormal product announcement returns over the year as the dependent variable. In Panel B, the dependent variable is the number of product announcements with 3-day event CARs above the 75% percentile. All regressions include firm-level and state-level controls and year fixed effects, not reported for brevity. Standard errors are clustered at state level and reported in parentheses. *,**, and *** indicate significance at 10%, 5% and 1% respectively.

Panel A: Sum of All Positive CARs(-1,1)

	Δ(Sum of All Positive CARs)$_{t+k}$				
	(k=1)	(k=2)	(k=3)	(k=4)	(k=5)
	(1)	(2)	(3)	(4)	(5)
Tax Decrease$_{s,t}$	.001	.001	-.00006	.002	-.004
	(.002)	(.002)	(.001)	(.0006)***	(.004)
Tax Increase$_{s,t}$	-.005	.002	-.003	-.002	.002
	(.002)*	(.003)	(.002)	(.003)	(.002)
Controls	Yes	Yes	Yes	Yes	Yes
Obs.	42184	37301	32535	28118	24086

Panel B: Number of CARs(-1,1) above 75% Percentile

	Δ Ln(1+# Major New Products)$_{t+k}$				
	(k=1)	(k=2)	(k=3)	(k=4)	(k=5)
	(1)	(2)	(3)	(4)	(5)
Tax Decrease$_{s,t}$	.007	-.011	-.0002	.034	-.003
	(.010)	(.011)	(.009)	(.008)***	(.023)
Tax Increase$_{s,t}$	-.051	.031	-.014	-.065	.057
	(.024)**	(.020)	(.023)	(.023)***	(.036)
Controls	Yes	Yes	Yes	Yes	Yes
Obs.	42184	37301	32535	28118	24086

Table 8: R&D Expenditures

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

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

where *i*, *s*, *t+1* index firms, states and years. We measure R&D as (1) R&D expenditure (*xrd*) scaled by total sales(*sale*) 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. Standard errors are clustered at state-level and reported in parentheses. *,**, and *** indicate significance at 10%, 5% and 1% respectively.

	$\Delta\ \frac{\text{R\&D}}{\text{Sales}}\ _{t+1}$	Δ Ln(1+R&D)$_{t+1}$
	(1)	(2)
Tax Decrease$_{s,t}$	.005	-.005
	(.004)	(.005)
Tax Increase$_{s,t}$	-.012	-.023
	(.006)*	(.012)**
Controls	YES	YES
Obs.	42192	42192

Table 9: Innovation Quality

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

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

where i, s, $t+k$ index firms, states, years with $k = 1$ to 3; Ln (1+#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. Standard errors are clustered at state-level and reported in parentheses. *,**, and *** indicate significance at 10%, 5% and 1% respectively.

	Δ Ln(1+Citations)$_{t+k}$			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$_{s,t}$	.0008	.025	.003	.002	.029	-.010
	(.032)	(.026)	(.032)	(.023)	(.021)	(.023)
Tax Increase$_{s,t}$	-.124	-.144	-.046	-.058	-.084	-.011
	(.039)***	(.063)**	(.068)	(.025)**	(.043)*	(.035)
Obs.	42192	37317	32557	42192	37317	32557
Controls	YES	YES	YES	YES	YES	YES
Obs.	42192	37317	32557	42192	37317	32557

Table 10: Innovator Turnover

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

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

where i, s, t index firms, states, years and $k = 1$ to 5; 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 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+Net Leavers)$_{t+k}$				
	(k=1)	(k=2)	(k=3)	(k=4)	(k=5)
	(1)	(2)	(3)	(4)	(5)
Tax Decrease$_{s,t}$	-.013	.0009	-.008	-.018	-.021
	(.011)	(.012)	(.013)	(.013)	(.013)*
Tax Increase$_{s,t}$	.048	.061	.035	.014	-.037
	(.023)**	(.034)*	(.037)	(.039)	(.026)
Controls	YES	YES	YES	YES	YES
Obs.	41557	36740	32040	27695	23721

Table 11: Innovator Productivity

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

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

where i, s, $t{+}k$ index firms, states, years with k = 1 to 3; $\text{Ln}(1+\ \frac{\#\text{Patents}}{\#\text{Employees}}\ \text{or}\ \frac{\#\text{Patents}}{\#\text{Inventors}})_{i,s,t+k}$ measures productivity of employees or inventors of firm i in 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. Number of inventors excludes innovators who have produced a patent at the sample firm within one past year but produce at least one patent at a different firm. All regressions include firm-level and state-level controls and year fixed effects, not reported for brevity. Standard errors are clustered at state-level and reported in parentheses. *,**, and *** indicate significance at 10%, 5% and 1% respectively.

	Δ Ln(1+$\frac{\#\text{Patents}}{\#\text{Employees}}$)$_{t+k}$			Δ Ln(1+$\frac{\#\text{Patents}}{\#\text{Inventors}}$)$_{t+k}$		
	k=1	k=2	k=3	k=1	k=2	k=3
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Decrease$_{s,t}$	.002	.031	-.014	.004	-.007	.0003
	(.018)	(.020)	(.013)	(.008)	(.005)	(.004)
Tax Increase$_{s,t}$	-.037	-.059	-.108	-.006	-.020	-.024
	(.027)	(.031)*	(.052)**	(.009)	(.010)**	(.009)***
Controls	YES	YES	YES	YES	YES	YES
Obs.	42184	37301	32535	42146	37274	32518

Table 12: Innovation Risk

This table studies the effect of tax changes on the riskiness of innovation projects. We estimate our baseline specification, i.e. column (7), (8) and (9) of Table 2, for different measures of riskiness of innovation projects. Panel A reports the results with Ln (1+σ(Citations)) as measure riskiness of innovation projects. We only keep tax changes in top brackets and surcharges, which are progressive in nature and do not consider non-progressive tax changes like NOL suspensions. In Panel B we repeat our baseline with Δ Ln(1+#Zero-cite Patents) and Δ Ln(1+#Highly Cited Patents) as dependent variables. All regressions include firm-level and state-level controls and year fixed effects, not reported for brevity. Standard errors are clustered at state-level and reported in parentheses. *,**, and *** indicate significance at 10%, 5% and 1% respectively.

Panel A: Standard Deviation of Citations

	Δ Ln(1+σ(Citations))$_{t+k}$		
	k=1	k=2	k=3
	(1)	(2)	(3)
Top Tax Decrease$_{s,t}$	.025	.010	.006
	(.023)	(.022)	(.038)
Top Tax Increase$_{s,t}$	-.045	-.136	.069
	(.064)	(.054)**	(.076)
Controls	YES	YES	YES
Obs.	8252	7362	6501

Panel B: Zero-cite Patents and Highly Cited Patents

	Δ Ln(1+Zero-cite Patents))$_{t+k}$			Δ Ln(1+Highly Cited Patents))$_{t+k}$		
	k=1	k=2	k=3	k=1	k=2	k=3
	(1)	(2)	(3)	(4)	(5)	(6)
Top Tax Decrease$_{s,t}$	-.022	-.018	.028	-.012	.012	.041
	(.022)	(.016)	(.025)	(.012)	(.013)	(.017)**
Top Tax Increase$_{s,t}$	-.072	.023	.002	-.045	-.001	.005
	(.030)**	(.018)	(.038)	(.026)*	(.040)	(.036)
Controls	YES	YES	YES	YES	YES	YES
Obs.	11643	10323	9067	11643	10323	9067

Table 13: Firm Leverage

This table provides evidence on how firm's actual changes in leverage following tax changes impacts future innovation. This table provides the results of the effect of tax changes on innovation activity for the firms that changed leverage, and did not change leverage following the tax change. 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 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.

	Δ Ln(1+Patents)$_{t+k}$		
	(k=2)	(k=3)	(k=4)
	(1)	(2)	(3)
Tax Decrease$_{s,t}$ $\times$ Δ $BL_{i,t+1}$ <0	.006	.004	.007
	(.011)	(.013)	(.017)
Tax Decrease$_{s,t}$ $\times$ Δ $BL_{i,t+1}$>=0	-.007	.0003	.003
	(.016)	(.010)	(.021)
Tax Increase$_{s,t}$ $\times$ Δ $BL_{i,t+1}$ >0	-.076	-.074	-.048
	(.031)**	(.038)*	(.062)
Tax Increase$_{s,t}$ $\times$ Δ $BL_{i,t+1}$<=0	-.047	-.072	-.041
	(.025)*	(.046)	(.045)
Controls	YES	YES	YES
Obs.	34039	29905	26003

Appendix A.1: 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, Jaffe, and Trajtenberg (2001, 2005))
Ln(Sales)	Natural logarithm of total sales at 2000 dollars
Ln(K/L)	Natural logarithm of capital-to-labor ratio, where capital is represented by net property, plants, and equipment(PPE), and labor is the number of employees
HHI	Herfindahl-Hirschman Index computed as the sum of squared market shares of all firms based on sales in a given three-digit SIC industry in each year
Profitability	Ratio of earnings before interest (*oibdp*)and taxes to sales (*sale*)
Tangibility	Ratio of net plant, property and equipment (*ppent*) to book assets (*at*)
Debt Rating	Dummy variable for firm-years rated by S&P
R&D/Sales	Ratio of expense on research and development (*xrd*) to sales (*sale*)
Tax Increase/Decrease	Dummy variable equal to 1 in the year of corporate tax increase/deduction for the firms headquartered in state s, else zero
Log(Real GSP)	Natural logarithm of real Gross State Product
Taxes (% of GSP)	Tax revenue as a percent of Gross State Product
Log(Population)	Natural logarithm of state population
Unemployment Rate	Unemployment rate in a state
Budget Surplus	Budget surplus as a percent of Gross State Product
Debt Outstanding	Debt outstanding as a percent of Gross State Product
R&D Tax Credit Increase/ Decrease	Dummy variable equal to 1 in the year of state R&D tax credit increase/decrease for the firms headquartered in state s, else zero. State R&D credit rate is the percentage of a firm's R&D expenditures that it can deduct directly from its state corporate income tax liability (in addition to the usual deduction against taxable income)
Personal Income Tax Increase/Decrease	Dummy variable equal to 1 in the year of state personal income tax (either tax rate on wages, or tax rate on long capital gains) increase/decrease for the firms headquartered in state s, else zero. State taxes on wages is the maximum state tax rate on wage income, estimated for an additional \$1,000 of income on an initial \$1,500,000 of wage income (split evenly between husband and wife). The taxpayer is assumed to be married and filing jointly. State capital gains tax rates is the maximum state tax rate on long-term capital gains

Appendix A.1: Description of Variables (Continued)

Variable Name	**Description**
Marginal Tax Rate	Blouin, Core, and Guay (2010)'s simulated marginal tax rates (after interest expense)
Exposure to Tax	Proportion of firm activity that takes place within the borders of the state that experiences the tax change. Exposure of firms to state-level tax changes is estimated from the degree of operations parent firms and their subsidiaries have in each state. We use apportionment rules in the state to measure exposure to tax changes for combined reporting states and assume that the exposure by non-combined reporting states is their aggregated fraction of sales
Combined	Dummy variable equal to 1 if the firm applying for the patent is located in a state with mandated combined reporting, else zero. States that require combined reporting in our sample period are OR, MT, ID, CA, AZ, UT, CO, NE, KS, ND, MN, IL, NH, ME, AK, and HI
New Product Announcement	We implement event study methodology by fitting a market model over (-246,-30) period to get the expected returns on the firm's stock, and then estimating cumulative abnormal returns over three (-1,1) day period around the announcement. To estimate the total number of material firm's announcements over the year, we either (a) sum all positive cumulative abnormal returns over the year, or (b) count the number of announcements with the cumulative abnormal returns above 75 percentile in the sample. Please see Section 3.6.1 for detailed description
Net Leavers	The difference between Leavers and Hires. Leavers refer to the number of the inventors who have produced a patent at the sample firm within one past year or a year before but produce at least one patent at a different firm, including the inventors who produce their last patent in the sample. Hires refers to the number of inventors who produce at least one patent at the sample firm after producing a patent at a different firm within one year and a year after, including the inventors who file their first patent with sample firm
Top Tax Increase/Decrease	Dummy variable equal to 1 in the year of corporate tax increase/deduction in top brackets and surcharges for the firms headquartered in state *s*, else zero. We only keep tax changes in top brackets and surcharges, which are progressive in nature and do not consider non-progressive tax changes like NOL suspensions

Appendix A.1: Description of Variables (Continued)

Variable Name	**Description**
σ(Citations)	Standard deviation of citations in next 5 years of patents applied by firm i in financial year t
Zero Cite Patents	Total number of patents applied by firm i in financial year t with zero citations in next 5 years
Highly Cited Patents	Total number of top 10% most cited patents in a year applied by firm i in financial year t
50% Votes Necessary to Pass Any Type of Tax Increase	Dummy variable equal to 1 if 50% votes is needed in the state legislatures to pass a tax increase
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
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
No Party Has Simple Majority in Both Houses	Dummy variable equal to 1 if neither Democrat nor Republican have control of both chambers
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
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)

Appendix A.2: Instrumental Variables Approach

In this appendix we employ an instrumental variables regression approach, exploiting state-level differences in the majority provision required to pass a tax increase, and its interaction with state partisan balance.

Specifically, we look at three categorical variable instruments. Our first instrument is a state-level dummy variable that takes a value of one if the state in question requires more than 50% votes in its legislatures to pass a tax increase. Our second (third) instrument is a dummy variable that takes a value of one when Democrats have enough legislators (no party has enough legislators) in the state legislative chambers to pass a tax increase. Of course, a one-party majority or supermajority (which is correlated with our second and third instruments) might have an effect on firm innovation through policies other than taxes. However, in our instrumental variable tests, we carefully control for such potentially direct effects on innovation of the underlying level of simple majority of any party in the state. We thus attempt to examine *incremental* explanatory power of our latter two instruments, coming solely from states where parties can have a majority but not enough legislators to pass a tax increase, and vice-versa.[25]

Our instruments are likely to satisfy the exclusion restriction 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, controlling explicitly for other types of majority, it is likely that our variables affect innovation only through the tax channel.

The state partisan balance data in this section is from Klarner (2003), as well as from the updates available on the State Politics and Policy Web site.[26]

We start our analysis by examining whether our instruments indeed predict future changes in taxes. The evidence is presented in Table A.1, column (1), shows that a list of macroeconomic variables we consider do not predict tax changes. However, this is not to say that the state's economic condition does not affect taxes, but rather that our list of *observable* past economic variables are not good predictors.

To this effect, in columns (2) and (3), we add our political variables. The results show that: (1) tax increases are 8.8-9.1% less likely when the state in question has a supermajority requirement for tax increases in place; (2) tax increases are 12.4-14.1% more likely when the Democratic Party has the majority required for a tax increase in a state's legislatures; and, (3) tax increases are 9.5% more likely when neither party has the majority required for a tax increase in the state's legislatures, with the latter two estimated in comparison to the base scenario of

[25]A point to note here is that the state level requirements we examine apply only to tax increases, so in this appendix we do not compare increases vs. decreases.

[26]See http://www.indstate.edu/polisci/klarnerpolitics.htm.

Republicans having enough legislators in both chambers. As mentioned above, note that these coefficients measure the *incremental* explanatory power of the political balance variables, since we directly control for Democrat (and no party) simple majority unrelated to tax increase laws, as well for as Democrats (and no party) having the required numbers to pass the budget (which is sometimes also subject to supermajority requirements). Of particular note is the fact that our instruments continue to be significant in the presence of the budget majority dummy. This shows that the difference between majority provisions required to pass a tax increase and that required to pass the budget matters. This is a 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.

However, the F-statistics for the joint significance of these instruments is less than 5 in all of our specifications (Table A.1). This implies that our instruments are weak – tax changes are hard to predict. The problem with the standard point estimator, when instrumental variables are weak, is that it can have severe bias and incorrect standard error distributions (Andrews and Stock (2005)). Thus, following Andrews and Stock (2005), we use fully weak IV-robust confidence intervals, based on the the Anderson-Rubin (AR) test.[27] Specifically, confidence intervals are formed by inverting tests that are robust to weak instrumental variables. That is, a confidence intervals for a parameter β, say, is the set of points $[\beta_1, \beta_2]$ for which a weak instrumental variable robust test fails to reject the null hypothesis H_0: $\beta = \beta_0$.

In the results presented in Table A.2, we use all state-level macro variables in Table A.1 and firm level variables in Table 2 as controls, except for our three instruments. Instead, we add predicted values of the tax increase variable based on the state-level regressions of Table A.1, taking care to ensure that our confidence intervals account for such two-step estimation. Results presented in columns (1)-(3) of Table A.2 correspond to the regression model in column (2) of Table A.1 (predictive power of instruments over and above the direct effect of Democrat/no party majority), while those in (4)-(6) correspond to column (3) of Table A.1. Our evidence shows that the instrumented tax increase variable significantly affects innovation in the third year after the tax change, while we cannot reject a hypothesis of no effect of taxes on innovation in the preceding years.

Unfortunately, although we are able to establish in this analysis that tax increases indeed have a negative effect on future innovation, our weak instruments do not allow us to provide precise point estimates of magnitudes.

[27] Andrews and Stock (2005) write, "We therefore have focused on testing and CIs (confidence intervals) for weak IVs for which a solution is closer at hand than it is for estimation."

Table A.1: 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 further include state partisan balance variables for tax increase and variables indicating overall state partisan balance. In column (3) we include state partisan balance needed to pass the budget as additional predictors. All regressions are with state 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.

	Tax Increase Dummy		
	(1)	(2)	(3)
More than 50% Votes Necessary to Pass any Type of Tax Increase		-.091 (.041)**	-.088 (.041)**
Democrats Have Sufficient Majority for Passing a Tax Increase in Both Houses		.124 (.043)***	.141 (.043)***
No Party Has Sufficient Majority for Passing a Tax Increase in Both Houses		.095 (.036)***	.094 (.040)**
Democrats Have Simple Majority in Both Houses		-.019 (.034)	
No Party Has Simple Majority in Both Houses		-.017 (.024)	
Democrats Have Sufficient Majority to Pass Budget in Both Houses			-.038 (.035)
No Party Has Sufficient Majority to Pass Budget in Both Houses			-.015 (.026)
Budget Deficit as % of $GSP_{s,t-1}$	.018 (.016)	.020 (.017)	.020 (.017)
Taxes as % of $GSP_{s,t-1}$	-.020 (.015)	-.016 (.016)	-.017 (.016)
$Log(GSP)_{s,t-1}$	.104 (.105)	.161 (.109)	.152 (.109)
Real GSP $Growth_{s,t-1}$	-.0006 (.003)	-.001 (.003)	-.001 (.003)
Unemployment $Rate_{s,t-1}$	-.014 (.009)	-.008 (.010)	-.008 (.010)
Obs.	824	807	807
F-Stat	NA	3.56	4.21

Table A.2: Instrumental Variables Approach

This table presents instrumental variables regression confidence intervals for our main variable of interest, Tax $\text{Increase}_{s,t}$. These CIs are constructed by inverting weak IV robust tests of coefficient significance. We present 95% CIs constructed by inverting Andersen and Rubin (AR) weak IV robust test. The dependent variable in our regression equation is Δ Ln(1+#Patents)$_{i,s,t+k}$ where i, s, $t+k$ index firms, states, years with $k = 1$ to 3. All specifications include all firm-level controls in Table 2, all state-level macro controls in Table A.1, as well as firm and year fixed effects. In column (1)-(3) we include variables indicating overall state partisan balance and state partisan balance needed to pass the budget in column (4)-(6). All CIs are robust to heteroskedasticity and clustering of standard errors at the state-level. *** indicates that the instrumented coefficient is significantly different from zero at the 99% level (that is, the 99% CI does not contain zero).

Instruments	More than 50% votes necessary to pass any type of tax increase, Democrats have sufficient majority in state legislatures for passing a tax increase, and No party has sufficient majority in state legislatures for passing a tax increase					
	Δ Ln(1+Patents)$_{t+k}$					
	k=1	k=2	k=3	k=1	k=2	k=3
	(1)	(2)	(3)	(4)	(5)	(6)
AR	[-.0606, 1.51]	[-.0606, 1.67]	[-.585,-.0202]***	[-.0606, 1.232]	[-.141, 1.353]	[-.424,-.0202]***
State partisan balance controls	Democrats have simple majority in both houses No party has simple majority in both houses			Democrats have sufficient majority to pass budget in both houses No party has sufficient majority to pass budget in both houses		
Other controls	State level controls from Table A.1, firm level controls from Table 2, firm and year FEs			State level controls from Table A.1, firm level controls from Table 2, firm and year FEs		
Obs.	42026	37167	32424	42026	37167	32424

Appendix A.3: Taxes in a General Equilibrium Model of Innovator Incentives

In this Appendix we summarize relevant sections of the structure in Jaimovich and Rebelo (2012), and show that corporate tax changes affect agents' decision whether to work as innovators or regular workers, and this decision has further implications for the number of patents and products produced in the economy.

Production: Final-good producers operate a constant-returns-to-scale production function that combines labor (L) with a continuum of measure n of intermediate goods (x_i):

$$Y = L^{\alpha} \int_0^n x_i^{1-\alpha} di$$

The final goods producer maximizes overall after-tax profits, given by:

$$\pi^f = (L^{\alpha} \int_0^n x_i^{1-\alpha} di) - \int_0^n p_i x_i di - w.L)(1-\tau)$$

where p_i is the price of the final good, w is the wage rate, τ is the corporate income tax rate.

The first order conditions for this problem yield:

$$p_i = (1-\alpha) L^{\alpha} x_i^{-\alpha} \tag{1}$$

$$w = \alpha L^{\alpha-1} n x_i^{1-\alpha} \tag{2}$$

with $\pi^f = 0$ in equilibrium.

Innovators own permanent patents on the intermediate good that comes out of their innovation, so each innovator is a monopolist over his intermediate good. Each unit of the intermediate good uses η units of the final good. So the profit from each intermediate good is given by

$$\pi^i = (p_i - \eta) x_i (1-\tau) \tag{3}$$

From (1) and (3), shows the optimal price-quantity pair is:

$$p_i = \frac{\eta}{(1-\alpha)}$$

$$x_i = L.[\frac{(1-\alpha)^2}{\eta}]^{\frac{1}{\alpha}} \tag{4}$$

These expressions demonstrate a key source of confusion that can affect studies like ours – in partial equilibrium set-ups, the price or quantity of the innovation good produced is not directly affected by corporate taxes (see above expressions, for example). This is because, while tax expenses increase with a higher tax rate, given tax deductibility of investments in innovation, tax benefits also rise.

However, as demonstrated below, in a general equilibrium setting, this does not imply that taxes do not matter for innovation. Even if price and quantity do not change in a partial equilibrium, the size of the after tax profits decline, which reduces the pie available to innovators and thus their incentive to innovate. Such an effect elicits a general equilibrium response in terms of occupational choice, making some innovators switch to less innovative tasks (become regular workers in the model), and thus affecting aggregate innovation in response to tax changes.

Given the structure of the model, all producers choose the same p_i and x_i yielding the after-tax level of (maximized) profits:

$$\pi = \alpha(1-\alpha)^{\frac{2-\alpha}{\alpha}}\eta^{\frac{-(1-\alpha)}{\alpha}}L(1-\tau) \tag{5}$$

From (2) and (4), the wage rate equals:

$$w_t = \alpha n_t[\frac{(1-\alpha)^2}{\eta}]^{\frac{1-\alpha}{\alpha}} \tag{6}$$

<u>The agent's optimization problem:</u>

Agents differ in their innovative ability, a. An agent with the ability a can produce $\partial a n_t$ new goods if he chooses to be an innovator. The utility, which an agent with ability a maximizes, is given by:

$$U(a) = \int_0^\infty e^{-\rho t}\frac{C_t(a)^{1-\sigma}-1}{1-\sigma}dt \tag{7}$$

with $C_t(a)$ denoting her consumption in period t.

In each period, the agent has to choose whether he will work in the job that gives him the best chance of coming up with a successful innovation, or move elsewhere in the job market. For modelling simplicity, we model the extreme case where he either remains an innovator, or switches to being a worker (a zero-one decision to innovate).

If he chooses to be a worker, he gets a per-period wage w_t. Therefore, the period-by-period budget constraint is:

$$\dot{b_t}(a) = r_t b_t(a) + w_t l_t(a) + m_t(a)\pi_t + \frac{\pi^f}{H} - C_t(a) + \frac{T_t}{H} \tag{8}$$

where $l_t(a) = 1$ if the agent chooses to be a worker in period t (0 otherwise); $b_t(a)$ is the agent's bond holding in period t; r_t is the real interest rate; T_t is the total (lump-sum) transfer from the government; H is the population size. $m_t(a)$ is the number of patents owned by an agent of ability a at time t. Assume that individuals with the same ability start from the same endowment of patents, and also that the initial endowment of bonds is 0. Then the equation describing the motion of m_t is:

$$\dot{m}_t(a) = \partial a n_t[1-l_t(a)] \tag{9}$$

The no-Ponzi condition for bonds is $\lim_{t\to\infty} e^{\int_0^t r_s ds} b_t(a) = 0$. This condition means that the agent cannot always plan to finance consumption by borrowing very large amounts.

Solving the model:

Since the structure of the model follows Jaimovich and Rebelo (2012), we only state the conditions that are relevant in our setting. The reader interested in the details of equilibria in the different markets is kindly referred to Jaimovich and Rebelo (2012).

Choosing between being a worker and an innovator is determined by a threshold ability-level $a^\star$, such that any agent with innovative ability greater than $a^\star$ becomes an innovator, while those with lower abilities become workers:

$$a^\star \partial n_t \frac{\pi}{r} = w_t \tag{10}$$

The number of new products C (which is also the number of new patents here) is then:

$$\dot{n}_t(a) = H \partial n_t \int_{a^\star}^{a^{max}} a dF \tag{11}$$

where $F(a)$ is the cdf of a, and a^{max} is the maximum value of a in the data.

This condition simply states that since anyone with ability greater than $a^\star$ innovates, the aggregation of their innovations gives the total per-period volume of patents in the economy.

The equilibrium condition in the labor market is then:

$$H \int_{a_{min}}^{a^{max}} l_t(a) dF = L \tag{12}$$

where a_{min} is the lower bound of innovative ability in the data (which can be 0 or otherwise).

The fraction of the population that works in the final goods sector (not innovators) is:

$$L = H.F(a^\star) \tag{13}$$

The threshold value of $a^\star$ in the general equilibrium of this model is then:

$$\partial H(1-\alpha) a^\star F(a^\star)(1-\tau) = \sigma \partial H \int_{a^\star}^{a^{max}} a dF + \rho \tag{14}$$

Condition (14) above shows that as the corporate tax rate τ rises, the threshold level determining occupational choice, $a^\star$, has to increase.

Given equation (11), which determines the law of motion for patents/products, this implies that the number of patents/products generated in equilibrium must decline as corporate taxes increase.[28]

[28] Note that as none of our discussion above referred to any distributional assumptions for F(.) that Jaimovich and Rebelo (2012) make, the result that patenting declines following an increase in corporate taxes is general.