Entrepreneurship and State Taxation

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Abstract

Entrepreneurship plays a vital role in job creation, productivity growth, and occupational choices. Yet there exists little well-identified research into the effects of business taxation on new firm activity and growth. Using recently developed county-level data on startups, we examine the effect of states' tax rates on new firm activity and test for cross-border spillovers in response to these policies. We find that startup activity is negatively—and disproportionately—affected by corporate tax rates. Our results are robust to changes in the tax base and other state-level policies. We find little evidence of an effect of personal and sales taxes on entrepreneurial outcomes.

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1 Introduction

Entrepreneurship is of vital importance to the US economy. New firms contribute disproportionately to both gross and net job creation, play a major role in business cycles and account for an outsized share of the innovation and aggregate productivity growth that raises living standards.[1] Additionally, entrepreneurship is seen by many as an essential element of the occupational choice set (Hurst & Pugsley 2011). Given this importance, it is not surprising that there exist large strands of literature studying national trends in entrepreneurial activities, determinants of entrepreneurship and the economic effects of entrepreneurial activity. One question that remains open is the extent to which public policies can either foster or hinder entrepreneurship. Understanding the link between policies and entrepreneurship has proven difficult due to limited data on entrepreneurial activity and a lack of credibly exogenous policy variation.

While there are many actions governments may take to affect entrepreneurship, few are as important or contentiously debated as the setting of tax policy. Taxes are viewed by many as the primary lever elected officials have at their disposal to change the business environment, promote growth and encourage innovation. The wide-ranging views on taxes and their economic effects are reflected both in the considerable attention given to them by politicians and in the substantial differences in observed tax rates across states and over time.[2]

To elucidate the taxation - entrepreneurship relationship, we begin by incorporating taxes into a canonical model of firm dynamics and exploring the mechanisms by which taxes affect new firm entry. We then empirically test this relationship by exploiting quasi-experimental variation in corporate, personal and sales taxes across state borders to provide some of the first causal evidence of their effect on startup activity. We do this using the recently released Quarterly Workforce Indicators (QWI) firm age dataset. The QWI is the first dataset to provide detailed publicly available county-level information on job creation, job destruction and other key labor market variables for firms in narrowly defined age categories, including firms with age less than two years. Using this new data resource, we isolate plausibly exogenous variation in state-level taxes over time and across state borders and examine how entrepreneurial activity responded in counties that experienced a change in their state corporate, personal, or sales tax rates relative to bordering counties whose state did not change

[1]See Haltiwanger *et al.* (2013) on job creation, Adelino *et al.* (2017) and Pugsley & Sahin (2015) on business cycles and Bartelsman & Doms (2000); Foster *et al.* (2001; 2006; 2008); Petrin *et al.* (2011); Alon *et al.* (2017) (among others) on innovation and productivity.

[2]For example, Wyoming has a corporate tax rate of zero while Iowa has a corporate tax rate of 12 percent. See Table 1 for states' corporate, personal and sales tax rates and Figures 1, 2 and 3 for the changes states have made to these tax rates.

rates. We explicitly test for the presence of spillovers of entrepreneurial activity across borders and use a variety of specifications that exploit different levels of variation and include different measures of policies that affect the tax base. As in much of the recent descriptive literature, we adopt an age-based definition of entrepreneurship; in particular, in the present study we define both "entrepreneurs" and "startups" as firms with age less than two years. To the best of our knowledge, this paper is the first to attempt and recover carefully identified estimates of the effects of these policies on economic activity of startup firms in the U.S.[3]

Using our border county identification strategy, we find that increases in corporate tax rates have a statistically and economically significant negative effect on employment among startup firms. Specifically, for every one percentage point increase in the corporate tax rate employment in startup firms declines 3.7 percent. In all specifications, new firm activity is shown to be considerably more responsive to corporate tax shocks than incumbent firm activity. Personal tax rates have no detectable effect on startup activity, and state sales tax rates have negative but typically statistically insignificant effects. We run a number of additional models that more deeply examine the corporate tax results. We also explicitly examine the extent to which our results reflect the shifting of activity across state borders, allowing us to test the internal validity of our estimates and identify entrepreneurs' ability to relocate their economic activity; we find no evidence that our estimated effect of corporate tax rates reflects increased activity across the border from tax-raising states. More broadly, our results are robust to a variety of specifications including models that omit states in which a majority of activity occurs along borders. We compare our results to a straightforward state-level panel model whose estimates, when statistically significant, suggest modestly larger negative effects of corporate taxes on employment.

Although novel in many respects, ours is not the first study to examine the relationship between taxation and measures of entrepreneurial activity. An earlier set of papers explored the theoretical relationship between tax policy and entrepreneurship (Gentry & Hubbard 2000; Cullen & Gordon 2007) and provided some evidence on entry into self-employment using data from the 1990s and earlier.[4] Other research has used more aggregated data sources to explore these relationships (Bruce & Deskins 2010; Da Rin *et al.* 2011; Djankov *et al.* 2010; Garrett & Wall 2006; Georgellis & Wall 2006; Primo & Green 2011).[5]

[3]A lack of geographically disaggregated publicly available data on new firm activity has hindered such research. The QWI data used in this paper were not released until 2014. Other related studies on entrepreneurial activity and policies such as Cullen & Gordon (2007); Mukherjee *et al.* (2017); Rathelot & Sillard (2008); Rohlin *et al.* (2014) are discussed below.

[4]Further discussion of these papers and the broader literature can be found in Section 2

[5]Bruce & Deskins (2010) explores the share of workers who are sole proprietors and the share of tax returns reporting small business income, finding little effect of tax rates but some evidence for an effect of combined

An exciting recent literature has applied more advanced causal inference techniques to estimate the effect of tax policies on proxies for entrepreneurship. Rathelot & Sillard (2008) identify the effects of corporate tax rates on new business creation using French microdata and a regression discontinuity design, finding that corporate taxes moderately reduce entry. Aghion *et al.* (2017) find that self-employment is responsive to tax code complexity, and Moretti & Wilson (2017) examine the extent to which star scientists migrate in response to state taxation changes. Rohlin *et al.* (2014) explore the role of reciprocal agreements on tax avoidance and new establishment location using data from 2002 and 2005. Mukherjee *et al.* (2017) show that corporate taxes reduce new product development and R&D activity. Similar to our paper, these latter two papers exploit policy discontinuities at state borders to identify the treatment effect on their respective outcomes.

This paper illustrates the particular sensitivity of young firms to policy shocks while extending the tax literature in several important ways. First, as already discussed, we exploit the detailed geographic measures of startup activity from newly available QWI data to derive what we believe to be the most convincing causal estimates of the impact of policies on overall entrepreneurship levels in the United States. The county-level data on startup firm activity allows us to perform recently developed econometric techniques that account for a wide array of endogeneity concerns. Second, since various components of tax policy can change simultaneously, we include multiple tax rates (corporate, personal, and sales) set by state governments as well as measures of the tax base which, as shown by Suárez Serrato & Zidar (2017), are crucial in determining the *effective* tax rate that firms face. Third, rather than focus on self-employment (e.g., Aghion *et al.* (2017)) or superstar inventors (e.g., Akcigit *et al.* (2016); Moretti & Wilson (2017)), we adopt an age-based definition of "entrepreneurship" that measures key outcomes of young employer firms: employment and job creation.[6] While we see self-employment and superstar inventors as vital components of the broad entrepreneurship picture, the development of the QWI creates an opportunity to focus on age and employment for all job creating new businesses.

reporting requirements. Using instrumental variables and cross-country European panel data, Da Rin *et al.* (2011) find that corporate tax rates reduce firm entry consistent with previous theory on incorporation incentives. Djankov *et al.* (2010) use cross-sectional data on 85 countries and find a strong negative relationship between corporate tax rates and investment and firm entry. Garrett & Wall (2006) find a negative relationship between sole proprietorship activity and corporate tax rates, minimum wages, and stringency of bankruptcy laws, and no relationship with personal tax rates. Georgellis & Wall (2006) find a U-shaped relationship between top personal tax rates and sole proprietorship activity with rising rates initially being associated with falling entrepreneurship, but having a reverse effect after top rates exceed 35 percent. Using a sole proprietorship measure as well as an additional measure based on venture capital expenditures, Primo & Green (2011) finds a negative role for bankruptcy law on sole proprietorship and venture capital measures.

[6]Note also that our definition of entry describes new firms only. New establishments of existing firms (e.g., a new Walmart location) do not count as startups in our framework.

Indeed, defining entrepreneurial activity by firm age is an important middle ground between self-employment, a large group of workers who may or may not have aspirations of growth and innovation, and superstar inventors who represent tremendous potential for productivity-enhancing growth but will only be responsible for a small fraction of overall job creation in the economy. Haltiwanger *et al.* (2013) show that the job creation role that is commonly attributed to small businesses—such as the sole proprietors studied in much of the literature—is more appropriately attributable to young businesses, since most small businesses create few, if any, jobs after their initial founding. While it is true that the growth of young firms tends to be accounted for by relatively few firms in any given cohort (Decker *et al.* 2014), it is also likely that this set of elite high-growth firms is broader than the extremely small collection of firms that count as superstar inventors or receive funding from venture capitalists.[7] Moreover, from the standpoint of a policymaker, policy levers for new firm creation are likely to play a prominent role in fostering a dynamic local economy. The existing stock of small businesses will mostly reflect economic developments of past years (or decades), and the exit of such small businesses often reflects lifecycle concerns of owners more than current policies.[8] Our results show that young employer firms are particularly susceptible to tax policy shocks relative to established firms, adding to existing work finding that young firms are especially sensitive to credit conditions (Fort *et al.* 2013), demand shocks (Adelino *et al.* 2017; Decker *et al.* 2018b), and idiosyncratic productivity shocks (Decker *et al.* 2018a). In these respects, we view our contribution as an important expansion of the literature.

A further motivation for the study is to shed light on the steady decline in rates of new firm formation and other measures of economic dynamism in the U.S. over the past several decades (Decker *et al.* 2014). Declining rates of entrepreneurship plotted in Figure A1 may be a concern for several reasons. First, as noted above, young firms are important for

[7]Decker *et al.* (2016) report high annual growth rates among firms at the 90th percentile of the employment-weighted growth rate distribution; these 90th percentile growth rates are such that a 50-employee firm almost doubles in size over one year. The set of firms growing at rates faster than the 90th percentile is much broader than the set of firms that is restricted to the superstar inventors of Akcigit *et al.* (2016) or the set that see the extreme success outcomes of Guzman & Stern (2016), yet these 90th percentile employer businesses are making significant contributions to job growth. The importance of this set of high-growth (yet perhaps not extreme superstar) firms motivates our focus on the employer universe.

[8]Importantly, our definition of entrepreneurship is based not only on firm age but also on an employer concept of business activity. In the QWI, a business must have payroll employees to count as a new firm. This contrasts with self-employment measures that can include non-employers. While restricting ourselves to employers does result in the omission of many business entities, it also clarifies our unit of observation by abstracting from the wide range of activities that comprise self employment (e.g., the gig economy, non-active owners of non-employer holding companies, etc.) and by avoiding measurement complications in widely used nonemployer data sets such as CPS (see, e.g., Abraham *et al.* (2017))

aggregate job and productivity growth; the decline in entrepreneurship has been associated with a decline in productivity growth (Alon *et al.* 2017; Decker *et al.* 2017; 2018a) as well as a decline in the job reallocation and labor market fluidity that is important for wage growth (Hyatt & Spletzer 2016). Second, declining opportunity for new business creation may imply a restriction of the occupational choice set of relevance to potential lifestyle entrepreneurs (Hurst & Pugsley 2011) and may concern local policymakers interested in the vitality and dynamism of local economies (EIG 2017). Fully understanding the consequences of declining entry requires evidence about frictions that potentially reduce entrepreneurial activity. Our estimates of the tax policy determinants of entrepreneurial activity shed light on the levers that policy makers have at their disposal to address this concerning trend.[9]

More broadly, this paper adds to a growing literature that examines the role subnational policies play in determining entrepreneurial and reallocative outcomes. Black & Strahan (2002) and Kerr & Nanda (2009) find that increased access to finance spurred by state banking deregulation during the 1970s, 1980s and 1990s boosted new incorporation activity and firm entry, respectively. Autor *et al.* (2007) find that wrongful discharge protections reduce entry of new establishments. A smaller literature has also emerged studying the impact of environmental regulations on economic dynamics (List *et al.* 2003; Walker 2011; Curtis 2014).

Aside from the entrepreneurship focus described above, the policies studied in this paper have recently received considerable attention in other contexts. Our research design based on state border counties follows Ljungqvist & Smolyansky (2016), who find that state corporate tax increases have modest negative effects on overall employment while tax cuts only have effects during recessions. Studying multi-state firms, Giroud & Rauh (2015) find that overall employment and establishment counts of C-corporations and pass-through entities are sensitive to corporate and personal tax rates, respectively. Suárez Serrato & Zidar (2016) use state variation in tax policy to study the incidence of taxation in a structural setting, finding that firm owners bear somewhat under half of the incidence of corporate taxes, with about a third borne by workers and the remainder borne by land owners.[10] Our contribution is to focus

[9]Directly studying the relationship between changes in tax policy and secular trends in startup activity over recent decades is outside the scope of this paper. However, in unreported results, we estimate naive regressions of state-level changes in startup employment shares on state-level changes in corporate, personal, and sales tax rates for the 2000-2013 period. We find that changes in corporate tax rates in this "long difference" setup are negatively related to changes in startup activity. That is, states that increased their corporate taxes more saw bigger declines in startup activity. Coefficients on personal tax rate changes are generally close to zero, while coefficients on sales tax rate changes are positive or close to zero. Only the corporate tax rate relationship is close to statistical significance. These results are not central to our purpose here, but they do suggest that understanding the policy determinants of startup activity generally can help shed light on recent trends.

[10]Akcigit *et al.* (2016) and Moretti & Wilson (2017) find that the location decisions of "superstar inventors" are responsive to tax rates. Fajgelbaum *et al.* (2015) find that variation in state tax rates is a significant source of spatial misallocation. Bartik (2017), Wilson (2009) and Chirinko & Wilson (2016) study other tax policies (such

specifically on the implications of these policies for entrepreneurial activity.

The remainder of the paper is organized as follows. Section 2 provides background information and details of the policies studied. Section 3 explores theoretical considerations relating tax policy and entrepreneurial activity through the lens of canonical models of firm dynamics. Section 4 describes the main data sources of the paper. Section 5 provides the econometric models used in the paper and the results from those models. Section 6 discusses the results. Section 7 concludes.

2 Policy Background

States have wide-ranging powers to set economic policies that affect the business environment. As such, there is considerable variation in the policies at the state level. We begin by focusing on three of the primary tax rates set by states and later explore a range of other policies for which there is cleanly defined state-level data. As discussed below, a key benefit of examining multiple policies together in the same framework is that states' decisions to change one policy may occur simultaneously with changes they make to other other policies.

2.1 Corporate Income Tax Rates

Most states use taxes on corporate income that are similar to the corporate taxes imposed at the federal level. Broadly speaking, firms in these state are taxed on a measure of the profits they earn. A few states, however, impose their primary corporate taxes on gross receipts, asset base, or other business outcomes, while a few states have no business income tax of any kind. For our purposes, we focus only on states with income taxes and exclude states with either gross receipt taxes or states that switched from gross receipt taxes to income taxes during our period of study. As shown in Figure 1, there is considerable change in these rates over time.

In Section 3 we describe a simple illustrative model framework for considering possible effects of tax changes on entry and young firm activity. In short, taxes may affect measured employment at startups through several channels. First, potential entrepreneurs deciding whether to start a firm must make significant upfront investments to enter; much of this investment is not tax deductible. Entry occurs when a prospective entrepreneur's expected

as job creation tax credits, R&D credits and reciprocal agreements between states). Additionally, an extensive literature has examined the consequences of minimum wages, which we include in some specifications, for employment levels and worker flows (Card & Krueger 1994; Dube *et al.* 2010; Neumark *et al.* 2014; Dube *et al.* 2016; Meer & West 2016).

after-tax profits from production exceed the costs of these upfront investments (embodied by a single entry cost in our illustrative model). At the margin, tax liability reduces economic profits, restricting the set of potential entrepreneurs whose expected profits exceed entry costs. This mechanism can be particularly salient in the presence of revenue function curvature (such as decreasing returns to scale technology or imperfect competition) that dampens the responsiveness of existing firms to shocks. Second, conditional on entry, tax policy affects labor demand via the dependence of firm-level labor demand on other production or revenue factors (some of which have costs that are not tax deductible). If some production or revenue factors depend on profitability conditions at entry due to, for example, profit-related financial constraints or profit-dependent accumulation of customer or supplier relationships, then firms may enter smaller or even grow more slowly after entry.[11] Third, there are likely to be important general equilibrium channels of tax policy; tax increases may result in lower wages (which may mitigate direct negative effects on entry) or weaker demand conditions (which may augment direct effects).

Firms can differ in the extent to which they face their state's corporate tax rates. Apportionment formulas are used to determine corporate income tax liability for multi-state firms. Many states equally weigh companies' payroll, sales and property to determine the location of taxable economic activity, but in recent years, many states have shifted to place higher weight on sales rather than employment.[12] Additional corporate tax variation comes from legal form of organization concerns. With few exceptions, corporate tax rates apply only to C-corporations; as discussed in Cooper *et al.* (2015), "Pass-through" entities, which are typically subject to different tax rates, have become increasingly popular in the past thirty years. Our county-level data do not allow us to distinguish between C-corporations and pass-through entities.

There is an important point about legal form of organization to be made here. In most states, firms organized as LLCs, S-corporations, sole proprietorships or partnerships will not be directly affected by changes to the corporate tax rate. The earnings of these firms are subject to personal income tax rates. Entrepreneurs may select different organizational forms based on existing corporate and personal tax rates; Chen *et al.* (2017) model the choice of legal form in a DSGE setting and argue that a reduction in corporate taxes not only incents substitution toward the C-corporation form but also boosts employment due to businesses of that form facing looser capital constraints.[13] With our data we are not able to isolate the

[11]For example, if advertising is particularly important for new firms without name recognition, pressure on profits and ability to fund advertising could disproportionately affect new firms. This may be thought of as a potential extension of models like that in Moreira (2017) (see also Foster *et al.* (2016)).

[12]In extensions of the main results we incorporate states' apportionment rules directly into our analysis.

[13]The legal form of organization decision for any business is complex, but there can be a number of advantages

impact of corporate tax changes on C-corporations (as is done by Giroud & Rauh (2015)).[14] Nonetheless, the likely heterogenuous effect of tax rates on firms in no way diminishes the importance of a key question: how do tax rate changes affect *overall* employment at firms of various ages? If few firms are affected or if entrepreneurs choose a different legal form of organization in response to changes in relative tax rates, then we will find muted, or perhaps zero, effect of taxes on overall employment. This finding would certainly be relevant. Given that pass-throughs may not be directly affected by changes to the corporate tax rate, the fact that we still find strong effects of the corporate tax rate implies that there are many firms that are either directly or indirectly affected. The overall effect remains a vitally important policy and economic elasticity, and it suggests that the effect on C-corporations is larger than the estimated overall effect.

2.2 Personal Income Tax Rates

Most states impose personal income taxes that apply in addition to federal income taxes (though, in some cases, states allow federal tax payments to be deducted from state taxable income). Figure 2 shows historical movements in state personal tax rates. Personal tax rates could, in principle, affect young firm activity through three channels. First, personal tax rates directly affect businesses organized as pass-through entities through the same logic linking corporate tax rates with corporate business activity (see Section 3). In this case, the discussion about corporate taxes above applies equally to personal taxes. Second, as noted by Suárez Serrato & Zidar (2016), personal tax rates may indirectly affect young firm activity through their effect on local labor supply which, as mentioned above, is a key determinant of business entry. Finally, when personal tax rates are above corporate tax rates, some workers have incentives to become entrepreneurs (in corporate form) to reduce their tax liability.

to selecting the C-corp form. C-corps allow non-individual shareholders such as trusts, other corporations, aliens, and partnerships; partnerships share this advantage but S-corps do not (with a limited specialized trust exception). More generally C-corps allow for a wider distribution of ownership, are preferred by venture capitalists, and can go public. C-corps also more easily accommodate flexible use of non-wage compensation such as retirement benefits for shareholders and stock option plans. "Unreasonable compensation statutes" governing the allocation of personal compensation between wages and dividends are less stringent for C-corps than for S-corps. Guzman & Stern (2016) find that choice of corporate form is an important predictor of extreme startup success. There are, of course, offsetting disadvantages to C-corps as well, such as the well-known double taxation issue, but the decision is multidimensional with particular advantages of C-corps for those startups wishing to grow significantly.

[14] There are only limited available public data on business' legal form of organization (LFO), particularly for new firms. Dun and Bradstreet records included in the National Establishment Time Series (NETS) indicate that, in 2000, "corporations" accounted for 34 percent of new firms and 48 percent of employment at new firms, where S-corps and C-corps are not differentiated. In robustness checks described below we exploit county-level variation in corporate shares from NETS, which does not significantly alter our main findings, but NETS has a number of limitations that make us reluctant to exploit this variation more extensively (Barnatchez *et al.* 2017).

The opposite may be less true; that is, if corporate tax rates are increased above personal tax rates, some marginal entrepreneurs may leave entrepreneurship to become workers, but they may also choose to remain entrepreneurs but under pass-through legal forms (though changing legal form can be costly, and opportunities to do so are limited; this margin is of little relevance to our work since we focus on recent entrants). This channel (of the relative rates of corporate and personal taxes) is more complex than it seems, however, since the interaction of personal and corporate tax rates also affects the riskiness of entrepreneurial endeavors; Cullen & Gordon (2007) find that entrepreneurial risk taking is actually increasing in the personal tax rate (holding the corporate rate constant). Given these considerations, the likely overall effects of personal tax rates on the level or share of entrepreneurial activity are considerably less clear cut than corporate tax rates. Yet another layer of ambiguity arises from the fact that the pass-through income that is subject to personal tax rates in a given state may be at least partially taxed in the state of residence of firm owners, which may not actually be the location of the entrepreneurial activity. Hence, while we see personal tax rates as an important element of our empirical exercises, there is less consensus about the overall effect of this tax given the variety of pathways through which personal tax rates may affect startup activity.[15]

2.3 Sales Tax Rates

Most states levy a sales tax on retail transactions; the exceptions are Alaska, Delaware, Montana, New Hampshire, and Oregon, none of which have a state sales tax in our time sample. Headline sales tax rates do vary considerably over time, as shown on Figure 3. We include state sales taxes in our analysis due to their importance for raising revenue and because they are sometimes adjusted simultaneously with income taxes.

Importantly, sales taxes are also heavily used by local governments, with the majority of states allowing localities to set their own rates in addition to state rates.[16] We focus only on state-determined sales tax rates, consistent with our identification strategy of relying on policy variation determined outside of the counties we study. While our omission of local

[15]States' treatment of capital gains may also deter or incent venture capital activity, a key source of funding for certain startups, and entrepreneurs are frequently compensated through stock options whose payoff depends on firm performance. The future income from these stock options is taxed at capital gains tax rate which varies considerably across states. The treatment of capital gains taxation has long been thought to be a determinant of startup activity (Poterba 1989a;b) but there remains limited empirical work on this subject. Moreover, the set of startups to which these concerns apply is likely to be small. Puri & Zarutskie (2012) find that use of venture capital is rare, accounting for less than 1 percent of firms and less than 5 percent of employment.

[16]Combined sales tax rates at the local level can differ markedly from state rates; see https://taxfoundation.org/state-and-local-sales-tax-rates-2015.

sales tax rates is a limitation of our approach, for reasons discussed below it is not likely to result in exaggerated estimates of the effects of sales taxes on local activity due to the strategic nature of local tax policy determination (Agrawal 2015). Another source of measurement challenges is the occasional use by states of special tax rates for certain retail items, most commonly related to food, alcohol, motor vehicles, and tourism. We focus only on the main headline sales tax rate.

In theory, while the effect of a sales tax on existing firms differs in important ways from the effect of income taxes, in terms of new firms specifically the effects are similar (see Section 3). A sales tax reduces the value of entry from the perspective of potential entrepreneurs. This effectively raises the productivity threshold governing the number of entrants. Under common assumptions about the curvature of firms' revenue functions, the effect of such a tax on entry can easily be larger than the effect on incumbent firms whose optimal scale is only modestly changed by the reduction in profits. Interestingly, however, a simple sales tax does not have significantly different effects on different factors of production, unlike an income tax for which certain factors are deductible while others are not. Rather, a sales tax enters all first-order conditions in the same way as do productivity or demand factors.

2.4 Other State Policies

The above tax rates do not reflect the entirety of states' overall business environment. State and local governments employ a range of targeted policies that provide credits for certain activities or modify the tax base. Among these are tax credits states offer for expenditures on research and development (Wilson 2009) and job creation tax credits (Chirinko & Wilson 2016). Research and Development credits typically take the form of a statutory rate indicating a portion of the expenditures that can be used to offset income tax liability. Job creation tax credits incent firms to locate workers in a particular region through the use of tax credits. As discussed earlier, states' tax base policies, such as their apportionment formulas, are important in determining the tax burden that firms face. Suárez Serrato & Zidar (2017) find that tax base policies such as apportionment rules, credits for investment or research and development, loss carry forwards, and similar rules are economically significant and influence the responsiveness of economic variables to tax rates. Given our focus on entrepreneurship, this is potentially an important source of variation for our variables of interest. In addition to apportionment and R&D credits, we also examine loss carry forward provisions. These allow firms to use losses to offset tax liability in tax years subsequent to the realization of the loss and are typically specified in terms of the number of years into which losses can be carried. One final control we include is states' minimum wage policies which have been the subject of considerable research (Dube *et al.* 2016; Meer & West 2016).[17]

As mentioned above, including multiple policies together in the analysis is important. If changes in policies occur simultaneously then regressions that exclude other policies may be biased. The direction of this bias is not always clear. A state may, for example, increase corporate tax rates to compensate for reducing personal tax rates. On the other hand, negative shocks to a state's budget may require it to raise all taxes simultaneously. Our flexible approach allows us to control for multiple policies while also examining them individually. Furthermore, the border discontinuity method exploits variation in policy changes that is unlikely to be correlated with changes in local entrepreneurial or economic activity.

3 Theory

Consider the following simple static model. Firms vary by productivity (which may be technical efficiency, entrepreneurial ability, and even demand conditions) and produce using decreasing returns to scale technology with labor and capital as inputs (the implications would be similar if, instead of decreasing returns, firms had constant returns but faced imperfect competition in product markets). A business tax is imposed on income defined as revenue minus labor costs (i.e., cost of capital is not tax deductible); this tax is similar to the corporate tax rate and the personal tax rate for corporations and passthrough entities, respectively. Additionally, a sales tax is imposed on revenue. We follow Suárez Serrato & Zidar (2016) by assuming firms are financed entirely with equity. The firm's objective function is given by:

$$\max_{k,n}\{(1-\tau_i)((1-\tau_s)Azk^{\alpha}n^{\theta}-wn)-rk\} \tag{1}$$

where τ_i is the income tax rate (corporate or personal), τ_s is the sales tax rate, A is an aggregate productivity (or, in this simple setup, demand) factor, z is an idiosyncratic productivity factor that is heterogeneous across firms with $z \sim G(z)$, k is capital, n is labor, w is the wage, and r is the cost of capital.[18] The technological parameters α and θ determine returns to scale with $\alpha + \theta < 1$ to allow for a defined firm size distribution.

[17] With the exception of job creation tax credits, we include measures of each of these policies in our analysis. Data sources on state-level measures of job creation tax credits are available for about half of states. Furthermore, these data are often city or county-specific with state level measures created by summing up these city or county level policies. As such they are unlikely to reflect the credits that would be given in border counties.

[18] In this model framework, personal and corporate tax rates have the same effect on firms of relevant legal form. However, as we note in Section 2.2, in reality the effects of personal tax rates on businesses may be more complex than the effects of corporate taxes due to complications with occupational choice, legal form choice, progressivity, and so on. We view the theoretical predictions for personal tax rates as ambiguous, a view with some empirical support.

The direct effects of tax rates on firm outcomes act through the first-order conditions. The first-order condition for labor is given by:

$$n = \left[\frac{w}{\theta(1-\tau_s)Azk^{\alpha}} \right]^{\frac{1}{\theta-1}} \tag{2}$$

Observe that the income tax does not directly affect the labor decision due to the deductbility of labor costs, while the sales tax affects the labor decision in the same manner as the aggregate shock A and the idiosyncratic shock z. The first-order condition for capital is given by:

$$k = \left[\frac{r}{(1-\tau_i)(1-\tau_s)Az\alpha} \left(\frac{\theta(1-\tau_s)Az}{w} \right)^{\frac{\theta}{\theta-1}} \right]^{\frac{\theta-1}{1-\theta-\alpha}} \tag{3}$$

Because of the dependence of labor demand on capital demand, both capital and labor are decreasing in the income tax rate even though labor demand depends on income taxes only through capital demand. Sales taxes affect both labor and capital demand directly. Therefore, for any given z, firms are smaller when any tax rate is higher.

In this simplified model framework, the first-order conditions do not directly generate a mechanism that renders new firms particularly sensitive to tax rates, but they do highlight the link between employment and other production factors. Suppose there are production factors that depend on initial profitability and are particularly difficult for young firms to obtain. For example, if capital demand is restricted by financial constraints that are based on a combination of assets and profits, then young firms that have not had time to accumulate assets will be particularly sensitive to profit shocks, even shocks that affect all firms' profits equally. Similar intuition could apply for revenue factors such as brand recognition; if reduced profits makes advertising difficult and new firms are particularly dependent on advertising, then the size of new firms even after entry could be disproportionately affected by aggregate shocks to profits, such as a tax increase.

This intuition for how taxes could disproportionately affect recent entrants is not the only reason aggregate young firm employment may be particularly sensitive to taxes. Taxes can directly reduce young firm activity by reducing the number of entrants, as is easily illustrated in our simple model. Standard models of entry, such as the canonical setup in Hopenhayn (1992), generate entry through the use of a free entry condition in which potential entrants compare expected profits to entry costs and enter when it is profitable, in expectation, to do so. Entry costs are defined broadly; what we have in mind is any set of upfront costs or investments, at least some of which are not tax deductible and do not appear in our highly stylized production functions. The intuition is that, unlike existing firms, potential entrepreneurs initially lack such productive capital and must make large investments. These investments could be standard types of capital like structures, equipment, and software as well as various forms of intangible capital such as advertising, intellectual property, and branding. The entry cost could also include search and matching costs for initial labor utilization or supply chain relationships. We summarize these various startup costs with the term c, though a more realistic model would explicitly link these costs to the production function. The economics of the free entry condition can be illustrated simply by using the special case of the framework above in which labor is the only factor of production. In this special case, labor demand among existing firms is given by

$$n = \left[\frac{w}{\theta(1-\tau_s)Az}\right]^{\frac{1}{\theta-1}} \tag{4}$$

The single-factor profit function for existing firms is given by

$$\pi(z; A, w, \tau_i, \tau_s) = (1-\tau_i)\left[\left(\frac{(1-\tau_s)Az}{w^\theta}\right)^{\frac{1}{1-\theta}}\left(\theta^{\frac{\theta}{1-\theta}} - \theta^{\frac{1}{1-\theta}}\right)\right] \tag{5}$$

Potential entrants, which differ by z, enter if and only if the following is true:

$$\pi(z; A, w, \tau_i, \tau_s) = (1-\tau_i)\left[\left(\frac{(1-\tau_s)Az}{w^\theta}\right)^{\frac{1}{1-\theta}}\left(\theta^{\frac{\theta}{1-\theta}} - \theta^{\frac{1}{1-\theta}}\right)\right] \geq c \tag{6}$$

where c is the fixed entry cost. This free entry condition yields a threshold value z^* that is a function of the tax rates, aggregate conditions A, the wage, and the entry cost such that potential entrepreneurs enter if and only if $z \geq z^*$.

Using this free entry condition, Figure 4 shows how, for a given wage and aggregate state A, the tax rate affects the entry threshold z^*. We study this relationship qualitatively, so the income tax rate and the sales tax rate have the same implications; hence, in the figure we simply refer to a single tax rate τ. The y-axis shows profits, and entry is profitable when the profit curve is above the entry cost c, shown by the horizontal dashed line. The x-axis shows entrepreneurial productivity, which for simplicity is observed by potential entrants prior to the entry decision. Consider three tax regimes: low, medium, and high. The first profit curve, in blue, corresponds with the low-tax regime and crosses the entry cost line at $z = z^*(low\,tax)$. As the tax rate shifts to the medium-tax regime (red line) then on to the high-tax regime (yellow line), the productivity threshold rises to $z^*(med\,tax)$ then to $z^*(high\,tax)$, generating a mass of potential entrants who would have entered under the low-tax regime but now cannot do so. The size of this effect depends on the distribution of z; for $z \sim G(z)$, the measure of potential entrepreneurs who are deterred by the change in tax regimes is equal to $G(z^*(high\,tax)) - G(z^*(low\,tax))$. In partial equilibrium, then, the free entry condition directly links tax rates to entry rates, with higher taxes corresponding with less entry.[19]

While Figure 4 shows the partial equilibrium consequences for entry of a change in the tax rate, Figure 5 provides an initial insight into general equilibrium concerns. As described above, an increase in the tax rate puts downward pressure on the wage as fewer potential entrepreneurs enter,[20] offsetting the initial profit-reducing effects of the tax increase. Figure 5 reports changes in the entry threshold z^* for three wage regimes—high, medium, and low wages—holding the tax rate constant. A rough way to interpret the figure is to assume that the tax rate recently rose and wages are adjusting downward in response. The blue line to the far right shows the profit curve for the high-wage regime, implying entry threshold $z^*(high\,w)$. As the wage adjusts down to the medium-wage regime then again to the low-wage regime, the profit curve shifts to the red line then to the yellow line. The productivity threshold accordingly moves down, incenting entry for measure $G(z^*(high\,w)) - G(z^*(low\,w))$ of potential entrants.

Finally, recall that A may be thought of as a traditional aggregate productivity shock, but in this simple model it can also be thought of as an aggregate demand factor (or a summary of conditions broadly) that is potentially endogenous to the tax rate. Figure 6 shows the effect on the entry threshold of a decline in A; we interpret this figure in similar manner to Figure 5. That is, immediately after a tax rate increase, suppose aggregate demand begins adjusting downward as workers have lower income and, reasoning beyond this simple model, downstream firms reduce purchases. The blue line to the far left shows the profit curve for the high-A regime prior to the tax increase, implying entry threshold $z^*(high\,A)$. As aggregate conditions deteriorate to the medium-A regime, the profit curve shifts to the red line, then again to the yellow line on the far right as aggregate conditions reach the low-A regime. Transitioning from high-A to low-A deters entry for measure $G(z^*(low\,A)) - G(z^*(high\,A))$. This is a mechanism through which, for example, corporate tax rates can affect even noncorporate firms. More broadly, any policy change that weakens aggregate demand conditions can have nontrivial effects on firm entry.

Figures 5 and 6 summarize the general equilibrium effects of a tax increase, with wage

[19]Note that, by raising the productivity threshold, the tax may actually increase the average employment of new entrants.

[20]In the more realistic model with capital, labor demand also falls among incumbent firms.

adjustment acting to mitigate the direct negative effect on entry, and aggregate conditions effects acting to enhance the direct negative effect. In this qualitative setting it is impossible to determine the net effect after direct and general equilibrium effects play out. A key parameter governing the wage effect is the labor elasticity, which will determine how responsive is the wage to tax rate changes; if labor supply is highly inelastic, the wage will adjust dramtically, while an elastic labor supply would require little wage movement. Likewise, aggregate conditions effects depend on parameters and other assumptions, not modeled here, about the economic environment.[21]

In sum, the business tax directly affects entrepreneurial activity through the free entry condition, with direct effects on profitability and indirect effects via general equilibrium mechanisms; whether the tax affects the size of young firms *after* entry depends on the tax treatment of specific production factors, and additional model machinery would be required for the effect on existing firms to be disproportionately borne by young firms.[22]

More generally, Suárez Serrato & Zidar (2016) provide a useful framework for understanding key channels of tax policy in equilibrium. The immediate effect of a corporate tax rate reduction is to increase the economic profits of existing local businesses (by reducing tax liability and narrowing the capital "wedge" created by lack of deductibility of equity costs, as in our illustrative model and Figure 4). In general equilibrium, however, an increase in economic profits induces the entrance of new businesses and a concomitant expansion of local labor demand, raising wages and offsetting (to some degree) the profit increase among existing businesses (as in our model and Figure 5). The total reaction of wages depends both on the expansion in labor demand caused by rising profits and on labor supply, the elasticity of which depends on, among other things, the ability of the local housing market to

[21] A particularly salient but not specifically shown aspect of the above analysis is that the free entry condition can interact with firms' revenue functions in a way that makes startups particularly sensitive to shocks. Revenue function curvature, whether arising from decreasing returns to scale technology or imperfect competition in product markets, dampens firm responsiveness to profitability conditions; Decker *et al.* (2018b) show this formally in a similarly reductive model setting, and Decker *et al.* (2018a) show this in a more fully specified and simulated model. The free entry condition implies that potential entrants respond to aggregate profit conditions (reflected via equilibrium prices and aggregate demand conditions) by entering until it is not profitable (after entry costs) to do so; the way existing firms' behavior is constrained by revenue curvature leaves room for entrants to account for a large share of the response to changes in aggregate profit conditions.

[22] Neira & Singhania (2017) make an interesting argument that a cut in the corporate tax rate actually reduces the share of activity accounted for by startups. They find evidence for this view using variation in sector-level changes in effective tax rates, which correlate positively with sector-level changes in startup rates in Business Dynamics Statistics (BDS) data. This positive correlation depends heavily on the construction and agriculture sectors; the latter is thinly covered in the BDS. They also propose a model of occupational choice that is similar in spirit to, but more fully specified than, the illustrative model we describe here. In their model, which specifies a tax on revenue (and hence non-deductibility of labor costs), strong general equilibrium mechanisms driven by a nearly fixed labor supply crowd out potential entrants when the tax rate is reduced. The assumption of a closed labor market is much less plausible in our county-level analysis than in their economy-wide analysis.

accommodate new workers as well as location preferences among workers previously living elsewhere. Hence, gains from corporate tax cuts are divided between workers (through wage increases), land owners (through increased demand for housing), and firm owners (via the net effect on profits after equilibrium wages adjust). The broad implication for our analysis is that the ultimate effects on employment at entering businesses depend on determinants of local labor supply; a tax cut need not necessarily boost new firm activity. Suárez Serrato & Zidar (2016) find that each of workers, land owners, and firm owners bear a significant share of taxation incidence, suggesting that there is room for entry to respond to tax cut-induced changes in profit opportunities but that these effects need not be extremely large. Additional general equilibrium effects could work through demand externalities (e.g., reduced income among owners or employees of heavily affected firms) or supply chain disruptions (as in Figure 6).

4 Data

4.1 Data Sources

To provide empirical evidence of the relationship between entrepreneurial activity and taxes we use data from the Quarterly Workforce Indicators (QWI). The QWI is derived from Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program which gathers administrative data from states' unemployment insurance programs, social security records, the Longitudinal Business Database and the Decennial Census among others. LEHD covers nearly the universe of private nonfarm employers in participating states (based on unemployment insurance enrollment). The QWI aggregates LEHD worker and firm characteristics data into public-use county-level files.[23] Private sector employment, job creation and job destruction are reported separately for five firm-age categories: new firms (0-1 year-olds), 2-3 year-olds, 4-5 year-olds, 6-10 year-olds, and firms 11 years old or older. For the present study, we use the terms "startup" and "entrepreneur" interchangeably to refer to new firms (having age less than two); these startup firms are the primary focus of our analysis. It is important to note that firms enter the QWI scope when they hire their first W-2 employee; hence, the entrepreneurs we are studying are employer businesses.

There are two unique features of the QWI that make possible the type of analysis performed in this paper. First, no other public dataset contains county-level data on firm age. Commonly used data sources on startups, such as the Business Employment Dynamics and

[23]See Abowd *et al.* (2006) for details on the QWI's construction.

the Business Dynamic Statistics contain information only at the state or MSA level. Second, the QWI provides information on both net and gross employment changes, reporting job creation (the number of additional jobs at expanding establishments) and job destruction (the number of lost jobs at contracting establishments).

While the data provide some major advantages, they do have their drawbacks. Although the QWI provides detailed information on new firms in narrowly defined geographic regions, it is not an establishment- or firm-level dataset. As such, we are unable to distinguish firms by their legal form of organization or track them through their lifecycle.[24] While we make some effort to perform heterogeneity analysis and provide an illustrative partial equilibrium model for the direct impact of corporate taxes on entrepreneurial activity (see Section 3), the limits of the QWI hinder our ability to fully disentangle the pathways through which corporate taxes affect our entrepreneurial outcomes.

Figure 7 displays the specific state borders that are used in the primary border discontinuity analysis. Ohio, Texas and Michigan are dropped from the dataset because they either tax C-corporations' gross sales rather than profits or they switched to a profit-based tax during our sample period. Counties that are in orange belong to states for which more that 50 percent of employment is located in counties that border another state; these states are excluded in some of the robustness checks.

The primary results explore log employment and employment growth for startup firms in these counties. We later explore job creation, job destruction and reallocation. Examining log employment separately from employment growth allows us to understand how these policies affected both levels and trends in entrepreneurial and economic activity. Panel models may be slow to detect changes in levels if these changes evolve slowly over time. However, changes in trends will more quickly show up in the data. Ideally, an event-study model would be used to capture the dynamics of the variable of interest over time. As seen in Figures 1-3, the frequency of within-state policy changes make event studies particularly difficult to estimate. Many states phase these policies in over multiple years or adjust these policies multiple times over the sample period. For this reason we explore the dynamics of the response using a distributed lag model.

Job creation and job destruction are also evaluated to understand the impact of these policies on reallocative activity. To construct the creation, destruction and employment change variables, we scale levels of job creation, job destruction and employment change by the county's 2006 total employment level. This is similar in spirit to Adelino *et al.* (2017) who

[24]The QWI does report county-level by firm-age category by sector; but these data are frequently suppressed when there are few establishments in a given county/firm-age/sector cell. For this reason, we do not explore sectoral heterogeneity in this paper.

scale these outcomes by a county's employment in 2000. Scaling in this way allows for job creation in a county to be comparable across time and across firm groups. The year 2006 is chosen because it is the first year in which all participating states provide data.[25] The study period consists of the years 2000-2013, and counties with fewer than 3,000 workers in 2006 are dropped from the sample.[26]

We merge county-level QWI data with a number of other datasets to obtain control variables and information on the policies of interest. The Census Bureau's Population Estimates Program provides annual county-level population data. State corporate tax rates and apportionment rules are drawn from the Tax Foundation and are supplemented with information from the Book of the States and state tax forms. Corporate tax schedules vary from state to state. Following Ljungqvist & Smolyansky (2016), we focus on changes in states' top statutory marginal tax rate. With few exceptions corporate tax rates are levied on firms' profits.[27] Personal income tax data are obtained from NBER's TAXSIM model. We use the reported maximum state income tax rate as a proxy for the personal tax rate that potential entrepreneurs would face.[28] Data on state sales tax rates are from the Book of the States; we use the headline tax rate, ignoring special rates on specific items. Additionally, we obtain data on research and development tax credits from Suárez Serrato & Zidar (2016). The minimum wage data are obtained from Meer & West (2016) and are updated through 2013 using the U.S. Department of Labor's State Minimum Wage Reports. States' nominal minimum wages during the sample period are adjusted into constant 2011 dollars using the national CPI deflator. Data for each of these policies are at the state-year level.

Table 1 reports summary statistics by state for startup firms and the three main policies we examine. Table 2 reports summary statistics for our sample, with specific numbers for border

[25]90 percent of states provide data for the entirety of the 2000-2013 study period. Note that there is an employment change identity whereby for each unit of observation $\Delta Emp_t = Emp_t - Emp_{t-1} = Creation_t - Destruction_t$.

[26]Results are not sensitive to this data restriction. Dropping small counties is useful for two reasons. First, the QWI suppresses data if there are few workers or firms in a particular county. In principal, suppression itself could be a function of the policies of interest. For example, if the number of new firms in a county drops to one or zero as a result of increased corporate tax rates then the county's data will be suppressed in that year. A second issue is that small counties have far more variation in the outcome variables. For small counties, even relatively minor creation or destruction events will result in large swings due to the small denominator. For the border discontinuity results we require that both counties in the border pair have at least 3,000 workers in 2006, the first year for which data are available in all states. Table A1 reports results based on the reduced threshold of 1,000 workers in a county. Results become increasingly noisy as the threshold is lowered below 1,000.

[27]The exceptions are Ohio, Texas and Michigan which we exclude from our analysis.

[28]This measure is calculated by the Taxsim program and frequently used by researchers. It differs slightly from the reported personal income tax rates reported in Table 1 because it accounts for federal policies such as the State and Local Tax exemption (commonly known as SALT) that alter the effective rates that residents of a particular state will face. See http://users.nber.org/ taxsim/state-rates/ for further details.

versus all counties and for new versus all firms. Border counties tend to be modestly smaller than counties overall but have similar activity rates in terms of firm entry, job creation, and job destruction.

4.2 Characteristics of Young Firms

Given our focus on young firms, it is useful to provide some basic details about their characteristics. Consistent with existing literature (e.g., Haltiwanger *et al.* (2013)), data from the Census Bureau's Business Dynamics Statistics (BDS) show that startups (firms with age 0-1) are overwhelmingly small.[29] In 2000, the year in which our analysis begins, firms with fewer than 10 employees accounted for about 90 percent of startups and about 40 percent of startup employment; these numbers changed very little over the period 2000-2013. Large firms (those with 500 or more employees) accounted for about 0.03 percent of startups and just under 7 percent of startup employment. The average size of startups throughout the period studied is about 7 employees, with the skewness of the distribution implying that the median is lower yet. As mentioned earlier, NETS data from 2000 show that about 34 percent of startups are formed as "corporations" and that these 34 percent of firms accounted for 48 percent of overall startup employment.

Startups in the QWI are more likely than older firms to employ young workers.[30] In 2000, workers with age less than 25 accounted for about 21 percent of startup employment compared with 17 percent of overall employment. Startups also disproportionately employed workers aged 25-34 in 2000, which comprised 26 percent of startup employment versus 24 percent of overall employment. By contrast, workers aged 35 or older account for 52 percent of startup employment compared with 59 percent of employment generally. These patterns hold qualitatively throughout 2000-2013.

Startups also differ modestly from other firms in terms of the education of their workforce. Setting aside those workers for which education data are not available (those of age less than 25), startups employed more workers lacking a high school diploma than did firms generally, with 15 and 13 percent of employment, respectively, in 2000. Similar gaps are apparent throughout 2000-2013. Shares of workers possessing only a high school diploma (or equivalent) are roughly similar between startups and firms generally. The share of employees with a bachelor's degree or higher was about 27 percent for both startups and firms generally

[29] QWI does not allow for studying firm age by firm size

[30] Using matched data from the Census Bureau's Longitudinal Employer-Household Dynamics and Longitudinal Business Database, Ouimet & Zarutskie (2014) show that the higher shares of young workers seen among young firms remain even within firm size, industry, and region cells.

in 2000, but in later years startups fell behind other firms in this measure. Broadly speaking, startups tend to employ somewhat less educated workers than do other firms.

Startup activity varies widely by sector, ranging from less than 2 percent of employment in utilities (NAICS 22) to over 9 percent of employment in accommodation and food services (NAICS 72) as of 2000. Other startup-intensive sectors include the "other services" sector (which includes businesses like auto repair shops, household maintenance services, dry-cleaners, laundromats, and funeral homes); professional, scientific, and technical services; and construction. In addition to utilities, sectors with low startup activity include manufacturing, mining, and finance and insurance. These rankings of startup activity are broadly consistent over time and match those reported by Hurst & Pugsley (2011) (who focus on small businesses rather than young businesses).

5 Econometric Models and Results

This section walks through the econometric models used and the primary results of the paper. We begin by performing standard panel regressions using data from all U.S. counties. We use this basic model to motivate our use of the border discontinuity method. After reporting and discussing the results of the border discontinuity method, we then test for two particular sources of bias that may affect our estimates. First, we study the extent to which cross-border spillovers may be driving the results by setting up a model specifically designed to estimate spillovers. Second, out of concern that states making policy changes may act strategically based on how much of their economic activity occurs on borders, we report results for a subset of states whose border counties comprise a small fraction of their overall economy. Restricting the data in this way assuages concerns that states in which state-border discontinuities would be especially salient are likely to make tax policy endogenous to local conditions.

5.1 Baseline Panel Regressions

To introduce the results and notation, we first estimate a straightforward panel regression model using all U.S. counties. The tax rate variables are each at the state-quarter level, log population is at the county-quarter level, and the outcome variables (log employment, employment growth, job creation and job destruction) are at the county-quarter level and are as defined above.[31] The specification takes the following form:

$$y_{ct} = \beta_1 CorpTax_{st} + \beta_2 PersTax_{st} + \beta_3 SalesTax_{st} + \gamma X_{ct} + \delta_c + \alpha_t + \epsilon_{ct} \tag{7}$$

The coefficients of interest are β_1, β_2 and β_3. X_{ct} represents the set of control variables, δ_c represents a set of county-level fixed effects to control for time-invariant differences between counties, and α_t is a set of quarter indicator variables that control for common nationwide temporal shocks. Because the policies in question vary at the state level, that is the level at which we cluster standard errors. To maintain consistency throughout the specifications, counties with fewer than 3,000 workers in 2013 are dropped from the sample. Note that with this set of fixed effects, the panel regression is essentially a difference-in-differences estimator.

The results of this model are found in Table 3. Table formats are similar throughout Section 5. Each column gives coefficient results from a separate regression. At the top of the column is the outcome variable used in the regression. The rows list the coefficients on the different policy variables. Columns 1-5 report regression results for "Startup Firms" (firms age less than two). Columns 6-7 report results for the "All Firms" category, which includes startups. The outcome variables examined here are log employment and employment growth, where growth is scaled as described in Section 4 (we consider job creation and job destruction further below). For startups, we first consider each tax policy individually then include all of them together.

Column 1 reports the coefficient relating corporate tax rates to log employment. The negative and statistically significant coefficient suggests that a one-percentage-point increase in the corporate tax rate results in a 4.4 percent drop in the number of workers employed at startup firms in a county. In column 2, the coefficient relating personal tax rates and log employment is moderately negative but far from statistically significant. In column 3, the coefficient relating sales tax rates and log employment is substantially negative but not statistically significant. Column 4 shows that the inclusion of all tax rates together brings all the coefficients closer to zero but does not significantly change the interpretation. Comparing column 4, which reports results for startups, to column 6, which reports results for all firms (including startups), reveals that startup employment falls much more than employment generally, consistent with intuition and previous literature discussed above in which startups are more sensitive to shocks than are incumbent firms. Columns 5 and 7 show that the effects of taxes on employment growth rates are generally not significant.

Strong assumptions are required for β_1, β_2 and β_3 to be interpreted as the causal effects

[31] Here and below the use of the word "quarter" refers to year-quarter rather than seasonal dummies. Note also that the tax variables and control variables vary from year-to-year rather than quarter-to-quarter.

of the respective tax changes. As with any difference-in-differences estimator, there should be common trends for both the treated and the untreated observations. States making these policy adjustments should have similar trends to states that do not make adjustments before the policy goes into effect. A related threat to identification is dynamic selection, whereby states make policy adjustments based on past, current or predicted economic activity. If states only raise taxes when labor markets are strong and employment is growing, this will upwardly bias the coefficients. On the other hand, if states raise taxes to shore up budgets when the economy is faltering, the results could be downward biased. Any unobserved geographic shock that is correlated with the policy change of interest can create bias in the results. These are important reasons standard panel data regression models may generate biased results, and they have been discussed in a number of previous papers on the subject (Dube *et al.* 2010; Ljungqvist & Smolyansky 2016; Meer & West 2016).

5.2 Border Discontinuity

Given these potential identification problems, we now turn to the border discontinuity method which, by exploiting differences in labor market outcomes between contiguous counties that straddle a state border, overcomes many of these concerns. Neighboring counties are likely to experience similar economic conditions and have similar local shocks, but by dint of falling on one side of a state border, one county will experience the policy shock while its neighbor does not. Even if states adjust policies based on their overall economic conditions and border counties experience similar economic trends as the state, it is still likely that their neighboring, cross-state county will experience similar conditions.

To perform this analysis we create a dataset consisting of all counties that share a border with a county from another state. To understand how we exploit cross-border differences it is useful to first consider the following specification:

$$y_{pct} = \beta_1 CorpTax_{ct} + \beta_2 PersTax_{ct} + \beta_3 SalesTax_{ct} + \gamma X_{ct} + \delta_c + \alpha_{pt} + \epsilon_{ct} \quad (8)$$

Here we observe the outcome variable y_{pct} for county c in time period t, where county c belongs to county-pair p. β_1, β_2 and β_3 are the coefficients of interest and X_{ct} continues to consist of control variables such as log population that vary at the county-year level. As with the panel specification, δ_c represents a set of county fixed effects. What distinguishes this model from the panel model is the inclusion of α_{pt}, a set of county-pair-quarter fixed effects. Inclusion of county-pair-quarter fixed effects absorbs any shock that is common to a county-pair in a particular period. Importantly, the variation used to identify β_1, β_2 and β_3 is now restricted to changes in within-pair differences.

This specification overcomes the identification concerns inherent in the standard panel regressions but has two shortcomings that require it to undergo a few additional changes. The first (and more pedestrian) issue with equation 8 is that it is computationally intensive. Inclusion of both county and pair-quarter fixed effects requires considerable computational resources. A second issue, and one that may potentially bias the estimates, is that the specification assumes that the bordering county, which serves as the control group, experiences no change in any of the policy variables or the control variables. Therefore, any change in within-pair differences that is driven by policy changes in the border county will not be attributed to the policy.

To address these concerns we perform a variable transformation similar in spirit to Dube *et al.* (2016) and Hagedorn *et al.* (2015). Consider two contiguous counties, i and j, that straddle a state border. For every variable z we perform the following transformation:

$$\widetilde{z_{it}} = z_{it} - z_{jt} \tag{9}$$

where z_{it} is the variable in county i in time t, and z_{jt} is the variable in county j, which borders county i, in time t. This transformation automatically captures any period-specific shock that occurs to any particular border-pair. We can now rewrite equation (8) in the following way, having transformed each of the variables in the equation to be the within-border-pair difference of that variable:

$$\widetilde{y_{it}} = \beta_1 \widetilde{CorpTax}_{it} + \beta_2 \widetilde{PersTax}_{it} + \beta_3 \widetilde{SalesTax}_{it} + \Gamma \widetilde{X_{it}} + \delta_i + e_{it} \tag{10}$$

In this equation $\widetilde{y_{it}}$ represents the within county-pair difference in the outcome variable (log startup employment, startup employment growth, etc). $\widetilde{CorpTax}_{it}$ is the difference in the counties' state corporate tax rates, $\widetilde{PersTax}_{it}$ is the difference in the counties' state personal tax rates, $\widetilde{SalesTax}_{it}$ is the difference in the counties' state sales tax rates, and $\widetilde{X_{it}}$ is the difference in their control variables. δ_i is a border-pair specific fixed effect. Any time-invariant difference in economic outcomes between two bordering counties is absorbed through the inclusion of δ_i. By including δ_i we are now identifying the effect of the policy off of *changes* in the within-pair *differences*. Following Dube *et al.* (2016) we also cluster at both the state and the border-segment level.[32]

[32]Counties enter the dataset as many times as they have a border pair in a contiguous state; as a result there may be correlation across both states and border-segments. To implement this clustering approach we use Stata's reghdfe command (Correia 2014) which allows for two-way clustering of standard errors following Cameron *et al.* (2011).

5.2.1 Main Results

Table 4 reports results from the locally differenced regression in equation (10). As with our basic panel models, here we report results for dependent variables log employment and employment growth, where the latter is scaled as described above. The first five columns report startup results. These results are qualitatively similar to the basic panel results, but notably, the coefficient relating corporate tax rates with employment is attenuated in the border discontinuity design. Column 4 shows that a one-percentage-point decline in relative cross-border corporate tax rates results in 3.7 percent lower employment among startups, compared with a 4.4 percent drop in basic panel regressions. This result indicates that even after addressing dynamic selection concerns, corporate tax rates have a significant negative effect on startup activity. As can be seen in Figure 1, a one-percentage-point change would be a large but not unprecedented change over the time period we are examining (the largest increase we observe is 2.1 percentage points). Comparing columns 4 and 6 again reveals that the effect of corporate taxes on startup employment is more than double the effect on overall employment.

In this specification, the coefficients relating tax rates with employment growth are not statistically significant; however, their magnitudes may puzzle some readers so it is useful to describe their quantitative interpretation briefly. As discussed in section 4, employment growth for startup firms is scaled by *total* firm employment in the county (job creation and destruction, though not reported here, are scaled the same way in results we report further below). By scaling both the "Startup Firm" variables and the "All Firm" variables by the same number, we are able to directly compare coefficients across firm age groupings, as is done later in the paper, and understand the portion of the effect on overall employment growth change that is attributable to the effect on startups (note that in some literature, these measures of employment growth, job creation and job destruction are described as "components" of overall flows).[33] The -0.133 coefficient in the first row of column 7 of Table 4 can be interpreted to mean that a one-percentage-point increase in a state's corporate tax rate results in a 0.13 percentage point decline in their quarterly employment growth rate. At first glance this may seem small. However, a decline in the quarterly growth rate of this magnitude can lead to substantial levels changes after a few years.

Because we have scaled startup employment growth by the same factor as overall em-

[33]In Table A2 we report results for all five of the firm age groupings provided by the QWI as well as for the "All Firm" category. Adding the employment growth coefficients for each of the firm age groups will equal the overall employment growth coefficient. The same holds for the job creation coefficients and the job destruction coefficients. In practice the arithmetic is not exact, since our county size thresholds result in slightly differing samples.

ployment growth, we can gain insight into the extent to which the coefficient in the "All Firms" column is being driven by changes in startup employment growth. A coefficient of -0.0463 on startup employment growth implies that an outsized portion of the overall employment change coefficient (-0.133) is accounted for by the employment change among startups. Roughly one-fifteenth of overall employment is located in startup firms (see Table 2). Therefore, if employment growth were equally affected across firm age groupings then the startup employment growth coefficient would be one-fifteenth the size of the overall employment growth coefficient. Instead it is more than one-third the size of the overall coefficient.

Given that many firms may choose to organize as S-corporations, LLCs, sole proprietorships or partnerships, it is important to examine the effect of personal income tax rates as well. We find no evidence of an impact of personal income taxes on employer startups. The lack of a strong result on startups suggests that personal income taxes may have competing effects on startup activity, consistent with our theoretical discussion in section 2.2.

The coefficient relating sales tax rates with log startup employment is negative but nonsignificant, as in base panel results. The coefficient is larger in magnitude when sales taxes enter the regression alone than when other tax rates are included, suggesting that simultaneous changes in sales, corporate, and personal tax rates may occur. The sign on the coefficient is intuitive given the theoretical discussion above, but the lack of statistical significance across a range of specifications pushes us away from making sweeping conclusions on the effect of sales taxes. The strong result on corporate taxes prompts us to more deeply explore the corporate tax - entrepreneurship relationship. In the remaining sections we explore the dynamics of the corporate tax effect, the extent to which results are driven by cross-border spillovers and the sensitivity of the results to the inclusion of tax base measures and other state policies.

5.2.2 Dynamics

We next explore the dynamics of the corporate tax rate effect on log employment in "Startup firms" and for "All Firms." Doing this allows us to better understand the timing of firms' response to corporate tax rate changes and to check for anticipatory effects. To do this, we run the model in equation (4) but, in addition to including the current year's value of the corporate tax rate ($\widetilde{CorpTax}_{i,t=\tau}$), we also include two annual leads of this variable ($\widetilde{CorpTax}_{i,t=\tau-2}, \widetilde{CorpTax}_{i,t=\tau-1}$) and two annual lags of the variable ($\widetilde{CorpTax}_{i,t=\tau+1}, \widetilde{CorpTax}_{i,t=\tau+2}$). We run this distributed lag model using log employment as the dependent variable.[34] Figure 8 displays the coefficients on the lead and lag variables for log employment of new firms

[34]See figure note for additional details. Many states undergo multiple changes in consecutive years, as such performing a clean event study model is not possible.

and log employment of all firms. By including leads and lags of the corporate tax variable this model is more demanding of the data and spreads the overall effect of the policy across all of the lags. As such, we lose statistical significance, but the pattern that emerges from the coefficients is revealing. Consistent with the regression models, young firms experience a larger effect from changes in corporate tax rates. Outcomes are shown to be most sensitive to the previous year's tax rate rather than the current year's tax rate. This is likely due to a combination of two factors. First, there could be frictions in setting up a business and in hiring workers. Second, our definition of startup firms includes all firms that are less that two years old. As such, the full effect of the policy will not be observed until the second year following a policy change once an entire cohort of new firms is exposed to the policy.[35] While the lead coefficients are slightly negative, it is clear that there is no significant anticipatory effect. Finally, the plot provides evidence that border counties do not experience differential pre-existing trends that are correlated with policy.

5.3 Border Spillovers

A primary concern with border discontinuity models is that they may overstate the size of the treatment effect if the control county is subject to spillovers from the treated county. In the context of our design, the concern is that increases in taxes may result in startups simply choosing to locate on the other side of the state border. If new firms react in this way then border discontinuity methods will find large negative effects of the policy when in fact there is (possibly) zero net change to entrepreneurial activity for the combined two-county area. Of course, negative spillovers may occur as well, whereby a negative shock to one county reduces rather than increases economic activity in bordering counties via demand or other channels. Existing research is mixed on the extent to which economic activity shifts across borders in response to policy shocks. In some contexts there is clear evidence of shifting (Agrawal 2015) while in others there is evidence of no shifting (Isen 2014).

To test for spillovers across state borders, we ask whether counties that lie on state borders experience positive economic outcomes relative to the interior counties in their state when their bordering state undergoes a policy change. We perform a similar transformation as in the border county specification, but the outcome variable is now the difference between the border county and the average interior county of the state to which it belongs while the policy variables are those of the border county's neighboring state(s). The specific regression

[35] This could also be due to our inexact measurement of the date of the tax rate changes. Because we do not have the exact date of the tax rate changes, all changes are assumed to have occurred in the first quarter of the year they were enacted. If tax rate changes occur at other times in the year then this will dampen the effect of the t=0 coefficient.

we estimate is:

$$(y_{it} - y_{\overline{st}}) = \phi_1 CorpTax_{jt} + \phi_2 PersTax_{jt} + \phi_3 SalesTax_{jt} + \gamma(X_{it} - X_{\overline{st}}) + \delta_i + \epsilon_{it} \tag{11}$$

where y_{it} is the outcome of interest for border county i in state s, and $y_{\overline{st}}$ is the average outcome of interior counties of state s in time period t. The regression seeks to understand whether the policies of neighboring states affect border county outcomes. As such, the policy variables in the model are the policies of the bordering state j. Drawing from the notation used in equation 9, $CorpTax_{jt}$, $PersTax_{jt}$ and $SalesTax_{jt}$ measure the policy in the state j adjacent to county i (where county i is in state s).[36] As before, X_{it} is a set of control variables, and the expression $(X_{it} - X_{\overline{st}})$ measures the difference between border county i and the average interior county of state s (to which county i belongs). Importantly, δ_i controls for time-invariant differences between the border county and the average interior county. As such the effect of border states' policies is identified off of changes in the difference between the border county and the average interior county. Any change in the border county's own-state policy that equally affects all counties in the state will be absorbed by this differencing method. This is akin to including all counties in the analysis and then including state-year fixed effects.

Table 5 reports results from equation (11). The coefficients in this model can be interpreted in the same manner as the coefficients in Table 4. Among startups, estimates of the $CorpTax_{jt}$ coefficient point negative and are substantively modest relative to the coefficients found in the baseline model. The only coefficient to show up as statistically significant is on percentage change in startup employment. This coefficient points negative, suggesting that there are negative spillover effects of corporate taxes across borders. Any negative coefficient suggests that the border discontinuity method may understate the size of the true effect. The positive coefficient in column 6 on the corporate tax variable, while statistically insignificant, suggests that some of the corporate tax effect on overall employment estimated in table 4 may be partially driven by cross border spillovers. The sales tax and personal income tax results also provide evidence that startup activity does not simply relocate across state borders—at least in measurable quantities—in response to tax changes. Coefficients for all firms are similarly modest and statistically insignificant.

The test for border spillovers suggests that startup activity does not simply shift across the border in response to corporate, personal, or sales tax rate changes. Rather, the negative effect of corporate tax rates that we find in our border discontinuity exercises appears to reflect overall reductions in startup activity in local economies. Before moving forward, it is important

[36] If county i borders counties in multiple states then we take the average of the counties it borders.

to note that this is only testing for spillovers across neighboring counties. For the purposes of verifying our main border discontinuity estimates, these types of spillovers are particularly salient. Nonetheless, these estimates do not rule out the possibility that spillovers occur over further geographic distances, either across states or even across countries. However, the results do provide evidence that the control group in our border discontinuity regressions are not materially affected by changes to the neighboring states' policies.

5.4 Other Policies, Outcomes and Robustness Checks

We provide a variety of robustness checks to examine the sensitivity of our results. A key element of our identification strategy is the assumption that state tax rates are not set in a way that is particularly endogenous to local economic conditions in the border counties we study. More broadly, if state governments directly internalize the degree to which changes in their policies affect startup activity in specific border counties, our estimates will be biased. In other words, states in which state-border discontinuities are especially salient might be disproportionately likely to set tax policy in reaction to economic conditions in, or goals for, border counties, in which case we may not be able to generalize our results to all states. While there is no direct way to test for this, we can restrict our sample to only states whose border counties make up a relatively small fraction of their overall activity. States with a low share of economic activity on their borders are less likely to consider the potential effect on border counties when making policy decisions. The orange (or lightly shaded) region in Figure 7 represents counties that belong to states for which greater than 50 percent of overall state employment is located in a border county. Regression results that exclude these counties are reported in Table 6. The key coefficients support the results found in Table 4, though the larger effects of corporate tax rates may suggest that states with important border counties do differ from others.

Table 7 examines the sensitivity of the corporate tax rate coefficient to the inclusion of other variables and to alternate dependent variables. For reference, Panel A reports our baseline border discontinuity results from Table 4. In addition to log employment levels and net employment growth rates, here we also report job creation and destruction rates among all firms, scaled by overall county employment as discussed above (such that the coefficient for net employment growth is the sum of the creation and destruction coefficients). Though not statistically significant, these gross flows coefficients indicate that the negative point estimates on net employment growth reflect not only lower job creation rates but also lower job destruction rates; that is, setting aside statistical significance, higher corporate tax rates are associated with lower job reallocation as well as lower net employment growth.

Suárez Serrato & Zidar (2017) find that variation in the tax *base* plays a significant role in state-level output and revenue variation; for this reason, on Table 7 we report regressions that include several tax base and related policy indicators. Panel B controls for a state-year measure of R & D tax credits from Suárez Serrato & Zidar (2016). R & D credits appear to have minimal effect on new firms and appear to have some small positive effect on overall employment while increasing overall creation and destruction rates. Panel C controls for the number of future years into which firms can carry losses for tax purposes (these data are also from from Suárez Serrato & Zidar (2016)); carry-forward years apparently provide modest boosts to startup activity and overall job creation. Panel D examines whether corporate tax results change when accounting for apportionment rules (see Section 2.4). We examine whether payroll apportionment matters by interacting the state's payroll apportionment share with the corporate tax rate and no meaningful change in the overall effect of corporate taxes.[37] The corporate tax rate coefficients are not particularly sensitive to inclusion of any of these tax base terms, but there is some suggestive evidence that loss carry-forward and R & D policies may affect startup activity.[38]

As noted above, many businesses are not directly subject to the corporate tax rate due to alternative legal forms of organization. Data on legal form among *new* firms are difficult to obtain. We use the National Establishment Time Series (NETS) (based on Dun and Bradstreet data) to obtain the county-level share of new firm employment that is "corporate" (NETS does not distinguish C-corps from S-corps) as of 2000. These data have a number of limitations (and strengths), particularly for annual-frequency analysis, that are explored by Barnatchez *et al.* (2017); as such, we consider this exercise suggestive and refrain from using the NETS data more extensively. We only use the data for 2000 due to the endogeneity of these shares to our policies of interest as well as the weaknesses of the NETS data at annual frequency. The year 2000 snapshot is used as an estimate of initial county propensity to form as a corporation, and we perhaps heroically assume that cross-county differences in total corporate activity comprise a good proxy for cross-county differences in C-corp activity specifically. These various caveats notwithstanding, the NETS data are the only source of county variation in corporate activity among new firms to which we have access for this study. Panel E of Table 7 reports our standard corporate tax rate regression with the addition of the interaction of the

[37]This may not be surprising given that new firms are unlikely to be active in multiple states. Furthermore, apportionment rules should primarily affect the tradable sectors, such as manufacturing, whose sales are largely outside of the states (Goolsbee & Maydew 2000).

[38]Ideally we would also like to control for various, often ad hoc, job creation tax credits and subsidies. A comprehensive dataset of such subsidies for our states is difficult to construct, largely because many job creation tax credit programs are city or county-specific rather than state-specific. In unreported exercises, we found little of note based on a partial (yet impressive) data set from Bartik (2017).

corporate tax rate and the county corporate share; we would expect the effect of corporate tax rates on new firm employment to be greater in counties where more activity is corporate and, while not statistically significant, the magnitude and sign of the interaction term coefficient bears this out. Finally, in Panel F, we control for the log minimum wage using data updated from Meer & West (2016). There is no evidence of an effect of the minimum wage on measures of startup activity or job reallocation through there is an effect on overall employment. The appendix reports a number of additional robustness results and specification checks.[39]

6 Discussion

We find evidence of an effect of corporate taxes on our measure of startup firm activity but we do not find evidence of an effect of personal income or sales taxes. We run straight-forward panel regressions as well as border discontinuity regressions that control for a variety of endogeneity concerns. Additionally, we explore the extent to which cross-border spillovers may be driving the border discontinuity results. Our results on spillovers suggest that simple cross-border movements of activity are not likely to be a main driver of our border discontinuity estimates for corporate taxes.

To better contextualize our finding, it is helpful to compare our coefficients to other related papers in the literature. Our results are similar in magnitude to Mukherjee *et al.* (2017) who find states that increased corporate tax rates saw a decline in patenting activity and new product activity of 5 percent. While not directly comparable, our results suggests a somewhat smaller effect of corporate taxes than Rohlin *et al.* (2014) who find that when reciprocal agreements exist between states new establishments are 34 percentage points less likely to locate on the side of the border with a one percentage point higher corporate tax rate. Using a different time-frame, Ljungqvist & Smolyansky (2016) find that overall employment levels decline between 0.3 and 0.5 percent for every percentage point increase in corporate taxes; our estimated coefficients relating corporate taxes to *overall* employment are significantly larger. The larger effect in our paper is likely driven by the differing time periods of our studies and

[39]Table A1 requires that counties on each side of the border have over 1,000 workers (instead of 3,000 workers as in other specifications). Results are not substantially sensitive to changing this threshold. Table A3 specifically examines the corporate tax results by limiting the data to only the three-year periods surrounding corporate tax changes in each state. Again results are similar to the baseline results (which is not surprising in light of Figure 8, which shows that tax changes have nearly immediate effects). Table A4 reports results where linear state trends are included in the model. The inclusion of these trends has been hotly debated in the minimum wage literature (Neumark *et al.* 2014). Their inclusion will absorb some of the treatment effect if the policy results in a shift in both trends and levels. Not surprisingly, results from Table A4 show that inclusion of these trends does in fact reduce the magnitude of some of the coefficients. Table A5 drops observations flagged by the QWI as having undergone significant distortion. Again, results are similar to the baseline.

the differing amount of variation in tax rates observed over these time periods.[40] As such, one potential explanation for the differing results is non-linear, decreasing effects for larger changes. Indeed, given the magnitude of our estimates, decreasing effects may be likely since large, universally linear effects would imply that policymakers could effect dramatic changes in startup activity by significantly reducing tax rates. Indeed, we refrain from making "out-of-sample" predictions of the likely effect of tax changes larger than what we observe in our data. Overall, our results on entrepreneurship are reasonably consistent with the existing research that has explored similar outcomes.

Consistent with the growing literature on young firm activity (Fort *et al.* 2013; Adelino *et al.* 2017; Decker *et al.* 2018b;a), our results indicate that new firms are particularly vulnerable to economic shocks. New firms account for a disproportionate share of the overall response of employment growth, job creation and job destruction to changes in corporate tax rates. In results discussed in the appendix (Table A2) we find that startup activity is particularly vulnerable even compared to other young firms. It appears that, consistent with related research, the firm entry margin is crucial for understanding broader employment dynamics. In the context of our illustrative model from Section 3, the empirical results are consistent with taxes affecting the productivity threshold below which it is unprofitable for new businesses to enter; additionally (or alternatively), corporate tax rates could be working through non-deductible production factors to reduce labor demand among very recent entrants (to the extent that these factor demand decisions vary by firm age). Finally, the results are consistent with the type of aggregate demand effects we describe in our model, in which tax increases reduce activity generally, which can disproportionately affect entrants. Our data do not permit us to distinguish between these possibilities. In any case, it does not appear that general equilibrium effects via the *wage* are strong enough to offset the immediate effects of tax liability on profits and labor demand.

Our analysis relies on variation in policies that are determined at the state level. A potential concern is that locally determined policies, such as county sales or property taxes, are adjusting simultaneously with state-level tax policies. If anything, this possibility may suggest that our results understate the effects of state corporate taxes on the outcomes we study; Agrawal (2015) discusses theory and shows evidence indicating that local tax rates are responsive to state tax rates in an offsetting way. That is, for localities on state borders, local taxes tend to be set lower on the high-state-tax side of the border (and set higher on the

[40]Ljungqvist & Smolyansky (2016) use data from the 1970s and 1980s, during which time there were far larger corporate tax rate changes. For example, the largest increase we observe over our sample period is 2 percentage points while the largest they observe is 8 percentage points. They observe 18 increases of 2 percentage points or more in the 1970s and 1980s.

low-state-tax side of the border). Local tax policy is likely to be set in a way that reduces the net impact of state and local tax policies on employment outcomes. If changes in state tax policy induce changes in local tax policy, our results can be thought of as measuring the *net* effect of these policy changes, with previous evidence suggesting that gross effects of state corporate tax rates alone may be larger.

The estimates of the effects of personal tax rates on economic activity do not point in clear directions. As discussed in Section 2.2 and the research cited therein, the theoretical considerations linking personal tax rates and new firm formation are complex and lead to ambiguous empirical predictions. We are not inclined to seek a rigorous interpretation of our empirical results for personal tax rates. These likely reflect the combination of multiple effects including substitution between legal forms, substitution between employer and nonemployer form, asymmetric payoffs for losses and gains caused by tax rate convexity, and more general risk incentives. Likewise, as discussed in the introduction, we are limited in our ability to pin down the mechanisms driving the corporate tax result. Our illustrative model framework (Section 3) describes the direct effect of corporate taxes on C-corp activity but the estimated effect is likely to include indirect general equilibrium effects as well, whereby new firm activity is affected through the negative impact of corporate tax increases on incumbent firms. Beyond our simple model, though, one potential mechanism for the employment effects of corporate tax rates is that corporate taxes incent substitution into non-corporate legal forms in which growth is more difficult (as in the endogenous legal form model of Chen *et al.* (2017)). The endogeneity of legal form is yet another justification for including both corporate and personal tax rates in empirical specifications.

Our estimates of the effects of sales tax rates, while not statistically significant, point in intuitive directions. That is, the sign of the coefficients suggests that sales taxes may reduce startup activity, with notably smaller effects on firms generally. As noted in our theory discussion above, to the extent that interpretation of these non-significant effects is appropriate, the results are consistent with strong effects of tax rates on entry incentives.

Given the findings of Moretti & Wilson (2017), it may be surprising that we find no evidence of entrepreneurial activity shifting across borders. There is clearly a subset of potential entrepreneurs with geographic flexibility that will enter regardless of tax policy in their existing state. While these entrepreneurs will have high expected growth, they represent a small portion of the overall entrepreneurial picture as most entrepreneurs start small and are unsure of the future prospects of their company. Our results do not speak to whether certain entrepreneurs are shifting activity over further geographic distances. Rather, we argue that those entrepreneurs capable of making these larger shifts are likely to represent only a small portion of the overall number of potential entrepreneurs.

Stepping back, it is important to reflect on how these results fit with the broader entrepreneurship trends being observed at the national level during the time period we study. In one sense, our results deepen the puzzles behind the decline in startup rates in recent decades. We provide evidence that corporate tax increases reduce entrepreneurial activity, but state and federal corporate tax rates have not risen in most states over the time period we study.[41] Barring strong national general equilibrium mechanisms, our results suggest that entrepreneurial activity would have declined even more had tax rates been flat in recent decades. As noted previously, in unreported results we do find that states that reduced corporate taxes less over the entire 2000-2013 period saw larger declines in startup activity over the same period (on average), suggesting that our main results are at least somewhat relevant for understanding recent trends.[42] More broadly, for state policymakers interested in promoting an entrepreneurial economy, our results highlight the role that states' policies can play.

Our estimated effects of state corporate tax rates on new business activity are certainly suggestive about the potential effects of changes in federal tax rates. We therefore view our findings as a potential contribution to discussions about national tax policy. However, we caution against excessive generalization of our results since state-based analysis differs from nationwide analysis in important ways. We find no evidence of immediate cross-border spillovers, but there may be reallocation of activity across larger distances (but still within the U.S.) that strengthens our estimated results but would be much less salient for federal tax rates that exist within less-permeable national borders. Federal tax rates tend to be much higher than state tax rates, and commonly discussed proposals for changing federal rates typically focus on changes that are large relative to the state-level variation existing in our data; as discussed above, existing literature suggests that there are likely nonlinearities in these effects. A key implication of our findings is that young firms are more responsive to corporate tax policy than are more mature firms, a finding that is consistent with previous evidence of young firm sensitivity more generally; our quantitative estimates may have limited scope for generalization to other settings, but this key qualitative result is likely to hold elsewhere.

[41] In our data, 30 states have seen no net change in corporate tax rates during 2000-2013, while 11 states have seen net rate reductions and 7 states have seen net rate increases (we omit Michigan, Ohio, and Texas from these counts).

[42] The unconditional correlation of net change in startup employment shares and net change in corporate tax rates is -0.2.

7 Conclusion

Despite the widespread consensus that entrepreneurship is vital to the US economy, there is surprisingly limited evidence on how our key economic policies can either promote or hinder it. To help fill this gap we explore the effect of state tax policies on startup activity, finding that entrepreneurship is particularly sensitive to changes in corporate tax rates. Startups are seen to be more sensitive to these tax changes than incumbent firms. This latter finding is consistent with a growing literature showing that young firms are more responsive to a variety of shocks. Changes in personal tax rates, as we measure them, have little effect on business activity and regressions on state sales tax rates appear point negative but are not statistically significant. The results are generally not sensitive to important non-rate tax policies.

Our use of the newly developed QWI county level firm-age data allows for some of the strongest evidence to date of the effect of state taxation on startup activity. Our focus on new firms contributes importantly to existing literature on tax policy and entrepreneurship broadly.[43] Understanding the role of policy in fostering entrepreneurial activity is important in light of the key role played by young businesses in job creation, productivity, and occupational choices.

[43]Our results are interesting and useful in their own right as they inform researchers and policymakers about the aggregate consequences of various policies for business activity generally and young firm activity in particular. However, future work should augment these results by exploiting additional sources of variation such as firms legal form of organization. Richer industry variation would also be useful as a means of understanding effect heterogeneity and cross-border spillovers. Even more variation can be obtained through the use of a longer time series of firm dynamics data. These added investigations require detailed microdata.

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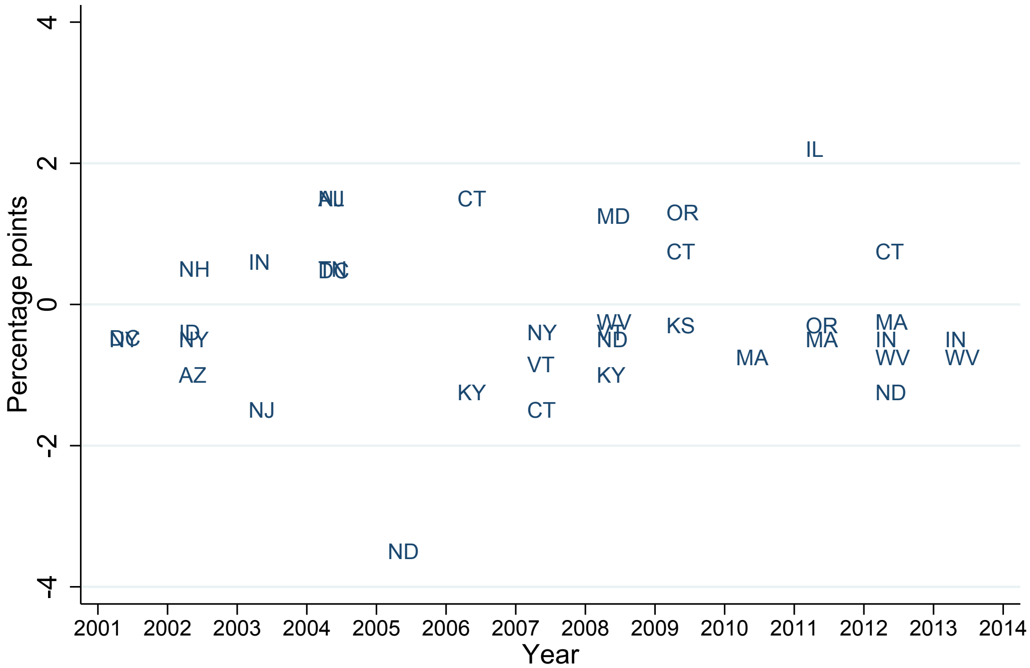
Figures and Tables

Figure 1: Corporate tax changes

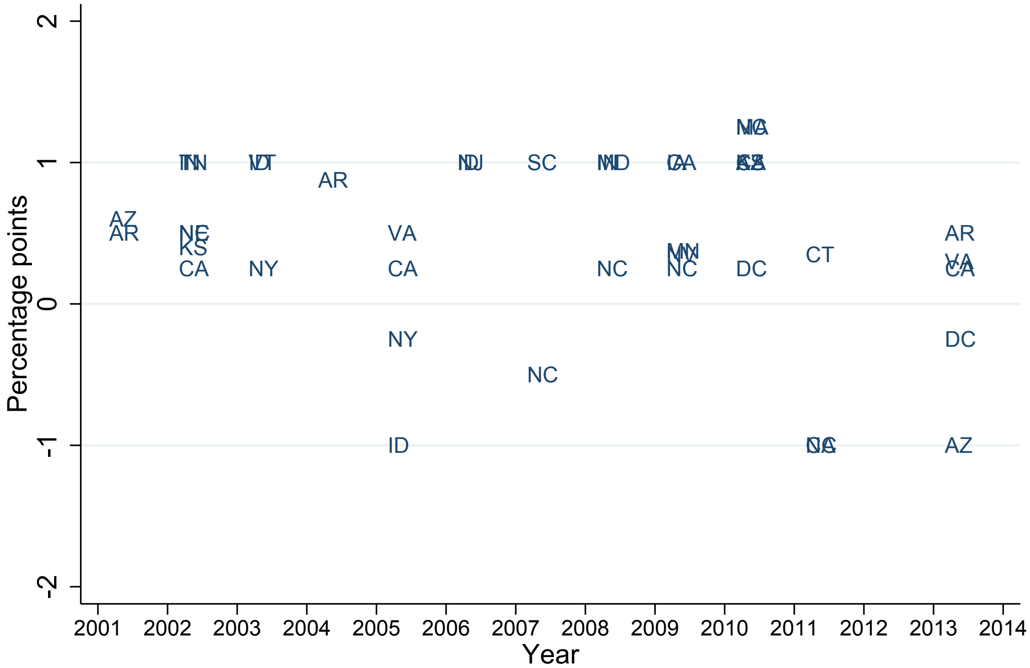


Note: Omits changes of less than 0.25 percentage point.
Data from Tax Policy Center, Book of the States, and state tax forms.

Figure 2: Personal tax changes

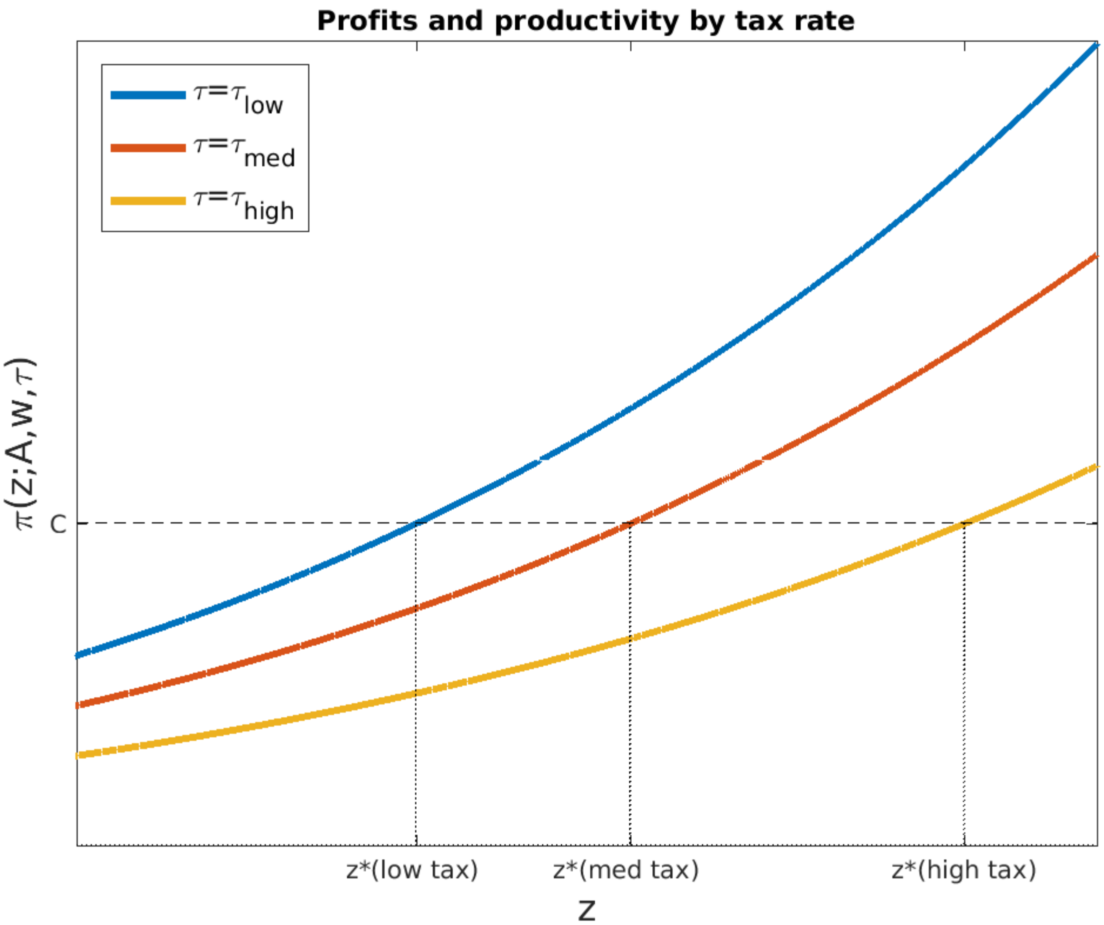
Note: Omits changes of less than 0.1 percentage point. Data from NBER TAXSIM.

Figure 3: Sales tax changes



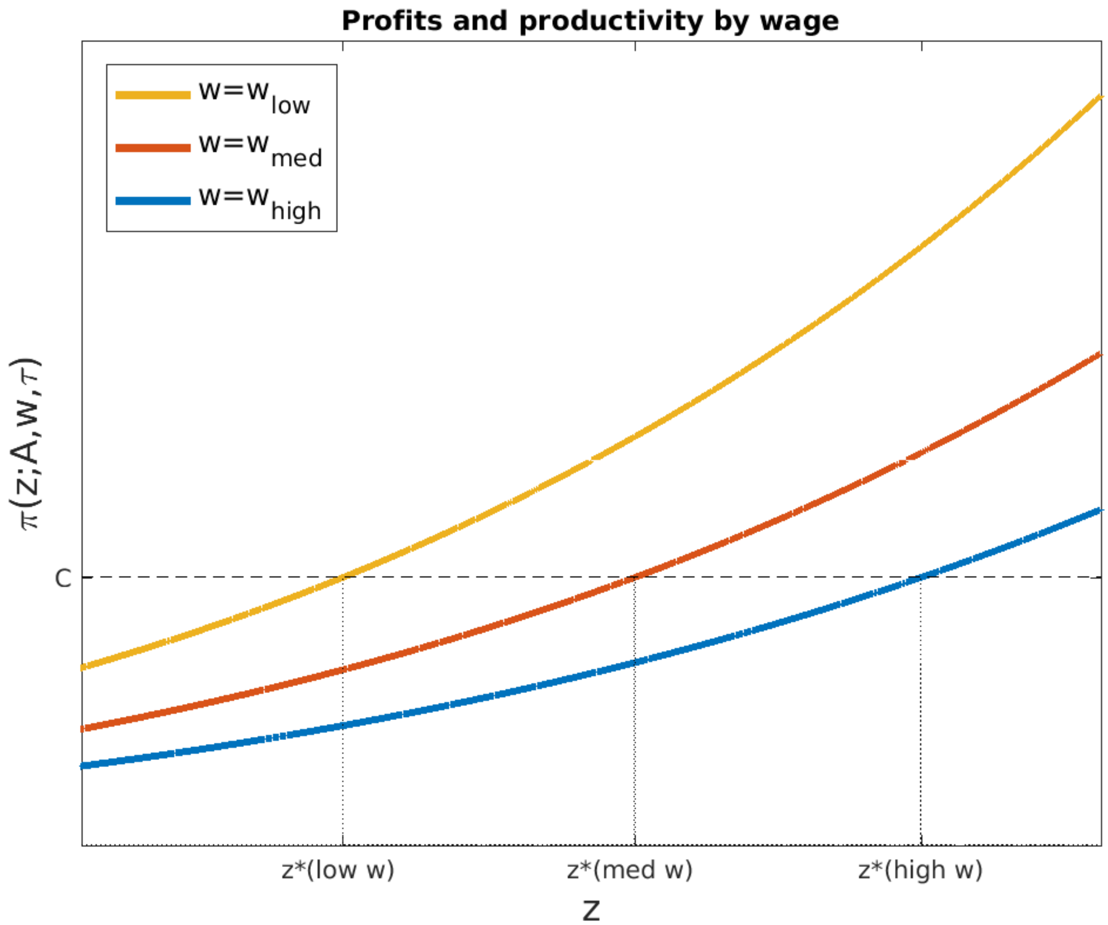
Note: Omits changes of less than 0.25 percentage point.
Data from Book of the States.

Figure 4: Productivity Thresholds for Various Tax Rates



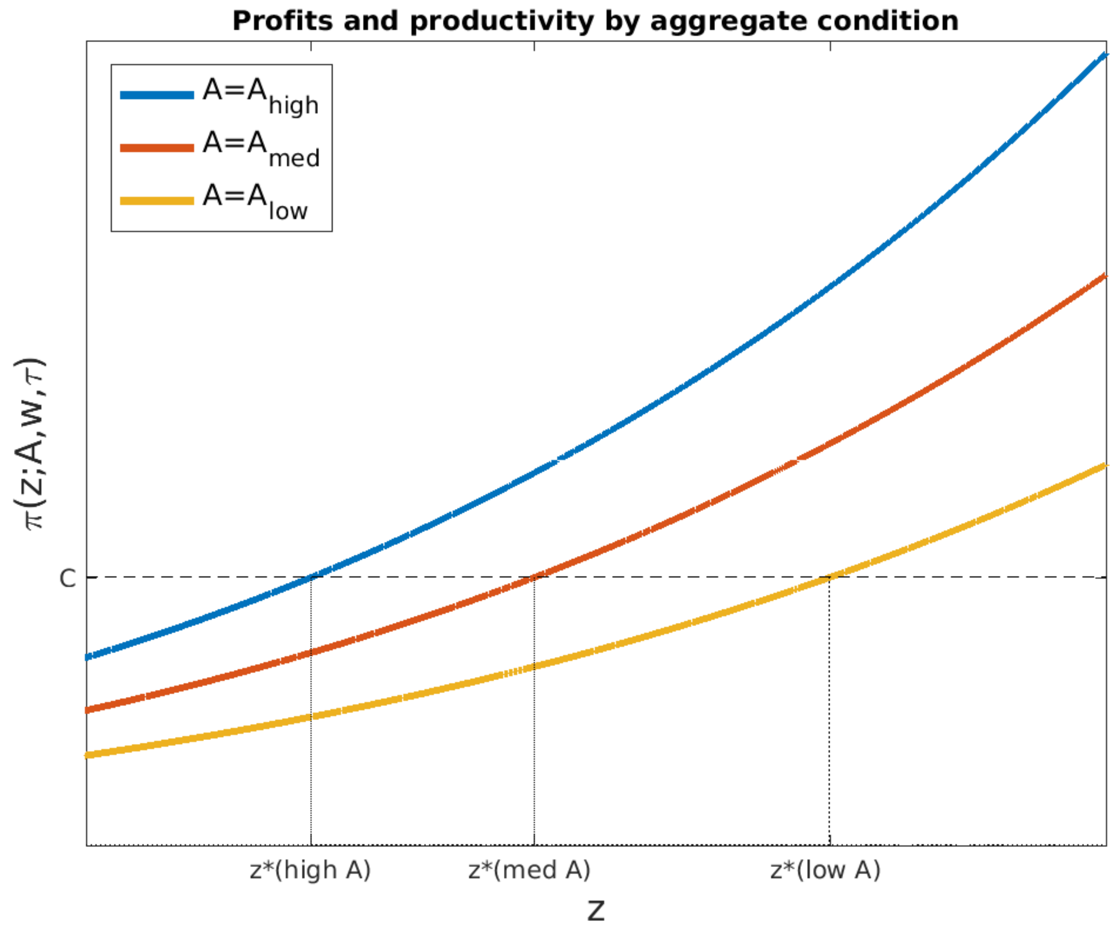
Note: Assumes constant wage.

Figure 5: Productivity Thresholds for Various Wages



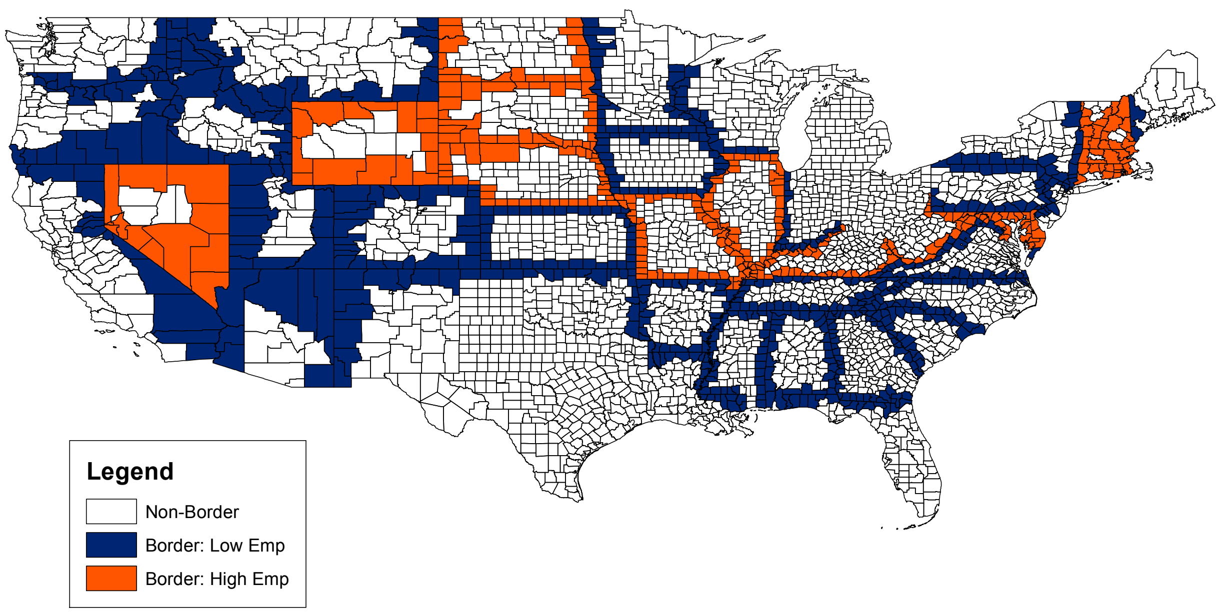
Note: Assumes constant tax rate.

Figure 6: Productivity Thresholds and Aggregate Conditions



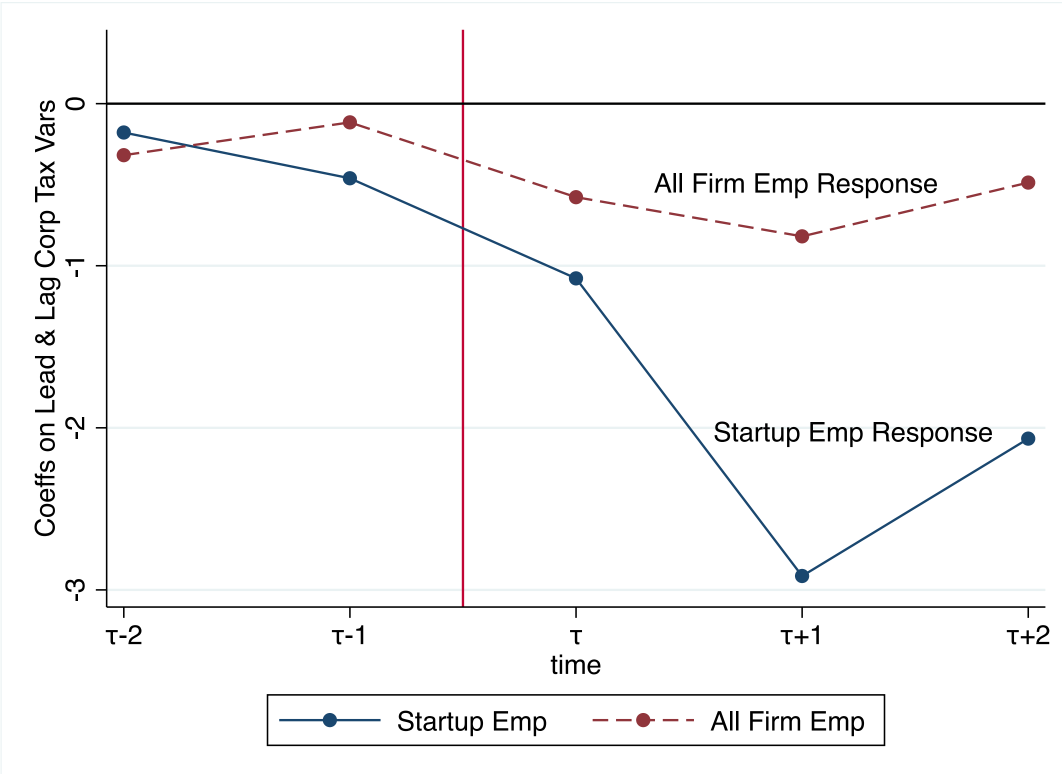
Note: Assumes constant tax rate.

Figure 7: Map of Border Counties in Sample



Note: The above figure shows the counties that are in our border sample. The counties in orange are the border counties belonging to states for whom more that 50% of their employment is located in a border county.

Figure 8: Corporate Tax Lead and Lag Coefficients



Note: The above figure reports annual lead and lag coefficients from a distributed lag specification. Specifically, we run the border discontinuity model in equation 4 and include two annual lead variables (τ-2 and τ-1) and three annual lag variables (τ, τ+1, τ+2) of the corporate tax rate variable. While none of the lagged variables are statistically significant at the 5% level, an F-test shows that the sum of the three lag coefficients is statistically significant. Using annual lags rather than quarterly lags smooths out noise in the model. Because we do not have the exact date of the tax rate changes, all changes are assumed to have occurred in the first quarter of the year they were enacted. This model necessarily excludes observation at the beginning and end years of the data for which lagged and lead variables cannot be observed.

Table 1: State Summary Statistics

State	(1) % Emp in Startups	(2) Creation Rate	(3) Startup Creation Rate	(4) Total Employment	(5) Corp Tax	(6) Income Tax	(7) Sales Tax
Alabama	0.037	0.045	0.0062	1,504,768	0.062	0.031	0.040
Arizona	0.040	0.059	0.0075	2,089,928	0.070	0.047	0.058
Arkansas	0.036	0.045	0.0064	940,756	0.065	0.071	0.059
California	0.055	0.050	0.0090	12,632,043	0.088	0.111	0.066
Colorado	0.041	0.074	0.0086	1,864,283	0.046	0.046	0.029
Connecticut	0.031	0.041	0.0052	1,403,473	0.081	0.045	0.061
Delaware	0.031	0.057	0.0059	350,093	0.087	0.076	0.000
District of Columbia	0.025	0.066	0.0053	461,161	0.099	0.092	0.058
Florida	0.046	0.062	0.0088	6,359,052	0.055	0.000	0.060
Georgia	0.040	0.051	0.0072	3,231,606	0.060	0.061	0.040
Idaho	0.052	0.057	0.0092	499,544	0.076	0.078	0.057
Illinois	0.033	0.042	0.0053	4,913,525	0.079	0.033	0.063
Indiana	0.032	0.046	0.0051	2,437,157	0.082	0.033	0.063
Iowa	0.032	0.045	0.0053	1,220,158	0.120	0.061	0.054
Kansas	0.034	0.048	0.0061	1,077,997	0.072	0.060	0.056
Kentucky	0.031	0.054	0.0056	1,448,841	0.070	0.056	0.060
Louisiana	0.039	0.070	0.0077	1,512,834	0.080	0.036	0.040
Maine	0.036	0.053	0.0066	490,793	0.089	0.088	0.050
Maryland	0.035	0.053	0.0064	2,008,510	0.076	0.054	0.055
Massachusetts	0.027	0.043	0.0053	2,839,880	0.086	0.054	0.058
Michigan	0.035	0.052	0.0060	3,481,722	0.039*	0.043	0.060
Minnesota	0.030	0.050	0.0054	2,250,684	0.098	0.084	0.067
Mississippi	0.036	0.053	0.0067	862,939	0.050	0.049	0.070
Missouri	0.036	0.044	0.0060	2,204,530	0.063	0.055	0.042
Montana	0.046	0.067	0.0093	333,263	0.068	0.071	0.000
Nebraska	0.034	0.042	0.0058	746,768	0.078	0.069	0.054
Nevada	0.048	0.050	0.0083	1,003,558	0.000	0.000	0.066
New Hampshire	0.032	0.046	0.0055	529,018	0.085	0.000	0.000
New Jersey	0.035	0.054	0.0067	3,253,716	0.089	0.073	0.066
New Mexico	0.044	0.067	0.0083	598,699	0.076	0.067	0.050
New York	0.039	0.054	0.0071	7,015,304	0.074	0.080	0.040
North Carolina	0.036	0.050	0.0065	3,228,569	0.069	0.078	0.045
North Dakota	0.039	0.053	0.0070	285,662	0.076	0.046	0.050
Ohio	0.030	0.044	0.0048	4,440,956	0.046*	0.066	0.055
Oklahoma	0.042	0.055	0.0075	1,178,840	0.060	0.057	0.045
Oregon	0.038	0.049	0.0070	1,366,102	0.070	0.096	0.000
Pennsylvania	0.030	0.051	0.0053	4,841,914	0.100	0.028	0.060
Rhode Island	0.033	0.050	0.0060	399,839	0.090	0.082	0.070
South Carolina	0.040	0.052	0.0072	1,485,633	0.050	0.070	0.055
South Dakota	0.040	0.051	0.0068	311,896	0.000	0.000	0.040
Tennessee	0.034	0.045	0.0058	2,252,021	0.064	0.000	0.069
Texas	0.043	0.047	0.0074	8,307,302	0.026*	0.000	0.063
Utah	0.049	0.056	0.0090	947,061	0.050	0.054	0.047
Vermont	0.034	0.057	0.0063	246,576	0.091	0.090	0.058
Virginia	0.035	0.059	0.0065	2,892,619	0.060	0.058	0.039
Washington	0.042	0.050	0.0080	2,298,824	0.000	0.000	0.065
West Virginia	0.035	0.048	0.0059	557,513	0.085	0.069	0.060
Wisconsin	0.029	0.041	0.0049	2,329,356	0.079	0.072	0.050
Wyoming	0.048	0.073	0.0099	202,579	0.000	0.000	0.040
Average	0.037	0.053	0.0067	2,227,344	0.067	0.053	0.050

Note: The state level summary statistics are calculated using QWI data from 2000-2014. The employment figures are for private-sector, non-farm jobs. The startup creation rate is calculated by dividing the number of newly created jobs at firms less than two years old by the *total* employment in the state. The ratio of the startup creation rate to the overall creation rate shows the percent of newly created jobs in a state that come from startup firms. Texas, Ohio and Michigan are excluded because of changing corporate tax systems. Massachusetts is excluded from the data as it did not join the QWI until 2010.

Table 2: Summary Statistics

	(1)	(2)	(3)	(4)
	All Firms All Cntys	All Firms Border Cntys	New Firms All Cntys	New Firms Border Cntys
Employment	42,535	36,780	2,388	1,938
	(161,436)	(136,036)	(10,061)	(6,922)
% Total Employment			0.0646	0.0643
			(0.0399)	(0.0395)
Avg. Monthly Earn	2,563	2,549	1,892	1,872
	(800)	(790)	(795)	(846)
Creation Rate	0.0603	0.0611	0.2029	0.2134
	(0.0200)	(0.0200)	(0.1070)	(0.1174)
Destruction Rate	0.0584	0.0592	0.1113	0.1189
	(0.0209)	(0.0203)	(0.1416)	(0.1492)
Population	94,091	83,045	94,091	83,045
	(306,479)	(238,102)	(306,479)	(238,102)
Counties	3,128	1,135	3,128	1,135
Observations	213,223	76,777	213,223	76,777

Note: The above table provides summary statistics for all counties, border counties, all firms and new firms. Border counties are shown to be slightly smaller on average. New firms comprise roughly 6.4% of employment for all counties and for border counties.

Table 3: Base Panel Regressions, All Counties

	Results: 0-1 Year Old Firms					Results: All Firms	
	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)	(5) Emp Growth	(6) ln(Emp)	(7) Emp Growth
	Corporate Taxes, Personal Taxes, Sales Taxes						
Corp Rate	-4.353**			-4.242***	-0.0419*	-1.844**	-0.108
	(1.621)			(1.571)	(0.0245)	(0.818)	(0.0860)
Personal Rate		-0.957		-0.630	-0.00991	0.0241	0.0420
		(1.159)		(1.012)	(0.0109)	(0.625)	(0.0333)
Sales Tax			-1.934	-1.156	-0.0127	-1.429	-0.0252
			(2.027)	(1.861)	(0.00827)	(1.169)	(0.0463)
Observations	143,380	143,380	143,380	143,380	142,756	144,728	143,710
R^2	0.934	0.933	0.933	0.934	0.059	0.996	0.080

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$

Note: These results are obtained from a basic differences-in-differences model where both border and non-border U.S. counties are included. See Section 4.1 and equation 1 for details on the model.

Table 4: Border Discontinuity Results

	Results: 0-1 Year Old Firms					Results: All Firms	
	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)	(5) Emp Growth	(6) ln(Emp)	(7) Emp Growth
	Corporate Taxes, Personal Taxes, Sales Taxes						
Corp Rate	-3.742**			-3.626**	-0.0463	-1.570***	-0.133
	(1.737)			(1.696)	(0.0287)	(0.422)	(0.0880)
Personal Rate		0.351		0.719	0.00887	0.332	0.00900
		(0.848)		(0.850)	(0.00822)	(0.371)	(0.0207)
Sales Tax			-2.694	-2.079	0.0101	0.904	0.0673
			(2.194)	(1.957)	(0.0154)	(0.560)	(0.0436)
Observations	70,949	70,949	70,949	70,949	70,402	70,949	70,416
R^2	0.908	0.907	0.907	0.908	0.064	0.996	0.020

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$

Note: Above are the main border discontinuity results from equation 4 in the paper. The unit of observation is a county-border with all variables defined as the difference between the two counties that share the border, as seen in equation 3. Border pair fixed effects are included to account for time invariant differences between the two counties. Any shock that occurs to a border pair in a particular quarter is absorbed through this differencing method. See text for additional details

Table 5: Spillover Results

	Results: 0-1 Year Old Firms					Results: All Firms	
	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)	(5) Emp Growth	(6) ln(Emp)	(7) Emp Growth
	Corporate Taxes, Personal Taxes, Sales Taxes						
Corp Rate	-0.600			-0.541	-0.00865**	0.240	-0.0174
	(0.463)			(0.459)	(0.00336)	(0.222)	(0.0138)
Personal Rate		-0.247		-0.177	0.00355	-0.303	0.0369
		(0.642)		(0.654)	(0.00313)	(0.339)	(0.0444)
Sales Tax			-1.056	-0.915	-0.000911	-0.657	-0.00983
			(1.130)	(1.133)	(0.00475)	(0.710)	(0.0237)
Observations	32,114	32,114	32,114	32,114	31,978	32,114	31,978
R^2	0.937	0.937	0.937	0.937	0.056	0.997	0.052

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$

Note: This table reports results from regressions that test whether border counties experience spillovers from policy changes that happen in adjacent states. The regressions are run only on border county observations. The outcome The variables "Corp Rate Border", "Personal Rate Border" and "Sales Tax Border" are each defined as the policy in the adjacent state to the border county. The left-hand side is the difference between the outcome variable of interest in the border county and average interior county of the state it belongs to.

Table 6: Dropping Border Dominated States

	Results: 0-1 Year Old Firms					Results: All Firms	
	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)	(5) Emp Growth	(6) ln(Emp)	(7) Emp Growth
	Corporate Taxes, Personal Taxes, Sales Taxes						
Corp Rate	-5.280***			-5.224***	-0.0313	-2.114***	-0.0979
	(1.949)			(1.927)	(0.0342)	(0.684)	(0.104)
Personal Rate		0.625		0.885	0.0128	0.108	0.0303
		(0.968)		(1.012)	(0.0104)	(0.425)	(0.0236)
Sales Tax			-2.569	-2.276	0.00137	-0.0606	0.0607
			(2.324)	(2.194)	(0.0140)	(0.509)	(0.0438)
Observations	50,825	50,825	50,825	50,825	50,417	50,825	50,423
R^2	0.904	0.904	0.904	0.904	0.052	0.996	0.015

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$

Note: This table reports results using the same border discontinuity method reported in Panel A of Table 4 but drop any state where more than 50% of employment is located in counties that border other states. If border counties comprise the majority of employment in a state then a state's policy decision is less likely to be exogenous to the economic conditions of it's border counties.

Table 7: Tax Base Controls & Job Flows Outcomes

	(1) ln(Startup Emp)	(2) Startup Emp Growth	(3) Reallocation Rate	(4) Creation Rate	(5) Destruction Rate	(6) ln(All Emp)	(7) All Emp Growth
Panel A: Baseline Results							
Corp Rate	-3.742**	-0.0446	-0.206	-0.166	-0.0397	-1.454***	-0.126
	(1.737)	(0.0280)	(0.169)	(0.126)	(0.0469)	(0.452)	(0.0866)
Observations	70,949	70,402	70,402	70,402	70,402	70,402	70,402
R^2	0.908	0.064	0.378	0.221	0.207	0.996	0.020
Panel B: Controlling for R & D Subsidies							
Corp Rate	-5.252***	-0.0218*	-0.126	-0.109*	-0.0176	-1.193**	-0.0915**
	(1.627)	(0.0128)	(0.105)	(0.0649)	(0.0449)	(0.476)	(0.0390)
R&D Tax Credits	-0.00259	-0.000779	0.0249**	0.0127**	0.0123**	0.133**	0.000559
	(0.248)	(0.00166)	(0.00980)	(0.00624)	(0.00590)	(0.0546)	(0.00720)
Observations	60,465	59,921	59,921	59,921	59,921	59,921	59,921
R^2	0.909	0.049	0.386	0.208	0.208	0.996	0.013
Panel C: Loss Carry Forward Years							
Corp Rate	-3.727**	-0.0439	-0.204	-0.163	-0.0407	-1.430***	-0.122
	(1.743)	(0.0281)	(0.169)	(0.126)	(0.0467)	(0.442)	(0.0871)
Loss Carry Forward Years	0.00119	0.0000486*	0.000129	0.000195**	-0.0000663	0.00167	0.000262**
	(0.00107)	(0.0000270)	(0.000112)	(0.0000985)	(0.0000511)	(0.00103)	(0.000110)
Observations	70,949	70,402	70,402	70,402	70,402	70,402	70,402
R^2	0.908	0.064	0.378	0.221	0.207	0.996	0.020
Panel D: Controlling for Apportionment							
Corp Rate	-3.876**	-0.0462*	-0.189	-0.152	-0.0366	-1.501***	-0.116
	(1.830)	(0.0269)	(0.164)	(0.121)	(0.0460)	(0.480)	(0.0838)
Apportion x Corp Rate	0.549	0.00654	-0.0677	-0.0549	-0.0128	0.191	-0.0423*
	(1.340)	(0.00773)	(0.0897)	(0.0510)	(0.0413)	(0.461)	(0.0241)
Observations	70,949	70,402	70,402	70,402	70,402	70,402	70,402
R^2	0.908	0.064	0.378	0.221	0.207	0.996	0.020
Panel E: Interacting Corporate Tax with County Corp Emp Share							
Corp Rate	-3.006	-0.0512	-0.276	-0.214	-0.0619	-1.839**	-0.152
	(2.311)	(0.0351)	(0.212)	(0.154)	(0.0630)	(0.811)	(0.103)
Corp Share x Corp Rate	-3.345	0.0300	0.320	0.219	0.101	1.751	0.117
	(7.192)	(0.0651)	(0.488)	(0.344)	(0.158)	(1.871)	(0.221)
Observations	70,949	70,402	70,402	70,402	70,402	70,402	70,402
R^2	0.908	0.064	0.378	0.221	0.207	0.996	0.020
Panel F: Minimum Wage							
Corp Rate	-3.738**	-0.0443	-0.206	-0.165	-0.0407	-1.421***	-0.125
	(1.737)	(0.0280)	(0.169)	(0.125)	(0.0470)	(0.454)	(0.0861)
ln(Min Wage)	-0.00983	-0.000744	0.000400	-0.00181	0.00221	-0.0773***	-0.00405
	(0.0878)	(0.000703)	(0.00480)	(0.00300)	(0.00292)	(0.0230)	(0.00347)
Observations	70,949	70,402	70,402	70,402	70,402	70,402	70,402
R^2	0.908	0.064	0.378	0.221	0.207	0.996	0.020

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.001$
Note: This table provides results for additional outcomes when other state policies are included in the regressions.

A Appendix

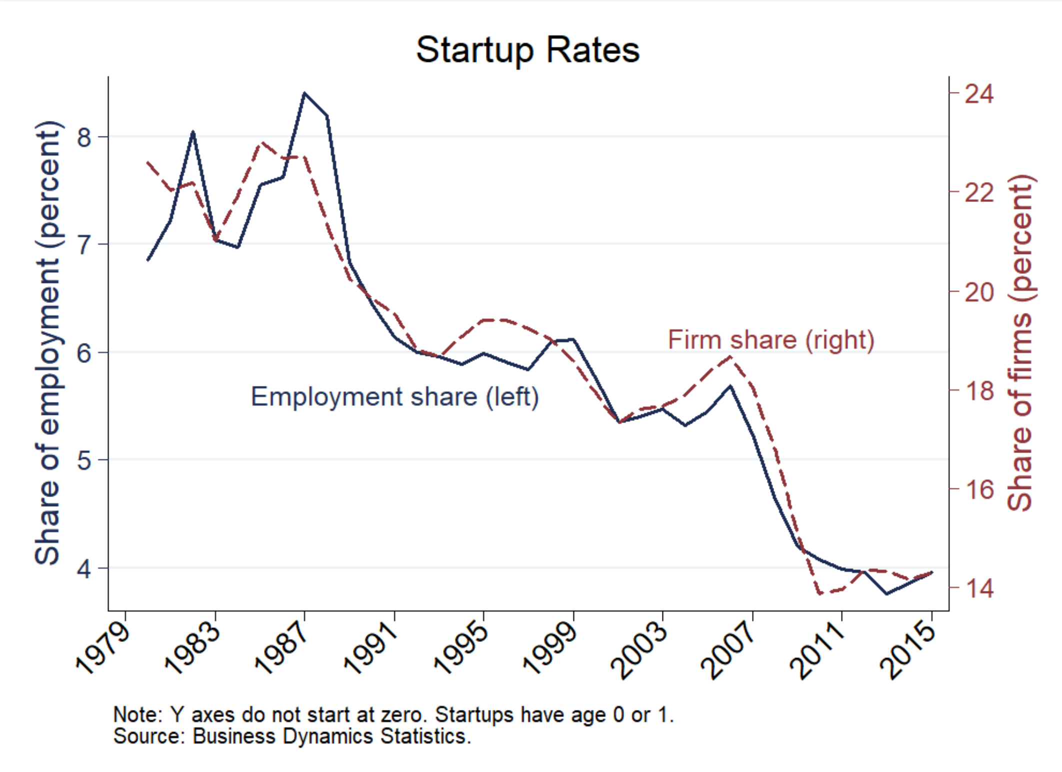
The primary data source used in the paper is the Quarterly Workforce Indicators data which were downloaded from Cornell's Economics Compute Cluster Organization. Corporate tax rate data are obtained from the Tax Foundation and supplemented with data from the Book of States and state tax forms and instructions. We use the top corporate tax rate, though the top bracket varies from state to state. States also differ in how they determine the amount of a firm's economic activity that is located in their state, though location of a firm's employment is key part of this determination; we obtain apportionment rules from Book of the States.

Personal tax rate data are obtained from NBER's Taxsim program. To obtain the maximum state tax rate by year, the Taxsim model assumes income of $1.5 million and includes a variety of other tax policies such as the mortgage interest deduction. The Taxsim model also allows users to input any income assumptions. We obtain median income estimates by state and year from the Census Bureau.[44] and input these data as labor income. We assume married filing jointly with zero dependents and zero other income (spouse, dividend, Social Security, unemployment compensation, etc.) and no itemized deductions.

We obtain state sales tax rates from Book of the States. Minimum wage data are largely based on the file provided by Meer and West (2016) but is extended using the Department of Labor's State Minimum Wage Report which can be found at https://www.dol.gov/whd/state/stateMinWageHis.htm.

[44]Table H-8 at http://www.census.gov/data/tables/time-series/demo/income-poverty/historical-income-households.html

Figure A1: Startup Rate Trends



Note: This figure was created using data from the Business Dynamic Statistics (BDS). The red dashed line is the share of new firms in the economy while the solid blue line is the share of workers employed at new firms. This downward trend is also observed in other measures of economic dynamism including job reallocation and worker turnover. We use BDS data because historical data from the QWI is state-specific, and many states do not report data prior to 2000.

Table A1: Dropping Counties With Fewer than 1,000 Workers

	Results: 0-1 Year Old Firms					Results: All Firms	
	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)	(5) Emp Growth	(6) ln(Emp)	(7) Emp Growth
	Corporate Taxes, Personal Taxes, Sales Taxes						
Corp Rate	-2.561**			-2.463**	-0.0322	-1.572***	-0.158**
	(1.258)			(1.234)	(0.0219)	(0.379)	(0.0800)
Personal Rate		-0.319		-0.0555	0.000184	0.411	0.00593
		(0.863)		(0.888)	(0.00773)	(0.267)	(0.0205)
Sales Tax			-1.954	-1.500	0.0182	0.790	0.0376
			(2.026)	(1.853)	(0.0135)	(0.644)	(0.0500)
Observations	94,871	94,871	94,871	94,871	94,136	94,871	94,170
R^2	0.894	0.893	0.893	0.894	0.060	0.995	0.019

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: This table reports results using the same border discontinuity method reported in Panel A of Table 4 but drops counties with 1,000 or fewer worker. Results are shown to be quite similar to the baseline border discontinuity results which drop counties with fewer than 3,000 workers.

Table A2: Results by Firm Age Group

	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)	(5) Emp Growth	(6) ln(Emp)	(7) ln(Emp)	(8) ln(Emp)	(9) ln(Emp)	(10) Emp Growth
	Results: 0-1 Year Old Firms					**Results: 2-3 Year Old Firms**				
Corp Rate	-3.742**			-3.626**	-0.0463	-1.821*			-2.034*	0.000933
	(1.737)			(1.696)	(0.0287)	(1.082)			(1.168)	(0.00515)
Personal Rate		0.351		0.719	0.00887		-0.130		-0.143	0.000193
		(0.848)		(0.850)	(0.00822)		(1.303)		(1.273)	(0.00230)
Sales Tax			-2.694	-2.079	0.0101			2.086	2.540	0.00277
			(2.194)	(1.957)	(0.0154)			(2.482)	(2.576)	(0.00506)
Observations	70,949	70,949	70,949	70,949	70,402	70,625	70,625	70,625	70,625	70,094
R^2	0.908	0.907	0.907	0.908	0.064	0.971	0.971	0.971	0.971	0.019
	Results: 4-5 Year Old Firms					**Results: 6-10 Year Old Firms**				
Corp Rate	-3.031			-3.208	-0.00208	0.0284			0.00893	-0.0314**
	(2.449)			(2.492)	(0.00488)	(0.769)			(0.763)	(0.0136)
Personal Rate		-1.462		-1.454	-0.00235		0.961		1.012	0.00441
		(1.149)		(1.144)	(0.00279)		(0.670)		(0.675)	(0.00338)
Sales Tax			2.573	3.534	0.000523			-0.681	-0.883	-0.00225
			(2.243)	(2.149)	(0.00565)			(1.561)	(1.595)	(0.00982)
Observations	70,591	70,591	70,591	70,591	70,060	71,110	71,110	71,110	71,110	70,576
R^2	0.955	0.955	0.955	0.955	0.019	0.955	0.955	0.955	0.955	0.015
	Results: 11+ Year Old Firms					**Results: All Firms**				
Corp Rate	-1.435***			-1.498***	-0.0533	-1.488***			-1.570***	-0.133
	(0.530)			(0.503)	(0.0426)	(0.448)			(0.422)	(0.0880)
Personal Rate		0.302		0.282	0.00327		0.254		0.332	0.00900
		(0.367)		(0.402)	(0.0143)		(0.364)		(0.371)	(0.0207)
Sales Tax			0.548	0.802	0.0553**			0.553	0.904	0.0673
			(0.725)	(0.702)	(0.0276)			(0.620)	(0.560)	(0.0436)
Observations	71,326	71,326	71,326	70,949	70,416	71,326	71,326	71,326	70,949	70,416
R^2	0.994	0.994	0.994	0.994	0.012	0.996	0.996	0.996	0.996	0.020

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Border Discontinuity results by firm age group. Importantly, it should be noted that these results do not track the same businesses over time. Therefore the composition of firms in each category is changing over the sample period.

Table A3: Three Years Surrounding Corporate Tax Changes

	Results: 0-1 Year Old Firms					Results: All Firms	
	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)	(5) Emp Growth	(6) ln(Emp)	(7) Emp Growth
Corporate Taxes, Personal Taxes, Sales Taxes							
Corp Rate	-4.133			-4.468	-0.0365	-1.704***	-0.140
	(3.106)			(3.165)	(0.0396)	(0.594)	(0.108)
Personal Rate		-1.460		-0.577	-0.0335	1.320	-0.0667
		(2.162)		(2.000)	(0.0267)	(0.832)	(0.0720)
Sales Tax			-4.863		-0.0109	0.867	0.0224
			(3.789)		(0.0226)	(0.534)	(0.0891)
Observations	21,687	21,687	21,687	21,687	21,458	21,687	21,460
R^2	0.910	0.909	0.909	0.909	0.096	0.997	0.037

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: This table reports results using the same border discontinuity method reported in Table 4 but keeps only data in the three years before and after changes in the relative corporate tax rate. Observations with no changes are not included in the sample.

Table A4: Including State-Specific Trends

	Results: 0-1 Year Old Firms					Results: All Firms	
	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)	(5) Emp Growth	(6) ln(Emp)	(7) Emp Growth
	Corporate Taxes, Personal Taxes, Sales Taxes						
Corp Rate	-2.385			-2.239	-0.0190	-1.101	-0.0446
	(2.076)			(2.067)	(0.0211)	(0.659)	(0.0664)
Personal Rate		0.511		0.741	0.0108*	0.384	0.0255
		(1.146)		(1.167)	(0.00631)	(0.377)	(0.0249)
Sales Tax			-3.113	-2.878	0.00925	0.791	0.117***
			(2.861)	(2.881)	(0.0164)	(0.650)	(0.0432)
Observations	70,949	70,949	70,949	70,949	70,481	70,949	70,416
R^2	0.908	0.908	0.908	0.908	0.070	0.996	0.023

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table reports results using the same border discontinuity method reported in Panel A of Table 4 but now includes state specific linear trends. As discussed in prior empirical literature, including these state-specific trends is likely to absorb some of the treatment effect. Whether to include these trends is debated considerably in the minimum wage literature as inclusion of the trends may absorb some of the treatment effect.

Table A5: Dropping Distorted Values

	Results: 0-1 Year Old Firms					Results: All Firms	
	(1) ln(Emp)	(2) ln(Emp)	(3) ln(Emp)	(4) ln(Emp)	(5) Emp Growth	(6) ln(Emp)	(7) Emp Growth
Corporate Taxes, Personal Taxes, Sales Taxes							
Corp Rate	-3.266*			-3.098*	-0.0482*	-1.571***	-0.133
	(1.775)			(1.733)	(0.0289)	(0.421)	(0.0880)
Personal Rate		0.161		0.520	0.0100	0.334	0.00935
		(0.848)		(0.876)	(0.00733)	(0.370)	(0.0207)
Sales Tax			-2.983	-2.438	0.00593	0.924*	0.0671
			(2.007)	(1.824)	(0.0139)	(0.560)	(0.0438)
Observations	68,632	68,632	68,632	68,632	68,096	70,841	70,311
R^2	0.916	0.916	0.916	0.916	0.079	0.996	0.020

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
This table reports results using the same border discontinuity method reported in Panel A of Table 4 but now drops observations that are flagged as distorted in the QWI.